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How Quickly We Forget: Comments on “A Historical Analysis of Evidence- Based Practice in Social Work: The Unfinished Journey toward an Empirically Grounded Profession”

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The *Social Service Review* article, “A Historical Analysis of Evidence-Based Practice in Social Work: The Unfinished Journey toward an Empirically Grounded Profession,” by Nathanael J. Okpych and James L-H Yu, is the kind of article that makes you sit up and say, “That’s revisionist history!” “What new ideas!” “I wish I had written that!” “But that’s not what I know!” Such an ambivalent response suggests that it is a groundbreaking article or at least one that will stimulate discussion and reassessment of what we know. Constructivism suggests both that there are many legitimate worldviews and that it is worth knowing those multiple worldviews. In that spirit, I would like to acknowledge the intriguing brilliance of the article but also pose some questions and other views.

ASSUMPTIONS

Okpych and Yu address the history of social work practice (casework) in the United States. They view US social work practice as paradigms or world-

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views akin to Thomas Kuhn's scientific paradigms ([1962] 1970). They propose three paradigms that legitimate social work practice historically: (1) a moral-based paradigm, in which intervention was rationalized by moral depravity (early charity organization society and settlement movements); (2) an authority-based paradigm, in which practice was legitimated by the authority of experts such as Mary Richmond, Sigmund Freud, and participants in the Milford Conference; and (3) an empirically based paradigm, in which evidence of effectiveness legitimates practice. They divide the last period into two contending parts, empirical clinical practice (ECP) and evidence-based practice (EBP).

Paradigm shifts occur after a crisis precipitated by the inability of "normal science" to answer key questions in the discipline (Kuhn [1962] 1970, 23). According to Okpych and Yu, the crisis that precipitated the shift from moral-based to authority-based social work paradigms was the concern with professionalization that led to claims of expert, organized knowledge that distinguished professionals from laypersons. The shift from an authority-based to an empirically based paradigm was precipitated by crises when the environment demanded accountability but the profession could not demonstrate the effectiveness of its interventions (the 1950s and 1960s).

The argument about practice paradigm shifts, while intriguing and compelling, rests on a key assumption: that a social work practice paradigm is equivalent to Kuhn's concept of a scientific paradigm. Okpych and Yu describe Kuhn's description of paradigm as "an orientation to and template for legitimate science," or a worldview or point of view (2014, 6). They conclude that social work shows "a strikingly similar pattern" to shifts in scientific paradigms: unanswered anomalies, a crisis, a new crude paradigm to explain the anomalies, and refinement of the paradigm (7). Their descriptions of the social work paradigms and paradigm shifts are clear and enticing. Still, there is no rationale for a social work paradigm except the claimed similarity of patterns. There is a logic gap: *A* shows a pattern, *B* shows a similar pattern, therefore *A* and *B* are the same thing. This key assumption needs more development and rationale.

If one is comfortable with the idea that social work practice paradigms are like Kuhnian scientific paradigms, there are still several questions about the authors' analyses. First is a philosophical question: How can a social work "empirically based paradigm" be a paradigm when it does not reach practice (Okpych and Yu 2014, 8)? Paradigms are "shared commitments to scientific beliefs, values, and symbolic generalizations" (6). But, in the

United States, the empirically based paradigm is an ideal in the university community but not among those who practice. The moral- and authority-based paradigms were based on practice wisdom and values and were accepted, indeed developed, by the practitioners. The empirically based paradigm comes from outside practice (the “academy”; 19), and after more than 40 years the practice community has not accepted its empirically based methods of solving practice problems. So, is it a paradigm? Can it be a paradigm without practitioner buy-in? Or are we in the middle of a long crisis with no accepted new paradigm to guide practice?

A second question raised by the notion of paradigm shifts is how to reconcile previous interpretations of social work practice history. Previous accounts concur that the Flexner report (1915) was a turning point in social work, the beginning of strivings for professional status. However, the previous accounts portrayed social work since Flexner as a more or less unbroken trajectory for inclusion of a scientific base for social work (Reid 1994; Kirk and Reid 2002; Fortune 2010). Even before 1915, leaders of the charity organization society talked of the science of charity as systematic and based in evidence. In this view, Mary Richmond’s *Social Diagnosis* (1917) codified scientific charity when applied to individuals (Kirk and Reid 2002) and did not, as Okpych and Yu suggest, simply compile the authoritative knowledge of an esteemed expert (the authority-based paradigm). Thereafter, efforts to evaluate intervention outcomes moved from sporadic attempts in the 1920s to larger-scale controlled studies in the 1950s and 1960s, including studies of intervention processes as well as outcomes (Fortune 2012). The results of these studies suggest that social work practice was not only ineffective but was detrimental to its clients (Mullen and Dumpson 1972; Fischer 1973). The ensuing outcry led to what was then called “the age of accountability” (Briar 1973, 2) and interest in the black box, or what elements of social work (or other counseling interventions) might be effective or not. Okpych and Yu view the same events as the crisis that precipitated the paradigm shift from the moral authority to the empirically based paradigm. Both accounts of history concur that the 1960s were a key turning point in social work practice. But Okpych and Yu view it as a paradigm shift, whereas previous accounts view it as validation for the efforts to build the research evidence base that began at the turn of the twentieth century. I do not know if either view is correct, but this is a good example of why revisionist history, or multiple viewpoints, helps interpret and reinterpret cherished symbols.

WHAT'S THE DIFFERENCE? EMPIRICAL CLINICAL PRACTICE (ECP) VERSUS EVIDENCE-BASED PRACTICE (EBP)

A central contention of the article (one that gives it the title) is that the recent social work practice paradigm that the authors initially label “empirically based” (Okpych and Yu 2014, 3) is two different approaches: empirical clinical practice (ECP) and evidence-based practice (EBP; based on the evidence-based medicine [EBM] movement from Britain). Okpych and Yu note that EBP as a movement entered social work “with all of the force and fury of a major revolution” (3) and that “most contemporary scholarship on EBP manifests a relatively short memory . . . [treating ECP] as a forgone chapter in the history of social work” (4). I agree with their assessment of forgetfulness. Scholars forget that ECP and EBP share many critical elements. As Okpych and Yu describe, ECP practice models include three steps: practitioners individually search empirical literature, select appropriate empirically tested interventions, and systematically evaluate client outcomes. These common elements of ECP are the same elements as EBP (Danya International 2008), although, thanks in part to Eileen Gambrill and Leonard Gibbs (2009; Gambrill 2012), EBP is more sophisticated. One essential difference between ECP and EBP is the alluring inclusion of British and medical authority in EBP.

LOST APPROACHES TO EMPIRICALLY BASED PRACTICE

Okpych and Yu define practice models as the process of information retrieval and assessment, application of retrieved knowledge, and evaluation of outcomes. The ECP movement also included evidence-based intervention approaches or tested interventions that were generalizable and intervention-focused (as opposed to problem-focused; Reid 1994; Fortune, McCallion, and Briar-Lawson 2010). They usually included tested interventions and research techniques disguised as practice techniques. Often, the developer used a research approach called “research and development” (R&D) or “design and development” (D&D), a long-term program of tests, incremental refinements, and additional tests (Rothman, Erlich, and Teresa 1976; Thomas 1978; Fraser et al. 2009). These approaches were portable or designed to apply in multiple settings. Practitioners could learn an approach and use it in almost any setting, without needing constant access to new

resources and often without distinguishing between research and practice techniques.

The most widely known of these approaches was William J. Reid and Laura Epstein's Task-Centered Model (Reid and Epstein 1972; Reid 2000), a generalist, short-term approach that could be used by itself or as a component of a longer-term approach in myriad settings, with many populations and many problems. Reid and colleagues improved the approach through developmental research programs from the 1970s through 1990s (Fortune 2012), but the typical practitioner did not need to know the corpus of formal research. Interventions such as the problem-specification assessment process, evaluation of clients' task accomplishment, and recontracting for new tasks were research methods that were incorporated into the practice approach as intervention skills.

Another approach built on research evidence was Sheldon D. Rose's behavioral group treatment (1980). Teams of practitioners, doctoral students, and master's degree students tested and improved the approach first for a core of effective interventions and then for applications with new populations and problems. Rose was one of the first to insist on detailed protocols that specified practitioner interventions, not only for fidelity but to study the effect of variations in practitioner behavior (Rose 1988). A third approach was Edwin J. Thomas's Unilateral Family Therapy, which aimed to enable cooperating nonalcoholic spouses to change the behaviors of uncooperative alcoholic family members (Thomas and Santa 1982; Thomas et al. 1987). Jack Rothman similarly developed an approach to social action (Rothman et al. 1976). More recently, several scholars used a developmental research approach to build school- and community-based approaches to prevent youth misbehavior (Catalano and Hawkins 1996; Catalano et al. 2002; Fraser et al. 2011).

My point in this catalog of empirically based intervention approaches in the 1960s–1990s (and beyond) is that the ECP movement included a strong component that developed generalized intervention approaches based on research evidence and continued to improve them based on research. Practitioners could use them in multiple situations with some confidence that they were effective, or at least empirically based, without having to rely on sophisticated problem-specific information retrieval and research methods that were not supported in the practice environment. Most current proponents of EBP have forgotten this legacy, or never knew of it. It is a shame

that one of the few genuine and successful research-based efforts in social work should have fallen into the morass of paradigm battles.

FUTURE CHALLENGES TO EMPIRICALLY BASED PRACTICE

Okpych and Yu do a nice job discussing challenges faced by both ECP and EBP. Three are especially important: the adequacy of the knowledge base about effective interventions, stakeholder perceptions that implementation is difficult, and inadequate dissemination efforts (Okpych and Yu 2014). Despite the improvements in the knowledge base, much of the evidence is from other fields, is focused on individual or family change, and does not address the problems social work deals with. Despite the increase in quantity and quality of social work research, there is still little research on social work practice interventions. Studies of doctoral dissertations, which launch new scholars' careers, show few dissertations on intervention or practice (Horton and Hawkins 2010; Lach, Rothwell, and Blumenthal 2014); indeed, one study showed a decrease from 11 percent to 6 percent between 1998–2000 and 2008–10 (Johnson, McMillan, and Powers 2012).

Second, as discussed, practitioners have embraced neither EBP nor empirically based interventions. Okpych and Yu note “stakeholder doubts about the feasibility of engaging in empirically grounded practice due to inadequate resources, time constraints, and other practical barriers, as well as concerns about inflexible treatment protocols” (2014, 35). In my opinion, the reliance on individual practitioners to seek and implement interventions for each specific problem they encounter is unrealistic. A different tactic may be to develop more generalized, portable evidence-based approaches such as an updated task-centered model, Chorpita’s modular collection of effective behavioral interventions (2007), or various approaches using generic common elements (Fortune et al. 2010; Barth et al. 2014). Another approach, underlying British EBM, is to build the knowledge, resources, and institutional or disciplinary authority to enforce conformity.

Third, when acceptable empirically based practice approaches are available, the methods used to disseminate them have been limited and crude and, as Okpych and Yu note, have not taken into account the context, especially larger systems. Fortunately, more sophisticated attempts at translational research (from research study to practice) have improved our knowledge of barriers, individual and organizational preparedness, and how to make the organizational environment more receptive to empirically based

practice (Fortune et al. 2010; Roberts-DeGennaro and Fogel 2011; Brownson, Colditz, and Proctor 2012; Glisson 2012). A different, promising approach requires the researcher to continue beyond highly controlled research like randomized controlled trials to develop approaches that account for the uncontrollable contingencies of the practice environment (Wilson and Fridinger 2008). This shifts the responsibility for acceptance of empirically based interventions from individual practitioners and organizations to the intervention developers: the researchers.

CONCLUSION

My thanks to Okpych and Yu for their stimulating article. They present a new view of social work practice history that raises exciting issues and challenges our assumptions of where we came from and where we are going with social work practice, research, and evidence-based practice.

REPLY

Rhyme and Reason of Practice Paradigms

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It is with great pleasure that we respond to Anne E. Fortune's reactions to our article, "A Historical Analysis of Evidence-Based Practice in Social Work: The Unfinished Journey toward an Empirically Grounded Profession" (2014). Dialogues between scholars are among the most intriguing submissions published in *Social Service Review* because they both help to clarify arguments and humanize the authors. Furthermore, the debates underscore that knowledge accretion, negotiation of key issues, and defini-

tion of the substance and identity of a profession is a dialogical process. As we argue in our article, such exchange is also critical to the process of practice paradigm creation, crisis, and replacement.

Fortune raises many good points throughout her response, some of which question our assumptions or characterizations; others that elaborate on topics in our article; and still others that capture her own thoughts on social work research, science, and practice. In this reply, we first answer her three major critiques and then comment on her reflections.

RESPONSE TO CRITIQUES

Fortune's first contention is that there is a logical gap in our argument about practice paradigms in social work. Specifically, she asserts that our claim about paradigms rests on the following logical sequence: scientific paradigms in natural sciences are observed to have certain characteristics, practice paradigms in social work are observed to have certain characteristics, therefore the two are equivalent. In short, the critique is that there is no basis for assuming that because paradigms exist among natural sciences, they must also exist, and exist in similar form, in social work. From a strictly logical standpoint, we do not disagree with this rationale. Just because similar general patterns are observed in the natural sciences and in social work does not mean that they are necessarily equivalent. However, while we observe similarities between scientific paradigms and practice paradigms, we are not arguing about their equivalence. Rather, we contend that there are fundamental differences between the two. More importantly, our argument in the article about practice paradigms is not based on the soundness of the above syllogism but rather rests on the degree to which our conceptual framework accurately, coherently, and cohesively fits the data. In other words, our main argument is not about how well practice paradigms align with scientific paradigms but rather how well the conceptual framework of practice paradigms aligns with events that transpired in the history of social work.

A succinct elaboration of the process of our theory formation of practice paradigms may add clarity. We originally sought to trace the origins of evidence-based practice in social work and, more broadly, the relationship between intervention research and practice throughout the profession's history. During this review, we noticed a few critical observations, such as the moral basis of philanthropic activity predating social work proper,

the strident calls for evaluation by some leaders in the field in the 1920s through the 1960s but the dearth of research that actually took place, and the overarching similarities between empirical clinical practice (ECP) and evidence-based practice (EBP) that seldom arose in contemporary EBP discourse. Importantly, we saw that the 1910s and the 1960s were two “unsettled and unsettling” periods in social work history, to borrow the phrase from Edwin Thomas (1977, 12). These were two pivot points when leaders seemed to be orchestrating a shift in direction for the profession. These observations led us to a closer reading of Kuhn ([1962] 1970), and with an attentive eye many parallels became apparent between the general pattern he described in the evolution of natural sciences and the evolution of the profession of social work. His description of paradigm creation, crisis, and replacement paralleled the sequence of events that we had observed in our review of social work direct practice. To put it figuratively, we found the music that matched the dancing we had been observing. However, the melody of scientific paradigms did not align exactly to the steps of professional social work, and this precipitated our formulation of what we call “practice paradigms.”

Thus, when we noted the striking similarity between patterns described by Kuhn in natural sciences and patterns taking place in social work (Okpych and Yu 2014, 7), we were speaking more to the process of our theory formulation of practice paradigms than proposing that the two are equivalent. In our view, the true test of whether our conceptualization of practice paradigms is credible hinges on whether our conceptualization cogently, coherently, and accurately describes the evolution of social work direct practice in the United States. To this end, we think that the pattern of paradigm creation and destruction is a useful framework, not because it corresponds to the pattern of scientific paradigm processes in the natural sciences but because it coheres with the unfolding of events in social work history. Similarly, the idea of practice paradigms consisting of a worldview and a template is not valid because it is equivalent to these features of natural science paradigms but because it is an apt description of the shared worldview and practice models that defined and animated legitimate social work practice during the moral- and authority-based eras, and which undergird the current paradigm shift attempt to empirically based practice.

Second, Fortune raises questions about our characterization of an earlier period of social work history. Particularly, some scholars portray the surge in research interest in the 1960s as an amplification of something that

was present in the previous years, whereas we claim that the 1960s are a deviation from the past. We see this discrepancy as reflecting a difference in emphasis, rather than two fundamentally different positions. In our article, we certainly do not deny social work's enduring impulse to cultivate a vibrant research base for practice. Indeed, our second sentence reads, "EBP taps into a long-standing ambition that has captivated and eluded the field since its inception: to forge a service profession grounded in empirical research" (Okpych and Yu 2014, 3). In our historical review, we also acknowledge that leaders in the authority-based era of social work practice (e.g., Porter Lee, Mary Richmond, and Richard Cabot) advocated for a ramping-up of empirical investigation of social work services. However, what took hold of the field, what organized direct practice from the 1920s to the 1950s, was not a drive to establish a research base but a preoccupation with developing and implementing practices based on authority (i.e., practices that were well-respected and prominent, conceptually appealing, etc.). The far-reaching influence of authority-based practice becomes even more salient when it is contrasted with the organized, formal initiatives of the 1960s and 1970s. In a sense, this was a period of heightened self-awareness, when a critical mass of social work scholars was spurred to respond to the ubiquity and sway of authority-based practice. Edwin Thomas (1977, 12) describes the circumstances that precipitated the ECP movement with great clarity: "Interpersonal helping might be much less effective than many think it is, knowledge about practice is insufficiently cumulative, [and] some practitioners take up the latest new therapy, whether or not it has been proved to be effective through empirical research." It is not that this impulse was nonexistent in the years prior to the advent of ECP but rather that outcries during the 1920s to the 1950s for more and better research were peripheral. The organizing principle that ruled the day was professional authority. In sum, we see the discrepancy Fortune points out about the post-Flexner to 1950s era as one more of a difference in emphasis than substantively different claims. Fortune and others view this period as an "unbroken trajectory or inclusion of a scientific base for social work" (see Fortune's response). We acknowledge the appeals for establishing a scientific base for social work before the 1960s but argue that the drumbeat of authority-based practice was far more booming.

Speaking to the larger issue of historical accounts, our article can certainly be classified as a revisionist history. Every historical account is written from a perspective that foregrounds certain aspects while downplaying,

blurring, or altogether missing other dimensions. Our narrative is no exception. Having said this, we think that it is still a useful framework for understanding where social work as a profession has come from, its present dilemmas and decision points, and the future courses that may be pursued. We also believe that our framework of practice paradigms can be useful for other professions such as clinical psychology, medicine, education, nursing, and criminal justice. Each profession has its own narratives, but the indiscriminate pressure of accountability and shared appeal of evidence-based practice as a viable solution will likely mean that recent chapters of various professions will have similar plotlines.

Fortune raises a third question that pertains to the definition of a paradigm: can the empirically based paradigm (in which the EBP falls) truly be called a paradigm if it has not taken hold of the profession? This is an issue of whether traction and professional uptake are necessary conditions of a paradigm. This is an important question and one we briefly address in footnote 4 of our article (Okpych and Yu 2014, 8). To our knowledge, this is an issue that was not directly addressed by Kuhn ([1962] 1970) in *The Structure of Scientific Revolutions*. However, his description of paradigm shifts gives an indication of a plausible stance. He asserts that a presiding paradigm that organizes a scientific discipline can be challenged by a new paradigm, and this new paradigm may or may not succeed in this replacement (78). This implies that the new paradigm can still be called a paradigm, even if it does not supplant the presiding one. We agree with the implication. That is, empirically based practice (as embodied in two distinguishable movements, ECP and EBP) can be considered a practice paradigm because it offers a fundamentally different worldview and template for social work practice than the presiding authority-based paradigm. However, since empirically based practice has not effectively been adopted by the field and it is fair to say that social work practice is not organized around the incorporation of empirical evidence, in our article we classify it as a “paradigm shift attempt” rather than a completed shift or a presiding paradigm (Okpych and Yu 2014, 5). Denoting empirically based practice as a paradigm shift attempt captures both its status as a paradigm (an alternative worldview and template) and its uncertain fate in social work direct practice.

A final point that deserves attention is Fortune’s observation that much of the productive work of developing versatile, portable, and tested intervention approaches during the ECP years was subsequently lost in the ether

with the rise of EBP.¹ In our article, we focus on identifying practice models, which we conceptualize as embodiments of the larger practice paradigm. In the case of the empirically based practice paradigm, a practice model outlines a generic process for incorporating research evidence into direct practice. Practice models are different from interventions supported by research. They specify the process of retrieving, vetting, and incorporating research evidence into treatment and the self-evaluation of client outcomes (and in some practice models, also the self-evaluation of execution of the previous steps). The four practice models we reference in the ECP section and the five-step EBP practice model are distinct from interventions in that they do not specify ameliorative techniques or processes but rather specify how empirical research can be incorporated into treatment. Since our focus was on practice models, we did not include some of the innovative and empirically tested interventions that Fortune catalogs. Indeed, we agree that these were important developments that took place during this time, and many have faded from recent memory. One explanation for this may be that many of the interventions mentioned by Fortune were portable, in that they could be used across service populations, settings, and presenting problems, whereas much of the intervention technology today has shifted to creating specific and targeted interventions. A point that Fortune raises in her reply is well taken. It is fair to question whether it is feasible to expect practitioners to research, learn, and accrue competency in the ballooning menu of interventions designed to treat specific conditions and populations.

REFLECTIONS ON FUTURE CHALLENGES AND DIRECTIONS

Many of the opinions Fortune expresses about future challenges facing EBP align with our own views. Among the challenges we discussed in our article, Fortune draws special attention to three obstacles that relate to the practical implementation of EBP (i.e., limited knowledge base of effective interventions, stakeholder perception, and inadequacy of dissemination efforts) and engages in a thoughtful reflection on each issue. Fortune provides

1. We would point out that ECP and EBP never contended with one another, because ECP had all but faded out before EBP emerged in social work. The two movements were successive movements embodying similar objectives rather than contemporaneous rivals.

the kind of productive elaboration and suggestions that we hoped our article would generate in the field.

We view the several challenges our article identifies as a web of interconnected complications that forestalls the spread of EBP in social work practice. While matters relating to the implementation of EBP are critical, it is equally important to consider philosophical and conceptual issues. Philosophical debates about legitimate science and practice, disputes about research method and knowledge products, and definitional disagreements of EBP undergird practical action and can thwart or energize advances of EBP.

In terms of stakeholder perceptions and attitudes, Fortune maintains that practitioners are reluctant to embrace EBP, potentially because it is perceived to be difficult. She contends that one source of resistance may derive from the unrealistic expectation placed on practitioners to engage in the bottom-up variation of EBP, in which the task of knowledge search and appraisal rests on their shoulders. She further suggests that alternative practice models, such as a revision of task-centered treatment or models that incorporate common elements, may be more amenable to practitioners. On the topic of practitioner perception, we suggest that future research might seek to understand in greater depth the variables that account for practitioners' attitudes about EBP. For instance, one might find it fascinating to imagine what a hierarchy of practice evidence would look like from the practitioners' point of view and if allegiance to specific treatment brands and theoretical orientations or employment in specific areas of social work might have a relationship with EBP perception. Literature that addresses questions like these is thin, but findings would have important implications for future directions of EBP.

Fortune also acknowledges two barriers that pertain to research evidence: lack of coverage in many areas of social work and the formidable gap between controlled research and application in practice. She notes that the proportion of doctoral dissertations that involve practice and intervention research appear to be on the decline. This is informative because dissertation topics reflect technical skills and substantive expertise that students have sharpened and can employ. The dissertation also marks a first installment of a scholar's research career. Thus, Fortune echoes our observation about improvements in the quantity and quality of the social work knowledge base over the past few decades and also notes reason for concern.

One line of research that has profound implications for the evaluation of social work interventions is a series of studies that suggest that certain

quasi-experimental designs can serve as reasonable alternatives for experimental studies. For example, in his plenary talk at the 2014 Society for Social Work and Research conference in San Antonio, Thomas Cook (2014) presented findings indicating that designs such as regression discontinuity and interrupted time-series studies produce treatment effect estimates that approximate estimates from randomized controlled trials.² Given that many social work services are mandatory or serve particularly vulnerable populations, practical and ethical dilemmas may be particularly poignant barriers to implementing rigorous methods such as randomized controlled trials. If evidence continues to support certain quasi-experimental methods as yielding reliable estimates of treatment effectiveness, and these methods avoid the ethical and feasibility barriers of randomized controlled trials, this can be grounds for a new era of evaluation training and research production in social work.

Fortune's second point about the gulf between evaluation research and practice points to challenges of transmitting both information about effective treatments and the treatment techniques themselves. Interest has waxed in areas such as translational research and implementation science, which work at the nexus of rigorously tested interventions and the contingencies of everyday practice. A more radical approach that Fortune acknowledges seeks to address the divide closer to the root of intervention design, where responsibility is placed on developers to create interventions that are intended to perform well in the flux and foible of practice settings. The promise of quasi-experiments as ways to reliably gauge treatment effectiveness in subexperimental conditions may prove to be an important complement to developing practice-ready interventions.

CONCLUSION

We would like to extend our appreciation to Anne Fortune for her thoughtful critiques, commentary, and reflections. Her feedback initiated a dialogue that allowed us to clarify controversial issues and elaborate on implications

2. Cook appropriately cautions that a given quasi-experimental design will not always yield results that replicate findings generated by randomized controlled trials. We add that comparability of findings between experiments and quasi-experiments may also vary by outcome. Although additional research is called for, these initial findings are nonetheless promising for interventions where randomized controlled trials are not always possible.

of the ideas expressed in our original article. More importantly, it is our hope that this exchange is useful to our readers in contemplating the enduring legacy of and future directions for social work practice and empirical research.

NOTE

Anne E. Fortune is associate dean and professor in the School of Social Welfare at the University at Albany–State University of New York. She teaches research and social work practice at the undergraduate and graduate levels. Her research is in the areas of task-centered practice, termination of social work treatment, aging, and field education.

Nathanael J. Okpych is a student at the University of Chicago, where he is pursuing a doctoral degree at the School of Social Service Administration and a master's degree in biostatistics and epidemiology through the Department of Health Studies. His primary research interests focus on mental health and education outcomes and services among older youth in foster care.

James L-H Yu is a clinical psychologist and a lecturer at the Open University of Hong Kong. He is an editorial board member of the *Asia-Pacific Journal of Counselling and Psychotherapy*. His research interests focus on theoretical investigations in psychology, mental health interventions, and qualitative methodologies.

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