

## Studying Optimal Healing Environments: Challenges and Proposals

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### ABSTRACT

The concept of optimal healing environments has strong face validity, but it is difficult to operationalize the complexity of the human dimensions that comprise the process of healing. Only rigorous evidence can justify the tremendous resources necessary to create optimal healing environments. This article outlines the current challenges of studying optimal healing environments from a clinical epidemiology perspective and discusses the major potential obstacles to include human resources, relationship-centered care competencies, dysfunctional educational and health care systems, research competencies, credibility, dispersed and unfocused current evidence base, and the actual logistical methodological difficulties. The concept of optimal healing environments is a paradigm shift in the delivery and study of medical care, and such an initiative will likely take decades to transition. However, there are feasible activities that can currently be undertaken to expedite this transition, chief of which is establishing a coalition of influential societies to forge an agenda on several fronts. Research activities presently should focus on validating and simplifying measurement tools for healing-related outcomes constructs, expanding the versatility and use of qualitative methods, as well as synthesizing research evidence to help clarify the current evidence base and the future research agenda.

### INTRODUCTION

Although the concept of optimal healing environments has strong face validity, and anyone who has cared for suffering individuals understands this concept, it is notoriously difficult to deconstruct and operationalize the complexity of the human dimensions that comprise the process of healing. Nevertheless, because only evidence demonstrating added value, based on rigorous data, can justify the tremendous resources necessary to create optimal healing environments, it is very important that we be able to study and accurately understand the nature of optimal healing environments. This is admittedly a daunting task, but it is nonetheless extremely important and represents an exciting opportunity for investigators interested in understanding the power of healing, a topic in which all health care providers

should be interested. This article will outline the current challenges of studying optimal healing environments in terms of who, what, where, and how.

#### *Building an adequate investigator workforce*

Although the first step might seem to be establishing a research agenda, I place workforce development first, because without a team of qualified researchers and competent providers of health care, an agenda is only secondary, and human resource issues tend to be ignored until it is too late.

Who will do the studying of healing environments, and what is our pipeline of human resources that will carry out such a huge research agenda? Logically, the best people to do this are health care professionals who are also re-

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searchers. Typically, this means physicians, nurses, and other allied health professionals who spend enough time immersed in the interaction of caring environments to understand the most important questions and issues, and who also have the research skills and “access” to carry out research in health care settings. Are there sufficient numbers of such professionals to accomplish the future research agenda, and is there a systematic workforce development plan that will assure there is one in the future? I believe there are some serious deficiencies on both counts to address this first and foremost issue of human workforce development in even beginning to study optimal health environments (OHE) in a substantive and systematic fashion.

Firstly, there is no well-trodden career path for young investigators who might be interested in studying OHE. Most people who are currently involved in such research have been unusually passionate, self-directed, and creative in carving out a research agenda and getting research funding. However, they have done this against all odds, and they are a small few, relative to the magnitude of the health care field. The typical professional paths for clinician-investigators, even for those inclined to the psychosocial aspects of the clinical sciences, are not paved to lead to careers in studying optimal healing environments. More often, they are biomedically oriented toward pharmaceutical, laboratory-based, or technology-related research.

Secondly, our medical educational systems are not producing psychosocially oriented graduates. Consider what currently happens to our medical students as they progress through medical school. Upon entry to medical school, their psychosocial orientation is toward the enthusiastically empathic range, which then transitions to a biomedical, emotionally removed attitude by the time they finish their fourth year or internship.<sup>1</sup> Something intangible is influencing their development—the so-called “hidden curriculum”—in their clinical years that is adversely affecting their psychosocial orientation, an attitude which is critical in effective relationship-centered care and healing environments. Obviously, this issue needs to be addressed so that the workforce from which this field would draw would have a modicum of competence and acculturation toward necessary features of optimal healing environments.

Beyond medical school, there are even more workforce and professional development structural and developmental problems. Most care is provided in primary care settings, and primary care providers are most inclined toward psychosocial issues. However, primary care residencies are not filling,<sup>2,3</sup> and we have a training culture of overspecialization (70% of our physician workforce are specialists),<sup>4</sup> all of which is made worse in the United States by a dysfunctional health care financing system with perverse incentives.<sup>5</sup> There are only limited formalized training opportunities for careers in clinical research related to OHE-type fields. General Internal Medicine fellowships have become more widely available, and they tend to account for a large

proportion of clinician-researchers involved in patient-centered care, but even those fellowships lead to poorly defined career paths; such fellowships are dependent on unreliable governmental funding; and there simply are not enough fellowships, nor enough graduating residents interested in such fellowships, to meet the human resource demand necessary for studying OHE. This is an area that needs vastly more proactive and deliberate planning and funding.

Independent of the pipeline, what is in place now to develop the knowledge, skills, and attitudes (K,S,A) among our current workforce? Much needs to be done in acculturating our current staff in the K,S,A that constitute OHE. Among providers who are not research oriented, but who will deliver the care in OHE environments, there are currently limited competencies in OHE, and worse, no development agenda to engender or teach the elements of OHE. Current CME content is largely knowledge oriented, but also skills and attitude components. This is slowly changing within graduate medical education with the advent of new accreditation and board certification requirements, which include 360-degree evaluations,<sup>6–8</sup> but even this has met with strong resistance (many specialists and the majority of the internal medicine field are now opting to not even recertify in internal medicine), and is likely to be insufficient to provide the professional development necessary to have a workforce fully competent in OHE. Other health care fields and professionals will also need to have appropriate competencies in OHE, though this is a hard argument to put forward at a time when even basic workforce numbers are in severe shortage (particularly nurses).

One solution to the RN shortage has been to transition to lower levels of nursing staff to carry out the more menial tasks of health care delivery. This, in turn, may create even further competency challenges for a more sophisticated caring environment, such as OHE.

In short, there are serious potential issues regarding the human resource element of studying optimal healing environments: in relationship competencies, educational systems aligned to teach such competencies, research competencies, and sheer numbers of both researchers and health care providers. Critical to the acceptability of all of these structural issues about human workforces, and the ease with which to advance this field, will be having a relevant evidence base to enhance the credibility of the OHE paradigm.

### *Establishing optimal healing environment research as a credible endeavor*

One major challenge for the field of OHE research is establishing credibility as a legitimate field, common to any innovative transition. This credibility is necessary among the research community (and funding agencies), the broad medical community, and the public.

Establishing credibility requires numerous converging forces and activities, such as an evidence base, a clear and

effective articulation of legitimate needs or gaps in evidence, and a cohesive unified message from major organizations that already have established credibility. Such a convergence has begun, but there is still room for improvement, and this will take time and great efforts. In looking back on the history of paradigm shifts of the practice and study of Medicine, there are many examples of fields of research that have gone through major struggles for decades prior to establishing the credibility necessary to advance a substantive research agenda. George Engel published his momentous paper on the biopsychosocial model in 1977, but had been writing about such a model since the 1950s, and it was not until the mid-1980s when most medical schools had formalized the teaching of biopsychosocial medicine as a discipline.<sup>9</sup> A testament to Engel's credibility with such a model was having it published in the reputable scientific journal *Science*, which, in turn, gave it more credibility among the skeptics. Contrast this with another landmark paper, by Miller et al., which I am convinced could similarly change the landscape and paradigm of studying health care delivery.<sup>10</sup> It is a clear and persuasive document of how to study OHE, but its impact will be limited unless these ideas are shared in high-impact journals and disseminated in schools of clinical epidemiology.

Another issue with credibility will be handling perceptions effectively. Names have meaning, and if part of credibility is "mainstreaming" a paradigm, or even having it take a leading edge position, then it will be difficult to create this perception if the study of healing environments falls under the rubric of "complementary" or "alternative" medicine.

There are likely lessons to be learned from the evolution of other health care fields. The evolution of cardiovascular disease (CVD) for example has spanned the better part of the last century, and it is now a vast multidisciplinary field with numerous professional organizations that are well organized and integrated in their approach to studying and delivering care for CVD across the biopsychosocial spectrum. The credibility of this field was established with massive efforts of research on large scales requiring large, international follow-up studies and rigorous intervention trials with generalizable findings, developed over decades. This information base has had a measurable and appreciable impact on the health of populations worldwide. In turn, public education efforts have successfully acculturated the public and policy makers to the point that "heart attacks" and its causes and treatments are widely known. And yet, there is still a substantial body of knowledge to be attained, for which there is a clear and focused agenda, and the resources to accomplish it.

It may not be entirely fair to think OHE can follow an analogous path as the field of cardiovascular research, since the study and practice of CVD care is relatively narrow, focused on a single organ system that is much more biologically based and relatively easy to control in study settings. Healing environments span a diverse breadth of chronic and

acute illnesses interacting with the complexities of the human personality and life experiences, as well as diverse cultures, educational level, socioeconomic, and systems of care. Nevertheless, on the issue of credibility, the community of OHE researchers and caregivers will need to be patient that progress will take time. But it will be important to think in a similarly large scale in its scope of study and organization of activities and medical societies, human resources, and public education.

### *Surmounting methodological challenges*

On a more specific, logistical level of the challenges of studying this field, I will break down the challenges into a common framework of research design and analysis.

### *Background*

The climate is harsh for studying healing environments. The current background and context of health care delivery is largely chaotic and dysfunctional.<sup>5</sup> More specifically, health care systems (even in societies with single-payer universal health care) are largely not truly integrated systems with sufficient predictability of resources and processes. In the United States, we have poor primary care systems that, for the average citizen, rank low in continuity, longitudinality, comprehensiveness, coordination, and access. There is overregulation (example, HIPPA), overspecialization (that is not friendly to healing environment concepts), and perverse financial incentives for delivering care.

Medical recordkeeping is not standardized across systems and is often incomprehensible, noncomprehensive, and fragmented, if even accessible in the first place. The transition to electronic records will help with this but will also create its own challenges as they are currently evolving into deconstructed data systems (i.e., pick lists for clinical history elements), which, by themselves, may have a dehumanizing effect on the quality of health care delivery.

The current state of the research agenda for studying OHE is not clearly articulated, even if it has been delineated. Medical societies and professional organizations that are typically interested in this field do not collaborate well. The body of evidence related to optimal healing environments is not well organized into a readily accessible form. Thus, it is challenging to understand which unanswered research questions are most important. Investigators need clearer direction and focus.

Getting funding for healing environment research will be even more difficult than for the clinical sciences. There is a bias toward funding bench research over clinical science research, and the clinical science research is often preferentially devoted to translational biological, pharmaceutical, and device research, as opposed to the human dimensional research often referred to as relationship-centered care.

Funding for health care delivery is more and more going to be linked with outcomes-oriented financing systems, under new “pay-for-performance” initiatives. This is a major problem for OHE when our current systems are designed to be able to measure only crude processes and outcomes, and less able to measure the quality-of-life outcomes, which OHE is more likely to impact. In the United States, there are inadequate coding systems for processes of care that accurately represent primary care activities. Worse yet, is that even for hard outcomes, our current information systems do not allow us to accurately and reliably adjust for comorbid risk or circumstances. Therefore, translating OHE outcomes into the current language of outcomes (e.g., illness burden, quality-adjusted life years), with appropriate adjustment for comorbidities and severity of illness, will be extremely difficult. However, unless these basic background parameters change, it will be the only way to justify its value and relevance.

Finally, there is probably insufficient data yet on quantifying the need for OHE. Where is the body of research demonstrating the needs assessment for OHE or another paradigm of care? Is this truly what patients want, and if so, where is the data to support this? With such dispersed knowledge in this area, it is difficult to justify this need cogently and succinctly. That is not to say one cannot believe it is necessary, just that there needs to be a more formal quantification of that need.

### *Study design*

Given the complexity of the human experience in clinical settings, traditional research methods simply will not be sufficient. Innovative research methods and designs are going to be necessary to capture the data to help us understand the healing environment. Researchers will be required to be experts in multiple methods as well. To adequately study OHE, a research scientist cannot simply be a “trialist.” As Miller and Crabtree have coined, researchers will have to take on a “multiplist” approach, employing multiple methods to triangulate on new revelations and explore realities from multiple perspectives.<sup>10</sup> The complexity of our methods are going to have to match the complexity of the phenomena involved in healing.

Much of OHE research will involve qualitative research. Although this field of inquiry has grown tremendously in interest and impact within the medical literature in the past 10 years, it is still largely a marginalized method among research scientists and even clinical epidemiologists. Qualitative research methods are not part of the standard curriculum for clinician-researchers in their Masters-level science programs. Therefore, there is relatively insufficient experience and expertise in both qualitative and multiplist techniques to study healing environments.

Miller and Crabtree have proposed one novel approach for clinical and prospective cohort studies where studies have parallel arms of qualitative and quantitative measure-

ments throughout the course of study, a so-called “double helix” research design. This is an example of a multiplist approach that can add qualitative richness to various quantitative findings. If only such a model had been used in the thousands of randomized trials in the past, we may have much more knowledge and insights surrounding the intricacies of why certain interventions succeeded or failed.

Since OHE cannot focus on single diseases or organ systems, by definition, it will be an area where there is tremendous uncertainty of diagnosis, undifferentiated symptoms, and heterogeneous syndromes. This creates great difficulty in making fair and valid comparisons across groups of individuals. Thus, definitions of the subject matter to be studied cannot be “diagnosis” specific but rather broadened to include diversity of illnesses, with proper measurement of such factors to be included in more complex analyses.

The domain of health care is extending beyond the clinic-based setting to include care in home settings. Therefore, research needs to expand its horizons of settings from the clinic settings to living situations. This creates a level of logistical complexity that makes a clinic-based study seem easy. Structure and processes of health care are also difficult to manipulate, in order to allow for study using multiplist approaches, particularly in such open-ended settings.

Confounding and bias opportunities are infinitely more numerous in the study of such complex systems that OHE would comprise, compared with typical research settings. Thus, innovative research designs and a skilled use of multiplist approaches will be necessary and very difficult given our current health systems and level of research competence among the body of researchers.

### *Participants*

The recruitment of participants will involve a broader array of people: patients, providers, family members, caregivers, and administrative staff, thereby creating a more difficult coordination effort of individuals, akin to directing an unscripted movie without really being able to direct the players. It is also important to be mindful of the possibility of how the mere study of clinical activities may affect the scenarios themselves. For example, direct observation, a method that will be most useful in this field of research can be threatening to some and may affect the dynamics which are part of the process to be studied. Similarly, the stress of participating will be added to an already distressing illness experience (for patients and providers), and how this added stress impacts other dynamics will be difficult to assess. Could the act of studying it adversely affect the experience and impair the healing environment? Particular attention to creating naturalist study environments will be particularly important.

### *Measurements*

Given the broad complexity and infinite permutations of behavioral, situational, interpersonal, and biological pro-

cesses, it is enormously difficult to measure all the variables necessary to adequately understand and predict these phenomena. There will be a need to discover parsimonious ways to measure multiple variables that capture broader colinear constructs. Focus and priority should be on the domains outlined by Miller and Crabtree: (1) Core OHE Conditions and Attributes (healing intention, motivation, information transfer, empathy, emotional engagement, trust), and (2) The components of relationship quality (adaptability, cohesion, growth, caring-in-relation, and commitment), spirituality, intrapsychic capacity to heal, and wholeness.<sup>10</sup> Indeed, the paper by Miller and Crabtree should be required reading for any researcher interested in OHE. Most of these constructs require substantial measurement development, and represent an important priority on the research agenda for studying OHE.

Another factor unique to studying OHE will be questionnaire burden, and so it will be particularly important to use parsimonious measurement tools without losing the construct validity of the tool. Further factor analyses of validated questionnaires may be needed to further reduce items.

Researchers will need to be mindful of how their presence, and the presence of their research teams, will affect the environments they study. Research teams will undoubtedly develop relationships with the research participants. The extent of interaction with participants will need to be gauged, and likely limited, to minimize any confounding effects.

### *Outcomes*

Unbiased assessments of outcomes in OHE will be difficult unless there is recording of encounters using direct observation. This will require having the video or audio equipment positioned in the clinical setting so that it is unobtrusive. *A priori* rigorous definitions of outcomes assessment will need to be established, with inter-rater agreement measured to assure the outcomes assessments are valid. This adds a major logistical barrier to doing large sample-size studies. Thus, innovative sampling methods may be necessary to perform such assessments on only representative samples of participants.

Which outcomes to measure will be important to ascertain. Miller and Crabtree lay out a nice framework for this. To begin with, there are the traditional outcomes: physiologic parameters, disease-specific markers, morbidity, mortality; symptom resolution; cure; and functional status. However, less well understood is how to measure broader concepts of healing, such as “wholeness” and “social functioning.”

### *Analysis*

Complexities of OHE factors require complex analytic techniques. Multivariate techniques are limited in ability to unveil complex webs of inter-relationships and instead are

only useful at deriving net direct effects of one independent variable on a dependent variable. There is a need for more robust analysis of interaction where insights regarding indirect effects would be important in understanding the whole mosaic of phenomena. Structural equation modeling (SEM) is one tool that is uniquely suited for this.<sup>11</sup> However, SEM is an underutilized analytic tool in the medical sciences, and there is little experience using this tool among medical researchers. Collaboration with the social science research community will be important to expand the breadth and depth of analytic capabilities.

### *Translating findings into practice change*

Reporting of results about OHE research will be challenging because the wealth of information obtained from rich, multiple designs will require great effort to simplify and report in a way that is readable and cogent. Given the qualitative nature of the work, it will be important to succinctly report themes and insights using actual stories and examples to further convey meaning. The medical community is not fully acculturated in the reading of qualitative research, and so researchers may experience a relative “publication bias” against qualitative research. On the other hand, with the use of a multiplist approach, there will be tremendous opportunity to create much more interesting reports with richer triangulation on insights from different perspectives, not possible with unidimensional research methods.

Unless we can change the current trends in workforce shortages and developmental career paths that are most needed for the delivery and study of OHE, as well as the other structural issues associated with OHE, attempting to study OHE may be nothing more than a pipedream. A coalition of influential forces is necessary to drive dramatic and large-scale change to establish an agenda, create a pipeline of resources, and the structural changes required to build an evidence base that demonstrates the need and value of optimal healing environments. I make several specific proposals below which I believe would be feasible next steps in moving closer to this goal.

### *Proposals for enhancing the who, what, and how of OHE research*

Change occurs more effectively over time when efforts are focused simultaneously on both incremental and radical levels. However, in order to accomplish this, it will require a community of professionals and professional organizations who are organized and focused on the need for changing to a new paradigm. One example of a broad coalition of organizations that could be an influential force would be a coalition of internal medicine, family practice, and pediatrics, which, together, make up the bulk of primary care, and 30%–40% of all physicians in the United States.<sup>12</sup> The initial goals of such a coalition would be to: (1) Build an infrastructure of human resources (researchers and providers),

educators, and policy makers with a focus on OHE (e.g., using research infrastructure grants); (2) Establish a research agenda after building a database of current available evidence; (3) Establish an agenda for influencing the professional development from the undergraduate to graduate level of health care provider education (to include the development of researchers for this field), as well as postgraduate, lifelong development for practicing providers; and (4) Establish an agenda for influencing health systems' development so that they are oriented toward evidence-based features of optimal healing environments.

On a more specific level of feasible, incremental changes today, there are many. Firstly, the validity of qualitative methods is well established as a legitimate method for providing important insights and knowledge, particularly about the human experience in clinical settings. This should be mandated to be a part of all publicly funded (i.e., NIH) human use protocols—a so-called mandatory double-helix model. Such a regulation will force all clinical researchers to become trained in qualitative research. Consider the vast waste of missed opportunities in prior large studies where there was no qualitative arm that could have added a significant level of richness and insight without considerable added cost. Secondly, more work needs to be done in demonstrating the need for a new paradigm of health care delivery that would involve OHE activities. A nice impetus for this would be to first establish an accessible repository of OHE science, regularly updated, which would then serve as a springboard for research questions. Thirdly, the public needs education on how OHE principles would enhance their health. Part of this will be done in building credibility among the scientific community through rigorous research, but more can be done through relationships with the media and policymakers. Along these lines, there should be strong consideration in how this initiative should be named, and this should include being open to abandoning the terms “complementary,” “alternative,” and perhaps even “healing” which take on less credible connotations in certain settings; and communicating with the use of more acceptable terms, such as “health” and “wellness.”

In the research realm, we need centers of excellence to construct models of OHE, preferably within relatively closed systems, in order to build a proof of concept. These centers would have the ideal infrastructure and resources for both health care delivery and also the study of this health care delivery with tools to perform minimally disruptive direct observation, and correlate relationship-centered processes of care with measurable, valid outcomes. This can help establish a system of OHE care that can prove to be feasible, desirable, associated with outcomes of care (hopefully better), and also build upon the evidence base to further enhance credibility. These centers should leverage other large health care systems and build collaboration for large-scale, multi-centered, international studies centered on studying the concepts of healing and relationship-centered care.

## CONCLUSIONS

From a clinical epidemiology perspective, studying OHE is indeed a daunting undertaking, but it is the next horizon of clinical sciences, and now is the time to organize and plan to do it right, so that we can more quickly implement the most efficacious advances in how to deliver optimal care for healing.

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