LETTERS TO THE EDITOR

Editor's Note—The authors of the Perspectives article made changes at the proof stage after the following letters were written. The changes did not substantially affect the primary message addressed by the letters, although they did alter the emphasis and tone of parts of the argument. I decided to proceed with the material on hand, without asking the letter writers to respond, because it was late in the editorial process and doing otherwise would have delayed publication.

In Defense of Qualitative Research

Responses to the Poses and Isen Perspectives Article

To the Editor:—An important objective of continuing medical education is to change doctors' behavior. However, traditional quantitative studies investigating the relation between education and changes in clinical practice have several limitations. First, only a few quantitative studies provide objective evidence of the effectiveness of continuing medical education programs in changing physician performance or health care.¹⁻⁴ Second, many such studies do not address scientific criteria for the evaluation of educational events such as the use of control groups, methods of randomization, adequate statistical analysis of results, and internal and external validity.^{5,6}

The importance of gaining a wider understanding of the relation between education and change has recently been recognized. In addition to the quantitative, correlational approach, other research methods are needed for exploring complex phenomena.^{4,7-10} Wergin et al. recommended a shift away from trying to document whether an isolated education event changes physicians' behavior, believing that the impact of continuing education can be understood only within the context of other important intervening variables.¹¹

We undertook a study to identify and categorize the complete range of factors that doctors recognize as changing their clinical practice and to describe the place of continuing education within this context.¹² Rather than investigating whether an education event led to change, we looked back to identify those factors that had initiated the change. We were therefore constrained by the study design from investigating instances in which no practice change occurred. The method chosen was appropriate for the study aim, that of developing hypotheses about what caused change to occur.

To quantify the relation between independent and dependent variables in our study, for example, by calculating relative risk scores for change, was beyond the scope of the project. Quantifying the impact of factors identified in our study on change in clinical

practice is of interest, and a prospective positivistic approach would be entirely appropriate for this.

Qualitative studies aim to describe and frame an issue, addressing it as an entity, while quantitative studies attempt to define and count variables and outcomes where the use of control groups may well be helpful. Articles describing methods of evaluating qualitative research studies are available.^{13,14}—**MICHAEL R. ROBLING, RESEARCH FELLOW, PENNY A. OWEN, GENERAL PRACTITIONER, and LYNNE A. ALLERY, SENIOR LECTURER IN MEDICAL EDUCATION, Department of General Practice, University of Wales College of Medicine, Llanedeyrn Health Centre, Cardiff, U.K.**

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To the Editor:—As Poses and Isen point out,¹ qualitative research has made substantial inroads into the literature of medicine recently. By sounding a cautionary note over the uses and misuses of qualitative research, the authors have performed a useful service. Both qualitative and quantitative research could benefit from heightened attention to rigor. However, I believe the authors have let their pique at epistemologic relativism unduly sway their conclusions about the role of qualitative research in medicine.

Poses and Isen are correct that there is a tradition within the social sciences that sees truth as socially constructed, and that this paradigm informs much of what constitutes qualitative research.² However, arguments over the nature and knowability of reality are common even within the hardest of the hard sciences.³ Rather than get bogged down in questions of ultimate truth, we should be content to ask,¹ do the results of a scientific inquiry enhance our understanding of some important phenomenon? And are the results useful? This is not a trivial standard. Researchers need to ask important questions, choose methods that are appropriate to the questions asked, and apply those methods with sufficient rigor to make the results credible.

With respect to our study,⁴ Poses and Isen allow that “this study’s methods were perfectly suited to develop hypotheses about why patients’ expectations are not met,” but they take issue with our claim that the findings “elucidate the multifactorial ontogenesis of [patients’] expectations.” They suggest that our research would have been on firmer ground had we determined whether “patients who had no clear expectations for their visits or whose expectations were met also had these concerns.”

If our study was intended to identify causes (or risk factors for) patients’ unmet expectations or to estimate the effect of these risk factors on the probability of having unmet expectations, then methods different from those we employed would have been better suited to the task. This, however, was not our intent, and we must take responsibility for the misunderstanding. Rather than pronounce on cause, our goal was to describe a developmental sequence by which patients’ expectations arise and become expressed. This is the meaning of ontogenesis, which concerns a thing’s origins and development, not its cause. To take a biological analogy, it is useful to know something about the stages of embryogenesis (i.e., a descriptive developmental sequence) even though that knowledge is only distantly related to the underlying causal mechanisms of human development. Of course, much of the knowledge of human embryology was obtained through careful observation, without experiment and without controls.

In this context, it is unclear what purpose would have been served in our study by a control group of patients who had no clear expectations or whose expectations had been met. Some of these patients may well have mentioned concerns about symptom severity, perceived vulnerabilities, etc. But our conclusions would not have been affected. We focused on patients who had *unmet expectations* precisely because these patients had been participants in a critical incident of considerable clinical and social interest. These patients were not case subjects to be matched with corresponding control subjects, but *witnesses* who could report on events and interpretations they were uniquely positioned to portray.

For scientists schooled in the positivist and postpositivist paradigms of Western scientific tradition, it may be discomfiting to rely on patients for reports of their subjective experiences and perceptions. However, for some research questions, qualitative methods are simply the only approach available. Poses and Isen

are correct in calling for higher and more uniform standards in qualitative research. However, they might also relax a bit. After all, until recently, standards for quantitative research were nothing to float over.⁵ As more researchers enter the field and readers’ critical sentiments evolve, qualitative research will improve. By opening up the lived experiences of patients and health care providers, these methods have much to offer clinical medicine.—**RICHARD L. KRAVITZ, MD**, Division of General Medicine, University of California, Davis.

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To the Editor:—In their Perspectives article,¹ Poses and Isen frequently cite our editorial² in their criticisms of qualitative research. This editorial invited authors to submit qualitative research articles to *JGIM*, made a stab at defining and appropriately naming qualitative research, and suggested some ways that qualitative research might contribute to primary care. Given these goals and the scope of an editorial, we could not give detailed explanations of how a diverse group of methodologies would answer specific questions, as Poses and Isen fault us for not doing. We did use an article published in the same issue as our editorial to give a concrete example of how one qualitative methodology answered a specific question.³ I will thus ignore Poses and Isen’s criticisms of what our editorial did *not* do and focus instead on correcting the selective citations from, and inaccurate paraphrasing of, our editorial that in sum give a misleading and inaccurate picture of our point of view.

Poses and Isen state that we stress the broad clinical applicability of qualitative research methods. I believe that research using qualitative methods may indeed yield important results for clinical medicine, but we made no sweeping assertions about their reach because, at the moment, this potential is mostly unrealized.

Poses and Isen state that we make strong and unjustified criticisms of quantitative research methods. Nowhere in the text do we “indict” quantitative methods. Poses and Isen go to great pains to create a straw-man argument between qualitative and quantitative methodologies that, I think, is simplistic and sterile. Our title, “Different Questions Beg Different Methods,” was intended to suggest a moderate, almost self-evident point of view—some pairings of methods and questions are better than others.

It was to exemplify this point that we discussed coronary heart disease (CHD) risk factors. After stating that “epidemiologic trials such as the Framingham study have greatly advanced our knowledge,” we went on to discuss a specific categorization problem that has appeared in the risk factor literature. Poses and Isen misrepresent our use of obesity to illustrate this problem. We

were not criticizing quantitative methodology when we observed that the apparent independent association between obesity and some CHD end points, in some of the earliest Framingham publications to many afterwards, weakens or disappears entirely when other factors such as hypertension and diabetes mellitus are taken into account. To the degree that this has been a consistent finding, this is an entirely noncontroversial fact. The clinical and policy problem is whether independent factors identified by stratification or multivariate techniques are necessarily the best point of intervention. Many of us would object to ignoring obesity in CHD prevention efforts for a variety of reasons—from its obvious influence on the independent risk factors to its other health consequences besides CHD. The point we are making was that an important *complement* to quantitative analysis of epidemiologic data might be qualitative studies that tried to understand which factors or behaviors were most amenable to change, e.g., open-ended interviews with subjects about their attempts to lower their CHD risk, historical studies of changing trends in medical and lay attitudes about risk, or ethnographic research into the family's role in shaping individual behavior.

We never stated that quantitative researchers ignore the reasons why people smoke. Nor did we say that quantitative studies in some general sense cannot make a contribution to understanding relations such as those between tobacco subsidies or advertising and smoking. Rather, we tried to show that risk factor equations, which are a specific and powerful quantitative tool and style of explanation, are probably not suited to study all possible etiologic relations, especially those that operate at the environmental and population levels. Every method has its advantages and limitations; we pointed out the limitations of specific quantitative methods for particular questions and of an "exclusive focus" on the quantifiable. It is difficult to model the cultural determinants of smoking in risk factor equations. Historical analysis of the tobacco industry's role in influencing smoking behavior might prove more illuminating.

We did not welcome or excuse bias, in the typical meaning of the term, in research. We used the term "bias" in quotation marks to make the point that researchers in some situations might profitably make use of their personal and subjective response to the data. We regret any misunderstanding, but we think that our citation of the research by Shimada et al.,³ the context in which the term was used, the use of the term in quotation marks, and common sense made it very clear that we were not calling for biased research.

Finally, I agree with Poses and Isen that it would be better if we had more explicit standards with which to judge qualitative research. At *JGIM*, we have rejected nonquantitative articles at a higher rate than quantitative ones. Many of these decisions have been excruciatingly difficult because we do not have such standards to help us assess the validity, reliability, and generalizability of specific approaches. Although I hope Poses and Isen's piece and the ensuing responses contribute to the development of such standards, we should acknowledge that such standards are going to be more difficult to specify for nonquantitative methodologies. I think it is misguided to use this relative difficulty to criticize in some general sense innovative and potentially important qualitative approaches.—**ROBERT ARONOWITZ, MD**, Associate Editor.

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To the Editor:—In the Perspectives article published in this issue, Poses and Isen raise worthwhile questions about the objectives of clinical research and the connections between evidence and truth.¹ Unfortunately, they misunderstand our editorial message,² and characterize science and knowledge in parochial ways. Dr. Aronowitz clarified our message in the preceding letter. I will comment briefly on the broader issues of research and science that emerge from their essay.

Poses and Isen challenge the message that qualitative investigation can be relevant to general internal medicine. When they express skepticism that this research can answer "traditional" clinical questions, they correctly suggest it cannot help determine the causal *mechanisms*—links between discrete clinical conditions and outcomes—that characterize current thinking about disease. But it can tell us something about causal *meanings*, the contextually dependent beliefs that influence people to think, feel, and act in certain ways. This is clearly important when social conditions and reactions affect outcomes. Of course, one can develop quantitative measures of these social variables and their effects, as Poses and Isen suggest, but the measures often provide little information about how people integrate multiple influences into a unified, sensible understanding or experience. There is certainly room and a need for this kind of information in clinical medicine.

Our example of obesity and cardiac risk was intended to demonstrate this point. If patients identify or experience their risk through being overweight (rather than through their asymptomatic blood sugar, blood pressure, or cholesterol levels), then monitoring their exercise program may be more effective in decreasing their cardiac risk than monitoring their traditional risk indices. Effective clinicians not only translate research evidence into practice, but also make the evidence fit with what is personally important in people's lives. Qualitative information is potentially useful to the extent that it can help physicians move more knowledgeably between these frames of reference.

It can also be useful for developing other causal explanations of clinical illness and outcomes. As Dr. Aronowitz suggests, our reference to tobacco advertising and price subsidies was meant to illustrate that there are broader economic, cultural, political, and other systems which, to the extent that they encourage the production and consumption of cigarettes, can also be said to make etiologic contributions to public illness. We were simply saying that one could give additional accounts of disease in terms of these social systems, and that some forms of qualitative analysis (e.g., historical analysis) are more likely than clinical epidemiology to suggest how complex economic, cultural, and political circumstances evolve and interact over time to affect attitudes and behaviors around smoking. One need look no further than the newspapers to see that the efforts of the President, FDA, Congress, and the tobacco industry to control tobacco use raise compelling questions about systems, stakes, interest, and authority (among other things), each of which could affect and even create real patients. These questions bear investigation, and cannot be reduced to a "cause-effect" or an "exposure-outcome" template.

Even if Poses and Isen could be convinced of the potential clinical relevance of qualitative research, they would rightfully persist in asking how to evaluate qualitative data. This question is crucial, but it cannot be satisfactorily addressed in a letter to the editor. Acknowledging that evaluation is the "weak link" in qualitative research, an adequate response would also require a critical assessment of the authors' ideas about science. This might start with their disputable claim that "the fundamental goal of science is to discover the truth." One could make a strong case that clinical research is undertaken and used to find out what interventions will work for patients rather than to discover what is true about them. From this perspective, qualitative and quantitative research both meet the same objectives: they produce evidence that helps doctors understand what "works" for patients, and can be evaluated by standards that ensure the evidence is trustworthy enough to be used in the right settings. Qualitative researchers could undeniably do more to convey the credibility of their evidence, but the evaluative guidelines Poses and Isen deride provide at least this standard for researchers and readers.

Whatever one concludes from the dialogue Poses and Isen have provoked here, readers should consider that these authors help no one by framing these engaging issues at the start in oppositional and reductive ways. These authors seem unaware that a rigorous and influential scholarship exists about the very issues they raise. Interested readers should turn to other sources that struggle with these same questions in ways that integrate responsible assessments of science with mature dialogue about social contributions to evidence, truth, and objectivity.³⁻⁶—**MICHAEL BERKWITS, MD**, Associate Editor.

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To the Editor:—In their Perspectives article,¹ Drs. Poses and Isen join a dialogue, ongoing since Plato opened it, about simultaneous and contradictory views of reality. Matthew Arnold, Thomas Huxley, both William and Henry James, Sigmund Freud, Karl Mannheim, C. P. Snow, and Lionel Trilling, among many others, have addressed the gulfs between what this article calls the positivistic and the naturalistic conceptions of reality. To have framed this local iteration of these questions within its august context in intellectual history may have helped Poses and Isen to achieve a more productive rumination of age-old questions.

As a constituent element of the world and culture, medicine cannot help but reflect, within its theory and its practice, universal conflicts and upheavals in how persons conceptualize the world and live their lives. Doctors and patients and scientists have no choice but to join other humans who address the great

epistemologic controversies about the nature of truth and the stance of the subject that today consume philosophy, literary studies, history, jurisprudence, psychology, anthropology, sociology, and even the natural sciences. The concept that methodologic validity, reliability, and objectivity can elide these deeply troubling modern and postmodern questions reflects an outdated and ultimately doomed longing for certainty that the world does not possess.

The divide, first of all, is much wider than the difference between qualitative methods and quantitative methods. All qualitative methods quantify many aspects of the phenomena under study, and all quantitative methods start with a hypothesis arising from an imagining human brain and proceed by making meaningful and irreversible methodologic choices that stamp the research with the signature of the individual researcher (and limit, by the way, the research's possible outcomes). Not only the generation of the hypothesis but also all stages of the research process—the choice of study design, sample selection, measurement tools, statistical techniques, and the interpretation of findings—are influenced by the investigator's own sense of what is worth studying and his or her qualitative judgment about the best means to do so. Further, even the most basic choice of which questions to investigate is influenced by political, social, and historical forces. The image of the detached, disinterested, objective scientist whose agenda emerges untainted by human forces is as false as the notion of the reliable truth.

The divide is, rather, between the particular and the universal, between a narrative understanding of a singular human being and a paradigmatic proxy statement about a "representative" human being. (The divide is crossed by another divide, the one between hypothesis generation and hypothesis testing. Poses' and Isen's dismissal of the importance of hypothesis generation in comparison with hypothesis testing may be symptomatic of some of the underlying tensions to which they are responding.) Although ultimately engaged in making sense of the world, these two kinds of research are divided at the level of the most fundamental assumptions and operations. Terms are not interchangeable. Rich, meaningful variation for one is unwanted noise for the other. To address the part of our article² cited by Poses and Isen, honest decoding of meaning for one is bias for the other. To import terms, with their explicit and subtextual meanings, from one enterprise to another is meaningless. We are "in for," that is to say, a much larger project than merely mastering one more set of research tools. By trying to understand the differences between the so-called quantitative and so-called qualitative, we have embarked on an intellectual journey of perhaps the deepest and most significant scale in medicine today.

More specifically, in the paper of ours cited, observer "bias" is described as an inevitable consequence when one human observes (or in our case, listens to) other humans in action. We are baffled that anyone could assert that an observer's own experience and values would not inflect his or her understanding of actual human speech, for the conceptual pathways toward understanding language have to pass through such individuated parts of the brain as symbol formation, myth recognition, and affective response. Understanding human speech and behavior, much like any clinical or basic research, requires human equipment; that human equipment, alas, "comes with" the rest of the singular human. Importing the term "bias" along with its negative implications to represent observer-generated realizations only muddies the field. It is as if we asked that quantitative research be accomplished without wakefulness. If the goal of understanding human

language and behavior has a place in medicine, then we are obliged to develop means to achieve that goal with accuracy. Such linguistic and narrative methods as those we adopt are part of that attempt that, ultimately, holds great meaning not only for the researcher examining questions from afar but also for each doctor and each patient, sitting in an office talking about pain.

What, then, does the qualitative researcher do once he or she accomplishes a careful and trustworthy understanding of the language and behavior of an individual human being? Here is where we rely on our positivist skills and methods. In our research (and we cannot generalize to the practices of other qualitative researchers), once we carefully examine and articulate that which we understand one human being to be doing, we attempt to collate the language and behavior of many human beings, so many that we might be able to test the relationships, for example, between setting and behavior or between age and hope. Included within the domains of qualitative science or narrative research, then, are efforts to generalize, to predict, and to relate initial states to outcomes. These efforts require the same evidence-based activities that are used in testing any hypotheses.

No one disagrees that any new research methods will need to be tested and explained to the uninitiated. We all learned how to perform critical reviews of conventional research literature, and no doubt we will all learn to perform critical reviews of more recently introduced, narratively based research literature as well. We were impressed to see the number of papers published during the early 1990s alone that clarify different aspects of qualitative methods, but the effort is by no means complete. However, this article—with its polarizing zeal and affectively loaded diction—is not merely “about” the need for methodologic textbooks. It reflects epistemologic and near-existential crises about very basic human understanding, perhaps triggered by threats to such strongly defended beliefs as reproducibility and an uncontested truth. Oddly, it enacts the tensions that it attempts to examine. On the one hand, a review written by quantitative researchers should have defined its terms and described the methods by which they chose papers for review. And, as an interesting and revelatory aside, we note that one needs to use such qualitative textual methods as framing, examining linguistic patterns, interpreting in a hermeneutic circle, and adopting multiple contradictory perspectives in order to know, very simply, what this paper is about.

Medicine, it seems to us, has an awesome opportunity today to bring clarity to a set of questions posed by Plato and open ever since. As a combination of conceptual and practical activity with simultaneous responsibility for both populations and individuals, medicine resides in both the narrative and paradigmatic domains, and its practitioners and researchers have to develop fluency in the languages and methods of both. Almost unique among human enterprises, medicine has the chance and the duty to achieve some productive congress between these unsurpassable aspects of human learning and human behavior. Rancorous bickering, rhetorical excess, and inflated claims for power have no further place in this ongoing dialogue, even if some of the early parlay has been characterized by posturing. Our failure to achieve mutually educative discourse will cost patients, medicine, and—at the risk of sounding grandiose—the culture at large. Such is the enterprise. May we all be equal to it.—**RITA CHARON, MD**, *College of Physicians and Surgeons, Columbia University*, **MICHELE G. GREENE, DRPH**, *Health and Nutrition Science, Brooklyn College, City University of New York*, and **RONALD D. ADELMAN, MD**, *Department of Medicine, State University of New York at Stony Brook*.

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To the Editor:—Our critique of the Perspectives article by Poses and Isen in this issue,¹ focuses primarily on its value as a means for engaging in a meaningful and productive dialogue between the two schools of thought colloquially referred to as qualitative and quantitative research. The authors propose to “contribute to the clarification” of these two approaches. In fact, they may have done quite the opposite. We respond briefly to four aspects of their work: the scholarship of their critique; their philosophical position; the political/ideological implications of their approach; and their critique of our previous work.

Poses and Isen use as the guiding principle for their article the maxim of evidence-based medicine that practice and policy “should be based on critical review of the best available evidence.” However, their critique falls woefully short of addressing the best available evidence. Take, for example, their claim that “as of now, it remains unclear whether qualitative research can address [clinical] questions.” This conclusion is based on isolated terms and fragments of sentences extracted from only three qualitative articles: the first an editorial, the second a description of the use of Balint groups in research, and the third on the application of methods from cultural anthropology to general practice research. Terms and fragments from these three articles can hardly be claimed to be the best evidence to address the question of the applicability of qualitative methods as a whole to clinical questions. Indeed, Poses and Isen go on to critique this entire area of inquiry without mentioning one seminal work or author in that area, without even referencing a single book. Their entire critique is of isolated terms and sentences (not even whole arguments) from disparate papers, some of which are editorials, other reports of research findings, none of which even appear in a journal dedicated to qualitative research in which peer review standards would be most stringent. To be fair, the authors do an equally poor job of mustering evidence in support of their own claims. For example, in citing Fletcher in support of their claim of a single “reality,” they rely on a single passage from the 1993 second edition,² that has been completely excised from the 1996 third edition.³ Plainly, Poses and Isen fail to meet the standards of rigorous scholarship that they are purporting to uphold.

Poses and Isen’s most far-reaching critique of qualitative methods concerns its philosophical stance. They present their own version of classical naive realism (for which they cite no authority), in which facts can be separated from theories, objects separated from their observers, and things are just what they are even if they can have different meanings for different people (the classic medical example of the ashtray is given). Deviation from these positions by advocates of qualitative methods leave the authors “puzzled,” demanding clarity on the nature of reality, and reconciliation with the goals of science on the part of qualitative researchers before their work can be accepted. In point of fact, from the time of Heisenberg’s writings on uncertainty (1925),^{4,5} through the works of Kuhn on scientific revolutions (1962),⁶ Rorty on the limits of modernist epistemology (1979),⁷ Quine on relativity and ontology (1969),⁸ Latour and Woolgar on the construction of scientific facts in the laboratory (1986),⁹ and too

many others to mention, the philosophical and sociological views of science from across the analytic, pragmatist, and continental spectrum have rejected this form of naive realism. Poses and Isen cite no authorities because there are no longer credible sources for such an arcane and simplistic position.

Furthermore, the ideological consequences of such an argument for the hegemony of a single way of knowing are most disturbing. Their stance, as they have stated it, fails to appreciate the importance of the move toward a more human science. The implications of the "naturalistic paradigm" are significant. It has allowed for an approach to complex human phenomena from the perspective of the research "subject," seeking understanding of phenomena rather than a singular "cause." Furthermore, by acknowledging the value-laden nature of inquiry and moving away from the notion of a single truth defined by a privileged few, it has opened the door to a diversity of perspectives including those of women and people of color, previously marginalized by the hegemony of the positivism. Poses and Isen's attempt to minimize the importance of such progress should be viewed with caution.

Finally, Poses reprises his critique¹⁰ of our article, "The Experience of Violent Injury for Young African American Men: The Meaning of Being a 'Sucker,'"¹¹ stating that we made statements about causation on the basis of a case series. As we have stated previously, this reading of our article is simply factually incorrect. It is not a case series—our respondents were not chosen on the basis of their being victimized because others viewed them as "suckers." They were victims of violence who described a logic of violent encounters that they observed taking place in their neighborhoods. As Poses and Isen should know from reading Fletcher (1996),³ this is not the definition of a case. Nor did we impute or infer causality in this report.

Poses and Isen are correct in pointing out the need for better understanding of qualitative methods among clinical researchers. Readers might note that Sage Publications offers numerous books and monographs on various aspects of qualitative methodology covering many of the disciplines, approaches, and traditions that fall under the label "qualitative." A number of journals also publish qualitative work, among them *Qualitative Health Research* and *Qualitative Inquiry*. It is clear that those of us who use and advocate the use of these approaches need to do a better job of educating our "quantitative" colleagues. However, efforts like that of Poses and Isen do little to advance understanding or even encourage meaningful dialogue.—**DAVID A. STONE, PHD**, Harvard Medical School, and **JOHN A. RICH, MD, MPH**, Boston University School of Medicine, Boston, Mass.

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In reply—We wanted to start a discussion about the role of qualitative research in medicine and health care, focusing on its goals and the development of rigorous methodological standards for it. Obviously we succeeded in generating considerable discussion. Whether it will lead to clarification is for the reader to judge.

There seem to be two areas of agreement: 1) if qualitative research is to be published in medical and health care journals, there is a need for more discussion of the critical evaluation of qualitative research in venues accessible to physicians and health care researchers, and 2) qualitative researchers will need to clarify the contribution of their work for this audience.

Unfortunately, no letters proposed new detailed methodologic standards for qualitative research that would enable readers to review articles in this area critically. Nor were any letters able to identify existing standards that are easily accessible to physicians and health care researchers, but that we had overlooked.

We believe that some letters clarified particular issues. Robling and colleagues and Kravitz explained that their studies were meant to describe phenomena and raise hypotheses, not to test hypotheses. We appreciate their clarifications. However, sometimes they seemed to argue that their studies did demonstrate causation. For example, Robling et al. said their study could "identify those factors which had initiated the change." The inability of this study to test causal hypotheses has nothing to do with the qualitative methods it used to collect and analyze data. Rather it arises from the study's design. To prove that a particular factor had generated a change would have taken a randomized controlled trial. This study, however, had no comparison group, and therefore was not even a case-control study. For clarity, papers that describe phenomena or raise hypotheses should clarify their intent and not draw causal conclusions unsupported by their designs. We hope that the eventual development of clear, detailed, operational, accessible methodological standards for qualitative research in medicine and health care will reduce the likelihood of such errors of interpretation in the future.

Charon and colleagues chided us for dismissing the importance of qualitative research to generate research hypotheses. We think this role is important and not controversial. So we did not emphasize it in an article that focused on controversies.

The importance of describing new phenomena and generating hypotheses should not be overstated, however. Many plausible hypotheses have proved to be false when tested. We in medicine have seen the bad consequences of acting upon untested hypotheses. For example, many physicians based antiarrhythmic drug treatment of patients with ventricular ectopy and coronary artery disease on the assumption that this treatment would prevent severe ventricular arrhythmias and sudden death.^{1,2} It took the CAST study, a randomized controlled trial, to demonstrate that antiarrhythmic therapy of such patients leads to increased mortality.³

Although this example is "clinical," the principal applies equally to studies of "biopsychosocial" issues. In the "biopsychosocial" area it may be easy to generate causal hypotheses, but it is

very hard to prove or disprove them. Assuming that an untested biopsychosocial hypothesis is true and acting upon it, however, could be just as harmful as doing so for a clinical hypothesis.

Finally, no study, qualitative or quantitative, that generates hypotheses should be exempt from requirements for methodologic rigor. For example, one generally should not construct hypotheses based only on case-series studies. It may be tempting to pick a factor (exposure or antecedent condition) present in such patients and hypothesize that it is a cause of the outcome. There may be numerous factors present in patients who experienced an outcome of interest, however, that would also be found in patients who did not experience that outcome. Knowing the prevalence of a particular factor of interest in a comparison group, therefore, is necessary to indicate whether such a hypothesis should be pursued. (An important exception is an instance in which the factor identified turned out to be very rare in the general population, a *de facto* comparison group.)

Aronowitz felt we had misunderstood the discussion of bias in his previous editorial. We are glad that he has now clarified that he does not "excuse or welcome bias." Rigorous efforts to reduce methodologic bias are a major factor that distinguishes science from other human endeavors of self-expression or opinion.

Some of the invited responses to our article seemed to reflect a major failure to communicate. For example, Charon and colleagues apparently had great trouble understanding us because they said that they needed to use textual analytic methods to fathom what we wrote. Without an explanation of unfamiliar methods such as "interpreting in a hermeneutic circle," we also have trouble understanding them.

Stone and Rich and Charon and colleagues criticized us for failing to present the best available evidence about qualitative research in what they seemed to believe was meant to be a systematic review of that topic. Both groups of authors misunderstood our purpose. We meant to write an editorial emphasizing controversial scientific methodologic issues affecting the role of qualitative research in medicine and health care, drawing upon a roughly representative but not systematic sample of work published where physicians and health care researchers might easily find it. We thought these ground rules would be clear, given our introductory remarks and the publication of our article in the "Perspectives" section.

Several letter writers suggested that qualitative research can answer specific research questions, i.e., test specific hypotheses, and are the only way to answer some kinds of questions, although there was no consensus about the nature of these questions. Without specific justification for these assertions, we remain skeptical about them. Furthermore, developing standards for the validity of qualitative research as a means to answer any questions will be very difficult. Qualitative methods such as open-ended interviews may seem easy and convincing, but using them correctly may be extremely hard. A hundred years of psychological research has shown how difficult it is to determine what goes on in people's heads, or predict their behavior from their narratives. In fact, it may be possible to infer people's interpretations and "experiences" of situations more accurately from their behavior than from their interview responses. There may be truth to the saying "actions speak louder than words."

Some of the letters seem to go even farther afield from the issues we meant to emphasize. We summarized the conceptual underpinnings of qualitative research from sources in clinical and health care journals to highlight possible origins of some methodologic problems and controversies. We did not presume to write a

philosophic treatise. Rather, we meant to show that a confusing philosophical debate among qualitative researchers seemed to be obstructing the development of scientific standards for qualitative research.

Charon and colleagues, after complementing us by putting us in the company of well-known philosophers, also feared we were afflicted by "epistemological and near-existential crises about very basic human understanding." We can reassure them that our mental health is excellent, however, and we are suffering from no such crises.

We note that Charon and colleagues mentioned several philosophers, but did not mention scholars such as Dewey, Tolman, and Herbart, to name just a few, whose theories lead to empirical purposivism, the idea that people's behavior arises from their goals and purposes and should be studied in that context. These views laid the groundwork for advancing the field of scientific psychology to include both systematic empirical observation of behavior and individual interpretation and meaning simultaneously, by focusing on study of behaviors in situations that could reveal these meanings. Indeed, much modern social and cognitive psychology, while being empirical and quantitative, considers the importance of meaning and interpretation in guiding cognitive processes and behavior. Thus our argument for scientific standards is not the same as an argument for logical positivism or against scientific constructivism (the understanding that the way people construe or interpret situations plays a role in how they react to them).

Several letter writers took further issue with our philosophic stance. Berkwits thought we characterized science and knowledge "in parochial ways. . ." and disputed our statement that "the fundamental goal of science [is] to discover the truth." Charon and colleagues characterized our concerns about methodologic standards as "an outdated and ultimately doomed longing for a certainty the world does not possess." Stone and Rich also dismissed our stance as "naïve realism." They claimed that multiple authorities on the "philosophic and sociologic views of science" have rejected our stance as no longer "credible" and "arcane and simplistic."

Despite these criticisms, we admit that we believe, as Stone and Rich charged, that "fact can be separated from theories, objects separated from their observers" and "that things are just what they are even if they can mean different things to different people," under different circumstances, in different contexts, and depending on the goals the person is trying to accomplish. We are realists, but not naïve. For us, the truth is a goal to be pursued, even if we only imperfectly achieve it.

Presumably Berkwits, Charon and colleagues, and Stone and Rich believe that all reality, not just the reality of unobserved mental processes, is socially constructed. We, however, believe we must operate as scientists under the assumption that there is an external reality and that even unobserved mental processes can be rendered observable and assessed using careful and appropriate methods.

Because we used a quote from the second but not the third edition of *Clinical Epidemiology* to support the claim that there is external reality, Stone and Rich suggested our beliefs are obsolete. Similar quotes, however, appear in other epidemiology texts.^{4,5} For example, in 1997 Sackett and colleagues defined "valid" to mean "close to the truth."⁶

Stone and Rich claimed and Charon and colleagues implied that there are "no longer credible sources for . . . [our] arcane and simplistic position." We disagree. There are powerful sources for

our position, which is neither arcane nor simplistic. Some come from qualitative researchers, like Helman⁷ and Shmerling and colleagues,⁸ who believe that the validity of their work, i.e., its correspondence to an external reality, is important. More recently, Greenhalgh and Taylor wrote "the strength of qualitative research lies in validity (closeness to the truth)."⁹ Finally, Aronowitz's letter also called for standards to "assess the validity" of qualitative research.

Furthermore, some of the letters fail to acknowledge that many would argue with the notion that realism is passé. For example, Hodgkin went beyond our position: "health is one of the few remaining social values that garners unambiguous support. This is largely due to our continuing and communal belief that there is one truth 'out there' which can be known, understood, and controlled by anyone who is rational and competent."¹⁰ Sokal decried "a particular kind of nonsense and sloppy thinking: one that denies the existence of objective reality."¹¹ Then he wrote "intellectually, the problem with such doctrines is that they are false. There is a real world; its properties are not merely social constructions; facts and evidence do matter. What sane person would contend otherwise?" There are also several books critical of the application of the "naturalistic" world view to science.^{12,13}

Stone and Rich cited several authorities to support their argument that our beliefs have been discredited, but did not explain what they wrote. Similarly, Berkwitz suggested we were unaware of sources of "a rigorous and influential scholarship," but did not explain what they were.

Actually, the authorities that they do cite provide neither decisive nor unopposed support for reality as a social construct. For example, Cheney wrote, "Kuhn explained scientific revolutions as a result of paradigm shifts that give scientists new angles on reality—quite a different thing from arguing that reality is nothing more than a social construct."¹⁴ For another example, Heisenberg developed the uncertainty principle, which states any moving body shares properties of a wave group and a discrete entity. This limits the precision to which its position can be measured. One way to express this principle is: $\Delta x \Delta p \geq h$ where Δx is the uncertainty in the position of the wave group, Δp is the uncertainty in its momentum, and h is Planck's constant. Because this constant is so minute, 6.63×10^{-34} joule-second, this phenomenon is important only for sub-atomic particles.¹⁵ Just because a sub-atomic particle acts partially like a wave does not mean it is not real. Furthermore, the effect of the uncertainty principle is so small that it only affects studies of objects smaller than atoms. Therefore, the principle should not be extrapolated to argue against even trying to make objective measurements in the clinical and behavioral sciences.

Indeed, Gross and Levitt also challenged the misinterpretation of Heisenberg's work in support of the argument that reality is a social construct: "Now, the uncertainty principle is undoubtedly one of the cornerstones of quantum mechanics and one of the most philosophically provocative developments in the history of science. Under [Stanley] Aronowitz's description, however, it seems rather to refer to a kind of epistemological and spiritual malaise, plaguing the minds and souls of contemporary physicists." Furthermore, "the argument . . . is that since physics has discovered the uncertainty principle, it can no longer provide reliable information about the physical world, has lost its claim to objectivity, and is now embedded in the unstable hermeneutics of subject-object relations. This, alas, demonstrates depressingly well the connotative power of words when they are allowed to drift apart from their contextual meanings."¹³ pp. 51-2

Finally, our paper was about scientific methods in health care and medicine, not politics. Yet Stone and Rich thought the "ideological consequences of such an argument for the hegemony of a single way of knowing are most disturbing." We are also most disturbed by their response.

We never stated there is a single way of knowing. There are obviously multiple ways of knowing, e.g., philosophy, religion, history, literature, and the arts. Our paper, of course, was only about science and therefore addressed none of these other important areas.

Stone and Rich warned against "the notion of a single truth defined by a privileged few." This does not follow. If there is truth, a powerful elite can lie about it, but cannot change (or "define") it. Only if one believes that an individual's understanding of the truth constitutes the truth can the powerful presume to define it.

Stone and Rich were concerned about the dire consequences of our beliefs for women and people of color, whom they claim have been "previously marginalized by the hegemony of positivism." This concern we find inexplicable. Personally, we are both dedicated to equal rights and opportunities for all people regardless of color and gender. Furthermore, the implication that science has led to the oppression of women and minorities is extraordinary. Although we are not experts on history or political science, we can think of many reasons to believe the opposite: that truth is liberating, and that science is the enemy of oppression.

At least since the middle ages oppressive forces have tried to stifle science. Galileo was forced to recant because the truth of his quantitative, empirical observations about the rotation of the planets did not fit the then-contemporary beliefs and interpretations. The hallmark of recent dictatorships like Nazi Germany was the suppression of truth and the "big lie" of propaganda. Graham catalogued the misery produced by Soviet rejection of rational science, "long after the death of Stalin . . . what was the Soviet insistence on maintaining inefficient state farms and giant state factories, if not an expression of willful dogmatism that flew in the face of a mountain of empirical data?"¹² p.34 Leys wrote about the power of the truth to liberate people from oppression, "In the empire of lies, the humblest truth is revolutionary, mere reality is subversive."¹⁶

In the United States, there have been examples of the liberating power of science and specific examples in which behavioral scientists' research helped improve the situation of minorities and women. Behavioral research played an important role in the landmark Brown versus Board of Education Supreme Court decision that ended official racial segregation in the United States.¹⁷ This work is mentioned prominently in the decision and itself forms the basis for the Court's famed conclusion that "separate is inherently unequal." In contrast, an earlier Supreme Court decision that established segregation via the oppressive doctrine of "separate but equal" was based on a constructivist argument: that official segregation stamped African Americans with "a badge of inferiority" only if "the colored race chooses to put the construction upon it."¹⁸ This example shows the idea that reality is socially constructed is not new and is not necessarily used only to defend the oppressed. Constructivism, like science itself, belongs to no political party or ideology. More recently, in a landmark gender discrimination case, testimony based on quantitative behavioral research on cognitive approaches to stereotyping decisively influenced the courts to broaden the definition of gender discrimination by taking psychological processes of stereotyping into account in identifying discriminatory decision making.¹⁹

In summary, we hope our paper and the resulting invited letters and this response were at least entertaining but also were interesting and thought provoking. We believe that well-done qualitative research can be useful to describe new phenomena and raise hypotheses in medicine and health care. We look forward to future efforts to clarify its goals in this context, and to refine standards for its conduct and critical review. The success of these efforts, however, may depend on the ability of qualitative researchers to settle the simmering controversy about the philosophic underpinnings of their field. Finally, although we realize that well-intentioned people may base arguments about scientific philosophy or scientific methods on political ideology, we are concerned about the soundness of this practice, and fear it may lead to undesirable, if unintended consequences.—**ROY M. POSES, MD**, Pawtucket, RI, and **ALICE M. ISEN, PhD**, Ithaca, NY.

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