

RESEARCH ARTICLE

The Demise of the Bumbler and the Crock: From Experience to Accountability in Medical Education and Ethnography

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ABSTRACT In this article, I consider Howard Becker's 1955 research among medical students in relation to my own late-2000s research on standardized patients, or SPs (i.e., people hired to portray patients in staged clinical encounters with medical students). Becker's mid-20th-century subjects used the term *crock* for patients who presented obstacles to their acquisition of valued kinds of clinical "experience." SP simulations, as one among many forms of simulation used to teach clinical skills today, exclude the possibility of crocks. While medical education has changed, so too has ethnographic practice. Becker's account of his fieldwork, like many at midcentury, portrayed the ethnographer as a clueless "bumbler" who, through experience, gains understanding and expertise and is transformed into a professional anthropologist. Today, by contrast, the necessity to account in advance for the risks, rewards, and outcomes of ethnographic research has rendered stumbling inadmissible. I argue that the disappearance of the "bumbler" and the "crock" as regular figures in the discourses of anthropology and medicine points toward a reevaluation of "experience" in both fields and a shift toward new regimes of accountability, grounded in the changing political economy of knowledge production. At risk of being lost in the process are faith, surprise, and humor. [*ethnography, medicine, history, education*]

RESUMEN En este artículo, considero la investigación de Howard Becker en 1955 entre estudiantes de medicina en relación a mi propia investigación de fines de los años 2000, sobre pacientes estandarizados, o SPs (es decir, gente contratada para representar a los pacientes en encuentros clínicos creados con estudiantes de medicina). Los sujetos de Becker de mediados del siglo XX usaban el término *lentos* para pacientes quienes presentaban obstáculos en su adquisición de tipos valiosos de "experiencia" clínica. Las simulaciones de SP, como una en medio de muchas formas de simulación utilizadas para enseñar destrezas clínicas hoy, excluye la posibilidad de lentos. Mientras la educación médica ha cambiado, así también la práctica etnográfica. El reporte de Becker de su trabajo de campo, como muchos a mediados de siglo, representó al etnógrafo como un despistado charlatán, quien a través de la experiencia, gana conocimiento y pericia, y se transforma en un antropólogo profesional. Hoy, por contraste, la necesidad de considerar por adelantado los riesgos, recompensas y resultados de la investigación etnográfica ha vuelto la charlatanería inadmisibles. Argumento que la desaparición del "lento" y del "charlatán" como figuras regulares en los discursos de antropología y medicina señala hacia una reevaluación de la "experiencia" en ambos campos y a un cambio hacia nuevos regímenes de responsabilidad, fundamentados en la cambiante economía política de la producción de conocimiento. A riesgo de perderse en el proceso están la fé, la sorpresa y el humor. [*etnografía, medicina, historia, educación*]

In 1993, the sociologist Howard Becker published a short essay titled “How I Learned What a Crock Was” (Becker 1993), reflecting on ethnographic fieldwork that he had carried out among medical students in the 1950s (see also Becker et al. 1961). In the essay, Becker described his arrival at the University of Kansas School of Medicine, a man ignorant of the most basic features of medical training and unclear about exactly what he would be researching and how: he began his fieldwork as what I will be calling a “bumbler.” As Becker recounts, he eventually stumbled onto a fruitful line of inquiry when he noticed that the students he was studying referred to some of the patients whom they saw as “crocks”; the process of puzzling out what they meant by this unfamiliar term, through ongoing questioning and observation, became an entry point for learning important lessons about the value system and social hierarchy of the social world of the medical school. *Crock*, as it turned out, was a term that medical students, intent on gaining clinical experience, used to refer to patients who they found difficult and frustrating in very particular ways. Becker’s essay concluded with a bit of methodological advice:

Learning what a crock was [was] thus a matter of carefully unraveling the multiple meanings built into that simple word, rather than the Big Ah-Ha . . . This little ah-ha may have a lesson for us when we experience the Big Ah-Ha. Intuitions are great but they don’t do much for us unless we follow them up with the detailed work that shows us what they really mean, what they can really account for. [Becker 1993:31]

I read Becker’s essay soon after it was published. At the time, as a graduate student trying to pursue fieldwork in a hospital-based setting, I was grateful for the insights that Becker offered—into the social world of U.S. biomedicine and into the question of how to do ethnographic research. Rereading the same essay today, however—20 years after it first appeared and nearly 60 years after the fieldwork that it describes was carried out—what strikes me most powerfully is the shock of realizing just how much has changed, in medical education and ethnographic practice alike. My own ethnographic research on U.S. medical education in the 21st century suggests that recent changes in how U.S. medical schools organize the teaching of “clinical skills” have more or less eliminated the conditions that allowed the “crock” to figure as a regular fixture of professional training in medicine. At the same time, my own experiences and observations as someone involved with ethnographic research—first as a graduate student and subsequently as a professor, teacher, and reviewer—suggest that recent changes in how U.S. institutions organize ethnographic practice have more or less eliminated the conditions that allowed the ethnographic “bumbler” to figure as a regular fixture of professional training in anthropology. Let me clarify that confusion, frustration, and mistakes have not been banished from either medical training or anthropological research—far from it. People with the kind of medically frustrating complaints that might once have earned them the label of “crock” still fre-

quently appear in the clinic; and ethnographers researching unfamiliar social worlds continue to make mistakes borne of ignorance as they seek understanding. As regular figures of professional discourse in these two fields, however, the bumbler and the crock have largely disappeared. The realities to which the terms point are with us still, but they are no longer admissible.

In this article, heeding Becker’s advice, I seek to follow up on this “little ah-ha” with the detailed work of showing what this change really means. As I shall suggest, the disappearance of the bumbler and the crock from the discourses of anthropology and medicine bespeaks a much broader shift, grounded in political-economic transformations in the social organization of knowledge production. I argue that under the pressure of changes in the political-economic landscape of higher education, medical training and ethnographic research both appear increasingly as matters to be carefully planned, controlled, policed, documented, and accounted for in terms of measurable outcomes, testable competencies, standardized and bureaucratized procedures, and controllable risks. The value once attached to certain kinds of experiences with other people has given way to a regime of accountability, with consequences that are not yet fully understood.

1955: THE BUMBLER DISCOVERS THE CROCK

Becker’s account of his entry into his field site will ring familiar to anthropologists who have read other firsthand accounts of ethnographic research carried out in the middle decades of the last century: the ethnographer arrived in the field as a bumbler, clueless about quite basic aspects of the social world into which he had entered but backed by a powerful authority figure and helped by the kindness and tolerance of the people upon whom he has imposed himself. Becker writes,

I had very little idea of what I was going to do beyond “hanging around with the students,” going to classes and whatever else presented itself. I had even less idea what the problem was that we were going to investigate . . . I knew next to nothing about the organization of medical education, and consoled myself about my ignorance with the “wisdom” that told me that therefore I would have no prejudices either . . . Fortunately, the Dean of the school took me in hand and decided that I should begin my investigations with a group of third year students in the Internal Medicine Department . . . With no problem to orient myself to, no theoretically defined puzzle I was trying to solve, I concentrated on finding out what the hell was going on, who all these people were, what they were doing, what they were talking about, finding my way around and, most of all, getting to know the six students with whom I was going to spend the next six weeks. [Becker 1993:28]

The third-year students whom Becker was observing were newly embarked upon the “clinical” phase of their medical training. Typically, the first two years of medical school were largely classroom based, while the final two years immersed students in bedside teaching and supervised

work with patients on the hospital wards. For Becker, the question soon arose of what degree of access he should and would have to interactions between the budding physicians and the patients with whom they worked:

None of us were sure what I was “allowed” to do or which things they did were “private” . . . the first time one of the students got up and said, “Well, I have to go examine a patient now,” I could see that I had to take matters into my own hands and set the right precedent. Neither the Dean nor anyone else had said I could watch while students examined patients. On the other hand, no one had said I couldn’t do that . . . If I let the situation get defined as “The sociologist can’t watch us examine patients” I’d be cut off from one of the major things students did. So I said, with a confidence I didn’t feel, “OK. I’ll come with you.” He must have thought I knew something he didn’t, and didn’t argue the point. [Becker 1993:28]

Having thus established as a matter of precedent that he could follow students around as they made their morning rounds, Becker did so. He noticed that among themselves, students referred to some of the patients whom they saw as *crocks* (shorthand for a “crock of shit,” which in everyday colloquial speech means “a lie” [Winick 2004]). He was baffled: What was it that the patients they called crocks had in common, and why did students find them so annoying?

Through repeated questioning and discussion, Becker eventually arrived at a satisfactory definition of the term *crock* as “a patient who had multiple complaints but no discernible physical pathology” (Becker 1993:30). Having this definition in hand, however, took him only part way toward understanding what crocks were about. As he wrote:

What students wanted to maximize in school, not surprisingly, was the chance to learn things that would be useful when they entered practice. But, if that was true, then it seemed contradictory to devalue crocks, because there were many such patients. In fact, the attending physicians liked to point out that most of the patients a physician saw in an ordinary practice would be like that. So a crock ought to provide excellent training for practice. [Becker 1993:30]

Pursuing this paradox led to insights about how students conceptualized the “clinical experience” that they sought to gain—to which crocks were perceived as an obstacle. Becker and colleagues explained, in their now-classic 1961 study *Boys in White*, that students understood clinical experience to be necessary because

“book knowledge” may be deficient in a number of ways. It may simply be wrong when tested against the staff member’s knowledge, gleaned from his own handling of patients. Or the book knowledge may not be available, the necessary research not have been done . . . it may be an insufficient basis for learning important and necessary things [such as how to recognize heart murmurs] . . . Finally, “the book” may not take into account the practical facts of life, as when laboratory tests which are in principle useful, are discounted because of practical difficulties in their administration and interpretation. [Becker et al. 1961:231]

Students also correctly perceived clinical experience to be associated with status and authority within the social hierarchy of medicine, in that “the major divisions—between students, interns, residents, and junior and senior staff—are

legitimated with reference to the increasing clinical experience of occupants of each higher position on the ladder” (Becker et al. 1961:234).

According to Becker, crocks were frustrating to medical students in at least three important respects:

- (1) crocks disappointed students by having no pathology you could observe firsthand;
- (2) crocks also liked to talk at length about their problems and previous medical encounters, so they took much more of your time than other patients while offering you much less of anything you wanted for your trouble; and
- (3) crocks were also frustrating, in that one cannot perform a medical miracle on someone who was never sick in the first place.

Crocks, in other words, were patients who, while wasting medical students’ time, failed to provide them the opportunity to gain the kind of clinical experience that would allow them to develop and demonstrate their medical skills, authority, and identity.¹

“EXPERIENCE” AT MIDCENTURY

Though Becker did not develop his argument in a reflexive direction, it is not difficult to see how this analysis of the meanings and social value of clinical experience among medical students might be adapted and extended to describe the meanings and social value of “fieldwork experience” among ethnographers. Becker’s description of his own younger self as a naïf in the field seems disarmingly open. His willingness to admit to his own initial ignorance, however, belies the considerable edifice of pedagogical and methodological commitments and professional hierarchies out of which Becker’s research, including its bumbling initial stages, emerged. If the medical students who he was observing were struggling to gain the kinds of “experience” necessary to establish themselves within their profession, the same was no less true of Becker himself as a young ethnographer.

The opposition between “book learning” and “clinical experience” that Becker and his colleagues identified within medical education closely parallels the way that (book-learned) “theory” and (experiential) “fieldwork” were configured in relation to each other in the social sciences during the middle decades of the 20th century. In his autobiographical reflections, Paul Rabinow wrote that he had been drawn to anthropology as an undergraduate student in the mid-1960s because “it seemed to be the only academic discipline where, by definition, one had to get out of the library” (Rabinow 1977:3). Having entered the Ph.D. program, he quickly learned that the special value accorded to fieldwork as a transformative experience was what structured hierarchical relations within the profession of anthropology:

In the graduate anthropology department at the University of Chicago, the world was divided into two categories of people:

those who had done fieldwork, and those who had not; the latter were not 'really' anthropologists, regardless of what they knew about anthropological topics . . . his intuition had not been altered by the alchemy of fieldwork. I was told that my papers did not really count because once I had done fieldwork they would be radically different. Knowing smiles greeted the acerbic remarks which graduate students made about the lack of theory in certain of the classics we studied; never mind, we were told, the authors were great fieldworkers. [Rabinow 1977:3]

Ethnographic fieldwork was thus accorded a very special status as a form of experience: it authorized the claims made by the individual researcher in his or her scholarly writings while also underwriting his or her claims to authority and status within the social hierarchy of the profession.

If fieldwork experience was critical to the professional formation of the ethnographer, the widely shared expectation was that this experience would begin with a certain measure of ignorance, disorientation, and confusion. In the discipline of cultural anthropology, which had long distinguished itself from Becker's home field of qualitative sociology by claiming expertise in "primitive," "small-scale," "traditional," and "foreign" cultures as its object of study, the expectation that ethnographic research would begin with a degree of bumbling about was dignified with the name "culture shock" and elevated to something of a methodological premise.

The organization of graduate training, meanwhile, more or less ensured that fieldwork would in fact begin with a lot of ineptness; many budding ethnographers had few relevant language skills and received little or no methodological training or advice before embarking upon their research. Laura Nader, reflecting on her own preparation for dissertation fieldwork in 1957, described it thus:

There was not much emphasis on methodological training at Harvard . . . When I asked Kluckhohn if he had any advice, he told the story of a graduate student who had asked Kroeber the same question. In response, Kroeber was said to have taken the largest, fattest ethnography book off his shelf, handed it to the student, and said, "Go thou forth and do likewise." The story did not reassure me. [Nader 1970:98]

Similarly, the anthropologist Laura Bohannon, writing under the pen name Elinore Smith Bowen, described arriving to do fieldwork in Nigeria in the early 1950s not only uninformed about many basic aspects of the society into which she had entered but deliberately without any common language in which to communicate:

That no one with me knew more English was my own fault, and my set intent. It had been the first advice given me. "Never use an interpreter," my professors had intoned, "or you'll never learn the language properly." [Bowen 1964:4]

The backdrop for this commitment to immersion as a mode of learning was, of course, a context of colonial power relations that made it possible for budding young U.S. and European ethnographers to impose themselves upon communities they wished to study. Perhaps not surprisingly, among the few other bits of advice that Bohannon describes

having received was a clear admonition not to annoy the powers that be:

The best advice, in the long run, came from the ripe experience of two professors of anthropology. One said, "Always walk in cheap tennis shoes; the water runs out more quickly." The other said, "You'll need more tables than you think." Both had added, without going into detail, "Enjoy yourself, and never, never be an embarrassment to the administration." [Bowen 1964:4]

Though oriented toward somewhat different objects of study, Howard Becker's field of qualitative sociology shared with midcentury cultural anthropology a commitment to ethnographic research and, with that, an investment in bumbling as a necessary and expected part of the knowledge-production process. That Becker didn't have a clearer sense of what he was doing at the outset of his research was not, in other words, simply an idiosyncratic failing on his part, nor was it simply an oversight on the part of those responsible for his graduate training. Rather, the expectation that ethnographers would begin their research as awkward, out-of-place, inept naïfs was integral to how the fieldwork experience was conceptualized as a method and how it was configured as part of the professional socialization of an ethnographer. Your job as a graduate student was to learn how to do ethnographic research, and the way it had to be learned was by doing it, which meant that you had to dive into an unfamiliar world and figure things out for yourself. Becker himself, while clearly acknowledging his own cluelessness at the outset of his fieldwork, has asserted that this constitutes the necessary initial stage in ethnography as a "systematic, rigorous, theoretically informed investigative procedure":

Successful researchers recognize that they begin their work knowing very little about their object of study, and that they use what they learn from day to day to guide their subsequent decisions about what to observe, who to interview, what to look for, and what to ask about. [Becker 2009:547; see also Sanders 2013:220]

The figure of the bumbler was thus embedded in the discourse of ethnographic methods as a recurrent trope, an image available to ethnographers as a template through which to interpret and describe their experiences. Like any trope, this one concealed and downplayed some elements while revealing and highlighting others; it likely encouraged individual ethnographers who in fact were quite planful, savvy, shrewd, and strategic to portray themselves textually as bumlbers. As it happened, this trope lent itself particularly well to ethnographic writing because it offered a ready-made narrative combining elements of the transformative journey story, the love story, and the coming-of-age story: the ethnographer travels to a strange world where he does not belong; overcomes hardship, loneliness, and misunderstanding; achieves a relationship of understanding with the locals; returns home with hard-won insight; and is (*voilà!*) transformed into an expert. This trope also had the significant narrative advantage of positioning the ethnographer within the text as someone with whom the reader could easily identify: like the bumbling ethnographer, the reader

too begins in ignorance of the new world into which she has (by opening the book) entered but gradually comes to understand who is who and what is going on.

In short, midcentury social science discourse about ethnography granted a clear, legitimate, authoritative, and valued place to the bumbler as a figure defined by an initial lack, and subsequent acquisition, of a particular kind of valued experience with other people.

THE DISAPPEARANCE OF THE CROCK

Fast-forward fifty-odd years, and move a bit north and west from Kansas to Seattle, where I pursued ethnographic research on medical education in the late 2000s, following in the footsteps of Becker and many other scholars (Bosk 1979; Conrad 1988; B. Good 1994; M. Good 1995; Hafferty 1991; Konner 1987; Lief and Fox 1963; Sinclair 1997).²

The overall structure of U.S. medical education has remained relatively consistent from the 1950s until today: four years of medical school (the first two primarily classroom based and the last two focused on clinical training), then one year of internship, now followed in turn by three to five (or more) years of residency. However, much else had changed in the intervening decades. Advances in many areas of medical science have vastly expanded the knowledge base that medical students are expected to master and have led to the development of many new medical specialties. The rise of evidence-based medicine (Timmermans and Berg 2003) and, with it, algorithms that specify courses of treatment to be followed in a wide range of specific situations; the growth of large managed-care organizations; and the advent of computerized medical records all have significantly altered conditions of work for U.S. clinicians. Globalization and successive waves of migration have yielded linguistic, cultural, and religious diversity in patient populations; in response, medical schools have developed curricula intended to impart “cultural competence” along with other skills and competencies (Taylor 2003a, 2003b; Jenks 2011; Shaw and Armin 2011).³ New diagnostic tests and medical imaging technologies, meanwhile, produced ever more new forms of information—to the point that some began to voice concern that clinical skills were being allowed to atrophy as physicians learned to attend more to “icons” of patients than to the embodied persons before them (Verghese 2008).

Within the field of medical education, many calls for curricular reform focused on the need to change how students were taught clinical skills. As hospital stays became shorter and shorter, and more and more care came to be provided in ambulatory outpatient settings, the traditional hospital-based approach to clinical teaching came to seem out of step with the realities that students would later face. Bedside teaching as it had been practiced for decades in U.S. medical education also came under criticism as out of step with current pedagogy, being “little more than unstructured apprenticeship experiences that lacked clear learning objectives” (Whitcomb 2000). Partly because they could be designed around such learning objectives, and partly because

they avoided the risk of harm to patients (Ziv et al. 2003), simulation-based teaching methods were hailed as a means of teaching clinical skills. Simulations also made standardization possible by ensuring that every student would be exposed to exactly the same learning situations, eliminating the variability entailed in other experiential forms of teaching involving work with patients (Hodges 2003) and thus rendering assessment easier and more efficient.

It was one of these relatively new teaching methods that I set out to study in the late 2000s. My focus was on standardized patients (or, as they are often called, “SPs”), one among many modes of simulation (alongside high-tech mannequins, virtual reality simulators, and so forth) that had by the mid-2000s become quite central to how the profession of medicine teaches and assesses students’ “clinical skills”—now defined broadly to include not only the basic skills of performing a physical examination (taking blood pressure, detecting lung sounds, pulse, and so forth) but also “questioning skills,” “information-sharing skills,” and “professional manner and rapport” (USMLE 2010).

SPs are people whose job it is to roleplay the part of patients with particular complaints and histories in staged clinical encounters with medical students and other health professions students. The majority of such encounters are structured to resemble an ordinary office visit: the medical student has a limited time to take a history, perform a physical examination based on the information gathered, and then communicate to the person portraying the patient a diagnostic assessment and plan. Many are learning opportunities and are followed immediately by a “debriefing” in which the SP provides the student constructive feedback on what they did well, what they ought to work on, and so forth. Others are high-stakes assessments, conducted under the critical scrutiny of evaluating observers, following which the SP fills out a checklist that becomes an element of the student’s grade. Reliance on this form of simulation has increased dramatically in the United States as “clinical skills” tests involving SP performances have become a component of the national licensing examination that medical school graduates must pass before they can legally practice medicine (Taylor 2011).

When the people involved in scripting and developing SP “cases” go about making them, they begin with questions not about the medical condition or situation to be portrayed but about pedagogy. They start by identifying the learning goals, as well as the knowledge base expected, of the students who will take part. Not only should SP scenarios realistically model clinical reality, in other words, but they should present it in measured ways carefully tailored to the students’ expected educational trajectory. Andrea, who directs an SP program at a West Coast university, explained:

Generally [medical faculty] try to start with educational objectives . . . like, “You know, it would be great if they could do a focused history and physical. And then, they should probably write it up.” And so from that you start pulling out, like, with an objective that everybody identifies as the essential components for a primary

care case-focused history. Yes. And so then, you write out the objectives. And then, you think about what might fit in, what might work for this. So for instance, okay. Migraine, would that be good? Yes. Because there's a physical exam component. There's a history component. And so then, we'd say, "What about stroke?" No. Because you're going to, probably, do that in the hospital, and they haven't learned a neuro exam yet. So they're not going to be able to necessarily know what to do with them. [interview, September 10, 2008]

Each SP scenario thus ideally presents students with situations from which they can learn something that is new and that fits well into their course of training—building upon what came before and leading up to what comes after.

This concern for pedagogy is the mark of a carefully designed educational program in which each element is part of an integrated, coherent whole that is attuned to progress of the student's learning. One consequence of this concern with pedagogical "fit" is that every SP performance has (at least in principle) a clear educational objective, a correct diagnosis to be discovered, and a specific lesson to teach—and it is carefully designed to be a lesson that the student both needs to learn and is ready to learn. What SP performances and other forms of simulation do not create or allow is an encounter with a patient from whom a student cannot learn anything that will be useful in furthering his or her progress through medical school. In other words, they exclude precisely the kind of difficult and frustrating patients that Becker's midcentury medical students called "crocks." To the extent that SP performances partake in and reflect the broader discourse about clinical skills and how one learns them, it seems fair to suggest that the figure of crock as a fixture of medical education has disappeared.

Once medical students enter their third year and begin clinical clerkships, of course, students encounter many different kinds of patients, including some who are frustrating and difficult—but no longer do they encounter crocks. The small, odd body of scholarly literature on crocks (including Coombs et al. 1993; Gordon 1983; Harrison 1963; Whitney 1981; Winick 2004) seems to end with Winick's 2004 article, based on research that was carried out in the 1990s. More recent research among medical students (Wear et al. 2006) documents continued use of derogatory terms to refer to patients—but not that one. The crock seems to have met its demise.

THE DISAPPEARANCE OF THE BUMBLER

Focused as I was on my object of study, even as I began to actively wonder about the disappearance of the crock from medical education, it only very belatedly dawned on me that ethnographic practice has undergone equally dramatic—and related—changes during the same decades. Consider, for example, how far my process of gaining entrée into the field differed from Becker's description of his own fieldwork experience in 1955.

Having heard about standardized patients from a colleague sometime in the 2000s, I became curious about them, contacted someone involved in hiring and training SPs lo-

cally, and arranged for a casual informational interview. I talked to her and to other colleagues and friends about SPs and searched out and read articles about them in newspapers and in the medical education literature. Then I wrote and submitted applications for various fellowships and grants to support an ethnographic study of SP performances. None were successful, but I went ahead and submitted a human subjects application to the University of Washington's Institutional Review Board (IRB).

My IRB application was 14 single-spaced pages in length and specified what the research activity would consist of, what its purpose was, how data would be collected, how many subjects would be enrolled, how subjects would be selected for inclusion, how they would be recruited, what steps would be taken to minimize coercion or the appearance of coercion in recruitment, whether I would give subjects any gifts or payment for participation, what risks and benefits the research might present for participating subjects, what steps would be taken to minimize risks of harm and protect subjects' welfare, what benefits were expected from the research, how the benefits were expected to outweigh the risks, what adverse events or effects might be anticipated and how they would be handled, how confidentiality of the research data would be protected, and whether the data would be used for other studies in the future—among other things. The application also included more than 40 pages of appendices: sample interview questions, sample recruitment scripts, and consent forms for each category of research participant.

After some weeks (and quite a few e-mail exchanges), the Human Subjects Division notified me that my application had been approved, and I set about the project. I never "went to the field," as such, but simply squeezed this research in as best I could, amid the constraints of teaching, family, and other obligations. Over the course of about two years, I ended up conducting 27 lengthy interviews, some in person and others by phone, all of which were audio-recorded and then transcribed. I also attended two national conferences of professionals involved in SP work, as well as another national conference focused on simulation and virtual reality in medicine. I subscribed to an SP e-mail listserv and wanted to be able to quote some of the messages that circulated there, so I submitted a modification application to the IRB to seek approval (which I received) of a process for obtaining informed consent from authors of e-mail messages that I wished to quote. Two years ago, I decided that I would not be doing any more interviews for this project and closed out the human subjects approval (an activity requiring submission of a separate series of forms and documents).

I belabor this description of the bureaucratic nitty-gritty details of my research process to make the rather obvious point (which I somehow nonetheless managed to not notice for a very long time) that, even when the geographic location, topic, and setting are broadly similar, ethnography as practiced in the 21st century is quite far removed from ethnography as practiced in the mid-20th century.

Long gone are the days when an ethnographer could bumble into the research setting, as Becker described himself having done, with no clear research question, no real idea of what is and is not allowed, and no indication at the start that the people he intended to study were willing to be part of the research project. Ethnographers and other researchers must now explain and account in advance for exactly what they will do, how, what new knowledge will result, and what its implications or practical applications will be.

IRB protocols are, of course, only one aspect of this large, complex shift from experience to accountability, and far from its only source. Broad-scale political-economic shifts in the world within which researchers operate (the end of the colonial era, and the advent of mass travel, mass communications, and large-scale globalization, to name just a few) have eliminated many of the structural conditions that had undergirded the ethnographic practices of U.S. anthropologists at midcentury. The postwar expansion of U.S. higher education, and with that the growth of anthropology as a profession, has brought a measure of formalization and bureaucratization to processes of graduate admissions, funding allocation, graduate methods training, peer review, job applications, and so forth—one consequence being the need for ever more documents that explain and account for decisions to fund, certify, and publish the work of ethnographers. Legislation and policies intended to counter “old boy networks” and diversify the profession have also led to the creation of new structures and mechanisms of accountability.

This shift has unfolded rather unevenly, and doubtless it takes on more pronounced forms at large public research universities (such as the one where I work) than at private colleges and universities, which do not rely to the same degree on government monies, are not legally required to make information publicly available, and thus are not held accountable in the same ways. I suspect, also, that similar processes have played out quite differently outside the United States. With those caveats, however, I think it is still fair to say that as ethnographers increasingly are asked to account explicitly, and in advance, for the value, outcomes, and impact of their work, the trope of the bumbler has been effectively dethroned within anthropological discourse.

A more detailed account of the somewhat different histories that lie behind the changes that have taken place in medical education and in ethnographic practice is beyond the scope of this article, but I would like to point out a certain symmetry in their outcomes: little legitimate place remains within the discourse of anthropology today for bumbling as part of ethnographic research, just as little legitimate place remains within the discourse of medical education today for the frustrating encounters that led midcentury medical students to label some patients as *crocks*. Clearly, the kind of patients that used to be called crocks have not disappeared—they (we!) are still around and show up frequently in clinical practice. Nor have ethnographers ceased to bumble about, as anyone who has actually carried out ethnographic research can attest. But the figure of the bumbler as a regular fixture of anthropological training has disappeared from discourse,

right along with the figure of the “crock” as a regular fixture of medical education.

SHOULD WE MOURN THE BUMBLER AND THE CROCK?

I have pointed out what I see as a certain parallel between changes in medical education over the past 60 years and changes in ethnographic practice: the disappearance of the bumbler and the crock. What, then, should we make of all this? Is there anything we should value that stands to be lost with the demise of these two tropes?

No thoughtful person with any sense of the history of anthropology can wax nostalgic for a past when ethnographers were free to impose themselves without permission or plan on communities of people they wished to study, nor can any thoughtful person who knows anything about the history of medicine look back nostalgically on the days when medical students were free, and tacitly encouraged, to mock and insult patients. There are many very good reasons, along with some more problematic ones, for the changes that have taken place. So let me be very clear that I do not wish to argue for a return to 1955. At the same time, however, it seems unlikely that these changes represent simple progress; the reality is bound to be more complicated and replete with unintended and ironic consequences.

I would like to suggest that what links these two “extinction events”—that is, the disappearance of the bumbler and the crock—is a subtle but profound large-scale shift in the meaning, valuation, and social organization of experience in U.S. higher education. It used to be the case, in both medicine and anthropology, that students were expected to throw themselves into certain kinds of messy encounters with other people and learn from these experiences in ways that would help them transform themselves into certain kinds of experts.⁴ Today, education and research appear more as something to be carefully planned, controlled, policed, documented, and accounted for. These logics are not easily applied to “experience,” which may in part explain the emphasis on “competencies” and “skills,” imagined as things that can more readily be tested, demonstrated, and assessed (Urciuoli 2008).

I see at least three factors contributing to this trend:

- (1) Education is very expensive, and as the state increasingly withdraws support from higher education as a public good, those costs fall increasingly heavily upon individual students and their families.⁵ Parents, lenders, state legislators, and academic administrators demand the promise of demonstrable skills and measurable outcomes.
- (2) Research is also expensive, and as the state increasingly withdraws support from all forms of research, the pressure and competition upon remaining funding sources becomes increasingly intense.⁶ Governing boards of private funding agencies, legislators involved in decisions about allocation of funds to public funding agencies, and scholars reviewing applications all feel

the need to direct scarce resources toward projects that promise to produce results. Indeed, one need nearly have completed the research already to be able to describe it in a manner sufficiently detailed and persuasive as to win support—a catch-22 situation that tends to further advantage faculty and students at wealthier institutions with in-house resources sufficient to support extensive pilot work.

- (3) Working with people as a means of learning how to work with people, whether as a clinician-in-training or as an ethnographer-in-training, is increasingly understood to be inherently risky—for the people that students encounter, for the students themselves, and for the institutions responsible for (and to) them. To be sure, the changes we are charting have been driven partly by the efforts of social activists and scholars who have objected to abuses of power within medicine and within anthropology. Their insights and concerns are, however, only very partially and imperfectly translated into the language of “risk” and the corresponding legal and bureaucratic mechanisms of risk management (Stark 2012). Moreover, the extension of those mechanisms, developed initially within medicine, to encompass research in the social sciences has proven to be problematic (Schrag 2010).

If I am correct in that the twin disappearances of the bumbler and the crock bespeak an underlying shift regarding the valuation and social organization of “experience” as a component of education, then pointing this out may, I hope, offer a starting place from which to inquire into the changing political economy and culture of knowledge production across multiple fields of specialization—medicine, anthropology, and beyond. I also hope that ethnographers and medical educators alike might also pause to consider and discuss the unintended and ironic consequences of changes in which all of us have been caught up, and reflect on their broader implications. Is there anything of value that stands to be lost when somewhat unpredictable interactions with other people become something carefully fenced off from the learning processes involved in becoming physicians or ethnographers? What is the downside for the human sciences of all this planning and accounting for what will be done and what will be learned? In short, should we mourn the passing of the bumbler and the crock?

I submit that yes, we should mark and mourn their passing or, rather, we should acknowledge what has been lost along with the figures of the bumbler and the crock, even if we would not actually want to have them back. I offer here a few thoughts on what I think may have been lost—a snapshot of the baby being thrown out along with the bathwater.

Faith

The bumbler and the crock emerged out of a world in which the institutions responsible for their training vested what in

retrospect seems like a remarkable degree of faith in budding physicians and ethnographers—that they would figure out how to deal with people and they would learn what they needed to learn. That faith was the flip side of a certain security of privilege, insofar as the reason these students could be free to bumble about was that the possibility of failure had largely been institutionally closed off already. Such institutional privilege went—as it still goes—hand in hand with other forms of privilege in a society deeply stratified by “race,” class, and gender divisions. Still, the place of honor accorded to “experience” with other people bespoke a degree of faith that has become rare in higher education today—faith in the person of the student as more than simply a bundle of skills, as someone who is capable of learning and growing through experience, including the experience of making mistakes, and who has a future in which the institution is invested.

Annemarie Mol makes a similar point, in the context of arguing for attention to failures and mistakes as necessary elements of the tinkering and learning from experience that is critical to good care in practice:

Not only is there a lot to learn from practices that work well. Failures, too, are instructive. The traditional case history often dealt with failures, because these surprised the doctor who reported on them almost as much as miraculous recoveries. What is more: if others were told about them, they might avoid making the same mistakes. In this light, it is remarkable that current accountability practices require professionals to prove that they do well. Professionals are constantly required to praise themselves. Here are the evaluation forms, account for what you have been doing! There is no room for doubt, self-criticism, or difficult questions. However, improvement begins with the recognition that something needs to be improved. That not everything is as it should be. It fits with the logic of care to attend to frictions and problems. To acknowledge that some things do not work well, no matter how well intended they may be. This suggests an entirely different accountability practice. Not one in which everyone has to say how wonderful they are, but one in which *people feel safe enough* to examine what in their practices tends to go wrong and why. [Mol 2008:88]

The budding ethnographer and the aspiring physician of today must regularly account for themselves, as must the institutions in which they are embedded, and much is at stake: admission, funding support, successful completion, employment, accreditation, reputation. The possibility of failure is always looming, and few can afford an easy confidence or faith in the future. In this respect, at least, the harried, debt-ridden student of today might well envy the bumbling ethnographers and crock-disparaging medical students of six decades ago.

Surprise

Another casualty of these changes, I’m afraid, is openness to surprise. In midcentury medical education, bedside teaching always encompassed the possibility that something entirely unexpected might at any moment walk through the door—that was, indeed, the whole point. As simulations have displaced bedside teaching, this openness to the

possibility of surprise has given way to the confidence that students will have been presented with a particular range of cases in a manner tailored to their current knowledge base. Of course, SP performances—unlike other forms of simulation training—do in fact involve interactions with real, embodied persons and, as such, the possibility of surprise can never be fully eradicated (Taylor 2011). On rare occasions, it can and does happen, for example, that a medical student will discover some real, previously undiagnosed health problem with the SP in the course of a simulated examination (Castillo 2014). Still, such surprises now have the status of rare anomalies rather than expected and valued learning experiences.

A similar shift can be discerned in anthropology as well. For example, grant and fellowship applications submitted by graduate students today exhibit, in my observation, two quite striking tendencies: on the one hand, many of these research proposals are extraordinarily polished and professional and grounded in a rather staggering command of the relevant scholarly literatures. On the other hand, many don't really have a question. They are so incredibly well worked out and they explain so thoroughly what the research will accomplish and prove that they have constructed airtight interpretations that effectively leave little or no room for surprise. It is often unclear what the authors hope or expect to learn from the ethnographic research that they propose to do, that they don't already know. Having banished the bumbling ethnographer, and having moved away from the confident assumption that fieldwork "experience" will naturally and inevitably lead to understanding and expertise, anthropology has perhaps produced a generation of scholars who have been rigorously schooled to account in advance for the risks, rewards, and outcomes of their research—at the expense of openness to the possibility of surprise. Surprises still do crop up in the course of fieldwork, of course, and thoughtful ethnographers still do learn from them, but there is no longer much room to admit such realities in the official discourse. One consequence, I fear, may be a widening gap between how ethnographers describe their research in forms of writing understood to be instrumental (such as applications of all stripes) and how they really think, talk about, and practice their research. As surprise has been forced underground, a habitual disingenuousness seems to take its place.⁷

Humor

One last reason we might mourn the passing of the bumbler and the crock is that they allowed for a sense of humor. When it was accepted that people would have messy learning experiences along the way to becoming experts, it was permissible to admit mistakes and laugh at them. Midcentury accounts of ethnographic fieldwork often poked fun at the idiotic mistakes made by the authors before they learned better. Similarly, accounts of medical education from decades past (most famously, Shem 1978 [republished in 2003]) told

rollicking stories about the bad behavior and stupid mistakes made by doctors-in-training.⁸

Both of these forms of humor sometimes had an ugly edge to them, insofar as they invited readers to join with the privileged protagonists in laughing not only at themselves but also at the people among whom they worked who had little choice but to endure their idiocies. One might reasonably hope and expect that budding ethnographers and physicians of today are far less cavalier, far more cognizant of their relative power and privilege, as well as the dangers and consequences of abusing it, and far more careful and conscientious about conducting themselves in ways that are ethically admirable. Not coincidentally, I think, writings about ethnographic fieldwork and about medical education are far more serious and earnest in tone these days, and rarely strike a humorous note. Midcentury accounts are, by today's standards, sometimes not very funny—but is nothing ever funny anymore? Has the quantum of absurdity in human life somehow diminished? Or, more likely, does it simply not feel safe or legitimate anymore to laugh about human beings and their (our) foibles?

Not all forms of laughter are "at" people, not all forms of humor are cruel. Puns work like poetry, creating delight by linking things in unexpected ways, opening counterintuitive possibilities of meaning. Indeed, for at least 40 years, the ability to grasp such multiple levels of meaning—to distinguish winks from twitches—has been held up as the paradigm for a successful ethnographic analysis (Geertz 1973). The various forms of irony, meanwhile, insofar as they work through heightening awareness of contradictions—gaps between expectations and outcomes, between literal and deeper levels of meaning, between what ought to be and what is—are arguably powerful tools for facilitating critical reflection, for ethnographers and physicians as much as for the people with whom they work (Lambek and Antze 2003).

Laughter may furthermore be, as Julie Livingston has suggested, "a spontaneous response to an overwhelming experience" (Livingston 2012:147) that counteracts its isolating and dehumanizing effects. Livingston writes of pain in the context of cancer care in Botswana, but her insight about laughter as a socializing force may apply as well to ethnographic research and medical training, both of which can be overwhelming and in some respects intensely isolating experiences. Transforming such experiences into stories that invite shared laughter may serve a valuable function for the discourse, insofar as laughter is "a form of social expression . . . [that] comes in moments when cultural norms fail to be enacted . . . and thus, in its recognition of the absurd, laughter reinforces the norm, by socializing it" (Livingston 2012:148).

Without urging a return to the bad old days, I would therefore nonetheless argue for reclaiming comedy, humor, satire, and irony as quite fundamental aspects of human interactions and as deserving of a place within the rhetorical repertoire available for describing the encounters with

people that are always involved in ethnography and clinical work.

CONCLUSION

In May of 1993, right around the time that Becker's essay appeared, I witnessed a partial eclipse. It was a warm, sunny day in Chicago, where I was living at the time, the sky a brilliant deep blue. When the eclipse began, in the early afternoon, there was no dramatic blocking out of the sun; instead, the brightness of the day simply diminished a bit, as if a dial somewhere had been turned, and some of the brilliance drained out from all the world's colors. The change was so global and so subtle that I might not have noticed it—until I looked down, and saw that every single little splash of sunlight that filtered through the leaves of the trees above had taken, as it landed on the sidewalk, the shape of a crescent moon. A great event was taking place in the cosmos, but its only visible sign was this scattering of tiny, dancing arcs of light on the ground.

We might regard the disappearance of the bumbler and the crock as similarly giving evidence of an immense but hard-to-perceive change. I have suggested that this change involves a transformation, grounded in larger political-economic shifts, in the discourse and social organization of knowledge production and the education of particular kinds of experts. The value once attached to certain kinds of experience has given place to a regime of accountability.

Discourses, disciplines, and institutions, being human creations, are thankfully amenable to human intervention in ways that far-off celestial events are not. My hope in setting forth this analysis is that scholars, teachers, and practitioners of medicine, anthropology, and other human sciences might engage in thoughtful, reflective discussion about changes in our fields, their causes, and their consequences. I hope that such conversations might lead to the articulation of shared values and aspirations for the future and concerted action to try to bring that better future forth. I hope that such discussions might begin here, with what Becker would have called a "little ah-ha."

Ah-ha!

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NOTES

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provided the occasion for developing this piece. Warm thanks also to Editor Michael Chibnik and the anonymous reviewers.

1. As Claire Wendland (2010) has persuasively argued, cynical and derogatory comments about patients are not a universal phenomenon. The medical students she observed and interviewed in Malawi, striving to provide care under very difficult material circumstances, never used such language about patients. They tended instead to turn their anger and cynical humor against the political system.
2. This literature continues to thrive and grow, and much excellent work has come out in the past few years. Highlights include Prentice 2013, Wendland 2010, and the ethnographic essays collected in the 2011 special issue of the journal *Culture, Medicine and Psychiatry* (Holmes et al. 2011).
3. New work on this topic continues to appear. See especially Willen 2013 and other essays included in a special issue of the journal *Culture, Medicine and Psychiatry* (Willen and Carpenter-Song 2013).
4. Interestingly, even as it has fallen from favor within anthropology and medicine, experience as a basis for knowledge has simultaneously been valorized and institutionalized—and in the same movement, arguably, marginalized—with the establishment of ethnic studies and women's studies programs, as well as service-learning programs. My thanks to Priti Ramamurthy for pointing this out.
5. At my own university, state support has been cut in half since the beginning of the recession in 2008. Tuition costs borne by individual students and their families, while still low by comparison with private colleges and universities, have increased dramatically during the same years. (On the bright side, I will note that explaining to students what *neoliberalism* means has gotten a whole lot easier.)
6. In 2013, "budget sequestration" resulted in dramatic across-the-board reductions to all U.S. federal agencies that fund research, including the NIH (which supports most medical research) and the NSF and NEH (which support most social sciences). As of this writing in May 2014, it seems unlikely that the funding lost will be restored any time soon. The Great Recession has also impacted many nongovernmental foundations that support research through reduced endowments, as well as a drop in charitable donations.
7. My thanks to Claire Wendland for helping me to figure out this point.
8. For a collection of essays addressing changes and continuities in medical resident education by way of reflections on Shem's 1978 novel *The House of God*, and on the impact of the book itself, see Kohn and Donley 2008.

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