

RHETORICAL BOUNDARIES IN THE “NEW SCIENCE” OF ALTERNATIVE MEDICINE

by

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A THESIS SUBMITTED IN PARTIAL FULFILLMENT OF
THE REQUIREMENTS FOR THE DEGREE OF

DOCTOR OF PHILOSOPHY

in

THE FACULTY OF GRADUATE STUDIES

(English)

THE UNIVERSITY OF BRITISH COLUMBIA

(Vancouver)

October 2009

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ABSTRACT

This dissertation investigates scientific studies of complementary and alternative medicine (CAM) as episodes of scientific boundary work: these studies shift, and then seek to fix, the boundaries between what counts as proper medical science and what does not. Rhetoric scholars have mapped sites of boundary work both in science and in various CAM practices, but there is still some question of how biomedicine itself responds to challenges to its borders—and, by extension, challenges to its social and epistemic authority. This dissertation examines the rhetorical constituents of biomedical boundary work by analyzing a corpus of CAM-themed special issues of the journals of the American Medical Association from 1998, in which members of the medical profession consider the implications of including under biomedicine’s purview health practices formerly considered outside it.

The project examines this corpus, and responses published in both medical and popular outlets, to illuminate some of the ways in which members of a culturally dominant profession evaluate medical therapies in the face of disciplinary unrest, both within and beyond the borders of their profession. The chapters move from contexts internal to medicine to those external, mapping, sequentially, the historical-professional, epistemological, clinical, and popular dimensions of biomedical boundary work. The project aims to provide a more nuanced, stratified account of the rhetorical negotiation of medical and scientific boundaries. Its main claim is that, despite the willingness of many medical researchers and practitioners to elide distinctions between mainstream and alternative medicine, this research on CAM, and its related activities (i.e., publication, clinical practice), ultimately strengthen those distinctions and expand science’s authority in medicine.

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LIST OF ACRONYMS

AMA	American Medical Association
CAM	Complementary and alternative medicine
DSHEA	Dietary Supplement Health and Education Act
EBM	Evidence-based medicine
FDA	Food and Drug Administration
JAMA	Journal of the American Medical Association
NCCAM	National Center for Complementary and Alternative Medicine
NIH	National Institutes of Health
OAM	Office of Alternative Medicine
PCC	Patient-centred care
RCT	Randomized, controlled trial
SCI	Socially complex intervention
TCM	Traditional Chinese Medicine

ACKNOWLEDGEMENTS

I have had the good fortune over the course of writing this dissertation to have received care and attention and support from numerous colleagues, mentors, and friends, without whom I could not have completed it. This thesis was completed with financial support from the Social Sciences and Humanities Research Council of Canada, as well as several scholarships from my home department, funding for which I would like to express my gratitude.

I am grateful to members of the UBC English Department for giving me a generative environment in which to work, and to the participants at the conferences and workshops at which I have presented portions of this thesis for their feedback. Deborah Dysart-Gale, Lisa Keränen, Joan Leach, and Philippa Spoel, especially, have gone out of their way to help me find my feet, and I am delighted and lucky to know them.

My committee members have challenged and energized me throughout this project, and each has made a strong imprint on my scholarly development. I thank Leah Ceccarelli and Janet Giltrow for their careful reading of and commentary on my work. And Judy Segal, my supervisor, I really do not know how to thank for her careful attention, incisive critiques, mentoring, collaboration, and good company over these years. When I first met her, as an undergraduate in her History and Theory of Rhetoric class, I really thought I was going to be a Shakespearean. She taught me what rhetoric is, and what it can do.

Closer to home, my friends have been indispensable in the completion of this project, especially Kim Duff and Heather Latimer, and my parents, Dorothy and Jim Derkatch, have encouraged and supported me all along. Finally, I thank Isla Whitford, such a bright light and a lot of fun, and Nathan Whitford, who carried so much of the load, for so long, to give me space and time to work. All I can say to him, inadequately, is thanks—“just for.”

INTRODUCTION

Traversing Professional Boundaries in Rhetoric and Medicine

The study of rhetoric at the fringes of medicine is illuminating for both medicine and rhetoric. Rhetoric helps us to investigate fringe patients, fringe illnesses, fringe practitioners, and fringe health models—to study the means through which they fail, somehow, to fit within the accepted boundaries of mainstream scientific (or bio-) medicine. Such medical fringes, in turn, offer to rhetorical theory productive ways of tracing what sociologist Thomas Gieryn calls the “boundary work” of science: they focus attention on the shifting border between what is deemed legitimate within science, and what, not.

This dissertation investigates the rhetorical constituents of boundary work within biomedicine by examining a specific moment, in the late 1990s, when certain of the fringe health practices known collectively as Complementary and Alternative Medicine (CAM) were subjected for the first time to concerted, large-scale scientific scrutiny. I view this moment as a period of disciplinary anxiety, with practitioners both within biomedicine and beyond struggling to identify their positions vis-à-vis this awkward union of different health models, loosely categorized here as “mainstream” and “alternative.”¹ This study contributes to scholarship on medical and professional discourses and to humanities-based research on health, and adds greater nuance to our understanding of rhetoric as communication among individuals and groups with disparate backgrounds, motives, ideologies, and expertise. The present chapter introduces and contextualizes the project, describes my rhetorical methodology, and outlines the chapters that follow.

¹ I return to definitions of mainstream and alternative health below but will note here that the category of CAM unites a disparate group of practices (e.g., chiropractic, energy healing, herbal medicine, homeopathy, meditation, naturopathy, Traditional Chinese Medicine) under a single rubric—a rubric that depends essentially on a negative relationship to conventional medicine. While the category itself is problematic, its negative definition reveals how such practices are conceived, generally, in biomedicine: as *not-biomedicine*.

CAM in Biomedicine: Developing a Rhetorical Account

Contemporary Western medicine is shaped predominantly by the biomedical model, which medical anthropologist Howard Stein identifies as having its roots in the “basic sciences”: anatomy, biochemistry, microbiology, pathology, and physiology (xiv). As a descriptive term, *biomedicine* does not refer to a fully fixed set of medical values or practices, but certain overarching tenets characterize the ways in which medicine is conceptualized and taught in North America (and elsewhere). In addition to having its foundation in sciences such as anatomy and pathology, notes Stein, the biomedical model assumes the following: that medicine is, and ought to be, predicated on “rational, scientific, dispassionate, objective, professional judgment”; that the causes of disease are rooted in organic pathology, typically at the cellular level; and that disease is “optimally” treated by interventions resulting in a cure (xiv). Further, Stein observes, biomedicine is structured around “organ systems” (e.g., cardiology, endocrinology) that determine the shape of both medical teaching and medical practice. These defining qualities, particularly the emphasis on pathology, place medicine squarely within the province of science, although numerous critics (see, e.g., Murray; Peabody) have demonstrated how this essentially mechanistic approach occludes medicine’s humanistic core. CAM practices are not themselves based on the scientific model but their relationship to mainstream practice is nevertheless shaped largely by how they are configured in biomedicine.

Thomas Kuhn and Paul Feyerabend observed, separately, some fifty years ago, that definitions of science depend entirely on context—historical, philosophical, epistemological, professional; in rhetorical terms, they depend on the sophistic notion of *kairos*, the elements of time, place, and circumstance. Thomas Gieryn describes science in cartographic terms as a bounded cultural space, which is neither permanent nor rigidly defined, but which is nevertheless

carefully protected and patrolled. He argues that, “The epistemic authority of science is..., through repeated and endless edging and filling of its boundaries, sustained over lots of local situations and episodic moments, but ‘science’ never takes on exactly the same shape or contents from contest to contest” (*Cultural Boundaries* 14). This “edging and filling” of boundaries makes up much of what Kuhn calls “normal science,” including the day-to-day accumulation of facts and figures in labs, observatories, and the field.² All of the data that scientists produce are interpreted, sorted, and sifted, results are tabulated and deemed significant or not, and conclusions are devised; all of these are, *inter alia*, rhetorical processes.³

A central concept in both Kuhn’s and Feyerabend’s theories of scientific knowledge is incommensurability—the lack of a common measure or standard by which to judge competing accounts of nature.⁴ The question of how to test, through scientific methods, health practices not based on the scientific model is, in part, a question of commensurability. Traditional Chinese Medicine (TCM), for example, is founded on the principle of energy flow through channels that do not correspond to any physiological structure known to Western physicians. Given that biomedicine and TCM are premised on radically different models of bodies, illness, and health, biomedical researchers are faced with a central problem regarding how they ought to proceed with scientific studies of TCM. I argue in this dissertation that the means through which researchers solve such problems in CAM research are boundary-focused and largely rhetorical, centred on persuasion. The solutions researchers adopt persuade us in various directions—about

² The opposite of “normal” science, for Kuhn, is “revolutionary” science, the period during which one guiding theoretical paradigm triumphs over another, which then results in a new, subsequent period of “normal” science.

³ Charles Bazerman, for example, shows how the experimental article stands as a “vicarious surrogate for the actual experiment” that persuades readers of the occurrence of events from which they are removed by both time and space (*Shaping* 72). Similarly, Greg Myers argues that the processes through which scientific “texts produce scientific knowledge and reproduce the cultural authority of that knowledge” are deeply social and can be traced by studying documents such as draft and published versions of biology articles, grant proposals, and popularizations (*Writing Biology* ix).

⁴ Kuhn’s classic example is the lack of a common standard between the Ptolemaic universe and the Copernican; as long as one sees the universe as centred on Earth, a heliocentric world can never make sense.

biomedicine's scope and limits; about the status and legitimacy of CAM practices such as TCM; about what counts as a contribution to knowledge; and about how we understand our own illness and health.

Incommensurability has long captured the attention of historians and philosophers of science, but, while it is a partly rhetorical problem, rhetoricians have not so avidly taken it up—until recently. Taking I. A. Richards' definition of rhetoric as the “study of misunderstanding and its remedies” as a rallying cry, Randy Allen Harris assembled in a recent collection some of the most important current scholars in rhetoric of science to spin out the implications of incommensurability for rhetoric, and vice versa. Harris argues that, while incommensurability—“a phenomenon of misaligned meanings [in context]” (Introduction to *Rhetoric and Incommensurability* 59)—should be disabling for science, it is not; his contributors investigate why this may be so.

Several contributions to Harris' collection provide insight especially relevant to the study of the rhetoric of medical “fringes” because they trace incommensurability in science along synchronic, not diachronic, axes, examining conflict among contemporaneous paradigms rather than across successive ones. Carolyn Miller's study of the convergence of physical and biological sciences, for example, finds something like incommensurability (“Novelty and Heresy”), while Charles Bazerman and René Agustín De los Santos' study of toxicology and ecotoxicology does not. Leah Ceccarelli maps how the very notion of incommensurability can become entrenched as a model of scientific argument, wherein rhetors envision debate about scientific controversy as an agonistic struggle—a “zero-sum game” (“Science and Civil Debate” 274). While their conclusions differ, these chapters usefully track potential conceptual and communicative problems in boundary-crossing research, and the possibilities for their resolution.

Other rhetorical studies of science by Leah Ceccarelli (*Shaping Science*), Greg Myers (*Writing Biology*), and Charles Alan Taylor share an interest in tracking boundary work—how different scientific specialties interface with one another. Taylor writes most directly on the demarcation of science from nonscience, a matter that he describes as thoroughly rhetorical: “demarcations of science are accomplished routinely in everyday social and scientific practice...[S]uch demarcations proceed not from ontological foundations but from symbolic inducements. They are, then, rhetorical accomplishments” (15). Taylor surveys scholarship from history, philosophy, and sociology of science with the aim of developing a uniquely rhetorical account of demarcation. His two case studies, on creation science and cold fusion, importantly illustrate that the demarcation of what counts as science is primarily a “discursive accomplishment” emerging out of scientists’ everyday practices (222).

Mary Lay⁵ and Philippa Spoel investigate challenges to scientific boundaries specifically in biomedicine. They examine, separately, the incorporation of the once-fringe practice of midwifery into North American health care systems. Both authors expand studies of cross-disciplinary projects in science because they describe the means through which practitioners of ostensibly nonscientific health practices negotiate their interactions with biomedicine. Lay illustrates, for example, how, in Minnesota in the mid-1990s, direct-entry midwives (lay-educated; contrasted with nurse-midwives) used licensing genres to assert jurisdictional claims over their area of practice—pregnancy and childbirth—and how those efforts nevertheless failed in the face of the greater institutional and professional resources of the biomedical community. Spoel has examined the rhetorical construction of midwifery as a profession (“Communicating Values,” “Midwifery”; Spoel and James), focussing most recently on the websites of two midwifery organizations in Ontario, Canada. She reports that, despite the websites’ description

⁵ See also publications under the name Mary Lay Schuster.

of midwifery as a communally-oriented, woman-centred approach to pregnancy and childbirth, the websites ultimately adopt a professional ethos that “replicates more than it resists mainstream assumptions about the (noncommunal) identities and relationships of health-care professionals and health-care publics” (“Communicating Values” 285). Lay’s and Spoel’s studies complement Judy Segal’s research (*Health*; “Illness as Argumentation”), which has, similarly, investigated the rhetorical life of fringe patients and fringe illnesses in biomedicine, such as patients with nonspecific symptoms and symptoms without immediately recognizable biological causes (e.g., achiness, fatigue). These three scholars open up space for the study of the rhetoric of medical fringes and, crucially, invite further research.

Lay’s and Spoel’s research on midwifery focuses on reproduction, itself an important area of study in the rhetoric of medical boundary work, since medicine is conventionally understood as the treatment of illness, a category into which childbearing does not fit. However, their focus on midwifery leaves open the question of how *primary* CAM modalities, such as TCM and chiropractic, which advocate the wholesale replacement of biomedical theory and practice with an alternate system, come into contact with biomedicine. What sorts of “misaligned meanings,” in Harris’ formulation, arise at such points-of-contact? How, for instance, do essentially competing approaches to health, based on apparently incommensurable understandings of bodies, illness, and health, find any common ground? And while Lay, Segal, and Spoel all provide accounts of how fringe patients/illnesses/practices enter into those encounters with biomedicine, there is still some question of how biomedicine responds to challenges to its borders—and, by extension, to challenges to its social and epistemic authority. This process of coping with external challenges, I maintain, constitutes a particularly intense

episode of “edging and filling” boundaries, and is a useful place to tease out some of the rhetorical constituents of boundary work within medicine.

Mapping a Rhetorical “Moment” in Biomedical Boundary Work

In North America, non-mainstream health practices such as TCM, chiropractic, and dietary supplements (e.g., herbal remedies and megavitamins) have conventionally been understood in opposition to—and often as incompatible with—biomedicine. Part of the allure of such practices, to many, is precisely their status as approaches “alternative” to biomedicine, particularly for those with chronic or ambiguous conditions, or for those who seek care in what they believe to be a more humanized, holistic approach to health. While uses of the term vary markedly, Eisenberg and colleagues’ definition from their seminal 1993 survey of patterns of use and expenditure on CAM in the US became fairly standard in the 1990s and early 2000s. This definition deems “alternative” those “medical interventions not taught widely at US medical schools or generally available at US hospitals” (“Unconventional” 246). This blanket definition encompasses interventions ranging from self-administered practices (such as meditation and prayer) to fully institutionalized and accredited health systems whose models of practice stand explicitly in opposition to biomedicine (such as TCM and chiropractic).⁶

As historians of medicine W. F. Bynum and Roy Porter and others (Saks, Starr, and Whorton among them) have shown, the definitions of mainstream and alternative medicine over history have been fluid, and the distinctions between them, socially negotiated. Accordingly, the division between such designations is, in part, rhetorical because the preservation or destruction of the categories themselves must be effected through persuasive means. Those means have

⁶ “Complementary” was added to “alternative medicine” during the mid-1990s. *Complementary* health practices are interventions used in conjunction with biomedical treatments, while *alternative* practices are interventions used in place of biomedical treatments (National Center). I return to questions of definition in Chapter One.

shifted over time, as the ground upon which the health practices excluded from mainstream medicine have likewise shifted. Roy Porter argues, for instance, that the “quacks” of the eighteenth-century, which he glosses as “fakers and charlatans,” operated parasitically on the mainstream medical marketplace; claims to territory were made and defended primarily within the terms of the marketplace itself. In the nineteenth century, practices such as homeopathy, naturopathy, Thomsonianism (“medical botany”), and mesmerism set themselves up in explicit opposition to mainstream medicine. Porter writes, “if this [new movement] was quackery, it was quackery with a difference,” as the tone of the debate about medical boundaries shifted from matters of the marketplace to those of “ideology, philosophy, and morality” (204). These new, organized alternative medical movements necessitated the medical profession’s engagement of more strategic and concerted efforts to preserve its professional and epistemic territory.

Sociologist Ayo Wahlberg extends Porter’s analysis to the late twentieth century, during which we began “once again witnessing the emergence of a quackery with a difference” with the rise of CAM as descriptive category (2315). This new phase in a long history of boundary-negotiation has been accompanied, in biomedicine, by a new set of strategies to preserve its boundaries. Wahlberg argues: “Rather than ban or restrict access to CAM practitioners (as happened in the UK and many other countries in the early 20th century), the aim of contemporary efforts to regulate CAM has been recast into what might be termed a normalization of its practice and use” (2315). Part of that normalization is the large-scale scientific testing of CAM: as CAM practices have been increasingly brought under biomedical scrutiny, the territories of mainstream and alternative practices started to converge. However, this convergence has been uneven, shaped by the greater rhetorical resources of biomedicine as the dominant model, and complicated by the emerging authority in the late twentieth century of sick persons not so much

as (passive) *patients*, but as (active) *clients*—or health care *consumers*. The testing of CAM through biomedical methods is embedded in a matrix of competing tensions, which I explore over the course of this dissertation.

In January 1998, Phil Fontanarosa and George Lundberg, editors of the *Journal of the American Medical Association (JAMA)* and its nine associated *Archives* specialty journals, issued a call for papers for concurrent theme issues on scientific research on CAM. At the time, the AMA had ranked CAM among the top three subjects, out of eighty-six, for the journals to address that year, a sharp increase from the previous year, when CAM was ranked sixty-eighth of a possible seventy-three (“Call for Papers” 2111). This spiked interest coincided with the foundation of a new research and funding body under the US National Institutes of Health (NIH). In October 1998, the NIH transformed its Office of Alternative Medicine, established in 1992 with a \$2 million annual budget, into the National Center for Complementary and Alternative Medicine (NCCAM), with a starting annual budget of \$50 million. NCCAM’s purpose was (and still is) to foster scientific scrutiny of CAM practices and collaboration with CAM practitioners; the concurrent AMA theme issues represent a first step in that direction.

Although the AMA coordinates themed issues every year, the 1998 installment (published that November) was unique, by the editors’ own account, because it offered researchers and readers a “multidisciplinary forum”: a venue for publishing studies on unconventional health practices in a space typically reserved for conventional biomedicine (Fontanarosa and Lundberg, “Call for Papers” 2111). Not all contributors to the journals favoured such a multidisciplinary approach, however. One professor at Harvard Medical School, for instance, equated research on CAM to the “scientific study of astrology,” maintaining in the *JAMA* theme issue that “we are in the midst of a fad that will pass” (Delbanco 1561). A number

of other contributors worried that biomedicine had succumbed to external pressures—pressures to which many practitioners had believed medicine immune. But it was not immune: a landmark study in 1993 had signalled a shift in the landscape of American medicine that, in turn, motivated the flurry of CAM research and the publication of the November 1998 theme issues.

In that 1993 study, Eisenberg et al., the authors of the landmark definition of alternative medicine offered above, stunned the medical community by revealing that, in 1990, the number of visits that Americans made to alternative health practitioners exceeded that of all those made to conventional primary doctors in that same year (approximately 425 million visits and 388 million visits respectively).⁷ Prior to the 1993 study, the medical profession was generally ambivalent about CAM: individually, practitioners were often concerned only insofar as CAM interfered with their daily practice of medicine, while collectively, members of the profession had long engaged in efforts to protect themselves against competition (Baer; Whorton, *Nature Cures*). Those efforts were generally aimed at limiting, through regulatory or legislative channels, the ability of alternative health practitioners to ply their trades. However, the levels of use identified by Eisenberg et al. were too great for the medical profession to continue to ignore CAM as worthy of serious, sustained scientific inquiry, and so began the concentrated push for clinical trials of CAM in the early- and mid-1990s.

From the perspective of the AMA journals' editors, the *JAMA-Archives* special issues are special because, for a brief time, the closely monitored territory of mainstream scientific medicine was opened up to practices normally beyond the scope of such journals. Articles on individual CAM practices had, in fact, long been published, sporadically, in mainstream medical journals. In *JAMA* alone, for example, an article calling for clinical trials of acupuncture had

⁷ Mainstream medicine was not losing patients, exactly: the Eisenberg study also showed that patients did not stop seeing regular physicians altogether, but tended, rather, to see alternative practitioners alongside them.

been published some twenty-five years earlier (Adler); this article led to several years of debate within the journal about the methodological and professional repercussions of such trials. What distinguishes the *JAMA-Archives* coordinated theme issues from such previous efforts to delineate the boundaries of legitimate health care is the journals' deliberate orchestration as an intensive (and public) meditation on CAM across a professional organization's network of texts. Their publication, I suggest, constitutes an important rhetorical *moment* (see Bazerman, "Nuclear Information") in the production and maintenance of biomedical boundaries.

The gravity of this moment registered both within biomedicine and outside it, leading to sustained debates about the theme issues in the medical literature and to enthusiastic coverage of the journals in the popular media.⁸ In 2000, the AMA itself commemorated the theme issues with the publication of *Alternative Medicine: An Objective Assessment*, an edited collection of the articles published in the original 1998 corpus (Fontanarosa). In his preface, AMA President Thomas Reardon notes the significance of the *JAMA-Archives*' efforts: "This [collection] is a milestone. The authors and editors delineate where the science begins and ends as of today, outlining where further study is needed" (v).

In what follows, I examine the coordinated *JAMA-Archives* theme issues to provide a rhetorical account of how a dominant system of thought copes with externally motivated challenges to its authority. How, for instance, do members of the medical community respond discursively to pressure (even internal pressure) to integrate complementary and alternative practices into their own system of thought? Does that pressure, in turn, influence how biomedicine itself functions? For example, historian of medicine James Whorton cites the

⁸ In *Newsweek*, for example, Cowley and Underwood report that, "If some of [the money spent in the US on alternative medicine in 1997] was wasted, some of it now appears very well spent. This month the American Medical Association stocked all 10 of its journals—including its flagship, JAMA—with articles on alternative remedies. And though several techniques withered under scientific scrutiny, others emerged looking better than mainstream treatments" (68).

“biomedical reductionism” of conventional medicine (i.e., the reduction of persons to diseased bodies) as a major factor in the movement of patients toward CAM. Does that movement, in turn, motivate doctors to transform their own practice to accommodate patients that prefer care that is more attentive to their experiences of illness? The conceptual, ethical, professional, and practical problems that arise in the scientific testing of CAM are, as I have suggested, problems of commensurability—the commensurability of theories, procedures, and evidence.

My main claim in this project is that, despite the willingness of many medical researchers and practitioners to elide distinctions between mainstream and alternative medicine, this research on CAM, and its related activities (i.e., publication, clinical practice), ultimately strengthen those distinctions and expand science’s authority in medicine. The corpus includes all ten AMA-associated journals published on CAM in November 1998 (described in detail in Chapter One); it includes approximately eighty articles, including research reports, letters, and editorials. The contributors are largely biomedically trained and the topics covered range from the plausible within the context of biomedical theory (e.g., St. John’s Wort for depression) to the implausible (e.g., homeopathy).⁹

Rhetorical Framework and Methodology

Professional borders are pertinent not only to medicine but to rhetoric as well. Rhetoric, perhaps more than any other discipline, has struggled since its inception with its identity *as* a discipline. As I write, debates circulate among those have made the study of rhetoric their profession—debates about “the viability of the rhetorical tradition” (e.g., Graff, Walzer, and Atwill’s edited collection of that name); about “sizing up rhetoric” as a field of inquiry (e.g., Zarefsky and

⁹ Homeopathy is founded on the principle of like-cures-like: substances that would induce certain symptoms in a well person (e.g., fever) are believed to eliminate such symptoms in someone suffering from them; these substances are so heavily diluted that it is arguable whether remedies contain even a single molecule of the original substance.

Benacka's volume, based on the 2006 Rhetoric Society of America conference on the same theme); and about "the health of the discipline" (e.g., the National Communication Association's 2005 conference theme¹⁰). These current debates about the state of rhetoric (as a tradition, a discipline, a mode of thinking) can be traced back at least to Aristotle's observation more than two thousand years ago that rhetoric "has no special application to any distinct class of subjects" (8). When critics talk about rhetoric, they most often talk about the rhetoric *of* things—the rhetoric of religion (e.g., Burke), for example, or the rhetoric of fiction (Booth), of midwifery (Lay), or of science (Gross, *The Rhetoric of Science*; Prelli). This prepositional relationship between rhetoric and its various objects of study is one reason critics throughout rhetorical history have asked a very important question, articulated here by Judy Segal: "just what is rhetorical criticism?" (*Health* 7).

Writing on rhetoric of health and medicine, Segal characterizes rhetoric as a practice guided by a "rhetorical subjectivity," a subjectivity nurtured through the study of texts within the "scholarly tradition on public discourse" (*Health* 7). Randy Allen Harris, writing on rhetoric of science, similarly frames rhetoric in terms of *allegiance*: "rhetoric of science is the analysis of scientific discourse by scholars whose primary allegiances are to the guiding notions of rhetorical theory, and who place their work in the tradition of others with those allegiances, some of whom invented those allegiances" (Introduction to *Landmark Essays* xxviii). The tradition that both Segal and Harris point to is long and richly varied, and, Harris argues, "each scholar will have a somewhat personal constellation of allegiances" (xxvii). As a result of those varied allegiances, rhetoricians often hold differently inflected definitions of *rhetoric*, and collecting others' definitions seems to have become an unofficial pastime of many who currently teach and

¹⁰ NCA is the umbrella group of numerous national rhetoric organizations, including the Association for the Rhetoric of Science and Technology, the American Society for the History of Rhetoric, the Kenneth Burke Society, and the Rhetorical and Communication Studies Division.

write on the topic.¹¹ The general consensus is that rhetoric is trained on human communication and offers a program for both the production and reception of discourse. Beyond this basic definition, views branch off considerably, each with different emphases—some focussing, for example, on rhetoric’s actional properties (e.g., Burke), others on its ethical obligations (e.g., Weaver, “Language is Sermonic”), and still others on its epistemic functions (e.g., R. L. Scott).

Problems of disciplinary identity are magnified, to some extent, in rhetoric of health and medicine (though, of course, not *only* in rhetoric of health and medicine). A vibrant and growing but diverse field in rhetorical studies, rhetoric of health and medicine developed, partially, out of rhetoric of science. It is recently coming into its own, with an increasing presence in graduate programs and academic journals and conferences; the publication of several major, field-mapping monographs and collections (e.g., Segal, *Health*; Heifferon and Brown, eds.; Leach and Dysart-Gale, eds.); and other generative scholarly activities (such as the “medical rhetoric” workshops led by Ellen Barton and Susan Wells at the 2007 and 2009 Rhetoric Society of America Summer Institutes).¹² Work in this field overlaps with diverse other fields, including other areas of language study such as composition studies, health communication, linguistics/pragmatics, and professional and technical communication. Often, distinctions among

¹¹ An online search at Google.com for websites containing the phrase “definition(s) of rhetoric” returned more than 10 000 results. The majority of these pages are websites for undergraduate rhetoric courses, which contain lists of definitions by major figures in the history of rhetoric. For example, the first entry among pages of search results is Andrea Lunsford’s “resources” page of her teaching website, which offers thirty-three definitions by well-known rhetoricians, all of which are unique in their various emphases. The official t-shirt of the 2008 RSA conference had thirteen definitions printed across the back, from Aristotle to “Dad,” who asks of his child’s college major, “rhetoric? doesn’t that just mean bullsh**?” (asterisks in original). Collecting definitions of rhetoric is not an entirely new habit, either: in 95 CE, for example, Quintilian devoted the entire fifteenth chapter of Book II of his *Institutes of Oratory* to summarizing and evaluating the definitions of various figures before him.

¹² Articles on rhetoric of health and medicine can be found in rhetoric journals, such as *Rhetoric Review*, *Rhetoric Society Quarterly*, and *Quarterly Journal of Speech*, as well as interdisciplinary journals such as *Journal of Medical Humanities and Social Science and Medicine*, and beyond. The Conference on College Composition and Communication (CCCC) has a “Medical Rhetoric” Special Interest Group and the Association for Rhetoric of Science and Technology (ARST), which meets at NCA, is likewise receptive to health and medical topics. RSA has recently seen a strong showing in research in the area, as have conferences further afield, such as the American Society for Bioethics and Humanities and the Society for Social Studies of Science. See Segal for further reflection on such disciplinary groupings (“Interdisciplinarity” 312-15; “Rhetoric of Health and Medicine” 237-39).

the various language-based approaches can be difficult to make, as Judy Segal has argued in her helpful survey of rhetoric of health and medicine's variety ("Rhetoric of Health and Medicine"). Scholarship in the field also borrows from, contributes to, and overlaps with other disciplinary studies of health and medicine in, for example, anthropology, history, philosophy, and sociology, as well as research in interdisciplinary areas such as bioethics, health/medical humanities, and science studies. Segal argues that rhetoric of health and medicine "defies disciplinary boundaries; at least, it blurs them" (236). She points out, however, that, as interdisciplinary programs in, for example, women's studies and science and technology studies have taken root, "researchers in general are indentifying less with traditional disciplines and more with fields of inquiry and transdisciplinary methodologies" (236); rhetoric of health and medicine is well-suited to this changing research climate.

These problems of disciplinary identity are generative, insofar as they invite rhetoricians to consider the scope and implications of their analytic and professional stance, but there is no pressing need to solve them. Indeed, numerous scholars have suggested that rhetorical criticism and theory are all the richer for not being able to answer those questions definitively. Writing on rhetoric, generally, Steven Mailloux, for example, cites the field's disciplinary elasticity as an asset rather than a liability:

We have an interdiscipline that is often institutionally situated as a subfield (or sometimes simply a method) within various disciplinary departments in colleges and universities. I believe we should continue exploiting this double placement as subdiscipline and interdiscipline: We should work within our different departmental homes to develop rhetoric as historical tradition, theoretical perspective, and critical practice in the courses we teach and the scholarship we publish. In addition, we should be

especially attentive to local opportunities for developing rhetoric as an interdisciplinary field, a field that combines different disciplinary methods and objects of study, a field that provides a transdisciplinary perspective on how disciplines do their business, a field that both explains and challenges traditional disciplinary boundaries. (“One Size Doesn’t Fit All” 8-9; see also “Using Traditions”)

Rhetoric’s identity as a sub-/inter-/trans-/post-discipline vitally enables studies of the rhetoric of science, medicine, and health, as both Harris (Introduction to *Landmark Essays*) and Segal (*Health*; “Interdisciplinarity”; “Rhetoric of Health and Medicine”) argue.¹³

Methodology is a complex issue in rhetoric because discourse depends on the conditions of its production, so no single analytic method will work in all circumstances. While some scholars have attempted to systematize rhetoric into defined critical categories or theoretical frameworks (e.g., Brock, Scott, and Chesebro, eds.; Foss, *Rhetorical Criticism*), a productive way of conceiving rhetoric might be as a flexible, responsive instrument calibrated to its specific task. Michael Leff praises a rhetorical method that “demands rigor from the critic even as it allows him or her the freedom to adapt to the changing demands of particular situations” (349). And Kenneth Burke, quintessentially, argued, “The main ideal of criticism, as I conceive it, is to use all that is there to use” to engage in literary and textual analysis (*Philosophy* 23). To this end, in the following chapters, I follow Segal’s definition of rhetoric as a critical practice guided by a “rhetorical subjectivity,” and Harris’ related definition of rhetoric as determined by its theoretical “allegiances.” While these definitions may appear somewhat circular, Segal points out that such a view is not: works within the 2500-year-old rhetorical canon “[suggest] lines of inquiry and a procedure for thinking” about human communication, methods that can be combined differently to examine the various persuasive features of texts or discourses (*Health* 21).

¹³ The prefixes sub- and inter- are from Mailloux; trans- and post- are from Segal (“Interdisciplinarity”).

As with Segal and others who identify their work under the rubric of rhetoric of health and medicine (e.g., J. B. Scott), I employ Classical and New Rhetorical principles in my analysis and range across scholarship in related fields (history, philosophy, sociology, and medicine among them), and I incorporate other methods of language/discourse study in my work (e.g., discourse-based interviews, pragmatics). Framing this methodological plurality as itself a boundary issue, I characterize my approach, following Ellen Barton (who in turn cites Julie Klein), as a kind of “disciplined interdisciplinarity” (313), a phrase that resonates strongly both in my own rhetorical praxis and in the area of medical research I study. I expand on my methods in the following description of chapters.

Overview of Chapters

Each of the following chapters approaches the *JAMA-Archives* corpus through one of four different filters or “terministic screens,” in Kenneth Burke’s framework (*Language*): the historical-professional, the epistemological, the clinical, and the public. The chapters move from professional (i.e., internal) concerns to public (external) concerns about biomedical research on CAM. The first two chapters examine what Bruno Latour has called the “upstream” activities of scientists, while the remaining two chapters heed Gieryn’s call for studies of science to move “downstream” into the public realm, where the borders of biomedicine are all the more apparent in their juxtaposition with everyday human life.

Chapter One, “Patrolling Professional Borders,” examines how the contributors to the *JAMA-Archives* corpus situate their texts, on subjects not typically under the purview of such journals, in relation to the historical-professional dynamics that have shaped biomedicine. I argue that, although the corpus generally frames the tension between mainstream and alternative

medicine as a tension between science and nonscience, the tension is equally one between dominant and marginal systems of knowledge and practice (see, e.g., Saks; Starr). I examine the corpus as a textual artefact within the context of professional discourse (e.g., Abbott; Bazerman and Paradis, eds.) and the rise of quantitative measures in biomedicine, particularly in evidence-based medicine (EBM), to show how the corpus reframes both biomedicine and CAM in terms that preserve biomedicine's historical coherence as a firmly defined discipline while simultaneously stretching its limits to reach practices formerly beyond its scope. This process serves inherently epideictic functions, reinforcing conventional community values and the perceived borders separating health practices, even while some community members seek ostensibly to eradicate those borders.

My second chapter, "Scientific Method as a Rhetorical Topos," isolates methodology as the key topos, or line of argument, in the alignment of CAM practices in relation to scientific borders. At the core of these journals is a debate about how research on CAM ought to be conducted, interpreted, and incorporated into practice because biomedicine's "gold standard" methodology, the randomized controlled trial (RCT), does not easily accommodate interventions such as acupuncture. The chapter examines how CAM practices fit awkwardly within the RCT format and, turning to studies of the rhetoric of experimental articles in both science and medicine (e.g., Bazerman; Berkenkotter and Huckin; Swales), it investigates how the experimental genre is mobilized in the specific case of biomedical CAM research. It then isolates the concept of *efficacy*—whether or not a health intervention "works"—as a central organizing principle of biomedical research on CAM, and one that can be strategically invoked to draw particular epistemic and professional boundary lines. My analysis in this chapter is augmented by discourse-based interviews with expert readers (research methodologists, TCM practitioners, a

physician-clinician). As a whole, Chapter Two argues that the problem of method in biomedical CAM research is in great part a problem of persuasion.

Central to the question of methodology in CAM research is the practitioner-patient relationship, the most unambiguously rhetorical element of clinical medicine. Increased interaction between practitioners and patients in any medical model may have unintended—and unquantifiable—therapeutic effects. Chapter Three, “CAM Research and the Rhetorical Conditions Governing Clinical Practice,” examines how the articles of the *JAMA-Archives* corpus configure practitioner-patient interaction, particularly in relation to prevalent models of medical practice. I argue that practitioner-patient interaction is postulated as a contaminant in trials of acupuncture and chiropractic, and I develop that claim by bridging Kenneth Burke’s concept of terministic screens and Arthur Kleinman’s notion of explanatory frameworks. In addition, I propose that attempts to control for placebo effects in CAM research are, in many cases, attempts to control for interaction effects; I suggest that probing these effects can further contribute to new understandings of how practitioner-patient interaction can influence health outcomes. Finally, I examine the idea of patient autonomy, central to discourses on CAM and closely linked to interaction. I argue that the actual extent of autonomy afforded to patients in medical settings, alternative or not, is often illusory, framed within generic and rhetorical processes that necessarily tilt the course of decision-making in particular, and predictably biomedical, directions.

Chapter Four, “Defining Professional Borders in Popular Media,” shifts further downstream into the public realm, to examine a special report in *Newsweek* magazine on the “new science of alternative medicine.” Building on Judy Segal’s recent work in several areas of popular health discourse (“Breast Cancer”; “Female”; “Internet Health”), I articulate a rhetoric of

popular medicine vis-à-vis theoretical models of popular science developed in rhetoric (e.g., Fahnestock, “Accommodating,” “Preserving”; Ceccarelli, “Mixed Metaphors”; Paul), discourse studies (e.g., Calsamiglia; Myers, “Scientific Popularization”), and social studies of science (e.g., Hilgartner; Weigold). I argue that health reporting is both typical of and exceptional in science reporting: it is typical because medicine’s research values, generic forms, and institutional structures are closely aligned with those of science, yet it is exceptional because members of the public are significantly more invested as a rhetorical audience of medical reporting, due both to their own bodily experience and expertise, and to their need for health information. I suggest, further, that CAM research, in particular, demonstrates the bidirectional nature of science reporting, in contrast to the unidirectional model proposed by Jeanne Fahnestock: the major push for CAM research was motivated by the public’s overwhelming interest in and use of CAM therapies. In the texts I study in this chapter, the products of that research are returned to the public that motivated it.

My methods of analysis are rooted, primarily, in what J. Blake Scott calls “a rhetorical-cultural approach.” Scott argues that such an approach involves “accounting for science’s broader conditions of possibility, mapping the shifting intertext of science in action, evaluating science according to its effects, and targeting opportunities to intervene in harmful effects” (21). For instance, my first chapter contextualizes the project within work on history of medicine, history and philosophy of science, and professional discourse to trace the ways in which the corpus authors marshal historical-professional relations among different health practices. Other chapters employ Classical and New Rhetorical principles more directly. The third chapter examines the intersection of expert-lay discourses in health and medicine as “terministic screens” (see Burke), and the fourth refines and expands current rhetorical theory on popular science.

Following Segal (*Health*), however, I aim, as a critic on health and medicine, not to intervene through *prescription* but through *description*: this project opens up for inquiry the means through which biomedical boundaries are effected through persuasion and are themselves persuasive; I do not, in my analysis, endeavour to comment on whether or how those boundaries ought to be.

My methods are also informed by Ceccarelli's model of "close textual-intertextual analysis" (*Shaping Science* 8), the testing of hypotheses about a text's rhetorical effects by examining responses to that text in subsequent discursive activities; and by both Dascal and Gross' and Stillar's integrative models of textual analysis, which connect pragmatic and rhetorical methods (and, in Stillar's case, social theory) into a synthetic approach. For example, the second and fourth chapters incorporate elements of pragmatics to enable thick description of textual features, while the second chapter also uses genre theory to access the persuasive functions of the clinical trial report as a recurrent, community-defining discourse type. The second chapter also takes seriously the charge that rhetoric of science needs to "move beyond the moment" of authorial intention and textual production (to rephrase Paul, Charney, and Kendall) toward studies of reception within the scientific community.¹⁴ To that end, I balance my analysis with both published responses to the *JAMA-Archives* theme issues and my own discourse-based interviews to get a sense of how different specialist readers approach such texts. These diverse methods enable me to examine a discrete discursive moment from multiple perspectives, at multiple levels of analysis.

This dissertation moves in an arc that begins with internal boundary-defining documents in medicine—in this case, the letters, editorials, and research reports published in *JAMA* and the *Archives*—and ends with how such boundary work ultimately informs rhetorical interaction

¹⁴ Like Harris ("Reception Studies") and Ceccarelli ("Hard Look"; *Shaping Science*), I do not approach this claim uncritically, however: rhetoric of science has always had an interest in reception. The question that remains, though, is how to corroborate such interests empirically; I explore this issue further in Chapter Two.

among health professions, professionals, and the public—although, as I suggest in Chapter Four, this process is at least partly bidirectional. The study of the discursive boundaries between mainstream and alternative medicine explores, simultaneously, a whole host of related boundaries, all of which can be illuminated by rhetorical study. These illuminated discursive boundaries return something to rhetoric as a whole: they offer a rhetorical account of how science and medicine, protectors of what Randy Allen Harris calls “the most robust knowledge of our culture” (Introduction to *Landmark Essays* xxix), shape—and are shaped by—the particularized experience of everyday human life.

CHAPTER ONE

Patrolling Professional Borders

One of the most generative premises of science studies over the past four decades, particularly in rhetoric of science, has been that science both produces and is produced by communities of scientists. The idea of the “lone genius” pervasive in popular accounts of science (see Charney) has been dispelled by scholars investigating the social dimensions of scientific activity, from upstream laboratory work and scientific publication (e.g., Latour; Myers, *Writing Biology*) to downstream policy and popular contexts (e.g., Charney, “Lone Geniuses”; Gieryn, *Cultural Boundaries*). Kuhn forcefully articulated the foundational role of “community” in science in *The Structure of Scientific Revolutions*. Pivotal to his theory of scientific change are scientists’ community-based “sets of commitments” (6), “consensus” (11; 161), “agreement” (32), “nest[s] of commitments” (41), “professional initiation” (47), “expectations” (59), and “allegiance” to community-based paradigms (151). Rhetoric is central to Kuhn’s model: members of scientific communities can be “persuaded to change their minds” (152); they are subject to “transformations of vision” (111), “conversion” (204), and shifts in their “professional allegiances” (158)—all of which are the product of community activity. So effective are scientific communities’ efforts at consensual knowledge-making that those community efforts are virtually invisible within the “robust” knowledge they produce (Harris, Introduction to *Landmark Essays* xxix). But it is only on the surface that scientific knowledge is independent of those that produce it.

In most of the biomedical literature, the key point on which debates about complementary and alternative medicine turn is whether CAM is, or even can be, scientific—that is, whether it does or can operate in accordance with the basic tenets of biomedicine to produce valid,

objective medical knowledge. (The next chapter takes this question up in detail.) Yet the historical narratives of CAM supplied by medical anthropologists, historians, and sociologists (e.g., Baer, R. Porter, and Saks respectively), tell a different story, one that places CAM on the fringes of North American health care not primarily because of its lack of scientific rigour but because of its inability to compete professionally with organized medicine. Sociologist Paul Starr advocated this then-provocative view in his landmark 1982 book *The Social Transformation of American Medicine*, in which he observes that, although the development of medicine as a sovereign profession was undergirded by a “dream of reason,” “medicine is also, unmistakably, a world of power where some are more likely to receive the rewards of reason than are others” (3-4). The rise of professional medicine, Starr argues, created a climate in which mainstream physicians were able to secure the authority and market share necessary to gain ascendancy over other groups of health care providers. Other significant factors contribute to the power of medicine as an institution, as sociologists have recently illustrated with the idea of medicalization, the process through which elements of everyday human life are reframed as treatable medical disorders, such as balding, erectile problems, and aging (see Conrad). These medicalizing processes depend, crucially, on the reinforcement and even expansion of medicine’s cultural authority, which is effected in large part through the activities of the medical community as a professional class.

This chapter examines how the contributors to the *JAMA-Archives* theme issues on CAM situate their texts, on subjects not typically under the purview of such journals, in relation to the historical-professional dynamics that have shaped biomedicine. I argue, following Starr, Mike Saks, and others, that the tension between mainstream and alternative medicine cannot be considered simply as the demarcation of those practices as scientific versus non-scientific, as it is

generally framed in the *JAMA-Archives* corpus, but perhaps more importantly, as dominant versus marginal systems of knowledge and practice. Regardless of their own orientation toward CAM, contributors to the corpus generally approach the episode of boundary work precipitated by the *JAMA-Archives* corpus in isolation, independent of their profession's long history of competition with outlying practices and practitioners.

Charles Bazerman and James Paradis argue that examining the bidirectional relationship between a profession and its texts can illuminate the roles those professions play in the drama of social life: “Out of provisional clusterings of people, activities, and language emerge highly organized professions of great social consequence. Once established, professions maintain their organization, power, and activity in large part through networks of texts. As these professions increasingly form the framework of modern existence, their texts set the terms of our lives” (4). My fundamental claim in this chapter is that the *JAMA-Archives* theme issues on CAM, as a set of texts curated by the publication arm of a professional medical association, preserve biomedicine's historical coherence as a firmly defined discipline while simultaneously stretching its limits to reach practices formerly beyond its scope. This process serves inherently epideictic functions, reinforcing biomedical community values and the apparent boundaries separating health professions, even while some community members seek ostensibly to eradicate them.

I begin, in the first section, by examining the *JAMA-Archives* corpus in the context of professional discourse as constitutive of the professions that engage in it. I suggest that in responding to the journals' call for papers, respondents identify themselves as members of a community that, in an important sense, came into being only with the publication of the journals themselves. In that section, I offer a detailed description of the corpus as a whole, including its range of articles, authors, and subjects. The second section takes up the various definitions of

CAM offered by contributors to the *JAMA-Archives* theme issues, as well as the histories they give of individual CAM practices. Several commentators have argued that, in biomedicine, CAM is generally defined in the negative, as a collection of disparate health practices assembled under a single category simply by virtue of counting as “not-biomedicine.” I argue that, in the context of boundary work, this negative definition is persuasive because the various modalities that CAM comprises are emptied of their own significance and redefined in biomedical terms. By redefining CAM in a biomedical framework, the medical profession can also redefine the criteria for determining whether or not a given intervention counts as legitimate. The final section examines recourse to the “evidence” in debates about CAM, in the context of evidence-based medicine, as a strategy of exclusion adopted, in part, to serve the interests of the medical profession.

The *JAMA-Archives* theme issues focus on contemporary biomedical research and debates but how they configure historical patterns of professional competition among different health care modalities is central to the boundary-work performed within them. Gieryn explicitly identifies his motivation behind *Cultural Boundaries of Science* as “to make the science wars historically mundane by showing that they are of a piece with [previous] episodes of cultural cartography [of science]” (337). In this chapter, I contend that the current debate about alternative medicine is likewise “of a piece” with previous moments of boundary work in the history of medicine—that it is, in effect, “mundane.” In the context of the sections that follow, this claim is significant because one of the reasons that biomedical boundary work is so effective at preserving biomedicine’s cultural and epistemic authority is that it reifies the boundaries it produces. In the *JAMA-Archives*, the contributors’ explicit and self-conscious efforts at defining

their profession's borders are seamless, almost invisible. Instead of foregrounding the fluidity of professional boundaries, they naturalize them, rendering them *more* stable, not less.

The *JAMA-Archives* Corpus as Constitutive Rhetoric

When the editors of *JAMA* and the *Archives* issued their December 1997 call for papers for the theme issues (Fontanarosa and Lundberg, "Call for Papers"), they called forth a *community* of scholars, one that had ostensibly not existed prior to the call itself. In this section, I argue that the *JAMA-Archives* corpus addresses a biomedical community that already encompasses research on CAM under its official purview. I approach the corpus as constitutive rhetoric, which Maurice Charland defines as a rhetoric in which subjects are constituted by the very discourses with which they are addressed.¹⁵ Charland's model mobilizes both Burke's notion of identification and Althusser's notion of interpellation¹⁶ to show that individuals are constructed as subjects discursively, a subjectivity that they then affirm as they engage in action in the social world. I trace this constitutive process in the *JAMA-Archives* corpus in the context of professional discourse, examining the role of editorial evaluation and peer review as gatekeeping mechanisms. Despite its centrality in validating knowledge claims, peer evaluation can itself threaten the very values it appears to preserve (e.g., communalism, objectivity). The community that is subsequently brought into being through peer-appraised texts is founded on an awareness of—and struggle over—professional boundaries.

¹⁵ Charland examined the emergence in Quebec, in the early 1980s, of the *peuple québécois*, a largely Francophone, sovereignty-seeking collective that, he argues, was brought into being through a 1979 policy document of the Province of Quebec. Framing this *peuple* as "oppressed" by the Canadian national government (Charland 216), the document advances a case for Quebec's independence from Canada.

¹⁶ Althusser explains that "*interpellation* or hailing... can be imagined along the lines of the most commonplace everyday police (or other) hailing: 'Hey, you there!'" Upon hearing this call, Althusser argues, "the hailed individual will turn round. By this mere one-hundred-and-eighty-degree physical conversion, he becomes a *subject*. Why? Because he has recognized that the hail was 'really' addressed to him, and that 'it was *really him* who was hailed' (and not someone else)" (174; original emphasis).

Citing Althusser, Charland argues that “Interpellation occurs at the very moment one enters into a rhetorical situation, that is, as soon as an individual recognizes and acknowledges being addressed” (220). Charland’s model can usefully be brought to bear on health and medical contexts, as Judy Segal has done in the case of the construction of the headache patient as a rhetorical audience in the clinical exchange (*Health*). In the case of the *JAMA-Archives*, biomedical researchers had certainly published widely on CAM-related matters before 1998, in both mainstream and alternative medical venues (e.g., *JAMA*, *British Medical Journal*; *American Journal of Chinese Medicine*, *Complementary Therapies in Medicine*). However, what distinguishes the *JAMA-Archives* corpus from previous publications on CAM is its meta-turn, its self-consciousness as “a planned, concerted effort,” in the editors’ words, to compile scholarship on CAM from an explicitly biomedical perspective (“Alternative Medicine” 1618). Editors Phil Fontanarosa and George Lundberg explain this purpose in the call for papers:

The 1998 coordinated theme issues will provide a unique, multidisciplinary forum for the publication of original research studies and scholarly articles that present new scientific information and innovative ideas on complementary and alternative medicine to the medical and scientific community. By stimulating research and giving emphasis to this topic, we hope to promote widespread attention in the medical literature and the lay media, foster education among health care professionals, and increase knowledge among patients and the public. (“Call for Papers” 2111-12)

Potential contributors were assured that “Submitted manuscripts are subject to our usual rigorous editorial evaluation and peer review” (“Call for Papers” 2112); this assurance indicates, among other things, the editors’ belief that accepted submissions would be admitted into their profession’s store of knowledge, on par with research that addresses more conventional themes.

Charland's framework shifts the set of questions we might imagine being asked and answered by *JAMA-Archives* theme issues. Instead of asking, for example, *whether* there is a legitimately biomedical community of CAM researchers, the journals seem to ask *where* that community can be found. (The answer they provide would then be: *right here*.) Importantly, not all of the contributors to the corpus write favourably of CAM, and I suspect that at least several would vigorously resist the suggestion that they were a part of such a community. However, as Charland illustrates, neither the meaning nor the membership of communities interpellated by discourse are stable. In reference to Michael McGee's analysis of the rhetoric of "the people" in the context of social theory, Charland notes that "not only is the character or identity of the 'people' open to rhetorical revision, but the very *boundary* of whom the term 'people' includes and excludes is rhetorically constructed: as the 'people' is variously characterized, the persons who make up the 'people' can change" (218). As we shall see in the section that follows this, questions of membership permeate the definitions of CAM advanced in the *JAMA-Archives* corpus. Implicitly, they ask: Who belongs in biomedicine? Who does not?

Fontanarosa and Lundberg argue in their editorial in the *JAMA* theme issue that the abundance of articles both submitted to and selected for inclusion in the *JAMA-Archives* corpus indicates that research on CAM is already within the medical profession's purview. They quantify this abundance as a critical mass, noting, for example, that *JAMA* alone received over two hundred submissions, with "many more" received by the *Archives* journals ("Alternative Medicine" 1618). The editors enthusiastically report that "The result, after our usual rigorous review process, is publication of more than 80 articles...in our 10 scientific journals, including 18 randomized trials and systematic reviews, on more than 30 different topics, and from more than 16 different countries" (1618). Their emphasis on editorial review in this statement—"our

usual rigorous review process”—merits special attention because it serves normalizing effects, reshaping the research on potentially contentious topics as somewhat ordinary: the process belongs to the biomedical community (“*our...process*”), it is routinized (the “*usual...process*”), and stringent (it is “rigorous”). The editors go on to specify that *JAMA* alone published six randomized controlled trials (RCTs) of six “diverse alternative medicine therapies,” in addition to a broad selection of other articles that they enumerate and describe over the course of their editorial. Tellingly, none of the editorials introducing the other annual coordinated theme issues published five years on either side of the 1998 CAM issues feature any such quantification, nor do they emphasize the review process. Fontanarosa and Lundberg’s emphasis on the quantity (and quality) of what they deem reliable research on CAM signals a self-conscious effort to present the theme issues as making legitimate contributions to biomedical knowledge.

The articles published across the *JAMA-Archives* corpus take a variety of forms: original RCT reports and systematic reviews (meta-studies of RCTs); review articles; preliminary research reports; editorials and commentaries; letters to the editor, newsbriefs; and articles on medical ethics, policy, and practice. All of the articles are organized and published within the journals’ regular sections and categories (e.g., “News and Views,” “Original Contributions,” “A Piece of My Mind”), which vary by publication. In addition to *JAMA*, the corpus journals include *Archives of Dermatology*, *Family Medicine*, *General Psychiatry*, *Internal Medicine*, *Neurology*, *Ophthalmology*, *Otolaryngology—Head & Neck Surgery*, *Pediatrics & Adolescent Medicine*, and *Surgery*.¹⁷ The journals offer varying degrees of coverage of the CAM theme: *JAMA*, for example, is entirely devoted to CAM, while *Archives of Dermatology* and *Family Medicine* dedicate about half of their pages. Several others feature only a handful of articles or fewer (e.g.,

¹⁷ *Archives of Family Medicine* ceased publication in 2000.

Archives of General Psychiatry, Otolaryngology). I expand further in the next section on these journals' coverage of the shared theme as it relates to the articles' various definitions of CAM.

The overwhelming majority of contributing authors are biomedically trained, most with MD degrees, and are employed at conventional biomedical institutions (mostly universities and hospitals). Only eight of the articles feature authors with formal CAM accreditation: three articles feature contributors with both doctoral-level biomedical designations (MD/PhD) and CAM accreditation (four authors in total), while five articles include authors trained solely in CAM (four authors total, including two acupuncturists, one Doctor of Oriental Medicine, and one Naturopath). Many of the contributors have dedicated their careers to studying CAM-related topics (e.g., Cardini and Weixin), while others are regular contributors to *JAMA* and the *Archives* on a wide range of topics, not primarily on CAM (e.g., Delbanco). It is difficult to tell whether the small proportion of articles by authors with CAM credentials is a factor of submission or of editorial selection; in either case, the overwhelming presence of MDs and comparatively few CAM-trained researchers may be a factor in the editors' claims that the research reported in the theme issues is biomedically valid.¹⁸

Several contributors are very prominent CAM researchers that have published widely in biomedical and CAM journals, each with several publications included in the *JAMA-Archives* corpus. Two of these researchers are important figures throughout this dissertation. David Eisenberg, MD, is lead author of one article in the corpus and co-author of three others. Eisenberg has been at the forefront of research on CAM since the late 1970s and authored the landmark 1993 *New England Journal of Medicine* survey of CAM use and expenditure, as well

¹⁸ I have excluded from this description of non-MD authors Doctors of Osteopathy (DO), whose training is akin to that of MDs but with an added focus on spinal manipulation that is similar to, but distinct from, chiropractic. Although DOs are often considered alternative practitioners, they have had full practice rights on par with MDs since the 1960s (see Whorton, *Nature Cures*).

as a 1998 follow-up survey in the corpus. At the time of the theme issues' publication, Eisenberg was Director of the Center for Alternative Medicine Research and Education at Harvard Medical School. Eisenberg's longtime collaborator and colleague at Harvard, Ted Kaptchuk, contributed two articles, one as lead author. Kaptchuk, Associate Professor of Medicine, is a Doctor of Oriental Medicine by designation and publishes prolifically on CAM, primarily in prestigious biomedical journals such as *JAMA*, *The Lancet* and *British Medical Journal*. As I argue in Chapter Three, Kaptchuk's analyses of research design and placebo effects have helped to spur important reconsideration of the role of intervention effects (i.e., effects of the act of treatment itself) on patient outcomes. In addition to their faculty appointments and active research programmes, Eisenberg and Kaptchuk have both maintained public visibility as boundary-straddling scholars, appearing in both print and television as representatives of the "new science" of alternative medicine (see Chapter Four).¹⁹

Although the *JAMA-Archives* corpus hails a biomedical community that considers research on CAM as simply a matter of course, some members of the medical profession have expressed grave concern about the potential implications of such an expanded purview. In June 1998, for example, physician Demetrios Theodoropoulos expressed "surprise" in a letter to the editor of *Archives of Pediatric & Adolescent Medicine* that the AMA journals would feature CAM as a special theme. He maintained that "with this presentation you unintentionally legitimize alternative medicine in a journal under the auspices of the American Medical Association....The purpose of this presentation is not clear and its implications will be harmful to the profession and our patients" ("Professional Identity" 606). His criticism here is grounded

¹⁹ Edzard Ernst, MD, PhD, is another prominent CAM researcher that merits mention. Lead author of three articles in the corpus and co-author of two more, Ernst holds the Chair in Complementary Medicine at the University of Exeter and is a prolific, outspoken critic of CAM. He has published widely in both scholarly and popular contexts on the need for more, higher-quality evidence of CAM's safety and efficacy, most publicly in his 2008 book *Trick or Treatment: The Undeniable Facts about Alternative Medicine*, coauthored with journalist Simon Singh.

in concern about the potential rhetorical effects of applying the AMA's imprimatur to the subject of CAM; that imprimatur, he suggests, would effectively authorize the practices under study. He concludes by asking, "if there is no 'alternative law' or 'alternative engineering,' why should our profession allow the laxity of attitude to recognize an 'alternative'?" (606). Theodoropoulos' concern rests on what it means for the medical profession to engage in research on unorthodox health practices that lay claim to the label *medicine*. The exchange subsequently sparked by his letter offers insight into what appears to be at stake in the *JAMA-Archives* corpus as a set of community-defining texts: the integrity and scope of medicine itself.

Catherine DeAngelis, editor of *Archives of Pediatric & Adolescent Medicine*, appended a short reply to Theodoropoulos' letter, explaining that, as editor, "I prefer to have what is submitted reviewed by those of us who follow the traditional 'rules and regulations' before making a judgment" (606). In her view, I infer, scientific reason alone will determine the merit of the submissions received and any perceived incursion on professional territory can be settled by objective means. Gerald Ente's response to Theodoropoulos, published in the corpus issue of the same journal, offers a slightly different perspective; Ente bases his criticism more squarely on what he considers a problem of professional parochialism: "[Theodoropoulos' letter] is the typical narrow-minded, 'scientific' physician's answer to a real problem that I believe is driving patients away from allopathic medicine into the waiting, open arms of alternative, complementary, integrative, or holistic medicine.... We need not accept as truth all we learn, but we must keep learning" (1154). Professional borders are, for Ente, maintained not simply through pure reason but also through reasoned and flexible professional judgement.

Theodoropoulos was unpersuaded by these critiques. In his response to Ente (published in the corpus alongside Ente's letter), he re-stated his concerns about professional turf more

candidly: “The question of alternative medicine...is based on attacks to the integrity of medicine, the relevance of science, and the effectiveness of modern clinical practice. These views may occasionally be voiced in the American Medical Association press, but they do not promote our profession” (Reply 1154). Theodoropoulos views the rise in popularity of CAM directly in terms of its potential effects on his own profession, concluding that mere “Acknowledgement of alternative forms of medicine would inflict enough damage to a profession of allopathic physicians to outweigh any benefits (if there are any in the first place)” (1154).

This exchange among Theodoropoulos, DeAngelis, and Ente illustrates the variety of perspectives that members of the medical community hold regarding the professional implications of biomedical CAM research. Theodoropoulos may seem the most limited of the three in his suggestion that even just opening up a discussion about CAM in a biomedical context could threaten the medical profession. However, Theodoropoulos offers perhaps the most shrewd appraisal of the potential implications of bringing CAM into the scope of biomedical research, if only because he explicitly recognizes the role of competition in professional boundary work. DeAngelis’ and Ente’s more even-handed analyses do not express such blatantly protectionist motives, but both authors place considerable stock in the ability of scientific method to adjudicate on matters that depend, in the end, on more than measurable phenomena—they depend also on the communities that measure them.

As Theodoropoulos points out, the contest over the boundaries of what counts as legitimate health care is determined significantly by the discursive activities of professions, such as the publication of professional journals. Since the texts that professions use and produce come to constitute the professions themselves (Bazerman and Paradis), Theodoropoulos’ concern is well-placed: simply publishing a study on, for example, the effect of lunar phases on post-

surgical outcomes (Smolle, Prause, and Kerl) within an AMA-sponsored journal seems to suggest that the AMA considers lunar phases a valid area of study—regardless of the study’s negative result or the authors’ dismissal of the intervention as “hav[ing] no relationship to the real world” (1369).

Fontanarosa and Lundberg’s emphasis in their *JAMA* editorial on the quantity of CAM-themed articles assembled in the corpus, a seeming embarrassment of riches, glosses over the jurisdictional dispute occurring within the pages of the *JAMA-Archives* corpus. That is, by calling forth an already CAM research-friendly biomedical community, the corpus does not reflect the inherently competitive nature of what sociologist Andrew Abbott describes as the professional ecosystem. Fontanarosa and Lundberg do not make a case for biomedicine’s jurisdiction over CAM, which Theodoropoulos takes as not yet established, but take that jurisdiction simply as given. Abbott’s theory of the professions and related scholarship by Starr, Saks, and Baer on the history of medicine as a profession are useful to my analysis because they shift the context of the jurisdictional debate over CAM research from one that is strictly epistemological (i.e., that scientific methods will resolve the debate) toward one that is more explicitly social, even political.

Abbott describes professions not as a set of distinct entities but as a system, governed by tensions that continually reshape the relationships among them. In Abbott’s definition, professions involve occupations outside the commercial/industrial sphere whose main trade is in expertise. Their stores of abstract knowledge enable their control of other, related, occupations because those stores govern the practical knowledge necessary to performing the work.²⁰ Abbott describes the system of professions in ecological terms: “Each profession is bound to a set of

²⁰ Abbott notes that “only a knowledge system governed by abstractions can redefine its problems and tasks, defend them from interlopers, and seize new problems.... Abstraction enables survival in the competitive system of professions” (9).

tasks by ties of jurisdiction, the strengths and weaknesses of these ties being established in the process of actual professional work. Since none of these links is absolute or permanent, the professions make up an interacting system, an ecology” (33). Competition is central to Abbott’s view: jurisdictional disputes arise necessarily when one profession claims territory formerly occupied by another. These disputes are part of the normal life of professions but, as Abbott points out, the more extensively and strongly organized professions tend to triumph over weaker ones.²¹

Professions are also inherently inegalitarian, as Paul Starr has shown. In his view, each “claims to enjoy a dignity not shared by ordinary occupations and a right to set its own rules and standards. These claims go against the democratic grain” (37). In order to attain and maintain a position of privilege (authority, high income, autonomy), a profession must distinguish itself from both the public it serves and from other similar (and perhaps competing) occupational groups. The jurisdictional claim over CAM made in the *JAMA-Archives* theme issues illustrates Starr’s point: in hailing a biomedical community that already encompasses CAM research, the journals lay claim to control over a broad range of disparate health practices whose practitioners (and patients, in self-administered therapies) themselves seek jurisdiction over the work they perform.

²¹ In the settlement of jurisdictional disputes, Abbott notes six possibilities: 1. *Full Jurisdiction*, where the profession claiming territory obtains it; 2. *Subordination*, where one profession occupies a position under the umbrella of another (e.g., nursing under medicine); 3. *Division of Labour*, with “functionally interdependent but structurally equal parts” (e.g., marriage counselling falling under the jurisdiction of psychiatrists, social workers, or clergy, among others; 73); 4. *Intellectual Jurisdiction*, where one profession’s theory dominates but practice is assimilated among several (e.g., psychiatry’s theoretical presence in other kinds of counselling); 5. *Advisory*, where one profession has some kind of influence over another, although that influence is not binding (e.g., the role of clergy in medicine); 6. *Workplace-client Differentiation*, with less prestigious professions providing service to underserved clientele (e.g., psychiatry treating wealthier patients; psychology, less wealthy ones; and social work, the poorest ones). I do not want to suggest that any one or more of these possible resolutions do or will apply to biomedical-CAM boundary work, but, together, the categories are useful for thinking about how occupations with different stores of cultural capital come into contact with one another.

Starr's view of the medical profession as inherently inegalitarian and antidemocratic resonates with more recent work in the social sciences on medicine. Anthropologist Hans Baer takes a slightly more extreme position than does Starr, framing the ascendance and maintenance of what he calls the "medical hegemony" of the United States as "the process by which capitalist premises, concepts, and ideology influence biomedical diagnosis and therapy" (35). Baer traces how different medical practices develop to reflect the varying ideologies of segments of the population, concluding that individuals' use of different (combinations of) health systems ultimately reflects their race, class, gender, and ethnicity. As a result of capitalist influence, he argues, "medicine became yet another hegemonic vehicle by which members of the corporate class indirectly came to legitimate capital accumulation and to filter their view of reality down to the masses" (4). The biomedical paradigm—which eventually came to shape all aspects of medical education, research, and practice—was then able to deflect the social origins of disease by emphasizing intervention, post-illness, over prevention, shifting to models of individualist care rather than of public health, thereby protecting the interests of the dominant class.²²

Sociologist Mike Saks advocates a similar view of medicine, though he defines his project not so much in terms of capitalist dominance as in terms of the "politics of work." From this perspective, he argues, "successful occupations are seen as gaining increased income, status and power in the marketplace by socially excluding their competitors, who conversely lose out in the struggle for such rewards" (4). In this context, Saks argues, the disparate practices included under the rubric of alternative medicine are defined principally by their political and professional marginality—their relative lack of research funding, exclusion from conventional medical

²² Ronald Glasser critiques such a model, arguing in a 2004 *Harper's* article that "policymakers have consistently preferred the most expensive and least efficient models of health care, proving once again that the apostles of privatization are motivated not by hard-nosed economics but by an incoherent ideology that is little more than a brittle mask concealing the most irrational species of self-interest" (39-40).

curricula, denigration in medical journals, and strict restrictions on their practice. Essential to Saks' perspective, and consonant with Starr's, is his conclusion that, ultimately, "orthodox and alternative medicine are two seamlessly interrelated sides of the same coin" (162). Which health practices and practitioners end up on which side of the coin at any given time depends in large part on what best serves the dominant profession's interest at that moment. One of the means through which that interest is protected is through editorial evaluation and peer review, processes that Carol Berkenkotter describes as "a strategic site of contention and negotiation" among members of professional communities (247).

Fontanarosa and Lundberg cite editorial and peer review in both their call for papers and their *JAMA* editorial as the main assurance that the material included in the coordinated CAM theme issues meets standards agreed upon by the biomedical community.²³ As Alan Gross points out, peer review certifies new knowledge, admitting once-tentative claims into a profession's shared store of knowledge ("Persuasion" 204). Consequently, he argues, "submission of a paper to a scientific journal counts as a request, a regulative act whose successful completion depends on shared social norms: communicative action is initiated and issues, eventually, in a decision to accept or reject" (195). Berkenkotter and Huckin similarly frame peer review's function as, primarily, gatekeeping, although they note that, despite its ostensible purpose to control the quality of scholarship *allowed in* to a discursive community, peer review can also be used to *block out* other scholarship, even that of high quality, to protect a reviewer's own interests

²³ All of the articles published in the corpus underwent editorial review, although not all underwent external peer review. In *JAMA*, for instance, all articles published in the "Original Contributions," "Brief Report," "Health Law and Ethics," "On Call," and "Research Letter" sections are sent for external review, while articles in the "A Piece of My Mind," "Letters to the Editor," and "Commentary" sections are not. For my purposes here, I consider both sets of articles as peer-reviewed, insofar as they have all been evaluated in some form by colleagues within the same professional community.

against competitors. Both of these analyses show that peer review serves crucial social functions because it depends on and gives structure to the activities of professional communities.

In the context of boundary work in the *JAMA-Archives* CAM theme issues, peer review cuts both ways: as DeAngelis and Ente argue, testing the submissions against the medical profession's established standards can eliminate those that do not conform, a process that appears to ensure the integrity of the articles eventually published in the corpus; however, as Theodoropoulos suggests, submissions that do pass the test do so ostensibly with the profession's official endorsement and so, by accepting any articles at all, peer review might actually compromise the integrity of the community that produced those standards. On the latter point, for instance, physician C. N. M. Renckens colourfully argued in 2003 that even "impeccable trials" of CAM ought to be dismissed on grounds of the "incomprehensible absurdit[y]" of the interventions under study. He warns that the consequence of even considering trials of CAM will be that "one's mind stays so open that the brains fall out!" (531).

To return to Gross' description of peer review as a "regulative act," one of the governing principles of the peer review model is that authors and reviewers belong to the same or similar discourse communities. In the case of CAM research, however, authors and reviewers may not have entirely "shared social norms" (195), and so the biomedical standards by which submissions are evaluated may not suit the CAM interventions under study. For example, reviewers may not be aware of the methodological complexity involved in trials of interventions such as acupuncture, and so they may not be fully equipped to judge the strengths and weaknesses of such a trial. (I take up trial design and evaluation in Chapters Two and Three.) In the context of the *JAMA-Archives* corpus, then, one of the regulative acts that peer review performs is to determine how biomedical CAM research will unfold because it selects which

work gets published and which does not. If we consider the corpus, in Bazerman and Paradis' terms, as an assemblage of profession-defining texts, then the picture of CAM that emerges within it becomes important because it sets terms for the CAM-researching biomedical community that it constitutes. That is, in the competitive ecosystem of the health professions, the biomedical community, as the dominant profession, can potentially redefine what CAM is, and even overwrite the histories of its constituent practices; this potential, I explore next.

The Persuasiveness of CAM as a Residual Category

Numerous commentators have identified “Complementary and Alternative Medicine” as foremost a “residual category,” a category best explained not by the logic uniting what it contains but by the logic excluding it from other categories within a classification system. For example, medical sociologist Paul Root Wolpe describes CAM as “defined not by its internal coherence but by its exclusion from other categories of medicine” (165). Similarly, health policy expert Mary Ruggie argues that, primarily, “alternative medicine is understood in the negative—as something that is not medicine” (41). In this section, I take up CAM as a residual category to examine how its various definitions and histories can be persuasive in the context of biomedical boundary work. Geoffrey Bowker and Susan Leigh Star write, of systems of classification, “Each standard and each category valorizes some point of view and silences another” (5). Defining CAM not by what it is but by what it is *not* is an inherently political act because it assumes the primacy of the biomedical model, marginalizing others. I argue that the ways that authors in the *JAMA-Archives* corpus define CAM and delineate the histories of the practices it comprises ultimately enhances and expands biomedical jurisdiction over matters of health and illness.

CAM's designation as residual, an umbrella for otherwise-unclassifiable practices, can be traced back at least to the Office of Alternative Medicine's (OAM) formal 1995 definition of CAM: "a broad domain of healing resources that encompasses all health systems, modalities, and practices and their accompanying theories and beliefs, other than those intrinsic to the politically dominant health system....CAM includes all such practices and ideas self-defined by their users as preventing or treating illness or promoting health and well-being" ("Defining and Describing"). This definition has been adopted widely both in the scholarly and popular press, often uncritically, even by those that advocate the various practices it encompasses. There are scholarly journals dedicated to CAM (e.g., *eCAM*), scholarly societies (e.g., Social Science Studies of CAM and Integrative Medicine [IM]), and patient handbooks (e.g., Mackenzie and Rakel). Though used increasingly in conjunction with *IM* over the decade since the publication of the *JAMA-Archives* corpus, the phrase *CAM* continues to have significant traction, even despite its residual nature.²⁴ Toward the end of this section, I will clarify my own use of the term in this dissertation, since the range of practices with which I am primarily concerned is narrower than those included under the OAM's broad definition. For now, however, the OAM definition will serve as my working definition, which I will fill in throughout this section with the various descriptions of CAM advanced within the corpus.

There is no consensus view of CAM in the corpus other than its ostensible opposition to biomedicine: some authors draw the biomedicine-CAM boundary more restrictively than others. The articles cover a wide range of health interventions—from those with some credibility in biomedicine (such as acupuncture) to those on the extreme end of the alternative fringe (such as the effects of lunar phases on health)—although nearly all contend that CAM threatens the

²⁴ I would suggest that the compliance of supporters of individual CAM practices in using the *CAM* designation is good evidence of biomedicine's ability to set the terms under which the boundary debate unfolds.

stability of biomedicine. Lumping such a disparate range of health practices together conceals whatever historical and philosophical coherence they might independently have, while at the same time accentuating biomedicine's own coherence. Star and Bowker refer to concealing processes of this sort as the "double silencing" of residual categories, which entails "first putting [the thing classified] into a 'garbage' category because unknowable and then constructing the category in such a way that no historical or social information can escape from it" (274). Double-silenced as health practices with historical-professional narratives of their own (some of which span hundreds or thousands of years), the individual modalities that comprise CAM are effectively re-made in the corpus in biomedicine's own terms.

Eisenberg et al.'s definition of CAM in their *JAMA* survey as those "interventions not taught widely in medical schools nor generally available in US hospitals" is cited throughout the corpus but its meaning is never stable, even within the Eisenberg study ("Trends" 1569). There, "CAM" refers ambiguously to a wide range of practices, from those that are organized and regulated (e.g., chiropractic, massage therapy, and Traditional Chinese Medicine) to those that are self-administered (e.g., relaxation, self-help, and self-prayer). This floating definition leaves much room for interpretation throughout the corpus, with many contributors unsure of what, exactly, counts as alternative medicine. Included among studies of acupuncture and herbal medicines, for example, are studies of yoga (Garfinkel et al.), spirituality (Thomsen), off-label use of dermatologic therapies (Li et al.), lanolin and breast shells for breastfeeding women (Brent et al.), and physician referral to clergy (Daaleman and Frey). Neither the authors of the off-label use study, nor those of the breast shell study question their designation of those interventions as "alternative," even though both are common biomedical interventions.²⁵ The authors of the final

²⁵ Off-label use is considered alternative in the Li study "because it does not conform to FDA regulations" (1449). The authors of the breast shell study, Brent et al., do not reflect on whether (and, if so, how) breast shells and lanolin

study, Daaleman and Frey, admit that they are unsure if physician referral to clergy constitutes CAM, leaving the question open to the reader's interpretation.

While many of the journals feature a wide range of alternative medical practices (e.g., *JAMA*, *Archives of Dermatology*, *Archives of Internal Medicine*), others offer only a limited range. For example, *Archives of Otolaryngology—Head & Neck Surgery* features only a short editorial on CAM, which explains that there is not yet a place in the field of otolaryngology for alternative practices (Krouse). Krouse indicates the need for a “paradigm shift” in medicine and urges readers to “actively participate in the development and evaluation of alternative techniques that impact on disorders of the head and neck” (1200). *Archives of General Psychiatry* offers two articles under the heading “News and Views” (Ernst, Rand, and Stevinson; Wong, Smith, and Boon), but features no clinical trials or systematic reviews, while *Archives of Ophthalmology* does not broach the topic of CAM at all, not even in its regular “In Other AMA Journals” section that reports on studies published concurrently across the *JAMA-Archives*. In *Archives of Surgery*, “alternative” takes on a very specific meaning, referring to alternatives *within* mainstream biomedical surgery. With one exception (Ernst and Pittler's systematic review of a homeopathic remedy on postoperative gastrointestinal discomfort), the “alternatives” discussed in this journal involve minimally invasive surgery. One pair of authors are even careful to point out that surgeons ought to avoid the “alternative” label altogether because of its association with CAM: “Alternative medicine, as it applies to trauma management, might be safely considered an oxymoron” (Britt and Cole 1177).

We might entertain several explanations of the limited coverage of CAM in these specialty journals. It may be, for example, that CAM interventions are more relevant to some

constitute CAM but, in a sidebar, editor Catherine DeAngelis describes the intervention as a “self-administered, simple, natural” alternative to “more sophisticated and costly remedies” available to breastfeeding women (1077).

medical specialties than others. This interpretation is compatible with the main conditions for which individuals seek CAM, including chronic, functional conditions such as back, muscle, or joint pain, headache, or digestive problems. Such conditions are more likely to be treated in primary care settings, and so the higher emphasis on CAM in journals such as *Archives of Family Medicine* than in specialty journals makes some sense. An alternate, possibly compatible, explanation is that some specialties, particularly more prestigious ones such as surgery, may be more fortified professionally against boundary incursions than others. For example, acupuncture, chiropractic, and massage therapy are all promoted as nonsurgical alternatives for musculoskeletal disorders, so one might be surprised not to find some discussion in *Archives of Surgery* of their safety and efficacy in comparison to surgery. The comparably lower coverage of CAM in these journals might be read as a strategic move to preserve these specialties' jurisdiction over the conditions on which they are focused. Whatever the explanation, the boundaries drawn through the degree of coverage in specific journals in the corpus show some areas of biomedicine to be less available to, or better guarded against, the encroachment of CAM than others. The model of CAM that emerges in the *JAMA-Archives* corpus, then, is somewhat restricted, possibly sensitive to internal biomedical hierarchies.

Demarcation of the boundaries between science and nonscience is also central to the various definitions of CAM in the corpus. Gieryn defines demarcation as the question of "how to identify unique and essential characteristics of science that distinguish it from other kinds of intellectual activities" ("Boundary-Work" 781). More simply, sociologist Robert Evans describes demarcation as the search for "what, if anything, makes science special" within the larger culture (3). Concerns about demarcation are usually implicit in the *JAMA-Archives*, as in the exchange among Theodoropoulos, DeAngelis, and Ente, above. Theodoropoulos' belief that the journals

should not publish on CAM at all is premised, in the first instance, on concern for his profession, but that concern is, in turn, premised on the idea that biomedicine and CAM occupy separate domains—they are scientific and nonscientific, respectively. Similarly, in *JAMA*, Tom Delbanco likens research on CAM to the “scientific study of astrology” (1561). For Delbanco, science is simply not an appropriate arena for evaluating CAM, and so clinical trials of practices such as homeopathy constitute a sort of category error: the trial of art or magic by the methods of science. DeAngelis and Ente take the opposite tack, arguing that the methods of science are precisely the means through which we can discern which practices are based on scientific evidence and which are not.

Authors in the corpus who discuss demarcation explicitly hold radically different views of the nature of biomedical boundaries. At one end of the spectrum are those articles that vigorously (and often humorously) decry alternative medicine in the name of boundary work. In *Archives of Dermatology*, for example, Rudolf Happle argues that CAM “represents a collective aberration of mind” (1455). He advances twelve theses toward an “epistemological demarcation” of the boundary between what he calls “rational” (mainstream) and “irrational” (alternative) medicine (1455), such as *The Paradigm of Regular Medicine is Rational Thinking*, *The Paradigm of Alternative Medicine is Irrational Thinking*, and *The Present Popularity of Alternative Medicine Can Be Explained by Romanticism*.²⁶ Happle cites Karl Popper’s principle of falsification as a litmus test for rational thinking. Popper holds that science is demarcated from other intellectual activities by logic and method, particularly the principle of falsification (rather

²⁶ This is Happle’s complete list: 1) Alternative and regular medicine are speaking different languages; 2) alternative medicine is not unconventional medicine; 3) the paradigm of regular medicine is rational thinking; 4) the paradigm of alternative medicine is irrational thinking; 5) the present popularity of alternative medicine can be explained by romanticism; 6) some concepts of alternative medicine are falsifiable and others are not; 7) alternative medicine and evidence-based medicine are mutually exclusive; 8) the placebo effect is an important factor in regular medicine and the exclusive therapeutic principle of alternative medicine; 9) regular and alternative medicine have different aims: coming of age vs. faithfulness; 10) alternative medicine is not always safe; 11) alternative medicine is not economic; and 12) alternative medicine will always exist (1455).

than verification of observation, which Popper notes, had been conventionally believed). Falsification entails that a theory can only count as scientific if it can be subjected to empirical testing and be either proven or disproven; anything that cannot be empirically proven or disproven (such as a theory of God) would then be categorized as nonscientific. As Evans points out, the falsification principle is a problematic demarcation criterion because “what is to count as falsification is always negotiable so that the conclusion is never determined by the data alone” (6). Furthermore, as I show in the next chapter, the methods through which hypotheses are proven or disproven, are themselves variable, open to negotiation. For Happle, though, falsification is a valid means of demarcating the boundary protecting biomedicine from other, possibly contaminating, models of health and healing. In an effort to distinguish those readers who would defend alternative medicine from those with whom Happle would align himself, he concludes: “if you who are reading this article do not know what rational thinking means, you are beyond help” (1457).

At the other end of the spectrum are those contributors who take very seriously the challenge to understand biomedicine’s relationship to CAM. They offer analyses compatible with Gieryn’s description of boundary work as cultural cartography. For example, Goodwin and Tangum couch their article in *Archives of Internal Medicine* within the context of the professions, à la Abbott, noting that proponents of micronutrient supplements were often dismissed over the twentieth century for reasons similar to those for which Galileo faced resistance: because they “did not respect professional boundaries” (2187). Lest readers assume that they are “apologists for megavitamins,” Goodwin and Tangum explain that their interest lies only in what the “vitamin controversy” can tell us about “the forces that influence medical practice other than those stemming directly from scientific discovery” (2187). They show that

systemic bias against vitamin research has produced bodies of data founded on incomplete or faulty evidence. Citing Foucault, Feyerabend, and Kuhn, Goodwin and Tangum argue that one of the important lessons that biomedical CAM research can offer to biomedicine, generally, is how the social context of medicine “influences everything we do as physicians—which diseases we recognize and which we ignore, which treatments we use, and which we reject. The more we learn about why we do what we do, the more likely we are to avoid errors in the future” (2190). Unlike Happle, these authors depict the biomedicine/CAM boundary as permeable and subject to social and cultural factors, including professional concerns.

Sugarman and Burk move the arena of debate from questions of demarcation to questions of practice. They argue in their *JAMA* article that, ultimately, while discussions of boundaries are useful for tracing out seemingly disparate epistemic realms, the real work of medicine is treating patients. Although they believe that “scientific method” is what divides biomedicine and CAM (they say biomedicine uses scientific method and CAM does not), they hold that this distinction is only relevant to practice because it affects the availability of evidence on health interventions’ safety and efficacy. They temper the boundary debate in the corpus by reminding readers that, in the context of helping patients meet health-related goals, physicians have an obligation to be able to address CAM-related issues—they must accept CAM’s popularity and gain knowledge about it rather than dismiss it out-of-hand, especially since patients continue to see alternative practitioners even without their physicians’ knowledge or approval. Sugarman and Burk warn physicians that when they do counsel patients on CAM, “it is essential to be vigilant in ensuring that these determinations are not clouded by hegemonic concerns about social status, market share, unfamiliarity [with CAM], or prejudice” (1624). By highlighting physicians’ “ethical obligations” toward patients on the subject of CAM, Sugarman and Burk offer a timely reminder

that boundary work affects more than simply the professions that perform it: physicians' knowledge of and attitude toward CAM can affect their patients' care.

Perhaps more importantly, Sugarman and Burk also illustrate how lumping the disparate range of health interventions that do not fall under the rubric of biomedicine into a single category of leftovers (i.e., *CAM*) makes those practices essentially “unknowable” (Star and Bowker). The definition of CAM as not-biomedicine, Sugarman and Burk argue, can highlight its otherness and block from view the many similarities biomedicine shares with the individual practices that CAM comprises (1624). Moreover, its negative definition can galvanize those who wish to dismiss CAM simply on definitional grounds (e.g., “if it’s not biomedicine, then it must not be legitimate”). The various definitions of CAM found within the corpus have a strikingly similar effect to those effects about which Sugarman and Burk worry: taken individually, many definitions are thoughtful and nuanced, some even moving beyond the biomedicine/CAM binary. But, taken together, CAM is so many different things at once that it is, in essence, nothing in particular—it is, that is, a residual category. In the corpus as a whole, then, we find a meta-level claim about CAM in which biomedicine, as the benchmark, sets the terms of what CAM is, and what it will be.

Definitions of CAM in the *JAMA-Archives* corpus provide evidence of the first movement within what Star and Bowker call the “double silencing” of residual categories, when phenomena that do not fit within a classification system are compiled into a category of leftovers. The historical relations established in the corpus between biomedicine and CAM provide evidence of the second “silencing”: the residual category is “then construct[ed]...in such a way that no historical or social information can escape from it” (274). The manner in which

CAM's historical context is established in the corpus closely mirrors the manner in which CAM is defined, and so I will offer only a basic sketch here.

A handful of articles do provide reflective accounts of various CAM practices within their historical contexts (e.g., Jacobs, Chapman, and Crothers; Jonas; Oumeish), but most discuss CAM as though the practices it comprises have no histories at all, beyond their contact with biomedicine. The lack of attention to CAM as a historical and social phenomenon reflects not as much on the individual character of the authors as on the medical profession as a whole.

Following Star and Bowker, I would argue that the apparent lack of curiosity about CAM in the corpus derives largely from its residual status: as not-medicine, CAM may simply not rank high enough to merit close study by busy researchers and practitioners, especially given the range of practices subsumed within the category. Further, by minimizing the philosophical, practical, and professional independence of the CAM practices under study, members of the medical profession can more easily claim jurisdiction over the investigation of those practices.

Most of the trial reports in the corpus treat the interventions under study as a kind of medicine à la carte, in which certain CAM principles can be adopted, piecemeal, into biomedicine. For example, in their *JAMA* study of acupuncture for AIDS-related pain control, Shlay and colleagues do not evaluate acupuncture in its context as a single treatment modality within a comprehensive, autonomous health system, Traditional Chinese Medicine (TCM). They instead adopt a research protocol fundamentally at odds with the TCM model, using a standardized acupuncture procedure that they admit “differs from the practice of most acupuncturists, who treat patients with individualized regimens” (1594). The ease with which the authors dismiss this shortcoming, especially given their study's negative finding, indicates their reluctance, whether conscious or unconscious, to consider acupuncture as part of a complete,

independent model of health care. Even though acupuncture has long been a subject of interest in American health care (see, for example, the *American Journal of Chinese Medicine*, founded in 1973), in the Shlay article, it is as if acupuncture has no history of its own.

In their study of yoga for the treatment of Carpal Tunnel Syndrome, also in *JAMA*, Garfinkel et al. likewise do not account for historical or cultural aspects of yoga as a constituent element of Ayurvedic Medicine. Like Traditional Chinese Medicine, Ayurveda is a complete, ancient philosophy of health encompassing prevention, diagnosis, and treatment (although not necessarily in those terms). The authors attribute their study's partially positive findings to biomechanical improvements, such as relieved compression, better joint posture, and improved blood flow, while in Ayurveda, such improvement would instead be attributed to changes in elements such as heat and wind. While it of course does not make sense to chastise biomedical researchers for not citing heat and wind as mechanisms of action in their study, their omission of these basic principles of Ayurvedic Medicine as matters relevant to, for example, potential problems for implementing the yoga techniques in biomedical practice is telling.

These two articles, on acupuncture and yoga respectively, are typical of the corpus research articles: they offer brief anecdotes or historical trivia about the interventions under study, and then evaluate them as though they exist independently of the philosophies of health from which they are derived. With CAM constructed primarily as a residual category, the authors in the corpus are able to proceed with potentially boundary-threatening work without compromising biomedicine's authority because they minimize their studies' associations with CAM. In this view, the absence of history regarding the individual practices that comprise CAM can be persuasive because it allows the allows researchers to study practices such as TCM not as

unique, autonomous, philosophically grounded practices, but as a disparate group of discrete techniques, such as acupuncture, that can simply be absorbed (or not) by biomedicine.

Of course, unlike TCM and Ayurveda, not all CAM practices are autonomous or philosophically grounded, and this is one of the most problematic aspects of CAM as a descriptive category. CAM aggregates into a single category a jumble of health interventions that are not necessarily of the same conceptual order: some are self-administered and unstructured (e.g., prayer), while others are fully professionalized with formal education and licensing (e.g., chiropractic); some consist of standalone treatments (e.g., megavitamins), while others are comprehensive health systems (e.g., Ayurveda); and some have philosophies of practice that are essentially compatible with biomedicine (e.g., massage therapy), while others' stand in opposition (e.g., TCM)—and still others operate parasitically on the biomedical model (e.g., live blood analysis).

I want now to restrict my own definition of CAM. Referring to such an unwieldy range of interventions in the analysis of biomedical boundary work would require so much explanatory apparatus along the way that the project itself would become unmanageable, particularly because I do not want to elide categories myself. I focus on acupuncture, chiropractic, and dietary supplements (e.g., herbal remedies and megavitamins), to which I will also refer, for convenience, as *CAM*. These interventions are united by three factors, although they remain otherwise distinct: they are among the most accessed by patients, they are the most researched, and they have the highest levels of physician patterns of referral and belief in their efficacy.²⁷ However, as I refer to “CAM,” I want also to retain a penumbral reference to the other interventions that are cast in the corpus as *alternative* to biomedicine. The interventions within

²⁷ The most accessed CAM practice in the 1998 Eisenberg study was relaxation, which I have excluded from my restricted definition of CAM because its popularity is not matched by correspondingly high levels of research. See Astin et al. on physician's attitudes toward and patterns of referral to CAM.

my restricted definition have posed the most formidable challenge to biomedicine as a profession but, as use of practices such as naturopathy and homeopathy rises further, and as research agendas follow suit, those less-studied interventions may well follow a similar trajectory in the context of biomedical boundary work.

As a residual category, CAM not only obscures the histories of the practices it comprises—it also obscures biomedicine’s own unstable past. By compiling all practices not explainable under the biomedical model, biomedicine itself appears more stable and coherent in relief, even though it does not itself encompass a fully fixed set of medical values or practices (see Stein). As Baer, R. Porter, Saks, and Starr illustrate, separately, the history of medicine is a history of boundary work, a narrative of efforts, both conscious and unconscious, to maintain and expand territory within the professional ecosystem. Over the years, that boundary work has become less visible, naturalized with the rise of quantitative measures such as randomized, controlled trials and the advent of evidence-based medicine (EBM). In EBM, the primary impetus behind biomedical CAM research, the evidence produced by RCTs seems to speak for itself. This evidence appears able to determine objectively whether health interventions are safe and effective. As I suggest next, however, EBM is also a strategy of professionalization not unlike other, more explicit strategies employed in biomedicine over its history. Unlike these other strategies, however, EBM can effect boundary work without seeming to do so at all, in large part through the discourse of quantification.

Evidence-Based Medicine and Jurisdictional Control

Historian of science Theodore Porter refers to quantification as a “technology of distance” (ix). An eminently *social* technology, quantification can bolster authority, or even stand in for it,

when professional boundaries are at stake: “The insistence in scientific communication on objectivity and impersonality is partly...a response to pressures from outside. Or rather, mechanical objectivity is especially prominent when inside and outside are not sharply differentiated” (229). In biomedical boundary work, numbers perform important social functions: “[quantitative] discourse helps to produce knowledge independent of the particular people who make it” (T. Porter ix). In biomedicine, numbers are emptied of their social interest by virtue of their method of production, the RCT, and so seem to present nothing but facts. In this section, I argue that the apparent disinterestedness of the evidence produced by quantitative methods is one of the primary means through which the *JAMA-Archives* corpus shifts, invisibly, and then seeks to fix, the boundaries between what counts as proper medical science and what does not.

Evidence-based medicine is often depicted as the great leveller in biomedical boundary work. Research methodologist Andrew J. Vickers argued in 2001, for example, that mainstream/alternative labels mean nothing, that “what matters in EBM is evidence, not how a treatment is currently categorized” (1). Similarly, Simon Singh and Edzard Ernst write that EBM “endorses any treatment that turns out to be effective, regardless of who is behind it, and regardless of how strange it might be” (26). The *JAMA-Archives* corpus is founded on this very idea: to put CAM practices to the test and then let the evidence determine whether or not they are legitimate. Framed this way, the individuals that perform the research appear simply to work in the service of the evidence they produce. For example, Fontanarosa and Lundberg argue that, regardless of individual researchers’ beliefs about CAM, “until solid evidence is available that demonstrates the safety, efficacy, and effectiveness of specific alternative medicine interventions, uncritical acceptance of untested and unproven alternative medicine therapies must stop” (“Alternative Medicine” 1619). Evidence is CAM’s sole arbiter, they argue, and

interventions that have not yet accrued enough will count as ineffective until proven otherwise. However, the discourse of EBM is also a professionally interested discourse, wherein talk about “the evidence” is, in part, talk about jurisdictional control.

The development of evidence-based medicine in the late twentieth century depended in large part on the intuitive appeal of the randomized, controlled trial, developed mid-century as a means of determining which medical interventions work and which do not. Innovations such as blinding, randomization, and placebo controls imbued the RCT with an ethos of disinterestedness that appealed to proponents of EBM, who sought to establish a new kind of medical practice in which “best practice” was rooted in an empirical evidence base, not in the experience and intuition of individual practitioners. First advanced formally in 1992, EBM was defined in Sackett and colleagues’ landmark 1996 essay as “the conscientious, explicit, and judicious use of current best evidence in making decisions about the care of individual patients” (71). This movement, which has spurred the creation of vast research networks, crucially undergirds the push for the large-scale testing of CAM practices such as those published in the *JAMA-Archives* theme issues.

EBM’s influence has been controversial but widespread. For example, physicians Alvan Feinstein and Ralph Horwitz worry about the implications of EBM for practice. In principle, EBM makes perfect sense: who would not want to practice (or receive) medicine that is shown to work? However, EBM is problematic in multiple ways, ranging from upstream, epistemic concerns (e.g., What counts as “current best evidence”? Where does that evidence come from? Who judges it, and according to what criteria?) to downstream concerns in the realm of practice (e.g., How ought one to proceed in the absence of evidence? Will EBM lead to an algorithmic or

“cookbook” medicine? What was clinical medicine before it became “evidence-based”?).²⁸

Despite these areas of concern, Feinstein and Horwitz note, EBM has “acquired the kind of sanctity often accorded to motherhood, home, and the flag” (529). Similarly, sociologists Eric Mykhalovskiy and Lorna Weir write: “Recently, a colleague, remarking on the enthusiasm for evidence within health care, noted that we live in a time of ‘evidence-based everything’” (1060).

The emphasis on evidence-production in the *JAMA-Archives* corpus creates the journals’ own occasion for speaking: collectively, the articles advance the case that evidence on CAM is needed, while they simultaneously answer the call for evidence within their pages. Importantly, both the call for evidence and its response came from biomedicine’s side of the biomedicine-CAM boundary—CAM practitioners tend not to rely on trial evidence in their practice (see, e.g., Villanueva-Russell), and patients seek CAM regardless of whether or not scientific evidence is available (Astin). Medical historian George Weisz maintains that EBM is an important mechanism for “defending medical authority from a variety of contemporary threats,” including insurance companies, patient demands, and CAM practitioners (388). In boundary work, EBM can adjudicate on who belongs and who does not.

EBM also helps researchers working on CAM to assert their membership within the medical profession. By acknowledging their adherence to the methods that are the hallmark of EBM (e.g., randomization, blinding, placebo controls), triallists can identify themselves as insiders even despite their work on outsider interventions. Hay, Jamieson, and Omerod, for instance, seek approbation in *Archives of Dermatology* for the design of their RCT of aromatherapy for alopecia areata. They point out, somewhat self-consciously, that, in addition to their study’s strong positive finding, they “also successfully applied an evidence-based method to an alternative therapy” (1349). CAM’s lack of evidence is often attributed, if implicitly, either

²⁸ An excellent primer on these questions is Wahlberg and McGoey’s 2007 special issue of *BioSocieties* on EBM.

to defects in the interventions themselves or the neglect of their practitioners (see, e.g., Angell and Kassirer). Even James Dalen, in his sometimes scathing critique of the medical profession's efforts to buttress its boundaries, situates his editorial in the corpus issue of *Archives of Internal Medicine* squarely within an EBM framework. He identifies himself as an adherent to the EBM model while at the same time he offers a reasoned account of why dismissals of CAM on grounds of it being unscientific are problematic. Dalen's own take on the paucity of evidence on CAM is apolitical: "The reason that most unconventional therapies are not evidence based as currently defined is that most of them were introduced long before (in some cases centuries before) the advent of the randomized controlled clinical trial" (2180).

To consider EBM more fully in its capacity as a profession-defining measure, rather than as simply a disinterested model of evidence-production and clinical decision-making, I now turn briefly to two of its historical antecedents. These measures, the AMA's Consultation Clause and the enactment of state-level Basic Science laws, were adopted by the profession ostensibly in the service of patients but they were rooted also in concerns about jurisdiction. Here, I want to install the episode of boundary work precipitated by the *JAMA-Archives* corpus within the context of these earlier efforts in order to highlight its lineage in the ongoing process of boundary work. Although the corpus, taken as a whole, promises that evidence will solve the question of CAM's validity once and for all, new exigencies for boundary work will always inevitably follow—such exigencies are a part of the normal life of the professions.

In the eighteenth century, the medical marketplace operated with a Wild-West mentality, with disparate groups of healers competing openly for patients. Proto-professionalized practitioners, those that were members of a guild and practiced outside of their homes, occupied a position relatively equal to that of their competitors, such as midwives and bonesetters.

However, by the 1800s, their status rose dramatically as they ramped up efforts to organize themselves into a full-fledged profession. Efforts at professionalization were resolutely challenged by a public that viewed healers of any kind with suspicion, and even by some physicians themselves. The ranks of physicians were deeply divided by sectarianism, which Starr describes as the “most virulent,” divisive force in medicine during the second half of the nineteenth century (94), when even faculty colleagues at medical colleges were frequently not on speaking terms. Some of these quarrels were fuelled by philosophical differences; others, by turf wars. During this period, sectarians such as homeopaths and regular physicians were often educated at the same institutions, although with different *materia medica* and clinical training.

Toward the end of the nineteenth century, regular physicians prevailed over sectarians as the dual forces of urbanization and science re-shaped society. The public relied ever more on the skills of professionals because, Starr notes, “the less one could believe ‘one’s own eyes’—and the new world of science continually prompted that feeling—the more receptive one became to seeing the world through the eyes of those who claimed specialized, technical knowledge, validated by communities of their peers” (19). This transformation took place on a grand scale in the United States; in medicine, it was helped along by key maneuvers such as the Consultation Clause and Basic Science laws, both of which sought to impede the activities of competing health practitioners.

When the AMA was founded in 1847, one of its members’ goals was to give physicians greater control over their profession and edge out competition in an otherwise unregulated medical marketplace. One of its initial legislative acts was to adopt the now-infamous Consultation Clause within its code of ethics. This clause aimed at preventing members from associating in any professional context with outside practitioners: “no one can be considered as a

regular practitioner, or a fit associate in consultation, whose practice is based upon an exclusive dogma, to the rejection of the accumulated experience of the profession” (qtd. in Whorton, *Nature Cures* 69). Such exclusionary provisions had been immensely successful in the UK, where practitioners feared ostracism for consulting with irregulars,²⁹ although the Consultation Clause did not have as powerful an effect in the US. Whorton suggests that, while restrictions on consultation appealed to Britain’s richly stratified society, the US’ populist leanings aligned public sympathy with the irregulars—and with patients’ right to choose among practitioners. The clause was difficult to enforce and by 1903, support had withered. It was replaced by an “advisory document” that discredited irregular practices but stipulated that physicians must consult with whomever necessary in their patients’ best interest. Prohibitions on consultation with irregulars nevertheless remained one of the few tools available to the AMA to restrict competition, and similar bans were enforced against osteopaths in 1938 (see Baer) and chiropractors in the 1960s (see Villanueva-Russell), the latter ban resulting in a protracted but eventually successful antitrust lawsuit against the AMA and co-defendants by five chiropractors, launched in 1976 and concluded in 1992.

The enactment of independent Basic Science laws across twenty-three states between 1925 and 1979 also sought to weed competing practitioners out of the health care market. These laws required all health care practitioners to demonstrate basic knowledge of the biological sciences prior to licensure. In his historical account of Basic Science board examinations, Norman Gevitz argues that gatekeeping was one of their primary functions. However, he notes,

²⁹ In one extreme case, when former British Prime Minister Benjamin Disraeli fell gravely ill in 1881, he was initially treated by a homeopath but his aides entreated Richard Quain, a specialist in bronchial disorders, to examine him. Quain refused the request, however, claiming that the College of Physicians would expel him for practicing alongside an irregular. He only agreed to treat Disraeli at Queen Victoria’s command. Despite the Queen’s orders, Quain’s acquiescence ignited a long political and professional debate within the College in the months after Disraeli’s death. This example is based on my own archival work on the Hughendon Collection, a Disraeli archive at Queen’s University in Ontario, but see also Kidd.

while the examinations were meant to keep out practitioners such as chiropractors, up to a third of US-trained MDs and over half of foreign-trained MDs failed the exams as well (59). The laws were eventually repealed, the first in 1967; the last, in 1979. Whorton suggests that, like the AMA's Consultation Clause, these laws may have backfired on regular medicine: instead of forcing practitioners deemed unqualified to abandon the health care field, the laws "forc[ed] irregulars to sink or swim. They chose to swim, and that meant they had to elevate the level of instruction they provided their students in medical science" ("Cultism to CAM" 232).

Both of these exclusionary measures, the Consultation Clause and Basic Science laws, sought to differentiate practitioners of mainstream medicine from those that do not share the profession's allegiance to scientific values. As Whorton suggests, these measures may have spurred alternative practitioners to action, to organize themselves to meet the scientific demands placed on them. However, the profession's recourse to such measures bespeaks a fundamental insecurity about its own jurisdiction over matters of health and illness, although that concern operates in disguise—in the case of the Consultation Clause, it was disguised, thinly, as concern for patients, while in Basic Science, as concern about scientific literacy. The rise of the RCT follows a similar trajectory, in which appeals for "solid evidence" on individual practices (Fontanarosa and Lundberg, "Alternative Medicine") are framed as concern about safety and efficacy but they serve also professional interests. As Ted Porter notes, quantitative measures "work mainly as social technologies, not guides to private thinking" (208).

As an example of what Ted Porter calls a "technology of trust," the RCT holds a nearly sacred status in EBM, its aura cast so widely that it has come to seem both natural and inevitable as a research methodology. The RCT emerged in the 1940s and 50s out of both the agricultural and statistical sciences but its acceptance in the medical world was slow, only gaining

widespread adherence in recent decades (Devereaux and Yusuf; T. Porter). The fabled first RCT was the 1948 British Medical Research Council trial of streptomycin for pulmonary tuberculosis, although some question its revered status as *first*.³⁰ This and other trials of the period exhibited a fledgling methodology that sought to apply a laboratory model to clinical research; this laboratory model promised to minimize bias and the effects of chance in evaluating medical treatments. The goal of the RCT method was to produce a body of data on specified health interventions that could be quantified through statistical analysis, providing researchers with accurate and reliable results.

The push for quantitative methods in medicine came not from practitioners—who resisted the idea of standardized approaches to care, viewing such standardization as a machination of legislators to curb their clinical authority—but from regulatory authorities. Ted Porter notes that regulators wanted to establish “uniform and rigorous standards” of practice, and so viewed physicians’ expertise, rooted in experience, as “a valuable and dangerous commodity” (206). Mid-century policy changes in the US helped sponsor acceptance of the RCT, particularly through two acts of Congress that expanded the regulatory purview of the Food and Drug Administration (FDA). The Federal Food, Drug, and Cosmetic Act of 1938 gave the relatively toothless FDA power to reject drugs deemed dangerous, although it still could not make determinations of efficacy. One result of this limitation was that many drugs on the market, though proven safe, had no real effect.³¹

³⁰ For example, Kaptchuk and Kerr dispute the “hagiographic history of the RCT,” particularly the canonization of the streptomycin trial, noting that strategies against bias (blinding, randomization, etc.) were not “magically introduce[d]” to medicine by the new statistical sciences but have a long history stretching back centuries and, in some cases, even millennia (250). They caution that epideictic accounts of the streptomycin trial ultimately strip the RCT of its complex social and political history, naturalizing it in the process.

³¹ Ted Porter points out that the FDA sometimes circumvented this restriction against making determinations of efficacy by rejecting inert drugs as dangerous on the grounds that they could be used in the place of effective ones.

While the experience of individual clinicians remained important to the FDA in its drug evaluations even after the 1938 Act, the research scene changed dramatically with the passing of the Kefauver-Harris Bill in 1962. Prompted by the Thalidomide scandal, the bill mandated that drugs be proved not only safe, but also effective, finally edging out clinical expertise and positioning the RCT as the ideal form of evaluation.³² Since that time, the emphasis in medical research has shifted from small RCTs with sensitive outcome assessment toward large-scale RCTs that focus primarily on “major clinical outcomes (e.g., death, stroke)” (Devereaux and Yusuf 106). These shifts in outcome measures reflect an overall shift in medicine toward ever cruder estimates of health and illness wherein, as Ted Kaptchuk argues, research becomes less about “emphasizing outcomes” than about “the purity of the means” of obtaining them (“Powerful” 1724). Shifting the question of *what* we know in medicine to *how* we know it, he explains, the RCT has thus come to be seen, both within the scientific community and beyond, as “medicine’s most reliable method for ‘representing things as they really are’” (“Double-Blind” 541). Yet the role of the RCT in evidence production in medicine can easily be overestimated, largely because of the RCT’s high position on the evidence hierarchy; this is particularly true in debates about CAM, where Sackett et al.’s reference to the “best available evidence” comes to mean almost exclusively evidence derived from RCTs, despite the authors’ own insistence that the highest-level *available* evidence at any time, such as evidence from observational studies, is grounds enough for evidence-based practice.

In their ideal form, once exclusion criteria have been applied, RCTs randomly divide experimental participants into groups that receive the therapy under study (“intervention” groups) and groups that do not (“control” groups). All participants in the intervention group

³² An “acceptable demonstration” of efficacy under this bill, notes Ted Porter, would be a “statistically correct experimental design, carried out by qualified practitioners and yielding a statistically significant difference between drug and placebo” (208).

receive the same treatment, regardless of their individual health history or concomitant conditions. Participants across both groups are matched to some extent for age, sex, and other characteristics triallists deem necessary. Participants are usually blinded to their intervention assignment, a precaution meant to prevent placebo effects, which occur when participants in the control group experience improvement despite having received only an inert simulation of the study intervention. Researchers are also usually blinded, to avoid bias in outcome evaluations and potential contamination from unintentionally communicated cues as to which group participants belong. (For more on RCT design, see Jadad and Enkin.)

Studies of CAM pose significant methodological problems because the practices do not generally translate easily into the “gold standard” RCT. Randomization and standardization are foreign concepts in health practices such as TCM and chiropractic, and often incommensurate with them. In contrast to biomedicine, these practices view patients as fundamentally unique, so two people with the same ailment might be treated altogether differently, depending on their unique constellation of symptoms and personal characteristics, such as height, weight, temperament, allergies, and health history (Barry 2647; Degele 118). Corollary to this emphasis on uniqueness is that treatments can be difficult to standardize in experimental settings: while biomedical treatment is largely symptomatic in the sense that a person may be treated separately for different complaints (even by separate specialists)—one pill for headache, say, another for constipation, and another still for irritability (these are Degele’s examples; 118)—CAM practitioners such as chiropractors and TCM practitioners would aim to address all symptoms together.³³

³³ I should clarify that my claim here is not that biomedical theory sponsors a one-size-fits-all approach to health care, or that CAM therapies are as individualized as their proponents suggest; these differences may be more apparent than real. See Chapter Three for discussion of models of practice in TCM and chiropractic.

Controlling and blinding studies of manual practices such as acupuncture and chiropractic are also difficult because their therapies include unmistakable physical actions (specifically, piercing the skin and moving the spine with an often audible popping sound respectively), which can be difficult to simulate. Pharmaceutical trials are usually controlled by substituting the active drug with a lookalike sugar pill; as a result, the studies are easy to blind because, once participants have been assigned to treatment or control groups, no one but the study's pharmacist need know to which group any participant is assigned. By contrast, controlling a practice such as acupuncture is more difficult because there is no available control that is both realistic and definitely inert (à la sugar pill), and practitioners usually cannot be blinded. These methodological problems leave researchers to puzzle out how such studies ought to be conducted, interpreted, and incorporated into practice. The question for biomedical researchers of how, then, to proceed with testing CAM in an EBM framework is nested in a complex web of factors—disciplinary, professional, epistemic, generic, philosophical, commercial, regulatory, and more.³⁴

In the context of EBM, the data provided by RCTs on the safety and efficacy of individual interventions are reified through their aggregation in systematic reviews, such as those of the Cochrane Collaboration, and in clinical practice guidelines. The metadata these aggregative methods produce become the firm ground upon which clinical recommendations are made within the EBM framework. The Cochrane Collaboration is a worldwide network of volunteers that produce and publish systematic reviews on all manner of health topics; each review evaluates the current state of evidence and makes recommendations for practice based on available data. In the corpus issue of *JAMA*, Ezzo and colleagues discuss the implications of the

³⁴ Evelleen Richards' study of the Vitamin C-cancer controversy pre-dates EBM's rise but provides a detailed historical analysis of such factors at work in research and medical cultures.

Cochrane Collaboration for CAM, noting that limitations on the availability and quality of data can skew evidence against interventions that may, in practice, have higher levels of safety and efficacy than trial data show. Ezzo et al. cite several factors that limit the comprehensiveness of such reviews, such as publication bias, which they argue might result in disproportionately high rates of publication of CAM studies with negative findings as compared to biomedical studies with negative findings. A further problem with availability of RCT-derived evidence on CAM is that trials themselves are hard to come by, particularly because they are difficult to fund without industry-based sponsors.³⁵

Given these limitations on the quantity and quality of RCT evidence on CAM (and there are more; see Chapters Two and Three), the call in biomedicine for rejection of practices without “solid evidence” can be used to shift boundary lines in biomedicine’s favour. It is in this way that EBM can function in biomedical discourses on CAM as an exclusionary measure on the order of the AMA’s Consultation Clause and the Basic Science laws. All of these measures were adopted, in the first instance, in the name of medical science and patient care, but each has served the interests of the medical profession at the same time. In the context of the *JAMA-Archives* corpus, EBM is the point-of-access on the biomedical border: any intervention can get in, *JAMA*’s editors suggest, provided that it has evidence to support its entry. But what EBM’s reliance on quantification as a “technology of distance” glosses over is the role that members of the profession play, wittingly or not, as attendants at the gate.

Writing on the rhetoric of the scientific article, Herbert Simons writes that “researchers are not free agents” (“Research Report” 152). Rather, he argues, “they are impelled and

³⁵ Commentators such as Held, Wedel, and Wilhelmssen and Angell identify persistent public funding shortages as a limitation of medical research generally, where funding from pharmaceutical companies has become so integral that important studies of older, off-patent drugs and newer, non-blockbuster drugs are simply not being conducted, despite the need for relevant data. I think we can usefully extend this observation to include studies of CAM, which offer relatively little patent control and profit potential.

constrained by the conventions of their disciplines, by the norms and counternorms of their professions, and by political and economic pressures coming from the larger society” (152). Take, for instance, the constraining force of the genre of the clinical trial report, the IMRaD structure (Introduction, Methods, Results, and Discussion). In this genre, there appears to be no space for anything but the straight facts. In the trial reports on acupuncture and yoga that I discuss above, for example, the fact that the authors do not reflect substantively on the philosophical implications of their research may not be surprising, given the generic constraints within which they write. However, the genre does more than marshal its authors’ words into a recognizable format: it abstracts the interventions under study from their usual contexts of delivery and re-makes them in biomedical terms. This process, I suggest in the next chapter, serves a central argumentative function in debates about the legitimacy of health practices and practitioners because the evidence produced in EBM can tilt the course of arguments variously to defend or expand professional boundaries.

In this chapter, I have argued that, while many of the commentators to the *JAMA-Archives* corpus contend that the debate about CAM can be settled with enough evidence, that evidence is inherently dependent on the communities that produce it. Given biomedicine’s greater rhetorical resources as the dominant model in the health care ecosystem, the evidence it produces can shore up its authority and expand its jurisdiction to health practices formerly beyond its scope. In his editorial in the corpus issue of *JAMA*, Wayne Jonas, then-head of the Office of Alternative Medicine (which was about to become the National Center for Complementary and Alternative Medicine), reflects on this process:

Historically, orthodox medicine fights [CAM] practices vigorously by denouncing and attacking them, restricting access to them, labeling them as antiscientific and quackery,

and imposing penalties for practicing them. When these therapies persist and even rise in popularity despite this, mainstream medicine then turns more friendly, examining them, identifying similarities they have with the orthodox, and incorporating or ‘integrating’ them into the routine practice of medicine. (1616)

Writing on competition in the medical marketplace in the seventeenth and eighteenth centuries, Roy Porter is more direct, if cynical, and he sums up one of this dissertation’s central claims: “quackery never prospers, for if and when it does, it becomes termed medicine instead” (207).

CHAPTER TWO

Scientific Method as a Rhetorical Topos³⁶

In their editorial accompanying the *JAMA* theme issue, editors Phil Fontanarosa and George Lundberg stake their claim in the debate over complementary and alternative medicine. The position they assume encapsulates what I described in the previous chapter as a virtually invisible process, wherein biomedical professionals' explicit and self-conscious efforts at defining their profession's borders are seamless, couched within an evidence-based terminology that defines CAM only in residual terms:

There is no alternative medicine. There is only scientifically proven, evidence-based medicine supported by solid data or unproven medicine, for which scientific evidence is lacking. Whether a therapeutic practice is 'Eastern' or 'Western,' is unconventional or mainstream, or involves mind-body techniques or molecular genetics is largely irrelevant except for historical purposes and cultural interest. We recognize that there are vastly different types of practitioners and proponents of the various forms of alternative medicine and conventional medicine, and that there are vast differences in the skills, capabilities, and beliefs of individuals within them and the nature of their actual practices. Moreover, the economic and political forces in these fields are large and increasingly complex and have the capability for being highly contentious. Nonetheless, as believers in science and evidence, we must focus on fundamental issues—namely, the patient, the target disease or condition, the proposed or practiced treatment, and the need for convincing data on safety and therapeutic efficacy. (Fontanarosa and Lundberg, "Alternative Medicine" 1618)

³⁶ A version of this chapter has been published. Derkatch, Colleen. "Method as Argument: Boundary Work in Evidence-Based Medicine." *Social Epistemology* 23.4 (2008): 371-88.

There is much one could say about this editorial statement from the perspective of boundary work. It creates sharp lines of community membership, for instance. The final sentence, governed by the group-defining modifier “as believers in science and evidence,” implies that those excluded from the “we” category of the main clause do not, as a matter of course, value patients, targeting diseases, or the safety and efficacy of their treatments. Framed in the terms of Audience Design, the theory of how speakers assign hearers their various roles, we might read that sentence as saying something like: “we, those of us that read and publish in *JAMA*, believe these things, while other people, *out there*, do not necessarily.”³⁷ Framed similarly but in more conventionally rhetorical terms—Burkean terms—we could say that this final sentence fosters *identification* among its readers as “believers” (in science and evidence) and illuminates their *discontinuity* with those that do not share such beliefs.³⁸

This passage also casually neutralizes a contentious subject, substituting a whole discourse—encapsulated here through the expansive binaries of East/West, unconventional/mainstream, mind-body/molecular—with a new, single binary: proven/unproven. In this process, the editors underestimate the role that extrascientific factors play in shaping biomedical boundaries, including conceptions of “Eastern” versus “Western,” different philosophies of health and illness, patient behaviours and preferences, economics, politics, regulatory bodies, and variations in professional orientation and skills. Of course, such a neutralizing quality is a common feature of both medical and scientific writing (see Segal,

³⁷ See Clark and Carlson, who theorize the ways in which speakers “design their utterances” to situate their listeners as, variously, the persons directly addressed, members of the addressee’s community, and overhearers (217). This “we” construction produces a defined community of addressees and simultaneously creates a category of not-addressed readers invited to “overhear” the conversations taking place in the journals.

³⁸ Here is Burke’s quintessential definition of *identification*: “A is not identical with his colleague, B. But insofar as their interests are joined, A is *identified* with B. Or he may *identify himself* with B even when their interests are not joined, if he assumes that they are, or is persuaded to believe so. Here are ambiguities of substance. In being identified with B, A is ‘substantially one’ with a person other than himself. Yet at the same time he remains unique, an individual locus of motives. Thus he is both joined and separate, at once a distinct substance and consubstantial with another” (*Rhetoric of Motives* 20-21).

“Strategies”); in this instance, however, it merits further reflection because it draws attention to the conditions in medicine that make it possible to introduce with such a clear-cut approach a collection of potentially controversial editorials and methodologically challenging studies. The editors make the problem sound so simple, as though the researchers’ work were straightforward: to prove which treatments work and which do not, and then they will be able to do away with the whole alternative medicine debate once and for all. This is a much-repeated refrain in the medical literature on CAM, which I suggested in the previous chapter functions, in part, as a strategy of exclusion. The present chapter argues that the very notion of proving/disproving is itself a significant source of strain on twentieth- (and twenty-first-) century medical research and practice.

At the core of the November 1998 *JAMA-Archives* theme issues is a debate about how research on unconventional health practices ought to be conducted, interpreted, and incorporated into conventional practice. As I noted in Chapter One, the “gold standard” of biomedical research, the randomized, double-blind, placebo-controlled trial (RCT), represents to members of the field their highest standard of knowledge-production. The RCT’s position of prominence has been bolstered, particularly, by the ascendancy of evidence-based medicine (EBM), which privileges RCT-derived data on the safety and effectiveness of health interventions over other sources of data. The RCT model poses considerable difficulty for the scientific testing of CAM interventions because they are not easily randomized, generalized across populations, or controlled. This difficulty of testing, which stems from their incommensurability with biomedicine both in theory and practice, is one of the central issues around which the contributors in the *JAMA-Archives* theme issues align themselves.

In this chapter, I study the problem of method in scientific research on CAM as a fundamentally rhetorical problem, situated within a boundary drama, and deeply rooted in the discursive practices of science and medicine. Importantly, not even mainstream biomedical research is conducted entirely through RCTs—observational and qualitative studies also fill the pages of medical journals—so the RCT itself seems to function in the corpus as a rhetorical *topos* (line of argument). As philosopher of science Kirstin Borgerson reports, the overwhelming majority of biomedical interventions in North America have not been tested by RCT. “Research into alternative medicine is required to meet the highest standards,” she notes, “even though many currently accepted medical practices have not met (and may never meet) those same standards” (506 n1). The *topos* of method, I argue, positions the research variously within and beyond scientific borders, depending on the researchers’ own attitudes toward CAM.

I explore the argument from method through analysis of two acupuncture RCTs reported in *JAMA*, and through five discourse-based interviews with expert readers about those studies’ methods and results. I begin by outlining my own methodology. In the second section, I contextualize the *topos* within the overlapping orbits of EBM, RCT design, and CAM, whose points-of-contact give shape to the argument from method. The third section examines the genre of the experimental article as an element itself in what comes to count as EBM; this section examines how that genre is mobilized in EBM-oriented CAM research. Reports of clinical trials are modelled on the conventional publication genre of the sciences, the IMRaD format (Introduction, Methods, Results, and Discussion); because this structure conditions our beliefs about how science and medicine are conducted (and, importantly, *not* conducted), it can illuminate how method can be invoked rhetorically. Finally, the fourth section isolates the concept of *efficacy*, whether or not a health intervention “works,” as a central organizing

principle of biomedical research on CAM. While the determination of efficacy appears to follow inevitably from the RCT methodology, this final section suggests that the process of determination itself is loaded with rhetorical power that can have significant implications for biomedical boundary work.

Biomedical research on CAM, I want ultimately to suggest, precipitates what Bertolt Brecht called (in a context unrelated to either medicine or rhetoric) the *Verfremdungseffekt*—the “alienation effect”: it makes the normally tacit procedures of medical research “strange” and, as a consequence, more readily open to inspection.³⁹ Studies of CAM bring the whole biomedical research apparatus into question, particularly because, while there is clearly knowledge to be gained from studying CAM, there is no established method of proceeding. Observing researchers as they attempt to perform effective and rigorous research on CAM, looking at how they design their studies and interpret their results, can offer a glimpse into the epistemic machinery of medicine as a whole. As medical ethics and policy specialist Haavi Morreim notes, “Any attempt to throw out or discredit CAM on grounds of scientific inadequacy is sure to toss out large portions of conventional medicine alongside. To ‘hold’ both to ‘the same’ standards appears to bode far worse for medicine than for CAM” (228).

I ought to note at the outset that, while rhetoric, as a theoretical and pedagogical practice, is not about unmasking per se, this chapter will do a little unmasking in service of a larger purpose, which is to illuminate some of the disconnections between how medical researchers and practitioners (and policymakers, the media, and the public) think about the conduct and value of medical research, and how that research tends actually to unfold. I do not mean to suggest,

³⁹ Brecht’s well-known “alienation effect” is precipitated in dramatic performance by the collapse of the so-called fourth wall, the one separating performers from the audience. By collapsing this wall, Brecht contended, the theatrical illusion would be broken and viewers, unable to lose themselves within the fiction, would be forced to think critically about what they were seeing.

however, that these theory-practice disconnections are by any account intentional or deceptive in nature; indeed, they seem to be at least partly functional, making the everyday work of medicine possible. Rather, I want to show that the RCT's attendant value system is part of the professional fabric of medicine itself. By illuminating this disconnection between theory and practice in research, we can map some of the epistemological dimensions of rhetorical boundary work in biomedicine.

Texts and Methods

The *JAMA-Archives* corpus contains thirteen original articles on studies conducted in the RCT format. Four of the trials evaluated orally administered dietary supplements (Bensoussan et al.; Heymsfield et al.; McCrindle, Helden, and Conner; Melchart et al.), three evaluated acupuncture (Cardini and Weixin; Shlay et al.; White, Resch, and Ernst), and the remainder evaluated, respectively, chiropractic, topical lanolin, yoga, aromatherapy, relaxation exercises, and homeopathy (Bove and Nilsson; Brent et al.; Garfinkel et al.; Hay, Jamieson, and Ormerod; O'Connor et al.; Smolle, Prause, and Kerl respectively). Five of the interventions studied were found to be at least partly effective (Bensoussan et al.; Brent et al.; Cardini and Weixin; Garfinkel et al.; Hay, Jamieson, and Ormerod). The stringency of the trials' methods, including randomization, blinding, and use of placebo controls, correlates with the interventions' modes of delivery: trials of interventions that can be administered somewhat like pharmaceuticals (e.g., aromatherapy, dietary supplements) are randomized, double-blinded, and placebo-controlled; while those that require practitioners and/or participants to engage in specific behaviours that vary by group assignment (e.g., acupuncture and chiropractic; relaxation and yoga) feature only partial blinding and limited use of controls. Trial populations ranged from 30 to 302 participants,

with a mean of 116 and a median of 76. All of the primary authors are biomedically trained, most with MD and/or PhD degrees, and only two studies feature as co-authors credentialed CAM practitioners (Bove and Nilsson; Shlay et al.).⁴⁰

From these thirteen articles, I selected two for extended analysis based on several criteria relating, first, to the intervention under study and, second, to the trials' own methods and results. I focused on acupuncture because it is both well-studied and necessitates a methodologically complex approach in research contexts. It is regarded by biomedical personnel as one of the most effective CAM interventions (Astin et al.) and as supported by more substantial and compelling data than other CAM interventions (Hsu and Diehl; Shang). Biomedical credibility seems to be an important asset for a CAM practice in the context of boundary work because, without that credibility, even the most rigorously designed studies might easily be dismissed on that ground alone.⁴¹ Acupuncture's credibility within in the mainstream medical community has been bolstered, especially, by emergent data on its effectiveness for pain control ("NIH Consensus"). And yet, despite its thirty-year history of biomedical evaluation, acupuncture continues to present considerable challenges to researchers because its underlying philosophy and its mechanism of action, the flow of qi ("chi," or energy) along meridians accessible by acupuncture needles, are relatively incommensurable with biomedical theory. Unlike trials of dietary supplements, which are comparatively more amenable to the RCT model, trials of acupuncture provide a window into the workaday elements of biomedical boundary work because their

⁴⁰ Bove and Nilsson are both Doctors of Chiropractic with biomedical PhDs and faculty appointments in medical schools (Harvard and Odense Universities respectively); Nilsson is also an MD. Shlay et al. coauthor Bob Flaws holds a Diplomate in Acupuncture (Dipl Ac) and is a well-known author and speaker on TCM.

⁴¹ Nina Degele argues, for example, that this is typically the case with studies of homeopathy and parapsychology, where the phenomena under study are so scientifically implausible that even the most stringent trial design cannot overcome the skepticism of mainstream scientific readers (111). See also note 66, below.

methodological challenges allow us to trace the accommodations researchers must make to fit their studies within accepted research standards.

Of the three acupuncture trials, I limited the number of my case studies to two for pragmatic reasons: my discourse-based interviews required that participants read and respond to the studies during one-on-one interviews, the length of which I wanted to cap at a maximum of one hour in order to attract participants; to allow for sufficient coverage of each article during that time, I chose to work with two rather than all three. At the beginning of the article selection process, I set several criteria, including trial size, duration, use of controls, outcome measures, and results, which were best met by the Cardini and Weixin and Shlay et al. studies. They feature the second and third largest trial populations of the entire corpus (260 and 250 respectively; the third acupuncture RCT, White, Resch, and Ernst's, had 76). Because larger trial populations generally allow for higher statistical power and greater sensitivity regarding treatment effects, the two larger studies seemed more likely to produce clinically significant results (see Jadad and Enkin for more on trial design and population size). For each of the other selection criteria, the two studies complement each other: Cardini and Weixin's trial was short, simply controlled, assessed by objective measures, and had a positive result, while Shlay et al.'s was longer, multi-armed, subjectively assessed, and negative. I summarize each study's design briefly here, and discuss them in further detail in the sections that follow.

Cardini and Weixin investigated the technique of moxibustion for the reversal of breech pregnancies. Moxibustion is the practice of burning rolls of *Artemisia vulgaris* (mugwort) on acupuncture points to stimulate the flow of qi, and has long been the standard treatment in TCM for breech presentation. Cardini and Weixin's fourteen-day, unblinded trial was conducted in Chinese maternity clinics. Participants in the intervention arm were treated by their partners

every day between the thirty-third and thirty-fifth weeks of pregnancy by burning a *moxa* roll on an acupoint on the outside of the right pinkie toenail; participants in the control group received only routine maternity care. The study's primary outcome measures were for cephalic (head-first) presentation at thirty-five weeks gestation, checked by ultrasound, and at birth. The results of the study were largely positive, leading the authors to conclude that moxibustion was effective for reversing breech presentation.⁴²

Shlay et al. compared acupuncture with both a placebo intervention and the current pharmaceutical standard of care, amitriptyline, for the treatment of HIV-related peripheral neuropathy (pain/sensitivity/numbness in the extremities). This fourteen-week, multicentre study was conducted at HIV primary care centres in ten US cities. It featured a modified factorial design with multiple treatment arms: one arm compared a standardized acupuncture intervention to a sham placebo acupuncture treatment (N=114); one compared amitriptyline to a placebo pill (N=11); and one featured a factorial option with four sub-categories of treatment comparing all combinations of acupuncture, placebo acupuncture, amitriptyline, and placebo amitriptyline (N=125).⁴³ Outcomes were assessed through subjective patient reports via pain diaries and pain scores. The study's results showed that neither acupuncture nor amitriptyline were more effective than placebos for HIV-related pain in the extremities.

My analysis of these two articles is augmented by discourse-based interviews with five health professionals from three areas of expertise: two medical researchers, two practitioners of Traditional Chinese Medicine, and one physician-clinician. The two medical researchers, both experts in trial design, worked at a hospital-based, multidisciplinary centre for clinical

⁴² At 35 weeks, 75% of fetuses in the intervention group were cephalic compared to 48% in the control group; at birth, 76% in the first group were cephalic compared to 58% in the second group.

⁴³ A. acupuncture + amitriptyline (n=32); B. placebo acupuncture + amitriptyline (n=33); C. acupuncture + placebo amitriptyline (n=31); D. placebo acupuncture + placebo amitriptyline (n=29).

epidemiology, which, in addition to its own research mandate, offered a consulting service on trial design and evidence interpretation. Dr Clarke, the centre's director, is a well-published, senior-level researcher on respiratory medicine and health literacy, while Dr Fournier is a junior researcher with expertise in pharmacoepidemiology. The TCM practitioners also span the career spectrum, and both maintain active clinical practices. Dr Yao holds a PhD in Education from a prominent Canadian university, is the founder and president of a major metropolitan school of TCM, and is a distinguished translator and author of TCM-related materials. Dr Connolly is a Western-born, early-career practitioner at a TCM clinic specializing in reproductive health. The physician-clinician, Dr McDonald, is a mid-career doctor maintaining a busy family practice and specializing in obstetrical care, including labour and delivery.⁴⁴

The purpose behind the interviews was to gain a sense of how readers with different professional orientations might respond to the studies' methods and designs. Although the sample size is too small to draw generalizations about attitudes held by *populations* of readers, the responses elicited do offer some insight into the different kinds of concerns individual readers bring to scientific texts that explicitly cross boundaries. The interviews were modelled on Odell, Goswami, and Herrington's discourse-based interview format, in which participants read aloud from a specific text and report, in real time, on their processes of writing or reading, depending on whether the texts were self-authored or not. In this case, the interviews unfolded in two stages for each article without a predefined set of questions. For the first stage of each article, participants read aloud from one excerpt from the Methods section and one from the Results section, and were asked to interrupt their reading at any time to comment if something

⁴⁴ All names have been changed. Potential participants were identified by referral and through staff listings on clinic websites. Participants were recruited via letter and response rates varied: both researchers, two of six TCM practitioners, and one of fifteen physician-clinicians initially contacted offered to participate. A second, wider round of physician-clinician letters produced no further participants. This study received institutional ethics approval (#H05-0831); see Appendix.

struck them as noteworthy. Any such commentary was further explored through general prompts such as “Would most people in your field agree with/say that?” as well as prompts particular to each reader’s area of expertise.⁴⁵ The second stage for each article focused on the article as a whole, prompted with the question, “Are there any other aspects of the articles that you would like to comment on?” Each interview lasted about an hour and was concluded when the participant had no further comments or concerns. The participants were not aware of the articles’ inclusion in the *JAMA-Archives* theme issues on CAM until I debriefed them at the end of the interviews.

RCT, EBM, and CAM: Ingredients of an Argument

The method topos is founded on the RCT’s entrenchment as the principal source of evidence in EBM. Methodology has long functioned as a “rhetorical resource” in scientific boundary work (Yeo 261) but the positioning of CAM within an EBM paradigm further expands its argumentative scope as it reaches across the diverse conceptual geographies of science, medicine, and alternative approaches to health. This section explains how CAM practices resist the RCT method, and how researchers such as Cardini and Weixin and Shlay et al. design their RCTs to frame them within accepted evidence-based standards.

A recent *New York Times* article encapsulates what I argue in this chapter is a deep ambivalence in the medical literature about methodology and evidence. In that article, Gary Taubes asked, “Do we really know what makes us healthy?” Using the case of Hormone Replacement Therapy (HRT) as an example, Taubes critiques the evidence used for public health recommendations, noting that today’s recommendations often wind up as tomorrow’s

⁴⁵ Other sample prompts: “Does the [study design/result] meet your expectations for a clinical trial?” (medical researcher); “Is this how most of your colleagues would treat this condition?” (TCM practitioner); “Do the results seem relevant to your own treatment of this condition?” (physician-clinician).

prohibitions.⁴⁶ Despite his article's title, Taubes is perhaps less interested in *whether* we really know what makes us healthy than in *how* we know it. He cautions readers against putting too much stock in either observational or experimental studies, both of which are more problematic than they first appear, as he emphasizes throughout his essay. Ultimately, though, he favours an experimental approach: marshalling the words of EBM father figure David Sackett, Taubes frames HRT as an object-lesson in the “disastrous inadequacy of lesser evidence,” such as that from the original HRT recommendations, derived from observational studies (n.p.). Taubes urges readers, in the end, to “remain skeptical until somebody spends the time and the money to do a randomized trial”—this, despite his earlier warnings about the potential deficiencies in the data produced by such trials.

Taubes' argument that observational studies can only suggest correlations between health interventions and outcomes but RCTs can give more concrete answers is uncontroversial. What is so interesting about his article is how elastic his notion of evidence is: Taubes is able to argue, in virtually the same breath, that we *cannot* trust the evidence from either kind of study and that we *can* trust the RCT evidence. It is as though he invokes, silently, the ubiquitous evidence hierarchies of the EBM literature, which rank the quality of evidence from RCTs and systematic reviews at the top and that derived from observational and other nonexperimental studies at the bottom.⁴⁷ That is, the relative stability of RCT evidence shifts in Taubes' article: the RCT, on its own terms, is given the warts-and-all treatment but, as the ranking methodology within the larger

⁴⁶ HRT had long been prescribed widely to menopausal women for prevention of heart disease and osteoporosis, based on data from several large, long-term observational studies, such as Harvard's Nurses' Health Study. Taubes estimates that, by 2001, some 5 million women took HRT preventatively. In 2002, however, the National Institutes of Health's Women's Health Initiative suspended its long-term RCT of estrogen and progestin, initiated in 1993, when participants in the intervention groups developed statistically significant higher rates of heart disease, breast and other cancers, stroke, and other negative outcomes (Writing Group). Millions of women stopped the therapy immediately. Since that time, the HRT tide has shifted several times and opinion is still divided as to its preventative effects and potential harms in different populations of women.

⁴⁷ Systematic reviews are meta-studies of RCTs. Typical evidence hierarchies can be found in Committee (97-98), Devereaux and Yusuf, and Grimes and Schulz.

context of evidence-production, it gains a kind of concrete authority. This shift is not surprising because, as Uffe Jensen has noted of EBM, “what is accepted as evidence always depends on ontologies enacted in a particular context. Different ontologies will embody or imply different standards” (104). By extension, in rhetorical terms, different evidence is differently persuasive in different contexts.

The kinds of evidence considered admissible in biomedical debates about CAM vary depending in large part on the context of the debate. In mainstream medical journals, those writing on CAM frequently occupy an ambivalent position: attentive to diverse ways of understanding and assessing health in accordance with the rhetoric of CAM (recognizing, for example, the value of both quantitative and qualitative evidence) but inattentive to that diversity when installing CAM into the discourse of biomedicine (such as relying solely on quantitative evidence). This privileging of certain kinds of proof within research contexts appears to be systemic, with researchers constrained by the genres of their own practice (especially the genre of the experimental report). This tension about evidence stands in particularly sharp relief when the clinical trial reports and systematic reviews of the *JAMA-Archives* corpus are read against other items published alongside them, such as editorials, historical essays, and letters. While each of the ten journals treat the subject of CAM with varying degrees of attention, all share a common trait, which is that the attitudes toward CAM evinced in the editorial materials importantly do not reflect the attitudes demonstrated in the clinical trial reports and systematic reviews published alongside them. In fact, they are very different, representing different approaches to what counts as evidence in evaluating the efficacy of health interventions.

Judy Segal authorizes this kind of comparative work on medical journal articles when she notes that “editorial writings supply good evidence that the ‘neutral’ nature of scientific texts is a

matter neither of the basic inclinations of scientific authors nor the nature of their subject matter; the neutral style is a cultivated style which has become conventionalized in a particular forum for scientific writing: the scientific article” (“Strategies” 528). Segal shows that the beliefs about research evinced in experimental articles are not simply the product of objective methodologies, although they appear to be. Rather, scientific articles are, as Burke would say, part of the “tribal idioms...*developed* by their use as instruments in the tribe’s way of living” (*Language* 44; emphasis in original). Put another way, no less than editorial writings, experimental articles are (again using Burke’s terms) the “dancing of an attitude” (*Philosophy* 9). Editorials in medical journals certainly have their own generic constraints, but, as Segal points out, they are comparatively less restrictive genres. They permit greater candour, allowing scientific authors to reflect more openly on issues that affect their profession—in turn, perhaps, giving the reader a better sense of their “basic inclinations,” however varied those may be. In biomedical-CAM boundary disputes, analyzing editorial and research articles in relation to each other can help destabilize long-held beliefs about the methods adopted in the service of evidence production. Reading Cardini and Weixin and Shlay et al. through this lens of genre, I suggest, can help bring into focus what I am calling the topos of method.

In his lead editorial in *JAMA*, for example, Wayne Jonas, then-director of the NIH Office of Alternative Medicine, praises CAM practitioners as helping to “address the confusion and suffering that accompany disease” (1616); he encourages biomedical doctors to emulate CAM practitioners in that respect. This focus on biomedicine’s interpersonal shortcomings is repeated throughout the editorial material in the *JAMA-Archives* corpus: one set of authors argues that biomedicine has alienated patients with its inattention to mind-body relations (O’Sullivan, Lipper, and Lerner), while a second group gives “frustration” with biomedicine as the main

reason patients consult Traditional Chinese Medicine (Koo and Arain). Another author criticizes the cure-obsessiveness of biomedicine, noting that MDs “often forget” that patients cannot always be cured. He pinpoints the attractiveness of CAM as the result not of its own efficacy but of the “perce[ived] lack of caring evinced by many MDs” (Elpern 1473).

Tom Delbanco describes biomedicine as, in a sense, a victim of its own success. Ever since its early triumphs with “antibiotic ‘miracle drugs’ and the polio vaccine” (1560), Delbanco argues, biomedicine has struggled to deliver on its promise of cure, especially in the realm of AIDS and cancer research. Delbanco notes that, as a result, “Public confidence in biomedical science has fallen, and alternative medicine, with its primal message of hope, is currently filling a vacuum” (1560). Other editorial pieces in the *JAMA-Archives* corpus convey a similar attitude, which is that alternative medicine has something to offer patients that mainstream medicine cannot: individualized attention. This attention becomes, for many of the authors, an object of praise, and perhaps even envy. These authors’ speculation on why patients seek alternative medicine is not always accurate (patient dissatisfaction, for example, is not a strong predictor of CAM-seeking; see Astin). However, it does nevertheless reflect their willingness to think hard about biomedicine’s shortcomings. (How that stress on the interpersonal dimension of medicine translates into both research and practice contexts is the subject of Chapter Three.)

Contributors to the *JAMA-Archives* corpus also recognize in the editorial material that randomized controlled trials are not necessarily the most effective means of getting at what treatments work best in practice. For example, in “What Counts as Evidence?” physician Dan Richard tells the story of one of his patients who, unmoved by trial evidence that peppermint oil works for headache, was only persuaded to try it when Richard relayed his own wife’s success with the treatment. In Richard’s view, medical research must ultimately serve practice: “As

clinicians, we must ask ourselves how much evidence is enough to assist patients with their use of complementary medicines....What about you—does the [RCT] evidence convince you to try peppermint oil? Or are you, like my patient, persuaded more by the testimonial?” (599).

When it comes to actually conducting and reporting on research on CAM within an EBM framework, the evidence story is quite different: as the editor of *Archives of Surgery* notes in the 1998 theme issue, “in God we trust but everyone else must have data” (Organ 1153). What counts as “data” in the RCT reports and systematic reviews is far more restricted than the kinds of data praised in the editorial articles. For example, while the editorial authors willingly attribute the enormous success of CAM to practitioners’ increased attention to patients’ social and psychological needs, those needs are largely ignored in the research designed to prove (or, more commonly, disprove) its efficacy. The rhetoric of EBM is a largely epideictic rhetoric, where *evidence* is taken as a good in itself and the central premise seems simply to be “the more, the better.”⁴⁸ But it is a slippery rhetoric, too, because, while evidence in the abstract is lavishly praised, certain kinds of evidence are more praiseworthy in certain contexts than others. This brings us back to my example of Taubes’ article on HRT data, where he indicts all forms of evidence as inherently unreliable but then insists that the RCT (and only the RCT) can generate the kind of evidence we can rely on. Part of this slipperiness is context-dependent: RCTs, on their own, are rife with problems but, taken within the context of the evidence hierarchies advanced in the EBM literature, they are the best we have got—and, as such, they deserve praise. However, the role of the RCT in biomedical evidence production can easily be overestimated, largely because of its high position on the hierarchy relative to, for instance, observational

⁴⁸ Thinking along this line seems pervasive in medicine. On end-of-life decision-making, for instance, Judy Segal notes that “The problem of death talk is...in part a problem of rhetorical control. Yet it is seldom viewed that way. Rather, there is a notion in liberal medicine that ‘communication’ between doctors and patients/families needs to be *increased*, apparently on the principle that more talk of any kind is better than less” (*Health* 100; original emphasis).

studies. This is particularly true in debates about CAM, where Sackett et al.’s reference to the “best available evidence” comes to mean almost exclusively that derived from RCTs, despite the authors’ own insistence that the highest-level *available* evidence at any time is grounds enough for evidence-based practice.⁴⁹

The problem with RCT-derived evidence on CAM is that the data are often difficult to interpret because their methodologies tend not to be straightforward. As Margolin, Avants, and Kleber observe in their corpus *JAMA* editorial, as if channelling Kuhn, RCTs depend foremost on community membership and shared assumptions, methods, and knowledge but “[t]hese conditions may not be satisfied with respect to considerations of alternative medicine, which typically involve heterogeneous groups comprising incommensurate cultural and evaluative frameworks” (1626). Without an overarching common standard between biomedicine and most CAM practices, designers of CAM RCTs must assess carefully whether the design is compatible both with accepted biomedical methodologies and with the principles of the practice under study; when the two do not match up, the solution seems to be exactly as we might expect: to re-shape the unconventional health practices so that they fit better into a conventional biomedical mold.

For example, to meet the biomedical norm of treatment standardization, Shlay et al. invented what they called a “Standardized Acupuncture Regimen” (SAR) for the control of HIV-related peripheral neuropathy. This standardized approach meant that all patients from the intervention group received exactly the same acupuncture treatment—needles applied to a fixed set of acupoints—rather than receiving a tailor-made treatment in accordance with the procedures of Traditional Chinese Medicine. Although the SAR was selected by a panel of

⁴⁹ In this chapter, I focus exclusively on the RCT because, as Feinstein and Horwitz point out, although the systematic review is highest on the evidence hierarchy, it “can aggregate and evaluate but cannot change the basic information, [so] the RCTs themselves become the fundamental source to be considered both for quality and scope of data, and for the scope of topics contained in the EBM collection” (530).

experienced acupuncturists, the practice as manifest in the study bears little resemblance to its clinical counterpart.⁵⁰ Dr Yao, the TCM college president I interviewed, responded to the idea of a SAR with disbelief (“Everybody receives the same point?”), noting that the form of acupuncture in the study was not even recognizable to him as TCM. Dr Connolly, the other acupuncturist, was more open to the idea of standardization from a theoretical perspective—that is, he could identify with the quantifying impulse of Western medical culture—but likened the idea of such an approach being applied to acupuncture to “trying to fit an ocean into a bathtub.” (He said of the SAR, sounding amused, “they think they standardized what I do.”) Not surprisingly, by contrast, Shlay et al.’s standardized approach is part of what made this study appear methodologically credible to the medical researchers, Drs Clarke and Fournier, because it eliminated individual variations in the treatment under study. To approach the study any other way would, to them, compromise its rigour by introducing contaminating variability.

The issue of blinding in the Shlay et al. study also deeply troubled the acupuncturists, particularly the use of sham control points in the placebo-group participants, who received three needles in locations on the leg that do not correspond to any known acupuncture points. The theory behind the control points was that, because they were not located along any traditional qi meridians, they should have minimal or no effect and yet maintain participant blinding because needles were still inserted into the skin. In the corpus issue of *Archives of Internal Medicine*, Ernst and White indicate in their systematic review of acupuncture for back pain that this kind of control, known as an *active control*, is common in acupuncture trials. In their view, active controls are an acceptable method of placebo control. However, both Yao and Connolly noted

⁵⁰ The SAR, selected by a panel of eight acupuncturists, was based on the premise that the peripheral neuropathies caused by diabetes and HIV were similar; the points most commonly used for diabetes were then adopted. Patients experiencing other localized symptoms were additionally treated with supplemental points if they answered “yes” to the corresponding question. Both the depth and duration of needle insertion were standardized.

that the idea of a placebo effect was irrelevant in TCM because a patient's mind and body would normally be treated together as one, so whether the treatment effect was imagined or not would make no difference to their practice.

For the purpose of the interviews, both TCM practitioners were willing to go along with the idea of a placebo control but they remained worried about the methods chosen. Yao was primarily concerned about the use of an active control point, a needle actually inserted into the skin, when, in his view, the triallists could have easily used an inactive control by simply tapping the skin to mimic the sensation of needle insertion. ("That's a mistake," he said. "This is negligent. They didn't think carefully about this.") Connolly was more concerned with the validity of the control points *as* control points, observing that "my intention, as someone treating, matters, so me knowing that I am...administering something that isn't going to be effective will affect the treatment." Without that *intention*, a sort of innate healing principle, behind the placebo treatment, he felt it would fail to equal the intervention treatment in all ways except the active ingredient (in this case, the needle-meridian connection), which is a basic requirement of a placebo.⁵¹ Curiously, Connolly's concern here stems from the fact that the study was not *double-blind*—a concern that is nested within the discourse of EBM, and yet was not expressed by any of the three biomedically trained expert readers, Clarke, Fournier, and McDonald.

These two examples, of standardization and placebo control in the Shlay study, are suggestive of the ways in which RCTs in the *JAMA-Archives* theme issues shift CAM practices to fit into a biomedical framework. Both acupuncturists were uncomfortable with the differences between acupuncture as manifest in the study and acupuncture as practiced within a TCM context, but these differences seemed subtle and mostly insignificant to my biomedical expert

⁵¹ Cf. pharmaceutical placebos, which are designed to look, smell, and taste like the investigational drugs so that the only real difference between them is the presence or absence of the drug under study. To Connolly, then, a control point lacking the practitioner's intention could perhaps be likened to a placebo pill that was the wrong size or shape.

readers, at least initially. Upon further reflection, Clarke and Fournier both developed concerns about the study's relevance to practice that seem to derive, at least in part, from values instantiated by the genre of the scientific article itself. I explore below the correspondence of research values and genre in the context of *method* as boundary-defining argument; what is salient for now is that, on first glance, both researchers found the study's methods of standardization and control immediately appealing, perhaps because those methods signalled in the article a commitment to established scientific procedures.

The other article I selected for analysis, the Cardini and Weixin trial of moxibustion (the burning of herb rolls on acupoints), stands out as a bit of an anomaly within the corpus RCTs, due largely to its strongly positive results, but also to its methodology and outcome measures, which are rather more amenable to biomedical standards while remaining consonant with TCM theory. For example, while more than half of the corpus RCTs were assessed solely by subjective measures such as symptom diaries and pain scores, Cardini and Weixin's primary outcome measures were objective: the fetus would be either breech or cephalic, an orientation that could be confirmed unequivocally (by ultrasound and at birth). Moreover, the intervention was easy to standardize in accordance with RCT conventions without distortion of TCM theory. (Both of my acupuncturist-readers agreed that the acupoint selected, BL 67, would be singularly used on all women with breech pregnancies.) Yet even this study, which struck me as relatively strong methodologically, had design features that Clarke and Fournier, my researcher-readers, deemed unacceptable.

Both researchers worried, for example, about the fact that some patients might "get more" of the treatment than others by virtue of their having a higher tolerance for heat or burning sensations. They also expressed concern about variability because the treatment was

administered at home by nonmedical personnel. Fournier searched even further for possible confounding variables, noting at one point that, “It seems to me, if you can burn stuff on your toes and it makes an influence then a lot of things might be able to influence this breech presentation at this point.” Meridian theory is so incompatible with his own theoretical orientation that moxibustion’s apparent efficacy seems best explained, in his view, along these lines: “if the burning of herbs on a toe can reverse a breech pregnancy, then pretty much anything might.” Cardini and Weixin’s trial report indicates clearly that participants were advised to consult the investigators before using “any other interventions or therapies that could contaminate the results of the trial” (1581). In light of this stated precaution, Fournier’s comment here seems more a product of his own disposition toward moxibustion than of any specific flaws in the study’s design. Both he and Clarke seemed concerned at many points that contamination could creep in unnoticed to these acupuncture studies, a worry that I suspect would not so pervade their analyses of more conventional biomedical trial reports.

Both Clarke and Fournier cited the potential for variability in the application and dosage of moxibustion as a significant flaw, while they commended the use of standardization and placebo controls adopted in the Shlay study. In each case, these expert readers isolate individual procedures within the RCT model and measure the acupuncture trials against them. However, as Clarke and Fournier further reflected on the validity and applicability of the studies’ findings, later in the interviews, they began to re-evaluate their own initial responses to these procedures. For example, on the topic of the SAR’s irrelevance to acupuncture practice, Clarke suggested that a standardized therapy—which he had earlier praised as one of the study’s “very positive features”—was not, in the end, all that essential to the quality of the design. He offered this comparison:

If you were giving a psychotherapy intervention, let's say, there'd be some sort of flexibility in terms of the approach you would take. There'd be some individualization of that treatment, I can understand this. But if you believe that these [energy] pathways are real and the people are trained to identify those pathways, it shouldn't [pose a problem], I guess. I'd turn the comment around and say, 'Well you should design a randomized trial where you do a personalized intervention versus sham and then see what happens.'

In suggesting that an individualized treatment would not necessarily compromise the integrity of a trial (particularly since individualization is also routine in certain kinds of biomedical trials), Clarke seems to have shifted his criteria for what counts as an appropriate methodology. At first, randomization served as a firm benchmark of quality in the Shlay study but, as the study's larger context and implications became clearer, it seemed less important.

What I want to suggest with this example is that, in the context of the argument from method in biomedical boundary work, standards that are held up as benchmarks of accepted methodologies, such as randomization and placebo controls, are often more flexible than they might seem. As Clarke is a well-established researcher, his ultimate ambivalence toward the SAR may be taken to suggest the possibility of an underlying ambivalence in biomedicine about how scientific methods are employed in practice. That is, although Shlay et al. clearly felt that the SAR was an important design feature irrespective of TCM theory's resistance to standardization, the fact that Clarke, a member of their shared discourse community, was relatively unmoved by the SAR seems to signal something larger than concern for methodological integrity going on in Shlay et al.'s standardized approach.

Shlay and colleagues' decision to adopt the standardized regimen despite their own admission of its irrelevance to practice (1594) betrays their awareness of the need to fit their

study within the prescribed genre of medical research, the RCT report, because the notion of standardization, though certainly conventional, is by no means essential to trial design—nursing and psychotherapy trials, for example, frequently feature individualized interventions without compromising their quality of evidence. This kind of generic over-reaching, I describe next, illustrates the potential for method to serve ends of argumentation in biomedical boundary work because the RCT is an idealized model that, even in mainstream medicine, frequently falls short of its reputation as the “gold standard.”

RCT Reports as Idealizing Genres

A key constituent of the method *topos* is the genre of the experimental article. As with the other RCT reports in the *JAMA-Archives* corpus, the Cardini and Weixin and Shlay et al. articles follow the IMRaD structure (Introduction, Methods, Results, and Discussion).⁵² Modelled on the genre of the experimental article borrowed from the sciences, this format and its constituent parts serve specific and recurrent rhetorical purposes in medical journals (see, e.g., Segal, “Strategies”; Simons, “Research Report”; Martha Solomon). Cardini and Weixin and Shlay et al. appear strategically to employ the genre to enhance their ethos as scientists in the face of their work on subjects not fully compatible with a biomedical purview. In the Shlay study, for instance, the very structure of the genre helps to downplay both the strangeness of TCM theory and the study’s incompatibility with that theory. However, the scientific article, as a genre, embodies an idealized model of research that seems, from the start, to set up the testing of CAM for failure. By tracing some of the ways in which even biomedicine fails these standards, we can learn more about the rhetorical workings of RCT-derived evidence, the centrepiece of EBM. The arguments

⁵² This format is recommended by the International Committee of Medical Journal Editors in the uniform requirements for manuscripts submitted to biomedical journals.

about method in CAM debates, I want to suggest, can be read, metonymically, to express more general anxieties in biomedicine about knowledge and evidence, community values, and professional boundaries. A little background on Shlay and colleagues' SAR will help me unpack that claim.

The authors first mention the SAR in the Introduction but the procedure is simply asserted, without contextualization, and is easy to miss:

To evaluate the effect of both a nonstandard and standard medical therapy for peripheral neuropathy, we performed a multicenter, modified double-blind, randomized, placebo-controlled study of the separate and combined efficacy of a standardized acupuncture regimen (SAR) and amitriptyline for the relief of pain caused by HIV-related peripheral neuropathy. (1590)

None of my expert readers even registered this first occurrence of the SAR as a textual entity, each of them asking, when the term came up at later points in the article, what a "SAR" was. The authors do not here explain why they chose a standardized approach, or even that such an approach is not consonant with traditional acupuncture. Instead, information about the SAR, which struck my expert readers as pertinent for evaluating the study's methodology and results, is not provided until the end of the article, in the Discussion. (Partway through our interview, for example, McDonald, the physician-clinician, was surprised to learn that this method was not the usual one, responding, "That's helpful because I wouldn't have inferred that.") Given that we know, from Carol Berkenkotter and Thomas Huckin, that Introductions and Discussions are among the last sections of experimental articles that scientists read (see note 59), the fact that Shlay et al. only explain the SAR in these peripheral discursive spaces minimizes their study's associations with TCM as an independent professional and philosophical practice.

We can frame the creation of *SAR* as a textual entity within Ellen Prince's taxonomy of assumed familiarity as, initially, "brand new" (235), something presumed not to be part of the medical community's shared store of knowledge. It is introduced first as indefinite: "*a* standardized acupuncture regimen." Amitriptyline, by contrast, is a definite expression from the outset, a named entity assumed to be familiar to medical readers.⁵³ When the *SAR* is mentioned elsewhere in the article, it is assumed already to be "on the table" of the discourse, so the reader must look back in the text to find the original reference to discover its meaning. However, given the heavy time restrictions that most readers of such articles are under (Grimes and Schulz; Sackett et al.), it seems unlikely that they would go back, and I suspect most readers would do as Clarke, Fournier, and McDonald did, which is to go along with the *SAR* as an uncontroversial given.⁵⁴

When Shlay et al. do revisit the *SAR* at the article's end, their explanation of its relevance to practice is matter-of-fact: "The *SAR* chosen for this study differs from the practice of most acupuncturists, who treat patients with individualized regimens" (1594); they go on to cite blinding, replication, and generalizability as the reasons behind their standardized approach. *The SAR chosen* in this statement is what Prince calls a "containing inferrable" (236), a textual entity whose meaning a speaker assumes hearers will be able to infer from the inferrable noun phrase itself (e.g., "What *SAR*? Oh, the one chosen in the study.") Although *the SAR chosen* is somewhat circular in its reference, in that it provides within itself the resources for its own interpretation, this construction does also suggest that *SARs* are generally an established

⁵³ From the perspective of audience design, the introduction of the standardized intervention as brand-new places those trained in acupuncture theory, such as Yao and Connolly, in the unusual position of having their own practice re-presented to them, nearly unrecognizably, as new information. In this process, they become, in a sense, double-overhearers, outsiders not only to biomedical discourse but also to discourse concerning their own professional practice.

⁵⁴ This initial presentation of the *SAR* fits with Segal's observation that, in medical journal articles, "Information is deemphasized in embedded structures; minor sentence elements reduce the effect of the information they contain—or imply the information is not new but given" ("Strategies" 527).

category. Phrased this way, it sounds almost as though Shlay et al. are explaining why they chose *this* SAR over another one, deflecting attention from the total absence in their article of what Lawrence Prelli calls the *topos of external consistency*, a characteristic rhetorical strategy in scientific articles. This topos, a claim for the consistency of scientific authors' current claims with the common store of knowledge of a field, appeals to scientific ideals such as communalism and universalism.

Although acupuncture theory would not be considered part of the store of shared knowledge in science, the fact that the study's SAR was not consistent even with the TCM community's own agreed-upon rules of practice was not lost on my researcher-readers Clarke and Fournier, perhaps because it violated a vaguer principle of universality. Fournier described the SAR's irrelevance to practice as "a bit concerning," noting that although the authors were seeking reproducible results, "we always want to do things that reflect practice, right? So the fact that this doesn't reflect practice...[means] this is of very little relevance, the results." Tapping into the scientific value of replicability, a value that serves important persuasive ends in the genre of the scientific report (Bazerman, *Shaping*), Shlay et al. further distance their study from acupuncture theory and practice. To biomedical readers unfamiliar with the SAR's poor fit with acupuncture practice, this standardization may well be persuasive; it certainly seemed so to Clarke, Fournier, and McDonald in the early stages of their interviews. This distancing of the study intervention from its philosophical base is one of several strategies that both Shlay et al. and Cardini and Weixin employ to enhance their articles' legitimacy within the biomedical community by aligning them more closely with the conventional genre of the RCT report.⁵⁵

⁵⁵ In studying how texts are put together to achieve their effects, rhetoricians often appear to imply a sort of premeditated calculation on the author's part. But this is an appearance, only: as Leah Ceccarelli points out, "Just as an organism might adopt a successful evolutionary strategy without being consciously aware of it, so too might an author adopt a successful rhetorical strategy without being consciously aware of it" (*Shaping* 5).

As we know from genre theory, particularly from Carolyn Miller's seminal formulation of genres as "typified rhetorical actions based in recurrent situations" ("Genre" 159), genres accomplish important rhetorical work: they shape the production of discourses and condition their reception; they are instrumental in processes of identification, particularly in academic/professional settings where writers must demonstrate a thorough understanding of their field's genres to assert their community membership and establish themselves as authorities; and they perform important epistemic functions, both in the discourses they enact and among the discursive communities that use them. Berkenkotter and Huckin note, "Genres are intimately linked to a discipline's methodology, and they package information in ways that conform to a discipline's norms, values, and ideology" (1)—they are, in short, "the intellectual scaffolds on which community-based knowledge is constructed" (24).

Genres can be difficult to isolate and describe, however, because, as Anthony Paré explains, "The automatic, ritual unfolding of genres makes them appear normal, even inevitable; they are simply the way things are done" (59). This sense of generic "inevitability" lends itself to a sort of generic *invisibility*, particularly in science, a world shaped by genres meant to disappear. Scientific articles, for example, seem to leave readers with only objective facts. By embodying unconscious, tacit social actions, genres are inherently ideological; in science, that ideology inheres in its institutionalized genres—grant proposals, lab reports, conference papers, experimental articles, and so on. These scientific genres are infused with what Paré calls the "naturalized ideology" of institutions (68), which can have profound implications in the case of biomedical research on CAM.

In medicine, the RCT report, modelled on the genre of the experimental article in science, is one of its central "intellectual scaffolds." Ted Porter argues that the "relative rigidity"

of the rules about conducting and publishing experiments and their results (i.e., the IMRaD model) “ought to be understood in part as a way of generating a shared discourse, of unifying a weak research community” (228). The idea that medicine, as a professional community, could be called *weak* does not mesh well with the social histories of medicine supplied by, for instance, Paul Starr, who identifies medicine as the exemplary profession. However, in Ted Porter’s lexicon, weak communities are simply those, such as medicine and psychology, that rely on science but are not themselves unequivocally scientific (cf. physics, the quintessential “pure” science). He observes:

Scientific knowledge is most likely to display conspicuously the trappings of science in fields with insecure borders, communities with persistent boundary problems. That is, one has to look at a wider context for science to understand even the accepted forms of scientific production, the standards by which work is judged. So, science is indeed made by communities, but communities that are often troubled, insecure, and poorly insulated from outside criticism. Some of the most distinctive and typical features of scientific discourse reflect this weakness of community. The enormous premium on objectivity in science is at least partly a response to the resultant pressures. (230)

From this perspective, the adoption of the IMRaD model in medicine could be seen as a persuasive move on the community level, where the users of the genre actively (though not necessarily consciously) foster identification between their methods and the valorized methods of science in order to secure their community’s boundaries—and their position within them. This anxiety is particularly intense in an EBM context, which frequently frames the need for greater production and consumption of evidence not as a matter of medicine *becoming* more scientific but of catching up with the *other* disciplines of science. Note, for example, the tenor of R. M.

Califf's admonishment in the *Journal of Internal Medicine*: "The failure of the medical professions to develop and implement data standards has left medicine far behind most other major enterprises" (427).

Other fields of study have capitalized on the mobilizing force of the scientific article, particularly the social sciences, by appropriating it in their own realms of publication. In his well-known essay, "Concealed Rhetoric in Scientistic Sociology," for example, Richard Weaver noted more than half a century ago that the methods of science had been so successful at formulating knowledge that sociologists began to apply the generic features of scientific articles to their own disciplines. Importantly, however, he argues, "they were not trying to state the nature of their subject; they were trying to get a value imputed to it" (143). While Weaver holds researchers themselves as somehow culpable in their use of "scientistic" genres, similar but more recent work by scholars such as Bazerman ("Codifying") and Berkenkotter and Ravotas views genre appropriation as occurring on the community level. That *value* associated with the sciences can be understood as a kind of linguistic capital, in Pierre Bourdieu's terms, where the discourses of science have come in some ways to be worth more than others.⁵⁶ In light of this increased wealth, other fields adopt the format as a means of fostering identification with science and its attendant linguistic capital. This "borrowing" of scientific genres is part of the communally instantiated action of genre.

The notion of genre borrowing is salient to my analysis of the Shlay et al. and Cardini and Weixin articles because, while they are ostensibly in the mainstream (published under the imprimatur of the American Medical Association and authored by established biomedical researchers), they can also usefully be thought of, in a sense, as borrowed genres: they apply the

⁵⁶ Schryer's study of "competing genres" in veterinary labs and clinics and Paré's study of social work reporting in a hospital setting both usefully develop this stratified view of genre.

habits of mind and modes of communication of one field (medicine) to another disparate, possibly incommensurate field (acupuncture). We might, then, think of the CAM RCT report as a Burkean terministic screen, where the terministic screen of experimental science “directs the attention” (Burke, *Language* 45) of researchers toward certain sorts of observations about acupuncture that fit that terminology, and away from those that do not. Since, as Burke says, any terminology necessarily reflects, selects, and deflects attention, the fact that Shlay and colleagues deflect the importance of an individualized approach in acupuncture in their account of the SAR seems largely to be a consequence of working within the RCT genre: there is no room, in that particular forum, for concepts such as *qi* or *meridians*, and, indeed, for nonstandardized approaches to health interventions.⁵⁷ Perhaps not surprisingly, then, Cardini and Weixin and Shlay et al. exhibit a kind of hyper-performance of the experimental genre as they “display conspicuously the trappings of science” (T. Porter). Most strikingly, both articles assume at times an exaggerated empiricity—a strategic overdescription of salient trial features that increase their association with scientific methods that seemed even a little suspect to my expert readers.

McDonald, the physician-clinician, was mystified, for example, at Cardini and Weixin’s description of the ways that the study’s pregnant participants were taught to sense, at home, whether fetal version had occurred. She spent nearly five minutes of the interview working through the following description: “[participants were instructed about] symptoms that version had occurred (decreased pressure in the epigastrium or hypochondrium, increased pressure in the hypogastrium, pollakiuria, a ‘different feeling’ in the abdomen)” (1581). Mapping on her own body where these different sensations ought to be felt, McDonald struggled to understand them:

⁵⁷ Published responses to the study’s SAR did not take issue with the authors’ choice to standardize the procedure, although this may speak more to *JAMA*’s readership than to the idea of a SAR. Yet the fact that even the acupuncturists that responded to the study disputed only the particulars of the SAR, and not the concept of the SAR itself, suggests to me that even those familiar with acupuncture techniques realize the limits of the RCT terministic screen (see, e.g., Kaptchuk, Letter).

I just read that and I thought, “Okay that’s the same area, I don’t know why they are saying it twice” and then “increased pressure in the hypogastrum”—honestly, I had a hard time literally trying to imagine what they meant by that and I thought “uh?” ’Cause as far as I can tell on my own body, your epigastrium, right up here by your tummy, so “decrease pressure there,” okay, but then they’re saying, well, on the other hand, you might have more pressure like, seriously, two inches below it and I thought, “Okay?” I didn’t know what they meant by that.

McDonald noted that since even she had a hard time trying to distinguish between the different sensations described, she did not feel that such a description, even described in lay terms, would be a useful “clinical clue to women to...keep an eye out for.” She also wondered about the authors’ use of terms such as *pollakiuria*, in this passage, and others elsewhere, such as *gestosis*, with which she was not familiar. Since both of these terms refer to common clinical presentations—excessive urination and preeclampsia—the use of lesser-known terms in their place seems to be an attempt to make their prose more “scientific.” As a whole, this description of version symptoms struck McDonald as disingenuous, as if the authors were “trying to make [their article] sound more slick.”

McDonald also noted that the outcome measures Cardini and Weixin describe, which included cephalic presentations, fetal motor activity, adverse events, cephalic versions, caesarean sections, spontaneous and induced deliveries, and Apgar scores, “seemed, if anything, overly comprehensive.” This idea of *overdescription* runs through both articles, but it is a strategic overdescription. In the Shlay study, for example, the explicitly detailed description of the needle insertion protocol is perhaps most revealing in what it *does not* say:

For the SAR and control points, acupuncture needles were inserted to a specified depth. Each location was manipulated both superiorly and inferiorly. Then the needles were reinserted into the specified point. After 10 to 15 minutes, the needles were remanipulated and replaced into the original location for another 5 to 10 minutes. The depth of insertion was between 1.28 to 2.54 cm (0.5 to 1.0 in) for spleen point 9, 2.54 to 3.81 cm (1.0 to 1.5 in) for spleen point 7, and 1.5 to 3.05 cm (0.6 to 1.2 in) for spleen point 6. For the control points, insertion was less than 1.28 cm (0.5 in). (1591)

Even the acupuncturists struggled to understand the manipulation technique described in this passage. “I don’t understand this little portion,” Connolly said. “I really read it, like, four times to get it and I can’t.” Yao reread it aloud to himself several times while confusedly manipulating imaginary acupuncture needles in the air in front of him. Both wondered: Why would the needles need to be re-inserted? Why would they be removed at all? What is superior manipulation? How were the depths determined?

What is still more curious about this passage, from the perspective of the method topos, is that there is no description, here or elsewhere, about *why* the needles need to be manipulated. The meridian theory of acupuncture is conspicuously absent in the Shlay study. The authors only gesture toward the intervention’s underlying mechanism in the Discussion, where they cite several biochemical possibilities: “mechanisms such as the release of endogenous opioids or activation of other brain and spinal cord pathways that reduce pain” (1594). The absence in the Methods section of any underlying theory makes the authors’ careful description of the needle manipulation protocol seem strangely undermotivated—indeed, both Fournier and McDonald seemed slightly bemused by the level of detail given, a bemusement that appeared to stem from the lack of reasoning behind it.

The question here then becomes this: why do the authors describe the protocol so carefully, too carefully almost, but not give any explanation of why the procedure was performed that way? One function of this description-without-explanation is that it downplays the study's association with TCM as an independent professional and philosophical practice. And yet it also seems to serve a higher purpose, related to the premium placed in biomedicine on method—the means to knowledge, as Kaptchuk notes (“Powerful”), rather than knowledge itself. Method is one of the primary ways that scientists identify themselves as members of their communities and persuade readers to take their results seriously; in boundary disputes, method can be invoked rhetorically to position a given health practice or study within or beyond the borders of science.

Methods sections of experimental articles ostensibly exist for the sake of replication, to enable other researchers to repeat a study's methods to confirm or discount its findings. (Note that Shlay et al. cite replication as one of main motives behind their standardized regimen.) However, rhetoric and genre scholars Charles Bazerman (*Shaping*) and John Swales, and sociologists Gilbert and Mulkay before them, have shown that methods sections are poorly suited to duplication because they are so “elliptical” that their methodologies are not explicit enough for other researchers to follow (Swales 169). Given their abstractness, I would argue, after Bazerman and Swales (and others, e.g., Giltrow), that methods sections might usefully be thought of as primarily arguments from *prolepsis* (i.e., anticipating and heading off objections by indicating the appropriateness of the study's methodology) and from *ethos* (the researcher's character *qua* researcher). That Shlay et al. frame their methods so carefully in a scientific lexicon, describing needle depths to the millimetre but avoiding the concept of qi, makes sense within this perspective because choosing not to do so would undermine their credibility as scientists and their study's validity as a contribution to community-oriented knowledge-making.

The problem with this emphasis on method in biomedical CAM research is that the chances of an intervention such as acupuncture meeting the methodological standards expected within the IMRaD format *while* remaining consonant with the core tenets of its own philosophical system, TCM, *and* demonstrating efficacy seem poor indeed. Importantly, however, the RCT genre idealizes standards of evidence that even biomedical studies frequently fail to meet, so I think we can read demands that CAM meet the same (or even stricter) standards as indicative of higher-order concerns in the medical profession about its methods of study and practice.

Swales observes that “the fact that the purposes of some genres may be hard to get at is itself of considerable heuristic value” (46); this is where the real ideological work of genres comes into play, particularly in science and medicine, where they seem not to have any purpose at all, aside from providing information. The IMRaD structure appears to support the objective reporting of the events and implications of a given research project, in which “[t]he authors seem only to be contributing a filler for a defined slot” (Bazerman, *Shaping* 28).⁵⁸ Despite its apparent neutrality, however, the IMRaD structure embodies certain systemic values about scientific knowledge that are central to science’s position of privilege in Western culture.⁵⁹ Central to this embodied value system is the idealized model of research that emerges through it: as Swales has shown, real-life science evolves more organically, more accidentally, than the IMRaD model would suggest. Experimental reports, unlike their correspondent laboratory notes, adopt a more solution-oriented stance and divide the various elements of laboratory work into finite sections.

⁵⁸ This perspective of trial reports was literalized by the 1996 publication of the first set of CONSORT (i.e., Consolidated Standards of Reporting Trials) guidelines, a 22-item checklist for reporting trial results (Begg et al.). For the most recent guidelines, see Moher, Shulz, and Altman.

⁵⁹ One significant value embodied in the IMRaD model is the production of knowledge, which Berkenkotter and Huckin call “news value.” Their research on scientists’ reading practices revealed that they read experimental articles like newspapers, first scanning titles and authors to determine interest, then abstracts, data (graphs, tables, etc.), and results (in that order), and then other parts of the paper depending on their level of interest.

The notes are not just “written up”—they are (re-)ordered, and their levels and kinds of claims are altered, their emphases shifted, and their vocabularies adapted to meet specific goals, which often vary markedly from the initial goals of the project (Swales 117-27). This is Medawar’s warrant for calling, somewhat tongue-in-cheek, the scientific paper “fraudulent”—because it “misrepresents” scientific processes.

The RCT genre fixes an ideal of research that most CAM practices cannot meet, particularly not in an evidence-based model, where the consequent shortage of evidence of their efficacy is often taken as “evidence of the lack of an effect,” in acupuncture researcher Elisabet Stener-Victorin and colleagues’ words (1942). The orientation of commentators within boundary disputes becomes essential, then, because they can shape their contributions to the debate by invoking stricter or looser interpretations of what a “proper” scientific methodology is, thereby limiting or expanding biomedicine’s scope, and complementary and alternative medicine’s place within it. For example, Tom Delbanco, writing in the *JAMA* theme issue, sees CAM as an imminent threat to the medical profession; he also demands much higher standards of evidence-production than Clarke, one of my researcher-readers, who seemed rather unconcerned about the prospect of CAM encroaching on biomedicine. While still alert to scientific protocol, Clarke was more relaxed about the idiosyncrasies of CAM research, showing a willingness, for example, to consider the similar design problems that some biomedical studies face, as in his reflection on psychotherapy trials quoted above.

Situating the biomedicine-CAM debate in the *JAMA-Archives* theme issues within the context of EBM can highlight the role of method in boundary-negotiating arguments. Although the EBM literature has largely valorized the RCT, it has, at the same time, fostered a thriving culture of critique both of its methods and its ideology. These critiques helpfully illuminate the

uneven ability of the RCT to demonstrate the safety and efficacy of health interventions; they also illuminate its own “self-authenticating logic.”⁶⁰ Sociologist Catherine Will, for example, studies “the ways in which modifications of the ‘pure’ world of the experiment may also be seen as strengthening the evidence it is intended to produce” (85); referring to this process as the “alchemy” of the clinical trial, she notes that the various contingencies of research—the many different sorts of people, technologies, institutions, funding bodies, and more involved in a particular project—are transformed, in part through “the ritual invocation of randomization and control,” into objective, hard facts about the world (97).

Evidence and the methods of obtaining it are reified through this transformation process, where they are framed no longer as *ideals* of research but as always attainable and necessary to research. Thus, editors Fontanarosa and Lundberg set the evidence-production bar high when describing what they believe to be the outcome of the *JAMA-Archives* theme issues, seemingly unaware of the deep ironies of their claims:

these investigat[ions] demonstrate that alternative medicine therapies and interventions can and should be evaluated using explicit, focused research questions along with established and accepted rigorous research methods (e.g., appropriate controls, effective blinding procedures, adequate power, state-of-the-art techniques for systematic reviews); incorporating measurable, objectively assessed end points (e.g., blinded assessment); and reporting meaningful patient-centered outcomes. (“Alternative Medicine” 1619)

This claim is ironic in two ways. Most immediately, the methodologies of (at least) the Cardini and Weixin and Shlay studies fail to meet the standards that Fontanarosa and Lundberg claim for

⁶⁰ This phrase is from Ian Hacking, whom Kaptchuk cites (“Intentional”), although Hacking is not himself writing on RCTs. On styles of reasoning, Hacking observes: “The truth is what we find out in such and such a way. We recognize it as truth because of how we find it out. And how do we know that the method is good? Because it gets at the truth” (“Statistical Language” 135). Kaptchuk argues that the same can be said for the RCT, which likewise authenticates itself through circular logic.

the corpus RCTs as a whole. On the studies' methodological quality, Fournier and Clarke both cited problems with their ethics, randomization, standardization, blinding, statistical power, and outcomes evaluation; in light of these problems, each expressed surprise that the studies were published in *JAMA* at all. When they learned, at the end of their interviews, that the articles were part of a special issue on CAM, both attributed the articles' successful publication to what they perceived as the lower standards required of such theme issues.

Fontanarosa and Lundberg insist, proleptically, that the same rules of evaluation for biomedical trials applied to these trials, and this is the bigger irony of their characterization of the corpus RCTs: biomedicine also routinely fails these standards. Feinstein and Horwitz point out, for example, that one review of methodological quality showed that less than half of the studies analyzed met basic scientific standards (533). Similarly, others have isolated blinding (e.g., Fergusson et al.), randomization (Chalmers), assessment scales (Jüni et al.), and placebo controls (Lakoff) as key areas of weakness in biomedical research. (See, also, Angell, who argues that nearly every aspect of biomedical research is suspect, particularly in pharmaceutical research.) Critiques of EBM are useful to the study of CAM research in biomedicine, then, because they point to a deeply penetrating instability in the evidence base—an instability that can largely be traced back to the RCT as one of EBM's organizing genres.⁶¹

In the passage from Fontanarosa and Lundberg that I quoted at the beginning of this chapter, the editors create a dichotomy between “proven” and “unproven” medicine: proven medicine, they say, is evidence-based, while unproven medicine lacks evidence. In this configuration, biomedicine falls under the first head, while interventions such as acupuncture fall

⁶¹ Physician Abd Hamid Mat Sain, for example, dismisses EBM due to its reliance on the RCT as “the panaceal standard bearer of evidence” (¶ 3), while Ted Kaptchuk wonders aloud if the RCT really is the gold standard, or if it is, instead, a “golden calf” (“Double-Blind”). Even psychiatrist Simon Wessely, who passionately defends the RCT in mental health trials, avers that it “is not the pot of gold at the end of the evidence-based rainbow” (116).

under the second—although with the potential to move into the first through adequate testing. While this proven/unproven dichotomy may initially sound reasonable, it seems more like a straw man argument than a legitimate claim about the conditions of knowledge-making in medicine when we consider that the majority of *biomedical* treatments have not been “proven” according to these same criteria (see Borgerson). Given that biomedical studies so regularly do not reach the same standards to which CAM studies are held, we might usefully think of Fontanarosa and Lundberg’s distinction between proven and unproven therapies instead as a reflection of the RCT’s *symbolic* value in medicine, as representative of the aims and ideals of research. This is how the rhetorical study of CAM research can illuminate biomedicine itself, by opening up its evidentiary methods to scrutiny through a kind of Brechtian alienation—that is, by making them “strange.”

It is not just the RCT’s claims to rigour that are problematic: its relevance, too, is often hard to demonstrate. For example, Fontanarosa and Lundberg’s assumption that adequate testing would necessarily ensure the safety and efficacy of any medical treatment in practice, alternative or otherwise, is also faulty. We know, from extensive research (e.g., Denis et al.; Dopson et al.; Grimes and Schulz; Will), that there is a poor association between the evidence base and clinical behaviour. Some characterize that weak association as an “implementation gap,” where practitioners simply lag behind their research counterparts (see Will), while others see it as the product of a more fundamental mismatch between the theory and practice of medicine (e.g., Feinstein and Horwitz). Part of the disconnection between research and practice certainly has to do with practitioners’ ability to keep up with the literature: Sackett et al. report that doctors have less than an hour per week available to literature review (McDonald reported having less than an hour per *month*), while Grimes and Shulz indicate that many doctors “feel unqualified” to read

the literature “critically” (57). Additionally, the assumption that research findings would translate so directly into practice naively underestimates the complex negotiations that clinicians must undergo in practice among competing bodies of data (RCT reports and systematic reviews, experience, professional habits and competition, commercial interests, clinical practice guidelines, insurance schemes...).

An even greater problem with the usefulness of RCT results is that, to meet the required standards of internal consistency, they must sacrifice their external consistency—philosopher of science Nancy Cartwright calls this the problem of “front-end rigour vs. back-end rigour” (19). Cartwright’s reasoning is worth quoting in full:

An argument that certain procedures achieve a given result much of the time may not be a good argument that they do so on any one occasion. External validity for RCTs is hard to justify. Other methods, less rigorous at the front end, on internal validity, can have far better warrant at the back end, on external validity. We must be careful about the trade-offs. There is no a priori reason to favour a method that is rigorous part of the way and very iffy thereafter over one that reverses the order or one that is less rigorous but fairly well reasoned throughout. (19)

This focus on internal consistency limits many studies’ clinical relevance because the selection criteria are so restricted and the trial populations so homogeneous that the results are difficult to extrapolate to general populations.⁶² Much of the art of medical research lies in striking an appropriate balance between front-end and back-end rigour for a specific research question, making the results as accurate as possible (i.e., internally valid) but also as useful as possible (i.e., externally valid). What it means to call a practice “effective” is largely a product of

⁶² Health services researcher Nick Black, for instance, found that one trial of coronary bypass had such rigid inclusion criteria that 96% of those currently receiving the intervention would not have qualified in the original RCTs (Bensing 19).

rhetorical negotiation, which is of especial importance to biomedical-CAM boundary work; I turn to this final point next.

Efficacy as a Rhetorically Mobile Boundary Object

Efficacy, and its sister term, *safety*, are cited, mantra-like, throughout the medical literature as the chief focus of research—we want to know which health behaviours and interventions are going to help us and not hurt us. (Cost-effectiveness is a third, but later, concern.⁶³) In EBM, clear determinations of safety and efficacy are assumed to be the natural and necessary outcomes of research, as Sackett et al. claim: “External clinical evidence both invalidates previously accepted diagnostic tests and treatments and replaces them with new ones that are *more powerful, more accurate, more efficacious, and safer*” (72; emphasis added). These two terms are the primary touchstones in determining the legitimacy of health interventions, particularly regarding CAM: they provide the terms of the debate and condition its outcome. Note, for example, Fontanarosa and Lundberg’s assertion that “There is no alternative medicine,” only proven and unproven medicine. “[A]s believers in science and evidence,” they conclude, “we must focus on fundamental issues [such as] the need for convincing data on safety and therapeutic efficacy” (“Alternative Medicine” 1618). The implication here, as I have suggested, is that if a CAM practice is proven to be both safe and effective, it will be integrated seamlessly into biomedical practice. It sounds simple enough but it just is not the case, for a whole host of reasons—epistemic, professional, practical, and more besides.

As keywords, *safety* and *efficacy* are flexible enough that they can function as gatekeepers: if an intervention meets one implied standard of efficacy, for example, skeptics can

⁶³ On cost-effectiveness, see Califf, who notes that “the idea that we pay more for a greater benefit is basic to most national economies. Yet in healthcare, there has been a tendency to expect that more effective therapies should save money. In fact, this is rarely the case” (429).

(and often do) invoke a more rigorous, and more exclusive, meaning. They thus seem to function in debates about CAM as god-terms, in Kenneth Burke's formulation, powerful, indeterminate terms that, he says, "[sum] up a manifold of particulars under a single head" (*Religion* 2). (*Freedom* and *love* are quintessential god-terms.) As summary terms, they carry within them various, and even conflicting, interpretations—they contain, Burke would say, the resources of ambiguity (*Grammar* xix), the fertile ground for persuasion. Any way of seeing, he reminds us, is also a way of *not* seeing, and this is true in the example of efficacy. The question of whether a CAM practice "works" depends very much on whom you ask, what their criteria are, and what the consequences are of their answering.

In this final section, I argue that biomedical researchers employ a variable principle of efficacy in studies of CAM in the service of the method *topos*, to reconcile their own disciplinary allegiances with both accepted methodologies and the practices under study. In this sense, efficacy functions as a rhetorically mobile boundary object that both *enables* research across disparate fields and can be used strategically, if unconsciously (see note 55), to advance certain argumentative ends. This claim modifies Susan Leigh Star and James Griesemer's well-known original sense of a boundary object, which they define as "an analytic concept of those scientific objects which both inhabit several intersecting social worlds...*and* satisfy the informational requirements of each of them" (393; original emphasis).⁶⁴

Star and Griesemer's study of the multidisciplinary Berkeley Museum of Vertebrate Zoology assumes an "ecological approach," where no single viewpoint takes primacy. They

⁶⁴ Star and Griesemer continue: "Boundary objects are objects which are both plastic enough to adapt to local needs and the constraints of the several parties employing them, yet robust enough to maintain a common identity across sites. They are weakly structured in common use, and become strongly structured in the individual-site use. These objects may be abstract or concrete. They have different meanings in different social worlds but their structure is common enough to more than one world to make them recognizable, a means of translation. The creation and management of boundary objects is a key process in developing and maintaining coherence across intersecting social worlds" (393).

focus on how divergent groups of actors in the museum's creation were able to sufficiently reconcile their various and often conflicting perspectives to enable cooperation, an ability the authors attribute to the stakeholders' exchange of numerous classes of boundary objects. Several scholars have taken up the boundary object concept and reframed it: in Joan Fujimura's case, it becomes part of the "standardized package," an idea that both facilitates work across collectives (which she says boundary objects are good at) and stabilizes facts (which she says they are not good at); while in Greg Wilson and Carl Herndl's case, the boundary object becomes a "rhetorical exigence" that leads to the integration, rather than the demarcation, of social-professional boundaries. The multidisciplinary situations these authors describe depend on the actors' sincere interest in collaboration and a sense of mutual respect, even for members of groups historically ranked lower than the others.

In the case of CAM research, the principle of efficacy—the idea that a given practice or intervention "works"—unites the researchers and enables their work, but the situation itself is not, in all circumstances, marked by a sense of equality. Even for those biomedical researchers that work earnestly with CAM practitioners, their relative hierarchies remain always on the horizon and the principle of efficacy invoked at a particular moment can either unite or divide the participants, depending on the researchers' orientation. There seems, then, to be a rhetorical potential within the boundary object concept that has not yet been fully explored.

The distinction between studies of efficacy and of effectiveness is a good case in point. To an outsider, the two may appear indistinct (both, for instance, are varieties of RCT) but the difference is crucial in the EBM realm, where "Current best evidence" has come to mean, almost exclusively, evidence obtained through efficacy studies. These studies feature rigid inclusion criteria, homogeneous populations and, ideally, unambiguous endpoints to minimize statistical

“noise”; they consequently have high internal validity but possibly limited applicability to real-life populations. (In Cartwright’s terminology, efficacy studies have high front-end rigour but low back-end rigour.) Effectiveness studies are large, community-based studies of more heterogeneous groups that trade methodological fastidiousness for applicability in what health care management researcher Steve Maguire calls the “real-world messiness” of clinical medicine (79); featuring lighter inclusion criteria, more varied treatment settings, “softer” endpoints, and the allowance of concurrent treatments, they produce less reliable results due to the greater statistical noise. (These studies have lower front-end rigour and higher-back end rigour.) The Institute of Medicine’s Committee on CAM helpfully distinguishes the two kinds of studies in teleological terms: “Efficacy refers to what a treatment *can* do under ideal circumstances; effectiveness refers to what a treatment *does* do in routine daily use” (104; original emphasis).

CAM practices do not fit well within an efficacy model. They are much more amenable to effectiveness studies because such studies can better accommodate the sorts of patients, symptoms, treatments, and outcomes typical of CAM. For instance, users of CAM tend to use other modalities concurrently, so there would be ethical questions about restricting their use, which an efficacy study would require for the sake of isolating causality. Likewise, endpoints of CAM studies usually need to be softer (i.e., more subjective, usually patient-reported) than those of efficacy studies because patients typically seek CAM for chronic, intractable conditions, those lacking clear prognoses and/or treatment, and those associated with hard-to-measure symptoms such as pain and fatigue. However, critics in the *JAMA-Archives* theme issues such as Delbanco, Happle, and Smolle et al. hold efficacy, not effectiveness, up as the criterion for evaluating CAM, even though much significant biomedical research is effectiveness-based (and still more is

based on observational studies, which feature neither randomization nor controls).⁶⁵ There is a sense in the corpus editorial material, even among CAM-friendly biomedical personnel (e.g., Fontanarosa and Lundberg; Margolin et al.), that we must hold CAM practices up to an even higher standard—Morreim deems it a double standard (228)—than biomedicine.

Not surprisingly, when CAM efficacy studies such as Cardini and Weixin’s and Shlay et al.’s are available, their methods tend to be subjected to greater scrutiny than are their biomedical counterparts (Borgerson; Morreim). Given the studies’ complex design challenges, it seems only fair to exercise some caution when evaluating their results. However, much of the skepticism about CAM’s potential efficacy in both the *JAMA-Archives* corpus and my interviews with Clarke, Fournier, and McDonald appears to stem from doubt about the intervention under study rather than the design. Fournier, for example, struggled to articulate his response to Cardini and Weixin’s positive result (75% of fetuses in the intervention group were cephalic at 35 weeks’ gestation vs. 48% in the control group). As he read through the results, he paused, silently, for more than thirty seconds, scanning the methods and results sections. He explained, as he continued to flip through the article, “Just trying to see if it’s biased in some way. I don’t know if I’m missing something.” He continued to read aloud and talk through the methods and results with himself, frequently pausing and re-starting as he tried to make sense of the study’s result, which he described, with a laugh, as “pretty high.” When I asked whether he would be as curious about the study’s design if the result were negative, he admitted, “I might not be as critical, to be quite frank.”

⁶⁵ Jadad and Enkin, well-known authorities on RCT design, argue that “[b]oth efficacy and effectiveness approaches are reasonable and complementary” (15). See, also, Maguire, who examines the role of effectiveness studies in HIV research. He notes, for instance, that, although efficacy is held up as the standard-bearer in FDA drug approval, effectiveness research was “legitimated” in 1989 as an acceptable means of data-production, when aerosolized pentamidine became the first drug approved based only on community-based studies (80).

Although Fournier had been reasonably satisfied with the study's methodology prior to reaching this section (and, by extension, satisfied with the method's ability to prove or disprove efficacy), he invoked a stricter set of criteria for proof of efficacy while reading through the results. In admitting that he would likely be less scrupulous with a negative finding, Fournier reveals the kind of double-standard that Morreim notes is characteristic of most biomedical research on CAM, and one evinced throughout the *JAMA-Archives* corpus (e.g., Delbanco; Happle; Smolle, Prause, and Kerl).⁶⁶ Efficacy seems, in this sense, to be a shared multidisciplinary concept that enables research on CAM (i.e., it is a boundary object) but one that can also be invoked in a more restricted sense by some participants to re-shape issues into a framework more amenable with their own perspective.

There is also a more insidious side to efficacy in drug trials that can illustrate its conceptual elasticity within medical research generally: measures intended to ensure objectivity can be actively manipulated to produce desired results; in such circumstances, *efficacy* takes on new meaning, literally, because it comes to mean whatever the triallists engineer it to mean. Former *New England Journal of Medicine* editor Marcia Angell's incisive exposé of pharmaceutical research and marketing is emblematic of the new culture of criticism surrounding medical research. In it, she describes the manifold ways that drug companies manipulate trial design and results to skew evidence in their favour. For example, the strategy of altering inclusion criteria in studies of interventions designed for elderly patients can "engineer out" untoward side effects by using younger participants, who tend to experience fewer. Likewise, as anthropologist Adriana Petryna points out, studies can "engineer up" a study drug's efficacy in head-to-head trials (i.e., drug-against-drug, rather than drug-against-placebo) by altering dosages,

⁶⁶ For some CAM skeptics, no level of rigour will ever be enough. See, e.g., Renckens, who writes, "The strongest attack [on biomedicine] arises when incomprehensible absurdities of alternative medicine are proven in impeccable trials" (531). See also Degele.

such as administering the comparator treatment, usually one with established efficacy, at half the normal effective dose or in a nonstandard format (e.g., tablet form rather than injected).⁶⁷ The most extreme altering of efficacy, of course, is results suppression, where sponsoring companies bury negative studies, present only partial evidence, or spin negative results to highlight, for example, subpopulations of the trial for which the drug did work (Angell 109-11).⁶⁸

To be sure, these sorts of manipulative strategies are not carried out in the name of boundary work: as sociologist John Abraham and others have illustrated, they result directly from pharmaceutical companies' involvement in research (54). (If the companies are paying for the studies, the thinking seems to go, then they should have a say in what sorts of results they produce.) If we think of *efficacy* as a boundary object with rhetorical mobility, however, then this highly specialized, if frequently deceptive, notion of "proving" health interventions is pertinent because it demonstrates how ephemeral a concept efficacy can be. Dismissals of CAM that hinge on the idea of efficacy seem to be motivated at least in part by a demarcation exigence, to use Wilson and Herndl's phrase, because those dismissals use a concept that is *flexible* within biomedicine to draw relatively *inflexible* boundary lines around it. That is, while "efficacy" has a range of meanings in biomedicine, some softer than others, that range is considerably less elastic at the edges of biomedicine. The criteria for determining whether or not an intervention "works" seem to be narrower, and harder, for evaluations of CAM.

⁶⁷ Petryna recalls the Chief Scientific Officer of one research facility as saying, "In my [participant] recruitment strategy, I can use subject inclusion criteria that are so selective that I can 'engineer out' the possibility of adverse events being seen. Or, I can demonstrate that my new drug is better by 'engineering up' a side effect in another drug (by doubling its dose, for example). That is the big game of clinical trials" (27; see also Angell 107-09).

⁶⁸ Angell writes about biotechnology firm Immune Response Corporation's attempts prevent its university-based contract researchers from disseminating the results of its ineffective AIDS "therapeutic vaccine," Remune. A landmark case of results-suppression is that of Nancy Olivieri, the University of Toronto researcher who violated confidentiality agreements in 1998 with pharmaceutical sponsor Apotex by publishing concerns about the trial drug in the *New England Journal of Medicine* (Olivieri et al.). For more on manipulation of trial design to reach desirable results, see also Abraham; Lakoff.

Tom Delbanco's damnation of CAM as a glorified placebo in the *JAMA* theme issue is a good case in point. Although he laments that "the public should not stand for spending tax revenues on studies not worth doing," he expresses relief that such studies will at least "shatter claims for activity beyond placebo" (1561). The potential for such an outcome bothered neither Connolly nor Yao, however, because, as Connolly explained, there "is no such thing" as a placebo effect in TCM. Body and mind are considered together so, in the end, it does not really matter whether effects of treatment occur in a patient's physical meridians or "spirit," as Connolly called it. Yao claimed this latter effect as an integral aspect of his practice:

I could just touch you, right, and you would think, "Oh the needle is in now." That, we call [a] "Positive Psychological Effect." We didn't do anything, actually, only your psychology will produce the results. And MDs and scientists have criticized us very often about this practice. They say, "Oh TCM-acupuncture is psychology." Well, psychology is good. [*Laughs.*] We can produce thirty percent results [that way], according to one [study]. Thirty percent.

In an EBM framework, this model of practice would likely constitute fraud, and even the most open-minded medical doctor or researcher might worry about its ethics. However, Yao's attitude toward patient psychology is not anomalous in CAM, and this is one of the leading charges against CAM practitioners, that their "healing" consists not in their therapies but in their clinical dispositions and their invocation of placebo effects. Since biomedical studies of CAM practices are often difficult to blind and control, one question with them often remains: if patients experienced improvement, was it due to actual physiological effects or just placebo effects? I would argue here that the criteria adopted in answering this question directly inform the ways in which efficacy can be invoked as a rhetorical boundary object.

The idea of the placebo effect is based on an additive model. Ted Kaptchuk notes that, since all arms of a study are expected to “receive equal and independent amounts” of this effect, the belief is that “one could simply subtract the amount of placebo effect to determine the presence (or absence) of specific drug effect” (“Powerful” 1724). However, this model does not allow for what he calls a “differential” placebo effect among arms, where participants receiving different treatments may experience different kinds or amounts of placebo effects, which is a very real possibility in CAM research because it is so difficult to create sham interventions that are both realistic and definitely inert. (Think, for instance, of the control points in the Shlay study: because both intervention and control needles were actually inserted into the skin, we cannot know for sure whether *neither* SAR nor placebo had effects, or *both* SAR and placebo had effects.)

Depending on where one sits in the biomedicine-CAM boundary zone, placebo controls take on different levels of importance in the determination of an intervention’s efficacy. For CAM skeptics, such as Delbanco, these problems with identifying appropriate placebo controls in CAM can be traced to defects in the practices themselves—his own take is that they cannot be tested against placebos because they *are* placebos. For those with a more moderate view of CAM, however, placebo controls play a less clear role in the evaluation of efficacy because, although they are central to RCT design, they are not nearly the safeguard against bias that the EBM literature would have one believe. Placebos can be manipulated to rig trial results, as Lakoff illustrates with the case of “targeted efficacy” in antidepressant trials (65), when researchers seek not the right drug for patients but “the right patients for the drug.” In many instances, placebo use can even be considered unethical, particularly when alternatives with known or suspected efficacy are available, as in the case of some HIV research (see Maguire).

Given that patients often seek CAM for chronic pain (as in the Shlay study), the possibility that half the trial population might be given nothing at all could be ethically troubling.⁶⁹ The relative weight commentators assign to the use (or lack of) placebo controls in debates about CAM seems to inform their assessment of a practice's efficacy.

I want to suggest with these examples that we can think of the meaning of boundary objects as having a certain degree of mobility for some of the actors involved—a mobility that is context-dependent.⁷⁰ Wilson and Herndl's study of "knowledge mapping" in a multidisciplinary work group at the Los Alamos National Laboratory is informative here: it recasts Star and Griesemer's boundary object concept into a rhetorical framework for interdisciplinary cooperation. Wilson and Herndl describe knowledge maps as visual representations of team members' areas of expertise, functions, and interrelations within the work group. These knowledge maps served in the group as boundary objects, enabling team members to speak to one another across disciplinary (and conceptual) lines. Wilson and Herndl describe the knowledge maps as producing what Peter Galison calls a "trading zone," a temporary site for the "local coordination" of distinct groups with "vast global differences" (Galison 783; emphasis in original).⁷¹ The examples I have cited in this section do not fit exactly within Wilson and Herndl's model, however: while cooperation is the order of the day in biomedical CAM research,

⁶⁹ Angell pinpoints the use of placebos to the 1962 Kefauver-Harris bill, which does not specify what drug manufacturers had to compare their products with to show efficacy. "It has since been taken literally to mean new drugs need not be compared with anything," she notes (286), since a placebo comparison would yield the strongest evidence of efficacy. Petryna points out that testing against placebo, while conventional in biomedical research, may actually violate the 2000 revision of the Helsinki Declaration of "Ethical Principles for Medical Research Involving Human Subjects," which asserts that new interventions ought to be tested against "the best current prophylactic, diagnostic, and therapeutic methods" (30).

⁷⁰ Other pertinent examples of *efficacy's* "mobility" could be explored, such as outcome measures, whose "hardness" can be scaled up or down to raise or lower the efficacy threshold.

⁷¹ Galison focused on collaboration among physicists and engineers on particle detectors and radar. He models the trading zone metaphor on "different cultures encountering one another through trade, even when the significance of the objects traded—and of the trade itself—may be utterly different for the two sides" (803).

there are specific, if limited, instances wherein biomedical actors can reshape *efficacy*, as a boundary object to alter favourably the terms of the debate.

There seems, then, to be some overlap between the boundary object trading zone, a site of interdisciplinary collaboration, and what Michael Gorman calls the “*élite*” trading zone. In an *élite* trading zone, Gorman argues, “a group of experts use their specialized knowledge to dictate how a socio-technical system will function. The expertise of such an *élite* is black-boxed for other participants in the network” (933). This is a zone in which no meaningful trade in expertise takes place. In CAM research, access to the notion of efficacy appears to be partially black-boxed to some of the participants some of the time, which allows other, more powerful participants to control, if temporarily, what it means to say a given health practice “works.” The rhetorical mobility of efficacy as a boundary object has important implications for both CAM-related research and medicine more generally: it shapes what we know about health interventions and how we know it. The study of efficacy from a rhetorical perspective can thus offer important insight when we ask, as Taubes did, “Do we really know what makes us healthy?”

Around the same time that the RCT began its ascent as the premiere means of discovering which health interventions work and which do not, Donald C. Bryant famously defined rhetoric as the “art of adjusting ideas to people and people to ideas” (420). While these two events are of course not causally linked, Bryant’s notion of “adjusting” people and ideas to one another is pertinent to the discussion in this chapter because the RCT is, among other things, a thoroughly rhetorical concept. In biomedical boundary work, the RCT can be invoked, variously, to sponsor identification or division among stakeholders in the research but only *certain* ideas and *certain* people need to be “adjusted”: those with fewer rhetorical resources.

Note Margolin et al.'s conclusion about the future of CAM research, that “controlled evaluation of [CAM] may require its practitioners to undertake a fundamental conceptual shift from a view of patients as requiring individualized treatment...to one in which trial participants are regarded as members of an equivalence class, defined by the diagnosis, who all will be given a standard prescribed treatment” (1627). Biomedicine is not asked here to undergo a “conceptual shift” because it provides the very context of the debate—it sets the terms and conditions its outcome. TCM practitioner Connolly realizes this, noting the importance for his practice of being able to move fluidly between Eastern and Western concepts of health and illness: “We have to [be able to switch]. The Western...medical community will not talk our language ever, so if we don't talk to them, it's like two people in different languages trying to communicate and get along. It doesn't work very well.” Of course, proponents of CAM may not all willingly go along with the idea of adjusting their basic premises and their principles of practice to the biomedical model. However, should they wish to attain legitimacy within the biomedical community, it seems likely that they will have to continue to “adjust” themselves because, as my analysis in this chapter makes clear, the fight is partly fixed, and not in their favour.

The function of methodology in the *JAMA-Archives* corpus as a line of argument that draws professional and epistemic boundary lines is complicated by methodology's own complicated position in biomedicine. Although the RCT is revered as medicine's preeminent source of “best evidence,” its *actual* evidence-producing ability is less assured, according to Cartwright:

There is no gold standard; no universally best method. Gold standard methods are whatever methods will provide (a) the information you need, (b) reliably, (c) from what you can do and from what you can know on the occasion. Often randomized controlled

trials (RCTs) are very bad at this and other methods very good. What method best provides the information you want reliably will differ from case to case, depending primarily on what you already know or can come to know. (11)

Cartwright's conclusion is compatible with Sackett et al.'s own view that although RCT-derived evidence is the most reliable form of evidence, it is certainly not the only *valid* form. In circumstances where RCT evidence is not available for a given question, "we must follow the trail to the next best external evidence and work from there" (72). In the medical literature on CAM, that "trail" often leads right back to the RCT, a circularity that can be traced, in part, to the EBM movement and, in part, to the "demarcation exigence" of boundary work that Wilson and Herndl describe. In the context of biomedical-CAM boundary work, *method* can be strategically invoked, eulogistically or dyslogistically, depending on a given commentator's perspective.

Studying how arguments from method in the *JAMA-Archives* corpus do not match up with the methods adopted in the everyday work of medical research can magnify problems that have always been central in biomedicine, but largely unarticulated. That is, these studies on CAM can offer clearer ways of seeing how certain forms of evidence can be marshalled rhetorically to draw professional and epistemic boundary lines. Of course, methodological rigour is the centrepiece of medical research and studies cannot be designed haphazardly. Since genres such as RCT reports are community-based—they represent communally-held values about what counts as proper evidence—trials of CAM can be illuminating because those communal values do not quite hold. CAM practices thrive outside of the biomedical sphere and, while the major ones (such as acupuncture and chiropractic) have increasingly come to work in concert with biomedicine, their practitioners remain strangers in the community of medical scientists. Their

very strangeness within biomedical borders can bring into relief biomedicine's own idealized model of research, manifest in the EBM model, wherein the best evidence for a particular health intervention seems to have little to do with patients themselves; this is the subject of the next chapter.

CHAPTER THREE

CAM Research and the Rhetorical Conditions Governing Clinical Practice⁷²

Chapters One and Two examined the rhetorical constituents of biomedical-CAM boundary work in the *JAMA-Archives* corpus through historical-professional and methodological filters respectively; the present chapter moves further downstream, examining it through the filter of medical practice. Biomedical CAM research traverses boundaries among models of medical practice—a process that can be illuminated by studying how the corpus configures practitioner-patient interaction, the most unambiguously rhetorical element of clinical medicine. The question of how to evaluate health interventions in which the clinical encounter may itself have therapeutic value has increasingly been foregrounded as health research agendas have adopted more encompassing views of health and illness.⁷³ This movement toward a broader focus in health research stems in large part from rising incidence of chronic, functional conditions, such as pain, fatigue, impaired cognitive function, and intestinal discomfort—conditions for which conventional medicine has not been able to offer much comfort. Patients with these kinds of conditions seem to benefit, especially, from care that involves an interpersonal dimension (Barry et al. 108; Wagner et al.).

Investigating health practices that depend extensively on interaction among practitioners and patients is difficult to do through biomedicine's gold-standard methodology, the randomized controlled trial. RCTs aim to isolate interventions from the context of their delivery through randomization, double-blinding, and placebo controls, but interventions are difficult to separate

⁷² A version of this chapter has been accepted for publication. Derkatch, Colleen. "Does Biomedicine Control for Rhetoric? Configuring Practitioner-Patient Interaction." *Rhetorical Questions in Health and Medicine*. Eds. Joan Leach and Deborah Dysart-Gale. Lanham: Rowman and Littlefield, in press. 146-70.

⁷³ The shift to a more encompassing idea for health is consistent with changes to the health research agenda in Canada, for example: the Canadian Institutes of Health Research (replacing the Medical Research Council) recognizes that advances in biomedicine are a key factor, but not the only factor, in improving the overall health of Canadians.

from their contexts in many CAM practices and areas of conventional clinical practice, nursing, and hospital units *qua* units. Such interventions are not easily randomized, blinded, or controlled because they comprise multiple, contingent, and interactive effects that cannot be isolated the way that a single drug's effects can be. As the previous chapter argues, methodology can be invoked in debates about CAM to shift the professional-epistemic boundary of what counts as safe and effective health care; this chapter extends that claim to argue that approaches to practitioner-patient interaction in research contexts are similarly persuasive: interaction, and ideas about interaction, can persuade us toward different views about the conduct of biomedical research on CAM and about the place of patients within health care contexts. I suggest that biomedical boundaries are negotiated in part through the ways that research accounts for practice, particularly because they can persuade us to value some interventions over others.

Fabio Firenzuoli captures part of this research-practice tension in the *British Medical Journal* when he writes, in response to Sackett et al.'s 1996 EBM article, "While clinicians are exhorted to use up to date research evidence to give patients the best possible care, actually doing so in individual patients is difficult: at the heart of clinical medicine is an unresolved conflict between the essentially case based nature of clinical practice and the mainly population based nature of the research evidence" (§ 3). The tension I want to illuminate in this chapter lies somewhat deeper than Firenzuoli's basically unidirectional focus on clinical implementation: rather than examine whether and how research figures into clinical practice, I examine whether and how practice figures into research—how, that is, interventions that depend significantly on practitioner-patient interaction are tested experimentally, and what is gained and lost in the process for health care delivery, patient care, and medical-professional boundary work.

Interventions with diverse and indeterminate interactive effects have been variously categorized in the medical literature as “complex” (e.g., Blackwood), “complex composite” (van Weel and Knottnerus), “socially complex” (Wolff), and “sentient” (Lindsay). While these designations vary in their specificity, they all align substantially with the UK Medical Research Council (MRC) description: “Complex interventions in health care, whether therapeutic or preventative, comprise a number of separate elements which seem essential to the proper functioning of the intervention although the ‘active ingredient’ of the intervention that is effective is difficult to specify” (1). While the descriptive limits of what I will here call “socially complex interventions” (SCIs) are blurry even in this formal definition, the MRC offers a useful heuristic: “The greater the difficulty in defining precisely what, exactly, are the ‘active ingredients’ of an intervention and how they relate to each other, the greater the likelihood that you are dealing with a complex intervention” (1).

Professionalized CAM modalities such as acupuncture and chiropractic fit well within the SCI rubric: they consist of what health services researchers Paterson and Dieppe call “complex packages of care” (1204; see also MacPherson, Thorpe, and Thomas; Mason, Tovey, and Long). Chiropractic, for instance, involves a multifaceted approach, no part of which is itself sufficient to care, including patient education, psychosocial counsel, sustained physical contact, spinal adjustment, and patient self-care (Oths). However, while CAM practices align with the SCI framework alongside more conventional ones (such as nursing), they remain strange enough within a mainstream medical context that their efficacy is often strongly suspect: skeptics often dismiss evidence of efficacy in CAM as the product of placebo effects rather than any real physiological effect. Recall, for example, Tom Delbanco’s dismissal of CAM cited in the previous chapter: linking CAM’s apparent efficacy to interactive effects such as increased

attention to patients' quality of life and needs for comfort, he confidently asserts in the *JAMA* theme issue that "academic medicine will soon shatter claims for activity [in CAM] beyond placebo" (1561).

Biomedical research tends to treat CAM's characteristically higher levels of practitioner-patient interaction as a potential contaminant, as something to be controlled for through innovative design. Research on CAM consequently sponsors a model of health care at odds with both the rhetorical conditions governing clinical practice and the guiding philosophies of CAM practices themselves. In this chapter, I follow the thread of practitioner-patient interaction through the varied theoretical geographies of CAM research, so to frame some of biomedicine's larger epistemic processes in rather more concrete terms. This process of re-framing biomedical CAM research within the context of practice vis-à-vis practitioner-patient interaction can, I believe, ultimately reflect something of the theoretical terrain of medicine itself.

The remainder of this introduction situates SCIs in relation to current theories of medical practice. The first section examines rhetorical interaction as a potential contaminant in medical research, while the second reflects, in particular, on what trials of CAM can tell us about what are usually thought of as placebo effects, which have strong links to practitioner-patient interaction. The third section considers patient agency and the limits of patient choice in clinical consultations, both mainstream and alternative. The fourth section further examines patient agency by focussing on discourses surrounding dietary supplements and the possibility that individuals use supplements, in part, to gain a sense of control over their own health. The awkward fit between medical research and the practice it is ostensibly meant to inform illuminates some of the disabling assumptions behind current theoretical approaches to health care. It also draws attention to the ways in which increasingly expansive views of health and

illness can simultaneously humanize health care delivery and bolster further medicalization of ordinary human life. As an extreme case within the larger domain of SCI research, then, CAM studies can amplify potential conflicts at the heart of medical research more generally.

The move in scholarship on health beyond a strictly biomedical purview—beyond, that is, a medical model that views disease as essentially organic, rooted in biological pathology—has unfolded awkwardly, shaped in great part by tensions between the evidence-based medicine and patient-centred care approaches to health care that gained currency in recent decades. These multidimensional models of practice have been the subject of much recent debate in medical and health policy contexts (e.g., Bensing; Elwyn and Edwards), particularly regarding how they fit in relation to each other. While patient-centred care (PCC) is a model of patient care, EBM provides a justification for clinical decision-making—it is a model not *of* professional practice but rather *governing* professional practice. Both EBM and PCC play a significant role in the testing of socially complex interventions.

The widespread impulse to test SCIs through randomized controlled trials, despite their poor fit with the methodology, derives from emergent trends toward risk management and accountability within the context of evidence-based medicine. This shift in turn sponsors the expansion of experimental methods into areas previously accessible only by other means (e.g., observation). As I argue in Chapter Two, the testing of interventions such as acupuncture became a major theoretical problem with the ascendancy of quantitative measures in health research. As EBM increasingly privileged data derived from RCTs, other methodologies began to fall short by comparison; through that process, EBM advocates consequently emphasized results derived from hard endpoints—“death, disease, and demography” (Committee 105)—over softer endpoints identified by subjective measures such as pain scales and patient diaries. This quantitative bent in

health research has contributed to the marginalization of practices that are not directly amenable to experimental methods. As nursing scholar Bronagh Blackwood notes, “A clear definition, the ability to control outside factors and standardization of the intervention are the cornerstones of the RCT” (612); socially complex interventions fail on all three counts, and more.⁷⁴ The marginalization of interaction-dependent interventions has led, in part, to the development of patient-centred care.

Health communication scholar Jozien Bensing notes that it is a bit strange to think of patient-centred care as merely a *model* of patient care, since everything in medicine is ostensibly done in the name of the patient. Yet, as rhetorician Charles Anderson has observed, while the patient is “the center of the medical event,” he or she tends, as a person, to be taken as “merely attached to the machine delivered up for repair” (6). Compounding this limited view of the patient-as-machine is a limited view of patient-as-interlocutor. In their landmark review of practitioner-patient communication, Ong et al. note that clinical encounters can frequently be rhetorically disabling for patients because they “involv[e] interaction between individuals in non-equal positions [and are] often non-voluntary” (903). In such a dynamic, anthropologist Christine Barry elaborates, “The voice of medicine has doctors maintaining control within a power imbalance. As a result the coherent and meaningful accounts of patients are suppressed,” leaving communication “disrupt[ed] and fragment[ed]” (489). For example, one survey of biomedical interviews found that that nearly 70% of physicians interrupted their patients’ opening statements within 18 seconds (Frankel and Beckman 88), while another found that doctors contributed at least 60% of the talk (Ong et al. 906). This asymmetrical discursive environment can impede a patient’s sense of agency in her own health care. Note, for example, the tenor of these current

⁷⁴ Critics vary in their emphases regarding the most significant problems posed by SCIs in RCT design, with the MRC and Lindsay isolating the problem of definition, van Weel and Knottnerus focussing on randomization, and Wolff identifying issues of standardization and control.

popular book titles: *How to Talk to Your Doctor: Getting the Answers and Care You Need* (Agnew); *What Your Doctor Really Thinks: Diagnosing the Doctor-Patient Relationship* (Blumer; emphasis in original); *Patient Beware! Dealing With Doctors and Other Medical Dilemmas* (Carver). The wide availability of these and similar self-help handbooks—even the American Medical Association has produced one (Perry)—signals that something has gone awry in doctor-patient communication.

Patient-centred care is predicated on the idea that we need to offset the technoscientific, doctor-oriented discourse that pervades much of medicine today, to restore the whole patient, as an individual agent, to the medical encounter. Epstein et al. define PCC as “care that is concordant with the patient’s values, needs and preferences, and that allows patients to provide input and participate actively in decisions regarding their health and health care” (1516). (Incidentally, this definition is not far off from the ideal of EBM. See, e.g., McCormack and Loewen.) Communication, broadly conceived, has been heralded as “the royal pathway to patient-centered care” (Bensing 23).⁷⁵ This view is corroborated by extensive empirical research on doctor-patient communication, such as Elliot Mishler’s pioneering work (and Stewart and Roter’s, and others’), which has demonstrated that communication is integral to successful medical encounters.⁷⁶ Patient outcomes have been positively correlated with, for example, increased encounter length, more detailed history-taking, and increased psychosocial talk, eye contact, and touching; outcomes have been negatively correlated with high levels of biomedical questioning, directive behaviour, and interruptions (see, e.g., Beck, Daughtridge, and Sloane).

⁷⁵ Siegfried Meryn writes in a 1998 *British Medical Journal* editorial: “Most complaints by patients and the public about doctors deal with problems of communication not with clinical competency. The commonest complaint is that doctors do not listen to them. Patients want more and better information about their problem and the outcome, more openness about the side effects of treatment, relief of pain and emotional distress, and advice on what they can do for themselves” (1922).

⁷⁶ Barry’s phrase “voice of medicine” in fact derives from Mishler, who differentiated that voice from the “voice of the lifeworld,” which represent, “respectively, the technical-scientific assumptions of medicine and the natural attitude of everyday life” (Mishler 14).

In CAM, communication is central to most clinical encounters and many practitioners consciously cultivate their skills in both their initial education and ongoing training. Tao Liu notes of acupuncture, for example, that the “treatment session is characterized, except for needle insertion itself, by the elaborate and comprehensive communication between the acupuncturist and the patient” (4). Similarly, MacPherson, Thorpe, and Thomas characterize acupuncture as fundamentally patient-centred, “based on a partnership model of interaction between practitioner and patient” nurtured through “establishing rapport with patients, active listening, and utilizing explanatory models” based on Chinese medical philosophy to create a common theoretical understanding of the patient’s condition and treatment (878).⁷⁷

Practitioners’ explicit interest in communication may contribute significantly to CAM’s growing popularity as incidence of chronic illness rises. Vincent and Furnham have identified that “concerns about communication with doctors” are among the most-cited reasons given by patients for seeking CAM (37). For example, Kathryn Oths argues, “Chiropractors are in demand because they supply what is often absent from overly logical and rational modern medicine—more social, psychological, and physical interaction, more listening, empathy, support, reassurance, and touching” (“Unintended” 88). Although I will suggest later in this chapter that CAM practices may not necessarily be as patient-centred as their proponents might suggest, their basic theoretical orientation nevertheless stands in sharp contrast to that of biomedicine regarding the priority accorded to practitioner-patient interaction.

Patient-centred care has its detractors too, most of whom rightly question the feasibility of implementing such a time-consuming model of practice in an age of managed care and fee-for-service programmes. And other concerns have been raised about patient-centred care

⁷⁷ Ho and Bylund point out that, although the quantity of talk in acupuncture practice may not always be high, the practitioner-patient relationship is still central to treatment, supported for example through nonverbal actions such as touching the patient’s body and listening to its sounds, and through its central focus on holism (513).

regarding pedagogy and training, trust and autonomy, institutional practices, and medicalization, which I reflect on briefly below. However, in the context of EBM, this model of care offers an important corrective to EBM's valorization of trial evidence—a valorization that physicians Feinstein and Horwitz worry will “produce inappropriate guidelines or doctrinaire dogmas for clinical practice,” wherein doctors “may often be diverted from the bedside to the library or computer terminal” (533). To what extent the patient-centred model of care translates into actual medical practice remains to be seen, however, as I discuss toward the chapter's end.

Rhetorical Interaction as a “Contaminant” in CAM Research

In this section, I examine how practitioner-patient interaction is transformed in randomized, controlled trials of acupuncture and chiropractic. Practitioner-patient relations are difficult to measure empirically in any medical model, mainstream or alternative, so it is difficult to develop an evidence base on matters of the interaction and its effect on treatment. However, as Chapter Two shows, disputes in the *JAMA-Archives* corpus over the validity of various CAM practices generally hinge on whether or not they have been tested in RCTs and, if so, on whether or not they have shown efficacy. In the context of biomedical boundary work, the difficulty of generating evidence on interactive effects in research on CAM elicits more than procedural questions (e.g., How can one measure the effects of clinical interaction in an RCT?): it also elicits boundary-defining epistemological questions that can ultimately cast the validity of the CAM practice itself into doubt (e.g., By what biological mechanism, if any, does the intervention actually work?). I argue that trials of acupuncture and chiropractic published in the corpus (Bove and Nilsson; Cardini and Weixin; Shlay et al.) document attempts to regulate rhetorical interaction between practitioners and patients in order to get at the so-called true knowledge that

may be gained from the studies. (I mean the phrase “rhetorical interaction” here in the expansive sense of all the ways in which people act on and with one another, consciously and not.)

By positing this interaction as a contaminant, a potentially confounding variable, the research proposes models of practice that conflict with what we know about the importance of practitioner-patient communication to patient outcomes (from, e.g., Beck, Daughtridge, and Sloane), and about why patients seek alternative practitioners (from, e.g., Astin). Attempts in CAM studies to control for biases and therapeutic effects arising from practitioner-patient interaction can be seen, more globally, as probes into the relationship in *mainstream* medicine between health care providers and patients, particularly in socially complex interventions. These probes can illuminate the interactional dimensions of some of biomedicine’s most intractable problems, such as patient compliance/concordance and overdependence on pharmaceuticals. This chapter closes with some reflection on these issues.

Among the primary reasons that patients give for choosing CAM is that they report receiving greater interpersonal attention, and that they find such practices more congruent with their own attitudes toward health and illness (Astin). For example, patients with chronic, functional symptoms, who make up the majority of CAM users, value the attention given in CAM not only to their diseases but, more importantly, to their illness experiences. Busby notes that people that feel their illness experiences have been “distort[ed]” within a medical lexicon tend to find CAM practices “more congruent with their experience of their own bodies, and lifeworlds” (qtd. in Barry et al. 489). Described in Mishler’s terms, the patient’s physiological condition does not generally take precedence in CAM but is, instead, deliberately situated within the larger context of his or her “lifeworld.”

Practitioner-patient communication lies at the centre of most discussions about why patients choose CAM. In the *JAMA-Archives* editorial materials that I discussed in the previous chapter, for instance, commentators such as Jonas, O’Sullivan, and Koo and Arain point to biomedical shortcomings in terms of clinical interaction as a driving force behind CAM’s popularity. More recent commentaries continue, and even strengthen, this theme. For example, Merijoy Kelner frames interest in CAM as largely interactive: that CAM practitioners are more empathetic and collaborative, and that the therapeutic relationship is a priority, fostering strong practitioner-patient rapport. Pappas and Perlman similarly cite longer appointments, more detailed explanation of illness and treatment, continuity of care, and “attention to personality and personal experience” as major attractions of CAM (3). They argue that patient satisfaction in CAM “often is not dependent on an improvement in the presenting complaint” (3), suggesting that symptom resolution is not the only reason why patients consult CAM practitioners, nor is it in many cases the most important. For patients with chronic conditions, this shift from quantitative assessment (lab tests, x-rays, scans) to qualitative assessment (questionnaires, interviews, diaries) makes sense because the conditions are typically ongoing. The trick, in such cases, seems not to be primarily to improve the chronic conditions but to improve patients’ *experience* of them. Ultimately, the appeal of CAM practices, as Kaptchuk and Eisenberg put it, may be that they provide patients “a participatory experience of empowerment, authenticity, and enlarged self-identity when illness threatens [patients’] sense of intactness and connection to the world” (“Persuasive Appeal” 1061).

In a biomedical framework, the kind of patient-centeredness found in CAM comes at the expense of evidence: the evidence base of practices such as acupuncture and chiropractic relies primarily on theory and anecdote, evidence not nearly sufficient to justify their use within an

EBM framework. (Note the widely circulated aphorism in EBM that the plural of “anecdote” is not “data.” See, e.g., Balmer and Blomhoff; Strom.) Evidence-based approaches to CAM reframe those practices within a biomedical terminology. In acupuncture for pain relief, for example, traditional Chinese medical theories of energy flow and blockage are replaced by biochemical and genetic mechanisms, such as endogenous opioids (Stener-Victorin et al.) and “gene expression of peptides for central pain control” (Ulett 1271). The Shlay et al. study of acupuncture and amitriptyline for HIV-related nerve damage described in the previous chapter is a case in point: it so carefully describes the acupuncture needle insertion and manipulation protocol but not its theoretical basis, only hinting in the article’s conclusion that its mechanism is likely biochemical: “the release of endogenous opioids or activation of other brain and spinal cord pathways that reduce pain” (1594). Stener-Victorin et al. argue that if we are to identify a *plausible* mechanism for acupuncture’s effect, we must do away with the meridian theory altogether; they shift the traditional Chinese explanatory model into a mechanistic framework, citing “ β -endorphin, an endogenous opioid with high affinity for the μ -receptor,” which is “released into the blood from the hypothalamus via the anterior pituitary” (1943).

These shifts away from traditional acupuncture theory reflect researchers’ anxiety to strengthen their ties to biomedicine by reframing the practice into a more amenable framework. This is a strategy long-adopted in, for example, the *American Journal of Chinese Medicine*, and, more recently, in the *JAMA-Archives* theme issues, as the previous chapter shows. In this process of reframing, however, the studies often end up eliminating the very qualities that draw patients to acupuncture in the first place. For example, George Ulett, writing in response to the Shlay et al. study, notes that physicians with conventional MD degrees are learning to perform office-based acupuncture, divorced entirely from traditional Chinese medical theory, in only a few

hours of training (as compared to the 1500 hours he cites as recommended for traditional acupuncturists; 1271). Physician-based acupuncture practice also tends to isolate acupuncture from the range of interventions typically provided concurrently in TCM, such as herbal therapies, cupping, dietary changes, exercise, moxibustion (burning herb rolls on acupuncture points), and massage. Patients seeking a holistic practice that emphasizes the primacy of the practitioner-patient relationship and the patient's individuality may not find the care they seek when needling is recontextualized within a biomedical framework. In such a case, we might think of the perspectives of doctors and patients as fundamentally misaligned.

The distance between the perspectives of doctors and patients can usefully be captured by Kenneth Burke's well known observation that language is not only a *reflection* of reality but also a *selection* and *deflection* of reality. As Burke says, "we can't say anything without the use of terms; whatever terms we use, they necessarily constitute a corresponding kind of screen; and any such screen necessarily directs the attention to one field rather than another" (*Language* 50). By examining the different "terministic screens" that permeate our interaction in human society, we can trace out how each kind of screen, in Burke's words, "le[ads] to a correspondingly different quality of observations" (49); those different observations, in turn, shape our relations with each other as symbolic agents.

The terministic screens of biomedical practitioners are shaped largely by an idiom of disease, within which patient care is predicated on the scientific model. Practitioners' expert knowledge enables them to re-present patients' subjective illness experiences in seemingly objective medical terms as a diagnosis (Cicourel; Hunter), and their treatments are also generally limited to that same idiom. Patients are not so restricted, however, and many will try a variety of

treatments, particularly those for whom mainstream medicine has not provided much relief.⁷⁸ Their terministic screens—the terms in which they see and understand their bodies, their health, and their health care—are determined more significantly by the experience of illness, including pain, suffering, fear, limited ability, interpersonal strain, and financial loss. That is, while the terministic screens of biomedical practitioners “direct the attention” toward explanations of disease as essentially organic, rooted in biological pathology, patients’ screens direct the attention toward how that disease plays out within individual bodies and lives. In the context of CAM, biomedical practitioners might, for example, dismiss an intervention such as chiropractic because its purported mechanism of action (the correction of spinal misalignment or “subluxation”) does not fit with their own theoretical “screens,” while patients may instead be concerned only about whether it will alleviate their pain or help them regain enough mobility to return to work.

Arthur Kleinman frames this disjunction between physician and patient perspectives as a problem of “conflicting explanatory models.” Explanatory models, Kleinman explains, are mental schemes for organizing and understanding the various dimensions of illness, particularly as they pertain to clinical decision-making. For any given episode of illness, there are several explanatory models—the doctor’s, the patient’s, the patient’s family—each of which may lead to different courses of action. Explanatory models are more pliable than terministic screens in the sense that they are fundamentally pragmatic, oriented to action, and can more easily be understood by those that do not hold them, while terministic screens shape the very ways that we “see” a given situation: we cannot see what is blocked by our terminology from view. These two

⁷⁸ For patients with conditions lacking clearly identifiable physiological causes (e.g., fibromyalgia, headache), or with chronic, degenerative conditions (e.g., arthritis), “objective diagnoses” are hard to come by—and, so, too, are helpful treatment options. Many such patients seek out alternative care, a trend recognized in many of the studies selected for the *JAMA-Archives* theme issues (which cover topics such as pain management and depression).

ways of understanding physician and patient perspectives can help unpack some of the rhetorical complexity of practitioner-patient interaction because, although the two frameworks overlap significantly, they highlight different dimensions of the clinical exchange. Terministic screens can illuminate deeply held conceptual understandings of a given health problem (e.g., what it is, what it means), while explanatory models can trace participants' somewhat more flexible beliefs about what to *do* about that problem. Phrased another way, while explanatory models determine what we think *about* in health care contexts (such as courses of action), terministic screens determine what we think *with*.⁷⁹

To return to my example of chiropractic's theory of action, physicians might not be able to identify with their patients' interest in seeing a chiropractor because their biomedical training screens from view the possibility that spinal subluxation contributes to illness. However, by eliciting their patients' explanatory models of their conditions, they might better come to appreciate the reasons for that interest and, with that appreciation, better negotiate regarding future courses of treatment. The reverse is true as well: patients not limited to a biomedical screen may not identify with their physicians' skepticism but, if privy to their explanatory models, might more easily accept that skepticism as rooted in valid reasoning, which could also influence treatment decisions.

Although Kleinman does not himself ascribe a rhetorical function to explanatory models, he demonstrates how they can persuade—and fail to persuade, as happens frequently in the care of chronic illness. For example, the physician who does not explain the particulars of his or her own explanatory model, who takes that model as already known, and even accepted, by the patient, will fail to persuade the chronically ill person in search of a cure about the limits of

⁷⁹ Kleinman describes explanatory models as “justifications for practical action more than statements of a theoretical or rigorous nature” (121). We might, I submit, view terministic screens as producing the latter sort of theoretical statement. Both have theoretical and actional components, but each emphasizes one more than the other.

medical treatment. Similarly, the physician that does not take the patient's explanatory model of his or her own illness seriously risks alienating him or her and "undermin[ing] the communicative foundations of care" (122).⁸⁰ This is the thinking that underlies the *concordance* model of care, the rhetoric of which holds that, unlike the compliance model where patients are expected to follow "doctor's orders," patient preferences and beliefs must be taken into account in clinical decision-making.

Patient perspectives often receive short shrift in biomedicine, where priority is generally accorded to physicians, who hold a wealth of cultural capital. This capital is transformed into *linguistic* capital through the generic structures of medicine itself, such as the regulated, and regulating, ways in which doctor-patient encounters unfold. Kleinman illustrates how clinical encounters can be particularly troubling for patients with chronic conditions because their illness experiences, themselves a variety of expertise, are routinely overlooked by those physicians that interrupt and redirect their patients' attempts to convey their explanatory models. Such interruptions and redirections, he suggests, derive from the physician's effort to glean the information that his biomedical explanatory model deems most salient, in effect, crowding the patient out of her own care. In such cases, Kleinman notes, the physician ultimately conveys to patients the message that "your view doesn't really matter much; I am the one who will make the treatment decisions; you do not need to be privy to the influences and judgments that inform those decisions" (130). For patients with chronic illness, the discounting of their own expertise

⁸⁰ Note that in both these examples, which are Kleinman's own, the focus is doctor-centric: in the first case, the doctor must persuade the patient to view his or her condition within the limits of the biomedical explanatory model, while, in the second, the doctor must elicit the patient's own explanatory model as a means of securing his or her cooperation in medical treatment. Kleinman does not explore patients' persuasive potential as interlocutors, which may in part be a reflection of professional bias (he is himself a medical practitioner, a psychiatrist), but may also be the product of pragmatic assessment, on his part, of the weaker rhetorical position that patients occupy in medical contexts. The onus in his book, *The Illness Narratives*, is squarely on practitioners to effect changes within medical practice to the benefit of patients; patients themselves are afforded no such agency.

on their conditions not only compromises the quality of their care—it may lead them to seek care elsewhere.

The discursive shape of a practice such as chiropractic diverges radically from this model. While the evidence in the doctor-patient communication literature overwhelmingly suggests that biomedical patients are expected to adapt to the physician’s explanatory model, there seems in chiropractic to be a stronger impulse to bridge the gap between the perspectives of practitioner and patient. Kaptchuk and Eisenberg argue, for example:

Chiropractic finds its voice exactly where biomedicine becomes inarticulate. Too often, biomedicine fails to affirm a patient’s chronic pain. Patients think their experience is brushed aside by a physician who treats it as unjustified, unfounded, or annoying, attitudes that heighten a patient’s anguish and intensify suffering. . . . Chiropractic’s ultimate lesson may be to reinforce the principle that the patient-physician relationship is fundamentally about words and deeds of connection and compassion. (“Chiropractic” 2221-22)

Framed in Kleinman’s terms, chiropractic excels at something that biomedicine generally does not: affirming, and even working within, the patients’ own explanatory models.

In her study of communication at a chiropractic clinic, anthropologist Kathryn Oths found that chiropractic’s overall tenor is more relational than instrumental, particularly after the initial consultation phase. Once the treatment protocol had been identified, she notes, practitioner-patient exchanges became increasingly more affective, with the chiropractor asking after patients’ home and work lives as he performed his spinal adjustments (“Communication” 93). Oths maintains that this discreet “amiable chatting” about patients’ lives is not meant simply to pass the time while the practitioner administers his physical treatment (“Unintended” 107);

rather, she suggests, affective talk in chiropractic operates, often unintentionally, as what Kleinman et al. call “a culturally disguised form of psychotherapy” (qtd. in Oths, “Unintended” 88).

Oths cites the ongoing clinical relationship, maintained primarily through affective talk, as the key to elevated levels of patient satisfaction and to chiropractic’s perceived efficacy. Her explanation of the therapeutic functions of affective talk runs along several axes: that it helps to address the psychosomatic dimensions of chronic conditions (e.g., back pain, headache); that it increases patients’ feelings of control over their illnesses and environments; that it provides vital social support by instilling a sense of hope and encouragement; and that it makes patients feel integral to their care rather than mere recipients of it. What Oths does not spell out in the course of her analyses, however, is that all of these dimensions are also importantly persuasive. She approaches, at times, the suggestion that the combination of these affective effects may contribute to patients’ compliance and continued treatment, but she stops short of suggesting that practitioners may capitalize, if unwittingly, on their patients’ explanatory models. For example, by discussing patients’ health within the context of their work lives (e.g., “Did you change shifts yet?” “Did you get your raise?”) and home lives (“How were the holidays?” “Did you talk it over with your wife?”), chiropractors do (at least) two things at once: they affirm patients’ lived experience of chronic illness within the context of their everyday lives, which may serve a therapeutic function; and they affirm their own position as a practitioner that “cares” and “listens,” serving their own professional interests by securing the continued patronage of patients attracted those aspects of care.⁸¹ While it may not be a conscious strategy, affective talk in the chiropractic clinic may, then, be as much a part of chiropractic’s sales pitch as it is a part of its model of care.

⁸¹ The quoted phrases in this sentence are from Oths, “Unintended” (107).

One potential drawback of incorporating high levels of affective communication into chiropractic, argues Oths, is that it could put patients on the defensive by imputing a psychosomatic dimension to their condition—something like, “there’s nothing wrong, it’s all in your head” (“Unintended” 108). However, this affective dimension seems to be enabled largely by the rhetorical work accomplished in the initial consultation phase. Chiropractic care typically involves an “intake” phase at the start of a clinical relationship, to assess the patient’s orthopedic health through, for example, range-of-motion tests, visual assessment, questionnaires, x-rays, and scans, and to outline a treatment plan in consultation with the patient. Education about chiropractic theory is also central to this process. Major chiropractic concepts are demonstrated on patients’ bodies and replicas of joints, as well as through films, posters, and handouts, all of which promote a chiropractic understanding of health and illness. This rapid and intense orientation to care seems effectively to align the patient’s explanatory model with that of the practitioner. Through this process, Oths concludes, “In essence, the chiropractor first manipulates a patient’s belief structure before setting about to manipulate his or her physical structure....A congruity between patient beliefs and behaviors gives a certain unity to the chiropractic experience, securing patient faith in and adherence to the system of therapy” (“Communication” 91).

There seems, then, to be a double shift in chiropractic, where the patient’s beliefs about her condition and her expectations for treatment are given priority in their own right *and* brought into closer alignment with practitioner’s explanatory model, even though their underlying terministic screens may remain distinct. The distinction between explanatory models and terministic screens becomes important here because it makes it possible for a patient to believe in the practical effects of an intervention without necessarily subscribing to its more deeply seated

philosophical foundations. For example, a patient can believe in the idea that manipulating the skeletal structure can restore mobility and improve health (an explanatory model) without believing in chiropractic's underlying concept of "Innate Intelligence" (a terministic screen), the God-like force professed to be released with the correction of subluxation (Keating 79). This double shift in chiropractic care, which harmonizes practitioner and patient explanatory models, seems to create fertile ground for a productive, ongoing therapeutic relationship by fostering between chiropractor and patient what Chaim Perelman calls a "meeting of minds." Perelman argues that "the aim of argumentation is not to deduce consequences from given premises; it is rather to elicit or increase the adherence of the members of an audience to theses that are presented for their consent. Such adherence never comes out of thin air; it presupposes a meeting of minds between speaker and audience" (9-10).

When chiropractic is adapted to research contexts, these efforts at practitioner-patient alignment falter: the comprehensive intake procedures standard in chiropractic care are not typically replicated in trials and the therapeutic potential of practitioner-patient interaction is controlled for by various means. The trials may consequently inhibit the development of that crucial common ground between practitioner and patient, which Oths links directly (though not wholly) to chiropractic's efficacy. In the *JAMA* theme issue, Bove and Nilsson, for example, compared spinal manipulation with a placebo laser treatment for headache, emphasizing that the treatment arms were indistinguishable beyond the isolated spinal adjustment. As in the other RCTs in the *JAMA-Archives* corpus (e.g., Cardini and Weixin; Shlay et al.), this study appears to have placed participants into assigned groups without providing further background or theoretical information about the interventions they would receive, overlooking the role that a meeting of minds, established in those crucial first appointments, might play in the intervention's

success. Similarly, the authors ruled out the evaluation of an affective dimension in chiropractic care by “controll[ing] for the treatment elements of personal attention and hands-on treatment” across both the intervention and control groups (1579). Identifying this design feature as one of the trial’s strengths, Bove and Nilsson assert that spinal manipulation is no more effective than placebo.

What Bove and Nilsson cite as one of their study’s strongest features—that they have isolated spinal adjustment itself as the “active ingredient” in chiropractic care, with all other aspects of the trial arms identical—becomes a critical weakness within the domain of research on socially complex interventions. Such a view of chiropractic is troublingly reductive in the context of the practice as a “complex package of care” (see Paterson and Dieppe). And yet, importantly, research on CAM also seems to have the potential for an ameliorative effect on some of the more programmatic aspects of EBM: trials of practices such as acupuncture and chiropractic have necessitated a number of methodological innovations that have led to more expansive discussions about how medical knowledge is produced, and for what purpose.

One such example is the need to control for placebo effects, which occur in clinical trials when participants report, or researchers observe, improvement in the absence of an active intervention. The idea of the placebo effect as mere statistical noise, a product of overactive imaginations, is pervasive, often linked to what I have called the contaminating effects of practitioner-patient interaction. But the placebo concept is being revisited in some quarters of medical research, particularly in studies of acupuncture. These studies highlight the importance of what were formerly considered, dismissively, the “nonspecific” aspects of care—aspects not linked directly to a known active ingredient. The investigation of placebo effects has contributed

to new understandings of how practitioner-patient interaction can shape health outcomes, not only in CAM but in socially complex interventions more broadly.

Placebo Controls and “Nonspecific” Effects

Pharmaceutical trials are usually straightforward to control and blind because they can employ identical placebo pills, so no one but the study’s pharmacist need know to which trial arm any participant belongs. With both participants and researchers blinded to treatment assignment, the results are unlikely to be contaminated by placebo effects because all participants would receive the same baseline “amount” of placebo. Writing on the use of placebos controls in research on psychotherapy, Herbert Simons suggests that the notion of the placebo itself is based on a tension between the “real” and the “rhetorical” (in the colloquial sense), which parallels the tension that has underscored rhetoric since ancient Greece. He observes that, “In the medical setting the placebo serves as a benign counterfeit, a form of deception or misrepresentation that trades on its metonymic resemblance to something real. The placebo, then, may be understood as a species of persuasive message in the sophistic sense of offering ‘mere’ rhetoric, ‘empty’ rhetoric, form without substance” (“Psychotherapeutic Placebos” 112).

The problem, in socially complex interventions, is that separating out the “real” to render a placebo as “form without substance” is a conceptually impossible task. While a sugar pill can take on the appearance of pharmacologically active pill without taking on its pharmacological activity, how does one feign the appearance of an “active ingredient” in a nursing intervention or a CAM practice when that active ingredient is unknown? Further, supposing that practitioner-patient interaction were isolated as the active ingredient, how would one devise a placebo of

“interaction,” a sham intervention that takes on the appearance of interaction without consisting of actual interaction?

Blinding a practice such as acupuncture is complicated by various factors, most significantly because it is difficult to simulate a needle that is visibly inserted into the skin and because practitioners cannot be blinded to participants’ intervention assignment, since they necessarily must know whether or not to insert needles. Even if the outcome were assessed by an independent evaluator, the possibility remains that the acupuncturist could communicate nonverbal cues to the patient about his or her group assignment. Further, acupuncture’s model of individualized care must be altered within research contexts: in practice, acupuncturists’ idiosyncratic skills and approaches are regarded as an asset, and they engage in a patient-specific, evolving process of diagnosis and treatment, but these approaches are difficult to standardize across multicentre trials.

Various methods have been developed in acupuncture trials to try to get around the first problem, of finding plausible controls, but all introduce new problems. For example, sham acupuncture, where needles are inserted into non-traditional points at shallow depths, may not be inert because the inserted needles might have untraceable effects of their own. Active controls (which use the standard of care, usually a pharmaceutical, as a comparator) and no-treatment controls (where participants in the control group receive no intervention at all, placebo or otherwise) eliminate the *relational* dimension of acupuncture, so they do not serve as proper placebo controls, narrowly construed.⁸² These sorts of methodological problems have frustrated researchers by limiting the validity of their results. With sham acupuncture, for instance,

⁸² The key feature of a placebo control is that it remains identical in all aspects to the study intervention beyond the active ingredient. Pharmaceutical placebos, for instance, are matched in taste, colour, size, texture, and dosing regimen to the intervention drug. The Shlay et al. study described in the previous chapter combined sham controls and active controls in its three-armed study of acupuncture for HIV-related peripheral neuropathy.

needling is isolated as the so-called active ingredient and all other aspects of each arm remain equal (much like spinal adjustment in Bove and Nilsson); if the intervention's effects lie in one or more of those other aspects (described next), then the trial would not be able to pick up the difference. That is, if practitioner-patient interaction contributes to acupuncture's efficacy, then trials that isolate needling would be unable to trace those interactive effects because participants in each arm would experience the same quantity and quality of interaction. Such a design may easily lead to a false negative result (Paterson and Dieppe); in such cases, the lack of proof of an intervention's effect is often taken as proof of a lack of effect (Stener-Victorin et al.).

Anxieties about avoiding placebo effects run high throughout the *JAMA-Archives* theme issues. Lynn Lamberg, for example, reports in *JAMA* of one researcher's hope that a recent, successful, placebo-controlled study of light therapy for winter Seasonal Affective Disorder will dispel misconceptions about the light treatment by physicians who view it as "not molecular enough, a bit too Californian-alternative, a bit too media overexposed, merely a placebo response by mildly neurotic middle-aged women who don't like nasty drugs" (qtd. in Lamberg 1558). This idea of placebo effects as the product of overactive imaginations is pervasive, leaving Francisco Tausk, in *Archives of Dermatology*, to ask of alternative medicine: "Is it all in your mind?" Like Tausk, O'Sullivan et al., writing in the same journal, attempt to reframe the placebo concept by postulating a link between psychosocial factors and "neuro-immuno-cutaneous-endocrine" processes (1431). By explaining this somewhat mysterious phenomenon in thoroughly biomedical terms, these authors bridge the apparent conceptual divide between arguing for increased attention to (and even active solicitation of) placebo effects, on the one hand, and urging more rigorous, placebo-controlled studies of CAM on the other.

Subsequent commentaries by Ted Kaptchuk and Paterson and Dieppe have queried the wisdom of the placebo control itself, suggesting that the quest to determine an intervention's "fastidious efficacy" (Kaptchuk, "Placebo Effect")—its proof of efficacy under rigidly controlled conditions—comes at the cost of its clinical significance. The notion of the placebo, they say, also teaches patients (and their physicians) to be suspicious of their bodies, and of their overall sense of wellness, thereby potentially undermining the health care they seek.⁸³ (Importantly, there is no concept of a placebo effect in any CAM practice that I know of—there is no division between real and illusory effects.) The theoretical questions precipitated by the design problems of acupuncture and other CAM trials have prompted innovative work on these effects, fostering a greater appreciation of their variety: rather than sending any and all sorts of "nonspecific effects" into the placebo wastebasket, this research (and similar research in, for example, nursing; see Lindsay) has allowed researchers to consider these effects' therapeutic potential and to develop multileveled trial designs that can get at some of their clinical nuances, rather than to try to control for them.

Conventional thinking about treatment effects typically aligns them under two heads: *genuine* and *placebo* effects; this dichotomy serves as a terministic screen that greatly restricts other ways of understanding the connections between health interventions and the conditions under which they operate.⁸⁴ Although this division of effects is widespread in biomedical understandings of efficacy, much recent scholarship has indicated that it is "not meaningful to

⁸³ Canadian columnist Stephen Strauss documents exactly this process when he writes of cortisone injections he had recently received: "But as I was starting to feel better I had a doubt: Was the relief due to real medicine or was I feeling merely the placebo effect? And what difference would that make? And what if painlessness were 20-per-cent placebo effect, 80-per-cent cortisone's healing power?" (¶ 3).

⁸⁴ Paterson and Dieppe frame this conventional binary as the division between *characteristic* (i.e., specific) elements—treatment factors that are rooted in theory and are both specific to and causally linked with health outcomes—and *incidental* (i.e., placebo, nonspecific) elements—"the many other factors that have also been shown to affect outcome, such as the credibility of the intervention, patient expectations, the manner and consultation style of the practitioner, and the therapeutic setting" (1202). Kaptchuk correspondingly frames these effects as the difference between "fastidious efficacy" and "performative efficacy" ("Placebo Effect").

split complex interventions” this way (Paterson and Dieppe 1202). New biomedical research models have been emerging since the 1998 *JAMA-Archives* theme issues as a result of these and similar discussions. For example, prominent journals such as *Annals of Internal Medicine*, *British Medical Journal*, *The Lancet*, and the *New England Journal of Medicine* have all featured essays extrapolating the problem of identifying placebo controls in CAM research to socially complex interventions more broadly construed.⁸⁵ Most of these models accord with what Nahin and Straus call the “whole system” approach (162), an approach that reinstalls interventions into their contexts of care, putting the whole package to the test rather than simply isolating the so-called active ingredient.⁸⁶ This process appears to shift the research enterprise dramatically by introducing within it principles of patient-centred care.

These new models are grounded, conceptually, in a more textured view of the purpose of medical research. In its *Framework* document on testing SCIs, for example, the (UK) Medical Research Council asks, “Why is it essential to know how (by what specific ingredients) a complex intervention is effective?” (2). Research design, the MRC argues, ought to depend foremost on the answer to that question, consequently limiting the power of a single study to investigating only *whether* a practice is effective or *why* it is so, but not both. The consequence of this limitation is that, in asking whether a practice works, so-called fastidious design may actually hinder rather than help because pieces of the intervention would necessarily be abstracted from the whole. (In the Bove and Nilsson study, for instance, spinal manipulation is isolated from all of the other factors characteristic of chiropractic care, any number of which could affect its apparent efficacy.)

⁸⁵ See, respectively, Kaptchuk, “Placebo Effect”; Nahin and Straus; Kaptchuk, “Powerful Placebo”; Hróbjartsson and Gøtzsche.

⁸⁶ In the case of acupuncture, it is worth noting that needling and practitioner-patient interaction is typically accompanied by a combination of herbal therapies, cupping, dietary changes, exercise, moxibustion (burning herb rolls on acupuncture points), and massage.

With the research enterprise framed in separate streams of “whether” and “why,” van Weel and Knottnerus argue, randomization makes more sense at the level of the practitioner or clinic, rather than that of individual participants. The aim of such a design, they say, would be to study the intervention’s “gross clinical effect” rather than its constituent parts:

This context brings back the generic, non-specific effects of the doctor-patient relationship that represent important interventions for the outcome of care. To designate those effects as a placebo effect...can remove such basic components from the evaluation by simply blinding or controlling for it. However, it is more appropriate to assess these effects in terms of the specific effects of interventions, and that is where a multilevel design can be helpful. (van Weel and Knottnerus 917-18)

Such a design reframes what I have called “rhetorical interaction,” the ways that people act on and with one another as symbolic agents, as a key constituent of socially complex interventions, rather than as a potential contaminant. This more expansive view of the practitioner-patient relationship has the potential to enhance the role of interaction effects in assessments of what counts as effective health care.

A 2008 study from the *British Medical Journal* signals that these new research models have started to bear fruit. Kaptchuk et al. investigated whether or not placebo effects can be divided, experimentally, into responses to three distinct elements of care: assessment and observation; a “therapeutic ritual (placebo treatment)”; and “a supportive practitioner-patient relationship” (999). The multilevel study’s design sought to differentiate among these possible effects by increasing and combining them over time. The trial arms consisted of a wait list, a placebo treatment option (a non-piercing sham acupuncture device) with limited practitioner-patient interaction (initial consult of no more than five minutes), and the same placebo treatment

augmented with increased practitioner-patient interaction (initial consult of forty-five minutes, with the practitioner demonstrating “warmth,” active listening, and concern for the patient’s illness experience). The study’s finding that nonspecific effects could indeed be divided into components and then combined in “graded dose escalation” corroborates the longtime claims of patient-centred care advocates that clinical interaction can itself have therapeutic value.

For example, Kaptchuk et al.’s conclusion that the practitioner-patient relationship is the “most robust” of these components (999) seems like something of a triumph for commentators such as Feinstein and Horwitz, who worry that the significance of the clinical relationship may be lost in the sweep of evidence-based methods. Of course, one could argue that the *kind* of clinical relationship at the heart of the patient-centred care is transformed beyond recognition just by virtue of its being installed within an experimental framework. Pat Bracken, for example, criticized the trial for subjecting nonspecific treatment effects to “the same positivist tools” used on specific effects (§ 3). Kaptchuk accounts for such criticism, countering in a follow-up letter that, in an evidence-based culture, we can and must “be specific about non-specifics,” regardless of how theoretically top-heavy such studies can be (Authors’ Response § 2). There is a certain incongruity in Kaptchuk’s pragmatism here—advocating the need to subject elements of care often considered beyond (and even antithetical to) the reaches of evidence-based methods to those very same methods—but the risk he and his co-investigators take is calculated: the only assured means through which nonspecific effects in health care can assume significance in an evidence-based hierarchy is to be validated *within* one.

Research on placebo effects, such as Kaptchuk et al.’s, has several important but conflicting implications in the context of biomedical boundary work. On the one hand, it widens the scope of clinical behaviours that may be deemed “effective,” making space for relational or

interactive effects alongside more straightforwardly validated interventions such as pharmaceuticals. This expanded scope could have a limiting effect on arguments that seek to invalidate practices such as acupuncture and chiropractic on the ground that their efficacy derives from “mere” placebo effects. Placebo research also seems to have potential for improving the lot of patients as both medical and rhetorical subjects because it may make it harder to dismiss nonspecific effects summarily as “imaginary,” the product of confused or gullible minds. Patients whose own bodily perceptions can be more reliably trusted make more persuasive patients. At the same time, however, studies such as Kaptchuk et al.’s end up strengthening, and even expanding, biomedical boundaries by affirming biomedicine’s epistemic and professional jurisdiction over an (expanding) array of health-related matters.

To be sure, Kaptchuk et al.’s pragmatism is necessary for their study to have credibility within the mainstream medical community. However, the tradeoffs for this pragmatism might come at high cost for both socially complex interventions and patient-centred care: once placebo effects have been broken down into constituent parts, the argument for evaluating SCIs as whole “packages of care” may be harder to make, since each layer of the intervention might conceivably be peeled back and tested separately. Further, the experimental isolation of specific dimensions of clinical care, and the subsequent validation of some and invalidation of others, could lead to the very same piecemeal or algorithmic approach to clinical practice for which EBM has been heavily criticized. Experimental confirmation of the effects of “warmth” or “active listening,” for example, could conceivably lead to clinical practice guidelines mandating the performance of these “proven” behaviours in patient consultations.

These potential concerns aside, Kaptchuk et al.’s validation of interaction effects as more than simply imaginary hooks into a larger movement in health research toward situating patient-

centred modes of practice and evaluation within an EBM framework, the same movement within which biomedical CAM research first gained its impetus. Patient-centred care's relentless focus on placing the patient, and not just his or her disease, at the centre of the medical encounter can shift our attention from biomedical knowledge to the conditions of its production. Studies of CAM can be instructive because they make the normally tacit procedures of medical research more readily open to inspection, since there is no established method of proceeding. By following such studies, we can, in essence, watch these boundary-negotiating methods unfold.

While Kaptchuk et al.'s point-of-entry into this process is at the front end, through the conceptual work in the design phase of attempting to isolate distinct components of nonspecific effects, others have focused instead on the back end, where innovations lie not so much in design but in evaluation, such as developing more appropriate assessment tools (i.e., outcome measures).⁸⁷ These kinds of projects represent a major shift in thinking about the place of patients within health research because they tweak the RCT format into a model more amenable to aspects of care not easily captured in conventional studies. However, even in the face of methodological innovation, the patient-centred approach fits only awkwardly within the realm of practice, whether that practice is evidence-based or not. Moreover, when examined more closely, EBM and PCC start to be compatible in perhaps surprising ways—ways that not only enable the development of a hybrid model, evidence-based patient choice (EBPC; see, e.g., Edwards and Elwyn, eds.), but also constrain it. In the context of biomedical-CAM boundary work, the EBPC model seems poised to serve as a template for doctor-patient consultations about CAM as

⁸⁷ Ritenbaugh argues, for example, that outcome measures in CAM trials are not adequate to the range of possible effects patients may experience, particularly in practices that are believed to have “an energetic or spiritual component” (¶1). The focus of her National Institutes of Health-sponsored project, currently underway, is to develop and test a “CAM whole person outcomes questionnaire,” which she hopes will better account for CAM's variegated and as-yet-unmeasurable effects.

research findings accumulate. As a template for interaction, EBPC can illuminate some of the potential implications for practice of biomedical studies of CAM.

Patient Choice in Biomedicine and CAM

To return to Epstein et al.'s definition cited in this chapter's introduction, the patient-centred care construct is predicated on the idea that providing health care "concordant with the patient's values, needs and preferences" will ultimately improve the overall health of persons. A large body of research indicates that patient-centred methods do result in material gains for many patients, particularly when they foreground practitioner-patient communication (see, e.g., Ong et al.; Stewart; Stewart and Brown). However, I want to suggest here that PCC's applicability is less assured under the rhetorical conditions governing clinical practice than its proponents suggest. There seems, that is, to be some slippage between PCC as a theoretical model and the enactment of that model in practice. I asked earlier in this chapter about the extent to which the patient-centred model of care translates into actual medical practice, and I am not sure that it does fully, or that it even can.

For example, patient-centred care's emphasis on the patient as an equal partner and primary decision-maker does not mesh well with biomedicine's institutional-/professional-discursive practices, which frame patients as ultimately scenic, in Kenneth Burke's terms—as sites or containers of disease rather than as autonomous agents (*Grammar*; see also Martha Solomon). These processes operate systemically in biomedicine: through genre, as rhetoricians Schryer and Spoel have shown with medical-professional identity formation; through conversation, as linguist Elisabeth Gülich has shown with expert-nonexpert exchanges in medical contexts; and through lexico-grammatical processes, as physician Perri Klass has shown

in medical training. On all of these discursive levels, patients are situated firmly as the objects and subjects of medicine, and not as agents within it. Judy Segal observes that a model of care that equalizes doctors and patients “charges patients both to seek advice and to decide whether to take it, while the resources to make the decision may be locked inside the advice itself” (*Health* 144). Such a model, she explains, “presumes that physicians who otherwise continue to work in an old paradigm are able to equip patients as decision makers in a new one” (145). As Segal shows, making medicine more patient-centred is a matter of more than simply giving doctors lessons in communication: it would necessitate substantial conceptual shifts in medical research, funding, training, planning, implementation, reimbursement, and more.

Patient-centred care is also somewhat difficult to square with medical practice from the perspective of patients: not all patients want to be in charge of their own health care, and some want to have only limited input. Swenson et al. found, for example, that nearly a third of patients in their study preferred directive care over patient-centred, particularly older patients and those with life-threatening conditions (see also Bensing 23). The patient-as-decision-maker model of practice applies an essentially economic metaphor to medicine, basing its emphasis on rational choice on a free market model in which “the typical autonomous agent seems like a sovereign customer with a coherent shopping list and a fat wallet in a well-stocked market” (qtd. in Elwyn and Edwards 3). But when patients seek health care, whether mainstream or alternative, they are not dispassionately shopping for groceries—they are seeking expertise, whether for advice, authorization, or treatment. And they seek that expertise within a power imbalance, where the notion of *choice* might represent, to many, a kind of abandonment. *Autonomy* might, under this valence, erode trust and compromise individuals’ feelings of control and confidence over their states of health.

The patient-as-decision-maker model may be especially problematic for those that prefer a more active role in their health care. Patients with chronic, functional conditions—which make up the majority of CAM users and are the most likely to access SCIs—certainly may value a model that places them squarely in charge of their own care but, even then, the actual degree of autonomy afforded may be less than advertised. There is some question, that is, as to what extent patient-centred care *really is* about the patient, or whether, as Segal argues, it is not also largely about better disposing the patient, through a dynamic interplay, to follow medical advice. Segal’s work on debates in the medical literature about models of compliance versus concordance is instructive here (“Compliance”; *Health*). She argues that concordance is touted as a model of collaborative medical decision-making but is essentially a dressed-up version of compliance, the paternalistic model of following “doctor’s orders.” The goal even in concordance models remains to persuade patients to adhere to prescribed courses of treatment. Indeed, as Segal argues, the very ability to offer choices at all invokes a kind of rhetorical authority unavailable to patients and bespeaks a residual doctor-knows-best mentality at work in even the most egalitarian of consultations.⁸⁸

As key terms, “choice” and “autonomy” figure prominently in discussions of patient-centred care and CAM but the possibility that a patient could choose wrongly is equally pervasive. Writing in the *JAMA* theme issue, for example, Sugarman and Burk urge doctors to practice shared decision-making with their patients that choose CAM, but they maintain that it is ultimately incumbent upon doctors to persuade their patients to make the *right* choices—to lead them, that is, toward “accepted” and “legitimate” health-related goals (1624; 25). Similarly, Oths

⁸⁸ Ironically, autonomy depends, in part, on its being granted by an authorizing source. Writing in the 1998 theme issue of *Archives of Dermatology*, for example, Robert Thomsen instructs physicians to “give your patient permission to pursue other forms of health care” (1446). Most will do it anyway, he says, so physicians will retain better control over the situation by preventing patients from seeking CAM behind their backs, thereby maintaining their own credibility as authorities.

writes of how chiropractors steer patients away from “unacceptable behaviors” through positive reinforcement and covert psychotherapy (“Unintended” 108), but the very idea of a patient’s behaviour requiring acceptance runs counter to the rhetorics of self-determination and non-judgemental care of practices such as chiropractic.

Much of what looks like choice in chiropractic may not, in the end, stem from mutual decision-making between practitioner and patient, but from the chiropractor employing explanatory frameworks persuasively. A patient’s sense of herself as an equal participant within that restricted purview might be to a great extent illusory as certain choices come to seem natural and inevitable—not as *preferred* options but as the *only* ones. Stewart and Brown corroborate my earlier claim that the chiropractic intake phase creates fertile ground for persuasion when they observe that, in biomedicine, “What clinicians often call ‘patient non-compliance’ may in fact be a patient’s expression of disagreement about treatment goals. Hence the patient’s adherence to the treatment plan is often contingent on finding common ground” (107). This is the sense in which professionalized CAM practices such as chiropractic and acupuncture may not be as patient-centred as their accompanying discourses might suggest, because what looks like patient choice and autonomy may be more paternalistic in orientation than it first appears.⁸⁹ As Segal explains, “This is not to say that the *appearance* of consultation-between-equals would not be a good persuasive strategy. But, in some cases, a strategy is all it would be” (“Compliance” 88).

In the last decade or so, numerous critics have advocated bringing EBM and PCC into a more formally integrated model of care, which has become known as evidence-based patient choice (EBPC). This model’s proponents maintain that EBPC combines the best of the two

⁸⁹ See, for example, Ho and Bylund, who argue that, in acupuncture, models of health (biomedical, biopsychosocial, holistic) and models of interaction (paternalism, consumerism, collaboration) are separable. While acupuncture is centrally premised on a holistic model of health, they note that, despite popular beliefs about patient autonomy in acupuncture, in practice, acupuncturists are split in adopting collaborative and paternalistic approaches.

approaches, and to some extent this is true. Elwyn and Edwards note, for instance, that the kind of choice evinced in the PCC literature can be “perversely impossible to manage” (9), and so, by couching the available options within the evidence base, practitioners can offer fewer yet potentially more effective interventions for patients to choose from. However, I would like to suggest that this new model is not so much a step forward in thinking about the connections between medical research and practice, particularly regarding SCIs and chronic illness care, but a sign that the thinking has stalled. To the extent that health research agendas have expanded beyond a narrowly biomedical purview, the problems that remain end up amplified within the more expansive, hybrid theoretical context of EBPC.

As in PCC, for example, *choice* in this model is frequently anything but a matter of choice. Ford, Schofield, and Hope maintain that EBPC entails “providing patients with evidence-based information in a way that facilitates their ability to make choices or decisions about their health care” (589) but, as Segal has shown, decisions within such a framework are not necessarily freely made. The manner in which choices are presented—their “framing effect,” as Ashcroft, Hope, and Parker call it (61)—can, for example, render patients more likely to choose some options over others simply by how they are presented. These framing effects are particularly powerful when taken in the context of the rhetorical dynamics of practitioner-patient consultations, where even the most egalitarian of practitioners holds the balance of power by virtue of her expert authority—her ability to diagnose and treat illness. (For extended discussion of expertise and medical decision-making, see Segal, ““Compliance””; *Health*.)

Choice is limited further upstream in EBPC as well, at the level of evidence-production. One of the reasons that Ashcroft et al. give for refusing patients their choice is that “what the patient wants is futile—the treatment is ineffective” (57). But this reasoning simplifies the

complex process of determining efficacy, a contingent exercise that I showed in the previous chapter is, in part, explicitly rhetorical. This contingency is particularly true in randomized controlled trials of CAM because the evidence they produce is necessarily provisional by virtue of their intractable design challenges. And yet, when that evidence is translated into clinical practice guidelines and insurance reimbursement schemes, it is reified in the process, becoming the firm ground upon which a slate of patient choices is formulated. A potential consequence of this reification, for example, is that, while a given patient may find relief in the nonspecific effects of chiropractic care, chiropractic may not be included among the options suggested by her family physician, on the basis that there is not enough evidence to support its use. The choice would in this sense have been made even prior to the clinical encounter—and not by the patient.

Further upstream still, the very idea that research would so directly inform practice at all is not borne out in studies of implementation: there is often a poor association between the evidence base and clinical behaviour (see Chapter Two). The assumption that research findings would translate so directly into practice naively underestimates the complex negotiations that clinicians must undergo in practice among competing bodies of data (clinical experience, professional habits and competition, commercial interests, RCT reports and systematic reviews, clinical practice guidelines, insurance schemes, and so on).

The “*JAMA* Patient Page,” “Alternative Choices: What it Means to Use Nonconventional Medical Therapy” (Hwang), offered at the back of the 1998 theme issue, seems to be an early indication of how an EBPC model could play out in advising patients on CAM. Meant to be photocopied and distributed to patients, this page offers several paragraphs defining “alternative medicine,” describing its prevalence and six of the more common CAM therapies (acupuncture, aromatherapy, chiropractic, folk medicine, herbal medicine, homeopathy), and a list of “Issues to

Consider about Alternative Therapies” (1640). The list of “Issues to Consider” is particularly pertinent to matters of patient choice and practitioner-patient interaction because, although it appears to facilitate patient decision-making, it situates that process within the context of a fairly paternalistic physician-patient relationship.

The items on the list offer opaque advice that does not appear to fit easily either with patients’ experience and expertise or the available evidence on CAM, seeming instead simply to redirect the patient toward his or her physician for advice. For instance, patients are instructed to “[d]etermine if the delivery of service adheres to standards for medical safety and care” but there is some question of whether, as nonmedical personnel, they are equipped to evaluate such standards. Likewise, they are told, “Specific information about the safety and effectiveness of any alternative or complementary therapy should be readily available,” but, as I have shown in the previous chapter, that information is difficult even for medical practitioners to obtain and evaluate, let alone for patients. The modal *should* in this statement also lends an air of irresponsibility to any practitioner that does not have data on safety and efficacy readily available. Ultimately, the first four items on the list of “Issues to Consider” point to the fifth: “Discuss any type of medical therapy with your doctor. Your doctor needs to know about any conventional and alternative therapies you have used or are currently using to treat you more effectively and to prevent medication interactions” (1640). The “framing effects” of this document position the physician as the arbiter of the decision-making process, confirming Segal’s findings in discourses about “concordance” that a process that appears to belong primarily to the patient depends instead upon the physician’s expert judgement.

The “*JAMA Patient Page*” gives some indication of how practitioners might envision the shape of future patient interactions regarding CAM, wherein the potentially boundary-crossing

encounter would unfold with the physician firmly in charge. From the perspective of patients, many of whom seek CAM to regain a greater sense of agency in their health care, the shift toward a doctor-approved iteration of CAM might not be desirable. Moreover, as I have suggested in this section, even practitioner-administered CAM practices such as acupuncture and chiropractic may be too restrictive for patients seeking a sense of control over their health because those practices' emphases on patient choice and autonomy may be illusory. We might, I suggest next, think of consumption of dietary supplements as an extreme case of patient choice, one in which users partially *opt out* of professionalized health care—alternative or not. The rhetorical appeal of dietary supplements, I argue, rests largely on their promise of greater patient (now, consumer) agency. Consequently, discourses surrounding dietary supplements can further illuminate the ways in which patients are positioned as medical-rhetorical subjects in biomedical CAM research.

Dietary Supplements and Patient Agency

Dietary supplements are among the most highly accessed CAM modalities, more than any practitioner-administered modality. In 2002, the Centers for Disease Control found that, after prayer, dietary supplements were the most-used CAM therapy by percentage of US adults in a twelve-month period (19%), while chiropractic, the highest-ranked professionalized practice ranked seventh (7.5%; Barnes et al.). As a regulatory category, *dietary supplement* refers to health-related products taken orally that are not under the purview of the FDA, including high-dose vitamins, minerals, botanical remedies (including herbs), amino acids, and “substances such as enzymes, organ tissues, glandulars and metabolites” (Nisly et al.). They range from highly

concentrated extracts of everyday food substances (e.g., garlic, ginger) to pharmacologically active and potentially dangerous substances (e.g., ephedra, St. John's Wort, valerian root).

Although some CAM practitioners do prescribe dietary supplements (and so, too, do some physicians, although certainly fewer), supplements are most readily accessed without prescription through natural health food stores and, increasingly, many pharmacies and grocery stores. They also have very high levels of use: in the *JAMA* theme issue, for example, Eisenberg et al.'s follow-up to their landmark 1992 national survey reports that between 1990 and 1997, use of herbal remedies rose 380%, and that of high-dose vitamins, 130%. During that same period, by contrast, fewer than 40% of all CAM users reported that use to their doctors ("Trends" 1575). In the decade since the second Eisenberg study, levels of use have risen even further, resulting both in staggering sales (over \$21 billion in 2006) and in widespread concern about potential overuse and adverse events stemming from interactions between concurrently taken dietary supplements and/or pharmaceuticals, a phenomenon known as "polyherbacy" (Nisley et al.).⁹⁰ Because so few patients disclose their use of supplements to their physicians, interactions pose a very real problem in the context of patient care—a problem that I want to suggest in this section is rooted partly in the matrix of practitioner-patient interaction.

A primary reason why patients choose, so overwhelmingly, to take dietary supplements seems to be to gain control over their health within a medical system that can, at times, be rhetorically disabling: as I illustrated earlier in my discussion of patterns of communication in clinical encounters, patients often struggle as interlocutors within the generic conventions of clinical practice. Anthropologists Mark Nichter and Jennifer Jo Thompson found in their ethnographic research on users of dietary supplements that a key factor in their use of

⁹⁰ "Polyherbacy" derives from polypharmacy, defined as "excessive and inappropriate use of medications...resulting in an increased likelihood of adverse drug events, drug interactions, and...increased costs" (Nisley 1).

supplements, in addition to or instead of conventional medical treatments, is precisely the potential for gaining greater agency and participation in their own health care.

In the *JAMA* theme issue, Mike Mitka jokes, “Perhaps opponents and skeptics of the safety and efficacy of medicinal herbs should take echinacea to improve their immune systems so they can ward off the growing deluge of requests from consumers for these alternative medicines” (1554). Mitka’s comment here is more than just cheeky because it highlights widespread concern about shifts in the doctor-patient dynamic, with patients increasingly placing demands on physicians who formerly held a position of privilege (and power) by virtue of their specialized knowledge and the generic constraints of doctor-patient interviews (see, e.g., Ong et al., above). Mitka cites Loren Israelsen, Executive Director of the Utah Natural Products Alliance, who argues that the issue of dietary supplements has moved beyond questions of *whether* patients should be using them to questions of *when* and *how*: “Consumers are voting with their feet, and that makes some in the medical profession unhappy.... These medical professionals say, ‘They should be coming to me instead of going to their pharmacists or health food store.’ But patients say they would visit their physicians if the physicians knew something about these products” (1554). Physicians’ lack of knowledge about dietary supplements is certainly a key factor in understanding why patients tend not to consult their physicians about them (Blendon et al.), but it is not the only, or indeed central, factor in patients’ reticence to discuss their supplement use.

The power dynamics of many practitioner-patient relationships, even strong ones, may leave patients worried about being judged or dismissed when disclosing their interest in, or use of, dietary supplements. Blendon et al. found, for example, that half of the respondents in multiple national opinion surveys felt that doctors were “prejudiced against supplement use”

(808). Moreover, the genre of the consultation itself may not invite such off-script discussions: just as doctors tend to follow an unwritten code in their performance of the genre, so too do patients. As the scholarship on clinical communication I cited earlier in this chapter shows, consultations follow familiar and generally predictable patterns—mentioning an interest in interventions from beyond the scope of that scripted environment may seem, to many patients, an intimidating prospect. Additionally, as Nichter and Thompson found, many individuals describe their use of supplements as something meant to support wellness and to not treat illness (in theory at least; see below), and so supplements may seem to be none of the doctor’s business in the first place.

In his corpus article, Mitka revisits the 1994 Dietary Supplement Health and Education Act (DSHEA), which redefined dietary supplements in 1994 as akin to food products, not drugs (despite their often-profound pharmacologic effects). This legislation specifies the kinds of claims that can be made on labels and other sales materials, and places responsibility for testing on manufacturers. Unlike legislation over pharmaceuticals, which must to some extent have proven safety and efficacy, DSHEA renders the Food and Drug Administration relatively toothless over products such as echinacea, bee pollen, and the now-banned ephedra, limiting its purview to aftermarket investigation of complaints of harm. This legislation emerged from an increasingly market-oriented approach to health care and its results have been far-reaching, creating an ever-freer health marketplace in which consumers are invited to “buy” better health in (typically) capsule form. Since that time, biomedical stakeholders such as the AMA have challenged that legislation but their arguments have not stood up well in the public arena: while biomedical discourse often sets the terms of debate in an increasingly medicalized society, dietary supplements—as readily available commercial products—seem beyond medicine’s reach.

Under DSHEA, dietary supplements can only make claims related to the structure or function of bodily systems but cannot make reference to disease. For example, a supplement can “support regularity” or “help maintain cardiovascular health” but not “alleviate constipation” or “lower cholesterol” (Mitka 1555). Structure/function claims are likely not supplement producers’ preferred descriptors, since the claims are necessarily vague and permit companies to offer little guidance on their product’s uses in their promotional materials (packaging, ads, websites). Yet these kinds of claims do, as the same time, create an attractive discontinuity (to borrow Burke’s term) between their ostensible purpose—to *maintain health*—and that of pharmaceuticals—to *treat illness*. If illness is the purview of the physician, then (one might think) a product such as bee pollen, intended to preserve wellness, may well seem irrelevant in a medical consultation, from the patient’s perspective. The rhetorical effects of structure/function claims turn out to be somewhat more complicated, however. On the one hand, Nichter and Thompson show, these claims do give patients the sense that they can engage in “harm reduction” (185) by substituting “toxic” pharmaceuticals with “natural” dietary supplements (192). And supplements do seem to support patients’ wishes for “an increased sense of agency through an alternative diagnosis for their health concerns, additional treatment options, and a renewed sense of hope” (194). On the other hand, though, Nichter and Thompson found, supplement-users can be fairly savvy in their interpretation of structure/function claims, with many simply translating the products’ wellness-oriented claims into illness-centred biomedical terms (e.g., reading “supports menopause” as “reduces hot flashes”).

With support from the AMA, which had vigorously opposed DSHEA, the FDA has recently attempted to expand the definition of “disease” to include symptoms formerly exempt from FDA jurisdiction, such as those encompassed by structure/function claims. This

redefinition of *disease*, which basically expands the term to include its prevention, is undoubtedly motivated in part by the medical profession's vested interest in expanding medical boundaries—boundaries that have been shifted by patients' consumer interest in dietary supplements. In a formal statement on DSHEA, the AMA wrote in 1998: "By allowing dietary supplement manufacturers to make 'structure-function claims' without FDA's pre-approval based on credible scientific evidence of safety and efficacy, the public must rely on the manufacturer's good faith assertion that they can substantiate their claim" (qtd. in Mitka 1555). DSHEA sponsors a Wild-West model of medicine with the watchword "buyer beware"; judging from the prevalence of supplement use in the US, members of the public clearly feel equipped to make their own decisions about their health care. One question I will ask in a moment, however, is: how prepared are they?

Through their redefinition as food-like substances, not drugs, dietary supplements have come to be seen by the public as more like eating one's vegetables than taking drugs, despite their sometimes profound pharmacologic effects. Their use in combination with pharmaceuticals can result in drug interactions and other related morbidity (DeAngelis and Fontanarosa; Palmer et al.)—and, in the case of hospitalized patients, serious complications and unexplained withdrawal effects when supplement use is not reported. For example, Garges et al. describe in a *JAMA* letter (in the corpus) the case of a 58-year-old-man who, while hospitalized, experienced withdrawal because hospital staff was not aware of his use of valerian root. On admission, the patient reported his regular medications—isosorbide dinitrate, digoxin, furosemide, benazepril, aspirin, lovastatin, ibuprofen, and multivitamins—but did not report concurrently taking 530mg-2 g of valerian root five times per day. Only on developing extreme symptoms of withdrawal

(including pulmonary symptoms, tremulousness, and delirium) did his family report the valerian root use to hospital staff (which the family noted he took “to help him ‘relax and sleep’”; 1567).

Garges et al. conclude that physicians should be aware of the symptoms of valerian root withdrawal in hospitalized patients, since herbal preparations are not provided by hospitals. The authors contend that such potential problems would likely be avoided by asking patients directly about their supplement use. This scenario and its proposed solution are instructive within the context of this chapter because we can pull from them several interrelated threads that further illuminate the role of practitioner-patient interaction in the context of biomedical boundary work: that dietary supplements are perceived as “natural,” and thus outside of biomedicine’s purview; that clinical communication is central to discourses about them; and that patients believe they have a right of access to them that transcends matters of health care into the realm of personal freedom. I will explore these threads in closing.

By focusing on legislating the testing of dietary supplements for safety and efficacy, the AMA, in the *JAMA-Archives* corpus, misses an important opportunity to query the reasons behind so many patients’ preference for what they perceive to be “natural” health interventions over more conventional and better-studied pharmaceuticals. Why patients choose dietary supplements is, perhaps, an even bigger mystery to doctors than why patients choose other available alternative medical therapies because, physiologically, supplements function so much like pharmaceuticals, and yet they lack the proof of efficacy that is the hallmark of EBM. Why a patient would choose a herbal remedy such as St. John’s Wort over Prozac, for instance, seems lost on many of the contributors in the *JAMA-Archives* theme issues. Numerous commentators seek to dispel what they believe to be misconceptions about herbal remedies and their pharmacological effects (e.g., Cirigliano and Sun; O’Hara), without much reflection on how they

initially come to be seen as “natural.” The reasons why patients flock to dietary supplements despite the lack of a quality evidence base could offer insight into how patients perceive their own position within health care contexts. Contributors to the corpus are more than willing to cite faultlines in the practitioner-patient relationship as a key reason behind patients’ use of professionalized CAM modalities (see Chapter Two), but that reasoning does not carry over into conversations about dietary supplements.

The appeal of dietary supplements as natural, safe, and readily available (no prescription needed), has invited patients to redefine themselves in some ways as *consumers*, as (active) agents of wellness rather than (passive) scenes of illness. Part of this redefinition derives from the inherent persuasiveness of dietary supplements themselves. Even superficially, although they typically come as capsules or tablets in bottles, as pharmaceuticals do, they can seem otherwise antithetical: their packaging often features illustrations of flowers or other symbols of nature (suns, rainbows...); their bottles are frequently green, blue, or brown (earthy colours, all); they are often purchased in health food stores with wood floors and organic produce displays; and they appear to be under consumers’ own jurisdiction in terms of what to take, how much, and how often. (Importantly, Kaptchuk and Eisenberg identify nature as the “guiding metaphor” of CAM [“Persuasive Appeal” 1061]). To a depressed person, whose condition has been thoroughly medicalized, whose physician has been unable to offer interpersonal support (due to lack of time or relevant training), and whose subsequent prescriptions for antidepressants have brought along with relief headache, nausea, dry mouth, weight gain, and the stigma of mental illness, St. John’s Wort may be appealing. Patients may prefer the herb’s association with a small yellow flower (*Hypericum perforatum*) and a high internal locus of control over antidepressants’ association with hugely profitable, impersonal pharmaceutical corporations and doctors that do not seem to

care. In this light, the popularity of dietary supplements seems to stem, in part, from consumer-patients' desire to regain control over their health within a medical system that can, as I have shown, be rhetorically disabling.

I asked, a moment ago, about how equipped patients are to make their own decisions about using dietary supplements. I indicated earlier in this chapter that I do not think, within the current discursive environment of medicine, that patients are well equipped, generally, for health decision-making, and I believe that to be the case with dietary supplements as well. The rhetorical conditions governing clinical practice, including the generic features of practitioner-patient consultations, the argumentative weight of *evidence* in both the offering and making of choices, the expanding geography of the concept of illness (which increasingly includes wellness as a kind of incipient illness)—all of these conditions leave patients considering dietary supplements in a double-bind. Those that choose to consult with their doctors about supplements face a paucity of evidence⁹¹ and perhaps fears of disapproval, while those that choose not to must negotiate a vast expanse of unverified and often conflicting information available on the internet, in newspapers and magazines, advertisements, and word-of-mouth, including that of retail clerks, without the guidance of a trusted expert. While a majority of commentators in the *JAMA-Archives* corpus suggest that improved communication with patients about CAM would better protect patients and even possibly stem the rising tide of individuals using/considering CAM, I would like to suggest that dietary supplements offer a good indication that CAM use connects to a larger discourse on patient agency and autonomy in biomedicine, one in which *interaction* is

⁹¹ Since users of dietary supplements frequently suffer from multiple, chronic, relapsing-remitting, or functional conditions, supplements are more reliably tested in effectiveness studies, which have less strict inclusion criteria. However, as I argue in Chapter Two, while both study designs are valid, efficacy, not effectiveness, is usually held up as the standard for CAM trials. Moreover, since those kinds of conditions can often only be assessed by subjective measures such as pain scales and patient diaries, their outcome measures can be dismissed as too “soft” to provide clear evidence of efficacy. The idea of efficacy can thus also be invoked argumentatively in discourses on dietary supplements, framing the choices available to patients and consumers, both.

itself a central point of contention. A brief example, this time from Canada, will illustrate my point.

On 8 April 2008, Canadian Health Minister Tony Clement introduced bill C-51, a bill meant to increase the power of Health Canada to enforce previously legislated standards regarding the safety and efficacy of therapeutic products, including dietary supplements. This legislation can be traced in great part to concerns about the potentially dangerous consequences of DSHEA in the US, and to the resulting struggles within the medical profession to bring dietary supplements under FDA jurisdiction (see DeAngelis and Fontanarosa). The introduction of this bill ignited a firestorm of debate about the rights of individuals as both patients (i.e., *recipients* of medical interventions) and consumers (i.e., *choosers* of interventions, as if from a list of options, like a restaurant menu).

The proposed legislation mobilized vast numbers of opponents: in the first two months, for example, an online petition generated almost 22 000 signatures (Klingbeil) and 53 groups were formed in opposition to the bill on Facebook, a social networking website. The Facebook groups, some with memberships in the tens of thousands, framed the bill as placing Canadians' "health at risk," as one group says—as impinging on the overall "rights and freedoms of Canadians," which will result in nothing less than the "erosion of democracy." The website *Stop C-51* warns visitors that the passage of the bill would result in restrictions of Orwellian proportions: the government, it claims, could conduct "search[es] and seizures without warrants"; where "no evidence will be required"; and "up to \$5 million fines [will be levied] if you are suspected of having unregistered natural health products." The site concludes that the bill will "apply crack house style of enforcement to the natural health industry" (*Stop C-51*). The opposition to C-51 seems well out of proportion to what the bill seems to be meant to do, which

is to offer some means of controlling the quality of dietary supplements, the regulation of which had not been formally enforced.⁹²

The outrage sparked by bill C-51, despite the bill's rather more innocuous mandate, evinces the pride-of-place accorded to dietary supplements by their users. Kaptchuk and Eisenberg suggest that, for their users, supplements “become daily activities of affirmation, assurance, and commitment. Dietary regimens...become liturgical acts of recognition with deeper implications for social, moral, and spiritual redemption” (1063; references removed). Revered as they are, dietary supplements appear to occupy a far greater place in their users' psyches than seems, on the surface, to be warranted. Because of this reverence, writer and self-identified skeptic Daniel Loxton argues, users of dietary supplements lose sight of the inherent contradictions in their arguments against C-51:

Many people are prepared to trust the manufacturers of natural health products, even as they glare in suspicion at anything “Big Pharma” touches (this is assumed to include federal regulators). This is a baffling double standard. Despite the warm, down-home marketing of the multi-billion-dollar alternative health industry, it is not a David to Big Pharma's Goliath. The natural health products industry is a commercial juggernaut of entirely mainstream, big business proportions...[I]t seems bizarre to arbitrarily trust one marketing brand of multi-billion-dollar, for-profit drug manufacturers while simultaneously *distrusting* other drug manufacturers. Yet, this seems to cause little cognitive dissonance. (¶ 25)

The panic about bill C-51 in Canada is not entirely unfounded, however, if we keep in mind the ways that the notion of efficacy can be invoked to draw particular professional-epistemic

⁹² Of course, not all commentators take a doom-and-gloom approach to the bill: one Facebook group yawned at the debate, its official response to the issue being simply “Meh.” The only other pro-C51 group on Facebook was formed ostensibly by a group of vampires praising the regulation of garlic as a dangerous substance.

boundary lines. And the leaps made in popular reactions to the bill from the protection of consumer health all the way to the erosion of consumer freedom, and even of democracy itself, can help us investigate at the same time what is at stake for individual users of dietary supplements in their protests against increasing regulation.

I think there is a way we can see the conflict in this arena as part of a larger constellation of concerns about the roles available to individuals in the context of their own health care, with the Wild-West American model coming to represent the preservation of consumer rights and freedoms in the face of the pending Canadian legislation, even though that legislation is itself ostensibly intended to protect those rights and freedoms. The debate is certainly about more than a few herbs or vitamins—coming to stand, I would argue, for a larger discourse on patient agency and autonomy.

A key question that arises out of the work of this chapter, which I have not fully explored here, is through what means, and to what extent, can patients achieve autonomy over their own health care in an evidence-based framework? As philosopher Anita Ho has argued, individualist models of autonomy can in many respects be coercive, particularly because they abstract decision-making out of patients' social contexts. A rhetorical approach can open up some of the dimensions of this complex question, particularly in the context of biomedical CAM research, because it can show, for example, how the idea of "choice" is framed within generic and rhetorical processes that necessarily tilt the course of decision-making in particular—and predictably biomedical—directions. It can also show how the idea of "interaction" can itself persuade in various directions: the promise of more interaction, and of higher quality, can persuade patients to seek chiropractors; the lack of control for interaction can persuade medical

researchers not to take a trial's results seriously; the desire to avoid interaction can persuade individuals to choose dietary supplements over conventional pharmaceuticals.

The testing of interventions that depend significantly on interaction, such as those in CAM, opens health research up to practices and practitioners normally beyond the scope of RCTs, which promises to humanize biomedicine's more positivist impulses. However, it can also introduce new problems into research and practice contexts, the potential implications of which could be far-reaching. For example, this wider scope threatens to expand biomedicine's authority through processes of surveillance and medicalization. Sociologist Sam Porter cautions that, "While the clinical gaze reduces us to our bodies, at least its surveillance is limited to those bodies. In contrast, by adding our psyche and our social circumstances to the gaze of health care workers, holistic care widens the trawl of surveillance to the most intimate parts of our lives" (19). An expanded understanding of the myriad factors affecting health and illness is a double-edged sword: it can enrich our knowledge of the things that make us sick and help us get well but can, at the same time, expand also the ways in which we *can* be sick, subsequently reformulating us increasingly as candidates for medical intervention. Put more simply, whatever their ameliorative potential, broader perspectives on health and illness have the potential, at the same time, to reinstall us anew within that same medicalizing system.

CAM practices such as acupuncture and chiropractic are brought into biomedicine's orbit as they are tested in RCTs. Even more conventionally biomedical SCIs, such as nursing, are fundamentally reframed in research contexts as they are rendered amenable to quantification, isolation, and manipulation. And yet there are certainly signs that biomedicine itself may be evolving through this expanded research programme, as evidenced by, for example, the emergence of stratified models of placebo effects, which highlight the relationship effects of

interventions. This chapter has offered an account of how a thoroughly rhetorical activity—the clinical encounter—can irritate the notion of evidence and the markers of efficacy in medical interventions. It has argued that following the thread of practitioner-patient interaction through the landscape of testing socially complex interventions, particularly CAM, can reflect something of the theoretical terrain itself; this process may, in turn, lay the conceptual groundwork for further discussion of the ways in which models of health research and practice circulate rhetorically, and of how they can shape the course of the practices, such as acupuncture and chiropractic, they describe.

CHAPTER FOUR

Defining Professional Borders in Popular Media

This final chapter shifts further downstream, beyond the realm of practice into the public realm. Its focus, as with the other chapters of this dissertation, is on the negotiation of biomedical boundaries, although this chapter moves beyond the *JAMA-Archives* corpus to investigate how those boundaries are configured in popular reporting on biomedical CAM research. Research such as that disseminated in the *JAMA-Archives* theme issues is noteworthy in the context of popular reporting on science and medicine because consumer behaviour downstream, in the public realm, has, in a sense, reverse-engineered upstream behaviour: biomedical interest in CAM did not develop in response to a problem identified as worthy of study within the field itself. Rather, this interest was stimulated from the outside, a fact belaboured by as many commentators within the biomedical community (e.g., Angell and Kassirer; Delbanco) as celebrated by those outside it (e.g., Cowley and Underwood; Glick). In the texts I study in this chapter, the products of that research are returned, filtered through science, to the public that motivated it. The nature of that return is my object of focus.

The chapter revisits some of the questions guiding Chapter One, following trajectories of influence in the negotiation of medical-professional boundaries, this time within a new set of interlinked texts: a special report of *Newsweek* magazine, “Inside the Science of Alternative Medicine.” The report was published within the regular weekly magazine on 2 December 2002; it features as both subjects and contributors many of the authors whose work appears in the *JAMA-Archives* corpus, most prominently David Eisenberg and Ted Kaptchuk. My interest in this special report is in how CAM, used every year by millions of Americans in spite of not

having received, to date, any substantial scientific approbation, is situated in relation to science as a bounded cultural space (see Gieryn, *Cultural Boundaries*).

In her study of representations of science in print media of the first half of the twentieth century, Marcel LaFollette argues that “mass circulation magazines serve as especially sensitive indicators of what their readers believed (or wanted to believe) about science” (20). Dorothy Nelkin suggests, similarly, that scientific reporting “reflects current fashions and editorial perceptions of what readers want to hear” about science (45). Both of these perspectives are pertinent to the study of scientific boundaries in popular media because they highlight the reciprocal nature of science reporting: audiences play an important role in how science is reported. In this chapter, I want to refine this notion of reciprocity further by suggesting that, more than merely revealing what the public wants to believe about science, popular reporting on CAM research can reveal also how ideas about biomedicine and its boundaries circulate, in all directions, in popular contexts—ideas about how biomedicine operates; about the quantity and quality of the knowledge that it produces; about its potential costs, benefits, and risks; about its agents and its consumers.

The movement of this dissertation from upstream contexts to down- is not itself an argument, from arrangement, either about the relative priority of any one position on the upstream/downstream spectrum in the negotiation of biomedical boundaries, or about the directions of influence among them. Rather, as Stephen Hilgartner describes, the scope of texts and contexts that can properly be called “scientific” is wide, from laboratory chat and research team meetings to scientific articles and grant proposals to textbooks and mainstream media, including newspapers, magazines, and television. The point on Hilgartner’s spectrum at which “science” becomes “popular science” (and so, by implication, not really “science” at all), is

blurry, context-dependent, and, he says, “a matter of degree. The boundary between real science and popularized science can be drawn at various points depending on what criteria one adopts” (528). In previous chapters, my rhetorical cartography of biomedicine has focused on texts and contexts that, if not fully internal to biomedicine, are at least heavily rooted in biomedical theory and practice. (Doctor-patient interviews, for example, are fairly far downstream but still unfold in accordance with biomedical values and conventions.) In this chapter, I examine a set of texts that have a different set of allegiances, beyond the sphere of biomedicine, to see whether (and, if so, how) they work with, and within, biomedical terms.

Studies of popular science have themselves contributed to the reification of boundaries between popular science and science proper. Jeanne Fahnestock’s seminal 1986 article, “Accommodating Science: The Rhetorical Life of Scientific Facts,” while not the first to adopt what is critiqued as the “dominant model” of popularization (Hilgartner; see also Broks; Gross, “Roles of Rhetoric”; Myers, “Scientific Popularization”), exemplifies that model. Fahnestock argues that accommodations (i.e., translations) of science are “primarily epideictic”—“their main purpose is to celebrate rather than validate” (“Accommodating” 333). Fahnestock’s framework presupposes a knowledge deficit in which scientific knowledge is interpreted by science reporters for the lay public. These reporters adapt “new knowledge to old assumptions and [try] to bridge the enormous gap between the public’s right to know and the public’s ability to understand” (“Accommodating” 331). In other words, according to Fahnestock, scientific information travels one way, from knowledgeable experts to an ignorant public. Later scholars such as Hilgartner and Alan Gross have argued vehemently against this unidirectional model, while Greg Myers roots his challenge in the fact that there is a lot of grey between the poles of “expert” and “lay” in discourses on science. Myers critiques studies, like Fahnestock’s, that trace

research articles to their appearance in popular venues—an activity that he argues maintains the “dominant” view of popular science as simply handmaiden to science.

As Gross, Hilgartner, and Myers show, popular science does not simply spread the word of science to the laity, reporting on its marvels and lamenting its missteps. On the contrary, Gieryn argues, science’s pride-of-place in contemporary Western culture is explicitly authorized by the very public that prizes it. He entreats us “to look at science from a different vantage—not upstream at facts in their making, but downstream at their consumption” (*Cultural Boundaries* ix), because this is where science and scientists accrue their power to make and remake facts about the natural world:

Nothing in the practices of scientists at their benches, nothing in their skillful mangle of gadgets or critters, nothing in the literary machinery that translates inquiry into facts on a page can alone explain why science is trusted (in so many and varied situations) to provide credible and useful accounts of nature. Or, more precisely, upstream science substantially underdetermines the epistemic authority that marks its consumption downstream. (ix-x)

Something within downstream science itself must, then, contribute to the authority ascribed to science to weigh in on an increasingly wide range of issues affecting human life. However, downstream science is not simply complicit in maintaining science’s cultural dominance; rather, I share with Danette Paul the view that popular science feeds back into the disciplines whose research it popularizes, not just “describing a science [but] helping to define it” (61). I argue in this chapter that once we “diversify” our understanding of what constitutes a popular audience, as Bernadette Bensaude-Vincent urges, we may begin to see real consequences for science in even unambiguously popular texts, such as mass-circulation newspapers and magazines.

One of this chapter's overarching claims is that medical reporting is a special case of science reporting, that it is both typical and exceptional: it is typical because medicine's research values, professional practices, generic forms, and institutional structures are closely aligned with those of science, yet it is exceptional because popular reporting on medicine can influence publicly-held attitudes toward health and illness, which can, in turn, shape individual health beliefs and behaviours.⁹³ This is not to say that popular readers necessarily find articles on health and medicine more important or more interesting than they do those on science. Rather, my suggestion is that we, all of us, get a sense of what the stories about health in public circulation are, and that they can affect us, even if we do not read them ourselves.

Writing on hypochondria, Judy Segal illustrates this process through the example of Mary McCarthy's novel, *The Group*. In the novel, a doctor tells the daughter of his patient, "We've noticed that now that we no longer speak of dementia praecox, we get fewer dementia-praecox patients. It tempts you to think sometimes that all mental illness has a hysterical origin, that they're all copying the latest textbooks. Even the illiterate patients" (qtd. in Segal, *Health* 80). Segal describes this phenomenon in terms of Ian Hacking's notion of the "looping effect," which Hacking explains as the process through which different kinds of people "can become aware that they are classified as such [kinds]. They can make tacit or even explicit choices, adapt or adopt ways of living so as to fit or get away from the very classification that may be applied to them.... What was known about people of a kind may become false because people of that kind have changed in virtue of what they believe about themselves" (Hacking, *Social Construction*

⁹³ Weigold cites Sharon Begley, a writer for Newsweek describing science and medicine as separate domains in science reporting. At *Newsweek*, she says, "science is basic research.... I cover everything from archaeology to genetics, neuroscience, and physics. I do not do medicine, which is defined as anything having to do with sick people. And I don't do technology. I'll do genetics. I'll do neuroscience. But once it gets into somebody sick, I give it to 'medicine'" (qtd. in Weigold 165).

34).⁹⁴ Stories in public circulation about health (and, as in this chapter, about research on health), can effectively re-story us—they can re-story how we think about ourselves as bodies, as patients, as people.

In making this partial distinction between popular science and popular medicine, I am not making a claim along the lines of what Miriam Solomon identifies as the “medicine is ‘not a science,’ or ‘not only a science’” cliché (her term) common to discussions about the humanities and medicine, for example, or narrative and medicine, or ethics and medicine (406). This cliché, she argues, misses the point about what she calls the “unity of knowledge”: “I have not found any of the epistemic approaches or skills in medicine to be unique to medicine, or even (with the possible exception of empathy) to be unique to the human sciences” (416). My own claim about popular medicine is an epistemic claim of a sort, to be sure, regarding the ways in which popular texts condition how knowledge about medicine, as body of theory and practice, circulates in North American culture, but this is not a claim about the epistemology of medicine itself. The split I am postulating here, that is, is not about whether or not medicine is a science, but about whether or not *popular* medicine is best thought of, from the perspective of rhetoric, as simply a variety of *popular* science. I think we can answer the second question without having to answer (or even acknowledge the validity of) the first.

The first two of the following sections take popular medicine as a typical case in popular science. I begin by examining the model of the “new science” of CAM introduced in the *Newsweek* special report, tracking, through both visual and textual analysis, its salient features. I suggest that, despite the magazine’s claims about this model’s novelty and scope, the model

⁹⁴ Hacking describes this as the difference between *indifferent* and *interactive* kinds. While interactive kinds become changed through changes in their self-awareness as members of particular kinds or categories, indifferent kinds are not so aware. To wit: “The classification ‘quark’ is indifferent in the sense that calling a quark a quark makes no difference to the quark” (105).

itself is a cover for what turns out to be a fairly standard story of science and scientific research. I then examine the model of science that does emerge in the report, exploring its various dimensions through a rhetorical construction that recurs throughout the special report; this construction asks of CAM, “Does it really work?” Both of these sections are concerned with medicine as it is installed within scientific modes of thinking and practice, and with the ways in which CAM is variously lined up with them.

The final two sections take up popular medicine as an exceptional case in popular science. I take up, first, questions of expertise in medicine, using the magazine’s clear differentiation between biomedical experts and nonexperts as a means of exploring where, and how, it establishes professional-scientific boundaries. In the final section, following on Judy Segal’s account of narrative suppression in breast cancer discourse, I begin to assemble the various narratives at work in the *Newsweek* report, collected over the course of the chapter, to examine what they do *not* tell us. The stories not told in the magazine about both CAM and biomedicine can point to potential areas of concern as biomedicine’s boundaries shift, expanding to increasing areas of human life. Given that, as Gieryn suggests, downstream science plays a significant part in the production and maintenance of science’s cultural and epistemic authority, the work of this chapter can return to my larger study of biomedical boundary work insight into the processes through which some health practices come to count as legitimate and others, not.

I selected the *Newsweek* special report, “Inside the Science of Alternative Medicine,” for analysis in this chapter for several reasons. I focused on the genre of the newsmagazine because I wanted to examine texts with a general focus and broad reach in the popular realm.⁹⁵ The broad reach of the report itself is signalled by the significant attention it has attracted, particularly as a

⁹⁵ In 2002, *Newsweek* had the second-highest paid circulation in its category (3.2 million), next to *Time* (4.1 million), and was ranked nineteenth overall in American magazines (Magazine Publishers of America).

flashpoint of debate about biomedical-CAM boundaries. When the report hit newsstands, for example, University of Maryland physicist and author of *Voodoo Science: The Road From Foolishness to Fraud*, Robert Park featured it on his weekly online newsletter. In his brief commentary, he draws firm boundaries that mirror those drawn in the *JAMA-Archives* corpus about what ought to count as medical science: “The cover story in the Dec 2 issue of Newsweek is The Science of Alternative Medicine. That’s an oxymoron. If these alternatives had a basis in science, they would just be medicine. Newsweek calls it ‘The New Science’” (¶1). The report also figures prominently in Evelyn Ho’s research on communication in a TCM clinic, in which she and coauthor Carma Bylund describe its polarizing effect, as an instance of popular reporting on CAM research, on the clinic’s practitioners. When the magazine came out, Ho and Bylund report, word spread quickly among the staff, prompting heated debates about both biomedical CAM research and the magazine’s handling of the subject (Ho and Bylund; E. Y. Ho). The special report’s prominence in these contexts suggest that it is an important site for investigating rhetorical boundary work in popular media.

The *Newsweek* report is also an attractive site of inquiry due to its similarity to the *JAMA-Archives* corpus as a multilayered, bounded discursive object. It is a self-contained version of what Sophie Moirand calls a “discourse moment” in science popularization: “the surge of intense and diversified media activity in connection with a single media event” (178)—in this case, the highly publicized boom of CAM research beginning around 1998. The report spans a considerable range of the genres, topics, and authorial expertise that constitute medical reporting: from sidebars offering explicit medical advice to complex, in-depth explorations of a given specialty’s state-of-the art; from deadly diseases to chronic and mental illness to health maintenance; from care of the young to that of the old; with articles by nonspecialist reporters,

seasoned medical journalists, and expert medical researchers and practitioners. The *Newsweek* special report does not cover all possible varieties of popular medicine—it does not feature, for example, sustained first-person accounts of illness—but it does provide a rich cross-section of the different kinds of medical reporting available among the popular media, presented together as a dossier on the “new science” of CAM.

Unlike single popular articles across disparate publications, these articles are meant, like those of a dossier, to be read together. The ways in which they can be read with and against one another are important from a rhetorical standpoint because they permit us to examine layered trajectories of persuasion within a multidimensional whole—in individual articles, across articles and sections, and among and across different sorts of authors. The report itself is structured as a magazine-within-a-magazine, with its own title page and table of contents in the middle section of the magazine. It is divided into three separate sections, each with two feature articles, other short articles, sidebars, and related infographics. The sections are interlinked by their visual format, with a common graphic design unique to the items in the report, and by a recurring sidebar feature, “Insights from Harvard Medical School,” that appears throughout on topics such as osteoarthritis, cancer, and “research.”

The texts within each of the three sections are themselves interwoven both thematically and through layout, with some articles set inside and around others, and all of them sharing common section headers. The first section, “The New Science,” opens with “Now, ‘Integrative’ Care” (Cowley), which doubles as the lead article of the entire report. This article, twice the length of the next-longest articles, sets the stage by surveying the current state of research on, institutional and regulatory support for, and patient use of CAM. “Learning from China” (Underwood) follows, exploring the potential contributions of Traditional Chinese Medicine to

biomedicine. The second major section, “Family Health,” looks at alternative medicine for children (Noonan, “For the Littlest Patients”) and for women (Kalb, “A Natural Way to Age”); and the third explores “Mind and Moods,” covering anxiety and depression (Kalb, “How to Lift the Mind”) and placebo effects (Kaptchuk, Eisenberg, and Komaroff, “Pondering the Placebo Effect”). Each of these sections features sidebars and infographics that take up the topics discussed in the articles themselves; I describe them below. Because these sometimes disparate articles are presented, on multiple levels, as an integrated set of co-texts—curated, as it were, to present a cohesive statement on the “new science” of CAM—they can provide crucial meta-level insight into how that new science is constructed for the public realm.

The *Newsweek* report as a whole is also a useful counterpoint to the *JAMA-Archives* theme issues. These sets of texts share a concerted focus on CAM as an area of scientific inquiry, for example, and cover of a wide range of health conditions and CAM practices. I have argued throughout this dissertation that the *JAMA-Archives* corpus can be read as an artefact of a specific rhetorical moment, with the AMA staking a claim, through its publication arm, in the debate surrounding CAM. I think we can read this *Newsweek* report similarly, as a rhetorical artefact, but with a quite different orientation. Unlike the *JAMA-Archives*, whose overall argument ultimately serves the particular interests of a specific community, the AMA, *Newsweek* offers a complex reflection on biomedical boundary work, particularly as its authors address the conflicting interests of their various stakeholders, such as advertisers, subscribers, editors, expert informants, and relevant regulatory bodies. The picture of the “new science” offered across the articles is multilayered; by tracking these layers across articles by different authors and on different subjects within a cohesive “discourse moment,” we can learn more about the variability of rhetorical boundaries in biomedicine.

I supplemented my analysis of the special report with articles in other issues of *Newsweek* so that I could get a sense of the fit of the report within the magazine's general coverage of CAM. I conducted an EBSCO search for the keyword "alternative medicine," which returned 46 hits between 1991 and 2007, with no hits before or after that date range.⁹⁶ I read all of the articles returned in my search and included them as supplementary material in my analysis of the 2002 report. Thirty percent of the hits were from the special report itself, while the remaining articles align into two "peak" moments: the early 1990s (1991-1993), as CAM use began to rise and medical researchers first began to take notice, as evidenced by the 1992 formation of the NIH's Office of Alternative Medicine; and the late 1990s (1997-1999), as the *JAMA-Archives* theme issues were published in tandem with the 1998 transformation of the OAM into the full-fledged NIH National Center for Complementary and Alternative Medicine. After 1999, excluding the 2002 report, the number of articles on CAM plateaus, holding steady at two or three articles per year until 2005, after which it is not mentioned again except in a quarter-page sidebar on the possible health benefits of kitchen spices (Kuchment). Tracking similarities and differences between these supplementary texts and the *Newsweek* special report is beyond the scope of my project but the texts do provide important context for the claims the magazine makes in the report about the "new science" of CAM.

⁹⁶ Related keywords "integrative medicine" and "complementary medicine" returned hits already among the 46 for "alternative medicine." The articles within this set of results *not* from the 2002 report are of three general sorts: short, slice-of-life articles, usually sidebars, whose primary focus is novelty value—"Animal Acupuncture" (Hamer), for instance, or the medicinal value of ordinary foods, as in "Chef! This Dish Needs Pain Relief" (Kuchment) and "The Magic of Mushrooms" (Underwood, "Mushrooms"); mid-length articles (one to two pages) that offer a brief sketch of a specific health topic, such as mind-body medicine (Begley and Rosenberg), or of a treatment, such as zinc-based lozenges (Hamilton); and more substantial reports similar to "The Science of Alternative Medicine," where CAM treatments are included among the slate of interventions covered in explorations of, for example, *How Kids Grow* (1991; Smith) and *Health For Life: What Every Woman Needs to Know* (1999; Smith).

***Newsweek's* Idealized Model of the “New Science” of CAM**

In the main table of contents of the special report, *Newsweek* asks, “what is the real science of alternative medicine?” (“Top of the Week” 3). The special report endeavours to answer this question by offering an account of what it argues is an essentially new model of science. This new model, the magazine suggests, is the product of research on CAM, which has required medical researchers and practitioners to reevaluate their approaches to practices they once dismissed as founded on superstition and quackery. I argue in this section that we learn three principal things about the “new science” of CAM in *Newsweek*, which can provide insight into how ideas—or perhaps, ideals—about biomedical boundaries circulate in public. We learn that there is indeed a new science of CAM; that this new science integrates biomedicine and CAM into a single model; and that the new, integrative science is very much like the old, non-integrative science. Following on LaFollette, I argue, in particular, that the magazine’s claim that the new science combines mainstream and alternative medicine is perhaps a better reflection of what people believe (or want to believe) about biomedicine than of how biomedicine really is. In this case, they want to believe in the possibility of a peaceable marriage between biomedicine and CAM.

The first thing we learn about this “science of alternative medicine” is that such a thing really exists—and everything within the report seems orchestrated to provide evidence of that fact. Parsing the magazine’s opening question indicates that *Newsweek* is indeed making an epistemic claim, a claim that we can draw out by examining the presupposition it triggers.

Presupposition, a concept from pragmatics, refers to a proposition in a given statement that must be taken to be true for a statement to be judged as appropriate in its context. Presupposition is useful to rhetorical analysis because it helps to articulate how trajectories of persuasion can be

effected by linguistic means. The *wh-* question, *what is*, combined with the definite article, *the*, presupposes that there is already in existence an entity known as *the science of alternative medicine*, which the report poises itself to tell us more about.⁹⁷ That same question simultaneously presupposes a separate category, a *not-real science of CAM*—a pseudoscientific CAM, perhaps, or just an indifferently unscientific one. However we might imagine this not-real science of CAM, the key here is that the question itself invites us to make distinctions among different kinds of CAM, only one of which is genuinely scientific.

Similar statements can be found throughout the special report, each somewhat differently inflected. The report's title, for example, promises to take us "Inside the Science of Alternative Medicine" (45). This title presupposes that alternative medicine *has* a science, which is cast in spatially exclusive terms, *inside/out-*, rather like the *real/not-real* split I just described. In this example, "the science of alternative medicine" is designated as territory that is in some sense already known to us, but unfamiliar—a bounded space into which we, as outsiders, need to be invited, even shepherded. A further example draws boundaries of a somewhat different sort: the first section header, "The New Science," modifies the nature of the science on which the report is based by specifying, 1) that it is individually identifiable: *the* new science, not *a* new science, or *one of the* new sciences; 2) that is *new*, i.e., unlike any extant variety or model of science; and 3) that it is specifically suited to alternative medicine—custom-made, as it were. The new science of alternative medicine is, in other words, appreciably different from the science we have known before: it is a science transformed.

This new science appears, most fundamentally, to be a science based on the resolution of several sets of oppositions, including *alternative/mainstream*, *old/new*, *East/West*, and *care/cure*.

⁹⁷ WH-questions presuppose the existence of the phenomena about which they inquire through their very structure. Levinson explains, for example, that the question, "Who is the professor of linguistics at MIT?" presupposes that someone is, in fact, the professor of linguistics at MIT (184).

It appears, in a word, to be *integrative*; this is the second thing we learn about the science of alternative medicine. I say that it appears to be so because the report's visual composition is, in fact, one of its primary means of persuasion about the integrative quality of the new science of alternative medicine. (A second reason for *appears*, I suggest later, is that the claim to integration turns out to be superficial for the most part, a veneer.) Greg Myers critiques popularization studies for overemphasizing text at the expense of other, primarily visual codes operating in popular science. The *Newsweek* report demonstrates that Myers' concern is well-founded, as its visual elements do not simply reinforce the articles' arguments; rather, while the images seem meant to prepare the ground upon which those arguments unfold, they sometimes work against them, signalling a disconnection between the magazine's overt claims about integration and the rather conventional model of science it advances.

Although the emerging field of visual rhetoric is still very much in a state of flux (see, e.g., Foss, "Rhetorical Schema"; Gross, "Presence as Consequence"; Peterson), current efforts to establish a model that accounts for the interactivity of the visual and the verbal are useful for examining the use of visuals in the *Newsweek* special report. Russell Willerton's approach, for example, examines what he calls "visual metonymy" in "stage-setting images" in technical communication. He develops his account through Karen Schriver's model of document design, which expands I. A. Richards' notion of interanimation in verbal contexts to the reciprocal influence of images and text on each another. Willerton defines stage-setting images as those that develop and support, or *interanimate*, themes advanced in written texts (4). Stage-setting images often perform the "distilling functions of synecdoche and metonymy," he argues, by depicting "tangible" aspects of a given theme (11), such as the image of a gavel as visually metonymic of the law (his example). Such tangible, stage-setting images, he argues, convey thematic content

by inducing readers to make specific associations related to that theme. In the *Newsweek* report, visual metonymy sets up patterns of association that develop the theoretical concept of integration through the depiction of “tangibles,” such as health practitioners and patients, in settings that reflect prominently the trappings of both mainstream and alternative medicine.

The cover, for instance, features a tight shot of an attractive woman’s face against a black backdrop. The woman is serene, with open, upward-facing eyes and three acupuncture needles placed just above and between her eyebrows. She seems to be of European origin and does not look like a person we might associate with a particular subculture, and this seems exactly to be the point: the photo is at once familiar and exotic, mundane and sensational. Generic, tranquilly open-eyed, and punctured by steel needles, this woman embodies, literally, the fusion of binaries that characterize this new science as it is depicted in *Newsweek*.

The text beside her image bolsters this characterization. It reads: “The Science of Alternative Medicine,” with *Alternative* about double the size of the previous text, and *Medicine*, at least triple. The difference in font size indicates a hierarchical relationship between these two words: the depicted needling may be alternative but it is, more significantly, medicine. This emphasis on medicine as an authorizing entity is further reinforced underneath the main text module and the three smaller-font modules advertising the report’s main articles, where we are promised as a bonus (“PLUS”), “Insights from Harvard Med School.”⁹⁸ This mention of Harvard creates a strong association between the somewhat sensational image of the woman with the needles in her face and one of the United States’ most prestigious medical schools, as if to say, “Not only is this practice medicine, it’s Harvard-approved.”

⁹⁸ The phrase “text module” is from Gudrun Held; it refers to groupings of text on magazine covers as persuasive units. The stories advertised on the cover include “Depression Treatments,” “Acupuncture & Herbs,” and “Natural HRT [Hormone Replacement Therapy].”

Similar visual patterns linking ideas of old and new, East and West, and alternative and mainstream, persist throughout the magazine. For example, a full-colour photograph spanning three-quarters of the initial facing pages of Geoffrey Cowley's lead article features radiologist Zang-Hee Cho and a colleague, both clearly of Asian descent, performing acupuncture on a patient's bare feet within an explicitly biomedical setting. Both wear white lab coats while their patient lies in a prominently pictured functional magnetic resonance imaging (fMRI) machine. Superimposed on the photo is the article's title, a claim that, given the strong visual associations within the photo itself, seems almost superfluous: "Now, 'Integrative' Care." This image is particularly salient to a discussion of how popular texts like *Newsweek* persuade us about the boundaries of science and medicine because it seems to make the case before we ever read a word of the articles themselves. We bring to the image the assumptions that enable the image to make the case for integration via associations triggered by the image's metonymic content (e.g., lab coats represent biomedicine; acupuncture needles represent CAM; their co-presence represents *integration*). Through these images, we come to the text primed to hear more about "The New Science" because we have already seen evidence of it in action. That is, when Cowley claims in the article itself that the main goal of CAM research is "to spawn a new kind of medicine—an *integrative* medicine that employs the rigor of modern science [to create] one health system instead of two" (48), it may not seem like news to us.

Other photographs in the report support this integration claim, particularly when the images are read against their surrounding text. For example, there is a stark juxtaposition on the second set of facing pages in Cowley's article between its text, which reports on institutional research cultures, political wrangling, and government funding, and its two photographs of patients engaging in behaviours that we would not normally associate with institutionalized

medicine—one holding a yoga pose on a wood floor, wearing comfortable-looking pants (importantly, not a paper gown); the other, in an in-patient setting, wearing her own pyjamas on a bed with a floral bedspread, singing along with a woman playing guitar. This pairing of text and image evokes a sense of balance among oppositions, such as cure/care, institutional/individual, mainstream/alternative. Such balance is crucial to activity that could fairly be called “integrative.” Similarly, Claudia Kalb’s “A Natural Way to Age” focuses on the state of research on menopause treatments but the photos depict women in everyday settings, one standing, smiling, in a pumpkin patch with her family, for example, while another waits, laughing, at a hot dog stand. Both women seem content in spite of their menopause, a sure sign, the implication seems to be, that integrative medicine works.

The articles that the images accompany similarly frame this new integrative medicine as the product of an eager, peaceful marriage between science and practices such as acupuncture and herbal medicine. Cowley makes the strongest claims about integration, opening his article with the story of Carol Green, who had an “epiphany” while applying to medical school, realizing that she wanted to explore a more “inclusive” mode of health care:

So Green tossed her med-school applications and pursued a degree in traditional Chinese medicine at the New England School of Acupuncture. Today she has a busy practice [and]...though she worried at first that conventionally trained physicians would shun her, she has found they’re as eager as she is to break down old boundaries. She sends her patients to MDs when she can’t help them—and MDs send just as many to her. She gets referrals from internists, orthopedic surgeons, even psychiatrists. ‘Why should people use just one modality?’ she asks. (47)

In this case, balance among binaries is represented in the professional activity of referral, a reciprocity that implies that TCM and biomedicine share a friendly, undefended border.

Similar claims about successful biomedical-CAM integration are made throughout the report. We hear of twelve-year-old Christie Blackwood in David Noonan's article on kids and CAM, who is referred to the Minneapolis Children's Hospital Department of Integrative Medicine and Cultural Care after a week of struggling with the side effects of chemotherapy for leukemia. Christie was taught about acupressure, biofeedback, aromatherapy, and guided imagery—the last of which, her mother reports, kept Christie from vomiting even once, despite vomiting severely with chemotherapy alone. Noonan writes that kids are “the most important ingredient” in integrative pediatric care—“Like Christie Blackwood, calm and cheerful in the face of cancer, combining the power of chemotherapy and the power of her imagination as she wages the fight of her life” (62). The marriage of biomedicine and CAM has given Christie agency over her treatment where, we might assume, she would have otherwise had none.

The medical director of Christie's clinic tells Noonan that, among primary-care pediatricians, “CAM is on the tip of everybody's tongue every day” (59). However, I am not convinced that such a claim accurately represents the sentiments of most practicing American doctors, particularly in light of my findings reported in earlier chapters. Certainly, Cowley and Underwood's similar claim, in a 1998 *Newsweek* article on the *JAMA-Archives* theme issues, that “*JAMA's* editors are now accepting yoga and acupuncture as facts of life” (68), could not withstand scrutiny. Similarly, in relaying his story of Carol Green, Cowley mentions that Green works at the Marino Center for Progressive Health in Dedham, MA, but he does not explain that the centre is specialty integrative medicine clinic and that Green's referrals to and from MDs may well be internal. Such clinics are far from the norm, even today, and so Cowley's opening

anecdote overstates the level of reciprocity among biomedical and CAM practices and practitioners.

The assertion that integrative care is already an accepted fact of medical life in the US is an essential component of *Newsweek*'s claim about the new science of alternative medicine but this bold claim is not held up by the articles meant to support it. I argued above that the first thing we learn from the report is that there exists a new science of CAM—one that we learn, second, is integrative. But we learn a third thing, too, which supersedes the first two: this new, integrative medical science is, in effect, neither new nor integrative. In fact, it is probably fair to say that biomedicine itself remains unchanged, and that, much as I argued in Chapter One of the model advanced in the *JAMA-Archives* corpus, in *Newsweek*, biomedicine only has a wider scope of interventions under its purview.

The idea of integrative medicine has long held an appeal in the popular media. Nearly ten years earlier, in 1993, Sharon Begley and Debra Rosenberg reported in *Newsweek* that Americans were “clamoring for alternatives to drugs, surgery and doctors who treat them as nothing but bags of symptoms” (61). It would seem that these alternative-seeking individuals have found their answer in integrative medicine: as physicians Wendy Weiger and David Eisenberg inform readers in the 2002 special report, “Unlike conventional treatments, which can leave a person feeling passive and helpless, many complementary therapies help patients become active participants in their care” (49). However, the accumulation of similar statements throughout the special report do not, in the end advance a claim to integration as much as they highlight the incongruity of the claim within the conventional model of science that underlies the report. Consider again the photos in the Cowley and Kalb articles I describe above: while they do set the stage, in Willerton's terms, for claims such as Cowley's that “Doctors are as unhappy as

patients about the current state of health care, and most are eager to make it more caring and humane” (48), evaluated more closely, the patients in these pictures seem superfluous to the drama that unfolds around them.

All we can see of Dr Cho’s acupuncture patient on the first page of Cowley’s article, for example, are his or her bare feet and clothed body; the rest is obscured inside the fMRI machine. And while we do see patients in the article’s other photos, note their captions: “A patient at Sloan-Kettering Cancer Center relaxes with yoga” (48); “Music therapist Lucanne Magill offers support” (49). In the first caption, the centre is identified by name but not the patient, whose face is covered with a small sachet. The focus in the second caption is the therapist, not the patient, even though both are equally visible. The women in the Kalb photographs, although they have names, seem no more three-dimensional. In fact, whereas the photographs in the Cowley article seem like stock images of patients in CAM settings, the women in Kalb’s article seem not only generic, but also somewhat foolish, at the pumpkin patch and hot dog stand, against Kalb’s narrative of the sprawling institutional and regulatory fallout of the hormone replacement therapy scandal. While these women seem meant to appear empowered by the health care they have sought outside the mainstream system, they are reduced to caricature in cute settings while rigorous, hard-nosed science goes on around them.

In the articles themselves, “integration” likewise proves elusive. Cowley sets up the special report’s key claim that a “new blend of medicine” is now emerging (47), one that will “employ...the rigor of modern science without being constrained by it” (48), but this claim founders over the course of his article. Although he first describes the new science as a “blend,” paragraphs later, he asks: “Can a system built on one paradigm accommodate another? Is there room for care and compassion within science-based medicine?” (50). What he describes here is

not two systems merging into one, as he first claims, but, rather, one system (biomedicine) absorbing another (alternative medicine).

As I have argued in previous chapters, the constraining force of “rigor” is precisely what makes science culturally recognizable *as* science, so it is no wonder that what is packaged throughout the *Newsweek* special report as “integrative” (see also, e.g., Underwood, “Learning” 56; Noonan 60) turns out instead to be straightforwardly biomedical. In its ostensible marriage with CAM, science retains all executive powers, holding exclusive authority to determine whether or not CAM practices really work. The disparity between the model of medical science about which *Newsweek* claims to report—the new science—and the model that actually undergirds the special report can bring into focus some of the tensions that underlie popular reporting on science and medicine. As I suggest next, the *Newsweek* special report’s underlying model of medical science is considerably more complex than the scholarly literature on popular science would suggest.

The Construction of Biomedicine in *Newsweek*

I explore the complexity of the model of science advanced in the *Newsweek* special report through a construction that recurs, in variation, throughout both the scholarly and popular literature on biomedical CAM research. This construction asks, of CAM, “Does it really work?” In the *JAMA-Archives* corpus, one of the aims of subjecting CAM to RCTs is to separate “real” effects from nonspecific or placebo effects in order to discern which CAM interventions are genuinely effective, and which just appear to be so. This was the motivation, for example, behind Bove and Nilsson’s isolation of spinal adjustment as the effective ingredient in their *JAMA* study discussed in the previous chapter. In *Newsweek*, the construct repeats not only throughout the

special report but across the entire range of articles it has published on CAM. In 1992, for example, the magazine asked, “Do remedies like acupuncture and herbal medicine really work?” (Glick 58); in 1997, it asked, of zinc for colds, “Does it really work?” (Hamilton 48); and in 1999, “Can massage, meditation and supplements really improve your health?” (“Weighing Alternatives” 92).

The “Does it really work?” construct gives shape to the *Newsweek* special report as a whole: it is the question that motivates the research about which it reports, and it is the question that the magazine itself seeks to answer, if provisionally, based on that research. In this section, I undertake a close reading of the *Newsweek* special report, to explore this construct’s various trajectories of persuasion in answer to the following question: “Of what are *Newsweek*’s readers persuaded about biomedicine, and through which means?” (Questions of who is persuading, and who is persuaded, are taken up in remaining sections.) I argue that, collectively, these trajectories point toward a model of science that seems, on first glance, inherently contradictory, in which science is depicted, 1) as objective and unchanging, but also as responsive and dynamic; and, 2) as able to give us reliable knowledge, but also as unreliable in the knowledge it produces. I suggest that these ostensible contradictions ultimately bolster biomedicine’s epistemic authority in the public realm by introducing within it levels of complexity that go beyond Fahnestock’s and Nelkin’s assertions that popular science flattens dissent and depicts science in monolithic terms. The model of science that undergirds the *Newsweek* report is multidimensional—and it is precisely this multidimensionality that enhances its jurisdiction over CAM.

The first rhetorical trajectory of the “Does it really work?” construct is that it positions science as the only legitimate means of separating interventions that really work from those that just appear or pretend to work. Within the 2002 special report, we hear Dr Cho, the radiologist,

tell us that his goal is to “learn how acupuncture really works” (Cowley 50), and Kalb, in her article on anxiety and depression, that trials are underway on St John’s wort to find out “how safe it really is” (“Lift” 68). Similarly, Anne Underwood writes, “If traditional Chinese medicine feels unscientific to the Western mind, that should come as no surprise. Its foundations were laid down more than 2,000 years ago in The Yellow Emperor’s Classic of Internal Medicine. Yet modern science is starting to verify that some of these age-old remedies really work” (“Learning” 54). As I have shown in previous chapters, such claims to epistemic jurisdiction are well within biomedicine’s sphere of cultural influence: science has accrued the kind of authority that makes it difficult to imagine a matter, medical or otherwise, about which it could not persuasively decide within the public realm.

The scientific aim of finding out whether or not CAM interventions work may itself seem innocuous, but the corollary implication, that only interventions validated by science are meritorious, is one that I demonstrate in Chapter Two is as much the product of concern for professional boundaries as it is for patient safety and care. Considering the vast swaths of everyday biomedical practice that are not themselves evidence-based, the idea that CAM practices are suspect simply by virtue of their not having been subject to RCTs is difficult to defend. Further, we can distinguish between this construction and the related but importantly different, unmodified question, “Does it work?” By separating appearance from reality by asking if CAM really works, this construction puts users of CAM in a tricky spot: it implies that those that have experienced improvement while using invalidated or not-yet-validated interventions may well be deluding themselves. Moreover, it discounts the real benefits patients often experience regardless of whether those benefits were from genuine effects or placebo effects. And yet, at the same time, by positioning science as able to see through effects that we might

experience as real, the construction endows it with an epistemic authority that we do not ourselves possess.

As an entity able to penetrate CAM interventions and assess their merits, science takes on an agency of its own in this “Does it really work?” construction; this is its second persuasive trajectory. Underwood, for example, writes that “modern science is starting to verify” TCM, as though science were itself capable of isolating research questions, formulating hypotheses, testing them, and then deriving conclusions based on the results (Underwood, “Learning” 54). Similarly, the deck of Cowley’s article states, “As science rigorously examines herbs and acupuncture, a new blend of medicine emerges” (47).⁹⁹ Randy Allen Harris describes the rhetorical force of constructions that position science as an independent epistemic agent: “Science has undergone an almost-literal apotheosis, taking over so many of the functions of religion that the primary meaning of *lay-person* has shifted from ‘non-cleric’ to ‘non-scientist’ It brings such commanding authority to assertions that we regularly get headlines like ‘Science proves Shroud of Turin fake’ and ‘Science close to cure for AIDS’” (Introduction to *Landmark Essays* xi). According science an agency of its own reinforces the idea that science can answer any question put to it. This model of science as effectively agentless sponsors the first of the two multidimensional views of the new science of CAM advanced in the *Newsweek* special report: that science is at once objective and unchanging, and also responsive and dynamic.

In the *Newsweek* special report, *science* is bifurcated, as a conceptual category, into two separate parts: research, configured as a seemingly independent agent of knowledge and mode of action, and those that engage in it—researchers. This conceptual split creates space within a single, cohesive model of science to meet conflicting cultural demands at the same time: that

⁹⁹ Decks are short summaries of magazine articles, often included between a story’s headline and its byline.

science be objective and recognizable as a monolithic entity, disinterestedly able to access truths about nature (these functions are performed by “research”), but also responsive to human needs and interests, operating not on high from “Mount Science” (Gieryn, *Cultural Boundaries* 6), but within and in relation to the culture that supports it (as performed by “researchers”). Both dimensions are persuasive because each addresses different sets of beliefs about how science works, or ought to work.

This bifurcated model is evident in, for example, the *Newsweek* special report’s strong sense of momentum, although that momentum has an uneven tempo: science, it seems, has variable speeds. For example, Cowley writes that, “after dismissing CAM therapies as quackery for the better part of a century, the medical establishment now finds itself *racing* to evaluate them,” while the resulting “huge clinical trials *plod along*” (48, 49; emphasis added). This pattern recurs elsewhere: “Experts are now *racing* to develop new study designs,” Kaptchuk, Eisenberg, and Komaroff explain, but the studies themselves will take time (“What Works” 73; emphasis added). While researchers “race,” working dynamically in response to the changing social conditions of their research, research “plods” indifferently, immune to outside influences.

The image of scientists “racing” to evaluate CAM is animated by the “Does it really work?” construction, and is its third persuasive trajectory. This trajectory derives from a model of science as explicitly serving the interests of the public—a model that most of the contributors to the *JAMA-Archives* corpus vigorously resisted. In the *Newsweek* special report, by contrast, users of CAM lead the way, while scientists and doctors run to catch up. If LaFollette is right that popular magazines can tell us something of what readers believe, or want to believe, about science, in this case, *Newsweek* indicates that readers want to believe in a science that is

receptive to the interests and needs of the general public. Portrayed as attempting to “catch up” with patients’ needs and interests, biomedical practitioners appear, finally, to be listening.

Given that many patients struggle to feel heard within biomedicine, this portrayal of scientists as responsive to public demand may be a strategy to secure audience goodwill by presenting readers with a vision of science that appeals to their own interests as patients.¹⁰⁰ What is often glossed over in the special report, however, is that enormous numbers of individuals were seeking CAM interventions whether or not they had been rigorously studied, so the keen emphasis on how quickly scientists are racing to study CAM may seem out of place. Yet, if we keep in mind the first trajectory of the “Does it really work?” construct, that scientists have a special acuity of vision that allows them to see what we cannot, their efforts to evaluate CAM through scientific principles are reframed as a response to public demand, even if the public does not know well enough to actually make that demand.

A high level of activity runs through *Newsweek*’s special report: the state of research itself is described in terms of movement and abundance. These descriptions contribute to the overall sense that scientists are mobilizing a swift response to a public exigence. Cowley, for instance, argues that, with the founding of NCCAM, “the money and excitement spread quickly” (48), resulting in a “*flurry* of research” (53; emphasis added). Many articles describe studies in rapid-fire succession, such as Kalb, who describes no fewer than sixteen trials in her 1500-word article, which consequently reads like a list of abstracts (“Lift”). Some articles evoke a sense of both forward movement and plenty in CAM research by describing one study and then shifting immediately to another in a manner not unlike the narrative style of old westerns films, which

¹⁰⁰ As Miriam Solomon notes, one of the highest praises one can give of a physician is to say, “he really *listens* to me” (407). The idea that successful rhetors must work within the value-systems of their audiences is as old as Aristotle, who contrasts the ease of praising “Athenians to an audience of them” with the daunting task of praising them instead to a crowd of Spartans or Scythians (51).

kept the plot moving by cutting from scene to scene with a screen wipe and voiceover announcing, “Meanwhile, back at the ranch...” Mary Carmichael reports, for example, “A six-year trial started in 1999 will likely yield more definite answers. Meanwhile, doctors are also studying the compounds often mixed with ginkgo...” (57). Similarly, Weiger and Eisenberg note, “The findings [of several cancer-treatment trials] will be reported over the coming decade. Meanwhile, it’s important to remember that ‘natural’ doesn’t always mean safe” (49). This “meanwhile...” construction places the reader in the middle of the action in a vast, bustling field of research. Although these articles frequently report, “We need more studies” (Kalb, “Lift” 70; see also Kalb, “Natural” 65; Noonan 60), there seems to be an embarrassment of riches in biomedical research on CAM.

The sweeping sense of motion in these examples conveys the impression that research activities are being reported as they happen, and results, as they are available. Dorothy Nelkin notes that science reporting tends to focus on the hyperbolic, the unusual, the scandalous, or the extreme, a focus that is often manifest, she says, in words ending in “-est,” such as *fastest*, *cheapest*, *biggest* (1; 166). In the *Newsweek* report, as in the *JAMA-Archives* corpus, the central focus seems, accordingly, to be on the *latest* on biomedical research. This live-on-location quality is supported by the “Does it really work?” construction, particularly by the bifurcated model of science that it advances. This bifurcated model makes it possible to imagine, as the *Newsweek* special report implies, that although scientists may be breathlessly playing catch-up, science itself has everything under control—studies are underway, it reassures us, and the results will be available soon. The question of results complicates the picture, however, because, even though the production of data is the *raison d’être* of medical research, there never seems in the *Newsweek* report to be enough data to answer the questions posed. This is the second apparent

contradiction of the “Does it really work?” construct, and its fourth persuasive trajectory: that science is able to give us reliable knowledge (i.e., we *can* know things about the world through science) but is unreliable in the knowledge that it produces (i.e., it is difficult for us to know something *for sure*).

Study results are reported in the magazine much in the way results are reported in the research literature generally: under heavy qualification. Carmichael notes, for example, that although “ginkgo is one of the most thoroughly examined remedies in complementary medicine, the verdict isn’t in yet” (57), while Shmerling et al. describe research results on CAM for osteoarthritis as still too limited: “rigorous evidence is still lacking”; “scientific evidence is too sparse”; “its benefits are not well established” (53). Results are described tentatively, as in Weiger and Eisenberg: “Several *small* studies have found...”; “exercise *may* ease...symptoms”; “*preliminary* evidence *suggests*” (49; emphasis added). There seems, in all of these articles, to be a moving target regarding how much data is enough to say that we know that an intervention really does work. Referring to a meta-study of twenty-nine trials of hot-flash treatments, for example, Kalb reports that we still have only “small amounts of reliable data so far” (“Natural” 65), but nowhere does she (or any other author) explain how much data is enough. To some readers, I would imagine, twenty-nine studies may seem like twenty-eight more than necessary. There seems in these articles always to be more room for more data, so how do we know when we know something?

In asking of CAM, “Does it really work?” the *Newsweek* special report bolsters science’s cultural and epistemic authority, but not along lines predictable from, for example, Fahnestock’s model of popular science. Fahnestock argues that popular accommodations of scientific texts demonstrate a greater degree of certainty about science by removing qualifiers and omitting

contradictory indications. These omissions reinforce the epideictic aims of accommodation, she says, noting that “only certainty can be the subject of panegyric” (“Accommodating” 338). However, as the examples just cited show, the *Newsweek* special report tends instead toward *uncertainty*. These two views of scientific knowledge can be recast, as shorthand, in Latour’s description of the two faces of science: “science in the making,” where there are always further questions one could ask and further answers for which one can probe, and “ready made” science, which takes knowledge as solid (enough) grounds upon which to recommend courses of action (13). While ready-made science is capable of statements such as “just do this...just do that...,” Latour says, science-in-the-making insists instead, “enough is never enough” (13). Ready-made science is predominantly the model of science advanced in popular media, as Fahnestock and others argue (Calsamiglia and Ferrero; Gross, “Roles of Rhetoric”; Michelle; Schwartz, Woloshin, and Baczek). However, the *Newsweek* coverage of CAM research instead depicts science explicitly as in-the-making, where the knowledge that it produces is never quite enough—at least not “yet.” So, how does this increased uncertainty about the status of scientific knowledge contribute to science’s epistemic and cultural authority? The uncertainty places an ever-higher premium on the knowledge that science does produce.

Reliable knowledge is a precious commodity in the *Newsweek* special report: as we are told again and again, we face a paucity of data on which to base judgements about the safety and efficacy of CAM interventions. This paucity comes into particularly sharp relief when set against the bustling scenes of research I have described, where researchers are hurrying and trials are moving along, and yet we have little to show for it. To include the production of results in the bifurcated model of science I introduced above, I offer this vignette: we might imagine, in this model, scientists, hurrying around in lab coats, gathering material to feed into “research,” a

monolithic machine that runs indifferently at its own predetermined and invariable pace; this machine, in turn, produces tiny, infrequent droplets of data—many of which are either faulty or in need of further droplets to be of use. Like extracting gold from ore, this process consumes a lot of time, energy, and expense, and its output (“data”) is considerably smaller than its waste (i.e., the many trials whose results are insufficient, inconclusive, or invalid). In this framework, the data produced by both research and researchers are rendered extremely valuable by virtue of the data’s rarity and expense. Ultimately, then, the apparently contradictory assertions that, 1) science is both socially disinterested and socially responsive, and, 2) that we know *a lot* but not *enough*, serve the twin goals of placing a premium on scientific knowledge and enhancing the ethos of the researchers that work so hard to produce it.

In this section, I have described what I identify as the four main persuasive trajectories of the “Does it really work?” construct: that it positions science as the only legitimate instrument for evaluating whether CAM interventions actually do work; that it gives science an agency of its own, which enables the bifurcated model of science I have described; that it facilitates the magazine’s depiction of the scene of research in terms of bustle and plenty; and that it underscores both science’s ability to produce results and the value of those results. I have used these trajectories as heuristics, to sketch out the model of science that underlies the *Newsweek* special report, which turns out to be quite different from the integrative model of the “new science” of CAM about which the magazine ostensibly reports. One of the most significant implications of this multidimensional model, in the context of professional boundary work in popular media, is that, by incorporating seemingly oppositional characteristics of science into a single, coherent model, the *Newsweek* report makes it hard to imagine that anything but science could determine, unequivocally, whether or not CAM really works.

Mapping “Gaps” between Experts and Nonexperts

In the first two sections of this chapter, I examined the broader contours of the *Newsweek* special report: I approached medical reporting as a variety of science reporting, and did not differentiate among the report’s different sorts of authors. In the remaining two sections, I sharpen my focus by examining popular medicine as an exceptional, rather than a typical, case in popular science, and by developing the claim that popular science *by* scientists might usefully be considered separately from that by journalists (and other authors).

In this chapter’s introduction, I discussed some of the crucial differences between popular science and popular medicine. These differences rest on the particular kinds of expertise members of the public have in popular texts on health and medicine, and on how the ideas circulated in those texts manage, also, to circulate beyond individual readers. The second distinction I want to make, among different kinds of authors, is often underemphasized in studies of popularization (but see Fahnestock, “Preserving”). Questions about speakers—about their character, their position in relation to both their speeches and audiences, and the contexts within which they speak—have always been central in rhetorical study but, while analyses of popular science have paid good attention to the matter of *who is being spoken to* in popular science, questions about *who is speaking* are often flattened out. The *Newsweek* special report is a good place to describe some of these differences because, to borrow from the philosophy of RCT design, the articles in that report were produced under largely “controlled” circumstances: they were published in the same venue, on the same rhetorical occasion, under the same (or similar) editorial conditions, and for the same audience. While they do vary by genre and by subject, their similarity in these other areas can help isolate how categories of authorship factor into popular accounts of biomedical research.

I take my cue in these two remaining sections from the questions guiding Judy Segal's study of breast cancer narratives, of which she asks: "what do breast cancer stories *do* when they circulate in public life? What do they do *for* us and what do they do *to* us?" ("Breast Cancer" 6; original emphasis). One of the things they do, she argues, is produce and maintain ignorance about breast cancer by making some stories more tellable than others. The stories that we cannot tell, the stories that are suppressed, she argues, narrate breast cancer differently; examining what those untellable stories say and how they are suppressed can highlight the radical limits of public discourse on breast cancer.

Segal's examination of this discourse is salient here because it demonstrates how we—as bodies, as people—can be invisibly re-storied in public discourse, and then have those stories returned to us as *our* stories. The story of biomedical CAM research told in the *Newsweek* special report similarly overwrites other stories that could have been told, stories that seem better to match the concerns and interests of the magazine's target audience, the US public, broadly construed—the majority of which uses (or has used) CAM. One of the factors that underlies the suppression of certain elements of the story of CAM research, I suggest, has significantly to do with the nature of medical reporting itself, particularly the conflicting demands its writers face. One of those demands is the authors' need to accommodate their readers' expert knowledge about some aspects of health and medicine but not others.

In this section, I return to the notion of a knowledge deficit or "gap" as it pertains to the particular case of popular reporting on CAM research. As critiques of the "dominant" view of popularization illustrate, the idea that popular science translates scientific knowledge, unidirectionally, from specialist contexts to popular ones essentially posits the public as "ignorant and dependent" (Bensaude-Vincent 101). Audiences of popular medicine have a very

specific kind of expertise simply by virtue of being audiences at all. While not everyone will have knowledge about, say, recent advances in mechanical engineering, all are expert on the experience of being a person with a body, one that is sometimes sick, sometimes in pain. The *Newsweek* special report provides an interesting site for exploring the idea of a knowledge “gap” in popular reporting on medicine because, in the report, CAM practices are re-presented, in biomedical terms, to the public whose use of CAM initially precipitated the research. How these popular texts negotiate the question of expertise can help us further track boundary work in medicine because it can show that, despite rhetorics of empowerment in popular discourses on health and medicine (see Segal, “Internet Health”), some forms of expertise are more valuable than others, even in popular texts.

Critics such as Bensaude-Vincent, Broks, Hilgartner, and Myers (“Scientific Popularization”) all argue, separately, that the model of popularization that privileges the practice of tracing popular reports back to their authorizing scientific sources, such as Fahnestock’s approach, is itself an act of boundary work that ultimately shores up science’s cultural and epistemic authority by reducing popular texts to the singular rhetorical function of “celebrat[ing]” the achievements of science (Fahnestock, “Accommodating” 333). The irony here is that, as Fahnestock carefully highlights the neutralizing effects of popularization (effects such as elevating the level of certainty in truth-claims and flattening areas of dissent), she simultaneously neutralizes the very discourses she studies. She divests them, that is, of any significance beyond their service to the interests and authority of science. Peter Broks reminds us to be alert to this risk of neutralizing the discourses we study because, ultimately, “we cannot extricate popular science...from wider issues of power, authority and the demarcation of what is

science from what is not” (123).¹⁰¹ The importance of expertise in popular medicine is not, then, that readers have more expertise on health and medical topics than they do on other scientific topics (i.e., that the “gap” is smaller). Rather, embedded in matrices of “power, authority, and . . . demarcation,” popular medicine has high stakes precisely because of readers’ own bodily expertise, and because even those of us that do not read popular texts on health can be affected by them.

Ideas about a gap between scientists and the rest of society, valid or not, permeate the *Newsweek* special report. The report’s very arrangement promotes this view, with the “Insights from Harvard Medical School” differentiated, visually, from the rest of the articles. Set within clearly delineated text boxes against a contrastive yellow background, each “Insight” features alongside its text artwork depicting the region of the body discussed (upper body, torso, leg), with x-ray style geometric shapes superimposed that reveal the underlying location of pathology (cells in the body, bones of the knee).¹⁰² This artwork literalizes the metaphor that doctors can “see” inside our bodies—that, through diagnostic technologies, especially imaging technologies, they can know our bodies in ways that we cannot. As Judy Segal illustrates (*Health*), these specialized ways of knowing about patients’ bodies effectively shut patients out of their own care: through these technologies, diagnoses can be made even in the patient’s absence.¹⁰³ One of the byproducts of that specialized gaze, as Foucault notes, is that the very act of *being seen* changes us and, in this case, the artwork serves as a reminder that medical specialists can see us

¹⁰¹ Gross similarly cautions that rhetorics of accommodation “deflect attention from the ethical and political issues science raises, or ought to raise.” In fact, he notes, such approaches “actually mask ethical problems” (“Roles of Rhetoric” 9).

¹⁰² Notably, while we can clearly see in the artwork the individual cancer cells invading the body in Weiger and Eisenberg, and the exact vertebrae causing the problem in Cherkin, Sherman, and Eisenberg, the head depicted in Craig Miller’s “Insight” on treatments for anxiety and depression is stubbornly opaque. The geometric x-ray reveals nothing but a lighter-coloured portion of the same picture.

¹⁰³ See also Reiser on the influence of medical technologies on physician-patient relations; and Dumit on the rhetorical effects of brain imaging technologies.

better than we can see ourselves. For *Newsweek* readers that have used CAM, the stark differentiation between their ability to know their own bodies and medical practitioners' ability to know those same bodies may bring into focus the transformation that CAM has undergone in its encounter with science.

The gap between experts and nonexperts in the *Newsweek* report is also maintained by the use of credentials, which further differentiate the articles by researchers from those by journalists. All but one of the Harvard "Insights" prominently identify their authors both by degree (all doctoral-level) and affiliation, such as "Wendy Weiger, MD, PhD, and David Eisenberg, MD." The sole exception is the article co-authored by Ted Kaptchuk, whose professional designation is OMD (Doctor of Oriental Medicine). He and his coauthors Eisenberg and Komaroff, both MDs, are signed instead as "Drs. Ted Kaptchuk, David Eisenberg, and Anthony Komaroff." Considering that the Harvard "Insights" are otherwise all so similar in terms of layout, style, tone, and purpose, and that all of the other author listings have parallel construction, this exception stands out. I suspect it came about as a means of enhancing the authors' ethos by highlighting Kaptchuk's status as *doctor* rather than as *TCM Practitioner*, but note how "doctor" has been taken up in a variety of contexts, often very public, to denote authority of an often unspecified sort.¹⁰⁴ In identifying clearly that these authors are authorities that we can trust, credentials in the *Newsweek* report thus further distinguish experts from nonexperts.

Pronominal reference is another means through which the special report draws lines of distinction between experts and nonexperts, this time by creating a variable sense of proximity by aligning the reader closely with the speaker in some cases, and farther away in others. The

¹⁰⁴ For example, Laura Schlessinger, PhD in physiology, uses the name "Dr Laura" for her work as a family and marriage counsellor in the popular media. Chiropractors and naturopaths are also designated as "doctors," even within their professional designations, DC (Doctor of Chiropractic) and ND (Naturopathic Doctor).

report, as a whole, is on the public's side, and the authors generally use inclusive pronouns to establish common ground with their readers, a technique that Diane Dowdey notes is conventional in popular science. For example, the blurb on the magazine's main table of contents states: "In this Special Report, NEWSWEEK explores how acupuncture, herbs, tai chi and biofeedback have many doctors reexamining how *they* treat all of *us*" ("Top of the Week" 3; emphasis added). Notably, "treat" takes on a double connotation here, as in *Please treat my headache, doctor*, but also as in *Please don't treat me like that*. This double meaning explains the high levels of public interest in CAM as largely the result of patient dissatisfaction with biomedicine, an explanation that is reinforced in many of the articles. Cowley also identifies himself inclusively, as one of us: *We* make more visits to nonconventional healers...than *we* do to MDs," he says, "and *we* spend more of *our* own money for the privilege" (48, emphasis added). Our distance from those holding the keys to biomedicine becomes clear later that same paragraph, as we are told that "*the* medical establishment now finds *itself* racing to evaluate..." (48). This distance is reinforced, predictably, in the researcher-authored articles, which routinely address readers in *exclusive* terms, in the second person. They advise readers to "integrate [CAM] wisely into *your* care" (Weiger and Eisenberg 49), and warn that taking herbal remedies for depression "may serve *you* well. *But be careful*" (Craig Miller; first emphasis added, second emphasis in original). While the journalist-authors generally speak as "one of us," that is, the researcher-authors generally speak *to* us.

In their articles, the researchers behave exactly as we might expect: they speak as physicians talking to patients. They write in a manner that might best be described through Bourdieu's assertion that there exists in linguistic production a certain "*relaxation in tension*" in which speakers "endow their linguistic performances with a casualness and ease that are

precisely recognized as the hallmark of distinction in such matters,” thereby asserting their authority within the speech situation (659; original emphasis). None of the researchers provides direct quotations or identifies by name the studies they summarize, with only the exception of the well-known Harvard Nurses’ Health Study (Haskell and Eisenberg). de Oliveira and Pagano note that lack of citation, direct or indirect, is an indication that popular science authors feel “comfortable enough” with the material they are referencing that they do not see a need to bolster their claims by pointing to an authorizing source (642). (Note that the journalists, by contrast, overwhelmingly do provide such identifying information.) Moreover, contrary to Fahnestock’s assertion that popularizations flatten out debates and present scientific ideas as ready-made, these articles present the state of knowledge on CAM as unstable and largely undefined, and the authors seem virtually unphased by that fact.¹⁰⁵ Finally, the articles’ pace is measurably slower, less frantic, than the journalist-authored pieces, although the researcher-authors do equally emphasize the volume and pace of the research they report. The writers each assume the tone of an experienced doctor offering explicit advice to a patient. This advice often comes in the form of lists, where the lines of transmission, from doctor to ostensibly advice-seeking patient, are clear: “Here are some potential remedies...” (Shmerling et al.).

These researcher-authored articles evince a firm conviction that biomedical approbation of CAM is not only necessary to *Newsweek*’s readers, but also desirable. The special report as a whole is shot through with the sense that CAM is an undiscovered country or frontier that the ambassadors of scientific medicine are hard at work mapping and analyzing, as if to clear the territory for public travel. The live-on-location feel of the report’s account of the intense research activity surrounding CAM contributes to this impression to the extent that it is not hard to

¹⁰⁵ Davida Charney suggests that the tide might well be changing in popular science regarding how popularizations approach areas of controversy, which seem now to be treating dissent as a fact of scientific life (“Introduction” 5).

imagine seeing a theodolite or GPS receiver in the hands of a scientist racing by. The assumption behind the Harvard “Insights,” for example, seems to be that readers, now framed as patients, will go out into the world of CAM, as if for the first time (whether or not they have used CAM before), and use these tipsheets as maps for navigating the terrain. Shmerling et al.’s list of arthritis remedies reads like a travel guidebook, briefly describing the state of research on five remedies and pointing out which ones are worth stopping at and checking out, and which we should just drive past. Frontier metaphors, common in emerging areas of science (Ceccarelli, “Mixed Metaphors”; Nelkin), appeal to high civic ideals such as adventure and the expansion of knowledge, but frontiers can also be dangerous places—at least for those living in them prior to their discovery. If we consider the specific expertise that audiences have in popular medicine, we might find that these maps come to overdetermine the terrain they attempt to represent, and that they may not, in fact, be wanted at all.

I have argued that one of the *Newsweek* special report’s most notable features is that it returned the products of CAM research to essentially the same public that motivated it. As I showed in the previous two chapters, CAM is transformed as it is mapped with biomedical tools, and those seeking CAM may not find in this new biomedically-approved version the same qualities that brought them to CAM in the first place. Ceccarelli points out in her study of genomic metaphors, particularly the *gene-as-map* metaphor, “A map that surveys the frontier is designed to open up a territory for settlement and for development, to make it possible for its owners to conquer the land for use and for profit. Although it represents things the way they are now, it does so with the express purpose of aiding in the alteration of that territory” (“Mixed Metaphors” 98-99). One of the stories that does not get told in the *Newsweek* report is the story of what happens to CAM as it is mapped by biomedicine, as it is settled and (re)developed, and,

in turn, what happens to the individuals who valued navigating the terrain on their own. The gulf between expert and nonexpert widens as these individuals are told, simply, “talk to your doctor.”

Suppressed Narratives of CAM Research

As a means of exploring some of the other stories not told in the *Newsweek* special report about biomedical research on CAM, I now turn to the journalist-authored articles. The authors of these articles are positioned at the intersection of various interests, including those of expert informants, administrative and regulatory bodies, advertisers, editors, and readers, as well as entrenched public values about the place and purpose of science within North American culture. These articles consist primarily of two intertwined narratives: a narrative of research activity and a narrative of the people that use CAM. These two narratives correspond, respectively, with two key features of popular science identified by theorists such as Fahnestock, Nelkin, and Bruce Lewenstein, that science journalists internalize scientific values (an expectation met by the narrative of research activity) and that they appeal, explicitly, to the interests and values of their readers (met by the narrative of CAM-users). Segal argues that narrative itself serves a regulatory function where the emergence of a “standard story” in public discourses on breast cancer makes other stories harder to tell (“Breast Cancer”). In this section, I examine the *Newsweek* report’s two interwoven narratives as standard stories that effectively suppress other possible narratives of CAM research. By examining these stories, we can learn more about the larger tensions that underscore popular medicine, particularly in relation to the negotiation of medical-professional boundaries.

The first narrative in the *Newsweek* report, about research activity, is less about individual CAM trials than it is about research itself. It makes a claim about what sort of

enterprise science is, particularly through the idea of activity. As I have suggested earlier in this chapter, this story of research does not conform to claims in popular science theory that science journalists suppress areas of dissent and present knowledge as ready-made. Scientific knowledge is portrayed in *Newsweek* as necessarily tentative and I think we can read this portrayal of science as in-the-making as evidence that scientific values are strongly internalized within the articles. That is, when talking about the biomedical knowledge base on CAM, the journalists in *Newsweek* begin to sound like scientists themselves, qualifying what we do know and carefully explaining what we do not know. They are notably cautious about generalizing findings, emphasizing the often provisional nature of scientific results.¹⁰⁶ These emphases on scientific values may be strategic: by sounding like scientists, the authors may seek to enhance their ethos, and by appearing faithful to the ideals of science, they may seek to nurture working relationships with their scientific informants.

In the other set of narratives in the *Newsweek* special report, we hear stories—many stories—about people that use CAM and have uniformly benefited from it. We hear about Sheldon and Tosha Janz (Noonan), for example, who responded to their pediatrician’s suggestion that their 2-year-old take steroids for asthma by seeking a second opinion; the second physician agreed with the first. Undaunted, they sought a third opinion, this time from a doctor of integrative medicine, who suggested that they cut dairy products from their son’s diet and, upon following that suggestion, the toddler’s wheezing promptly stopped. We hear about Marisa Harris, 54, who had refused chemotherapy and was given nine months to live but then joined a

¹⁰⁶ Guiomar Ciapusio’s study of prepublication interviews between scientists and journalists provides a window into what is otherwise an invisible process. She notes that, when journalists gather information about the stories they write, “Scientists are always in charge of content presentation...[with] the journalist merely ratifying, asking questions, demanding explanations and nodding” (216). We could infer, from this process, the possibility that scientists’ frameworks for thinking about the nature of their work might bleed into journalists’ own frameworks for describing scientists’ activities. Lewenstein suggests, also, that science writers have increasingly had formal scientific training, which may further contribute to shaping how journalists think about and describe science.

meditation class and support group, which changed her mind about chemotherapy. Now, Cowley reports, “Four years later, she’s as happy as she has ever been in her life” (52). We hear about Ann Moffat, 50, the woman from the hot dog stand who felt she had received such a strong benefit in a clinical trial of black cohosh for hot flashes that she began taking it once the trial was over, even though she admits to not knowing whether she received a placebo or the real thing (Kalb, “Natural”); we hear about Christie Blackwood, the preteen whose chemotherapy-related nausea was controlled by guided imagery; and we hear about many others besides.

All of these stories in the *Newsweek* special report, together, are remarkable in several ways. First, their volume alone is dizzying: we hear about a lot of people that use CAM. But, second, we do not learn a lot about any of them, as the stories are told in the same rapid-fire style in which the research stories are told. We hear about this patient and that one, but we end up knowing nothing much about any of them. If we wanted to transcribe a general formula for each of these stories, it might go something like this: *[Name], [age], could not find relief for [condition] so s/he tried [CAM practice] and now feels better (than ever)*. What are the functions of these stories? Individually, they offer points of identification for readers, perhaps so they can recognize bits of themselves within the larger story about the new science of CAM. They also humanize the research narrative, providing contrast to its institutional flavour. These stories appeal to popular interests and values by, for example, portraying CAM, which so many Americans use, in a positive light. The personal stories also contribute to the authors’ ethos by conveying the sense that, despite all the authors’ talk about placebos and multi-armed trials and the generalizability of results, they really are “one of us.” (Note, for instance, that none of the researcher-authored articles feature personal stories.)

Together, these individual personal stories add up to a larger narrative, one that says, essentially, that CAM-seekers are regular people, motivated by good reasons, and have received both help and solace from the CAM practices they have sought. This larger narrative bolsters the special report's claims about the arrival of the new, integrative medicine. However, this narrative seems also to be an attempt, on the one hand, to placate CAM-using readers that may be disappointed to find out how few CAM interventions have been proven safe and effective, and, on the other hand, to keep advertisers happy, since the report is peppered with advertisements for dietary supplements such as megavitamins, black cohosh, and soy isoflavones. Both of these strategies point to larger tensions underlying popular science.

The two narratives in the *Newsweek* special report about research and CAM-users offer what Segal might call “standard stories” about the science of alternative medicine. These dominant narratives suppress other narratives that might be told, narratives that reveal a more complex view of research culture. What these stories *do not* tell us about medicine can sometimes reveal far more than what they *do* tell. Working within the framework that Segal and others, such as Broks and Carolyn Michelle, have outlined, I want to highlight, in conclusion, some of those suppressed stories, and what they might mean in the context of rhetorical boundary work in biomedicine.

With respect to the narrative of medical research, we learn that scientists race, that scientific work takes time, and that the results that science produces are valuable for being difficult to obtain and expensive to produce. However, we do not hear about why the scientists are “racing” in the first place—or rather, do we not hear the whole story about it, the one that embeds very legitimate concerns about patient health and safety within the larger web of motivations governing biomedical CAM research, such as the concerns I describe in Chapter

One about professional jurisdiction. We also do not learn about the limits of and barriers to research. With science constructed as an almost omniscient power comes the sense that, given enough time and money, science can get to the bottom of anything. If we do not have answers on a particular question, the *Newsweek* special report suggests, it only means that we do not have them *yet*. Although some articles do describe methodological barriers in CAM research, such as the problems of randomization and placebo controls (e.g., Kaptchuk, Eisenberg, and Komaroff, “Pondering”), none engage with the professional disincentives researchers on CAM might face, such as problems of securing funding for their studies (especially given that so much funding now comes from pharmaceutical companies), problems of finding appropriate scholarly venues in which to publish their research, and problems of maintaining their own professional credibility as scientists studying interventions and practices that have questionable status in biomedicine.

The articles similarly deflect the role of politics in research, a strategy that Nelkin identifies as common practice in popular science. Noonan, for instance, criticizes the FDA for its “lack of regulation” of dietary supplements such as herbal remedies and high-dose vitamins, but fails to point out that, under the Dietary Supplement Health and Education Act (DSHEA), the FDA actually does not have jurisdiction over them. DSHEA was legislated in response to public demand and heavy lobbying from the supplement industry, and was vigorously opposed by the AMA (Mitka). Glossing over this history, Noonan instead describes biomedical researchers in heroic terms, as willing to step in and do the FDA’s work in order to save the public from being “lured with false promises and bogus claims” (62).

The many individual stories we hear about patients perform similar deflecting functions. Segal observes that dominant breast cancer narratives block, generically, stories of women with breast cancer that are not energetic optimists, those that do not become better people as a result

of their disease, and those that die. In the *Newsweek* special report, we see a similar pattern. There are no stories, for example, of those that feel they have been defrauded, or those who were injured or became sick as a result of their CAM use. We do not hear from people that resent being told to ask their doctors about health interventions they have used, without physician approval, for years. We do hear a lot about risk, about whether CAM practices are safe, but, as Broks points out, the danger of risk-talk is that we end up “asking simply whether a technology is safe rather than whether it is necessary” (126).

Broks’ question anticipates perhaps the biggest question that is obscured by the personal stories in the *Newsweek* theme issue: why is all this medicine necessary in the first place?

This question is essentially “crowded out,” to use Carolyn Michelle’s phrase (62), by a stream of happy CAM-users, such as the women in the Kalb article on menopause treatments whose photographs I discussed earlier in this chapter. Michelle investigates “anecdotal personalization” in media discourse on cord blood banking programs, which prominently feature testimonials from parents who express both relief that they have secured their child’s future health, and gratitude to CordBank for making that future possible. Michelle’s observation that “anecdotal personalization privileges the experiences and testimony of individuals at the expense of in-depth interrogation and critique” (62) resonates with Segal’s concern that stories praising individual health habits (such as those of Sheryl Crow, who “never skips her vitamins” [7]) and individual health triumphs (rhetorics of “survivorship”) silence other important stories, such as stories about environmental carcinogens.

Part of the emphasis on individually-oriented explanations of illness, as both Michelle and Segal point out, links back to consumer values, and to the idea that we are solely responsible, for better or worse, for our own health. In the case of *Newsweek*, we do not hear, for instance,

about the cultural conditions that make it possible to think of menopause as a medical condition requiring treatment. With the evidence suggesting that hormone replacement therapy may do more harm than good, women are depicted in Kalb's article as essentially stranded, desperately in need of treatment, as though menopause were a condition from which one could die. The suppression of these kinds of questions about CAM, about not simply whether or not it works, but about the entailments of bringing CAM under the purview of biomedicine, has consequences that play out, literally, in the bodies of CAM-users themselves.

Fahnestock argues that popular science is primarily epideictic, in the sense that it reinscribes conventional values about science. I would suggest, following Fahnestock, that the *Newsweek* special report also reinscribes *cultural* values, even though those values, as manifest in the magazine, are somewhat contradictory. Taken as a whole, the report praises individualistic qualities such as pioneerism, savvy, and steely determination, exemplified by the Janzes, who bucked conventional medical wisdom and found a doctor that would listen to them. But the report couches the exemplars of those individualistic qualities—the patients, practitioners, and researchers depicted—within narratives that instead reinscribe science's hegemonic influence over other aspects of North American life, such as consumer behaviour and individual health habits. The Janzes, for example, conduct their experiment with CAM under a doctor's supervision. These narratives stress, ultimately, the necessity of risk-avoidance and tightly controlled consumer behaviour, and the importance of public respect for hierarchy and expertise—respect, that is, for science.

The borders between biomedicine and CAM that materialize in the *Newsweek* special report mirror those within the *JAMA-Archives* corpus that I have discussed over the course of this dissertation. In both cases, biomedicine emerges from its boundary encounter with CAM

reinvigorated, with a wider scope, perhaps, but still recognizable as biomedicine. The model of CAM returned, as the “new science,” to the public realm in the *Newsweek* report can illuminate how boundary work operates even beyond the limits of upstream science. It can show, for instance, how the activities of science itself can be separated, conceptually, from those of scientists, re-making science as at once both steadfast and nimble. The re-making of science and medicine in popular contexts is persuasive because it conditions how we think about how they operate, and about how they affect our lives. Further, studying these boundaries as they emerge, differently, within the same “discourse moment” (Moirand) can help us to think in more complex terms about popular science and popular medicine as related but distinct. Biomedical boundary work occurs at all points along the spectrum of upstream/downstream science. As this chapter argues, popular reporting on CAM research is a crucial site for the “repeated and endless edging and filling” of biomedical boundaries (Gieryn, *Cultural Boundaries* 14) because popular reports not only reinforce boundaries drawn further upstream: they also draw boundaries of their own.

CONCLUSION

Boundaries as Points-of-Entry

The progress and shape of this dissertation can be described in Kenneth Burke's metaphor of photographic filters: describing his theory of "terministic screens," Burke explains, "I have particularly in mind some photographs I once saw. They were *different* photographs of the *same* objects, the difference being that they were made with different color filters. Here something so 'factual' as a photograph revealed notable distinctions in texture, and even in form, depending on which color filter was used for the documentary description of the event being recorded" (*Language* 45). In the preceding chapters, I have viewed the *JAMA* and *Archives* coordinated theme issues on CAM as a set of snapshots of the medical profession's boundary-negotiating discursive activity. Each chapter has examined biomedical boundary work through different filters: the historical-professional, the epistemological, the clinical, and the public. Each revealed different aspects of medicine—and of rhetoric.

The first and second chapters highlighted what I have called the "internal" or upstream dimensions of biomedical boundary work by applying historical-professional and epistemological filters respectively. Chapter One illuminated the *JAMA-Archives* corpus as a network of profession-defining texts, in Bazerman and Paradis' terms, which I argued, following Charland, called forth a community of scholars in which biomedical research on CAM was simply a routine matter. I showed that, although editors Fontanarosa and Lundberg cite CAM as already well within biomedicine's purview, their claim was less descriptive than it was agenda-setting, as CAM's legitimacy as an area of research was far from settled. I illustrated, by invoking Star and Bowker, how the *JAMA-Archives* corpus set a course for future biomedical study of CAM by defining the practices CAM comprises in essentially negative terms, as *not-*

biomedicine. Emptied of their own internal significance, CAM practices are taken up in the corpus within the framework of evidence-based medicine—a context in which, I argued, the randomized, controlled trial functions in part as a technology of exclusion.

Chapter Two applied an epistemological filter through the example of the RCT in CAM research. I argued that methodology operates in the *JAMA-Archives* corpus as a rhetorical topos that can position research on CAM variously within and beyond biomedical boundaries, depending on a given commentator's orientation within the debate. I illustrated how the experimental article, as a genre, can persuade us to view biomedical research in terms that underemphasize the degree of variability involved in both the design of RCTs and the interpretation of their results. As a case in point, I isolated the concept of efficacy to show that, despite claims on all sides that the evidence on CAM will speak for itself, evidence can be marshalled persuasively in arguments about whether not a given health intervention works. This is true, I illustrated, both in research on CAM and in biomedical research more generally.

The third and fourth chapters moved from the medical profession to its points-of-contact with the public realm, applying clinical and public filters respectively. As Gieryn argues, while upstream science goes a long way to determine how science gets mapped as a bounded cultural space, the map itself depends crucially on reciprocal boundary work further downstream. Chapter Three examined boundary work in the context of clinical practice. I argued that, because CAM practices are frequently cited as attractive to patients because of their greater emphasis on practitioner-patient interaction, interaction itself can be a useful place to investigate how boundaries are configured by biomedical research on CAM. What I found was that practitioner-patient interaction is framed in trials of acupuncture and chiropractic primarily as a contaminant, as something to be controlled for through innovative design. I placed Burke's concept of

terministic screens in relation to Arthur Kleinman's notion of explanatory models, which are defined as mental schemes for organizing and understanding illness, and for making decisions based on those schemes. Putting Burke's and Kleinman's theories in relation to one another helped me to articulate a framework that could account for how interaction, and ideas about interaction, can persuade us toward different perspectives on how research on CAM ought to be conducted. I used the idea of placebo controls as a heuristic to explore how the RCT model accounts for the nonspecific and interaction effects of clinical encounters, which are often discounted in CAM research as imaginary.

The popular media also play an important role in the downstream production and maintenance of medical boundaries; this was the "public filter" of the fourth chapter. I took as my example the 2002 *Newsweek* special report on biomedical CAM research, which announces, much like the *JAMA-Archives* corpus, the arrival of a new, integrative model of medical research and practice. I examined the magazine as a cultural artefact and probed, first, the contours of the "new science" about which it reports, and then the rather conventional model of science that underlies it. I illustrated how the magazine, again like the *JAMA-Archives* corpus, configures CAM research as both new under biomedicine's purview and yet old and already accepted.

I suggested in that chapter that one of the important features of popular reports on CAM research is that the products of CAM research are essentially returned to the public that motivated it. However, the CAM that is returned to the public in *Newsweek* is both biomedicalized and implicitly hierarchical: while the promise of greater patient agency and autonomy is framed as a major attraction of CAM (as I indicated at the end of Chapter Three), the model of CAM that appears in the pages of *Newsweek* is thoroughly in the hands of the biomedicine. I argued, following Segal ("Breast Cancer"), that by reconfiguring CAM so fully

within a biomedical framework, the story told in the *Newsweek* special report about the “new science” of CAM makes other stories about CAM research harder to tell, such as stories about political, professional, and epistemic barriers to research on CAM, and about how CAM practices are transformed as they are brought into the realm of biomedicine. Perhaps the most important story not told in *Newsweek*, I suggested finally, is about why so many patients are seeking so much health care to begin with, whether that care is mainstream, alternative, or “integrative.”

In her review of scholarship in rhetoric of health and medicine, Segal argued that the “usefulness [of rhetorical projects on health] often lies in their ability simply to pose questions that are prior to the questions typically posed by other health researchers” (“Rhetoric of Health and Medicine” 228). The example she gives is of plastic surgery: while health research will inquire about, for instance, how to make such surgery safer or about whether or not it should be reimbursable by insurance, a conceptually prior question would be about how “people [are] persuaded to see themselves as improvable by cosmetic surgery in the first place” (228). The question I raised at the end of Chapter Four, about how so much medicine came to be necessary at all, was a similar sort of “prior question,” one that underlies this dissertation but which I took up only indirectly. I return to that question as I conclude below because it frames potential areas for future inquiry. First, however, I will expand one of the prior questions that I did ask explicitly in the course of these pages because it can illustrate how, as I argued in my introductory chapter, “the study of rhetoric at the fringes of medicine is illuminating for both medicine and rhetoric.”

Throughout this dissertation, especially in Chapters Two and Three, I examined how CAM interventions such as acupuncture and chiropractic fit (and do not) within biomedical research contexts. As I indicated in those chapters, much health research scholarship, generally,

has focused on questions of how best to translate medical research into practice. (This is, for instance, one of the primary concerns within the EBM model.) My own focus—my “prior question”—was instead on the models of practice presupposed in research contexts themselves. What can they tell us about how we *conceptualize* medical practice—how we think medicine happens, as compared to how it actually does happen in clinics and hospitals? As I argued in Chapter Three, a rhetorical approach can articulate how different approaches to practitioner-patient interaction in research contexts can persuade: they can persuade us to view patients as particular kinds of decision-makers, or to view medicine as unproblematically scientific, or to value some interventions over others. Biomedical boundaries can be negotiated in part through the ways that research accounts for practice.

A rhetorical approach illuminates what happens when patient-centred interventions are integrated into a system of medical practice that seeks to be essentially patient-free. Although research on doctor-patient interaction has consistently demonstrated the vital importance of the clinical exchange in patient care, this knowledge has not translated well into research contexts, where interaction effects are often dismissed as statistical noise. The effort to purify health interventions of their contingency is in part rhetorical, boundary-focused: it constitutes an argument that draws lines of allegiance and of epistemic authority. For example, underneath the impulse to isolate spinal adjustment as chiropractic’s “active ingredient” lie questions about allegiance to professional values, such as “What counts as an appropriate methodology?” and the corollary question, one of epistemic authority, “Who decides?” The challenges of placebo controls further open up to scrutiny the criteria for what qualifies as a contribution to knowledge.

The picture that emerged in my investigation of the models of practice effected in research contexts was one in which patients, in all their particularities, seem rather like statistical

noise themselves. This observation is not new, exactly—it is a key idea behind critiques of evidence-based medicine—but rhetoric can add texture to other critiques by offering a framework for tracing the slippage between conceptual models of medicine and the enactment of those models in research and practice.

This project contributes to health research by drawing attention to the persuasive element of biomedical research. While research produces vitally important knowledge about health and medicine, it simultaneously produces the professions that engage in it: biomedicine is ultimately recognizable *as* biomedicine through its association with scientific methods and procedures. Boundary work is, accordingly, a constituent of medical research because the ways that researchers line up their work with those methods and procedures will either assert the researchers' membership within the professional community or declare them outsiders. Fontanarosa and Lundberg claim, for instance, that the CAM research reported in the *JAMA-Archives* corpus is unproblematically scientific because the contributions were judged through the journals' "usual rigorous editorial evaluation and peer review" as conforming to the methods and procedures set by the biomedical community (Fontanarosa and Lundberg, "Call for Papers" 2112). Research on CAM, I have argued, is particularly well-suited to the investigation of the rhetorical dimensions of biomedical research because the practices that CAM comprises do not fit easily with accepted scientific methods. Examining how such studies are designed and evaluated illuminates the persuasive aspects of biomedical research, generally, because the studies magnify the various contingencies that operate within it.

This project contributes to rhetoric by opening up a view on the means through which members of a culturally dominant profession respond to challenges to their jurisdiction while the profession appears, in effect, not to have been challenged at all. As I have argued throughout the

foregoing chapters, the boundary work performed in the *JAMA-Archives* corpus is virtually invisible, even as the contributors air publicly their concerns about the potential impact of CAM research on their profession. Boundary conflict is part of the normal life of the professions as they compete for space within what Abbott calls the professional ecosystem. But, even if such conflict is normalized as routine in that ecosystem, normalization does not account for the ability of the medical profession to engage in boundary work without appearing to have done so: something about the rhetoric of boundary work seems to cover its own tracks.

Examining this self-concealing aspect of the rhetoric of boundary work can return to rhetorical theory insight into processes that appear, on the surface, not to be rhetorical at all. While commentators on all sides of the biomedicine-CAM debate point to evidence produced by clinical trials as the key to settling where the boundaries of legitimate medicine ought to be, their own arguments about that evidence set boundaries, too. The study of these arguments can contribute to rhetoric a better understanding of, for instance, how boundary objects, such as the notion of efficacy, can both enable research across disciplinary borders and strategically draw lines of distinction among disciplines. Some boundary objects contain within them a degree of latitude that can be manipulated by dominant actors. Such boundary objects are, I have argued, “rhetorically mobile.”

The rhetorical study of biomedical boundary work can also enrich our understanding of how categories persuade by showing how residual categories can be invoked to lay claim over professional territory: by emptying the individual practices included in the category of CAM of their own particularities, the medical profession positions itself to overwrite them in biomedical terms. That is, as a “garbage” category, to borrow Star and Bowker’s term, CAM is more easily claimed within a biomedical framework, or dismissed.

By way of conclusion, I would like to turn to the following story. It illustrates, in miniature, one of the larger claims I have made over the course of this dissertation—a claim I framed in Chapter One in Roy Porter’s words, which bear repeating here: “quackery never prospers, for if and when it does, it becomes termed medicine instead” (207). As I neared the end of this project, I came across an article about the use of melatonin in children insomniacs on Babble.com, a website for trend-conscious urban parents. Author Cole Gamble reports that, when his three-year-old daughter would not, or could not, sleep, an acupuncturist friend of his suggested trying melatonin, a hormone available in the US as a dietary supplement. Gamble responded dismissively: “‘Oh. Witchcraft.’ This girl is nice, but she plans her week by astrological charts, so I tend to take her advice with a grain of not-crazy. I am not one for holistic medicine. I trust hard, pharmaceutical drugs with extensive clinical trials. If it hasn’t been injected into a rat’s eyeball or been ingested by thousands of monkeys, I can’t take it seriously.” Only when his friend explained that it was her pediatrician that led her to suggest melatonin did Gamble take her seriously and follow up with his own child’s doctor, who agreed that melatonin might be worth a try. Gratiified that the supplement came with the approval of the medical establishment, Gamble decided to give melatonin to his daughter and that night, she slept.

In the story of Gamble’s daughter, we see first-hand how the clinical trial, as a research methodology, has accrued such rhetorical force that it is taken even in popular contexts to be “medicine’s most reliable method for ‘representing things as they really are’” (Kaptchuk, “Double-Blind” 541). The priority Gamble accords to the RCT may seem to contradict my claim, in earlier chapters, that individuals that seek CAM interventions, particularly dietary supplements, without much regard to whether they have been successfully tested in clinical

trials. However, what I think this example shows is how successfully CAM has been reframed, over the past two decades, within a biomedical framework.

Gamble's article was published in 2007, nearly a decade after the *JAMA-Archives* theme issues on CAM were published, and half a decade after *Newsweek* announced the arrival of the "new science" of CAM. The efforts of these earlier discursive activities to redraw biomedicine's boundaries to include CAM seem to have been brought to fruition in Gamble's article. This is how he concludes: "All of my parenting efforts were nothing compared to that simple little pill. I can't help but feel a little powerless. But now that I sleep soundly and wake to a smiling, rested child, I think I can learn to live with this witchcraft in the pill bottle." In these lines, as in Gamble's article as a whole, the discourses of biomedicine and CAM are fused—or rather, CAM is described *in terms of* biomedicine. Gamble's attitude toward CAM and biomedical research illustrates the shift I have described throughout this project as the domain of biomedicine has increasingly expanded to other health care practices. As the various practices that constitute CAM are reframed in biomedical terms, they become themselves, in an important sense, biomedical. However, it is not just that the domain of biomedicine has expanded to other health care modalities—it has also expanded to other diverse areas of human life.

To return to the implicit prior question that underlies this dissertation, about why so many individuals are seeking so much health care, regardless of modality, I want, finally, to suggest that the story of Gamble's daughter is also, in part, a story of how a difficult and unsettling problem, such as a toddler's struggle to sleep, is defined as a medical problem that is solved with a dietary supplement, a CAM intervention that I argued in Chapter Three is rooted not in the idea of illness but of wellness. While biomedical discourse often sets the terms of debate in an increasingly medicalized society (see Conrad), dietary supplements, as readily available

commercial products, seem beyond medicine's reach. And yet, the idea of wellness itself—conceived broadly as *health maintenance*; contrasted with *illness treatment*—seems, paradoxically, to have the capacity to expand the domain of illness, bringing those who choose wellness-oriented products and services such as dietary supplements and other CAM practices into a realm defined instead by *dysfunction*. While these products appeal to individualist models of resistance against the so-called medical-industrial complex, the idea of wellness itself has been transformed into a new kind of dependence (on handbooks, supplements, websites, consultants, practitioners) that leaves its users resembling the disempowered, medicalized patients they seek not to become. “Wellness” has become, in effect, an incipient illness, a transformation, rooted in discourse, that is ripe for further research.

Rhetorical study of biomedical boundary work can open up space for such research by isolating and describing the specific mechanics of the negotiation of borders between, for example, mainstream and alternative medicine, accepted and unaccepted methods and evidence, real and illusory treatment effects, and even between illness and health. Rhetoric at the fringes of medicine importantly determines what medicine is, and what it will be; the study of that rhetoric can consequently help us better understand the processes through which some health practices and practitioners have come to ascendancy over others. By examining the means through which biomedical boundaries are effected through persuasion and are themselves persuasive, we also gain crucial insight into the means through which we come to know about our bodies and our health, and about our own place, as rhetorical subjects, in the realm of health and medicine.

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APPENDIX



The University of British Columbia
Office of Research Services
Behavioural Research Ethics Board
Suite 102, 6190 Agronomy Road, Vancouver, B.C. V6T 1Z3

CERTIFICATE OF APPROVAL- MINIMAL RISK RENEWAL

PRINCIPAL INVESTIGATOR: Judy Z. Segal	DEPARTMENT: UBC/Arts/English	UBC BREB NUMBER: H05-80831
INSTITUTION(S) WHERE RESEARCH WILL BE CARRIED OUT:		
Institution		Site
UBC Other locations where the research will be conducted: N/A		Vancouver (excludes UBC Hospital)
CO-INVESTIGATOR(S): Colleen Derkatch Janet L. Giltrow		
SPONSORING AGENCIES: Social Sciences and Humanities Research Council of Canada (SSHRC) - "Scientific Method as Rhetorical Topos"		
PROJECT TITLE: Scientific Method as Rhetorical Topos		
EXPIRY DATE OF THIS APPROVAL: May 1, 2009		
APPROVAL DATE: May 1, 2008		
The Annual Renewal for Study have been reviewed and the procedures were found to be acceptable on ethical grounds for research involving human subjects.		
<p>Approval is issued on behalf of the Behavioural Research Ethics Board</p> <hr/> <p>Dr. M. Judith Lynam, Chair Dr. Ken Craig, Chair Dr. Jim Rupert, Associate Chair Dr. Laurie Ford, Associate Chair Dr. Daniel Salhani, Associate Chair Dr. Anita Ho, Associate Chair</p>		