How Do We Know What is the Best Medicine?
From laughter to the limits of biomedical knowledge

by

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Abstract

Medicine has been called a science, as well as an art or a craft, among other terms that express aspects of its practical nature. Medicine is not the abstract pursuit of knowledge. Medical researchers and clinical practitioners aim primarily to help people. As a first approximation then, given its practical focus on the person, the most important question in medicine is: what works? To answer that question, however, we need to understand how we know what works. What are the standards, methods and limits of medical knowledge? That is the central focus and subject of this inquiry: how we know what works in medicine.

To explore medical knowledge and its limits, this thesis examines the common notion that laughter is the best medicine. Focusing on laughter
provides a robust case study of how we know what works in medicine; it also, in part, reveals the thin, perhaps even non-existent, distinction in medicine between empirically-grounded knowledge and intuition.

As there is no single academic discipline devoted to laughter in medicine, the first chapter situates and charts the course of this unusual project and explains why inquiry into laughter in medicine matters. In the following chapters, we encounter claims from distinguished sources that laughter and humor are the best medicine. These claims are examined from a variety of perspectives including not only the orthodox view of evidence-based medicine, but also from narrative, evolutionary and complexity views of medicine. The rarely explored serious negative side of laughter is also examined. No view provides a firm foundation for belief in laughter medicine.

A general conclusion from this inquiry is that none of the approaches effectively tame the complexity of medical phenomena; indeed each starkly reveals a greater complexity than found at first glance. A narrower conclusion is that providing a basis for claims about laughter in medicine poses its own specific challenges. A third conclusion is that, as things stand, none of the existing approaches seems up to the task of determining whether something such as laughter is the best medicine.
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I am unable to acknowledge the poodle incident.
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1. Laughter in Medicine

This project examines laughter in medicine as a means of exploring medical knowledge and its limits more generally. Laughter plays a large role in medicine, appearing not just as therapy but everywhere from doctors' offices to hospitals to medical schools (Robinson 1977). Consequently, laughter can be considered a proxy for medical customs and interventions that we don't know much about, many of which are not even deemed to be biomedical, but are nevertheless used by most people throughout the world.

Laughter has also been taken seriously as something that works in medicine. When Norman Cousins (1976) famously reported that he had cured himself of a debilitating disease using vitamin C and laughter, which he described as "jogging for the innards", he re-energized scientific inquiry into the role of laughter in medicine. If it seems implausible that a patient would abandon orthodox medicine for laughter, it's important to observe that many patients choose equally implausible unorthodox treatments, such as remote intercessory prayer and homeopathy. Cousins did exactly that for laughter when he abandoned the hospital in which he had been receiving treatment and moved into a hotel to treat himself. Although he had no medical training, Cousins projected his ideas even further into the medical mainstream when he secured a faculty position at UCLA medical school. Cousins has also been attacked as a quack and a fraud, not just hailed as a healer ahead of his time. But the popularity of his work stimulated research into laughter, humor and health that continues today.
1.1 From Popular to Scholarly Laughter

Far from being a brief guffaw in the recent history of medicine, interest in the role of laughter in medicine and health remains strong. A Google search of the phrase "laughter is the best medicine" returns 2,800,000 links including more than 3000 links to books and nearly 2000 links to videos. Reinforcement of the theme of laughter as medicine starts as early as breakfast: packaging for Quaker Oatmeal offers "Tips for a Happy Heart: Have Fun. Laughter is good medicine." After that meal, or any other, comes a brush with conceptions of laughter from popular science provided by Rembrandt toothpaste advertising:

As we continue to make great advances in oral care, sometimes we have to stop and enjoy a good laugh. Laughing releases endorphins, neurotransmitters that make you feel good. Exposing yourself to the laughter of others can have the same effect. At Rembrandt, we think laughing is essential to a healthy, happy mouth. After all, white teeth don't matter if there's nothing to smile about.

The laughter bandwagon continues into the evening entertainment: the well-known oncologist, author and broadcaster Robert Buckman (2008) entertained a live audience for a later CBC radio broadcast on the subject of laughter as medicine. The latest film in the Batman franchise, The Dark Knight (Warner Bros. 2008), which set box office records and reaches an audience population in the millions, includes a chase and gunfight scene in which the evil Joker and his clown gang are careening around in a circus truck that has "Laughter is the best medicine" painted on the side with a large red letter "S" drawn to change Laughter to SLaughter. Hollywood also brought Patch Adams M.D., the physician clown, to the big screen. A page at the University of Maryland Medical Center website, inviting a general
readership to "ask the expert", boldly asserts: "The old saying that 'laughter is the best medicine,' definitely appears to be true when it comes to protecting your heart" (University of Maryland 2011). At worldlaughterdayerday.org, we are told that laughter is not only healthy for the individual body, but it can also cure the social ills of aggression and war. If popular culture is any indication, the notion of laughter as medicine is plump and healthy.

That laughter as medicine is a long-believed, popular story, a plausible story, and even a desirable story, does not substantiate the story. Countless long-believed popular stories are false. The following chapters will show that there is no scientific basis for the general claim that laughter or humor improves health. There is likewise no scientific basis to claim the opposite, that laughter or humor does not improve health.

These are complex, difficult matters to study. One central problem that plagues any inquiry into laughter in medicine is unconfounding various unknown confounding influences. In this inquiry we encounter not only this general difficulty of science, but also specific problems in pinning down what medical researchers are doing and reporting. What are they claiming to study when they refer to amorphous notions such as health, disease, pain, happiness, humor and laughter? What counts as improvement in health and happiness? How do they measure individual subjects, then gather and interpret population data and analyze it, and make linear connections from a non-linear complex of observations and ideas, then apply what they have found in the studied subjects to different individuals not studied? Exploring questions such as these in the case of laughter will illuminate other cases.

Laughter may be one of the most intractable aspects of being human. The scholarly study of laughter may turn out to be like the scholarly study of humor, poetry, art, music and other things that change into something different or vanish altogether when researchers get too nosey. This limitation
is expressed in E.B. White's widely misquoted complaint that, "analyzing humor is like dissecting a frog: few people are interested and the frog dies of it." A brief Internet search will provide copious other variants attributed to White. The great stylist actually said something more positive:

Analysts have had their go at humor, and I have read some of this interpretive literature, but without being greatly instructed. Humor can be dissected, as a frog can, but the thing dies in the process and the innards are discouraging to any but the pure scientific mind... [humor] has a certain fragility, an evasiveness, which one had best respect. Essentially, it is a complete mystery (White 1941, 243).

His words are far from a dismissal of humor or laughter analysis. Indeed, the very thing he is doing is analyzing humor. His analysis, like that of many other researchers, ends in mystery. As a humorist he was not greatly instructed by interpretative literature about humor. But is the artist usually instructed by interpretive literature? White left open a whole realm of inquiry for minds unlike his, particularly the scientific mind he mentions, that does not seek to create humor but nevertheless inquires into and interprets its nature. Those other minds include the philosophical mind that for the most part leaves the creation of humor and laughter, and any science experiments, to others while only peering into the mystery, perhaps to clarify its assumptions and its boundaries.

Although fiction and religion do not occupy much space in science journals, it is significant that many scientific papers on the subject of laughter quote non-scientific sources including fiction, the popular press and religious texts such as the Bible when setting out the background of experimental and theoretical reports. Discussion of laughter in medicine seems to linger between everyday usage and medical usage and thereby connect them. Partly this is an issue of the place of narrative in medicine, both in the content and in the style of reporting medical research, as will be
discussed further in the chapters on stories and anecdotes in medicine. Compelling narratives in medicine rely on dynamic use of language, including analogy, metaphor, and common and medical usage; in the same way that Newton’s use of “force” was both precise (the three laws of motion and law of gravitational attraction precisely placed its role in the dynamics of motion) and imbued with significance from common usage (the metaphors of force, forces of evil for example, psychologically enriched, by connotation, the power of Newton’s term). This duality of common and medical usage is also partly a question of linguistic and conceptual precision in medical research and literature. Science demands more precision than the metaphors of common language provide. Laughter and related concepts are difficult to define and measure with the required precision; this results in a propensity to rely on common usage with caveats. Another feature of medicine viewed as a science is that like science more broadly, it attempts to reduce the world to manageable parts and at its most extreme says the parts alone can explain everything. The if-then type of explanations in science have trouble reducing the idiosyncratic and personal aspects of laughter to manageable or even comprehensible parts.

Throughout this inquiry, tensions are always present between the desiderata of science, such as precision and reduction, and the linguistic richness of ordinary discourse, a richness exploited, not tamed, by historical narratives, fictional creations, cultural commentary and the like. There is a lingering question whether medicine-as-science fails to get the central point. Maybe the notion of laughter as medicine is wholly metaphorical, with no precise meaning, and was never meant to be dissected like a frog in a reductive process. Perhaps the reductionist approach of medicine-as-science cannot penetrate the mystery of laughter by carving it at its joints; in dissecting laughter, like dissecting the frog, it kills laughter. Yet researchers
persist in their efforts to prove that laughter is healthy and medicine-like. It is reasonable to conjecture, at this point, that this is a consequence of promoters of laughter in medicine such as Norman Cousins claiming something vague about laughter curing disease, or relieving pain, or just improving perceptions of illness. The whole subject of laughter and its relations with medicine and health may be constructed of metaphors all the way down.

Laughter as medicine could be a metaphor that represents, say, the good things in life, wellness, happiness. Medicine itself could be a metaphor for doing whatever it takes for whatever ails you. Or laughter and its relations with medicine and health could be metaphors for something completely different since metaphors and other imaginative, artistic, poetical devices, unlike science, have no particular methodologies, no right or wrong.

1.2 Methodological Laughter Medicine

Medicine, however, does have prescribed methodologies. The kinds of evidence that matter in Western biomedicine are currently determined by a cluster of notions that have been gathered under the slogan of evidence-based medicine. Within evidence-based medicine, the preferred answer is that randomized controlled clinical trials are the way to determine what works best in medicine. All other sources of medical knowledge are less valued in the view of evidence-based medicine. A typical randomized controlled trial (RCT) compares the outcomes in two groups of people who are randomly chosen to receive one of two medical interventions. Usually one intervention is deemed to be the standard or control against which the other is measured. Often the control is something called a placebo. The label "randomized
controlled” applied to an experimental trial is thus derived from the use of random allocation and a control group. More complex trials can have multiple groups and multiple interventions. A statistically significant difference between treatment and control groups is required before a treatment is deemed to work in evidence-based medicine.

Adding to the many critiques of evidence-based medicine is not the principal goal of this thesis. Indeed, the success or failure of such critical analysis does not affect the thesis. With its emphasis on evidence hierarchies, however, and randomized controlled trials as the gold standard of evidence, evidence-based medicine pervades analysis of the claim that laughter is the best medicine. Therefore an account of evidence-based medicine at the outset renders more clearly the central thesis that the evidence demanded by evidence-based medicine in support of the claim that laughter is the best medicine has not been and may never be provided. Nonetheless, as we shall see in subsequent chapters, a wealth of medical literature and purported medical research either takes the claim for granted or provides alternate forms of evidence for this claim, such as narrative, anecdotal and evolutionary evidence. If the evidence demanded by evidence-based medicine cannot be provided to support the claim that laughter is the best medicine, then determining the credibility of the claim, if that is possible, must lie with one or more different kinds of evidence. If these alternative kinds of evidence are not up to the task, then we have uncovered a limit to medical knowledge; that is, there are some claims about what works in medicine that cannot be resolved through current research methods. Hence, the validity of claims such as "laughter is the best medicine" cannot be determined. As a consequence, there is no rational basis for accepting or rejecting them.

Before turning to the detailed arguments for this thesis, the tenets of evidence-based medicine are explained in this chapter. Chapter 2 then looks
at a range of laughter experiments. These experiments generally do not meet the standards of evidence-based medicine and do not tell us much at all about laughter and medicine. The best we get from most laughter experiments according to current scientific methods and standards is weak and inconclusive results. So if we are to understand laughter in medicine, the inquiry must extend beyond evidence-based medicine.

A prominent reaction to evidence-based medicine has been called narrative-based medicine. Chapter 3 tells the stories of two inspirational figures, one from each side of the division between the sciences and humanities, who are often cited in relation to their personal experiences with laughter, humor and medicine. When contemplating a prediction of their own premature death, each rejected dispassionate scientific quantification by their medical providers in preference for the humane stories that preserved their hopes while they lived and now keep them alive in our minds after their death. They relied primarily on personal experience, not on randomized placebo-controlled trials, to determine whether laughter and humor works in medicine. By the end of the chapter we will have encountered claims from distinguished sources that laughter and humor are the best medicine. But the basis for the claims is primarily what in medicine is disparagingly called "anecdotal evidence".

The apparent omission of anecdotes such as these from evidence-based medicine is taken up in Chapter 4 that explores the role of anecdote in medicine. Anecdote is where we often find not only laughter itself but also medical and health anecdotes about the effects of laughter. Here the claim is that "anecdotal" should not be used as a term of insult as it is currently used in evidence-based medicine. Anecdotes play valuable roles in medicine. Indeed, evidence-based medicine places great faith in single-case experiments that share many features of anecdotes. The plausibility of the story matters,
whether it's a story of one person or the story of a systematic review of many stories of clinical trials, which themselves are reported as the stories of what the experimenters did.

The focus of Chapter 5 is laughter language. Evidence-based medicine relies on precise measurements and calculations; what precisely is being measured and calculated in laughter studies? We find no agreed upon language for studying laughter, and little precision, unlike in science and medicine that proceed on the basis of many precise standards. Laughter appears in many forms and is associated with many feelings. Laughter in any form consists of a complex series of processes spread through various parts of the body. To discuss laughter as if it were one unitary thing, laughter simpliciter, is a mistake. But to discuss its many aspects is an overwhelming challenge. Carving laughter at its joints requires a sharp scalpel.

All medical treatments have been said to have a placebo component, and many are said to be nothing but placebos, yet nobody knows exactly how placebos work. Still the mysterious things called placebos are fundamental to clinical trial methodology and the view within evidence-based medicine that those trials are the best way to know what is the best medicine. No inquiry into laughter, medicine and health will be complete without considering placebo. So in Chapter 6 we examine whether laughter acts as a placebo. Comparing anything with placebos as a basis for knowledge of what works in medicine is deeply problematic. Instead of finding the answer that laughter does or does not act as a placebo, we see that the placebo concept itself is at best confusing and should be abandoned.

Chapter 7 turns to the neglected serious negative side of laughter. We want good news, and there is a positive bias not just in the case of laughter but in most of medicine, including the reporting of clinical trials so highly prized by evidence-based medicine. For one thing, it's hard to get money to
pay for research that produces negative results. Here the claim is that laughter medicine is not always positive, free and harmless.

Laughter in the context of evolutionary medicine becomes the focus in Chapter 8. Evolution is not the first theme that comes to mind when thinking of laughter, medicine and health. But according to Dobzhansky’s dictum, nothing in biology makes sense except in light of evolution. Evolution has made sense of other things in biology and medicine, so the hope here is that it can make sense of laughter in medicine. For instance, the fittest but not necessarily the happiest survive. Those who would use laughter to eliminate negative emotions can be compared to those who would use drugs to eliminate pain. Pain has an adaptive purpose, to force the sufferer to find and remove the cause of the pain. So eliminating pain before the cause is found can be dangerous to health. Those who advocate using laughter to improve health suggest that the relationship is simple: the more positive the emotion, the more health. Evolutionary medicine indicates that the picture is more subtle and complex. Although experts have made claims in discussions of human evolution about the myriad health benefits of laughter, by any standards the basis for those claims is weak at best.

In the final chapter before a brief conclusion, the complexity of this project becomes the focus in a discussion of limits. Many researchers call for more evidence, as if it is only a matter of time before science establishes beyond doubt whether laughter truly is the best medicine. But as the previous chapters have shown, inquiry into laughter and medicine reaches into a complex network of themes. The network extends from the very language used to grasp the issues, to the nature of science and medicine to the connections between mind and body to the evolution of laughter in primates to the dogma of evidence-based medicine and the drama of narrative-based medicine. Many aspects of laughter are uncertain, from where laughter fits
within human evolution to what makes someone laugh. Knowledge of laughter invokes networks of vague, elastic entities such as health, sense of humor, anxiety, stress and many others, each with its own internal networks and degrees of freedom. Medicine is a realm of uncertainty both from the perspective of uncertainty in the world and from the perspective of limits to the use of available knowledge (see for example Fox 1957). The notion of uncertainty is related to notions of complexity, probability, odds, statistics and forecasting. Every medical event involves a prediction, such as that the diagnosis will turn out to be correct, that the treatment will work and that the patient will improve. Researchers often call for more funding and research, predicting that it is only a matter of time until science shows the way to an accurate diagnosis, a predictable treatment and eventually eradication of disease. Yet the Laplacean vision of a world that is completely predictable in principle has long been replaced by less certain visions that base their predictions not on stone-carved certainties but on the hit and miss of probability distributions.

Laughter research has limits as does all scientific research (see for instance Barrow 1998). One limit has been the apparent lower status of laughter in relation to other philosophical and scientific research. Laughter researchers (gelotologists, from Gr. gelo-, geloto-) have often noted that others may consider the subject to be lightweight and unworthy of serious investigation. But the very status of laughter in biomedical inquiry goes to the heart of the question of what is accepted as biomedical knowledge and thus how we know what works in medicine. Philosophers of medicine often dwell on science, rationality and logic. Insofar as medicine is an art, albeit a science-using art, medical knowledge and expertise are not limited to scientific knowledge and expertise. Laughter, music, art and individual responses do not fit smoothly into the prevailing philosophy of
standardization, statistical studies and evidence-based practice. Nor does laughter conform to an impersonal, pharmaceutical, silver-bullet, quick-fix medical model. Beliefs about laughter provide an entry into broader conceptions of medicine. A growing thread in the literature makes the case that medicine has lost sight of the patient while focusing on science and technology. Attempts to invoke a science of medicine, as this exploration of laughter demonstrates, eventually bump against the boundaries between the objective and the subjective, the outside world and personal experience, statistics and the individual, science and the humanities.

A principal aim of these chapters is to show that the orthodox cluster of statistical, hierarchical notions within evidence-based medicine is not the only, the best, or in some cases even the most sensible answer. If this inquiry were limited to a conventional case study within the safe confines of the standards, methods and practices already deemed acceptable in orthodox biomedicine, then we might overlook issues at the boundaries or beyond the orthodox view. A pluralist and networked view is proposed rather than the linear and hierarchical answer of evidence-based medicine. To make the case for such a view, the familiar claim that laughter is the best medicine is used to explore the question how do we know what is the best medicine. The need for a plurality of perspectives is the central point. Knowledge of what works in medicine depends on multiple viewpoints. What also emerges is that, even when a plurality of perspectives is brought into play, some subjects such as laughter may ultimately be too complex to yield to current methods.
1.3 The Nature of this Inquiry

As the title of this work indicates, and the previous section demonstrates, this is an interdisciplinary inquiry. Its subject, laughter in medicine, crosses numerous disciplinary boundaries including psychology, sociology, medicine, anthropology and philosophy at the intersection of medical knowledge and laughter. The challenge faced by this investigation, however, is that there is no single well-developed intellectual enterprise focused on this subject. The study of medical knowledge might seem, at first glance, to be situated in the philosophy of medicine; that, after all, is where epistemological aspects of medicine are investigated. But as we will see in the following discussion, there is debate about whether philosophy of medicine even exists and if it does what its boundaries are. Moreover, to determine what philosophy of medicine is requires the related determination of what its subject, medicine, is, and there is no settled opinion on that issue (see Thompson 2010).

Medicine encompasses domains as disparate as endocrinology, neurosciences and the like on one side, and pharmacological therapeutics, surgery and so on on the other. The simple taxonomy that places one in the category of medical science and the other in clinical medicine breaks down when one reflects on the profound interactions between the domains, notwithstanding significant differences in the methodologies (clinical research has an emphasis on randomized controlled trials while medical sciences seldom use them), employment of theories and models, among other differences. Moreover, medicine encompasses the body and the mind; it has sociological, social work, psychological, political, economic and legal dimensions, with the first three being firmly embedded in the practice of medicine. None of this is surprising because the focus of medicine is well-being in all its dimensions. To complicate things more, the study of laughter also merges multiple
disciplines and is the focus of none in particular.

The inquiry is also hindered by the fact that neither philosophy of medicine nor philosophy of science has paid much attention to laughter in medicine. So unlike a standard discipline, such as philosophy of biology, the present subject comes without a received map to direct the inquiry. The following discussion maps this unusual subject in the context of philosophy of medicine.

1.4 Philosophy of Medicine

An often-cited exchange began with the bold claim by Arthur Caplan (1992) that philosophy of medicine does not exist. He argued that philosophy of medicine is not well integrated with other inquiries and disciplines and does not have an established literature or a set of distinctive and defining problems. In reply, Velanovich (1994) did not disagree with Caplan’s criteria but argued that philosophy of medicine is a developing field with the potential to meet those criteria. Wulff (1992) addressed the question not by dwelling on the literature but by examining the participants, for instance whether they are medical professionals or professional philosophers. He took the view that philosophy of medicine is a medical subdiscipline even if not a philosophical one. Pellegrino (1998) broke down the problem into components, distinguishing medicine from science and philosophy of medicine from philosophy of science. He also defined several philosophies: philosophy of medicine, philosophy and medicine, philosophy in medicine and medical philosophy. He argued that philosophy of medicine does exist as a unique field of study. More recently, Stempsey (2008) argued for a broad conception of philosophy of medicine as what philosophers of medicine do. He surveyed
625 articles in two leading journals focused on philosophy of medicine as well as 36 books of essays and monographs in a series called *Philosophy and Medicine*. Although he found that the majority were about bioethics, in his view, philosophy of medicine is not limited to bioethics or philosophy of science. He says its components, philosophy and medicine, cannot be precisely defined so it too cannot be precisely defined. Rather, he quotes the well-known judicial pronouncement about pornography that, "I know it when I see it." Stempsey suggests that we recognize as philosophy of medicine any work that illuminates the theory and practice of medicine.

In the same year that Caplan initiated debate over the existence of philosophy of medicine, an influential article credited to 25 authors describing themselves as the Evidence-based Medicine Working Group proposed what has become the orthodox view of medical evidence:

A NEW paradigm for medical practice is emerging. Evidence-based medicine de-emphasizes intuition, unsystematic clinical experience, and pathophysiologic rationale as sufficient grounds for clinical decision making and stresses the examination of evidence from clinical research. Evidence-based medicine requires new skills of the physician, including efficient literature searching and the application of formal rules of evidence evaluating the clinical literature. (Evidence-based Medicine Working Group 1992, 2421).

Evidence-based medicine has also been distilled to this frequently-cited definition: "the conscientious, explicit, and judicious use of current best evidence in making decisions about the care of individual patients" (Sackett et al. 1996). Many other definition have also been proposed (Upshur 2005).

The Evidence-based Medicine Working Group illustrated their proposal with a scenario contrasting the difference between the outdated practices before evidence-based medicine and their proposed future practices. Facing a clinical problem, a junior medical resident in a teaching hospital of the past would have relied on the authority of the senior resident supported by the
attending physician. Their unsystematic observations from clinical experience would have been the basis for diagnosis, prognosis and assessing the efficacy of treatment. They would also have relied on their understanding of basic mechanisms of disease and pathophysiological principles. Hence traditional medical training would have encouraged the resident to rely on the past along with plain old common sense to evaluate procedures and treatments. Expertise and clinical experience, not systematic review of published literature, were the basis of clinical practice. Using evidence-based medicine, the resident would be wary of tradition, clinical experience and intuition. Reasoning from basic disease principles would also be considered untrustworthy. Guided by specific rules of evidence, the resident would instead go to the library, search the medical literature and solve the problem based on review of published research (but see Nunn 2008 for critical analysis of the literature search). Such a scenario has been clarified over the years by proponents of evidence-based medicine, for instance to say that the resident should not entirely reject reasoning from basic principles, but it nevertheless encapsulates the tenets of evidence-based medicine (Worrall 2007).

The basic procedures of evidence-based medicine can be listed in 5 words: question, search, appraisal, application, evaluation. That is, clinicians are exhorted to ask an answerable question, search for the best evidence, critically appraise the evidence, apply the evidence in the particular circumstances and evaluate the results. A second-order agenda in evidence-based medicine is to standardize medicine in guidelines, checklists and summaries (such as the above five-step list). Similarly each aspect of the evidence-based program has been assigned its own checklists. For instance a critical appraisal checklist might ask whether the reported experimental results are valid, if so what are the particular results, then how can they be
The notion of evidence has also been reduced in evidence-based medicine to a list of evidence types. Although dozens of hierarchies have been proposed and other approaches have been offered for grading evidence, nevertheless randomized controlled trials are invariably among the most highly prized evidence (see Upshur 2003 and Worrall 2007 for hierarchies; for grading evidence see www.gradeworkinggroup.org; see also Worrall 2002, Upshur 2005 and Cartwright 2007 for critiques of randomization, external validity and other aspects of the "evidence-base"). Experience and expertise are low down in the untrustworthy zone together with unsystematic clinical observations and anecdotes.

Despite the apparently uncontroversial notion of basing medicine on evidence, the evidence-based medicine program has generated an enormous and fractious literature. Even among proponents of evidence-based medicine there is disagreement concerning the order in which the various standardized lists have been set out, and indeed with the very reduction of medicine to lists and statistics. For instance, two leading advocates of evidence-based medicine, Jadad and Enkin (2007, 106), temper their enthusiasm for randomized controlled trials in these strong terms, "We believe that the still present tendency to place RCTs at the top of the evidence hierarchy is fundamentally wrong. Indeed, we consider the very concept of a hierarchy of evidence to be misguided and superficial. There is no 'best evidence', except in reference to particular types of problems, in particular contexts." In a more sweeping attack, Cassell questions the whole program of basing medicine on the statistics and guidelines of evidence-based medicine. He writes,

> Medicine is still talking about how every patient is different (as it has for millennia) but searching for sameness, invariance, and basing its clinical practice on that idea. Witness evidence-based medicine and practice guidelines, both of which have achieved contemporary popularity by
treating all patients as though each person with the same disorder is the same (2004, 253).

Still evidence-based medicine, according to Caplan’s criteria, is the preeminent philosophy of medicine today, with its own literature and professionals focused on specific problems acknowledged within the research community. Indeed, long after postulating the non-existence of philosophy of medicine, which he did before the rise of evidence-based medicine, Caplan wrote,

it is hard, or rather, completely impossible, to avoid the phrase ‘evidence-based medicine’ in the august halls of academic medical centers in the United States, Europe, Australia, and New Zealand these days. There are journals, resource centers, toolkits, web sites, and more publications than anyone interested in evidence-based medicine could possibly ever read” (2006).

In the same article, Caplan called on medical schools and academic research centers to make the philosophy of medicine a part of the culture of academic medicine.

Evidence in medicine includes anecdote, hearsay, direct personal experience, expert authority, folk belief, randomized controlled trial, systematic review, meta-analyses and reviews of reviews. A first-order question, as in any empirical inquiry, is what evidence connects laughter, medicine and health. Higher order questions are also important, including what is deemed to be evidence in medicine.

Evidence is not sufficient to end a debate or make a decision. Evidence must be interpreted, weighed and connected to a body of knowledge in order to be applied. Moreover, a burden of proof, explicit or implicit, must be met when a decision turns on evidence. Supporters of laughter as medicine put the burden of proof on those who say it doesn’t work while skeptics say there must be evidence that it does work. In the realm of laughter and health, as in much of medicine, supporters frequently give weight to individuals and
anecdotes whereas skeptics demand theory and experiment. So before assuming that the answers to questions about laughter in medicine are to be found in experimental science, we must address the believers’ claim that controlled experiments are not the only evidence. For the skeptics, in particular the proponents of evidence-based medicine, anecdotes are not admissible evidence in medicine, or if admitted they are to be given minimal weight.

    Medicine, however, is only one discipline among many that rely on evidence and proof. Why not admit evidence that is highly prized in other disciplines such as journalism, history, philosophy or law? A simple answer is that these other disciplines are not primarily scientific. If medicine is a science, then medical evidence must be derived by the accepted methodologies of medical science. Statistics and repeated controlled experimental trials matter but individual stories do not. If medicine is an art, a broader range opens up to include anecdote, folklore and even, in the case of laughter, nonsense and absurdity. If medicine is a craft, then the focus might simply be on techniques that work, or seem to work. In turn, answers to questions such as whether laughter is the best medicine, good medicine, bad medicine or any medicine at all will change depending on how medicine is construed. As already noted, there is no consensus, although common options include claims that medicine is science, art, craft, technology, magic or all of these (or none of these, with medicine being sui generis).

    To select only a few examples from a vast literature, Inlow (1946) says medicine is a science, art, professional practice and social relationship, a whole in its own right; Dixon (1978) says medicine, offering both comfort and cure, is not synonymous with science; Pellegrino and Thomasma (1981) say medicine is science, art and virtue; Cassell (2004) says medicine is neither art nor science; Solomon (2008) resolves the matter by refusing to cut the subject
in half, arguing instead that the dichotomy between art and science of medicine is not helpful; Montgomery (2006) says physicians pretend to be scientists while working quite differently; Bausell (2007) says medicine is applied science; in Delkeskamp-Hayes and Cutter (1993) seventeen authors consider, without reaching a consensus, whether medicine is science, technology or art.

Today the standard view is that the epistemic authority of medicine is based on its use of science that distinguishes it from quackery. But the authority of medicine also flows from other factors, including the physician’s own authority and social status, the societal control of the practice of medicine, which privileges physicians and restricts others, the economic organization of health care, the power wielded by experts (those presumed to have knowledge) and the success of medicine. Moreover if medicine is a science, it is not clear what the governing theory is; it seems not to have one.

Indeed, perhaps questions about medicine as science should be inverted. Traditionally, physics has been deemed to be the most basic science, a unique species of inquiry and the model for all activities that aspire to be called science. Instead of asking is medicine a science, perhaps the question should be whether other sciences should emulate medicine. That is, instead of conceiving of physics as the quintessential science, perhaps medicine is a better model (Schafer 1993). Still, conceiving of medicine as a unique species, and physic not physics as the model species, does not tell us what that species is.

Medical practice provides many clues about the species. Physicians treat individual patients. Science, by contrast, concerns aggregates, populations and general scientific laws. The contrast can be expressed in many terms including:

a. the ideographic, individual, contingent, subjective, narrative,
particular, exceptional, idiosyncratic and private

b. the nomothetic, general, cohort, class, population, scientific, objective, law-like, and public

The individual nature of the medical patient defies the notion of a general medical nature or medical law. Indeed there is much philosophical controversy over scientific laws, including the fundamental question whether there are any laws, or just observed regularities.

Consider a general mathematical relation involving the definition of multiplication:

\[ 2n = n + n \]

Individual examples include \( n = 1 \), so \( 2n = 1 + 1 \); or \( n = 17 \) so \( 2n = 17 + 17 \). Every individual number multiplied by 2 always equals the addition of the number to itself. But scientific laws are not like this; they do not originate from axioms. Scientific laws incorporate individual observations. Individual observations can never conclusively prove general laws. There's always a possible exception. Medicine does rely on generalizations such as: H1N1 causes flu. But there is no law that says the H1N1 virus must cause flu in a particular individual or that the same virus cannot have effects other than those of influenza in some person. There is no law that says the presence of a virus always does this or that.

According to Pellegrino and Thomasma, the "epistemological problem central to medicine" is "how theoretical knowledge can be applied to concrete, individual body-persons with therapeutic results" (1981, 84). In Pellegrino's words,

Medicine has been called a “science of particular.” This is a useful, but not a precise, description. It is useful in placing emphasis on the necessity of taking into account the existential particularities of illness and the requirements for curing, caring, or healing in particular patients. But medicine-qua-medicine is also interested in, and capable of, generalized principles, that is to say, of a theory of medicine. The sciences, for their
part, are also deeply concerned with particularities. It is out of particular instances and specified experimental or observational conditions that general scientific laws are derived by induction or applied by deduction. In its practice, science draws validity from the richness and reliability of its particulars. Solution chemistry must deal with particulars – temperature, ionic strength, ionization constants, ionic sizes, hydration envelopes, equilibrium conditions, etc. It is as much a science of particulars as medicine. But the theory of solutions must abstract from these particulars to a general set of laws governing all types of solution (1998, 330).

Pellegrino is staking a claim that there is a philosophy of medicine with general theories. His comparison of medicine with solution chemistry does not take us very far in that direction, however. His exposition is only a restatement of the problem. The very problem is whether solution chemistry is "as much a science of particulars as medicine." Yet that problem is also Pellegrino’s conclusion.

Pellegrino uses the notion of particulars loosely, to apply both to details and to individuals. The particulars of a chemical solution are not individuals although they are details. Ions and their sizes are details, not individuals.

Chemistry is more like physics in having invariant not just probabilistic processes. Even the gas laws that are stochastic processes at the level of individual molecules have invariant regularities at the level of a gas. An individual particle may be emitted from uranium 235 at random, yet physicists can say collective things about radioactive substances. In medicine, groups can be viewed using a probability distribution over a population, for instance the probability that an experimental group remains healthy, with the healthy ones in the center and those not so fortunate at the tails of the distribution. But what factors put those individuals in the tails of the distribution?

Gorovitz, whom Pellegrino cites for his source of "science of particular" as a "useful, but not precise, description" actually has a more nuanced analysis
of particulars. Gorovitz's main project concerns medical error. He analyses particulars to make the case for recognizing the fallibility of the individual physician. He too refers to solution chemistry in the form of molecules and ice cubes. As if anticipating Pellegrino, Gorovitz says of molecules and ice cubes that, "The basic mistake made by that interpretation of science which considers that all genuine scientific knowledge is of universals is to suppose that all particulars are of this kind. But this is clearly false. Many particulars--salt marshes, hurricanes and the higher primates, for example--cannot be understood solely as the sum-total of the physical and chemical mechanisms that operate on them" (1975, 16). He addresses the above issue of the uncertain effects of a virus by considering the example of smallpox vaccination in one individual. "Of course, the effect of the vaccination on him will be determined by natural laws, his condition, and perhaps the way he interacts with his environment subsequent to vaccination. But we do not and cannot know all the relevant laws and conditions, thus our knowledge of this individual is limited and our predictive ability is constrained" (1975, 17). Gorovitz argues for a revised science that allows a place for particulars along with generalizations.

Scientific studies report results, such as the number of experimental subjects who got better or worse. By contrast, physicians and patients experience much more than outcomes. To return to the mathematical theme, mathematics and the science based on it are not self-aware; a number has no experience, no history, no subject. In physics and chemistry it could be argued that particles and molecules have a history if not an experience or subject, but that history does not matter to the particle and may not even be available to the observer. In the background of every medical encounter is the life story of the patient, the life story of the physician and the particular story of the suffering that brought the patient and physician together. For some
physicians, the numbers and the outcomes are all that matter to the efficacy and efficiency of medicine. The patient, however, may actually be less concerned about the outcome than about the process and the treatment. But that concern is not generally reported in scientific experiments.

In medicine, ordinary knowledge and science are entwined like the snakes of the caduceus. Pellegrino and Thomasma point out that medicine always includes non-scientific ordinary knowledge:

Theory in medicine is theory about the world of everyday reality, about fundamental human values played out in daily life. Thus, in medicine, reflective reasoning operates along with ordinary, unreflective knowledge (1981, 53).

Within the realm of laughter and medicine, important fundamental notions remain as ordinary knowledge, having yet to succumb to the rigors of science. For instance according to Ruch and Kohler (1998, 203), "We take the position that the 'sense of humor' is still more of a folk-concept and has not been explicitly converted into a scientific construct so far..." Given that the raw frequency or some other measurable quality of laughter in an individual’s life is difficult to assess, the sense of humor, often measured on a sense of humor scale, is used on the assumption that there is a connection between sense of humor and some action that influences health, such as frequency of laughter. But proxies introduce another set of conceptual problems. Say instead of laughter and health that we were inquiring into exercise and health. To see if exercise is healthy, we do not use a proxy called sense of exercise. Similarly for sleep-health, diet-health and so on we do not commonly use sense of sleep or sense of diet. The higher order notion of a sense of something, such as a sense of humor, compounds the difficulty of an already difficult concept. This is not to say that humor and sense of humor have no value when inquiring into laughter and health. They just do not simplify or clarify. Simplifying and clarifying are not always the highest goals, of course. But when experiments
and theories based on a sense of something produce inconclusive results, it’s worth asking whether the basics are sufficiently clear and simple.

No clear line can be drawn between folk and expert knowledge in medicine (see for instance Santino 1985, Shaw 2002, Mcclean and Shaw 2005). To the extent that patients always need magic, folk views will remain at the forefront in the physician-patient relationship (Buckman and Sabbagh 1993, 248). When considering folklore, the usual assumption is that the patient is the one with the folk knowledge, not the physician. The expert’s knowledge is limited, however. Not all beliefs of physicians are scientific. For instance, many physicians share spiritual beliefs with their patients. Similarly the notion of placebo links patient and physician in a common belief system that neither participant fully understands. In the specific case of laughter and health, folk and scientific views merged with the publication of the first article by a layman in 50 years in the New England Journal of Medicine (Cousins 1976, Santino 1985).

According to Harrington (2008), patients want a meaningful scientific story that includes mind and personality. She divides mind-body medicine into six narratives (categories or templates of stories). Laughter and health is situated in a narrative she calls the power of positive thinking. Greenhalgh (1999) further tries to reconcile narrative with evidence-based medicine. Narrative in medicine also raises the broader theme of reconciling analytic and empirical traditions with hermeneutic and phenomenological traditions. For instance, Daniel (1986) considers the patient to be like a text that can be interpreted using techniques derived from biblical and literary criticism. Daniel’s hermeneutic model has been criticized as vague and confusing (Schwartz 1986). Still, approaches such as these raise important issues that extend beyond the strict confines of biomedicine, science, philosophy of science, philosophy of medicine or indeed any single viewpoint.
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2. Evidence-Based Laughter Medicine

Evidence-based medicine (EBM), as we saw in the previous chapter, promotes the notion of codified levels of evidence in the determination of efficacy and cause-effect relationships and has become the prevailing standard for determining what works and what to do in clinical medicine. This chapter begins with the general results of efforts to study laughter experimentally, then analyzes the specific methods used in laughter experiments. From examining the weak and inconclusive overall results, together with specific methodological issues that make laughter experiments so challenging, it becomes apparent that squeezing laughter into an evidence-based checklist or guideline has yet to be achieved and that EBM may not even be up to the task of answering the question whether laughter is good medicine.

2.1 Weak and Inconclusive Results

To see if laughter works in medicine according to the tenets of evidence-based medicine, we have to find persuasive statistics in randomized controlled trials and systematic reviews of them. Assuming without deciding that a randomized controlled trial is among the highest forms of medical evidence, the very notion of conducting controlled tests of laughter is problematic.
Randomized controlled trials are in essence very simple: treat two randomly selected groups differently and see what happens. But laughter, unlike the drugs that are commonly subject to experiment, is not so simple. Someone taking a drug won’t know the exact ingredients since only the external form of delivery (the pill, capsule, ointment or liquid) is apparent. Consequently a drug treatment can be concealed and thus compared with an inactive ingredient or a different active drug that merely appears to be the same. Laughter is not delivered in a container that can hide an apparently non-laughter equivalent. Although many laughter experiments attempt to use a control group that does not laugh, the reflexivity of awareness of one’s own laughter can never be eliminated, except perhaps among the occasional experimental subjects who are laughing in their sleep, and even then they could be dreaming that they’re laughing. Experiments in laughter share methodological problems with experiments in surgery, psychiatry, acupuncture and other interventions that are apparent to the patient. Moreover, laughter, unlike say surgery, can break out at any moment and for reasons that are difficult to explain. It is difficult to conceive of a meaningful control group in which some participants don’t laugh or smile or think silently and subversively of something amusingly absurd, especially if they are instructed to be serious, or in which participants do not know that they are not laughing.

Although there is a growing body of experimental research (recent surveys include Bennett and Lengacher 2006 and 2007, Martin 2001, 2004 and 2007 and McCreadie and Wiggins 2008), controlled studies of laughter in the context of medicine and health are rare. Bennett and Lengacher (2006) reviewed the literature and found that "there are a very limited number of studies that document the effects of laughter on physiological outcomes, and no controlled studies have been identified that document the effects of
laughter on clinical health outcomes." Their four-part review of the literature whispers the tentative title, *Humor and Laughter May Influence Health*. This is the most that the current state of the art permits anyone to declare. The concluding words of the final paper in the series succinctly summarize the present situation:

Research results concerning humor and healing are thus far rather tentative, and more work is needed before broad claims can be made concerning an effect of humor upon health outcomes. While in a frequently cited case study, Cousins (34) attributed his cure from ankylosing spondylitis to complementary therapies including laughter, empirical research supporting this type of response is currently not available. In fact, as can be seen from the above review, documentation of the effects of humor on various health related outcomes in healthy populations is still in the infancy stages, and research documenting benefits in a clinical population (such as persons with cancer) is yet to be established (Bennett and Lengacher 2007).

An earlier literature review by Martin (2004) reached similar conclusions:

Despite the popular belief in health benefits of humor and laughter, and the proliferation of therapeutic interventions based on these ideas, the empirical evidence for these claims to date is actually quite weak, inconsistent, and inconclusive (for reviews of this research see Martin 2001; 2002). Due in large part to a lack of adequate funding for such research, most of the studies have been small scale, with inadequate control groups and other methodological weaknesses, making it difficult to draw firm conclusions one way or the other. In general, although the research has not provided convincing evidence that humor and laughter have beneficial effects on health, it also has not demonstrated that they do not (Martin 2004, 2).

The conclusions from medical investigations of laughter resemble those from research into many unconventional medical interventions. For instance, Bardia et al. (2006, 5457) systematically reviewed cancer pain therapies and concluded that, "Hypnosis, imagery, support groups, acupuncture, and healing touch seem promising, particularly in the short term, but none can be recommended because of a paucity of rigorous trials." Sloan et al. (1999, 667) reviewed studies of religion and health and concluded that "Even in the best
studies, the evidence of an association between religion, spirituality, and health is weak and inconsistent.” In a similar vein, a literature review recently concluded that positive affect is not unequivocally associated with health, and even explored the possibility that too much positive affect can be unhealthy (Pressman and Cohen 2005).

Laughter researchers clearly accept that something called laughter and something called health exists and that a causal relationship between them can be studied. They go further and refer to a combined entity called "humor and laughter" that might affect health. These conjunctions of very different things bring to mind other related amorphous entities like the economy and the weather, as in, The Economy and the Weather May Influence Health. No doubt somebody is researching that possibility at this very moment and calling for more research to firm up weak and inconclusive results. Or closer to our laughter theme, we could broaden the topic to include all kinds of positive attitudes, and throw in smiling just for fun, to get the topic, Smiling and A Positive Attitude May Influence Health. Or to encourage writers of documents such as this one, perhaps the title could be, Thinking and Writing about Laughter May Influence Health. If the literature on these research programs were also found to be weak and inconclusive, it would come as no surprise. Many great minds have considered laughter over the centuries. Why are their results not stronger? Maybe we should be asking whether there are limits to the study of laughter.

A fundamental question is whether general answers about health and laughter can be tickled out of experiments, randomized, controlled or otherwise. There is a longstanding debate about whether results of experiments apply beyond the laboratory (see for instance Anderson et al. 1995). Difficulties inherent in laughter experiments are illustrated in the following examples.
2.2 Laughter Experiments

Mahony et al. (2001) designed an experiment to control expectation of benefit from laughter. Not only does this experiment illustrate the general difficulties of pinning down laughter, but it also goes some way toward addressing the question whether laughter acts as a placebo. Associations between laughter and placebo are more fully addressed in the chapter that examines that question in detail.

In the experiment, undergraduate students, 50 male and 84 female, were randomly assigned to watch either a funny or a relaxing video. Those two groups were each further divided into two sub-groups, one told to expect an increase, the other a decrease, in their "discomfort threshold". A fifth group watched the funny video without being told what to expect. Participants completed a sense-of-humor questionnaire. An initial discomfort level was measured using a blood pressure cuff on their preferred arm to induce discomfort (not to measure blood pressure). The pressure was recorded when the participant felt "any discomfort at all". Participants read a paragraph of instructions that set their expectations for an increase or decrease in discomfort threshold. They watched the videos and rated them on a seven-point scale for eleven qualities such as boring, pleasant, funny and disturbing. Those qualities were later boiled down to three factors, amusement, pleasantness and sadness. In the result, expectations related to humor and relaxation were not distinguishable from each other or from placebo responses. But participants told to expect less discomfort while watching a video indeed reported less discomfort. The authors conclude, "It is possible that the beneficial effects of laughter, although not quantitatively
superior to the effects of relaxation, distraction, or expectation, are qualitatively superior in some way. If so, then considerable ingenuity will be required to identify and isolate such outcomes” (2001, 225).

Considerable ingenuity indeed, if it is even feasible, given the many problems in conducting such experiments. For instance, the experimenters set out to isolate the effects of laughter, not humor, fun, amusement, tittilation or other related notions: "The purpose of this study is to control expectation of benefit from laughter" (Mahony et al. 2001, 217). Yet there is no Laughter group in the statistical analysis. Laughter is reported in the Amusement group with a comment, "each of these participants was heard to laugh at least once" (a vocal measure, not visual such as facial movements). Each audible laugh might have been from an amusing gag in the video or a remembered joke from last night or because of the absurdity of finding oneself watching videos in an experiment or from the enjoyment of being relieved for the moment from attending psychology classes. Mixing laughter and amusement implicitly assumes that benefits of both follow from general, emotional, mood lifting mechanisms, not something specific to laughter such as increased respiration. Amid the apparent scientific rigor of statistics, with symbols of mathematical precision such as p< .001, it is difficult to see where a criterion about hearing at least one laugh fits. In one sense it could be considered to be single data point, while in another sense it could be considered to be anecdotal evidence (Larry laughed but Sally snorted). It could also be evidence of increased respiration during the laugh that supports a model of laughter-specific mechanisms, as distinct from amusement-specific mechanisms that provide emotional benefit without increased respiration or other aspects of laughter. It is not clear how variations and aggregates can be compared, for instance whether hearing a laugh at least once is better evidence than seeing smiles at least twice.
Nor is it clear how often someone must laugh or smile and during what time interval in order to have an influence on the outcome; each experiment uses its own criteria. A common view is that measurement is essential for scientific knowledge. Lord Kelvin famously remarked that, “when you cannot measure it, when you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind” (Merton et al. 1984). Applying the simplest act of measurement to laughter, however, such as counting laughs, involves considerable judgment. The need for judgment is pervasive in science but in the case of laughter there are no standards and few published examples with which to compare research reports. In the words of linguist Wallace Chafe, "There is no wholly satisfactory way to count laughs. Provine wrote of 1200 'instances' of laughter in his data, but what counts as a single laugh is not entirely clear" (Chafe 2007, 7).

Since even the most basic measures are in dispute, derivatives from these quantities, such as the health effects of increased "instances" of laughter, are also pliable. For instance, a more recent study tested whether children in a hospital were happier during music rather than play therapy. Happiness was defined as the frequency of smiles during a three-minute period (Hendon and Bohon 2008). At least that is a specific measure, but it is not clear what three minutes represents outside the experiment. These scientific experiments demand that something be measured and counted, but the perennial problem of construct validity and the meaning of the results remains unsolved (Sechrest 2005).

In this study, variations in measured sense of humor did not make a difference to the results. The video could have been so funny that everybody found it funny, including those without much sense of humor, as the authors suggest. Or it could challenge the metaphysical notion of a tangible humor sense that can be measured. Yet there is rarely a hint in the experimental
literature of the historical development or even the fluidity of the notions of sense, humor and laughter that make up the so-called sense of humor. The following description of the shifting notion of sense of humor points out that it has not always been considered a timeless personality trait:

The term "sense of humor" retains within it a set of changes that point to the development of a new concept of personhood, of a notion of the individual which yokes together an extreme particularity of self-hood and a hyper-abstraction of "everyman" and "everyday life." First, the meaning of humor itself moved from a physiological, to a characterological, and finally to a discursive referent-- in short, from an objective to a subjective meaning. Second, the understanding of laughter underwent a revolution by which the site of laughter production was relocated from the nature of the object to the activities of the mind, while at the same time humor was becoming newly associated with laughter. Third, an elevation of particular senses with the capacity to judge the particular in an immediate and unmediated fashion fed into a general middle-class culture of sensibility, creating a proliferation of internal senses. The convergence of these three long-term changes in the middle of the nineteenth century gave birth to the sense of humor. When we use the phrase today, the accretion of all these changes is present. (Wickberg (1998, 73)

Martin (2007, 225) offers the following response to the question what exactly is a sense of humor as the phrase is used today:

... this concept has taken on many positive connotations over the years, while becoming increasingly vague and ill-defined. The research reviewed in this chapter suggests that sense of humor is not a unitary construct. Instead, it can be conceptualized and measured in a number of different ways, each focusing on different aspects of humor. Furthermore, these different ways of defining it are not necessarily highly correlated with one another, and they relate in quite different ways to other personality traits.

The vague and ill-defined sense of humor is experimentally correlated with pain and discomfort. Just as sense of humor is difficult to pin down, so are pain and discomfort. It is difficult to say how discomfort from a blood pressure cuff on the preferred arm relates to pain. The expression "pain and discomfort" is often used in ordinary speech, as it is in the published report of this experiment, but making the conjunction meaningful in a rigorous
experimental context is another matter. There may be little connection between pain, discomfort, relaxation and laughter in a laboratory compared with pain in ordinary life (see for instance Spiro’s (1986, 63) criticism of laboratory studies of pain generated by a known cause that cannot simulate the anxiety over chronic pain or pain from unknown causes). Discomfort from a blood pressure cuff is a different experience from frightening pain of unknown cause. Participants picked their preferred arm to be cuffed. In ordinary life, there is rarely a choice of the location of pain or discomfort.

To the extent that pain and placebos are about personal control, choice could be a significant confounding factor. The relationship between personal control and placebo will be further explored in a later chapter that focuses on placebo, but it is important here to point out the significant relationship between pain and control. Pain that stops when the blood pressure cuff is released is different from continuing or mysterious intermittent pain or pain accompanied by spurting blood. Compare a frightening, unknown source of constant pain with this obvious controlled source that can be turned off at the first sign of discomfort. They are clearly different sources and likely to lead to different outcomes.

The participants were tested individually. This restriction may benefit the experimental setup and analysis but may be far removed from their daily lives. Pain may be felt individually but laughter is commonly a social activity. Indeed more laughter breaks out during ordinary conversation than in the restricted confines of formal jokes and humor (Provine 2000).

Then there is the notion of expectation: is the expectation in this experiment, having been set by reading a paragraph, comparable to expectation from everyday life and previous experience with pain and discomfort? Participants were asked whether they found the instructions credible. Two participants who did not were excluded from the reported
results. How believable the instructions were and whether the participants would reliably report their beliefs could have been a study in itself. Possibly the sight of a blood pressure cuff was a reminder of medical interventions. In addition to expectation of pain and discomfort there could be expectations associated with the experimental setup. Possibly the blood pressure cuff was confusing to the participants since its expected use is to measure blood pressure not to serve as a proxy for pain. The popular belief that laughter is healthy might also be influential in setting the particular expectation in this experiment. Compare two hypothetical participants: the first, who uncritically accepts that laughter is the best medicine, and the second, a skeptical undergraduate who thinks that the notion of watching episodes of a television sitcom has little to do with pain or relaxation.

Another experiment that further illustrates these issues was conducted by Zweyer et al. (2004). They similarly add to the growing line of research that uses funny videos to test pain tolerance. Whereas Mahony et al. (2001) used a blood pressure cuff, this study used a cold pressor test. Participants submerged a hand in warm water for five minutes then in water at the freezing point. The resulting feeling was described in the report as a measure of pain, not as discomfort. Other differences from the Mahony et al. (2001) study include: coding of facial expressions, so-called genuine (Duchenne) expressions were distinguished from fake expressions, and laughter was measured explicitly (on a 5-point scale). Participants were divided into 3 groups representing 3 factors described as "cheerfulness (as a mood), exhilaration (as affect) and humor production (as involving more cognitive elements in enjoyment)." All participants watched the same film. No control groups were used as the researchers only wanted to study humor and assumed from other studies that humor was a superior analgesic to other types of films or no film. Participants were instructed differently about how
to react. All groups were instructed that they would see a funny film. The Cheerfulness group was instructed to "feel cheerful, but without laughing or smiling as you do so." The Exhilaration group was told to "try to laugh extensively." The Humor production group was instructed to "try to give funny comments to the film loudly." It is noteworthy that 20 of the 76 paid female subjects did not report pain or did not follow these odd instructions. Contrary to many inspirational stories about the benefits of deliberate daily laughter, only Duchenne smiles, not laughter, were found to be correlated with pain reduction. Indeed, forced laughter increased the pain. The researchers concluded from their study and previous research that, "we can state with some confidence that this phenomenon of humor-induced pain tolerance is real" and they recommend future studies to detail the underlying mechanisms (2004, 115).

Again many questions arise. As in the previous study, a principal concern is what they were measuring. Tolerance of cold water depends on beliefs about the consequences of tolerating cold water (Quattrone and Tversky 1984). Some subjects might believe that good things come from putting up with the cold and such beliefs might be associated with instructions about being cheerful. Repetition also influences beliefs about painful cold water. In this experiment, the cold pressor test was used to measure pain threshold and tolerance several times: much before watching the film, immediately before, immediately after and twenty minutes after. In non-experimental activities commonly associated with freezing water, such as ice skating, fishing, sailing, winter survival courses or ditching an aircraft in the ocean, the sheer experience of freezing water makes the survivor's next experience less frightening and painful (this is the basis for a whole industry of safety and survival training). Is it reasonable to assume that such confounding effects were evenly distributed among the 56 subjects who were willing to
follow the instructions? What about the others? To explain the use of a personality questionnaire, participants were told that personality could influence pain perception, but there was no accounting for the consequence of setting the expectation that personality could have an effect. The researchers recognize some of these problems when they ask, "Could it be that greater pain tolerance somehow leads to greater enjoyment of humor, as shown in facial expressions (especially since the difference was already present at pretest, even before humor stimuli were presented)? Or maybe there is a third variable, perhaps having to do with temperament at a physiological level, that influences both pain tolerance and cheerfulness/amusement. We need further studies to answer these questions" (2004, 112).

There was no discussion of cultural implications of these test for the German female subjects compared to other cultures or to males. Half of the subjects had previously seen the film, a Mr. Bean comedy, with sound effects but no dialog, but there was no discussion of the implications of watching something twice. Was it equally funny the second time? When they answered questions about how funny it was, would those who had seen it previously be able to separate how funny they thought it was the first time from how funny it was during the experiment? The Zweyer study used *Mr. Bean at the Dentist* while the Mahony study used the Seinfeld television sitcom episode *The Soup Nazi*. It is unlikely that the content had no bearing on participants' feelings. Could it be that someone willing to watch the same Mr. Bean performance more than once is by definition someone who can tolerate pain?

Recently children have become a focus of laughter research. Stuber et al. (2007) conducted a small feasibility study of laughter and pain in 12 boys and 6 girls ages 7 to 16. A preliminary phase screened 37 children to determine what comedy shows and movies made them laugh. The experiment consisted of administering pain before, during and after watching one of these videos.
The measure of pain was how long they could hold a hand in cold water. The number of laughs made no significant difference to the result, and there was no effect on how much pain the kids reported, but they did leave the hand in the water longer while watching the video. The report concludes that, "Clinically, the results of this study support the ongoing efforts to provide humorous distraction for children undergoing painful procedures." This optimistic conclusion is hardly justified by the experiment. The authors point out that studies of adults have used funny, sad or frightening videos to demonstrate that distraction, not just laughter, increases pain tolerance. But they note that experimenting with sad or frightening videos may not be allowed with children and in any event would have no clinical use.

Another experiment tests the plausible hypothesis that laughter reduces anxiety. Using 53 undergraduates, Yovetich et al. (1990) increased their anxiety by threatening to shock them. They were instructed, "On the screen before you, you will see slides counting down from 12 min. to 0 sec. in 10-sec intervals. Every 30 sec. you are to dial how much stress you are experiencing (or how nervous you are) on a scale of 1 to 10 (1, very little; 10, extreme amount) on this dial-a-stress machine. At the end of 12 min. you will be receiving a strong shock" (1990, 53). While waiting for the anticipated electric shock, some students listened to comedy, some listened to a discussion of volcanoes and other geological phenomena and a third group had no audio distraction. There was no mention in the report of stress from trying to listen as well as do something else every 30 seconds. Heart rate and smiling (as zygomatic muscle tension) were monitored along with self-reported anxiety. The students were also rated according to their sense of humor. But the effects on heart rate were not consistent for high sense of humor scores or for humorous distraction. Yovetich et al. (1990) cite previous research showing that laughter initially increases heart rate, followed by relaxation and
decrease, and comment that pulse rate is a "complex dependent variable."
Bennett and Lengacher (2007) repeat this theme in their comments on the experiment, saying "laughter tends to increase heart and respiratory rate in itself, and therefore heart-rate may not have been the best physiological measure of reduced anxiety for this experiment." What's more fundamental for the current discussion, however, is not the empirical relationship between laughter and heart rate or respiration that is the focus for these researchers. It's that laughter is a change in respiration, heart rate and many other physiological parameters. There's a circularity in any hypothesis about whether laughter induces one of the features of laughter. A further obstacle for this kind of experiment is the nature of anxiety. Anxiety could be expressed as a momentary measure, "how much stress you are experiencing (or how nervous you are)". Or anxiety could be a fundamental state of mind (Wulff et al. 1990). Or is it a disease? That is, any connection between laughter, heart rate and anxiety, stress and nervousness takes us only a short distance toward a connection between laughter and medicine or health.

The final experiment considered here examines physiological responses to laughter that have often been compared to exercise. For instance, Cousins (1979, 145) famously claimed that laughter, "provides internal exercise for a person flat on his or her back-- a form of jogging for the innards." Can you lose weight by laughing and can you eat that second dessert and giggle the calories away? Buchowski et al. (2007) tested how much energy is expended during laughter in an airtight room calorimeter. The thorny question of what exactly laughter is was handled by a definition: "A laugh is characterized as a single laugh episode or utterance, usually involving one exhalation and/or inhalation." They found a statistically significant increase in energy expenditure and heart rate while participants laughed, up to a 20% difference, compared to resting values measured during a half hour of sitting
quietly in a reclining position. Yet the energy expenditure from laughter was
the same as other everyday sedentary activities such as light clerical work,
writing or playing cards. Real jogging, not just innard jogging, or even
household chores, according to this report, can increase energy expenditure
by 100% or more. So the moral of the story is not necessarily that laughter is
good exercise but that resting is no exercise, which has not generally been a
controversial hypothesis. Indeed, by definition resting is not exercising.
Measured in calories, Norman Cousins got just as much jogging for his
innards by writing about laughter as he did by laughing.

The aim here is not just critical analysis of these experiments. On the
contrary, the experimenters are to be commended for their ingenuity. The
aim here is rather to illustrate the difficulty of squeezing laughter into any
experimental project that fits into an evidence-based experiment-guideline-
checklist program. Having sifted laughter through the sieve of evidence-
based medicine, we still don't know if laughter works in medicine. Laughter
does not score high on any evidence-based hierarchy. Whether that is a blow
to the view that evidence-based medicine is the best medicine or to the view
that laughter is the best medicine remains to be seen. The objective of
evidence-based medicine is to generalize. Controlled methods are used to
infer statistically appropriate conclusions. Laughter, by contrast, may be
irreducibly idiosyncratic. Statistical slices are broad whereas individual slices
are deep. Evidence-based medicine has yet to provide evidence that it can
address the question whether laughter is the best medicine or any medicine.
Lacking answers that meet the standards of evidence-based medicine, we will
broaden our inquiry to include other views.
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3. Story-Based Laughter Medicine

Having found only mixed results from exploring laughter experiments in the previous chapter, this chapter turns to the other side of the divide between the sciences and the humanities. A similar turn has appeared in critical responses that propose a more narrative and qualitative approach to medicine than the controlled and statistical approach of evidence-based medicine. The following stories of prominent individuals lead to the conclusion that inquiring beyond the numbers into the narrative is essential if we are to answer questions about laughter in medicine.

3.1 Stories of Laughter and Survival

From a general perspective, laughter seems far removed from statistics, equations, science and technology. Indeed few things are more human, more easily understood, less technical and more obvious than laughter. For instance, in his widely quoted essay about his own cancer, the renowned paleontologist Stephen Jay Gould writes:

Attitude clearly matters in fighting cancer. We don’t know why (from my old-style materialistic perspective, I suspect that mental states feed back upon the immune system). But match people with the same cancer for age, class, health, socioeconomic status, and, in general, those with positive
attitudes, with a strong will and purpose for living, with commitment to struggle, with an active response to aiding their own treatment and not just a passive acceptance of anything doctors say, tend to live longer (Gould 1985).

On what basis does this scientist claim that positive attitude causes longer life than passive acceptance? Not on scientific research. Not on randomized controlled trials. He does not turn to the systematic reviews so highly valued by evidence-based medicine. Although he searched the literature on abdominal mesothelioma to learn that his disease was incurable with a median mortality of only eight months after discovery, he doesn’t say whether he searched the literature on positive attitude and longevity. Instead, he says he grounds his claim on an argument from authority, that of his "personal scientific guru" Sir Peter Medawar. Medawar in turn prescribes a "sanguine personality" for success against cancer, thus echoing back to the four humors of ancient medicine. Also Gould bases his claim on his background knowledge and what he calls his materialistic perspective. Both of these sources of knowledge are devalued in evidence-based medicine. But for Gould, the scientific literature was apparently more directly relevant to the disease than to the positive attitude to fight it.

Most curious in the context of laughter, medicine and health, however, is a statement near the end of Gould's essay after he says death is the enemy. He continues the martial metaphor with a striking pronouncement about humor: "The swords of battle are numerous, and none more effective than humor." Humor is apparently something we just know is healthy. More than healthy, humor is the most effective sword. How does he know? That he does not question or even suggest that there are conflicting views about the most effective swords vividly demonstrates the strength of his assumptions: there is no need to question or debate something so obvious.

Strong yet ungrounded endorsements of laughter and humor, such as
Gould’s "none more effective than humor" are frequent both in the expert and non-expert literature. Countless websites repeat similar statements until they appear to build a solid, unified wall of truth around humor and laughter as medicine. Patch Adams M.D., the physician clown, writes, "Humor has been strongly promoted as health-giving throughout medical history, from Hippocrates to Sir William Osler... I am astounded that anybody feels the need to prove something so obvious" (Adams 1993, 66). These endorsements are not limited to the latest revival of laughter physic. Over a century ago, laughter exercise was part of the American water cure craze (Whorton 1989, 43). Nor are enthusiastic endorsements limited to authors of self-help books or the obiter dicta of science writers. Informality also infuses professional accounts of laughter and health. For instance, in a short paper not peripherally about humor, laughter and health, but on those very subjects, Professor Sir Kenneth Calman (2001) writes,

I looked at more that [sic] eighty publications on humour in medicine for this study. Few gave clear indications as to its benefit, most being anecdotal. There were some negative aspects to humour, mainly in those with mental health problems. However, there are a couple of publications which suggest that the pain threshold is raised following laughter and thus patients require fewer analgesics. Perhaps there is a need for a laughter clinic in all hospitals.

Imagine that this had been the report of a popular recreational drug instead of the intervention called "humour". Let us call the drug "Happypill 2001" to recognize the date of Calman’s paper and substitute that drug for "humour":

I looked at more that [sic] eighty publications on Happypill 2001 in medicine for this study. Few gave clear indications as to its benefit, most being anecdotal. There were some negative aspects to Happypill 2001, mainly in those with mental health problems. However, there are a couple of publications which suggest that the pain threshold is raised following Happypill 2001 and thus patients require fewer analgesics. Perhaps there is a need for a Happypill 2001 clinic in all hospitals.

If Happypill 2001 were real, Calman’s paper probably would not have been
published. Nor would many of the mostly anecdotal reports he mentioned. The negative aspects would have merited a close examination. The switch in language between the first half and the last half of the paragraph would need explanation: the last two sentences refer to laughter whereas earlier sentences refer to humor, although any assumption that they are equal in effects or even closely related is questionable. Most laughter does not originate from humor (Provine 2000). After a real clinical trial of Happypill 2001, a concluding recommendation based on this report might quietly call for further study. Few researchers would stake their reputation on these findings and call for a clinic in all hospitals. Yet this is how the laughter bandwagon travels, loudly and informally and in the best of company, not only among popular boosters but also among medical experts.

A leading example of the laughter and health bandwagon and the elite company it keeps is the story of Norman Cousins. He has been so prominently linked to notions about laughter as medicine that his story merits detailed examination. Cousins was the influential editor of the Saturday Review who gained a self-created reputation for using laughter to cure himself of a debilitating illness. Although Cousins had no medical training, in 1976 the New England Journal of Medicine provided him with the rare and strange service of publishing his autobiographical story about a mysterious illness he suffered in 1964. Then in 1979 Cousins' bestselling book Anatomy of an Illness as Perceived by the Patient included an edited version of the article in the first chapter. The book is still popular and is often credited as a seminal work on laughter, medicine and health.

Cousins describes an experience at age 10 that convinced him of the power of the mind over disease. He does not elaborate on what he means by the mind or how it can have power over disease. Frail and underweight, he spent six months in a tuberculosis sanitarium, although later "it was discovered
that the doctors had mistakenly interpreted normal calcification as TB markings" (1979, 155). Wrong diagnosis and consequent mistrust of doctors becomes a recurring theme in Cousins' work.

He says the patients in the sanitarium divided into two groups, the confident optimists and the resigned "bleak brigade". The boys in his confident group "had a far higher percentage of 'discharged as cured' outcomes than the kids in the other group" (1979, 156). He doesn't explain how, as a 10 year-old, or for that matter as any patient, he could have worked out that percentage without access to medical records of the other boys. He makes no mention of the probability that the bleak boys had tuberculosis and so, unlike himself, didn't have normal calcification, were accurately diagnosed, were actually sick and had a basis for their bleakness. Still, "The lessons I learned about hope at that time played an important part in my complete recovery and in the feelings I have had about the preciousness of life" (1979, 156).

Cousins says he was misdiagnosed so it's never clear what he had or recovered from, and that is also the case for his later adult illness described in the New England Journal of Medicine. But this sketchy story from such an early age is the model for much of Cousin's medical writing. His stories proclaim the rugged American individualist dream: if you do it your own way and have strong enough hope, you will succeed against all odds.

As the storyteller he is, Cousins knows that magical things in stories happen in threes. He tells of a second "major episode" at age 39 when he was turned down for insurance. "The company doctors turned me down, saying the cardiograms showed evidence of a serious coronary occlusion" (1979, 156). He gets hearsay advice from the insurance agent, his aunt, to lie down: "...here was my aunt telling me that the insurance doctors said that if I became completely inactive, I might be able to stretch out my life for a year
and a half" (1979, 157). Characteristically his response was to play 45 or 50 games in a singles tennis tournament the next day.

A decade later, the third and main event he deems worth recounting, the illness that rocketed Cousins to medical stardom, was another probable misdiagnosis. He says it started with a slight fever. "The malaise, which took the form of a general feeling of achiness, rapidly deepened. Within a week it became difficult to move my neck, arms, hands, fingers, and legs. My sedimentation rate was over 80" (1979, 28). Throughout his story, Cousins makes much of his erythrocyte sedimentation rate as a key measure of his health. Despite disagreement about the diagnosis, Cousin’s experts settled on ankylosing spondylitis, which Cousins summarizes as, "the connective tissue in the spine was disintegrating." But as with his childhood tuberculosis, there were no confirming X-rays as evidence of spinal disintegration. Cousins convinces himself that the cause of his illness is to be found in a trip to Russia where he was tired, frustrated and exposed to exhaust fumes from diesel trucks near his hotel and from a jet on the tarmac at the airport. He also makes his own diagnosis of "adrenal exhaustion". His causal reasoning is revealed when he pauses momentarily to wonder why nobody else, including his wife, was similarly afflicted:

As I thought back on my experience abroad, I knew that I was on the right track in my search for a cause of the illness. I found myself increasingly convinced, as I said a moment ago, that the reason I was hit hard by the diesel and jet pollutants, whereas my wife was not, was that I had had a case of adrenal exhaustion, lowering my resistance. (1989, 34).

Setting aside his simple conception of causation, it is clear that evidence matters less to Cousins than a good story. In a later book, he states his philosophy of evidence in which the anecdote is the highest form:

Physicians are taught to shun conclusions based on single experiences and to look for evidence based on a substantial number of cases. Consequently their approach has to be statistical. Writers, however, seek out anecdotes
as a way of making larger statements, a way not just of capturing attention but of highlighting a point. The anecdote or individual story is the natural language of the writer. Similarly, writers tend to shun statistics. In the writer's world, statistics obscure souls. ...If nothing is real to medical researchers except as it happens to a significant number of people, nothing is real to a writer save as it happens to a single person (1990,12).

By comparison with the tenets of evidence-based medicine, this account of evidence flips the hierarchy of evidential authority on its head. Evidence-based medicine drops anecdote to the bottom, or completely off the list of evidence sources, and raises the statistics of the randomized controlled trial, the so-called gold standard of medical evidence, to the highest level of credence.

But Cousins' philosophy does have something in common with evidence-based medicine, which deprecates obedience to expert authority. He too disparages experts and their evidence: "I was also on the lookout for scientific evidence concerning the way human beings were able sometimes to refute the grim predictions of experts" (1990, 15). He continues the theme in later passages, such as when praising the general practitioner. "Plainly, the trend to medical specialization has gone too far" (1990, 30). He does not mention the vast explosion of information and technology that demands specialization. Instead of expertise, Cousins, true to his American individualist ideology, relies on his own experience. Truth is where he sees it. And seeing is believing. For example, he observes that:

The biochemical manifestations of mental powers are being well-documented. Competent observers have written about yogis in India, for example, who were trained to slow down their pulse to a few beats per minute, or who can order their skin to resist burning from hot surfaces. I myself have witnessed such demonstrations in India, so I know them to be true (1979, 117).

Cousins asked his doctor about his chances for a "full recovery". His doctor relayed the opinion of one specialist who said that he had one chance in five
hundred. Until then he had let his doctors worry about his condition. This was another cue for the individualist theme: "It seemed clear to me that if I was to be that one in five hundred I had better be something more than a passive observer" (1979, 31). In Cousins' world, everyone can be an exception. Note that the odds just hang there as "one in five hundred". We are not told if they are odds of full recovery, partial recovery, relief from chronic illness, or survival. Throughout the book, however, often referring to the will to live, Cousins implies that he was definitely dying. For instance, he concludes the book with this paragraph:

    It all began, I said, when I decided that some experts don't really know enough to make a pronouncement of doom on a human being. And I said I hoped they would be careful about what they said to others; they might be believed and that could be the beginning of the end. (1979, 160).

A dying hero makes for a better story. He is wary of believing experts. He provides little evidence, however, to substantiate the belief that he was near the end. To cure his "adrenal exhaustion" Cousins wonders, "If negative emotions produce negative chemical changes in the body, wouldn't the positive emotions produce positive chemical changes? Is it possible that love, hope, faith, laughter, confidence, and the will to live have therapeutic value?" (1979, 34).

Cousins concludes that the painkillers he was taking were toxic. For instance, he draws an analogy between aspirin and bloodletting, implying that aspirin will also one day be seen to do more harm than good. He decides to stop taking any harmful medication and to engage in recovery measures that he summarizes as, "a well-developed will to live, laughter, and large intravenous doses of sodium ascorbate" (1979, 125). He had read about ascorbic acid and thought it might "feed" his adrenal glands. He had the financial means to hire his own medical staff and, in an era when most people watched films only in a theater or on television, he had a famous television
producer send him films and a projector. He checked out of hospital and into a hotel where he had his nurse show him funny films and read from funny books. He is not clear, however, about whether he continued to take prescribed medications.

A frequently quoted passage in the literature of laughter and health is his conclusion that, "It worked. I made the joyous discovery that ten minutes of genuine belly laughter had an anesthetic effect and would give me at least two hours of pain-free sleep" (1979, 39). It is not clear what the "It" is that worked, the whole program, or just the belly laughter. At times he seems to say laughter worked, at times it was the laughter, the vitamins and his private hotel-hospital that worked and at other times he uses laughter as a metaphor for the whole gamut of positive emotions as if it was hope that worked. His 1976 article in the *New England Journal of Medicine* began with reference to a popular 1975 book, *Powers of the Mind*, by the pseudonymous Adam Smith, who had published a brief account of Cousins' claims and quoted physicians who said Cousins, "probably would have gotten better anyway" (Smith 1975, 15). Cousins wrote, "I feel justified in providing a fuller picture than was contained in Mr Smith's account" (Cousins 1976, 1458). But Cousin's retelling of the story in his 1979 bestselling book buries the reference to Smith in a later paragraph that says Smith asked Cousins' doctors,

to explain why the combination of laughter and ascorbic acid worked so well. The answer he got was that neither laughter nor ascorbic acid had anything to do with it and that I probably would have recovered if nothing had been done. Maybe so, but that was not the opinion of the specialists at the time. Two or three doctors, reflecting on the Adam Smith account, have commented that I was probably the beneficiary of a mammoth venture in self-administered placebos. Such a hypothesis bothers me not at all. (Cousins 1979, 45).

The placebo after all is just another example of what he had known since the
age of 10 about the mind influencing the body. Yet Cousins also claims non-placebo mechanisms for laughter’s health effects, including emotions and exercise. For instance, he describes the advice he offered to a 23 year-old woman whose legs were gradually becoming paralyzed:

What was significant about the laughter, I said, was not just the fact that it provides internal exercise for a person flat on his or her back—a form of jogging for the innards—but it creates a mood in which the other positive emotions can be put to work, too. In short, it helps make it possible for good things to happen (1979, 145).

Cousin’s *Anatomy of an Illness* has a controversial basic theme. René Dubos called it patient responsibility (Cousins 1979, 11). Critics have called it blaming the patient; you’re dying because you’re not thinking positively and not laughing enough. It is ironic that Cousins thanks Lawrence Kubie in the book’s acknowledgments. Kubie wrote a cautionary paper in which he argued that humor in psychotherapy has the potential to be destructive: "Over long years of experience in supervising in private hospital practice both analytically informed psychotherapy and young students of analysis, I have seen humor tried countless times. Yet I cannot point to a single patient in whose treatment humor proved to be a safe, valuable, and necessary aid" Kubie (1971). In a footnote at the end of his paper, Kubie extends his cautions about humor beyond psychotherapy to all forms of medical and surgical practice. Kubie’s complaint was not based on his own trials of humor, and many other psychotherapists cite him to dismiss his complaint. He is their straw man. Cousins, who was on the side of laughter, did not suffer the same fate; the laughter-loving Cousins became a hero while the humor-hating Kubie became a footnote. Both relied on personal experience, anecdote and informality as the basis of their beliefs.

Although Cousins says he received letters from 3000 doctors, he characteristically dwells only on the positive. By contrast, the sharp tone of
criticism from doctors not mentioned by Cousins can be briefly illustrated in
three comments: "This is the mentality of the cock who perceives clearly that
his crowing made the sun rise" (Ruderman 1980, 58); "I think it is a very bad
book although written by one of the best men I know" (Kahn 1981, 314);
Cousins’ story, "might have seemed as unfathomable to the physician as the
casting-out of devils, had it not been published in the New England Journal
of Medicine" (Spiro 1986, 107).

Nevertheless as a direct result of what Dixon (1978, 104) terms "one of the
most bizarre medical sagas of recent years", Norman Cousins was invited to
teach at five medical schools. After 30 years as editor of the Saturday Review,
at age 62, he became a medically-untrained professor at the UCLA School of
Medicine. Cousins came full circle from anti-medical-establishment hero to
advising patients and doctors. His authority was based on his authority, on
his own fairytale path from precocious child observer of mind-body medicine,
to rebel individualist who defied the odds and the medical establishment, to a
member of the medical establishment himself.

After spending 10 years on the medical faculty at UCLA, Cousins said the
number one item he learned in those 10 years is that 85% of illness is self-
limiting. In his earlier work long before becoming a medical lecturer he also
observed, "Studies show that up to 90 percent of patients who reach out for
medical help are suffering from self-limiting disorders well within the range
of the body’s own healing powers" (1979, 55). He does not examine how we
can know either of those numbers, whether physicians see a sicker subset of
humanity or anything else more detailed. His informal logic is: if 85% (or
maybe 90%) is self-healing then we should try to use the mind to heal. What
if only half or one quarter or one tenth were self-healing? How is trying to do
something related to the not doing something that he calls self-healing? His
conclusion that all this self-healing means we must explore mind-body
collaboration does not follow from his logic. Nor does his conclusion that laughter works in medicine: according to his own estimates, there is but a 10 to 15% chance that laughter had anything to do with his recovery.

Another critic of Norman Cousins delivered a lecture, later broadcast on radio, that highlights the informality of approaches to this subject (Buckman 2008). It also highlights the currency of the controversy over laughter in medicine. Robert Buckman is a renowned oncologist, medical professor, broadcaster and author who has written fourteen books and appeared in award-winning television series. He began his lecture by stating his dual objective to inform and entertain. He informed the audience that laughter was the second best medicine, that medicine was the best medicine, and that Cousins was wrong to claim that laughter cured his illness. Ironically, however, although he emphasized his expertise in medicine, citing his credentials as oncologist and professor, he relied on the same storytelling approach as Cousins. Like Cousins, Buckman stretched the facts to create the desired effect, claiming for example that most of the films that Cousins watched were the Three Stooges, implying that he had questionable taste. Cousins, for what it’s worth, says he watched Candid Camera and old Marx Brothers films (1979, 39). Buckman provided no new evidence and did not cite any research to back up his conflicting claims that Cousins was wrong yet that, "finding - and tickling - our funny bone is definitely good for our health." Instead, like Cousins, Buckman told stories. He too related a personal anecdote that supported his own belief that humor relieves pain. Buckman’s talk was advertised as providing the scientific basis for his beliefs yet he informed the audience that his beliefs were based on his personal experience. Conflicting messages were also apparent in the medium, since the lecture was broadcast not as comedy but on the long-running CBC Radio series called "Ideas" that is about contemporary thought.
Like Cousins, Gould, Kubie and many others, Buckman relied on his own authority and personal experience to justify his beliefs and also to influence our beliefs. Because Buckman delivered his message in a funny way, perhaps it is unfair to comment at all. Should we simply lighten up and enjoy the fun? This question goes to the core of the informality of the subject of laughter in medicine and the conflict between the seriousness of disease and the medium of laughter.

3.2 Reinforcement of Belief

Beliefs and knowledge arise from various sources including experience, memory, intuition, reasoning and other people. The belief that laughter is healthy is reinforced by all of these sources. We experience and remember laughter positively. We intuit that it is beneficial. We reason about a mechanism, such as an effect on our immune system. People everywhere say laughter is healthy. We laugh, feel good and others agree. Makes sense. In a word, the laughter, medicine and health story is plausible.

Reinforcement of this plausible story feeds back on itself in the typical endorsement of laughter medicine. The thesis of Metta Spencer’s (2006) book, for example, is succinctly summarized in the title, *Two Aspirins and a Comedy: How Television Can Enhance Health and Society*. Spencer summarizes the Norman Cousins story with approval and cites various scientific sources supporting, but none refuting, the notion that laughter is the best medicine. Among the sources is the frequently cited paper by Berk et al. (1989) claiming that mirthful laughter reduces neuroendocrine and stress hormone responses. That paper feeds back in its opening sentence to Norman Cousins, crediting him among others for initiating scientific interest in the
health effects of laughter. Like so many laughter boosters who rely on Berk et al. (1989), Spencer uses the paper to say laughter boosts the immune system, but without considering any details. Berk et al. (1989) reported an experiment done with a total of five men around the age of twenty-seven in the experimental group. They watched a humor video for an hour. Five other men sat quietly as controls. All men had blood samples collected every 10 minutes and various hormone levels measured. Some of the hormones changed significantly while others did not. For instance, although in theory laughter and positive affect influence endogenous opioids (Pressman and Cohen 2005), beta-endorphin levels did not increase in the laughter group. Still, if scientists measure enough things, some will change and the scientists can report those things that changed significantly. That’s the nature of experimentation. But to base any claim for laughter medicine on a small study such as this is not significantly different than basing the claim on Norman Cousins’ plausible story itself that in turn inspired the experimenters to draw blood from a few men that in turn Spencer reports to inspire readers to believe that laughter is the best medicine.

Stephen Jay Gould was known for his scientific writing unlike Norman Cousins who was known for his non-scientific works. Both ultimately relied on story, not science, when it came to their own health. Both ultimately challenged the assumption of biomedicine that general laws derived from observing many individuals are better evidence than the idiosyncrasies of one individual. The average sucked out of the many cannot breathe life back into the one. They insisted on being the odd live person, not the average dead statistic.
3.3 Interpreting The Individual Case

The question "how do you know in my case?" raises the ancient problem of a science of the individual, which Plato and Aristotle denied was possible. To break out of the boundaries of analytic philosophy and the biomedical model of medicine, Wulff et al. (1986) turned to continental philosophy and hermeneutics. Similarly in a paper published in the same year, Daniel (1986) proposed a hermeneutical model of clinical decision-making in which the patient is interpreted by analogy to a literary text (for an update see Cooper 1994; for opposition to the textual metaphor see Schwartz 1986, Baron 1990; see also Upshur 1999 and Svenaeus 2003 who link hermeneutics to phronesis). Suffice it to say that interpretation can be seen as a bridge between dichotomies such as science and art, mind and body, subjective and objective, individual and statistical.

In the context of laughter and health, an interpretive approach may be able to make sense of the connection between a funny idea and health effects (or perhaps a hermeneutic approach to laughter can help to make nonsense). Greenhalgh (1999) endorses the hermeneutic approach and takes the view that there is no paradox in a science of the individual:

How, then, can we square the circle of upholding individual narrative in a world where valid and generalisable truths come from population derived evidence? My own view is that there is no paradox. In particle physics the scientific truths (laws) derived from empirical observation about the behaviour of gases fail to hold when applied to single molecules. Similarly (but for different reasons), the "truths" established by the empirical observation of populations in randomised trials and cohort studies cannot be mechanistically applied to individual patients (whose behaviour is irremediably contextual and idiosyncratic) or episodes of illness. She thereby tells us how laws do not work for individual molecules and patients, but that's a restatement of the problem of a science of the
individual. If laws don’t work, what does work and how do we know? The hard part is how to make things work when laws don’t help, mechanistically or otherwise. How is the evidence to be applied to individual patients? Greenhalgh’s answer is what she calls an interpretive paradigm in which the physician integrates the specific patient, the general evidence and guidelines, conflicting rules of thumb and so on. How that integration is to be done is not clear, however. Interpretation appears to be synonymous with judgment. Use good judgment for each individual; easy to say but hard to do.

In the context of science generally, not just medicine, Marcos (2004) discusses Aristotle’s concern with the distinction between universal scientific knowledge and particular individuals. To resolve the distinction, Marcos finds in Aristotle a turn to the practical: “The concept of practical truth, then, is thought of especially in the ethical and anthropological dimensions, as the self-realization of each human being by agreement of intellect and desire, as a process of the actualization of human abilities occurring in action and guided by prudence” (2004, 85). This practicality and prudence is also how science applies to the individual: “it is rather a matter of applying scientific theories, laws and concepts (science in potential) to the contemplation of reality with the guidance of prudence” (2004, 86). Marcos uses the word prudence as a synonym for phronesis, which is also translated as practical wisdom (Montgomery 2006). As with Greenhalgh’s interpretive paradigm, prudence and practical wisdom are easy to demand but difficult to deliver. If they are saying that these qualities come from experience, then they are returning to the deprecated error-prone and biased experience that evidence-based medicine was devised to supplant.

Sissons et al. (2007) propose a technological solution to the application of statistics to the individual. They succinctly state the problem in these terms,

To summarize the problem, it remains a very basic tenant [sic] of the EBM
movement that an individual patient is not representative of how a population will behave but that aggregated population data can be expected to represent how an individual will behave. (2007, 12).

In textual-narrative-hermeneutic approaches, in contrast to approaches of the basic sciences, the complexities of individual variation are handled using the interpretation-judgment-practical wisdom of the physician. Sissons would bring computers, specifically artificial intelligence, to the aid of this overloaded individual physician. Paradoxically then a machine running a generic computer program could help to make general laws more applicable to individuals, thereby personalizing medicine with the help impersonal technology. Also paradoxically, a computing machine is offered as an essential means of replacing the biomedical body-as-machine model with something more humane.

It is important to note, however, that Sissons deals with prognosis and treatment, not diagnosis. This may be a consequence of the difficulty of using qualitative information in computers. After diagnostic information has been interpreted by a person and entered quantitatively in the computer, however, then automated comparisons between an individual patient and previous patients becomes possible. In essence, Sissons’ approach augments the physician's memory by comparing multiple aspects of a patient's case with multiple aspects of previous cases. No one could remember all of the details or all of the previous cases. Such a many-to-many comparison is hard for humans but easy for computers. Incorporating the individual into scientific generalization is more complex; it takes more than the statistical melding of the individual in an aggregate (see for example Hutchinson 2000). Computers are not the first place physicians would typically look to preserve the individual against the statistic. But again ironically, without computers the best that science can do is to muddle around with averages. Computers have
made possible the literature searching so highly prized by the proponents of evidence-based medicine (see Nunn 2008). Computers are also central in managing the complexity of tailoring medicine to the individual.

In addition to computers, knowledge of genetics and evolution has been offered as a bridge between science and the individual in medicine. Childs et al. (2005) argue that emphasizing in medical education the unique genetic, developmental and experiential background that accounts for individual human variation can lead to a medical science of the individual. They write, "It is immensely complicated, but by approaching medicine in a context of variation we are moving to resolve that contradiction between the singularity of the patient and the generality of treatments and prevention" (2005, 326).

The science, the statistics, the medical jargon and computer output all are meaningless, however, without the context of a story about a patient, a diagnosis, a treatment, held together in a meaningful whole. Stories are tellable. A mathematical formula is not a story. A chart of statistics is not a story. Much of the background knowledge we use in our work and generally in our lives is not amenable to experiment, only to telling. Yet as Polanyi (1966, 4) has so eloquently told us, "we can know more than we can tell." Neither science nor story can express all background knowledge.

The question of laughter as medicine is also a question of how the individual relates to science. Proponents of laughter as medicine may solve the problem with informal reasoning and stories even if trusting the storyteller (because the person is an authority, or because of good past experience with the person, or because the story is plausible and the rhetoric persuasive) can be misleading.

No satisfactory account of the place of the individual in science has yet been proposed. Gould insisted that the median is not the message. Cousins was determined to be the lucky 1 in 500. They expressed their views in
rational and scientific terms. Yet at best they were hopeful and believing storytellers who only wanted laughter and humor to be the best medicine, the most powerful sword. In the face of death, how the sword works or how it can slice a winning number out from all the other dying numbers doesn’t matter. In accordance with E.B. White’s claim that analysis kills humor as dissection kills a frog, these uplifting inspirational stories may best be read uncritically, without the analytical dissection that appeals only to the scientific or philosophical mind. There is, however, a methodology for experimenting on individual patients that some proponents of evidence-based medicine claim, going beyond hope and belief, is the best source of medical knowledge. That is the subject of the next chapter on anecdotal evidence.
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4. That's Just Anecdotal

As we saw in the chapter on story-based medicine, what kept Norman Cousins and Stephen Jay Gould from despair was the conviction that their own cases were special. They were also convinced that laughter and humor were the best medicine, the most powerful sword in the battle against their maladies. But they did not base their convictions on rigorous scientific experiments.

Single-case experiments such as N-of-1 trials, however, have been offered as a solution to the ancient problem of a science of the individual discussed in that chapter. Evidence-based medicine has deemed N-of-1 trials to be among the highest forms of medical evidence, but this chapter will show that they may not be much different from the anecdotes that have now become the lowest form. In doing so this chapter bridges the division between evidence-based and story-based perspectives of what works in medicine. Here we look at what's wrong with anecdotes in medicine and what's right about them. Then a detailed comparison of the features of N-of-1 trials and anecdotes leads to the conclusion that they both rely on the plausibility of the story.
4.1 Antithetical Anecdotes

Anecdotes, informal stories, once were the primary medium for medical knowledge. Analyses of individual persons or events have also been common in many other inquiries (including anthropology, accident investigation, education, law, psychiatry, psychology, public policy) that would be impossible if individual narratives were devalued or dismissed entirely as evidence. But in medicine, "anecdotal evidence" has become an insult. To say, "that's just anecdotal" is medical jargon for "you don't know" or "that's not true." As Eric Cassell puts it,

Medicine abounds in stories ... However, the essence of all that is believed to be antithetical to modern scientific medicine is captured in the phrase 'anecdotal medicine.' The limitation of medical scientists has been to associate the individual storyteller with irreducible subjectivity and, consequently, with knowledge that cannot be shared. (Cassell 2004, 189).

Referring to his formative years as a physician, Howard Brody writes,

If anything remotely resembling the concept of narrative entered our workday discussions, it was the dreaded charge of 'anecdote'-- a pejorative term applied to out-of-date evidence that ought to be supplanted with 'real' scientific data from the latest medical journals (Brody 2003, v).

Kathryn Montgomery expresses the role of narrative generally, whether fiction, anecdote or case narrative, in these these terms:

As medical students, they learn to construct, record, and present cases—above all, to think with them—and at the same time they are taught a suspicion of anecdotal evidence, the singular occurrence that can skew perception. This only seems contradictory. Skepticism about narrative is an entirely justified, although sometimes myopically misunderstood, part of clinical rationality. Patrolling the borders of medical discourse, suspicion of the anecdote restrains medical narrative and blocks incursions of the irrelevant or the emotional (Montgomery 2006, 48).

The medical anecdote is not merely a form of communication among professionals and between professionals and patients, however. The following discussion goes beyond inquiry into forms of medical communication to
inquire into the role of anecdotes as sources of medical knowledge.

4.2 What's Wrong With Anecdotes

Anecdotes, indeed any unsystematic accounts, are said to be unscientific and untrustworthy; controlled experiments (in particular, RCTs, randomized controlled trials) provide knowledge while anecdotes do not. Just because a treatment appears to work (or not) once upon a time does not show that it will work generally, or that it ever did work. As a first approximation, the problem with anecdotes is that they are poor grounds for inferring general scientific laws. Reasoning from one or a few stories to support general laws is chancy. Populations not individuals matter, both in the sense of populations of experimental subjects within each experiment, and in the sense of populations of replicated experiments that can be systematically reviewed and pooled into massive populations of evidence.

Their singularity or limited replication is only one feature of anecdotes, however. Meanings and problems attributed to anecdotes depend on which features are emphasized. Three illustrative features, among many others, are how specific, how observable and how plausible the story is. If the anecdote describes one specific person, then a question arises whether the story applies to anyone else. If the anecdote is about a patient or group of patients from long ago or far away then a question arises whether the story has been transmitted reliably over time or distance. Tracing unobservable hearsay evidence is not the same problem as extrapolating from one person to another. If the anecdote is about something implausible, that does not fit with background knowledge independent of the anecdote, then by definition its plausibility is in question. That the story is unconvincing or just too
Strange to be believed is again different from its specificity or its traceability.

More generally, anecdotes do not have the indicia of science. Biomedicine, with its nomothetic methodology, views diseases as physical, causal events in biomechanisms. Statistics reveal quantitative governing laws in diseased populations. Physicians are seen as objective scientific observers who diagnose and treat those diseases with specific remedies. By contrast, anecdotes and ideographic methodology are concerned with illnesses as subjective (unscientific, qualitative, interpretive, narrative, phenomenological, existential) stories that are at best a means of organizing uncertainty. Although science and stories share a sensitivity to evidence, which must render the account plausible, stories are not usually controlled, repeatable or generalizable.

Medical anecdotes in particular can be criticized for various biases such as selective memory and confounding factors such as relevant treatments not mentioned in the story. But the more highly valued randomized controlled trials also suffer from their own biases and confounds (for a detailed list of biases in randomized controlled trials see Jadad and Enkin 2007). Many trials are methodologically unsound and not trusted, which is why only selected experiments are reported in systematic reviews of randomized controlled trials. Ultimately in science and medicine we assess the plausibility of the story, whether anecdote or formal experiment or review.

4.3 To Define or Not To Define

This may be a convenient point early in the discussion to expose the elephant in every philosophical room, namely language. One approach to language is simply to use words as clearly as possible and trust the reader to infer
connections to other words. For instance, in the previous quotation Cassell uses the words story and storyteller in reference to anecdote, so anecdotes must have a family resemblance to stories. Brody and Montgomery both refer to narrative in their accounts of anecdote, so anecdotes must also be in the narrative family. Their familial relations arise in context, not in advance.

Another approach is to attempt definitions. There is always the critic who dismisses as too vague any work that hasn’t defined the terms, or that uses them without consistency. What precisely do you mean by anecdote? Exactly how is narrative different from story? Defining these terms and mapping their genealogy would be a large project, if indeed it were possible. Many others have contributed to such a project. For instance, Macnaughton (1998) asks, what are anecdotes? Brody devotes a whole chapter to description of narrative, beginning with the question, what is a story? (2003, ch 2). Rather than restricting their contribution with a definition, Moore and Stilgoe (2009) discuss how the term anecdotal evidence is constructed among experts and non-experts.

The present discussion will take a Wittgensteinian approach and rely on the network or family of resemblances arising out of the words and related background knowledge. This approach is consistent with the focus in medicine not on concepts in the abstract but on the person who rarely has the luxury of formal definition or extended terminological discussion.

### 4.4 Stories All The Way Down

As shown in the following illustration, various forms of medical evidence inform the medical story, each form being itself a story and a part of the overall story. Stories may be composed of stories all the way down. The
A binary distinction is often made between "anecdotal evidence" and "scientific evidence" as if there are two distinctly different kinds of medical evidence, science and stories (or sense and nonsense). Anecdotal evidence, according to the received skeptical view, seduces the listener and distorts the cold hard facts. It’s also easier to understand and identify with one person’s story of suffering than to be moved by abstract percentages and standard deviations. The storyteller focuses on the individual beautiful baby who tumbled down the well, not on general childhood accident statistics. Testimonials provide such a persuasive picture that attempts have been made to translate statistics into pictures so that statistics have a better chance against the seductive anecdote (see for instance Gigerenzer 1996, Fagerlin et al. 2005). To the extent that the medium determines the message, however, numbers and statistics deliver a message that may not be capable of expressing what is appropriate in all circumstances. As we saw with Norman
Cousins and Stephen Jay Gould, statistics were the worst enemy not the best evidence or explanation.

Science generates particular kinds of stories. Science tries to generate new laws whereas ordinary stories rely on previously understood laws (Murphey 1973, 123). For instance, at one time a plausible medical story about treating a patient with a gastric ulcer would be told in terms of reducing stress and eating a bland diet. To tell a story based on a germ theory made no sense. Now the story of the same patient cannot be told without mentioning *H. pylori*. Science has substituted a law-like association of bacteria causing disease for the former law-like association with a too spicy life. The story has changed but it remains merely the current plausible story. Science deals in universal laws that are not limited to individual persons, things, times or places. But consider the anecdote cited by Murphey about an individual, John, who gets a rash every time he eats spinach. Murphey says that despite the individual reference to John, "Such statements are predictive, support counterfactuals and have explanatory power" (1973, 76).

Yet despite these virtues, individual references to John, Stephen Jay Gould, Norman Cousins or a physician’s experiences with any individual story are devalued in medical currency. Consider possible answers when a patient asks her physician how do you know this therapy works? (adapted from Upshur and Colak 2003):
1. Because I’m a doctor (intuition/authority)
2. Because in my experience it works (experience)
3. Because it interferes with a specific biochemical process (basic science)
4. Because many well designed studies have shown that the drug is effective (clinical science)
5. Nobody knows (but trust me)
6. I don’t actually know but I believe it works and you should too (possible
placebo)
7. I don't know but I am told it works (referral beyond physician to higher authority)
8. It's what most physicians prescribe (convention, protection from malpractice claims)
9. It can't hurt to try (particularly applicable to so-called placebos and other seemingly harmless interventions)

Answer number 4 is the privileged answer in evidence-based medicine. Other answers carry much less weight or are crossed off the list of possibilities. According to the tenets of evidence-based medicine, experimental evidence, preferably randomized controlled trials that generate statistics, not individual stories, numbers not narrative, show what really works (for mediating views see Enkin and Jadad 1998, Greenhalgh and Hurwitz 1998). Physicians are not to do something just because it has always been done, or is based in hearsay, anecdote, authority or even the personal experience and expertise of the physician.

But what is the right answer for the typical elderly patient taking more than a half dozen medications that have never been tested together in any controlled experiment? Answer number 4 is not available in all instances. Moreover, even if science shows that the drug works in experiments, and even if the drug also appears to work in the broader population, that does not mean the drug works in this particular patient. The drug may cause improvement but that Mavis improved after taking the drug does not say the drug caused her improvement.
4.5 Advocates for Anecdotes

Advocates for anecdotes in medicine cite various reasons for their enthusiasm. In the context of clinical stories, Cox (2001) says stories expand expertise, are the unit of clinical work, provide a framework for complex details and explain decisions. In regard to adverse drug events, Aronson (2005) lists seven reasons to publish reports of single cases:

- describe a newly recognized adverse reaction or interaction
- generate hypotheses
- test hypotheses
- demonstrate diagnostic techniques
- elucidate mechanisms
- elucidate methods of management
- remind or educate

For reminding and educating, Aronson cites the case of two patients reported not in terms of a new discovery but as a reminder of existing knowledge that liquorice can cause hypertension with hypokalaemia. Although Aronson directs his comments to the problem of adverse drug reactions, such cautionary stories have broader implications.

More generally, negative stories can be viewed as instances of the positive scientific method of falsification. Early reports of treatment are often positive. Later widespread use may reveal a negative side. The case of thalidomide serves as a vivid example. Early in its history, thalidomide was considered to be a safe and effective medical intervention. Then a more complete story emerged, as evidenced by the following letter, composed in a few fateful sentences, to the editor of The Lancet:

Sir,-Congenital abnormalities are present in approximately 1-5% of babies. In recent months I have observed that the incidence of multiple
severe abnormalities in babies delivered of women who were given the drug thalidomide ('Distaval') during pregnancy, as an antiemetic or as a sedative, to be almost 20%.

These abnormalities are present in structures developed from mesenchyme—i.e., the bones and musculature of the gut. Bony development seems to be affected in a very striking manner, resulting in polydactyly, syndactyly, and failure of development of long bones (abnormally short femora and radii).

Have any of your readers seen similar abnormalities in babies delivered of women who have taken this drug during pregnancy? (McBride 1961).

Montgomery Hunter (1991, 76) quotes the author of the letter presenting this story and saying that he had no grounds for speaking as a scientist but as a human being he could not keep silent. In their quest for laws of averages, scientists reject the individual as the potential outlier on the tail of the population distribution. The thalidomide anecdote merges medicine and story, scientist and human being. A clearer case of being human in the context of being scientific would be hard to find.

Evidence-based medicine and science may seem to be far removed from stories. They share many features, however. Stories are never finished. Like science, they change with each generation, sometimes with each retelling. Stories are more or less believable, like scientific hypotheses. Plausible stories and experiments are not so obviously correct as to be trivial and not so obviously wrong as to be rejected outright. Science and story are not as different as they may seem. Say for instance that the report of a randomized controlled trial shows that 90% of patients using the drug got better while all patients on placebo did not. Would you use the drug?

Despite this impressive statistic, likely your answer is: not without more of the story. One typical story behind the results might be that 10 young otherwise healthy male volunteers with mild symptoms were recruited for the study. But how does that study intersect with real patients? A more
applicable story would be that 10,000 women and men, of all ages, with severe symptoms, and taking multiple other drugs, were recruited. But that study probably will not be done. And in this example, nothing has been said about the risk side of the story.

Evidence is part of some historical context. The story of the events that generated the evidence informs the story of applying the results to one patient. Conversely, suppose there has never been a randomized controlled trial of a treatment. Would you use it? Of course, since that is the case for most of medicine. Again, it depends on the story.

The form as well as the content of medical reports also supports a view that medicine is based on stories. The standard IMRAD script (introduction, methods, results, analysis and discussion) of an experimental report is like a structured story of a single event that meets the Aristotelian requirement of beginning, middle and end. Critical assessment of those story elements is a core practice in evidence-based medicine. Like literary critics, research reviewers ask: Are the characters (authors, subjects) fully drawn and reliable? Are their actions and methods appropriate? Are we persuaded by their story? The evidence-based story is a highly structured statistical story. The foundation is the randomized controlled trial, sometimes magnified by meta-analysis into a pooled statistical story. The meta-story is observational not experimental (observation of the experiments that were analyzed). Similarly a systematic review recites a script composed of selected individual experimental stories.

Another advantage of anecdotes is that they are often the basis of scientific insight. For instance, in 1600, Admiral James Lancaster noted that lemon juice is the best remedy for scurvy (see Hughes 2003 for a succinct history). A century and a half later, James Lind put this nautical wisdom to the test on 12 sailors in 6 pairs. Lind observed that oranges and lemons cured
scurvy while cider, sulphuric acid, vinegar, sea water or nutmegs did not work. No randomized controlled trials or systematic reviews have been reported in the intervening centuries (Hughes 2003) yet everyone knows that citrus fruits prevent scurvy. We know it works in part because the story is so dramatic, or in other words, the effect size is large, and generally so. By comparison, not only anecdotes but all the properly conducted randomized controlled trials ever done of water treatments (homeopathy) are suspect (Ernst 2002).

We prefer the scurvy story that has few formal experiments to the homeopathy story that has many randomized controlled trials and systematic reviews in its pedigree. That water could be a powerful drug or retain memories of substances no longer present strains against background knowledge. More generally, a randomized controlled trial is most needed to shore up a weak story in those very instances when there are small, undramatic differences. The story of water therapy is not plausible and the effects are not dramatic, so even a positive randomized controlled trial resembles luck. It’s the story, and its plausibility, that influences belief in the experiment not just the experiment that provides evidence for the story.

A well-known anecdote that has become the entry point for discussion of these issues comes from one of the great figures in experimental methodology. In his 1935 book *The Design of Experiments*, R.A. Fisher described the example of a lady who claimed that she could tell the difference between tea poured into milk and milk poured into tea. Fisher doubted her claim, since according to his hypothesis, either way she was tasting the same mixture of milk and tea. In his book he describes an experiment with 8 cups, half with milk poured first and half with tea first. Fisher doesn’t report whether the experiment was actually conducted that way, although there was a real lady, Dr. Muriel Bristol, and an experiment in which she guessed
correctly (Box 1976, 793). The story has spawned a large literature about the lady tasting tea. This single-person experiment not only stands at the foundation of experimental theory, but also as an aside shattered theories of tea mixtures, which soon changed from skepticism about her taste buds to postulates of heat differentials and protein chemistry that changed the taste of the tea.

4.6 N-of-1 Trials

Experiments on one person can range from simply observing the individual before and after intervention to invoking the formalities of randomization, blinding, placebos and statistical analysis (for methodological details see Guyatt et al. 1988, Backman and Harris 1999; see also Worrall 2007 who suggests that randomization is unnecessary but that is an issue for another forum). Whether they are case studies or double-blind placebo-controlled randomized single person (N-of-1) trials, they are all attempts to attach the indicia of science to the individual case. Single-subject methods promise a kind of individualized medicine. An N-of-1 trial applies the widely endorsed methodology of randomized controlled trials to the study of one individual instead of a large group of participants, so it is worth comparing this purported science of the individual with anecdotes or stories of the individual.

A trial might, for example, compare two treatments during randomly alternated treatment periods for one patient seeking relief from symptoms of a stable condition over several weeks or months. N-of-1 trials are conducted for various reasons. Perhaps no reports of randomized controlled trials with many participants are available for these alternatives or this condition. Or if there are reports, perhaps they are not applicable to this patient. Although
the terms single-subject and N-of-1 suggest that only one person is the subject, that is not invariably so. Experiments on one person can be grouped with similar experiments on others. For example, a recent study investigating the feasibility of N-of-1 trials followed 9 patients with osteoarthritis of the knee comparing two knee supports or two drugs (Brookes et al. 2007).

Anecdotes assume no particular ideology or methodology, whereas N-of-1 trials are designed to eliminate bias and confounding. In essence, however, these stories trace the basic plot of any experiment: trying one thing after another to see what happens. Jane Macnaughton writes, "Anecdotes and randomized controlled trials can come together in the case of the so-called "N-of-1" study, an internally randomized study in an individual patient" (1998, 207). Having detailed the features of anecdotes, she goes on to say that the reporting of such a trial becomes an anecdote. That is, an experiment designed to be formal and rigorous, not mere anecdotal evidence, ends up being a story. Of course this is no surprise, whether the aim of science is to tell a true story of what the world is like or only to tell an empirically adequate story (van Fraassen 1980, 8). There is no unique story of an experiment, as there is no unique account of any event in history. Variations depend on the complexity of the experiment, how clearly it was reported, how the report was interpreted and so on.

Yet viewed as science not story, the single-person experiment has been deemed by leading proponents of evidence-based medicine to be the highest form of medical evidence. In evidence-based medicine, the individual story oscillates between the devalued anecdote and highly valued N-of-1 experimental anecdote. For instance, in their influential work Guyatt and Rennie (2002, 7) place the N-of-1 trial at the top of their evidence hierarchy for treatment:
1. N-of-1 randomized controlled trial
2. Systematic reviews of randomized trials
3. Single randomized trial
4. Systematic review of observational studies addressing patient-important outcomes
5. Single observational study addressing patient-important outcomes
6. Physiologic studies (studies of blood pressure, cardiac output, exercise capacity, bone density, and so forth)
7. Unsystematic clinical observations

Tsapas and Matthews (2009) recently stated that N-of-1 trials should be used more but are too rigidly regulated. In reply Montori and Guyatt reiterated the highest regard for N-of-1 trials saying, "they in fact represent the highest standards of establishing the benefits and harms of therapy in an individual" (2009, 1023). So a story of one person can trump the systematically reviewed stories of many experiments. Yet an N-of-1 story also shares the singularity of an anecdote.

A common reaction to this comparison between N-of-1 trials at the top of a hierarchy with anecdotes at the bottom is simply to dismiss this particular hierarchy. Since dozens of evidence hierarchies have been proposed, and this hierarchy is just one of the later additions, the comparison made here could be a mere straw man for debate. Another reaction is that it's just plain wrong to say that the single person experiment has been deemed to be the best evidence and in any event, hierarchies are now being assimilated into evidence grades (for instance, www.gradeworkinggroup.org). A related attack is simply to note how few N-of-1 trials are conducted, again as a way of containing their significance.

A narrow response to such comments is simply to observe that this particular hierarchy does hold up N-of-1 trials as the best evidence, so it is
correct to say they have been deemed to be the best evidence, even if there is disagreement about this particular deeming.

A broader response is that anecdotes and N-of-1 trials both can provide the basis for further investigation and can show the way to the next level of analysis. Neither anecdote nor N-of-1 trial can provide reliable evidence of general treatment efficacy, but they provide some evidence and should not be discounted entirely.

An even broader response is to note how deeply entrenched is the statistical population view. This meta-level discussion mirrors the underlying debate about statistics and the individual. The argument goes that there are lots of evidence hierarchies, and within most hierarchies in the population of hierarchies, anecdotes are not highly ranked. So by comparison to the average hierarchy, this single hierarchy with its single person at the top is one lonely anecdotal outlier to be ignored. It deviates significantly from the rest of the population of population-based evidence hierarchies.

But evidence hierarchies are not evidence, and neither their popularity nor general agreement automatically increases their correctness. Regardless of what others have proposed, that an influential group of experts continues to hold up N-of-1 trials as the best evidence is noteworthy. It is important to note that randomized controlled trials are rare in biology and not done at all in some other sciences.

It could be argued that formal N-of-1 trials are different, more scientific, than less formal anecdotes, case reports and other stories, and that comparing informal stories about one person or event with other scientific experiments loses sight of the specific purpose of an N-of-1 trial. That purpose, unlike a story about someone else or a randomized controlled trial involving other subjects, is not to generalize the results to people outside the story or the trial, but only to see what works for the specific person in the N-
of-1 trial. Well, yes and no. Yes, that could be the purpose and no, the results of the trial become evidence and contribute to experience anyway despite such a narrow purpose. An N-of-1 trial of the feathers of a black swan may have implications for all swans.

More fundamentally, however, that an intervention did or did not work for an individual yesterday cannot necessarily be projected to work for the same individual tomorrow or even be said conclusively to have worked at all, given the vast range of other possible influences on the results observed yesterday that may or may not be observed tomorrow. That a coin with no determinable bias has landed heads up seven times in a row does not establish that it is either an unfair coin or a fair one. An unexpected outcome, however, like a surprising anecdote, may prompt a further level of investigation, such as looking more closely for a determinable bias in the coin.

### 4.7 Qualitative Mistrust

So can anecdotal evidence prove anything in medicine? One famous example of further investigation prompted by an uncontrolled single person experiment is that of Nobel Prize winner Barry Marshall who experimented on himself by swallowing bacteria to demonstrate the connection between *Helicobacter pylori* and gastric ulcers. From his autobiography (as published in Grandin 2006):

> Becoming increasingly frustrated with the negative response to my work I realized I had to have an animal model and decided to use myself. Much has been written about the episode and I certainly had no idea it would become as important as it has. ... I had a successful infection, I had proved my point.

This particular medical scientist clearly states that his anecdotal evidence "proved" his point. Not everyone infected with *H. pylori* gets sick; maybe he
was merely lucky (or unlucky depending on your viewpoint) to get sick exactly when he wanted to get sick. It was no miracle, however, that his hypothesis led to confirming evidence in the form of his sickness. Later randomized controlled experiments provided further support but it is clear that when he successfully infected himself he needed no further evidence to warrant his claim. For him, his own anecdote trumped any previous highly valued randomized controlled trials or other experiments that were interpreted in terms of theories that did not respect the role of *H. pylori*.

Proponents of evidence-based medicine explicitly deprecate such qualitative observational evidence. Physicians are exhorted to rely on impersonal objective scientific evidence not stories. The seeds of mistrust of stories are sown at various levels including:

• individual anecdotes
• experience and expertise collected in many anecdotes
• individual experiments
• experiments that have not been widely replicated
• groups of experiments that have not been systematically reviewed
• systematic reviews that have not been recently updated

Evidence-based medicine has yet to extend its mistrust as far as the philosophers and physicists who mistrust our one universe. After all, ours may be only an individual story among many possible universes. The pattern is clear, however. Evidence-based medicine values numbers over narrative, observation over theory, experiment more than explanation, experience and expert opinion. The plausibility of this framework is consistent with a view of medical knowledge as experimental scientific knowledge that is orderly, ahistorical and progressive. The moral of many stories that have surfaced throughout the history of medicine is that the only trustworthy stories are those of controlled experiments published in scientific journals and confirmed
by multiple experimenters. But evidence-based medicine neglects parts of the story to focus on technological interventions afforded by the richest countries where experimenters and systematic reviewers work, and in any event methods other than science are needed to decide among conflicting scientific stories about experimental evidence.

**4.8 Anecdotal Resistance**

Although the evidence-based medicine movement has turned away from stories, there is also widespread dissatisfaction with medicine conceived as an impersonal objective science in which patients become cases, doctors know the names of myriad diseases but not the names of their patients and personal life stories are neglected in comparison to impersonal lab reports (Cousins 1976, 1979, Cassell 2004, Groopman 2007). Patients want more than the typical 18 seconds they get to tell their stories (Borkan et al. 1992).

In response there are various narrative, hermeneutical, phenomenological approaches to medicine that view medicine as a hybrid science-using practice (Montgomery 2006). Narrative based approaches not surprisingly exhort physicians to pay more attention to stories. They point out that stories matter not only in the patient history, but also as stories framed by the physician in medical terms throughout the medical encounter. Inspirational stories also have their own special place in medicine. The story of stories in medicine often ends at narrative in clinical practice and generally in communications with the patients. A more complete account of stories in medicine, however, extends further into the basis of medical knowledge.

As the single person experiment illustrates, anecdote and science are not opposites. In apparently story-less scientific contexts, not just in the storied
physician-patient context, experimental reports can be interpreted as stories of what experimenters did. Systematic reviewers later generate their own observations of the collected stories of experiments. Reviewers of systematic reviews in turn report their observations of systematic reviews. At each of these levels the story of what works in medicine is revised and retold. Medicine is based in stories, some of which are of single persons and events, some of which are of experiments and some of which are of selected groups of experiments.

Medicine is not the abstract intellectual pursuit of scientific knowledge. Medicine has a practical focus, the person. A chemist does not consider the meaning of an experiment for an individual water molecule. A geneticist does not ordinarily express concern for the individual fruit fly clinging to the edge of the bottle. But even medical researchers who never see patients nevertheless focus on patients. Everything medical can be filtered through patient-centered questions; what does this evidence mean for the patient? The answers to such questions are integrated by meaningful stories.

Another plausible story is that all science is a form of storytelling (see for instance van Fraassen 1980, Grobstein 2005). Schaffner proposed that scientific theories are stories:

A theory is like a historical novel with a cast of characters, some of whom are known to have existed, some of whom are quite different from any known historical personage, but who, if they had existed, would explain history. The scientist proposes such a historical novel in the hope that it will turn out to be accurate history. But if it turns out to be false or nonconfirmable or nonfalsifiable, it may be read as fiction; it is not a 'meaningless' story (Schaffner 1993, 132).

The present account concerns only medicine, not all science, although there is an interesting story to tell of medicine as the standard science, of physic not physics, as the model for all sciences (Schafer 1993). When it comes to patients, however, and that is what medicine comes to, even the most
determinedly scientific views such as evidence-based medicine resolve to stories.

Like many classic stories, this one has come full circle: from anecdote-as-insult, to randomized controlled trials as plausibly better evidence, to N-of-1 anecdotes as the best evidence and back to science as story all the way down. Contrary to narrow hierarchical representations of evidence in medicine, not all anecdotes are worthless and not all randomized controlled trials are gold. Devaluation of anecdotes in medicine has gone too far. The moral of this story is that the expression "anecdotal evidence" should be used sparingly as an insult, if at all.

Although patients and storytellers, like Norman Cousins and Stephen Jay Gould, do not express their suffering these terms, for them conclusions drawn from randomized controlled trials on other people, not anecdotes, are the insult when it comes to conducting their own lives. They remind us that each life story is also a unique uncontrolled N-of-1 experimental trial. Their answer to the question how we know that laughter is the best medicine or any medicine at all might be the same as the most scientific of answers: it's a plausible story.
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5. Language of Laughter

It is likely that in *Philosophical Investigations* Wittgenstein was serious when he described philosophy as a battle against the bewitchment of our intelligence by means of language. Still, he was reported to have said, "that a serious and good philosophical work could be written that would consist entirely of *jokes* (without being facetious)" (Malcolm 1984, 27, italics in the original). Can a serious and good work on laughter be written consisting entirely of philosophy? Language matters even if only to understand the joke.

As discussed in the chapter on evidence-based medicine, it's often not clear what researchers are measuring in their laughter experiments. If we are to understand laughter and its connections to medicine and health, we must examine what laughter is and the language that laughter researchers use to study it. This chapter begins with attempts to define laughter that show how intrinsically contextual and elusive laughter is. Then we focus on the terminology used to specify positive emotions associated with the connections among laughter, medicine and health. We find no agreed language, but instead mostly one-word explanations, binary opposites and other surrogates for theory.
5.1 Attempts to Define Laughter

The question of what exactly laughter is remains unanswered despite a long history of attempts to define it. Elizabethan scholar Thomas Wilson found that he simply could not do so. In his words,

Now to tell you in plaine words, what laughter is, how it stirreth and occupieth the whole body, how it altereth the countenance, & sodainly brasteth out that we cannot keepe it in: let some mery man on Gods name take this matter in hand: for it passeth my cunning, & I think even thei that can best moue laughter, would rather laugh merily when such a question is put forth, then giue answere earnestly, what, & how laughter is in deed (Wilson 1560 Book II).

Wilson’s attempt is reminiscent of that by a more famous Elizabethan, William Shakespeare, whose character Bottom the Weaver had similar difficulty when trying to describe a dream:

The eye of man hath not heard, the ear of man hath not seen, man's hand is not able to taste, his tongue to conceive, nor his heart to report, what my dream was (Shakespeare, A Midsummer Night’s Dream, Act 4, Scene 2).

So here at one extreme we face the possibility that we may never get any farther in saying what laughter is than merely to laugh, and to dream that we know. Recent observers of laughter might agree with those Elizabethan sentiments. For example, Ruch and Ekman (2001) point out that laughter is loosely described in the literature:

Laughter is not a term used consistently, nor is it precisely defined in research articles and even encyclopedias. In everyday life a smiling face is often referred to as "laughter" although the vocal element is entirely missing. Research studies usually focus on one or two systems of laughter rather than the integration of all the components involved. For example, a study of the acoustics of laughter typically restricts the investigation to the phonatory system and may arrive at the conclusion that the duration of laughter is, say, below two seconds. Sound production is contingent on air flow and the deviation from resting breathing exceeds the rhythmic forced exhalations underlying laughter by far; therefore duration estimates of eight seconds are not uncommon in respiration studies (e.g., Habermann,
In studies of primate laughter the face gets most attention and also studies of joy in humans typically focus on the face, or even on the emotion-specific actions neglecting the mouth opening altogether.

Ruch and Ekman (2001) provide a detailed review of the physical aspects of laughter including respiration, facial action, acoustics, and body movements. They conclude, "The list of unanswered questions and unsolved problems seems endless, but the study of laughter is a worthwhile subject as it is a window to ancient affective experience; a prototype of the prelingual utterance of joy."

Much has been written about the history of laughter but no comprehensive history of laughter has yet appeared (Wickberg 1998, 46). Moreover, the same could be said of laughter literature as of humor literature, that "the body of literature concerning humor is so large that it is not pragmatically possible for any single scholar to cover it in its entirety" (Attardo 1994, 15). It is possible, however, to generalize about the scientific literature concerning laughter: one of the weaknesses of the scientific literature is its ahistorical treatment of laughter. Today's notions of laughter are generally assumed to have applied at all times. But it is worth remembering that laughter has not always been associated with humor, nor has laughter always been considered a subjective experience distinct from the object of the laughter. After listing the dominant theories of laughter, namely superiority, incongruity and relief theories, Wickberg says, "By reducing understanding of laughter to a static and abstract system of types, twentieth-century philosophers, psychologists, and critics have removed laughter from history. This ahistorical categorization obscures the changes that have taken place in the general understanding of what laughter is, what it means and how it affects the nature of social relationships" (Wickberg 1998, 47).

The scientific literature concerning laughter also tends to put the
technical cart before the definitional horse. For example, a recent review of
the neurology of laughter claims that there is agreement on the location of
laughter in the brain:

To summarize, nearly all authors agree that there must exist in the
brainstem a final common pathway for laughter, integrating facial
expression, respiration and autonomic reactions. There is good evidence
that only dorsal mesencephalic lesions cause a diminution of facial
emotional expressions, whereas ventral lesions lead to pathological
laughter. The data cited from animal experiments, as well as the newer
case reports summarized above, lend support to the notion that such a
laughter-coordinating centre must lie in the dorsal area of the upper
pontine mesencephalon and is connected to the PAG and the RF (Wild
2003, 2130).

Having determined the location of laughter, the authors go on to say that
there is no agreement on what laughter is:

Although operational definitions of 'laughter,' 'humour' and 'funny' have
been formulated for individual studies, a broad consensus on their exact
meanings has yet to be reached. This is not a trivial handicap: it is obvious
that what one means by humour and laughter will influence what kinds of
experiments one designs for their analysis. The relationships between the
subjective feelings of an emotion (in this case, exhilaration) and its motor
expressions (in this case, smiling and laughter) have been discussed for
over a century (James, 1950) and continue to be the subject of lively

So it is apparently possible to know where laughter is without knowing what
it is. Laughter neuroscience proceeds despite this puzzling handicap.

In the laughter literature the word laughter is so commonly associated
with the word humor that they have become intertwined and are often
conflated (see for instance Attardo (1994, 10ff) who considers laughter in the
context of linguistic theories of humor and cites Kant's and Shopenhauer's
definitions of laughter in support of incongruity theories of humor). The
Oxford Companion to Philosophy defines them as follows:

laughter. Laughter is a psychophysical phenomenon against which a
number of philosophical theories of mental and personal identity can be
tested. If laughter is essential to the psychology of humour, then a creature endowed with humour must be embodied. But since the bodily postures and motions which are characteristic of laughter can occur in the absence of amusement, laughter cannot be a simply physical occurrence. Aristotle uses these considerations to support a theory of human nature according to which a person is not identical with a body, yet does not exist without a body. More recently John Wisdom has hinted at how humour and its objects can provide helpful pointers to the analysis of the relation between subjective and objective elements in the nature of value. The topic deserves more attention in the philosophy of mind. (Evans 2005)

The closest we get to laughter in this description is "psychophysical phenomenon." Note the mixture of laughter, humor and amusement. Here laughter is defined with reference to "humour and its objects" not just laughter's objects. The description of humor suffers from a similar conceptual interbreeding:

humour. Although laughter, like language, is often cited as one of the distinguishing features of human beings, philosophers have spent only a small proportion of their time on it, and on the related topics of amusement and humour. Of the two most widely held theories, the first, that humour expresses a superiority of the individual who is amused over the object of amusement, is the most venerable. The second is that amusement is a response to incongruity. Amongst the topics that have surfaced in recent discussion, three catch the attention. We talk of a sense of humour, and this seems to assume that some are equipped to see what is funny about a situation whilst others cannot. Does it follow that the situation is itself funny antecedent to anybody finding it so? Are we then committed to realism about humour? Secondly, is humour a virtue? How does a sense of humour connect with other virtues, and is its absence a defect in an otherwise good man? Connected with both these issues is the general relevance of moral considerations to humour. Does the fact that a joke is racist or sexist mean that it is not really funny, or that it is merely a fault in us if we laugh at it? (Sharpe 1995)

As the definition of laughter spoke primarily in reference to humor, in a similar circularity, the definition of humor does not begin by defining humor. Instead this definition of humor begins with laughter: "Although laughter, like language, is often cited as one of the distinguishing features of human
beings, philosophers have spent only a small proportion of their time on it..."
Humor is then added as a related topic to laughter. Perhaps this should come
as no surprise. According to cartoonist Saul Steinberg (quoted in Gutwirth
1993, 1), "Trying to define humor is one of the definitions of humor."

Neither of these definitions contemplates the possibility that laughter and
humor have distinct lives of their own. Conversational laughter, nervous
laughter, ticklish laughter and pathological laughter, among other laughter
species, are not necessarily triggered by or even associated with humor.
Laughter can also be triggered by tragedy, disaster and other humorless
events (such as bereavement: see Keltner and Bonnano 1997). Note also the
use of the problematic word "funny" in the definition of humor. According to
Provine, funny is not very informative: "'Funny' and 'unfunny' are simply
ways of saying that you laughed or did not laugh at somebody or something." (2000, 46). Nevertheless, these definitions are somewhat more informative
than others. For example, what the Oxford English Dictionary gains in
brevity it loses in circularity, defining laughter as the action of laughing and
laughing as the action of the verb laugh. It finally lands on more independent
terms with laugh defined in this unintentionally funny way:

To manifest the combination of bodily phenomena (spasmodic utterance of
inarticulate sounds, facial distortion, shaking of the sides, etc.) which
forms the instinctive expression of mirth or of sense of something
ludicrous, and which can also be occasioned by certain physical sensations,
esp. that produced by tickling. Also transf. to have the emotion (of mirth,
amusement, scorn) which is expressed by laughing (OED second edition
1989).

To define a concept before exploring and discussing it is like writing the
headline before reading the story. Sometimes the whole discussion is the
definition. Sometimes an entire literature provides only partial description.
That is the case with laughter.

There is also the Gertrude Steinian view that there is no there there when
looking for laughter. Definitions will not locate laughter or deliver us to an understanding, or much else for that matter. We could follow Wittgenstein’s lead, and stop trying to find necessary and sufficient conditions for laughter, as he does in his well-known example of games: “Consider for example the proceedings that we call "games". I mean board-games, card-games, ball-games, Olympic games, and so on. What is common to them all?” (Philosophical Investigations 66). He briefly demolishes the illusion that games share common features:

When we pass next to ball-games, much that is common is retained, but much is lost.-- Are they all 'amusing'? Compare chess with noughts and crosses. Or is there always winning and losing, or competition between players? Think of patience. In ball games there is winning and losing; but when a child throws his ball at the wall and catches it again, this feature has disappeared (Philosophical Investigations 66).

He says, "we can go through the many, many other groups of games in the same way; can see how similarities crop up and disappear. And the result of this examination is: we see a complicated network of similarities overlapping and criss-crossing: sometimes overall similarities, sometimes similarities of detail" (Philosophical Investigations 66). Then he introduces his famous metaphor of family resemblances, saying that games are a family. Their boundaries are not sharply drawn. There is no unique list of necessary and sufficient conditions for something to be a member of the family. As is true for games, there is also a huge variation in laughter but we recognize its family members when we see and hear them.

So Wittgenstein provides further support for the network metaphor used throughout this discussion. Definitions can’t take us as far as a whole network, yet as this discussion shows, many researchers are still seeking definitions, if for no other reason than as focal points for sharing thoughts. In any event, it helps to sketch what little we know of a country before going
there.

5.2 Emotions Associated with Laughter

A common joint at which to carve the laughter world is to separate the physical activity from the associated emotions. When we add emotions to the scope of the inquiry, however, we have to confront another potential quagmire while exploring and delimiting emotions. The visible act of laughter is problematic enough but emotions, like angels and elves, are not always apparent and their nature is subject to much debate. The most basic concepts remain unsettled including the very name of the emotion associated with laughter. Is it amusement, enjoyment, merriment, happiness, hilarity or glee? Apparently not. Ruch (1993) uses the word exhilaration, Panksepp calls it social joy (Panksepp and Burgdorf 2003), Martin (2007) prefers mirth while Chafe (2007) argues for nonseriousness.

None of the suggested terms completely captures the feelings associated with laughter and each term has its shortcomings. As Martin (2007) notes, Ruch’s choice of exhilaration has not caught on among researchers. Martin’s own choice of mirth has the advantage of not being overexposed in daily speech but for the same reason it has an archaic, dusty aspect, unchanged since Shakespeare’s *Taming of the Shrew*: "And frame your mind to mirth and merriment, Which bars a thousand harms and lengthens life." Chafe’s nonseriousness also drags in unfortunate notions including the preconception that laughter and seriousness are incompatible. There is much evidence to the contrary. For instance, laughter seems to be compatible with the seriousness of bereavement (Keltner and Bonnano 1997).

Mirth and other positive words exclude negative emotions. It is not clear
whether there could be a term that captures all of laughter's emotions, which are not only feelings but also might be causes and consequences of laughter. Indeed, there is no agreement on what emotions are. Barrett (2006) suggests that emotions are not natural kinds at all but are emergent features of the mind. According to this view, emotions emerge from interacting systems. On the other hand Chafe (2007, 66), challenging Barrett, favors the idea of emotions as natural kinds despite inconsistencies in the associated overt behavior. Researchers are only beginning to experiment with emotional mixtures that include positive and negative aspects such as amusement and disgust (Schimmack 2001, Hemenover and Schimmack 2007). They have also recently constructed the emotion of "elevation". In the context of this discussion of laughter, it is noteworthy that when studying elevation, the experimental group might watch elevating videos while the control group watches amusing videos (Haidt 2006, 196). So elevation apparently is not amusement, but it is too early to say how elevation relates to exhilaration or other proposed laughter emotions.

Is laughter a subdivision of humor? Laughter may have evolved before language and be quite separate from what we now call humor (Gervais and Wilson 2005). For Martin, however, one conception places humor as the broader concept that contains its physical manifestation, laughter. At the beginning of his comprehensive review of the psychology of humor, Martin (2007, 5) divides humor into 4 components: social, cognitive, emotional and behavioral. He also occasionally uses the word "humor" to refer only to the cognitive component. Laughter fits into the behavioral component, which in turn he divides into laughter and smiling.

According to this conception, humor is the more general class, laughter the more specific. Laughter is the visible and audible behavioral expression of humor. Some responses to humor, however, such as silent appreciation or a
groan or grin absent a smile or laughter, are not an explicit part of this ontology. Laughter not associated with humor is also excluded.

Perhaps the problem then is that the notion of laughter is too broad to be categorized. Various species of the animal we call laughter may be as distinct as elephant and earthworm. Is it sensible for the laughter researcher to view the elephants and earthworms of laughter through the same lens that sees them as just a bunch of animals? Forms of laughter include chuckling, chortling, giggling, guffawing, hooting, howling, roaring, snickering, sniggering, tittering and twittering. Perhaps we must be more specific.

Humor also has been classified into many types including anecdote, blooper, buffoonery, caricature, cartoon, clown, comedy, foolery, gag, irony, jest, joke, malapropism, mockery, parody, pun, quip, repartee, riddle, ridicule, quip, sarcasm, satire, teasing, wisecrack, wit, witticism, and wordplay among others.

It is difficult to separate these notions from humor techniques that include absurdity, ambiguity, exaggeration, irony, mimicry, mockery and nonsense. Using a blunt scalpel to carve the laughter world at its joints, we might consider many of these labels to be superfluous. But consider this reminder of the importance of fine distinctions:

"A huge psychological chasm separates a wisecrack from a witticism. The difference between them is the difference between a dart and a firecracker. A wisecrack conveys scorn and is usually soon forgotten. A witticism explodes-- radiates, endures; it remains quotable because, unlike a wisecrack, time does not enfeeble its relevance or blunt its humor" (Rosten 1994).

To the extent that such distinctions do reflect psychological chasms, it is not clear what is being reported in research that purports to study the effects of something broadly called "humor" or "laughter".

Laughter can occur in response to a range of stimuli, none of which are
necessary and many of which are insufficient to trigger laughter. Some of the sparks that can ignite laughter include conversation, tickling, teasing, embarrassment, anxiety, nervousness, contagion, direct brain stimulation, ignorance, deception, ingratiating, derision, jokes, cartoons, puns, drugs, sheer absurdity, nonsense and the aptly named laughing gas. Gervais and Wilson (2005) prefer the three-legged expression "nonserious social incongruity" for the sudden unexpected change that triggers spontaneous laughter, however stimulated (by tickling, jokes etc.). They consider nonserious social incongruity to be an umbrella term covering non-human primate laughter as well as infant and adult human laughter.

Laughter can also be sliced across a continuum of values, say positive and negative, good mood and bad e.g. mirthful, joyful, playful, happy, delighted, excited, pleasing, or bitter, contemptuous, derisive, disdainful, ironic, sardonic and sad laughter. These values are all highly contextual.

Laughter is an everyday word. The imprecision of the everyday is not usually sufficient for science or philosophy. Although there is no technical equivalent of the everyday word "laughter", laughter-related technical terms exist depending on the discipline and the purpose of the language. In medicine, for instance, various clinical terms describe pathological laughter, such as gelastic epilepsy (seizures accompanied by laughter or crying), gelotophobia (fear of being laughed at), risus sardonicus (smile-like facial spasms), kuru (laughing death or laughing sickness), cataplexy (sudden weakness), among many others (for descriptions of pathological laughter and its consequences see Provine 2000).

"Duchenne laughter" has become the hot technical term for genuine laughter or laughter-that-matters-in-experiments. Duchenne laughter and smiling are distinguished by contraction of particular facial muscles, those that raise the corners of the mouth (zygomatic major) and wrinkle the skin
around the eyes (orbicularis oculi). The distinction between Duchenne and non-Duchenne laughter opens the door to various dichotomies for expressing what underlies the observed facial movements, including genuine versus fake, natural versus artificial, authentic versus forced, innate versus learned, controlled versus uncontrolled, spontaneous versus voluntary, accidental versus deliberate, emotional versus emotionless, conversational versus humor-driven, natural versus artificial, felt versus unfelt, together with their varied connotations.

Gervais and Wilson (2005) say that spontaneous versus not spontaneous is the wrong pair, because some non-Duchenne laughter appears to be spontaneous but, curiously, is still volitional. Gervais and Wilson define non-Duchenne laughter as "spontaneous conversational laughter that occurs in the absence of attempts at humor" (2005, 401). The significance of the Duchenne category is not clear. Russell et al. (2003, 337) note, for instance, that Duchenne smiles, "may simply be more intense smiles."

5.3 Surrogates for Laughter Theory

Gigerenzer (1998) claims that almost anything passes as theory in psychology and sets out what he calls surrogates for theory, namely, "one-word explanation, redescription, drawing vague dichotomies, and data fitting." One-word explanations are labels with "no underlying mechanism or theoretical structure." He selects three labels from the heuristics and biases literature that he has frequently attacked. "As long as they are plausible and remain unspecified, they are hard to falsify. And if one has three to choose from-- such as representativeness, availability and anchoring and adjustment-- at least one of them can 'account' post hoc for almost any
phenomenon” (1998, 197). Redescription is exemplified by the "dormitive property" or "soporific" invoked by Molière's comic doctor to explain why opium induces sleep (and which has long been invoked by philosophers, including Friedrich Nietzsche: see Beyond Good and Evil, section 11). Dormitive means it makes you sleep, so without further discussion of how opium works, the explanation is merely that it makes you sleep because it makes you sleep. In the laughter literature we see all of Gigerenzer's surrogates for theory. Some are even conjoined. For instance, labels such as "nonseriousness" provide one-word explanations and redescriptions. Binary thinking includes the many pairs cited earlier such as genuine or fake laughter. Data fitting has been less evident in laughter research only insofar as mathematical and statistical techniques have been less commonly used.

Saying what laughter is remains no easier now than when the Elizabethans with whom we began considered the problem almost half a millennium ago. Should we give up, have a good laugh, talk about something more serious or keep muddling along? What these problems suggest is the need for consensus so at least we're discussing the same things. For laughter researchers, agreed standard descriptions of laughter and its network of associated notions could help to move our understanding of laughter to the next stage.
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6. If Laughter Is A Placebo, How Can We Know?

Previous chapters have pointed out that placebo is fundamental to the randomized placebo-controlled clinical trials so highly valued in evidence-based medicine, and in the case of laughter, have hinted at the possibility that laughter acts as a placebo. The word placebo, however, was used as an ordinary term, without close scrutiny. But given the extent of controversy over the history and philosophy of placebo, in most uses it would be better to substitute the inelegant expression "so-called placebo". This chapter explores the so-called placebo and whether we can know if medical and health effects of laughter are placebo effects.

The following discussion combines two distinct threads. The first thread begins with links between laughter and placebo then describes the many definitional problems and puzzles associated with placebos. This thread shows that the placebo construct is so problematic that it should be abandoned. It follows then that nothing should be labeled as a placebo. Other language should be used. For instance, everything in medicine has a context and a meaning within which sometimes we find laughter. From this viewpoint, instead of being an oddity at the periphery of medicine, the nature of context and meaning is at the heart of medicine. This conception conflicts
with the prevailing biomedical paradigm. Most research has been conducted under the less radical assumption that the placebo construct has some value. The second thread examines laughter, medicine and health in a world that no longer believes in the notion of placebo.

6.1 Laughter as Placebo

To associate laughter and placebo is nothing new. For instance, the laughter-as-medicine advocate Norman Cousins admitted that not only the laughter but everything he did in his self-cure saga, *Anatomy of an Illness as Perceived by the Patient*, in which he claimed to have cured himself with laughter and Vitamin C, might have been a demonstration of the placebo effect. But he also said, "Such a hypothesis bothers me not at all" (1979, 45). More recently, according to Provine (2000), laughter may act as a placebo or distraction:

> Given our present state of knowledge, laughter would deservedly be banned from the marketplace if it were a drug with unknown side effects. However, the advantageous cost/benefit ratio of laughter is such that there is no need to await FDA approval before giving laughter programs a try... the potential downside of laughter is small indeed, particularly if elected by patients. The promise of improved mood and quality of life without notable negative side effects is reason enough to implement experimental laughter or humor programs in health-care settings, even if welcome relief is provided only through placebo or distraction-- there are worse outcomes than providing entertainment to patients desperately in need of it (2000, 208).

Both laughter and placebo involve translating ideas into physiological responses. A funny idea is somehow translated into the gasping and grimacing called laughter. Similarly, in common conceptions of placebo, an idea about the medical benefit of an inert treatment is somehow translated
into a medical response. With both laughter and placebo, symbols become responses that are not entirely in the participant’s control. Another general relationship is the location of laughter and placebo at the boundary between the humanities and natural science. Laughter and placebo both involve culturally conditioned stories that are difficult to pin down, dissect and measure with the methods of science.

Specific clues also link laughter to placebo. Laughter and placebo have been used to treat similar conditions. Among the many claimed health benefits of laughter that have been the subject of research, pain relief shows the most consistent results. Placebos are also strongly associated with pain relief. Placebos appear to be useful for treating depression, which also has a relationship to laughter. Suggested processes underlying placebos, such as expectation, conditioning and endorphins, also parallel those for laughter.

6.2 Looking for Placebo

It sounds like a simple detective work: get a description and see if it matches the accused. We could simply stand them side by side and see if laughter is a placebo. Then if laughter were indeed a placebo, we could benefit from the wide-ranging research into placebo and its effects. For example, Bausell (2007, 275) reviews many popular forms of complementary alternative medicine (CAM), with a massive share of the medical marketplace such as herbal medicine, chiropractic, homeopathy, hypnosis and acupuncture, and concludes that "CAM therapies are nothing more than cleverly packaged placebos. And that is almost all there is to say about the science of CAM." He does not look at laughter medicine. Could we do a similar analysis for laughter to see whether laughter perhaps is nothing more than a humorously
unpackaged placebo? This case cannot be solved easily. Laughter, medicine, health, placebo, and the standards of evidence needed to connect them, are all elusive characters that make this case anything but simple detective work.

To determine whether something is a placebo requires some precision about what a placebo is. The paradigmatic placebo is the fake pill that resembles treatment, is described as effective, and that mysteriously is so. Somehow the mere description determines its potency. Placebos described as costing $2.50 were more effective analgesics than the same pills described as discounted to 10 cents (Waber et al. 2008). From a systematic review of 12 studies, de Craen et al. (1996) concluded that color influences the effectiveness of drug formulations. Red, yellow and orange are perceived as stimulants, blue and green as tranquilizing, although not consistently. Of course a pill is just one form of placebo. Sham surgery is another part of the puzzle: a mere slice through the skin can be as effective as deeper surgery (Moseley et al. 2002). The bigger the pill, or the more invasive the procedure, the more effective it seems to be: painful injections and surgery work better than less impressive interventions (Ernst 2001). There are countless other examples of the strangeness of placebos.

Apparently almost any treatment can be a placebo and a wide variety of medical conditions respond to placebo. Even something that works for other reasons, such as an active drug, can act like a placebo. That is, placebos are not even needed to generate placebo-like effects. For instance, secretly injecting morphine appears to be less effective than doing the same thing and telling the patient about it (Colloca and Benedetti 2005). Separating out these observations is problematic. Effects in medicine are rarely linked in a linear sequence to a single cause. A patient taking a drug may get better or worse despite the drug, or with the help of the drug, or from what are called
placebo effects, or for some other reason, or from all of these (Kaptchuck 1998).

Defining placebo is like taking a placebo-- almost any definition seems to work but only if you believe it does. According to one of the most influential authorities on placebo, the placebo is, "the only treatment common to all societies and all cultures ... its effectiveness has been attested to, without exception, for more than two millenia" (Shapiro and Shapiro 1997a, 1). The Shapiros famously tried to pin down various legalistic definitions of placebo, one of which is:

A placebo is any therapy (or that component of any therapy) that is intentionally or knowingly used for its nonspecific, psychological or psychophysiological, therapeutic effect, or that is used for a presumed therapeutic effect on a patient, symptom or illness, but is without specific activity for the condition being treated. (1997a, 41)

This attempt suffers from the same problems as many legislative definitions composed of abstract terms and multiple disputable subordinate clauses. Many seemingly unrelated terms are used in discussing placebos including inert, fake, real, non-specific, expectation, belief, hope, anticipation and transference (Wall 1996). Ernst (2007) lists numerous terms that he says confuse the placebo story, including doctor-patient relationship, context effect, non-specific effects, incidental effects and Hawthorne effect. And as if defining placebo now is not problematic enough, the concept like so many others in medicine changes over time (Kaptchuck 1998, Harrington 2006). Gøtzsche (1994) writes, "I have tried to define placebo in an unambiguous, logically consistent, and testable way, and I have failed" (1994, 926). Not only has he failed, but he claims that nobody can ever succeed in the quest for a consistent definition.

Placebo is such a loaded term that researchers frequently suggest abandoning it for other terms such as "non-specific effects" or "context effects"
An alternative move adds more precision, so that for instance in controlled trials there would be something called a "placebo effect maximising group" (Hróbjartsson 1996). Ernst and Resch (1995) add precision by subdividing the notion into "true" and "perceived" placebo effects. Moerman prefers “meaning response” to "true placebo effect" (Moerman 2003). Many authorities distinguish "placebo effect" from "placebo response" to emphasize the patient response and to distinguish generally observed improvement in the placebo group from just that part of the improvement attributed to placebo (but see Benedetti 2009, 32 who suggests using effect and response interchangeably). Or in a pragmatic turn, placebo can be defined as whatever researchers say it is: "We therefore defined placebo practically as an intervention labeled as such in the report of a clinical trial." (Hróbjartsson and Gøtzsche 2001).

A pragmatic approach, that has often been applied to undefinables, including beauty and obscenity as well as placebo, is to use examples as definitions: "Rather than getting bogged down in arguing with other authors about definitions, I prefer to define through example. Although it may be difficult to say exactly what the placebo effect is, like many other phenomena, its students can certainly recognize it when they see it" (Frenkel 2008). Frenkel provides a phenomenological account of the body responding to meaning without need of concepts or language. Yet how are we to be sure that students of placebo phenomena recognize and see the same "it"? Miller and Rosenstein (2006) shift the problem sideways with a "typology of modes of healing" that separates "placebo-induced healing" from "healing produced by clinician-patient interaction." Benson and Friedman (1996) propose the expression "remembered wellness." Louhiala and Puustinen (2008) advocate the term care effect. But no formulation is immune from criticism. Keinle and Keine (1997) among others remind us that "non-specific effects" is a
contradiction in itself (see the discussion of placebo puzzles below). They take the view that changing words is not the solution. They cite multiple factors and subfactors that can lead to false impressions about placebo effects.

Absent a tractable definition, or even a manageable group of words to begin our inquiry into placebo, how can we know if a medical intervention is a placebo? The Wittgensteinian may say that placebos are a family, that there are no necessary or sufficient conditions for membership in the family, yet we recognize them by their family resemblances. The pragmatist may claim there is no problem, that what matters is what works, not what we call it. We do not need to know everything about it to use it. Whether something is a placebo or not is irrelevant to its use. Just try and see. If the pill works, swallow it. Such a carefree, knowledge-free view is frequently offered, particularly by clinicians who need practical solutions for their patients and are wary of abstract philosophizing. For present purposes, the absence of a tractable definition is a strong hint that the whole problem needs to be recast.

6.3 The Performance Effect

Imagine that you are a stranger visiting this world. You happen upon a small crowd watching a man dressed in black. As you study the faces in the audience, suddenly they burst into smiles and laughter. Then they quietly watch again. Then more laughter. Then quiet. This cycle of attention then explosive reaction then attention happens several times. What is this strange performance effect?

From your viewpoint standing behind him, what he does is obvious. He merely hides a coin in the palm of his hand. The audience reacts. To be exact, not everybody laughs equally loudly or frequently. Some members of the audience could be deemed to be performance responders who are particularly influenced by the performance effect. Of those who succumb to the performance effect, why do they react? From the viewpoint of the audience, the magician actually made the coin vanish. Of course they react. To someone who does not understand the meaning of the magician’s words and actions, however, the performance effect is mysterious. Viewed only mechanistically, the crowd gasps and claps for no apparent reason. Why does showing them the ace of hearts create a performance effect? Why do they fail to react to the ace of spades? No, the hypothesis that the ace of hearts has special powers turns out to be false. The ace of hearts was selected by someone in the audience then replaced in the deck. They expected the ace of hearts to be somewhere in the shuffled deck, so they laughed when the magician extracted that very card from the ear of a child in the audience.

What do we gain from constructing the notion of a performance effect? Nothing, and much of what is happening is obscured by the notion of a performance effect and related notions such as audience responders.

The physician-magician with a magic inert pill seems to induce a mysterious placebo effect in the patient-audience. On a different conception of the patient, however, the patient is not a mechanism. The patient is a conscious person who takes meaning from the whole sickness story, with its plot including diagnosis, prognosis, treatment and rehabilitation and its emotions associated with worry, pain, suffering, uncertainty, concern and reassurance.

The mechanisms may vary, as do the magician’s tricks. They may seem mysterious and unexplained. The professional entertainer may not know for
certain why some jokes and illusions get better laughs than others. Full knowledge of the exact mechanism is not required to make the audience laugh or to improve the patient’s health. But that does not mean there is a general class of mechanisms attributable to performances or placebos. Neither performances nor placebos have been fully explained, yet only one, the placebo, has been isolated as a special effect.

Placebos carry baggage, of mystery, deception, fakery, quackery and other negative connotations. Yet put positively, everything has a potential meaning. The person’s interpretation of events, the meaning, has medical and health consequences. Any pills, including sugar pills, have meaning to some people. And the bigger the pill, the more expensive, the more power they are meant to contain. The big name performer and the big pill may have big effects for similar reasons.

The frequent suggestion that the word placebo be replaced by some other expression assumes that placebo is something worth preserving. But using a similar construct expressed in different words leaves us trapped within the same problematic biomedical partitioning of the organism from its context.

6.4 A Plethora of Placebo Puzzles

Placebos are inherently puzzling. They are apparently effective yet have no effective content. A placebo seems to produce an effect when we don’t expect a cause. If we expect a cause, we don’t call it a placebo. For instance, among those who expect that some day scientists will figure out how acupuncture works, the nature of acupuncture is just another medical puzzle that will eventually be solved. Despite the mystery, they believe acupuncture works. Some may even claim that they know acupuncture works. They don’t say
acupuncture is a placebo. For those who don't expect that acupuncture will ever be shown to be the cause of its purported medical benefits, acupuncture is a placebo. They don't believe acupuncture works. Some may say that they know acupuncture doesn't work. They say acupuncture is a placebo. Placebo is a creature of expectation and belief. So the question, "how can we know if something is a placebo" may not be answerable.

It is surprising to see references in the literature to The Placebo Paradox, as if there is only a single paradox or puzzle associated with placebos. The Placebo Paradox, according to Cave and then Clark (Cave 2001, Clark 2002), arises from the question whether you can ground a belief in a belief. Here is one statement of this ancient problem:

Although it may be true that this pill will cure me, and also true that it will cure me only because I believe it will, I cannot believe that it will cure me only because I believe it will. (Clark 2002, 136).

It is logically impossible to pull a belief up by its own bootstraps, goes their argument. Saying I believe because I believe cannot pick me up logically or, if sick, clinically. In their view, a belief has to be in something. A belief that a placebo works because of that very belief is belief in nothing. Something outside the belief itself is necessary, they say, such as some property of a pill that makes it work. Such a property avoids a regress of belief in belief in belief ad infinitum. If the patient is aware that there is no working property apart from belief in the placebo, support for the belief is gone.

What of reports that a placebo can sometimes work even if the patient knows it is a placebo (for instance Cassell 2004, 113; Egeth 2009)? Their response would be that the belief is still in something, say the pill, the physician or the process that surrounds the taking of the placebo, which is sufficient grounds for the belief that makes it work. There's always another explanation, such as: the patient was actually deceived but didn't know it, or
didn’t believe it was a placebo, or simply didn’t understand. Or maybe the patient believes that strong beliefs, hopes, and expectations are health-inducing, no matter how those beliefs may arise.

The Placebo Paradox ignores a plethora of related placebo puzzles. Whereas the type captured by Cave and Clark dwells on belief, others emphasize emotion, values, knowledge, ignorance, purpose and experimental methodology, among other attributes. All are related to belief but each has its own twist.

What follows is not an exhaustive list of placebo puzzles. In this discussion, puzzles are broadly construed to include that which is contrary to expectation, or beyond belief, or just puzzling, ironic, enigmatic or surprising and that helps to delimit the problematic placebo concept. Here, as mathematician T.H. O’Beirne put it, puzzle or paradox can mean "something whose truth and explanation can ordinarily be established only in the face of some initial sales resistance" (1965, ix). There are so many related placebo puzzles and problems, as to leave the notion of placebo itself, well, simply inert. Now to the placebo puzzles:

A threshold metapuzzle exists about the existence of placebo puzzles. No placebo puzzle can arise without the notion of a placebo. If you do not believe that there is such a thing as a placebo, you cannot believe in a placebo or in your belief that it will work. All other placebo puzzles vanish without belief in the existence of placebo effects. This is not simply a case of there actually being a placebo effect but not believing it. This is not a puzzle of the form "it is raining but I don’t believe it" (often attributed to G.E. Moore). There simply is no placebo puzzle if there is no such animal as a placebo effect. Kienle and Kiene (1997) have suggested as much. They ask whether the placebo effect is fact or fiction. They say the received view of Beecher (1955) that placebos are powerful fails to account for other explanations, including natural history of
disease, regression to the mean, additional therapy and misquotation. Hróbjartsson and Gøtzsche (2001 and 2004) examined trials that compared placebo groups with untreated groups, and have provided strong evidence that placebo power is illusory, but for a small effect on pain that could not be distinguished from bias. After re-analyzing some of the same reports, Wampold et al. (2005) reasserted the power of placebo as "robust". Hróbjartsson and Gøtzsche (2007) countered that Wampold et al., "put powerful spin on their conclusion despite similar results as the original review." Meissner et al. (2007) also examined some of the same trials and suggest that placebos can improve "physical parameters" (in diseases such as hypertension and asthma) but not "biochemical parameters" (in diseases such as diabetes and hypercholesterolaemia). Benedetti (2009, 29) says of the the meta-analysis by Hróbjartsson and Gøtzsche (2001) that, "now most researchers do not take it very seriously" given the broad inclusion criteria and omission of critical factors such as expectation and context.

Assume for now that there are grounds for believing in placebo effects (as for instance does Ernst (2007) who believes they exist based on real and sham acupuncture studies among others). One puzzle lurks in the definition of placebo as something inert that has an effect. Or something effective that is inert. That doesn't seem to make sense of either the word inert or the word effective. Similarly, to define a placebo as something with non-specific effects seems strange. If you can't specify the effects, then how do you know what they are? On the other hand, if you can specify the effects, then they're specific effects.

The Placebo Test Puzzle has subdivisions that might be termed the Who-Watches-The-Watchers Puzzle. Placebos are often the controls against which drugs are measured in clinical trials. But there are no standard tests to determine if they are placebos. What's more, although experimenters go to
great lengths to eliminate bias in drug testing, there's a loophole for placebos. The drug companies that have a stake in the results may also be involved in determining the content of the so-called placebos. So-called, that is, because finding an inert substance is something like finding the universal solvent or the immovable object. For instance, to the extent that sugar has some effects, sugar pills are not inert. The same is true of many other commonly used placebos. Golomb (1995) writes, "It is paradoxical that there is no standard of evidence to support the standard of evidence." To show that a placebo is a placebo, would you compare it with a known placebo? Then you'd have to have tested that placebo to prove that it is a known placebo, and so on, down into the abyss of an infinite regress.

Why not compare a placebo against a no-treatment group? This question introduces the No-Treatment Puzzle. In practice, such comparisons are done (e.g. Hróbjartsson and Gøtzsche 2001). But no treatment is not the same as doing nothing. Participants who do not receive treatment are nevertheless participants in an experiment who, unlike the person on the street, must consent, be tested, know they are being tested and so on. Those interventions, while not being drug treatment or surgery, are far from doing nothing. In short, there will be expectations associated with the no-treatment group. Expectations can be effective. You can't study someone inertly: "except under the most extraordinary circumstances it is logically and conceptually impossible to have a no-treatment group" (Moerman (2002, 26); see also Hróbjartsson (1996); Jopling (2008, 125) says Moerman's concerns are too absolute but pragmatically relaxing the requirements for a no-treatment group does not change the conceptual problem). In addition, for medical conditions that have an effective treatment, withholding that treatment and using a no-treatment group instead would be unethical. So studies that do have a no-treatment group would probably be comparing an ineffective
treatment with a placebo. Which brings us to the next set of puzzles.

Puzzles also appear in moral and ethical concerns about placebos. A major ethical question, and the source of a raging debate, is whether placebos should be used at all, since those who get the placebo are deprived of other treatments. A goal in clinical trials is to reduce the number of participants who get ineffective treatment, and not incidentally to reduce the cost of trials. One route is simply not to use a placebo but instead to do an active trial comparing two active treatments. But the distinction between placebo controls and active controls rests on shaky ground (Howick 2009, Nunn 2009b). Leon (2002) has pointed out what he calls a Paradox of Power in these circumstances. The difference between two active drugs is likely to be smaller than the difference between an active drug and a placebo. More participants are needed to detect a small difference than a large difference. Bigger trials using active drugs mean more participants on the losing side of the experiment, more people who don't get better and who do get adverse effects. So avoiding placebos can actually result in the very problems attributed to using placebos. This puzzle is part of the general ethical question of the extent to which resources should be devoted to a proliferation of competing drug brands (see also Weiss Roberts et al. (2001) for what they call paradoxes in relation to placebos and psychiatric research).

A puzzle that has been named Philip's Paradox represents a challenge to blinding or masking in clinical trials (Ney 1986). Trial participants often guess accurately whether they are receiving a placebo. They are not easily fooled (some may even have their pills analyzed and then drop out of a study if they're getting the placebo). The more potent the intervention, the more apparent are the effects. Creating an active placebo that has similar effects to a potent drug is almost impossible. Philip's Paradox recognizes this problem: the more potent the intervention, the less likely it can be blinded and the
more difficult it is to claim that the trial was actually blind. Drugs that act like placebos are more easily masked but the point of a trial is to see if they are different from placebos. Drug manufacturers want their drugs to be detectably different from placebo. So failure to blind does not necessarily mean failure of the drug to pass the test but could mean the opposite, that the trial showed a detectable difference between drug and placebo. Can this puzzle be dissolved by comparing a treatment with no treatment instead of comparing treatment with placebo? No, then we’re back into the No-Treatment Puzzle and the different expectations associated with no treatment compared to an active treatment or to the untested person on the street.

A related puzzle, or mere irony, is that drug companies tout products that have fewer side effects. That is, fewer side effects are good. But drugs, and placebos, with more side effects work better. This could be called the Effective Side Effect Puzzle.

It is worth noting that blinding is an issue of limiting knowledge, but not necessarily belief and expectation. Double-blind trials use various methods to prevent researchers and participants from knowing what intervention is allocated to each participant. Still, everyone has beliefs and expectations, notwithstanding their knowledge. They believe and expect various things about their moment-to-moment existence, some of which may be based on knowledge, some of which may be based on feelings, intuitions, misapprehensions and many other grounds. To the extent that beliefs and expectations influence outcomes, blinding does not erase or equalize mental factors in clinical trials. Blinding, like randomization, has become such a dogmatic standard, formalized in the double-blind randomized trial, that it is easy to forget that neither methodology has magical powers to generate meaningful experimental observations (Benedetti 2007, Worrall 2007).
The Efficacy-Effectiveness Puzzle concerns the difference between what works for participants in clinical trials (efficacy) and what works for actual patients in clinical practice (effectiveness) (see also Ashcroft 2002 for different interpretations of the word effectiveness). The more tightly controlled the trials, the less representative they are of clinical practice. Clinicians try to increase the placebo effect while in clinical trials the placebo effect is something to be minimized like noise interfering with music (Ernst 2007). For those who think placebos are noise, and only specific non-placebo effects are legitimate music, the logic is that the treatment must be louder than mere noise. For those want to use the placebo effect itself in clinical practice, the noise of the placebo is music. It's hard to test where specifics end and non-specifics begin, or appearances and non-appearances for that matter (see for example Caspi and Bootzin 2002 and Grünbaum 1985). But it's easy to see that clinical trials that exclude aspects of clinical practice such as the relationship between physician and patient (by methods such as blinding) will exclude evidence that those aspects have effects. Is there a case for admitting some interventions that don't pass the test of experimental efficacy in a lab but do appear to be effective in the real clinical world, like the student who fails exams but succeeds in life? Yet absent a passing grade, we would not know, unless we redefined knowing to include something other than passing the test of experimental efficacy.

A related puzzle is the Individual-Group Puzzle. It is not limited to placebo-controlled trials but is inherent in the statistical approach of biomedical science. Clinicians treat individual patients. It is puzzling that groups in clinical trials are measured to provide knowledge about treatments for individuals. Knowledge about other people in other circumstances is the basis for treating this individual in these circumstances.

The Comorbidity Puzzle or Multiple Disease Puzzle is a subspecies of this
puzzle. Experiments are often designed to test one treatment for one disease. Typical patients, on the other hand, more likely have multiple problems and receive multiple treatments. If relatively healthy patients with one disease are the subjects of clinical trials, then the effects may be overestimated. But if typical patients with many diseases are included in clinical trials, the experimental results may be intractable.

Then there is the Mystery Puzzle or Ignorance Puzzle. This variant, focusing on knowledge, is distinct from Cave and Clark's Placebo Paradox which focuses on belief. Consider the empowered patient who wants to be a partner with the physician, not a passive recipient of medical paternalism. The patient is all fired up with knowledge after surfing the Internet, seeking every opinion, hoping to distinguish quackery from efficacy. Has this informed patient, by trying to penetrate the mystery, foreclosed the opportunity to benefit from placebo effects? Knowing spoils the experience, like reading a review that gives away the plot before you read the book or see the film. The more mystery, the more ritual, the less questioning or searching for evidence, the more powerful the magic. Questions make the illusion collapse. As Hans Christian Andersen demonstrated long ago in a different context, if it weren't for a child's questions, the Emperor would still be fully clothed (or in our present case, the Emperor would be cured by any new potion). To ask if the Emperor has clothes is to remove them. More than that, not to see the invisible clothes is to prove that one is too stupid or incompetent to see what everyone else pretends to see. The less is known that could interfere with belief, the more effective the placebo. If this is correct, then it can be a good idea not to do anything to disturb the powerful lack of specific knowledge that powers the placebo (but see for instance Egeth 2009 who speculates about benefits even if patients know they receive placebo treatments).
The Ignorance Puzzle can also be stated as a Trust Puzzle. A physician who inspires trust may have better results but a fully trusting patient has little motivation to learn more by seeking advice, getting another opinion or searching the Internet. Knowledge is power but ignorance and trust are placebo power. Ignorance need not be restricted to patients. Willful ignorance in physicians may violate various rules, including legal and moral obligations to be competent, but those are additional issues that do not change the fact that ignorance can be powerful.

The next placebo puzzle we might call the Enthusiasm Puzzle or Optimism Puzzle. The more enthusiasm surrounding a placebo, the more likely it is to work. Conversely lack of enthusiasm can weaken the magic. But enthusiasm is also likely to be accompanied by less desire to scrutinize the basis for the enthusiasm. Enthusiasm and skepticism are sleepless bedfellows. The enthusiast shouts, it works! The skeptic wonders how and why it works. The enthusiast says ours is not to reason why. Who cares why it works? If it helps the patient, that's the important thing! Knowledge for the enthusiast is secondary. This is not a puzzle only of belief, or even of strength of belief. This is not just about reasons. The enthusiast and the skeptic may both believe that the placebo works sometimes. But they have different emotional temperatures and viewpoints. Say they agree that it works for one person in three. The enthusiast sees the one person who gets better while the skeptic sees the other two who don’t. Enthusiasm and optimism, in addition to belief and knowledge, may influence whether the placebo turns the patient into the fortunate one, or into one of the two less fortunate others.

Another variation is the Purposeful Placebo Puzzle. This species focuses neither on the physician nor on the patient but on those who exhort physicians to use the powerful placebo. Placebos could be much less trouble and would be much less expensive than active remedies. According to this
view, the Emperor should continue to model his invisible fashions for the benefit of those who can see them. All that matters is that the placebo works for some patients. But here's the puzzle: if you urge physicians to use the placebo because you believe it works, then you don't believe it is inert. So then how can you call it a placebo? Or if you prefer to call it a placebo because the mechanism is unknown, then how does that differ from other interventions with unknown mechanisms (which includes much of medicine) that are not called placebos?

Or suppose you prefer a different definition of placebo, not based on being inert but on being non-specific. Say we accept Evans's hypothesis that placebos are most useful for conditions related to the immune response, such as pain and swelling, and that placebos can deliberately be made more effective with strong doctor patient relationships, rituals, touching, and various other known inducements (Evans 2005). If you urge physicians to use a placebo for its specific power over specific conditions known to respond to placebos, then you have turned the non-specific remedy into something specific and not a placebo (see also the Placebo Definition Puzzles).

The Placebo Expertise Puzzle is related to the Purposeful Placebo Puzzle. Evidence-based medicine shifts the focus from the physician's personal experience and expertise to the impersonal statistical averages reported in medical journals. A general problem for medicine is that, "the more medical science does for disease, the less physicians seem to do for patients" (Spiro 1986, 28). Increasing reliance on technology and specialization limits the role of the individual physician, "Yet it is paradoxical that the more powerless the individual physician becomes, the louder the clamor for family physicians, for guides, for friends" (Spiro 1986, 29). So if medical experts were generally known to study and use placebos, would they still be considered medical experts, or just professional guides and friends? The Placebo Expertise Puzzle
challenges physicians to assess how much of medicine is science and how much is magic (see for instance Buckman and Sabbagh (1993, 248) who conclude that "Diseases need medicine, but human beings who suffer will always need a touch of magic").

Related to the Placebo Expertise Puzzle is the Placebo Career Puzzle or Placebo Funding Puzzle. Drug companies fund research and randomized controlled trials. But they don’t pay if they can’t profit. Who will fund study of placebos in themselves, not just as foils for the sale of pharmaceuticals? Absent funding, who will be the investigators? Hoffer (2003) puts the question succinctly in the form of a career choice: "What bright young researcher would choose to devote a scientific career to confirming the inefficacy of implausible treatments?" In a similar vein, Ernst (2009) notes that, "...CAM research is not generally considered to be a wise career move: trained researchers from other fields have little impetus to go into CAM research, as it is neither well funded nor academically rewarding." Although the placebo has been hailed as cheap or free medicine that deserves more research, that very fact limits placebo research.

The More Certainty from More Chance, or simply Unpredictability Puzzle is another feature of clinical trials, although not specifically dependent on the notion of placebo. A curious thing about controlled clinical trials is that the element of chance is deliberately introduced in order to compensate for the chance of bias. Participants are randomly assigned to the experimental intervention or control group, in the hope that uncontrolled factors will be evenly distributed and (together with blinding) so nobody can deliberately or unconsciously direct participants to a particular group (although critics such as Worrall 2007 reject these and other reasons for randomizing). The uncertainty of randomness is used to make the results less certainly biased. Kunz and Oxman (1998) frame this puzzle in these terms, "It is a paradox
that unpredictability is introduced into the design of clinical trials by using random allocation to protect against the unpredictability of the extent of bias in the results of non-randomised clinical trials.” They use the word unpredictability although it’s as much a problem of controllability as predictability.

There is a species of placebo puzzle in the origin of the human species. The Placebo Permission Puzzle comes from an evolutionary view of placebos, which will be further elaborated below. Although humans are self-organizing organisms, placebos demonstrate a need for external influences to effect internal repair. "Why should it be that we so often need what amounts to outside permission before taking charge of healing our own bodies?" (Humphrey 2002, 259). This puzzle is for those who do not challenge the very existence of placebos but accept that placebos exist, are external influences and have effects. On the other hand, it may not be possible to separate internal mechanisms that may be at work, always or at any time, from specific external influences.

Explanations for placebo, such as suggestion, expectation and conditioning, raise their own puzzles. Does the concept of suggestion similarly rest on similar ground? Borch-Jacobsen (2009, 109) says it does:

"Here is the paradox of suggestion: how can you induce someone to become passive (suggestible) if this passivity requires his prior acceptance? If he accepts, it is because he was already willing. But if he was willing, can we say that he passively executed a suggestion?"

These puzzles, paradoxes, contradictions, enigmas and ironies illustrate what difficult experimental subjects humans are. Consider by contrast the well-known agricultural experiments of R.A. Fisher that ushered in the era of randomized controlled trials. He could compare treated and untreated crops with relative certainty, unfettered by placebo problems or concern for the well-being of seeds.
Many influences including belief, expectation, knowledge, emotion, and of course the powers of politics and economics, bear on the notion of placebo. The more we want to know, not merely believe, about placebo, the less it seems to be. This is analogous to a Heisenberg Uncertainty Principle of the Placebo (see for instance Hróbjartsson 1996). The act of trying to pin down placebo makes it slip away.

These puzzles or this puzzle if you think they all stem from a concept that itself is inert, ultimately go to the fundamentals of the skeptical attitude of science and the evidence-based view of medicine as compared to other accepting, believing, enthusiastic, faithful and trusting attitudes. The more faith, emotion and belief, the less science, reasoning and evidence, the more life placebo seems to have. That is the case at least for those who believe in placebo. For those of us who say placebo is an illusion, the invisible cloth is perfectly clear (Nunn 2009a).

6.5 Penetrating the Placebo Puzzles

Science attempts to capture the overwhelming complexity of organisms by viewing them as mechanisms composed of parts. The parts are conceptually reducible to subparts all the way down to their biochemical and subatomic components. The person, a complex entity, has been partitioned into a mind and a body. The body has been further partitioned into separate complex entities such as the immune system, the nervous system and so on. Each of these conceptual parts has been further partitioned into subsystems and components down to individual cells and molecules. Physicians, researchers and other biomedical experts are trained to think in terms of these separate parts. They learn to become specialists in particular parts, although the
systems interact with each other in many ways and have no definite boundaries. This partitioning has been successful for understanding and treating specific diseases of specific parts. But placebos don’t fit neatly into any of the parts or specialties. The ubiquitous placebo straddles the division between mind and body, and every subdivision in every subspecialty all the way down. The narrowly conceived notion of a placebo as a curious artifact at the fringe of medicine is incommensurable with a broader view of the placebo as a phenomenon affecting an integrated organism.

Consider a sugar pill that relieves pain. Compare it with a pill that contains a drug known to alter specific biochemical pathways. The sugar pill is deemed to be the anomaly and subordinated to the biochemical mechanism. The drug and the sugar may not be using the same systems to relieve pain. But it is not entirely clear how either pill relieves pain. Patients do not all respond identically to any drug, just as they do not all respond identically to a placebo. The same uncertainty applies to most medical interventions. Say instead of looking at placebos as the anomaly that we were to look at the separate systems as the anomaly. Yes, partitioning complex organisms into manageable systems has been the source of great success for biomedicine. But the partitions are only conceptual. Immune systems and nervous systems are not separate from the organism. Some parts are more easily partitioned than others, of course. But even those parts are not the same when disconnected from other parts of the organism. As Cassell (2004, 36) argues, “Unlike other objects of science, persons cannot be reduced to their parts in order to better understand them.”

What do we gain by reminding ourselves of the artificial partitioning of complex systems? We are reminded to look for anomalies in the partitioning. For instance, we are reminded that a drug-pain partition is a conceptual convenience that subordinates a placebo-pain picture. A drug-pain view omits
the meaning of the treatment to the patient. So of course the sugar pill seems anomalous in a such narrowly partitioned model. Meaning has no place in a drug-pain model. But the placebo fits neatly into a meaning-drug-pain model or an expectation-placebo-neuroendocrine model or other increasingly detailed refinements. Such models straddle the partitioning of the person and remind us that partitioning removes relevant parts from consideration.

A model that includes meaning, not just mechanism, context not just components, is consistent with a recent shift in the focus of medicine from disease to the person (Cassell 2004, Moerman 2002). Cassell suggests that "meaning is the medium through which thought flows into body and the body flows into thought" (2004, 223). Using the metaphor of being "immersed in a sea of meanings" to describe the construction of meaning, he says, "We need this constant flow of information to order our lives and determine our actions from the next second to next year" (Cassell 2004, 246). The terminology shifts from scientific language of drugs causing effects to the phenomenological language of meaning for the individual. This shift entails crossing from the scientific, impersonal, objective, general and lawlike to the unscientific, personal, subjective, individual and idiosyncratic.

These dichotomies merge when we examine the origins of meaning in the everyday responses of individuals to choices in their lives. Meaning is not just an abstract philosophical notion. It is a practical matter faced by every organism. How does my predicament, illness or treatment fit into the meaning of my life? What should I do? How should I decide? What do I want next in my life? The answers to these questions and consequent actions depend on the meaning in the particular context. From the origin of organisms to the present, meaning matters. This conception suggests that evolution might have something to say about placebos.
6.6 Evolution and Placebo

The placebo has recently been examined within the nascent science of evolutionary medicine. The widespread observation of apparent placebo effects throughout history and across cultures has prompted many investigators to look for an evolutionary explanation. For instance, Shapiro and Shapiro (1997b, 31) conclude their overview of the history of the placebo with this question:

Does the ubiquity of the placebo effect throughout history suggest the possibility, popular but hardly testable today, or perhaps ever, that positive placebo effects are an inherited adaptive characteristic, conferring evolutionary advantages by reducing despondency, depression, and hopelessness, and that allowed more people with the placebo trait to survive than those without it?

Humphrey (2002) draws an analogy between the body’s self-healing capability and the National Health Service in his native Britain. He considers the placebo to be an emergent property of the body’s own managed health care system:

The human capacity for responding to placebos is in fact not necessarily adaptive in its own right (indeed it can sometimes even be maladaptive). Instead, this capacity is an emergent property of something else that is genuinely adaptive: namely, a specially designed procedure for "economic resource management" that is, I believe, one of the key features of the "natural health-care service" which has evolved in ourselves and other animals to help us deal throughout our lives with repeated bouts of sickness, injury, and other threats to our well-being." (2002, 261).

Resource management is another way of talking about deciding what to do next. What does the future hold for this person? Pain, for example, is not just something happening now. Pain clouds the future. Should effort be devoted to eliminating the pain? Should effort be devoted to something else despite the pain? Is the future doomed to be painful? What resources are needed to do the next thing? In some situations, for humans as distinct from animals,
resource management can mean deciding whether to live or die. Having big
brains with the capacity for self-reflection, humans can ask themselves
whether they're having enough fun and laughter to make life worth living.

Inspired by Humphrey, Evans (2003, 2005) also adopts the resource
management metaphor, although he differs in the details. Evans (2005) says
that, "There is evidence that pain, swelling, stomach ulcers, depression, and
anxiety are all placebo-responsive. These conditions have all been linked, to a
greater or lesser extent, with activation of the acute-phase response (the
innate immune response). The placebo effect may therefore be mediated by
alteration of one or more components of the acute-phase response." Evans
relies heavily on Wall's theory of pain as a need state which in turn relies on
the assumption that medicine has been around long enough that it is an
evolved social support mechanism (Evans 2003, 111).

Evans says placebos inhibit the innate immune response. This inhibition
view is opposite to that of Humphrey who sees the placebo as something that
releases resources. Evans says, "most, if not all, genuine cases of the placebo
response involve precisely the opposite-- the suppression of an immune
response, rather than its activation. And the kind of immune response that is
suppressed does not tend to involve antibodies and other aspects of the
acquired immune system, but rather elements of the innate immune
system"(2003, 117). So finding a placebo response not associated with the
innate immune would resolve these differences. Evans (2003, 117) also
distinguishes his view that placebo is triggered by belief that one has
received powerful medicine from Humphreys' reliance on the broader notion
of hope that help is on the way.

A common thread in these accounts is a model of optimizing limited
resources. Every organism must continually solve the problem of what to do
next and with what combination of resources. No theoretical consensus has
formed yet, however, and each theory has its merits and flaws, but
evolutionary theory does offer support for replacing the notion of placebo with
the context of decisions, conscious and unconscious, about what to do next.
Meaning and context of medical treatment influences the patient’s decision
about what to do next.

6.7 Placebo and Laughter Medicine

Taking a sugar pill may affect health through various mechanisms including:
• reduced stress of uncertainty
• positive emotional response
• social support
• conditioning from previous treatments
• decisions about health in general including diet, exercise, occupation,
  lifestyle, adherence to medical treatment (see for example Cassells 2004, 227ff).
This list is comparable to many of the proposed mechanisms for the health
effects of laughter (Mahony 2001 et al., Martin 2001, Zweyer 2004). All of
these mechanisms and more may be at work whenever a patient is confronted
with the decision what to do next. Moerman (2002, 133) situates the placebo
as a subset of meaning effects. But is anything gained by giving some
meanings the name placebo? No more than is gained by constructing a
performance effect.

We began with harmless placebos and hopeful laughter and have arrived
at their effect on pain and suffering. Is it surprising to connect laughter with
suffering? Not through the lens of meaning. Laughter and placebos are
catched in the subjective realms of belief and pain relief. Whenever the
discussion goes beyond the subjective, into the realm of mechanisms that do not depend on the subjective report of experience, many alternatives are available. Laughter is only a single and remarkably quiet sound among the symphony of alternatives.

The answer to the question how would we know if laughter is a placebo goes something like this: if we could resolve the controversy over the very existence and power of placebo, and could agree on a description of laughter and how to size up both placebo and laughter, then we might begin to apply what we know about placebo to see if laughter is one. Meanwhile we can only note that placebo, if this category means anything, and laughter, vaguely delimited, both appear to influence similar things, such as hopelessness, pain, suffering, sadness and depression. But that is only suggestive, not enough to solidify belief into knowledge or to replace a false belief.
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7. Serious Laughter

In previous chapters we encountered the often unstated assumption that laughter is positive and harmless, accompanied by the stated conviction that laughter should be promoted as medicine. For instance, as we saw in the chapter on story-based medicine, Professor Sir Kenneth Calman (2001) wrote, "Perhaps there is a need for a laughter clinic in all hospitals." In the discussion of placebos, which like laughter are often are considered to be risk free, we encountered Provine’s (2000) recommendation to try laughter programs in medicine:

"...the advantageous cost/benefit ratio of laughter is such that there is no need to await FDA approval before giving laughter programs a try... the potential downside of laughter is small indeed, particularly if elected by patients. The promise of improved mood and quality of life without notable negative side effects is reason enough to implement experimental laughter or humor programs in health-care settings ... (2000, 2008).

Researchers are now showing increased interest in the serious negative side of laughter, however. In light of the equivocal evidence connecting laughter and health (Bennett and Lengacher 2007, Martin 2001, 2004), they are re-examining the assumption that laughter and humor are positive. This could be the start of a serious negative turn in laughter, humor and emotion studies. The following discussion examines this turn, including the rare cases linking laughter and death, and related costs and risks of laughter.
7.1 Negative Laughter and Humor

Examples of the serious turn include investigations of negative humor styles, studies of laughter associated with mixed emotions and modifications to humor theory to include negative forms. Martin et al. (2003) identify aggressive and self-defeating styles of humor that could be damaging to health. Kuiper et al. (2004) recast the undifferentiated positive notion of a sense of humor into a more complex multidimensional conception that distinguishes adaptive from maladaptive forms. Keltner and Buswell (1997) consider whether embarrassment is a separate emotion. They point out the many unknowns about embarrassment, and for the purposes of this discussion, that includes unknowns about laughter, amusement and embarrassment:

The factors that govern the systematic relations between displays of embarrassment and observers’ responses of amusement, laughter, and sympathy need to be explored, as do the relations between observers’ anger and disgust and individuals’ embarrassment and shame (1997, 265).

Hemenover and Schimmack (2007) investigated mixed feelings of amusement and disgust. They showed 102 psychology students a movie clip “in which the main character (a female impersonator) eats dog faeces in a humorous manner.” Students were randomly assigned to watch the movie either as participants or outsiders. Participants were asked to imagine that they were the main character experiencing the situation in the movie. Outsiders were instructed to imagine that they had no connection with what they saw. It is hardly surprising that the researchers observed, ”After taking the perspective of the protagonist, participants felt less amused after the disgusting humorous clip than they did after taking an outsider’s perspective,
producing less intense mixed feelings in these participants" (Hemenover and Schimmack 2007, 1111). For present purposes, setting aside the methods and conclusions, this peculiar experiment illustrates the early state of current inquiry into a more layered understanding than the standard view that laughter is an unmixed positive behavior. We know little about how and why disgust, embarrassment, nervousness and similar seriousness may be accompanied by laughter.

As discussed in the chapter on evolution and laughter, proponents of evolutionary medicine have made claims about the health benefits of laughter. Evolutionary medicine also has something to say about the serious negative side of laughter. Nesse (2004) considers the evolution of emotions and the evolutionary basis for happiness. In answer to the question what is the adaptive value of feeling bad, he writes:

Nausea, diarrhea, cough, fatigue and anxiety all are distressing; they must be to carry out their protective functions. Indeed, one can argue that all bad feelings are components of defenses. Natural selection has molded each kind of bad feeling to help protect against a specific threat. A person who does not experience nausea as aversive is liable to eat the same toxic food again and again; a person who does not get fatigued will suffer damage to muscles and joints. Emotional suffering can be just as useful as physical discomfort. Emotions adjust a person’s response to the task at hand. (1991, 34).

Nesse specifically addresses the dangers of positive emotions:

"Only rarely are the dangers of positive emotions considered, except in the context of drug abuse. However, the utility of any emotion depends entirely on whether it is expressed in the situation in which it is useful. In other situations, it can be expected to be harmful. The case for the dangers of positive emotions is made most straightforwardly by individuals with mania. Their joy is infectious, their optimism and self-confidence unbounded. However, they usually lack the ability to stick with one thing long enough to accomplish anything, they often are incensed by the least criticism, and their respect for other people diminishes to the vanishing point. One manic may give away his life’s savings on a whim, while another joyfully drives 100 m.p.h. to a sexual liaison with a potentially
dangerous stranger” (2004, 1341).

These examples revive concerns that were raised in a previous generation. Legman’s (1975) essay *No Laughing Matter* begins with quotations from Milgram’s (1963) renowned investigation into the extremes of obedience. In Milgram’s experiments (which have recently been replicated by Burger 2009), naive subjects were ordered to administer increasing intensities of electric shocks to victims on the pretext that it was a learning experiment. The victims were confederates of the experimenter and the shocks were simulated. Electrical levels were marked on a simulated generator both numerically from 15 to 450 volts and verbally from Slight Shock to Danger: Severe Shock. Of the 40 male subjects, 26 administered the highest level of shock. But some subjects became extremely tense and exhibited what Milgram described as an unexpected and unexplained sign of tension, nervous laughter.

One sign of tension was the regular occurrence of nervous laughing fits. Fourteen of the 40 subjects showed definite signs of nervous laughter and smiling. The laughter seemed entirely out of place, even bizarre. Full-blown, uncontrollable seizures were observed for 3 subjects. On one occasion we observed a seizure so violently convulsive that it was necessary to call a halt to the experiment. The subject, a 46-year-old encyclopedia salesman, was seriously embarrassed by his untoward and uncontrollable behavior. In the post-experimental interviews subjects took pains to point out that they were not sadistic types, and that the laughter did not mean they enjoyed shocking the victim (Milgram 1963, 375).

Lewis (2006) also explores the dark side of humor in the context of military obedience. He discusses the example of American soldiers laughing at naked and dead Iraqi prisoners. Concerning a soldier convicted for torturing (technically "maltreatment of") prisoners, Lewis poses the question: "Was he a sadistic murderer who took pleasure and found amusement in the suffering of others or a stressed-out employee who sought relief through laughter, hoping to access the healing power of humor?" (2006, 11).
Although scholarly research on laughter and humor is only recently examining these themes, general literature has long expressed them. Legman (1975, 45) cites various sources, including the Bible, Shakespeare, Victor Hugo and Mark Twain, in support of his musing that, "Perhaps laughter is just a less painful kind of tears?" In Shakespeare's play *Titus Andronicus*, for example, Titus' daughter is raped, her hands are cut off, her tongue is cut out, his sons are beheaded and Titus amputates his own hand. Titus' response to these horrors? He laughs. When his brother asks why, Titus replies "Why, I have not another tear to shed." Shakespeare has sprinkled his play with dozens of lines that refer to tears. Tears and laughter are often associated in life and literature, and not just crying for joy (for a review see Sakuragi 2005).

Weeping your way to health is no less plausible than laughing your way to health. In his critique of the Norman Cousins phenomenon, Kahn (1981, 309) observes,

Some people, Cousins says, find that uncontrollable laughter 'leaves the individual released almost to the point of an open sprawl.' Yet people who have spells of weeping to exhaustion often describe this same after sense of relaxation and cleansing calm. It is a common observation that weeping, when appropriate to the occasion, relieves tension. And, we are told, so does aggressive behavior--screaming or any other emotional outburst. The sensation Mr. Cousins describes after laughter is not unique for laughter. Perhaps we simply prefer to laugh. Perhaps we prefer to ignore pain. Or could it the other way around, that laughter is evidence of pain? Brottman explores the extreme possibility that laughter is just plain seriousness: "Is it possible that human laughter is connected not to feelings of good will at all, but to a nexus of deep emotions revolving around fear, aggression, shame, anxiety and neurosis? Is it possible that laughter is, in fact, the most serious thing we do in our lives?" (Brottman 2004, xx; for the opposite view that laughter is the expression of nonseriousness see Chafe 2007).
The serious negative turn in laughter research has prompted reconsideration of previous accounts of laughter. For instance, Hempelmann (2007) investigated the frequently cited "laughter epidemic" of 1962 that was reported to have started in an East African girl's school then spread to the whole country. This event has been characterized as an example of contagious laughter, a case of giggles among girls joyfully spreading across the nation. Hempelmann concludes, however, that "the good news is that there is a real event underlying the reports, but the bad news is that it had nothing to do with humor and only very little with laughter" (2007, 50). He characterizes the event not as contagious laughter but as mass psychogenic illness (MPI), of which both laughter and crying were symptoms. He resists the general tendency in the literature to accentuate the positive view of laughter. Hempelmann even goes so far as to suggest that his article, "should be the last to discuss the 'laughter epidemic' in research on humor, other than to illustrate the dissociation of laughter and humor" (2007, 67).

Laughter appears in a wide range of dysfunctions and disorders. Researchers are currently working out the ontology and nosology of abnormal laughter. For instance, Wild et al. (2003) list various terms: pathological laughter, involuntary laughter, pseudobulbar affect, dysprosopoeia, sham mirth, inappropriate, uncontrollable and non-epileptic laughter and emotional incontinence. Others have fashioned broader categories for abnormal laughter and crying, including involuntary emotional expression disorder (IEED) and pathological laughing and crying (PLC) (see for example Cummins et al. 2006 and Wortzel et al. 2008). Askenhazy (1987) lists 6 categories ranging from psychotic laughter to "giggle incontinence". There is, however, no standard classification system for disturbances of laughter (Wild et al. 1983). Consequently, "Inasmuch as laughter is such a ubiquitous component of human behaviour, the notion of 'pathological' laughter can
refer to anything from laughter at politically incorrect jokes to laughter as a manifestation of chromosomal aberrations in the Angelman syndrome” (Wild et al. 1983, 2122).

In a chapter titled "Abnormal and Inappropriate Laughter" Provine discusses this negative side of laughter which he describes as a "mix of the unusual, the improper, and the bizarre...[a] foray to the fringes of human experience" (2000, 153). As his chapter title suggests, there is a normative assumption in attempts at classification of laughter dysfunctions and disorders. Under the abnormal, Provine includes not just disease classifications such as kuru, dementia and amyotrophic lateral sclerosis but also laughter at funerals. Laughter in the face of tragedy might be abnormal and inappropriate, or actually a healthy response. Boosters of positive associations between laughter and health shy away from the negative side in the belief that even thinking about negatives induces poor health. For example, Cousins believed that negative words (such as his specialist's prognosis) could cause death. He ends his bestselling book *Anatomy of an Illness as Perceived by the Patient* with these words:

> It all began, I said, when I decided that some experts don't really know enough to make a pronouncement of doom on a human being. And I said I hoped they would be careful about what they said to others; they might be believed and that could be the beginning of the end (Cousins, 1979).

It's unclear whether this suggests that the absence of positive words could cause death. It could be an example of nocebo. Or a special subset, nocebo from the absence of placebo. If the important thing is the "pronouncement of doom", that is, the presence of negative words, then as Santino (1985, 165) observes, Cousins' fears are those of the cursed, his pronouncement of doom that of a voodoo death. As the childhood chant goes, sticks and stones may break my bones but names will never hurt me. Yet here the negative words themselves, with accompanying doom constructed and reified, have taken on
a life and death of their own (Hacking 1999; see also Benedetti et al. 2007 on negative expectation and pain).

### 7.2 Laughter and Death

That brings us to the connection between laughter and death. If laughter is the best medicine, what is the basis for the opposite notion that you can die laughing? Dying laughing, corpsing, could be just another metaphor associated with laughter; maybe death from laughter is not even worth taking seriously. As it is inconsistent to believe at the same time that laughter is healthy and deadly, perhaps the resolution is that laughter is so good, the most powerful sword, that too much of a good thing can kill.

According to Voltaire (1764), "Laughter may sometimes extend to convulsions; it is even said that persons may die of laughter. I can scarcely believe it; but certainly there are more who die of grief." Provine (2000, 183) probes how risky laughter really is. He focuses on two outcomes, death and fainting (syncope) from laughter. He cites anecdotes associated with famous names in history, including Galen, Leibniz and Trollope. He also cites the reported case of a 62 year-old man with chronic cardiovascular problems who fainted repeatedly while watching episodes of the television show Seinfeld. From his review, Provine concludes that victims are probably predisposed to illness and, "Such laugh victims are notable because of their novelty. Although comparative statistics are lacking, rage has probably felled more victims than fits of laughter. Laughter remains highly recommended for a long and good life" (2000, 184).

By contrast, Bloomfield and Jazrawi (2005) claim to make the first report of laughter syncope in a healthy man:
...a 32-year-old, previously healthy barber was standing and cutting a client’s hair. The client related a funny story, upon which the barber broke out into a very strong, sustained, loud, and unrestrained laughing fit during which, according to observers, he "blackened out" and fell to the floor.... The patient had no personal or family history of diabetes, or of cardiac or neurological disease. He was taking no medications. He did not smoke, drink alcohol, or use recreational drugs.

The authors conclude, "Laughter has frequently been proposed to be the best medicine. However, as with any intervention, an excessive dose may result in adverse events." There remain, however, only a handful of reports of laughter syncope in the medical literature (Braga et al. 2005, Bragg 2006, Amaki et al. 2007).

### 7.3 Laughter Costs

Laughter health advocates paint a picture of little or no risk or cost to laughter in return for great benefits. Anyway, what’s the worst you could do, tell a bad joke? Laughter seems far removed from the problems associated with drugs such as Thalidomide, Vioxx, Laetrile or any other sign of hope that later proved worthless or dangerous. So the ratio of benefits to costs seems high, given the apparent zero cost or low cost of laughing. However we have already seen some of the admittedly rare negative consequences of laughter including death. But there are more common and more general problems with laughter.

One problem is opportunity cost, the cost of not choosing the next best alternative. For instance, spending money on lottery tickets precludes investing the same money for future use. Similarly the promotion of laughter in medicine could displace something more effective. Laughing is fun but perhaps the time and effort devoted to medical uses of laughter could be
diverted to something that could achieve greater results than those, if any, provided by laughter. Opportunity costs appear both for the patient who believes in laughter as medicine, perhaps encouraged by Norman Cousins, popular movies and uncritical review on supportive Internet sites, and for the physician who must take time to research and respond to the anecdotes and other less scientific evidence from such sources.

Costs also appear in what might be called the trust account. Patient advocates like Cousins wanted all medical professionals to learn from reading about illness as perceived by the patient. Imagine that the doctors who originally pronounced the words of doom, and figuratively sentenced him to death, had read his book and had learned to accentuate the positive, to stop pronouncing words of doom. Next time Cousins was sick, and his doctors said don't worry be happy, just laugh yourself to health, could he have trusted them and their hopeful words?

Ernst (2005) cautions against anti-science and superstition. He criticizes the permissive view expressed in the phrase "who cares how it works, as long as it does?" Although he was referring to complementary alternative practices such as spiritual healing of cancer, his warning is equally applicable to belief in the health benefits of laughter. Not only is there a general danger of accepting, as Ernst puts it, "mystical nonsense," but there is also a real danger when, "Used as an alternative, even the most harmless treatment can become positively life-threatening" (2005, 131). He questions whether apparently harmless interventions actually are harmless. Laughter seems harmless. But without any laughter-harm reporting system and an incentive to look for negative effects of laughter, indeed with a mindset that assumes there are none so there's nothing to look for, there will be no balanced evidentiary basis for the claim that laughter is harmless. Ernst reminds us of the truism that "absence of evidence is not evidence of absence of an effect"
For those unfortunate sufferers who have tried everything else without success, perhaps there is no other opportunity, no alternative with a higher value. But our theme is how we know what works in medicine, not what should we do when all else fails in medicine. As we have seen, laughter is ubiquitous in medicine (Robinson 1977). Advocates of laughter as medicine do not limit their advocacy to last resorts. Even in dire cases, there is an assessment to be made of the risks among various last resorts.

Then there is actual out-of-pocket cost. For instance, Lewis (2006) notes that humor and laughter is not the free medicine often touted as such, given that consultants are not working for free (2006, 65), nor are professionals who work with those consultants working for free. Laughter books, videos and other products also are not entirely free.

Another problem is the cluster of guilt, blame and shame. As we saw in the discussion of story-based medicine, Norman Cousins advocated the positive side of self-help and patient responsibility (Cousins 1979, 11). A negative side is to blame the patient. If laughter doesn’t work for you but it worked for Cousins, you’re not only sick, you’re a failure at curing yourself.

Doctors and their patients prefer something that works, not something that might work. Skeptical about the health claims for humor and laughter, Lewis asks, "if you had to get sick, would you rather have a disease with a known and effective cure or one for which you have to rely on the healing power of humor? Not laughing now, are you?” (Lewis 2006, 68).

There may also be a risk of false confidence. A focus on the positive, ignoring the negative, could be a form of denial. Lewis (2006) characterizes the juxtaposition of humor and health as denial of the lack of a better remedy. On the other hand, is fantasy ever better than truth? Perhaps facing the facts is unhealthy. Perhaps denial produces better results. Taylor and
Brown (1988, 1989, 1994) argue that false confidence, or what they call positive illusion, is indeed healthy:

In conclusion, the overriding implication that we draw from our analysis of this literature is that certain biases in perception that have previously been thought of as amusing peccadillos at best and serious flaws in information processing at worst may actually be highly adaptive under many circumstances. The individual who responds to negative, ambiguous, or unsupportive feedback with a positive sense of self, a belief in personal efficacy, and an optimistic sense of the future will, we maintain, be happier, more caring, and more productive than the individual who perceives this same information accurately and integrates it into his or her view of the self, the world, and the future. In this sense, the capacity to develop and maintain positive illusions may be thought of as a valuable human resource to be nurtured and promoted, rather than an error-prone processing system to be corrected. In any case, these illusions help make each individual’s world a warmer and more active and beneficent place in which to live (1988, 205).

In response to criticism of these generalizations, and of the purported link between illusions and mental health, Taylor and Brown acknowledge problems with the concept of illusion that underlies their thesis. Of particular relevance to the present discussion is their rhetorical question, "If a person thinks she has a wonderful sense of humor, who is to say that she is wrong?" They offer no answer but stick with their conclusion summarized above. It is important to point out, as they do, that their view, originating from Taylor’s research with cancer patients, makes no claim that positive illusions cure cancer.

The link between illusion and health extends to higher order considerations. Beyond the first order of research into laughter and humor are higher order reflexive issues, not only the self-assessed "wonderful sense of humor" considered by Taylor and Brown, but also the philosophical self-examination of the nature of humor and laughter research. There is an ever-present subtext in laughter research, a potential bias against inquiry into the
negative side of laughter. This bias is consistent with that found in complementary and alternative medicine (Ernst and Pittler 1997). Positive research results may be conflated with positive viewpoints. If laughter is so good and healthy, however, what harm could come from examining its dark side? It’s clear which project is more appealing to funding agencies and more likely to interest peers and public when the choice is between disgusting dog faeces in movies or clown noses in hospitals. It’s also clear which laughter stories are more integrated with the common wisdom when the choice is between stories of hope and laughing oneself to health, or the serious stories of torture and uncontrollable laughter seizures discussed here. Brottman hints at the connection between such choices as these and mental health:

One of the notable hallmarks of Western culture is that denial of the human condition is regarded as necessary to sustain mental health, rather than being a symptom of psychopathology. The acceptance of denial under the guise of 'humor' not only makes regression possible, but hides the underlying censored impulse. It is probably for this reason that any kind of analysis of 'humor' usually meets with such strong resistance (2004, 151). It is as if we should not examine too closely our denial and illusion for fear of losing touch with our chosen unreality. It is as if we are not hearing much about the dark side of laughter because it doesn't sell, not because it isn't true.

This chapter has examined aspects of the serious negative turn in laughter research. Our network of laughter themes now includes the dark, hostile, aggressive, and to judge from Milgram’s experiments, tortured sides of laughter that reveal it to be something other than harmless, free, or best medicine. But knowledge in these branches of the network is sketchy. As many commentators have noted, these themes are rarely explored. The sounds of laughter so far are mostly those of one hand clapping.
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8. Does Evolutionary Medicine Say Laughter is the Best?

This chapter considers laughter from the perspective of evolutionary medicine. Despite a rapidly expanding literature of laughter research and of evolutionary medicine, little attention has been paid to their intersection, the medical and health implications of the evolution of laughter. The main issue addressed here is what evolutionary medicine has to say about the medical and health benefits of laughter. But it is important to note at the outset that laughter and humor have often been conflated in the literature, despite their many differences. So the following discussion reviews leading accounts of the evolution of both laughter and of humor, including the most recent one that claims to synthesize all previous evolutionary accounts of laughter and humor. Beginning with reasons for taking an evolutionary view of laughter, and the criteria for assessing the adaptive value of laughter in an evolutionary context, we then look at the range of evolutionary accounts of laughter and their limits. We see that evolutionary stories connecting laughter, medicine and health are readily constructed, as shown by two new theories constructed here, but difficult to test. This discussion reveals that each account proposed so far has little to say about laughter in medicine beyond untested assumptions, and therefore no evolutionary account provides specific answers to questions about how or whether laughter works in medicine and health.
8.1 Making Sense of Laughter in Light of Evolution

All biomedical knowledge has an evolutionary component since the objects of study are living organisms. So one way to organize that knowledge would be to proceed from a description of the evolution of some trait to hypotheses about the medical and health implications of that trait. From there we might extrapolate to new observations and hypotheses about the trait and to other similar traits and implications. Evolutionary thinking can also expose compromises in the engineering of the human body as solutions evolved to ever-changing survival problems. One common example is the human eye, that has evolved with various compromises such as a blind spot and light-sensitive tissue at the rear rather than at front. Another example is the human body's use of the same passageway for both food and air, not a more safely separated structure, with the resulting tendency of humans to choke while eating. Evolutionary explanations may point to proximate explanations and suggest where to look for solutions to medical and health problems. If the goal is to understand human health, we can do better if we understand how humans have been and are constructed.

Without an evolutionary framework, we can, of course, try poking, prodding or doing something else that seems to work. But according to Dobzhansky's frequently quoted dictum, nothing in biology makes sense except in light of evolution. Perhaps evolutionary accounts can provide knowledge about the connection between laughter and health. Or perhaps the Stone Age is the wrong place to look for laughter. Laughter may be adaptive or not or may tag along as a consequence of other adaptations. By suggesting what evolutionary roles laughter plays, evolutionary theory can suggest new
areas for laughter research.

As in any evolutionary account, in an evolutionary account of laughter, at some point there must be heritable variation in phenotype that influences survival and reproduction. In laughter we are looking for the following:

• an identifiable trait (is laughter such an identifiable trait?)
• that varies (in what ways does laughter vary from person to person?)
• and is heritable (is laughter inherited?)
• and influences probability of survival and reproduction (how does laughter influence survival and reproduction?)
• in some environments (in what environments does laughter affect survival and reproduction?)

This list is an oversimplification of the complexities of evolution by natural selection but it provides a place to start. There are no simple answers to any of these questions. Many related issues arise, such as the level of selection (genes? individuals? groups?), selection pressures associated with laughter, differences in present environments from Stone Age environments, and how predators and prey, or friends and foes, perceive laughter.

8.2 Adaptive Value of Laughter

Weisfeld (1993, 143) succinctly summarizes evidence that laughter, despite consuming energy and potentially attracting predators, nevertheless has adaptive value. He uses the following criteria to test for the evolved basis of a given behavior such as laughter:

• "cross-species prevalence": he argues that despite cultural variations, all humans laugh. The universal distribution suggests that laughter is inherited.
• "early onset": although there is debate about whether laughter appears in infants soon enough to support an evolved basis, he argues that laughter could nevertheless be like other inherited traits, such as puberty, that appear late in development.

• "specific neurological mediation": like many others, he finds a neural basis for laughter in evidence such as brain pathology.

• "presence in related species": he argues that not only laughter but also humor appreciation is evident in apes as well as humans.

• "stereotypy": citing the example of laughter in the congenitally blind and deaf, he says that "laughter is clearly stereotypic across cultures" (1993, 145).

Each of these claims can be challenged. The apparent universality of human laughter might not stand up to scrutiny, or if it does, it may mean that all humans are exposed to something similar in their environments and cultures (although this is unlikely given the variety of environments and cultures). Laughter circuits in the brains of infants have not been specifically located, what may be pre-wired is not laughter but only certain capacities expressed in appropriate environments, and in any event infant and child behavior says little about adult behavior, and the time of onset does not determine whether or how adaptive it is. That some non-humans appear to play and make laugh-like panting sounds does not necessarily mean they laugh or experience laughter as humans do. We refer to laughing hyenas but do not consider their behavior to be like that of apes or humans. Yet despite such challenges, the evidence is suggestive. This is not to say that every aspect of laughter fits neatly into an evolutionary template. A better understanding of laughter, however, cannot be reached without considering evolutionary origins together with psychological, social and other layers.
Nesse and Williams (1994) have set out an ambitious program for filtering medicine through the lens of evolution, from elaborating basic concepts through training physicians to thinking in evolutionary terms. For example, they draw the amusing sketch of mice who hate the smell of a cat. The mice ask their physician to treat this annoying symptom. Should the physician give them a drug to dull their sense of smell? The warning value of that annoying cat smell clearly argues against relieving their symptoms. No matter how much the mice detest cat smells, evolution has shaped them to rely on the sense of smell and not incidentally to avoid cats. Yet physicians often relieve symptoms that are telling patients to avoid potential dangers. They prescribe drugs, and in their absence patients self-medicate, to dull fever and headache without considering the adaptive value of those symptoms that are telling the patient to behave appropriately while allowing the body to rid itself of disease. Of course we are not now in the same environment in which early humans evolved. Most people today live like mice in a world where cats do not eat us because they have been eliminated or rendered harmless. So an evolutionary account would have to situate traits now in addition to possible adaptations long ago.

Like adaptive pain and fever, similarly understanding the adaptive role of laughter may inform and perhaps radically change how laughter is viewed in medicine. Unfortunately Nesse and Williams, along with many other experts, say little about the health implications of laughter. They do point out that the fittest individual is not necessarily the healthiest, and emphasize that, "natural selection cares not a whit for our happiness, and it promotes health only when it is in the interest of our genes" (1994, 14). Unless, perhaps, happiness influences our chances of survival and reproduction. They are speaking loosely here, implying that natural selection operates at the level of genes, as distinct from individuals and groups, as if there were no
controversy over that very level of selection that ultimately affects survival and reproduction and allows genes to get into the next generation. They also appear to adopt, without scrutiny, the folk belief that laughter is healthy when they claim that, "our ancestors also had the ability to look on the bright side in times of adversity and to find reasons for laughter" and they conjecture that our Stone Age ancestors, "would have groaned at many of the same jokes" as someone in the sixth century or nineteenth century (1994, 142). But they do not put these grand ahistorical assumptions to the test. Is laughter an instance of natural selection promoting health in the interest of our genes? They do not discuss whether laughter is adaptive, was adaptive or whether it is the result of some other adaptation. Indeed, their focus is almost exclusively on the medical downbeat, disease, not on the upbeat, health.

8.3 Accounts of the Evolution of Laughter

Rival and overlapping evolutionary accounts often talk about different issues. When they do converge on the same issues, they often use different language and background assumptions. For example, many accounts are rooted in the idea that humor appears to have originated in play, which in turn suggests that questions about evolution of humor and associated laughter might be answered by looking at the evolution of play. Other accounts suggest that answers lie in the study of positive emotions such as joy and happiness, and still other accounts combine these aspects. Some accounts accentuate the negative in laughter and humor, the ostracizing and aggressive social ordering, while others accentuate the positive, the relaxation, social bonding and creativity. Few accounts address the logical necessity that inclusion of
those who bond implies exclusion of those outside the bond; except in an
idealized heaven where everyone is a happy insider, there cannot be an
insider group without an outsider group, or happiness without unhappiness,
or positive without negative. Some accounts nevertheless end up with a
lopsided attention to only one or the other. The proliferation of notions,
terminologies, accounts and hypotheses is partly a consequence of the
complexity and broad range of the subject.

In addition to varying overall conceptions, many also differ in myriad
details. Some deal with combinations including conversation, humor, tickling,
play, smiles and laughter, while others dealing more narrowly with one or
two behaviors differ in focus; some dwell on formalities such as jokes and
cartoons while others consider informalities in conversation and play. Some
consider cognition but not emotion. Some address basic questions such as
what laughter is and when it evolved, while others do not even mention
basics, such as the difference between humor and laughter. A single aspect,
for instance the issue of when laughter evolved in relation to other
evolutionary events, opens its own subdivisions. Issues that spring from
considering the sequence of events include whether laughter appeared before,
after or together with smiling, whether laughter evolved before language,
associated with earlier evolved parts of the brain, and whether human
laughter was contingent on bipedalism and a resulting increased control of
human respiratory apparatus. The latter issue in turn depends on related
matters, such as whether quadrupeds can laugh, which in turn depends on
what is meant by laughter.

The following table summarizes the leading evolutionary accounts of
laughter and humor:
<table>
<thead>
<tr>
<th>Source</th>
<th>Summary</th>
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<tbody>
<tr>
<td>Alexander (1986)</td>
<td>humor as &quot;a social activity that alters the status of the humorist positively and that of the object or victim negatively&quot; (1996, 253)</td>
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<td></td>
<td>&quot;My general hypothesis is that humor has developed as a form of ostracism, and that, historically, at least, ostracism has tended to affect the reproduction of the ostracized individual (or group) deleteriously, especially in relation to the reproduction of the ostracizers, by restricting access to significant resources&quot; (1996, 255)</td>
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<tr>
<td>Chafe (1987)</td>
<td>laughter as disabling mechanism</td>
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<td></td>
<td>&quot;Simply put: While you are laughing, you cannot do anything else&quot; (1987, 20)</td>
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<tr>
<td>Fridlund and Loftis (1990)</td>
<td>laughter and tickling as related reflexes (Darwin-Hecker hypothesis)</td>
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<td>&quot;Conceivably, the tendency to dissipate physical or psychological tension using the saccadic abdominal movements found in laughter and crying is a trait whose intensity and elicitation threshold are variable and heritable. This trait may express itself first with tickling, and later in humor, grief, embarrassment, etc.&quot; (1990, 147)</td>
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<td>Source</td>
<td>Summary</td>
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<tr>
<td>Weisfeld (1993)</td>
<td>laughter as rewarding emotional expression</td>
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<td>&quot;Laughter evolved to allow us to continue to receive amusement. Laughter is a pleasant social signal that prompts the humorist to persist in providing this edifying stimulation.&quot; (1993, 141)</td>
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<tr>
<td>Dunbar (1996)</td>
<td>smiling and laughing as vocal grooming</td>
</tr>
<tr>
<td></td>
<td>&quot;We can now, quite literally, groom at a distance. Telling jokes allows us to stimulate opiate production in our grooming partners even when we don't have the time to sit there doing it physically&quot; (1996, 191)</td>
</tr>
<tr>
<td>Ramachandran</td>
<td>laughter as false alarm</td>
</tr>
<tr>
<td>(1998)</td>
<td>&quot;I suggest that the main purpose of laughter is for the individual to alert others in the social group (usually close relatives that are likely to share the same genes) that the anomaly detected by that individual <em>is of trivial consequence</em>. The laughing person is, in effect, announcing his discovery that there has been a false alarm...&quot; (1998, 352, italics in the original)</td>
</tr>
<tr>
<td>Miller (2000)</td>
<td>sexual selection; laughter as fitness indicator</td>
</tr>
<tr>
<td></td>
<td>&quot;The evolved function of indicators is basically to advertise an individual’s fitness to other individuals&quot;</td>
</tr>
</tbody>
</table>
**Source**  
Provine (2000)  
Summary  
laughter as ritualized panting sound

"Laughter began as a ritualization of the panting sound of rowdy play of which tickle was a trigger and central component... Human laughter is a further ritualization (second-order ritualization) in which "ha-ha" is one step removed from the ape pant-laugh and is elicited by a wider range of stimuli..." (2000, 124)

Owren and Bachorowski (2001)  
"selfish gene" account

"applying the basic tenets of selfish-gene selection to the evolution of communication in early hominids points to the emergence of honest signaling of positive affect as a valuable mechanism for forming and maintaining cooperative relationships" (2001, 177)

Howe (2002)  
humor as mind reading

"Those of our ancestors who could read the thoughts of others and get pleasure from being able to interpret their expressions would have achieved power within the community. That power would have translated into a strong selective advantage" (2002, 254)
### Source | Summary
--- | ---
van Hooff and Preuschoft (2003) | Laughter evolved in the context of joyful play not aggression; broad smiling did not originate as a diminutive form of laughter but in fearful submission as an expression of friendliness. "Laughter has evolved out of the intention movement of biting or gnawing that is such a prominent part of playful wrestling. The broad smile has its origins in the baring of the teeth, which signals terror and fear" (2003, 279)

Gervais and Wilson (2005) | Duchenne laughter (defined below) as a medium for playful emotional contagion. "Laughter became ritualized in hominids as a mechanism whereby individuals and groups could more readily capitalize on fleeting moments of safety and satiety in order to 'broaden and build' their thoughts and resources through social play" (2005, 413)

Flamson and Barrett 2008 | Humor as honest signaling in the form of encryption. "It allowed for previously unprecedented benefits to coordination of activities by individuals who share knowledge and attitudes" (2008, 280)

Each account in the table has its attractions. They are plausible stories that are not so obviously correct as to be trivial and not so obviously wrong as to be rejected outright. As noted earlier, the language is not always clear. Some accounts, for instance, conflate laughter and humor. Many have significant omissions and unsubstantiated inclusions. It is surprising in the context of
health claims for laughter that none of the accounts mentions the possibility that laughter could be a placebo (see the chapter on placebo for a discussion of evolution and placebo).

Some evolutionary hypotheses about laughter are no more testable than the laughter-as-placebo suggestion offered here. It is disappointing that many authors, like Nesse and Williams discussed above, appear simply to repeat folklore that connects laughter, humor and health. For example, Chafe refers in passing, without more elaboration, to "the well-known therapeutic benefits of humor" (1987, 23). Nearly two decades later, Gervais and Wilson also mention in passing the effects of laughter on increased positive emotion and reduced negative emotion. They cite unnamed "myriad other health benefits" from laughter (2005, 403). The connection between evolution and these unnamed health benefits would seem to be ripe for elaboration.

8.4 Duchenne or Not Duchenne, That is the Question

Two of the most comprehensive accounts of the evolution of laughter and of humor, set out by Gervais and Wilson (2005) and Owren and Bachorowski (2001), are also two of the most problematic in their contrasting conceptions. They are roughly comparable on one major dimension, however, in that they both give a significant role to the distinction between genuine and volitional laughter.

Owren and Bachowrowski (2001) say that smiling and laughing evolved as affective bonding signals in early humans as they left the trees for other habitats where cooperation was selectively advantageous. Smiling and laughing developed as signals of positive affect fostering cooperation: friends smiled and laughed, foes did not.
Selfish-gene theory, they say, suggests that natural selection would favor those who could selfishly exploit others by means of dishonest smiling and laughter. At first, honest signals evolved. Then cheaters emerged. Friends smiled and laughed, but then some foes also smiled and laughed dishonestly. The honest and the dishonest each used different brain mechanisms. Owren and Bachowrowski hypothesize that spontaneous emotion-dependent laughter evolved as a reliable indicator of positive affect. In their selfish-gene scenario, first honest signals of cooperation evolved, then dishonest signals, then reliably honest signals, so now humans could reliably distinguish friend from foe.

Gervais and Wilson (2005) offer a synthesis of previous work on the evolution of laughter and humor. They begin with a substantial list of complaints about their predecessors including:

- **Empirical neglect**
  They say researchers have overlooked significant empirical issues such as the following distinctions:
  - between Duchenne and non-Duchenne laughter (they cast doubt on all research that failed to recognize these two species of laughter)
  - between laughter following deliberate humor and the more frequent laughter that punctuates conversation (such as nervous laughter associated with stress)

- **Theoretical incompleteness**
  They say previous accounts have:
  - overlooked the role of emotion in humor and laughter
  - neglected the potential multiplicity of laughter kinds
  - used impoverished evolutionary frameworks that neglected the
multilevel nature of evolution
- failed to specify the mechanism of emergence of a novel trait from existing traits
- conflated current utility with historical genesis
- proposed contradictory adaptive functions for laughter and humor
- characterized effects as functions
- failed to recognize the changing function of a trait (cooption, preadapation, exaptation)

- **Social isolation**
  - They say researchers have failed to interact with outside research programs such as neuroscience and psychology.

In light of these problems, they claim, "to provide a first approximation of a comprehensive evolutionary framework within which future laughter and humor research and theorizing can be conducted" (2005, 396). They offer the apology that their stated purpose was not to tear down past research, but instead to construct a new account. In a similar vein, the present discussion is not just to show where their claims are weak, for instance by noting that they are not the first to offer a comprehensive framework, or to point out what they have omitted, and show that their framework is hardly comprehensive. Instead, present purposes are only to show that a lot more work needs to be done before any evolutionary account can be said to adequately address laughter in medicine and health.

For Gervais and Wilson, laughter is an evolved instinct with a cultural overlay. They note among other evidence of hard-wired instinct the same evidence that Weisfeld noted, namely that human laughter is universal, develops spontaneously in infants and resembles similar behavior in our
nearest primate relatives. Of the well-known theories of humor (such as superiority theories, incongruity theories and relief theories that have been widely discussed and will not be rehearsed here) they adopt the incongruity theory of formal laughter-evoking humor. To avoid the philosophical baggage attached to the word humor, however, they say all "Duchenne" laughter in apes and humans is stimulated not by humor but by "nonserious social incongruity". They reserve the word humor for formal attempts at eliciting laughter in adults. Other forms of nonserious social incongruity such as play and tickling are termed protohumor. That is, they have repacked traditional philosophical categories of laughter and humor into fancier luggage.

They ground their framework for the evolution of laughter and humor on the following propositions (2005, 403):

- Two types of laughter exist, Duchenne and non-Duchenne laughter
- Duchenne laughter is genetically predisposed to develop
- Duchenne laughter is derived from the primate "play face"
- Duchenne laughter in apes and humans shares a common trigger: nonserious social incongruity
- Human laughter is codetermined by biology and culture
- Duchenne laughter is inherently linked to positive emotional experience
- Laughter is contagious
- Laughter serves myriad functions in modern society

Fundamental to their account is the distinction between Duchenne laughter, which they describe as "stimulus-driven and emotionally valenced," and non-Duchenne laughter, "self-generated and emotionless" (2005, 396). The two types of laughter are based in different neuronal mechanisms. Duchenne laughter, they argue, involves a system of mirror neurons: "Duchenne laughter is essentially a medium for emotional contagion operating through an intersubjective mirror system" (2005, 423). Gervais and Wilson endorse
the hypothesis that empathy and shared experiences such as laughter are based in a system of mirror neurons that are active not only during an individual’s actions but also when an individual perceives actions of others. Briefly stated, when you see other people’s eyes light up and they begin to laugh, you feel happy and begin to laugh. They adopt the term "shared manifold" for this mirror system. They distinguish laughter signals from the apparently similar contagious signal of yawning, on the ground that yawning is a more ancient reflex not dependent on mirror neurons.

Since all laughter, they say, falls into one of the two categories, Duchenne or non-Duchenne, Gervais and Wilson have to deal with the problems facing anyone whose program squeezes a complex world into two boxes. Consider, for example, spontaneous conversational laughter. Laughter is not just the product of humor. Laughter often punctuates conversation. Some of that laughter seems spontaneous. But that kind of apparently spontaneous laughter, they say, is "simply volitional laughter that has achieved a degree of automaticity, and so appears spontaneous." Unraveling the interwoven threads of stimulus, response, emotions, learning and so on is not a simple dichotomy. Hence,"Spontaneous non-Duchenne laughter presents a problem because it exists in varying degrees across this conceptual divide, being strategic and learned but largely beyond conscious control" (2005, 401). Gervais and Wilson therefore settle on a qualified and ungainly definition of non-Duchenne laughter as, "spontaneous conversational laughter that occurs in the absence of attempts at humor" (2005, 401).

This complexity suggests that, waiting to be added to their ontology, there is the equally ungainly beast of spontaneous non-conversational laughter that occurs in the absence of humor, including laughter for no apparent reason (some laughter for no reason might be deemed to be pathological, perhaps even classifiable under the Diagnostic and Statistical Manual of
Mental Disorders, but no assumption should be made that all ordinary laughter has obvious reasons, such as sudden laughter observed at funerals or during torture). Other potential complications in this proliferation of laughter kinds could be devised from the distinction between voiced and non-voiced laughter, and subdivisions within those distinctions, such as snorts and snickers. Gervais and Wilson put voiced laughter into the Duchenne box (2005, 406). Is all Duchenne laughter voiced or is there any unvoiced Duchenne laughter? If so, how would unvoiced Duchenne laughter be related to a Duchenne smile? (compare for example Owen and Bachowroski 2003, 189 who emphasize the significance of the voiced-unvoiced distinction).

Consider this non-exhaustive table of laughter words distinguishing the two types of laughter on which Gervais and Wilson and others base their claims:

<table>
<thead>
<tr>
<th>Duchenne laughter</th>
<th>Non-Duchenne laughter</th>
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<tbody>
<tr>
<td>emotionally valenced</td>
<td>emotionless</td>
</tr>
<tr>
<td>stimulus driven</td>
<td>self-generated, strategic</td>
</tr>
<tr>
<td>genuine, authentic, felt</td>
<td>fake, facsimile, false, phony</td>
</tr>
<tr>
<td>real</td>
<td>simulated</td>
</tr>
<tr>
<td>honest</td>
<td>dishonest</td>
</tr>
<tr>
<td>involuntary, automatic, reflex</td>
<td>voluntary, intentional, willed, non-reflex</td>
</tr>
<tr>
<td>innate</td>
<td>learned</td>
</tr>
<tr>
<td>uncontrollable, uncontrolled</td>
<td>controllable, controlled</td>
</tr>
<tr>
<td>unconscious, non-conscious</td>
<td>conscious</td>
</tr>
<tr>
<td>spontaneous</td>
<td>volitional (but may seem spontaneous)</td>
</tr>
<tr>
<td>natural</td>
<td>artificial</td>
</tr>
<tr>
<td>free</td>
<td>forced</td>
</tr>
<tr>
<td>cohesive</td>
<td>aggressive</td>
</tr>
<tr>
<td>humor-generated</td>
<td>conversational</td>
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</table>
The columns suggest a simple binary model, or a linear continuum between extremes. But these dichotomies may not be reflective of the world or even meaningful. To include various strengths of non-Duchenne laughter that can be so practiced as to appear spontaneous, there could be a range instead of pairs. Or perhaps a more dynamic interaction could be expected: perhaps there is feedback between two or more simultaneous mechanisms that emerge somewhere on a laughter scale. The breadth and complexity of this network of notions suggests that they cannot be isolated in two meaningful columns.

They say non-Duchenne laughter is voluntary. Trained actors, however, can make themselves produce seemingly genuine laughter. Can Duchenne laughter be the consequence of voluntarily laughter that becomes contagious (volunteered by professional actors and comedians, or laughter yogis, for example)? If contagious laughter begins voluntarily, it is not clear if the resulting contagious laughter is genuine or reflex (see also Martin 2007 who prefers "fixed action pattern" to reflex because laughter depends on context).

A key distinction tied to their emphasis on the link between Duchenne laughter and emotion is which emotion exactly is involved. Under their model, only Duchenne laughter is emotional, although perhaps non-Duchenne laughter is associated with different emotions. A non-exhaustive list of emotion words associated with laughter includes hilarity, mirth, amusement, joy, exhilaration, happiness and nonseriousness (and these are only the positive emotions). The picture is again more complex than a simple dichotomy between emotional and emotionless laughter.

As noted already, Gervais and Wilson mention in passing the effects of laughter on increased positive affect and reduced negative affect. They refer to "myriad other health benefits" from laughter (2005, 403). But they anchor
this vague mention in a cited four-page commentary that begins with quotes from the Bible and the Talmud, and ends with laughter as a supplementary method for treatment of illness together with prayer and chicken soup (Rosner 2002). They also endorse the claim that only Duchenne laughter can undo negative emotions and stress, and elicit positive affect in others (2005, 406). On the latter point they appear to subsume fully vocalized laughter and emotionally-driven laughter under the heading of Duchenne laughter.

Putative functions of Duchenne laughter cited by Gervais and Wilson (2005, 403) include:

- increase positive affect and improve mood
- mitigate negative affective responses to stressful events
- promote myriad other health benefits
- a defense mechanism against stressful circumstances (includes nervous or non-Duchenne laughter)
- enable infants to learn and develop socially
- multiple social functions such as lighten mood, make listeners more receptive, increase in-group feelings, exclude out-groups, ease tensions, establish hierarchies, coordinate behaviors, couple group emotions

Without tackling the difficult issue of what a function might be in the context of evolution, suffice it to say that these functions are congruent with social play generally. Gervais and Wilson argue that laughter and play share a common evolutionary and mechanistic basis and, "Duchenne laughter functions to promote group cohesion and resource-building social play by spreading playful emotions" (2005, 408). Given that social play appears generally in mammals, they must distinguish laughter and humor from the more generally occurring social play. They develop an account of the adaptation of the ancient mechanisms for tickling and social play into the
more recent forms of humor and laughter. They view laughter as a preadaptation: Duchenne laughter emerged in our hominid ancestors then non-Duchenne laughter was a later preadaptation (2005, 404).

Owren and Bachorowski (2001, 158) by contrast start not with laughter but with smiling. As already noted, they say that genuine smiling evolved as a signal of cooperation, which in the evolutionary arms race was followed by dishonest smiling to gain the benefit of the signal. Then third to develop, after honest and dishonest smiling, was honest, spontaneous laughter. This vocalized version of smiling, in their account, served as a more reliable emotion-dependent signal, from which one person's grin was not another's groan.

Gervais and Wilson adopt the view that selection acts at multiple levels, including within and between groups, not just at the gene or individual level. On the opposing side, Owren and Bachowrowki rely on Williams' well-known refutation of group selection theory. A recent statement of this view is that, "A few decades ago, such 'group selection' explanations were taken seriously by professional biologists, but not now" (Nesse and William 1994, 15). Of the various levels from gene to individual to group to species, they say that selection acts only to maximize transmission of genes. For Gervais and Wilson, however, "Laughter favors group selection in the same way that culture does" (2005, 417). Groups that laugh had a selective advantage:

Those who were more adept at becoming playful during the appropriate times while also eliciting in others a playful state would have found themselves with relatively increased within-group fitness through the benefits of social play... (2005, 415).

Within-group fitness is a controversial term that has meaning for those who accept that the vague notion of a group has evolutionary significance. They make their case only tentatively, with the following caveat:

...even if the traits associated with laughter do not provide a strong within
group advantage, neither do they impose a strong within-group
disadvantage ... (2005, 416).

Without a special group advantage, they say, laughter can nevertheless
benefit the group as a whole compared to other groups, without cost to
individuals in the group:

Even a single person who laughs can change the mood and behavior of an
entire group, thereby increasing between-group differences. If laughter
increases the social play in a group, such a difference would increase the
fitness of that group relative to others ... thus driving group selection

They explicitly contrast their account with the selfish-gene, non-group
selection account of Owren and Bachorowski (2003) in which laughter is said
to have evolved from smiling. The selfish-gene version views smiling and
laughter as emotion-dependent signaling mechanisms for communication
between genetically unrelated individuals. For Gervais and Wilson, however,
Duchenne laughter (as distinct from volitional laughter) took a different
evolutionary path from that of smiling, and uses different mechanisms than
smiling. They adopt Fredrickson's (1998) theory that social play acts to
"broaden and build" resources. They claim,

In short, hominids became more playful through an evolutionary process
that made more aspects of experience appreciable as 'funny' (2003, 414).

Note the melding here of various notions including funny, play and laughter.
More playful is associated with more funny. But they do not clarify the
relationship between funny and laughter. It could be a one-to-one, one-to-
many or many-to-many relationship. They do not say which it is. They say
potential sources of social stress, such as near accidents, flatulence and
excretion, and sexual mischief, were converted to social play. Laughter was
also ritualized into a more perceptible signal. They point to costs for groups
that fail to become more playful and take advantage of safe situations and
thus they conclude,
Arguably the most potent stimulus in our EEA [environment of evolutionary adaptation] for signaling safe times, eliciting an individuals [sic] playful affect, and undoing lingering stress and negative emotional valence was the laughter of others originating from social play or 'false alarms' (Gervais and Wilson 2005, 415).

Consider slipping on a banana as the time-worn exemplar of a funny near-accident. If it is funny to some people because, for them, it appears serious until they realize it's the opposite of seriousness, then they would shift from stressful concern to a state of don't-worry-be-happy, let's play. An equally plausible story could be told that natural selection prefers the ones who did not laugh, those reliable rescuers who would have been more alert to potential danger and would have been quicker to help a real accident victim instead of laughing and playing.

Gervais and Wilson distance laughter from negative emotions such as fear. Laughter, they say, is a signal not just a response. "There is a distinct difference between fighting or running and laughing: the former work to influence the individual experiencing the emotion, the latter functions to influence other individuals" (2005, 409). This apparent distinction seems too convenient. Surely many negative emotions including fear influence others. That is, they too, not just laughter, serve as signals (for instance mass panic, mob violence, communal wailing). For Gervais and Wilson, laughter rides on the back of the adaptive advantages of social play. Laughter invokes play which brings advantages. Those advantages include facilitating development and broadening and building resources. In their account the work is done by play. Is there no adaptive advantage for the act of laughter itself? Possible advantages for the physical actions associated with laughter will be discussed below.

In the hypothesized evolutionary scenario proposed by Gervais and Wilson, hominoids developed a relaxed open-mouthed display around 6.5
million years ago. Laughter and humor emerged after hominids split from other hominoids 7 to 5 million years ago. Bipedalism brought the anatomical changes that distinguish human laughter from the vocalizations of chimpanzees 4 million years ago. They say laughter evolved before language 4 to 2 million years ago as a signal for inducing play in a group and was not dependent on higher cortical areas. Consequently their model can simply omit complexities such as volitional control of laughter, cultural overlay, theory of mind and language that are required by other models (Gervais and Wilson 2005, 412). These were all derived from primate play. Later as the higher brain structures evolved, this fundamental system was coopted in the form of volitional non-Duchenne laughter for the dark side that includes aggressive, nervous and manipulative laughter. They also suggest that language and humor may have evolved together. In the result, however, their stories tell us little about the details of laughter as opposed to other playful and positive signals and even less about laughter and health.

8.5 Limits to the Evolution of Theory

This discussion exposes several common themes that limit evolutionary accounts of laughter in the context of medicine and health:

There is a striking absence of formalisms. A century and a half after Darwin, evolutionary theorists of laughter are preserving his prosaic tradition, speaking primarily in words, and only rarely using mathematical, computational or logical symbols. Visual images or models are also rare compared to prose. There appears to be even less formalism and visualization in humor and laughter studies than in medicine and social sciences generally. Tufte (2006, 138), in a survey of publications in 2006, found that "evidence
published in major scientific journals such as *Nature* and *Science* is distinctly more visual and nonverbal than the evidence published in major journals in medicine and social science." But not one of the major publications on the evolution of humor and laughter has a significant formal or visual component.

Another common theme is reliance on relatively linear accounts. The linearity may partly be a consequence of a deficit of formalisms and partly a reflection of the lack of detailed evidence to support the hypotheses. It may also be a symptom of the more general intractability of complex, many-to-many interactions. A dynamic relationship such as a feedback loop is difficult but not impossible to describe in words. Prose has even greater limits, however, when it comes to modeling nonlinear dynamic social systems with a complex ontology and a combinatorial explosion of issues. Tickling, ordinary conversation, funny movies and stand-up comedy are strange bedfellows. Where are the models? The prosaic literature on the evolution of laughter only nods toward models of this complexity, as Owren and Bachorowski (2001, 186) do when describing the evolutionary process in the selfish gene account as "unruly and nonlinear" with "chaotic trajectories". They adopt concepts from nonlinear dynamics, and say it is erroneous to expect to find a single mechanism or linear process, but they take the complexity view no further.

By contrast, in a paper published with theirs in the same book, Mayne and Ramsey (2001, 16) declare that a nonlinear dynamic view is essential: "In a very real way, emotion is the synchronization of physiological, cognitive, behavioral and social systems." This is important: not one system is involved but all parts and systems are working together. Taking their cue from a quintessential nonlinear dynamic system, weather, they compare analysis of emotional episodes to analysis of hurricanes. They describe a scenario from
waking in a bad mood, through various daily events that exacerbate an already distressed emotional state, until a funny email invokes laughter and feedback into a happy mood. Mayne and Ramsey, however, propose a complexity view of emotions in general, not in relation to evolution or laughter in particular. The evolutionary accounts of laughter that we have so far are only beginning to incorporate the necessary complexity.

Coupled with the absence of formalism and complexity is an absence of detail. For instance, all accounts assume that laughter is a universally inherited human trait. But that broad generalization does not address details about the distribution of laughter kinds and proclivities. How does hypothesized adaptation to Stone Age environments relate to current environments? Why do some people rarely laugh? If people who rarely laugh nevertheless survive, does that mean frequent laughter was adaptive in some environments and rare laughter adaptive in other circumstances? In what ways is serious negative laughter adaptive? Similar details arise in the distribution of Duchenne and non-Duchenne laughter, if indeed that is a meaningful dichotomy.

Also common to evolutionary accounts of humor and laughter is a tendency to use the same language regarding all mammals. Yet comparisons between animal laughter and human laughter are not direct. If dogs and rats can be said to express laughter with sound, and perhaps wagging their tails, it’s an even greater stretch to claim that they have a sense of humor (see for example Douglas 1971 and Panksepp 2003). Although researchers may deprive rats of the opportunity to play, or breed them to emit ultrasonic chirps (laughs?) when tickled, or find similar activity in human and animal brains, a gap between animals and humans remains to be bridged. There is a residual sense that human laughter is different in kind, not degree, from anything similar in animals. Only chimpanzees make a sound resembling,
yet distinctly different from, human laughter, and even those close non-human relatives have limited positive facial expression to go with the sound (Owren and Bachowroski 2001). Gervais and Wilson trace laughter back to hominoid open-mouth display but do not explicitly address whether there was a prior continuum applicable to earlier evolutionary branching such that notions of laughter and humor apply to other animal lineages (see for instance Ross, Owren and Zimmerman 2009).

For the purposes of this discussion, the most important feature of these accounts is their lack of attention to evolutionary medicine and health. If the survivors are assumed to be the healthy ones, or health is equated with fitness, then we encounter the circularity of assuming that laughter increased fitness, when arguing that laughter increased fitness. Of course, fitness is not just health, and laughing is not essential for survival. Most surviving species on this planet do not laugh. If laughter had not evolved in humans, and we had remained as dour as our pre-laughing ancestors, we would still have had to survive in order to be able to theorize about our lack of laughter. Applying a counterfactual test to the evolution of laughter does not take us much farther into its relationship with medicine and health.

8.6 Keep Breathing Hypothesis and Clarity Hypothesis

As noted at the start of this discussion, plausible evolutionary stories are easy to construct. Two new variations are constructed here, a one-dimensional account based on the characteristics of respiration associated with laughter, which is then extended into a second more multi-dimensional account.

Many evolutionary accounts would be unchanged if the signals of laughter
had evolved in alternative body parts, say ears, for example. This is not far-fetched. Consider how communicative are the ear movements of dogs and elephants. Human ears also communicate through blushing. Imagine then that humans laughed by twitching their brightly glowing ears. Theories of social communication might remain relatively unchanged if we exchanged the sight and swish of flapping ears for the sight and guffaws of laughter. Yet the purpose of this thought experiment is to remove the spirit of laughter by taking the breath away. With that in mind, it would be a small step to construct yet another variant of a release theory, one that focuses on the health effects of laughter breathing.

This new account could be based on the observation that laughter is always a forced exhalation, followed naturally by inhalation and consequent movement of air through the lungs. These actions could then be said to have all the pieces needed to fit an evolutionary framework. The adaptationist would look at laughter as a heritable trait with variation. Tickling presents its own evolutionary puzzles that will not be considered in detail here, including whether it is a simple reflex protecting vulnerable body areas, plays a role in family bonding or is combat rehearsal, among other hypotheses (Provine 2000).

The adaptationist would ask what medical problem is solved by laughter. A plausible story could be constructed around the adaptive advantage of this trait of forced exhalation. Those who breathe deeply are fitter than those who hold their breath. In stressful times, the fight or flight response would dominate. Relaxation would be rare. Those who inherited a laughter reflex would be forced to relax and gain all the health benefits of relaxation, increased air flow in the lungs and consequent oxygenation of the blood. The story would be plausible, as it is consistent with the advice of yoga instructors, physical fitness trainers and martial arts masters who all assert
that breath is the key to their respective endeavors.

Let's call this the Keep Breathing Hypothesis of laughter. The hypothesis could be simply stated: we laugh because it prevents us from holding our breath in times of stress. Based on the plausible story that our ancestors survived because they laughed to reduce stress, we could spin out details of respiration, aerobic and anaerobic metabolism, blood oxygenation, negative effects of breath-holding and so on. We could measure changes in the immune system correlated with breath-holding and exhalation. We could postulate that holding our breath during stressful times is a design flaw in the fight or flight mechanism. Laughter would then be a partial compensation for this flaw. Laughter in this account is a slap on the back, an encouragement to lighten up and keep breathing. We could say that in the evolutionary arms race, the selective advantage went initially to those who could fight or flee, then that advantage was lost to those who could fight, flee and also relax and breathe well. With the development of our vocal apparatus, the peculiar sound of laughter became associated with the deep breathing reflex. Individuals who had this breathing and relaxation advantage would not only be fitter but they would also attract mates, just as individuals who were ill would be avoided.

Such a Keep Breathing Hypothesis suggests other experiments. We could investigate whether people who do not breathe deeply or who hold their breath more frequently also die sooner or are sicker longer or more often. Our investigation would necessarily spill over into comparisons with crying, yawning and other forms of adaptive breathing, as well as relaxation techniques such as tensing and releasing muscles, and visualization techniques such as imagining being on a warm beach (see Fridlund and Loftis 1990, 143 and Darwin on laughing being related to piloerection, blushing and crying). Further investigations could compare the adaptive
consequences of laughter and deep, relaxed breathing in the Stone Age with today's environment.

Like many other accounts, however, this one-dimensional Keep Breathing Hypothesis also fails to recognize different levels from the molecular to the social and the interactions among and within levels. The hypothesis would be equally consistent with solitary breathing and social breathing. Yet laughter appears to be more frequent in social than solitary situations. We would have to modify the account to add a broader range of factors.

Let us then call the modified Keep Breathing Hypothesis the Clarity Hypothesis of the evolution of laughter. This new account would operate on many levels, not just the physiology of breath but also individual psychology and group interactions. Breathing, attention, sight, smell, sound, taste, touch, and the sense of incongruity, if there is such a thing, all would conspire to enhance the reproductive success of those who laugh. Of course, neither of these stories comes close to a satisfactory explanation. And they continue the prosaic tradition of the literature. All of this story-spinning is merely to say that we need a clear conception that models this complexity.

Some accounts of the evolution of humor and laughter, such as those reliant on false alarms and disabling mechanisms, are essentially one-dimensional. Owren and Bachorowski (2001) have a more nuanced selfish-gene, Red Queen competition account, but they still attempt to squeeze most of laughter and smiling into one box, that of animal signaling. Even more narrowly, they see signals as primarily selfish survival measures intended to influence but not to inform the recipient. Similarly Gervais and Wilson view laughter primarily as signaling, but from the perspective of ritualized play signals. They do seek to combine the views of multiple disciplines, including social neuroscience, positive psychology and play research, evolutionary psychology and multilevel selection theory. They also view evolution from
multiple levels, including group selection. Still they have not gone far enough
to capture the complexity of laughter in their synthesis. For instance, they
focus on the social aspects of signaling while passing over the physiological
effects of laughter. They say little about the dynamic interactions feeding
back and forth among levels (not to be confused with their other use of levels
in reference to group selection). They remain on a one-to-one or one-to-many
level of interaction. Laughter appears instead to be connected to a
multidimensional network of many events, functions, forms and interactions.

In discussions of evolution, the concept of multiple levels can have
multiple meanings. Gervais and Wilson fault previous researchers for not
emphasizing the multilevel nature of evolution, by which they mean selection
at the group level, not just the individual or genetic level. Yet they too have
not emphasized a different meaning of the multilevel nature of evolution, the
various levels at which the trait under consideration operates. Laughter
involves mechanisms operating at many layers from micro to macro,
including molecular, cellular, physiological, cognitive, affective, individual,
social and cultural. Gervais and Wilson fix their attention on the social
group level, generally leaving the other levels to fend for themselves. Humor
and laughter are too complex to understand without slicing and combining
them into a multiplicity of multiple levels. They cannot be reduced without
losing essential interactions.

Current conceptions of the origin of laughter are too simple, linear,
informal and conflicting to answer interesting questions about laughter,
medicine and health. Indeed they have little to say about medicine and
health. They give priority to one level, such as the social level, to the neglect
of other levels such as the physiological and psychological. How cultural
evolution of laughter relates to biological evolution is unclear. They differ on
the controversial question of group selection. Some assume that the evolution
of laughter provided general health benefits while also acknowledging that health is only a side show to the central drama of natural selection. Significant issues, such as the possibility that health effects of laughter are placebo effects, have not even been addressed in the context of the evolution of laughter. Evolutionary accounts of laughter need more work before they can help us understand medicine and health. Then for the time being let us continue our inquiry elsewhere.
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9. Limits and Laughter

In previous chapters, we encountered the complexity of laughter, medicine and health and the typically prosaic methods that are currently devoted to such a complex inquiry. We have seen that the connections among laughter, medicine and health remain unclear despite centuries of investigation. In particular we saw that the randomized controlled clinical trials at the core of evidence-based medicine have yet to capture the complexity of laughter in medicine. Even if laughter could be sufficiently controlled and measured, treatments that have been tested by clinical trials in conformity with the most exacting standards of evidence-based medicine are not subjected to conditions simulating the multiple personalities, treatments and comorbidities found outside the controlled confines of experiments. Perhaps there are no limits and complete understanding is only a matter of time; perhaps we just have to be patient. On the other hand, this chapter explores the possibility that there are limits to this inquiry and suggests what some limits and obstacles might be. Then a networked complexity view is offered that says some answers about laughter and medicine, if they are possible to provide, may not appear without the funding, non-linear modeling and related techniques that are devoted to other complex matters such as
meteorology and genomics.

One limit lies in prediction, and that includes predicting limits: it is perilous to use today’s limited knowledge to predict tomorrow's limits. Talk of limits may be self-contradictory, for to envision a limit is to envision what is beyond. Another limit is that any discussion of limits, including this one, will necessarily be limited. But let us begin.

9.1 Obstacles

It is useful to distinguish theoretical limits from practical ones. For convenience let us simply call them limits and obstacles. In this context, limits are those inherent aspects that cannot be overcome while obstacles potentially can be overcome. An example of a limit is the impossibility of exact measurement: no matter what the measure is, there is always an error factor that can never be eliminated. Obstacles include:

- Funding: compared to drug research, for example, the amount of funding for laughter research is, well, a joke. Imagine advertising to raise funds for laughter research when so many other and more urgent causes need money. Who will pay for the knowledge? Who will support implausible research? Laughter is not patentable, so private corporations are less likely to invest in laughter research than in drug research. "Most researchers in this area have noted that it is very difficult to obtain funding for humor studies, particularly those involving expensive physiological outcome measures" (Bennett and Lengacher 2008). Funding is just one of many influences (such as politics and the plausibility of the research) on understanding laughter and health.
• Mindshare: Although much attention has been paid to claims about the benefits of laughter, little attention has been paid to laughter research, perhaps because of sheer familiarity with the subject. As Ruch and Ekman (2001) note, "Laughter is a conspicuous but frequently overlooked human phenomenon" Laughter does not seem to be mysterious. It isn’t revered like rocket science. What else is there to know? Laughter research is often deemed to be lightweight and, not surprisingly, insufficiently serious to warrant further inquiry.

• No discipline: Laughter doesn’t fit neatly in a discipline, department, faculty or focal point of inquiry. Boundaries around laughter research are elastic, as evidenced by the need for awkward hybrids to describe possible disciplines into which laughter falls, like psychophysiology, or biopsychosocial or mind-body medicine. Nobody takes ownership of laughter. Part and absentee owners of laughter research range from the sciences to the social sciences to the humanities to the arts.

• Publication bias: in a sense all publications are biased in that they attempt to screen out the unpublishable, however that term is defined. Reports of scientific research tend to be biased toward positive results (Dickersin 1990, Koren and Klein 1991, Scargle 2000). Despite praising systematic reviews, Bausell (2007, 203) complains, "that positive trials tend to populate systematic reviews, while negative trials tend to populate their investigators’ file drawers." With respect to laughter research, positive bias has at least three meanings. The first meaning is technical, connoting statistically significant results. Researchers and publishers prefer to report evidence of a correlation as opposed to absence of evidence. The second meaning is colloquial, connoting good, as in positive emotion. Laughter is assumed to be good. How could anyone not write positively about laughter and hope? It is contrary to
common wisdom to draw negative conclusions about laughter; common wisdom does not allow an easy explanation of mocking or derisive laughter (as discussed in the chapter on the serious negative turn in laughter research). It is counterintuitive to abandon the plausible story that laughter is good. A third meaning of positive bias is the generally accepted idea that everybody prefers good news.

Although obstacles to inquiry deserve attention in their own right, the following discussion will focus on limits, with an emphasis on a complexity view of laughter, medicine and health.

9.2 Limits

One general reason for our limited understanding of laughter in medicine and health is that there are limits to science. As Barrow notes, this may or may not be a comforting thought:

Any talk of limits to science will alarm many people and comfort others. There are some who would equate the very idea of limits to scientific knowledge with a violation of our freedom of thought and action. Limits of cost are one thing, but absolute limits are surely something completely different. Show me one of those and I’ll jump over it, tunnel under it, or simply skirt round it. Yet, the more we try to grasp what science is, and how it relates to the activity of human minds, the more we are drawn towards the possibility that limits might be deeply rooted in the nature of things (Barrow 1998, 190).

Barrow focuses primarily on the basic sciences, in which many limits have been recognized, such as Heisenberg’s uncertainty relation, the speed of light and the impossibility of perpetual motion machines that violate the second law of thermodynamics. These limits appear within the context of current theories, and it is possible that future theorists will find creative ways to revise or circumvent them. Laughter, medicine and health are naturally
bounded by such underlying physical and computational limits, whatever they may turn out to be.

In addition to limits from basic science there are conceptual limits. A common response to incomplete understanding is that we need more research. So it is no surprise to encounter a constant demand for more research in the laughter-humor-medicine-health literature. Examples include this observation from two decades ago: "The actual connection between humor and healing remains rather sketchy, but testimonies, case histories and a growing belief in mind-body interactions warrant further research" (Haig 1988, x). Ten years later, Watt et al. (1998) reviewed the evidence related to wellness programs including those using laughter. They concluded, "Clearly, further research is needed to obtain convincing answers to the questions of whether these programs have scientific validity, reduce health care costs and enhance a patient’s quality of life." A more recent review asks, Does this mean that researchers should abandon the idea of health benefits of humor and laughter? On the contrary, given the long-standing popularity of this idea and the importance of such a finding if it does exist, I would argue that the current state of the research calls for greater research efforts, employing more careful theoretical formulations and more sophisticated and rigorous methodological approaches (Martin 2004, 14).

The serious of four review articles discussed earlier ends with a similar plea: Research results concerning humor and healing are thus far rather tentative, and more work is needed before broad claims can be made concerning an effect of humor upon health outcomes. (Bennett and Lengacher 2007).

While these efforts and exhortations are admirable for their persistence, we must beware of assuming the Laplaceian possibility of perfect knowledge or ever conducting the mythical definitive study. Medicine is by nature uncertain (Fox 1980). The study of laughter, medicine and health is like the study of weather and climate. Indeed in many respects, the present state of medicine resembles the distant past of weather forecasting:
A century ago, weather forecasting was a haphazard process, very imprecise and unreliable... forecasting was more an art than a science.... The forecaster used his experience, memory and a variety of empirical rules to produce a forecast map (Lynch 2008, 3432).

Medicine has become more refined, as weather forecasting has, but within limits: as the complex chaotic nature of atmosphere and oceans limits weather and climate knowledge, medical and health knowledge too is limited by the nature of living organisms and their internal and external environments. Any inquiry into laughter, medicine and health is limited by these underlying dynamics.

Recall the frog metaphor for the study of humor: "Humor can be dissected, as a frog can, but the thing dies in the process and the innards are discouraging to any but the pure scientific mind" (White 1941). The metaphor of frog dissection and killing readily encompasses both humor and laughter. To kill laughter or kill a joke are common expressions. A frog reduced to anatomical parts is a dead frog. A joke dissected and explained is not funny. We might learn something from dissecting a frog or a joke, and harm no other frogs or jokes in pursuit of our goals, but we are unlikely to laugh during the surgery. Long before White's widely quoted statement, John Stuart Mill's version of the frog metaphor was, "Following life, in creatures we dissect, We lose it in the moment we detect" (quoted in Schaffner 1993, 153). Such deadly reduction is also as much of a concern at the level of populations and statistics as at the level of the individual frog. According to Hilaire Belloc, "Statistics are the triumph of the quantitative method, and the quantitative method is the victory of sterility and death" (Gould 1985, 40).

The dead frog raises the issue of what might be termed meta-reduction, or the reduction of science and medicine to reductionist thinking. Various holistic approaches to medicine have been proposed as being capable of addressing the whole live frog. They often adopt metaphors from the arts and
humanities that are viewed as being the opposite of reductionist. For example, Evans (2007) considers the relationship between music and clinical medicine. He contrasts what he terms an existential relationship versus scientific reductionist and biological relationships such as physiological explanation of music and clinical application of music in diagnosis and therapy. On the existential side he refers to broad notions including harmony, balance and wellbeing. The existential is not scientific but is experienced as specific, personal and subjective. Science in contrast is general, impersonal and claims to be objective. In this affirmation of a cultural divide between science and the humanities, Evans is saying that science is limited when it comes to music. Explaining and understanding music and medicine musically is set off against explaining and understanding music and medicine scientifically. For Evans, considering medicine musically amounts to making metaphorical comparisons between music and medicine. That in turn, he claims, leads to affirmation of patient-centered medicine. Both music and laughter are metaphors for such things as happiness and wellbeing (in expressions such as the music of life, for instance). Of course laughter and music are not in exactly the same position with respect to medicine, although both are used in various medical therapies. For one thing, laughter occurs in everyday conversation while music does not (apart from the metaphorical music of language). Laughter also occurs almost everywhere in clinical settings (Robinson 1977) while music does not. Laughter itself is often the product of metaphor in humor. Nevertheless, assuming without deciding the validity of the meta-reductionism that reduces science to reductionism, we reach a limit if complex entities can neither be fully understood by reducing them to their parts nor by leaving them untouched in their mysterious wholeness.

More than a century ago, the French philosopher and psychologist T. H.
Ribot expressed this view:

If we were to recount only a few of the numerous definitions of laughter current in books, we should find none that was not in some way open to criticism, because there is none which embraces the question in all its manifold aspects ... In conclusion, laughter manifests itself in circumstances so numerous and heterogeneous physical sensations, joy, contrast, surprise, oddity, strangeness, baseness, etc. that the reduction of all these causes to a single one is very problematical. In spite of all the work devoted to so trivial a matter, the question is far from being completely elucidated (1897, 352).

Are the medical holists and reductionists simply looking at different views of the same elephant (or frog)? Medical reductionists slice into tissues and peer into microscopes to find explanations in the cells and molecules. Medical holists look for emergent features that are impossible to find in the dissected parts and consider what’s going on in a connected universe, environment, society and family surrounding the beast. They pick different levels of intervention. Explanation and understanding at one level does not automatically transfer to another level.

Moreover, "the problem of the intrinsic limits of explanation is exacerbated by a tendency to overestimate the depth of our own understandings. It is bad enough that explanations have indefinite depth; it is much worse if we realize too late when we are out of our explanatory depth." (Keil 2005, 235). As we have seen, Norman Cousins, Stephen Jay Gould, E.B. White and others who offer narrative, anecdotal, phenomenological approaches appear to agree: science experiments do not explain laughter and humor. These writers do not even bother to enter the holist-reductionist debate. Instead of frog experiments they rely on frog stories. Theirs are like religious miracle stories that are simply beyond science or medicine (see for example Stempsey 2002). In the case of Cousins’ or Gould’s accounts of survival despite the odds, their versions also fold
selected scientific results into quasi-medical explanatory stories. They may speculate about the mechanisms involved in their survival but they avoid reducing their stories to mechanistic accounts. Yet a quintessential medical reduction is the idea of a specific cause for a specific disease.

Under this conception, as exemplified by Koch's famous postulates in the 19th century, preventing or curing disease is reduced to finding and removing a single necessary, if not sufficient, cause. So for example hand-washing removes germs that cause disease hence hand-washing is a way to prevent disease. Despite its obvious oversimplification, this form of reduction remains central to medicine and is one of the foundations of the current focus on genetics and individualized medicine (see for instance Conrad 1999). At least three conceptual reductions are contained within such a linear theory of disease or similar notion of specific etiology:

a) single cause versus something broader
b) physical disease versus something broader
c) a split between disease and illness

These reductions are interrelated. For instance if disease is conceived as dis-ease, being ill-at-ease, then dis-ease blurs the divide between disease and illness, objective and subjective, physical and mental.

That is, alternatively:

a) a disease is likely to have multiple causes
b) the mind could also be involved
c) a patient could feel ill, dis-eased, in the absence of observable causes

These and other possibilities quickly multiply into a complex nonlinear network. A simplistic network conception is that a germ is causally connected to disease:

\[ \text{germ} \rightarrow \text{disease} \]

Disease here is in the body, not muddied by the mind. To be more complete,
this conception would have to grow from a one-to-one relation into a one-to-
many relation:
  germ -> physical + mental
or
  germ -> physical -> mental
or
  germ -> mental -> physical
These simple linear relations would have to be enhanced to show feedback from the physical and mental effects of germs that influences the germs themselves through, for instance, the immune system. As soon as we add the mind to the model, the complexity explodes:

  The absence of a designer and millions of years of tiny sequential changes have shaped a mind that is not just complex but indescribable by words and concepts simple enough to be satisfying (Nesse and Ellsworth 2009, 132).

Also a germ does not always cause disease in every person and germs might operate together. So we could model the situation with a many-to-one relation:

  some combination of germs
  + some environmental conditions
  + various conditions of the person
  -> disease

Putting these models together, we get a many-to-many model in which many factors influence many effects that feed back on the influencing factors:

  some combination of germs
  + some environmental conditions
  + various conditions of the person
  -> physical effects + mental effects (grouped into a disease category).

This model is also unrealistically linear and ignores feedback and other
interactions. To assume a discrete germ entity is also problematic. We could unpack the germ entity in each model to further multiply the complexity. Still the model would be incomplete. For example, since it is a disease model, it does not adequately address the problem of illness. This is yet another branching that adds to the complexity of the model which Cassell (2004, 45) expresses in these terms:

Problems of staggering complexity arise when we attempt to understand the known dimensions of person and their relationship to illness and suffering. These problems are no greater than those initially posed in trying to find out how the body worked. The difficulty is not how to finish solving the problems-- it's how to start.

It is also a question of how to express the complexity. As the examples show, a model rapidly becomes unwieldy when even the most basic branching is represented.

Laughter, as well as associated notions such as amusement, humor and exhilaration, would have to be incorporated into a better model if we are to understand how laughter influences disease and illness. The model would have to show various branches including different types of laughter, also separating laughter as comfort from laughter as cure. Implicitly or explicitly the nature, goals and scope of medicine would have to be incorporated to address laughter’s role in relation not only to disease but also to suffering and personal relationships. The model would would have to distinguish laughter as medicine from laughter as an influence on health and again from laughter as a part of life in general. It would also have to specify whether medicine is conceived narrowly, as a pill that can be swallowed, or broadly as a whole medical care system, and whether medicine has any limits short of health, or whether the scope of medicine includes all matters of health and happiness. The World Health Organization (2006) definition of health, unchanged since 1948, expands medicine to include a state of complete
physical, mental and social well-being. Although this is not the forum to resolve longstanding debates about medicalisation, a discussion of limits cannot avoid considering just how much of human activity is deemed to be medical (for a brief introduction see Metzl and Herzig 2007).

Rose (2007, 701) emphasizes the broader social context of medicalisation. Referring to psychiatric drugs for depression, he writes,

Such a medicalisation of sadness can occur only within a political economy of subjectification, a public habitat of images of the good life for identification, a plurality of pedagogies of everyday existence, which display, in meticulous if banal detail, the ways of conducting oneself that make possible a life that is personally pleasurable and socially acceptable. In engaging with these formulae in inventive ways, individuals play their own part in the spread of the diagnosis of depression and shaping new conceptions of the self.

The same might be said of the medicalisation of smiling and laughter. Hufford (2003) points out that such remedies as fresh air, chicken soup and prayer do not need a physician’s assistance. Hufford concludes that, "the inflation of medicine to mean all valid health knowledge and practice, though an understandable product of its success, is disastrous to medicine" (2003, 210). Although he does not explicitly refer to laughter, he probably would include laughter in his list of things that may be healthy but are not necessarily medical. In any event, the boundary must be drawn somewhere.

To the extent that laughter in medicine and health can be addressed using science, the breadth and complexity of this inquiry suggests the kinds of research programs that are designed to manage complexity, such as those applied to weather forecasting and the various human and animal genome projects. A common element in the success of those projects is the use of computers to reach beyond human limitations. This is not to say that extending the mind with artificial computing power is the only approach. On the contrary, in many circumstances, "fast and frugal" heuristics may be
helpful (see for instance Gigerenzer 1996). But a research model can be more
detailed and inclusive than a day-to-day clinical decision model.

Or to return to the weather analogy, "red sky at night, sailor's delight"
may be an adequate heuristic for a picnic, but when tracking a hurricane,
more powerful tools are required. Many claims have been made about the
health benefits of laughter. They are often in the form of a simple syllogism
or linear transitive relation such as:

increased respiration is healthy
laughter increases respiration

therefore laughter is healthy

Replace "respiration" with "exercise" and you get Cousins' (1979, 145) claim
about laughter being "jogging for the innards."

exercise is healthy
laughter is a form of exercise

therefore laughter is healthy

Or substitute some other healthy attribute that is not a characteristic of
laughter itself (as respiration and movement are) but something claimed to
be a consequence of laughter. For example:

a boosted immune system is healthy
laughter boosts the immune system

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so laughter is healthy

The syllogism may be logically valid, but its questionable premises render it unsound. Even if the premises were true, simple logic cannot provide the basis for understanding and explaining complex phenomena. The limits of such a simple linear approach to complex nonlinear phenomena are analogous to linear forecasting of weather and climate phenomena. For instance, the maxim "clear moon frost soon" has a similar structure to "laughter is the best medicine":

lack of cloud at night permits cooling that could lead to frost  
a clear moon occurs on cloudless nights  
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so frost may form under a clear moon

This logic won't help if the conditions are not otherwise conducive to frost, say, in the tropics. For general application, so many unstated assumptions and qualifiers need to be considered that the overt simplicity is misleading at best. Still, it's a plausible heuristic as are the purported connections between laughter and health. Few discussions of laughter, whether in ordinary conversation or in the scholarly literature, note the limits of such linear conceptions or even make a distinction between linear and nonlinear models.

Much of laughter research is of the red-sky-at-night variety: laughter as the best or second best medicine. There it will likely remain without the help of more sophisticated models and simulations. And even if massive scientific research programs were devoted to laughter, still another bifurcation of the model would be required that illustrates a limit. The model would have to show the areas that are not reachable by science, and specify where the
unreachable boundaries of medicine lie. There might be blank areas, as in old maps, labeled "there be dragons here" for it is hard to see how a scientific approach could contain within its own models those aspects of medicine that are not scientific. This is more than a question of viewpoint. It reflects the general problem of a system that tries to be both complete and consistent. This is also a reflection of the general question whether medicine is science or something else (for example Cassell 2004, Montgomery 2006, Dixon 1978).

To claim that laughter and humor are complex phenomena is nothing new. For instance, in a review of hypotheses about connections between laughter and physical health, Martin (2004, 2) writes, "Humor is a very complex phenomenon, involving cognitive, emotional, behavioral, physiological, and social aspects." Berger (1995, 169) says that the complexity of humor is what makes it difficult to study and demands a multidisciplinary approach. Berger compares humor to the elephant of the famous poem about six blind men observing parts an elephant. He supports his view by quoting from research of a generation earlier that also emphasized the complexity and variability of roles and functions of humor. Each generation rediscovers the complexity of laughter in its own way but none so far has tamed it.

Writing almost a century before Berger, Sully (1902, 24) declared of laughter, "The subject so conceived is a large and complex one, and it will be hard to deal with it at once thoughtfully and familiarly, with the genuine ring of laughter ever present to the ear. The present writer will account himself happy if, in a line where so many appear to have missed success, he attain to a moderate measure of it."

In reference to locating laughter by brain stimulation, Wild et al. reached a complexity limit in their brain research: "In those conditions in which pathological laughter is part of a global behaviour pattern (i.e. in which the laughter is congruent with a feeling of exhilaration), issues of causality are,
at present, simply too complex for analysis..." (Wild et al. 2003, 2122). They explain why the phenomenon of laughter is "simply too complex" to provide meaningful results from direct brain stimulation:

To summarize these findings, although over half a century has passed since Penfield first stimulated the cortex during a brain operation, and although the technique of transcranial cortical magnetic stimulation has been widely applied as an alternative method of focal cortical stimulation, there have been few reports of induced laughter during these procedures. One possible reason for this is that laughter may be so complex a phenomenon that it cannot normally be stimulated from any single brain area ... (Wild et al. 2003, 2129).

It's too easy, however, just to say that the world, and laughter, is complex and cannot be reduced to simple ontologies and linear relationships. Teasing simplicity out of complexity is the daily trade of science, so what could make laughter immune to inquiry? Science is an activity of finding simple patterns in confusion. Much of science would not exist without simple models of complex phenomena (see for instance Boyd and Richerson 2005, 397ff).

Indeed, "conscious and cautious over-simplification, far from being an intellectual sin, is a prerequisite for investigation. We can hardly study at once all the ways in which everything is related to everything else." (Goodman 1983, xx). But to be tractable, the patterns have to be at an appropriate level of generality. A claim that laughter is the best medicine, or merely good for health, is too general, yet a specific claim that laughter increases respiration rate is no more insightful than saying if you aren't breathing a lot then you aren't laughing. Neither claim does any work.

An exception to the linearity of models in the literature is the brief discussion by Paulos (1980) who attempts to explain jokes and humor using the topological models of catastrophe theory, a precursor to chaos and complexity theory. He uses nonlinear topology qualitatively as a metaphor for incongruity, ambiguity, and the sudden discontinuity associated with jokes
and humor. But his focus is modeling humor so he does not specifically address laughter, medicine or health. Jantar (1999) subsequently draws a connection between chaos theory and Bergson’s theory of laughter. For instance, he compares Bergson’s metaphor of life’s turbulence with the physical notion of turbulence. But like Paulos, Jantar is concerned with the nature of laughter, not with its role in medicine and health.

More recently, an approach centered on nonlinear system dynamics has been applied to questions about the benefits of positive emotions. Fredrickson and Losada (2005) distinguish between flourishing and languishing, terms that describe mental health. They claim that, "The commonalities between affect systems and nonlinear dynamic systems raise the possibility that the complex dynamics of chaos underlie the proposed link between positive affect and human flourishing" (2005, 679). More specifically, they hypothesize that the ratio of positive to negative affect can predict human flourishing. Their analysis was based on daily responses from 188 participants over 28 days about the extent to which each participant felt each of 20 emotions. "Positive emotions included amusement, awe, compassion, contentment, gratitude, hope, interest, joy, love, pride, and sexual desire. Negative emotions included anger, contempt, disgust, embarrassment, fear, guilt, sadness, and shame" (2005, 682). Although this work does not specifically address laughter, it suggests a similar approach for laughter research.

A complexity view has also been applied to various aspects of medicine. For instance, Wilson, Holt and Greenhalgh (2001) apply complexity theory to clinical practice, health organizations, education, research and professional development. In reference to clinical practice, they describe complexity in these terms:

We all know from experience that the management of clinical problems is rarely simple. Yet most of us were taught about and tend to adopt a mental
model of the human body as a machine and illness as due to malfunction of its parts. Such linear models drive us to break down clinical care into ever smaller divisions and to express with great accuracy and precision the intervention to be undertaken for each malfunction.

Complexity science suggests an alternative model—that illness (and health) result from complex, dynamic, and unique interactions between different components of the overall system. Effective clinical decision making requires a holistic approach that accepts unpredictability and builds on subtle emergent forces within the overall system (2001, 688).

Indicia of complex systems include sensitivity to initial conditions, nonlinearity, network interactions, reflexivity and emergence (see for example Nunn 2007). Laughter readily fits into a complexity viewpoint. For instance, laughter is reflexive: a funny situation generates expectations of laughter that in turn breed laughter and more positive assessments of the situation (Leventhal and Mace 1970). Linear systems are ordered. Small changes produce small effects, big changes big effects. Laughter is not so ordered. Big and frequent laughs may come from small or large stimuli, and the effects of laughter also appear to be chaotic. After someone laughs at an image, joke, metaphor, or idea, the same stimulus no longer works in the same way. Linearity is also linked to measurement. A common linear assumption that often fails in medicine is that some is good so more is better. This failure is exemplified by the common assumption that a small dose of a drug has a small effect so a bigger dose has greater effect. Or the assumption that jogging is healthy so running a marathon is healthier. Neither assumption is valid. With laughter, what can be quantified and tested for linearity? Not much, yet we often get simplistic linear extrapolation: a laugh feels good so more laughter must be more healthy. Complex systems are difficult to dissect (the frog metaphor again). Laughter too emerges as something different from its parts. Indeed, it is difficult even to enumerate the parts of laughter.
When laughter is considered not in isolation but in relation to medicine and health, the complexity increases. For example, the brain, commonly modeled as a complex network of networks, exhibits indicia of complexity (for instance Sarbadhikari and Chakrabarty 2001). The network conception has been extended to all living tissues (for instance Vertosick 2006). Health itself may be defined in complexity terms: "The health of a population can be viewed as a complex adaptive system" (Pearce and Merletti 2006). The point is not to enumerate all of the indicia of complexity in laughter, medicine and health, which could occupy a book or more of its own, but only to sketch a complexity view of laughter, medicine and health in the context of limits. Complexity limits the extent to which we can know what works in laughter, medicine and health.

Another limit is the number of things that can be examined and tested at once. Measuring multiple factors is likely to lead to more erroneous results (Ioannidis 2006). This is one of the general limits to experimentation. Yet complexity theory itself arose with development of computers that can track multiple factors. Ironically we may need machines to reach farther into understanding and explanation of the very human act of laughter. Indeed, machines may help to humanize medicine generally by extending population science to a science of the individual (for instance by tracking individual details) and in particular by modeling individual genomes and epigenomic interactions.

Evidence-based medicine also has limits that have been enumerated in a growing critical literature. As Upshur (2000) points out, evidence will always be limited. Evidence-based medicine is itself based on the empiricist methodology of classical statistics that privileges the population over the person. No individual person matters in a randomized controlled trial. An individual metric that stands out from the crowd may be ignored as a mere
outlier or error. Straus and McAlister (2000) searched the literature for criticisms of evidence-based medicine, from which they list "limits universal to the practice of medicine" and "limits unique to evidence based medicine." Their categories for universal limits are, "shortage of coherent, consistent scientific evidence", "difficulties in applying evidence to the care of individual patients", and "barriers to the practice of high-quality medicine." Their categories for limits unique to the practice of evidence-based medicine are, "The need to develop new skills", "limited time and resources" and "paucity of evidence that evidence based medicine 'works'." Among the proposals to address these limits are calls for a more complexity-based medicine (for example Grossi 2007, Helgason and Jobe 1999).

Advocates of complementary alternative medicine have turned to alternative research programs described by such terms as whole systems and complex interventions (Boon et al. 2006). Whole systems are:

approaches to health care in which practitioners apply bodies of knowledge and associated practices in order to maximise the patients’ capacity to achieve mental and physical balance and restore their own health, using individualised, non-reductionist approaches to diagnosis and treatment. In whole systems the practitioner–patient relationship plays an important role and continues to evolve over time (Verhoef et al. 2004).

Methods for actually doing whole systems research have yet to be defined in detail (Boon et al. 2006, Bell and Koithan 2006). Proponents invoke concepts from complexity theory, arguing that complex emergent interventions cannot be evaluated by reductionist methods, while critics say that complexity theory will not alter the evidence that complementary alternative methods often do not work better than placebo or conventional medicine (Bell and Koithan 2006, 303). Absent details, there is no way to assess whether these alternative models and methods constitute a better game plan or are just rigging the rules of the game as a means to an unsubstantiated end. Still, for
greater understanding of the role of laughter in medicine, a better game plan is essential.

A cluster of theories about thinking and deciding also points to limits. Elements of the cluster of cognitive concepts include bias, memory and time limits. For instance Keil (2006, 235) explores intrinsic limits of explanation and understanding in broad terms:

Explanations are ubiquitous, come in a variety of forms and formats, and are used for a variety of purposes. Yet, one of the most striking features about most explanations is their limitations. For most natural phenomena and many artificial ones, the full set of relations to be explained is enormous, often indefinitely large and far beyond the grasp of any one individual.

He also points out that, "the problem of the intrinsic limits of explanation is exacerbated by a tendency to overestimate the depth of our own understandings. It is bad enough that explanations have indefinite depth; it is much worse if we realize too late when we are out of our explanatory depth." No satisfactory explanation of the health effects of laughter has been provided, although many overestimates of understanding have been offered.

Placebos present their own limits, so to the extent that laughter is the best placebo, this is another source of limits. For instance, medical interventions such as acupuncture that cannot be strictly controlled will always be subject to bias (Bausell 2007, Evans 2003). Even if the patient can be fooled, the acupuncturist knows who is getting real needles at the conventional locations and who is getting fake insertions and insertions at the wrong locations. This limit applies to any intervention that relies on the healer's presence. Little value is gained by so-called single-blinded studies in which only the patient doesn't know what is happening. If the healer knows who is getting the placebo, then there is no accounting for the healer's knowledge and no telling what the patient believes or learns about what is
happening. Laughter is also subject to placebo limits. For instance, compared to which placebo group do we test the effects of laughter? We have seen in the discussion of the serious negative side of laughter that people can laugh in almost any situation including at funerals and during torture. Perhaps no human grouping is ever without the threat of laughter breaking out; human nature abhors a laughter vacuum.

Limits are also a function of the claimed benefits. If the benefits of laughter are narrowed to plausible ones (such as social contact, relaxation) that do not reach too far (say curing cancer, or any other disease) then laughter becomes a plausible intervention for those limited uses. What if laughing merely makes people feel better, with perceived personal and social influences but no measurable influence on disease? Does that exclude laughter from medicine altogether? Sufferers can always decide to take the conventional treatments such as surgery, radiation and chemotherapy together with the laughter. But then it's hard to say laughter adds to the mix.

When the patient becomes a consumer of medical services then science alone does not determine the course of the medical experience, nor do expert judgments. In the case of laughter, Norman Cousins provides a textbook example. He had strong views about the course of his illness and about laughter in particular. He explicitly stated his preference for anecdote over statistics. He explicitly demanded not to be doomed with talk of probabilities. He preferred to do what he thought was right, even if it conflicted with science, and to some extent because it conflicted with science. To him, what works in medicine cannot be determined in isolation from what satisfies the patient.

The negotiation between his views and the views of experts illustrates another limit of science. Science is based on scientific evidence not patient values and preferences. Comparing conventional and alternative medicine,
Bates (2002, 26) goes so far as to say that, "a biologically and scientifically oriented medicine cannot avoid falling short of full patient satisfaction, however much it may try to do otherwise" [italics in the original]. The individual patient’s values are not readily captured by the impersonal, statistical, quantitative, experimental, pharmaceutical, silver bullet, quick fix model of science. Moreover, individuals do not see themselves as replicated biological mechanisms that can be plugged into a medical system of technological instrumentation and biochemical fixes.

The complexity of laughter and its relationship with medicine and health raises the general issue of limits to individual understanding. Facing such complexity, can any single person claim to understand more than a fraction of the whole project? If not, then solutions may require much larger teams and organizations than are currently devoted to laughter research. This limit is illustrated by the teamwork that produced the computer-assisted solution of the famous four-color problem. The question of how few colors are needed to draw a map had long puzzled mathematicians. In 1976, a solution was provided. The story of that solution is full of unlikely statistics: 10 million person hours (MacKenzie, 1999, 9), a single computer session of 1200 hours at $1000 per hour, over four months of hand checking by the authors and their family members, a final paper of 140 pages with 400 additional microfiches (MacKenzie, 2000, 129ff). This problem helps to put laughter research in perspective. All of those resources were devoted to a small discretely contained intellectual puzzle about the minimum number of colors for drawing a map. Such dedicated resources are rarely if ever applied toward solving even the tiniest puzzle related to laughter in medicine. If they were, similar questions would arise as they have in many computer-assisted matters: does any single person really understand the solution? If no single person has the answer, then the individualistic approach of Norman Cousins
who distrusts experts and shared knowledge and so heads off heroically to do it alone will seem less plausible.

A recent survey of physician's epistemological beliefs and knowledge suggests that many physicians acknowledge limitations to their knowledge:

Science does not admit relativistic or subjective attitudes in relation to the knowledge of reality, but rather general and universal ones. For that reason, it is quite remarkable that almost half of respondents chose the relativistic position, e.g., knowledge is possible, reality does exist, but nobody is the owner of truth, each has its own opinion and nobody can impose it to others. Only one fifth of them chose the realistic-authoritarian enunciation, e.g., knowledge is possible, reality exists independently from us, universal truths are possible, as well as their acceptance by everyone. If the above reflects respondents' beliefs and attitudes, it reveals certain incompatibility with universality, objectivity and authority, which are characteristics of the scientific paradigm. (Peña et al. 2007).

Then there is the problem of representation. Only rarely are models or numbers or diagrams or sounds or simulations, to name a few non-verbal alternatives, used to represent what we know of laughter in medicine. Despite the complexity of the subject, words are the workhorses of laughter research. Limited representation is a current constraint on laughter research, but one that can be overcome. As tools from other disciplines are borrowed and newer tools are developed, non-verbal methods will no doubt add to laughter knowledge. This is not to suggest that words are incapable of representing complex systems. On the contrary, in some ways linguistic models are ideal for representing complexity (see for instance Pattee 1977). Yet recall the weather forecasting analogy: without mathematics, computer simulations and other non-verbal expressions, we struggle, as in this very inquiry, with maxims such as "clear moon, frost soon" and "laughter is the best medicine." The very simplicity of the claim that laughter is the best medicine enhances its plausibility if simple models are the accepted conception of science.
To return to Barrow's treatise on impossibility that began this discussion of limits, consider what he calls metaphorical impossibilities. He cites Myhill's claim that, "No non-poetic account of reality can be complete." In support of this limit, Barrow writes:

Not every attribute of things is listable or computable. The property of being a true statement of arithmetic is an example. Attributes which are neither listable not [sic] computable are called 'prospective': they can be neither recognized nor generated in a finite number of deductive steps. They show that there is a place for ingenuity and novelty. There are things which cannot be encapsulated by any finite collection of rules or procedures. Beauty, simplicity, ugliness, and truth are all prospective properties. There can be no magic formula that can generate all possible examples of attributes like these, even in an infinite lifetime. They are inexhaustible. No program or formula can generate all examples of beauty or ugliness; nor can any program recognize them all when it sees them, and nor can we, in the way that the romantics imagined (Barrow 1998, 215).

Laughter, like beauty and truth, may be at this poetic boundary between science and the humanities. And that statement itself is, of course, too simple to adequately express the limitations of inquiry into laughter, medicine and health. A simple way out of the problem would be to deem any question at the limit of inquiry to be illegitimate and thus define our inquiry by what can be answered: if there is an apparent limit then anything beyond the limit is deemed to be only philosophical, not scientific or medical. We would then be accepting that we are constructed to be unable to solve problems of mind and body scientifically but perhaps only philosophically (as for example posited by McGinn 1989). But such a legislative approach only pushes the problem into the background to provide an illusion that there are no limits.

Another way out is to think harder, create new approaches, change viewpoints, and perhaps along the way prompt current theories to tumble in a cascading rearrangement. This is what Darwin did when he broke through the limits imposed by non-evolutionary conceptions of species, and what
Einstein did when he reconceptualized space and time. Laughter needs a new view. To conclude this discussion of limits on a positive note, and so be consistent with the positive bias in the literature, limits to understanding can be seen as an opportunity. Limits are generators of ideas, as is partly expressed in the maxim that necessity is the mother of invention. Examples of limits from physics, biology and psychology included the speed of light, the limits of exponential growth that have shaped biological and economic theory and the limits of human memory that determine much of what we do both within our minds and with extensions to our minds such as paper and computers. These and other limits spur creativity. Limited by the speed of light, for example, can we warp spacetime as in science fiction to bring the destination closer? Complexity is not just about limits and constraints but is also a way of looking for simple relations to explain complex phenomena, including the emergence of life and other wholes emerging from their parts. Perhaps the complexity of laughter, medicine and health will save it from the fate of the dissected frog. Limits to understanding of laughter, medicine and health are likely to spark creative, novel and, with luck, even funny ideas.
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This inquiry has explored laughter in medicine as a case study of medical knowledge and its limits. We began with the continuing popular and scholarly interest in laughter as medicine that motivates this research into unorthodox medical beliefs and practices. The inquiry was then situated within philosophy of medicine, where we considered the uncertain boundaries around both that subject and its own subject, medicine.

Looking at laughter from the viewpoint of evidence-based medicine, the current orthodox standard of what works in medicine, provided little evidence for claims that laughter works in medicine. Although randomized controlled trials have been deemed to be the most highly valued evidence, they have not penetrated far into the mysteries of laughter. In any event, as part of a larger picture, such experiments are ultimately woven into plausible stories. No single trial is sufficient evidence to support a convincing and conclusive story that something works in medicine. Systematic reviews and meta-analyses help to solidify the story. Reviews of systematic reviews add more weight. But the skeptical nature of science ensures that no story remains unchallenged for long. Much of the story of laughter has yet to be told in any form let alone systematically.
Prominent story-based views of laughter led to more personal accounts behind the belief that laughter works in medicine than what has been provided by the impersonal randomized controlled view of evidence-based medicine. There is an ever-present danger of confusing comfort and cure. Medicine includes both. Laughter at times is a comfort and at times is a source of suffering. Is it ever a cure? The answer is equivocal although the balance of current authority says laughter does not cure disease. But that doesn’t eliminate laughter’s role in medicine and health.

The exploration of laughter language showed the difficulty of delimiting the network of concepts associated with laughter. The current scientific picture of laughter and those connected concepts, such as emotions associated with laughter, remains only a preliminary pencil sketch rather than a fully painted portrait.

Probing into placebo disclosed a plethora of puzzles. Can we know if laughter acts as a placebo? The answer is contingent upon accepting the outdated notion of placebo. Much of medicine remains mysterious. Until more is understood about the fundamentals underlying the mysteries that are now the collected stories of placebo, nothing is to be gained by calling laughter, or anything else, a placebo.

Inquiry into the serious negative side of laughter sifted through rarely acknowledged costs and risks of laughter. There is an unwarranted presumption in favor of the medical and health benefits of laughter. Everyday experience supports this plausible idea. We feel good when we laugh. This presumption shifts the burden of proof. Before putting a new drug treatment into practice we have some idea about what makes it work and above all whether it is safe and effective. With laughter the answers to these questions are often presumed. Instead of asking, “What makes laughter any more healthy than, say, crying or heavy breathing, or conventional
questions about laughter are permitted to leap into practice and practical questions, such as "How can we use laughter in medicine?" The presumption stifles inquiry. Naturally the world wants laughter to be harmless and healthy, whether it is or not. Nobody wants to bear bad news and few researchers want to be the messenger who gets shot for doing so, or perhaps worse, be accused of failing to get the joke.

An examination of the evolution of laughter revealed a variety of conflicting accounts, none of which satisfactorily probes the connections among laughter, medicine and health. Indeed, leading evolutionary theorists often appear simply to assume that laughter is positive and healthy. They know that laughter begins somewhere in the brain, for sure, but nobody knows when or how it got there or where to find it, let alone what specifically it contributes to human health.

Limits and complexity abound in any conceptualization of laughter, medicine and health. No wonder our knowledge remains sketchy. No wonder that researchers call for more research, as researchers are wont to do in all disciplines. There is much more to do. But without the tools, teams and funding to manage its complexity, the products of laughter research in medicine are likely to remain equivocal. It's not easy doing research in an undefined discipline that has low status and correspondingly low funding. We need more research but not more of the same. We need agreement. We need to develop more complex models so that, recalling the analogy to weather forecasting, we are not limited to the maxims that an individual or small group can manage. We need this if we are to take laughter research to the next level.

On the other hand there's also the view that we should lighten up and laugh. What's the big problem with laughter anyway that we have to worry about, let alone spend time and money studying? Maybe, as laughter
researchers, we just don’t get the joke. Maybe "laughter is the best medicine" can only ever be a slogan, a rallying cry, like "evidence-based medicine" that has inadvertently led to a serious deliberation. Maybe we should step back and stop analyzing laughter, medicine and health to death.

Meanwhile, even for skeptics, the story of laughter as medicine remains plausibly positive, simple, free, harmless, natural, holistic and therefore, presumably, healthy. These characteristics also seem apparent to anyone who has ever laughed; so experts and expert opinion about laughter, including this very inquiry, may seem to be a laughable idea.

To return to the pressing problem of how we know what works in medicine, we have seen in the case of laughter that the answer depends on which nodes are included and emphasized in a network of notions and how they are characterized and connected to provide answers. Science tells part of the story, anecdotes tell another part, and untellable background knowledge provides untold beliefs. The question how we know whether other things, non-laughter things, work in medicine, will also be illuminated by the network of themes discussed here.

It is possible that the emotions and sensations associated with laughter have generated the idea that laughter is the best medicine and all the theorizing in the world cannot change this embodied knowledge. But that possibility hints at an embodied confirmation bias as the basis for medical belief and knowledge related to laughter, which is a whole other work. To end this work on a high laughing note, laughter just may be an all-purpose remedy for the symptoms of living, even if nobody knows whether that’s only a plausible story.