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Where One-Size-Fits-All Health Care Comes From

ELIZABETH O'MALLEY

Mrs. O'Malley didn't go to the hospital anymore when she had chest pain. She was ninety-six and had had "heart attack" as a diagnosis on her last three hospital admissions. She had ended up in the hospital because, not wanting to bother me, she called either the apartment manager or the woman next door when she was having trouble. They responded the way every normally socialized member of the public would: they called an ambulance.

Each time, Mrs. O'Malley would come back out of the hospital with standard textbook doses of heart attack–preventing drugs that gave her a very slow heart rate and very low blood pressure. As soon as the admitting doctors saw Mrs. O'Malley's slightly elevated level of troponin in her blood (the gold-standard test for heart attack), they would make the heart attack diagnosis and treat her accordingly. Mrs. O'Malley was one of those people whose troponin level was just high for some obscure biological reason.

None of these hospital specialists would have ever dreamed of calling me. In fairness, they may have been accustomed to not being able to reach family doctors. If I found out Mrs. O'Malley was in the hospital before she got out, there was no way I could reach any of the specialists on the telephone, and if I could have, my argument about past problems probably wouldn't have counted for much compared with the profession's and the hospital's rules of practice.

Once Mrs. O'Malley got home and I very carefully got her back onto the absolutely minute doses of heart attack–preventing medication that she could tolerate, she was fine. One day she may have the kind of heart attack that has a big effect on circulation, and she will probably die quickly of it, maybe even in her sleep.

> Is it not really strange that human beings are normally deaf to the strongest argument while they are always inclined to overestimate measuring accuracies?
>
> ALBERT EINSTEIN, *THE BORN-EINSTEIN LETTERS*

> The central point is that we have created a world we don't understand.
>
> NASSIM NICHOLAS TALEB, *FOOLED BY RANDOMNESS*

MRS. O'MALLEY IS getting what modern health care has to offer: consistent, reliable, scientifically proven, evidence-based preventive care and high-tech rescue from crisis. What is the matter with that? Isn't our health care technology one of the undisputed crowning achievements of civilization? And if it has limitations, aren't very capable people working on them and making progress daily?

I'm sure this is all true. The problems with the medical system have nothing to do with its not being good at what it does or with the people in charge. What it does—and usually does extremely well—is (1) prevent health problems and (2) rescue people who, in spite of prevention, develop health problems. But what modern health care does so well is the wrong thing for people like Mrs. O'Malley.

Why does our old friend Mary McCarthy come out of the hospital ten times worse than when she went in? Is she just an exception, a rare but predictable occurrence that bucks a trend of otherwise unbroken success? My practice is full of Mary McCarthys. Sudden deterioration after getting textbook treatment is a regular event. We keep paying a price for health care's success through its overriding priorities, and the bill gets presented to people like that old lady, at life's end.

The trouble is, I don't think you can constructively criticize a well-entrenched and almost universally credited method without attacking its foundations. Picture me, if you like, trying to keep the powder (my conviction about how we should be looking after old people) dry for the next twenty pages or so. I'm wading, you'll have no trouble imagining, into a river, with my eye on the opposite side. Here be alligators and who knows what else. Shall we wade in a bit deeper?

Somewhere at the bottom of this river is science, with deep roots that go back to ancient Greece, even though its earliest real showing at the surface was around the end of the 16th century. Up to our chins in the current, we will brush up against epidemiology, the science of health care, bottom-feeding a diet of legitimacy from its history of the defeat of infectious diseases. Much more recently this big friendly fish has evolved into an all-consuming predator of pretty well everything else in the

water by lending its logic—and that legitimacy—to a fast-moving cousin, the randomized controlled trial.

Farther out in the water and closer to the surface we can see the more ordinary and numerous fish: doctors, physiotherapists, social workers, nurses, people from the health industry, health insurers, and, swimming around trying to keep our noses above the water, health care consumers—patients, clients, adults, whatever we choose to call ourselves. Going down into this fanciful health care river, I imagine, for the third time, would be your typical fragile elderly person from my home care practice.

In we go.

It is easy to believe in the modern medical system. How could anybody not feel gratitude for its stunning accomplishments: cataract and reconstructive joint surgery (they almost never fail), the conquest of cancers with chemotherapy, and the near-magical survival of tiny premature babies. Three examples among dozens.

Unless you live on an island with no TV, radio, or Internet, you will hear the messages modern health care sends, in the way the media interpret them. We are blasted and caressed from all sides with health information based on scientific studies: the dangers of trans fats, the risk of cancer from sunlight (or is it osteoporosis benefit from sunlight?), the risk to children playing with toys made in China. It seems that the public-responsive media have a rule that every half-hour TV news show or morning paper has to tell us about what to do, or keep away from, to not get sick. And in an odd way we love it, don't we? Our appetite can never quite be satisfied.

What's with this fascination with health? It doesn't stop when we turn off the TV. The whole food industry has overturned itself in the interest of health, especially prevention. Just try to get, in

a supermarket or a restaurant, a steak, deliciously marbled with fat, that would have met high quality standards in the 1940s. It is practically against the law. There is no way any informed consumer would touch it.

Safety and avoiding injury dominate transportation and the workplace. If you travel at all, you will be familiar with flight attendants' preoccupation with keeping seat belts on and seat backs in their full upright position. Never mind that you're about as likely to survive a real air accident as being chopped into thirty little pieces. I work part-time in a government health department office, and for over a week much of the energy of the unit was directed to learning about the health risks of mouse droppings after one of the little gray creatures was discovered under someone's desk. I'm not joking. You can imagine a world not too far off where smoking a cigarette or riding your bike without full protective armor will be punishable by six months in jail.

I scramble to qualify this. Recklessness where health and safety are concerned is dangerous and should be discouraged. But somewhere between throwing caution to the winds and running your life according to evening news reports of the latest study linking your favorite pastime to a dread disease, there has to be some sort of common sense. Trust me: paying close attention to talk-show traffic isn't going to tell you where that reasonable compromise is.

I sense something a bit furtive in our fascination with health—or is it really fascination with death and disease? We are free to buy what we want, and I wonder if we keep pushing for more and more about health and sickness because we get a perverse thrill from it. The media are only giving us what we choose to consume. Getting sick, getting hurt, and particularly having

these things happen insidiously through some process or exposure nobody would ever have suspected as being dangerous are the kind of thing we hate but in a way are secretly eager to hear about. It's the same awful instinct that draws us to the scene of an accident or a house fire and then (if we're brutally honest with ourselves) lets us down when there aren't any victims. Bad news is good news. It's not fair to expect the free media to resist tapping in to that kind of sentiment. And human nature isn't going to change.

A natural connection that probably nobody intended exists between health care "information" and journalism's needs. As a result, health care is in your face whenever you're awake. It's hard to argue that that isn't a good thing, unless you believe, as I do, that modern health care is in certain ways *bad*. Bad, at least, for some people.

Modern health care seems wonderful to us partly because we are told it rests on science. Science is taught to every schoolchild, and most people now consider science as close to an absolute authority as you can get in this world. It almost seems to offer what some of us would otherwise expect from religion. Scientific experts enjoy unquestioned credibility. But what is science, really?

Philosophy of science textbooks tell us that science is a *method*. And the more curious I get about what science really is, the less I find to support the idea that it can give us a satisfying answer to fundamental questions like, What is real? The great pioneers of science in the Enlightenment probably never intended science to be a search for answers to that kind of question. So possibly there isn't a lot of justification for thinking of science as a shortcut to absolute authority.

But isn't it true that one of the strengths of science is its ability to *prove* things? Doesn't it give us an important way to be certain

about things? Isaac Newton's predictions about the planets were proven correct by observations through telescopes, right? And those observations were repeated again and again.

Maybe, but then for a short time in the early 20th century the scientific brain trust waited on the edge of its academic chair for the results of measurements that would confirm, or not, predictions by a famously obscure physicist named Albert Einstein. He had come up with the idea that Newton's understanding of the physical universe wasn't the whole story. It turned out his idea fit the measurements perfectly. Proof, right? But the "proof" of Albert Einstein's universe meant that Newton's idea of it didn't hold up anymore.

Back to the philosophy of science textbooks. They tell us we are supposed to get something extra once we have scientific proof. Newton predicted planetary movements, and Einstein predicted certain measurements to do with light. And they were both right, in context. Much of the magic of science comes from its apparent ability to predict future events or to understand unexamined cases because of its experience of examined ones.

The 18th-century Scottish philosopher David Hume threw a wrench into those works when he pointed out that science doesn't really ever absolutely prove *anything*. The famous example of this, first used much later by Karl Popper, about whom a little more to come, has to do with swans, of all things.

If for some reason you got interested in the color of swans, the scientific method would tell you to go out and have a look at some of them. So you get a folding chair and sit yourself on the bank of the Danube River and start watching. You see a white one, and another white one, and eventually lots of white ones. And the next day you go back again and see a hundred or more swans, all of them white. At this point the idea might form in

your mind that swans are white, or even that *all* swans are white. And according to your limited experience so far, this idea would be correct. Then a black swan appears. The idea that all swans are white is, of course, destroyed.

"Scientific laws," which are built up from observations, are never 100 percent reliable. Hume pointed out that it doesn't make any difference how many white swans you see—there might always be a black one. There is no final certainty using the scientific method. If we're talking about predicting the future, the notion that all swans are white means that the next one coming around the corner on the river will be white. But will it?

Another philosopher, Karl Popper, put this a different way in the 1930s in Vienna, in his famous and still-current definition of what "scientific" means. He said that the central characteristic of a scientific belief is that it is *falsifiable.* Wait a minute. Isn't the big deal about science that it is *provable?* Not so, said Professor Popper. What is unscientific about astrology is that you can never prove it wrong. What is scientific about Einstein's theory of relativity is that if there were ever a reliable observation out of line with its predictions, the theory would be falsified. Gone into the historical garbage heap along with the conviction that the earth is flat.

I love that humility about a real scientific idea. "I'm only around as long as nobody proves me wrong," it says. And then it sits out in open view and waits for somebody to do just that. This is a humility not respected by health science practitioners, as we will see.

I hope you are starting to wonder about the authority of the so-called scientific material oozing out of your TV set. How much of it withstands the test of falsifiability? And how sure can we really be of science's predictions about the future?

The kind of science that the medical system relies on and that informs TV news stories involves probability and populations. It is called "epidemiology." In physics, a generalization (usually called a theory or a law) like Newton's description of gravity must apply in all cases or it's false. If it ever proved not to be predictive of future measurements confirming its truth, it's finished. Newton's and Einstein's physics meet Karl Popper's falsifiability.

Epidemiology dares make no such claims. On the contrary, its examination of probability in populations puts zero importance on the next observation—or on any observation. Its conclusions are trends. Epidemiology can tell us, for example, the proportion of male to female babies that will be born in a year, but it can't tell us whether the next baby will be a boy or girl. Any halfway competent insurance company epidemiologist will be bang on about the average date of death of the company's clients but will certainly be wrong about the date of death of every one of those clients.

Epidemiology in medicine is about people and is based on the assumption that people are similar. For example, we all have a liver, blood that circulates, a brain, weight never greater than thirteen hundred pounds. But trouble arises when we assume we all have similar characteristics that not everybody actually has—intact memory, ability to move around the environment effortlessly, liver and kidney function close to some norm, and willingness to behave according to certain rules, for example. An assumption of similarity is necessary for most science and works perfectly when you are dealing with electrons, molecules of potassium, or volumes of a pure gas such as helium. The more complicated the things being studied, the less similar they tend to be. Countries, banks, marriages, Labrador retrievers, and people are all potentially pretty complicated. Epidemiology is fine as long

as it is talking about something that *is* the same for all individuals, whether they are Inuit, infant, disabled, demented, angry, vegetarian, or sick in the hospital. Otherwise, look out.

But epidemiology is the science, the only science, used to justify most medical treatment. Every day, millions of times over, we use its information about populations to decide the treatment of individuals. Is that okay? Epidemiology's predictions (about when someone will die, for example) are generalities. They are correct about the average but have nothing to say about any particular case. When we use epidemiology to predict someone's future, we are using quite reliable information about *absolutely nobody*.

The only way to falsify a conclusion reached by an epidemiology study would be to do another, somehow bigger or better or more convincing epidemiology study. So epidemiology is scientific in a way but fuzzy when it comes to falsifiability. Which actually makes it unscientific, according to the usual definition.

One of my patients illustrates this problem. Nellie Fedoruk is a survivor. At the age of seventy-eight she's had breast cancer and bowel cancer and operations for both of them. She's fine; the specialists lost interest in rechecking her years ago. Surgeons have also worked their magic on the blood vessels in her legs, the blood supply to her heart, and three of her four major weight-bearing joints. She's still walking around her little, perfectly maintained bungalow smoking cigarettes. Her husband, Arthur, died of a brain hemorrhage about a year and a half ago, and frankly, Nellie survived that, too, with flying colors. Her daughter, Michelle, is a busy insurance executive in a big city a thousand miles away, but she gets down to visit her mother four or five times a year.

I started seeing Nellie at home because problems with her legs kept her from easily getting out of the house. Her memory is fine, but she is incredibly fussy. The fussiness has a good side—her life is extremely orderly—but it does boil over into anxiety at times. And once in a while Nellie gets uptight enough to reach for her old friend rum and cola—too much rum and cola.

Nellie Fedoruk has diabetes and takes two types of pills for it. Her blood sugar is unpredictable and not often in the nice, safe textbook range where it should be. The problem is that Nellie gets in a tizzy and forgets to take her medication, so her blood sugar goes up. When she feels a little less anxious again, she remembers the medication and takes a little extra. If the tizzy is bad enough in the first place, she might have taken a few drinks in the evening, which also doesn't help her blood sugar or the chances of her taking her diabetes pills in any kind of reasonable way.

Then every couple of months Michelle flies into town, camps in the spare room for a few days, and straightens her mother's life out. This includes making sure Nellie takes every single one of her medications every single time she's supposed to. Michelle is a very orderly person; the acorn does not fall far from the tree.

Several times in the last year or two Nellie has fainted and landed in the hospital with low blood sugar. Sometimes this happens when Michelle is in town and makes Nellie take *all* her blood sugar-lowering medication, sometimes it happens when Nellie has been drinking, and sometimes it happens when she realizes she's missed her medication and decides she needs a double dose.

I face a real problem as her doctor. Seventy-eight is young by the standards of my practice, and Nellie could in theory live another dozen years or more. Some of the trouble diabetes can cause late in life might still be preventable by keeping her blood

sugar closely and carefully controlled. But Nellie has gotten into serious practical problems every time I try to increase the dose of her diabetes medicine. Her life is just chaotic in a way that, in practical terms, nobody is going to be able to control.

I maintain Nellie on the very minimum amount of diabetes medicine that keeps her from getting into danger from really high blood sugar. Her diabetes blood test numbers are always awful by every textbook standard, but there is no other way to keep her from the more serious danger of *low* blood sugar.

Reenter epidemiology. Based on the experience of thousands of patients, it tells me that in general, diabetics won't get diabetes complications as soon if their diabetes is tightly controlled. That's if a close check is kept on their blood sugar and other measures of diabetic control and if the medication is adjusted as often as necessary to keep the blood sugar down. And if they are put on another preventive medication to protect the kidneys. That's the probability in that population. But probability and population are both abstract ideas that say nothing about the individual, real-life situation of Nellie Fedoruk.

Nassim Taleb is a probability mathematician who is also a stock-market trader. In his book *Fooled by Randomness,* he explains that probability can be characterized by how much randomness is associated with it. Probability with a lot of randomness is just what we get when we try to measure anything about fragile elderly people. And big-league, out-of-control randomness is anathema to generalities like the ones epidemiology produces about diabetes.

Nellie Fedoruk's future is not predicted by the epidemiology relevant to her disease, diabetes. She is atypical enough that the rules don't apply to her, for obvious practical reasons that

anyone with common sense can see. And where the people in my practice are concerned, Nellie Fedoruk is typical. The difference between fragile elderly diabetics and younger diabetics is heterogeneity. The young ones tend to be similar with respect to all the things medicine measures. The fragile elderly tend to be very different. Typically atypical, you might say. They're all like that!

The example of Nellie may help you to understand how my medical practice has turned me into an agnostic. Epidemiology's generalizations about probability in populations are wrong so often in my practice that I have become very cautious—skeptical, in fact—about them. When I draw one conclusion from examining a group and exactly the opposite one from examining an individual, I need to be able to make a choice. Practicing medicine the way I do, for the people who are my patients, I have absolutely no question in my mind about that choice.

I can hear good friends and colleagues—a hard-core epidemiologist, evidence-based internal-medicine specialist, or endocrinologist, for example—saying, "Of course the rules apply to Mrs. Fedoruk. It's just that she's not taking her medication properly and she drinks." I'm sorry; this kind of thinking is seriously, dangerously backwards. My responsibility, and ours in medicine, is to the patients. When we start thinking that we are first responsible for providing consistent rules, and then the people need to conform to those, and that when they don't that's just too bad, then we really quit doing what we're paid for and begin to do harm. As I think of my dear friends whose practice and philosophy are evidence based, I know perfectly well that each of them has the best interests of their patients at heart, too. But that's not what we're teaching students and not what we present as the best we have to offer.

When scientific rules of medical practice derived from epidemiology say one thing and common sense about the seventy-eight-year-old lady in front of me this afternoon says something completely different, I have no trouble making that choice. *She's* real. Epidemiology's predictions about her future are not. Attractive though abstractions are, they can never replace our real-world responsibility to other people.

Epidemiology's most important tool is the randomized controlled trial. When health professionals, journalists, and members of the public talk about scientific evidence in health care, this tool is what everybody is talking about. These trials are, in our ordinary language, scientific experiments. They can be very simple, but they are usually incredibly complicated.

The whole collected group of randomized controlled trials on any question in medicine is called the "evidence base." This evidence base may, depending on whom you talk to, include all the studies that have ever been done on the subject, only the studies that have been published, only the studies that were controlled, only the studies that have end points that seem to make sense, only the studies not funded by drug industry, and so on.

Randomized controlled trials typically involve hundreds or thousands of subjects (people), millions of dollars, big organizations such as drug companies or government research agencies, and years of organization and work. There is a billion-dollar industry of research design consultants, research-conducting organizations, statistical analysts, ethics review boards, and the journals that assist in the production and publication of these studies.

The reliability of these huge and influential experiments is rarely questioned. But we can already figure out that they suffer from the weaknesses of their mother science, epidemiology.

The results tell us, at best, what probabilities are in populations. Theoretical and approximate. They matter to each of us only if we really are members of the artificial populations set up for the trials. If not, in no way does the outcome of any trial predict *our* particular future. Quite a few approximations for a science that we depend on for the rules of medical care, don't you think?

Especially if we happen to be unusual. Like pretty well every single one of my patients. Randomized controlled trials tell us about a group of people given a treatment (usually a drug treatment) compared with a similar group given no treatment. Are any of the "subjects" in the trial anything like Elizabeth O'Malley or Nellie Fedoruk?

Is the question the trial is trying to answer meaningful to these people (would it matter, for example, to Elizabeth O'Malley if her bone density changed by 1 or 2 percent)? More generally, could there be a bias to a clinical trial's outcome because the doctors doing the trials are paid by drug industry sponsors or because their careers are advanced by a positive result or because the drug company doing the trial benefits from a positive result? Does it matter that trials that show that a drug works are more likely to be published? Are we concerned because once there are several positive randomized controlled trials for a particular treatment, it becomes unethical to test it any further?

Ironically, very important rules of ethics can weaken the logical relevance of trials that use them. For sound ethical reasons, frail elderly people are excluded from drug trials, and often the necessary confirming trials simply aren't allowed.

Clinical trials are the best method we have come up with so far for "proving" the benefit of a treatment. A chain is as strong as its weakest link. And the randomized controlled trial as it is practiced and interpreted in today's medical system involves a

very long chain and quite a few corroded links. That chain starts with an idea (commercial, scientific, career oriented, or however it gets into someone's mind), and it ends when Elizabeth O'Malley or Mary McCarthy gets out of the hospital on a ton of preventive drugs.

Why did randomized controlled trials assume such overwhelming importance? The answer is that reliability and consistency appear to be assured by its methods. And reliability and consistency are important for several reasons.

Business, insurers, government health care agencies, and policy makers all need reliability. More reliable care should mean a better outcome for a population, but we only get that if the health care rules that epidemiology coughs up for us are applied consistently. And the more consistency we accomplish, the less likely we are to accommodate the needs of outliers—oddballs. People who could never have been members of the original study population, who don't fit the mold.

Clinical guidelines are supposed to give us consistency. These are like recipes for health care (which is why I call their consistent use "cookbook care"). Clinical guidelines are evidence based: they come from the information gathered from randomized controlled trials. These days there are several different guidelines for most common clinical problems. This happens because different experts have different opinions about what trials should be included in the evidence base. But the tendency is not toward diversity among the guidelines.

I have been teaching medical students and residents in family practice for decades; it is one of the most enjoyable and stimulating parts of a full practice. Without trying to criticize any of the lovely people who have put up with my quirky teaching style, I have noticed a trend over the years that I think comes from the

guidelines approach to care. Students trained recently are very likely to recommend identical treatment—identical to what their classmates would recommend and identical for every patient. This trend isn't confined to medical students. Nutritionists, pharmacists, and other health care professionals coming out of their training programs also tend to be consistent in their proposed treatment.

This is no surprise. If we wanted to get the best possible benefit for the largest number of people, and if we believed that randomized controlled trials give us really reliable information about benefits, we would do our best to pull together the results of those trials and write down the best treatment, creating clinical guidelines. There's not much point in doing this unless we make sure the people giving the treatment always give that same best treatment. And what better place to start than the schools where we train the professionals?

What I'm seeing among my students is the result—consistency. The evidence-based, guidelines-driven approach is working like a charm. I try as hard as I can to get these wonderful students to *think:* is this treatment really safe and helpful for this old man we're seeing? Sometimes it's an uphill battle, and not because these young students aren't smart enough.

This situation worries me because at the same time as we are teaching young doctors, pharmacists, and nurses to practice according to reliable evidence, we might be discouraging them from thinking creatively about unusual one-off situations. Over my few decades of practice I've seen an evolution in the culture of health care professionals. Consistency has replaced preparation for the unexpected; answers have replaced questions. The state of the art is the state of the science. Not, as far as I can tell, necessarily a good thing.

Consistency is self-reinforcing. As clinical guidelines for common diseases become better defined, there is less and less chance that experts will disagree with one another about the most effective treatment. There is a certain machinery in the way we do business that guarantees this kind of pulling together of opinion.

Imagine for a moment that a doctor, under pressure, tired, and driven nuts by a really bad day full of ungrateful people who blame him for problems he didn't cause and can't do anything about, doesn't get along with someone whose family member gets sicker and dies, in spite of treatment. Imagine that the doctor looked at the guidelines for treatment and realized they couldn't possibly apply to this person and decided not to follow them. He treated the *person,* not the population, as his best instinct and common sense told him he should. And then he failed to show grace under pressure when the son was looking for someone to blame for his mother's misfortune.

It is well known that people who sue doctors and other health professionals do it not because they didn't get better but because things went badly and the doctor acted as if he were infallible and failed to communicate. If there is a malpractice lawsuit, the outcome will depend on the testimony of experts, who will say that clinical guidelines should have been followed. It takes a creative and determined ordinary general practitioner to run that kind of risk, to break the rules, even when the rules don't make sense.

Recently there have been some changes to the way doctors are paid where I work. Some of these are wonderful from my point of view. The house-call fee item was increased. Nursing home care is now better paid, and there is a lot more money for each service for the very elderly. All good. But payment bonuses

also exist for following clinical guidelines. This sword cuts both ways. Just as we are paid to be consistent, we are paid to be rigid. One size fits all.

Clinical guidelines kick in for every doctor when a patient meets a disease definition. More and more these days, diseases (or being at risk) are defined to make care consistent and to avoid dependence on "soft" clinical findings like a complaint of pain, swelling of a joint, or sounds the doctor hears through a stethoscope. Blood tests and results of measurements by machines are reassuringly black-and-white when it comes to whether you have the disease or not and whether or not you are treated according to guidelines. The disease definitions are often written directly into the guidelines so that there can be no mistake.

Diabetes is defined strictly by a blood test. I went to a local laboratory early one morning because I woke up with an irregular heartbeat and wanted to confirm what it was with a cardiogram. At the same time, I got a bunch of "routine" blood tests done to send to my family doctor. My fasting blood glucose was 6.1; diabetic by just a hair. I was very relieved when a repeat test was just below the definition of the disease. My doctor, always reassuring and of course never wrong, told me that it was the stress of the irregular heartbeat that had put my blood sugar up. I was happy to agree.

Heart attack is now diagnosed by a blood test. Heart failure, which is a different thing, to do with the heart's function, is also now defined by a measurement tool outside the hands of the examining doctor: an echocardiogram. Osteoporosis is defined by a bone-density reading from a machine. Elevated cholesterol is defined by a blood test, and so on. It's almost as if you didn't need the doctor to make the diagnosis. There is very little room even

for the interpretation of the diagnosis, and because the guidelines are usually pretty clear, you probably also don't need the doctor to tell you what the treatment is going to be.

This is part of the whole movement toward reliability, consistency, and reproducibility in diagnosis and treatment. Not only do we define the treatment in clear, consistent, absolute terms, we also define the disease that way. No human error: you have it or you don't.

And I can see my doctor friends shaking their heads. What is he worried about? Does he want us to go back to the days when diabetes was defined as passing huge amounts of urine and being always thirsty? Should we wait for someone to show up in the emergency room gasping for breath before we diagnose heart failure? Please. The sword has two edges. Of course we gain when consistency leads to earlier diagnosis and more accurate treatment. I'm simply saying what we all know already: the price we pay for greater consistency is a less flexible attitude toward treating outliers. And the fragile elderly are nearly all outliers.

The medical system is wonderful as long as you can benefit from what it has to offer. It is scientific but involves a kind of science loaded with questionable assumptions, such as the idea that everybody is similar, and partly sound logic, such as extending conclusions from one group to a different one. Many of its assumptions are conditional: you're going to be fine as long as you resemble everybody else. Scientific studies have moved to the center of the medical universe, and everything is considered legitimate to the extent that it aligns with the studies' methods and results. We are out to prevent death and disease and, when we can't, to rescue people from them. Consistency in care

is guaranteed because scientific evidence informs clinical guidelines that everyone follows.

Is it possible to be scientific about doing medical care without having to live with and follow the rules of the kind of science that powers epidemiology and clinical guidelines? What about a scientific study, a clinical trial, where the size of the population is *one?* Is there a crisis response that would work better for certain people than dialing 911? Would anyone even want to think about such things?