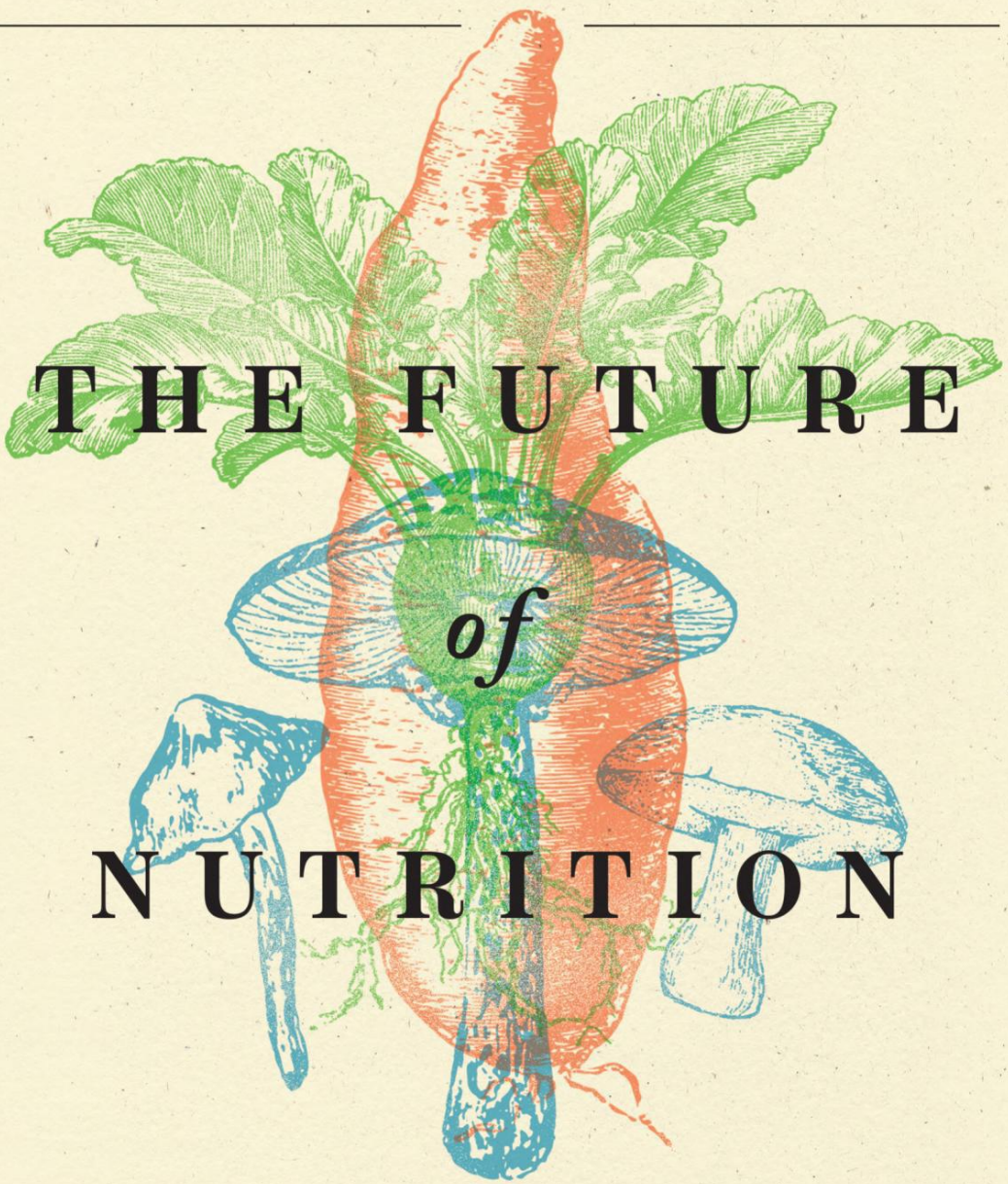
An Insider's Look at the Science • Why We Keep Getting It Wrong • *and* How to Start Getting It Right

THE FUTURE *of* NUTRITION



Coauthor of *The China Study*

T. COLIN CAMPBELL, PhD

with NELSON DISLA

THE FUTURE *of* NUTRITION

An Insider's Look at the Science,
Why We Keep Getting It Wrong,
and How to Start Getting It Right

T. COLIN CAMPBELL, PhD

with NELSON DISLA

BenBella Books, Inc.
Dallas, TX

BenBella Books, Inc.
10440 N. Central Expressway
Suite 800
Dallas, TX 75231
www.benbellabooks.com
Send feedback to feedback@benbellabooks.com

BenBella is a federally registered trademark.

Printed in the United States of America
10 9 8 7 6 5 4 3 2 1

Library of Congress Cataloging-in-Publication Data:
Names: Campbell, T. Colin, 1934- author. | Disla, Nelson, editor.
Title: The future of nutrition : an insider's look at the science, why we keep getting it wrong, and how to start getting it right / T. Colin Campbell ; [edited by] Nelson Disla.
Description: Dallas, TX : BenBella Books, Inc., [2020] | Includes bibliographical references and index. | Summary: "Colin Campbell, author of The China Study (3M sold across all formats and editions) and New York Times bestseller Whole, returns with The Future of Nutrition, a book examining the shortcomings and confusion within the nutrition industry and outlining steps to improve"— Provided by publisher.
Identifiers: LCCN 2020028143 (print) | LCCN 2020028144 (ebook) | ISBN 9781950665709 (hardback) | ISBN 9781950665730 (ebook)
Subjects: LCSH: Nutrition. | Diet.
Classification: LCC RA784 .C23522 2020 (print) | LCC RA784 (ebook) | DDC 613.2—dc23
LC record available at https://lccn.loc.gov/2020028143
LC ebook record available at https://lccn.loc.gov/2020028144

Editing by Leah Wilson
Copyediting by James Fraleigh
Proofreading by Lisa Story and Sarah Vostok
Indexing by WordCo Indexing Services
Text design and composition by Aaron Edmiston
Cover design by Faceout Studio, Spencer Fuller
Cover photo © Shutterstock / Paladin12 (paper); Morphart Creation (kohlrabi); Little Apple (yam and mushrooms)
Printed by Lake Book Manufacturing

Distributed to the trade by Two Rivers Distribution, an Ingram brand
www.tworiversdistribution.com

CONTENTS

PREFACE AND ACKNOWLEDGMENTS

Before I can share what I've learned about the science of nutrition, its past, and hopefully its future, there are so many people that I must acknowledge. Without these people, going all the way back to my youth, both my career and this book would have been impossible.

For context, it's important to emphasize that throughout my career in experimental research, I often encountered results that not only surprised me but also challenged many beliefs dear to both the public and my peers. Deciding to confront those beliefs was not always easy, even when the evidence was worth pursuing. For one thing, I did not want to jeopardize my financial support, which required professional peer approval. Neither did I want to be considered a fool. But despite those (and other) obstacles, some of the findings simply could not be ignored, for they had profound implications for our society's future.

It is here that I must first acknowledge the rock-solid support of my parents, who worked tremendously hard to raise me and my younger siblings while running a family dairy farm 365 days a year—cows don't take holidays! My mom kept a top-grade garden that provided most of our food year-round, and had me work in it when I was not in the barn or the fields working with my dad and brothers.

Dad, an immigrant from Northern Ireland, arrived at Ellis Island when he was only seven years old. He had a couple years of schooling, then worked

very hard for the rest of his life. Because of his lack of formal schooling, he was exceptionally committed to the importance of education for his children. He wanted us to receive the education that he did not. He therefore did not want me to attend the local, rural high school, from which some students did not graduate and very few went to college. But the nearest really good tuition-free public high school was a little over fifty miles away, in Washington, DC. So, for five years, I drove our family car just over a hundred miles per day to attend that school. This allowed me to get a high-quality education at almost no expense (my uncle, whose small construction company lay along my route, paid for gas). Still, it was not easy to balance school and work on the farm. Because work usually awaited me after school each day, I had virtually no time to do homework, except for a study period during the school day.

After graduating high school, I passed through undergraduate school (pre-veterinary, Penn State), one year of veterinary school (University of Georgia), then graduate school (Cornell, master's and doctoral degrees in nutritional biochemistry). Several times along the way, I received generous, unsolicited offers of support by mentors and others. Many people, mostly professors and administrators, did generous favors for me, often uninvited and sometimes hardly knowing me. Without their collective generosity and goodwill, I might not have been the first on either side of my family to go to college.

How, then, did I come to pursue a professional career in nutrition and health that challenged such cherished beliefs concerning the food we eat, especially when those beliefs were such important parts of my upbringing? Did I not respect the discipline that gave me a professional career, and the people who helped me get there? Did I not respect the customs of my family, or the hardworking people in the farm community of my youth?

The research findings that drove my career were often culturally and economically challenging and disruptive. But they were also tied up in the personal story I have just described. Findings that questioned the health value of animal protein, as first (and repeatedly) indicated by experimental results showing that cow's-milk protein would be the most relevant chemical carcinogen ever, if it were officially tested, are culturally and economically challenging. But they were also personally challenging. Findings suggesting that nutrition plays a far greater role in cancer development than genetics were

culturally and economically challenging. But they also challenged what I had been taught by people to whom I am still indebted. These findings challenged the entire status quo, the very one that had nurtured my career. There are many other examples—findings that undermined the pharmaceutical industry, or that showed experimental disease progression is reversible (i.e., treated) simply by removing its nutritional stimulus, or that the third- or fourth-leading (*but unlisted*) cause of death in America is the use of prescription drugs, or that optimal nutrition advances human health more than any combination of drugs, or that nutrition can both prevent *and* treat a wide range of illness and disease, with the benefits often appearing within days to weeks.

Thankfully, I felt I had no choice but to interpret our research findings to the best of my ability, no matter how provocative and challenging they were. When I think about that challenge, I think once more about my parents, especially my dad, who made sure I fully appreciated the combined power of work ethic and honesty. He reminded me more than once that I should "tell the truth, the whole truth, and nothing but the truth," and that reminder served me as armor more than once.

I believe that most people in science can relate to the spirit with which I pursued these research findings. That's why, for the most part, I've immensely enjoyed the scientific research community. Most of these scientists do not seek personal wealth. They are driven by curiosity, knowing that science, at its best, seeks truths in a way that leads to lively conversation. These experiences are very personal and social. Although I have experienced and treasured such exchanges, I also know that this is often not the public image of science, and for good reasons: scientists, unfortunately, are too often not at liberty to express their inner selves as a result of institutional expectations and boundaries. This is understandable within for-profit institutions, when scientists are contractually obliged and willing to stay within certain boundaries. But academic institutions are another thing altogether. They are endowed with *a public responsibility of seeking truths wherever they lead us*, whether in the research laboratory, the lecture hall, or policy boardrooms. Scientists are bound by a trust between academic institutions and the public to seek those truths, and when that trust is broken, all of society pays the price.

Sadly, there has been a serious drift away from these ideals in recent decades. The granting of academic tenure, and the freedom of speech and

thought that it protects, has declined to such an extent that many of today's scientists in academia—especially in disciplines related to human health—are vulnerable. As of 2017, only 17 percent of US faculty were in tenured positions, and the proportion of non-tenured (adjunct) positions had quadrupled since 1975.[1] Most new faculty now have time-dependent terms of employment, which means that they may not be reappointed at the end of their term if they veer too far from the institutional "party line." Because they remain untenured, such faculty must be careful to not question their institutions' interests. To make matters even worse, most of those institutions are becoming increasingly tethered to external funding sources.

Though I discuss this threat at greater length later in this book, these acknowledgments would be incomplete without at least some mention of academic freedom. I was most fortunate to earn tenure exactly fifty years ago, in 1970. Without that privilege, this book and its predecessors would have never been written. I place it alongside parental guidance as another critical factor in my career.

But two-legged stools are not stable. The third leg is my wife of fifty-eight years, Karen. Though not trained in science, she had a more precious gift. She expressed it to me after we first met, simply stating that she did not tell lies. And such it has been. It was she, more than anyone else, who pressed me in 2002 to write my first book, *The China Study* (co-authored with our son, Tom, now a family physician). She and I are a team. She took up where my dad left off. With Karen by my side, I could never have failed to tell *the whole truth*, even if I were so inclined.

I muse on these matters of truth telling not because they are unique to me, but because they reflect some of the reasons why I followed the path I did in my research and academic career. That path was sometimes a delight, but also sometimes tortuous and bothersome. *The China Study* (2005, 2016) was written to share with the public some of the most provocative research that I could not ignore. *Whole* (2013) was written to explain the underlying philosophy and evidence that supported that research.

Here, I hope to answer another question: why does nutrition *still* struggle so much to be heard? I am not speaking only of recent struggles that I have faced personally, but of patterns that go back *centuries*. For me, though I didn't know it at the time, work on this book began in 1985 when I was on

sabbatical at Oxford University, working with my colleagues Sir Richard Peto and Jill Boreham. I spent considerable time in Oxford and London libraries trying to understand why nutrition was so difficult to comprehend—for my colleagues in research, for colleagues in the food and health policy development arena, and for the public. I am therefore grateful to my colleagues for allowing me the time to do that research. The document I completed during that year, which summarized my findings on the respective histories of cancer and nutrition, was the initial basis for this book. A blurred copy of the document was faxed from Oxford (the first fax I ever saw), and I saved it for many years until it was retyped by Director of Digital Marketing Sarah Dwyer, allowing me to finally tell this story and explain how it relates to what I've learned over six-plus decades of research.

This brings me to the dozens of graduate students, undergraduate honors students, and postdoctoral students who studied and worked under my mentorship—without those experiences, both personal and professional, I would not be where I am. Senior technicians Marty Root, PhD, and Linda Youngman, PhD, who spent about fifteen years each running my laboratory, and Banoo Parpia, PhD, chief administrator of our research program in China, also deserve great credit. They made this and my earlier books possible. I am also indebted to my colleagues at large, including more than two dozen who worked in my laboratory, among them many visiting professors and senior scientists from China. From this group, I owe a special debt of gratitude to Chen Junshi, MD, PhD, who was the first Chinese senior scientist to visit the US, and who spent a year as a visiting professor in my laboratory before later serving as the co-director on the China project with me and two other colleagues, the aforementioned Sir Richard Peto of Oxford University and Dr. Li Junyao of China. Our partnership spanned more than twenty-five very active years. Sir Richard Peto was, and continues to be according to many, the world's leading biostatistician and epidemiologist. He and Dr. Jill Boreham at Oxford were primarily responsible for organizing, collating, and displaying the original data in an 896-page monograph, jointly published by Oxford University Press, Cornell University Press, and People's Publishing House of China.

Perhaps oddly, but seriously, I acknowledge those few individuals who represent powerful institutions in our society and who position themselves for private gain at the expense of public welfare. These individuals within universities

acquire personal funding from corporate consultancies and outsized honoraria, sometimes also getting institutional funding to conduct focused research projects for the benefit of those same corporations. I acknowledge these individuals because they illustrate the danger of powerful institutions exerting control over academic research and government policy, mostly beyond public view. In my experience, such individuals illustrate an existential immorality that must be excised. We have extremely important things to do without having to suffer costly distractions and, at times, the threat of professional annihilation over such a fundamental thing as sharing the truth with others.

I am also grateful to the nonprofit organizations that have put science-based, whole food, plant-based nutrition at the center of their own operations, including the Center for Nutrition Studies (CNS), under the direction of Jenny Miller, Jason Warfe and staff, and now having as president my daughter LeAnne Campbell (PhD, Education and Curriculum Development)*; Plant Pure Communities, founded by my son Nelson, underwritten by CNS, and directed by Jody Kass†; and the CNS-partially underwritten research program at the University of Rochester Medical Center, run by my son Tom (MD) and his wife, Erin.‡

I must acknowledge family—twenty-two children, spouses, and grandchildren in my immediate family—who have not only put up with me and the time I spend on the computer, but who have also wholeheartedly adopted this whole food, plant-based lifestyle. Except for one who very occasionally may veer off course, they all eat this way. Eleven work professionally in various ways in this area. Their support has been priceless, in so many ways. Son Nelson's extensive review of the manuscript is very much appreciated. And grandson Nelson Disla, graduate of the University of North Carolina with highest honors, my "with" author—I can easily say that his writing skills are without peer, in my experience.

Lastly, I have the utmost professional and personal respect for the exceptional work of Leah Wilson, Alexa Stevenson, James Fraleigh, Alicia Kania, Monica Lowry, Jennifer Canzoneri, and everyone else at BenBella Books.

* www.nutritionstudies.org/courses/plant-based-nutrition

† www.plantpurecommunities.org

‡ Tom: www.urmc.rochester.edu/people/27426401-thomas-campbell; Erin: www.urmc.rochester.edu/people/22553782-erin-campbell

FOREWORD

Growing up during World War II on a large Montana dairy farm, I never doubted the value and quality of the food we were producing. I was sure the meat and milk from our farm were the keys to a healthy future. And when it came time to decide my future occupation, that upbringing was ingrained in my decision. Even though farming was not very profitable, I believed that our growing world population would make it so.

After deciding to be a food producer, my next step toward mastering agriculture was to get a college degree. So I attended Montana State University and obtained a BS degree in Agriculture Production. I was now prepared to take the food production world by storm.

Yet I soon became aware of a problem: millions of producers were selling to only a handful of buyers. My farm would have to get bigger or get out. So I got bigger: I eventually controlled thousands of acres of crops and owned thousands of cattle. My college lessons dictated my production processes: chemicals to control the weeds, factory feedlot to fatten slaughter cattle, and big equipment to grow and harvest grain. I did begin to notice that my soil quality was declining, and that our animals had become numbers instead of valued companions. But I was too busy to think much about these issues. If they were important, I thought, we'd have studied them at the university. Plus my personal life had become busy, too: I was married and had five children.

Then, everything changed. I lost feeling from the waist down and was diagnosed with a spinal tumor. Before my surgery to address the growth, the doctor told me that if the tumor was inside the spinal column, my odds of

walking after the operation were about one in a million. This got my attention. The night before the operation, many things crossed my mind, including the worsening condition of the soil and my relationship with our animals. I resolved that no matter the outcome of the procedure, I would attempt to rectify these issues.

It turned out that the tumor *was* inside the spinal cord, but against all the odds I was able to walk out of the hospital. I considered it my miracle. Through it all, and during my lengthy recovery, I did not forget about the soil or the animals.

After the surgery, physical labor was beyond my ability. I found reading to be a great way to pass the long days. It was at that time when I first became aware of Dr. T. Colin Campbell, a researcher at Cornell University. But at that phase of my life, his work was a bridge too far for me.

During my recovery, I became convinced that my farming methods were causing significant environmental damage. I decided to become an organic farmer. When I shared this plan with my banker, however, he laughed and said the bank would not lend me any money unless it was cycled through local chemical dealers. Unable to change my farming methods, and burdened with debt, I had two choices: continue with conventional agriculture, or liquidate my operation. I chose the latter.

After a failed congressional campaign against a multi-term incumbent, I accepted a lobbying job with a small family-farm organization in Washington, DC. For a small-town boy from rural Montana, working in the halls of government was a real eye opener. Seeing the doings of Congress up close was very different from reading about them in a civics book.

While in Washington, I continued to eat much the same way as I had on the farm. But I was getting much less physical activity, and I was becoming as fat a market-ready hog. I knew I had to make a significant change or else I was a heart attack waiting to happen.

I recalled Dr. Campbell's work, and decided to change my eating without telling anyone. I became a plant eater while working for meat and dairy producers. Over time, I lost in excess of 100 pounds.

At about the same time, a new problem called mad cow disease was rearing its ugly head in England. The symptoms were similar to issues I had seen in cattle in confined feeding on my farm, and the cause was thought

to be feeding animal waste to live cattle, a common practice in most US confined-feeding operations. Not only was this a big problem for animal agriculture, it was now thought that humans who ate infected meat could develop the disease. This issue had the potential to upend the multibillion-dollar animal feeding industry, and no amount of money was too much for the cattle industry to spend in protecting business as usual.

The foundation of science is truth, but the American diet is built on so many falsehoods that it is almost impossible to tell truth from mistruth. Corporate agriculture in no way wants to clarify this situation, or for American consumers to discover that what they believe to be true is really false. Their tried and proven strategy was to disrupt science and rely on herd mentality. Again, and again, we are told to go with the herd.

I was working for the Beyond Beef campaign when I met Dr. Campbell in my office for the first time. Both of us being farm boys, we established an instant rapport that continues to this day.

Soon after this meeting, Oprah Winfrey decided to do a show about mad cow disease. As one of the few addressing the public about this issue, I was invited to appear. With millions of viewers set to tune in, the cattle industry panicked. They were represented by a lobbyist I had worked with in Congress and knew very well, but he represented the industry poorly on the show. Oprah ultimately stated on the program that she would never again eat a burger. What a disaster for cattlemen! The industry was put into total disarray.

When the cattlemen recovered, some of them decided that one way to discourage media coverage of mad cow disease was to sue Oprah and me for millions of dollars. This legal experience lasted for years, but we won every time. The base of our defense was the work of Dr. Campbell and the China Study. Because the cattlemen could not find any flaw in the research linking animal protein and cancer, they were unable to base their suit on the facts. We prevailed in the eyes of the jury not just because of the right of free speech, but also because our statements were grounded in science and truth.

This same drive to document how the food, medical, and pharmaceutical industries, in conjunction with vested government interests, have worked to discredit the benefits of a plant-based diet can be found throughout Dr. Campbell's new book, *The Future of Nutrition.* While reading it, all I could

think of was how much easier my transition away from animal-based foods would have been if this book had been available when I was becoming a plant eater. It's a pleasure to read truth from a truly gifted scientist.

I owe Dr. T. Colin Campbell a debt I will never be able to pay. In my view, he should receive the Nobel Peace Prize.

—Howard F. Lyman
Author, *Mad Cowboy*

INTRODUCTION

There are few things more provocative than the food we choose to eat, both in terms of its effect on health and people's sensitivity surrounding those choices. Any suggestion of dietary change is rife with potential agitation. This has been the case for at least four decades, and I have had the unusual "privilege" of witnessing and experiencing that agitation up close many times since my professional career began some sixty years ago. My experience includes thirteen years at MIT and Virginia Tech; one year each at Oxford University and the headquarters of the Federation of American Societies for Experimental Biology and Medicine (FASEB) in the Washington, DC, area, where I was US Congressional liaison representative for the Federation; and forty-five years at my alma mater, Cornell. From all of that experience, one episode in particular stands out for its ability to illustrate the sensitivity and controversy surrounding nutrition.

In 1980, I was invited by the US National Academy of Sciences (NAS) to join a thirteen-member expert panel tasked with studying the relationship of diet and nutrition with cancer. Three years earlier, a US Senate committee chaired by Senator George McGovern had released a landmark report on diet and heart disease. Its dietary goals were ultimately modest, encouraging things like lower dietary fat intake and greater consumption of fruits and vegetables.[1] Nevertheless, the report triggered hostile reactions from the immensely powerful and wealthy food industry. Senator McGovern told me some years later that this report was the proudest achievement in his public life, but it wasn't won easily. Six of his Senate colleagues lost their 1980

reelection efforts as a result of their support for the report's findings, he said. They were from farm states, where agribusiness exerts serious influence on the political process.

Naturally, the public wanted to know whether diet might have a similar effect on other common diseases, especially cancer. The question was reasonable enough: Might the dietary recommendations best suited for controlling heart disease also be consistent with controlling cancer? The authority on this question should have been Dr. Arthur Upton, director of the US National Cancer Institute (NCI)—a subdivision of the US National Institutes of Health (NIH)—who was invited to testify before the Senate.* Unfortunately, Dr. Upton was unable to answer the question to anyone's satisfaction, and instead revealed NCI's negligent attitude toward nutrition research. When asked how much of his budget was devoted to nutrition, Upton replied, "2–3 percent." The Senate responded in early 1980 by appropriating $1 million to the NCI for a review of the literature on nutrition and cancer. The NCI in turn contracted with the NAS to conduct the study. This was organized by Dr. Sushma Palmer of the NAS and Dr. Peter Greenwald, director of the new Division of Cancer Prevention at NCI, both of whom expressed interest in research on the nutrition–cancer connection.

Political considerations were immediately intense and ugly, even just in deciding which group should write the report, highlighting once again how controversial such a report could be. Within the NAS (situated just down the street from the Capitol and embedded amid large marble structures of national power both descript and nondescript), the Food and Nutrition Board (FNB) immediately jockeyed for control. This is the same group that has been tasked every five years since the early 1940s with estimating and publishing recommended daily allowances (RDAs) for individual nutrients. As far as they were concerned, preparation of the nutrition and cancer report was both their institution's right and responsibility. They also knew how incendiary a report on this topic could be. But the decision wasn't theirs to make. Concerned about several of the FNB members' food industry

* Dr. Upton sent his proposed testimony to me and our Division of Nutritional Sciences director at Cornell for our comments prior to his presentation.

associations, Dr. Phil Handler, the president of the NAS at that time, opted for a new, outside committee of experts—that thirteen-member panel to which I was invited.

As you can imagine, the FNB wasn't thrilled by this decision. In what I came to understand as an attempt to usurp our report and preemptively undermine whatever conclusions we might make, they published their own twenty-four-page report, titled "Toward Healthful Diets,"[2] in 1980, just as we began our work. Here is a brief excerpt:

> *In the case of diseases with multiple and poorly understood etiology, such as cancer and cardiovascular disease, the assumption that dietary change will be effective as a preventive measure is controversial. These diseases are not primarily nutritional, although they have nutritional determinants that vary in importance from individual to individual . . .*
>
> *Those experts who . . . seek to change the national diet in the hope of preventing these degenerative diseases assume that the risk of change is minimal and rely heavily on epidemiologic evidence for support of their belief in the probability of benefit. Neither the degree of risk nor the extent of benefit can be assumed in the absence of suitable evidence . . .*
>
> *The Board expresses its concerns over excessive hopes and fears in many current attitudes toward food and nutrition. Sound nutrition is not a panacea. Good food that provides appropriate proportions of nutrients should not be regarded as a poison, a medicine, or a talisman. It should be eaten and enjoyed.*

It may not be obvious to those unfamiliar with nutrition policy, but this report is pregnant with all manner of nuance and commentary intended to protect the status quo—the status quo that McGovern's report, and ours, threatened to upend. It first cleverly admits a few widely held understandings (e.g., disease causation is poorly understood, dietary change will be controversial, individual responses will vary, excessive hopes and fears arising from dietary change are concerning) that could serve as a means to shut down any nutritional recommendations. The report then posits that the adults in the room are its authors, that they are more reasonable and protective of the public than anyone else could possibly be, and that they know

best—thereby shutting down any outside attempts at proposals for public benefit that might challenge corporate interests.

In one sense, the authors of this passage were absolutely correct: "that dietary change will be effective as a preventive measure *is* controversial" (emphasis added). But to imply that the controversy generated by dietary recommendations in any way undermines those recommendations' truthfulness is a clearly faulty premise. No matter how controversial any evidence may be, the controversy itself is never enough to rule that evidence out. Moreover, "controversy" does not necessarily mean that contradicting evidence exists. The notion that smoking causes cancer was once viewed as extremely controversial, not because of an impressive body of evidence proving the healthfulness of tar and nicotine, but because it challenged prevailing norms. The notion that huge industries like the pharmaceutical and food industries "make a killing" by selling their products to a population that only becomes more ill as a result is controversial—and it should be! Evidence that disputes the status quo will *always* be controversial, whether it is true or not, because that's the very definition of controversy: disagreement over conventional understanding. Interestingly enough, the same definition could apply to all of science—if a theory cannot be scientifically disputed, refuted, or falsified, it is often viewed as pseudoscience. In other words, controversy is the sound that science makes. To downplay scientific evidence because it is controversial is to downplay scientific evidence for the very same, fundamental reason that science is celebrated.

The "outsiders" panel on which I sat worked on our report for three years and included six three-day conferences and considerable staff input. The report had two parts: a 478-page summary of the available scientific evidence,[3] then 74 pages of recommendations on research needs.[4] Once published in 1982, it quickly became the most sought-after report in the history of the NAS. This was both a blessing and a curse. On the one hand, the level of interest our report generated verified the public's interest in this message and the importance of the topic. On the other, the attention that followed was not without consequences. Like the McGovern report that preceded it, our work—though modest, in my opinion—enraged authorities in the food industry, their consultants, and their apologists in academic science communities. One prominent voice, Professor Tom Jukes of the University of

California, even lamented the moment as "the day that food was declared a poison."[5]

Within two weeks, the industry-controlled* Council for Agricultural Science and Technology (CAST) retaliated with a summary[6] of their own, which included the critical views of forty-five scientists (forty-two university faculty for added authority). Most were beholden to the agriculture industry. Some were prominent members of the aforementioned FNB, which had been excluded from writing the nutrition–cancer report. For good measure, copies of the critique were placed on the desks of each and every one of the 535 legislators of the US Senate and House of Representatives. Congress was thus served skepticism on a golden platter by a seemingly legitimate group of scientific authorities—and through them, so too was the public.

Additionally, I learned that the American Institute of Nutrition (AIN, now the American Society for Nutrition), a society of professional nutrition researchers of which I was a member in good standing, was angered by our committee's report. I became especially aware of this after being featured in a then relatively new consumer magazine, *People*; appearing on PBS's *McNeill-Lehrer NewsHour*; and giving expert testimony before House and Senate committees. This increasing visibility made me an easy and obvious target among my professional nutrition science community, and the AIN quickly set about making an example of me. First, my nomination by the executive council and election as the AIN president was aborted.† Next, the society revoked my nomination for its most prestigious award. Last and most significantly, the AIN's two most influential members filed a petition to expel me from the society. Although a formal hearing in Washington, DC, eventually and unanimously cleared me of any wrongdoing, it was clear that I had broken a few too many unspoken rules. Expulsion from the AIN would have been devastating to my reputation given that it was the only

* Established in 1972, CAST is a nonprofit 501(c)(3) organization that, according to its mission statement, "assembles, interprets, and communicates credible, balanced, science-based information to policy makers, the media, the private sector, and the public." Among its many sustaining members, you probably recognize several stalwarts of credibility and science-based information, including Bayer CropScience, the Coca-Cola Company, Land O'Lakes, Tyson Foods, and the preposterously named Merck Animal Health.

† According to an AIN staff member who was privy to the vote count.

professional organization of its kind, requiring a doctorate in nutrition and the publication of at least five peer-reviewed papers. In fact, I had the strange honor of being the target of the first expulsion effort in the society's history.

Ultimately, the AIN's attempts to besmirch me, nasty though they were, amounted to little more than petulance. As angry and shocked as I was at the time, I'm thankful for them now. I would not be where I am today without such episodes, and I wouldn't trade my place for anything. The reason I share them now is to illustrate just how sensitive our institutions are and how vengeful they can become when the conventional knowledge they espouse, and their authority to do so, are challenged.

Perhaps the most surprising aspect of this fracas was that the dietary goals outlined in our NAS report were quite moderate. As in the McGovern report before us, we recommended decreased dietary fat intake and increased consumption of fruits, vegetables, and whole grains. Although I insisted that the report include a chapter on the association between protein and cancer—a major focus in my work and in this book—and prepared the main draft for that chapter, this was meant primarily to encourage future research, and the report made no recommendations about eliminating meat products from the diet.[3] But even the inclusion of this section on protein was too much for most of the other committee members. I was later told by a colleague on the AIN Council, who was privy to the aborted presidential election and expulsion attempt, that I had "fundamentally betrayed" the interests of the nutrition research community. I had done this by publishing nutrition research that was not within the realm of "acceptable" knowledge, even though the research was twice vetted by professional peer review, once to acquire research funding and once to publish it in professional journals.

To build on the earlier point, then, evidence that threatens the status quo *in nutrition research* will always be controversial, whether it is true or not. The evidence in favor of reducing the intake of dietary fat was controversial then, and remains so now. Even without dietary recommendations on the subject, the mere inclusion of a chapter about protein and cancer was extremely controversial.

In the time since, I have seen many examples of how the scientific community selectively prohibits certain "controversial" subjects (when they threaten the status quo) from discussion. Even before the 1982 report, I

witnessed and experienced much the same timidity of thought and stagnation in the sciences of cancer and nutrition. In virtually every arena of science, including laboratory settings, classrooms, health policy boardrooms, and public lecture halls, I witnessed the same patterns. More often than I care to remember, I felt pressured to quit asking controversial questions and "return to the flock" (something I have discussed to some degree in previous books, especially *The China Study* and *Whole*).

The question this book will be asking is: *Why*? Why was it that the subject of animal protein in particular came to be forbidden from the study and discourse of nutrition? Why was it that nutrition came to be forbidden from the study and discourse of cancer? Why are these such incendiary issues in the first place?

HARNESSING THE CONTROVERSY OF THE WHOLE FOOD, PLANT-BASED DIET

The research I presented in *The China Study* and expanded in *Whole*, both from my career and from the careers of others, supports the adoption of a whole food, plant-based (WFPB) diet for the promotion of health and the prevention and treatment of disease. My research has been a deep source of controversy, which I believe offers a unique case study of the many challenges and opportunities facing science and our society as a whole. But let's take a moment first to review what I mean when I refer to a WFPB diet.

At its simplest and most accessible, the WFPB diet can be described in a dozen words, distilled into two recommendations:

1. Consume a variety of whole plant-based foods.
2. Avoid consumption of animal-based foods.

A WFPB diet is not the same thing as a vegan diet, which is defined by what it eliminates: animal foods. A WFPB diet is defined also by what it emphasizes: a variety of whole plant foods. By *whole*, I mean all a food's nutrients are consumed together, regardless of whether the foods are diced, sliced, cooked, or blended. I also mean that added oils and refined carbohydrates

such as table sugar should be used sparingly if at all. So-called convenience foods like potato chips are not whole. High in refined ingredients, they undermine health in every respect: they are calorically replete, nutrient deficient, and absolutely *inconvenient* in the long term. (Or can you imagine a scenario in which sudden coronary death is convenient?)*

I offer these dietary recommendations for optimal health on the basis of a wide range of evidence. This evidence includes:

- experimental laboratory-animal studies that observed a strong and mostly causal association between modestly high consumption of animal protein (anything in excess of about 10 percent of calories) and cancer—an effect that was not observed in the consumption of plant protein;
- experimental laboratory-animal studies that found at least ten mechanisms by which this animal protein effect was working, both in the early initiation phase and later promotion phase of cancer (adding what researchers call "biological plausibility," and suggesting that the cancer growth was not being caused by something else);
- a wide range of international correlation studies that show a linear correlation of animal protein with multiple cancers, cardiovascular disease, and other chronic diseases;
- human intervention studies that have demonstrated the reversal of heart disease by a diet absent of animal protein and composed of whole plant-based foods;
- and other corroborating evidence.

No other diet has ever been shown to not only prevent but also reverse heart disease, and there exist no large-scale, international correlation studies

* Though it will be covered in greater depth later, the subject of weight control often arises in discussions of a WFPB diet. It has been widely presumed that the WFPB dietary regimen requires no calorie counting, and in most cases I agree that this is not necessary. However, for those who are unable to lose body weight and sustain its loss, it should be noted that consuming excess calories, usually in the form of calorie-dense foods (e.g., nuts or avocados), or failing to get enough exercise, are also important considerations.

that show the opposite effect (increased animal protein consumption associated with decreased heart disease, cancer, etc.).

Moreover, there are virtually no nutrients contained in animal foods that are not better provided by plant foods. The adjoining chart shows the relative amounts of nutrients found in intact plant and animal foods for five nutrient groups. The differences are huge, as are their relative effects on health. Antioxidants, complex carbohydrates, and vitamins, all unique to plants,* have been repeatedly shown to prevent and treat heart disease, cancer, and other chronic degenerative diseases when consumed in whole foods (not supplements). Additionally, plant foods easily provide the necessary intakes of fat and protein long recommended by authoritative institutions, rather than the excessive amounts provided by animal foods.†

Nutrient Compositions*

COMPONENT	PLANT	ANIMAL
Antioxidants	Only Made By Plants	Almost None
Complex Carbs	Only Made By Plants	None
Vitamins	Made By Plants	Almost None
Fat	~9–11%	~15–20%
Protein	~9–11%	~15–20%

**PROCESSED FOODS are varied, likely worse.*

This compelling body of evidence in favor of a WFPB diet has already been surveyed and interpreted at much greater length in other books, including *The China Study*, so I will not cover it comprehensively here. My point is that I have been privileged to share this evidence for many years, through books (*The China Study, Whole,* and *The Low-Carb Fraud*), documentary films (*Forks over Knives* and *PlantPure Nation*), and nearly a thousand public and professional lectures around the world since *The China Study* was published in 2005 (and many, many more before then). And what I have learned during this time, and especially since I began to share this information more publicly in 2005, is that the WFPB diet is fascinatingly controversial among certain groups.

* Certain plants (nuts and avocado, for example) are higher in fat, but their activities when ingested as a whole food are favorable. Likewise, our bodies produce "vitamin D" given the right amount of sun exposure—deficiency is only a problem for those living closer to the poles.

† Certain plants (nuts and avocado, for example) are higher in fat, but their activities when ingested as a whole food are favorable. Likewise, our bodies produce "vitamin D" given the right amount of sun exposure—deficiency is only a problem for those living closer to the poles. Our bodies produce all the retinol we need when we consume the beta carotene produced by plants (beta carotene is the real vitamin A. Likewise, our bodies produce "vitamin D" given the right amount of sun exposure—deficiency is only a problem for those living closer to the poles.

I believe there are three primary reasons for this controversy:

1. The WFPB diet and its supporting research findings challenge the conventional understanding of **disease**, both its causes and treatments. This is especially true of cancer, long regarded as a genetic disease triggered by environmental carcinogens, not poor nutrition. Likewise, the treatments for cancer considered best practice have traditionally been invasive, targeted protocols—surgery, radiation, and chemotherapy—versus nutritional treatment (which admittedly needs additional discriminating research). The WFPB diet and its supporting evidence could seriously undermine these long-held beliefs and practices.
2. The WFPB diet and its supporting research findings challenge the conventional understanding of **nutrition** itself, especially orthodox attitudes toward animal protein, which has long been regarded as the most influential nutrient and has played a determining role in our dietary preferences.
3. Perhaps most fundamentally, the WFPB diet and its supporting research findings challenge the conventional understanding of what reliable **science** and scientific evidence look like. Modern science is increasingly specialized, reductionist, and bent toward the production of technological solutions. In "nutrition science," this means the production of pharmaceutical solutions and nutrient supplements. The WFPB diet is controversial because it disputes this prevailing norm and demands a more *wholistic* view of evidence.

When we dissect these points of controversy, a bigger picture emerges about how and why our institutions codify what kinds of science—what hypotheses, research proposals, and data interpretations—are (and are not) accepted for funding, publication, and policy development. This, in turn, impacts both the way in which we (mis)use past science and the possibilities for future science. In short, by investigating the three points of controversy above, we can learn a great deal about the entanglement of science and institutions—from academic institutions such as Cornell University, to

professional institutions such as the AIN, to public policy agencies and advisories such as the Dietary Guidelines Advisory Committee.

I am excited to elaborate on this controversy and institutional dysfunction because it transcends the subjects of the WFPB diet, nutrition, and even science in general. In nutrition, it has led to mass confusion about the most scientifically justified approach to eating, and even about how nutrition operates, with devastating consequences for our society's health. But it also has a tremendous impact on other fields and raises questions of great importance for politics and ethics. The institutional dysfunction that I am describing has led not only to excessive health care costs and imposing environmental problems, but also to mass public and professional confusion, disillusionment, and disengagement.

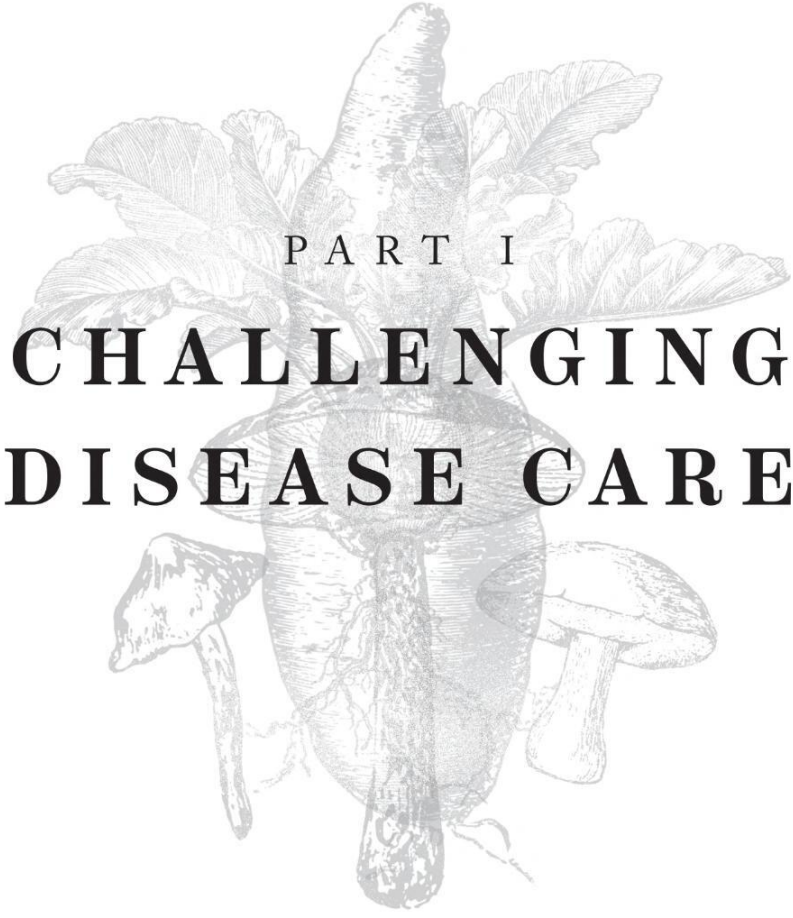
A ROADMAP

This book is organized around the three areas of controversy listed above. We will look at each of them in turn, to bring into greater focus the challenges facing nutrition, science, and the health of all society. We will then conclude with a number of suggestions for how we might evolve and restore function to the institutions affecting science (funding, publication, education, etc.), thus changing the future of nutrition—empowering the public to better their health, the health of their communities, and the health of the planet.

My primary hope is not that everyone reading this book will eat the same diet that I eat (though I would obviously recommend it), for I think the themes and implications of this investigation are of an even greater, universal concern. The reason I devote this particular book to discussing the science of nutrition is not to pigeonhole these topics, but because it is the science to which I have been dedicated for more than six decades. Likewise, the reason I discuss the controversy generated by the WFPB diet is not to alienate or convert anyone, but because I cannot possibly escape that controversy, and because it offers the most profound case study of institutional dysfunction that I could possibly imagine.

In that spirit, I am not interested here in debunking fad diets, advertising superfoods and quick fixes, or heaping onto the controversy that already

exists. Rather, I want to embrace what controversy exists and admit it for inspection—not because controversial evidence is patently false, but because controversy is the inevitable result of challenges to the status quo. I would like to make sense of the origin and promotion of this controversy because of what is at stake. When it comes to human health, the status quo is an ugly thing: every day, human lives are impoverished, disabled, and ended by avoidable diseases. Is that a status quo worth preserving? And so, I return to the controversy to make sense of it, so that we might begin to make sense of ourselves.



PART I

CHALLENGING DISEASE CARE

CHAPTER ONE

DISEASE CARE TODAY

Nothing is more expensive than a missed opportunity.
—H. Jackson Brown Jr.

We can no longer deny it—the health of our society is in critical condition and has been for quite some time. The culprit? Preventable lifestyle-related diseases—including heart disease, stroke, cancer, type II diabetes, obesity, kidney disease, rheumatoid arthritis, and any other disease for which the patient's outcome is strongly associated with lifestyle choices such as diet—and a fundamental misunderstanding in our society about where these diseases come from.

Chances are that you, like the vast majority of the population, have had firsthand experiences with one or more of these diseases. Perhaps you have lost friends or family to heart disease, stroke, or cancer, or maybe battled these or other diseases yourself. These are the villains of real-life horror stories, and their cost to society, both in dollars spent and lives lost, cannot be overstated.

The lives prematurely lost to heart disease alone—647,000 every year—boggle the mind. That's more than the population of many American cities, including Baltimore, Memphis, Atlanta, Miami, Albuquerque, and Sacramento. Can you imagine losing the equivalent population of one of those cities, each and every year, for the foreseeable future? Imagine the public outcry if 647,000 Americans died every year in a needless war against an invented enemy. Worse yet, imagine if it were already happening and no one addressed it! *And that's* only *heart disease*. What about other preventable diseases? The Centers for Disease Control and Prevention (CDC) lists the top five causes of death in 2017[1] as heart disease (647,000), cancer (599,000), accidents (170,000), chronic lower respiratory disease (160,000), and stroke (146,000). But here's the kicker: these are not inevitable deaths. Estimates suggest that up to 90 percent of heart disease deaths,[2] 70 percent of cancer deaths,[3] and 50 percent of stroke deaths,[3] plus my estimate of 80 percent of medical error deaths (more surgeries and cancer treatments = more chances for mishaps), could be prevented by informed use of nutrition.

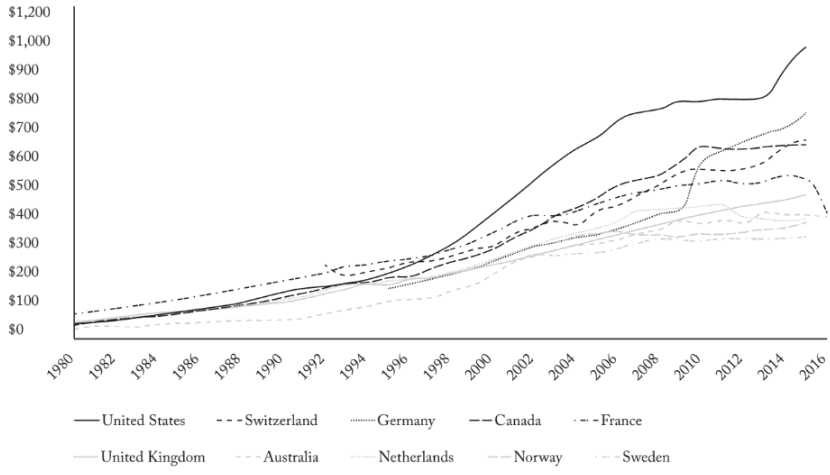
That these diseases could be prevented offers hope—and it should—but also condemns our current approaches. If so much suffering and its attendant costs could be prevented through better nutrition, then why have we not done so? Have we forgotten that these are more than just numbers, that these are lives lost prematurely and families left behind? Like you, I know this personally. In March 1969, my mother-in-law discovered blood in her stool and went to the hospital, where she was seen by a doctor before being promptly sent home with a laxative. With no money (or insurance), no knowledge of her problem, and no information on how she might have avoided the problem, she was the victim of a broken system. She did not tell her daughter—my wife—or get the opinion of another doctor. By the time we did know, nine months later when she returned to the hospital, it was too late. This time she received a proper diagnosis: advanced colon cancer. Barely older than fifty, she spent the following three months, the final three months of her life, in the hospital. In March 1970, a year after that first appointment, she passed.

Two years later, while I was working in the Philippines, my father passed prematurely as a result of cardiovascular disease. My mother and a family friend had to travel over country roads to bring him to the nearest hospital, about twenty minutes away, and he never made it. I was shocked. Here was

a man who was not overweight, who spent many hours working outdoors on the farm, and who ate what was considered to be a healthy American diet—a model of the "good" behavior encouraged at the time—and yet he died anyway.

Little has changed in the decades since. If anything, disease has become an even more normal part of American life, as illustrated by the continued growth of the pharmaceutical industry. In 2017, the average American's out-of-pocket pharmaceutical costs (including for those covered by insurance) amounted to a shocking $1,162.[4] Fifty-five percent of Americans take prescription drugs—four per day, on average[5]—and many of these people, as well as many of the minority who do not regularly take prescription drugs, take dietary supplements, too. We are also one of only two countries in the world that permits direct-to-consumer TV advertising of drugs, instead of advertising only to qualified physicians.* By any measure, we are fixated on magic pills, certainly more than any other country in the world. This does not suggest health, but rather the normalcy of disease.

National Trends in Per Capita Phamaceutical Spending, 1980–2015



* As of this writing, I was told by an interviewer in New Zealand, the other country where they allow direct-to-consumer advertising, that her country is in the process of changing their regulations.[6]

Along with our current approach to treatment, the economic costs of preventable disease are unsustainable. And they are rising: in 2020, health care costs occupy nearly 18 percent of our national budget, more than three times greater than in 1960 (5 percent), and total $3.5 trillion.[7] According to a PBS television program that reported on a comprehensive survey of health care,[8] the US pays two-and-a-half times more, per capita, for health care than thirty-five similarly affluent countries (members of the international Organisation for Economic Co-operation and Development).[9] This is not the result of higher infrastructure or labor costs, as some might expect. In fact, the US only has 2.4 physicians and 2.6 hospital beds per 1,000 people, both of which are less than the average among its OECD sister countries (3.1 physicians and 3.4 hospital beds per 1,000 people). Using these averages,[9] I calculate that the US spends a far greater proportion of its health care costs on pills than similar countries (approximately 3.3 times more). This estimate, which I call a "drug intensity index," reflects an unrivaled, historic emphasis on using drugs as the principal means of health care.

And how effective has this approach been? From my perspective, not effective at all. Although many commentators among the public and the media point to statistics on *life expectancy* as proof positive of our improved health, these statistics should be taken with a grain of salt. Life expectancy as a simple indicator of our health is limited. It's important to know not only *how long* we expect to live but also *how well*. Long life coupled with disabling, painful disease that exacts a heavy toll on family resources is not what most people want. All the same, changes in life expectancy do comprise an important part of our collective health history and deserve some attention. During the past two centuries, when most Western countries were transitioning from poverty to affluence, life expectancy greatly increased. This is because total *mortality* decreased, mostly due to a reduction in childhood infectious diseases.[10] Starting in 1840, life expectancy increased at a rate of three months per year, until the 1950s and '60s, at which point the rate of increase slowed to two months per year (once we had reduced mortality from infectious diseases, there was less potential for extending lifespan).

Our life expectancy continued to increase at a rate of two months per year, from seventy-one years in 1960 to more than seventy-eight years in

2014.[11] But in 2015, the rate of increase dropped by half, to only 1.2 months for that year. This sparked concern, though some thought it was a statistical fluke. Not so. For the next three years (2016–2018), average life expectancy actually *decreased*, from 78.8 years to 78.6 years—the longest sustained decline in expected lifespan since 1915–1918, when the decline was "partially attributed to the casualties of World War I and the devastating 1918 influenza pandemic."[12] A decline in life expectancy of 0.2 years may not seem very impressive, but it is nonetheless highly statistically significant. In a population of 300 million, 0.2 fewer years of life expectancy translates to six million people failing to live an extra ten years, or three million people failing to live an extra twenty years.*

The director of the CDC called this backslide in life expectancy "a wakeup call."[13] Many have linked it to increasing rates of drug overdoses and suicides, but I would argue that these deaths do not occur in a vacuum, and may also be partly linked to preventable lifestyle-related diseases. Chronic preventable disease is a chronic drain on our quality of life, and thus has a profound negative impact on psychological wellness, which feeds overdose and suicide. Some may counter that overdose and suicide are more closely related to economic hardship than health concerns, but again, these phenomena are closely entwined, as illustrated by the overwhelming cost of health care. Disease is expensive, especially chronic disease.

In a nationwide study published in the *American Journal of Medicine*, Harvard and Ohio University researchers found that 62.1 percent of all bankruptcies in 2007 could be attributed to medical expenses.[14] It gets worse. Three-quarters of those debtors had health insurance and most "were well educated, owned homes, and had middle-class occupations." In other words, the system is so broken that even well-off people end up with unmanageable debt. Where does that leave the less fortunate, who are disproportionately affected by lifestyle diseases? Compared to a study conducted in 2001—just six years prior—"the share of bankruptcies attributable to medical problems rose by 49.6 percent." These figures are scandalous, but not surprising when you consider the rising costs of standard treatment. It costs at least $20,000

* The derivation of this number was contributed by a friend, Damon Demas, PhD, a professional mathematician.

per year to treat heart disease with stents and statins, and the average cost of one round of chemotherapy ranges from $20,000 (office-managed) to $26,000 (hospital-managed).[15]

Still, life expectancy *did* improve up until 2015. Surely that's a sign of progress, right? Yes and no. Some may be surprised to know that our increasing life expectancy from the '60s until recently is not due to improved health as much as improved strategies for responding to disease events. More and more people suffering from cancer, stroke, obesity, and diabetes are able to live longer with their diseases than before. Improved survival rates have been especially significant for those who suffer heart attacks. Indeed, about 60 percent of our increase in total life expectancy since 1960 can be attributed to improved rapid response in heart disease alone.[16] Yet our overall health has not improved significantly during this time. Incidence rates (new cases of disease) have remained relatively stable for heart disease and stroke, declined slightly for cancers (mostly because of less smoking-related lung cancer), and increased for diabetes (associated with our increased rate of obesity). Improvement in personal living conditions (for example, improved access to programs in stress management, physical conditioning, and routine medical care) following diagnosis has resulted in a modest increase in years of life lived with disease, but not the eradication of disease.[10,17]

Putting all of these trends together, one might tentatively argue that we have improved our treatment of disease. By responding to crises more quickly and improving living conditions, we manage our morbidity better than before. But we have not addressed the underlying causes of these diseases, or given any attention to the possibility of developing more effective means of treating and even reversing disease other than with the use of pharmaceuticals. The result is many more people requiring care, which has increased the burden on our health care system. This phenomenon could be called a failure of success, and it may get far worse yet. Some may point out that rising drug costs, which for a long time outpaced total health care costs, have slowed down as of 2019, but, as a proportion of total health care costs, drug costs in the US are still much higher than in other OECD countries.[18] As long as we remain dependent on those drugs to maintain life, without examining the factors affecting disease prevalence, we will continue to suffer the financial and quality of life consequences. This "failure of success" is no

true success at all; true success would combine an increased life expectancy with less disease. Still, the parade marches on.

Despite some improvement in the *management* of disease, the struggle to treat disease remains elusive. One of the main reasons for this continued struggle has already been introduced: our outsized dependence on drugs, which attack symptoms without addressing their lifestyle roots, and divert resources and attention away from other strategies. Moreover, these drugs are causing a health crisis of their own:

- According to a report by Donald Light of the Safra Center for Ethics at Harvard, "Few people know that prescription drugs have a 1 in 5 chance of serious reactions *after* they have been approved" (emphasis added), and approximately 2.74 million hospitalizations per year can be attributed to adverse drug effects, not even including cases of misprescription, overdose, and self-medication.[19]
- "About 81 million adverse reactions are experienced by the 170 million Americans taking drugs."[19]
- According to a report by Public Citizen's Health Research Group, "Every day more than 4,000 patients have adverse drug reactions so serious that they need to be admitted to American hospitals."[20]
- In 2014, according to WebMD's citation of *Consumer Reports*,[5] nearly 1.3 million people "sought emergency room treatment for adverse effects of prescription drugs and about 124,000 people died."
- Prescription drug use is the fourth leading cause of death in the US, an estimate similar to a 1998 estimate by Starfield.[21] According to a 2018 US Food and Drug Administration report, annual US deaths from adverse effects of prescription drugs are estimated to be 106,000.[22]

The counterargument to these startling figures is that we need to balance the rate of adverse drug-related incidents against the number of individuals who benefit from drug use (i.e., efficacy) if we are going to properly assess the usefulness of drugs. As one report states, "if we suppose that all [170 million estimated drug users in 2014] benefit [from drug use], then the 2.7 million

severe reactions is only about 1.5%."[19] However, this is a very low estimate of adverse reactions and it supposes that *all* users are benefiting from their drugs (an outrageously optimistic assumption). Nor does it consider adverse reactions that do not result in hospitalization, of which there are thirty times more.[19]

Of course, I don't mean to underplay the medical advances we have made over the last few decades, especially the rewards of faster response times. It's good to know that today my father would likely be able to get to the hospital much more quickly. Likewise, I'm impressed by the caretakers throughout our health care system. According to the Kaiser Family Foundation, there are over thirteen million such folks: neighbors, friends, professional specialists, and health care workers of all kinds.[23] I'm certain that virtually all of them are dedicated and compassionate servants of health. But overall, we're struggling. Our declining life expectancy leaves America ranked forty-fourth in the world,[24] an astonishing and disturbing rank considering that we have the highest per capita health care costs in the world, by an eye-popping margin. How do we reconcile our massive medical bills with such a low rank? Considering all of these trends and statistics simultaneously—very high drug use, declining life expectancy, and unusually low rank in life expectancy—it's hard to believe that we are on the right path.

And neither will this issue resolve itself. Virtually all reports advocating the use of pharmaceuticals are driven by the profit motive, and the profit has been undeniable. In 2017, global pharmaceutical revenue totaled $1.143 trillion, with a projected growth rate of 4.1 percent.[25] That's more than the revenues of national government budgets in *all but five* countries.[26] With such wealth comes tremendous power, and with that power, even greater influence on public and professional perceptions. In short, as long as the pharmaceutical industry continues to profit from our disease, crouched beneath the God of Fertile Markets, our questionable dependency on drugs will perpetuate itself, no matter how ineffective this approach has proven to be. Unless we do something about it, the health of our society will worsen still.

THE ROLE OF MALNUTRITION

The answer, then, isn't more or better drugs. It is understanding and addressing the primary culprit behind many of these diseases: malnutrition.

Malnutrition is a word I use advisedly. Although the word is usually reserved for descriptions of diets that are calorie deficient or missing certain essential nutrients, its literal meaning (faulty nutrition) also applies to dietary patterns of excess, which pose a much greater threat to most Americans.* This includes many Americans living in poverty. The poorest members of our society generally consume foods higher in simple sugars and excess oils, both of which contribute to obesity and higher risk for diabetes and cardiovascular disease, because these foods tend to be cheaper. Research going back many decades, including the landmark, decades-long Framingham Heart Study,[27] has linked heart disease with various risk factors, including high blood cholesterol and high blood pressure, that are symptoms of malnutrition. Furthermore, a combination of international[28] and migration studies[29-31] suggests that diet, as an environmental factor, plays not a minor role but in fact *the most significant* role in heart disease risk. Corroborating experimental research verified this more than sixty years ago: in a 1946–1958 study, Dr. Lester Morrison[32] split a group of heart attack survivors into two groups, one control and one experimental. In the experimental group, he instructed patients to reduce their consumption of fat and dietary cholesterol from 80–160 grams of fat and 200–1,800 milligrams of dietary cholesterol to 20–25 grams of fat and 50–70 milligrams of dietary cholesterol. After twelve years, every patient in the control group was dead, while 38 percent of the experimental group had survived. More recent research[2,33] suggests that this 38 percent survival rate can be elevated much further (upward of 90 percent), given a more complete dietary shift than the low-fat protocol designed by Morrison (in his study, for example, patients were still allowed to eat small amounts of lean meat). Nevertheless, the results could not have been clearer: *what we eat plays a significant role*

* Although I characterize Americans' dietary patterns as excessive, it is also true that certain deficiencies are common. Many Americans suffer from a lack of fiber and vitamins and minerals that are only found in plants.

in determining heart disease outcomes. Similar forms of evidence, including international correlation studies, migration studies, and experimental laboratory animal studies, have similarly linked diet with cancer, diabetes, obesity, kidney disease, and more.

By incorporating this research, and conservative estimates of the potential influence of malnutrition—which, as we saw earlier, suggests that massive numbers of deaths from heart disease,[2] cancer,[3] stroke,[3] and medical errors (assuming decreased need for drugs and other medical interventions that provide opportunity for such errors) could be prevented by good nutrition*—you can see the CDC's previously introduced list of the top causes of death transform.

Top Causes of Death in 2017	Adjusted for Malnutrition
Heart Disease: 647,000	Heart Disease: 65,000
Cancer: 599,000	Cancer: 180,000
Accidents: 170,000	Accidents: 170,000
Chronic Lower Respiratory: 160,000	Chronic Lower Respiratory: 160,000
Stroke: 146,000	Stroke: 73,000
Medical Errors: 250,000–440,000	Medical Errors: 50,000
	Malnutrition: 1,275,000

That's more than a million lives lost every year to unnecessary disease in the United States alone. If ever there was a situation befitting the phrase "room for growth," this is it; with proper nutrition, these lives lost to unnecessary or premature disease can be saved, and the enormous financial burden redirected toward funding projects and policies that improve our communities' well-being.

If I am correct in this assessment, as the evidence suggests, then why are more people not focusing on nutrition as a solution? Why did my father, and so many others, not learn of Morrison's heart disease study, which was conducted well before my father's second, fatal heart attack? Why isn't nutrition fully embedded in the training and practice of cardiologists, oncologists, and other medical practitioners, from top to bottom? Why are we not more interested in learning from the dietary patterns of other cultures that show almost no incidence of heart disease,[35] our number one killer? Why do we

* Although these numbers are approximate, I have erred on the side of caution and provided conservative estimates. For instance, although nutrition can play a role in chronic lower respiratory disease, there is no reliable estimate of the percentage of lives affected, so I have therefore chosen to leave that number as is. In the case of medical errors, I have made calculations using the lower estimate of deaths caused by medical errors (250,000, though estimates have been as high as 440,000).[34] Moreover, I have not even accounted for the many more preventable diseases not listed in the top six (perhaps the most notable of these is type II diabetes, which can be treated by nutrition in nearly 100 percent of cases).

continue to underplay nutrition's significance, and instead devote massive amounts of time and resources to invasive procedures and pharmaceutical Band-Aids?

Two fundamental observations help to answer these questions. The first is that the prevailing cultural narrative in our society tells us that malnutrition and disease are only partially connected. The extent to which people believe this depends on the disease (e.g., more people will say that nutrition plays a role in heart disease than cancer), but in general our society does not consider malnutrition to be the primary cause of most disease, and certainly not the preferred cure. Even in cases where we do admit nutrition's role, it is often secondary. For instance, you have probably at some point been advised to eat well in order to minimize the risk of developing a genetically determined disease. The notion that nutrition can do more than minimize such risk—that it can even eliminate it, and in many cases trump genetic determinism—is not widely accepted. We give lip service to nutrition by advising "heart-healthy diets" and the like, but these are discussed superficially and always in conjunction with other lifestyle choices like exercise.

Crucially, though, we are also confused about nutrition. This is the second fundamental observation: our prevailing cultural narrative tells us that *even if* nutrition and health are closely connected, we *still* aren't sure what the healthiest diet looks like.

For the rest of this chapter and the two that follow it, I will be focusing on the first of these observations: that (mal)nutrition is not fully appreciated as a determinant of disease and health. The second point—the confusion infecting our attitudes toward and uses for nutrition—will feature heavily in Parts II and III of this book. For now, though, it bears repeating that the WFPB dietary lifestyle is controversial because it challenges *both* prevailing narratives in our society.

CANCER AS A CASE STUDY: THE NEVER-ENDING WAR

Nowhere is the power of nutrition, and its inverse, malnutrition, more underappreciated than in the field of cancer. It also happens to be the field to which I have devoted the majority of my research career, and so I can speak to the attitudes that pervade it with more authority than on any other field.

Consider, then, the following "Findings and Declaration of Purpose," copied and pasted from legislation[36] passed by the United States Congress. I like this example because it illustrates the failure of our disease-care system better than most:

(a) The Congress finds and declares:

(1) that the incidence of cancer is increasing and cancer is the disease which is the major health concern of Americans today;
(2) that new scientific leads, if comprehensively and energetically exploited, may significantly advance the time when more adequate preventive and therapeutic capabilities are available to cope with cancer;
(3) that cancer is a leading cause of death in the United States;
(4) that the present state of our understanding of cancer is a consequence of broad advances across the full scope of the biomedical sciences;
(5) that a great opportunity is offered as a result of recent advances in the knowledge of this dread disease to conduct energetically a national program against cancer;
(6) that in order to provide for the most effective attack on cancer it is important to use all of the biomedical resources of the National Institutes of Health; and
(7) that the programs of the research institutes which comprise the National Institutes of Health have made it possible to bring into being the most productive scientific community centered upon health and disease that the world has ever known.

(b) It is the purpose of this Act to enlarge the authorities of the National Cancer Institute and the National Institutes of Health in order to advance the national effort against cancer.

You may understandably think this sounds like a good start. After all, who could argue against increased efforts against cancer, the coordinated effort and use of all the NIH, and the enlarged authority of institutions engaged in this critical fight? As this law observes, cancer is a leading cause of death, so these efforts seem apt and well timed.

At least, they seem apt and well timed until you realize that this legislation, the National Cancer Act, was not passed just recently. I'm sorry to have misled you, but I feel it proves a point. This bill was not passed this year or last, but all the way back in 1971, precisely between the passing of my mother-in-law and the passing of my father. It was passed in the same year that Nixon signed an amendment to lower the voting age to eighteen; a year when forty cents was enough to purchase a gallon of gas; the year when Apollo 14 was launched only a few months before the opening of a new theme park named Disney World.

Clearly, many things have changed in the nearly fifty years since Congress passed the National Cancer Act of 1971. But what concerns me most are the things that have *not* changed. Cancer remains a leading cause of death. Advances across all biomedical sciences continue to astound, and have contributed greatly to increasing "the present state of our understanding," but what benefits have we reaped from that understanding? Our ability to treat cancer has not improved, despite an extraordinary amount of resources dedicated to this mission. Last and most important, nutrition remains as undervalued and underutilized now as it was back then.

Hailed as the first strike in the "war on cancer," the National Cancer Act of 1971 is not the result of bad intentions, but of a faulty premise. It updated and retooled the National Cancer Institute to its current form, established new cancer research centers, and signaled a new, proactive campaign against one of our most dreaded diseases. The faulty premise undermining it—the flawed assumption that hasn't been proven out—is that the NCI and NIH were suitably armed for the war on cancer, when in fact their armory was and is missing the single most potent weapon in the war on cancer: nutrition.

Among the twenty-seven institutes and centers at NIH, not a single one is dedicated to its study.

It's not just hopeful defenders of nutrition who would critique the war on cancer. Many established cancer professionals agree. In an article published in *The Lancet* a few years ago,[37] one critic characterized the war on cancer well: "Despite extraordinary progress in our understanding of disease pathogenesis, in most cases and for most forms of cancer this war has not been won." I'm sure you will agree with the author's most sobering concerns about cancer in the twenty-first century: (1) "cancer treatments are very expensive," (2) cancer treatments "[produce] only transitory clinical benefit," and (3) "the instrumental mutations and rearrangements of the human genome in the transformed cancer cells are extremely complex," and therefore extremely difficult to study.

Ultimately, however, the author does not demand a radical shift in strategy, and definitely not a prominent role for nutrition. Rather, he intensifies and doubles down on the war metaphor. He describes a "military battlespace" strategy capable of "incorporating information about the enemy's characteristics and armamentarium, precise topographical maps of all potential battlefields and war zones, the weather, and other environmental factors, along with a census of friendly forces and their capabilities, in all relevant geographical locations." In other, simpler words, he calls for a more sophisticated battle plan, but ultimately one that still relies on a technological understanding of cancer and medicine. The author does not discount the war on cancer's efforts, but instead argues for a more technically impressive application of what we have learned: "Although the dual metaphors of the war on cancer and of magic bullets to kill cancers *have been useful*, *now is the time to refine them*, factoring in extraordinary advances in knowledge about cancer science and medicine" (emphasis added). Rather than question the magic bullet premise, in which each specific disease can be combated by a specific drug, without side effects, he encourages us to invent a more perfect, more targeted magic bullet that does not hit anything other than its target. Assuming such a thing even exists (no small assumption), I wonder how long it will take to find it?

Meanwhile, the war has gone global. In another *Lancet* article, researchers Paolo Vineis and Christopher P. Wild[38] at the International Agency for

Cancer Research (of the World Health Organization) make the case that "an increasing proportion of the [cancer] burden [is] falling on low-income and middle-income countries . . . urgent action is needed . . . [and that] primary prevention is the most effective way to fight cancer." I agree with all three of these statements. However, they refer only to strategies of primary prevention, whereas I would add that it is also time to consider the effect of this same nutrition protocol on cancer *treatment*. If our primary prevention strategies cannot integrate the strongest research findings on cancer, including those involving nutrition, then our organizational and structural interventions will never reach their full potential. The war on cancer will continue to amass an alarming body count and place tremendous demands on our resources and attention, only now on a global level.

I could critique the strategies of cancer researchers all day, but we must not forget about other branches of the biomedical establishment. If researchers specializing in disease are like strategists, holed up in their bunkers and studying the enemies' defenses, out on the field we have that valiant class of soldier we call Doctor. Understand, I am not blaming any individual here, but the system as a whole, and its disregard of nutrition. These soldiers are fighting a losing battle, because their weapons, their thoughts and actions, are limited. Given scalpels, pills, and radiation, they do not consider (or comprehend) strawberries, potatoes, and radicchio as agents of health.

And how could they? Not a single US medical school trains doctors in nutrition. Of the approximately 130 official medical specialties for which services can be reimbursed, nutrition isn't one of them. Doctors and nurses are the face of health care, responsible for delivering information and treatment to the public, yet they're given no financial reimbursement for nutritional services or education on the medical marvels of nutrition. It's as if they've been blindfolded, spun round and round, and asked to lead the way. Is it any surprise that they sometimes seem to be stumbling in the dark?

The failed war on cancer displays contemporary attitudes toward nutrition and disease better than any other example I can think of. As with the broader trends in our society's health, it illustrates a stubborn persistence that has not paid off. The incidence of cancer has declined somewhat in recent decades as a result of decreased smoking-related lung cancer, but overall

we're losing the war. One might think that, faced with such a struggle, we would be more open to alternative approaches, but this has not been the case at all. Instead, we have seen nearly the opposite. Despite the impotence of conventional cancer prevention and treatment strategies, the medical establishment clings to them. Nutrition has received almost no attention, and any suggestion that it might deserve attention is regarded with skepticism.

To understand how nutrition came to be so underutilized, and why these attitudes persist today, it's helpful to examine the history of research into the relationship between nutrition and disease, especially cancer. It is in this history that key patterns emerge—patterns that continue to dominate our attitudes and practices, often beyond our awareness.

CHAPTER TWO

THE HIDDEN HISTORY OF NUTRITION AND DISEASE

Nothing is more responsible for the good old days than a bad memory.
—Franklin Pierce Adams

The episode I discussed in the introduction, the 1982 National Academy of Sciences (NAS) report on diet and cancer that I coauthored and the unusual opposition that followed, was a key moment in my career—not only because it shattered my naïveté and revealed how controversial dietary recommendations about protein could be, but also because it gave me many questions to explore in the years that followed. It encouraged

me to reflect on the role institutions play in the dissemination of information, the responsibility of dissident voices within institutions, and, in general, the painful side effects of scientific advancement. Crucially, it also encouraged me to look deeper into the history of nutrition and disease research, especially when it came to cancer.

Like others on the NAS committee, I had assumed that our findings on the diet–nutrition–cancer association were relatively new, and that new ideas in science naturally attract criticism. After all, most of the research cited in our report was published in the 1960s and '70s. The earliest of all the research papers we cited was published in 1931.[1] Still, I sensed that there might be something more insidious in the reaction we had received, something worth unpacking further. The supposed novelty of our report failed to explain the *degree* of criticism we faced. It seemed to me that there was something more to this story than new science versus old science. In fact, the criticism seemed beyond intellect. It was visceral, intense, and clearly tethered to food industry interests, especially those of animal-protein-based foods.

Eventually, I turned to the past for insight. I took a deep dive into the history of nutrition and cancer, hoping to find some greater sense of context—additional vantage points from which to consider the vitriol I'd experienced, both personally and professionally. I had the perfect opportunity to do just that when I spent a year at Oxford University on sabbatical from 1985 to 1986. The year I spent there could not have come at a better time. As I dug deeper into the history, I tried as much as possible to read original manuscripts and reports. Consequently, I lived most of that year in four libraries: the Bodleian Library and the Wellcome Trust Library in Oxford, and the Royal College of Surgeons and Royal College of Physicians in London.

Because I was unsure of where and when the disciplines of cancer and nutrition may have previously overlapped, if ever, the little I did know could be described, at best, as superficial. Thankfully, it didn't take long to find a starting place: Frederick Hoffman's *Cancer and Diet* (1937),[2] brought to my attention by a postdoctoral resident in my Cornell laboratory, Tom O'Connor.*

* Now a senior professor at University College Cork in Ireland.

HOFFMAN: A HIDDEN PIONEER

Hoffman was an author I had never heard of before, and what I found in *Cancer and Diet* was exceptional: a 749-page book with a vast number of references investigating the possibility of a nutrition–cancer association. To my surprise, it quickly and definitively proved to me that our 1982 NAS report was not especially new and that research into nutrition and cancer had once overlapped. In my initial scan of the book, I was especially impressed by Hoffman's exhaustiveness. In his own words, "every work referred to, otherwise than in abstract, has been read carefully by myself from beginning to end to make sure that no important observation should be missed." He goes on to say that his review[2] is limited to approximately 200 authorities because he had "neither the strength nor the time to visit other libraries for the purpose of amplification and completeness."*

Reading this was an eye-opener, to say the least. *Limited* to 200 authorities? The science supporting the conclusions of the 1982 NAS committee was surprisingly deeper, perhaps far richer, than we'd thought. Contained within that realization was, I believe, a valuable lesson and warning about the nature of science. Too often, "trailblazers" of the day fail to investigate the full breadth of the scientific literature. They assume, often smugly, that their discoveries are uniquely novel. Our NAS committee was guilty of this, too. We assumed that our survey of past literature was relatively comprehensive, when in fact we barely scratched the surface.

Hoffman's *Cancer and Diet* proved to be an invaluable resource, both for its exhaustive content and for its professional presentation, but it also raised many questions. Chiefly, who was Frederick Hoffman and why had I never heard of him before? He died in 1946, only thirty-five years before our 1982 report, yet he was a complete mystery to me. The more I learned about his work, the more perplexed I was by his erasure. He was by any measure one of the most productive and professional scientists I have ever encountered, yet it was very difficult to find details about his life—though I did find at least one other author, Francis Sypher, who in a 2012 journal

* Hoffman had been suffering from Parkinson's disease for about ten years when he wrote his 1937 book.

article asks similar questions about why Hoffman was so abruptly forgotten when he died.[3]

What few biographical details do persist can be summarized quickly.[3] Hoffman arrived in the United States from Germany in 1884. He had a restless youth and certainly didn't come from wealth. In fact, he was unable to afford secondary school, and he never attended university. In his early years, he had a yen for traveling the world and learning new things. Perhaps in order to maintain that lifestyle, he worked on an assortment of odd jobs, until he finally landed a more permanent position with the Prudential Insurance Company in Newark, New Jersey, where he worked for the next forty years. Despite his lack of formal education, his aptitude for statistics made him an ideal candidate for actuarial work, including calculating and predicting disease risk. He was apparently quite good at this; so good, in fact, that he was able to ascend into the higher echelons of professional statistics and eventually become president of the American Statistical Association.[4]

In these biographical details, we get perhaps the first glimpse of why he has not been celebrated in the histories of cancer research. His poor immigrant upbringing, missing as it did a typical education, squares with his later outsider status. What's extraordinary, though, is how far he went despite that combination of obstacles. His work ethic is hard to deny, and his productive output was surely enough to rival even the most privileged researchers of his day. Throughout his career, he published "1300 items, including 28 major works of 100 or more pages," according to Sypher.[3] Early in his career, he was especially interested in the effects of the "dusty trades" industry[4] (a term used to describe occupations in which workers were exposed to heavy amounts of dust, including sand blasters, graphite miners, and carpet mill workers) on respiratory problems like tuberculosis and "dust phthisis in the granite stone industry."[3] His work in this area had a significant impact on labor legislation affecting occupational hazards.[5] As such, he was a charter member of the National Tuberculosis Association.[4]

But it was cancer that dominated his focus, especially at the peak of his career. On this topic alone, he authored sixteen books and an estimated one hundred professional publications.[5] His early interest was in trying to understand why cancer rates had increased so much since the beginning of the 1900s and why cancer rates varied so much both within the US[6] and

internationally.[7] In 1915, he published an 826-page tome that tackled this question directly, documenting the great range of cancer rates in different parts of the world.[7] Eight years later, he studied cancer death rates, controlled for age, in twenty-two cities and localities in the US and elsewhere.[6] One of the most interesting details of this study, from my perspective, is that he surveyed consumption of various food groups, including "green vegetables, fresh fruits, cereals, white bread, condensed and conserved food, meat, sugar, salt, etc."

Another striking biographical detail, given his later obscurity, was Hoffman's very central role in founding the American Cancer Society (ACS; founded as the American Society for the Control of Cancer). In 1913, he delivered a much anticipated speech to the American Gynecological Society titled "The Menace of Cancer," in which he expressed alarm at rising cancer rates.[8] This speech led directly to the founding of the ACS, as the organization itself acknowledges in a picture in a lobby showcase in their Atlanta headquarters. In that speech, he recommended that "the nutritional influences on the induction of cancer be analyzed." He also called for greater proactivity: "The time has come for a nationwide interest in the problem of prevention and control [of cancer]." In *Cancer and Diet*, published twenty-four years later, he took an even firmer stance on nutrition's role in cancer. The evidence by that time was, he said, "fully sufficient to prove that cancer from the earliest times has been looked upon as a question involving dietary and nutritional considerations."[2]

If such bold assertions about the connection between nutrition and cancer come as a surprise to you, you're not alone. For decades, the status quo in cancer research and treatment has followed the exact opposite line of thinking. From the earliest times that I had studied, prior to my discovery of Hoffman, cancer was not looked upon as a question involving dietary and nutritional considerations. Rather, since the beginning of my career, and certainly now, cancer has been seen as a question involving *genetic* considerations, with discourse centering on mutation-causing environmental toxins (mutagens)—which fits the presumption that cancer is caused by specific agents acting locally. (I will have much to say about local theories of cancer in the coming pages.) Likewise, local treatment protocols have been completely dominant. The notion that dietary and nutritional considerations are relevant

to the development or treatment of cancer is so far removed from existing thinking that many professionals, then and now, reject it out of hand. And yet here was a seemingly authoritative figure—a central figure in the formation of the ACS!—claiming the opposite. Clearly something had changed.

Again, I wondered, why have we not heard this information? The fact that Hoffman, by many accounts, was so involved in the formation of the ACS suggests to me that his story is far more than that of a forgotten statistician. It's easy to understand how a brilliant but less visible professional figure might be forgotten over time. Surely this is a common occurrence. But in Hoffman's case, he *was* visible—visible enough even to give what many regarded as the organization's founding speech. He was indeed a forgotten statistician, but also an abandoned leader. His call for increased research on the role of nutrition in cancer, his warnings, and his evidence were all neglected, as if by mandate.

Perhaps you think Hoffman's obscurity can be explained another way. Maybe his ideas were already outdated by the 1980s, when those of us on the NAS committee surveyed the literature on cancer and nutrition? Perhaps they are even more outdated now? Maybe his findings simply could not stand the test of time, and were later proved wrong? These are good hypothetical questions, but again they do not match the historical evidence. Take a closer look and you will see that many of his observations have aged well. Some even seem downright prophetic.

Hoffman's massive 1915 study of cancer mortality rates[7] is surely a classic. It cites 579 sources and includes a meticulous presentation of statistical methodology and conclusions in its first 221 pages. There he provides critical commentary on the importance of using age-standardized data, a method of adjusting data to account for differences in the distribution of ages of populations now widely accepted as necessary when conducting epidemiological research. Although impressive in its own right, this study also formed the basis for the first United States Cancer Census.[9] In other words, far from fading into irrelevance, Hoffman's work here set the foundation for future progress in the field.

In 1923, he organized the San Francisco Cancer Survey, which went on to publish nine reports over the next eleven years.[6] It was in this survey that he first analyzed the effects of tobacco and eventually concluded that "the

increase in cancer of the lungs observed in this and many other countries is, in all probability, to a certain extent directly traceable to the more common practice of cigarette smoking and the inhalation of cigarette smoke. The latter practice unquestionably increases the danger of cancer development."[10] He also warned against the growing trend of cigarette smoking by women. Of course, these observations are obvious to us now, but Hoffman's findings came *twenty years before* the classic studies on smoking and lung cancer published by Wynder and Graham[11] and Doll and Hill,[12] *thirty-three years before* the US Surgeon General's report on smoking,[13] and *more than fifty years before* the debates on smoking and lung cancer still taking place in the mid-'80s, when I was first discovering Hoffman's work. When I asked Sir Richard Doll, the famous Oxford epidemiologist who was rightfully nominated for the Nobel Prize several times for his discovery of the link between tobacco smoking and lung cancer in the 1950s, if he knew of Hoffman's work in the '30s, he could not initially recall. After some reminding, he did remember Hoffman, but only as "that insurance man"—another example of how scientists (myself included, at times!) often don't do a very good job of recording and recalling the findings of those who preceded us, and sometimes fail to appreciate other points of view when they do not originate from established scientific institutions. Nonetheless, Hoffman's work has aged exceptionally well. That is not to say that his findings were not controversial, but that perhaps he was ahead of his time.

And what did he have to say about cancer and diet? His position was unequivocal: "excessive nutrition" is either cancer's "chief cause" or "at least a contributory factor of the first importance." By excessive nutrition, he meant the overconsumption of rich foods found in industrialized nations, particularly meat.

When I read Hoffman's book, I had already been an experimental researcher on these topics for over twenty years, and so I was fascinated to discover in his work many of the same things I had observed in my own research, anathema though they were to the medical establishment. But my first reaction to these parallels was not glee or gratification. No, as a scientist, I was ashamed—that this information was published as recently as 1937, and reviewed such a long and revealing history of research efforts, yet I had never heard it. I was confused and concerned, but mainly ashamed of what seemed

to me a vast and collective amnesia. Few individuals, if any, contributed more than Hoffman to our knowledge on cancer causation during the years 1913 to 1937. And yet you wouldn't know it. Today, I cannot find even a single reference to his paper on smoking[10] or to his monumental 1937 book on diet and cancer.[2]

The cancer research barons of the period were apparently willing to let him collect data for a cancer census, but not interpret the data he'd collected. H. T. Deelman, professor of pathology at the University of Gröningen, the Netherlands, acknowledged Hoffman's 1915 cancer atlas as "good and very serviceable" at the 1926 American Cancer Society conference at Lake Mohonk, but then attacked Hoffman's right to interpret the data. According to Deelman, Hoffman had overstepped his role as a statistician when he "arrogated to himself the part of cancer investigator."[14] At the same ACS conference, he reiterated the skepticism of British tumor transplant* researcher Ernest Bashford,[15] who suggested that statistics on varying cancer rates around the world, like those cited by Hoffman, were not to be trusted. (Bashford claimed that statistics on cancer incidence from Ireland were less precise than those from England, and that statistics from poorer countries were even less precise, though I never found any compelling evidence to support this speculative dismissal.) In short, Deelman denied any connection whatsoever between cancer and diet. He dismissed the work of Hoffman and others out of hand, describing their conclusions as "specious statements." And to them, he exclaimed: "Bring proof of what you are writing!" An ironic suggestion, I think, given that (1) he was unwilling to consider the statistics already provided and (2) his targets were consistently excluded from such conferences.

What threat did Hoffman and other researchers of nutrition and cancer pose? I can think of many possibilities, based on similar backlash I have received throughout my career. Did their views threaten the market for surgical services, as mine have sometimes done?[16,17] Did their views, tending toward vegetarianism (though not always explicitly supporting the label[2]), upset social norms and make them seem timid and effeminate? Is it possible

* In these studies, tumor tissue was transplanted from one animal to another to see if it would grow.

that diet, nutrition, and cancer reports were ignored and vilified because surgeons and other medical men simply could not comprehend a complex nutritional issue for which they had no training?

On Hoffman specifically, did his conclusion that "the principal dietary errors of the present day consist of a too heavy intake of protein and . . . sugar," in the Eighth Annual Report of his San Francisco Survey,[6] anger the relevant food industries? Might his views on other topics have contributed? He spoke and published on a wide range of controversial topics, including birth control,[18] public health policy,[19,20] national health insurance,[21] race,[4,5] and workplace legislation.[21–25] Did he personally irritate his peers by discussing some of these issues?* Did he threaten the establishment's preferred method for communicating with the public and therefore undermine the role of institutions such as the ACS[26,27] and the British Empire Cancer Campaign (BECC)?[28–30] George Soper, ACS managing director, was very clear[31,32] in how he viewed the role of cancer institutions: he believed they should be developed, managed, and informed only by physicians, especially surgeons, who should serve as the primary (if not only) source of public information on cancer.† Was Hoffman's work dismissed because he wasn't beholden to any medical institution? Although this gave him greater freedom to explore hypotheses wherever they might lead, did his outsider status also limit those institutions' respect for him?

Do similar questions apply to cancer research and health care as a whole in the twenty-first century?

I don't mean to suggest that Hoffman was without fault; it would be a dangerous mistake to make an idol of him. Nevertheless, he does provide an excellent foil for both the cancer researchers of his day and more recent ones. Unlike many of his colleagues, he didn't arrive to the field of cancer

* Some of his earliest work concerning mortality trends among African Americans was critiqued, but there's no contemporary indication that this work contributed to his ostracism, or that it had anything to do with his work on cancer.

† The reasoning behind this is clear. In the words of Howard Lilienthal: "The physician belongs to that enlightened class which, in principal at least, believes in the efficacy of the early and radical extirpation of malignant growths"[33] (as opposed to alternatives, such as nutrition). In Britain, another cancer specialist even claimed that propaganda would play some role in the control of cancer.[34]

research with preconceived ideas about nutrition. On numerous occasions, he was very careful not to overextend his views. Rather than claiming proof, he nearly always encouraged further study. In the large case-control study he began in 1924[35] and reported in 1937,[2] he concluded that he had found no evidence to support an effect of eating meat on cancer risk. This isn't proof of him defending meat intake, but simply of him being a competent scientist. About 99 percent of the cases and controls in that study ate meat, thus limiting any conclusions he could have made in either direction. In some cases, he may have been exceedingly conservative. In 1925,[36] he recommended that contemporary treatment procedures of surgery, radiotherapy, and early diagnosis were the best available methods of cancer control. This conclusion was based on the existing data supporting those procedures (data that were deeply flawed in many ways, as I will discuss in chapter three). Yet, unlike most of his colleagues, he wasn't afraid to reevaluate his own views and even the usefulness of statistics in certain cases. In 1927, he began to waver in his support of the use of cancer survival data to evaluate treatment. When studying statistics from Mexico,[37] he was "inclined to think that errors are more common in which non-malignant tumors are diagnosed as malignant than otherwise."

If not for their forgetfulness, what would the cancer research establishment of today make of Hoffman? What would they make of his willingness to adopt new perspectives, never leap to conclusions, and generally keep an open mind? Is there something about that flexibility that is fundamentally incompatible with the mentality that has come to dominate the field? Here is another reason why we should not make an idol of Hoffman: these traits—flexibility, open-mindedness, and vigilance—are merely proof of a competent scientist. They require no genius or sainthood. They should be the standards to which we hold *all* of our researchers. A world in which flexibility and open-mindedness are exceptions rather than rules is not a fertile breeding ground for truth.

For that matter, what would the cancer research establishment of today make of Hoffman's peers and predecessors?

THE COMPANY YOU KEEP, OR DISCARD

As I dug deeper into this history, I tried as much as possible to read the work Hoffman referenced. As a result, I came to discover a wide and fascinating cast of other historical characters wrestling with questions similar to my own about nutrition and cancer. That these questions were apparently off limits by the time I "came of age," forbidden enough that simply raising them threatened to undermine my reputation among my peers, suggests that the discourse surrounding cancer and nutrition research had become more limited since the years of Hoffman and his peers. The discourse then was not utopic either. There were certainly issues of professional reputation, real or imagined, affecting what Hoffman was or was not allowed to say throughout the era I surveyed, and there is no denying that he ran up against many boundaries. But well before Hoffman, especially during the nineteenth century, there was at least a more open, rich, and vibrant interchange of controversial information.

Throughout the two hundred years of literature that Hoffman reviewed, many foods were accused of contributing to the prevalence of cancer. However, the prevailing recommendation was to avoid "overnutrition" (synonymous with the "excessive nutrition" that Hoffman warned against). Overnutrition was not characterized by caloric excess alone, but also by the type of food being consumed in excess. On individual food groups, the most common recommendations were those against meat consumption and those encouraging more vegetable and fruit consumption. According to Hoffman, protein was the first and most frequent individual nutrient associated with overnutrition. On this last point, Hoffman refers to William Lambe and the early years of the nineteenth century.

William Lambe was a Fellow of the Royal College of Physicians in London. Both in 1809[38] and 1815,[39] he warned "against the danger of excess in food consumption, particularly meat and other protein products."*[f] Twice he proposed to study the effect of the "vegetable diet" on breast cancer patients

* This verbatim comment cited both by W. Roger Williams (see page 44) in 1908 and Hoffman may have been a paraphrase of Lambe's very strong views against meat consumption, as I could not locate it in either of Lambe's books. It is, however, an accurate representation of his views published elsewhere.

at the famed Middlesex Hospital in London, and twice his colleagues turned him down.[40] Accounts suggest that they considered Lambe a crank and that his advocacy of meatless diets (the word "vegetarian" did not enter the nomenclature until the middle of the nineteenth century) drew great scorn from many, including the cancer surgeons at Middlesex[40] who denied his study proposals. Lambe was thus an important figure, and a forerunner of later researchers into the association between cancer and nutrition, but far too ostracized to ever achieve his full potential.

But that doesn't mean his recommendations were without support or application. Indeed, one of his highly respected contemporaries, John Abernethy, recommended that "the powers of the [dietary] regimen recommended by Dr. Lambe should be fairly tried." In his own words, Dr. Lambe had suffered "ill health and ailments" until age eighteen. At that point, he "finally" undertook (in February 1806) "what he had been contemplating for some time—to abandon animal food altogether and everything analagous to it, and to continue to confine himself wholly to vegetable food." He wrote that he "never found the smallest real ill consequence from this change . . . and sank neither in strength, flesh, nor in spirits."[41] According to another friend and colleague, Lambe at seventy-two years of age was

> *very gentlemanly in manners and venerable in appearance . . . He told me . . . that his health was better now than at forty . . . [and] he considers himself as likely to live thirty years longer as to have lived to his present age . . . Although he is seventy-two years of age he walks into town, a distance of three miles from his residence, every morning and back at night.*[41]

Personal life aside, Lambe later "began to use his diet as a cure for patients ill with cancer," a practice that Abernethy once again supported. Abernethy reasoned "that the body can be perfectly nourished by vegetables," that "all great changes of the constitution are more likely to be effected by alterations of diet and modes of life than by medicine," and that Lambe's diet offered "a source of hope and consolation to the patient in a disease in which medicine is known to be unavailing and in which surgery affords no more than a temporary relief." Still, despite this support from

the renowned Abernethy, Lambe's colleagues twice denied his research proposals.*

In Hoffman's opinion,[2] it was not Lambe but John Hughes Bennett in 1849 who provided "the first definite indication of the recognition of cancer as a nutritional disease." Bennett was a senior professor of clinical medicine at the University of Edinburgh, where he studied the relationship between cancer and body fat. On that relationship, he said, "an excessive cell development (as in cancer) must materially be modified by diminishing the amount of fatty elements, which originally furnish elementary granules and nuclei; the circumstances which diminish obesity, and a tendency to the formation of fat, would seem *a priori* to be opposed to the cancerous tendency."[42] In much simpler words, behavior that reduces the formation of fat (including diet) *should* lower the risk of cancerous growth. In 1865,[43] he remained convinced that tumor growth was associated with an "excess of nutrition" and added a more specific recommendation: "in carcinoma . . . the body . . . is for the most part fatty, and a diminution of this element in the food should be aimed at." On these points, modern-day evidence backs him up; there is considerable evidence linking obesity and cancer. Of course, not all of his assertions were bulletproof. His suggestion that reduced fat consumption necessarily controls levels of body fat is an oversimplification, given modern evidence.

In the postscript to his book in 1849,[42] Bennett recommended an 1845 book by George MacIlwain,[44] yet another researcher and physician who

* Now, after more than 200 years, Lambe's proposed trial on breast cancer patients is finally underway. My son Tom and his wife, Erin, both physicians, are at last conducting a professionally approved research study on stage IV breast cancer patients. This new study was approved after careful Institutional Review Board scrutiny by his institution, the University of Rochester Medical Center. Receiving that approval was neither quick nor easy. There remains profound doubt in the professional cancer community that nutrition, provided by a "vegetable diet," as Lambe called it, could have anything to do with cancer, especially as a possible treatment. The conditions required by the review board for this new proposal—limiting the testing of the whole food, plant-based (WFPB) diet only as an adjunct to traditional pharmacologic treatment instead of testing it alone—illustrate the medical establishment's cautious paternalism. Even for individuals who would elect to use the WFPB diet alone, medical authorities insist on the concurrent use of "proven" chemotherapy drugs. Never mind that the effectiveness of these drugs ranges from highly questionable to unproven.

linked cancer with dietary excesses and warned strongly against "grease, fat, and alcohol" because of their toxic effects on the liver. MacIlwain further observed that "of the cause of [cancer], I am at least certain of this, that either the food contains something unusual, or that some of the assimilating organs are acting on it in some unusual manner, or both. This seems indisputable." What makes MacIlwain unique from my perspective is that he considered the broader effects of the whole diet on cancer, rather than only specific nutrients. There's little doubt that he would have shared many of Bennett's concerns about dietary fat, but his focus was not nearly as singular.

As the decades passed, this lineage of medical authorities speaking out about diet's role in cancer showed no sign of fading. John Shaw, of the Royal College of Surgeons, England, recommended in 1907[45] increased consumption of vegetable foods and decreased use of animal foods, alcohol, tea, tobacco, and drugs for controlling cancer. And just one year later, W. Roger Williams, member of the Royal College of Surgeons in London, published an extensive book on the history of cancer arguing that nutrition should have a central role in cancer research. According to Hoffman, this book should have been a classic in the field: "[marking] an epoch in cancer literature, reviewing the whole subject with absolute impartiality and resulting in a cancer classic of the first importance."

According to Williams's text, "probably no single factor is more potent in determining the outbreak of cancer in the predisposed, than excessive feeding." This concern for excess should by now be a familiar one. To elaborate this point, Williams targets the "gluttonous consumption of proteids—especially meat—which is such a characteristic of this age," insufficient vegetable intake, and sedentary lifestyles as other contributing factors. (I wonder, what would Williams make of our meat consumption now, more than a century later, and of our amplified gluttony?)

One last point of interest in Williams's book is his emphasis on the environmental origin of cancer and the effects of migration on cancer risk. The uneven distribution of disease throughout the US and world was also a subject that fascinated Hoffman. Combined with research on migration (mentioned in chapter one), uneven distribution of disease suggests that cancer is related to lifestyle factors (also said to be "environmental"). Similar views were expressed much earlier, in 1846, by another eminent physician

and researcher, Walter Hayle Walshe,[46] who presented cancer mortality data to show that it was primarily a disease of "civilization."

RESURRECTING THE VOICELESS

I could surely devote an entire book to these individuals and their greatest works, but I think I have made my point: there is a very long tradition of investigating and believing in a role for nutrition in the formation of cancer (and indeed, disease more generally). If progress in this field has been slow, as the allegation often goes, then it is for neither lack of effort nor lack of interest, at least on the part of certain groups of scientists. There have, however, been many impediments, as seen with Lambe, whose proposals to study the effect of diet on cancer were denied by his surgeon colleagues; with the forgotten Hoffman; and well beyond. Those authorities who bemoan a lack of convincing evidence on these issues are often the same ones who both ignore and impede, especially proactively, the flow of the ample evidence that does exist. I am not suggesting this as a full-blown conspiracy, but as a historical fact.

There are many other figures from this period whose research I could cite. My own, albeit incomplete, review of the early literature suggests that Hoffman's *Cancer and Diet* captured only a fraction of the discourse surrounding nutrition and cancer (perhaps 20–30 percent). Nevertheless, many of these findings are uniquely perceptive in light of modern evidence. Here are a few gems:

- John Howard in 1811,[47] fellow of the Royal College of Surgeons and author of practical observations on cancer, and many other writers in the next 175 years (including extensive comments by W. B. Thomson in 1932[48]), argued that constipation was an important predictor of cancer. Howard came to this view after forty years of practice with cancer patients. The consensus then, as it is now, was that plant foods prevent constipation. This association of colon cancer and other Western diseases with constipation has for many years been attributed to insufficient consumption of dietary

fiber, a nutritional component only found in plants, as reviewed by Dennis Burkitt in 1975.[49]

- John Hughes Bennett in 1849[42] recommended that nutrition standards should reflect both upper and lower limits, saying, "In the one case, we should do all we can to bring the nutrition up to and above the average (to reduce the risk of tuberculosis); in the other, down to and below it (to reduce the risk of cancer)."
- J. Braithwaite in 1901[50] suggested that three of the principal causes of cancer were salt, high nourishment (especially meat), and "old cells with effete [ineffectual] nourishment."
- Francis Hare in 1905[51] described an "old standing idea in the profession that the increase of malignant disease is in some way associated with the increased cheapness and improved quality of the world's food supply." *An old standing idea* . . . in 1905?
- In 1908,[52] the aforementioned Roger Williams demonstrated a parallel relationship between "good nutrition" of the day (that is, an affluent diet including more meat) and cancer, heart disease, diabetes, arthritis, and gallstones.
- Thomson in 1932[48] claimed that "food is undoubtedly of great importance in the study of cancer." *Undoubtedly of great importance* . . . in 1932? He also worried that "many surgeons, radiologists, and chemotherapists scoff at the idea of food exerting any influence in the cause, arrest, or cure of the disease, and they carry their conviction so far as to put their patients on ordinary fare as soon as possible after an operation and also during treatment by radiation." On this point, almost nothing has changed. Cancer professionals continue to ignore a nutritional effect, relying heavily on surgery, radiology, and chemotherapy, and we often hear about hospital patients being given "ordinary fare" following operations.

It bears emphasizing that just as our 1982 NAS report was but one part of a much larger procession, so too were these nineteenth- and twentieth-century cancer professionals. The literature on diet, nutrition, and cancer goes back far earlier than most contemporary readers would think, at least to ancient Greece[53] and China;[54] it certainly was a surprise to me. Hare

was absolutely correct when he suggested that the nutrition–cancer link was an old standing idea. We have only forgotten this old wisdom.

In the 1980s when I was discovering this work, and certainly still today, the predominant belief was that cancer is a genetic disease for which nutrition can do little. Similar attitudes also pervade our study and treatment of other lifestyle-related diseases, as I discussed in the previous chapter. But all the way back in 1676, Richard Wiseman[55] concluded that cancer "might arise from an errour [*sic*] in Diet, a great acrimony* in the meats and drinks meeting with a fault in the first Concoction,† which, not being afterwards corrected in the Guts, suffers this acrimonious matter to ascend into the bloud [*sic*]." His preferred cure? To perform an exact "regulation in diet and way of living, advising to abstain from such salt, sharp and gross meats, as may dispose the bloud [*sic*] to acrimony." That's right—doctors have been calling for dietary interventions in the prevention and even *treatment* of cancer for more than 350 years! But who remembers their voices?

To return to the central theme of Part I, and add a new wrinkle, the WFPB diet is controversial because it challenges conventional attitudes and prevailing narratives about the causes and treatments of disease. Yet clearly these attitudes have not always been conventional and those narratives not always prevailing. Although the many historical figures cited in this chapter did not advocate for the WFPB diet exactly as I do, the general message is consistent: the food we eat *does* matter, including when it comes to cancer, and certain foods (especially foods containing animal protein) are especially harmful in that respect. The process by which this concept came to be forbidden deserves greater attention and raises many questions:

- How is history recorded and preserved in the sciences?
- What efforts have been made to study that history?
- How is discourse shaped in the sciences, and has this process changed over time?
- How are research questions and approved methods of study subsequently shaped by discourse?

* Biting sharpness to the taste or other bodily sense, pungency; irritancy; acridity.
† Digestion (of food) in the stomach and intestines.

- How do research results reach the public?

The more I gave these and other questions my attention, the more I found that our practiced forgetfulness could be traced to the founding of cancer institutions, which have a tremendous power to shape all of the above—history, education, discourse, research questions, acceptable methods, communication with the public, and more.

CHAPTER THREE

DISEASE CARE INSTITUTIONALIZED

Philosophy is not a theory but an activity.
—Ludwig Wittgenstein

That so many past commentators have acknowledged nutrition as an important factor in causing cancer is an important and revealing insight, but it only takes us so far. Clearly this acknowledgment of the nutrition–cancer link no longer jibes with the contemporary research and treatment of cancer, or indeed most other diseases commonly found in economically developed countries, and so the question remains: What changed?

To explain why advocates of nutrition have been ignored, we must take a more nuanced look at the early debates surrounding nutrition and other competing practices. What we discover in those debates is one question, above all others, that influenced the acceptance or rejection of nutrition's role: *Is cancer a local or a constitutional disease?* From the earliest times, cancer

professionals wrestled with this question, for it determined every aspect of their approach, from prevention to treatment, and from what experimental research was done to education and public policy making.

To define these terms, a *local* disease is one that attacks a particular part of the body and has a specific cause, and therefore can be dealt with precisely. *Particularity*, *specificity*, and *precision* are the key words here. Early proponents of the local theory of cancer believed that cancer was caused by isolated and identifiable agents such as wounds, bacteria, parasites, and viruses. (Today, cancer researchers who focus on single gene mutations or single environmental toxins are reflecting these same principles.) The corollary of that belief was that cancer could be locally (and simply) treated. In the earliest days of this debate, "local" treatment meant surgery. It's easy to understand how this theory became popular. Surgeons occupy positions of prestige and power, and the simplicity of the theory appeals to our rational minds. It translates well to both diagnosing a disease ("this cancer is of the breast, and it is caused by that specific agent") and prescribing a treatment ("remove the breast, remove the cancer").

On the other hand, the *constitutional* theory of cancer proposes that the disease has deeper origins, likely involving the complex pathways of metabolism that characterize nutrition's function.[1,2] Compared to the local theory's reliance on specific cancerous agents, the constitutional theory suggests more elusive causes. Far from the wounds, bacteria, parasites, or viruses credited by local-theory advocates, early proponents of the constitutional theory even suggested the possibility of *multiple* factors.[3–5] Suggestions that cancer might be multifactorial appeared in numerous publications:

- In 1888, the aforementioned W. Roger Williams (chapter two) cited British surgeon Campbell De Morgan, who observed that no matter how many clay pipe smokers get cancer of the lip or chimney sweeps get scrotal cancer, the "majority will not become cancerous, irritate how you will."[6] Ironically, De Morgan favored the local theory, but his statement here and Williams's interpretation certainly suggest the possibility of less visible factors, or perhaps even a combination of factors, in the cases of lip and scrotal cancer.

- In 1924, physician J. E. Barker hypothesized that cancer was caused by vitamin deficiency,[7] to which another doctor, Andrea Rabagliati,[3] responded that total diet played the greater part. (By virtue of the fact that nutrition a short while later was found to comprise multiple complex vitamins and other factors working in synchrony, any suggestion that nutrition plays a role in cancer is an argument for multifactorial causes, and supports the constitutional theory of disease.)
- In both 1907[8] and 1912,[4] R. Russell emphasized the multiple causes of cancer. Although he listed the consumption of animal flesh as one of those main causes, he also had the insight to warn that "animal flesh by itself without other stimulants does not appear of necessity to cause much cancer."[8]

On this final point, both my own experimental findings and the evidence of others tend to agree with Russell. The correlation between animal protein consumption and cancer risk is very strong, but the equation is not so elementary as animal protein = disease. (Sometimes people argue against this straw man, but it is not an honest representation of my own interpretation.) Instead, there are both direct and indirect effects on cancer from consuming animal protein. One indirect effect is that the more animal-based foods one eats, the less one consumes cancer-preventive plant-based foods packed with antioxidants, fiber, and other protective nutrients.[9] Recall the comment on nutrient composition in the Introduction, replicated here, which illustrates this point well. In particular, note the virtual exclusion of crucially important antioxidants, complex carbohydrates, and vitamins from animal foods (excepting small amounts of antioxidants and vitamins sometimes found in tissues of animals having recently consumed plant foods):

NUTRIENT COMPOSITIONS*

COMPONENT	PLANT	ANIMAL
Antioxidants	Only Made By Plants	Almost None
Complex Carbs	Only Made By Plants	None
Vitamins	Made By Plants	Almost None
Fat	~9–11%	~15–20%
Protein	~9–11%	~15–20%

**PROCESSED FOODS are varied, likely worse*

Unlike the surgical approach that advocates of the local theory champion, nutrition is a vastly intricate and interconnected process. Those who vouch for its role in causing or preventing cancer are *by necessity* considering multiple factors. This is evident in the greater nuance (and even uncertainty) expressed in early commentary. According to Frederick Hoffman,[10] physician Lucius Duncan Bulkley recognized nutrition's role in cancer causation in 1921,[11] but not without cautioning that "to understand and rightly treat the systemic condition belonging to cancer, which is indeed its basic factor, one needs to take a very broad view of the complex processes which pertain to metabolism and nutrition." Hoffman himself, in 1923,[12] questioned "if a single 'cause' will be found responsible for cancerous affections, for it would seem much more likely that a multitude of conditioning circumstances are responsible." He restated this view in 1924,[13] 1933,[14] and 1937.[10] Hoffman also references Bernhard Fischer-Wasels, director of the Pathological Institute of the University of Frankfurt, who emphasized the complexity of nutrition in 1935. The aforementioned Williams also recognized the importance of multiple factors.[15] In 1908, he argued against the notion of simple solutions (e.g., the chemotherapy marketed at the time) when given such complex biological problems. And of course, the "excessive" nutrition about which he wrote so much included countless nutrients interacting in a highly complex causal pathway.

What we see in the history, then, are two mostly incompatible theories. However, these theories are not incompatible on *all* levels. For example, it would be possible to view cancer as a constitutional disease but still deal

with the crisis of a tumor by performing surgery, especially when there is sufficient evidence that the cancer being removed is self-contained, as in so-called benign or nonmetastatic tumors. The difference is in how one views the cause of that disease, and how one proceeds following that surgery. Those who vouched for the local theory focused on avoiding specific cancer-causing forces in the environment, whether a toxic chemical (poison), a virus, or a wound, and counted a removed tumor as a victory against that cancer. They did *not* consider nutrition to be an important part of cancer prevention or treatment. Conversely, those who vouched for the constitutional theory would also want to avoid toxins and viruses in the environment, for a host of other reasons not related to cancer alone, but would also follow up surgery with strategies to address what they viewed as the underlying causes of cancer, like nutrition.

Where these two theories become more incompatible is in a complete adoption of the constitutional theory, for that largely decreases the need for any local treatment protocols. Likewise, the complete adoption of the local theory leaves little room for constitutional treatment protocols, even as a supplemental measure. Just as the complete adoption of the constitutional theory questions the need for local treatment protocols, the complete adoption of the local theory decreases our focus on constitutional origins and treatments of disease. By confining cancer professionals to only one of these perspectives, we effectively limit the breadth and variety of treatment options available, to the great detriment of the public.

THE LOCAL THEORY PREVAILS

These two theories of cancer causation battled for well over a century, and certainly longer if you consider the competing, underlying beliefs and assumptions of each position. As early as 1784, Benjamin Bell of Edinburgh argued that breast cancer is a local disease, best cured by surgery.[16] In 1816, physician John Abernethy disagreed, as did William Lambe[17,18] and John Howard[19] during the same decade. Abernethy suggested that "the best timed and best conducted operation brings with it nothing but disgrace if the diseased propensities of the constitution are active and powerful."[2]

A couple decades later, at an 1844 French Academy of Medicine surgeons' conference in Paris, J. H. Bennett[20] reported that French physician Jean Cruveilhier was in a minority when he restated this view: "Cancer always depended upon a constitutional disorder, that local disease was the effect and not the cause, and to remove the first, while the latter was allowed to remain, was an irrational practice." But as you might expect, surgeons have long favored the local theory of disease, and Cruveilhier's colleagues were no different in that respect. They argued that "the best practical rule to be followed was always to excise [tumors] as early as possible." In other words, they argued for the philosophy more attuned to their own practice.

The debate continued back and forth much in the same way throughout the 1800s. The Pathological Society of London[21] brought greater awareness to the issue when it sponsored a debate in 1874 on cancer as being local versus constitutional, highlighting just how important this topic was for the future of medicine. Nothing conclusive came of this London debate—for better or worse, it ended in stalemate—in large part because the appealingly simple local theory was still favored by many, especially surgeons. In 1879, R. Mitchell[22] reflected on the local theory when he said that "every specific disease depends on one single and indivisible cause for its origin and existence, and not on a combination of causes." The biggest offenders, or at least the most commonly accused, included things like betel nuts, chimney soot, and hot clay pipes. Each was assumed to cause a different type of cancer—oral, scrotal, lip—and each was accepted by surgeons with little resistance.* The aforementioned Bulkley[11,23] ably characterized his contemporaries' narrow-minded focus on single causes of cancer when he said, "The search has been persistently made for some extraneous cause, such as parasitism, (and 'local injury and irritation') but in vain."

Given the popularity of surgery in the contemporary medical establishment, it's unlikely that the local theory could have been overcome. Surgery was unlikely to suddenly disappear or yield to theories of nutritional control.

* Incidentally, evidence today shows that single carcinogenic agents at typical levels of exposure are rarely, if ever, enough to increase cancer risk; although early researchers such as Mitchell couldn't have known back then, there's no excuse for the many professionals who remain ignorant today about the questionable evidence supporting a causal role for single cancer-causing chemicals.

Nevertheless, it's possible that the constitutional theory of disease causation might have eventually triumphed, given a little more attention and resources, thus changing the course of history. But it was not to be. Instead of a gradual shift toward the constitutional theory, at the end of the century the debate swung strongly and decisively toward the local theory. This was not because the constitutional theory was proven incorrect, or because surgery had particular excellence, but as a result of two emerging technologies: radiation and chemotherapy. Proponents of the constitutional theory were no longer outnumbered by surgeons alone, but also by a new class of radiotherapists and chemotherapists. By targeting disease at a very precise, local level, these technologies lent themselves to the very same style of treatment as surgery, which left those grappling with the complex metabolism of cancer causation as an even tinier minority.

Following the rise of chemotherapy and radiation, the constitutional theory was progressively delegitimized. The local camp had won, and the human impact of its dominance over the field ever since cannot be overstated. Easy as it may be to get caught up in the more abstract and theoretical levels of this debate, or to philosophize about the appeal of simplicity versus complexity, we must never forget the human element beneath it all. Most importantly, there are the uncountable lives lost as a result of ineffective protocols for cancer prevention and treatment. Anyone who has ever dealt with cancer, as a patient, doctor, or loved one, has been affected by the "progress" made during the early twentieth century. But there are also the many professionals, then as now, who were not only ignored but also punished for their views. Bulkley was expelled from his professional society, the American Association for Cancer Research,[24] at the age of eighty-three for criticizing surgery. And I've already discussed Hoffman's erasure from history, despite his pioneering work and role in the establishment of the ACS.

I also believe that arrogance has had a profound impact on this debate, speeding the dominance of surgery, radiation, and chemotherapy. The belief that vastly complicated diseases can be cured by very simple solutions is at best naïve, but more often simply arrogant. This attitude has continued until relatively recent times, as discussed by celebrated cancer researcher Joan Austoker[25] in reference to breast cancer surgery. Michael Shimkin, perhaps the most influential spokesman in the field of cancer for half a century,

perpetuated this belief again in 1957.[26] Also arrogant is the cursory dismissal of alternative views. Did early scientists who were concerned with the complexities of nutritional causation really have nothing to offer? Supporters of the local theory often argued that the constitutional theory was not sufficiently focused and was therefore not even scientific. Such an attitude, in suggesting that there is only one worthy approach to science, is both closed-minded and arrogant. The remarks of W. S. Bainbridge, professor of surgery at the New York Polyclinic Medical School and Hospital, in his 1914 book *The Cancer Problem*,[27] illustrate this arrogance better than most: "that surgical technic [*sic*] has (now) developed to *such a degree of perfection* as to be able [for] one to say with assurance that it is possible to effect a cure of the disease by means of surgical intervention" (emphasis added). (We'll see shortly just how "perfect" that intervention was.) Bainbridge goes on to denigrate skeptics as "the ignorant and . . . timid who fear the knife."

Okay, I admit, this one surpasses arrogance. It is a blatant fallacy—an ad hominem attack focused on the critics of the local theory, rather than their critiques—and nothing short of an assault on logic.

DOMINANT TREATMENT PROTOCOLS

Despite the triumph of the local theory and the celebration surrounding its preferred treatment preferences, the evidence in the early 1900s supporting surgery, radiation, and chemotherapy was not impressive.

Radiotherapy, a treatment method that doses affected areas (e.g., tumors) with high levels of focused radiation in an effort to kill cancer cells, was introduced near the start of the 1900s.[28] It drew considerable interest during the next quarter century, but this interest was not supported by strong evidence. In the largest study of its kind, surgeon Charles L. Gibson[29] reviewed 573 cases of varied cancers for the years 1913 to 1925 at the New York Hospital. He concluded: "Our personal impression is that *no real improvement has been attained* by radiotherapy" (emphasis added).

Despite that conclusion, unwarranted hope for radiotherapy continued to swell. As indicated by the minutes of the ACS national council meeting,[28] the group found it necessary to restrain public optimism in 1914[30] and 1921.[31] In

1925, the managing director of the ACS, George Soper, spoke candidly about the failure of radiation therapy in England.[32] That same year, a series of reports by Hoffman[33] and others[34,35] indicated that excessive radiation exposure was related to *increased* cancer risk and other serious injuries. By 1928,[36] however, the ACS was no longer trying to restrain public confidence in radiation; in fact, they issued a memorandum to ease public fears, so that the disciples of radiotherapy could get on with developing a better product.

By the 1930s, the best that could be said for radiotherapy was the following:

- selective radiotherapy action (i.e., targeted radiation) on cancer cells grown in culture in the laboratory produced inhibited cell growth;
- radiation was simultaneously carcinogenic (promoted cancer by causing mutations) and carcinostatic (restricted cancer by destroying cells, but only if the radiation beam could be focused narrowly enough); and
- useful information on radiotherapy's effectiveness eventually might be found, but only if careful studies in radiobiology were organized.[28]

Contemporary evidence in favor of radiotherapy ranged from unimpressive to nonexistent. Studies that compared the survival rates of patients undergoing radiotherapy as opposed to surgery[29] were perhaps the most impressive. But the data from such studies need to be taken with a handful of salt, for they were fraught with major analytical flaws that I will discuss shortly.

Meanwhile, in the emerging field of chemotherapy—a treatment method that mostly uses highly toxic chemicals in an effort to kill cancer cells*—favorable evidence was virtually nonexistent.[37,38] In fact, chemotherapy was indiscernible from a long line of snake oils used by charlatans, quacks, and well-meaning but misguided physicians of the period. Tellingly, as the concept of chemotherapy became more popular at the turn of the century, it

* If that sounds familiar, good; the premise underlying both radiotherapy and chemotherapy is the same: cancer cannot be reversed, only killed.

became more necessary to establish a legal distinction between quack remedies and the remedies of established practitioners.[28] On its face, this sounds like a good thing. Only a fool would argue against efforts to crack down on quackery in medicine, right? So you would think. But we should at least scrutinize the threshold for quackery set by our "established practitioners," and see what kind of solutions they proposed.

What we find when we do so is not impressive. In 1926, the ACS organized a landmark conference at Lake Mohonk, New York, to assess the evidence in favor of chemotherapy. According to Francis Carter Wood,[37,38] then vice president of the ACS and professor of clinical pathology at Columbia University, the best available chemotherapy treatment exhibited there was the Blair Bell method of intravenously injecting colloidal lead (upward of 600 mg in one course of treatment). Many other chemicals were also considered, both during earlier and later years (e.g., selenium in 1912[39] and 1913,[28,40] metabolic inhibitors of respiration, and vital dyes—colorants that can stain living cells without destroying them).[24,28,41] And what did all these chemical agents have in common? *No convincing evidence of human efficacy.*

The line between "legitimate" chemotherapy practices based on "scientific principles" and the unscientific products peddled by quacks was apparently very thin. And who were the authorities determining the placement of that line? Who, for example, sanctioned *intravenously injecting lead* as a therapy?[37,38] All too clear is that the determination to find a specific cancer antidote was so strong that the ACS was willing to organize trials for dangerous chemicals.[37,38] During these years, it seems that legitimacy, along with any respectable standard of evidence, depended most on whose snake the oil was being extracted from. In short, no reasonable person could have claimed sufficient evidence for the young and seemingly improvised field of chemotherapy.*

Last, although contemporary evidence supporting surgery was widely celebrated, it was no less flawed than the evidence supporting radiation and chemotherapy.[23,29,37,43] Flaws in the evidence supporting surgery included:

* On a more comforting note, this era was not entirely wasted. Anxious to develop chemicals, and especially hormones[42] to treat cancer, there was an increased interest in theoretical cancer-biology research.

- Failure to statistically control for earlier diagnoses (an early diagnosis, which allows surgeons to go to work earlier, does not establish that surgery is a better cure, and it says nothing about long-term survival, but it does increase the odds of reaching the three-year or five-year benchmark of "survival").
- Giving equal weight to relatively nonfatal cancers and fatal cancers.
- Determining survival rates by comparing cohorts with more operable cases against cohorts with fewer operable cases.
- Categorizing recurrences as "new" cancers, so as to not report the previous surgery as a failure.[23,25,43]
- Reluctance on surgeons' part to count remissions in nonsurgery cases.[23,44]

Despite these serious flaws in the data, there were many who touted the success of surgery. One of the fiercest champions of surgery was Howard Lilienthal, professor of clinical surgery at Cornell.[45] At the 1926 Lake Mohonk ACS conference, he suggested that the most favorable reports on surgery were those of the aforementioned Gibson,[29] surgeon Alexis V. Moschcowitz,[43] and M. Greenwood.[37] But these sources, especially the first two, were distorted by Lilienthal.

When I compared Lilienthal's conference account[45] of Gibson's study with Gibson's own account,[29] I found only blatant misrepresentation. Gibson was a surgeon at the Cornell Division of the New York Hospital, and his report documented the follow-up histories of 573 cases of varied cancers between 1913 and 1925. Here's what he had to say about surgery and the evidence so many used to support it: "We have been living in *a fool's paradise of fallacious statistics* . . . and all the older figures should be ruthlessly junked and so-called radical operations should only be performed after the most painstaking search for metastases is exhausted" (emphasis added).[29] Lilienthal completely ignored this account and perverted Gibson's data to reach a much different conclusion: "The chances, with surgery, of remaining alive for a given period are double those without its aid in the same length of time." He went on to say that "many of the cases reported are fine examples of operative skill and surgical judgement with results brilliant in the extreme." Results *brilliant in the extreme*? What a far cry from Gibson's own account:

"No sadder report of the disheartening status of cancer surgery has come to our attention."

I have the luxury of hindsight, and of being able to highlight these reports' differences through side-by-side excerpts, as if in dialogue. Sadly, Gibson was unable to defend his report's findings in the same way, because he wasn't even invited to the 1926 ACS conference. Despite being the architect and author of the most comprehensive study of its kind, he was left at home. Shamefully, so were the devastating results of his study. And clearly Gibson's exclusion from the conference was not the result of an innocent oversight, considering that Lilienthal was obviously aware of his work and willing to "analyze" (misconstrue) it in his absence.

Likewise, the actual results of Moschcowitz et al.'s research[43] at Mt. Sinai Hospital in New York (also celebrated at the conference) were mysteriously omitted from Lilienthal's paper, and it's not hard to see Lilienthal's motivations. Lilienthal began by praising the (subsequently recognized as macabre) Halstead mastectomy and concluded by stating that "the modern operation is usually successful in eradicating the local process, as is evidenced by the very large number of cases dying from distant metastases, without even a suspicion of a recurrence." But his claim that metastases were not even suspected of being recurrences directly contradicts Moschcowitz, who made a point of saying that it wasn't possible to distinguish for sure between recurrences and metastases. Moschcowitz also cautioned that survival rates "are not as favorable as one might be led to believe from a cursory examination of the literature."

Meanwhile, arguments that opposed surgery during this time also included those* of Robert Bell,[46] J. Shaw,[47] and Bulkley;[23] Austoker's review notes additional dissent.[25] Taking those critiques into account, as well as the contemporary and more recent analytic reviews of the practice, the flawed data that supported surgery, and the intense emotionalism and bias surrounding the entire issue,[25,48,49] it's clear that surgery's early twentieth-century

* Dr. Bell rejected surgery after many years of practice, and when attempting to inform his colleagues of his disenchantment with it, encountered great hostility and professional ostracism. After considerable frustration, he wrote a book to tell his story, depositing copies in a few key libraries. Upon discovering his book, I had to separate uncut pages in two places; obviously I was the first reader of his book in the Bodleian Library at Oxford in eighty years!

dominance (alongside radiotherapy and chemotherapy) was not won or justified by merit alone.

EVIDENCE FROM THE SAME PERIOD SUPPORTING NUTRITION

Treatment of cancer is more urgent and personal than prevention of cancer. Thus, treatment tends to attract a focused approach like those offered by surgery, chemotherapy, and radiotherapy, where cancer is approached as a local disease to be treated locally. Nutrition did not offer this possibility during the late 1800s (and neither does it now!), partly because many types of nutrients, which might hypothetically focus their effects, had not yet been discovered. Thus, with the growing philosophical adoption of the local theory, combined with the urgency of treatment, nutrition's association with cancer was not considered a possibility for cancer treatment. Nutrition was a constitutional or lifestyle effect that, at best, might only help prevent cancer.

Nutrition's potential effect on cancer prevention, however, eventually led to several types of human studies: studies that compared the cancer mortality rates of populations with nutrient patterns and gradations of dietary practice; time-trend comparisons of mortality rates given the availability of certain foods; studies that observed a correlation among migration, food-consumption trends, and cancer risk (i.e., how cancer risk rose or fell as individuals or groups moved and adopted new diets); and at least one very large case-control study (in Hoffman's case). Early experimental animal studies (1913–1914)[50,51] also showed that lower calorie consumption significantly reduced growth of transplanted tumor tissue.

The most convincing evidence of cancer's association with lifestyle and environment was illustrated by the effects of migration on cancer risk. As mentioned in chapter two, this was a favorite form of evidence for Hoffman,[52] Williams,[15] Russell,[8] and many others. The most common hypothesis was that "excessive" nutrition was responsible for cancer. How else could they explain that cancer rates were highest among the most "robust" and seemingly healthy members of the population? In 1908, Williams[15] proposed that excessive nutrition sparked tumor growth at the cellular level,

until eventually that tumor exhibited growth independence or "proliferative power." On the effects of external versus internal factors in tumor development, he said, "It is probable that in the past, the value of extrinsic factors, as formative stimuli, have been underrated; it, nevertheless, seems probable, from the whole course of cancer growth, that, in tumor formation, as in normal growth, intrinsic factors usually predominate." Here, "intrinsic factors" refers to complex functions of metabolism; in other words, a constitutional origin of disease.

None of this evidence in support of nutrition was esoteric. It was well known throughout the period, especially among the most powerful leaders in cancer research and education societies. Hoffman could not have been clearer in his 1913 "Menace of Cancer" speech, which led to the founding of the ACS. He made ten recommendations for the new society, most of them encouraging improved statistical procedures and data for recording cancer prevalence among different populations. But he also made two very specific recommendations on determining the causes of cancer: that "incidences of occupational hazards with respect to cancer be exactly determined" and that "nutritional influences on the induction of cancer be analyzed." In the history of the society written by E. H. Rigney,[53] Hoffman specifically said, "Since an erroneous diet is a probable causative factor in cancer occurrence, the nutrition of cancerous patients should be investigated in conformity with . . . strictly scientific and conclusive methods." Although the new society embraced Hoffman's recommendation on the development of statistical surveys, they ignored his recommendations to study nutritional and environmental factors. And this early neglect set a pattern that has dominated the ACS ever since.

Nutritional theories were also well known in Britain. A major study on diet and cancer among religious orders,[37] conducted by the British Empire Cancer Campaign (BECC), acknowledged in 1926 that "certain English medical men, whose names deservedly carry great weight," took nutrition seriously and, furthermore, that "a bibliography on the subject of diet and cancer would extend to many hundred titles."

Unfortunately, to return to the central point of interest, everything changed toward the end of the nineteenth century when the local theory of cancer causation became dominant. Its dominance is clearly reflected in the

medical practice of that time and in the medical practice since. The dominance of surgery, chemotherapy, and radiotherapy, in the *absence* of convincing evidence, testifies to the power of dogma; the disregard for other protocols, in the *presence* of evidence, testifies to a longstanding tendency toward repressing controversial views. These tendencies became even more predominant in the early twentieth century with the rise of several cancer institutions.

THE RISE OF INSTITUTIONS

We have seen so far how the local theory of cancer causation triumphed over the constitutional theory around the turn of the twentieth century, and how that affected our approaches to both treatment and the irresponsible misrepresentation of data. Why this battle and so many of its participants have been deleted from the history of cancer research, and why the same questionable theory and practices regarding cancer causation and treatment persist today, can be explained by the formation of a few powerful cancer institutions in the early 1900s: the Imperial Cancer Research Fund (ICRF), the American Association for Cancer Research (AACR), the ACS introduced in chapter two, and the BECC. The power of these four institutions was, and remains, inescapable. Nearly all professional activities concerning cancer research have been developed, funded, and controlled by these institutions, in addition to one more: the US government's all-powerful, taxpayer-funded National Cancer Institute (NCI) of the National Institutes of Health (NIH), which was founded by leaders of the ACS and the AACR.

Obvious though it may be, it's important to remember that institutions begin as nothing more than groups of like-minded people, and that groups of like-minded people tend to become increasingly like-minded as time passes. This is a matter of human nature: as with any group of humans seeking harmony and stability, professional institutions tend to encourage conformity far more than outstanding individual opinion. Even among groups of self-identified outsiders (I'm thinking here of various countercultural movements), the process of grouping eventually bears conformity. When paired with tremendous power, such conformity becomes a dangerous force,

limiting public will and tending toward institutional self-preservation and stagnation. This is true even when the vast majority of individuals within an institution have nothing but the best intentions.

Debates among independent and freethinking individuals—say, for example, nineteenth-century debates about the constitutional versus local theories of cancer causation—may be fiery and controversial, but they are at least more permissive of minority opinions than the same debates within an institution whose position already has been established. Minority opinions are less likely to be expressed, for fear of reprisal—no one wants to be shunned or exiled from their professional society—and so the character of the debates themselves shifts, too. Always one eye is fixed on the party line; independent individuals are downgraded and bunched into homogeneous hives, and freethinking is subsumed by groupthink (which I describe further in chapter five).

Bleak as this may sound, the histories of our most celebrated cancer societies illustrate this pattern all too well. Both in Great Britain and the United States, these organizations were founded and controlled by a small and exclusive group of medical authorities, whose biases uniformly favored the local theory of disease causation and their own treatment protocols. Unsurprisingly, and without exception, none of them gave credence to the recommendations for research on nutrition made by Williams in 1908[15] and Hoffman in 1913.[52] They neglected those recommendations, I'd suggest, not as a matter of conspiracy, but due to a combination of more mundane human defects like stubbornness, bias, and conformity. By these forces, both conscious and subconscious, they settled on recommendations more to their liking. Also, they were clearly influenced by the for-profit sector, which embraced the local theory of disease because it supported the marketing of products.

Why did no institutions arise to support the constitutional theory of cancer causation? Despite the evidence in nutrition's favor, the emerging fields of chemotherapy and radiotherapy had a greater potential for profit, because they lent themselves to the continued and simultaneous discovery of identifiable cancer-fighting products. Further, since the development of new chemical agents and cancer-fighting technology was amenable to intellectual property protection needed for the marketplace, funding was far easier

to obtain. Last, the public had no reason to distrust the four primary cancer societies. Though public skepticism toward the health care system is not uncommon today, projecting this recent fashion onto attitudes of the past century would be a mistake. We were younger then, more trustful of institutions, and not yet the mass victims of chronic disease. As a result, there was virtually nothing to counterbalance the institutions' impact on our society's health, and no one to question their inordinate influence.

EXAMPLES OF INSTITUTIONAL BIASES

Few individuals exerted a greater influence over British cancer research efforts than Ernest Bashford, the ICRF's first research director and the man responsible for outlining the organization's original research plan. He was also deeply predisposed toward the local theory of cancer causation. In 1914,[54] he denied Hoffman's proposition that cancer rates were increasing in the Western world.[52] He referred to a 1905 ICRF report on cancer statistics, written by himself and J. A. Murray, concluding (as I mentioned in chapter two) that statistics on cancer rates in Ireland were less accurate than those from England, and that statistics from poorer countries on the periphery of the British Empire were even less reliable. Based on this interpretation of the statistics, Bashford claimed, the nutritional hypothesis, which largely depends on statistical analyses of population characteristics, was seriously flawed, and England had nothing to worry about. Unfortunately for Bashford and the English nation, this was pure speculation.

The report went further still: "As was to be expected from the facts already made known [from the first ICRF report] . . . diet exerts no primary influence on the occurrence of cancer in various races of mankind." Besides a purely speculative dismissal of data accuracy based on the national origins of that data, how did Bashford and the ICRF justify their disregard for large-scale statistical studies linking nutrition to cancer?

Bashford and Murray's ulterior motives are revealed elsewhere in the report. They claim that "it has been proved that cancer is only experimentally transmissible by actual transplantation of tissues," and further, "it is useless to attempt to establish by statistical means such as a cancer census a

relationship between sporadic cases of cancer." Now, if this sounds confusing to you, don't worry—it should! After all, what do statistical efforts to trace cancer rates have to do with tumor implantation studies? There's no obvious conflict between these two points; why should they be mutually exclusive, or even compete at all?

Why, then, did Bashford and Murray even make a point of discussing tumor transplant studies in this report? When I discovered that ICRF's research efforts at the time were disproportionately focused on tumor transplantation studies, and that Bashford's own personal research experience was concerned with this very topic,[24] everything else slid into place. Bashford and Murray's neglect for statistical studies had less to do with the studies themselves and more to do with their own preestablished research interests, and the interests of their institution. To claim that data from Ireland and less-developed parts of the world are somehow tainted is to find an easy way out of taking those findings seriously, or of having to make a meaningful connection between transplantation studies and statistical analyses.

Regardless of where this bias originated, their insistence on singing the praises of tumor transplant studies, in a seemingly unrelated report on cancer statistics, sounds an awful lot like upper management toeing the company line. It suggests to me that Bashford and Murray weren't all that interested in an honest assessment of nutrition, or in fact any other perspective that didn't fit neatly into the ICRF's existing research agenda.

Such bias continued to dominate cancer research in Great Britain throughout World War I. There was, however, a concern among medical professionals that the ICRF was too focused on laboratory research and wasn't funding enough clinical research.[55] To meet this need, another group of doctors organized the BECC. In the first year after its formation in 1923, there was quite a bit of political maneuvering behind the scenes. The secretary of the British Medical Research Council (MRC), Walter Morley Fletcher, demanded control of the newly formed BECC, including its propaganda and publicity effort, to effectively steer how the public would perceive cancer and its treatment.[55] With help from the Board of Trade, he achieved this control within the year. This effectively gave him the power to funnel BECC funds directly into those areas of cancer research favored by the MRC, and especially toward his own radiobiological investigations.[55]

Considering its preoccupation with MRC-mandated research topics, it's unsurprising that the BECC published almost nothing about nutrition, whether supporting its involvement in cancer or rejecting it. Still, two exceptions stand out: "The Truth About Cancer" in 1930,[56] and a report written four years later by surgeon John Percy Lockhart-Mummery.[57] The second of these reports[58] took an especially aggressive stance against nutrition research: "Various suggestions have been put forward that the incidence of cancer is related to certain foods, or absence of foods, but there is no evidence whatever to support such an idea, and a very great deal of evidence to refute it." I find this quote interesting, given that the BECC had ignored nutrition for nearly all of its early history. Why did they suddenly feel the need to call out nutrition? Were they threatened by the increasing evidence in favor of a nutritional hypothesis? I can only speculate, but the Lockhart-Mummery report does seem to mark a change in strategy. After ignoring nutrition for some time, the BECC seemed here to move toward a more proactive smear campaign.

Other attempts to undermine the evidence supporting nutrition were generally clumsy and dishonest. In one instance, the BECC claimed that "experimental investigations in animals to test these theories have so far been *entirely* negative"; later, the report casts doubt on the "*supposed* unequal geographical distribution of cancer" (both emphases added). This uncompromising refusal to consider the nutritional hypothesis echoes the earlier claims made in 1930's "The Truth About Cancer": "There is no shred of reliable evidence that consumption of or abstinence from any particular article of diet leads to the occurrence of cancer, and that *definite* evidence exists that there is no difference in the liability to cancer of strictly vegetarian communities" (emphasis added).[56] Not only are most of these claims patently false, they also demonstrate the systemic closed-mindedness and intolerance that Hoffman lamented in *Cancer and Diet*.[10]

These bold conclusions rely on a 1926 study by Copeman and Greenwood.[37] Sponsored by the BECC itself, the study claimed to have found no difference in cancer rates among selected religious orders consuming a vegetarian diet. Naturally, I took considerable interest in this study. What I found was not convincing evidence against nutrition, but rather one of the most wickedly misinterpreted studies I've ever read (although I know of some

good present-day competitors). Death certificate data showing lower cancer rates among religious houses adhering to a vegetarian diet were deformed in all manner of ways:

- The authors artificially increased apparent cancer rates in vegetarian houses by rediagnosing death certificates (i.e., renaming the cause of death) and by counting "probable" cases, but made no such adjustments when figuring cancer death rates among the general population.
- When they found cancer incidence rates of only 20 to 40 percent of the expected rate among a large cohort of Continental European houses practicing vegetarianism, they discarded the data.
- They further obfuscated the data by using irrelevant statistical analogies and also alleged a greater practice of vegetarianism than really existed.
- Corresponding to their interest in degrading claims favoring a vegetarian practice, they suggested a conclusion based on Hoffman's study of Indigenous peoples in North America[59] that was the complete opposite of what Hoffman himself had concluded.

Even with all that, the data in Copeman and Greenwood's study showed that the religious houses most strictly observing vegetarianism had the fewest cases of cancer, categorized as having "exceedingly rare" occurrences or none.

Apparently, this escaped the authors' notice. They conclude: "A perusal of our report will convince most impartial persons that no scientific value whatever attaches to assertions, supported merely by the vague pseudo-statistical evidence which is customarily cited, respecting the roles of certain articles of common consumption in the genesis of cancer."

* * *

It would be difficult to discuss these institutions and their biases without including a word on some of their most prominent leaders, like Charles

Childe,[60] who was president of the British Medical Association when the BECC was founded. In 1923 he claimed that "the most important fact we know about [cancer] is that in its beginnings it is local and that its course is a centrifugal spread from its local point of origin." Sounds familiar, right? This is the local theory of disease, cemented as "truth" by institutional authority. As for the concept of centrifugal spread—the idea that diseases spread from a single, central point of origin (local disease origin)—Childe inherited this from the earlier observations of W. Sampson Handley,[61] a very influential surgeon at the Middlesex Hospital cancer ward, which was founded all the way back in 1792.[62] Handley's theory of centrifugal spread is an exceedingly rudimentary explanation for an exceedingly complex problem. Nevertheless, it was very influential. According to Austoker's review,[25] it had provided the supposedly scientific basis for William Halstead's "macabre" radical mastectomy, introduced at the end of the nineteenth century.[63]

Besides developing the theory of centrifugal spread, Handley was also vocally opposed to large-scale statistical studies, or those that can identify the spread of risk factors across many people at the same time—the opposite of the local-origin theory of disease. In 1931,[64] he disputed such studies as well as any nutritional hypotheses that they might have suggested. His adherence to the local theory of cancer causation is reflected in his preference for narrowly focused research methods. Rather than statistical studies, Handley favored "the patient study of *individual* cases of the disease" (emphasis added). He additionally referred to the work of Charles Moore, surgeon at the Middlesex Hospital. According to Handley, Moore proved the local theory of causation "in 1867. . . [when he] showed that recurrence after operation is due, not to an organic or constitutional taint, but to incomplete removal of the primary growth and its surrounding satellite nodules." It's easy to see how this theory, and its emphasis on early and complete removal of the so-called primary growths, eventually inspired Halstead's use of radical mastectomy.

Handley claimed further success on behalf of the local theory by celebrating the success of "local treatment by radium." On that point, it bears repeating that in 1925 Hoffman[33] and others[34,35] showed that radiation therapy *increased* cancer risk, and that even Handley's American counterparts at the ACS had actively tried to *restrain* public optimism for this practice

in the preceding decades.[30] But like so many before and after him, Handley was unperturbed by these contradictions. In fact, he continued to speak out in favor of the local theory in his 1955 book *The Genesis and Prevention of Cancer*.[65] In that book, he also revealed a startling fondness for authoritarian information control and "direct public propaganda" disseminated by institutions like the BECC. Although he hedged somewhat by labeling this propaganda "of secondary importance" to the institution's mission, compared to the goal of promoting surgery, he was not shy in discussing its potential. (Has the term "spin doctor" ever been more apt?)

The fate of the nutrition–cancer hypothesis in Britain was sealed by 1936, when the Health Education and Research Council published its survey on cancer research.[66] Its author, Maurice Beddow Bayly, flippantly dismissed nutrition: "This need not detain us long, for the reason that throughout the entire history of investigations carried out under the two great research funds the writer has failed to discover anything whatsoever that might be dignified by the term 'scientific'"—when, as we have seen, research spanning decades, including experimental animal studies, human population studies, and empirical accounts, showed a connection to nutrition.

I'm reminded here, once again, of both the arrogance of science's gatekeepers in selectively disregarding certain types of research, and of the power of institutional definition. By the latter I mean these organizations' unopposed ability to define one thing as scientific and another as unscientific, merely because it suits their interests. The organizations described above limited "science" to research that adhered to the local theory, a stance that the cancer establishment has carried forth to the present. This institutional definition is all about maintaining power structures, so that influential doctors like Bayly can maintain complete control. Thus, the absence of nutrition from the "great research funds" of Britain is neither a surprise nor an argument against nutrition; it is merely the policy of those institutions and further proof of their biases.

Last, even though the evidence wasn't originating from the BECC or the ICRF, evidence for a nutritional association *did* exist. Bayly obviously felt the need to address some of this evidence, particularly the increasing number of animal studies showing the effect of diet on tumor development. But again, he dismissed these findings, asserting that animal studies "can

produce no results of any value, and the experiments would be ludicrous if it were not for . . . the tragic delay in the progress of scientific knowledge . . . it is surely unnecessary to comment upon the scientific valuelessness of such inanities." Surely unnecessary to comment? And yet he couldn't restrain his own comment, blurting the words "ludicrous" and "inanities" like an impulsive ventriloquist torn between science and the script that ruled him.

* * *

In the United States, the AACR and the ACS were just as beholden as British organizations to the dogmatic research biases of their founding fathers: both denied the role of nutrition in cancer development. This dogma permeated all levels of research funding, choice of experimental methods, and publication. It also carried over to the founding of the NCI as a government agency in 1937. The NCI went on to become the most dominant cancer research agency in the world, a position it maintains today.

The professional backgrounds of the AACR's early leaders illustrate this point well. Of the eleven charter members in 1907, nine were either surgeons or pathologists. None had any background whatsoever in nutrition. Much like the ICRF, the AACR's founders were enamored with the recent wave of tumor transplant studies. In particular, they were very excited about the work of two groups of researchers, one in England and one in the US.[24] The hope at the time was that, by studying tumor transplants, researchers might discover some kind of cancer immunity.

I can understand the appeal of these early studies. But that doesn't justify the exclusion of research on nutritional effects, particularly when other avenues of research didn't face nearly as many obstacles. In fact, the AACR focused great attention on surgery, X-radiation, radium, and "caustic [lye-based] pastes," and their hunt for biological materials with carcinostatic (cancer-halting) potential gave great momentum to the rising field of chemotherapy, which was still struggling to find its feet.[24] Notably, this focus on carcinostatics may have stemmed from G. H. A. Clowes (one of the two non-surgeon, non-pathologist charter members of the AACR), who argued that cancer was caused by a virus. Clowes's example demonstrates just how important representation in these research organizations can be: it's

quite likely that Clowes's inclusion among the charter members was directly responsible for the ramped-up search for immunization procedures that might have the potential to inhibit cancer growth.[67] Unfortunately, nutrition had no such advocate.

But the AACR's contempt for nutrition was not only obvious by its negligence. They also had a very low tolerance for dissent. Bulkley,[24] the chief organizer and first director of the New York Skin and Cancer Hospital, learned this the hard way when, despite his distinguished background and faultless reputation, the AACR decided to excommunicate him for merely suggesting that there was a nonsurgical way to treat cancer.[25] Let's be perfectly clear: there was nothing especially showy or incendiary about Bulkley's small and isolated revolt, if it can even be called a revolt. He simply spoke out about the shortcomings of surgery, particularly breast surgery,[25] in a time when many of his colleagues believed surgery was God's perfect procedure. According to Hoffman,[10] Bulkley also questioned why nutrition "had never yet been given a fair and fully intelligent trial." Two years later, he restated this point with greater conviction and more convincing evidence. Citing the findings of more than thirty-five cancer surgeons, he concluded that no more than one in ten cancer patients could expect a cure from surgery.[23]

I'm not sure whether the AACR was more incensed by Bulkley's advocacy of nutrition or by the evidence he provided against surgery. I suspect it was a combination. And at the end of the day, it doesn't really matter. Both were considered cardinal sins.

Despite Hoffman's early involvement and later research contributions,[24,52,68] introduced in chapter two, the ACS was no more tolerant of the nutritional hypothesis than the AACR. Hoffman's continued work in and around the ACS, despite never being truly welcome, says less about the society's impartiality than it does about his own impressive stature. There was surely a time when the society would have been happy to get rid of him. But by the time he delivered that de facto inaugural speech, he had already established a reputation as the preeminent statistician in the country.

Besides, his persistence did not guarantee the support and respect of the ACS's leading authorities. While not entirely silenced, his recommendations were only ever taken selectively and his impact only ever rewarded minimally. He was kept at arm's length and never given due credit. For example, when

he was awarded the ACS's seventh annual Clement Cleveland Medal, there was no mention of his contributions and recommendations on nutrition. Unfortunately, he was unable to accept the award in person, due to failing health.

Among the six pre-Hoffman recipients of the ACS Medal, four were media and fundraising individuals and organizations, and two were scientists.[24,53] James Ewing was one of those scientists. As well as being a charter member of the society in 1913 and a professor of pathology at Cornell Medical School, Ewing was a prominent charter member of the AACR and an original member of the National Cancer Advisory Council (NCAC) of the NCI in 1937. In other words, there were very few scientists in cancer circles, if any, whose influence could have surpassed Ewing's in the first forty years of the twentieth century;[24,53] given that, it's no surprise that he would have been awarded the Cleveland Medal before Hoffman. But I think it also speaks to the priorities of the ACS. At the symposium dedicated to Ewing,[69] Welch emphasized that Ewing's recognition was "abundantly justified by the strikingly improved results of treatment by *radical surgery* or by *radiotherapy*" (emphasis added).[70]

The other pre-Hoffman scientist recipient of the award was Frances Carter Wood, director of the Institute of Cancer Research at Columbia University. Like Ewing, he also represented ACS on the NCAC and his research interests aligned with mainstream treatment, especially the Blair Bell colloidal-lead method. Again, it's no surprise that Wood, Ewing, and big donors would have received the Cleveland Award before Hoffman. The pattern was set.

In the grand scheme of things, the award itself doesn't matter. It's not my place, a hundred years later, to claim victimization on Hoffman's behalf. However, what is important is what it tells us about these institutions and their biases during this formative period.

Though the ACS didn't have its own formal research program until the 1940s, it did exercise considerable control over discourse in the field of cancer, ensuring a joint stranglehold (along with the AACR) on practically all cancer research until the formation of the NCI. Crucially, it also controlled the flow of information, determining what topics were suitable for debate and under what circumstances. This power is clear in its NCI involvement,

its National Cancer Advisory Council, the *Journal of Cancer Research* (now called the *Journal of the National Cancer Institute*),[24,53] and the seminal inaugural Lake Mohonk research conference.[53] It was in journals like these and at conferences like this where the most important debates on cancer took place, and where the very parameters of those debates were established. Of the NCAC's initial seven members, the ACS selected four, plus the chair.

Unsurprisingly, when Ewing and others who later became NCAC members prepared the roster of speakers for the Lake Mohonk conference,[53] they excluded outliers like Hoffman and radiology critic Gibson. Given that the conference was focused heavily on the interpretation of cancer statistics and mortality trends, the exclusion of Hoffman is especially reprehensible. Nearly every one of the thirty-one presenters was either a surgeon, a pathologist, or a specialist in clinical medicine. There were no nutritionists and only one statistician, a virtual unknown. In his talk,[71] Ewing belittled recommendations on diet and cancer as "semi-medical literature," and showed his favoritism for targeted radiation as a means of treatment—a means, I might add, in which he had invested a large chunk of his own professional life.[70]

FORGETFULNESS IS BLISS

When I emerged from this densely wooded history of nutrition and cancer research at Oxford in 1986, it was with a new pair of eyes. Looking back on that history, I'm more impressed than ever by how many lessons it contains on the formation of our most cherished beliefs about nutrition and cancer—many of which persist today—and their suspect origins, too. Some may point out that this history is deeper than what I've covered here, and surely messier. I won't deny this point. (Isn't history always deeper and messier than our narratives can contain?) Neither will I presume to understand every level of irrationality in the cancer research community. Though I've highlighted a few elements—the appeal of overly simplistic explanations, the pervasiveness of professional biases, and the power of institutions—there are other possible reasons why priorities have been low for research on nutrition and cancer. For example, diet has always been closely tied to tradition and class hierarchy. I don't know the exact degree of impact, but it's possible

these and other elements also played an important role in the rejection of nutritional hypotheses.

To keep this discussion accessible and reasonably succinct, I had to make choices about which events and reports to include. I could have cited many more authors, medical authorities, and scientists for further emphasis. As much as possible, I based my decisions on which to include on two things: the prominence of the individual writer and my ability to read firsthand accounts. In doing so, I hope to have appropriately represented the field and accurately reflected the authors' views.

However, with these limitations in mind, I noticed certain well-documented patterns emerging:

1. Theory determines practice, and vice versa. The victory of the local theory over the constitutional theory reinforces the victory of surgery, radiotherapy, and chemotherapy over nutrition, and the apparently successful (even if for a short duration) use of local treatments reinforces our belief in the local theory.
2. Practice, in turn, shapes the formation of institutions. If more practitioners had taken the "vegetable diet"[72] of the early 1800s seriously, then perhaps they might have formed a research institution of their own and raised funds for studies.
3. Finally, institutions circle back to shape the discourse, including through national policy, surrounding theory, and practice. Their biases tend toward the theories and practices they know best, the same theories and practices that were their makers, thus perpetuating the vicious cycle. This is most intentional when they protect economically rewarding practices.

Within this schema are a vast number of individuals, many with good intentions. My view on these individuals is simple: their good intentions have been institutionalized. Their valiant efforts have been imprisoned, their philanthropy locked up. They have become stuck in a loop, and so has the society that receives their messaging. Crucially, they have also forgotten their own institutionalization. In the foundational years of these institutions, the constitutional theory and nutrition were ignored, uninvited, disincentivized;

later, they were outright forgotten, even erased from these institutions' histories because they deviated from what had become known as science itself.

Moving forward, we might say that the greatest threats to human health are not the ineffectual strategies and protocols that we call "treatments," costly and damaging though they may be, but rather the far more enveloping practice of forgetfulness. If institutions are harsh in their treatment of nonconformists, it is likely not because the participants in this enterprise (researchers, policy makers, etc.) are evil conspirators, but because they have ignored or forgotten the past. They are out of touch with the relevant and important work that preceded them. This is a problem for all scientists, but it is especially detrimental when it comes to research on biologically complex diseases like cancer: we focus far too much attention on the near future, on framing questions and designing projects, and not nearly enough attention on surveying and integrating past lessons.

During the early part of my career, it was generally accepted that any new research project should be preceded by a review of past literature, generally going back at least a couple of decades. Now, it seems that anything published more than five years ago is considered outdated, even irrelevant. As more and more people have entered research, the pace and amount of publishing has accelerated, and "history" is now squeezed from only a few years prior.

I tried to confront this issue some forty years ago when I was on sabbatical leave at the headquarters of the Federation of American Societies for Experimental Biology and Medicine outside of Washington, DC. While there, I was appointed to a Congressional liaison position responsible for monitoring biomedical research funding in Congress. It was an overwhelming job that involved far more lobbying and politicking than I could put up with. Frankly, I did a rather poor job; it was not a good fit for my persona. In any case, as the first academic to have held that position, I was asked to summarize my thoughts, experiences, and whatever useful guidelines I might have for possible successors. Mindful of the environment I was working in, in which most were focused on the near future, with little regard for lessons from the past, I came up with a somewhat tongue-in-cheek suggestion: that we shut down all new NIH research funding (except for ongoing salaries) for five years and instead devote the time and funding to holding

conferences and deliberating on the lessons of the past century. I suggested that we might give the future of biomedical research a new direction by mapping the old routes.

My senior colleagues thought this upstart idea was crazy; everyone knew that the research enterprise, narrow though it was, was moving along *just* right without the need for further deliberation, and that any such discussion would hamper the all-important forward momentum of science (never mind where that force was taking us). In the end, the federation did not want to publish my paper[73] and I relented. Their view was that such a statement would cast too much light on inefficiency within NIH. I agreed that they had a good point. But remember, that was forty years ago! We now have far more scientific information to organize, translate, and put to good use. Most of it will never see the light of day. Most will be forgotten before the ink dries. And so, the wheel of science will continue its spin, not forward on the earth like a tire, but hoisted and fixed to the frame of our institutions, perpetually, redundantly turning.

Research and treatment today must fit within an ever-narrowing scope. I have no doubt that if it were possible to distill the power of nutrition into a single, identifiable agent, like a pill or a procedure, then it would receive much more support and representation. Meanwhile, the old treatment protocols championed by the local theory of disease more than a hundred years ago continue to dominate the field of cancer, despite their ineffectiveness. Indeed, if these had been effective treatment protocols from the beginning—if the results were truly "brilliant in the extreme" as some commentators asserted—then we might have completely avoided the so-called war on cancer introduced in chapter one.

Instead, that war rages on.

Our modern, "advanced" approach to cancer treatment inflicts mind-numbing costs and often-fatal physical trauma. According to 2014 estimates, the average cost of a round of treatment for office-managed chemotherapy is approximately $20,000; for hospital-managed chemotherapy, the price rises to $26,000.[74] Worse still, these costs are rising faster than the average cost of living, so that many individual patients today must make the impossible choice between forgoing treatment and being consumed by medical bills. It's disgusting that so many citizens of such a wealthy country

should be financially devastated by the mere effort to stay alive. I'm sure most would agree. And as we've seen, the effectiveness of many of these efforts is highly questionable at best. According to a 2004 joint Australian-American research group that assessed a large body of data concerning twenty-two types of cancer, our "treatment" is no treatment at all. Five-year survival rates for patients using cytotoxic chemotherapy drugs increased by an average of only 2.1 percent[75] compared to nontreatment, a significant portion of which may be attributed to nothing more than the placebo effect. And if that isn't bad enough, a recent report from the European Medicines Agency found that the majority (57 percent) of authorized cancer drugs between 2009 and 2013 came to market *without any evidence* that "they improved the quality or quantity of patients' lives."[76] Yet, many were introduced in the marketplace as "breakthrough therapies." This "quality or quantity" threshold for measuring success is not very specific or rigorous. It doesn't even distinguish between short- versus long-term effects. Yet most drugs released during the surveyed time frame failed to show this benefit. So poisonous are our cancer "medicines" that patients are directed to flush their toilets twice after using them.[77] This is important because the medication stays in your body for about 48 hours after treatment and it can harm healthy people in your home. And if it can harm healthy people, what might this "medication" be doing to the extremely ill patients who receive it?

In short, though many decades have passed, our ability to treat cancer has not improved. We have merely "updated" the ineffective treatments of one hundred years ago, and all because we have continued to misunderstand and ignore nutrition's role in causing and potentially treating this disease.

REMEMBERING THE ALTERNATIVE

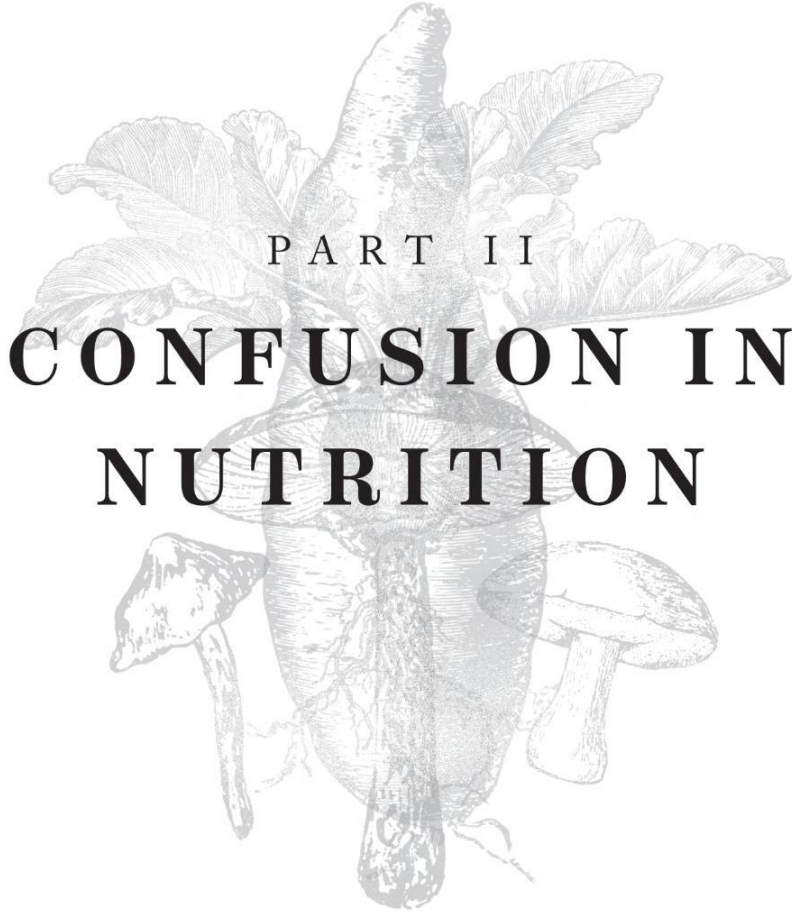
The whole food, plant-based (WFPB) diet and its supporting research are controversial because they challenge the prevailing cultural narrative that diet and disease are only partially and narrowly related. In the last two chapters we have discussed this in the context of cancer, but the predominance of this cultural narrative is more or less apparent for all diseases. My preference for discussing cancer is based on my own expertise in the area and the rich

history that I uncovered in Oxford during the mid-'80s. But make no mistake: the same broad lessons apply to other fields of disease as well.

What I hope to have shown here is that this narrative is a relatively recent phenomenon, in the grand scheme of things. There exists no indisputable mandate to justify our exclusive belief in local causes and local treatments. The evidence in the early twentieth century did not warrant adopting an exclusive belief in local causes and local treatments, and neither does the evidence today. Why should we persist in that exclusive belief, given the costs? Has the time not finally come to give something else a try, as William Lambe proposed two hundred years ago?

The WFPB diet is controversial because it resurrects that old debate about local versus constitutional disease. The status quo today proclaims that this debate has already been put to bed, but the research supporting the WFPB diet suggests otherwise. By establishing the connection between nutrition and disease, it resurrects an old source of controversy—one that the status quo would much rather continue forgetting.

But it's not only the broken disease-care system that the WFPB diet challenges. As I will discuss in Part II, it also challenges our conventional understanding of "good" nutrition, especially orthodox attitudes toward animal protein.



PART II

CONFUSION IN NUTRITION