

HISTORY OF SCIENCE

Was there ever really a “sugar conspiracy”?

Twists and turns in science and policy are not necessarily products of malevolence

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Over the past quarter-century, historical research has revealed how major industries from tobacco to lead to petroleum have meddled in science to conceal the hazards of their products. Drawing on secret industry documents, these studies have shown how special interests have used financial incentives to influence scientists, fabricate doubt, and delay regulation (1). Recently, similar allegations have been made against the sugar industry, with claims that prominent industry-backed researchers in the 1960s downplayed or suppressed evidence linking sugar and heart disease. Building on a newly popular narrative holding that the low-fat campaign of the 1980s was not based on solid science, these allegations have suggested that if not for the machinations of the sugar industry and its cadre of sponsored researchers, the history of U.S. dietary policy might have unfolded very differently. In this article, we argue that the historical evidence does not support these claims. Although we do not defend the sugar industry and cannot address every aspect of this history, we believe recent high-profile claims come from researchers who have overextended the analogy of the tobacco industry playbook and failed to assess historical actors by the norms and standards of their time. Our analysis illustrates how conspiratorial narratives in science can distort the past in the service of contemporary causes and obscure genuine uncertainty that surrounds aspects of research, impairing efforts to formulate good evidence-informed policies. In the absence of very strong evidence, there is a serious danger in interpreting the inevitable twists and turns of research

and policy as the product of malevolent playbooks and historical derailments. Like scientists, historians must focus on the evidence and follow the data where they lead.

The current controversy over sugar has its origins in the rise of obesity as a policy issue near the turn of the 21st century and concomitant concerns that existing dietary guidelines were not achieving their intended ends. As nutrition scientists increasingly acknowledged benefits of “healthy fats” and possible metabolic dangers of added sugars, critical new accounts questioned whether the architects of the low-fat campaign had placed too much faith in weak epidemiologic findings and brushed aside countervailing evidence. Some scientists particularly lamented the fate of John Yudkin, a British nutrition scientist from the 1960s who they noted had “preached in the wilderness” about the dangers of sugar, only to be sidelined and ignored (2).

One article called this historical failure by low-fat enthusiasts to heed Yudkin’s Cassandra-like warnings “the sugar conspiracy” (3).

The case for industrial malfeasance builds on this revisionist foundation, expanding and enlarging the size and seriousness of the “sugar conspiracy.” In September 2016, researchers with the University of California, San Francisco (UCSF), announced that they had uncovered archival documents showing that in the mid-1960s, the sugar industry secretly paid nutrition scientists at Harvard to write a key literature review downplaying the evidence linking sugar and coronary heart disease (CHD) (4). As the UCSF authors recounted, “By the 1960s, 2 prominent physiologists were championing divergent causal hypotheses of CHD: John Yudkin identified added sugars as the primary agent, while Ancel Keys identified total fat, saturated fat, and dietary cholesterol.”

But according to the authors, after the sugar industry “paid off” a Harvard review team led by D. Mark Hegsted, the effect was to “derail” scientific discussions of sugar’s potential role in heart disease, with the dietary fat hypothesis subsequently coming to dominate the field.

Marion Nestle, a nutrition professor and authority on corporate influence, suggested that the documents were a “smoking gun” (5). The Harvard scientists “knew what the funder expected, and produced it,” she said, accepting a “bribe” that may have shaped the field for years. A *New York Times* report asserted that, “five decades of research into the role of nutrition and heart disease, including many of today’s dietary recommendations, may have been largely shaped by the sugar industry.” Recently, a new study by the UCSF group claimed that the sugar industry “suppressed” damaging research it had funded.

We believe that these narratives are wrong. There was no “smoking gun.” There was no “sugar conspiracy”—at least not one which we have identified. Here, we offer a brief



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review of postwar nutrition research on fat and sugar and attempt to explain the emergence of these conspiratorial stories.

A NEW FOCUS ON NUTRITION

In the United States, a new movement to develop knowledge about nutrition emerged in the context of World War II, at a time when health officials believed deficiency diseases posed a threat to economic productivity and military fitness. Concurrently in late 1941, the Rockefeller Foundation gave \$100,000 (~\$1.6 million in 2018, adjusted for inflation) to Harvard to create a nutrition unit, and 15 food firms amassed nearly \$1 million (~\$16 million in 2018) to launch a research organization called the Nutrition Foundation to fund studies and prepare for rationing. At the center of both was Frederick J. Stare, a clinician and biochemist hired by Harvard to chair its new department and by the Nutrition Foundation to edit its journal.

Nutrition research thus began at Harvard at a moment when the interests of nutritionists and food executives seemed to be aligned; deficiency diseases presumably would be addressed through fortified products or efforts to increase distribution and consumption. Initially, the department focused on war-related research commissioned by the U.S. Army and other federal agencies. But in the years after the war, with circulatory diseases accounting for roughly 40% of U.S. deaths, Stare decided to focus on two conditions commonly linked with the “rich” American diet: obesity and heart disease (6).

By 1951, Stare had secured 27 funders, including the U.S. Public Health Service, the Nutrition Foundation, the American Meat Institute, the National Dairy Council, the American Cancer Society, the Eli Lilly Company, and the Sugar Research Foundation (SRF). According to Bernard Lown, a cardiologist hired by Stare in 1958, it was “natural” and “proper” for Stare to turn to the food industry in accord with the mores of the time. “There was no sense of ‘being bought,’” recalled Lown, who later accepted, along with a Soviet colleague, the 1985 Nobel Peace Prize awarded to International Physicians for the Prevention of Nuclear War. Nor were there demands from journals that authors provide financial disclosures, Lown said. “It wasn’t required. It never entered my mind!” (7).

DEVELOPING THE LOW-FAT PARADIGM

As nutrition scientists at Harvard began to examine the possible link between diet and heart disease, the group initially looked askance at the claims of researchers such as Ancel Keys, an eminent physiologist, who believed heart attack prevention should begin with a diet low in fat. Such beliefs threatened the “sound American diet,

where nutritional adequacy is built around meat, milk and eggs.”

But within a few years, the Harvard team became deeply engaged in research contributing to a new causal paradigm in which heart attacks were the end result of a long-term accumulation of fatty material in the coronary arteries, suggesting possibilities for prevention. Many heart disease sufferers had high serum cholesterol levels, and various investigators had produced arterial disease in animals by feeding foods such as eggs. Wartime data appeared to show European populations deprived of fatty animal foods experienced a decline in coronary deaths. Influential early studies of this emerging dietary fat hypothesis were supported by the U.S. dairy industry.

During the 1950s, Harvard nutritionists participated extensively in collaborative field investigations: a study involving four research centers to test the predictive value of blood lipid assays by examining 15,000 workers at Chrysler, Kodak, Met Life, and two dozen other organizations; international studies examining the diets and cholesterol levels of populations in Guatemala, Costa Rica, and Nigeria. These investigations added to insights gained from ambitious cohort studies such as Framingham that would eventually identify multiple “risk factors” (such as elevated blood cholesterol) by following thousands of individuals prospectively over time.

Despite broad agreement that more evidence was needed, the dietary fat hypothesis seemed scientifically plausible to many researchers. As the media queried experts for guidance in the wake of U.S. President Eisenhower’s heart attack in 1955, some tentatively backed the low-fat diet. Stare himself did so in a 1956 magazine column, joining with the President’s eminent heart consultant, Paul Dudley White, in recommending that, “perhaps less fat in the American diet... would be desirable.”

By 1960, a dominant paradigm was forming around the belief that replacing saturated “animal fats” with vegetable oils could lower serum cholesterol levels and possibly fight heart disease. Reflecting this view was a new statement from the American Heart Association (AHA), coauthored by Stare, recommending that the “coronary-prone” consider limiting intake of foods such as whole milk, butter, and meat (8). Hoping to demonstrate causality, scientists began organizing a pilot study for a large “definitive” trial.

THE SUGAR HYPOTHESIS

By the time John Yudkin emerged as an outspoken critic of the fat theory, he was well aware he was fighting an uphill battle. Trained in medicine and biochemistry like Stare, Yudkin had taught nutrition at Lon-

don University since 1946. Situated in a college of household science, however, he was initially unable to obtain funding from the government’s Medical Research Council (MRC). Yudkin thus turned to industry for support. “I’ve always been a consultant to the food industry,” said Yudkin in a 1979 interview, arguing that it would be “highly illogical” for a nutrition scientist to refuse to work with food companies.

Yudkin’s entry in the diet-heart debates began with a 1957 paper challenging prevailing interpretations of the widely discussed hypothesis that countries that consumed more fat had higher rates of coronary mortality. He argued that several modern exposures correlated with coronary deaths as well or better than fat, including animal protein, sugar, and access to radio and television. Focused on writing a popular diet book, Yudkin did not initially implicate sugar in heart disease, seeking merely to establish that his recommended low-starch slimming regimen, rich in meat and cheese, was not a recipe for cardiac arrest. (He actually endorsed saturated fat restriction for the coronary-prone.)

But after learning of a study that attributed elevated rates of heart disease among some immigrants to their transition from a meaty diet to more sugar-laden fare, and new claims suggesting a sugar-sensitive constituent of the blood called triglycerides might predict heart attacks better than serum cholesterol, Yudkin sought to refine his hypothesis. In 1964, he performed a case-control study that used questionnaires to assess the sugar intake of 25 men without known heart disease, 20 coronary patients, and 25 men with arterial disease. Finding a significant difference in sugar intake between his cases and controls, Yudkin proposed in the *Lancet* that “people who take a lot of sugar—for example in their tea or coffee—are far more likely to have a heart attack than those who take little” (9).

The publication stimulated 20 letters to the editor, several of them with questions about Yudkin’s failure to adjust for factors such as smoking and body weight. MRC scientists asked why Yudkin’s controls averaged 77 grams of sugar per day when his own data suggested the typical Englishman ate 139. But Yudkin’s sugar theory attracted wide press attention, as well as new offers from book publishers.

AN INDUSTRY-SPONSORED REVIEW

Despite Yudkin’s work, the central focus of diet-heart research remained on fat. At Harvard in 1962, Stare’s top scientist, Mark Hegsted, launched a controlled feeding study at Danvers State Hospital, a Massachusetts psychiatric institution. Backed by a new dairy industry fund created to give producers a

firsthand look at the possible role of dairy fats in disease, the Danvers study involved feeding 20 schizophrenic men various diets in which intake of vegetable oils, butterfat, eggs, starch, sugar, and lactose were manipulated so as to quantify their impact on blood cholesterol levels.

Over his 20 years at Harvard, Hegsted had earned a reputation as a data-driven scientist. Among his first Harvard studies was an analysis of adult protein requirements that disappointed its sponsor, the American Meat Institute, by concluding that the National Research Council's recommended daily allowance was much too high and that even vegetarian diets could supply adequate protein. Under the Reagan administration, Hegsted would be fired from his job devel-

the "abundant evidence implicating dietary fat." The Danvers data buttressed the case against saturated fats, providing the basis for a new formula relating fat intake to serum cholesterol that came to be called the "Hegsted equation." The findings helped nudge the AHA to extend its warning about dietary fat to the entire U.S. population.

It was a disaster for the dairy industry. But for sugar executives—one attended Hegsted's talk—the findings appeared to constitute a scientific basis for countering the claims of Yudkin. One month after Hegsted's presentation, an SRF executive reached him by phone to discuss his research and the prospect that SRF might hire him to conduct a review of those articles "which find some special metabolic peril in sucrose." Hegsted agreed

THE BATTLE OVER SUGAR AND FAT

Increasingly challenged by his scientific peers, Yudkin continued to gather evidence on sugar: examining national patterns in diet and disease, refining his questionnaire-based study linking sugar and heart disease, and conducting clinical studies of the effect of elevated sugar intake on insulin and platelet adhesiveness. Assisting him in amplifying his ideas was a group of powerful commercial entities whose interests aligned with his beliefs. In 1966, Yudkin reported that he was receiving ~£25,000 per year (~\$530,000 in 2018) from "the big food manufacturers."

Yudkin was an asset because he brought scientific legitimacy to events such as the high-protein breakfast promotion organized in 1966 by the British Egg Marketing Board. The next month, for International Milk Day, he joined the National Dairy Council to publicize its new "seventeen day milk diet." He conducted research showing the "value of taking milk before alcoholic drinks"—an implicit test of the "Drive Safely on Milk" slogan the dairy industry had used since 1961. As a dairy consultant, he toured the United States, promoting his sugar theory and an industry statement he had written titled "Sense and Nonsense about Dairy Foods." In one industry meeting, Yudkin would later note that his research on sugar could also be viewed as "diversion tactics" that might "prove beneficial by freeing butterfat from any 'guilt.'"

By the eve of the 1970s, Yudkin had acquired some critics. "I regard Yudkin as a menace and a deterrent to good nutrition policy," wrote Hegsted to a colleague in 1969. That year, the sugar industry convened a panel of heart disease consultants, including a National Institutes of Health (NIH) scientist, which debated a possible "anti-Yudkin" effort because "although British scientists are critical of him and his flimsy data, he does have the interest of the press." Indeed, British government scientists had become sufficiently concerned about Yudkin's sugar hypothesis that they decided to put it to an authoritative test. Multiple government research teams, some of them part of a multicenter working party organized by MRC, tried without success to replicate Yudkin's finding that heart attack sufferers tended to be heavy sugar users. The eminent MRC panel reported in 1970 that the evidence in favor of the sugar hypothesis was "extremely slender" (12).

These publicly funded studies, along with other forceful critiques, marked the beginning of the end for Yudkin's sugar hypothesis. In 1971, he retired and began summarizing his case against sugar in a popular book. Proponents of the fat hypothesis soon faced disappointment as well: NIH declined to fund the "definitive" trial, despite persis-



Enjoying ice cream in the 1960s, when battle lines were being drawn over the roles of dietary fat and sugar.

opening the first U.S. Dietary Guidelines after his low-fat approach provoked the ire of the beef industry.

The Danvers study would become another example of Hegsted's independence. In a talk on 6 May 1965, at a meeting of the Nutrition Foundation, Hegsted began by addressing recent findings linking carbohydrates with heart disease, acknowledging that Yudkin's claims were "worth considering" but hard to credit "without controlled laboratory data" (10). He expressed skepticism about new claims that triglycerides might be a better risk indicator than cholesterol. Moving to the Danvers data, Hegsted said the results suggested that the link between dietary carbohydrates such as starch or sugar and serum cholesterol elevation was "rather minimal" compared with

to cover SRF's "particular interest," but only within the context of a review "sufficiently broad to make it worth doing."

For Hegsted, the sugar review was but one of several Danvers-related articles to be written, including one that expanded on his Nutrition Foundation talk—a draft of which he shared with SRF. In it, he argued that practical manipulations of the American diet should focus on dietary fat, noting that the "potent role" of fats had been "amply demonstrated," but that such a role for carbohydrates had "not yet been shown." The SRF-sponsored review, published in the *New England Journal of Medicine* in 1967, expanded on these themes and reviewed additional studies but did not disclose that it had been commissioned by SRF (11).

tent exhortations that if strong trials could not be mounted, then dietary advice would have to be promulgated based on the best existing evidence.

By the 1970s, nutrition had become a subject of heated public discussion, and a U.S. Senate committee—after extensive expert testimony and stern warnings from several well-credentialed skeptics who simply did not believe the evidence was sufficient to warrant the issuance of high-profile congressional recommendations for dietary change—embraced the low-fat concept as one aspect of a broad program of healthy eating (13). The committee's 1977 report *Dietary Goals for the United States*, which set the model for the low-fat policy paradigm, mentioned Yudkin's theory only in passing. Written by committee staff but edited mainly by Hegsted, the *Dietary Goals* did not, however, overlook sugar. Taking note of sugar's link with tooth decay and possibly diabetes, the report recommended a 40% reduction in sugar intake.

CAUGHT IN THE CROSS FIRE

As we have shown, by the 1960s the paradigm that dietary fat was a likely risk factor for heart disease prevailed among a coalition of scientists closely linked with NIH and AHA and was based on extensive research. By contrast, the sugar theory was developed by a small number of researchers, was supported by limited evidence, and was not accepted by key authorities. Normal science is a social project in which a community of scientists develops consensus over theory. Heart disease epidemiology, in adopting a multifactorial model, could plausibly have accommodated sugar if the theory had withstood testing. But Yudkin's claims were seen as weak and antagonistic, and his signature finding could not be replicated. Moreover, sugar did not appear to meaningfully affect serum cholesterol—the only then-accepted lipid pathway to coronary disease.

As we have also shown, the sugar industry approached Hegsted only after learning of the results of his dairy industry-backed study suggesting that fat and not sugar was a factor in heart disease. “There was no, ‘We’ll get money from them and make the results come out this way,’” recalled Lown, who worked in the department. “It didn’t happen that way,” he said. “The sugar industry didn’t find researchers at Harvard who would make up a story they didn’t believe in order to cash in on the sugar industry money,” asserted Gary Taubes, author of *The Case Against Sugar*, a 2016 book that delves deeply into the sugar industry's involvement in nutrition research. “What industry does is find people who already believe something that that industry finds convenient, and then they pay those people to make those beliefs known” (14).

During the period in question, food industry funding of nutrition research was routine, and disclosures were “rarely required,” as Marion Nestle has written. When NIH moved to defund Framingham in 1968, its former director rescued it by soliciting grants from the Oscar Mayer Foundation and other private entities—none of which were routinely disclosed in publications. Today, food industry funding of research remains common, although most journals require disclosure of conflicts of interest. Compliance is inconsistent, however, and some argue existing policies do not address important sources of bias such as investigators' dietary habits and beliefs.

Our study raises questions about how to assess the historical influence of special interests on nutrition science and policy. Certainly, there is ample evidence that various sectors have tried to influence scientists, and we agree with those who suggest that food companies fund research with an eye toward marketing. (Indeed, an internal sugar industry document states that SRF was created “for the basic purpose of increasing the consumption of sugar.”) We do not claim the sugar industry had no influence on nutrition work at Harvard, nor on the field in general. But we believe that there is no good reason to conclude that SRF's sponsorship of a literature review meaningfully shaped the course of dietary science and policy. Moreover, we think it is an error to demonize, almost as a reflex, scientists and their research when there is evidence of private funding. Such a response can create an intellectual template that short-circuits a fuller investigation of alternative explanations. For example, arguments that the sugar industry “suppressed” evidence should be tested against alternative hypotheses.

Our history also underscores the fallacy of emphasizing the machinations of one commodity sector when multiple food industries were deploying similar techniques of influence in the battle for market share. It is notable that during the low-fat era of the 1980s, when suspicion fell heavily on the meat and dairy industries, it was argued that, “The ‘fat lobby’ has not only influenced our nation's food and nutrition policies, it has *determined* those policies” [emphasis original] (15). Nearly 40 years later, at a moment when some have said “butter is back” and sugar is toxic, “Big Sugar” is the behemoth accorded these dramatic powers. Caught in the cross fire of these “diet wars” have been the reputations of historical nutrition scientists, whose statures have risen or fallen based on the extent of their contribution to current theories.

Interpreting history requires attention to the logic and tools of the period under study.

Over the course of the diet-heart debates, the techniques of epidemiology and causal inference evolved substantially. The randomized controlled trial had not yet attained the hegemonic “gold standard” status it is often accorded today. It is thus peculiar to reject as unscientific the beliefs of those who pressed for action on the basis of then cutting-edge epidemiologic theory and research. As the great historian of science Thomas Kuhn once wrote, “Out-of-date theories are not in principle unscientific because they have been discarded.”

Historical investigations of “merchants of doubt” have been invaluable in showing that scientific uncertainty is sometimes the product of deliberate acts of deception. Such studies underscore the essential insight that the existing evidence base is powerfully shaped by social forces and political choices, and that had decisions unfolded differently, our areas of knowledge (such as genomics) and blind spots (such as obesity prevention or gun violence) would be shifted. But ahistorical accounts thwart our ability to critically evaluate the often long and zigzag process of scientific conjecture and refutation. They provide spurious cover for changes to policy by suggesting that old ideas are illegitimate. And, they advance a false impression that doing the “right” kind of science will somehow avert the messy business of making policy based on incomplete evidence, public values, and democratic politics (16). ■

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16. Suggested further reading is provided in the supplementary materials.

SUPPLEMENTARY MATERIALS

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