

When is Science Corrupted?

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A [paper](#) by Cristin Kearns just published in *JAMA Internal Medicine* makes the case that the Sugar Research Foundation – an industry advocacy group – contributed to the catastrophe that is coronary heart disease in the United States by paying some prominent scientists to blame fat for CHD, not sugar, despite evidence to the contrary. According to Kearns, in the mid-1960's, the SRF paid Harvard professors led by Mark Hegsted \$48,900 (in 2016 dollars) to critically review studies linking coronary heart disease to excess sucrose consumption. As the article went off to print, the SRF representative reported to them his assurance that “this is quite what we had in mind and we look forward to its appearance in print” (qt E3). The resulting review (which made no mention of the authors' ties to the SRF) made a herculean effort to discredit the research linking sugar consumption to CHD, sometimes even inconsistently: it first denied that epidemiological evidence was relevant in determining dietary causes of CHD, but (in the next part) also that implied that epidemiological evidence pointed to dietary cholesterol and saturated fat as the primary causes of CHD (E3).

Was this corrupted science? There are legitimate, honestly-held scientific disagreements, and certainly legitimate clinical ones about what to do with that science. The question is where we draw the line between a dispassionate study of the evidence and cases where that study was sufficiently nudged by inappropriate factors to cross an ethical line. Kearns' research makes Hegsted's work look like a nearly paradigmatic case of corruption. If so, it was consequential corruption: coronary heart disease is of course the [number one cause of death](#) in the United States; only cancer (all of them combined) comes close. That implies that large numbers of people are now dead because of bought-and-paid-for science, and a non-trivial part of the blame for this rests on Hegsted (who died in 2009). This would be up there with the tobacco industry's efforts to bury the link between smoking and cancer, or the fossil fuel industry's efforts to bury the link between carbon emissions and climate change.

On the other hand, [one can argue](#) (as does Adele Hite, a PhD Candidate in Communication, Rhetoric and Digital Media at NC State) that the situation is more complicated than that. Hegsted and colleagues apparently thought that consumption of fat was the main contributor to blood cholesterol (which was in turn the preferred predictor of CHD) before being approached by the SR. At least, they published an [article to that effect](#) that appeared in November, 1965. The timeline is a bit murky: Kearns says that the SRF visited Hegsted on July 1, 1965, but ordinary journal timelines suggest that the original study was probably complete before July. Hite suggests that this means the SRF knew beforehand what Hegsted would say, because of his publication record, and that this

exonerates him from the pay-to-produce charge (she does not, however, explain how the SRF had his research months before it appeared in print).

Hegsted *et al.* also did not consider that anyone would embark on a low-fat, high sugar diet: supremely cheap corn syrup was a thing of the future. It is the status quo now; in his *Salt, Sugar, Fat*, Michael Moss argues that the convenience food industry has essentially rotated between these three additives in response to consumer demand for convenience foods that can be stored easily and require minimal or no preparation to eat. Whenever popular opinion decides that fat is a problem, lower fat but higher sugar and salt alternatives are offered. When sugar is discredited, fat and salt go up. And so on. But this was arguably not Hegsted's world. Indeed, [research in science studies](#) dating at least to 1997 points to an extremely complicated socio-political-scientific nexus that generated dietary recommendations to cut fat and cholesterol; the science is and was disputed, but was only part of the debate. And of course the role of sugar is also still debated. One lesson is that food research is hard, because double-blinded studies where you precisely control food intake long enough in two different groups to measure health outcomes are essentially impossible. Hence the reliance on debated proxies like serum cholesterol, or isolated populations with similar diets, as in Okinawa.

So: was Hegsted's work morally compromised by the payment he received from the SRF? And, separately, to what extent is the SRF morally compromised? They have apparently pursued a [marketing strategy since the 1950s](#) encouraging people to eat a lower fat (and therefore presumably higher sugar) diet. For that matter, when is research into the relationship between food and particular health outcomes robust enough to serve as a basis for policy? I don't have the answers to these questions, but given the prominence of CHD and other diseases like Type II diabetes that appear to be - most of the time, at least - generated by lifestyles, it is urgent that we ask and answer them as honestly as we can.