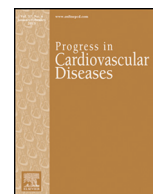




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The obesity wars and the education of a researcher: A personal account

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ABSTRACT

A naïve researcher published a scientific article in a respectable journal. She thought her article was straightforward and defensible. It used only publicly available data, and her findings were consistent with much of the literature on the topic. Her coauthors included two distinguished statisticians. To her surprise her publication was met with unusual attacks from some unexpected sources within the research community. These attacks were by and large not pursued through normal channels of scientific discussion. Her research became the target of an aggressive campaign that included insults, errors, misinformation, social media posts, behind-the-scenes gossip and maneuvers, and complaints to her employer. The goal appeared to be to undermine and discredit her work. The controversy was something deliberately manufactured, and the attacks primarily consisted of repeated assertions of preconceived opinions. She learned first-hand the antagonism that could be provoked by inconvenient scientific findings. Guidelines and recommendations should be based on objective and unbiased data. Development of public health policy and clinical recommendations is complex and needs to be evidence-based rather than belief-based. This can be challenging when a hot-button topic is involved.

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I was a senior scientist at the Centers for Disease Control and Prevention (CDC) for almost 30 years. Beginning in 2000, I began working with a CDC colleague and two expert statisticians from the National Cancer Institute (NCI) on a method to estimate the number of deaths associated with overweight and obesity. We thought the topic was interesting and the previous literature inadequate. As federal employees, we had no outside funding or conflicts of interest. Our intent was to use more recent data and better statistical methods to provide more accurate estimates than hitherto available.

Unbeknownst to us, a somewhat similar project was underway elsewhere within CDC. That project resulted in the 2004 publication in JAMA of an article by Mokdad and other CDC authors, including the then-CDC director.¹ Their article concluded that obesity was poised to overtake smoking as a leading cause of death in the US. These findings were widely publicized although they met with some controversy, including concerns from anti-tobacco activists.² The Mokdad et al. article had many flaws, however, including older and largely unrepresentative data sets, erroneous coding of smoking data in one data set,³ a statistical method that failed to adjust correctly for confounding factors,⁴ and easily identifiable calculation errors that required a correction to be published.⁵

For our project, we developed a method that provided appropriate statistical adjustment for confounding factors. In addition, we used recent and nationally representative data sets from CDC surveys. Our results were published in JAMA in 2005.⁶ A comparison of some features

of our article with the 2004 Mokdad et al. article is shown in Table 1. We found that obesity was indeed associated with excess deaths relative to normal weight, although our estimate of less than 5% of deaths was considerably lower than the 2004 Mokdad et al. estimate of over 15%. CDC accepted our results for obesity as the better estimate a month after our article was published.⁷ We also found that overweight was associated with slightly but significantly fewer deaths than normal weight. A quick glance at the literature suggested that our findings about overweight were not particularly unusual. We were unprepared for the firestorm that followed.

Our article attracted attention because it appeared to be inconsistent with the dramatic conclusions of the 2004 Mokdad et al. article.⁸ I fielded dozens of press calls as soon as our article was published. To my surprise, after the first few hours, many of the journalists who called me had already spoken to a professor, Walter Willett, (let's call him Professor 1) from a prestigious school of public health (PSPH). He was not a statistician and had no expertise in estimating the number of deaths associated with obesity. Our article was not intended to have anything to do with his work. He had apparently begun pre-emptively contacting the press, inserting himself into the discussion, positioning himself as an expert, and providing negative and antagonistic comments on our article before reporters had spoken to me. He used strong language to disparage our article, describing it as "really naive, deeply flawed and seriously misleading".⁹ At a scientific conference, a little over a week after our article appeared, Frank Hu (let's call him Professor 2), another professor from PSPH, took the unusual step of pre-empting a planned presentation by someone else to take the stage and deliver a critique of our just-published article. When I presented a seminar at UC Berkeley

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Table 1
Comparison of Mokdad et al.¹ and Flegal et al.⁶ articles.

	Mokdad et al., 2004	Flegal et al., 2005
Results	Estimated overweight and obesity-associated deaths in 2000 as 365,000	Estimated obesity-associated deaths in 2000 as 112,000
Features in common	Used BMI measured once at the beginning of each study Did not limit sample to healthy never-smokers	Used BMI measured once at the beginning of each study Did not limit sample to healthy never-smokers
Data	6 data sets, older data, not representative, only one from NHANES	The 3 most recent nationally representative data sets from NHANES
Weight and height data	3 studies with measured weight and height and 3 with self-reported data	All height and weight data were measured
Hazard ratios adjusted for	Age, sex and smoking	Age, sex, race, smoking, alcohol consumption
Estimates of variability	None	Standard errors for estimates
Attributable fraction method	Method that did not adjust for effects of age, sex, smoking or other factors on mortality ^{4,19,51–54}	Method that adjusted for the effects of age, sex, race, smoking and alcohol consumption on mortality
Errors in smoking data	Smoking data for NHANES I were incorrect ³	Correct smoking data used for NHANES I
Calculation errors	Article contained simple calculation errors that anyone could have identified from the published data	No identified calculation errors

a week after our article appeared, an unidentified young woman stood at the door giving out a handout of 4 pages of faxed and photocopied material that included an abstract from PSPH and several news articles that discussed PSPH research on obesity.

Our 2005 article had been reviewed extensively by scientists within CDC and NCI, cleared for publication by both agencies, reviewed by peer reviewers at the journal and accepted by the JAMA editors. Nonetheless, less than a month after its publication, a speaker from the American Cancer Society (a PSPH graduate) suggested in a talk at NCI that our article should not even have been published, with one of his PowerPoint slides saying: “Because of the importance of these estimates, scientific controversies should be addressed in a scientific forum that seeks consensus, rather than immediately publicizing widely divergent estimates through the media.”

Perhaps feeling that lower estimates of obesity-associated deaths were detrimental to public health goals, some began casting around for explanations that would show that our estimates were less valid than the 2004 estimates by Mokdad et al. “Fact sheets” and lists of “talking points” (one entitled “Damage Control for the Flegal article”) began to circulate from various public health-oriented groups describing our estimates as problematic and giving misleading arguments as to why the 2004 estimates were better. The “Damage Control” talking points, for example, asserted that the 2004 paper was superior because it had used data on “diet and physical activity” even though the 2004 paper had not in fact used any data on diet and physical activity. A group from PSPH, including both Professor 1 and Professor 2, published a long speculative article¹⁰ in 2007 about “reconciling the differences” that failed to mention the errors in the previous statistical method or the inadequacies of the data sets used in the 2004 Mokdad et al. paper and ended up announcing that the real problem was that we had asked the wrong question (although it was the same question that the 2004 Mokdad article had asked).

Almost as soon as our article appeared, a symposium was scheduled at PSPH for the express purpose of criticizing our article.¹¹ One of the organizers wrote to me to say that they viewed this as an opportunity to engage in a respectful and constructive examination of the issues and provide a more in-depth view for the media so they could acquire a deeper understanding. The line-up consisted of a small number of vocal critics, mostly from PSPH itself, all attacking our work and asserting that their previous research somehow showed that our estimates should have been higher, although their previous research had not even addressed the topic of estimating numbers of deaths. The presentations at the symposium did not mention the multiple errors in the 2004 Mokdad et al. article. One speaker described us as having no biomedical background, even though the four authors of our article were well-published senior scientists, all with doctoral degrees in nutrition or statistics and one with a medical degree from Harvard Medical

School. Seeking to maximize media coverage, the organizers arranged for the entire symposium to be web-cast live and encouraged reporters to view and report on it.

Further attacks, many but not all emanating from PSPH and its alumni, continued over a number of years. These ranged over a broad gamut: criticisms that we repeatedly refuted, generic minor criticisms that would apply to most articles in this general field (for example that, like the 2004 Mokdad et al. paper, we had used body mass index instead of a more precise measure of adiposity), misinformation, content-free insults and name calling, and sometimes outright falsehoods. It took me far too long to understand that our findings were being treated by some as a partisan issue rather than as a topic of scientific discussion. Our work was attacked in a surprising variety of non-scientific forums, including internet blog posts, social media posts, in-house newsletters, widely distributed fact sheets, and Wikipedia entries. Trying to get errors corrected was stressful and time-consuming. We repeatedly demonstrated that the criticisms being raised would have little or no effect on our results, but these demonstrations were ignored or dismissed.

A number of researchers prepared papers to attack our work, employing convoluted analyses of unclear validity. I began to call these “Flegal is wrong” papers because their primary intent appeared to be to prove that something was wrong with our paper that had caused our estimates to be too low. Such papers often contained a speculative “rescue hypothesis” – claiming with no evidence that if some particular feature of our research had been different, our estimates would have been higher. In several cases, we went to the effort of writing and publishing a new article to demonstrate that one or another speculative hypothesis did not explain our results.^{3,12–19} For example, Manson et al.¹⁰ had incorrectly speculated that older ages at measurement had led to downward bias in our estimates; we published an article that showed that their speculation was incorrect.¹⁶ One research group repeatedly tried to publish a paper with the claim that although we had used age as the time line, if we had also included age in our models, we would have gotten different results. To forestall the eventual publication of this erroneous claim, we published a brief article to demonstrate that such an inclusion would not have changed our results.¹³

Although the “Flegal is wrong” papers referred specifically to our article, they often misunderstood key details. These papers tended to focus on analytic methods rather than on data, but in fact our use of more updated and better-quality data sets accounted for much of the difference. We had used nationally representative survey data with measured weights and heights. Critics rarely if ever noted that our findings might be due to our use of better data.

Some criticisms employed a rhetorical approach known as “paltering,” defined as the active use of truthful statements with the intent to deceive.²⁰ Critics would emphasize that our article found different

results than previous articles had and then mention some criticism of our article, with the implication that this was the reason for the differences. However, they would not mention that the same criticism would apply equally well to the 2004 Mokdad et al. article and thus could not explain the differences. For example, a laboratory exercise for graduate students in epidemiology at Johns Hopkins University compared our results unfavorably to those of Mokdad et al., stated four different times that our study had only used a single measure of BMI and then asked students to “Discuss the appropriateness and effect of using a single measure of BMI in attributing subsequent deaths to obesity” without noting that Mokdad et al. had also used a single measure of BMI. Other examples of paltering are shown in Table 2.

Attacks on our paper continued and appeared in some unexpected places. A 2007 story appeared in Scientific American²¹ by a leading health journalist who had never even contacted the CDC press office or spoken to me but nonetheless asserted that our conclusions were “probably wrong,” quoting two PSPH faculty at length. “It’s complete nonsense, and it’s obviously complete nonsense, and it’s very easy to explain why some people have gone astray,” said one.

In the same year, a post-doc at PSPH posted the following on a blog: “Numbers from Flegal’s paper had been subsequently RETRACTED [sic] by the CDC, and she has subsequently been demoted at the CDC for writing the erroneous paper.” Every single one of these statements was false. CDC had not retracted our findings, and I had not been demoted. In fact, our paper had received CDC’s highest science award, the Shepard award, in 2006. After I called the post-doc to point out his errors, he apologized and deleted the post. He was unable or unwilling to tell me where he had gotten his misinformation, although he assured me it was not from anyone at PSPH.

A 2007 article²² from a different PSPH group claimed falsely that CDC had “recanted” our 2005 article. I was impressed that this unreferenced statement could have been written to begin with and then could get through reviewers, editors and copy editors without anyone asking for clarification or evidence. At our request and after some negotiations, the authors reluctantly published an erratum.²³

Around the same time, some unusual statements were anonymously inserted in the Wikipedia entry on “overweight.” These statements asserted with no references that our article had been “widely discredited and regarded as fatally flawed by researchers from the Harvard School of Public Health, Harvard Medical School, American Cancer

Society, and even the CDC agency itself, which has backtracked on the findings from the Flegal report.” This was part of what appeared to be an ongoing campaign to present our article incorrectly as having been repudiated by reputable sources.

In 2007, I accepted an invitation to give a named lecture at the 2008 meeting of a scientific society. The invitation included no mention of a rebuttal. When I received the final program a month before the meeting, to my surprise Professor 2 from PSPH had been added as a rebuttal speaker. This is an unusual way to treat an invited lecturer. As part of Professor 2’s rebuttal, he presented a slide supposedly “based on” our research that strangely showed precisely the opposite of what we had found. It turned out that Professor 2 and his group had misunderstood a table in our published article and misinterpreted the results. Although I wrote him an email to clarify the table, Professor 2 and his colleagues nonetheless submitted an article for publication with the same erroneous analysis. Fortunately, their article was rejected. This led me to realize that if such an article were to get published with such an erroneous analysis, it would likely be quite difficult for me to ever correct the situation. This episode as well as others also led me to realize that some, perhaps many, of our critics had very little understanding of our article. For example, Professor 2 gave a completely incorrect description of our method on page 46 in his book published in 2008.²⁴

Another line of attack was something like “this is just one study.” According to the 2007 hit piece in Scientific American, “Decades of research and thousands of studies have suggested precisely the opposite ...”, adding “Flegal is not necessarily wrong, but the preponderance of evidence clearly points in the other direction.” In fact, many other studies had already shown no excess mortality associated with overweight. The 2013 obesity guidelines²⁵ put out jointly by the American Heart Association, the American College of Cardiology and The Obesity Society, also reported the finding that overweight did not appear to be associated with excess mortality, rating the strength of the evidence as “moderate.” Professor 2 was a coauthor of these guidelines.

A study using nationally representative Canadian data appeared in 2010 with findings similar to ours.²⁶ Subsequently, CDC and NIH co-authors, the Canadian researcher and I carried out a systematic literature review,²⁷ which was published in JAMA in 2013. Before publication, our article had been reviewed extensively by scientists within CDC and NCI and cleared for publication by both agencies. The summary results from 97 published studies with a total of almost 3 million participants

Table 2
Examples of “paltering” – using true statements in a deceptive way.

	Step 1, emphasize that our article found different results than previous articles had	Step 2, mention some criticisms of our article with the indirect implication that these criticisms might explain the differences	Step 3. Do not mention that the criticisms of our article apply equally well to the previous articles.
Willett ⁵⁵	“In their study of deaths associated with underweight, overweight, and obesity, Dr. Flegal and colleagues conclude that excess mortality due to obesity and overweight is much lower than previously reported.”	We believe that their analysis is flawed and misleading.... In the main analyses, the study apparently did not exclude persons with known chronic disease at baseline	Willett does not mention that the previous estimates also did not exclude persons with known chronic disease at baseline.
Moore ⁵⁶	In 2005 a controversial study by Flegal et al. estimated that 26,000 deaths per year in the United States were attributable to excessive body weight, which contrasted strikingly with a previous estimate of 280,000 deaths due to excess body weight.	Flegal et al. may underestimate the proportion of deaths attributable to excess adiposity because they do not exclude persons with a history of smoking or preexisting disease.	Moore does not mention that the “previous estimate” also did not exclude persons with a history of smoking or pre-existing disease.
Harvard Health Letter ⁵⁷	In April, a study published in the Journal of the American Medical Association (JAMA) reported that obesity increased the risk of premature death much less than previously estimated.	Other researchers (including several at Harvard) believe the unexpected findings came from methodological errors in the study. These critics say the CDC researchers didn’t take into account two important variables: smoking and illness. That arise when attributing death to excess weight.	The report does not mention that the values previously estimated did not take smoking and illness into account.
CA-Cancer journal for clinicians ⁵⁸	The new figures were vastly different from those in an earlier CDC analysis	The main concern regarding the newer CDC analysis is that it did not adequately account for weight loss from serious illnesses such as cancer and heart disease. Including such individuals in the analysis created the false appearance that being overweight protected against death during the follow up.	The report does not mention that the earlier CDC analysis did not account for weight loss from serious illnesses and did not exclude such individuals from the analysis.

were that overweight was associated with slightly but significantly lower mortality than the “normal weight” reference category. An anonymous peer reviewer commented: “[This study] documents the conclusion that I suspect most people who follow the health and obesity literature have concluded but not formalized. In spite of the labeling of BMI 25–<30 with the pejorative title ‘overweight,’ the data on mortality do not support that this category of body mass index has an increased mortality.”

As was clear from our review and from many articles published since, our findings were not unusual. Professor 1 was evidently aware of this, since he was quoted²¹ as saying “About every 10 years this idea comes along that says it's better to be overweight. And we have to stomp it out.” When I reviewed the literature for our 2013 meta-analysis, I noticed that although almost all the articles, including some from our critics, had found either no increased mortality for the overweight or else slightly decreased mortality, few of them mentioned this in the abstract or gave this finding any prominence. No wonder people thought our findings for the overweight category were unusual; it was not evident how common they actually were.

Apparently, according to some of our critics, new and better scientific results are dangerous and cause confusion if they fail to buttress what you already believe. In 2005, Professor 2 claimed²⁸ that our findings had “caused a great deal of confusion among the general public.” When our 2013 review appeared, Professor 1 fired off an email to my employer in the person of the CDC director, reprising the themes of ‘damage’ and ‘confusion’ and saying that he thought a meeting was important to begin to repair the serious damage done by our review article, which, according to Professor 1, had not only caused public confusion over this issue but had also contributed to undermining confidence in science in general.

A second tribunal was convened at PSPH, this time to attack our 2013 literature review. The speaker line-up was almost identical to the symposium in 2005. According to a news report, the panelists “expressed concern that much of the popular journalism and commentary about Flegal's research could undermine the credibility of science”.²⁹ The symposium didn't even pretend to be objective or even-handed – its purpose, as was laid out clearly in an in-house newsletter, was “to elucidate inaccuracies in a recent high-profile JAMA article which claimed that being overweight leads to reduced mortality”.³⁰ According to the in-house PSPH coverage: “Each panelist presented a clear, compelling case as to why the general public should not rely on these flawed study findings, giving attendees numerous reasons to question the validity of the study”.

In an interview with the BBC³¹ Professor 1 announced, regarding our 2013 review and meta-analysis, “This is an even greater pile of rubbish” than our study in 2005. In a radio interview on NPR,³² Professor 1 again called our 2013 article “rubbish” (which he described elsewhere as a polite term for the word he really wanted to use) and said that no one should even read it. Hearing him say this aloud made a bad impression on several listeners who wrote to me about it. One woman wrote that Professor 1 sounded like a “bully.” His behavior was criticized by the editors of the scientific journal *Nature*.^{33,34}

Although much of the furor has died down, the attacks have continued. For example, in a commentary³⁵ in 2014 about dietary intakes and a review article³⁶ in 2015 about dietary intakes, Professors 1 and 2 included gratuitous comments about how misleading and contrary our meta-analysis findings were and cited our meta-analysis as an example of confusing and dangerous conclusions. Neither of these articles about dietary intake had anything to do with our meta-analysis or with obesity and mortality. Professor 2 organized a group to publish his own “Flegal is wrong” paper in the *Lancet* in 2016,³⁷ itself with questionable methods and demonstrable flaws.^{38–40}

The initial intent of these attacks seemed to be to discredit our work completely. They employed denigrating and insulting remarks (“rubbish,” “ludicrous,” “complete nonsense,” “fatally flawed and widely

discredited”) implying that our work was not worthy of serious consideration. There were also suggestions that we were unqualified, and my integrity and competence were questioned. Some attacks were surprisingly petty. At one point, Professor 1 posted in a discussion group regarding salt intake that JAMA had shown a track record of poor editorial judgment by publishing “Kathy Flegal's terrible analyses” on overweight and mortality. Similarly, again using a diminutive form of my name, Professor 1 told one reporter: “Kathy Flegal just doesn't get it”.⁴¹ It became clear that one of the things that critics found disturbing was that what they called the “lay media” or the “popular press” (which apparently extended to the *New York Times*, *Scientific American* and even *Nature*, a leading scientific journal) had reported on our findings as though they were worthy of serious discussion. One of the effects of the public insults may also have been to deter or intimidate other investigators. An anonymous researcher was quoted⁴² elsewhere as saying if character assassination is the price for publishing data that contradicts established beliefs, fewer academics will be willing to stick out their necks and offer up fresh thinking.

Our findings were simply findings, not arguments, explanations, recommendations or statements of personal opinions. However, some apparently had trouble grasping this, referring to our findings as “claims,” as though this was a matter of questionable assertions, not of data. For example, a 2017 Facebook post (since deleted) from a senior NCI scientist (and PSPH graduate) referred to our “dangerous (and persistent) claims.” Even though their work had little relevance to our estimates, the group from PSPH created a false narrative in which they and I were adversaries, taking sides and duking it out rhetorically. This myth even made its way into a lecture at NIH given by an eminent researcher, who stated incorrectly that some PSPH faculty and I were feuding and refused to appear on the same platform together. When I pointed out that this wasn't true, he graciously apologized and said it was something he had “heard.”

Both our 2005 article and our 2013 article were straightforward and transparent. Both are still cited frequently in the scientific literature. We presented our findings objectively and even-handedly, without cloaking them in any spin^{43–45} designed to obscure possibly inconvenient results (sometimes called “white hat bias”⁴⁵); indeed, this lack of spin may have been one of the reasons why our findings were considered to be surprising. Our articles drew only on data that were free and readily publicly available and could easily be checked. The controversy was something deliberately manufactured, and the attacks primarily consisted of repeated assertions of preconceived opinions. Nonetheless, these attacks were surprisingly effective. A small number of vocal critics succeeded in raising considerable doubt about our work while concealing major errors in the estimates that they preferred. One result was that unlike other researchers who had published articles on the same topic, we ourselves were sometimes treated as though we were advocates, not scientists striving to be objective.

At first, I was startled, but eventually I came to expect partisan attacks masquerading as scientific concerns. I had expected some modest interest in our findings, pursued through normal channels of scientific discussion. I had not expected an aggressive campaign that included insults, errors, misinformation, behind-the-scenes gossip and maneuvers, social media posts and even complaints to my employer – many more instances than I have space to describe here. It seemed that some felt that our work should be judged not on its merits but rather on whether its findings supported the goals and objectives of the interlocutors. I saw first-hand the antagonism that can be provoked by inconvenient scientific findings.

Guidelines and recommendations should be based on objective and unbiased data. Development of public health policy and clinical recommendations is complex and needs to be evidence-based rather than belief-based.^{46–50} This can be challenging when a hot-button topic is involved. Scientific findings should be evaluated on their merits, not on the basis of whether they fit a desired narrative.

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