

LETTER TO THE EDITOR OPEN ACCESS

# Response to “Stopping Spin at the Source: Learning From an Example in Obesity”

Huasheng Xiang<sup>1</sup> | Louis Goffe<sup>2</sup> | Viviana Albani<sup>3</sup> | Nasima Akhter<sup>4</sup>  | Amelia A. Lake<sup>4</sup>  | Heather Brown<sup>1</sup> 

<sup>1</sup>Division of Health Research, Lancaster University, Lancaster, UK | <sup>2</sup>Health Determinants Research Collaborative (HDRC) Gateshead, Fuse Centre for Translational Research in Public Health, Gateshead, UK | <sup>3</sup>Population Health Sciences Institute, Fuse Centre for Translational Research in Public Health, Newcastle University, Newcastle Upon Tyne, UK | <sup>4</sup>Fuse Centre for Translational Research in Public Health, Teesside University, Middlesbrough, UK

**Correspondence:** Heather Brown ([h.w.brown@lancaster.ac.uk](mailto:h.w.brown@lancaster.ac.uk))

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*“There is only one problem—confidence, and how to establish it; public assurance, and how to create it—in a perhaps long interim when scientific doubts must remain. And, most important, how to free millions of Americans from the guilty fear that is going to arise deep in their biological depths every time they light a cigarette.”*

Hill & Knowlton [industry Public Relations firm]  
(1953) [1].

The authors of the Letter to the Editor “Stopping Spin at the Source: Learning from an Example in Obesity” (hereafter the authors of the Letter to the Editor) start their letter with an allegation of “spin.” This insinuation, made without providing any evidence, suggests that our research findings and communications are driven by an agenda (an allegation which if founded would then extend beyond the authors of our paper to the editors of *Obesity*, who at the time of publication included one of the authors of the Letter to the Editor as Editor-in-Chief of the journal, the peer reviewers, and by extension the academic community as a whole). We are bound to observe that the authors of the Letter to the Editor’s conflict of interest statement outlining the “grant, contract, equity, gift, travel, or consulting fee” is compressed as a list yet still runs to 283 words, detailing a range of food industry organizations with a history of lobbying to directly influence government policy around food and the food environment [2–5]. It is impossible for us to establish their relationship with each entity within an extensive list of industry associations. Note that our conflict of interest statement amounts to four words: no conflict of interest.

As authors we reject the notion that the communication of this research constitutes spin. This is a peer reviewed article describing a quasi-experimental evaluation of a real-world policy implementation. We transparently presented our study objectives, outcomes, and findings (including uncertainty levels and limitations). We stand by the findings of this paper and reiterate that we have no commercial, academic, or other interests relevant to an evaluation of this policy.

The tobacco, alcohol, food, and agriculture industries use “Research Washing” such as funding research and/or academic positions to influence scientific principles and discourse [6–8]. By creating links with researchers and scientists, these industries can make the claim that the evidence they disseminate to the public regarding their products is evidence-based. There is an increasing body of evidence detailing how the food industry is shaping principles of scientific integrity in nutrition science [9]. Two of the authors of the Letter to the Editor who receive grant, contract, equity, gift, travel, or consulting fees from the food industry have a history of arguing against policy to reduce obesity rates [10, 11].

The foundation of the criticisms against our paper made by the authors of the Letter to the Editor incorrectly makes the claim that the **second** objective: *to explore whether the policy is more effective in areas of higher deprivation that previously had a higher concentration of outlets, leading to a reduction in inequalities in childhood OWOB* is a **secondary** objective. This is categorically not the case. The aim of the paper as clearly stated in the abstract is: *to investigate whether this policy led to any*

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reductions in childhood overweight and obesity prevalence and the inequalities in these outcomes. Looking at the overall impact (population level) and inequalities in outcomes by deprivation level (subgroup analysis) has equal importance in providing evidence on the potential effectiveness of the policy. Obesity is complex and the food environment is one component that may influence the likelihood of a person living with obesity. Since the composition of the food environment varies, understanding what may work, where, provides important contextual insights. The effectiveness of policy can vary based on deprivation levels [12] or population characteristics [13]. Thus, subgroup analysis by deprivation level is useful to guide policies to reduce socioeconomic gradients of poor health. Reducing health inequalities is a policy objective in the United Kingdom. This policy objective reflects the principles of proportionate universalism guiding resource allocation and service delivery proportionate to the degree of need [14]. This clearly justifies the rationale behind examining population-level and subgroup (by deprivation level) analyses. The authors of the Letter to the Editor misinterpreted the deprivation analysis as a secondary outcome and evidence of spin. Both objectives declared in our paper are grounded by theoretical underpinning and are equally important to the overall aim of the research.

Building on the arguments we made in the preceding paragraph, another concern of the authors of the Letter to the Editor claims: “Further, seeing an association only in a middle quantile-defined category implies a non-monotonic relationship in the relation between the association of fast-food policies with area deprivation; that is, the results would require special explanation as to why only a middle level of deprivation could result in an effect of policy.” In Table 1 in our paper, we clearly detailed that fast food restaurants are not equally distributed across deprivation quintiles. There is evidence that fast food outlets in the United Kingdom are concentrated in more deprived areas [15–17]. Thus, one would not expect to see a monotonic relationship. The fact that we found a significant difference in childhood overweight and obesity prevalence compared to the control group in areas with the highest density of outlets pre-policy (in quintiles 2 and 3) is not surprising. This finding is consistent with the data shown in Table 1.

The authors of the Letter to the Editor also claim that the broader science communication from the research overstated the findings. In the Commentary released by *Obesity* [18], there was no suggestion that the results show the laws were effective. The following are examples taken directly from the Commentary related to the results of our paper.

Despite targeting the whole population, the study did not show effectiveness of these planning laws in reducing childhood OWOB rates more generally across the local authority compared with comparison areas.

Most importantly, the study points to the need for these laws to be part of a systems approach to obesity prevention. No single law will be enough to address our current obesogenic environments.

Future research examining the effectiveness of using planning laws to reduce existing inequalities in obesity and poor diet would benefit from investigating other spillovers.

Therefore, this comment from the authors of the Letter to the Editor is misleading and does not accurately reflect the content or communication of our paper. This could be seen as spin by the authors of the Letter to the Editor, the academic practice they are purporting to expose. Throughout our paper we fully acknowledge the limitations of quasi-experimental approaches to estimating the effectiveness of policy, always couching findings with “may” or “could” to highlight uncertainty. This cautiousness with interpretation of the results is also reflected in the university press release [19].

The public facing criticism highlighted by the authors of the Letter to the Editor [20] contains a quote from the lead author with the same claims as raised in his Letter to the Editor. Therefore, this constitutes repetition rather than corroboration and does not suggest wider evidence of criticism beyond the author team of the Letter of the Editors. As we describe here in our rebuttal, these claims are unsubstantiated and reflect a misinterpretation of the paper.

To us, this Letter to the Editor raised bigger questions—what should be the role of industry in academic research related to policy to promote a healthy environment? Should academic journals be a mouthpiece for these “manufactured controversies” [21] to help cultivate suspicion of possible tools to help people live their best lives? To the reader, academic, and policy community, we leave this to you to decide.

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#### Conflicts of Interest

The authors declare no conflicts of interest.

#### Data Availability Statement

Data sharing is not applicable to this article as no datasets were generated or analyzed during the current study.

Huasheng Xiang  
Louis Goffe  
Viviana Albani  
Nasima Akhter  
Amelia A. Lake  
Heather Brown

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