among the scholars who have worked on this evidence. The two points of dispute, which do relate to our article and to my own research concern the nineteenth-century urban mortality evidence and the application of the social capital concept.

On the mortality evidence Razzell and Spence’s critique seems entirely misconceived or mis-stated. They conclude by stating that, ‘Overall, the evidence does not indicate that mortality increased in large towns in the 1841–71, contradicting a major part of Szreter’s thesis.’ This is not at all my thesis, nor does it correspond with the trends I published with Graham Mooney.3 This research indicates that although provincial, industrializing towns and cities probably suffered worse mortality than the national average at the beginning of the 19th century, they also endured a very serious further deterioration in their mortality conditions in the 1830s, which continued unabated during the 1840s. As Razzell and Spence should be aware, these findings (including those for the period 1800–30) are not based solely on data for Glasgow and Bristol, as they misleadingly imply in their letter. (They do this by choosing to talk only of ‘large industrial cities’, which also enables them to include London, whereas my research and interpretation relates to provincial, industrializing towns and cities of both medium and large size, while it specifically excludes London). The 1850s saw an improvement, but not to a level of mortality above that which had prevailed before the catastrophe of the 1830s. The 1860s saw no further improvement, but the level of life expectancy achieved in these cities during the 1870s at last lifted them above the level of the 1820s and thereafter they continued to improve further above this level in each succeeding decade.

On social capital Razzell and Spence state, ‘It implies a degree of social consensus and cohesion, whereas many medical and public health measures were implemented not as a result of a humanitarian social concern, but were due to sectional and social class interests.’ I can only reiterate that, to the contrary, our use of a social capital analysis precisely indicted the problem of bonding social capital as a key part of the reason for urban deterioration becoming so serious in the 1830s and 1840s; and argued that a politically mediated transformation in social relations, including in the forms of social capital (more bridging and linking) was necessary for cities like Birmingham to find the collective resources to begin the long haul of continuously improving their entire environments from the 1870s onwards. Only this larger ambition, and not sectionally motivated preventive measures targeted at specific problems, such as the smallpox legislation of the 1850s which Razzell and Spence mention, could bring about the substantial all-round continuous and cumulative improvements in the mortality of industrial towns and cities which occurred after 1870.

References

Infant sleeping position and sudden infant death syndrome: a systematic review
From WARREN G GUNTHEROTH and PHILIP S SPIERS

The systematic review and meta-analysis by Gilbert et al.1 with chronological, graphic presentation of odds ratio with confidence intervals, provides an excellent summary of the literature that finally leads to the acceptance of the supine sleeping position for infants. But the conclusion that 60 000 infants could have been saved worldwide if only action had been taken in the early 1970s seems an overstatement, considering the reality of the way in which public health decisions are made. By 1970 only two papers had been published,2,3 and neither of them emphasized their statistics on sleep position nor did they make any recommendations to stop using the prone position. Therefore, it does not seem surprising that there was a gap of 16 years before Beal’s paper4 was published in 1986 containing a clear recommendation against prone sleeping.

In the US, our failure to recognize the strength of the evidence from the British and Dutch back-to-sleep campaigns was unfortunate, to say the least. Not until 1992 did the first medical publication concerning sudden infant death syndrome (SIDS) and sleep position appear. We published, in JAMA,5 our analysis of reports from seven countries, using Hill’s criteria,6 and urged a change to the supine sleeping position. Marion Willinger, the Head of the SIDS programmes for the US Public Health Service, appeared on US television one week later and announced that she did not think these statistics and recommendation should apply to American infants!

One and a half years later, we published7 follow-up statistics on US SIDS rates before and after the recommendations by the American Academy of Pediatrics.8 Willinger then convened a conference (cited by Gilbert et al. as reference 77). By the time a public announcement was made in support of the recommendation, 2 years had passed. Since the rate of SIDS in the US dropped thereafter by ~50%, nearly 5000 infant deaths from SIDS could have been avoided.

References

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Authors’ response to letter from Guntheroth and Spiers

From RUTH GILBERT¹ and GEORGIA SALANTI²

We agree with Guntheroth and Spiers¹ that delays in the 1980s and 90s by national policy bodies in implementing ‘back to sleep’ advice caused thousands of avoidable deaths worldwide. However, our study highlights the much greater loss of life owing to the flawed introduction of advice favouring front sleeping in the early 1970s. Introduction of changes in practice should be based on the evidence by which benefits outweigh harms. Yet when UK clinicians started to recommend front sleeping to prevent cot death, the two studies that addressed this question at the time showed unequivocally no evidence of benefit.²,³ This point was made in the report of the UK study.² In addition, when these two studies were put together, there was statistically significant evidence of harm.⁴ No evidence of benefit combined with evidence of serious harm should cause a ‘pause for thought’ even for those that dispute the findings. Consequently, we do find it surprising that it took 16 years for further studies to be published and that it was investigators outside the orbit of the UK and US research communities who performed these studies.⁵–⁷ We argue that had simple methods of reviewing and combining what was already known from research been used in 1970 60 000 deaths would have been avoided. This is an underestimate as we left out cot deaths classified as other causes before SIDS was widely used for death registration and deaths before 1970.

During the 1990s, evidence-based health care and national technology assessment bodies were established in many countries to challenge the introduction of interventions that were not based on evidence of overall benefit. As our study shows, waiting for evidence of harm can take years for avoidable deaths to accumulate to a level sufficient to overturn established practice. We also showed that evidence of harm might be attenuated when uptake of health advice is high, possibly owing to a ‘healthy adopter’ phenomenon.

We recognize that medical attitudes to science in the 1960s and 1970s meant that patho-physiological reasoning took precedence over clinical research findings that showed what actually happened when babies were placed on the front. The adoption of harmful advice was a collective failure of professional culture and would have been hard for any individual clinician or researcher to avert. The point of our report is not to attribute blame, but to illustrate the dangers of public health recommendations that are not based on sound evidence from systematic reviews and careful weighing of benefits and harms.

References


