Suicide, the theme of this issue, remains one of the last taboos in many societies, particularly those with strong religious roots. In this issue, Spoerri et al. describe the associations between religion and suicide in Switzerland, echoing Durkheim’s study published over a century ago that showed suicide rates were higher among Protestants than Catholics and Jews. Spoerri et al. also found lower rates of suicide in people who affirmed a religious belief than those who did not. Durkheim attributed the effect to the social organization of specific religions rather than religion itself. His work on suicide covered a range of other factors—gender, civil status, education, geography and ambient temperature. For most of these associations, he was able to attribute a social explanation. Bhui, commenting on this article prefers a more individualistic explanation and states ‘These data suggest, whether one believes or not in a particular religious teaching, that religious people do think differently about the meaning of life and tend to be more reluctant to consider suicide (assisted or not) as an option.’ I prefer the wider explanatory power of the ‘social integration’ theory more than the ‘meaning of life’ which is really only plausible for the effect of religion vs no religion and is not a good explanation for the differences between religions.

Secular trends in suicide provide clues to causes and the 150 year time frame examined by Thomas and Gunnell in this issue shows the peaks associated with the great depression and the troughs during the two world wars. While these troughs may seem paradoxical—losses of property and bereavements might be expected to increase suicide. Explanations highlighted by Thomas and Gunnell include the greater cohesiveness of wartime society—an explanation also provided by Durkheim—and competing causes of mortality.

Epidemiological studies provide some understanding of causes but individual case studies are perhaps more revealing. Consider Sylvia Plath, an American poet who committed suicide in 1963 aged 30 years by gassing herself. Her choice of method was among the most favoured in the 1960s—she died at the height of the suicide by gassing epidemic (see Figure 5 in Thomas and Gunnell’s paper). Accounts of her death suggest that she hoped to be found, leaving a note saying ‘call Dr Horder’, her general practitioner but he considered the preparations she had made indicated her suicidal intent. Others considered the causes of her suicide were due to long-standing depression and to infidelity by and separation from her husband, Ted Hughes, also a poet. Tragically, her son also committed suicide in 2009 by hanging himself—he suffered with depression too. Ted Hughes was devastated by her death and considered his life was now over. He never published the poem he had attempted to write about her death which was only found in October this year—’Last Letter’ which ends ‘Then a voice like a selected weapon/or a carefully measured injection/coolly delivered its four words deep into my ear/your wife is dead.’

Suicide is a miserable subject and perhaps the assembling of other people’s data into meta-analyses is almost equally miserable. The extraordinary rise of meta-analysis as a basic technique in epidemiology has apparently gone unnoticed by teaching institutions in many places. My own renowned institution has yet to get around to including the topic in its Masters courses but this year did hold its first 1-week course. Remarkably, very few US (Bloomberg School of Public Health is a notable exception) universities offer courses in systematic reviews as part of their mainstream training in epidemiology or public health. So why don’t we teach skills in systematic reviews and meta-analysis to everyone coming through our training programmes?

In this issue, we have a surfeit of applications of meta-analysis. Perhaps one of the reasons for the unpopularity of systematic reviews is that once all the relevant evidence is assembled, good stories are shown to be false. War-related stress remains ever present and the evidence is conflicting on effects on death rates. In this issue, Roelfs et al. systematically reviewed the literature and found 30 studies in 8 countries comprising 9 million people followed for a median of 14 years. We are not told about the number of deaths observed, which is a pity as the absolute
death rates are always important in determining public health importance. War-related stress was not measured in any direct way in any of the studies but is inferred by being a combatant, a prisoner of war, a civilian exposed to war and so on. No strong evidence of any effect of war stress was found. Pooling does hide potentially interesting differences: Abla Sibai’s study from the Lebanon used a more nuanced marker of stress by asking about loss of property and loss of loved ones among civilians and found that men had increased death rates if they had lost property but women were adversely affected by loss of loved ones. Men really are from Mars.

We routinely teach doctors and nurses to ask sensitive questions but getting information on sexual behaviour in large-scale surveys is more difficult and the typical face-to-face interview may yield answers that are socially acceptable. The systematic review and meta-analysis conducted by Phillips et al. compared face-to-face with innovative methods of interviewing (audio-assisted computer interviews, informal confidential voting interviews, polling booth surveys) and found that for questions ‘ever had sex’, ‘non-condom use’ and ‘number of partners’, there was marked heterogeneity of the effect of innovative methods across the 15 studies included. There was no heterogeneity for ‘forced sex’, which was more often reported by innovative methods. Exploration of the causes of heterogeneity included study participant characteristics, context and study design but did not include analyses by the type of innovative method or by study quality which might well explain some of the heterogeneity. Given the unexplained heterogeneity, no clear answer is forthcoming. It does indicate that for many sexual behaviour questions people are prepared to give answers and using innovative methods is not a complete solution.

A further application of meta-analysis in this issue is the examination of non-inferiority trials. Whenever new drugs need to be evaluated for widespread use, the issue of whether they are more effective than standard treatment has to be determined. The pharmaceutical industry has a duty to deliver profits to its shareholders as well as better health outcomes to patients but in the last decade few new ‘block-buster’ drugs have been discovered to rival the statins or sildenafil which still are the top profit-making drugs in the world. Pfizer instead drugs currently coming to market belong to the ‘me-too’ variety and the main question asked is not are they any better than other treatments but are they no worse than competitor drugs—the non-inferiority trial. As these are accepted as evidence by drug regulatory authorities, they have become an essential part of the industries’ research investment. Allegations have been made that non-inferiority trials are deliberately designed to hide that a new drug performs worse than standard treatment. Soonawala et al. have conducted a non-systematic review of 170 non-inferiority trials gleaned only from PubMed and report a pooled relative risk of 1.002 (95% confidence interval 0.996–1.008). The authors conclude that this null finding indicates that new treatments do not seem to be less effective than standard treatments and this is reassuring (no conflicts of interest declared). So can we relax and trust the validity of these trials? Our commentators, Alessandro Liberati and Roberto D’Amico think not. The debate continues.

The Delhi monsoon has given way to the cold season (it is now only 29°C during the day and 17°C at night and people have scarves wrapped around their heads to keep out the cold) and my cough and wheezing have started again. Hebert et al. have no comfort for me, reporting a graded and increased risk of all cause mortality by FEV1 among over 110 000 adults living in Mumbai. Effects are higher in women and are independent of smoking and suggest that air pollution may be playing a role. After almost a decade of cleaner air from use of compressed natural gas in Delhi’s taxis, buses and auto-rickshaws, the levels of breathable particulate matter (PM10) have now increased to three times the levels in 2005, which is probably due to lack of tax on diesel fuels (farmers need a break) which has promoted a surge in sales of private diesel engine cars—that and the Diwali festival of light which launched millions of tons of fireworks in the sky has left us all with a dusty hangover and the taste of sulphur—and a warm religious glow (for some of us) from a festival well-celebrated which will surely ward of depressive feelings as the weather gets colder.

References

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