Commentary on *Dowling et al.* (2016): Is it time to stop conducting problem gambling prevalence studies?

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The dual-frame survey conducted by Dowling and colleagues should cause gambling researchers to re-evaluate the scientific value of routinely conducting problem gambling prevalence studies. A better use of resources would support research designed to reduce the incidence of gambling-related harm or reduce its duration rather than perpetuate its ongoing mismeasurement.

Many jurisdictions conduct problem gambling prevalence studies routinely. In Australia alone, more than 40 separate studies have been undertaken since 1991 with the specific purpose of measuring the population prevalence of problem gambling, interviewing a combined total of more than 275 000 individuals at great public expense. Australia is not unique in this regard, with one report documenting more than 200 such studies world-wide [1]. Clearly this is a massive, collective effort in what remains a relatively small research field. Dowling et al. [2] contribute to this effort by attempting to improve the accuracy of telephone surveys and hence establish 'best practice in population gambling research'.

The accuracy of prevalence studies is important, because they are used to achieve three epidemiological objectives [3]:

1. to assess the burden of disease in a population and to assess the need for health services;
2. to compare the prevalence of disease in different populations; and
3. to examine trends in disease prevalence or severity over time.

On the face of it, then, the efforts of Dowling et al. appear to serve important, pragmatic ends. However, closer consideration of their contribution raises questions about the theoretical and practical utility of problem gambling prevalence studies in general.

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On objective 1, in terms of the 'burden of disease', problem gambling is itself a conceptually ill-defined category [4]. Measurement of its prevalence is highly dependent upon a host of methodological variations (e.g. [5, 6]). As Dowling et al. illustrate, a dual mobile–landline telephone sample frame may increase the measured prevalence rate by a factor of 4. In addition, the somewhat arbitrary nature of the cut-points used to categorize 'cases' means that the accuracy of counts of problem gamblers becomes questionable (e.g. [7]). The issue of respondents concealing the extent of their gambling problems compounds these difficulties. If fewer than 30% of problem gamblers answer questions about their symptoms in health surveys honestly [8], then 'the number' produced by prevalence studies should be treated with extreme scepticism.

On point 2, the comparison of problem gambling prevalence between populations at the jurisdictional level has been hampered by the inconsistent design of survey instruments. Prevalence studies have been most useful in establishing differential risk among demographic subpopulations contacted within a single study (e.g. [9]). However, the scientific usefulness of new demographic risk factor studies must surely be close to exhaustion.

The potentially redeeming objective of routine prevalence studies is thus point 3, the ability to make comparisons over time within a single jurisdiction. The contribution of Dowling et al. [2] is to demonstrate that this potential may be a false hope, even where consistent survey instruments have been used. Dowling and colleagues show that what was assumed previously to be a relatively stable sample frame (those adults who answer a landline telephone and agree to participate in research) has become decreasingly representative of the general population over time. Declining landline usage is not the only trend that may lead to spurious trends in comparisons between prevalence rates. Typical response rates to prevalence studies have plummeted since prevalence research began, from approximately 70% in the mid-1990s to an average of approximately 20% today [1, 2, 10]. In addition, population ageing is rarely adjusted for, despite the existence of uncontroversial methods such as rate standardization. Consequently, instead of measuring change in prevalence over time, it appears likely that much problem gambling research has been comparing prevalence rates among systematically differing population subsets. The problem is not just changing survey instruments, but also changing contactable populations.

This conclusion should cause us to re-evaluate the emerging consensus regarding a supposed slight downward trend in problem gambling prevalence estimates (e.g. [12-14]). This downward trajectory may be artefactual, a spurious trend resulting from temporal confounding with the shift to mobile telephones, ever-diminishing response rates and demographic change.

More fundamentally, Dowling and colleagues' results should provide a provocation for gambling researchers and their funders to take stock and ask: is the routine conduct of problem gambling prevalence surveys worthwhile? If prevalence studies fail to meet any of the three epidemiological objectives listed above, it becomes increasingly difficult to justify their continued support. If we are correct, and problem gambling prevalence studies are no longer scientifically useful, then we need to explain their continued popularity among some researchers and funders. One explanation is that prevalence studies now function primarily as a device for the political legitimation of the gambling industries and the governments that support them [15, 16]. In this light, we hope that Dowling et al.'s contribution might prompt the reconsideration of routine problem

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gambling prevalence studies, with a view to their eventual abandonment. Resources should be redirected towards research that might prevent the incidence of gambling-related harms, rather than their ongoing mismeasurement.

Declaration of interests

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