Methodological factors affecting estimates of the prevalence of gambling harm in the United Kingdom: A multi-survey study

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Executive Summary

In 2019, a YouGov survey found substantially higher rates of gambling harm than had previously been estimated using the 2018 Health Survey for England using the Problem Gambling Severity Index (PGSI). This discrepancy has led to concerns about what the true level of gambling harm is in the adult population of Great Britain.

In this report we investigate how methodological differences between surveys affects the accuracy of estimates of gambling harm by eight surveys using a broadly consistent set of questions but different sampling and data collection methodologies.

The primary variable of comparison between surveys is the Problem Gambling Severity Index (PGSI) (Orford et al, 2010). In order to maximise the sample sizes for our comparisons, we focus primarily on the proportion with a score of 1 or above on the PGSI.

The key finding of the research is that surveys using predominantly or entirely online self-completion produce consistently higher estimates of gambling harm compared to surveys using a paper self-completion questionnaire as part of a face-to-face interview.

We have been able to rule out a number of potential causes of this difference, namely: true change over time, differences in population coverage, and differential measurement error.

This leads us to conclude that the primary cause of the difference in estimates of gambling harm is selection bias in the online surveys. This is when survey estimates differ systematically from true population values due to unrepresentative samples. Selection bias is a particular risk for the online surveys because they either have low response rates, or use non-probability sampling.

Comparisons of estimates revealed a pattern of systematic differences between surveys: the online surveys contained gamblers who were more likely to gamble online and to gamble frequently, while the health surveys contained higher proportions of in person gamblers and less frequent gamblers.

These differences in sample composition seem likely to underpin the differences in rates of gambling harm. This is because the online surveys skew toward people who are more online and 'tech savvy' and these sorts of people are more likely to be online and frequent gamblers. Disproportionately high numbers of online and frequent gamblers mean the online surveys over-estimate gambling harm because online and frequent gambling are independently associated with a higher risk of gambling harm.

This conclusion combined with other considerations leads us to make the following recommendations:

1. Given the high cost of face-to-face probability surveys and the limits this imposes on sample size and the frequency of surveying, measurement of gambling prevalence and harm should move to online surveying. This will enable more fine-grained sub-group analysis and more regular provision of key estimates than is currently possible.
2. The move to online interviewing should be combined with a programme of methodological testing and development focusing in particular on strategies for mitigating selection bias and on improving the accuracy of measurement of gambling harm.
3. In person surveying should not be abandoned completely, regular but infrequent surveys using probability sampling and face-to-face interviewing should continue in order to calibrate the estimates from online surveys and to serve as a benchmark by which methodological innovations and developments can be evaluated.

Introduction

Since 2012, official statistics on the prevalence of gambling and gambling harm in Great Britain have been collected using the combined national health surveys for England and Scotland, and a bespoke survey in Wales. These surveys use what are considered to be 'gold standard' methodologies of random sampling and in person interviewing. They have estimated comparatively low rates of gambling harm in the general adult population. For example, the most recent survey covering Great Britain in 2016 estimated the prevalence of problem gamblers to be 0.7% and the rate of adults at risk of experiencing gambling harms to be 4.2%. Similar rates of 0.4% and 3.9% were estimated in the 2018 Health Survey for England.

In 2019, GambleAware commissioned YouGov to carry out a survey of reported demand for treatment and support amongst people experiencing gambling harm. This survey had a quite different methodological approach to the health surveys, using non-probability sampling and online self-completion of questionnaires. Although the objective of the YouGov Treatment and Support survey was not to estimate national rates of gambling prevalence and harm, this was a necessary first step in the survey to identify members of the target population to whom questions on demand for treatment and support were administered. The survey found substantially higher rates of gambling harm, with 2.7% identified as 'problem gamblers' and 13.2% at risk of experiencing gambling harms, more than threefold higher than the health survey had estimated for England less than a year previously.

Given the high-profile nature and policy importance of the level of gambling harm in the general population, GambleAware commissioned Professor Patrick Sturgis to produce a report in March 2020 to assess which of the two surveys was likely to be the more accurate. That report concluded that it was not possible to answer this question in a definitive way empirically but, on the balance of probabilities, "the true value probably lies closer to the combined health surveys than to the YouGov survey." (Sturgis, 2020 p.3).

This conclusion was primarily due to the health surveys' use of random sampling. The reason it was not possible to determine empirically which survey is the more accurate was

because, with only two surveys and each one using quite different methodological approaches, there was no satisfactory means of determining how differences in methodology were related to variation in estimates of gambling harm.

In order to address this problem and to provide better insight on which methodological approaches yield the most accurate data on gambling behaviour and gambling harm, GambleAware commissioned this follow-up study and report. A core part of the research involved conducting new surveys, with each conducting fieldwork at approximately the same time and containing the same set of questions on gambling participation and gambling harm. We compare these new surveys with the most recently collected YouGov and Health Surveys for England in order to better understand how different methodological design features affect the accuracy of survey measures of gambling harm.

Methodology

We compare estimates of gambling behaviour and gambling harm across eight surveys using a broadly consistent set of questions but different sampling and data collection methodologies. These are the 2016 and 2018 rounds of the Health Survey for England, the 2019 and 2020 GambleAware Treatment and Support surveys carried out by YouGov, and specially commissioned surveys for the purposes of this study conducted in November and December 2020 by Yonder, NatCen, and Kantar Public. Additionally, Ipsos-MORI have kindly provided us with data from a survey that they collected for their own purposes in January 2021.

The key variable of comparison between surveys is the Problem Gambling Severity Index (PGSI) (Orford et al, 2010). The PGSI is based on respondents' answers to 9 questions about their gambling, each with 4 response alternatives: 0= never, 1=sometimes, 2=most of the time, and 3=almost always. The total PGSI score is the sum of the scores for the individual items. The total score is also recoded into four categories indicating 'non-gambler', 'low-risk', 'moderate-risk', and 'problem gambling' for scores of 0, 1 to 2, 3 to 7, and 8 or above, respectively. In order to maximise the sample sizes for our comparisons, we focus primarily in this report on the proportion with a score of 1 or above on the PGSI, which hereafter we refer to as PGSI+1.

For the gambling modules in each survey, respondents were first asked a set of questions about whether they had participated in a range of gambling activities during the previous 12 months. Those who reported no gambling activities were not administered the PGSI questions and are given a score of zero on the index and the category variables derived from it.

Respondents were also asked about how frequently they gamble and their motivations for gambling. For the online surveys, respondents were additionally asked whether they had participated in the same gambling activities during the previous 4 weeks. The question wordings and response alternatives for the gambling questions are provided in Appendix 3.

While most of the surveys cover the whole of the UK or Great Britain, we focus in this report on England only. This is because health survey estimates covering Great Britain are not available for 2018 and, while a combined GB data set was produced in 2016 (Conolly et al 2018), these micro-data are not publicly available. Like-for-like comparisons between surveys can therefore only be made for England. Estimates covering GB and the UK are included in Annex 1 for the surveys for which such estimates are available. The key design features of the eight surveys are described in detail in Appendix 1 and summarised in Table 1 below.

Table 1 Summary information on the sample designs of the eight surveys

Survey	Sample design	Mode	Sample size	Fieldwork	Age range	Response rate	Question order
HSE 2016	Probability sample, Postcode Address File (PAF) as the first-stage sampling frame, all adults in a household are interviewed, £10 unconditional incentive	Paper self-completion in face-to-face (f-t-f) interview	6691	Annual continuous	16+	55%	After mental health questions at end of f-t-f interview
HSE 2018	As for 2016 HSE	Paper self-completion in f-t-f interview	6927	Annual continuous	16+	54%	After mental health questions at end of f-t-f interview
Kantar	Probability, PAF, up to 2 adults, £5 conditional incentive	Online + phone	1795	24/11/20-13/12/20	16+	5%	First in questionnaire
Ipsos	Probability, PAF, up to 2 adults, £10 conditional incentive	Online		21/1/21-27/1/21	18+	4%	After politics, vaccination, views of local area
NatCen	Probability, PAF, 1 adult, £10 conditional incentive	Online + phone	2049	19/11/20-20/12/20	18+	14%	
YouGov 2019	Quota sample (with age, gender, ethnicity, social grade, and region as quota variables), incentive = points toward money	Online	10499	24/9/2019-13/10/2019	18+	N/A	First in questionnaire
YouGov 2020	Quota (age, gender, ethnicity, social grade, region), incentive = point towards money	Online	16401	19/11/20-11/12/20	18+	N/A	First in questionnaire
Yonder	Quota (age, gender, region, social grade), incentive = points towards money	Online	6944	18/11/20-29/11/20	18+	N/A	

Sample sizes for England only

Results

A full table of estimates of rates of gambling activities and gambling harm are provided in Table A1 in Appendix 2. Figure 1 below shows estimates of our key variable of interest: the proportion of the adult population in England with a PGSI score of 1 or above with 95% confidence intervals for each of the eight surveys.[1] The estimates for the two health surveys, at 3.9% and 4.1%, are substantially lower than the online surveys[2] which range from a low of 7.4% for the Ipsos-MORI survey to a high of 16% for the Yonder survey.

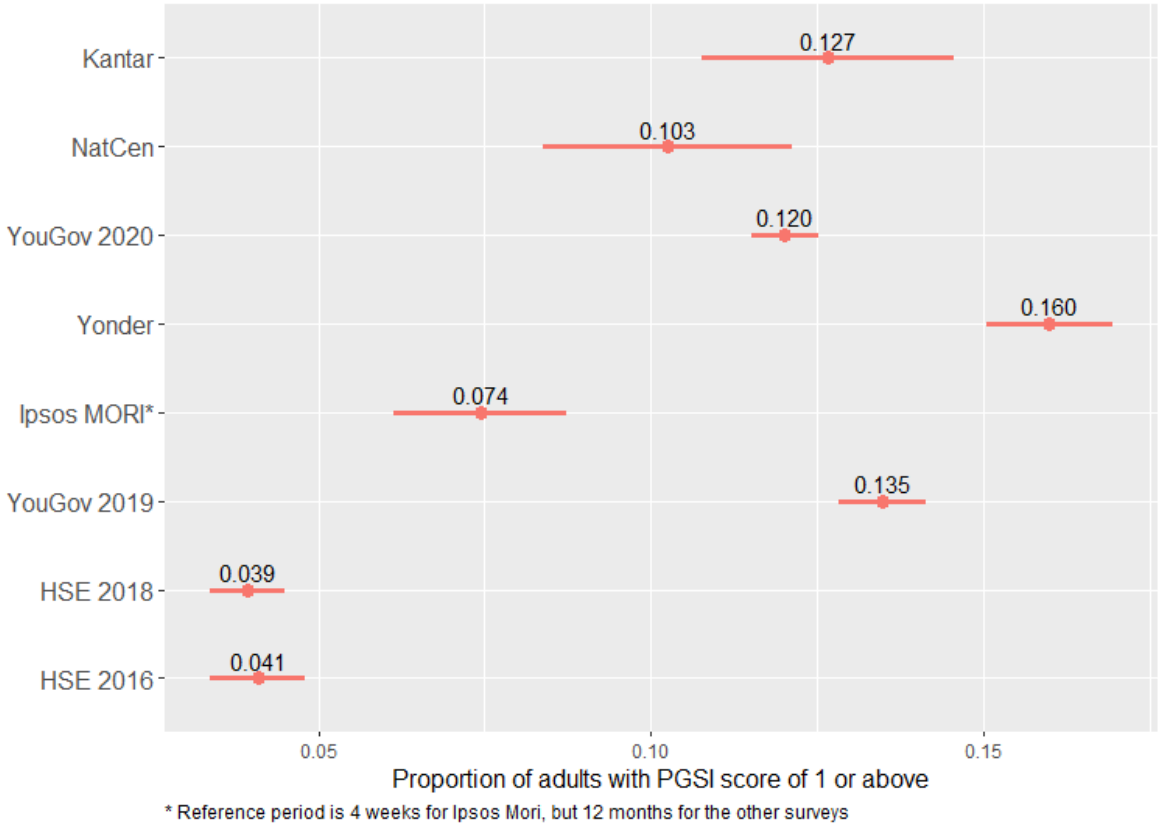

Figure 1 Estimates of proportion with PGSI+1 across surveys

[1] For the random probability surveys, confidence intervals are calculated using Taylor-series linearization to account for complex design features. For the non-probability samples, the same approach is used to account for the calibration weights and while this is technically not correct due to the non-random selection of population elements, it serves as a reasonable approximation.

[2] For simplicity, we refer to the surveys that used online self-completion as 'the online surveys' even though the Kantar and NatCen surveys used both online and telephone interviews.

It is important to note the Ipsos-MORI survey uses a reference period of 4 weeks for previous gambling behaviour as a filter to asking the PGSI questions, while all other surveys use a 12-month reference period. This is likely to reduce scores on the PGSI for the Ipsos-MORI survey compared to the other surveys. If we compare the proportion reporting any gambling over the previous 4 weeks (Figure 2), the Ipsos-MORI survey has the lowest rate, although it is not particularly different from the other surveys on this metric.

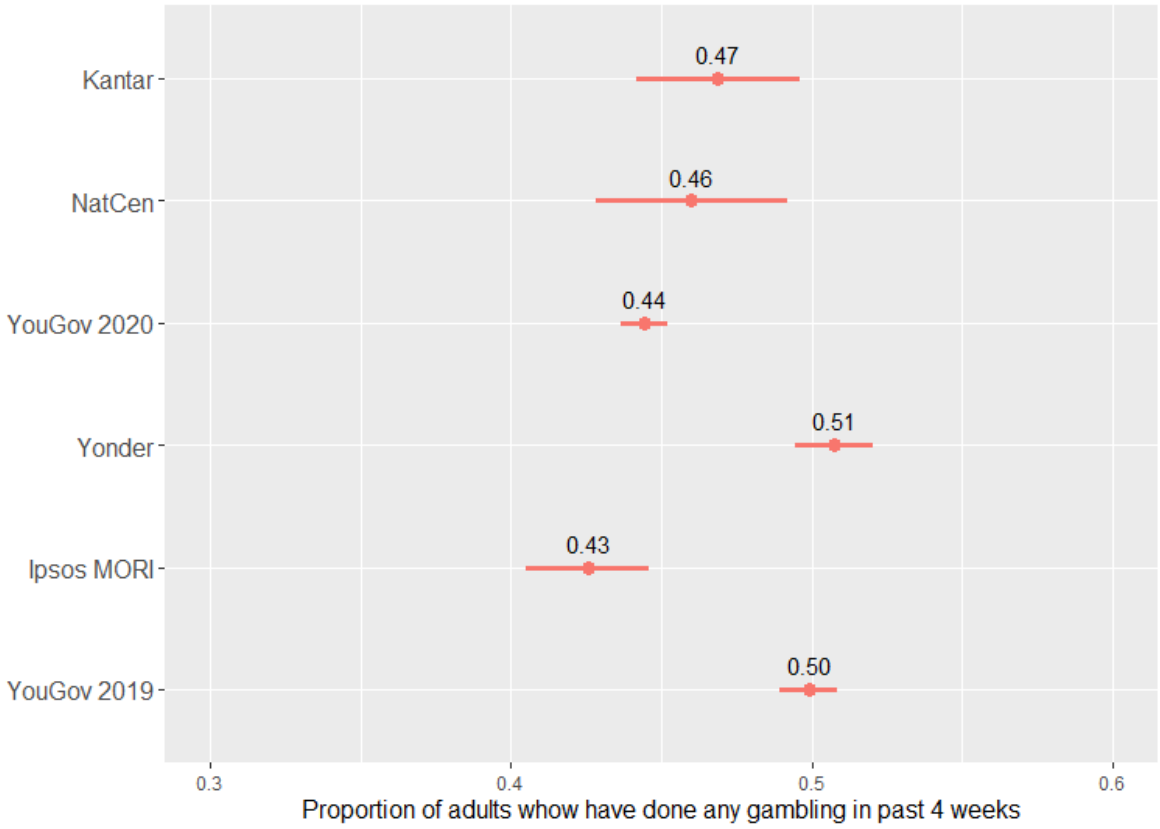

Figure 2 Estimates of proportion who have done any gambling over the previous 4 weeks

We can obtain an estimate of the likely effect of the shorter reference period on the PGSI+1 rate in the Ipsos-MORI survey by setting PGSI+1 to zero for respondents who reported no gambling during the previous 4 weeks for surveys that used both the 12 month and the 4-week reference period. Doing this for the NatCen, YouGov, and Kantar surveys reveals that the PGSI+1 rates are 12%, 18%, and 21% higher[3], respectively, for the 12-month compared to the 4-week reference period. It therefore seems reasonable to assume that, had the

[3] 9.6% v 10.7% for NatCen, 10.1% v 11.9% for YouGov and 10.2% v 12.4% for Kantar.

Ipsos-MORI survey also used a 12-month reference period, the PGSI+1 rate would be 1.5 to 2 percentage points higher, at approximately 9%, bringing it closer to the other online surveys.

Note also that the health surveys use a target population of adults aged 16 years and above while the online surveys, apart from Kantar (which also uses 16+), use 18 years and above. Given the small size of this group and the low incidence of PGSI+1 in the population, this difference will have little or no effect on the estimates for the general population, though any small difference that it might produce will be in the direction of *increasing* the PGSI+1 rate in the health surveys.

Potential causes of the differences in estimates between surveys

In this section we consider the evidence for a range of potential causes of the differences in estimates of gambling harm between surveys, focusing in particular on why the health surveys estimate harmful gambling to be so much lower than the online surveys. We structure this discussion according to the major sources of error set out in the Total Survey Error Framework (Groves, 2010), although we first consider whether some of the difference might be due to true change in harmful gambling in the population over time.

True change over time

One potential cause of at least some of the difference in estimates between surveys is true change in harmful gambling over time. After all, the primary purpose of monitoring surveys such as the Gambling Prevalence and Health Surveys is to detect actual change in gambling behaviour and there have been a number of changes in gambling policy and regulation in recent years that might plausibly have increased the frequency of harmful gambling (Wardle et al, 2021).

Additionally, the majority of the online surveys were conducted during the coronavirus pandemic which may have changed patterns of gambling behaviour as a result of people having more time on their hands and fewer opportunities for in person gambling activities. While we cannot rule out the possibility that there may have been some increase in the true rate of gambling harm in Great Britain between 2018 and 2020, this does not seem likely to

be a major contributory factor to the differences in estimates for two reasons. First, there was a threefold increase (from 4.2% to 13.2%) in the estimate of PGSI+1 between the 2018 HSE and the first Treatment and Support survey in 2019, an interval that seems too short to accommodate such a substantial increase and one which could not have been affected by the pandemic.

Second, YouGov carried out a follow-up survey during the first lockdown in May 2020 which found a statistically significant *decline* of ten percentage points in the frequency of gambling reported over the previous 4 weeks from October 2019 (Gunstone et al 2020). A separate study also found a reduction in gambling amongst sports betters in July 2020 (Wardle et al 2021b) It therefore seems unlikely that the increase in harmful gambling estimates from 2018 to 2020 is due to a real increase in this behaviour in the population but, rather, reflects differences in the designs and error properties of the surveys used to measure gambling harm, a possibility to which we turn next.

Coverage error

Coverage error is the discrepancy between the defined target population, such as all adults in Great Britain, and the population elements listed on the sampling frame. If some elements do not appear on the sampling frame, they have zero probability of being included in the sample. To the extent that the non-covered elements are different on the population characteristic of interest, survey estimates will be biased. In Sturgis (2020), it was noted that there are differences in the covered populations between the health surveys and the YouGov survey which might have caused some of the difference in estimates of gambling harm.

The sampling frame for the HSE is the Postcode Address File (PAF). PAF excludes people who do not have a residential address, such as homeless people and people who live in institutional addresses such as halls of residence, hospitals, prisons, and military barracks. The YouGov panel, on the other hand, potentially includes some members of these groups but excludes the offline population completely; this currently amounts to approximately

10% of adults in the UK.[4] However, with the addition of the new surveys, it is now possible to rule out coverage error as a cause of the difference in estimates of gambling harm because the Kantar, NatCen, and Ipsos-MORI probability panels also sample from PAF and so have the same coverage properties as the health surveys.

Sampling variability

All surveys are subject to sampling variability, the random variance in estimates that results from the fact that only one sample is realised from the many that could potentially have been drawn. It is therefore necessary to consider the possibility that the differences in estimates observed between surveys might be attributable to this source of error.

We can assess this by considering the extent of overlap between the 95% confidence intervals around each estimate. These intervals can be thought of as representing the likely range of plausible values for the relevant quantity in the population, given the sampling design of the survey – in only 5% of repeated samples with the same design would we expect the true value in the population to be outside this interval. Where the intervals do not overlap, we can be confident that the differences in estimates are not due to sampling variability. Figure 1 shows clearly that the confidence intervals for the health surveys do not overlap with any of the online surveys and we can therefore reject the possibility that the differences between them are due to sampling variability.

Of the online surveys, the Yonder estimate of PGSI+1 is significantly higher than for the other surveys, while the Ipsos-MORI estimate is significantly lower than the others, except for NatCen, which has a wide confidence interval due to its smaller sample size. As was noted earlier, the Ipsos-MORI estimate would likely be closer to 9% had a 12-month reference period been used for the filter to determine whether respondents are administered the PGSI.

NatCen, Kantar, and the 2020 YouGov survey are not significantly different from one another, although the 2020 YouGov estimate is significantly lower than the 2019 YouGov

[4] Our own analysis using the 2019 British Social Attitudes survey.

survey. This reflects the decline in gambling noted earlier that seems to have resulted from the coronavirus lockdown, which reduced the available opportunities for in person gambling. Table X1 in the Annex shows 95% confidence intervals for all univariate estimates, including proportions of people who have taken part in the different gambling activities.

Measurement error

It is possible that some of the variability in estimates of gambling harm between surveys derives from differences in the measurement properties of the survey instruments used. One possibility here is that the answers to the gambling questions might have been affected by the content of questions that preceded them in the questionnaire, so-called 'order effects' (Schuman and Presser, 1981).

The gambling questions in the health surveys followed questions addressing mental health and well-being, while for all but the Ipsos-MORI survey (which first asked questions on politics and vaccination) the online surveys asked the gambling questions first (see Table 1). While this pattern is consistent with the possibility that preceding the gambling items with questions about mental health and well-being reduces the frequency of self-reported gambling harm, there is no obvious theoretical reason why this should be so. Without experimental evidence to support such a hypothesis, we conclude that the case for order effects being a contributory factor is weak.

There are also differences between surveys in the questions and response alternatives used for the gambling behaviour questions. This could potentially affect the estimates of gambling harm because answers to these questions are used to filter respondents to the PGSI questions (respondents who report no gambling in the previous 12 months are not asked the PGSI questions and are assigned a score of zero on the index).

The health surveys use 19 questions to measure total gambling during the past 12 months while the online surveys use 15 and, while there is a great deal of overlap between them, there are also differences (see Table in Appendix 4). There are some forms of gambling that are asked about explicitly in the health surveys but not in the online surveys (football pools, spread-betting, private betting), and other health survey questions exclude online options

while the corresponding question in the online surveys do not (e.g. bingo excludes online in the HSE but includes online bingo in the online surveys).

Some questions use different descriptions for what seem like the same gambling activities (e.g. 'Virtual gaming machines in a bookmakers to bet on virtual roulette, poker, blackjack or other games' in the HSE and 'Gaming machines in a bookmakers' in the online surveys). And for in person betting, the online surveys combine horse and dog racing into a single category while the HSE asks separate questions for each and the HSE does not explicitly mention betting on football, while the online surveys do.

In short, there are a large number of differences in the questions used to measure whether a respondent has gambled in the previous 12 months and these differences could affect the estimated rate of gambling harm. That being said, however, the two sets of questions cover a large range of gambling activities and both include an 'any other type of gambling' question at the end, so it is far from certain they would produce notably different rates of gambling prevalence.

Without experimental evidence, whereby respondents are randomly assigned to question blocks, it is impossible to say with certainty what the effect of this difference in the survey instruments might have on estimates of gambling harm and further research is merited on this question. Nonetheless, our assessment given the evidence before us now is that this is unlikely to be a major contributory factor to the differences in PGSI+1.

A less obvious difference in the measurement instruments is that the health surveys use yes/no answer boxes for the questions which ask whether respondents have engaged in different forms of gambling over the past 12 months and these require a response to each question. The online surveys, on the other hand, provide a single tick box for these items and ask respondents to 'tick all that apply'. This may lead to differences in the level of reporting between surveys because respondents tend to pick the first items from a list in 'select all that apply' questions without considering the items at the end, while forced choice questions are less susceptible to this sort of 'satisficing' response style (Schwartz et al, 1989).

However, while this difference in response options may lead to variation in how respondents answer these questions, it seems that this would be in the direction of *under*-estimation of gambling behaviour in the online surveys rather than the health surveys. We can therefore also reject the possibility of order effects contributing to the difference in estimates of gambling harm between surveys.

Also relevant in this regard is the fact that the health surveys included a skip instruction at the bottom of the page of questions on gambling activities over the previous 12 months. The instructions advised respondents who answered 'no' to all of these questions to skip further forward in the questionnaire (answering no further questions on gambling). This might have led some respondents to answer 'no' to all of the questions when some of their answers would otherwise have been 'yes' in order to proceed more quickly through the questionnaire (Yan and Tourangeau, 2008).

However, these instructions are at the bottom of the page and are not especially prominent. As there had been no filtering questions in the self-completion questionnaire up to that point, there was no opportunity for respondents to have learned that skipping questions in this way could help them to progress faster. We therefore consider it unlikely that this had a material impact on the estimates of gambling prevalence in the health surveys.

The form of measurement error that is most germane to our considerations here is *socially desirable responding*. Socially desirable responding is when a respondent over-reports socially desirable attitudes and behaviours or under-reports socially undesirable attitudes and behaviours (Tourangeau, Rips and Rasinski, 2000). Problem gambling is clearly an example of a socially undesirable behaviour, so it seems reasonable to expect that some respondents will under-report it in surveys.

Importantly, people are less willing to admit to socially undesirable attitudes and behaviours in the presence of another person (Tourangeau and Yan, 2007). For this reason, we might expect the online survey estimates to represent better measures of harmful gambling because no interviewer is present when the respondent completes the survey.

To minimise the risk of social desirability bias, the health surveys use a paper self-completion questionnaire rather than interviewer administration for the gambling and other sensitive questions. Nonetheless, it is still possible that the presence of the interviewer and other household members (recall that the health surveys interview all adults in a household so some respondents complete the questionnaire in the presence of other household members) might still lead to under-reporting of gambling behaviour in the self-completion questionnaire. It is possible to make a partial assessment of this possibility because the HSE interviewers recorded whether other household members were present while the respondent filled out the self-completion questionnaire.

Comparing the PGSI+1 rate between respondents who completed the questionnaire alone or in the presence of another household member shows a small difference in the expected direction; of the 63% who completed the gambling questions in the presence of another household member, 3.9% had a PGSI above zero compared with 4.7% for the 37% who completed the gambling questions alone. However, this difference is not statistically significant (Chi Square = 0.92, df=1, p=0.354) which leads us to conclude that socially desirable responding in the health surveys is unlikely to be a significant contributory factor to their lower estimates of gambling harm.[5]

Nonresponse error/selection bias

The final source of error to be considered is nonresponse (or selection) bias. In probability sampling, nonresponse bias results from the failure to contact sampled elements or from their refusal to take part in the survey once contacted. If the propensity to respond to the survey is correlated with the survey variable of interest, estimates of it will be biased (Groves, 2006).

This can be seen from the equation below, where the magnitude of the bias in the mean of the survey variable (indicated by $\bar{y}$) in the responding sample is a function of the covariance between the mean of the survey variable and the propensity to respond to the survey, σ_{yp},

[5] This difference is also not statistically significant controlling for age, sex, marital status, and number of people in the household.

divided by the mean of the response propensities of the sample elements, $\bar{p}$ (and where $\bar{p}$ is equal to the response rate for the survey),

$$Bias(\bar{y}_r) \approx \frac{\sigma_{yp}}{\bar{p}}$$

What this tells us is that, holding σ_{yp} constant, the magnitude of nonresponse bias in the mean of *y* increases as the response rate decreases. In general, σ_{yp} is unknown and so we can only say that the *risk* of nonresponse bias increases as the response rate declines, although recent research has shown that the correlation between response rate and nonresponse bias appears to be much weaker than has hitherto been assumed (Sturgis et al 2016).

In non-probability sampling, for example the quota sampling used by the YouGov and Yonder surveys here, there is no response rate as such, because people choose to join the sample rather than being randomly selected from a sampling frame. It is therefore more appropriate to refer to selection bias rather than nonresponse bias in the context of non-probability samples. The key point though is that, if (after weighting adjustments) the kinds of people who volunteer to complete the survey are different from people in the general population on the variable of interest, estimates of the characteristic(s) the survey is seeking to measure will be biased (Sturgis et al 2017).

These observations should lead us to expect, on theoretical grounds alone, that the online surveys are more likely to be subject to nonresponse bias than the health surveys because they have markedly lower response rates (3.9% to 15% compared to 53%-54% for the health surveys, see Table 1) or use non-probability sampling.

In addition to this theoretical expectation, we can compare estimates across different questions in the surveys to see whether there is evidence that the compositions of the achieved samples are substantially different. Figure 3 presents point estimates and 95% confidence intervals for a selection of estimates from Table A1. At the top of the chart we can see that all surveys give broadly similar estimates of the proportion who report having

bought a National Lottery ticket, ranging from 36% in the 2018 health survey to 46% in the Kantar survey. For in person betting on horse or dog races, however, the estimates are notably and significantly higher for the health surveys (9%-10%) than for the online surveys (1%-5%).

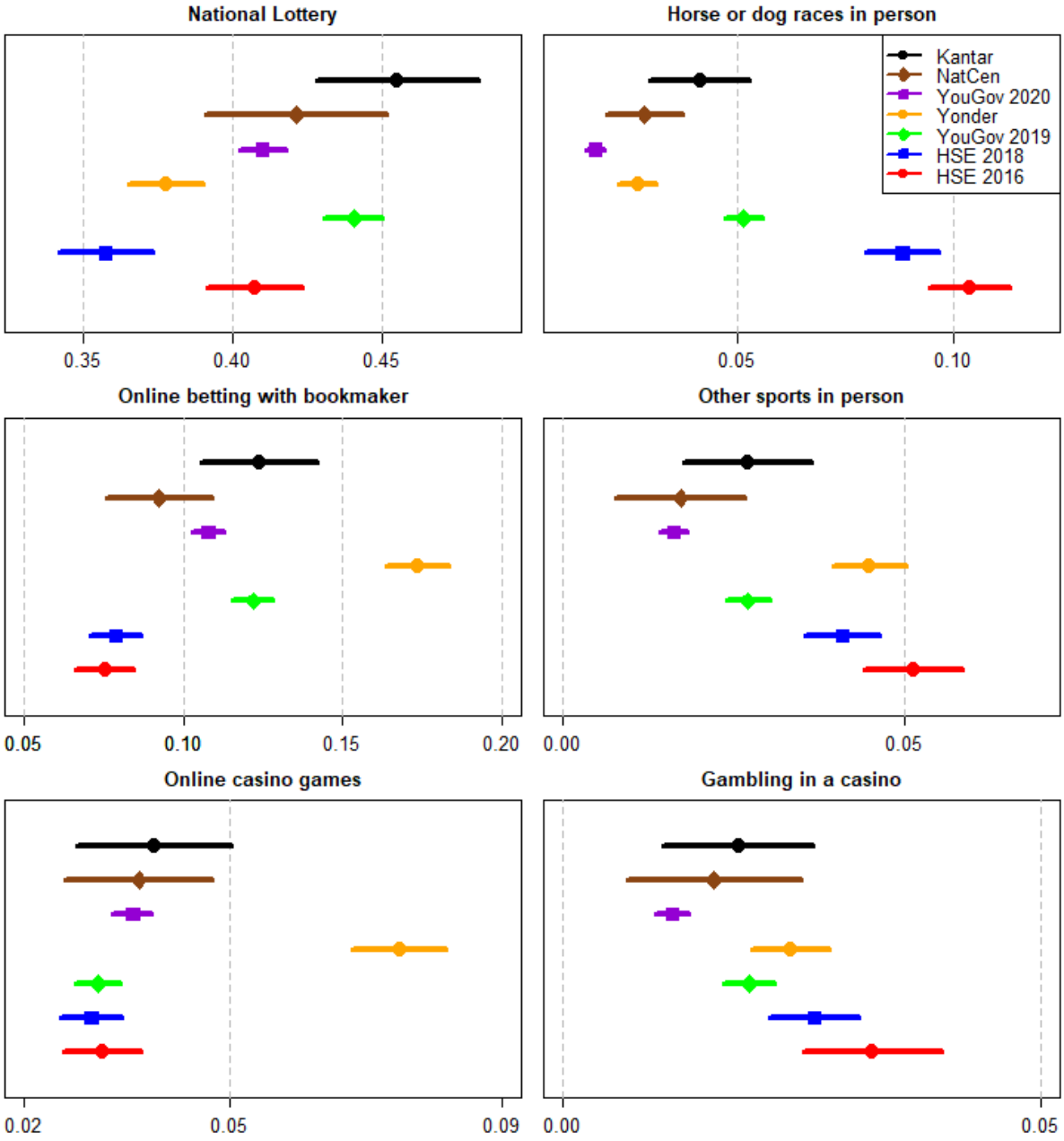

Figure 3 Estimates of the proportion of people have taken part in different gambling activities over the previous 12 months.

The same pattern is also evident for 'other sports event in person', for which the health survey estimates are generally higher compared to the online surveys. Some of this difference no doubt reflects the fact that in person betting on sports events ceased in March 2020 due to lockdown, although the 2019 YouGov survey also shows a lower estimate than the health surveys for these types of in person betting.

The opposite pattern is evident for online betting at bookmakers, where the health surveys have lower estimates than the online surveys (although the difference from the NatCen survey is not statistically significant) and for online casino games, where the health surveys have the lowest point estimates, although these are not statistically significant differences.

There is evidence here then that the health surveys are not just picking up different rates of gambling harm but are also detecting different rates of gambling activities, with in person gambling more common in the health surveys and online gambling more prevalent in the online surveys. It is also worth noting that Yonder is something of an outlier relative to the other online surveys, particularly for online betting with a bookmaker, other sports in person, and online casino games.

Table 2 shows estimates of the frequency of betting across seven of the eight surveys (this question was not included in the 2019 YouGov survey). This reveals a marked difference in the reported frequency of gambling, with the online surveys showing a range of 15% to 26% who gamble more than once a week, compared to 10% for the 2018 HSE.

This higher rate of gambling in the online surveys is also evident at the opposite end of the scale, with 10% to 20% reporting gambling only once or twice a year in the online surveys compared to 28% in the 2018 HSE. Again, Yonder is an outlier with considerably higher reported gambling frequency than the other surveys.

The higher frequency of gambling in the online surveys cannot easily be attributed to the changed context of the coronavirus pandemic because, as noted earlier, we know from the 2020 YouGov follow-up survey that the frequency of gambling reported actually *decreased*

during lockdown, with 45% of gamblers reporting gambling at least once a week in October 2019, dropping to 37% in May 2020.

	Kantar	NatCen	YouGov2020	Yonder	HSE2018	HSE2016
Frequency of spending money (%):						
more than once a week	15.1	14.6	18.8	25.7	10.2	12.7
once a week	22.9	27.9	26.4	27.8	23.67	27.3
less than once a week	8.6	10.9	8.6	9.3	10.8	10.1
once a month	18.9	19.0	17.7	16.3	13.3	12.0
every 2-3 months	14.9	11.2	12.7	10.8	14.2	13.6
once-twice a year	19.5	16.4	15.8	10.0	27.9	24.2

Table 2 Frequency of spending money on gambling

It seems, then, that the composition of the health surveys and the online surveys are different. The health surveys contain gamblers who are more likely to participate in 'traditional' forms of in person gambling, while the online surveys contain more people who use more modern forms of online gambling and who gamble more frequently.

In addition to these univariate estimates, we can also assess whether the *relationships* between demographic variables and gambling harm are consistent across surveys. Existing research in survey methodology has found that univariate estimates from non-probability samples are more prone to selection bias than measures of association (Pasek and Krosnick, 2020).

Table 3 shows estimates of the PGSI+1 rate by sex, age, ethnic group (white v minority ethnic[6]), and education across the eight surveys. We can see that there are clear differences in the demographic profiles of gamblers experiencing harm between surveys; in the health surveys harmful gambling is considerably more likely amongst men relative to women, younger people to older people and ethnic minorities relative to White people compared to the online surveys. There is, though, no notable difference between surveys in how education is related to harmful gambling.

[6] We are not able to present estimates for minority ethnic groups separately due to limitations of sample size.

Clearly, then, differences in sample composition between the health surveys and the online surveys are not limited to univariate estimates but are also evident in the relationships between measures of gambling harm and demographic variables.

row.names	Kantar	NatCen	YouGov2020	Yonder	IpsosMORI	YouGov2019	HSE2018	HSE2016
Sex								
Male	0.17	0.14	0.16	0.21	0.11	0.17	0.06	0.07
Female	0.08	0.07	0.08	0.11	0.04	0.10	0.02	0.02
Age group								
18-34	0.17	0.14	0.18	0.25	0.12	0.19	0.07	0.08
35-44	0.15	0.15	0.18	0.25	0.10	0.18	0.05	0.04
45-54	0.13	0.09	0.11	0.13	0.07	0.14	0.03	0.03
55-64	0.10	0.10	0.08	0.10	0.05	0.09	0.03	0.03
65+	0.07	0.05	0.04	0.04	0.02	0.06	0.01	0.01
Ethnic group								
White	0.12	0.10	0.11	0.15		0.12	0.04	0.04
Non-white	0.14	0.10	0.20	0.25		0.21	0.03	0.03
Qualification								
Degree-level	0.10	0.08	0.11	0.18		0.13	0.03	0.03
Below degree	0.15	0.12	0.12	0.14		0.14	0.05	0.05
No qualification	0.11	0.11	0.16	0.17		0.15	0.04	0.04

Table 3 – Proportion with a PGSI score > 0 by Sex, Age, Ethnic Group and Education across surveys

How might these differences in sample composition be related to the higher estimates of gambling harm in the online surveys? We can gain some leverage on this question by assessing how different types of gambling and the frequency of gambling are related to the probability of experiencing gambling harm.

Figure 5 plots marginal effects from a logistic regression model using the 2018 HSE data. The dependent variable in this model is PGSI+1 and the predictors are an indicator of gambling frequency and the eighteen gambling activities reported over the previous 12 months, grouped into seven categories. The points in the figure can be interpreted as the expected change in the probability of having a PGSI score above zero, when comparing individuals who do and do not engage in each gambling activity.

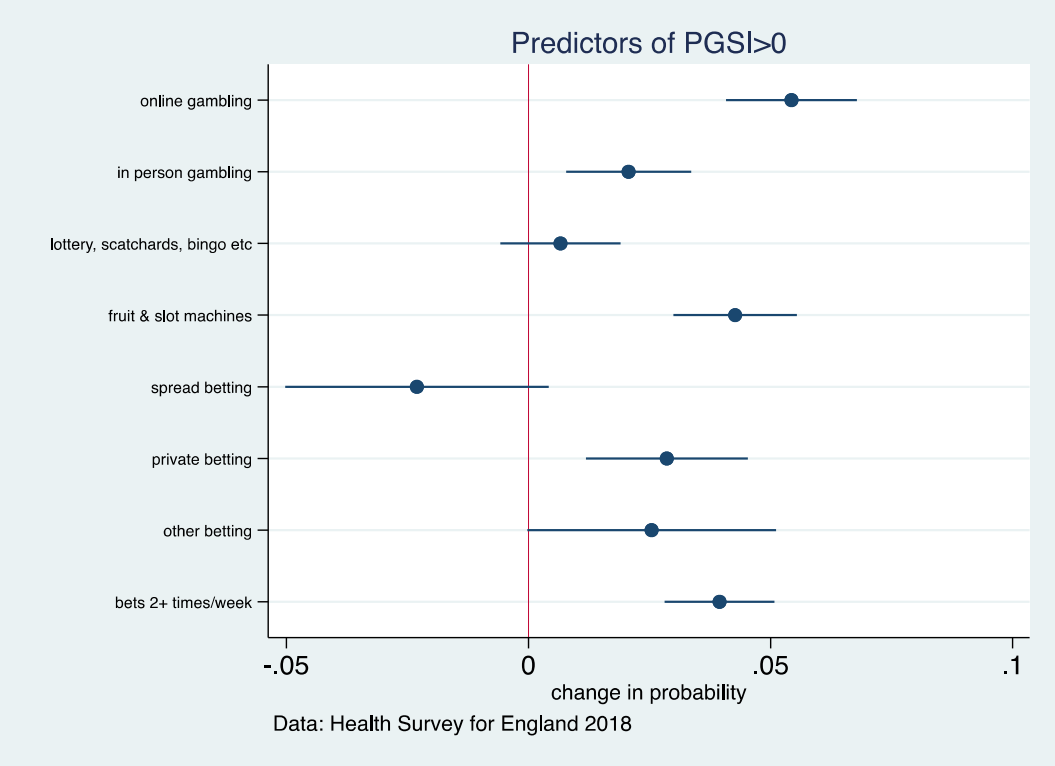

Figure 5 Change in the probability of PGSI+1 by gambling activity and betting frequency

Apart from spread betting (which has a small number of observations and hence a wide confidence interval), all forms of gambling increase the probability of gambling harm. The point estimate is highest for online gambling (5.4%) which is significantly higher than for in person forms of gambling (2%). We can also see that more frequent betting is associated with a higher probability of gambling harm, with an increase in the probability of PGSI+1 of 4% for people who bet twice or more per week.

These findings suggest that, not only do the online surveys have rather different sample compositions compared to the health surveys, the nature of these differences is likely to result in higher estimates of gambling harm. The online surveys contain more people who gamble online and who gamble frequently and these characteristics are associated with an elevated risk of harmful gambling.

Finally, we consider how these differences in sample composition might come about. An obvious starting point here is that the non-probability online surveys exclude the offline population and would seem, therefore, to be more likely to contain people who participate

in online gambling and less likely to include those who gamble in person. In effect, the sample composition is skewed towards more 'tech savvy', high frequency internet users and these characteristics are also likely to be associated with being an online gambler.

Previous research has found surveys that exclude the offline population produce substantially biased estimates of behaviours relating to internet and technology use (Keeter et al, 2015). This does not, though, apply to the random probability panels which include the offline population, either through phone interviews (Kantar and NatCen) or by providing internet access and a tablet (Ipsos-MORI). This type of coverage error cannot therefore explain this compositional difference between the health surveys and the online probability surveys.

Two possibilities suggest themselves for why an online bias might be evident for these probability panel surveys. First, while the offline population and infrequent internet users *can* be in the panel, it is possible that they are not represented in their correct proportion relative to the general population. As noted earlier, this sort of sample imbalance is a particular risk for surveys with low response rates, as is the case here.

We can see some evidence for how probability panel surveys select for more 'internet savvy' in Figures 6 and 7 below. These compare the frequency of internet use and use of social media accounts at the recruitment survey stage and after seven waves of the panel for Kantar Public Voice[7].

The amount of time people spend on the internet and the proportion who have social media accounts is higher at wave 7 than at the recruitment interview survey. Note that these figures compare variables measured at the recruitment survey only, so this change represents the effect of less frequent internet users dropping out of the panel, rather than people changing their online behaviour over time.

[7] We are grateful to Kantar Public for providing these additional analyses for us.

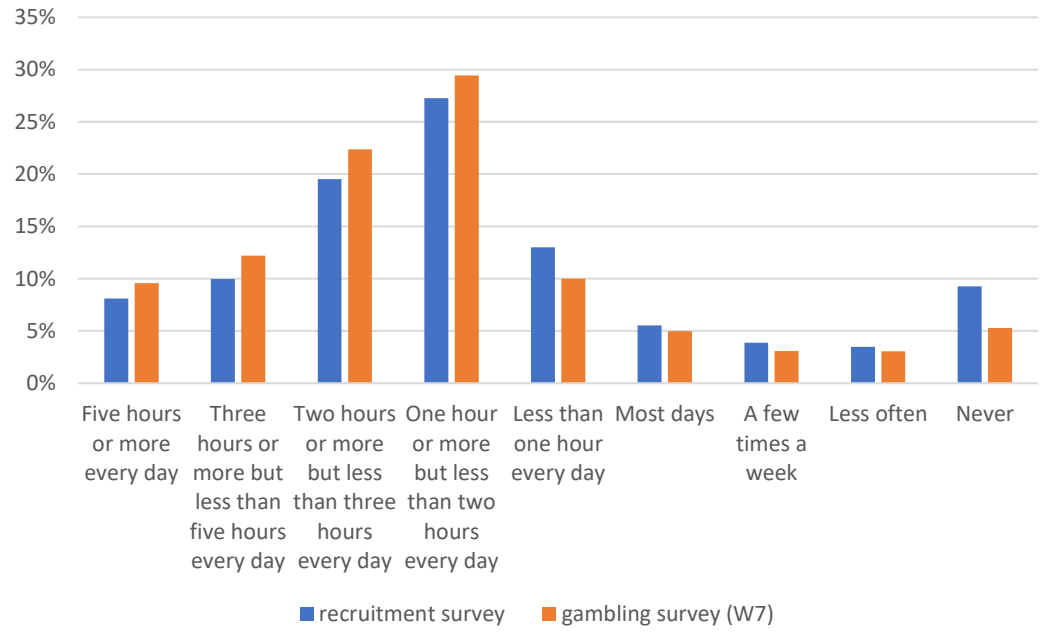

Figure 6 Frequency of internet use, Kantar Public Voice: recruitment survey v wave 7

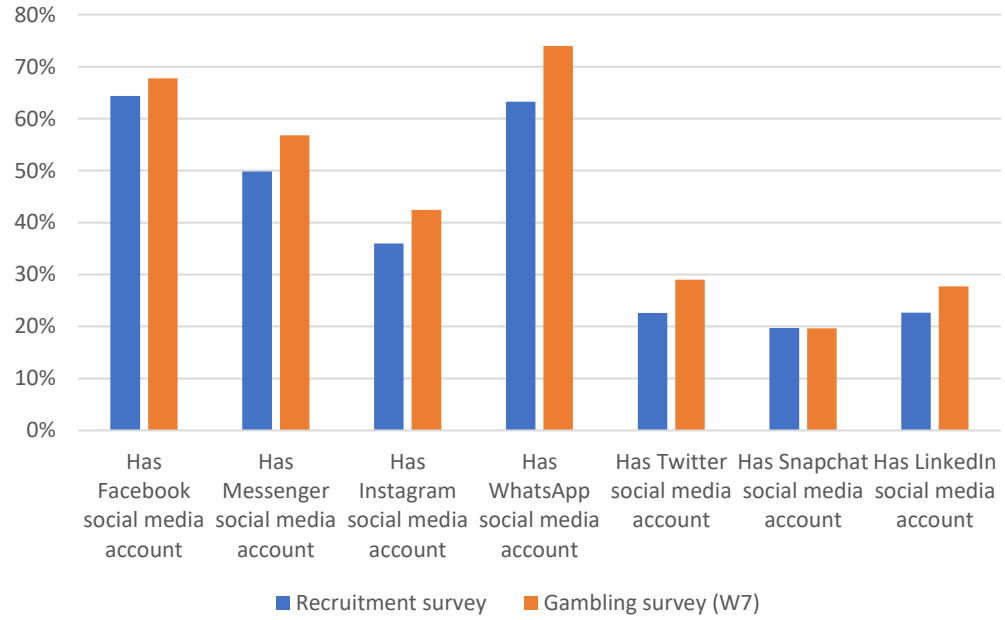

Figure 7 Social media account ownership, Kantar Public Voice: recruitment survey v wave 7

It is worth noting, in this context, that the Ipsos-MORI survey had the lowest rate of PGSI+1 of all the three probability panels (even accounting for the shorter reference period) and that the gambling survey was conducted earlier in the life of the panel compared to the Kantar and NatCen surveys. For the Ipsos-MORI survey, this was the first survey for 44% of

panel members while for Kantar, panel members had completed up to 7 surveys and for NatCen up to ten surveys had been taken. It is possible that the smaller scope for attrition in the Ipsos-MORI panel by the time the gambling survey was fielded partly explains the lower PGSI+1 estimate for this survey.

Of course, an advantage of online panels is that these kinds of attrition effects can be measured and corrected for with weighting, and Kantar Public Voice does indeed adjust for this in its weighting strategy. That being said, it is difficult to know if weighting of this kind completely removes selection bias and, in any event, there is a clear tendency for less frequent internet users to drop out of panels over time.

Are more frequent internet users more likely to take part in online gambling activities? Figure 8 shows rates of gambling for four types of online gambling (casino games, horse or dog racing, football, and other sports) in the Kantar survey for three different levels of internet use: heavy users (3+ hours per day), regular users (<1 hour per day to 2+ hours per day), and occasional users (< a few times per week to most days) and offliners.

For all four online gambling activities the heavy internet users are considerably more likely to report participation compared to the occasional users and offliners. This lends further support to the hypothesis that the online surveys select for people who are more 'tech savvy', who are themselves more likely to be online gamblers and are therefore more likely to report gambling harm.

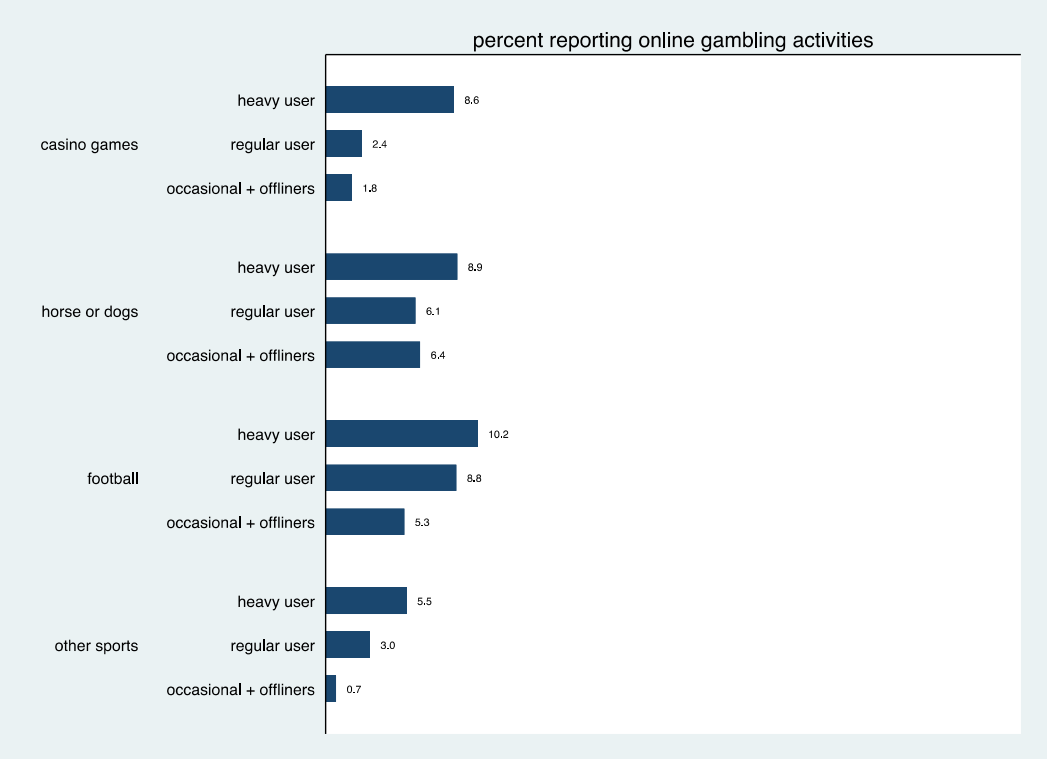

Figure 8 Rates of reported online gambling activities by frequency of internet use: Kantar survey (n=1995)

Conclusion

In October 2019 YouGov published the results of a survey on Treatment and Support for people experiencing gambling harm that they had undertaken on behalf of GambleAware. It produced estimates of problem gambling and gambling harm that were three times higher than the 2018 Health Survey for England which uses what is considered to be the methodological gold standard. In March 2020, GambleAware commissioned Professor Patrick Sturgis to produce a report assessing which of the two surveys was likely to be the more accurate.

That report (Sturgis, 2020) concluded that, on the balance of probabilities, the HSE estimate was likely to be closest to the true level of gambling harm. This conclusion was reached primarily on the basis of the HSE's use of probability sampling and its comparatively high response rate, compared to the YouGov survey's use of non-probability sampling. However, this was deduced entirely on theoretical considerations as there was insufficient data to determine the question empirically.

The purpose of this follow-on report is to shed additional light on which of the surveys is likely to be more accurate by assessing how methodological differences in implemented survey designs are related to variability in estimates of gambling behaviour and gambling harm. In doing this we are able to draw on six surveys containing the same core set of gambling questions conducted within the same two-year window which we compare to the 2016 and 2018 HSEs.

For all six surveys, data collection was done via online self-completion with two using a mixed-mode (online and telephone) design, although for the mixed mode surveys the vast majority of interviews (90%) were carried out online. Three of the online surveys used probability sampling and three used non-probability quota sampling.

We have been able to rule out several potential causes of the differences between the health surveys and the online surveys. First, we can exclude the possibility that the differences are due to true change in harmful gambling in the population – it is not plausible that a threefold increase occurred between 2018 and October 2019 particularly when evidence from a separate survey carried out in May 2020 by YouGov showed that gambling actually *decreased* somewhat during lockdown.

We can also rule out the contingency that the differences between surveys are due to sampling variability because the differences between the health survey estimates and the online surveys are statistically significant in all cases. Coverage error, too, can be excluded as a plausible cause of the differences because the online probability surveys use the same sampling frame (PAF) as the health surveys.

There are a number of differences in the content, wording, and format of questions between the surveys that could in principle account for some of the difference in estimates due to differential measurement error. In particular, the health surveys' use of interviewer administration raises the possibility that these estimates are biased downward due to social desirability bias. However, we conclude that the use of a self-completion questionnaire for the gambling questions in the HSE likely minimises this effect. We also found little evidence that respondents under-report gambling harm in the presence of other household

members, which suggests bias due to self-presentation is not making a strong contribution to responses to questions on gambling harm in the health surveys.

While other differences in measurement properties of the survey instruments seem unlikely on theoretical grounds to be a significant contributory factor, this cannot be ruled out entirely and further research would be valuable here, particularly on the effect of differences in the batteries of questions which are used to detect anyone who has gambled over the previous 12 months.

By a process of elimination, this leaves us with one remaining major source of error that could be the cause of the difference in estimates of gambling harm – nonresponse/selection bias. This is when survey estimates differ systematically from true population values due to unrepresentative samples. The risk of selection bias must be considered quite high for the online surveys, given their use of either non-probability sampling or low response rate probability samples.

Comparisons of estimates revealed a pattern of systematic differences between surveys: the online surveys contained gamblers who were more likely to gamble online and to gamble frequently, while the health surveys contained higher proportions of in person gamblers and less frequent gamblers. There were also differences in the kinds of people who report gambling harm between surveys: the gender and age differences were larger in the health surveys, while the difference between White people and ethnic minorities were smaller.

These discrepancies in sample composition seem likely to be driving the differences we observe in rates of gambling harm. Through processes of self-selection, the online surveys skew toward people who are more online and 'tech savvy' and, perhaps unsurprisingly, these sorts of people are more likely to be online gamblers compared to people who gamble in person. And, if samples contain disproportionate numbers of online and frequent gamblers (compared to the general population), surveys will tend to over-estimate gambling harm because, as we were able to demonstrate, online and frequent gambling are independently associated with higher probabilities of gambling harm.

It is important to note that these do not represent generalised problems of selection bias in these online panels. Selection bias is a variable-specific phenomenon and it cannot be assumed from identifying the possibility of bias in one substantive topic area that bias will also be evident for other questions and topics (Groves and Peytcheva, 2008). As noted previously, existing research suggests that behaviours relating to online behaviour and technology use seem particularly prone to selection bias in online surveys, an association which makes intuitive sense.

We found no systematic pattern between the probability and non-probability online samples in estimates of gambling and gambling harm, although it should be acknowledged that we were only able to draw on two non-probability providers and so cannot draw any general conclusions on this matter. The YouGov estimates were generally well aligned with those of the probability panels and, in many instances, were closer to the health survey estimates. The other non-probability survey (Yonder) yielded outlier estimates on a number of indicators and were generally furthest from the health surveys.

Of the probability panels, the Ipsos-MORI survey was closest to the health surveys on the key estimate of gambling harm, even after accounting for its use of a 4 week rather than a 12-month reference period. Whether this was due to the earlier stage in the life of the panel that the gambling survey was fielded, or some other feature of the Ipsos-MORI panel is difficult to determine with the data available. One possibility is that the Ipsos-MORI panel's provision of a tablet and internet access to respondents without internet access might be more effective than phone interviewing in covering this part of the population, a question on which additional research would be valuable.

This, then, brings us to the question of how estimates of gambling and gambling harm should be produced in the future. Our findings and conclusions in this report suggest that this should be done via probability sampling and in person interviewing. However, the very high cost of this type of design and the ongoing moratorium on their use due to coronavirus makes this an unrealistic option in the current context. Moreover, even if in person probability surveys are continued, their high cost places limits on sample size and the regularity with which surveys can be fielded over time.

These limiting factors make online surveys an obvious choice for future delivery of gambling prevalence estimates. Online survey costs are considerably lower than face-to-face, meaning sample sizes can be larger and populations more regularly surveyed. Recent developments in sample design, internet speed, and device technology mean that probability designs are also now straightforward to implement in this mode.

Of course, these benefits must be set against our core finding in this report, that online surveys seem to substantially over-estimate gambling activity and gambling harm. We turn to how this might be mitigated shortly but it should also be noted in this context that even if a survey design provides biased estimates of a population parameter, this does not imply that estimates of *change* in the parameter over time using the same design will also be biased. In short, it is still possible to produce accurate measures of change even if measures of level are biased. As a key purpose of gambling prevalence surveys is to monitor change in gambling behaviour and gambling harm over time, this can still be achieved even if estimates of gambling activities and harm are over-estimated cross-sectionally.

Another alternative would be to switch to telephone interviewing, though a consideration of the feasibility of telephone surveys for this purpose is beyond the scope of this report. We note, though, that phone surveys in the UK face substantial challenges relating to coverage and nonresponse, while also requiring human interviewers to administer questionnaires, which would heighten the risk of social desirability bias in the measurement of gambling harm. These factors make phone surveys an unattractive option for replacing in person interviewing in the short to medium term at least.

If our two conclusions here are correct – that online surveys over-estimate gambling harm and are also the most feasible alternative to in person interviewing – then caution over interpretation of key estimates, alongside an on-going programme of methodological testing and development will be necessary in the years ahead. In particular, we would encourage research on strategies for sample recruitment and weighting in online surveys that more effectively mitigate the selection biases we have identified in this report.

Although we have not touched on the measurement properties of the main instruments currently used for measuring gambling harm in this report, we would recommend research and development in this area too. It may, for example, be possible to produce more robust and accurate estimates of gambling harm by using different methods of measurement and scaling.

While a shift to online surveys for measuring and monitoring change in gambling behaviour and gambling harm seems to be the most attractive option in the short to medium term at least, this should be combined with periodic estimates using in person probability surveys. This will be necessary for calibrating online estimates and to evaluate the effectiveness of methodological developments that are implemented to mitigate the biases we have highlighted in this report.

Recommendations

1. Given the high cost of face-to-face probability surveys and the limits this imposes on sample size and the frequency of surveying, measurement of gambling prevalence and harm should move to online surveying. This will enable more fine-grained sub-group analysis and more regular provision of key estimates than is currently possible.
2. The move to online interviewing should be combined with a programme of methodological testing and development focusing in particular on strategies for mitigating selection bias and on improving the accuracy of measurement of gambling harm.
3. In person surveying should not be abandoned completely, regular but infrequent surveys using probability sampling and face-to-face interviewing should continue in order to calibrate the estimates from online surveys and to serve as a gold-standard by which methodological innovations and developments can be evaluated.

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Appendix 1 Sample Designs of the eight surveys

The 2016 Health Survey for England has a multi-stage stratified probability sample design with postcode sectors as the Primary Sampling Units (PSU) randomly selected with probability proportional to size from the Postcode Address File (PAF). Within each PSU, 18 addresses are randomly selected and interviews attempted with all adults aged 16 or above at sampled addresses. Where an address contains multiple households, one is selected at random. Interviews are conducted by trained interviewers using Computer Assisted Personal Interviewing (CAPI).

A £10 unconditional incentive was included in the advance letter sent to selected households to increase participation. The gambling questions are administered via a self-completion paper questionnaire rather than by the interviewer in order to minimise the potential for under-reporting due to socially desirable responding bias. Immediately preceding the gambling questions in the self-completion questionnaire respondents were asked about their well-being and mental health. The individual level response rate was 54% and the achieved sample size was 6691. The survey is weighted for unequal selection probabilities in the sample design, for nonresponse, and to match population totals for sex, age, and region. **The 2018 Health Survey for England** used the same design, although the response rate was one percentage point lower at 53% and the achieved sample size was 6927.

The NatCen survey was conducted between 19th November and 20th December 2020 using the NatCen probability panel, which is an established panel of respondents who have agreed to participate in surveys at a frequency of approximately one per month, lasting for 15 to 20 minutes. Panel members are drawn from the British Social Attitudes (BSA) survey via an invitation at the end of the BSA interview. The BSA has a similar sample design to the HSE, a stratified sample of postcode sectors is drawn at random from the PAF with probability proportionate to size, and a systematic random sample of addresses is then drawn at the second stage. Where addresses contain more than one household, a single household is selected at random.

Unlike the HSE, the BSA randomly samples a single adult in each responding household on households containing more than one adult. Interviews are conducted in person by trained interviewers using CAPI. Panel members were offered an incentive of £10 to complete the survey. The NatCen survey comprised respondents from the 2018 and 2019 waves of the BSA, respondents who had previously taken part in up to ten surveys on the panel.

Approximately 10% of panel members do not have personal access to the internet and are interviewed on the phone by trained interviewers. Accounting for nonresponse to the BSA, refusal to join the panel, and nonresponse to the invitation to complete the gambling survey, the response rate was 14%. Estimates are weighted to adjust for unequal selection probabilities in the BSA sample design, nonresponse to the BSA, refusal to join the panel, and nonresponse to the gambling survey amongst eligible panel members. The sample size was 2318.

The Kantar survey was conducted using the Kantar Public Voice panel between 24th November and 13th December 2020. Like the NatCen panel, Kantar Public Voice is a standing panel of respondents who have agreed to complete surveys at a frequency of approximately one per month and of 15 to 20 minutes duration. Panel members were recruited through a combination of face-to-face and 'push-to-web' mail invitation surveys. The in-person recruitment survey used a similar design to the BSA: a stratified random sample of postcode sectors was drawn from the PAF, with addresses randomly selected within postcode sectors and a single randomly selected household sampled at addresses containing multiple households. Up to two adults were randomly selected at each eligible responding address. For the mail recruitment, a stratified random sample of addresses was drawn from the PAF and invitations to join the panel sent out in the post. Respondents to the recruitment survey signed up to be members of the panel through unique personal identifiers included in the invitation.

Two unique identifiers were included in each invitation, so that up to two adults in each household containing multiple adults could join the panel. Panel members are provided with a £10 incentive for completing each survey, although for the gambling survey this was reduced to £5 because the survey was of shorter than usual duration. The approximately

10% of panel members who do not have internet access are interviewed by phone. By the time of taking part in the gambling survey, respondents had completed up to 7 surveys as part of the Public Voice panel. Respondents who failed a data quality check were removed from the sample. The sample size for the gambling survey was 1995. Estimates are weighted to account for unequal selection probabilities in the recruitment survey and for nonresponse at each stage of the survey process. Accounting for nonresponse at each stage, the net response rate for the gambling survey was 5% and the achieved sample size was 1995.

The **Ipsos-MORI survey** was carried out between 21st and 27th January 2021 using the Ipsos-MORI Knowledge Panel. This has a similar design to the Kantar Public Voice panel, although all recruitment is done using a mail push-to-web approach. A stratified random sample of addresses is drawn from the PAF in a single stage and two unique identifiers are sent out in an invitation letter to sampled addresses, meaning that up to two adults can join the panel in each participating household. Respondents to the recruitment survey who do not have access to the internet are provided with a basic internet connection and a tablet on which to complete future surveys. Respondents were provided with a £10 monetary incentive for completing the survey.

The net response rate, accounting for nonresponse at each stage of the sample design for the gambling survey was 3.9% and the achieved sample size was 6743. For 44% of respondents to the gambling survey, this was the first survey they had completed as members of the panel, and the remaining respondents had completed a maximum of five previous surveys (7% of respondents). Note that the Ipsos-MORI survey administered the PGSI questions only to respondents who reported having gambled in the 4 weeks preceding the survey, while the other surveys administered the PGSI to all respondents reporting any gambling over the previous 12 months.

The **2019 YouGov survey** was carried out from 24th September to 13th October 2019 using the YouGov online panel of survey volunteers. Members of the YouGov panel are not recruited using random sampling so it is not possible to report a response rate. Instead, people are encouraged to sign up to become panel members using a range of promotional

and advertising strategies. The panel currently comprises x00,000 active members. Surveys are implemented using quota sampling, whereby the achieved sample matches the target population across a set of known population totals. Additional matching of the sample to population totals is implemented using calibration weighting on the following variables: age, gender, ethnic group, social grade, and region.

The survey was conducted using online self-completion only, and people who do not have access to the internet are excluded from the survey. Panel members are rewarded with points for completing each survey, which can be converted to money. Exactly the same methodological approach was used for the **2020 YouGov survey**, with fieldwork conducted between 19th November and 11th December. The achieved sample size was 12161 for the 2019 survey and 18879 for the 2020.

The **Yonder survey** uses the same methodological approach as YouGov. Respondents to each survey are drawn from Yonder's opt-in panel of people who have signed up to complete surveys online in return for monetary reward, using quota sampling and calibration weighting. The panel consists of approximately xxxx members. The offline population are excluded from the survey. The sample for this survey was matched to population totals using quota sampling and calibration weighting on age, gender, region, and social grade. Fieldwork was carried out between 18th and 29th November 2020 and the achieved sample size was 8081.

Appendix 2 – Estimates of gambling activities and gambling harm

Table A1 Univariate estimates of PGSI and gambling activities across surveys

Variable	Kantar	NatCen	YouGov2020	Yonder	IpsosMORI	YouGov2019	HSE2018	HSE2016
PGSI >0	12.66	10.26	12.01	16.00	7.43	13.47	3.93	4.09
PGSI categories:								
0	87.34	89.74	87.99	84.00	92.57	86.53	96.07	95.91
1-2	7.65	5.03	6.44	6.89	4.59	7.34	2.71	2.47
3-7	3.27	3.31	3.17	4.56	1.60	3.31	0.84	1.15
8-27	1.73	1.92	2.40	4.55	1.25	2.82	0.38	0.47
Forms of gambling in last 12 months:								
National Lottery	45.49	42.11	40.99	37.75		44.03	35.73	40.73
any other lottery	20.35	19.80	13.68	16.59		13.56	14.33	13.62
scratch cards	21.20	20.13	16.88	21.25		19.14	17.78	20.24
gaming machines at bookmakers	1.60	1.21	0.94	1.89		1.21	2.23	2.99
fruit or slot machines	3.44	3.27	2.16	4.57		3.65	5.61	6.23
bingo (including online)	4.62	4.33	3.46	6.98		4.92		
gambling in casino	1.83	1.58	1.15	2.38		1.94	2.63	3.24
online casino games	3.91	3.68	3.59	7.50		3.08	2.98	3.15
horse or dog races online	6.94	5.21	5.04	7.57		6.45		
horse or dog races in person	4.16	2.89	1.74	2.72		5.16	8.82	10.35
football online	9.33	5.88	7.81	12.88		8.40		
football in person	2.45	1.72	1.44	3.46		2.38		
other sports online	3.02	3.20	3.54	8.41		4.29		
other sports in person	0.85	0.36	0.61	2.30		0.76		
any other type of gambling	2.52	1.85	1.76	1.23		1.71	1.21	1.30
any online betting with bookmaker	12.38	9.24	10.79	17.36		12.18	7.87	7.54
betting on sports event, in person	2.71	1.72	1.63	4.49		2.71	4.09	5.13
last 12 months: any gambling	64.38	58.51	56.64	62.08		61.96	52.56	54.99

Appendix 3 – Gambling survey questions

[Q1] {multiple} **Which of these have you spent money on in the past <u>12 months</u>? Please tick all that apply.**
<1> Tickets for the National Lottery Draw, including Thunderball and EuroMillions and tickets bought online
<2> Tickets for any other lottery, including charity lotteries
<3> Scratch cards
<4> Gaming machines in a bookmakers
<5> Fruit or slot machines
<6> Bingo (including online)
<7> Gambling in a casino (any type)
<8> Online casino games (slot machine style, roulette, poker, instant wins)
<9> Betting on horse or dog races – online
<10> Betting on horse or dog races – in person
<11> Betting on football – online
<12> Betting on football – in person
<13> Betting on other sports – online
<14> Betting on other sports – in person
<15> Any other type of gambling
<99 xor> None of the above
<98 xor> Don't know

#All who have participated in last 12 months
#Display those selected at Q1
[Q2] {multiple} **And which of these have you spent money on in the past <u>4 weeks</u>? Please tick all that apply.**
<1 if 1 in Q1> Tickets for the National Lottery Draw, including Thunderball and EuroMillions and tickets bought online
<2 if 2 in Q1> Tickets for any other lottery, including charity lotteries
<3 if 3 in Q1> Scratch cards
<4 if 4 in Q1> Gaming machines in a bookmakers
<5 if 5 in Q1> Fruit or slot machines
<6 if 6 in Q1> Bingo (including online)
<7 if 7 in Q1> Gambling in a casino (any type)
<8 if 8 in Q1> Online casino games (slot machine style, roulette, poker, instant wins)
<9 if 9 in Q1> Betting on horse or dog races – online
<10 if 10 in Q1> Betting on horse or dog races – in person
<11 if 11 in Q1> Betting on football – online
<12 if 12 in Q1> Betting on football – in person
<13 if 13 in Q1> Betting on other sports – online
<14 if 14 in Q1> Betting on other sports – in person
<15 if 15 in Q1> Any other type of gambling
<99 xor> None of the above
<98 xor> Don't know

The following questions are about **gambling, including the National Lottery and scratch cards as well as sports betting, casino games, gaming machines and bingo.** <
><
>
For the purposes of this survey, please consider 'gambling' and 'betting' to mean the same thing.
#All who have participated in last 12 months
[Q3]**Thinking about <u>all</u> the gambling activities covered in the previous questions, would you say you spend money on these activities…**

<1> Everyday / 6-7 days a week
<2> 4-5 days a week
<3> 2-3 days a week
<4> About once a week
<7> About once a fortnight
<8> About once a month
<9> Every 2-3 months
<10> Once or twice a year

#All gamblers (participated in last 12 months)
[Q4]{grid roworder=randomize displaymax=8} **The questions that follow show reasons that some people have given about why they take part in gambling. For each one, please state whether these are reasons why you take part in gambling.**

I take part in gambling…
-[Q4_1] for the chance of winning big money
-[Q4_2] because it's fun
-[Q4_3] as a hobby or a pastime
-[Q4_4] to escape boredom or to fill my time
-[Q4_5] because I'm worried about not winning if I don't play
-[Q4_6] to compete with others (e.g. bookmaker, other gamblers)
-[Q4_7] because it's exciting
-[Q4_8] for the mental challenge or to learn about the game or activity
-[Q4_9] because of the sense of achievement when I win
-[Q4_10] to impress other people
-[Q4_11] to be sociable
-[Q4_12] because it helps when I'm feeling tense
-[Q4_14] to make money
-[Q4_15] to relax
-[Q4_16] because it's something that I do with my friends or family
<1>Always
<2>Often
<3>Sometimes
<4>Never

PGSI questions
#All gamblers (participated in last 12 months)
[Q5] **Thinking about the last 12 months:**
-[Q5_1] Have you bet more than you could really afford to lose?
-[Q5_2] Have you needed to gamble with larger amounts of money to get the same excitement?
-[Q5_3] When you gambled, did you go back another day to try and win back the money you lost?
-[Q5_4] Have you borrowed money or sold anything to get money to gamble?
-[Q5_5] Have you felt that you might have a problem with gambling?
-[Q5_6] Has gambling caused you any mental health problems, including stress or anxiety?
-[Q5_7] Have people criticised your betting or told you that you had a gambling problem, regardless of whether or not you thought it was true?
-[Q5_8] Has your gambling caused any financial problems for you or your household?
-[Q5_9] Have you felt guilty about the way you gamble or what happens when you gamble?
<1>Never
<2>Sometimes
<3>Most of the time
<4>Almost always

Appendix 4 – Gambling activity questions: health surveys compared to online surveys

Health surveys	Online surveys
1> Tickets for the National Lottery Draw, including Thunderball and EuroMillions and tickets bought online	1> Tickets for the National Lottery Draw, including Thunderball and EuroMillions and tickets bought online
<2> Tickets for any other lottery, including charity lotteries	<2> Tickets for any other lottery, including charity lotteries
<3> Scratch cards	<3> Scratch cards
The football pools	MISSING
Bingo cards or tickets, including playing at a bingo hall (not online)	<6> Bingo (including online)
<5> Fruit or slot machines	<5> Fruit or slot machines
Virtual gaming machines in a **bookmakers** to bet on virtual roulette, poker, blackjack or other games	<4> Gaming machines in a **bookmakers**
Table games (roulette, cards or dice) in a **casino**	<7> Gambling in a **casino** (any type)
Playing poker in a pub tournament/league or at a club	
Online gambling like playing poker, bingo, instant win/scratchcard games, slot machine style games or casino games for money	<8> Online **casino** games (slot machine style, roulette, poker, instant wins)
Online betting with a **bookmaker** on any event or sport	<11> Betting on football – online
	<9> Betting on horse or dog races – online
	<13> Betting on other sports – online
Betting exchange	MISSING
Betting on horse races in a bookmakers, by phone, or at the track	
Betting on dog races in a bookmakers, by phone, or at the track	<10> Betting on horse or dog races – in person
Betting on sports events in a bookmakers, by phone or at the venue	<12> Betting on football – in person
Betting on other events in a bookmakers, by phone or at the venue	<14> Betting on other sports – in person
Spread-betting	MISSING
Private betting, playing cards or games for money with friends, family or colleagues	MISSING
Any other form of gambling	<15> Any other type of gambling