

## Young People and Gambling in Britain: A Critique of the DCMS Technical Paper No. 8

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This report is a brief critique of the report commissioned by the Department of Culture, Media and Sport (DCMS; [May-Chahal, Measham, Brannock, Amos, & Dagnall, 2004](#)) to examine adolescent gambling in Britain. This brief critique is split into three main sections that cover general comments followed by more specific comments in relation to the executive summary and the rest of the report. The report also includes our own executive summary and an appendix of comments from other people in the field of youth gambling relating to the DCMS report. The reason for writing this critique is that the conclusions made in the DCMS report are the ones most likely to be heard at Ministerial level. Given that the report is selective in the use of the literature and the executive summary is written in such a way as to seriously minimize the problem and issue of adolescent gambling, we present a similar picture but with a different emphasis.

*Keywords:* Gambling; Adolescent gambling; Addiction; Youth; DCMS report.

### Executive Summary

- The report of the Department of Culture, Media and Sport (DCMS; [May-Chahal, Measham, Brannock, Amos, & Dagnall, 2004](#)) was highly selective in the use of the adolescent gambling literature and the executive summary was written in such a way as to seriously minimize the problem and issue of adolescent gambling.
- The DCMS report missed out at least 30 studies (when the number of studies was low to begin with) which does not suggest that the review is systematic. There are a whole host of studies—particularly those carried out between 1985 and

1992 that are not even mentioned in the DCMS report. The conclusions are therefore based on incomplete data.

- The DCMS report makes reference to the fact that drug and alcohol studies have overcome some of the problems with surveys by using observational qualitative studies. The observational studies in the gambling field were completely ignored in the DCMS report. Furthermore, the DCMS report systematically ignored almost all of the qualitative research on adolescent gambling more generally.
- The DCMS report clearly showed that Britain has around 5% adolescent problem gamblers. This was not even mentioned in the executive summary. This level of problem gambling is of significant concern and should not have been ignored in either the executive summary or the report conclusions. The DCMS report appears to be an attempt at minimizing the adolescent gambling problem in how it was reported. A level of 5% problem gambling represents a serious public health issue and is not something that would normally be referred to as “a small minority” in public health terms.

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- The biggest non-national study of adolescent gambling by [Wood and Griffiths \(1998\)](#) is reported as giving lower rates of problem gambling than the general population. However, the DCMS executive summary says that it is the studies using the largest samples and widest coverage find the lowest rates. There is clearly inconsistency between what is in the DCMS report and what is reported in the executive summary.
- The DCMS report's authors spend a considerable amount of time arguing about what they feel is wrong with studies without pointing out the relative strengths or putting the studies into context. The report also spends more time trying to find fault with each of the smaller studies methodologically when in fact for triangulation purposes they all say the same thing and concur with the larger scale studies (that there is a significant number of children who have gambling problems). Triangulation of the smaller studies shows that results are very similar, indicating that they are measuring the same things.

### General Comments

- The DCMS report claims at various places including the title, to be a systematic review of the adolescent gambling literature. To miss out at least 30 studies (when the number of studies is low to begin with) does not suggest that it is systematic.
- To be more specific, there are a whole host of studies—particularly those carried out between 1985 and 1992 that are not even mentioned in the DCMS report (e.g., [Ashdown, 1987](#); [Barham & Cormell, 1987](#); [Bentall, Fisher, Kelly, Bromley, & Hawksworth, 1989](#); [Beverley Area Management Committee, 1989](#); [Centre for Leisure Research, 1990](#); [Fisher, 1993a](#); [Huxley, 1993](#); [Huxley & Carroll, 1992](#); [Lee, 1989](#); [Moran, 1987](#); [National Housing and Town Planning Council, 1988](#); [Spectrum Children's Trust, 1988](#); [Waterman & Atkin, 1985](#); [Wyatt, 1988a, 1988b](#)). One of the reasons that there was far less research on fruit machine gambling in the late 1990s is that the more than 20 studies conducted in the late 1980s and early 1990s all revealed the same finding—that adolescent gambling was widespread, and that it was problematic for a small minority. Some of these studies surveyed thousands of children. For instance, the National Housing Town Planning Council ([NHTPC, 1988](#)) surveyed nearly 10,000 children and the Spectrum Children's Trust ([SCT, 1988](#)) surveyed nearly 2500 children. There seems to be no rationale for leaving out these studies given the sample size. The authors could simply have looked at one of Griffiths' ([1995a, 2002](#)) books on adolescent gambling, and assessed or reviewed the studies in question. In [Griffiths' \(1995a\)](#) overview, 23 survey studies were listed. The DCMS report only mentions one of these in its “systematic” review (i.e., [Griffiths, 1990a](#)).
- The authors claimed they had contacted GamCare. If so, GamCare would have been able to provide all their annual reports which provide information on who is accessing their helpline and counselling services, many of which are adolescent gamblers or their family members. Even without these, the authors could have reviewed published helpline data in peer reviewed journals that make reference to adolescent gambling statistics (e.g., [Griffiths, Scarfe, & Bellringer, 1999](#)).
- One of the most major omissions in the report is [Griffiths and Sutherland's \(1998\)](#) survey of 4500 lottery players and the relationship between smoking, drinking and drug taking. Given this was published in a widely abstracted peer-reviewed journal (the *Journal of Community and Applied Social Psychology*), there really does not seem to be a good excuse as to why it was completely overlooked for a major review like this. There are also studies by Griffiths ([1994a, 1994b](#)) examining cross addiction which make particular reference to adolescent gambling sub-groups.
- The DCMS report makes particular reference to the “tracking study” by [Ashworth, Doyle, and Howat \(2000\)](#). This study is not a single study—it is three separate studies. Sue Fisher has published the first two of these studies independently in peer reviewed journals (e.g., [Fisher, 1999](#)). It is academic ‘sleight of hand’ to claim this is one study and contributes to the overall picture that adolescent problem gambling is being minimized by the way it is reported.
- The report makes reference to the fact that drug and alcohol studies have overcome some of the problems with surveys by using observational qualitative studies. If that is the case, why did the authors not review qualitative studies in the gambling field (e.g., [Fisher, 1993b](#); [Griffiths, 1990b, 1990c, 1990d, 1991a](#); [Griffiths & Minton, 1997](#); [Trott & Griffiths, 1991](#))? For instance, [Griffiths \(1991a\)](#) reported the results of a two-year observational study and [Fisher \(1993b\)](#) reported her findings from a one-year observational study. These were both summarized in a whole chapter in [Griffiths' \(1995a\)](#) book on adolescent gambling. There is also the issue of why some qualitative research was highlighted (e.g., [Wood & Griffiths, 2002](#); [Wood, Griffiths, Derevensky, & Gupta, 2002](#)) while other studies were overlooked (e.g., [Griffiths, 1993a, 2003](#)). Theory needs to be derived from qualitative studies and these entail relatively small samples (theoreti-

cally-driven). The report systematically ignored most of the qualitative research.

- The report is consistently written in a way that minimizes adolescent problem gambling at every opportunity by reporting things from a majority perspective. The report clearly shows that Britain has around 5% adolescent problem gamblers. This is not mentioned in the executive summary. What is mentioned is that a majority of adolescent gamblers do not have a problem. No-one has ever claimed that a majority have a problem. This is differential and biased reporting.
- The report spent more time trying to find fault with each of the smaller studies methodologically when in fact from triangulation purposes they all say the same thing and concur with the larger scale studies (that there is a significant number of children who have gambling problems). Every single research study has limitations. What we need to do is to see if there are any consistent findings across the many studies. Small-scale studies are valid snapshots of specific areas and moments in time. Triangulation of these small studies shows that results are very similar, indicating that they are measuring the same things.
- The authors make no mention of [Orford, Sproston, Erens, White, and Mitchell's \(2003\)](#) review of prevalence studies (Ch. 3) in *Gambling and Problem Gambling in Britain*—a thorough, recent, and well-reviewed work.

### Specific Comments About the DCMS Executive Summary

- The DCMS executive summary claims that it is not possible to give reliable prevalence figures for problem gambling in childhood. Given that the DCMS use just one adult gambling prevalence survey ([Sproston, Erens, & Orford, 2000](#)) to give reliable adult problem gambling prevalence, somehow the national adolescent gambling prevalence surveys are given little credence.
- We agree that it is difficult to compare adolescent gambling research in Britain with that in other countries (p. 4) because of different types of gambling that adolescents have access to, the different cultures of gambling, and the ages at which people are legally allowed to gamble.
- The DCMS executive summary claims that national samples using rigorous designs have found the lowest prevalence rates. What the executive summary failed to highlight is that these are all around the 5% level and that is still a significant problem. This is a deliberate attempt at minimizing the problem in how it was reported.
- The DCMS executive summary says there is no evidence of pools competitions causing problematic behaviours. No-one has ever suggested that they do or would. It seems strange to include this unless it is another way of trying to (overall) minimize the problem of adolescent gambling.
- The DCMS executive summary concludes that there is insufficient evidence to conclude that earlier onset of gambling leads to more problem gambling. This conclusion was based on the citation of just one qualitative study by [Griffiths \(1990a\)](#). The authors omitted British studies by [Ide-Smith and Lea \(1988\)](#), [Fisher \(1993a\)](#), and [Huxley and Carroll \(1992\)](#)—all showing the same finding as Griffiths with larger sample sizes. This finding has also been shown in most other studies around the world (see [Derevensky & Gupta, 2004](#), for a recent full review). The authors even point this out as a general finding by citing the work of [Shaffer, LaBrie, Scanlan, and Cummings \(1994\)](#) on p. 16!
- While there is no study that has shown a direct causal link between other factors (e.g., drug use) and problem gambling, there is a lot of correlational evidence. For instance, [Griffiths and Sutherland's \(1998\)](#) survey of 4500 adolescents and the links between gambling, drinking, smoking and drug taking (which as stated previously was omitted).
- The authors claim only “one study” (i.e., [Ashworth et al., 2000](#)) can be generalized to the current population of young people. This ‘one study’ is in fact three separate studies, the first two of which by Fisher have been published in journals (e.g., [Fisher, 1999](#)). All of these three studies show a similar finding of around 5% of adolescent problem gamblers and yet the conclusion that the authors chose to reach is that 70% of children gamble. This is a deliberate shift towards minimizing the problem of adolescent gambling. This is made even clearer in the final bullet point on p. 4 which states “there is no evidence to suggest the *majority* of under 16 year olds...are adversely affected” (emphasis added). No-one has ever suggested the *majority* are affected. What is clear is that a significant number are affected and that this is two to three times higher than the problem found in the adult population. Since those in the position to make decisions (e.g., Secretary for State) are unlikely to read anything more than the executive summary, they will be given a deliberately misguided and unfair appraisal of the adolescent gambling situation.
- The authors highlight the decline in lottery and scratchcard use over time without mentioning that this is also the case with adults and not specific to adolescents.

## Specific Comments

### *Introduction (pp. 6-11)*

- On p. 6 (end of paragraph 1), it is stated that “the data on children, young people and gaming machines is extremely limited,” which appears to be too strong a statement. There is actually considerable data on that subject, although of course there are many questions that it cannot answer and further research would undoubtedly be helpful.
- The second paragraph on p. 6 is an example of confusing presentation of findings, in this case from the opinion survey carried out by the Gambling Review Body. It is said that 42% of respondents disapproved of children playing on fruit machines with a prize limit of £5. It then goes on immediately to talk about one particular age group and the percentage who said they approved so long as the child was accompanied by an adult. That partial presentation of the results leaves it unclear what was the overall balance of opinion in favour or against children playing fruit machines.
- On p. 6 (paragraph 5), the report is correct to point out that Britain is distinct in allowing legal gaming machine playing by children and young people.
- On p. 7, the authors attempt to put forward the “gambling as experimentation” argument. This again appears to be a presentational device to minimize the adolescent gambling problem. The same arguments can be used for drinking and drug taking yet no-one would suggest that these forms of experimentation are a good thing for adolescents experiencing those activities.
- Paragraph 2 on p. 7 appears to have been plagiarized without acknowledgement from [Fisher and Griffiths \(1995\)](#).
- On p. 8 (paragraph 5), there is confused thinking here (and later on the same issue) about whether problem gambling can be attributed to machines or to the people who play them. Of course that is an intriguing issue and a question to which there can only be a very complex answer. The bold statement that “there is evidence that it is not the machines themselves that are the cause of problematic gambling (see [Prus, 2004](#))” seems somewhat naive.
- The inclusion of “video game machines” as gaming machines is unhelpful in the context of a report on youth gambling. There is a well-established literature in this area quite separate from gambling (see [Griffiths, 2002](#), for an overview).
- The inclusion of personality variables in adult gambling (p. 9) demonstrates the authors have no real understanding of the area’s complexities. For instance, a whole area is summarized with one sentence on sensation-seeking in gambling taken from

a paper almost two decades old ([Blaszczynski, Wilson, & McConaghy, 1986](#)). Given the contradictory findings of the more than 20 studies that have been done on sensation-seeking alone, such selective citations are not helpful to this particular review. Most of pages 8, 9 and 10 is irrelevant for this type of review.

- On p. 9 (paragraph 4), the findings of a [MORI \(1999\)](#) poll reported that 55% of a sample of people who had won at least £50,000 on the National Lottery were happier now than before. But how is that figure to be interpreted? Maybe that is good. However, on the face of it, it seems strange that as many as 45% did not say they were happier after winning that amount or more.
- On p. 10, the second paragraph finishes up with the statement “Gambling is, therefore, an acceptable social behaviour which the majority of adults are involved in.” It is not clear how that conclusion is reached, since the paragraph tells us that a higher percentage of people answering an NOP survey (referenced only on the web) had favourable attitudes than unfavourable towards lotteries (53% versus 20%) but that the reverse was strongly the case for internet gambling (7% versus 63%). The figures are interesting, but where does the conclusion come from?
- The second paragraph (p. 11) referring to the range of measures introduced by Camelot, seems out of place and looks tacked on here.

### *Methodology (p. 13)*

- In their Methodology section (p. 13), the authors claim to have used reference lists from relevant primary and review articles. This is clearly not the case as there would not have been so many omissions. The only British books on adolescent gambling by Griffiths ([1995a, 2002](#)) would have been good starting places. For instance, in [Griffiths \(1995a\)](#), there is a two page table of 23 adolescent gambling studies (pp. 66-67). Only one of these studies (i.e., [Griffiths, 1990a](#)) is mentioned in the review. This hardly appears to be “systematic,” as the authors claim.
- The authors also claim in their Methodology section (p. 13) to have contacted known researchers specific to gambling by children and young people. The first author of this report contacted all the leading people in the field straight after publication of the DCMS report and could not locate one person who had been approached for help on this review.
- The authors assert in their Methodology section to have obtained material from the *UK Forum On Young People and Gambling*. We find this hard to believe, as the UK Forum disbanded seven years ago

and the first author of this report was its national chair for the seven years previous (1990-1997).

- We do not understand why the research team has given an overview about the research methodology (p. 13) without saying: (a) something about the terms they were searching for, (b) how many empirical studies were found overall and (c) what inclusion criteria were applied (e.g., we checked the keywords “gambling” and “adoles\* in the electronic database “PsycInfo” and there were 162 records). What kind of studies were chosen out of these records? Using the terms “youth,” “child,” “teen” and so on, would generate thousands of records.

### *Review: 1. Screening Instruments (pp. 14-17)*

- In the first paragraph of the section on screening instruments (p. 14, paragraph 2), two separate issues seem to be run together here: the accuracy of the South Oaks Gambling Screen (SOGS) and the advisability of measuring current rates of gambling (incorrectly referred to here as “incidence”; presumably, what is meant is point prevalence or 12-month prevalence) or “lifetime” experience. Those are two quite separate issues.
- On p. 14, the authors refer to one of the major new screening instruments developed by Ferris and Wynne, yet it is referenced as Ferris and Harold (sic). A very minor point, but anyone in the field of gambling would not have made this mistake.
- In the last paragraph of p. 15, there is another example of findings being presented in an incomplete and unclear way. This is the Canadian study by Poulin (2000). Prevalence figures are given according to two definitions, broad and narrow. It is then stated that Poulin used a cutoff point of three criteria to classify problem gamblers, but we are not told whether that relates to the broader or narrower definition.
- The authors point out on p. 17 that observational research in the drug and alcohol field has been used to overcome some of the limitations of collecting self-report data highlighted by Parke and Griffiths (2002). So, why did the authors not summarize the relevant observational research that has been carried out by Griffiths (1990b, 1990c, 1990d, 1991a), Trott and Griffiths (1991), Fisher, (1993b), and Griffiths and Minton (1997)? This appears to be a major omission.

### *Review: 2. Prevalence (pp. 18-26)*

- Much weight is attached to the report by Ashworth et al. (2000, pp. 18-20). We do agree that this is a major piece of research but the main findings on p. 19 are nowhere to be found in the executive sum-

mary. Surely if it is the only study that the authors think is methodologically sound with regards to adolescent problem gambling they should at least flag this up in the executive summary. Once again, the problem has been minimized and the main finding suppressed. On p. 20, the authors conclude that adolescent problem gambling has “plateaued” without mentioning the fact that the figures are still worryingly high.

- The authors of the report appear to be very impressed by the tracking survey carried out by Ashworth et al and they quote it at length (but why is there no acknowledgement of Fisher’s, 1999, study, which presumably constitutes the first data point in the tracking study, and which is at least in the published literature, unlike Ashworth et al.’s, 2000, study, which presumably has not yet been peer-reviewed?). They make a lot of the changes that appear to have occurred between 1999 and 2000. Although the possibly unrepresentative nature of the sample is pointed out, some rather confident conclusions are drawn. For example on p. 20 it is stated that that the study, “suggests that both gambling per se and problematic gambling by young people under 18 appears to have plateaued.” That will be a comforting conclusion for Government and the gambling industry, but more caution should be exercised in drawing conclusions about longer-term trends from changes over one 12 month period suggested by a study that is yet to be properly peer reviewed. The authors say several times that there is insufficient research to suggest the seriousness of the size of the juvenile gambling problem in Britain. However, the prevalence of problem gambling among under-16-year-olds in 1997, as suggested by the Ashworth et al. (2000) tracking survey, was 5.6% apparently falling to 5.4% in 1999 and 4.9% in 2000. That appears to confirm the conclusion that many of us have been coming to that the prevalence of problem gambling amongst adolescents in Britain is several times higher than the prevalence amongst adults, and that, at around 5%, it is a very serious problem indeed. Research in the US has drawn essentially the same conclusion, that adolescence is a period of particular vulnerability for problem gambling.
- The authors use the national prevalence study by Sproston et al. (2000) to highlight trends yet seem to omit the fact that problem gambling is at its worst during adolescence. The 5% adolescent problem rate is again highlighted on p. 22 and yet this is still seen to be something that we should be unconcerned about. The DCMS accepts the 1% level of adult problem gambling and sees this as something to be concerned about yet why no concern for a prevalence figure 5 times higher?

- On p. 22 (paragraphs 1 and 4), [Moore and Ohtsuka's \(1997\)](#) report from Australia is cited several times on this page. Their study is incorrectly interpreted as being a prevalence study. In fact a close reading of that study makes it clear that they used no standard screening measure, although they do report rates of individual problems (e.g., 29% reported often trying to win back money lost in gambling; 14% said they had gambled more than was meant at times; 8% had sometimes tried to keep the amount gambled secret from family and friends). It is misleadingly stated that 3% only of their 14 to 25 year-old respondents classified themselves as problem gamblers, but that is very different from (and is always considerably lower than) the proportion who score positively on a screening instrument. The report appears to go on to draw the conclusion from that study that rates of problem gambling may be low when positive attitudes are held towards gambling activities, as may be the case in Australia. In the earlier paragraph on that page the report cites the surprising statement from the [Moore and Ohtsuka \(1997\)](#) paper, that their results were, "... in line with generally low estimates of problem gambling emanating from surveys of the general population of Australia" ([Moore & Ohtsuka, 1997](#), p. 228). That is of course an extraordinary statement since Australian surveys put that country in the lead regarding gambling and problem gambling world-wide, and Australian authorities have expressed extreme concern about gambling problems there. The report's use of [Moore and Ohtsuka's \(1997\)](#) work is a bad example of loose argument.
- We have only made a few specific comments on the country-by-country comparisons (see below). We shared these country-by-country "mini reviews" with colleagues in the respective countries and they were almost unanimous in their view of the highly selective nature of the reviews. Please see the selection of the comments at the end of this report in the [Appendix](#).
- On an international level, there are many important and large-scale studies missing, such as [Volberg's \(2002\)](#) research, as well as the latest findings from the meta-analysis of [Shaffer and Hall \(2001\)](#). There is additional research from Spain (2001) and Romania (2002) that clearly demonstrates the robustness of the phenomenon of "problem gambling in adolescence" within Europe, despite differing regulation policies (see [Hayer, Griffiths, & Meyer, in press](#)).
- The largest non-national study of adolescent gambling by Wood and Griffiths ([1998, 2002](#)) on 1200 children in the East Midlands (p. 25) is reported as giving lower rates of problem gambling than the

general population. However, the executive summary says that it is the studies using the largest samples and widest coverage find the lowest rates. There is clearly inconsistency between what is in the report and what is reported in the executive summary.

### *Review: 3. Characteristics and Motivations of Problem Gambling Behaviour (pp. 26-31)*

- Almost an entire page (pp. 27-28) is devoted to reporting a single study of kindergarten children playing a game with chips. Although such a study may have some bearing on the topic of the paper, giving it such space is an illustration of the arbitrariness with which evidence has been drawn on. The study outlined by [Tremblay, Huffman, and Drabman \(1998\)](#) while interesting tells us little about the real situation concerning adolescent problem gambling in the UK. Is it really the case that a study with 102 children playing for sweets should demonstrate "the potential normalcy of gambling games for children" and that "early gambling behavior...promoted responsible behaviour." Maybe the conclusion is right, but it cannot be drawn from this experiment. Again, this appears to have been used to dilute and minimize the UK adolescent problem gambling situation.
- Concerning risk factors in adolescence, almost no research with neuropsychological focus was cited (e.g., [Chambers & Potenza, 2003](#))
- On p. 24 (last paragraph), Moore is cited here again with a quotation, as if this has the same status as a finding ([Moore & Rosenthal, 1993](#)). [Moore and Rosenthal \(1993\)](#) are cited as describing gambling as a "common and fairly benign characteristic of the youth experience, not unlike experimentation with sex, alcohol, and other 'acting out' behaviours." That is again an extraordinary statement that begs all sorts of questions about the dangers connected with various youthful experiences. Drug-taking is notably missing from the passage quoted.
- On p. 25 (paragraph 5), when reviewing studies of Norwegian youth, it is stated that prevalence rates found in that country are broadly comparable to the [Ashworth et al. \(2000\)](#) tracking survey prevalence rates for British youth in 1999. But the paragraph immediately goes on to say that the cut off points on the screening instrument were different in the two countries (a higher threshold in the British study) and that the Norwegian study covered all forms of gambling whereas Ashworth et al covered only lottery and machine gambling. The correct conclusion therefore might be that the British rate was considerably higher than the Norwegian.

- The sub-section on “Characteristics and motivations of problem gambling behaviour” (pp. 26-31) is an unsatisfactory section because it tries to cover such a large topic, and inevitably is rather arbitrary and, in places, quite confused as well.
- The fourth paragraph on p. 26 is a particularly confused paragraph. It seems to suggest that there is evidence for a link between problem gambling and impulsivity (although it does not discuss the difficulties of defining and measuring impulsivity), but then it cites work that seems to contradict that, and goes on to introduce further complicated ideas such as loss of control, and interaction between learning and negative moods.
- On p. 28, the authors somehow think that nine problem gamblers out of 50 is 4.5%. It is, of course, 18%. This study was a qualitative study that was in no way trying to be representative; moreover, this research was clearly not a prevalence survey. Why was this qualitative study and that of [Wood and Griffiths \(2002\)](#) reported and not others? For instance, the authors failed to mention either observational studies (e.g., [Fisher, 1993b](#); [Griffiths, 1990b, 1990c, 1990d, 1991a](#)) or case studies ([Griffiths, 1991b, 1993a, 1993b, 2003](#)). There seems to be no rationale underlying the choice of studies that were included and those that were omitted.
- A level of pathological gambling found in the study by [Griffiths \(1995b\)](#) is cited (p. 28) as if it were a prevalence study, but that was not its purpose.
- On p. 28 (paragraph 5), a study by [Gupta and Derevensky \(1998\)](#) is used to suggest that adolescents may “grow out” of gambling problems as they mature. Of course many do, since there is a strong negative correlation of problem gambling with age, as there is for alcohol and other drug consumption. That is not the point. The issue surely is whether adolescents experience problems that need to be taken seriously when they occur in adolescence (whether or not they later grow out of them), and secondly the numbers of people who do not grow out of problems and for whom problem gambling becomes more chronic. (The same point is made on p. 30 when the authors draw on [Orford’s, 2001](#), model of excessive appetites to support the idea of maturing out of problems.)
- The “addiction” overview (sub-section 3.4; pp. 29-31) is so selective as to be almost meaningless in the context of problem gambling. To solely concentrate on [Orford \(2001\)](#) misses the complexities and nuances in the field. The point of the report was to review the adolescent gambling literature. The addition of this section again makes an overall assessment less clear. Problem gambling behaviour derives from a complex combination of biological, psychological and social factors. It is naive to suggest that universal causal relationships can be identified.
- Furthermore, it is certainly true that gambling addiction is the result of an interplay between risk factors related to the individual, the environment and the substance or technology. It is also true, however, that one can differentiate the addictive potential by analyzing the structural and situational characteristics of different gambling forms (e.g., fruit machines vs. bingo). Therefore, specific changes of gambling forms will most likely change their addictive potential.
- p. 30 (paragraph 3) contains an important but unsupported statement that gambling research, in contrast to alcohol and drugs research, appears to suggest two distinct groups of gamblers rather than a spectrum.
- The work of [Ide-Smith and Lea \(1988\)](#) is cited in reference to environment yet was omitted in reference to age of onset.
- The [Ashworth et al. \(2000\)](#) study is used to try and show that parental approval of gambling activity had declined yet the decrease is clearly non-significant statistically.
- The first paragraph of p. 31 contains the important but potentially misleading conclusion: that, if the causes of addiction are multi-factorial ([Shaffer, 1999](#)), then changing the opportunities for gambling or making specific changes to gaming machines will not in themselves affect problem gambling prevalence in a simple and linear fashion. But the evidence surely suggests that opportunity to gamble is one of the biggest factors. Simply because there are other factors involved as well, does not mean that opportunity does not have quite a straightforward effect on prevalence.

#### *Review: 4. Environment (pp. 31-34)*

- When discussing familial influences (p. 33), the authors cite [Becoña, Labrador, Echeburua, Ochoa, and Vallejo \(1995\)](#) to the effect that there is such a social acceptance in Spain of parents showing their children how to play gambling machines, that it would be difficult to prohibit under-age gambling. That is an extraordinarily negative statement, particularly since gambling machines are relatively new in Spain, so there can be no long-term tradition of family involvement in gambling machine playing in Spain. Even if there was, if it was concluded that parents were putting their children at risk by encouraging machine playing then from a public health point of view the right thing to do would be to try to educate parents about the dangers involved and to alter their behaviour (in much the same way

as it may be thought appropriate from a health point of view to educate parents about the nutritional value of different foods and the risks of childhood and later obesity, or about tobacco smoking for example).

#### *Review: 5. Offending Behaviour (pp. 34-35)*

- The authors spend considerable time arguing about what they feel is wrong with studies without pointing out the relative strengths or putting the studies into context. A good example of this is the study on crime and machine use by [Yeoman and Griffiths \(1996\)](#). This was a very innovative study and the only one of its kind that the authors are aware of. Pointing out limitations that the original authors were well aware of is hardly original. The authors also criticize a study by Griffiths (2002; sic—actually 2000) and do not appear to realize that the group studied was not meant to be representative but came from a deprived area and were mostly of Asian and Afro-Caribbean ethnicity. Given the paucity of research on these ethnic groups, this study should have been highlighted in terms of possible risk factors increasing the incidence of adolescent problem gambling. The study was also criticized because the data were collected by a research student. This makes no difference whatsoever and should not even have been mentioned by the authors. The authors also criticize the study by [Wood and Griffiths \(1998\)](#) for relying on third-party assessments of parental attitudes and behaviours; however, [Ashworth et al.'s \(2000\)](#) study involved a similar approach, yet it received no such criticism. The qualitative studies on adolescent lottery play by [Wood et al. \(2002\)](#) and [Wood and Griffiths \(2002, pp. 38-39\)](#) are somewhat belittled as being qualitative. These were very detailed papers but got a paragraph each which shows where the preference of the report authors' lie.
- The short sub-section on gambling and offending (first paragraph on p. 35) finishes with the potentially misleading statement that there are wider problems related to machines and children and young people than simply problematic gambling. That statement appears to be minimizing the importance of problematic gambling on the grounds that the relationship between problem gambling and offences such as stealing is not a simple one. But exactly the same point has been made by several authors regarding the relationship between drug-taking and offending. But no-one would deny that problems directly associated with youthful drug-taking need to be taken very seriously by society. Why should the same not be true for problem gambling?

#### *Review: 6. Small Scale Related Studies (pp. 35-39)*

- The sub-section 6.6 on underage Internet gambling (pp. 38-39) was also highly selective, and completely omitted both the only prevalence study in the UK (which included a small proportion of adolescents; [Griffiths, 2001](#)) and the published work on the poor social responsibility practices shown by UK online gaming operators in relation to adolescents ([Smeaton & Griffiths, 2004](#)). What were the authors' reasons for leaving out the few studies that have been conducted in this area?

#### *Findings (pp. 40-41)*

- In the Findings section (p. 40), the extent of adolescent problem gambling is again minimized by leaving out the 5% figure from the tracking study. The authors claim work on Internet gambling is "non-existent." This simply is not true (see section above). [Ashworth et al.'s \(2000\)](#) study is criticized for only looking at fruit machines and lottery games. Sue Fisher originally designed the study to look at these two particular activities because previous research had clearly indicated that these were the main activities of concern. Research is attacked because it has not used standardized instruments. There seems to be an assumption that the only way to tell if gambling causes problems is through the use of a screening instrument. This is clearly not the case as a whole host of studies carried out between 1987 and 1991 used other behavioural measures (e.g., stealing money to gamble, truanting from school to gamble, getting into trouble with teachers and parents because of gambling etc.), and demonstrated that for a significant minority, gambling was problematic. Early onset is again ignored as a possible risk factor even though only one out of four studies was reviewed and all international evidence was disregarded too.
- In the Findings section (p. 40), most of the points made here are fair, but some of the most important are misleading. In particular the first and third bullet points make it sound as if evidence is lacking on the prevalence of problem gambling in adolescence. The second author's conclusion on reviewing the evidence ([Orford et al., 2003; Ch. 3](#)) was that there was rather good international agreement that problem gambling rates in adolescence were substantially higher than those in adulthood, and that prevalence was of the order of 5 to 6% in Britain and in the range for 4.5 to 7.5% in the USA and Canada. This seems to constitute fairly solid evidence that problem gambling amongst adolescents constitutes a problem to be taken very seriously.

- In the Findings section (p. 40), it is claimed that only three studies provide evidence of underage lottery and scratchcard use. This is simply not true. Even in the authors' own review they cite five studies (Ashworth et al., 2000; Griffiths, 2000; Pugh & Webley, 2000; Wood & Griffiths, 1998, 2002). There are other studies omitted including one of the biggest with 4500 adolescents (Griffiths & Sutherland, 1998). The authors again repeat the claim that national prevalence studies show lower rates than regional samples when this is not the case and again problem gambling is minimized by making reference to the fact that the majority of adolescent show no adverse affects (p. 41). One of the main conclusions contradicts itself by saying that there is no evidence that early onset is more problematic (p. 42) and then in the next paragraph states that there is evidence.
- Although it is true to say that there is no clear longitudinal evidence to support the theory that early onset is a predictor of later problem gambling (bullet point 8), there are several studies suggesting that link (including Shaffer et al., 1994, cited by the authors on p. 16, and Griffiths, 1995b, cited on p. 28). There is also evidence from closely related fields (e.g., alcohol and drug consumption and sexual behaviour), as well as behavioural theory, which should lead one to be confident of such a link even in the absence of very hard-to-come-by longitudinal data.
- The authors make a lot of the apparent decline in lottery and scratch card playing by adolescents in the tracking study (bullet point 15), but, as stated earlier, trends are notoriously difficult to establish from a limited number of data points, and in any case, the apparent rate of problems remains alarmingly high.
- The first paragraph on p. 42 is muddled because it makes no distinction between current problems for adolescents and problems later in life. Nevertheless, it seems to make a strong statement that people in the field have assumed a relationship between youth and onset of problem gambling to be true rather than it being proven.
- The first part of paragraph 2 (p. 42) seems odd, saying that some research studies claim 4-7% of adolescents gambling pathologically while others find between 5 and 6%. There does not seem to be much difference in those two estimates except for the range!
- There is a citation here from Prus (2004, p. 42) that is peculiarly phrased and impossible for us to understand.
- On p. 42 (paragraphs 4 and 7), the language used becomes polemical when making the point that the majority of children and young people gamble without having problems. In paragraph 4 it is said to be "crucial" to remember that fact. In paragraph 7 we are told that "It should not be forgotten amidst all the debate of the potential dangers..." that many people enjoy it. In the latter paragraph there is also reference to problems likely to arise "in a small minority of cases..." No-one has suggested that problem gambling is experienced by the majority of young people who gamble, and it would be ridiculous to suggest that. The public health issue is about the dangers of gambling, the prevalence of problem gambling, the addiction potential of certain forms of gambling, and the particular vulnerability of young people. In the context of that public health research and debate, why is it "crucial" to remember that the majority gamble without experiencing problems? And why is around 5%, which by any public health yardstick is a very high figure, referred to as "a small minority." The latter expression is one that we know is favoured by the gambling industry (the same preference is shown by the drinks industry in relation to drinking problems, which also affect only a minority of people, but which are recognized to be of major public health importance).

### *Conclusions (pp. 42-43)*

- In general, the all-important Conclusions section of the report (pp. 42-43) seems to go much further than the evidence would allow, and shows clear signs of bias.
- The conclusions state that 4-7% adolescents gamble at a pathological level. Surely this is a significant problem and needs to be flagged up in the executive summary? The authors claim there is very little research on time spent playing, money spent, where the money comes from etc. Almost all of the early research on fruit machine gambling (1985-1992) collected this type of data. The authors have completely ignored all the early research in this area and cannot claim that this type of data has not been collected.
- The first paragraph on p. 43 seems to suggest that the gambling research field has been remiss in not investigating the positive value of gambling. It implies that gambling may have value as a form of controlled excitement, as a way of coping with stress, and in developing responsible adult gambling. It is not clear why such a statement should appear in a relatively brief conclusion section to what purports to be a scientific review paper, and it does make one question the independence of the authors.
- The second paragraph on p. 43 deepens that concern. The paragraph correctly questions the validity

of problem gambling screening instruments. But it goes further in giving two hypothetical examples of possible false-positive item responses to questions derived from the *DSM-IV* criteria. There is a large literature on the validity of screening instruments (e.g., Orford, Sproston, & Erens, 2003), some of which the authors review. It is a complicated subject, no screening instrument in any field is perfect (if detection were that easy, screening instruments would hardly be necessary at all), and there is reason to expect instances of false negatives as well as false positives. To describe only the false positives is exactly what those who wish to downplay gambling problems wish to hear.

- The final paragraph on p. 43 will also give comfort to those, including the government and industry, who wish to claim that legislative change will make no difference to the volume of gambling or prevalence of problem gambling. It may be true to say that there is no research evidence to prove that the regulatory regime affects juvenile gambling and problem gambling, but that is presumably because the research that would enable us to draw conclusions on that topic does not exist. To suggest that the way an activity such as gambling is regulated has no influence on behaviour or problem rates is surely to fly in the face of history and common sense.
- On p. 43, a 12 year-old quote by Fisher (1992) is used to sum up the current field of adolescent gambling research. Why did the authors not quote from Fisher's more recent work, such as her 1999 prevalence study (i.e., Fisher, 1999)? What she reports in this later paper is very different from what she wrote almost a decade earlier.
- On a minor note, there are many references included that do not appear in the text. For instance, three papers by Griffiths (1993a, 1993b, 1995b) are in the reference list and not even referred to in the text of the report. The authors were clearly aware of these papers by the fact that they were in the reference section, but they were omitted from the review.

### Personal Communication with the Lead Author

We would also like to point out that the lead author of this report (MG) has now had a number of conversations with the lead author of the DCMS report (Corinne May-Chahal). When MG first contacted CM-C on November 29, 2004, she was unaware that her team's report had been published by the DCMS. The many omissions were pointed out to her and she claimed that her team only had two months in which to write the report, and that they were not able to read

everything in this constrained period. This hardly ties in with being a "systematic" review. Given the authors had no expertise in either gambling or adolescent gambling, this must have been a hard task to do. If the team had approached us and others in the field, we could have sent the report team a lot of material to review. However, our help was not sought. MG also had a conversation with CM-C on December 8, 2004. She was asked what she thought of MG's initial criticisms and replied that they seemed to be right and that MG "was the expert."

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## Appendix

### Other Comments on the DCMS Report

In this Appendix we have briefly collated some comments and feedback from others in the international research and practitioner community about the DCMS report.

Jeff Derevensky (McGill University, Canada): “A very poorly done review.”

Masood Zangeneh (Centre for Addiction and Mental Health, Toronto, Canada): “I managed to quickly read through this report and found the inconsistencies that you mentioned. The authors were highly selective in their review for sure. Please feel free to include my name in the critique that you will be sending to the DCMS.”

Thomas Nilsson (Spel Institutet, Sweden): “Two things are clear: I have not been contacted and I agree, they have made some serious mistakes. So include my name in your critique.”

Judy Abbott (Youth Action Group, Australia): “From a quick scan of the section on young Australians who gamble (pages 22 and 23), if the summary given there of Australian research (namely one study) is anything to go by I fear for the rest of the report as it hardly represents the scene over here. There is no mention as far as I can see of Alun Jackson’s research nor anything else of significance.”

Kerry Sproston (National Centre for Social Research, UK): “It seems odd that it’s a review of ‘adolescent’ gambling, and many of the figures they quote are for those aged 16-24 from the (UK) prevalence survey. That’s a late puberty!”

Faith Freestone (Gordon House, UK): “I am surprised that Sue Fisher’s research wasn’t there. We (Gordon House) weren’t contacted at all about this. What are the credentials of those who undertook the research? I haven’t come across them before.”

Henry Lesieur (US): “You may include my name as agreeing with the general tone of your criticisms. I have looked at the executive summary of the UK Overview and it appears to make many of the errors you pointed out. I was particularly surprised that they did not point out the dangers of machine gambling among youth in the UK that both you (Mark Griffiths) and Sue Fisher have so eloquently documented.”

Harold Wynne (Alberta, Canada): “I share your concerns, so feel free to include my name in the covering letter or critique.”

Jackie Lemaire (Addictions Foundation of Manitoba, Canada): “I would agree that the ‘overview’ is highly incomplete and lacking significant research that has been done in this field.”

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