Fixed Odds Betting Terminals, the Code of Practice, and problem gambling

Second round research

The combined response of Europe Economics and Ipsos MORI to the peer reviews

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INTRODUCTION

In the following commentary we reproduce the report of each reviewer and insert our own comments where relevant. Our comments are highlighted in yellow, so, for the avoidance of doubt, any text not highlighted after this introduction is that of the reviewers. We have amended the reviewers' paragraph formatting only in order to produce a consistent appearance in this document; their text is of course left unchanged.

Comments on the review of Professor Linda Hancock

Assoc. Prof. Linda Hancock
Director, Corporate Citizenship Research Unit
Deakin University, Melbourne, Australia
Former Chair, Independent Gambling Research Panel, Victoria, Australia.

Review Brief:

Are the questions posed by the study sensible? If not, what would be more appropriate?

- Is the study well designed to answer the study questions?

- Are the conclusions drawn supported by the evidence collected?

- What are the policy implications of the study if any?

- Any other matters you wish to raise?

The Study Brief:

"To measure and explain levels of problem gambling amongst FOBT users, in the context of benchmarks of other gambling activities (particularly machines) both within and outside of the betting shop.

To assess the effectiveness of the FOBT Code of Practice, and the individual elements within it, in providing protection against problem gambling."

Para 2.1.6 of the previous Round 1 (Europe Economics 2005) report (refers to a desire to assess the impact of the code after twelve months of operation.

Introduction

The policy context

This study raises important public policy issues since FOBTs have been recognised as having characteristics that are similar to gaming machines outside casinos and hence the ABB Code of Practice is aimed at addressing fears about the detrimental impact of FOBTs placed in localised non-casino High Street locations that are readily accessible. [**Comment**: most betting shops may

fairly be described as on the High Street, but their accessibility is limited to those aged 18 or over, and among this adult population under 3 per cent choose to visit them regularly.] It would appear that revenue from FOBTs is rising (GamCare 2004; Bettingmarket 2006[1]) and GamCare reported 'a steady growth in the number of callers to the GamCare Helpline and those attending for face to face counselling mentioning FOBTs as their primary form of gambling' (GamCare 2004). [**Comment**: we do not regard GamCare statistics, derived from telephone calls, as reliable for our study. They are in a statistical sense a biased sample. The fact that revenue from FOBTs has been rising is not by itself an indicator of any particular social problem, nor were we asked to look at the financial returns of FOBTs.] So a key issue for this Europe Economics research is whether rising (and perhaps consolidating) revenue is due to the roll out of FOBTs (said to have now levelled off), players using machines recreationally (and the extent to which the Code contributes to responsible gambling), whether the machines as presently configured encourage problem patterns of play by nature of the product and or its accessibility; and whether they cause broader community impact problems when players spend funds otherwise needed for family and household budgets.

The UK government's concerns are understandable, given Canadian, Norwegian and Australian findings on the impact of localised access to continuous play gaming machines, and the need to put in place measures aimed at preventing development of problem gambling and keeping gambling spend within household budget limits. [**Comment**: while spend on FOBTs is rising, this appears to be at the expense of other forms of gambling, particularly betting on horse and dog races. In other words there is some redistribution of gambling spend.]

Overviewing the two main parts of this study report:

Main points

Review of the Omnibus survey (some issues are discussed further below)

(1) the Omnibus survey was conducted within budget limits unknown to the reviewer (and of relevance to assessing whether a national stratified random sample was possible; since this would be preferable to a non-probability quota sample).

[**Comment**: a stratified probability sample would be preferable to an Omnibus quota sample if budget permitted. Nevertheless, quota sampling is a recognised and widely-used methodology with a good empirical record. For the sample size adopted here (12,000 respondents) a stratified random sample would have cost between five and six times as much as a quota-based sample.]

(2) The chosen methodology of a 'quota-based, multi-purpose survey' puts the methodology into a category where the internal results may be used to discuss various relativities; in this case to create a 'context of benchmarks of other gambling activities (particularly machines) both within and outside of the betting shop'. However as a non-probability sample, these study results have dubious validity in comparisons over time (ie with the previous Europe Economics 2005 study) or with the UK 1999 prevalence survey. Although the report acknowledges various limits on such comparisons, they are nevertheless made –at times quite forcefully- and should probably be removed from the report or couched in extremely broad terms.

[1] 'At William Hill each machine is contributing 350 pounds per week to EBIT, whilst at Ladbrokes, the figure is 304 pounds (bettingmarket 2006).

[Comment: we disagree. One good test of any methodology is replicability of results. Well conducted quota surveys can report consistent findings over time in the measurement of population characteristics which are unlikely to have changed, and a degree of variance similar to that which would be expected from a probability survey of the same sample size, while at the same time showing proper responsiveness to change where this occurs, and which can often be verified by matching tends in parallel probability surveys. This is generally true of the MORI Omnibus. It might be noted that in the 1970s, when probability and quota surveys were both being used for election prediction in Britain (one of the rare opportunities to compare attitudinal survey data with unambiguous empirical truth), the record of the quota surveys was uniformly better than that of the probability surveys; none of the research companies now uses pure probability sampling for election prediction.

While this is only an inductive justification of quota sampling, any assumption that probability sampling rests purely on more certain grounds of arithmetic proof may be mistaken. Many of the theoretical advantages of probability sampling rely on the assumption of a high response rate, which in practice can rarely be achieved; the possibility of non-response error (i.e. that failure to respond is not random, and that non-response may well be correlated with some of the characteristics being measured by the survey) means that there can be no more absolute assurance that the standard confidence limits are really applicable to a probability than to quota sample.

It seems odd to single out in particular for attack the validity of comparisons between quota samples over time. On the contrary, since the most obvious weakness of quota sampling is that it might introduce a consistent bias into findings through its means of respondent selection within the confines of the quotas, the comparison of different surveys over time using the same methodology is one of the tasks for which it is best suited, since there is a reasonable expectation that the consistent bias may cancel itself out, leaving a clean reading of change over time.]

(3) The major problem with the Omnibus survey is the ambiguity of question 1, which relates to all respondents and is the only measure in the survey of general population data on participation in various forms of gambling. [**Comment**: this is incorrect - Q2 goes on to ask about individual forms of gambling in more detail. We needed to ask a general question first, as in the current Q1, so as to eliminate respondents who do not gamble at all (or less than once a month). Waves 1 and 2 show that over 50 per cent of respondents do not gamble at least once a month and waves 3 to 6 show over 90 per cent. If we did not initially exclude these respondents it would add significantly to the cost of the Omnibus survey.] Although it asks about betting shop gambling, there is no item asking respondents about use of FOBTs in question 1. This question resulted in ambiguity as it *asked about 'overall frequency of gambling across all forms of gambling (using a single code for all)' rather than for each activity.* This resulted in omission of important data of the general population reported frequency of use of each gambling activity and in particular FOBTs (which were not included in the list). From that point on, the researchers use a filter question restricting on-going respondents to those classified as 'regular' defined as gambling at least once per month. This cut out casual gamblers gambling less than once a month who might be problem (binge) gamblers or 'at risk' of problems. This means that the frequency of activities and all subsequent data is restricted to once a month or more

'regular' gamblers, with no capacity to identify those at risk of problems or to undertake comparisons that include non-gamblers and casual or non-regular gamblers (including binge or pay-day gamblers). [**Comment**: the essential issues here are whether it was right or wrong to eliminate respondents who gamble less than once per month and whether we should have made explicit reference to FOBTs in the Omnibus. On the first point, there is no one right frequency that makes all others wrong. Even if we had set a higher value than one month as the cut-off for identifying the relevant subset of population, we would still have risked missing occasional gamblers. Within research, a general rule for regular is defined as once a month or more often. If you extended this, to say once a quarter, you would no doubt pick up a number of respondents who only gamble on 'big events' such as the Grand National, the Derby, Cheltenham Festival, major golf/tennis tournaments, and football/rugby cup finals. Furthermore, a limit of once a month or more often will pick up these so called 'binge' gamblers mentioned. UK practice means that these gamblers will be paid weekly, fortnightly or monthly and so will fall within the spectrum of regular gamblers as we have defined them. On the second point, if we had specified FOBTs as one of the forms of gambling to be considered, we should logically have had to include the other forms of gambling that go on in betting shops, and, arguably, greater detail under other headings too, such as different forms of play available under the National Lottery. It seems to us perfectly sensible to ask "high level" questions to begin with and then proceed later to greater detail (which we do).]

(4) A further issue is the use of DSM-IV as a problem gambling screen, which has no capacity to pick up those 'at risk' of problem gambling. Progressive gambling researchers are using screens such as the CPGI (Canadian Problem Gambling Index) (to obtain a base sample of a cross section of problem gamblers and those at risk) or a range of measures (see for example the Focal Research study 1998). In their later research Focal have used a 'modified CPGI', which focused only on video lottery use (in their case); so that the problem gambling could be tagged to that form of gambling in the cases of multiple forms of gambling. This is a specialized area and perhaps calls for such research to be informed by gambling research. [**Comment**: the CPGI was not realistically available to us for Round 1, which meant that it could not be used for Round 2, since both rounds were required to be as near-identical as possible. In any event, we were looking at problem gambling overall (i.e. among FOBT players, non-FOBT players and others), so the same questionnaire needed to be used to cover all gamblers for a reliable comparison.]

(5) An additional problem with use of DSM-IV in this study was the high non-response rate, mainly due to the decision to delete respondents who did not answer all 10 questions (when interviewers could not pick up the non-responses, mostly to the last question, as the screen was privately self-administered). This reduced the number of respondents in the problem gambler sub group and with the small numbers involved, may have contributed to distortions. [**Comment**: with the exception of six respondents, those who did not complete the DSM-IV questions did not complete any of them. There is no sub-sample who scored three or more positive answers but were not classified as problem gamblers on the grounds that they did not complete all ten questions.]

(6) The researchers have a large sample for this study (n= approx. 12000) so it is a pity that a probability sample was not drawn. Nova Scotia conducted a highly respected and

statistically robust study with a general population survey of 400 and a 'video lottery' players survey of 711 (which screened 9,339 households and 18,650 adults and interviewed all adults over 18 in the chosen households in a telephone interview) (Focal Research 1998). Although larger than this study the lack of detail on actual choice of sampling units (respondents) make judgment of the possibility of alternative methodologies difficult. In this study, multiple forms of gambling for FOBT machine users is used to imply that the problems stem from gambling other than FOBTs. A screen like the modified CPGI discussed above focused specifically on FOBTs, would overcome this ambiguity.

[**Comments on paragraphs 4, 5 and 6**: we considered only the SOGS and DSM-IV tests because (to the best of our knowledge) only these have been applied in the UK, specifically by the Prevalence Study, which, before Round 1 began, we considered as a useful point of comparison. In fact reliable comparisons with the Prevalence Study proved to be difficult, but we were by then committed to the DSM-IV for Round 1, and therefore for Round 2 as well. Interestingly, the second UK Prevalence Study, recently commissioned, retains use of the DSM-IV screen. It is the SOGS screen which is replaced by the CPGI. That apart, no DCMS commentator suggested we should use, or even look at, other problem gambling screens. On the question of sample size, it appears that Prof. Hancock has confused the Omnibus survey with the betting shop survey – the 12,000 sample is taken from the Omnibus study.]

(7) The Omnibus survey is extremely short and seemed to miss the opportunity to ask further questions pertinent to FOBT policy such as community attitudes as well as frequency of involvement for all in all types of gambling including FOBTs as discussed above[2]. [**Comment**: by definition, Omnibus questionnaires have to be relatively short. Ours was not "extremely short", and the questionnaire covered its specific objectives. We stick to our view that the Omnibus surveys were well conceived for obtaining a general gambling context in which to fit the more detailed evidence obtained from the LBO survey. Note also that Q2 within the Omnibus study asks about frequency of involvement in all types of gambling (and Q5 specifically for FOBTs).]

(8) The write-up of sampling and methodology needs to be clearer and include details such as how the survey was administered and what the 10 units in each cluster were. (These details may have been missed but were they households, individuals interviewed on the street etc?). Use of flow diagrams would help explain waves 1/2 and 3/6 and the differences in questions asked.[3] [**Comment**: this Omnibus study, like all Omnibus studies,

[2] Questions included: On average, how often, if at all do you spend your own money on any of the following activities?
What do you consider to be the main reason why you gamble?
- For what other reasons do you gamble?
- Before today, have you ever heard of, or come across, betting terminals in betting shops? (The interviewer then uses a show card to clarify what a FOBT is.)
- On average, how often, if at all, do you use betting machines?
- How much do you spend in an average week or month on all types of gambling? (The interviewer then clarifies 'spend'.)
- Then proceed to the self-completion DSM-IV questions listed in Appendix 4.

[**Comment**: The footnote below is incorrect – Prof. Hancock has confused the Omnibus and LBO studies.]
[3] The 2004 survey waves 3-6 included 945 interviews in 86 interview shifts in 43 betting shops.

interviews single respondents, face-to-face in the home. The technical detail given at Appendix 2 fully explains the methodology and the differences between waves 1 & 2 and waves 3 to 6.]

On data presentation

(9) The write-up of results needs in all cases to cross reference general results statements with the table in the relevant appendix which gives the appropriate data. Without this the reader has to scroll through countless tables to find the source. Some more tables could be usefully included in the report itself and use of pie charts and other modes of presentation would enhance the report. At all times, the base sample and number should be included so that where numbers are small, results may be judged accordingly. [**Comment**: we disagree about including more tables in the main report. Because of the compression required for a MORI table to fit on an A4 page, and the fact that one "table" (in MORI terms) spreads over several pages, we took the decision to keep all the survey tabs in appendices. This report was written for a non-specialist audience, not for an academic reviewer.]

(10) All appendices need to be headed with the appendix number and content title and the index supplied needs correct page numbers. It appears that all are headed the same 'ABB FOBT Research' which is confusing. The Omnibus waves 1and 2 and the Betting shop results all need separate headings on all pages, consistent with the appendix title. Wave 1 and 2 and wave 3-6 results should be headed Omnibus survey: wave 1/2 and 3-6; and the other titled: Betting Shop Survey for clarity. [**Comment**: we will amend the headings as suggested.]

(11) Table 2 (question 1) on ABB FOBT research is confusing. It says it is based on all respondents but the data on FOBTs and weekly spend would appear to draw on later questions asked only of those who gamble once a month or more. [**Comment**: it is not clear what page or table Prof. Hancock is referring to. If she is referring to GAM2 in the waves 1&2 Omnibus, the tab makes quite clear that the base is "all who gamble at least once a month", not all respondents.]

The Review of the ABB Code of Practice Governing the Supply and Use of Fixed Odds Betting Terminals in Licenced Betting Offices

(12) As a review of a code of practice, this study was focused on only some elements of the code as presented in Appendix 3 and on the ABB website, but excluded other important dimensions. This was a mystery to this reviewer as the elements in the code focused upon in the study seemed to focus somewhtat on technical or regulatory aspects which have been put in place to protect players but about which (for some) one would not expect players to know. Both the Appendix and the ABB website are clear that the FOBT

The 2005 survey waves 3-6 included 1.545 interviews in 130 interview shifts in 65 betting shops. This is an increase of

A.1.2.6

'We estimate that there are now around 20,000 FOBTs installed across some 8,500 UK betting shops'.

Despite the increase in sample size in Round 2 which gives more robust numbers for analysis, representativeness of results still hinges on the sampling method.

Code is to 'operate in conjunction with the Good Practice and Social Responsibility Code for Betting Offices agreed by ABB and GamCare' (ABB 2006). Therefore one would expect that a broader examination of the Code would be undertaken as part of this research, no matter what was focused on in the previous study.

(13) The focus on venues is the way forward in researching FOBTs since general population surveys are a poor tool for finding large enough samples of regular players and since the Code is to be implemented by LBOs. [**Comment**: it is not clear whether this is a criticism or a comment. We did interview in betting shops because the LBO survey was our chosen method of carrying out research in the relevant venue.] Canadian research clearly indicates that regular players form the main pool of potential problem and at risk gamblers (one in two). However, it would appear that some opportunities afforded by the focus on betting shops were lost (see next points). [**Comment**: we had no remit to conduct international comparisons.]

(14) The Code covers the following areas:

Those shown below in bold are those investigated by the research under review. [**Comment**: there was nothing in bold in the original version of Prof. Hancock's text, except at footnote 5.]

It is unclear why other parts of the Code are not included in this study and why for example, the interviewers did not also complete an audit of each venue visited regarding compliance to various observable aspects of the Code and why there was no attempt to ask LBO workers about their views of their own obligations under the Code. [**Comment**: we could have said explicitly that certain aspects of the Code would not be covered, and are happy to remedy this deficiency. The fact is that our discussions with ABB and DCMS focused entirely on those aspects of the Code that bear upon players. It was never even discussed that we should cover the implications of the Code for (e.g.) FOBT suppliers or for LBO employees. The survey objectives did not include an audit of each shop visited.]

(a) Terminal Operation

A maximum of four machines (FOBTs or a mix of FOBTs and AWPs) per LBO. This number not to include the type of terminal used to accept traditional 'over the counter bets'.

A maximum payout per single transaction of £500.

A maximum stake of £15 per bet and £100 per transaction.

To prevent potential problem gamblers from 'chasing losses', the minimum cycle time between customers inputting money, selecting their bet(s) and settlement shall be not less than 30 seconds. When customers are betting from their original stake or from accumulated winnings, the minimum cycle time for this type of transaction will be not less than 20 seconds.

No cash payouts directly from terminals. All payouts to be made at the counter.

No credit, debit or smart card transactions to be accepted on terminals.

Odds for each betting event to be clearly displayed on all terminals.

An on-screen balance meter allowing customers to track winnings/losses per session.

The ability for customers to 'cash-in' at any time following the completion of each betting event and to set a limit on the time they play and on the amount they wish to spend per session.

Point of sale promotional material not to encourage excessive play.

All random number events to be organised independently of the bookmaker.

Random number communication to be independently audited by a third party.

ABB members to obtain FOBT/FOBT products only from manufacturers and suppliers approved by the ABB Compliance Committee.

(b) Terminal content

Clear help pages to be present on all terminals, including contact information for GamCare and warnings on excessive gambling. Access to help pages to be available at all times by use of button/icon.

GamCare signage and leaflets provided by the ABB Compliance Committee to be prominently displayed adjacent to terminals.

Representations of casino games other than roulette or numbers games are not permitted[4].

2. The following conditions must be met by suppliers

FOBT manufacturers/suppliers wishing to supply LBOs must register with and be approved by the ABB Compliance Committee (under the terms of Section One of this Code ABB members will not be supplied by non-approved companies).

To be authorised to supply LBOs, companies must state in writing that they accept and are prepared to fully comply with the ABB Code.

[4] A casino game is defined as any game which is authorised under P. 5 the Gaming Clubs (Bankers' Games) regulations or is any derivative of such a game or is promoted in such a way as the players may believe they are participating in such a game.
The following bet process must be adhered to:
(i)Customers must make a positive selection
(ii)Customers must choose a stake
(iii)Odds must be clearly visible at time of selection
(iv)Customer to select 'start' or 'bet' button/icon
(v)Result to be announced/displayed on terminals (The Code 2005)

In addition, manufacturers/suppliers must undertake to:

(a)Comply with all regulatory requirements relating to the use of FOBTs.

(b)Display prominently on help files any notices that may be required by ABB.

(c)Operate in a manner that will not bring the betting industry or the ABB into disrepute.

(d)Maintain strict security controls over all FOBT hardware and software.

(e)Ensure that number generation is entirely random and that no third party access (including by bookmakers) is possible.

(f)Ensure that all customers (betting office operators) are adequately trained in the operation, maintenance and control of machines as appropriate.

(g)Establish that all employees of suppliers/manufacturers are familiar with this Code and with the betting industry's commitment to fair and responsible trading.

(h)Supply FOBTs and/or similar products only to bona-fide licensed bookmakers, according to the criteria outlined in this Code.

(i)Ensure that all betting events offered on FOBTs comply with this Code and have been approved by ABB as suitable for this purpose.

The researchers gave an inadequate justification of the reason for inclusion of only some Code items (shown in bold). [**Comment**: we have already covered this point. We were asked to deal only with those features of the Code that bear upon FOBT users.]

As the report states in A1.4.2

> 'So far as the paying customer is concerned, the principal features of the Code are these: "A maximum of four machines (FOBTs or a mix of FOBTs and AWPs) per LBO. 4 This number not to include the type of terminal used to accept traditional 'over the counter bets'. A maximum payout per single transaction of £500. A maximum stake of £15 per bet and £100 per transaction. 5 To prevent potential problem gamblers from "chasing losses", the minimum cycle time between customers inputting money, selecting their bet(s) and settlement shall be not less than 30 seconds. When customers are betting from their original stake or from accumulated winnings, the minimum cycle time for this type of transaction will be not less than 20 seconds.'

There are clearly dimensions of the Code and its impact and implementation, relevant to the 'paying customer' that should be part of research focused on LBOs. To focus exclusively on players' perceptions is to only take one dimension of the Code. Another important dimension

is staff training in and awareness of the Code and staff perceptions of the effectiveness of Code measures in stemming problem forms of play[5].

The study was limited to asking respondents about awareness of the Code, whether in favour or opposed and whether they themselves were affected by it. The research had the potential to ask respondents whether they thought various measures were effective if the code has been affective, for example, the limits on machine speed aimed at preventing 'chasing losses'. There were no questions on chasing losses. [**Comment**: Q23 looks at what FOBT users did if they had a win, and chasing losses is covered within the DSM-IV questionnaire.] Similarly, both staff and players are important sources of perceptions of what protective measures might work. They could also be asked whether from a player safety point of view, FOBTs are a good idea in LBOs. [**Comment**: we were not asked to solicit the views of LBO staff.]

(15) In my experience as a reviewer and as Chair of an independent government funded Gambling Research Panel, it is most unusual to find a research project of this nature and with this policy context, with no literature review and no comparative research base or justification of for example, the choice of research tools such as the DSM-IV. [**Comment**: this may well be so, but it was not part of our brief to include all these elements. This report was never intended to be an academic treatise.]

(16) The FOBT Code of Practice has undergone minor changes (para 1.1.10). The report needs to explain what these are and discuss any implications for this evaluation. [**Comment**: we said that in our view the changes are so small as not to have any effect on the research.].

The Betting Shop survey

(17) Given a focus of the study to 'measure and explain levels of problem gambling amongst FOBT users, in the context of benchmarks of other gambling activities (particularly machines)' it seems around the wrong way for the problem gambling screen to be administered generally and not more closely related to FOBT use. The fact that problem gamblers gamble on a range of products was used to argue that other forms of gambling were the problem. [**Comment**: we do not say this at all. We say that, so far as we could determine from the evidence, problem gambling is associated with no particular form of gambling.] This could have been resolved in this study with more careful questioning. One is left asking whether FOBTs are creating problems for gamblers or if they are not? [**Comment**: it would have been unwise to focus the problem gambling questions purely on FOBT users as not all betting shop customers use FOBTs. As

[5] **An Australian study examined implementation of a voluntary code with some similar attributes in the state of Queensland and focused on factors in venues that facilitate or block implementation of the code. Facilitators included adequate staff training and education, understanding of the philosophy of the code, regular audits and so on and impdiements as high staff turnover, managerial apathy, remote location and so on (Breen, Buultjens and Hing 2005).**

applied, the gambling screen enables us to consider the words “in the context of benchmarks of other gambling activities”.]

(18) The sample selection of betting shops.

Appendix 9:

> Chains were asked to construct a list:
>
> ‘Each chain participating in the survey was asked to select a random sample of shop(s) within their chain. To ensure that the selection was carried out to our precise requirements, each chain received a customised set of instruction’
>
> ‘Ipsos MORI worked in close collaboration with the ABB and Europe Economics to decide on the final number of shops required across each of the chains to ensure we achieved a balance across the different chains.’ P. 4. From later comments (p. ‘we provided a different sampling interval for each of the chains which was dependent on the total number of shops in the chain and the number of shops we required to select to the gross sample’

Does this mean that the number in the sample from each chain and independents was a proportionate stratified sample chosen from the over 8,000 across the UK? [**Comment**: yes.] The narrative explaining the sampling and its representativeness could be clearer in the report. [**Comment**: we will recheck for clarity and amend the report accordingly.]

(19) The write-up of results needs in all cases to cross reference general results statements with the Appendix table which gives the appropriate data (OK) and at all times give the N on which the data is based (not OK). Without this the reader has to scroll through countless tables to find the source. The presentation of data is very confusing as it is not made clear in all instances, whether date refers to *weighted data* and whether only to ‘*regular gamblers’*. Much of the data presenting age, region, social class, children in the household and income is too small for meaningful analysis. [**Comment**: we say quite explicitly that data presented is weighted except where otherwise stated.]

(20) The *reason for the weightings* in the Betting Shop survey (discussed below) is not considered merited or based on sound reasoning or, for that matter, robust scoping research to back up assertions. Weighting argued upon a need for representative betting shop frequency of visits seems without justification. [**Comment**: the weighting *is* based on sound reasoning, but we would be happy to explain in greater detail. The sampling method described gave betting shop customers a probability of selection proportional to the time they spend in betting shops (on Thursday/Friday or Saturday); but the desired reporting metric is the proportion of gamblers rather than the proportion of man hours spent in betting shops. Therefore it is necessary to re-weight the data to give each gambler rather than each man hour an equal weight. For this purpose it was considered that the evidence of gambling habits derived from the Omnibus survey would be more reliable than any internal evidence that could have been drawn from questions included in the betting shop survey.]

(21) The data for LBOs should be re-run without the weightings. Weighted data needs to be used sparingly as it can unnecessarily distort the data and tables based on weighted data prevent the reader extrapolating from the data. This reviewer sees no justification presented to merit these weightings. The sample includes a week day (Thursdays & Fridays) and the weekend so this would appear to give a valid selection yielding a range of players. There may be some interesting points of difference when unweighted data is run and in any event, the data would be more transparent and useful. [**Comment**: presenting unweighted data will give rise to more problems than it solves. If any reader attaches importance to seeing unweighted data, it is visible in the MORI tabs. Alternatively, a new study would be needed which reruns the LBO survey across all 6 or 7 days of the week.]

Further points/elaboration

The summary is not at all times clear

Since the Code of Practice is one of the principal foci of the study, the Summary should state what it covers. It should also give a brief background on what FOBTs are and a brief history of how these studies came about, with regard to underlying issues. The previous Round 1 study (Europe Economics 2005) included such details and this report should not relegate this or the policy context to an Appendix. [**Comment**: for the audience at which both reports are aimed, we do not agree that the Round 2 report should include substantial pieces of Round 1. The Round 2 report would then become needlessly big. Although we regard our process and analysis as robust we say again that the reports were written for non-specialist readers rather than for academic review.]

1.16 –the three points mentioned need brief description [**Comment**: presumably 1.1.6, not 1.16. We are happy to expand slightly, but not in such a way as to undermine the intended brevity of Section 1.]

1.2.1 – re Omnibus survey, the summary needs to clearly state the methodology of the survey ie the 2004 study described the methodology as 'quota-based, multi-purpose surveys' as distinct from the 1999 UK prevalence study which used a household survey method. The summary needs to outline the sampling methodology for the study, and explain that it is not truly representative (in the statistical sense) and therefore not comparable to the Round 1 results. [**Comment**: despite Prof. Hancock's suggestion we do not propose to add significant technical detail to Section 1. We have already argued that the Omnibus survey *is* statistically representative. The Omnibus surveys used identical methodology as between Round 1 and Round 2, so we would contend that the two Rounds are comparable. A broader observation we would make is that Prof. Hancock seems confused on terminology here. A "representative sample" is one that resembles the target population sufficiently in relevant respects to enable measurements to be made which are accurate enough for the purposes of the research. It is not synonymous with a random or probability sample, which is simply one recognised methodology for attempting to generate a representative sample. In fact a probability sample may be unrepresentative, whether through sampling error or non-response error.]

1.2.10 – the reason for Omnibus survey weighting needs to be given in the summary [Comment: no, it is in the Technical Note, Appendix 2, which in our view is enough.]

1.2.17 alludes to 'limitations inherent in the sampling' but does not give details. [**Comment**: no – we say "limitations inherent in sampling" (which we contend is self-explanatory) not "limitations inherent in the sampling." The word "the" is all-important here, and we did not use it.]

[Further comment: weighting is, in our opinion, an essential part of the methodology of a well-conducted quota survey. There is a practical limit with quotas to the number of factors which can be used to control, but it is possible to add controls for further factors at the weighting stage. The MORI Omnibus, for example, controls for social class and the number of cars in the household at the weighting rather than the quota stage, as well as the interaction between gender and working status, which are controlled for independently in the quotas. Furthermore, since interviewers can only interview whole numbers of respondents within any given category, the survey totals are unlikely to match the population totals exactly. For example, any reasonably sized quota which is representative of its sampling point will be likely to require an equal number of interviews with men and women, since there are few if any parts of the country where there is a great imbalance between the sexes; but in the national adult population, 52% are women and only 48% men, and an unweighted quota sample would therefore suffer from an inbuilt permanent bias towards men's responses. A probability survey can rely on the law of averages to iron out such imbalances over the totality of sampling points, subject to the calculated confidence intervals; but the theoretical underpinning of a quota sample is different, and reliant instead on the final (i.e. weighted) sample resembling the target population exactly in as many measurable respects as possible. In fact a quota survey may be best viewed as being based on a highly complex system of stratification, and requires weighting to ensure that each stratum is accorded its due weight in the final result.]

The methodology:

The basic study design was sound, with (1) an Omnibus survey used to establish problem gambling among FOBT users within the broader context of other forms of gambling 'a snapshot of gaming and gambling activities across Great Britain (England, Scotland and Wales)' (appendix 2), followed by (2) a survey conducted within betting shops, that could then focus more on gamblers using FOBTs. (Since a population prevalence survey would need to screen thousands of people to get to a sample of casual and regular FOBT gamblers this basic design makes sense.)

The study is based on a filter question defining 'regular gambling' as activity undertaken at least once per month. In the omnibus survey waves 1-2 from question 2 onwards, only those gambling at least once a month are included and a similar filter question was applied in the wave 3-6 surveys of betting shop customers. This effectively drops those respondents who might gamble less frequently than monthly but nonetheless have problems eg binge gamblers and for example, those paid fortnightly who gamble away pay packets on pay day [**Comment**: this is not right. Those who binge gamble on pay day and are paid fortnightly will therefore gamble at least twice a month and so would fall into our definition of regular gambler. The same would apply to those paid weekly]. It also precludes any possibility of broader screens (such as CPGI) which identify those at risk of problem gambling as well as those found to be problem gamblers.

However, why this was the case and a justification for the chosen definition of 'regular' and the early exclusion of all others, are lacking. This methodology may be seen as wasting opportunities for a more inclusive study that includes other patterns of gambling that may cause problems. [**Comment**: we would be happy to add something about why this definition was used, but do not agree that we have wasted opportunities. If we had gone for a longer time for the definition of regular gamblers, we would risk picking up "big event" gamblers. And for some, it would be difficult to remember more than one month ago.]

The sampling methodology:

The report describes the sampling methodology thus:

> 'The sample design is a constituency based quota sample. There are 641 parliamentary constituencies covering Great Britain. From these, we select one in three (210) to be used as the main sampling points on the Ipsos MORI Omnibus. These points are specially selected to be representative of the whole country by region, social grade, working status, MOSAIC rurality, tenure, ethnicity and car ownership. Within each constituency, one local government ward is chosen which is representative of the constituency. Within each ward or sampling point, we interview ten respondents whose profile matches the quota. The total sample therefore is around 2,100 (10 interviews multiplied by 210 sampling points per wave). Quota controls are set within area by gender, household tenure, age and work status as follows: Gender: Male; Female Household Tenure: Owner occupied; Council Tenant/HAT; Other Age: 15 to 24; 25 to 44; 45+ Working Status Full-time; part time/not working These quotas reflect the socio-demographic makeup of that area, and are devised from an analysis of the 2001 Census. Overall, quotas are a cost-effective means of ensuring that the demographic profile of the sample matches the actual profile of GB as a whole, and is representative of all adults in Great Britain aged 15 and over.' (Appendix 9 Omnibus survey)

This however, does not state how each unit in the Omnibus survey (ie interviewee) was selected or where these took place. ie Were these street interviews or interviews with households? Where were they conducted, how were interviewees chosen? With only 10 from each chosen constituency, with quotas on gender, household tenure, age and working status, this is a stratified quota sample and not a random sample [**Comment**: yes, it is indeed a stratified quota sample, and we have already said that we see nothing wrong with that. We have also explained that interviews are conducted "face-to-face, in home, using CAPI (Computer Assisted Personal Interviewing)". We are happy to add that the sampling points are based on census wards (i.e. local government wards as they existed at the time of the 2001 census, not as they are currently constituted), and that some sampling points consist of more than one adjacent ward, or of only part of a ward, to ensure more equal population sizes.]

Claims to representativeness

The weightings are used as an argument that the sample is 'representative of the GB population' in the case of wave 1.

> 'This is to adjust for any variance in the quotas or coverage of individual sampling points so that the sample is representative of the GB adult population' (p. 10 Appendix 10).

However, one cannot make this assertion from the methodology adopted. There seems to be a confusion between commonsense and statistical assumptions of representativeness. [**Comment**: there is no confusion and no assumptions as to "representativeness".]

Nor is it valid to attempt to extrapolate from a quota sample the aim stated in para 2.2.3:

> 'In summary, the aim of the Omnibus surveys was to establish gambling prevalence among adults in Great Britain and to help assess the overall level of problem gambling among them.'

3.4.17 states:

> 'Waves 1 and 2 of the Omnibus surveys in 2004 identified 20 problem gamblers in a weighted sample of 4,023, equivalent to a central rate of 0.5 per cent, with a 95 per cent confidence interval of 0.3 to 0.7 per cent. Although the central rate has fallen between 2004 and 2005, the difference is, from a statistical point of view, not sufficient for us to be confident that the prevalence of problem gambling overall has fallen – but we suggest it may not have increased.'

1.2.9 states:

> 'Omnibus waves 1 and 2 indicate a (central) problem gambling rate of 0.4 per cent. We estimate that the 95 per cent confidence interval is 0.2 to 0.6 per cent. In 2004 the central rate was 0.5 per cent, with a 95 per cent confidence interval of 0.3 to 0.7 per cent. We conclude that the problem gambling rate across the adult population has probably not increased, and may have decreased.'

Para 2.3.6 and 2.3.7 attempts to draw comparisons between the UK national Centre for Social Research 2000 study and despite listing fundamental differences, goes on to argue that broadly comparable levels of problem gambling have been recorded. [**Comment:** OK – we will delete "comparable" and substitute "similar".]

3.6 Principal differences between Round 2 and Round 1 results . This section, comparing two point in time quota samples needs review.

> 3.6.1 'The differences are small, both in number and extent'.
>
> 3.6.2 'Gambling prevalence ("regular gambling") among GB adults changed hardly at all between Round 1 and Round 2. In waves 1 and 2 the rate was 42 percent in both years. In waves 3 to 6 the rate reduced slightly from 8 per cent in 2004 to 7 per cent in 2005'.
>
> 3.6.3 'Gambling preferences in waves 1 and 2 likewise changed by only a little. In 2005, 89 per cent of regular gamblers played the National Lottery and 12 per cent bought scratch cards, compared with 88 per cent and 11 per cent respectively in 2004. Among other forms of gambling there were small gains

and losses. In 2004 8 per cent of regular gamblers visited betting shops, compared with 7 per cent in 2005. There were no shifts of 3 percentage points or more between 2004 and 2004.'

[**Comment**: none of the differences in the two quoted paragraphs would be regarded as statistically significant whether derived from a quota or random survey. Probably these paragraphs should be rephrased to indicate that there is no evidence from the surveys of any change in habits, rather than describing differences between the survey figures as "changes".]

Similarly, section 4.8 of the report Differences between Round 2 and Round 1 should be questioned as differences could well be the outcome of sampling differences.

[**Comment**: as we are comparing like with like over time (i.e. the same methodology used both times), then we contend that we *are* able to make comparisons.]

A statistically random sample should be used to establish gambling prevalence and to make comparisons between point-in-time national sample findings. Although the small number of problem gamblers identified in Round 1 (n=20) and Round 2 (n=17 weighted) is acknowleged, the report nonetheless goes ahead with comparisons of the prevalence rates from two non-random, quota samples.

Furthermore the differences in the regional distribution of the wave 3-6 Betting Shops is acknowleged:

> 4.1.6 'The regional distribution of betting shops in which interviews took place was slightly different from last year's. This year there were greater proportions of customers in the North and in Scotland, and fewer in the Midlands/East Anglia and Wales. In our view no significance attaches to the differences: last year and this year, the sample of betting shops used was representative of bookmakers' estates, not a random sample of all betting shops in Great Britain.'

This casts doubt on the validity of comparisons between Round 1 and Round 2 data. [**Comment**: again, we disagree. The two rounds of research *are* comparable. A stratified random sample was used, which is regarded as pure. For both waves of research the sample was chosen based on each bookmaker's market share (derived from number of shops). We do therefore consider the research comparable. We have undertaken no analysis based on region. Two reasons for the different geographic distribution of shops this year are (a) that we went for a larger sample (which increases the chances of selecting bookmakers in England) and (b) since round 1, each of the three main bookmakers (Ladbrokes, William Hill and Coral) were involved in acquisitions of smaller chains, which in turn affected location distribution.]

Use of DSM-IV and method of administration of the screen.

Para 2.2.5 states:

> ‘To measure problem gambling we used an established, authoritative questionnaire developed by experts in the field of testing for gambling addiction.’ DSM-IV was one of the earliest screens developed and appears to have been chosen in preference to the SOGS screen because it has half the 20 items in the SOGS screen. This overlooks the development and use of alternative screens such as the CPGI.
>
> DSM-IV was developed in the US, is published in the American Psychiatric Association Diagnostic and Statistical Manual, for use in a therapeutic context as a screen in treatment settings for gambling dependence and abuse syndrome. Dickerson et al (1997) on *The Definition and Incidence of Problem Gambling,* are critical of the focus of DSM-IV on individual pathology and a medical model of addictive compulsive or pathological behaviour. (Although five or more of the ten behaviors must be present for a diagnosis of pathological gambling, the current study adopted the (lesser) definition of problem gambler with a positive answer to three of the 10 items.) ‘

In the UK context, it is probably valid to argue that problem gambling is not viewed as a mental illness or a pathology and that harm to self and others for example, is a dimension of problem gambling which the DSM-IV does not encapsulate. The Canadian *Inter-Provincial Task force on Problem Gambling* (Ferris et al 1999) defines problem gambling as: ‘gambling behaviour that creates negative consequences for the gambler, others in his or her social network, or for the community’. Moreover, jurisdictions have been turning more towards a public health model which includes the nature of the gambling products themselves and interactions between patrons and those products as the focus for preventative actions (an approach reflected in the FOBTs Code). The public health focus on harm to self, others and the community (eg economic and social impacts) lead policy in clearly different directions than a mental illness clinical treatment model. More recently, consumer protection and product safety have been emphasized. Thus, a reading of the Round 1 and Round 2 reports finds no reasoned argument for adoption of DSM-IV and no assessment against more recently developed (and favored) screening tools. It would appear that the report under review (Round 2) has adopted the DSM-IV for comparability reasons and (because it has 10 items rather than the SOGS which has 20), thus precluding other measures which may have other features more suited to the current evolving policy environment. For example, the CPGI enables gamblers to be placed on a continuum (thus enabling inclusion of binge gambling) and for those ‘at risk’ of developing problems to be identified.

[**Comment**: we say in the report that we were not psychologists, and that we therefore relied on the work of others in selecting the DSM-IV test. The UK Gambling Prevalence Study of 2000 uses both DSM-IV and SOGS screens, and itemizes advantages of the DSM-IV over the SOGS. Apart from the fact that using a different screen for Round 2 (as compared with Round 1) would destroy comparability of findings, we know of no application of the CPGI to UK gambling.]

Re method of self-administration of the screen:

Appendix 2, p. 6 is confused:

> ‘There are a variety of measures available to identify problem gambling and the ABB, Europe Economics and Ipsos MORI decided upon SOGS. [**Comment**: we must apologise for a typing error here. The text should read “DSM-IV” not “SOGS”.] Given the sensitive nature of these questions and with the aim of boosting the level of completion of these DSM-IV measure questions to its maximum, it was decided to ask respondents to self-complete this section of the questionnaire (i.e. Questions 7 and 8).’

The report would really need to draw on other literature to substantiate the need for the self-administered screen. [**Comment**:. we have explained why we used DSM-IV.]

The non response rate for DSM-IV is of concern contrary to the claim below.

Appendix 9 states:

> ‘Problem gambling is associated with various socio–demographic factors such as a problem gambler in the family, low income, being male and being separated or divorced, etc. There are a variety of measures available to identify problem gambling and the ABB, Europe Economics and Ipsos MORI decided upon DSM IV as a test for problem gambling. Given the sensitive nature of these questions and with the aim of boosting the level of completion of these DSM IV measure questions to its maximum, it was decided to ask respondents to self-complete this section of the questionnaire (appended). In this way the respondent’s answers would be kept confidential and the interviewer would not be able to peruse their response. 79% of respondents agreed to fully complete this section of the interview – a high level of completion for questions of this nature. The remaining 21% of respondents either refused to answer all or some of the questions.’ Page 8

In para 4.6.3 it is stated regarding DSM-IV non response rate:

> ‘Not all questions produced the same percentage of non-responses, but answers are required to all ten for a return to count as complete’.
>
> Given that 15% omitted to answer question 10 it seems regrettable to disregard other responses. It seems strange to insist that all items must be answered when a score of 3 out of 10 is sufficient for designation as a ‘problem gambler’.

[**Comment**: as a matter of principle, we think it right to regard the test as whole and indivisible, and that is why we classified any incomplete responses (i.e. fewer than 10 questions answered) as “not stated”. However, we looked further at betting shop customers’ responses in order to establish whether the inclusion of respondents who answered questions 1 to 9 but did not answer question 10 would have made any difference to the results. The answer is that it would not. There were 39 respondents who answered Questions 1-9 but did not answer Q10. Of these, 27 gave one or fewer “positive” responses, so that even if the 10th response had been positive, these 27 would still have been classified as non-problem gamblers. Three people among the 39 gave two positive responses, which means that the 10th response would have been critical in determining whether they should be classified as problem gamblers. These

people were correctly classified as "missing" in the statistical sense. Nine respondents gave 3 or more positive responses within the first 9 questions and therefore would have been classified as problem gamblers had we accepted an incomplete return as valid. In summary, this further analysis reduces the number of "non stated" responses by 36, increases the number of known non-problem gamblers by 27 and increases the number of known problem gamblers by 9, a ratio of three to one. The effect of these adjustments on our analysis of problem gambling is so small as to be negligible. It is important to emphasise that this further analysis covers only those respondents who did not answer question 10. It was question 10 that attracted the highest non-response rate, and we assume that that is why Professor Hancock singled it out, but full analysis would require an examination of those who did not answer any of questions 1 to 9. Since the effect on our analysis of the adjustment arising from question 10 is negligible, we do not propose to repeat the tests for questions 1 to 9.]

No focus groups/qualitative dimension

The statement that 'we did not re-run the qualitative research (focus groups) because there was no need' requires some additional justification. It was used in an exploratory sense in the previous Round 1 study, but there is no reasoned assessment of the usefulness of qualitative research may in this Round 2 study for other reasons. One possible use would be in relation to points raised above regarding employee training in the ABB Code and aspects of the code left out of the current study. [**Comment**: we completely disagree. There was no need to re-run focus groups again in order to orientate the Omnibus/LBO surveys, since that had been done in Round 1 – and no point in re-running focus groups with a different aim in mind from Round 1, since we would not then be able to perform comparisons.]

The way results are presented in the text

weightings

In section 3.5 reporting on the waves 3 to 6, the summary and lack of detailed presentation of data is very misleading as the weighting of results is not discussed and the potential for distortion is not made clear. [**Comment**: again we completely disagree. The MORI technical notes explain clearly why and how the weightings were done. In the main body of the report we strove for brevity and a clear narrative – so made a conscious decision not to burden the text with excessive detail.]

4.3.10

'FOBTs still rank fifth in popularity within the betting shop, substantially behind longerestablished betting shop products.'

There seems to be the implication that if comparisons with the last survey show little difference or if patrons use other forms of gambling that this means that FOBTs are somehow not harmful? [**Comment**: we make no such claims based on the points described in 4.3.10.]

It is unclear if data discussed in 4.4 is weighted or unweighted. [**Comment**: weighted.]

Re section 3.5.8 –3.5.11 on Gambling Preferences, it is hard to see how it is relevant that regular FOBT gamblers have higher rates of gambling on fruit machines and other forms of gambling when the products are very different. Which ones are related to problem gambling is the issue at hand. [**Comment**: first, DCMS said it wanted to know about FOBT usage in relation to other machine gambling – hence the reference to fruit machines. And common sense would suggest that one should take serious note of all the things that problem gamblers do, not pick on just one.]

Diversity of gambling preferences is then used later in para 4.4.22 to argue that the impact of FOBTs is fairly marginal.

'4.4.22 'Given that only 1 in 5 betting shop customers use FOBTs at all, we conclude that FOBTs have been only marginally instrumental in attracting customers to betting shops. This is consistent with evidence in paragraphs 4.3.9 and 4.3.10 that FOBTs do not rank high in betting shop customers' preferences.'

4.4 FOBT user section

The re-definition of 'regular' in the FOBT user results discussion

4.4.5

'In the analysis which follows, and except where otherwise stated, we concentrate mainly on the 9 per cent of betting shop customers who use FOBTs "always/every time" or "usually or most times". For convenience these are referred to as "regular FOBT users".'

This is confusing and another term should be sought for this side-discussion. [**Comment**: we do not agree that our use of the word regular here is confusing, but we will attempt another.]

Among FOBT users who use them 'usually/most times or 'always/every time' there are some interesting results which are not included in the Summary of results.

4.4.6 Regular FOBT users are:

- overwhelmingly likely to be male (94 per cent)

- predominantly younger (59 per cent are under 35, compared with 28 per cent for betting shop customers generally; and 28 per cent are under 25, compared with 12 per cent of betting shop customers generally) – predominantly in lower socio-economic groups (13 per cent are ABs, 23 per cent C1, and 61 per cent C2DE). These are very roughly the same proportions as for betting shop customers overall (8 per cent ABs, 22 per cent C1, and 67 per cent C2DE).

- almost certainly in work (80 per cent)

- among the less frequent betting shop visitors (62 per cent of regular FOBT users visit between once per week and once per month, compared with only 11 per cent of users who visit 4+ times per week)

- overwhelmingly interested in playing roulette (99 per cent)

- mainly interested in using FOBTs "to win money generally" (36 percent) although 25 per cent say they play just for "fun or amusement".

4.4.7 Almost one third (32 per cent) of all FOBT users have been using them for under 12 months. A further third (35 per cent) have been using FOBTs for between 12 months and two years.'

and

4.4.9 The weekly gambling expenditure of FOBT users is interesting. Those who use FOBTs "always/every time" have an average weekly gambling expenditure (that is to say, expenditure on all forms of gambling together) of £50.73, some £11.03 higher than for betting shop customers as a whole. Those who use FOBTs "usually/most times" have an average of £61.72 (£22.02 higher), and those who "rarely" or "sometimes" use a FOBT have an average of £98.07, £58.37 higher. The study thus suggests that more frequent FOBT usage is not correlated with higher spend on gambling.'

These results clearly need to be re-visited when the weightings are dropped from analysis in LBO study. [**Comment**: we are not proposing to re-analyse and re-report on the basis of unweighted data.]

4.4.19 '20 per cent of regular users in said their spend had increased a great deal, and 20 percent said it had increased a little. Only 10 per cent said it had decreased.'

4.4.21 'Twenty two per cent of regular users and 14 per cent of all users said they now visited betting shops more often because of FOBTs.'

From a policy point of view, this section is of relevance in highlighting risk factors [**Comment**: it is not for us to make policy. Here we have simply reported what we found.]

The Weightings in wave 3-6

4.3.5 'MORI weighted the frequency of visit among the betting shop customer sample back to the frequencies of betting shop visits gained from the Omnibus surveys. On this basis: – 6 per cent of betting shop customers visit 6 to 7 times per week or more – 3 per cent visit 4 or 5 times per week – 24 per cent visit 2 or 3 times per week – 41 per cent visit once per week – 26 per cent visit between once per week and once per month'

The report does not specify which table presents this data in the Appendix so it is not know where it comes from or whether it is weighted data. [**Comment**: we will insert the reference.]

4.1.2 'In each betting shop selected for the sample two interview shifts were carried out – one on a Thursday or Friday, the other on a Saturday. The reason for this, as in Round 1, was that these three days are known by bookmakers to be their busiest days. MORI could thus be reasonably sure of having a sufficient number of customers available for interview. However, this approach

would result in a frequency of visits that is not representative of the generality of betting shop customers, so MORI then weighted the results in line with the frequency of betting shop visits revealed by the Omnibus surveys.'

The aim of interviews in LBOs is to gain access to betting shop and FOBT gamblers. There is inadequate justification for the weightings and the data needs to be re-run without them. [**Comment**: the justification for weighting is exactly as described, and we have no intention of re-running the analyses and re-writing the report on the basis of unweighted data. To repeat what we have already said: the sampling method described gives betting shop customers a probability of selection proportional to the time they spend in betting shops (on Thursday/Friday or Saturday); but the desired reporting metric is the proportion of gamblers rather than the proportion of man hours spent in betting shops. Therefore it is necessary to re-weight the data to give each gambler rather than each man hour an equal weight. For this purpose it was considered that the evidence of gambling habits derived from the Omnibus survey would be more reliable than any internal evidence that could have been drawn from questions included in the betting shop survey.]

Future implications:

The report could draw out any implications for FOBTs and for responsible gaming practices in light of the findings and of implementation of the Gambling Act 2005 coming into force. [**Comment**: this would go beyond our terms of reference. We do appreciate that since the report was completed FOBTs have been reclassified as gaming machines, but we have not studied gaming machines either for their own sake or as analogues of FOBTs.]

In light of the implementation of the new Act and the above points, any further evaluation of the impact or implementation of the Code would be enhanced by a new methodology that addresses the shortcomings of the 2005 and 2006 studies. [**Comment**: with more time and/or budget, and the advantages of hindsight, the research could of course be enhanced. But we emphasise that the timescale and method were agreed with DCMS before Round 1 began and that DCMS had the opportunity to re-shape the study before Round 2 began. We stand by the process adopted and the results it produced.]

[**Final comment**: we should like to emphasise that, whatever reservations Prof. Hancock may have about our research method, the fact is that DCMS, as a principal recipient of the results, was involved in shaping the project. Indeed, the substantial increase in the size of the LBO survey sample was a direct response to DCMS' comments at the end of Round 1. After two rounds of research we are very well aware of the desirability of refocusing it, a point apparently recognised by other researchers who now extol the virtues of longitudinal surveys.]

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Comments on the review of Dr. Gerda Reith

This is a well-designed and executed study. The questions posed by it seem sensible and these, as well as the methodology utilised, seem to be appropriate to the aims and objectives of the research in general. There are however some issues of interpretation and analysis where I feel that the study is less successful, and I am not convinced that the evidence presented supports some of its recommendations. These issues are detailed throughout the review.

The aims of the research were:

'To measure and explain levels of problem gambling amongst FOBT users, in the context of benchmarks of other gambling activities (particularly machines) both within and outside of the betting shop', and

'To assess the effectiveness of the FOBT Code of Practice, and the individual elements within it, in providing protection against problem gambling'

To deal with the latter first: it seems sensible, on the part of the research team, not to attempt to assess the causal impact of the Code of practice on betting behaviour. They note that it is not

possible to distinguish between effects that have occurred since the arrival of the Code, and those that have been caused by the Code. Additionally, they demonstrate sensitivity to the issues involved here, noting that at least 45% of users began using FOBTs after the code came into force, and therefore would not be able to compare the situation 'before' with any situation 'after'. They also note that many users would not actually be familiar with the Code as it stands, although they may inadvertedly be aware of some of its strictures, simply through playing the machines. It makes sense, then, to gather respondents' views of the Code, and compare these with those from the previous study although, as the research notes, such views do not imply the effectiveness of otherwise of the Code. The statement that, 'on balance, over the last 12 months the Code has been neither decisively effective nor wholly ineffective in influencing the patterns of FOBT usage' [4.5.12] seems quite justified.

It is also sensible to note the climate in which the Code is operationalised; i.e the range of measures taken by Government and organisations such as GamCare to reduce problem gambling. Having made these points however, the report goes on to conclude that, although it cannot disentangle the effects of these measures from those of the Code itself, 'the likely fall in the problem gambling rate among betting shop customers suggests that it [the Code] has been effective' [5.1.25]. I do not think that the evidence supports this conclusion. In fact, given the increased use of FOBTs among problem gamblers (discussed later in this review), the contrary view could rather more easily be taken: that the Code has *not* been effective in reducing problem gambling among this group. Overall, only the more modest assertions that appear earlier in the report seem justified by the evidence presented. [**Comment**: Dr. Reith's observation is entirely fair. In paragraph 5.1.25 we should have said that the Code *taken in conjunction with other measures adopted by bookmakers* has probably been effective. We will put this right in the final version of the report.]

Methodologically, it seems sensible to split the sample into two basic sets: waves 1 -2 and waves 3 - 6, with the latter leaving out lottery play, bingo, scratchcards and football pools. This enables the team to leave out what can be broadly termed 'mass gambling' (especially the lottery, which has very high rates of participation) and focus instead on a more specific sub-set which they term 'betting and gaming'. The only point that could be raised here is that it may have been useful to include more discussion about the rationale for leaving out these forms of gambling in waves 1-2, and also about the implications – if any - for the results as a whole. [Comment – we will gladly add something in support of Dr. Reith's observation.]

The size of the betting shop survey sample has been dramatically increased since the first study in 2004. This is a commendable move, which has allowed for the generation of larger sub-samples, and so more robust overall analysis.

Overall, the research appears to indicate that rates of problem gambling have not increased, and may even have decreased since the last study, although not by statistically significant amounts [3.6.9]

Specific findings about FOBTs shows that overall awareness of them among betting shop customers is quite limited, and that few actually play them - only 6% do so regularly, meaning that over three quarters of betting shop customers do not currently use them at all. Regular FOBT

players also constitute a demographically different group from betting shop customers in general, being considerably younger than the latter, although still within the same lower socio-economic demographic.

It could be speculated that they actually constitute an emerging group, as almost one third of regular players have been using betting machines less than a year [4.4.8]. In addition, this group may have their own patterns of playing: the study suggests that frequency of playing is not correlated with higher spending on gambling, with less frequent players actually spending more in their sessions than more regular players [4.4.9]. In addition, it appears that FOBTs have encouraged some regular players to visit betting shops in the first place (17%), and even more to visit more often because of them (22%). It would be interesting to know how all of these features (i.e the length of time customers had been playing FOBTs, the reasons they began visiting betting shops, and whether their visits had increased), was in any way correlated with the incidence of problem gambling behaviour. Unfortunately, this was not explored in the report. I believe that some additional analysis here would have been beneficial. [**Comment**: we will include some new text to explore the point that Dr. Reith makes.]

While the methods of the study appear appropriate and rigorous enough, at times the interpretation and presentation of results lacks the same levels of clarity, and does not really discuss in any detail what seem to be quite significant results.

For instance, although the research suggests that overall levels of problem gambling among betting shop customers has not increased, it also appears to point to another trend: a significant increase in the use of FOBTs among problem gamblers. Since the 2004 survey, this has escalated from 12% to 40%. This makes FOBTs the third most popular type of gambling among problem players, only slightly behind betting on dogs, which is 47%, and well behind horse race betting, at 80%. Overall, it has seen the most dramatic increase. While betting on horse and dog races by problem gamblers have declined, by 7% and 16% respectively, and betting on machines and football increased by 6% and 18%, the most striking change by far concerns FOBTs, whose popularity among this group has increased by 28%. [**Comment**: all this is perfectly true, but we do not hide it. We do go on, however, to conclude from the econometric modelling that no one form of gambling is more associated with problem gambling than any other.]

However, the report does not discuss this increase in any detail, and, quite often, when it is mentioned, moves on to emphasise the far greater popularity of horseracing and the numbers of problem gamblers who do not play FOBTs at all. These are interesting enough points of comparison, but this appears to ignore what is surely one of the most interesting features to emerge from the study as a whole – i.e. the increased numbers of problem gamblers playing FOBTs. Given that one of the aims of the study was: 'to measure and explain levels of problem gambling amongst FOBT users', this seems curious. [**Comment**: this seems to us a question of balanced presentation rather than one of substance. If (as we show) the proportion of betting shop customers who are problem gamblers has declined, and if the proportion of betting shop customers who use FOBTs remains very small, it seems to us important not to overstate the significance of the fact that FOBT usage among problem gamblers has increased. Furthermore, we go on to say that there is no evidence to suggest that FOBTs are more significantly associated with problem gambling than any other gambling form, so why would the increase in FOBT usage

among problem gamblers matter? We could hardly do more than dwell speculatively on the rise in FOBT usage, whether among problem gamblers or non-problem gamblers.]

A more minor, but related point, is that there seems to be some confusion with numbers here, with the percentages of problem gamblers who bet on FOBTs reported inconsistently throughout the study. For example:

At [1.3.34], it is stated that 45% of problem gamblers use FOBTs

At [4.6.14], the number is 42%

At [4.6.20], the report says that 40% of problem gamblers use betting machines regularly, compared with 14% last year

At [4.8.7], it is stated that FOBT usage shifted from 12% to 40%

These discrepancies should be checked.

[**Comment**: we apologise for any confusion caused, but all the numbers are correct. The differences are in definitions, which we now clarify:

Paragraph 1.3.34 – the figure of 45 per cent was taken from MORI table 41 (in the LBO survey). The question (Q12) asked all who visit betting shops at least once per month whether they use betting machines (no matter how frequently). Among those identified as problem gamblers, 45 per cent (37 weighted out of 82 weighted) replied that they use FOBTs (compared with 1 per cent who said they had never seen a FOBT, 33 per cent who said they were aware of FOBTs but had never used one, 13 per cent who said they had used one once or twice, and 6 per cent who had used them but no longer did).

Paragraph 4.6.14 – the figure of 42 per cent comes from MORI table 32. The question (Q9) asked all who visit betting shops at least once per month which activities they bet on at least once per month. Among problem gamblers (the same 82 weighted) 42 per cent said they used FOBTs at least once per month. Therein lies the difference between 45 per cent above and 42 per cent here.

Paragraph 4.6.20 – the figures in 4.6.20 are taken from MORI table 8 (for gambling activities outside the betting shop) and table 47 (for FOBT usage). By this point in our report we have defined a regular FOBT user as one who plays “always/every time” or “usually/most times” on visiting a betting shop, and the base is “all who use betting machines”. Among problem gamblers (again, the same 82 weighted) 40 per cent use FOBTs. The equivalent to MORI table 47 in Round 2 was table 41 in Round 1. Here, among 78 problem gamblers (weighted) there were 11 (weighted) who said they use betting machines – a figure of 14 per cent.

Paragraph 4.8.7 refers to the same 40 per cent from the same MORI table 47. The figure of 12 per cent for Round 1 comes from Q9 (MORI table 32), which asked all who visit betting shops at least once per month which activities they bet on at least once per month. Among 78 problem gamblers (weighted) 9 (weighted) used FOBTs, equivalent to 12 per cent.]

While each figure we have quoted is correct, we accept that it may be more helpful to put them on a common base or explain the differences where necessary. We will make these changes to the final version of the report.]

However, whatever the actual figures, the more important point is that they all show increased usage, and such increases do not support the claim that 'FOBTs do not figure prominently in problem gamblers activities' [4.6.19]. It is clear that, for some players, they do. [**Comment**: Dr Reith's assertion ("It is clear that…") is not supported by the evidence. All that one can say is that, *on average*, FOBTs loom larger in problem gamblers' activities than they did in the previous year. We are unable to discern those problem gamblers for whom FOBT usage is prominent. However, we do know that 52 percent of problem gamblers have used FOBTs either not at all or no more than twice, and we also know is that the incidence of problem gambling is not greatest among FOBT users who use FOBTs most frequently.]

On the basis of these figures, I am not convinced about the criteria for assigning FOBTs to a 'second rank' of problematic gambling pursuits which, the report states, 'sit a long way behind the favourite betting pursuit, namely horseracing' [1.3.35]. While it is true that horseracing is by quite a considerable amount the activity most popular with problem gamblers, at only 7% (or 5%, depending on which figure is correct) behind dogs, FOBTs do not seem to lag so far behind as to justify claims that they fall into a separate, 'second rank' status. This is especially the case if numbers continue to rise in the manner they have done between 2004-5. [**Comment**: we do think that FOBTs form part of a "second rank cluster" of betting activities, but we should have said that there are four members in the cluster, i.e. greyhounds, fruit machines, betting on football and FOBTs. The final report will include this correction.]

In any case, some of this is a matter of interpretation. What is not open to interpretation, however, is the fact of the substantially increased figures of FOBT use among problem gamblers. Taking into account the primary aims of this research, I feel that this should have been analysed and discussed in much more detail. [**Comment**: in fact we have analysed the point that Dr. Reith makes. The logit model – calculating the marginal impacts of FOBT usage – is precisely where (section 4.7 of the report) we carry out the analysis.]

In one place where the study does discuss the implications of FOBTs for problem gambling, its logic appears flawed. It is stated that, because the machines do not induce frequent or prolonged play, and as few users re-stake all their winnings in them, they do not appear to have 'fuelled addictive gambling'. These aforementioned factors are the ones the research evidence has shown to be most strongly associated with problem gambling. However, whether or not these machines encourage frequent play and / or the re-staking of winnings is, in a sense, a separate point. The assumption that FOBTs do not appear to have increased problem gambling because they do not meet the criteria usually associated with it, is to draw unfounded conclusions from the premises of a statement. Again, these become issues of speculation and interpretation. However, they need not be: to discuss the implications of FOBTs with more confidence, it would be more useful to simply look more closely at their increased use by problem players. [**Comment**: with great respect, we think it is Dr. Reith's logic that is flawed. It seems to us *relevant*, though not necessarily *conclusive*, that FOBTs do not induce frequent or prolonged play, and that few users re-stake all their FOBT winnings. We do not see that these two factors are separate points. And

we have already argued, on the basis of the econometric modelling, that the increased use of FOBTs by problem gamblers is not evidence that problem gambling and FOBT usage are related any more than (say) problem gambling and betting on horses or dogs.]

The report states that the significant associations with problem gambling are the age at which the individual begins gambling, their marital status, and the frequency of betting shop visits, with greater frequency correlated with problematic behaviour. However, I am not entirely convinced by the robustness of this latter variable. In a sense, it is something of a tautology to note that the more often an individual visits a betting shop, the more likely they are to be a problem gambler. Of all the possible associations, it is no surprise that frequency is correlated highly with problem gambling. The concern is that this is such a strong association that it may actually mask other, underlying factors. [**Comment**: Dr. Reith's use of the word "may" is significant here. It implies that she does not know – and we acknowledge that we do not know either – whether betting shop visiting frequency masks other factors. If it is masking other factors, we have been unable to identify them. It is certainly the case that, inside a betting shop, customers can participate in quite a wide variety of forms of gambling. What we were trying to establish was the effect of specific forms of gambling, controlling for the frequency with which a betting shop is visited. If we had not included this variable, the analysis would have been less robust: forms of gambling that are associated with more frequent gambling would have become significant, and there would have been no way of knowing whether problem gambling is associated with frequent gambling or with some particular form of gambling. For this reason, we included both so we could pick out the two effects separately. In any event, one cannot simply assert that frequency of visit is bound to be associated with problem gambling. In her review Prof. Hancock points to the existence of "binge" gamblers who might gamble much less than once per month but go completely over the top when they do.]

In their presentation of the results, the reports' authors recommend that future research look at players rather than types of gambling. I can find nothing in the report to support this recommendation, or to suggest that this might be particularly beneficial. [**Comment**: this suggestion flowed from discussions we had with DCMS and ABB but we would be happy to draft additional words earlier in the body of the report to substantiate the later reference.] In fact, such a focus on a single area is actually at odds with much of the current research into gambling behaviour, which is starting to move towards the investigation of the complex relations between individual problem gamblers, types of games, and their wider environment. [**Comment**: presumably "focus on a single area" refers to FOBTs, or are we missing Dr. Reith's point?]

There are certain implications for policy suggested by the findings of the study. If the use of FOBTs is increasing by problem gamblers to the extent that the figures here seem to suggest, then the question of whether or not the Code is not working as effectively as it could is raised. In turn, this leads to questions of how best to limit potential harms associated with the machines, and raises the possibility that new ways to advise players about their playing should perhaps be explored. This issue is one that will become particularly pressing as FOBTs are introduced to increasing numbers of venues, and particularly casinos, over the coming years. [**Comment**: as we said in response to one of Prof. Hancock's points, we had no remit to consider FOBTs in the context of supposedly comparable machines in casinos. Whatever the comparison with casino-based machines may yield, it is the case that, based on our research, the prevalence of problem

gambling in LBOs has decreased. But we do agree that there is no case for treating FOBTs and other forms of gaming machine in inconsistent ways.]

[end]