

**FOBTs and the Code of Practice**

**Pre-publication report dated 2 December 2004**

**Response of Europe Economics and MORI**

**to the peer reviews of**

**Dr. Serge Chevalier and Dr. Susan Purdon**

For convenience, the documents of Dr Chevalier and Dr Purdon have been left in their original format. The responses of Europe Economics and MORI are inserted after the relevant text, and are entered in blue.

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**15 March 2005**

**APPENDIX 1**  
**Dr Chevalier's Peer Review**

**Survey of Betting, gaming and gambling activities**

Methodological comments

Material taken into account

Appendix 3 – MORI Technical note on the Omnibus surveys

Appendix 6 – An analysis of the Omnibus surveys

Appendix 7 – Tabulations - Omnibus waves 1 and 2

Therefore comments will be focused on the Omnibus research

Comment 1: We are grateful for Dr Chevalier's comments. While we are happy to comment on his observations in relation to the material he has reviewed, we must point out that the main report and other appendices provide reasons for and additional detail on the approach we chose.

The objectives of the survey are well described.

The methodology used merits close attention.

1. Getting a sample by way of six data collections (called waves) does not pose intrinsic problems.
2. The use of quota sampling is, as the authors state, a method that costs less than others. It is widely used but statisticians have demonstrated that the obtained sample can be biased.

Comment 2: we agree that quota sampling, which forms the basis of the majority of Omnibus surveys, is more cost effective than (for example) probability sampling. Had we opted for probability sampling on the interviewing scale used in this study, the costs would have been prohibitive and certainly outside the budget available.

MORI points out also that the quotas are very carefully controlled. For each wave of the Omnibus survey, quota controls are set within each Electoral District (ED) selected to the sample by gender, age and work status. This ensures that, across GB, the resultant sample is representative of the adult population. Thus, the sample profile of respondents selected via a quota sampling methodology will differ little from one selected via probability sampling.

We are unclear what Dr. Chevalier means, in context, by "biased". What kind of bias might arise, and what might be the impact of such bias on our conclusions?

3. It is not clear to me why the data analysis were presented separately for waves 1-2 and waves 3 to 6. It seems to me that the methodology used should have been able to furnish global results stemming from a representative sample.

[Comment 3 appears on the next page.]

Comment 3: We deliberately separated the Omnibus study into two parts. Our reasons for presenting waves 1 and 2 separately from waves 3 to 6 are given in paragraph 6.1.4. of the report. We used waves 1 and 2 to obtain a “snapshot” of gambling activities in relation to all available forms of gambling. Then, in waves 3 – 6, we excluded from our detailed questioning the activities of gamblers in relation to current and former “mass gambling”, namely the National Lottery, football pools and bingo.

4. The questionnaire seems adequate for the purpose of the research.

Comment 4: We are glad to have Dr. Chevalier’s approval. Europe Economics worked in close collaboration with MORI and spent a great deal of time and effort on constructing the questionnaires. In particular, the areas of coverage, the precise wording of each of the questions and the accompanying interviewer instructions, the sequence in which the questions were asked and the content of the pre-coded list of possible responses. Also, careful consideration went into our definition and explanation of gambling “spend”.

5. The use of computer assisted personal interview (CAPI) is a collection mode that is very rarely used in gambling studies because of the costs involved. This method of collecting data could be the best, for what seems a delicate subject in Great Britain. The researchers should be commended for using this approach as it should yield the most valid answers from respondents.

Comment 5: Again, we are glad of Dr. Chevalier’s approval. The CAPI method is indeed the most accurate, efficient and effective way of asking such questions as part of an Omnibus survey.

6. Since answers can be subject to collection mode bias, mostly when probing into delicate subjects, this CAPI approach could make comparison with other studies difficult.

Comment 6: We certainly agree that answers can vary according to the method of data collection, and that different methods of capture may therefore make comparison difficult. However, it was not our objective to make or invite comparisons with other studies.

7. The exclusion criteria applied in this study also makes it stand apart. By only retaining people that gamble at least once a month, comparison with results obtained from previous studies is made difficult.

Comment 7: It was not one of our objectives to make comparisons with other studies. This survey was designed with other specific objectives in mind and the methodological approach was tailored specifically to achieving these.

8. The fieldforce has been sufficiently well managed to insure data quality.

9. Having the gambling problem screening (SOGS) questionnaire self-completed by interviewees seems to be unwarranted. Such a measure is best utilized for domains of great sensitivity. Previous studies in Great Britain have not concluded nor even raised the issue that gambling problems are such a disturbing or sensitive topic among the general population.

[Comment 8 appears on the next page.]

Comment 8: We have several comments here.

1. After careful consideration of DSM-IV and SOGS (both recognised tests) we opted for the DSM-IV test. We did not use the SOGS test. Our reasoning is explained in full in paragraphs 4.1.2 to 4.1.6 of the report. We fully agree with Dr. Chevalier that “such a measure is best utilised for domains of great sensitivity”, and the relatively high non-response rate suggests that we were right on this point.

2. It is hard to conceive that the response rate would have been higher if interviewees had been required to give their answers aloud to the interviewer (imagine, for example, an Asian lady being asked to respond aloud to a male interviewer). The Market Research Society Code of Conduct is strict on this point: neither interviewee nor interviewer must be placed in a position of embarrassment.

3. Dr. Chevalier highlights “previous studies in Great Britain”. Could we have more detail on these?

10. The use of this technique might have influenced the responses by creating an environment that inhibited respondents from answering this section.

Comment 9: We believe the opposite is true. After careful consultation with MORI, we agreed that the self-completion methodology chosen allows respondents to provide not only a confidential response but one which is completely anonymous. This is not possible if the questions are interviewer-administered. MORI undertakes numerous studies on sensitive subject areas and recommended this approach as the best way forward – for maximising the response rate and, importantly, minimising the respondent’s level of potential embarrassment.

11. While the authors state that the completion rate of 75 % for the SOGS represent a «high level of completion», it seems to me that this result is closer to being abysmal than good. Typically, the SOGS response rate is of 98-99 % in telephone surveys.

Comment 10: Both Europe Economics and MORI believe that a 75% rate is high for the subject matter and question the 98-99% response rate outlined in Dr. Chevalier’s point. Response rates sometimes differ depending on how they are calculated. Our response rate is calculated by dividing the total number of those completing the DSM-IV questions by the total sample size.

12. Furthermore, if 25 % of respondents failed to answer that section, the authors should have performed representativeness tests; that is, they should demonstrate that, for that section, the respondents are sufficiently similar to non-respondents with respect to such interesting variables as gender, social class, etc. so as to ensure that the results are not biased.

Comment 11: We are happy to undertake a demographics-based analysis of non-respondents and will submit this analysis separately.

13. Response rates to the survey have not been presented; that is the proportion of respondents out of the total number of persons approached to participate in the survey. This rate represents a precious tool in determining the quality of the sample (even when using quota sampling)

Comment 12: It is not usual to present the response rate for a quota based sampling approach.

All in all, the methodology used exhibits some flaws and lack of disclosure. The missing elements of information can still be obtained. The methodological

drawbacks identified do not disqualify the results.

Comment 13: We are glad to have Dr. Chevalier's overall endorsement of our survey and have indicated above the additional information we propose to supply in response to his observations.

Serious concerns subsist about being able to compare the results of this study to that of other studies obtained previously in Great Britain. The main shortcoming stems from the exclusion criteria of only questioning people that have participated in a gambling activity at least once a month. The net effect of this strategy is to underestimate by 30% the gambling population (compared to the previous Prevalence Study [Europe Economics' underlining]) – therefore the total population being at risk of developing a gambling problem.

Comment 14: This study was a bespoke exercise carried out to fulfil certain specific objectives. We did not propose comparisons with other studies. So far as we are aware, there are no other published studies relating to perceptions or use of FOBTs. The closest we got to a comparative study was the Gambling Prevalence Study published in 2000, and we explained at some length in the report (see paragraphs 3.4.1 to 3.4. 16) why our study was not comparable with that. We understand that ABB intends to ask Europe Economics and MORI to repeat this study later this year. If so, we will certainly use the experience gained from the current design.

This strategy will also underestimate problem gambling prevalence. The gambling problem screen test typically frame the temporal scope of the questions to one year: e.g. «Have you in the last 12 months been criticized for your gambling?». The exclusion criteria therefore excludes problem gamblers that would have stop or reduce their gambling activities within the last year. The criteria also eliminate those problem gamblers that sporadically abuse of gambling or binge on gambling.

Comment 15: This survey did not aim to replicate the methodology adopted for the Prevalence Study. Our aim was to focus our analysis on regular gamblers. The survey tabulations (all six waves of the Omnibus survey and the survey of betting shop customers) clearly show the profile and actions and opinions of those who are not regular gamblers, i.e. those who gamble less than once per month.

Within itself the results are of sufficient quality but outmost caution is called for if these results are to be compared to those of other surveys in the field. I would also strongly recommend that it should be made much clearer to the reader that the results in the tables and figures are focused on **regular** gamblers rather than **total** gamblers.

Comment 16: We acknowledge the difference between “regular” and “total” gamblers (i.e. those who gamble at all). The final report will make readers fully aware of the distinction.

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**APPENDIX 2**  
**Dr Purdon's Peer Review**

Comment 17: We welcome Dr. Purdon's comments, and are of course aware of her contribution to the Gambling Prevalence Survey.

**FIXED ODDS BETTING TERMINALS AND THE CODE OF PRACTICE**

**General comments**

1. Overall, the research design seems fit for purpose although the achieved sample of FOBT users (in both the omnibus and the betting shop surveys) is rather low for detailed analysis. Focussing more of the research on the betting shop interviews rather than running the question on so many waves of the omnibus might have, in retrospect, been a better use of resources. The recommendations for follow on work should include some recommendations on future sample size.

Comment 18: We agree. At the time this study was conceived and commissioned we did not know the relative size of each of the various populations that needed to be investigated. We made the best estimates we could within the budget available. We certainly intend to discuss with ABB the lessons learned from this study for the second round study.

2. The report is clearly written. But a short summary at the start of each section saying what the section is about would help readers to negotiate through the fairly dense sets of statistics.

Comment 19: We are glad that Dr Purdon found the report clear but would be happy to add a summary at the start of each section.

3. However, there seems to be a mis-match between the stated objectives of the research and the presentation of the findings. The purpose of the research is described as 'to measure and explain levels of problem gambling amongst FOBT users, in the context of other gambling activities (particularly machines)...'. Yet at no point is the rate of problem gambling amongst FOBT users presented (unless I missed it), nor are comparisons with other gambling activities made. (In fact FOBT and problem gambling are treated as rather separate issues throughout the report.) The lack of a link seems like a fairly serious omission, and makes the interpretation of the research difficult for the reader (are FOBTs an area for concern or are they not?). I appreciate that the sample size of FOBT users from the omnibus survey is too small for this analysis, but the betting shop sample size is probably just about large enough.

Comment 20: In the betting shop survey, our sequence of analysis, so to speak, was to focus on betting shop customers, then to scrutinise problem gamblers as the first subset and FOBT users as the second. We see nothing wrong in that, although we acknowledge that one could have analysed FOBT users then problem gamblers. This would have required more FOBT users than we had in order to provide a reasonably large population at the lowest level. The fact is that problem gamblers use multiple forms of gambling (as the Prevalence Study showed, and as our study also showed, albeit from a different sampling method). Paragraphs 7.4.12 and 7.4.13 of the main report deal with the perceived level of problem gambling within the sample.

4. The authors appear to conclude that, because regular FOBT users represent no

more than 0.6% of the population, they are not a concern in terms of problem gambling. But an alternative conclusion could be that, if all these 0.6% became problem gamblers then the rate of problem gamblers in GB would more than double. Under this scenario FOBTs could be a very serious issue!

Comment 21: We do not say that, because there are so few FOBT players in the adult population, they are not a potential concern in relation to problem gambling. The sample is too small to be used for speculation.

5. There is an interesting set of figures given in 7.3.16 to the effect that a quarter of regular FOBT users began visiting betting shops because of FOBTs. It would be worth checking how many of these are problem gamblers. This would give some indication of the impact of FOBTs on problem gambling (on the assumption that most of these people were not problem gamblers before).

Comment 22: Only one out of the 20 betting shop customers claimed to have started visiting betting shops as a result of FOBTs, and one further respondent was categorised as a “problem gambler” (there were no refusals in this category). Please refer to Table 83 relating to question 25 within the detailed computer tabulations for the betting shop survey. The assumption that “most of these people were not problem gamblers before” would be questionable.

6. The report repeatedly talks about the ‘incidence’ of problem gambling. This should surely be ‘prevalence’. (Incidence cases would be new cases.)

Comment 23: We disagree. The Oxford English Dictionary does not confirm that “incidence” refers to new cases.

### Specific comments

Paragraph number	Comment
1.3.3	Not all Omnibus surveys use quota methods, the main exception being the ONS omnibus which uses probability sampling. A fair point. However, the majority of Omnibus surveys across the industry are carried out using quota based sampling. MORI’s Omnibus is a quota based sample with quotas set within Electoral District (ED) by age, gender and work status.
1.3.10	The Gambling Prevalence Survey was a survey of individuals not a survey of households. (The individuals were selected via households, but that does not make it a household survey) A moot point, surely? The sampling was done by household, the individual responses came from individuals within each participating household, but the survey results were presented for individuals. Nothing hangs on this point as regards our own findings.
1.4.4	The percentage within brackets should say (0.6 per cent of the population). Agreed. To get it completely right, the final report will say “of the adult population”.
1.4.5; 4.1.17, 4.1.18 (plus various other paras)	The refusal rates for the prevalence and omnibus surveys are not comparable. The prevalence survey used probability based sampling methods which allow for the rate of refusal by households and individuals within households to be calculated. Quota samples do not collect data on refusals, so the refusal rate is unknown – but most market research

	<p>organisations would acknowledge that the response rate is likely to be much lower than with a probability sample. Furthermore, the response rate for the prevalence study is mis-quoted because out of scope addresses have not been removed from the base. Para 4.1.18 is almost certainly incorrect. I suggest that comments on the relative response rates be removed from the report.</p> <p>We are happy to remove paragraph 4.1.8 on the grounds that it is contentious rather than “probably incorrect” but we do not wish to remove the factual statements in paragraphs 4.1.15 to 4.1.17. For the avoidance of doubt paragraphs 4.1.19 through to 4.1.21 will stay in.</p>
1.6.2	<p>Is covering just three days a week potentially biasing? Is it not likely that customers on these days are different to customers on quieter days?</p> <p>This is a point that MORI, Europe Economics, and ABB members considered very carefully. We took advice on whether customers on quieter days were in any material way different from customers on busier days, and were told not. The important thing for us was to make sure that we picked days when there were a sufficient number of customers to enable the interviewers to cover enough customers within the time and budget available. Compromises had to be made, since we were told that attendance on Mondays, Tuesdays and Wednesdays is low. If MORI had interviewed on six or all seven days of the week, the time and cost involved in getting nearly 1,000 interviews would have been seriously increased. A compromise was struck whereby MORI interviewers undertook the interviews during the weekday (Thursday or Friday) and over the weekend (Saturday). MORI further ensured that, for each betting shop selected to the sample, interviewing took place on a Thursday or Friday and always included a Saturday. We have no reason to think that the interviewing days we used led to bias.</p>
1.6.3/7.3.3	<p>The rationale for the weighting should be explained. What bias is dealt with by it? And why weight to the omnibus stats rather than to self-reports on the number of visits made per month?</p> <p>We will explain the rationale in the final report. The answer is that the weighting was done simply in order to obtain representative betting shop visiting frequencies. We used the Omnibus surveys because they gave the widest basis for weighting. It would be self-evidently incorrect to rely on the stated frequencies only of those who came in on the busier days.</p>
1.6.6/1.7.4	<p>Do these paras refer to ‘all’ or ‘regular’ users?</p> <p>All.</p>
1.8.3	<p>What does ‘problem gambling is a people-related issue’ mean?</p> <p>We will clarify this point in the final report. What we intended is that problem gambling is better looked at by asking problem gamblers what they do and why, rather than by looking at the usage of particular forms of gambling.</p>

4.1.21	<p>Does including the non-response category in the total sample mean that they are effectively treated as not problem gamblers? And if so, is that not biasing?</p> <p>This is not the case. Those who did not complete the self-completion questionnaire (to identify problem gamblers) were analysed separately as a category in their own right (i.e. they were not included as a “problem gambler” or those classified “not to be a problem gambler”).</p>
5.2.2	<p>A sentence saying how the focus group members were recruited would be helpful.</p> <p>We will add the following to the final report:</p> <p>“All respondents were recruited in person by MORI’s fully trained and experienced recruiters. Recruitment took place across four geographic locations – Leith, Stockport, Bristol and Watford. Two groups were held at each location. At each location, MORI recruited potential respondents in betting shops, pubs and amusement arcades. In Leith, MORI also recruited a number of respondents in a local casino to fulfil the quota of those who visit and use this type of establishment.</p> <p>To ensure that the recruits included a good cross-section of those who gamble, Europe Economics and MORI designed a recruitment questionnaire to screen respondents. Quotas were set within location by age, work status, social class and FOBT usage. MORI also ensured that they included a number of problem gambling and online/telephone gambling account holders. All respondents received a £30 incentive for their cooperation.”</p>
5.4.3	<p>The reasoning seems upside down here. Between area differences would be an argument for covering all the country not for focussing on particular areas.</p> <p>We disagree. The qualitative research was exploratory in nature and not meant to be statistically analysed – it provided valuable background information to help us “get under the skin” of those who gamble, and allowed us a better understanding of the issues surrounding the subject. When deciding upon the location of the groups, MORI ensured they were geographically spread around GB.</p>
6.1.1	<p>Should perhaps explain that the omnibus interviews are done face-to-face in people’s homes.</p> <p>We will insert suitable text in the final report.</p>
6.1.4	<p>Was the decision to change the omnibus questions made at the start of the study or mid-way through? What are the implications of the change for any follow-up study?</p> <p>We did not change our minds part way through the Omnibus waves – we had decided in advance on the split. The questions put to interviewees are identical as between the first two and the last four waves. For any second round of research, we would be minded to drop waves 1 and 2, but would want to discuss the implications with ABB.</p>
6.3.9	<p>Is this para relevant? We think so.</p>

6.3.15	Is the 326 weighted or unweighted? <a href="#">Weighted.</a>
6.4.11	Why not add the 23 cases from Waves 1 and 2 to the sample of 48? And what percentage are problem gamblers? <a href="#">Dr. Purdon's comment does not appear to relate to paragraph 6.4.11.</a>
6.5.2	The questions identifying problem gamblers are not time-specific as far as I can see, so the 'last week/past year' issue seems irrelevant. <a href="#">We are not sure which questions Dr Purdon is referring to. The Prevalence Study questions were time-specific, but ours were not. However, at this point in our text we have not embarked on any comparison.</a>
6.5.4	I don't understand why Waves 1 to 2 are more comparable to the prevalence study than waves 3-6 for problem gambling rates. Can an explanation be added? <a href="#">Yes – we are happy to add one. The explanation is that, like the Prevalence Study, waves 1 and 2 consider all forms of gambling available at the time, whereas waves 3-6 consider only a defined subset of gambling activities.</a>
6.5.6-6.5.16	This section does not seem relevant to FOBTs and seems out of place in this report, especially given that the prevalence survey did not cover FOBTs. If the section is included it would make far more sense to use the omnibus survey rather than the prevalence survey and to include FOBTs as a category of analysis. (The estimates are based on the whole sample size and are not a sub-group analysis of problem gamblers (which are the numerator rather than the denominator), so the objections to presenting the figures seem over-cautious.) <a href="#">We cannot agree that the section defined by Dr. Purdon is irrelevant. It seeks to make the point that problem gamblers use multiple forms of gambling, and uses the Prevalence Study to remind our readers that this was already a known fact.</a>
6.5.13	See endnote 1 page 16 of the prevalence survey report. Thank you. <a href="#">We will amend 6.5.13 accordingly, although the fact remains that our study did distinguish between fruit machine and jackpot machines, whereas the Prevalence Study did not. We observed the distinction in order to respond to the DCMS' interest in machine gambling.</a>
6.5.16	I think most of the non-response is unit non-response rather than item non-response. Certainly the tables in the report suggest this is the case. <a href="#">Agreed. We are happy to clarify this point in the report.</a>
Section 7.1	A paragraph could usefully be added explaining how people were selected for interview and any likely biases that that might create (if any). Plus the rationale for the weighting should be explained (either here or at 7.3.3) <a href="#">Interviewers were instructed to work their way systematically around the whole of the betting shop in a clockwise or anticlockwise direction from a given point. As they worked their way around, they were instructed to approach every</a>

	<p>customer or, if busy, every “nth” customer. In many cases, the interviewers aimed to undertake a census of those in the shop (when the shop was not busy enough to select every “nth”). Also, to boost the response rate to its maximum level, respondents unable to take part when initially approached were given the option of being interviewed at a later time when it was more convenient. The most important factor was to ensure that each customer had an equal opportunity of being selected to the sample. i.e. that the interviewers were not to stop only those who looked interested in being interviewed.</p> <p>It is likely that those interviewed would be people who stay in the betting shop for some length of time, rather than those who visit in a hurry and cannot spare the time to be interviewed. We weighted to correct this and would be happy to clarify our reasoning further at 7.3.3.</p>
7.3.5	<p>FOBTs seem to be the only one on the list for which ‘regularly’ is applied. Is this intentional, and if so, why? And how is regularly defined here?</p> <p>All those interviewed, whether in the Omnibus surveys or in betting shops, counted as regular gamblers in the sense that they participated in nominated forms of gambling at least once per month. In the case of FOBT users we distinguished between, on the one hand, those who say they have used them but not longer do so or have used them once or twice, and, on the other “use them”. The latter we have termed regular FOBT users. We would be happy to clarify in the final version of the report, although 7.3.14 does so later.</p>
7.3.14	<p>Why 9% here and 8% in 7.3.5?</p> <p>It is para. 7.3.5 that contains the error– it should indeed be 9 per cent.</p>
7.3.15	<p>Add sample size.</p> <p>Agreed – it is 88.</p>
7.3.16	<p>This para seems important. Shouldn’t more be made of this finding?</p> <p>We have reported it as openly as the other findings that emerged from the study. What further emphasis would be appropriate?</p>
7.3.20	<p>It seems odd to suddenly switch here to all users when previous paragraphs have concentrated on regular users.</p> <p>We did this to get larger numbers of respondents for analysis into the categories reported in 7.3. 21 to 7.3.23.</p>

[end]