The Risk of Gambling Problems

in the General Population: A Reconsideration

by

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November 2018

Forthcoming, *Journal of Gambling Studies*

ABSTRACT.

We examine the manner in which the population prevalence of disordered gambling has usually been estimated, on the basis of surveys that suffer from a potential sample selection bias. General population surveys screen respondents using seemingly innocuous "trigger," "gateway" or "diagnostic stem" questions, applied before they ask the actual questions about gambling behavior and attitudes. Modeling the latent sample selection behavior generated by these trigger questions using up-to-date econometrics for sample selection bias correction leads to dramatically different inferences about population prevalence and comorbidities with other psychiatric disorders. The population prevalence of problem or pathological gambling in the United States is inferred to be 7.7% rather than 1.3% when this behavioral response is ignored. Comorbidities are inferred to be much smaller than the received wisdom, particularly when considering the marginal association with other mental health problems rather than the total association. The issues identified here apply, in principle, to every psychiatric disorder covered by standard mental health surveys, and not just gambling disorder. We discuss ways in which these behavioral biases can be mitigated in future surveys.

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Prevalence studies of disordered gambling have been conducted in many countries over three decades (Williams, Volberg, and Stevens [2012]). In consequence there is widespread consensus that gambling disorder, at least of a level of severity warranting clinical intervention, is a relatively rare mental disorder, though one that has become more common in many jurisdictions as a result of more widespread gambling opportunities. Based on the application of econometric methods for identification and control of sample selection bias, we question this consensus, concluding that prevalence of gambling problems in the general population is likely to be significantly larger than generally thought. The issues identified here apply, in principle, to every disorder for which prevalence is estimated using surveys based on psychiatric screening instruments, and not just gambling disorder.

Scholarly research consistently finds high shares of commercial gambling revenue to be derived from proportions of populations that are much smaller that the large proportion who occasionally or frequently gamble. For the United States 15% of revenue derives from 0.5% of the population, for Canada 23% derives from 4.2% of the population, for Australia 33% derives from 2.1% of the population, and for New Zealand 19% derives from just 1.3% of the population.[1] It is primarily among the ranks of these high-spending gamblers that one finds those who currently have, or are at greatest risk for having, clinically diagnosable gambling problems. And the largest share of casino gambling floor revenue now derives from electronic slot and poker machines, which are strikingly characterized as constituting an *Addiction by Design* by Schüll [2012].

We examine in detail the manner in which the population prevalence of disordered gambling

[1] See Goldstein et al. [1999], Williams and Wood [2004], Australian Productivity Commission [1999] and Abbott and Volberg [2000], respectively. Definitions of those most at risk of gambling-control problems vary across studies, for reasons we discuss in detail, but all are conventional in the extant literature.

has been estimated by psychologists and psychiatric researchers. Surveys of disordered gambling have traditionally used screens designed to detect individuals who engage in gambling activity that might lead them to clinically "present" and meet criteria for diagnosis of a psychiatric disorder. This is a valid scientific goal for the design, calibration and application of such surveys, although it is not the only possible goal or the most interesting for broader public health assessments.[2] We reconsider the manner in which inferences about gambling problems in the general population are made based on these surveys. We suggest that there are different kinds of inferences possible than have traditionally been emphasized, and that there is a recurring, major sample selection bias that has not been accounted for. When that bias is corrected we infer significantly *greater prevalence* of gambling disorders, and notably *fewer comorbidities* with other mental health problems than are typically reported. Thus we contribute to isolating gambling disorder as a partly discrete public health problem to which policies can be specifically targeted and their efficiency evaluated.

Most of the inferences that have been drawn based on analysis of prevalence surveys have concerned general population prevalence, socio-demographic correlates, and comorbidities. They have typically focused on the binary classification of individuals as "disordered," "pathological" or "problem" gamblers, or not, where these terms are defined either directly or approximately in terms of *DSM-IV* (American Psychiatric Association [1994]) clinical criteria. Since the classification of the condition under "Substance-Related and Addictive Disorders" in *DSM 5* (American Psychiatric Association [2013]), it has become standard usage to refer to it as "Gambling Disorder".

Henceforth, where we refer to the clinical phenomenon *ex cathedra* we will follow *DSM 5* and use "gambling disorder" (GD). We will refer to a representative person who has acquired the

[2] Kessler and Pennell [2015; p. 144ff.] provide a valuable review of the historical evolution of survey research on mental disorders.

condition as a "disordered gambler" (DG). Where we refer to previous work set in clinical contexts that used either "pathological" or "problem" gambling without distinguishing them, or intending that they be distinguished (for example, in some work applying the Problem Gambling Severity Index (PGSI) (Ferris and Wynn [2001[), we will anachronistically use the terms "gambling disorder" and "disordered gambler." Where we are referring to a context in which "problem gambling" and "pathological gambling" are distinguished, with the former denoting a pre-clinical or warning state for the latter, we retain the distinction and use these older terms. Finally, when we talk about harmful consequences of gambling outside the clinical context we use "gambling problems" as a non-technical term of everyday English.

An original goal of *DSM 5* was to shift focus away from categorical classifications emphasized in *DSM–III* and *DSM–IV* (e.g., "pathological/non-pathological") to continuous measures, understood as probing continua between normal and disordered functioning. However, the American Psychiatric Association ultimately decided to defer this ambition, and GD continues to be clinically regarded as a pathology from which a person either suffers or does not. We implicitly consider that classification, but expand the analysis to include the range of gambling problems as an *ordered* hierarchy. Our interest is in the latent continuum of gambling problems, as a complement to studying binary classifications with thresholds.[3] This interest corresponds to our ultimate focus, as economists concerned with the general impact on welfare of gambling and public health policy, on evaluating the severity of all problems associated with gambling, which include but are not limited to the form of addiction that *DSM 5* labels as GD.[4]

[3] Examples of the many studies of the continuum of gambling disorders include Toce-Gerstein, Gerstein and Volberg [2003] and Blanco, Hasin, Petry, Stinson and Grant [2006].

[4] Harrison and Ng [2016] is an example of our general approach, applied to the problems of making decisions over an insurance product to evaluate the welfare cost to the individual of observed choices. That cost is measured by the foregone income-equivalent of the observed choice compared to what a latent

In Section 1 we reconsider inferences from the *National Epidemiologic Survey on Alcohol and Related Conditions* (NESARC) in the United States. The first wave of NESARC was conducted in 2000 and 2001, and had a sample of 43,093 individuals.[5] The instrument for measuring gambling problems was based on the *DSM-IV* criteria, and *DSM-IV* criteria were likewise used for the instruments measuring other major psychiatric disorders.[6]

The most significant statistical issues arise from the difficulty of drawing inferences about GD prevalence and comorbidity when one attempts to account for the sample selection bias of "trigger," "gateway" or "diagnostic stem" questions. Such questions ask whether a respondent has ever gambled more frequently than some threshold rate or number of occasions, and/or whether they have ever gambled away more than some threshold amount of money on any single occasion. Only respondents who report meeting the relevant thresholds are asked the remaining gambling screen questions. A main motivation for use of trigger questions is not to irritate respondents by asking them about gambling problems after they have effectively said that they are not regular gamblers, or perhaps not gamblers at all. This motivation is particularly easy to appreciate in the case of surveys such as the NESARC, which address multiple potential disorders; the surveyor does not want to risk reduced cooperation on other survey modules by annoying respondents about gambling

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structural theory predicts that the individual should have made. Calculating this income cost, which in the case of insurance arguably maps relatively straightforwardly onto welfare costs, requires a different set of data about the individual than one finds in surveys, but the end result is more usefully compared to non-binary measures of the severity of behavior.

[5] The second wave of the NESARC was conducted in 2004/5, and was a longitudinal panel of 34,653 re-interviews from the first wave. The third wave was conducted in 2012/13, with a fresh sample of 36,309 individuals. Gambling prevalence questions were removed from waves 2 and 3 of the NESARC. Our analysis was prepared using a limited access data set obtained from the National Institute on Alcohol Abuse and Alcoholism (NIAA) and does not reflect the opinions or views of NIAAA or the U.S. Government.

[6] A comparable national survey that could also be evaluated in the same manner is the National *Comorbidity Survey Replication* conducted in the United States between 2001 and 2003 with a primary sample of 9,282 individuals. We discuss the *Canadian Community Health Survey of Mental Health and Well-Being* of 2002 and the *British Gambling Prevalence Survey* of 2010 below.

problems they (apparently) manifestly do not have.

The *potential* for sample selection bias arises when there is some systematic factor explaining why someone might not want to participate in the full set of questions, and therefore deliberately or subconsciously selects out of that full set by answering a certain way in response to the trigger question.[7] Sometimes this potential leads to no difference in inferences from the observed sample: for instance, if respondents want to spend more time in a face-to-face interview with more attractive interviewers, and the attractiveness level of interviewers is random, there will be no *a priori* reason to expect an effect on inferences about gambling risks. On the other hand, if someone wants to hide their gambling problems, they might reasonably choose to lie in response to the trigger question. Indeed, hiding gambling problems is explicit in one of the criteria used in the full set of questions for determining the extent to which someone is at risk for GD or should be classified as a DG! There are no perfect statistical methods to correct for this bias, but the bias appears to be significant in the case of several major, influential surveys of gambling problems that used trigger questions. We therefore take some time in section 2 to review the rationale for these trigger questions, and note the vigorous rhetoric sometimes used to defend them. We suspect that the strength of these

[7] Hernán, Hernández-Diaz and Robins [2004] survey the many types of selection bias considered in epidemiology, and provide a general causal framework. The selection bias of concern here is a mixture of what they call "nonresponse bias/missing data bias," "volunteer bias/self-selection bias," and "health worker bias" (p. 618). Various statistical correction methods are discussed in major epidemiology texts, such as Rothman, Greenland and Lash [2012; ch. 19]. To our knowledge, there are no applications of epidemiological *corrections* for these biases to general population surveys with trigger questions, although there are recognitions of their potential importance (Tam, Midanik, Greenfield and Caetano [1996] and Tam and Midanik [2000]). Caetano [2001; p.1543] editorialized on this issue in a clear fashion: "So, are survey respondents different from non-respondents in their use of alcohol and illicit drugs? The answer from the small number of studies mentioned above seems to be positive. But are my critics right in assuming that non-respondents are more likely than respondents to be drinkers, heavier drinkers or dependent on alcohol and illicit drugs? The evidence then suggests that, to use a common American expression, the 'jury is still out.' This is so partly because for the past 40 years those of us facing the critics have been complacent about the validity of survey research. The uncertainty regarding selective non-response should not justify the apparent lack of attention to the issue."

defenses is thought to be justified by an expectation that they have no effect on inference, and the efficiency gains in the time needed to conduct surveys that are apparent from their use.

Section 3 draws some conclusions, including recommendations for future survey design and analysis.

1. Estimates for the United States from NESARC

A. Comorbidities

The prevailing view is that GD typically co-occurs with a variety of other mental disorders. Petry et al. [2005; p.564] evaluated this using NESARC data and concluded that GD is "highly comorbid with substance use, mood, anxiety, and personality disorders, suggesting that treatment for one condition should involve assessment and possible concomitant treatment for comorbid conditions." Panel A of Table 1 replicates their methods and essentially obtains the same results, using a logistic specification.[8] All calculations with the NESARC correct for the complex sampling design.[9] In each row the independent binary variable is whether the respondent is defined as having

[8] Their analysis, and most of those using the NESARC to study DG, suffers from an unfortunate coding error explained in Appendix A. There are in fact 207 respondents that meet the *DSM-IV* criteria, not the 195 used in most studies. The incorrectly coded classification had 21 respondents that should have been classified as DGs, and 9 that should not have been so classified. The effect is to change estimates slightly. We only use the correct *DSM-IV* classification of pathological gambling from the NESARC. None of our qualitative conclusions are affected by using the incorrect classification.

[9] The NESARC used a three-stage sampling design, with a sampling frame of adults aged 18 and over in non-institutionalized settings. Stage 1 was primary sampling unit (PSU) selection using the PSUs from the Census 2000/2001 Supplementary Survey, a national survey of 78,300 households per month. Stage 2 was household selection from the sampled PSUs. Finally, in stage 3, one sample person was selected at random from each household. In stage 1 there were 401 PSUs that were so large that they were designated "self representing," meaning that they were selected with certainty; another 254 PSUs were selected in proportion to 1996 population estimates for each of 9 strata within a state (so there are 10 strata, including the state). Self-representing PSUs within a state are correctly treated as being selected with certainty, and hence contributing nothing to the estimated standard error *as a PSU*.

the indicated psychiatric disorder or not.[10] Petry et al. [2005] examine the risk of being what we would now call a DG, and this is the sole risk level used in Table 1. Each of the odds ratio (OR) estimates in Panel A are much greater than 1, and statistically significantly greater than 1: the lower bound of the 95% confidence interval is well above 1.

These analyses of comorbidities examine "total effects" rather than "marginal effects." We say that one has measured the *total* effect of some secondary psychiatric disorder X on the focus disorder Y when there are no controls for the presence of other psychiatric disorders A, B, C … etc. The *marginal* effect of psychiatric disorder X is measured when one controls for the presence of other psychiatric disorders. Both types of effects can be of interest for public health and clinical purposes, and answer different questions.[11] The total effect answers a question along these lines: "If all I know about a group of people is that they abuse alcohol, how likely is it that they are also DGs?" Another total effect question might be, "If all I know about a group of people is that they are chronically depressed, how likely is it that they are also DGs?" Assume, as is the case, that both total effects are positive and statistically significant. The marginal effect answers a different question, of the following kind: "If I know that people abuse alcohol and/or are chronically depressed, what is the incremental correlation of each disorder with their also suffering from GD?" It could be that the incremental correlation of alcohol abuse is low or non-existent and the incremental correlation of chronic depression is high. These particular correlations *suggest*, but of course do not prove, that there is causality from chronic depression to GD, none from alcohol abuse to GD, and some from

[10] A constant term is always employed as well. This is what Petry et al. [2005] refer to as "model 1," where there are no additional covariates added.

[11] Epidemiologists often report "adjusted odds ratios," which control for covariates. Typically the list of covariates is very small.

chronic depression to alcohol abuse (or *vice versa*).[12] If this suggestion is correct, it has direct implications for treatment for GD. We would argue that marginal effects are closer to what we want to learn about from evaluation of general population surveys, at least for purposes of designing and choosing public health interventions, than total effects.

Panel B of Table 1 shows the estimates of comorbidities, focusing on marginal effects and the implied OR. We use the same econometric specification as Panel A, for comparability. The point estimates are much closer to 1 than the total effects, as are the lower bounds of the 95% confidence intervals. In one case, the comorbidity of anxiety and GD, the OR is not statistically significantly different from 1. The *upper* bound of the 95% confidence interval of marginal effects in Panel B are all well below the *lower* bound of the 95% confidence interval of total effects in Panel A.

Panels C and D of Table 1 show comparable estimates of total and marginal effects if one includes a long list of socio-economic and socio-demographic covariates.[13] There is a slight lowering of most of the OR compared to Panels A and B, respectively, but no significant change from the conclusions drawn from Panels A and B.

B. Sample Selection

Panel E of Table 1 lists additional covariates from the logistic model estimated to obtain the marginal effects in Panels C and D. To *informally* motivate the concern with sample selection bias,

[12] We know well the dangers of inferring causality from correlations, and indeed this concern is why many modern surveys of mental health take time to ask additional questions about "age of onset." This information is particularly important when asking about incidence over lifetime frames, since the correlation might have any one of three temporal sequences (prior, simultaneous, and posterior). This is also why one-shot general population surveys are not the same as clinical evaluations that occur over several meetings, despite the attempt to ask questions about the clinical significance of symptoms. Moreover, it becomes difficult in general surveys to ask enough about the history of an individual to establish if a disorder is "substance-induced," which is one exclusion criteria used for mood disorders, for example.

[13] This is "model 3" of Petry et al. [2005].

focus on the OR ratios in Panel E in bold. Imagine we encounter men, Blacks, those separated by divorce or death, people living in the West, those without a college or graduate degree, and those with a personal income over $70k at the time of the survey. The value of these OR estimates, and their statistical significance, tell us that respondents with these characteristics are more likely to be DGs. So suppose we encounter respondents with these characteristics who happened not to respond affirmatively to the trigger question? Without knowing their responses to the trigger question we would be inclined to suspect them of *some* greater-than-baseline risk of GD, *ceteris paribus*. The only reason they are not so classified is that their response to the trigger question led to them being *assumed* to have no current or past gambling problems and, therefore, no risk of GD. This involves two fallacious inferences: first, that no one who says "no" to the trigger question has any current or past gambling problems, and, second, that there are no other potential indicators of risk. We can easily imagine some degree of sample selection bias if the responses to the trigger question are correlated with the characteristics that constitute these additional indicators.[14] This is loose and informal, since it is based on a "chicken and egg" fallacy – we are looking at estimates that ignore this sample selection correction to motivate the possibility of sample selection bias. But as long as we check this with appropriate methods, this motivation is acceptable.

The sample selection models developed by Heckman [1976][1979] meet this need. They require the researcher to specify a sample selection process, characterizing which respondents appear

[14] This example also points to the logic of the correction for sample selection discussed below. If there is a correlation between the unobserved characteristics that affect one's selection into the sample and the unobserved characteristics that affect one's chance of being at risk for gambling problems, then the *residuals* from equations measuring these two behavioral responses (to the trigger question, and then to the full set of questions) will also be correlated. This correlation of the residuals, or covariance, is used to infer what the responses would have been to the full set of equations if there had not been this systematic selection into the sample responding to the full set of questions. Note that we stress the idea of a "systematic" selection bias, with no presumption that it is a deliberate choice to lie in response to the trigger question.

in the main survey and which do not. Typically this is a simple binary matter, so one can specify this process with a probit model. In our case the sample selection consists of some trigger questions we examine in a moment; if the respondent passes these, they are admitted to the main survey and asked the *DSM* criteria questions. The Heckman approach also requires a model of the data generation process in the main survey. In our case this might consist of a binary choice statistical model explaining whether someone meets the *DSM* threshold for being *potentially* classified as a DG.

In the original setting studied by Heckman [1976][1979] the main data generating process of interest, and potentially subject to sample selection bias, had a dependent variable that was continuous, and the specification was Ordinary Least Squares. In our case, at least initially, the main data generating process underlying the classification as a DG is binary, and the same ideas carry over: Van de Ven and Van Praag [1981] is the first application of sample selection to a probit specification of the behavior of interest, and Lee [1983] and Maddala [1983] provide general expositions.

One important assumption in the standard sample selection model is to specify some structure for the errors of the two equations, the sample selection equation and the main survey question. If both equations are modeled with probit specifications, for example, the natural first assumption is that the errors are bivariate normal.[15] We assume instead a flexible semi-nonparametric (SNP) approach due to Gallant and Nychka [1987], applied to the sample selection model by De Luca and Perotti [2011]. This SNP approach approximates the bivariate density function of the errors by a Hermite polynomial expansion.[16]

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[15] The methods we use are full maximum likelihood. The "limited information" estimator of Heckman [1976][1979] did not require all of the properties of the bivariate normal distribution. All that was required was that there be a linear relationship between the errors of the two equations, and that the error of the sample selection equation be marginally normal (so that one could calculate the inverse Mills ratio).

[16] This SNP approach is computationally less intensive than comparable approaches based on the estimation of kernel densities. There is some evidence from Stewart [2005] and De Luca [2008] that this SNP approach has good finite sample performance when compared to conventional parametric alternatives and

In addition, another important assumption in the sample selection model, said to be "good for identification," is to find variables that explain sample selection but that *a priori* do not explain the main outcome. In many expositions one sees the comment that in the absence of these "exclusion restrictions" the sample selection model is "problematic." Often this is a major empirical challenge, since it can be hard to exclude something from potentially affecting the main variable of interest, but to include it as likely to affect sample selection. In epidemiology, for instance, a spirited defence[17] of the use of sample selection corrections to estimates of HIV prevalence in Bärnighausen et al. [2011a] came from Bärnighausen et al. [2011b] on the grounds that they had access to ideal exclusionary restrictions: the identity of the survey interviewer. We agree that this exclusion restriction is an attractive and reasonably general one, but it is not universally applicable.

What is *particularly* "problematic" in the absence of *a priori* convincing exclusion restrictions is that one must rely on having the right econometric specification if the sample selection model is to correct for sample selection bias. This specification in turn refers to the specification of the two equations as probit models, and specifically to the assumed bivariate normality of errors.[18] The

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other SNP estimators. Stewart [2004; §3] provides an excellent discussion of the mild regularity conditions required for the SNP approximation to be valid, and the manner in which it is implemented so as to ensure that a special case is the parametric (ordered) probit specification. Appendix C presents the formal statistical model.

[17] Criticisms were raised by Geneletti, Mason and Best [2011] in response to epidemiological applications of corrections for sample selection by Chaix et al. [2011] and Bärnighausen et al. [2011a].

[18] Thus one finds comments such as: "Theoretically, we do not need such identifying variables, but without them, we depend on functional form to identify the model. It would be difficult for anyone to take such results seriously because the functional form assumptions have no firm basis in theory." (StataCorp [2013; p. 782]). A similar comment from Bärnighausen et al. [2011b; p. 446] in an epidemiological setting is that the "... performance of a Heckman-type model depends critically on the use of valid exclusion restrictions...." It is agreed that the functional form assumptions, including the bivariate normal error assumptions, have no firm basis in theory, but we make such assumptions all the time in other settings. If we can indeed test them, that would be ideal, but it is not clear why we should in this instance not use them if we have to. Our view is that these models should be viewed as statistical "canaries in the cave," in the sense of pointing to potentially disastrous conditions that warrant immediate investigation. In other words, and to put the inferential shoe on the other foot, if some estimates show great sensitivity to sample selection corrections

importance of having the right specification of the error distribution also applies even when one does have exclusion restrictions.

As it happens, there are ways to construct exclusion restrictions in NESARC that have some *a priori* credibility. For instance, we know the day of the week on which the interview was conducted, and can condition on Friday, Saturday or Sunday interviews as potentially generating differential response. We also know how many trigger questions for other disorders a subject had answered affirmatively by the time the gambling trigger questions were asked, as one measure of how much time and "patience" had been taken up by that stage of the interview. Additional characteristics of the individual are available from baseline questions, and can be used to identify the sample selection equation. But such exclusion restrictions do not always arise in other surveys of gambling, even major epidemiological surveys. In general we recommend survey methods that do not require these sorts of tradeoffs (between finding attractive exclusion restrictions and reliance on the assumed stochastic structure for identification), but with existing surveys some tradeoffs are often needed.

To set the stage for the evaluation of sample selection corrections, Table 2 and Figure 1 show the estimated OR between GD and other psychiatric disorders when using the SNP approach rather than the parametric logistic specification. The total comorbidity estimates in Panel A of Table 2 are comparable to those in Panel C of Table 1; similarly, the marginal comorbidity estimates in Panel B of Table 1 are comparable to those in Panel D of Table 1. With the SNP approach, however, the marginal comorbidities are not quite as close to 1 as with the parametric model. However, the same qualitative conclusions about the relationship of total and marginal comorbidity still apply.

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with these assumptions, and some decent effort to find good specifications, then one should not ignore that evidence because some of the parametric assumptions are untestable.

Figure 2 shows marginal effects of comorbidities when one undertakes sample selection corrections.[19] The covariates used for this exercise are the same full set used in "model 3" of Petry et al. [2005], and are used for both equations.[20] In addition, for the sample selection equation, we used a set of 29 variables reflecting recent events in the life of the respondent (e.g., family deaths or illness, job layoff, change in job, problems with neighbors or friends, criminal problems), height and weight, days of the week for the interview, and the number of previous trigger questions answered affirmatively. The variables reflecting life events only referred to the last year or last few months prior to the interview, and we are examining GD incidence across the lifetime frame. Table 3 presents detailed estimates: for now, focus on Panel C, which shows OR with respect to the GD risk level. The effect of sample selection corrections is clear: the OR estimates are generally much lower. The estimated correlation between the two equations in the selection model, a measure of the importance of sample selection corrections, is -0.19.

C. The Hierarchy of Gambling Disorders

We turn to the hierarchy of gambling disorders, and inferences about general population prevalence. For example, the PGSI classifies samples into the categories "Non-Gambler," "Low Risk for Problem Gambling," "Moderate Risk for Problem Gambling," and "Problem Gambler."[21] Previous statistical evaluations of these hierarchies have not, to our knowledge, formally recognized

[19] We undertake sample selection corrections for GD, but not for the other psychiatric conditions. Instead we use the NESARC determinations of diagnosis. An important extension of our approach would be to simultaneously undertake sample selection corrections for all conditions and then assess comorbidity with respect to the corrected diagnoses for all conditions.

[20] Appendix B documents these covariates.

[21] The intended interpretation of risk here is not prospective (the probability of developing GD at some point in the future). Rather, it is intended as the risk that the respondent would currently be diagnosed as a DG if he or she participated in a full clinical interview with more reliable discrimination.

the *ordered* nature of the categories used in standard survey screens, which are derived directly from clinical screens. When several categories are ordered there are appropriate estimation procedures that use this information. The most popular are ordered probit models in which a latent index is estimated with "cut points" to identify the categories. We employ a SNP version of this type of ordered response model, developed by Stewart [2004] and extended by De Luca and Perotti [2011] to allow for sample selection corrections. We classify respondents into 4 categories: **Non-Indicated** individuals have no *DSM-IV* criteria or were not asked about them; **Weakly Indicated** individuals meeting 1 or 2 *DSM-IV* criteria; and **Moderately Indicated** individuals meeting 3 or 4 *DSM-IV* criteria.[22] We retain the terminology used in the NESARC, and refer to individuals who meet 5 or more *DSM-IV* criteria as **Pathological Gamblers**.

Figures 3 and 4 report estimates from a SNP ordered response model that ignores sample selection and estimates that correct for it. We use the estimates from these models to predict the fraction of the population in each of our four categories above. As a control, it is useful to note that the fractions of the population from the raw data found in each *DSM-IV* response number "bin" are recovered by the estimated ordered response model when we do not correct for sample selection:

[22] Most of the DSM criteria include the requirement that the symptoms be "clinically significant." This is normally identified by questions asking if the symptom(s) led to any contacts with medical professionals, use of medication more than once, or led to interference with "life or activities." For reasons of survey efficiency, these questions are normally asked only if the respondent meets some threshold level of symptoms. Hence one must be careful to recognize that anyone that has met fewer than the threshold level of symptoms will not have been asked about clinical significance (and, more generally, that these thresholds can be applied differently across general surveys, leading to apparent discrepancies in prevalence estimates, as stressed by Narrow, Rae, Robins and Reiger [2002]). There are no such criteria for GD evaluation in *DSM 5* since the symptoms themselves are viewed as evidence of "clinically significant impairment or distress" (American Psychiatric Association [2013; p. 585]). However, *DSM-III*, *DSM-IV* and *DSM 5* all contain exceptions for anyone whose gambling behavior is not "better explained" by a manic episode. This exclusion criterion is also only asked in surveys if someone met the threshold level of symptoms. For NESARC there are only 25 (7) out of 68 respondents to this question who said that any (all) of the times they gambled happened "during a period when they felt extremely excited, extremely irritable or easily annoyed." These respondents constitute only 0.042 (0.016) *of a percentage point* of the population. For consistency of interpretation across the hierarchy, we do not apply this exception.

94.6% Non-Indicated, 4.0% Weakly Indicated, 0.9% Moderately Indicated, and 0.4% Pathological Gamblers. Hence we know that the base statistical model we have estimated is not biased relative to the raw data, as we have binned it. These base predictions are referred to as the Uncorrected predictions in Figures 3 and 4. We therefore find a common result, that the prevalence of Pathological Gambling is around 0.4%. To the extent that our "Moderately Indicated" individuals are taken to approximately correspond to what some researchers (e.g., Pietrzak et al. [2007], Algeria et al. [2009] and Nower et al. [2013]) categorize as sub-clinical "Problem Gamblers," the sum of the two most troubled categories produces a figure of 1.3%, familiar from much of the GD prevalence literature. The Corrected predictions, allowing for sample selection biases, are again dramatic. The fraction of Weakly Indicated increases from 4.0% to 8.3%, the fraction of Moderately Indicated increases from 0.9% to 3.9%, and the fraction of Pathological Gamblers increases from 0.4% to 3.8%. Hence prevalence of Pathological Gamblers plus Moderately Indicated is 7.7% when sample selection bias is corrected, compared to 1.3% when no correction is applied.

It is worth stressing that this result obtains not simply because the sample selection model predicts that more people will get through the gateway of the trigger question, although it does predict that. The observed fraction being selected by their responses to that question is 27%, and the predicted fraction from the sample selection model who would have been selected if they answered the trigger question accurately (according to the empirical specification) is 58%.[23] The issue is also a

[23] Because the predicted fraction to be selected exceeds the observed fraction, one might just assume that the selection equation is mis-specified, and this is the simple explanation for our findings of a higher prevalence of individuals at risk. However, the predicted probability of being selected in the sample selection model is the predicted sample conditional on covariates *plus an error term* for that selection equation. In the usual parametric sample selection specification this error term is *assumed* to be zero, so these observed and predicted fractions should be more or less the same. However, the semi-nonparametric specification does not assume this error term to be zero, as emphasized by DeLuca and Perotti [2011; p.218]. Hence the predicted fraction could be larger or smaller than the observed fraction. This point further illustrates how the sample selection model benefits from not having to impose a parametric stochastic structure.

matter of which *profile* of subjects is predicted to be selected. The sample selection model predicts *more* of the types of people predicted to flag *more DSM* criteria, and *fewer* of the type of people predicted to flag *fewer DSM* criteria. Thus sample selection is, as emphasized by Heckman [1976][1979], fundamentally an issue about allowing for unobserved heterogeneity.[24]

Figure 4 displays the distribution of predictions, with and without sample selection corrections, as well as indicators of the statistical significance of the effect of sample selection. Consider the top left panel in Figure 4, for the "Non-Indicated" category of gambling risk. The Uncorrected distribution of predictions reflects the results of simulating 100 random draws for each NESARC respondent from the predicted marginal probability of Non-Indicated, using the estimated SNP ordered probit model. Each random draw is from a normal distribution whose mean is the point estimate of the marginal probability for that subject, and whose standard deviation is the standard error of that point estimate, again for that subject. Thus the 100 random draws for each subject reflect individual-specific predictions, taking into account the statistical uncertainty of the prediction. The Corrected distribution of predictions is generated similarly, using the estimated SNP ordered probit model allowing for sample selection. Since there are 43,093 respondents to NESARC, each of the kernel densities in Figure 4 reflect 4,309,300 predictions.

These densities in Figure 4 allow one to see the average effects shown in Figure 3, the decrease in predicted Non-Indicated respondents from 0.946 to 0.839, but also to visualize the

[24] The survey of gambling disorders in the Canadian Community Health Survey (CCHS) of Mental Health and Well-Being of 2002 illustrates this point perfectly. Their gateway questions resulted in only 1,754 of 36,884 subjects being asked the full set of questions from the Canadian Problem Gambling Index (CPGI), the full clinical assessment protocol from which the PGSI short field screen is derived. In the raw data one observes 2.8%, 1.5% and 0.5% classified as Low Risk, Moderate Risk and Problem Gambler, respectively, using the categories defined by Statistics Canada for the CCHS. Thus 4.8% are classified as "at risk." After sample selection corrections these become 0.6%, 1.7% and 2.3%, respectively, or 4.6% in total. So virtually the same fraction are classified as "at risk," but the composition is more heavily weighted toward those at greatest risk for GD.

precision of this difference. A *t*-test for each NESARC respondent generates a *p*-value for the hypothesis that the predicted marginal probability is the same with and without sample selection corrections. The 90th, 95th and 99th percentiles of this distribution of 43,093 *p*-values are tabulated in the top-left panel of Figure 4. We find that the predicted *decrease* in No Risk is statistically significant, in the sense that the 99th percentile of these *p*-values is 0.0001 or lower.[25] Similarly, the average predicted *increases* in the Weakly Indicated, Moderately Indicated and Pathological Gambler categories (Figure 3) are also statistically significant, with the 99th percentile of *p*-values again being 0.001 lower in each case (Figure 4).

Figure 5 shows a decomposition of the processes underlying the sample selection correction, to better understand the logic. For each category of gambling problem or risk, it displays the *conditional probability* of being classified in that category depending on whether the subject is predicted to be "selected out" or "selected in" by the trigger question. For instance, if someone is predicted *not* to be selected in, the probability of them being classified as Weakly Indicated is 0.142; if that person *is* predicted to be selected in, the probability of them being classified as Weakly Indicated is 0.040. Since the predicted probability of being selected in is 0.580, this implies that the weighted probability of being in the Weakly Indicated bin is [0.580 × 0.040] + [(1-0.580) × 0.142] = 0.083, which is the value shown in Figure 4 for being Weakly Indicated with sample selection correction.

Table 3 shows the predicted OR with respect to other psychiatric disorders for each category of the gambling hierarchy model with and without sample selection corrections. For each category of gambling problem or risk the OR for each disorder is much smaller when corrections are made for sample selection. Again, the upper bound of the 95% confidence interval with sample selection

[25] The percentile value is purely descriptive, as a summary statistic for 43,093 *p*-values. The *p*-value is the *inferential* statistic.

corrections is always below the lower bound of the same confidence interval without sample selection corrections.

2. Sample Selection Bias and Gambling Survey Screens

Survey screens have been traditionally designed to provisionally identify individuals who are likely to meet clinical criteria for GD. This has various implications for the design and format of the survey questions, which have evolved over time. Here we evaluate some of the issues that flow from that design objective as those relate to the use of trigger questions, ending with constructive suggestions to mitigate the sample selection biases such questions generate.

A. The Evolution of Trigger Questions

The history of the South Oaks Gambling Screen (SOGS) provides an important exemplar of these origins and concerns. The initial stages of the development of the instrument involved South Oaks Hospital patients already admitted for some alcohol or drug dependency, and was prompted by knowledge from previous clinical treatment of the correlations between these addictions and gambling problems (Lesieur, Blume and Zoppa [1986]). In the initial pilots of screen designs, if "the patient denied any gambling, he or she was not interviewed further" (Lesieur and Blume [1987; p. 1185]). On the other hand, later care and conversations might reveal that some deception had occurred, in which case the patient was re-interviewed (*ibid.*). The pilot questions, and the subsequent finalized SOGS, were directly motivated by the criteria stipulated in *DSM-III* (American Psychiatric Association [1987]), albeit with modifications to focus less on late stage, "desperation

phase," symptoms.[26]

The final instrument, presented in Lesieur and Blume [1987; Appendix 1], was cross-validated by being given to 213 members of Gamblers Anonymous, 384 university students, and 152 hospital employees. The logic of this cross-validation was that the first group are self-identified as having gambling problems, while the last two groups were presumptively expected not to be DGs. Hence the detection of GD propensities of 98%, 5% and 1.3%, respectively, by the SOGS response scores was viewed as providing evidence of 2% false negatives, 5% *tentative* false positives, and 1.3% *tentative* false positives, respectively.

The clinical origins of SOGS did not mean that it automatically translated into an ideal epidemiological instrument, and indeed it was subsequently largely supplanted from that use by other instruments, such as the PGSI, thought to be more accurate. An important early warning was raised by one of the SOGS authors, Lesieur [1994], who carefully noted how seemingly minor changes in sampling procedures and question wording might completely change the interpretation, and claims of validity, of the instrument.

An important exception to the emphasis on clinical objectives for GD survey instruments is offered by Currie, Miller, Hodgins and Wang [2009], who argue that many gamblers who report no occurrent or historical gambling problem might be "at risk" in a broader public health sense. That is, someone identified in a survey as having no gambling problems might have a heightened propensity to engage in other behaviors that predict vulnerability to GD, and for that reason might be of interest to public health forecasting.

There is no mention of a trigger question in the first epidemiological applications of SOGS

[26] The original *DSM-III* criteria stressed disruption of personal, family and employment activities. The revised criteria in *DSM-III-R* added physiological symptoms such as withdrawal problems.

in the United States reported by Volberg and Steadman [1988][1989], or in the revised SOGS surveys for New Zealand reported by Abbot and Volberg [1996]. One of the first surveys to have used a trigger question appears to be Dickerson, Baron, Hong and Cottrell [1996]. Since then, the use of trigger questions has become standard, particularly in large-scale epidemiological surveys, as the review of national prevalence studies by Williams, Volberg and Stevens [2012] shows. There are continuing debates about the nature of those trigger questions, but they generally concern whether participants should be asked about their gambling over lifetime or only past-year frames. There is also critical discussion about whether monetary loss thresholds should figure in questions. Stone et al. [2015] emphasize these issues, while also signaling awareness of potential sample selection bias introduced by use of trigger questions, but do not address measures to explicitly correct for it.

A somewhat aggressive defense of trigger questions is provided by the Australian Productivity Commission [1999; volume 3, page F14]:

> The [Australian] *National Gambling Survey* did not administer the SOGS to all respondents – indeed there are good reasons why gambling surveys do *not* ask the problem gambling screen of *all* participants:
>
> - questions about what people do when they gamble are clearly of *no relevance* to non gamblers. In the *National Gambling Survey*, respondents were classified as a non gambler only after they had answered 'no' to thirteen separate questions about whether they had participated in any of twelve specified gambling activities and an 'any other' gambling category. Hence, this detail of questioning should reliably identify a genuine non gambler.
> - a problem gambling screen is of *little or no relevance* to infrequent gamblers because their gambling is very unlikely to be associated with problematic behaviour; but
> - it *is* most appropriate to administer a problem gambling screen to those respondents whose gambling has a greater likelihood of giving rise to problems.
>
> Indeed, as the NORC [National Opinion Research Center] study (Gerstein et al. 1999) noted:
>
> > We chose to use these "filter" questions in the national survey after our pretesting indicated that nongamblers and very infrequent gamblers grew impatient with repeated questions about gambling-related problems (p. 19).
>
> For these reasons, the problem gambling instrument was administered only to that subset of gamblers considered most likely to experience problems related to their gambling – all 'regular' gamblers as defined by filter 2 and 'big spending' and other non-regular gamblers

captured by filter 3.

We would rephrase the last sentence as follows: for these reasons, the GD screen used by Gerstein et al. [1999] was administered only to that subset of gamblers considered most likely on the basis of *ex ante* theory to experience gambling problems. As we discuss below, best-practice survey design *should* bring prior theory to bear, but for the purpose of gathering data that contributes to modeling sample exclusions, rather than as a basis for filtering out some information altogether.

The form of the trigger question is raised as an issue by Volberg and Williams [2012; p. 9] as follows:

> A final important methodological variation that is known to have a significant impact on problem gambling prevalence rates concerns the threshold for administering problem gambling questions. Engaging in any gambling in the past year is a common criterion used to administer questions about problem gambling. However, Williams and Volberg [2009][2010] found that this criterion results in too many false positives on problem gambling screening instruments (as assessed by subsequent clinical assessment). These false positives can be significantly reduced by (a) using a higher threshold for the designation of problem gambling (i.e., CPGI[27] 5+ versus CPGI 3+); and/or (b) requiring a minimal frequency of gambling in the past year (i.e., at least 10 times on some format) before administering problem gambling screens; and/or (c) resolving these cases of inconsistent gambling behaviour by automatically asking people to explain the discrepancy between their problem gambling classification in the absence of significant gambling behaviour, or intensive gambling involvement in the absence of reports of problems.

Indeed, Williams and Volberg [2009] conducted a careful evaluation of three survey administration features, and report disturbing effects on inferred GD prevalence:

- they found that just referring in the introduction to a "gambling survey" rather than a "health and recreation survey" caused a 133% increase in estimated GD prevalence;
- using face-to-face interviews rather than telephone interviews led to a 55% increase; and

[27] The citation, strictly speaking, refers to the PGSI, the short scored field screen of the CPGI.

- using a trigger question with a cutoff of C$300 in annual gambling losses, compared to the trigger of any gambling in the past year, would have implied a 42% decrease.

The conjectured rationale for the first effect is that gamblers like taking gambling surveys, which economists regard as a classic sample selection effect. The second effect is simply demographic, and would be easy to correct with the right sample weights in the population: men respond more to one mode of interview than women, and men gamble much more than women. No explanation for the final effect is offered, although Williams and Volberg [2009; p.112] note that one of their subjects who was in this category revealed an interesting issue:

> There was one individual with a CPGI score of 12 despite not reporting any past year gambling. It is interesting to note that this person reported having a history of problem gambling prior to the past 12 months, which may have influenced his responses to the CPGI past year questions.

This subject, it seems, had gambling under control in the year before the survey, but based on earlier history might be conjectured to still be vulnerable to GD under certain conditions. Such a fact might not be clinically important at point of presentation, but should be relevant to public health forecasting, or to regulatory officials deciding whether to license new gaming facilities.

Possible ambiguity of some threshold questions also raises sampling concerns. Blaszczynski, Dumlao and Lange [1997] cite evidence suggesting ambiguity in interpreting the question "How much money do you spend on gambling?" Over five case study vignettes considered by their subjects the most popular interpretation was the net amount of money spent in a session. But other subjects interpreted the same vignette in terms of initial stake, turnover, or even just losses, as well as some random responses disconnected to the information. Blaszczynski et al. [1997; p. 249ff.] suggest

> that the most relevant estimate of gambling expenditure is net expenditure. [...] It is recommended that future prevalence studies provide adequate instructions on how to calculate the net expenditure by drawing subjects' attention to the difference

> between amounts invested and the residual at the conclusion of each session. It is suggested that wins reinvested during particular individual sessions should be ignored.

A similar issue was examined by Wood and Williams [2007], who evaluated 12 different ways of asking this question, and concluded (p. 72) that, "In general, retrospective estimates of gambling expenditures appear unreliable." To be sure, some ways of asking the question elicited more reliable responses, by some sensible metrics. And it does not follow that other forms of detecting a gambling threshold suffer the same ambiguities. For instance, asking if someone has gambled five times in the past year may be easier than asking them to tell you how much they spent on gambling in the past year, or even if they recall losing a certain amount of money in any one day in the past year.

There is widespread recognition of the difficulty of asking "how much money have you lost" questions. Some DGs erase prior losses within a gambling session from cognitive book-keeping as soon as they win; Rachlin [1990][2000] and Rachlin et al. [2015] argue that this is one of the characteristics that distinguishes DGs from self-controlled gamblers. Concern with this issue led Sharp et al. [2012], in a South African prevalence study, to pose the question as follows:

> Thinking about the last time you participated in [ASK FOR EACH GAME EVER PLAYED, FROM A PREVIOUS QUESTION], approximately how much money would you say you staked on that occasion – that is the total amount in rands you put down to bet on that activity during that whole evening or day, not the amount you won and not the amount you ended up with at the end? Please take your time to think carefully about this.

They found that subjects identified as DGs based on their PGSI scores tended to take significantly longer to answer this question than people who reported regular gambling but did not score in the GD range.[28]

[28] Sharp et al. [2012] further tried to encourage accurate responses by asking this question separately for each game the respondent reports playing. Since they know mean general house advantages for game types as set by South African regulations, this allows them to compute expected losses to the extent that subjects reported expenditures in the strict sense of that word. Of course this approach was profligate with

B. Mitigating the Effects of Trigger Questions

How might one mitigate some of the effects on prevalence estimates of survey screens that use trigger questions, whatever the form of the question?

First, if possible one could design surveys that do not naively assume that trigger questions lead to no sample selection bias, and indeed we have done that in Denmark as a result of the concerns identified here (see Harrison, Jessen, Lau and Ross [2018]). In this study, questions based on two different loss threshold quanta were asked of respondents at the end of the survey that was administered to all participants. This allowed analysis to compare the actual estimation of GD prevalence, across all levels in the hierarchy of risk, with the hypothetical estimates that would have been generated had those who failed to meet one threshold or the other been assigned to a "Non-Gambler" or "No Risk" category due to being excluded from further screening. We recognize that this can only be done if a few specific psychiatric disorders are the focus of the survey, given limitations on time needed for subject responses.

Several surveys have come close to this ideal, by employing extremely "light" trigger questions that only exclude from the sample those that have never engaged in any gambling over some period, including the mere purchase of a lottery ticket. One example is the *British Gambling Prevalence Survey* (BGPS) of 2010, which asked PGSI and *DSM-IV* questions for 73% of their entire sample of 7,756: see Wardle et al. [2011]. Figure 6 shows that although there are differences in prevalence when correcting for sample selection bias using the *DSM-IV*-based screen, they are not

subjects' time, which can cause them to become impatient and consequently respond less accurately to questions in general.

statistically significant or even quantitatively significant for policy purposes.[29] The analysis of the BGPS also demonstrates that the sample selection correction does not always increase the fraction of the population predicted to be at risk: in this case that fraction drops from 5.0% to 4.8% with correction.

Another example of the value of asking an extremely light trigger question is the first wave of the *Victorian Gambling Survey* (VGS) of 2008, which asked PGSI questions for 75% of their entire sample of 15,000: see Billi et al. [2014][2015] and Stone et al. [2015].[30] Again, Figure 7 shows that although there are economically significant differences in prevalence overall when correcting for sample selection bias using the PGSI, the differences for the most important categories of Moderately Indicated and Pathological Gambler[31] are not statistically significant[32] or quantitatively significant for policy purposes. Of course, if the policy objective is to identify demographic slices that might be at risk, one would need to go beneath these aggregate population prevalence estimates to know if there is a sample selection bias.

On the other hand, the same VGS illustrates the risks of using *additional* threshold questions in order to reduce respondent time during the interview.[33] Starting with the 75% of the sample that

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[29] The U.K. National Centre for Social Research, the Gambling Commission, and the UK Data Archive bear no responsibility for our analysis or interpretation of the BGPS. Figure B1 in Appendix B documents the claim about statistical insignificance of the differences.

[30] Stone et al. [2015] also present results from the *Swedish Longitudinal Gambling Survey*. The data from that study are not available for replication or review (Ulla Romild, Public Health Agency of Sweden; personal communication, October 23, 2016).

[31] For consistency we repeat the categories of gambling problems and risk used in the NESARC data analysis (Figures 3, 4 and 5) rather than the categories reported by the original BGPS and VGS reports from the *DSM-IV*, PGSI, and NODS screens. In Figure 6 the original category is "Problem Gambling," with a *DSM-IV* score of 3 or more. In Figure 7 the original categories are "Low Risk," "Moderate Risk," and "Problem Gambler," respectively; as noted earlier, the PGSI uses "Problem Gambler" as synonymous with the *DSM-IV's* "Pathological Gambler" (and, therefore, the *DSM 5's* "Disordered Gambler." In Figure 8 the original categories are "At Risk," "Problem Gambler," and "Pathological Gambler," respectively.

[32] Figure B2 in Appendix B documents the claim about statistical insignificance of the differences.

[33] Another example of additional threshold questions being used is the Canadian Community Health Survey of Mental Health and Well-Being of 2002. Of the sample of 36,984, 24.6% said that they had not

had gambled at all in the last 12 months, these surveyors added 1,057 individuals who had gambled before then, to arrive at a lifetime sample of gamblers of 81%. But they then employed an additional pre-screening procedure when applying the National Opinion Research Center DSM (NODS) screen of Gerstein et al. [1999] to measure lifetime gambling prevalence. This procedure asks 5 questions about gambling behavior, and only follows up with the additional questions of the full NODS instrument if someone responds affirmatively to one of those 5 pre-screening questions. This procedure drops the VGS sample by 11,075, so that we end up with only 8.5% of the full sample being evaluated with the NODS instrument. Unfortunately, this procedure is not statistically innocent: as Figure 8 shows, it leads to a statistically and quantitatively significant sample selection bias when inferring lifetime prevalence.[34] The fraction of the Victorian adult population that is indicated as being at risk of GD jumps from 5.6% to 10.5% after correcting for sample selection bias, and the fraction of DGs increases from 0.9% to 2.3%.

Second, one can design surveys that use criteria for GD that do not rely on historical gambling experience to measure whether someone is at risk, as is the focus of virtually every trigger question we find in the literature. To the extent that one has a theoretically motivated structural model of the nexus of causal factors for GD, some of which may be present in the absence of gambling opportunities in a person's environment, one can gain information about a respondent's risk for developing GD that is independent of any historical gambling behavior meeting a threshold

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engaged in any of 13 gambling activities in the past year. Then 46.3% of the total sample was not asked the full set of CPGI questions because they had only gambled between 1 and 5 times, at most, for each of the 13 activities. And then 24.0% of the sample was not asked the full set of CPGI questions because they said that they were a non-gambler on the first CPGI question. There were 98 subjects that refused to answer the initial questions about gambling activity, resulting in only 1,759 being asked the full set of questions and having any chance of being scored as "at risk." These deviations from the CPGI screen, and the PGSI index derived from it, were "approved by the authors of the scale" (Statistics Canada [2004; p.19]).

[34] Figure B3 in Appendix B documents the claim about statistical significance of the differences.

of frequency or financial loss. Note that in such modeling, the sense of "risk" is prospective, in contrast to the current risk of misdiagnosis that is operationalized in the PGSI and in DSM-based screens. The Focal Adult Gambling Screen (FLAGS) designed by Schellinck et al. [2015a][2015b] is an instance of such a screen, which has been used by industry analysts around the world, but has been deployed by no prevalence study prior to the use of it by Harrison, Jessen, Lau and Ross [2018]) in Denmark. The value of prospective risk forecasting for policy around gambling facility licensing should be obvious, and this policy goal naturally complements the point we are making about using trigger questions that are not reliant on past gambling experience. When considering jurisdictions that have had bans on certain forms of gambling, or where the transactions costs of engaging in gambling have changed, it is quite possible that someone exhibits traits that would lead them to be at risk of gambling problems in different circumstances than they have experienced.[35] Mitigation of sample selection bias and enhanced policy guidance might thus be achieved by the same research design strategy.

Third, where there is a need for some sort of trigger question or questions to avoid taking too much time in surveys, one can build in random treatments to make it easier to identify sample selection bias. These treatments would be conditions that affect the likelihood of someone participating in a full survey, or engaging in deception due to sensitivity around a question. An example of the former would be financial incentives for participating in surveys, of the kind employed in some surveys and experiments.[36] An example of the latter would be any one of a myriad

[35] To take a stark example, assume that gambling is illegal until one reaches a certain age of consent. Surveys of individuals who have just reached that age would show nobody at risk, but of course that says nothing about the future propensity of the individual to have gambling problems.

[36] For an example from surveys, consider the follow-up to the longitudinal Movement to Opportunity (MTO) field experiment, in which 30% of the sample was randomly assigned to more intensive follow-up; see Orr et al. [2003; Exhibit B, §B1.3] and DiNardo, McCrary and Sanbonmatsu [2006]. This randomized follow-up was in addition to the primary randomization to treatment: (i) a housing voucher with

of survey techniques for using "randomized response" methods to ensure that subjects are not revealing with certainty some sensitive information in response to a question.[37]

Fourth, one could recruit subjects from an Administrative Registry, so that one can again better control for sample selection biases by knowing characteristics of all of those recruited, whether or not they agree to participate. This is not a general option, since few non-Scandinavian countries have general registries, although recruiting from a Census may suffice if access to characteristics of the individuals recruited is possible.

3. Conclusions

Measurement of the population prevalence of the risk of gambling problems, and the psychiatric disorders with which they are correlated, play critical roles in public health policy and policy around the licensing of casinos and other gambling facilities. It should make a substantive difference to policy assessment and forecasts of consequence whether the fraction "at risk" of being current DGs is 1.3% or 7.7%, and that is the pure effect of allowing for sample selection bias in the application of a major, widely-cited, and conventional survey of the U.S. population. In fact, one conjecture as to why investigation of GD prevalence was dropped from follow-up waves of the NESARC in the United States and from the Mental Health module of the Canadian Community

some strings attached and some counseling, (ii) a housing voucher with no strings attached and no counseling, and (iii) a control group. This additional randomization to more intensive follow-up had virtually no effect on results, since the effective response rates for the long-term MTO follow-up were around 90%, and similar across primary treatments. This methodological step was striking, since it provided some controlled basis for inferring sample attrition, which is formally identical to sample selection, albeit in the opposite direction (selecting *out* of the longitudinal sample). For an example from field experiments, see Harrison, Il and Lau [2014], where subjects were offered different non-risky incentives to participate and effects on measured risk aversion assessed after correcting for sample selection.

[37] See Warner [1965] for the original idea, and Blair, Imai and Zhou [2015] for a recent review and application of variants. There are other non-randomized survey methods for encouraging truthful responses to sensitive questions.

Health Survey is that the uncorrected population prevalence was too low to justify resources and interview time asking the questions. Hence it becomes "settled" belief that the prevalence of GD is tiny, and no data are ever then collected to question that belief. It would represent a shameful failure of linkage between best research practice and best policy design if these statistical biases in measurement of population prevalence drove substantive decisions concerning the regulation of gambling or the allocation of resources toward the treatment of GD.

One immediate substantive implication of our findings is to ask if comparable biases distort inferences about other psychiatric disorders, since prevalence surveys for every major psychiatric disorder typically use comparable trigger questions.[38] Although it is conceivable that the distortion could be in any direction, our *a priori* expectation would be that the distortions lead to understatements of prevalence across the board, given the sensitive nature of the trigger questions.

[38] For example, Harrison [2017] applies the same approach to evaluate the population prevalence of nicotine dependence, which is *DSM-IV* code 305.10, and finds comparable biases in the United States using *NESARC*.

Table 1: Logistic Odds Ratio Calculations for Pathological Gambling

	Odds Ratio	Standard Error	*t* for H_0: OR = 1	*p*-value	95% Confidence Interval	
A. Total Effects with Logistic Model and No Covariates						
Alcohol	6.1	0.52	21.1	< 0.001	5.1	7.2
Drug	5.2	0.61	14.3	< 0.001	4.2	6.6
Nicotine	7.0	0.62	22.0	< 0.001	5.8	8.3
Mood	4.8	0.44	17.3	< 0.001	4.0	5.8
Anxiety	3.4	0.32	13.1	< 0.001	2.8	4.1
Personality	9.0	0.81	24.2	< 0.001	7.4	10.7
B. Marginal Effects with Logistic Model and No Covariates						
Alcohol	2.6	0.19	13.1	< 0.001	2.2	3.0
Drug	1.3	0.14	2.2	0.03	1.0	1.6
Nicotine	2.7	0.21	13.2	< 0.001	2.4	3.2
Mood	1.7	0.21	4.2	< 0.001	1.3	2.1
Anxiety	1.1	0.13	0.9	0.39	0.9	1.4
Personality	4.1	0.65	8.8	< 0.001	2.9	5.6
C. Total Effects with Logistic Model and Covariates						
Alcohol	5.2	0.48	17.8	< 0.001	4.3	6.2
Drug	3.9	0.45	11.9	< 0.001	3.1	4.9
Nicotine	6.1	0.51	21.4	< 0.001	5.1	7.2
Mood	5.1	0.52	16.3	< 0.001	4.2	6.3
Anxiety	3.8	0.35	14.5	< 0.001	3.2	4.6
Personality	7.8	0.68	23.6	< 0.001	6.6	9.3

D. Marginal Effects with Logistic Model and Covariates

Alcohol	2.2	0.18	9.4	< 0.001	1.8	2.6
Drug	1.1	0.13	1.2	0.24	0.9	1.4
Nicotine	2.8	0.22	13.5	< 0.001	2.4	3.3
Mood	1.9	0.26	5.0	< 0.001	1.5	2.5
Anxiety	1.3	0.15	2.1	0.04	1.0	1.6
Personality	3.7	0.58	8.4	< 0.001	2.7	5.1

E. Marginal Odds Ratios for Demographic Covariates with Logistic Model

Female	**0.4**	**0.04**	**-9.3**	**< 0.001**	**0.3**	**0.5**
Black	**3.1**	**0.25**	**14.1**	**< 0.001**	**2.7**	**3.7**
Hispanic	0.9	0.11	-1.2	0.24	0.7	1.1
Age 30-44	0.9	0.17	-0.3	0.77	0.7	1.4
Age 45-64	1.3	0.22	1.7	0.09	0.9	1.8
Age 65+	1.1	0.18	0.7	0.51	0.8	1.5
Separated	**1.5**	**0.15**	**4.3**	**< 0.001**	**1.2**	**1.8**
Midwest	1.1	0.14	0.8	0.45	0.8	1.4
South	0.9	0.10	-0.9	0.35	0.7	1.1
West	**1.7**	**0.22**	**4.0**	**< 0.001**	**1.3**	**2.2**
High School	0.9	0.13	-0.1	0.91	0.7	1.3
Some College	1.0	0.08	-0.3	0.78	0.8	1.1
College	**0.5**	**0.07**	**-5.3**	**< 0.001**	**0.4**	**0.6**
Graduate	**0.7**	**0.13**	**-2.2**	**0.03**	**0.4**	**0.9**
Income 2	0.9	0.12	-0.4	0.72	0.7	1.2
Income 3	**0.6**	**0.06**	**-5.3**	**< 0.001**	**0.5**	**0.7**
Income 4	**1.5**	**0.17**	**3.3**	**0.002**	**1.2**	**1.9**

Table 2: Semi-Nonparametric Odds Ratio Calculations for Pathological Gambling

	Odds Ratio	Standard Error	95% Confidence Interval	
A. Total Effects with No Covariates				
Alcohol	6.0	< 0.001	6.0	6.0
Drug	6.2	< 0.001	6.2	6.2
Nicotine	6.7	< 0.001	6.7	6.7
Mood	4.8	< 0.001	4.8	4.8
Anxiety	4.2	< 0.001	4.2	4.2
Personality	8.2	< 0.001	8.2	8.2
B. Marginal Effects with No Covariates				
Alcohol	4.2	0.004	4.2	4.3
Drug	1.6	0.001	1.6	1.6
Nicotine	3.3	0.003	3.3	3.3
Mood	2.4	0.002	2.4	2.4
Anxiety	2.3	0.001	2.3	2.3
Personality	4.6	0.005	4.6	4.7

Figure 1: Comorbidity of Gambling Disorder and Other Psychiatric Disorders

Estimated Odds Ratios using Semi-Nonparametric Ordered Response Model
Source: *National Epidemiological Survey on Alcohol and Related Conditions (NESARC)*

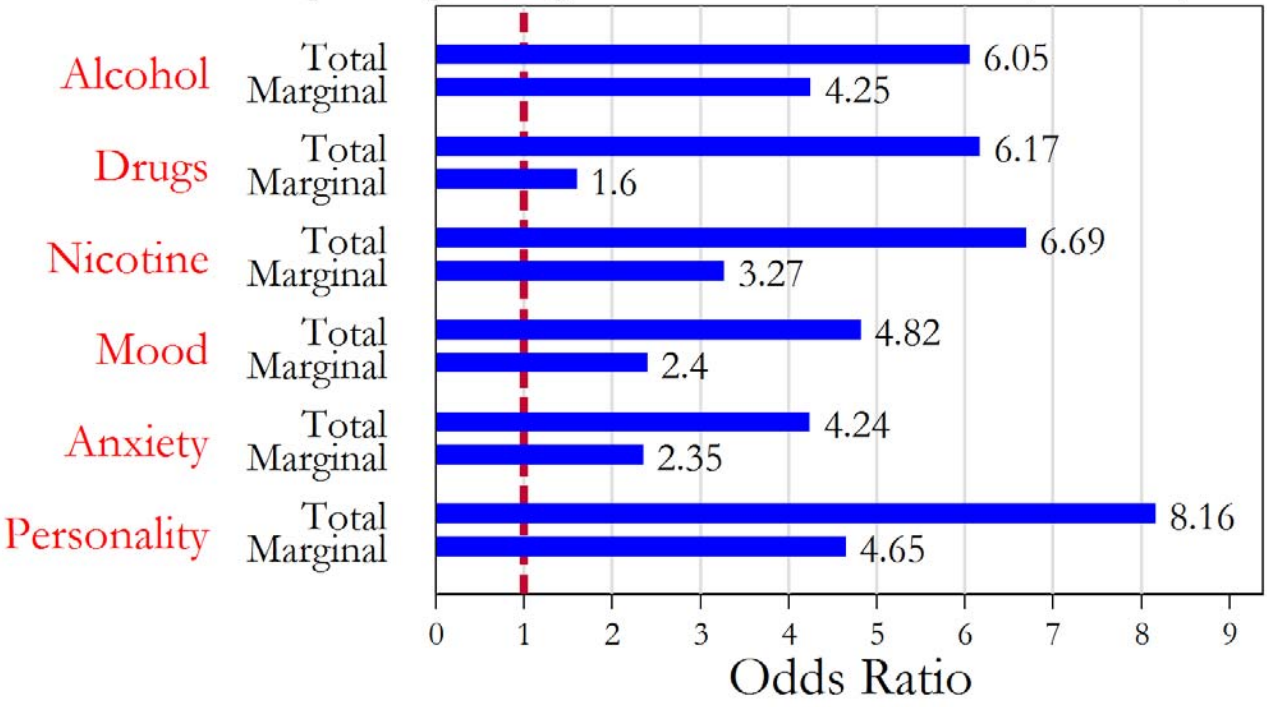


Figure 2: Effect of Sample Selection Correction on Estimates of Comorbidity Marginal Effects with Gambling Disorder

Estimated Odds Ratios using Semi-Nonparametric Ordered Response Model
Source: *National Epidemiological Survey on Alcohol and Related Conditions (NESARC)*

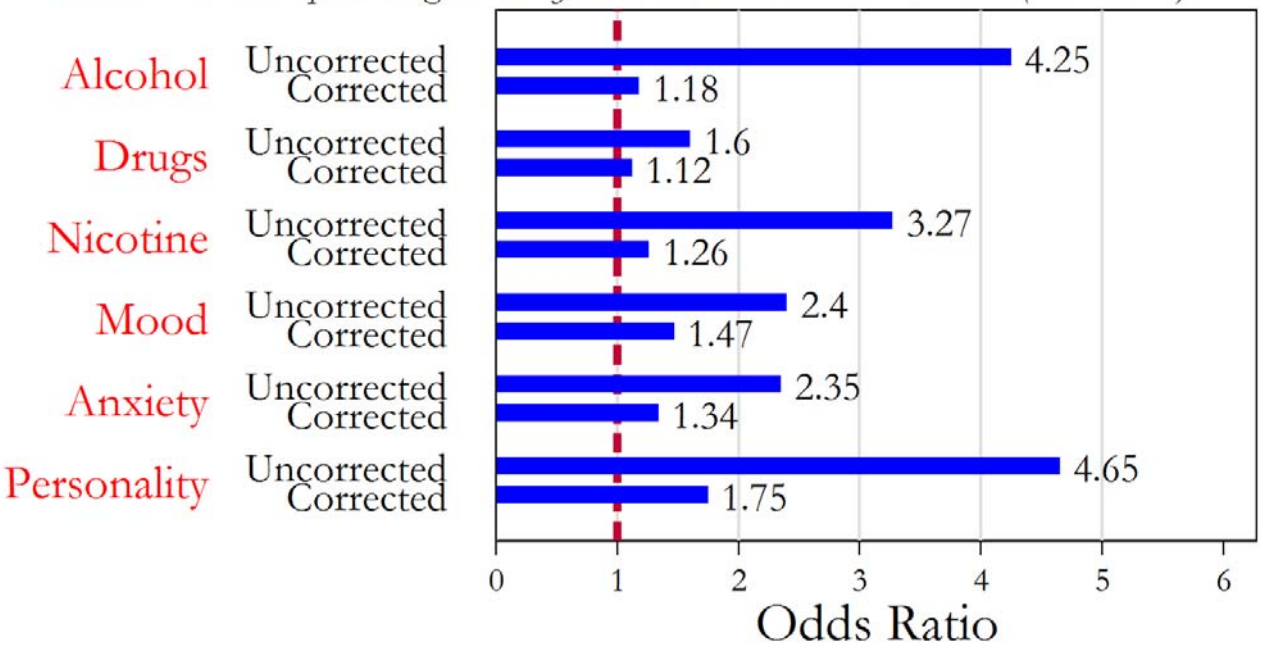


Table 3: Semi-Nonparametric Odds Ratio Calculations for Gambling Risks

Each standard error is less than 0.002, so 95% confidence intervals are the same

	Odds Ratio with No Sample Selection Correction	Odds Ratio with No Sample Selection Correction
	A. Weakly Indicated	
Alcohol	1.95	1.02
Drug	1.25	1.02
Nicotine	1.72	1.03
Mood	1.50	1.05
Anxiety	1.48	1.04
Personality	2.01	1.07
	B. Moderately Indicated	
Alcohol	2.91	1.10
Drug	1.42	1.06
Nicotine	2.40	1.14
Mood	1.91	1.24
Anxiety	1.88	1.18
Personality	3.08	1.35
	C. Pathological Gambler	
Alcohol	4.25	1.18
Drug	1.60	1.12
Nicotine	3.27	1.26
Mood	2.40	1.47
Anxiety	2.35	1.34
Personality	4.65	1.75

Figure 3: Predicted Prevalence of Gambling Risk With and Without Sample Selection Correction

Estimated Probabilities using Semi-Nonparametric Ordered Response Model
Source: *National Epidemiological Survey on Alcohol and Related Conditions (NESARC)*

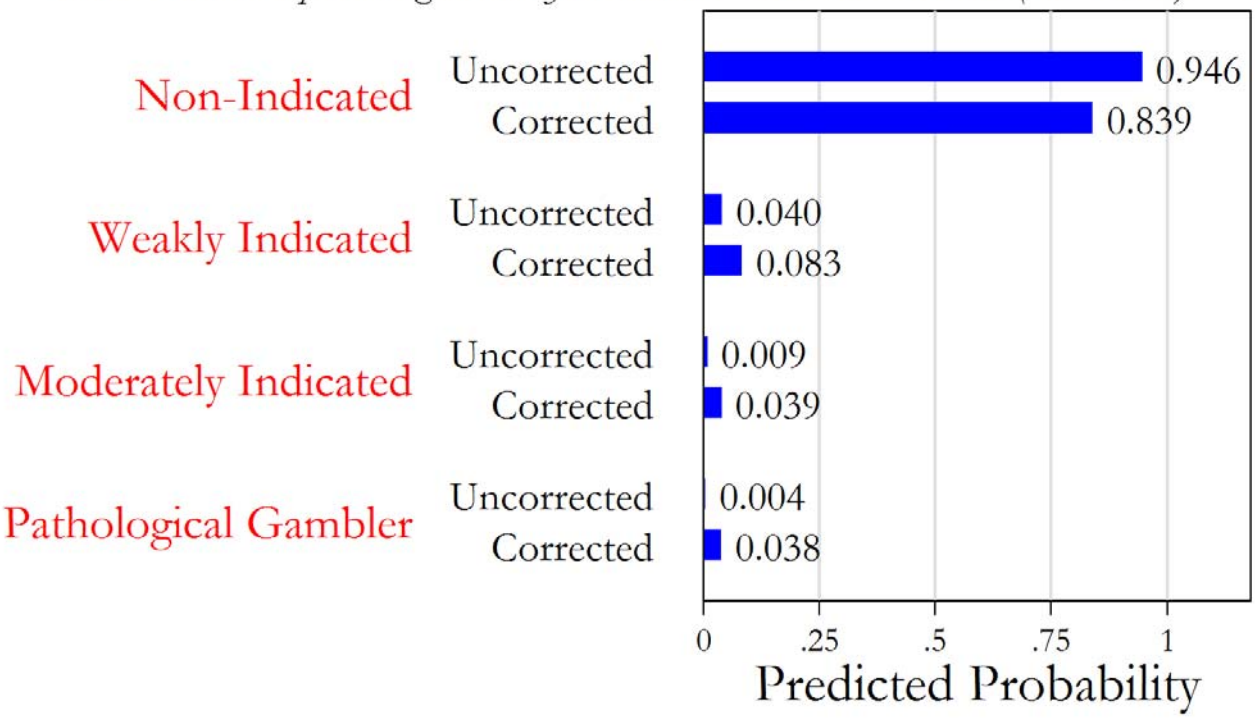


Figure 4: Statistical Significance of Sample Selection Corrections for Gambling Risk

100 predicted marginal probabilities from each model, for each individual, reflecting covariance of estimates

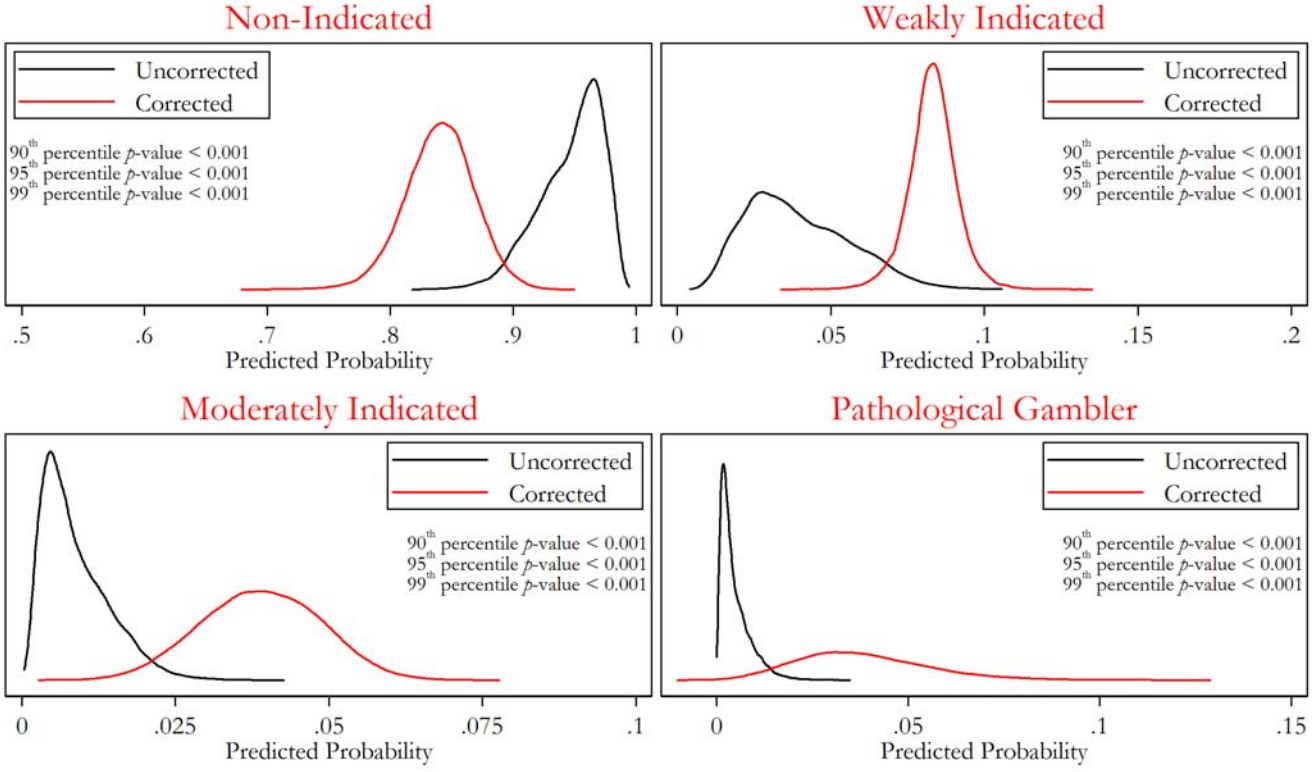


Figure 5: Predicted Probability of Gambling Risk Conditional on Sample Selection or Not

Predicted Probability of Sample Selection = 0.58
Estimated Probabilities using Semi-Nonparametric Ordered Response Model
Source: *National Epidemiological Survey on Alcohol and Related Conditions (NESARC)*

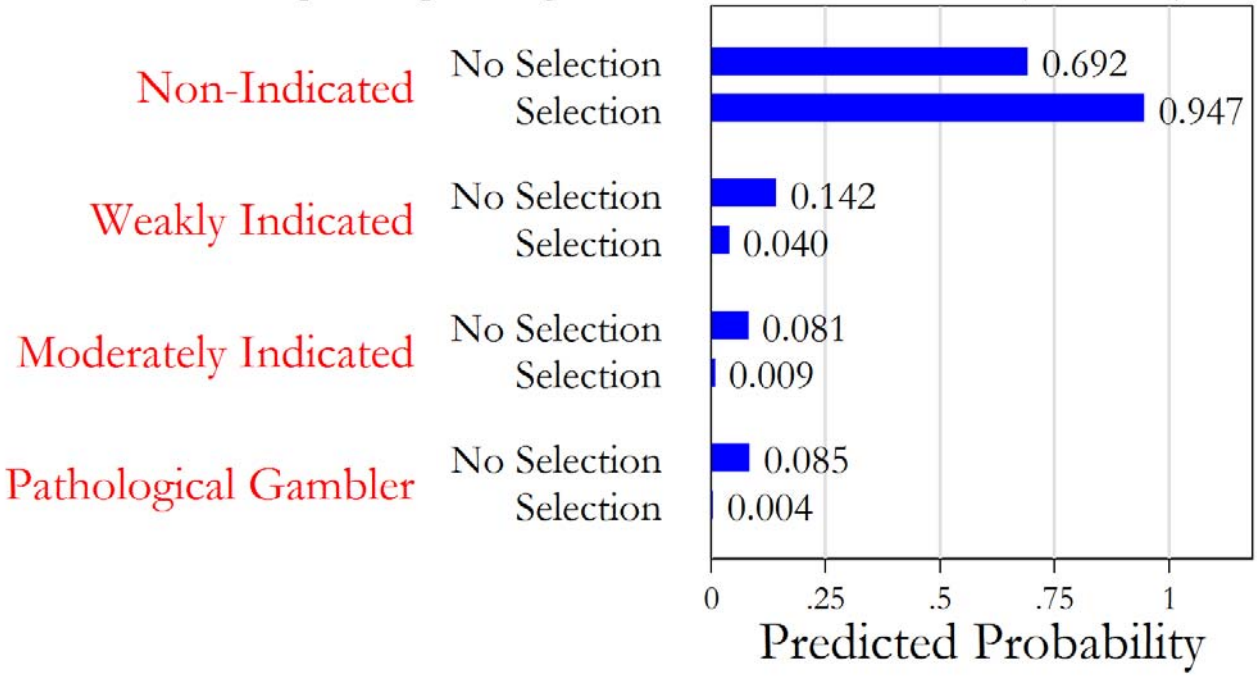


Figure 6: Predicted Prevalence of Gambling Disorders in the U.K., Measured with the DSM-IV Screen, With and Without Sample Selection Correction

Estimated Probabilities using Semi-Nonparametric Ordered Response Model
Source: *British Gambling Prevalence Survey* of 2010
Fraction at *any* risk level changes from 0.050 to 0.048 with correction

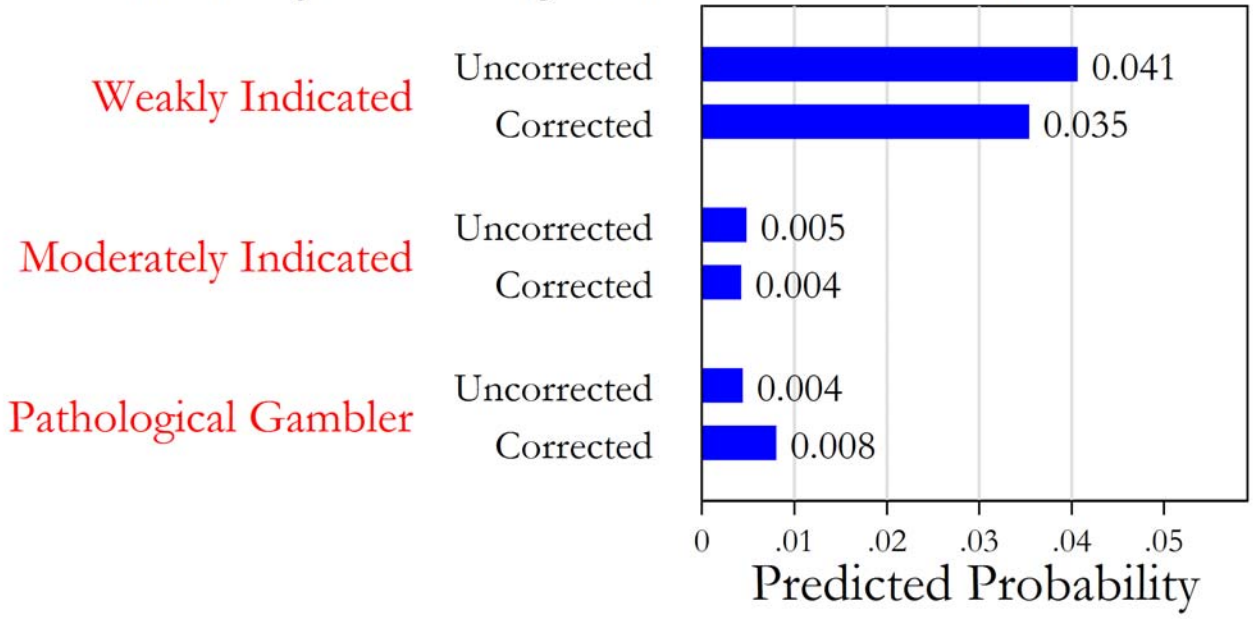


Figure 7: Predicted Prevalence of Gambling Disorders in Victoria (Australia), Measured with the PGSI Screen, With and Without Sample Selection Correction

Estimated Probabilities using Semi-Nonparametric Ordered Response Model
Source: Wave 1 of the *Victorian Gambling Survey* of 2008
Fraction at *any* risk level changes from 0.088 to 0.116 with correction

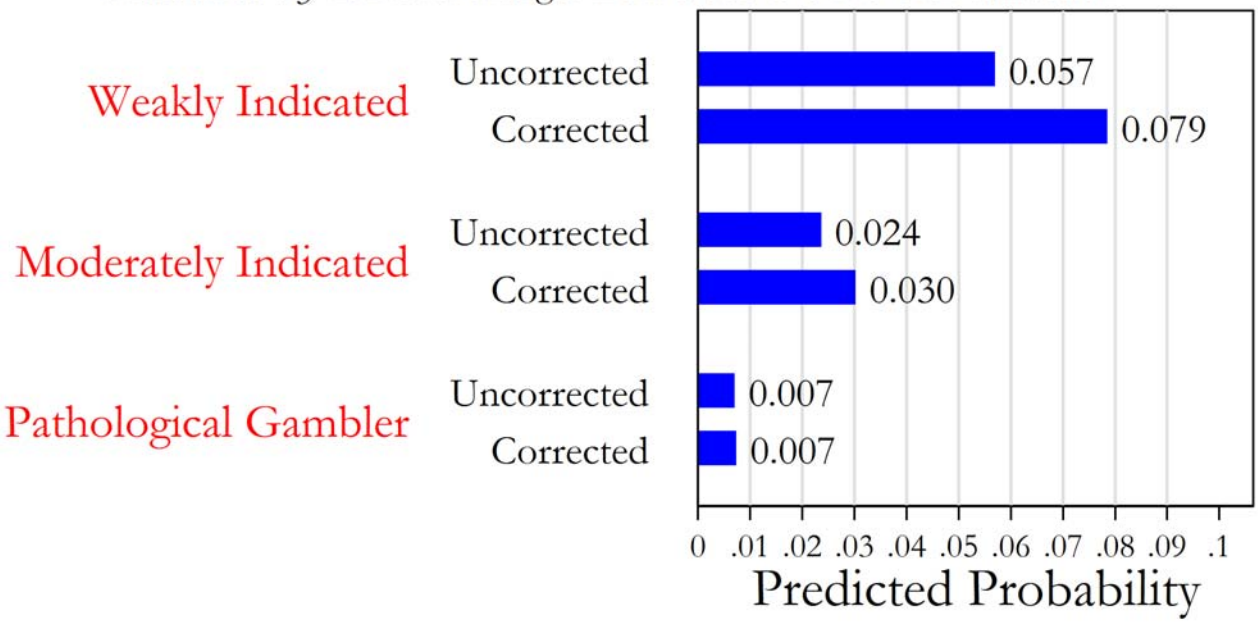
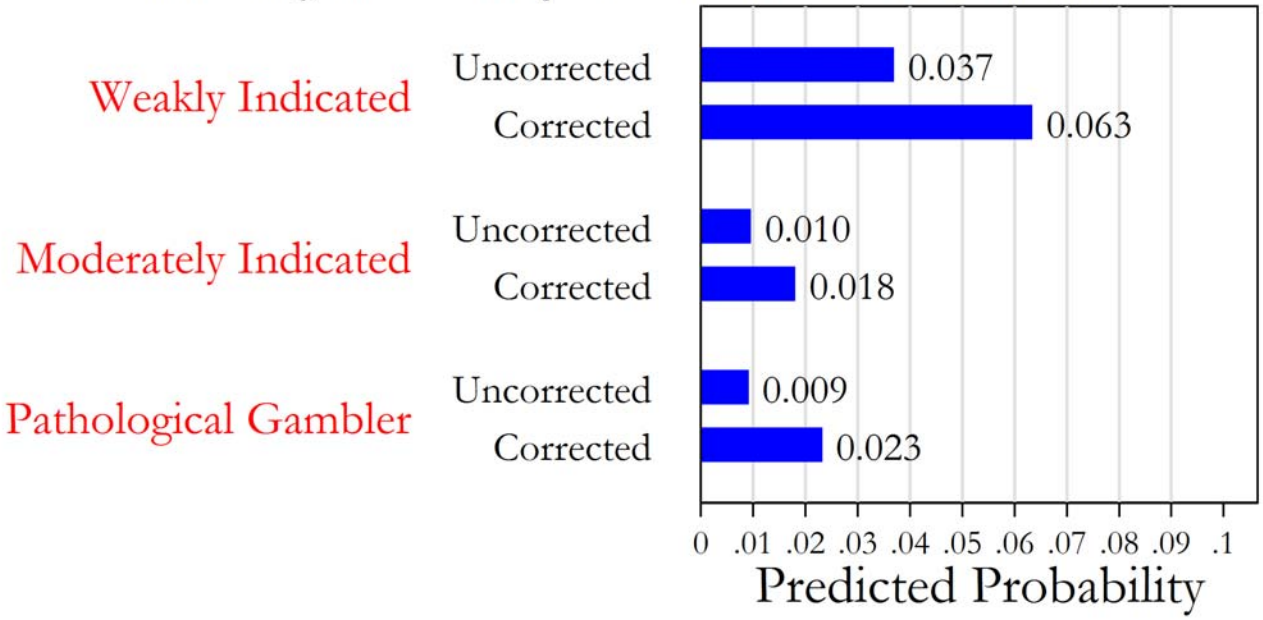


Figure 8: Predicted Prevalence of Gambling Disorders in Victoria (Australia), Measured with the NODS Screen, With and Without Sample Selection Correction

Estimated Probabilities using Semi-Nonparametric Ordered Response Model
Source: Wave 1 of the *Victorian Gambling Survey* of 2008
Fraction at *any* risk level changes from 0.056 to 0.105 with correction



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Appendix A: Classifying Pathological Gamblers in NESARC (NOT FOR PUBLICATION)

The NESARC provides information on responses to 15 questions about gambling, which map into the 10 diagnostic criteria of DSM-IV. There appears to be no public documentation about how that mapping occurs, although it does seem "obvious" on an *a priori* basis, as explained below. On the other hand, there is an apparent mis-match between the mapped classifications from the raw data on these 15 questions and the "summary classification" of pathological gambling provided by the NESARC data files. This is of some significance because it appears that every researcher that has used the NESARC data has used this summary classification, rather than re-generating it from the ground up.

That summary measure is provided in section 13 of the data file codebook:

3637-3637	GAMB12DX		PATHOLOGICAL GAMBLING IN LAST 12 MONTHS

		43014	0. No
		79	1. Yes
3638-3638	GAMBP12DX		PATHOLOGICAL GAMBLING PRIOR TO THE LAST 12 MONTHS

		42920	0. No
		173	1. Yes
3639-3639	GAMBEVDX		PATHOLOGICAL GAMBLING - LIFETIME

		42898	0. No
		195	1. Yes

We focus here on lifetime questions, although the same issues apply to questions about the last 12 months.

The 15 NESARC questions are displayed on the next two pages. Pay attention to the Check Item 12.1 at the bottom of section 2, containing these 15 questions:

CHECK ITEM 12.1	Are at least 5 Boxes marked in 2, column c, pages 123 - 124?	1 ☐ Yes 2 ☐ No - *SKIP to Check Item 12.4*

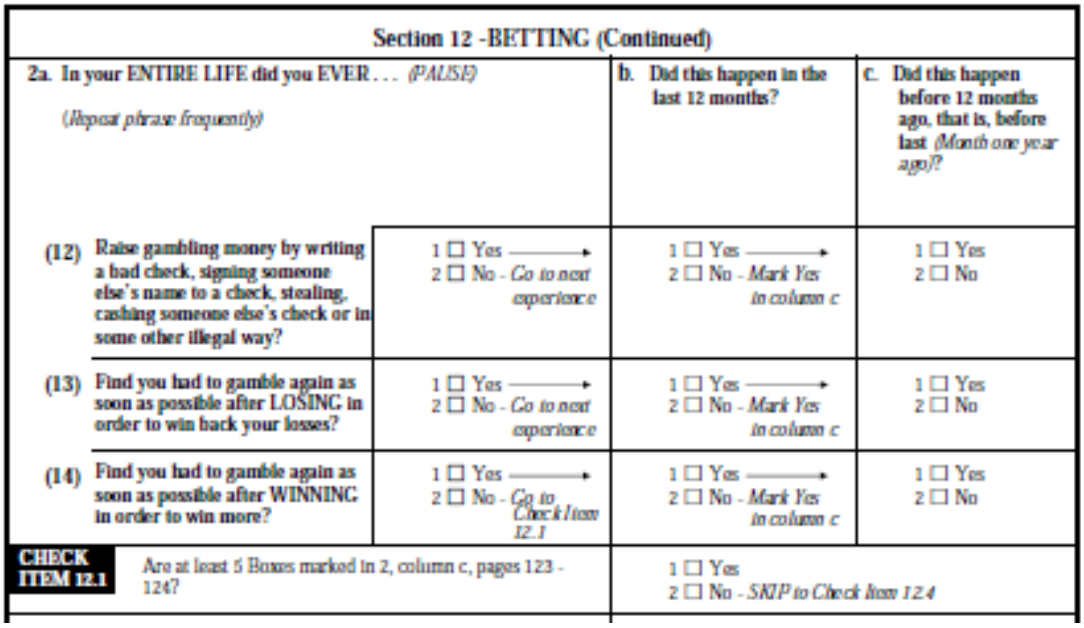
What this means is that someone is declared to be PG if they answer 5 of the 15 NESARC questions, not if they meet 10 of the DSM-IV criteria that these 15 question responses map into. This is in direct conflict with how the PG classification is defined in the papers reporting data from the NESARC. For example, Petry, Stinson and Grant [2005; p. 567] say that "Lifetime AUDADIS-IV diagnoses of pathological gambling required the respondent to meet at least 5 of the 10 DSM-IV criteria in the 12-month period preceding the interview and/or before that 12-month period. Fifteen symptom items operationalized the 10 pathological gambling criteria."

Section 12 – BETTING

(SHOW FLASHCARD 27)

Statement Q Now I'd like to ask you a few questions about gambling. By gambling I mean playing cards for money, betting on the horses or dogs or sports games, playing the stock or commodities market, buying lottery tickets or playing bingo or KENO or gambling at a casino, including playing the slot machines.

1. Have you ever gambled at least 5 times in any one year of your life?		1 ☐ Yes 2 ☐ No - *SKIP to Section 13, page 126*	
2a. The next few questions are about experiences that people have had with gambling. As I read each experience, please tell me if it has EVER happened to you. In your ENTIRE LIFE did you EVER . . . *(PAUSE)* *(Repeat phrase frequently)*		b. Did this happen in the last 12 months?	c. Did this happen before 12 months ago, that is, before last *(Month one year ago)*?
(1)	Gamble to get out of a bad mood – like feeling nervous, sad or down?	1 ☐ Yes → 2 ☐ No - *Go to next experience*	1 ☐ Yes → 2 ☐ No - *Mark Yes in column c*	1 ☐ Yes 2 ☐ No
(2)	Gamble to forget your problems?	1 ☐ Yes → 2 ☐ No - *Go to next experience*	1 ☐ Yes → 2 ☐ No - *Mark Yes in column c*	1 ☐ Yes 2 ☐ No
(3)	More than once try to quit or cut down on your gambling, but found you couldn't do it?	1 ☐ Yes → 2 ☐ No - *Go to next experience*	1 ☐ Yes → 2 ☐ No - *Mark Yes in column c*	1 ☐ Yes 2 ☐ No
(4)	Find that you had to increase the amount of money you would gamble to keep it exciting?	1 ☐ Yes → 2 ☐ No - *Go to next experience*	1 ☐ Yes → 2 ☐ No - *Mark Yes in column c*	1 ☐ Yes 2 ☐ No
(5)	Spend a lot of time gambling, planning your bets or studying the odds?	1 ☐ Yes → 2 ☐ No - *Go to next experience*	1 ☐ Yes → 2 ☐ No - *Mark Yes in column c*	1 ☐ Yes 2 ☐ No
(6)	Spend a lot of time thinking about ways to get money together so you could gamble?	1 ☐ Yes → 2 ☐ No - *Go to next experience*	1 ☐ Yes → 2 ☐ No - *Mark Yes in column c*	1 ☐ Yes 2 ☐ No
(7)	Spend a lot of time thinking about the times when you won or lost?	1 ☐ Yes → 2 ☐ No - *Go to next experience*	1 ☐ Yes → 2 ☐ No - *Mark Yes in column c*	1 ☐ Yes 2 ☐ No
(8)	Have job or school trouble because of your gambling – like missing too much work, being demoted at work, losing your job or dropping out of school?	1 ☐ Yes → 2 ☐ No - *Go to next experience*	1 ☐ Yes → 2 ☐ No - *Mark Yes in column c*	1 ☐ Yes 2 ☐ No
(8a)	Break up or come close to breaking up with anyone who was important to you because of your gambling?	1 ☐ Yes → 2 ☐ No - *Go to next experience*	1 ☐ Yes → 2 ☐ No - *Mark Yes in column c*	1 ☐ Yes 2 ☐ No
(9)	Try to keep your family or friends from knowing how much you gambled?	1 ☐ Yes → 2 ☐ No - *Go to next experience*	1 ☐ Yes → 2 ☐ No - *Mark Yes in column c*	1 ☐ Yes 2 ☐ No
(10)	Have such financial trouble as a result of your gambling that you had to get help with living expenses from family, friends or welfare?	1 ☐ Yes → 2 ☐ No - *Go to next experience*	1 ☐ Yes → 2 ☐ No - *Mark Yes in column c*	1 ☐ Yes 2 ☐ No
(11)	Find that you became restless, irritable or anxious when trying to quit or cut down on your gambling?	1 ☐ Yes → 2 ☐ No - *Go to next experience, page 124*	1 ☐ Yes → 2 ☐ No - *Mark Yes in column c*	1 ☐ Yes 2 ☐ No



Section 12 -BETTING (Continued)

2a. In your ENTIRE LIFE did you EVER . . . *(PAUSE)* *(Repeat phrase frequently)*		b. Did this happen in the last 12 months?	c. Did this happen before 12 months ago, that is, before last *(Month one year ago)*?
(12) Raise gambling money by writing a bad check, signing someone else's name to a check, stealing, cashing someone else's check or in some other illegal way?	1 ☐ Yes → 2 ☐ No - *Go to next experience*	1 ☐ Yes → 2 ☐ No - *Mark Yes in column c*	1 ☐ Yes 2 ☐ No
(13) Find you had to gamble again as soon as possible after LOSING in order to win back your losses?	1 ☐ Yes → 2 ☐ No - *Go to next experience*	1 ☐ Yes → 2 ☐ No - *Mark Yes in column c*	1 ☐ Yes 2 ☐ No
(14) Find you had to gamble again as soon as possible after WINNING in order to win more?	1 ☐ Yes → 2 ☐ No - *Go to Check Item 12.1*	1 ☐ Yes → 2 ☐ No - *Mark Yes in column c*	1 ☐ Yes 2 ☐ No
CHECK ITEM 12.1 Are at least 5 Boxes marked in 2, column c, pages 123 - 124?		1 ☐ Yes 2 ☐ No - *SKIP to Check Item 12.4*	

As it happens, it is relatively easy to see how the 15 questions map into the 10 DSM-IV criteria. From page 618 of the DSM-IV Manual, these 10 criteria are listed below, and Table A1 shows the 15 NESARC questions and the DSM criteria number they map into.

Diagnostic criteria for 312.31 Pathological Gambling

A. Persistent and recurrent maladaptive gambling behavior as indicated by five (or more) of the following:

(1) is preoccupied with gambling (e.g., preoccupied with reliving past gambling experiences, handicapping or planning the next venture, or thinking of ways to get money with which to gamble)
(2) needs to gamble with increasing amounts of money in order to achieve the desired excitement
(3) has repeated unsuccessful efforts to control, cut back, or stop gambling
(4) is restless or irritable when attempting to cut down or stop gambling
(5) gambles as a way of escaping from problems or of relieving a dysphoric mood (e.g., feelings of helplessness, guilt, anxiety, depression)
(6) after losing money gambling, often returns another day to get even ("chasing" one's losses)
(7) lies to family members, therapist, or others to conceal the extent of involvement with gambling
(8) has committed illegal acts such as forgery, fraud, theft, or embezzlement to finance gambling
(9) has jeopardized or lost a significant relationship, job, or educational or career opportunity because of gambling
(10) relies on others to provide money to relieve a desperate financial situation caused by gambling

B. The gambling behavior is not better accounted for by a Manic Episode.

Table A1: Mapping from 15 NESARC Gambling Criteria to 10 DSM-IV Criteria

NESARC Gambling Questions	DSM Criteria
(1) Gamble to get out of a bad mood – like feeling nervous, sad or down?	5
(2) Gamble to forget your problems?	5
(3) More than once try to quit or cut down on your gambling, but found you couldn't do it?	3
(4) Find that you had to increase the amount of money you would gamble to keep it exciting?	2
(5) Spend a lot of time gambling, planning your bets or studying the odds?	1
(6) Spend a lot of time thinking about ways to get money together so you could gamble?	1
(7) Spend a lot of time thinking about the times when you won or lost?	1
(8) Have job or school trouble because of your gambling – like missing too much work, being demoted at work, losing your job or dropping out of school?	9
(8a) Break up or come close to breaking up with anyone who was important to you because of your gambling?	9
(9) Try to keep your family or friends from knowing how much you gamble?	7
(10) Have such financial trouble as a result of your gambling that you had to get help with living expenses from family, friends or welfare?	10
(11) Find that you become restless, irritable or anxious when trying to quit or cut down in your gambling?	4
(12) Raise gambling money by writing a bad check, signing someone else's name to a check, stealing, cashing someone else's check or in some other illegal way?	8
(13) Find that you had to gamble again as soon as possible after LOSING in order to win back your losses?	6
(14) Find that you had to gamble again as soon as possible after WINNING in order to win more?	2

If one undertakes this mapping for each individual in the survey, there are actually 207 PG subjects in these data, not 195 as tabulated in section 14 of the data. Here is a tabulation:

Number of PG DSM-IV criteria met	pg_lifetime 0	Yes	Total
0	40,814	0	40,814
1	1,251	0	1,251
2	448	0	448
3	257	0	257
4	107	9	116
5	19	59	78
6	1	42	43
7	1	39	40
8	0	28	28
9	0	13	13
10	0	5	5
Total	42,898	195	43,093

We see that 9 individuals were incorrectly defined to be PG when they only met 4 of the DSM-IV criteria, but that 21 individuals were defined as *not* being PG when they did indeed meet 5 or more of the DSM-IV. We do not know that all of these 21 were *incorrectly* defined, since we need to decide whether or not to check the DSM-IV exclusionary criterion, that they did not suffer from Manic Episodes.

As it happens, the NESARC has information on that criteria, and 3 of these 21 did exhibit such Manic Episodes if we choose to implement this exclusionary criteria using these NESARC data. We do not rule out cases associated with recent illness or bereavement. If we drop anyone with such episodes, we see something more problematic:

```
. tab pg_dsm_count pg_lifetime if manic_episodes==0, missing
```

Number of PG DSM-IV criteria met	pg_lifetime 0	Yes	Total
0	39,515	0	39,515
1	1,159	0	1,159
2	401	0	401
3	218	0	218
4	92	5	97
5	16	45	61
6	1	33	34
7	1	29	30
8	0	19	19
9	0	9	9
10	0	2	2
Total	41,403	142	41,545

In other words, many of the 195 originally classified as PG should have been excluded. Specifically, 53 subjects:

```
. tab pg_lifetime manic_episodes, missing

pg_lifetim |    manic_episodes
         e |         0          1 |     Total
-----------+----------------------+----------
         0 |    41,403      1,495 |    42,898
       Yes |       142         53 |       195
-----------+----------------------+----------
     Total |    41,545      1,548 |    43,093
```

We do not endorse this way of implementing the DSM-IV exclusionary criteria, but simply note here the consequences for sample size.

The hypothesis about what was done to generate the summary measure of PG that appears to have been used by all researchers using NESERC is confirmed if one does a "count" of the number of NESARC questions answered affirmatively:

```
. tab pg_count pg_lifetime, missing

 Number of |
 PG NESARC |
  criteria |      pg_lifetime
       met |         0        Yes |     Total
-----------+----------------------+----------
         0 |    40,814          0 |    40,814
         1 |     1,066          0 |     1,066
         2 |       486          0 |       486
         3 |       257          0 |       257
         4 |       133          0 |       133
         5 |        77         12 |        89
         6 |        46         29 |        75
         7 |        13         35 |        48
         8 |         5         30 |        35
         9 |         1         27 |        28
        10 |         0         18 |        18
        11 |         0         21 |        21
        12 |         0         12 |        12
        13 |         0          5 |         5
        14 |         0          3 |         3
        15 |         0          3 |         3
-----------+----------------------+----------
     Total |    42,898        195 |    43,093
```

So we see that an individual was only classified as being PG in the summary measure if they had 5 of the 15 NESARC criteria. The fact that researchers used this summary measure is apparent from inspection of the reported sample size of 195 PG (e.g., Petry et al. [2005; Table 1, p. 568] or Blanco et al. [2006; Table 1, p.947]).

On the other hand, the above tabulation poses a puzzle, since there are many individuals with 5 or more of the 15 NESARC criteria that are *not* classified as PG. In fact, there are 77+46+13+5+1 = 142 such individuals. It is not as if these 142 individuals met some of the

NESARC criteria that do not map into DSM criteria, since the NESARC criteria that they meet span all 15 NESARC questions. There appears to have been some algorithm used to defined PG in the summary measure that is not documented. For this reason we prefer to re-generate our own measure of PG using the DSM-IV criteria.

There are 207 correct DSM-IV based PG individuals in the NESARC:

```
. tab pg_dsm_correct pg_lifetime, missing

   Correct |
    DSM-IV |
  criteria |
applied to |
    NESARC |
  data for |      pg_lifetime
  lifetime |         0        Yes |     Total
-----------+----------------------+----------
         0 |    42,877          9 |    42,886
         1 |        21        186 |       207
-----------+----------------------+----------
     Total |    42,898        195 |    43,093
```

There have been some variants of the DSM-IV threshold criteria applied in some research. For instance, as noted in the text, Fisher [2000] prefers the term "problem" to "pathological" outside of a shared, clinical diagnosis, and then (p. 33) defines problem gamblers using either 5 or more "yes" responses to the DSM-IV criteria *or* 3 or 4 "yes" responses that include at least one from items 8, 9 or 10 of the criteria. These last three refer to the adverse consequences of gambling. This definition can only logically increase the number of "classified" gamblers compared to the use of 5 or more "yes" responses. In fact, for the NESARC data this results in 256 "classified" gamblers compared to the 207 correct DSM-IV PG individuals:

```
. tab pg_dsm_correct_extra, missing

pg_dsm_corr |
  ect_extra |      Freq.     Percent        Cum.
------------+-----------------------------------
          0 |     42,837       99.41       99.41
          1 |        256        0.59      100.00
------------+-----------------------------------
      Total |     43,093      100.00

. tab pg_dsm_correct_extra pg_dsm_correct, missing

           |    Correct DSM-IV
           |  criteria applied to
           |    NESARC data for
pg_dsm_cor |       lifetime
rect_extra |         0          1 |     Total
-----------+----------------------+----------
         0 |    42,837          0 |    42,837
         1 |        49        207 |       256
-----------+----------------------+----------
     Total |    42,886        207 |    43,093
```

It is also worthwhile doing the correct analysis using the DSM-V criteria. The new criteria drop the "illegal acts" question, and require that only 4 criteria be met. In this respect we mimic the approach of Petryy, Blanco, Jon and Grant [2014] to "re-cycling" the NESARC data in this manner:

```
. tab pg_dsm5_correct pg_lifetime, missing

   Correct |
     DSM-V |
  criteria |
applied to |
    NESARC |      pg_lifetime
      data |         0        Yes |     Total
-----------+----------------------+----------
         0 |    42,772          0 |    42,772
         1 |       126        195 |       321
-----------+----------------------+----------
     Total |    42,898        195 |    43,093
```

So we now have 321 individuals with PG, using the DSM-V criteria.

We contacted researchers that have used NESARC to find out if there is some simple explanation for our finding of an error in the coding of PG. We keep their identity anonymous, for reasons that will become apparent.

One responded:

> Yes, NESARC is not easy to navigate. There are some sort of weights you need to add to the calculations, to weight by baseline characteristics because some subgroups were oversampled for the survey. I have not done this in many years, and no longer have the syntax. I believe they are described in many of the NESARC papers, as everybody appears to use these weights. My guess is that is what is incorrect, rather than how you're coding the DSM criteria, which is more straightforward. In any case, I'm certain the numbers reported in the 2005 paper are correct, because many subsequent papers have been written about the PGs in that dataset, and no one's reported difficulty replicating the n's or analyses. I am not a statistician, but there are a lot of statisticians who are experienced at NESARC analyses and could probably help.

In fact, we find NESARC relatively straightforward to navigate, and it is well documented apart from this apparent coding error. The use of survey weights, which we are aware of and use for appropriate tabulations (e.g., prevalence and odd ratio estimation), is not the issue since this refers to the raw sample count reported in published papers. The final point, that we should trust that everyone else that has used the data has checked this, is not germane to a scholarly evaluation of these data. There is a simple error or there is not.

A second researcher, a statistician and experienced at NESARC analysis, then responded:

> I have read your attachment and have 2 initial comments that might help resolve this issue. First the skip item is only used during the interview to decide whether to ask additional pg questions and does not come into play with regard to creating diagnoses. Second we never use the ever columns to determine diagnoses. We use the 12 month and prior to the 12 month columns only and if positive for any of those 2 time frames we declare a lifetime diagnosis. If this does not resolve your issues please let me know so I can examine them more closely.

The first point, about the skip question, is irrelevant to the issue. The second point is worth checking, and would arise from some inconsistency between reports made by the same individual about the same NESARC criteria. In fact, however, if one reviews the earlier extract from the questions involved, it is apparent that consistency here is *enforced* if the interviewer followed the logic of the skip pattern correctly.[39] There are 4 such inconsistencies. In 3 of these 4 there was a positive responses to the direct lifetime question, but both the "last 12 months" and "before the last 12 months" are left blank; perhaps the respondent said that they could not be bothered with the follow-up. In 1 of these 4 the "last 12 months" was left blank but the "before 12 months" was marked NO. This cannot be the cause of the discrepancy we are pointing out.

[39] NESARC criteria #1 is asked on a lifetime basis. If the response is NO then the 12-month and before 12-month responses are left missing, which consistently implies a NO for lifetime. If the response to the direct lifetime question is YES then the "last 12 months" question is asked. If there is a YES to the "last 12 months" question then the "before 12 months" question is asked, and whatever the response to that question we have consistency. If there is a NO to the "last 12 months" question then the "before 12 months" question is *automatically* filled in as a YES to *ensure* consistency.

Appendix B: Additional Documentation of Results (NOT FOR PUBLICATION)

All variables are defined from the original data files of wave 1 of the NESARC, and *Stata* code explaining the detailed definitions is available on request. The variables are as follows:

variable name	variable label
pg_dsm_correct	Correct DSM-IV criteria applied to NESARC data for lifetime
alcohol	Any lifetime alcohol disorder
drug	Any lifetime drug use disorder
nicotine	Any lifetime nicotine disorder
mood	Any mood disorder
anxiety	Any anxiety disorder
personality	Any personality disorder
female	Female
black	Black or Afro-American
hispanic	Hispanic
age30_44	Aged between 30 and 44
age45_64	Aged between 45 and 64
age65plus	Aged 65 and over
separated	Separated by being widowed, divorced or separated
midwest	Midwest region
south	South region
west	West region
high_school	Completed High School or GED
some_college	Completed some college
college	Completed a college degree
graduate	Completed a graduate degree
income2	Personal income between $20,000 and $35,000
income3	Personal income between $35,000 and $70,000
income4	Personal income of $70,000 or more
height	Height in feet
weigh	Weight in stones
friday	Interview conducted on a Friday
saturday	Interview conducted on a Saturday
sunday	Interview conducted on a Sunday
pg_Ntriggers	Number of triggers activated when pg trigger question asked
personality_battery1	Responses to questions on usual feelings and actions
personality_battery2	Responses to questions on effects of feelings and actions
ss_1	Sample Selection variable 1 from Screening Question #18 Part 1
ss_2	Sample Selection variable 2 from Screening Question #18 Part 2
ss_3	Sample Selection variable 3 from Screening Question #19 Part 1
ss_4	Sample Selection variable 4 from Screening Question #19 Part 2
ss_5	Sample Selection variable 5 from Screening Question #20
ss_6	Sample Selection variable 6 from Screening Question #21 Part 1
ss_7	Sample Selection variable 7 from Screening Question #21 Part 2
ss_8	Sample Selection variable 8 from Screening Question #21 Part 3
ss_9	Sample Selection variable 9 from Screening Question #22
ss_10	Sample Selection variable 10 from Screening Question #23 Part 1
ss_11	Sample Selection variable 11 from Screening Question #23 Part 2
ss_12	Sample Selection variable 12 from Screening Question #23 Part 3

```
ss_13           Sample Selection variable 13 from Screening Question #23 Part 4
ss_14           Sample Selection variable 14 from Screening Question #23 Part 5
ss_15           Sample Selection variable 15 from Screening Question #23 Part 6
ss_16           Sample Selection variable 16 from Screening Question #23 Part 7
ss_17           Sample Selection variable 17 from Screening Question #23 Part 8
ss_18           Sample Selection variable 18 from Screening Question #23 Part 9
ss_19           Sample Selection variable 19 from Screening Question #23 Part 10
ss_20           Sample Selection variable 20 from Screening Question #23 Part 11
ss_21           Sample Selection variable 21 from Screening Question #23 Part 12

-------------------------------------------------------------------------------
```

Summary statistics are as follows:

Variable	Obs	Mean	Std. Dev.	Min	Max
pg_dsm_cor~t	43,093	.0046875	.0683057	0	1
alcohol	43,093	.2748242	.4464308	0	1
drug	43,093	.0944005	.2923885	0	1
nicotine	43,093	.1609774	.3675144	0	1
mood	43,093	.2111944	.4081607	0	1
anxiety	43,093	.1732764	.3784904	0	1
personality	43,093	.1460794	.3531899	0	1
female	43,093	.5702782	.4950421	0	1
black	43,093	.1995684	.3996806	0	1
hispanic	43,093	.1927923	.394496	0	1
age30_44	43,093	.3105377	.4627191	0	1
age45_64	43,093	.2979602	.4573672	0	1
age65plus	43,093	.1904022	.3926229	0	1
separated	43,093	.2579769	.4375263	0	1
midwest	43,093	.2086418	.4063425	0	1
south	43,093	.3749101	.4841053	0	1
west	43,093	.2259532	.4182133	0	1
high_school	43,093	.291161	.4543028	0	1
some_college	43,093	.2063212	.4046685	0	1
college	43,093	.2093844	.4068739	0	1
graduate	43,093	.1109925	.3141265	0	1
income2	43,093	.2314761	.4217808	0	1
income3	43,093	.2095236	.4069733	0	1
income4	43,093	.2906969	.4540892	0	1
height	43,093	5.549293	.3356052	4	7
weigh	43,093	12.17406	2.914927	4.428571	35.71429
friday	43,093	.1413919	.3484294	0	1
saturday	43,093	.1438981	.3509904	0	1
sunday	43,093	.0768106	.2662938	0	1
pg_Ntriggers	43,093	.1886385	.7641052	0	7
personalit~1	43,093	3.466642	4.772873	0	55
personalit~2	43,093	3.443552	4.769596	0	55
ss_1	43,093	.348154	.4763907	0	1
ss_2	43,093	.3008841	.4586477	0	1
ss_3	43,093	.2764718	.4472581	0	1
ss_4	43,093	.2422435	.4284459	0	1
ss_5	43,093	.3640963	.4811814	0	1

```
        ss_6 |     43,093      .76256     .425519          0          1
        ss_7 |     43,093    .7989697    .4007753          0          1
        ss_8 |     43,093    .4865291    .4998243          0          1
        ss_9 |     43,093    .2665166    .4421426          0          1
       ss_10 |     43,093    .3197503    .4663851          0          1
       ss_11 |     43,093    .3476435    .4762276          0          1
       ss_12 |     43,093    .1458938    .3530038          0          1
       ss_13 |     43,093    .0624463    .2419673          0          1
       ss_14 |     43,093    .0896897    .2857401          0          1
       ss_15 |     43,093     .079224    .2700912          0          1
       ss_16 |     43,093    .2083633    .4061427          0          1
       ss_17 |     43,093    .0647437    .2460759          0          1
       ss_18 |     43,093    .0561576    .2302285          0          1
       ss_19 |     43,093    .1168867     .321289          0          1
       ss_20 |     43,093    .0552526    .2284753          0          1
       ss_21 |     43,093    .0641636    .2450469          0          1
-------------+---------------------------------------------------------
```

The sample selection screening questions come from the "Background Information" section of the NESARC questionnaire, reproduced below.

(SHOW FLASHCARD 11C)

18. **During the past 4 weeks, how much of the time have you had any of the following problems with your work or other regular daily activities as the result of your physical health?**

(1)	**Accomplished less than you would like.**	1 ☐ All of the time 2 ☐ Most of the time 3 ☐ Some of the time 4 ☐ A little of the time 5 ☐ None of the time
(2)	**Were limited in the kind of work or other activities.**	1 ☐ All of the time 2 ☐ Most of the time 3 ☐ Some of the time 4 ☐ A little of the time 5 ☐ None of the time

(SHOW FLASHCARD 11C)

19. **During the past 4 weeks, how much of the time have you had any of the following problems with your work or other regular daily activities as the result of any emotional problems such as feeling depressed or anxious?**

(1)	**Accomplished less than you would like.**	1 ☐ All of the time 2 ☐ Most of the time 3 ☐ Some of the time 4 ☐ A little of the time 5 ☐ None of the time
(2)	**Didn't do work or other activities as carefully as usual.**	1 ☐ All of the time 2 ☐ Most of the time 3 ☐ Some of the time 4 ☐ A little of the time 5 ☐ None of the time

(SHOW FLASHCARD 11D)

20. **During the past 4 weeks, how much did pain interfere with your normal work including both work outside the home and housework?**

1 ☐ Not at all
2 ☐ A little bit
3 ☐ Moderately
4 ☐ Quite a bit
5 ☐ Extremely

(SHOW FLASHCARD 11C)

21. **The next few questions are about how you feel and how things have been with you during the past 4 weeks. For each question, please give the one answer that comes closest to the way you have been feeling. How much of the time during the past 4 weeks...**

(1) **Have you felt calm and peaceful?**

1 ☐ All of the time
2 ☐ Most of the time
3 ☐ Some of the time
4 ☐ A little of the time
5 ☐ None of the time

(2) **Did you have a lot of energy?**

1 ☐ All of the time
2 ☐ Most of the time
3 ☐ Some of the time
4 ☐ A little of the time
5 ☐ None of the time

(3) **Have you felt downhearted and depressed?**

1 ☐ All of the time
2 ☐ Most of the time
3 ☐ Some of the time
4 ☐ A little of the time
5 ☐ None of the time

(SHOW FLASHCARD 11C)

22. **During the past 4 weeks, how much of the time has your physical health or emotional problems interfered with your social activities like visiting with friends, relatives, and so forth?**

1 ☐ All of the time
2 ☐ Most of the time
3 ☐ Some of the time
4 ☐ A little of the time
5 ☐ None of the time

23.	**Can you please tell me if you have had any of the following experiences in the last 12 months?** **In the last 12 months. . .** *(Repeat phrase frequently)*	
(1)	**Did any of your family members or close friends die?**	1 ☐ Yes 2 ☐ No
(2)	**Did any of your family members or close friends have a serious illness or injury?**	1 ☐ Yes 2 ☐ No
(3)	**Did you move or have anyone new come to live with you?**	1 ☐ Yes 2 ☐ No
(4)	**Were you fired or laid off from a job?**	1 ☐ Yes 2 ☐ No
(5)	**Were you unemployed and looking for a job for more than a month?**	1 ☐ Yes 2 ☐ No
(6)	**Have you had trouble with your boss or a coworker?**	1 ☐ Yes 2 ☐ No
(7)	**Did you change jobs, job responsibilities or work hours?**	1 ☐ Yes 2 ☐ No
(8)	**Did you get separated or divorced or break off a steady relationship?**	1 ☐ Yes 2 ☐ No
(9)	**Have you had serious problems with a neighbor, friend or relative?**	1 ☐ Yes 2 ☐ No
(10)	**Have you experienced a major financial crisis, declared bankruptcy or more than once been unable to pay your bills on time?**	1 ☐ Yes 2 ☐ No
(11)	**Did you or a family member have trouble with the police, get arrested or get sent to jail?**	1 ☐ Yes 2 ☐ No
(12)	**Were you or a family member the victim of any type of crime?**	1 ☐ Yes 2 ☐ No

Figures B1, B2 and B3 show distributions of the predicted marginal probabilities of each gambling disorder category for the DSM-IV screen from the *British Gambling Prevalence Survey* of 2010, the PGSI screen of the *Victorian Gambling Survey* of 2008, and the NODS screen of the Victorian Gambling Survey of 2008. In some cases the horizontal axes include labels for probabilities below 0 or above 1: this is an artefact of the kernel density procedure used to display distributions as well as the use of the "delta method" to *approximate* non-linear combinations of estimated parameters. These are of no substantive or statistical significance for our purposes.

Figure B1: Statistical Significance of Sample Selection Corrections for Gambling Risk in the U.K., Measured with the DSM-IV Screen

100 predicted marginal probabilities from each model, for each individual, reflecting covariance of estimates
Source: *British Gambling Prevalence Survey* of 2010 and author estimates

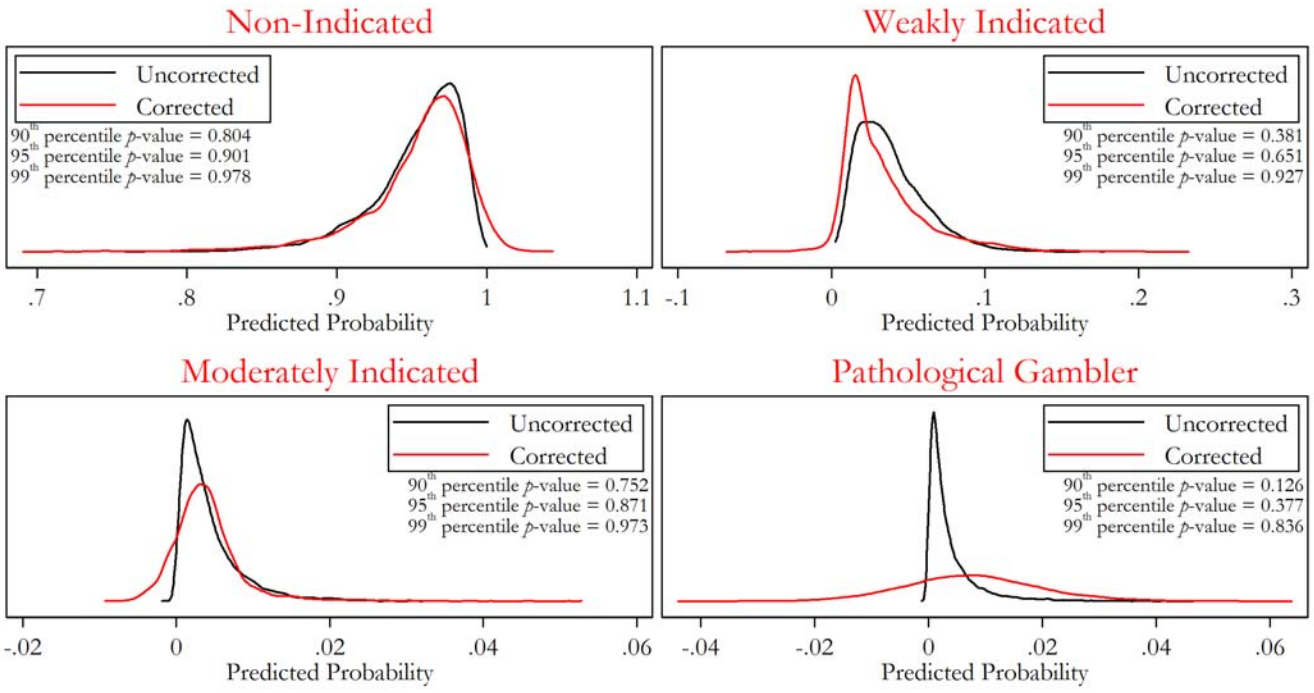


Figure B2: Statistical Significance of Sample Selection Corrections for Gambling Risk in Victoria (Australia), Measured with the PGSI Screen

100 predicted marginal probabilities from each model, for each individual, reflecting covariance of estimates
Source: Wave 1 of the *Victorian Gambling Survey* of 2008 and author estimates

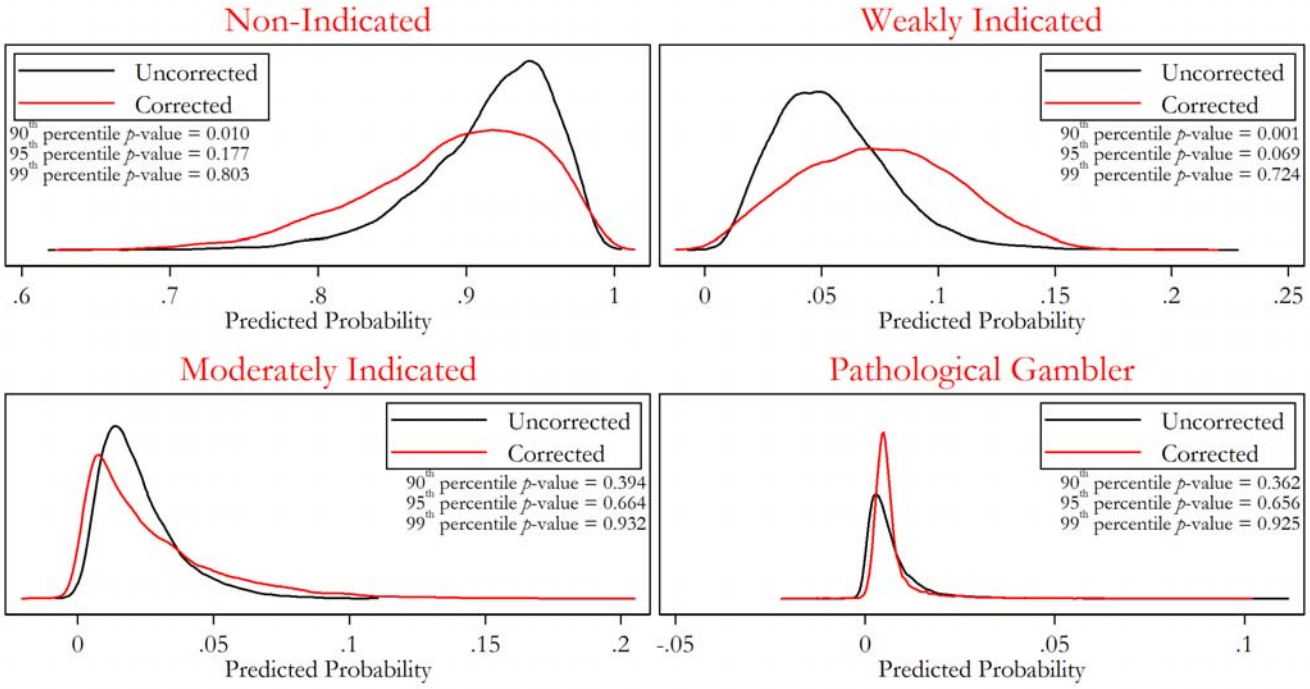
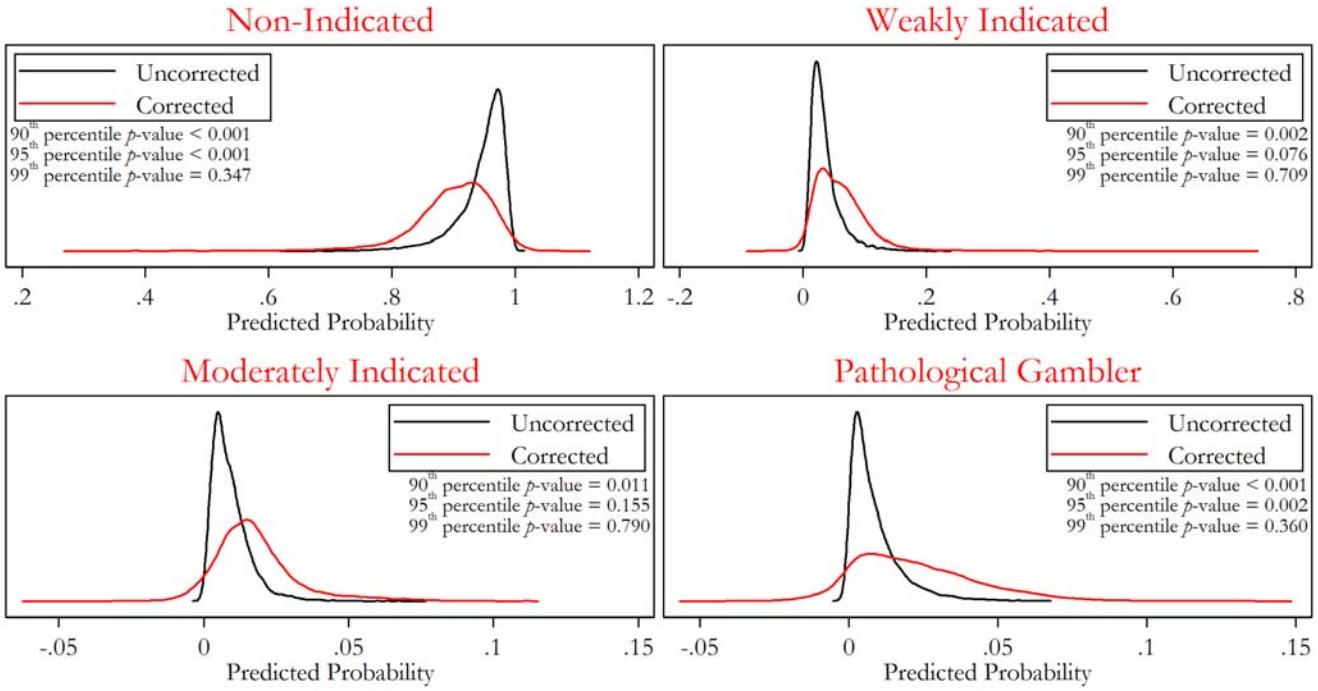


Figure B3: Statistical Significance of Sample Selection Corrections for Gambling Risk in Victoria (Australia), Measured with the NODS Screen

100 predicted marginal probabilities from each model, for each individual, reflecting covariance of estimates
Source: Wave 1 of the *Victorian Gambling Survey* of 2008 and author estimates



Appendix C: Formal Statistical Model (NOT FOR PUBLICATION)

The statistical model is an extension of the standard Heckman model in two ways:

- The main equation is an ordered probit instead of OLS specification
- The bivariate distribution between the selection equation and the main equation is specified in a semi-nonparament (SNP) manner.

Following De Luca and Perotti [2011; p. 215], the ordered probit sample selection model can be defined in three equations:

$$Y_j^* = \beta_j^\top X_j + U_j \qquad j = 1, 2 \qquad (1)$$

$$Y_1 = \mathbf{I}(Y_1^* \geq 0) \qquad (2)$$

$$Y_2 = \sum_{h=0,\,H} h\, \mathbf{I}(\alpha_h < Y_2^* \leq \alpha_{h+1}) \qquad \text{if } Y_1 = 1 \qquad (3)$$

where Y_1^* is a continuous latent variable for the sample selection equation, Y_2^* is a continuous latent variable for the risk of GD, β_j denotes k_j vectors of parameters to be estimated, X_j denotes k_j vectors of exogenous variables, the U_j are random errors, $\mathbf{I}(\cdot)$ is the indicator function, Y_1 is the binary variable indicating the observed sample when $Y_1 = 1$, Y_2 is the observed level of GD, H+1 denotes the ordered categories of GD, and $(\alpha_0, ..., \alpha_h, \alpha_{h+1}, ..., \alpha_H)$ are thresholds to be estimated, with $\alpha_0 = -\infty$, $\alpha_h < \alpha_{h+1}$ and $\alpha_H = \infty$.

Equation (2) defines the sample selection process by which we observe the sample for which $Y_1 = 1$, and by itself is just a probit equation. Equation (3) defines the ordered probit, conditional on sample selection, which means conditional on responding affirmatively to the trigger question for GD. The H cutpoints $(\alpha_0, ..., \alpha_h, \alpha_{h+1}, ..., \alpha_H)$, to be estimated, define H+1 intervals over the latent variable Y_2^*. The correlation of the latent regressions errors U_1 and U_2 determines selectivity effects. If this correlation is positive (negative) then it means that unobservables have the same (opposite) effect on selection and the risk of GD.[40]

[40] The traditional parametric specific of the model assumes that the errors U_1 and U_2 follow a bivariate Normal distribution with zero means, unit variances, and a correlation coefficient ϱ. The SNP innovation is to approximate the marginal distribution functions of U_1 and U_2, and their joint distribution function. The approximation starts with an approximation of the joint density by the product of a standardized normal density for U_1; a standardized normal density for U_2; a polynomial of order R in U_1 and U_2, with R×R polynomial coefficients to be estimated; and a normalization factor. Once this joint density is approximated, one can use it to approximate the marginal distribution functions of U_1 and U_2 (De Luca [2008]). The fact that the standardized normal densities are used for the first two terms of this approximation means that a special case of the SNP specification is the parametric specification, allowing a direct test of the hypothesis that the SNP estimates are the same as the parametric estimates.

There are four identifiability restrictions on this model. The first is familiar from the parametric ordered probit and ordered logit models, that the intercept in β_2 be set to zero. The second is that the exogenous variables X_1 contain at least one variable not contained in X_2. Although this "exclusion restriction" is not formally needed in the parametric model, identification is then only likely to be weak (Meng and Schmidt [1985] and Keane [1992]). However, this restriction is formally needed in the semi-parametric case (Lee [1995]). In our case we have a long list of variables that meet this exclusion restriction. Third, X_1 and X_2 must each contain one continuous variable, or have sufficient coverage from a rich list of discrete variables (Manski [1988]). In our case we have variables denoting age (and the square of age) and the weight of the individual, as well as rich array of discrete variables. Fourth, since the means of U_1 and U_2 are not constrained to be zero with the SNP estimator, the intercept of β_1 and the first threshold α_1 are set equal to the parametric estimates.

The estimation of this SNP ordered probit model is based on software developed by De Luca and Perotti [2011], which in turn is based on components developed by Stewart [2005] and De Luca [2008]. The software runs on *Stata* version 15, although all estimations run on *Stata* version 14.

Additional References

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