2002 National Survey on Drug Use and Health

Statistical Inference Report

Contract No. 283-98-9008
RTI Project No. 7190
Deliverable No. 28

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Substance Abuse and Mental Health Services Administration
Rockville, Maryland 20857

Prepared by:
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1. Introduction

Statistical inference occurs whenever data obtained from sample observations belonging to and considered representative of a larger target population are used to make generalizations concerning the larger population. The target population for the 2002 National Survey on Drug Use and Health (NSDUH)[1] was the U.S. civilian, noninstitutionalized population, aged 12 or older in the year 2002. Measurements for this target population were the responses to the survey questions provided by persons selected to participate in the 2002 survey.

Statistical inferences concerning characteristics of interest for this population were made from estimates obtained from these measurements. Examples of the inferences made for the 2002 NSDUH include estimates of the number of persons who were substance users during the past month, past year, and lifetime, and the associated percentages (prevalence rates) of substance use for these reference periods. Inferences were also made for such categories as substance initiation, risk and protective factors, substance dependence and abuse, serious mental illness, and treatment for substance abuse and mental health.

This report is organized as follows: Section 2 provides background information concerning the 2002 NSDUH; Sections 3 and 4 discuss the prevalence rates and sampling errors and how they were calculated; Section 5 describes the degrees of freedom that were used when comparing estimates and Section 6 discusses how statistical significance of differences between estimates was determined. Section 7 discusses confidence interval estimation. Section 8 describes how the rates for initiation or incidence of drug use were computed and Section 9 describes the computation of retrospective lifetime prevalence estimates of drug use. Finally, Section 10 discusses the conditions under which estimates with low precision were suppressed.

[1] Prior to 2002, the survey was called the National Household Survey on Drug Abuse (NHSDA).

2. Background

Starting in 1999 and continuing through 2002, the NSDUH was implemented as part of a 5-year 50-State sample design to provide national and State estimates of drug use through 2003. A major change to the study protocol was the introduction of computer-assisted interviewing (CAI) methods for both the screening and interviewing of selected respondents.

For the 5-year 50-State design, 8 States were designated as large sample States (California, Florida, Illinois, Michigan, New York, Ohio, Pennsylvania, and Texas) and provided with samples large enough to support direct State estimates. For 2002, sample sizes in these States ranged from 3,554 to 3,792. For the remaining 42 States and the District of Columbia, smaller, but adequate, samples were selected to support State estimates using small area estimation (SAE) techniques. Sample sizes in these States ranged from 674[2] to 977 in 2002.

Using the 50-State design, States were first stratified into a total of 900 field interviewer (FI) regions (48 regions in each large sample State and 12 regions in each small sample State). Within FI regions, adjacent Census blocks were combined to form the first-stage sampling units called "segments." Eight sample segments per FI region were fielded during the 2002 survey year. These sampled segments were allocated equally into four separate samples, one for each 3-month period during the year, so that the survey remained in the field year-round.

During the 2001 survey, an experimental study was conducted to evaluate the effectiveness of respondent incentives on improving response rates and also to examine the results of incentives on data quality, survey costs, and substance use estimates. The study compared the effectiveness of $0, $20, and $40 incentive payments. The results of the experiment showed that the $20 and $40 treatments produced significantly better interview response rates than the control group (Eyerman & Bowman, 2002). Based on the results of this experiment, it was decided to institute a $30 incentive payment beginning with the 2002 NSDUH. As expected, there were significant improvements in response rates in the 2002 survey. Due to these higher response rates, fewer selected households were required in 2002 as compared to previous surveys.

An additional change was implemented in the 2002 survey. Due to the concerns about the sample size of pair-level data, the number of pairs selected in 2002 was increased. The new pair sampling strategy increased the number of pairs selected in dwelling units with older persons on their roster. For more detailed information on the sample design, see the 2002 NSDUH sample design report (Bowman, Chromy, Martin, & Odom, 2004).

In addition to the use of incentives, the change in name may also have contributed to better cooperation. As a result of a program of interview observation implemented in 2001, a number of training reinforcement procedures were implemented to produce more uniform

[2] The small sample size was for New Mexico due to the decision to drop certain cases conducted by three interviewers in this State whose work was determined through verification to be falsified. Smaller numbers of cases in Nevada and Mississippi were also deleted due to the discovery of interviewer falsification. The next two smallest sample sizes were achieved in Mississippi and New Jersey, with 839 and 854 completed cases, respectively.

compliance with the intended study protocols. Because of the many improvements to the survey in 2002, estimates from the 2002 NSDUH should not be compared with estimates from 2001 and earlier NHSDAs to assess change over time in substance use. Therefore, the 2002 data constitute a new baseline for tracking trends in substance use and other measures.

The final respondent sample of 68,126 persons for the 2002 NSDUH was representative of the U.S. general population (the civilian, noninstitutionalized population) aged 12 or older in the year 2002. In addition, State samples were representative of their respective State populations.

3. Prevalence Rates

The national prevalence rates were computed using the multi-procedure package called SUDAAN©: Software for Statistical Analysis of Correlated Data (RTI, 2001). The final, nonresponse-adjusted, and poststratified analysis weights were used in SUDAAN to compute unbiased design-based drug use estimates.

Prevalence rates are the proportions of the population who exhibit characteristics of interest (such as substance use). Let $\hat{p}_d$ represent the prevalence rate of interest for domain *d*. Then $\hat{p}_d$ would be defined as the ratio

$$\hat{p}_d = \frac{\hat{Y}_d}{\hat{N}_d},$$

where

$\hat{Y}_d$ = estimated number of persons exhibiting the characteristic of interest in domain *d*, and

$\hat{N}_d$ = estimated population total for domain *d*.

$\hat{N}_d$ is estimated as $\sum w_i \delta_i$, where w_i represents the analysis weight and δ_i represents an indicator variable, which is defined as:

$\delta_i (d)$ = 1 if the i^{th} sample unit is in subgroup *d*,
0 otherwise.

4. Sampling Error

Like the prevalence rates, all of the variance estimates were calculated in SUDAAN using the SUDAAN option called DESIGN=WR, which is unbiased for linear statistics based on multistage clustered sample designs where the first-stage (primary) sampling units are drawn with replacement.

Because of the nature of stratified-clustering sampling design, key nesting variables were created for use in SUDAAN to capture explicit stratification and to identify clustering. For the 2002 NSDUH, each FI region consisted of its own stratum. Two replicates per year were defined within each variance stratum. The first replicate consisted of those "phasing out" segments (i.e., those which would not be used in the next survey year). The second replicate was made up of those "phasing in" segments (i.e., those which would be fielded again the following year), thus constituting the 50 percent overlap between survey years. Each variance replicate consisted of four segments, one segment for each quarter of data collection.

Estimates of means or proportions, $\hat{p}_d$, such as drug use prevalence, take the form of nonlinear statistics whenever the variances cannot be expressed in closed form. Variance estimation for nonlinear statistics in SUDAAN is based on a first-order Taylor series approximation of the deviations of estimates from their expected values (RTI, 2001).

Estimates of domain totals, $\hat{Y}_d$, corresponding to estimates of domain proportions, $\hat{p}_d$, can be estimated as

$$\hat{Y}_d = \hat{N}_d \cdot \hat{p}_d,$$

where

$\hat{N}_d$ = estimated population total for domain *d*, and

$\hat{p}_d$ = estimated proportion for domain *d*.

The standard error (SE) for the total estimate is obtained by multiplying the SE of the proportion by $\hat{N}_d$, that is,

$$SE(\hat{Y}_d) = \hat{N}_d \cdot SE(\hat{p}_d).$$

This approach is theoretically correct when the domain size estimates, $\hat{N}_d$, are among those forced to Census Bureau population projections through the weight calibration process (Chen, Dai, Gordek, Shi, Singh, & Westlake, 2004). In these cases, $\hat{N}_d$ is clearly not subject to sampling error.

For estimated domain totals, $\hat{Y}_d$, where $\hat{N}_d$ is not fixed (i.e., where domain size estimates are not forced to Census Bureau population projections), this formula may still provide a good

approximation if it can be reasonably assumed that the sampling variation in $\hat{N}_d$ is negligible relative to the sampling variation. For most NSDUH estimates, this is a reasonable assumption.

However, for a subset of tables produced from the 2002 data, it was clear that the above approach yielded an underestimate of the variance of a total because $\hat{N}_d$ was subject to considerable variation. In these cases, a direct estimate of the standard error of $\hat{Y}_d$ was taken from SUDAAN.

5. Degrees of Freedom

To determine whether the observed difference between estimates is statistically significant, the degrees of freedom are needed to locate the corresponding probability level (*p* value) of the test statistic. The test statistic is computed from the sample data and represents a numerical summary of the difference between the estimates under consideration; it is a random variable that has a predetermined distribution (such as normal, chi-square, or F). The "degrees of freedom" refer to the amount of variation allowed due to sampling error and are used in conjunction with the test statistic to determine probabilities and evaluate statistical significance.

SUDAAN automatically calculates the degrees of freedom as the number of primary sampling units (variance replicates) less the number of strata for the dataset being analyzed. SUDAAN also allows the user to run analyses on populations (or subgroups) of interest through the SUBPOPN statement. However, even though the SUBPOPN statement is used, SUDAAN will complete analyses using the total degrees of freedom of the entire dataset unless the user specifies otherwise. This can be done in SUDAAN by specifying the appropriate degrees of freedom using the DDF option.

In NSDUH analyses, the degrees of freedom are based on the first-level stratification (i.e., the FI regions). When producing estimates on the national level, there are 900 degrees of freedom. If an analysis only involves certain States, the degrees of freedom change depending on whether the State is a large sample or small sample State. The large sample States (California, Florida, Illinois, Michigan, New York, Ohio, Pennsylvania, and Texas) each have 48 degrees of freedom. All other States (small sample States and the District of Columbia) have 12 degrees of freedom.

6. Statistical Significance of Differences

Once the degrees of freedom have been determined, various methods used to compare prevalence estimates may be employed. This section describes some of these methods. Customarily, the observed difference between estimates is evaluated in terms of its statistical significance. Statistical significance is based on the *p* value of the test statistic, and refers to the probability that a difference as large as that observed would occur due to random variability in the estimates if there were no difference in the prevalence rates being compared. The significance of observed differences is generally reported at the 0.05 and 0.01 levels.

Although the design of the 2002 survey is similar to the design of the 1999 through 2001 surveys, there are important methodological differences between the 2002 NSDUH and prior surveys; because of these changes, the 2002 estimates were not compared with 2001 and earlier estimates. However, comparisons between prevalence estimates for various populations (or subgroups) of interest were conducted.

When comparing prevalence estimates between two populations, the null hypothesis (no difference between the prevalence rates) was tested against the alternative hypothesis (there is a difference between prevalence rates) using the standard difference in proportions test, expressed as

$$Z = \frac{\hat{p}_1 - \hat{p}_2}{\sqrt{\text{var}(\hat{p}_1) + \text{var}(\hat{p}_2) - 2\,\text{cov}(\hat{p}_1, \hat{p}_2)}},$$

where

$\hat{p}_1$ = 2002 estimate for population 1,

$\hat{p}_2$ = 2002 estimate for population 2,

$\text{var}(\hat{p}_1)$ = variance of 2002 estimate for population 1,

$\text{var}(\hat{p}_2)$ = variance of 2002 estimate for population 2, and

$\text{cov}(\hat{p}_1, \hat{p}_2)$ = covariance between $\hat{p}_1$ and $\hat{p}_2$.

Under the null hypothesis, *Z* is asymptotically distributed as a normal random variable. Therefore, calculated values of *Z* can be referred to as the unit normal distribution to determine the corresponding probability level (i.e., *p* value). Since the covariance term is not necessarily zero, SUDAAN was used to compute estimates of *Z* along with the associated *p* values; this assured that the covariance term was calculated by taking the sample design into account. A similar procedure and formula for *Z* were used for estimated totals.

When comparing population subgroups defined by three or more levels of a categorical variable, log-linear Chi-square tests of independence of the subgroup and the prevalence variables were conducted first to control the error level for multiple comparisons. If the Chi-square test indicated overall significant differences, the significance of each particular pairwise

comparison of interest was tested using SUDAAN analytic procedures to properly account for the sample design. A detailed description of the test statistic, which is based on the Wald statistic, can be found in the SUDAAN user's manual (RTI, 2001, pp. 317-319).

7. Confidence Intervals

In some NSDUH publications, sampling error was quantified using 95 percent confidence intervals. Because the estimates in the NSDUH are frequently small percentages, the confidence intervals are based on logit transformations. Logit transformations yield asymmetric interval boundaries that are more balanced with respect to the probability that the true value falls below or above the interval boundaries than is the case for standard symmetric confidence intervals for small proportions.

To illustrate the method, let the proportion P_d represent the true prevalence rate for a particular analysis domain *d*. Then, the logit transformation of P_d, commonly referred to as the "log odds," is defined as

$$L = 1n[P_d / (1 - P_d)],$$

where "1n" denotes the natural logarithm.

Letting $\hat{p}_d$ be the estimate of the domain proportion, the log odds estimate becomes

$$\hat{L} = 1n[\hat{p}_d / (1 - \hat{p}_d)].$$

The lower and upper confidence limits of *L* are formed as

$$A = \hat{L} - K\left[\frac{\sqrt{\text{var}(\hat{p}_d)}}{\hat{p}_d(1-\hat{p}_d)}\right],$$

$$B = \hat{L} + K\left[\frac{\sqrt{\text{var}(\hat{p}_d)}}{\hat{p}_d(1-\hat{p}_d)}\right],$$

where $\text{var}(\hat{p}_d)$ is the variance estimate of $\hat{p}_d$, the quantity in brackets is a first-order Taylor Series approximation of the SE of $\hat{L}$, and *K* is the constant chosen to yield a level of confidence (e.g., *K* = 1.96 for 95 percent confidence limits).

Applying the inverse logit transformation to *A* and *B* above yields a confidence interval for $\hat{p}_d$ as follows:

$$\hat{p}_{d,lower} = \frac{1}{1+\exp(-A)},$$

$$\hat{p}_{d,upper} = \frac{1}{1+\exp(-B)},$$

where "exp" denotes the inverse log transformation. The lower and upper confidence interval endpoints for percentage estimates are obtained by multiplying the lower and upper endpoints of $\hat{p}_d$ by 100.

The confidence interval for the estimated domain total, $\hat{Y}_d$, as estimated by

$$\hat{Y}_d = \hat{N}_d \cdot \hat{p}_d,$$

is obtained by multiplying the lower and upper limits of the proportion confidence interval by $\hat{N}_d$. For domain totals $\hat{Y}_d$, where $\hat{N}_d$ is not fixed, the confidence interval approximation assumes that the sampling variation in $\hat{N}_d$ is negligible relative to the sampling variation in $\hat{p}_d$.

8. Incidence Estimates

To assist in the evaluation of trends in the initiation of drug use, NSDUH data also were used to generate estimates of drug use incidence or initiation (i.e., the number of new users during a given year). Incidence rates measure the rapidity with which new drug users arise and can suggest emerging patterns of drug use.

The measure of incidence is defined as the number of new cases of drug initiation divided by the person time of exposure. For diseases, the incidence rate, *IR*, for a population is defined as the number of new cases of the disease, *N*, divided by the person time, *PT*, of exposure, or

$$IR = \frac{N}{PT}.$$

The person time of exposure is measured as the net time that individuals in the population during an observed period of time are at risk of developing the disease. This period of time can be for the full period of the study or for a shorter period. The person time of exposure ends at the time of diagnosis (e.g., Greenberg, Daniels, Flanders, Eley, & Boring, 1996, pp. 16-19). Similar conventions were followed for defining the incidence of first use of a substance.

Beginning in 1999 and continuing through 2002, the NSDUH questionnaire allowed for the collection of year and month of first use for recent initiates. The month, day, and year of birth for the initiates were also obtained directly or imputed during the processing of the data. In addition, the questionnaire call record provided the date of the interview. By imputing a day of first use within the year and month of first use reported or imputed, the key respondent inputs, in terms of exact dates, can be computed. Exposure time can be determined in terms of days and converted to an annual value.

Having exact dates of birth and first use also allowed the determination of person time of exposure during the targeted period, *t*. Let the target time period for measuring incidence be specified in terms of dates. For the period 1998, for example, the specification would consist of

$$t = [t_1, t_2) = [\text{1 Jan 1998, 1 Jan 1999}),$$

a period that includes January 1, 1998, and all days up to but not including January 1, 1999. The target age group can also be defined by a half-open interval as $a = [a_1, a_2)$. For example, the age group 12 to 17 would be defined by $a = [12,18)$ for youths at least age 12, but not yet age 18.

If person *i* was in age group *a* during period *t*, the time and age interval, $L_{t,a,i}$, can then be determined by the intersection

$$L_{t,a,i} = [t_1, t_2) \cap [DOB_i\ MOB_i\ YOB_i + a_1,\ DOB_i\ MOB_i\ YOB_i + a_2),$$

where the time of birth is defined in terms of day (DOB_i), month (MOB_i), and year (YOB_i). Either this intersection was empty ($L_{t,a,i} = \varnothing$), or it was defined by the half-open interval, $L_{t,a,i} = [M_{1,i}, M_{2,i})$, where

$$M_{1,i} = Max\{t_1, (DOB_i\ MOB_i\ YOB_i + a_1)\},$$

and

$$M_{2,i} = Min\{t_2, (DOB_i\ MOB_i\ YOB_i + a_2)\}.$$

The date of first use, $t_{fu,d,i}$, is also expressed as an exact date. An incident of first use of drug *d* by person *i* in age group *a* occurs in time *t* if $t_{fu,d,i} \in [M_{1,i}, M_{2,i})$. The indicator function, $I_i(d,a,t)$, used to count incidents of first use is set to 1 when $t_{fu,d,i} \in [M_{1,i}, M_{2,i})$, and to 0 otherwise. The person time exposure, measured in years and denoted by $e_i(d,a,t)$ for a person *i* of age group *a* depends on the date of first use. If the date of first use precedes the target period ($t_{fu,d,i} < M_{1,i}$), then $e_i(d,a,t) = 0$. If the date of first use occurs after the target period or if person *i* has never used drug *d*, then

$$e_i(d,a,t) = \frac{M_{2,i} - M_{1,i}}{365}.$$

If the date for first use occurs during the target period, $L_{t,a,i}$, then

$$e_i(d,a,t) = \frac{t_{fu,d,i} - M_{1,i}}{365}.$$

During leap years, the denominator used to compute person time exposure is set to 366. Note that both $I_i(d,a,t)$ and $e_i(d,a,t)$ are set to 0 if the target period, $L_{t,a,i}$, is empty (i.e., person *i* is not in age group *a* during time *t*). The incidence rate is then estimated as a weighted ratio estimate

$$IR(d,a,t) = \frac{\sum_i w_i I_i(d,a,t)}{\sum_i w_i e_i(d,a,t)},$$

where w_i is the respondent's analytic weight for 2002.

Since the incidence estimates are based on retrospective reports by survey respondents (as was the case for earlier estimates), they may be subject to some of the same kinds of biases. Bias resulting from differential mortality occurs because some persons who were alive and exposed to the risk of first drug use in the historical periods shown in the tables died before the 2002 NSDUH was conducted. This type of bias is probably very small. Incidence estimates are also affected by memory errors, including recall decay (tendency to forget events occurring long ago) and forward telescoping (tendency to report that an event occurred more recently than it

actually did). These memory errors would both tend to result in estimates for earlier years (i.e., 1960s and 1970s) that are downwardly biased (because of recall decay) and estimates for later years that are upwardly biased (because of telescoping). There is also likely to be some underreporting bias because of the social stigma of drug use behaviors and respondents' fears of disclosure. This is likely to have the greatest impact on recent estimates, which reflect more recent use and reporting by younger respondents. Retrospective reports used for incidence estimates may include reports from recent immigrants whose substance use initiation occurred before they became NSDUH study-eligible as U.S. residents; this leads to some positive bias in the estimates that is not likely to be offset by emigration occurring over the same period.

For drug use that is frequently initiated at age 10 or younger, estimates based on retrospective reports 1 year later underestimate total incidence because 11-year-old (and younger) children are not sampled by the NSDUH. Prior analyses showed that alcohol and cigarette (any use) incidence estimates could be significantly affected by this. Therefore, for these drugs, only 2001 age-specific rates and the number of initiates 18 or older were reported.

9. Retrospective Lifetime Prevalence Estimates

Retrospective measures of lifetime substance use prevalence were obtained for prior years based on the 2002 sample. As of a specified date, lifetime prevalence measures are defined as the ratio

$$PR = \frac{N_{users}}{N_{all}},$$

where the numerator, N_{users}, represents all persons who report lifetime use as of that date and the denominator, N_{all}, represents both lifetime users and nonusers. For NSDUH current year estimates, the specified date is the date of interview for each respondent.

As was described in Section 6, complete data on a respondent's exact date of first substance use is known or imputed during the processing of the current year's data. In addition, the date of interview and date of birth are on the current year's data file. These data make it possible to retrospectively estimate lifetime prevalence measures for prior years based on the current year respondents.

Because comparisons of prevalence rates across years from this analysis are based on a common sample, the precision of trend estimates is improved. On the negative side, the retrospective measures do not properly reflect the impacts of migration and mortality.[3] To control for the possible effects of mortality, the retrospective estimates are limited to the younger age groups: 12 to 17 and 18 to 25. In addition, retrospective prevalence estimates may be biased due to memory errors. As noted in the discussion of incidence estimates (Section 6), recall decay leads to a general downward bias. Forward telescoping (the tendency to report initial substance use more recently than it actually occurred) will create downward bias in early years, but have little impact on recent estimates. It also should be noted that, due to the sampling strategy that selects older persons with lower probabilities of selection, the estimates for early years (reported by persons who are now 26 or older) are based on much smaller sample sizes and subject to more sampling error.

[3] The same limitations apply to the estimates of incidence rates for prior years based on the current sample responses.

A key assumption for computing retrospective lifetime prevalence estimates is that the month and day of the respondent use and age status in prior years is based on the same month and day as the date of interview in the current survey year. Retrospective estimates, *PR(d,a,t),* of lifetime substance *d* use were prepared for 1965 to 2002 as a simple ratio estimate for year *t* and age group *a* as

$$PR(d,a,t) = \frac{\sum_i w_i y_i(d,a,t)}{\sum_i w_i x_i(a,t)},$$

where w_i is the respondent's analytic weight for 2002. The values of $x_i(a,t)$ and $y_i(d,a,t)$ are determined from:

- date (day, month, and year) of interview (DOI_i MOI_i YOI_i), designated by *t*;
- respondent's current age, a_i;
- respondent's lifetime substance *d* use status; and
- respondent's reported date of first use of the substance *d*, $t_{fu,d,i}$ (if the respondent is a lifetime user).

For the current survey year, $x_i(a,t)$ has a value of 1 if the current age of respondent *i* is in the interval *a*, and a value of 0 otherwise. If the age interval is 12 to 17, then the respondent must be at least 12, but not yet 18. For the current survey year, $y_i(d,a,t)$ has a value of 1 if $x_i(a,t)$ has a value of 1 and respondent *i* is a lifetime user of substance *d*. For current lifetime users, this means that their reported date of first use is on or before the date of interview (i.e., if $t_{fu,d,i} \le DOI_i MOI_i YOI_i$). Otherwise, $y_i(d,a,t)$ has a value of 0.

For prior years, it is first necessary to compute the difference in years as $\Delta t = YOI_i - t$. Then, $x_i(a,t)$ has a value of 1 if respondent *i* 's retrospectively adjusted age, $a_i - \Delta t$, is in the interval *a*, and a value of 0 otherwise. Also, $y_i(d,a,t)$ has a value of 1 if $x_i(a,t)$ has a value of 1, respondent *i* is a lifetime user of substance *d*, and the reported date of first use is on or before an adjusted date of interview (i.e., if $t_{fu,d,i} \le DOI_i MOI_i YOI_i - \Delta t$). Otherwise, $y_i(d,a,t)$ has a value of 0.

10. Suppression of Estimates with Low Precision

Direct survey estimates that were considered to be unreliable due to unacceptably large sampling errors were not reported, but rather were noted by an asterisk (*). The criterion used for suppressing all direct survey estimates was based on the relative standard error (RSE), which is defined as the ratio of the standard error (SE) over the estimate.

For proportion estimates $(\hat{p})$ within the range $0 < \hat{p} < 1$, rates and corresponding estimated numbers of users were suppressed if

$$\text{RSE}\,[-1\text{n}(\hat{p})] > 0.175 \text{ when } \hat{p} \le 0.5,$$

or

$$\text{RSE}\,[-1\text{n}(1-\hat{p})] > 0.175 \text{ when } \hat{p} > 0.5.$$

Based on a first-order Taylor series approximation of $\text{RSE}\,[-1\text{n}(\hat{p})]$ and $\text{RSE}\,[-1\text{n}(1-\hat{p})]$, the following suppression rule was used for computational purposes

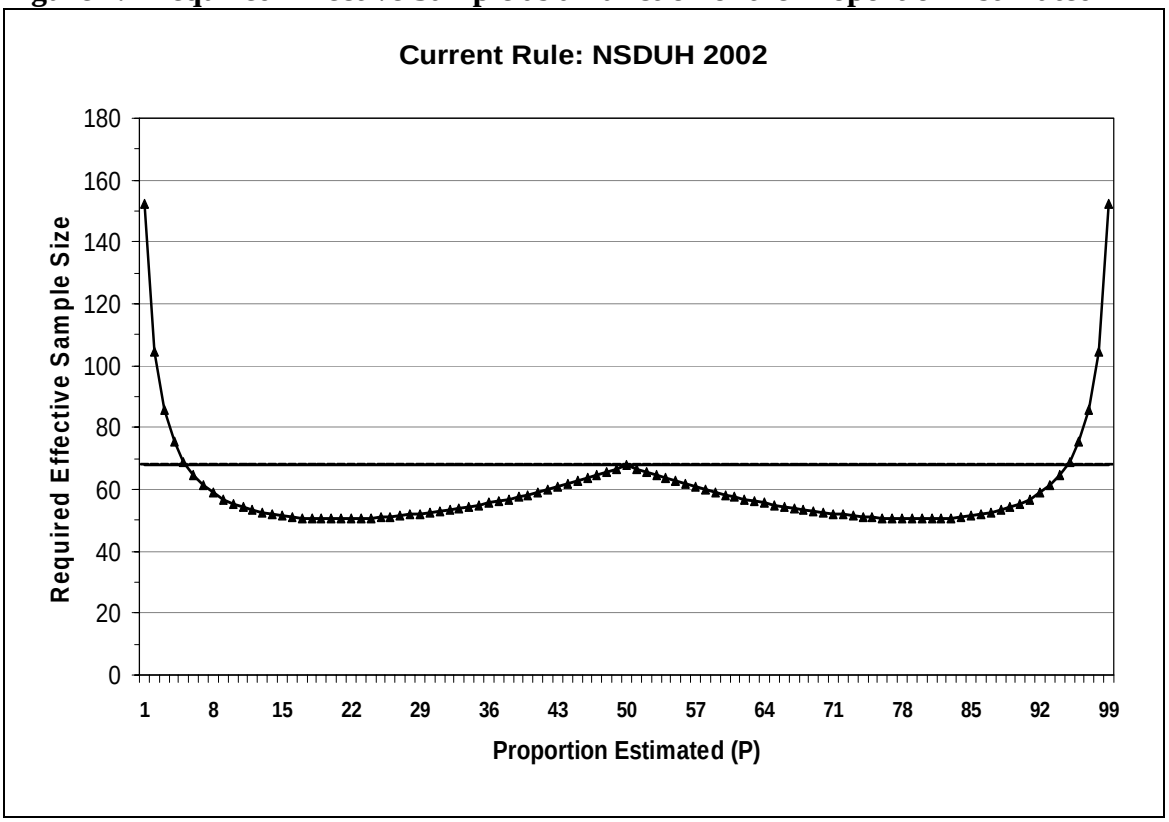
$$\frac{\text{SE}(\hat{p})/\hat{p}}{-1\text{n}(\hat{p})} > 0.175 \text{ when } \hat{p} \le 0.5,$$

or

$$\frac{\text{SE}(\hat{p})/(1-\hat{p})}{-1\text{n}(1-\hat{p})} > 0.175 \text{ when } \hat{p} > 0.5.$$

The separate formulas for $\hat{p} \le 0.5$ and $\hat{p} > 0.5$ produce a symmetric suppression rule; that is, if $\hat{p}$ is suppressed, $1-\hat{p}$ will be suppressed as well. See Figure 1 for a graphical representation of the required minimum effective sample sizes as a function of the proportion estimated. When $0.05 < \hat{p} < 0.95$, the symmetric properties of the rule produce local minimum effective sample sizes at $\hat{p} = 0.2$ and again at $\hat{p} = 0.8$, such that an effective sample size of greater than 50 is required; this means that estimates would be suppressed for these values of $\hat{p}$ unless the effective sample sizes were greater than 50. Within this same interval of $0.05 < \hat{p} < 0.95$, a local maximum effective sample size of 68 is required at $\hat{p} = 0.5$. So, to simplify requirements and maintain a conservative suppression rule, estimates of $\hat{p}$ between 0.05 and 0.95 which had effective sample sizes below 68 were suppressed.

Figure 1. Required Effective Sample as a Function of the Proportion Estimated



A minimum nominal sample size suppression criteria (n = 100) that protects against unreliable estimates caused by small design effects and small nominal sample sizes was employed in 2002. Prevalence estimates were also suppressed if they were close to 0 or 100 percent (i.e., if $\hat{p} < 0.00005$ or if $\hat{p} \geq 0.99995$).

Retrospective lifetime prevalence tables were produced for the first time in 2002. The same suppression criteria used for prevalence estimates were used for retrospective lifetime prevalence estimates.

Estimates of other totals (e.g., number of initiates), along with means and rates that are not bounded between 0 and 1 (e.g., mean age at first use and incidence rates) were suppressed if the RSEs of the estimates were larger than 0.5.

Additionally, estimates of mean age of first use were suppressed if the sample sizes were smaller than 10 respondents; also, the estimated incidence rate and number of initiates were suppressed if they rounded to 0.

The suppression criteria for various NSDUH estimates are summarized in Table 1.

Table 1. Summary of 2002 NSDUH Suppression Rules

Estimate	Suppress if:
Prevalence rate, $\hat{p}$, with nominal sample size, *n*, and design effect, *deff*	(1) The estimated prevalence rate, $\hat{p}$, is less than 0.00005 or greater than or equal to 0.99995, or (2) $\frac{\mathrm{SE}(\hat{p})/\hat{p}}{-1\mathrm{n}(\hat{p})} > 0.175$ when $\hat{p} \le 0.5$, or $\frac{\mathrm{SE}(\hat{p})/(1-\hat{p})}{1\mathrm{n}\ (1-\hat{p})} > 0.175$ when $\hat{p} > 0.5$, or (3) Effective *n* < 68, where Effective $n = \frac{n}{deff}$, or (4) *n* < 100. Note: The rounding portion of this suppression rule for prevalence rates will produce some estimates that round at one decimal place to 0.0 percent or 100.0 percent but are not suppressed from the tables.
Estimated number (numerator of $\hat{p}$)	The estimated prevalence rate, $\hat{p}$, is suppressed. Note: In some instances when $\hat{p}$ is not suppressed, the estimated number may appear as a 0 in the tables; this means that the estimate is greater than 0 but less than 500 (estimated numbers are shown in thousands).
Mean age at first use, $\bar{x}$, with nominal sample size, *n*	(1) $\mathrm{RSE}(\bar{x}) > 0.5$, or (2) *n* < 10
Incidence rate, $\hat{r}$	(1) The incidence rate, $\hat{r}$, rounds to less than 0.1 per thousand person years of exposure, or (2) $\mathrm{RSE}(\hat{r}) > 0.5$
Number of initiates, $\hat{t}$	(1) The number of initiates $\hat{t}$, rounds to fewer than 1,000 initiates, or (2) $\mathrm{RSE}\ (\hat{t}) > 0.5$

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