Journal of Official Statistics
Vol. 3, No. 4, 1987, pp. 439–448


# Measuring Drug Use Among Swedish Adolescents

## Randomized Response Versus Anonymous Questionnaires

*Berth Danermark and Bengt Swensson*[1]

**Abstract:** A survey with an embedded experiment of 1 290 pupils in the ninth grade of comprehensive school compares two anonymity preserving measurement techniques, viz., randomized response and anonymous questionnaires. The two techniques showed no significant difference for measuring the percentage of pupils who had ever smoked hashish or marihuana. The survey was also designed to allow a study of the impact of nonresponse. The estimated percentage of pupils who had ever smoked cannabis was found to be significantly higher for nonrespondents.

**Key words:** Anonymity protection; anonymous questionnaires; drug use; nonresponse; randomized response.

### 1. Introduction

In Sweden, as in many other countries, great concern has been expressed about adolescent drug use. During more than a decade the Swedish Board of Education (SBE) has sponsored annual surveys, based on anonymous questionnaires, to measure drug use among pupils in the ages 12–13 and 15–16. Table 1 below shows the extent of cannabis use according to the SBE surveys among girls and boys aged 15–16 from 1971 to 1983.

*Table 1. Estimated percentage of pupils in the ninth grade who have used cannabis*

| Sex | Year | | | | | |
|---|---|---|---|---|---|---|
| | 1971 | 1977 | 1979 | 1981 | 1982 | 1983 |
| Boys | 14 | 8 | 6 | 9 | 8 | 5 |
| Girls | 17 | 8 | 6 | 9 | 8 | 6 |

[1] Ph.D. in Sociology, Department of Sociology; Senior Lecturer in Statistics, Department of Data Analysis, respectively, University of Örebro, P.O. Box 923, S–701 30 Örebro, Sweden. The research was supported by the Delegation for Social Research, the Ministry for Social Affairs. The authors wish to thank the editor Ingrid Lyberg and three anonymous referees for their very valuable comments and suggestions.

The figures are averages for the whole country, and thus do not mirror the large variations among municipalities.

Critics claim the SBE figures are too low because of faulty measurements and nonresponse. The most vigorous critics (Kühlhorn and Åslund (1983)) maintain that the correct percentages are at least three times as large as the SBE percentages. These claims, however, have not been supported by reliable empirical studies. The need for studies that illuminate the impact of measurement and nonresponse errors is obvious.

The purpose of this paper is to give an account of an empirical study which (i) compares two different anonymity preserving methods for measuring cannabis use (viz., randomized response interviews and anonymous questionnaires), and (ii) evaluates the impact of nonresponse. As points of departure we will first describe the main components of the SBE surveys, and give a very brief account of alternative measurement methods.

## 2. The SBE Annual Surveys

The SBE survey population consists of pupils in the sixth and ninth grades of compulsory school. Since 1978 the sample design has been stratified cluster sampling. Counties form the strata and school classes form the clusters, where the cluster size is approximately 25. The measurement design employs anonymous questionnaires. The SBE contacts the schools where classes have been selected for the survey approximately one month before the data collection period in April and May. The questionnaires are distributed to the home room teachers for selected classes. The pupils are asked to answer the questionnaires anonymously at a home room session, and to put the completed questionnaires in unmarked envelopes. The sealed envelopes are then collected by the teacher, and delivered to the survey staff.

The SBE survey tries to measure the percentage of pupils who have ever used cannabis and the number of times cannabis is used during the month preceding the day of data collection.

The survey estimates are based on the answers from respondents present on the day of data collection only, e.g., there is no regular follow-up of nonresponse. The overall nonresponse rate is approximately 13–15%. According to the survey staff, the nonresponse has only marginal impact on the survey estimates. This assertion seems to be based on a single unpublished follow-up survey.

## 3. Alternative Methods for Measuring Drug Use

Surveys dealing with sensitive matters like drug use, are at the risk of encountering substantial evasive answer bias unless precautions are taken. These precautions include the use of some kind of anonymity preserving technique – the more sensitive the topic, the greater the demand for anonymity protection.

### *3.1. Anonymous questionnaires*

The anonymous questionnaire (AQ) used for the SBE surveys has certain drawbacks, although it gives a high degree of anonymity protection. The most serious drawback is that a pupil in a crowded home room may have difficulties in completing the questionnaire without revealing his/her answers to adjacent classmates. Furthermore, the technique can only be used in survey situations where natural groups can be formed.

Kühlhorn and Åslund (1983) maintain that the AQ approach with direct questions on drug experience (e.g., "Have you ever used cannabis?") will lead to serious under-estimation, since many respondents with experience of drugs will not tell the truth. The critics recommend the use of indirect questions, for example "How many pupils in this class do

you believe have ever used cannabis at least once?" Using the answers given to the indirect questions estimates are computed based on, for example, medians. The critics give no convincing evidence on the superiority of indirect questions over direct questions.

### 3.2. *Randomized response interviews*

Another measurement method is known as the randomized response technique (RRT). This technique was introduced by Warner (1965) and is designed for data collection based on personal interviews. The technique rests on the assumption that respondent cooperation will increase with increasing anonymity. Thus, evasive answers are eliminated, or at least reduced. The original Warner method of assuring the respondents' privacy consists of asking every respondent in a probability sample to select – using a randomization device, e.g., an ordinary die – one of two complementary statements, $S$ and $\overline{S}$. The statement $S$ has the general form "I have the (sensitive) characteristic $A$," and the statement $\overline{S}$ has the general form "I do not have the (sensitive) characteristic $A$." In our case this translates as: "I have used cannabis at least once" and "I have never used cannabis." The respondent is then asked to answer (true or false) the randomly selected statement without revealing which of the two statements he/she is answering. Knowing the probability of selecting the statement $S$, say $P$, the survey researcher will be able to estimate unbiasedly the proportion of individuals, $\mu$, in the population who have the sensitive characteristic $A$ although the true value is unknown for each single individual (if every respondent in every sample responds truthfully). To exemplify this statement, let $y_k = 1$ if the $k$th individual in the finite population of size $N$ has the characteristic $A$, $y_k = 0$ otherwise ($k = 1,\ldots,N$). Furthermore, for $k$ which belong to the selected sample $s$, let $x_k = 1$ if the $k$th individual gives the answer *true*, $x_k = 0$ otherwise. Then it is easily shown that the estimator $\hat{y}_k = (x_k + P\text{–}1) / (2P\text{–}1)$ is unbiased for $y_k$, provided that $P \neq 1/2$, with its variance given by $V = P(1\text{–}P) / (2P\text{–}1)^2$. Hence, the population proportion $\mu$ can be unbiasedly estimated from a probability sample $s$ selected according to a sampling design with first-order inclusion probabilities $\pi_k$ ($> 0$ for every $k$ in the population) by $\hat{\mu}_{RRT} = \Sigma_s \hat{y}_k / (N\pi_k)$. The variance of this estimator is given by

$$V(\hat{\mu}_{RRT}) = V(\hat{\mu}) + V_P ,$$

where $V(\hat{\mu})$ is the variance of the ordinary Horvitz-Thompson estimator $\hat{\mu} = \Sigma_s y_k / (N\pi_k)$ (which could be used if every $y_k$ in the sample $s$ were known), and where

$$V_P = \frac{V}{N^2} \cdot \Sigma_1^N 1/\pi_k .$$

The second variance component $V_P$ is the price to be paid for introducing the random choice of statement. It can be very high if $P$ is chosen near 0.5 as seen in the following table.

*Table 2. The variance V as a function of P for a few selected P-values*

| $P$ | $V$ |
|---|---|
| 1.0 | 0.0 |
| 0.9 | 0.14 |
| 0.8 | 0.44 |
| 0.7 | 1.31 |
| 0.6 | 6.0 |
| 0.55 | 24.75 |

Nevertheless in many instances the unbiased estimator $\mu_{RRT}$ will be preferred to a biased estimator with unknown (but non-negligible) bias, see, e.g., Warner (1965).

The innovative paper by Warner (1965) spurred a rapid and extremely interesting development of RRTs and similar alternatives for data collection and data protection. No account of this development will be given

here; reference is given to Horvitz et al. (1976), Swensson (1977), Boruch and Cecil (1979), Daniel (1979), Fox and Tracy (1980), and Chaudhuri and Mukerjee (1987).

To ensure a successful application of an RRT, it must be presented to the respondents in a way that elicits a substantially lower degree of evasiveness. The degree of evasiveness should be low enough to justify the increase in variance. To investigate the efficiency of RRT a number of empirical studies using various RRTs – some of which are compiled in the appendix – have been undertaken, most of them in the U.S. The only Swedish study we know of – beside our study – is a small validation study on receipt of public relief by Eriksson (1973).

### *3.3. Urine analysis*

A seemingly more reliable measurement method (for recent cannabis use) would be a urine analysis. We will not discuss such methods here, since their use in compulsory school – after an unprofessional application in a rural district – has been made virtually impossible in Sweden.

## 4. The Empirical Study

In this section we will present our study design, justification for our choice of important design constituents, and the study results. A concluding discussion will then be given in Section 5.

### *4.1. The study design*

#### 4.1.1. The survey population and the principal study variable

The survey population was defined as pupils belonging to 50 classes with an average size of approximately 25 pupils in Örebro, a city with 120 000 inhabitants. These 50 classes formed the ninth grade of comprehensive school in the 1983 spring term. The principal study variable was taken as the dichotomy whether or not the pupils had ever smoked hashish or marihuana.

The 50 classes were randomly divided into two groups, with 20 units in group 1 and 30 units in group 2. Pupils in group 1 were to answer an anonymous questionnaire at a home room session, while pupils in group 2 were to be individually interviewed using a variant of the RRT (to be described below) for two sensitive questions on drug use.

To study the possible impact of nonresponse on survey estimates, resources were reserved for intensive follow-ups on pupils not present at the data collection at the home room sessions.

#### 4.1.2. The anonymous questionnaire (AQ) design

The anonymous questionnaire approach we used (with direct questions) differed in two respects from the one in the SBE surveys. First, the questionnaires were presented, administered and collected by the survey staff (not by the teacher), and second, the length of the questionnaire was shorter for our study.

#### 4.1.3. The randomized response design

The following is a description of the randomized response design used in our study. For the principal sensitive question, each respondent received a plastic cup containing two ordinary dice. The respondent was told to shake the cup with the two dice and then count the resulting total number of dots. The respondent was then instructed to give an answer, depending on the outcome, as follows.

I. If the total number of dots is 4, 5, 6, 7, 8, 9, or 10

   If you have smoked hashish or marihuana at least once give the answer "A."
   If you have never smoked hashish or marihuana give the answer "B."

II. If the total number of dots is 2, 3, 11, or 12

If you have never smoked hashish or marihuana give the answer "A."
If you have smoked hashish or marihuana at least once give the answer "B."

The probability of outcome I is 5/6, and that of outcome II is 1/6.

Of course, precautions were taken so that the interviewer was not able to reveal the true drug use status of the respondent. Also, a thorough instruction and a practical demonstration preceded the actual RR interview.

### *4.2. Some design considerations*

A few comments will be made on our choice of design. In particular we will consider the choices of AQ design and RRT design, and the allocation of class units.

#### 4.2.1. Choice of AQ design

We wanted our AQ approach to be close to the one used for the SBE surveys, so that relevant comparisons can be made. We had one reason for not copying the SBE measurement design exactly. It was deemed necessary to have maximum control over the survey situation, in order not to jeopardize the comparison between our own AQ and RRT.

#### 4.2.2. Choice of RRT design

Today there is an abundance of RRTs with different design parameters and different performance characteristics. Many vital aspects of an RRTs performance are still unknown; most pertinent is to what extent candid responses are elicited. Thus, the choice of RRT design is a delicate business.

For the young, unsophisticated population to be studied, it was considered essential to choose a randomized response design characterized by the use of simple procedures, with instructions that are easy to understand. The respondents should be familiar with the randomization device in order not to raise suspicion concerning its randomness. The device should also entail a subjectively perceived high degree of anonymity protection. Finally, it was deemed important to use symmetry of response with respect to the sensitive characteristic. Each possible response, of itself, should convey no information on the state of the respondent with respect to that characteristic – a property recommended by Bourke and Dalenius (1976). The notion of symmetry of response is further elaborated in Bourke (1984).

The demands of simplicity and symmetry considerably limit the available number of randomized response designs, and led to the choice of the original Warner design presented in Section 3.2.

As randomization device candidates we thought that dice and a deck of cards would be sufficiently familiar to the pupils. However, a deck of cards was deemed inferior to dice because it is difficult to achieve a random permutation of cards through ordinary shuffling, and the respondents may suspect that the cards are marked in a way that is hard to disclose, which in turn could lead to evasive answers.

Our choice of two dice and the $P$-value 5/6 reflects a compromise between anonymity protection and estimation efficiency. The survey researcher is willing to give respondents full anonymity protection. To achieve this a $P$-value "not too far" from 1 is sufficient. This may be difficult to explain to the respondents who intuitively understand that a $P$-value close to 1/2 is favourable for their protection. Therefore, the survey researcher will try to choose a $P$-value close to 1 and to organize the randomized response interviews in such a way that the respondents subjectively perceive a high degree of anonymity protection. If the survey researcher is successful, he will achieve satisfactory cooperation from the respondents

and still maintain an acceptable variance (a low $V_P$).

The $P$-value 5/6 can obviously be obtained using only one die. However, it was supposed that the use of two dice would result in a higher degree of perceived anonymity protection, since many respondents presumably would be influenced by the fact that the eleven outcomes are split into two groups of seven and four. This may, erroneously, create the impression that a $P$-value near 1/2 ($7/11 \approx 0.63$) is being used. Other outcome splits, with associated $P$-values closer to 1 were considered. However, a pilot study (although small) implied that such $P$-values might endanger respondent cooperation.

We believed that having the pupils answer "A" or "B" instead of "Yes" or "No" would be less embarrassing to the pupils themselves.

#### 4.2.3. Allocation of the class units

The random allocation 20–30 of the 50 classes does not minimize the variance of the difference between the two estimators $\hat{\mu}_{RRT}$ and $\hat{\mu}_{AQ}$ of $\mu$ (the proportion of pupils who have used hashish or marihuana at least once). The variance depends, essentially, with respect to unknown parameters, on $\mu$ and the probability of telling the truth associated with each method and each pupil. A theoretical analysis showed that an allocation near 15–35 would be better. However, the lack of sufficiently skilled interviewers made such an allocation unattainable.

As a check on the approximate normality of the test statistic $(\hat{\mu}_{RRT} - \hat{\mu}_{AQ}) / (\text{standard error})$ a number of Monte Carlo studies were made. These did not contradict the hypothesis of approximate normality.

### 4.3. *Results*

Approximately 90% of the pupils were present at the first call in the home room. After the last call-back the total nonresponse rate had been reduced to 1.2% (1.5% and 0.9% for AQ and RRT, respectively).

Tables 3a and 3b give an overview of the outcome of the study. A "yes" answer from a pupil on the sensitive AQ question, means that the pupil has given an affirmative answer to the question of whether he or she has ever smoked hashish or marihuana.

From the data in Tables 3a and 3b estimates of the percentage of pupils who have smoked hashish or marihuana at least once can be computed. These estimates are given in Table 4 below.

*Table 3a. Randomized response interviews. Number of pupils by call and type of answer*

| Call | Answer | | Response | Nonresponse | Total |
|---|---|---|---|---|---|
| | A | B | | | |
| 1 | 143 | 545 | 688 | 74 (9.7%) | 762 |
| 2 | 23 | 44 | 67 | | |
| Total | 166 | 589 | 755 | 7 (0.9%) | 762 |

*Table 3b. Anonymous questionnaires. Number of pupils by call and type of answer*

| Call | Answer | | Response | Nonresponse | Total |
|---|---|---|---|---|---|
| | Yes | No | | | |
| 1 | 23 | 450 | 473 | 55 (10.4%) | 528 |
| 2 | 7 | 40 | 47 | | |
| Total | 30 | 490 | 520 | 8 (1.5%) | 528 |

*Table 4. Estimated percentage (±2 standard errors) of pupils who have smoked hashish or marihuana by data collection method and by call*

| Call | Randomized response | Anonymous questionnaires | Total |
|---|---|---|---|
| 1 | 6.1 (±4.3) | 5.0 (±1.6) | 5.6 (±2.5) |
| 2 | * | * | 21.7 (±8.1) |
| Total | 7.9 (±4.3) | 5.9 (±1.9) | 7.1 (±2.4) |

* Indicates too few observations to calculate an estimate.

Although the RRT estimates are higher than the corresponding AQ estimates the differences are not significant at common significance levels.

Turning to the question of whether the nonrespondents differ from those present at the data collection in the home rooms, the differences between the two estimates 21.7 and 5.6 in the right-most column is significant at levels lower than the 0.1% significance level.

## 5. Discussion

In Sweden little interest has so far been shown in trying to incorporate randomized response interviews in empirical studies, despite the fact that several theoretical developments are attributed to Swedish statisticians. During the initial planning stage of this study many sceptics doubted the prospects of implementing the RRT practically. Was our study, then, successful with respect to the practical implementation? We do sincerely believe so. Overall the respondents seemed to understand that they were given good anonymity protection, and the interviewers thought that the technique was simple and ran smoothly.

The next crucial question is whether or not the respondents gave truthful answers. This study cannot directly answer the question of truthful responses since we have no way of knowing which students have ever used cannabis. We can answer this question only indirectly through a comparison with the AQ result. Since the RRT estimate (7.9%) did not differ significantly from the AQ estimate (5.9%), we cannot conclude that the two methods differ with respect to bias. There is always a risk, and in this study a large risk, due to the large random variation incurred by the random selection of statement to answer, of erroneously failing to reject the null hypothesis of equal bias. Hence, tentatively, the chosen RRT is as good or as bad as the AQ approach when considering bias. So the AQ approach must be considered superior to the RRT for measuring drug use among Swedish pupils because of its substantially lower random variation and its lower cost. On the other hand, the RRT shows promising potentials for situations where the AQ approach cannot be used.

Finally, turning to the question of the impact of nonresponse on estimates of the percentage of pupils who have ever used cannabis, the study unambiguously points to the biasing effect of nonresponse. Thus our result is in accordance with the general experience of similar studies. Our study suggests that the SBE figures should be raised by, approximately, a factor of 1.2.

It would be of interest to relate the results of our study to those of other empirical studies. However, this is extremely difficult due to the large inherent differences in experimental setups. Studies differ in aim, cultural setting, type of population, sensitive variables under study, variant of RRT used, objective degree of anonymity protection, randomization de-

vice, etc. Furthermore, journal articles often do not provide sufficient detail for a close comparison. Because of these reasons, we will comment only on the study most similar to our own, Goodstadt and Gruson (1975), which concerned drug use among Canadian adolescents approximately the same age as the pupils in our study. Goodstadt and Gruson's results indicate that their randomized response procedure produced (1) significantly fewer response refusals, and (2) significantly higher drug use estimates than a traditional direct questioning method. With the results of our study in mind one is tempted to guess that the higher estimates are partly a consequence of the lower nonresponse, since nonrespondents are likely to differ from respondents.

# Appendix*

**Results of Field Studies and Experiments Undertaken with Randomized Response Methods**

| | Sample size for RR method | Rounded percentage response to RR | Rounded percentage response to standard | Notable reduction in response bias (crude) | Topic |
|---|---|---|---|---|---|
| Abernathy, Greenberg, and Horvitz (1970) | 3 113 | 97 | NR | YES | Abortions |
| Illinois Institute of Technology (1971) | 1 200 | 100 | NR | INC | Organized crime |
| I-Cheng, Chow, and Rider (1972) | 1 021 | 89 | 89 | YES | Abortions |
| Brown and Harding (1973) | 1 100 | NR | NR | INC | Drugs |
| Eriksson (1973) | 76 | 97 | NR | INC | Public relief |
| Daves (1974) | 270 | 100 | NR | NR | i.e. drugs |
| Folsom (1974) | 423 | 100 | | | Drunken - driving |
| Krotki and Fox (1974) | 352 | 97 | 73 | INC | Abortions |
| Brown (mail) (1975) | 2 114 | 18–50 | 32–65 | INC | Drugs |
| Goodstadt and Gruson (1975) | 431 | 95 | 87 | YES | Drugs |
| Reaser, Heartstock, and Hoehn (mail) (1975) | 2 400 | 60–68 | 48–90 | INC | Abortions |
| Reinmuth and Geurts (1975) | 342 | 99 | | | Shoplifting |
| Wiseman, Moriarty, and Schafer (1975–76) | 340 | 100 | 100 | INC | Various |
| Barth and Sandler (1976) | 64 | 100 | NR | YES | Drinking |
| Liu, Chen, and Chow (1976) | 353 | 85 | NR | YES | Abortions |
| Locander, Sudman, and Bradburn (1976) | 233 | 60–78 | 48–90 | INC | i.e. drunken-driving |
| Zdep and Rhodes (1976) | 995 | 98 | 75–85 | YES | Childabuse |
| Berman, McCombs, and Boruch (1977) | 156 | 100 | 100 | INC | i.e. drugs |
| Fidler and Kleinknecht (1977) | 132 | 100 | 100 | YES | i.e. sex |
| Lamb and Stern (1978) | 312 | 96 | | | Examination results |
| Shimizu and Bonham (1978) | 9 797 | 99 | NR | YES[a)] | Abortions |
| Begin, Boivin, and Bellerose (1979) | 202 | 100 | 99 | INC[a)] | Various |
| Brewer (1981) | 187 | 96 | NR | INC | Drugs |
| Fox and Tracy (1981) | 410 | NR | NR | YES | Arrests |
| Cahalan and Ekstrand (1982) | 1 825 | NR | | YES | Tax cheating |
| Edgell, Himmelfarb, and Duchan (1982) | 54 | 100 | | | Various |
| Danermark and Vintheimer (1984) | 762 | 99 | 98 | INC | Drugs |

Note: The first column: The gross number of respondents involved in a survey using RR methods.
The second column: Cooperation, registered by crude response rate, for the randomized response sample.
The third column: The response rates for direct questions.
The fourth column: A judgement, based on the cited reference, as to whether the use of randomized response led to reduced bias in reporting by respondents.

NR: The information was not included in the report. INC: The results are inconclusive. YES: A clear reduction in bias, relative to a standard. A blank indicates that the study was not designed to provide the information.

a) A significant reduction of response bias was reported for 8 of 32 questions.

* This overview is essentially based on Table 3 in Boruch and Cecil (1979) supplemented with additional information, field studies and experiments.

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Received April 1987
Revised November 1987