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ORIGINAL ARTICLE

# Validation of self-reported start year of mobile phone use in a Swedish case–control study on radiofrequency fields and acoustic neuroma risk

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The possible effect of radiofrequency exposure from mobile phones on tumor risk has been studied since the late 1990s. Yet, empirical information about recall of the start of mobile phone use among adult cases and controls has never been reported. Limited knowledge about recall errors hampers interpretations of the epidemiological evidence. We used network operator data to validate the self-reported start year of mobile phone use in a case–control study of mobile phone use and acoustic neuroma risk. The answers of 96 (29%) cases and 111 (22%) controls could be included in the validation. The larger proportion of cases reflects a more complete and detailed reporting of subscription history. Misclassification was substantial, with large random errors, small systematic errors, and no significant differences between cases and controls. The average difference between self-reported and operator start year was − 0.62 (95% confidence interval: − 1.42, 0.17) years for cases and − 0.71 (−1.50, 0.07) years for controls, standard deviations were 3.92 and 4.17 years, respectively. Agreement between self-reported and operator-recorded data categorized into short, intermediate and long-term use was moderate (kappa statistic: 0.42). Should an association exist, dilution of risk estimates and distortion of exposure–response patterns for time since first mobile phone use could result from the large random errors in self-reported start year. Retrospective collection of operator data likely leads to a selection of "good reporters", with a higher proportion of cases. Thus, differential recall cannot be entirely excluded.

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## INTRODUCTION

Many studies on mobile phone use and risk of brain tumors have been conducted since the end of the 1990s. The vast majority of these had case–control designs with exposure assessment based on retrospective self-reported history of mobile phone use.[1–4] Prospectively collected exposure information was available in one case–control study[5] and two cohort studies.[6–9] Results have been conflicting. Most studies found overall risks around or below unity,[1–3] but a few studies reported large risk increases after only 1–4 years of mobile phone use.[10–12] Methodological differences have been suggested as a possible reason for the discrepancy.[1] The international INTERPHONE studies of glioma and acoustic neuroma found increased risk estimates in the highest decile (≥1640 h) of cumulative calling time. However, implausible reports of cumulative calling time and inconsistent exposure–response patterns — risk estimates were decreased in the 4th to 9th deciles — raised questions about recall bias.[2,3] Recall bias have also been indicated in a validation of number of calls and calling time in the INTERPHONE case–control study, where an increasing tendency to overestimate calling time and number of calls with time before interview was seen among cases but not among controls.[13] No study has yet validated self-reported information on the start year of mobile phone use in adults, information that is also of importance for estimation of cumulative calling time.

The epidemiological evidence on potential long-term effects of mobile phone use is still insufficient.[1,4,14] Empirical information about long-term recall of mobile phone use is very limited, and there is a lack of information concerning adult respondents' ability to report the start of mobile phone use. This is of particular concern when studying slow growing tumors like meningioma and acoustic neuroma, whose onset can occur many years before diagnosis.[15,16]

In this study, we validated information about mobile phone use history reported in postal questionnaires by cases and controls in a large Swedish population-based case–control study on mobile phone use and acoustic neuroma risk.[17] The primary aim was to evaluate long-term recall and possible differential recall between cases and controls. A second aim was to investigate the feasibility of using operator records for long-term retrospective exposure information ascertainment.

## MATERIALS AND METHODS

Network operator records were used to validate self-reported exposure data collected in a Swedish case–control study of mobile phone use and acoustic neuroma risk.

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### Case–Control Exposure Data

A detailed description of the identification of cases, selection of controls and data collection for the case–control study used for this validation has been published elsewhere.[18] Incident acoustic neuroma cases ($n = 542$) between 20 and 69 years of age at diagnosis were identified from September 2002 to August 2007 in Sweden. Controls ($n = 1095$) were randomly selected from the Swedish population register, matched on age, sex and health-care region. Of these, 451 (83%) cases and 710 (65%) controls participated. The controls were assigned a reference date that corresponded to the date of diagnosis of their matched case. Self-reported exposure information was collected through postal questionnaires, sent to cases and their matched controls simultaneously, starting in October 2007. The participants were asked whether they had ever been regular users of mobile phones, defined as having made or received on average at least one call per week during at least 6 months. Participants who reported regular use were asked what year they became regular users and approximate number of calls and calling time per unit time in predefined categories every 3 years starting from 1987, when handheld mobile phones became available in Sweden. A translated version of the questionnaire is available.[17]

### Self-Reported Subscription Data

To collect information about the participants' past and current mobile phone subscriptions (refers to both post-paid and prepaid mobile phone services), regular mobile phone users were asked to fill out a mobile phone calendar detailing information about start year, stop year, name of the operator, phone number and type of network (NMT, GSM or 3G) for all subscriptions they had used during the period they had been regular users of mobile phones. We also asked whether any of the subscriptions were written in someone else's name, or whether any of the subscriptions also had been used by someone else, and in that case, how much. The participants were asked whether they were willing to sign a written permission to allow us to request data about their subscriptions' start and stop date, number of calls and calling time from the mobile phone operators.

### Operator Data

The four major network operators (TeliaSonera, Tele2, Telenor, Tre) in Sweden during the study period were contacted with a request of collaboration to provide data for validation of self-reported use. Two of the operators (TeliaSonera and Telenor) could provide retrospective data about start date, stop date and traffic information for subscriptions held by participants who had given written permission, whereas the other operators had data stored in a format not easily accessible. TeliaSonera (formerly Televerket or Telia) was the only operator on the Swedish market until 1991 and still had the largest number of subscribers at the end of the study period (43% of the subscriptions in 2007). Telenor operated under a variety of names since 1992 as the third largest provider of mobile phone services (17% of the subscriptions in 2007).

Self-reported information about subscriptions from the mobile phone calendar and the personal identification numbers were sent to the network operators. The operators returned information about subscriptions' start and stop date together with traffic data aggregated by calendar month, to the extent it was available, for subscriptions that matched either the participant's personal identification number or self-reported phone numbers.

### Inclusion Criteria

Eligible for inclusion in the validation were participants who reported that they became regular users of mobile phones at least 1 year before reference date. Data were matched up to 1 year before the reference date.

### Matching of Questionnaire and Operator Information about Start Year of Mobile Phone Use

Both collaborating operators could provide information about subscriptions' start and stop dates for the whole period they had been active on the Swedish market. The participant's first reported subscription was compared with the earliest subscription in the operator data matching on either personal identification number or phone number. To reduce the risk of erroneous matching, data were linked only if the participant had specified one of the collaborating operators in the mobile phone calendar. Subscriptions with missing information about network operator were not matched. As an additional criterion, for making the assumption that the first reported subscription was referring to the one they used when they became a regular mobile phone user, the self-reported start of the first subscription in the mobile phone calendar had to match the self-reported start of regular mobile phone use.

### Validation of Number and Duration of Calls

Owing to the implementation of new legislation regarding the duration of storage of mobile phone traffic data, traffic data from TeliaSonera were not available to the extent that was expected before study start. Data on calls and calling time were available from March 2006 from TeliaSonera and from January 1999 from Telenor. Unequivocally linked overlapping periods of operator and self-reported information about frequency and duration of calls could be identified for only 6% of the participants (19 cases and 32 controls). This proportion was determined to be too small for valid results and we did not analyze these data further.

### Statistical Analyses

Self-reported start year of regular mobile phone use from the study questionnaires was compared with operator-recorded start year. Analyses were carried out for cases and controls separately to assess possible differences in recall.

To evaluate the agreement between categorized self-reported and operator-recorded time since first use, Kappa statistics were used. Time since first use was categorized in short ($< 5$ years), intermediate (5–9 years) and long-term use ($\geq 10$ years) in accordance with the analyses of time since first use of mobile phones in most previous studies on mobile phone use and cancer risk. Participants who started mobile phone use within 1 year before the reference date or later were considered unexposed and were not eligible for validation.

Agreement between self-reported and operator-recorded start year as continuous variables was estimated through calculations of the arithmetic mean, standard deviation, median and interquartile range of the error, defined as the difference between self-reported and operator-recorded start year. Positive values reflect self-reported start year of mobile phone use later than the operator-recorded start and negative values reflect earlier self-reports than the operator-recorded. Sensitivity analyses were performed, calculating these measures stratified by type of matching, that is, matched by the personal identification number (where the phone number may or may not match) or by phone number only (i.e., phone numbers for which the personal identification number did not match, foe example, unregistered prepaid cards, corporate phones, or if someone else had signed the subscription contract).

To assess the dependence of the error on the start of mobile phone use, operator-recorded start was plotted against self-reported and a linear regression model was fitted to the data. In addition, Bland-Altman analyses, plotting the difference between self-reported and operator-recorded data against the mean of the two, were used. A Pearson's correlation coefficient was calculated for the error with self-reported and operator-recorded data separately.

To analyze the dependence of the error on the total time between start of mobile phone use and filling out the questionnaire, the median and interquartile range of the ratio of self-reported to operator recorded time was stratified by short ($< 5$ years), intermediate (5–9 years) and long-term ($\geq 10$ years) use, as defined by self-reported and operator-recorded data, respectively.

In addition, we calculated the proportions of regular users who had reported an unrealistic start year of regular mobile phone use, that is, earlier than 1987 (when handheld mobile phones became available in Sweden) stratified by case–control status and by inclusion in the validation.

## RESULTS

We were able to validate self-reported start year for 96 cases and 111 controls out of the 326 cases and 505 controls who reported regular mobile phone use at least 1 year before the reference date. A significantly larger proportion of cases (29%) than controls (22%) could be included in the validation. The cases more often than controls gave permission to contact the network operators (87% compared with 77%), filled out the mobile phone calendar (97% and 93%, respectively) and entered a complete phone number on the first subscription (45% compared with 39%). A larger

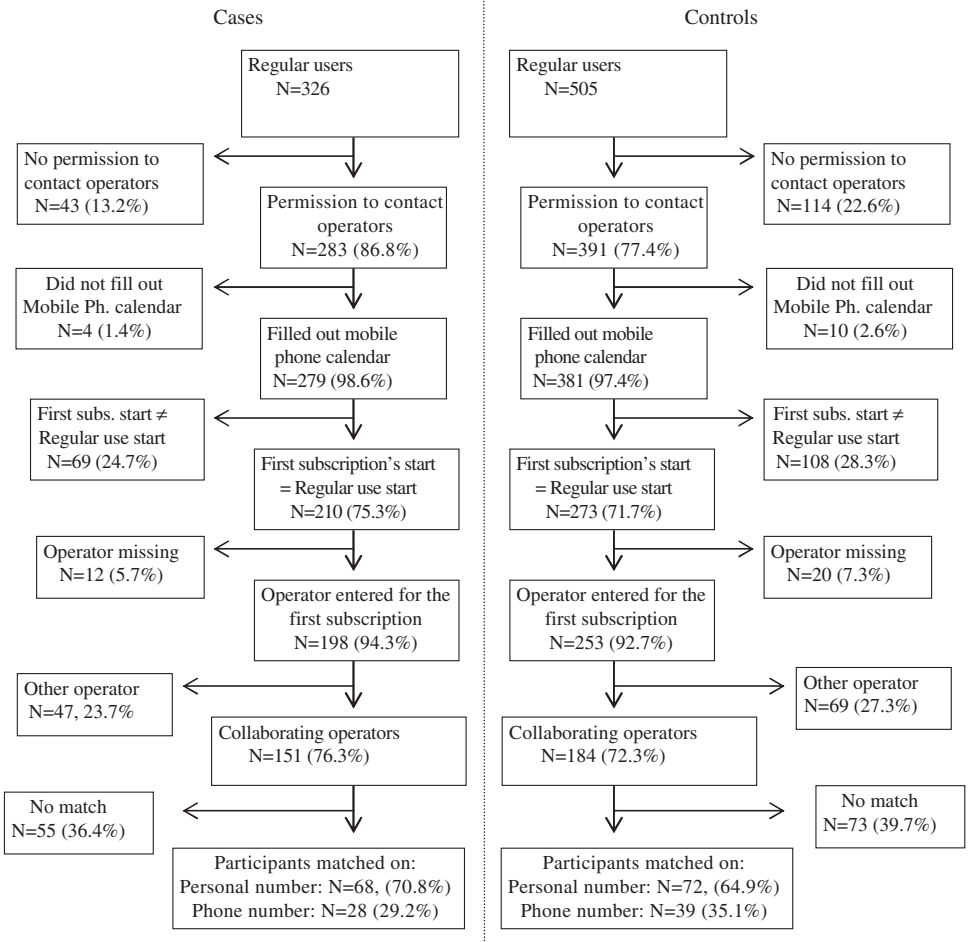


**Figure 1.** Flow chart of the validation process.

proportion of the controls were lost at each step of the validation process. Figure 1 shows a flow chart of the process of matching the two data sources.

Demographic characteristics differed only slightly between participants who could be included in the validation and the other regular mobile phone users. Participants reporting intermediate-term use (5–9 years) were overrepresented among both cases and controls who could be included in the validation (Table 1).

The agreement between self-reported and operator-recorded data categorized in $< 5$ years, 5–9 years and $\geq 10$ years since the first use of mobile phones was moderate with 63% correct classification according to operator data for cases and controls combined, and an overall kappa statistic of 0.42 (95% CI 0.32–0.52), differing slightly between cases and controls (Table 2). According to operator data, 55% of the participants included in the validation started mobile phone use 5–9 years before reference date, whereas according to self-reported data, the proportion was 44%. This net flow from the intermediate category to the short- and long-term categories was more pronounced among controls than among cases (Table 2).

The average difference between self-reported and operator-recorded start year was − 0.62 (95% CI − 1.42, 0.17) years for cases and − 0.71 (95% CI − 1.50, 0.07) years for controls. The standard deviation of the error was 3.92 (95% CI 3.43, 4.57) and 4.17 (95% CI 3.68, 4.80) years for cases and controls, respectively. The 10th and 90th percentiles of the error distribution were − 7 and 3 years for cases and − 6 and 4 years for controls (not shown). There were no statistically significant differences between cases and controls. The sensitivity analyses revealed no apparent differences between data matched by personal identification number (where the phone number may or may not match) and phone number (Table 3).

In a linear regression model, the variation in self-reported start year explained about 20% of the variation in operator-recorded start year, slightly higher for the cases ($R^2 = 0.22$) than for the controls ($R^2 = 0.18$) but the difference was not statistically significant. The fitted regression lines differed considerably from the identity line (Figure 2). The Bland-Altman analyses showed a positive correlation between the difference of self-reported start and operator-recorded start, and the mean of the two measures, which suggested a tendency for positive errors when reporting late start years and negative errors when early start years are reported (Figure 3). The pattern was slightly stronger for controls than for cases, but the difference was not statistically significant. The error, expressed as the difference between self-reported and operator recorded start year, was strongly and positively correlated with the self-reported start year (Pearson's correlation coefficient: 0.785), whereas a weak negative correlation was observed with operator-recorded start year (Pearson's correlation coefficient: − 0.208).

The median and interquartile range of the ratio of self-reported to operator-recorded time from start of mobile phone use to filling out the questionnaire was 1.00 (0.83, 1.16) for cases and 1.00 (0.87, 1.20) for controls. The median ratio was higher for self-reported long-term users and lower for self-reported short-term users. This

**Table 1.** Characteristics of participants who could be included in the validation in relation to all participants and proportions of participants included, regular users only.

| | *Cases* | | | *Controls* | | |
|---|---|---|---|---|---|---|
| | *All data no. (%)* | *Included no. (%)* | *Proportion included %* | *All data no. (%)* | *Included no. (%)* | *Proportion included %* |
| *Basic characteristics* | | | | | | |
| All regular users[a] | 326 | 96 | 29 | 505 | 111 | 22 |
| Women (%) | 128 (39) | 36 (38) | 28 | 187 (37) | 43 (39) | 23 |
| Men (%) | 198 (61) | 60 (62) | 30 | 318 (63) | 68 (61) | 21 |
| Age at reference date, mean (SD) | 51.0 ± 11.3 | 50.8 ± 12.6 | | 50.6 ± 12.0 | 52.0 ± 11.2 | |
| 20–39 years (%) | 63 (19) | 22 (23) | 35 | 105 (21) | 22 (20) | 21 |
| 40–49 years (%) | 75 (23) | 19 (20) | 25 | 109 (22) | 19 (17) | 17 |
| 50–59 years (%) | 102 (31) | 26 (27) | 25 | 158 (31) | 38 (34) | 24 |
| 60–69 years (%) | 86 (26) | 29 (30) | 34 | 133 (26) | 32 (29) | 24 |
| *By self-reported data* | | | | | | |
| Time since first use | | | | | | |
| < 5 years | 89 (27) | 25 (26) | 28 | 154 (30) | 33 (30) | 21 |
| 5–9 years | 128 (39) | 46 (48) | 36 | 171 (34) | 45 (41) | 26 |
| ≥ 10 years | 109 (33) | 25 (26) | 23 | 180 (35) | 33 (30) | 18 |

[a]Includes also participants who reported that they always used hands-free.

**Table 2.** Cross-tabulation of time since first regular mobile phone use between operator data and self-reported data.

| *Self-reported data* | *Time since first regular use — Operator data* | | | *Total* | *%* |
|---|---|---|---|---|---|
| | < 5 years | 5–9 years | ≥ 10 years | | |
| *All participants* | | | | | |
| < 5 years | 33 | 23 | 2 | 58 | 28.0% |
| 5–9 years | 11 | 70 | 10 | 91 | 44.0% |
| ≥ 10 years | 9 | 21 | 28 | 58 | 28.0% |
| Total | 53 | 114 | 40 | 207 | |
| % | 25.6% | 55.1% | 19.3% | | |
| *Cohen's kappa coefficient: 0.42 (95% CI: 0.32: 0.52)* | | | | | |
| *Concordance: 131/207 = 63%* | | | | | |
| *Cases* | | | | | |
| < 5 years | 14 | 10 | 1 | 25 | 26.0% |
| 5–9 years | 5 | 34 | 7 | 46 | 47.9% |
| ≥ 10 years | 3 | 8 | 14 | 25 | 26.0% |
| Total | 22 | 52 | 22 | 96 | |
| % | 22.9% | 54.2% | 22.9% | | |
| *Cohen's kappa coefficient: 0.43 (95% CI: 0.28: 0.58)* | | | | | |
| *Concordance: 62/96 = 65%* | | | | | |
| *Controls* | | | | | |
| < 5 years | 19 | 13 | 1 | 33 | 29.7% |
| 5–9 years | 6 | 36 | 3 | 45 | 40.5% |
| ≥ 10 years | 6 | 13 | 14 | 33 | 29.7% |
| Total | 31 | 62 | 18 | 111 | |
| % | 27.9% | 55.9% | 16.2% | | |
| *Cohen's kappa coefficient: 0.41 (95% CI: 0.28: 0.55)* | | | | | |
| *Concordance: 69/111 = 62%* | | | | | |

dependence was not seen when stratifying on operator-recorded time since first use (Table 4). The mean time between reference date and interview was 3.2 and 3.3 years for cases and controls, respectively, for the participants included in the validation.

The proportion of regular users in the case–control study who self-reported an unrealistic start year of regular use of handheld mobile phones before 1987 was 8.1% (7.4% of cases and 8.5% of controls). The proportion among the participants included in the validation was lower: 5.8% (6.2% of cases and 5.4% of controls), whereas the study participants who were not included more frequently reported start years before 1987: 8.8% (7.8% of cases and 9.4% of controls).

## DISCUSSION

This is to our knowledge the first study to provide information about the validity of self-reported information of the start year of mobile phone use, and possible recall differences between cases and controls in adults. We found modest systematic errors and large random errors. Misclassification from the intermediate category of time since start of mobile phone use to the short- and long-term categories was common. No statistically significant differences were observed between cases and controls for any of the evaluated recall error characteristics, although larger errors or misclassification rates among the controls were found in all analyses. A smaller proportion of controls than cases provided sufficiently detailed information about their first subscription to allow validation with operator data.

Lack of sufficient data hindered further analyses on duration and frequency of calls. Adequate data were unavailable because of missing self-reported data about subscriptions, lack of information from operators, or because participants did not give permission to contact the operators. The proportion of participants whose data on duration and frequency of calls could be validated was judged too small for valid results.

### Comparison with Earlier Studies

We could validate approximately one-quarter of the regular users' self-reported start years of regular mobile phone use. This is similar to the proportion in two earlier studies that used operator data to validate case–control data.[13,19]

In a validation study of self-reported time since first mobile phone subscription conducted in a case–control study of children and adolescents, systematic overreporting and large random errors were found, but no difference in recall between cases and controls.[19,20] Median and interquartile range for the ratio between self-reported and operator-recorded start year was 1.25 (0.98–2.40) for cases and 1.28 (0.98–2.37) for controls.

**Table 3.** Univariate analyses of the difference between self-reported and operator-recorded start year of regular mobile phone use divided on case–control status and method used to match data.

| *Type of Matching* | *N* | *Mean (95% CI)* | *STD (95% CI)* | *Median (IQR)*[a] |
|---|---|---|---|---|
| *All matches* | | | | |
| All | 207 | −0.67 (−1.23, −0.12) | 4.05 (3.69, 4.48) | 0 (−2, 2) |
| Cases | 96 | −0.63 (−1.42, 0.17) | 3.92 (3.43, 4.57) | 0 (−2, 2) |
| Controls | 111 | −0.71 (−1.50, 0.07) | 4.17 (3.68, 4.80) | 0 (−3, 1) |
| *Matched on PID*[b] | | | | |
| All | 140 | −0.90 (−1.58, −0.22) | 4.09 (3.66, 4.63) | 0 (−2.5, 1) |
| Cases | 68 | −0.99 (−1.93, −0.04) | 3.89 (3.33, 4.68) | 0 (−2, 1) |
| Controls | 72 | −0.82 (−1.83, 0.19) | 4.29 (3.69, 5.14) | 0 (−3, 1) |
| *Matched on phone numbers*[c] | | | | |
| All | 67 | −0.19 (−1.16, 0.77) | 3.93 (3.37, 4.75) | 0 (−1, 2) |
| Cases | 28 | 0.25 (−1.27, 1.77) | 3.91 (3.09, 5.33) | 0 (−1, 2) |
| Controls | 39 | −0.51 (−1.80, 0.78) | 3.98 (3.25, 5.12) | 0 (−1, 2) |

[a]IQR: interquartile range (1st, 3rd quartiles). [b]Matched on Swedish personal numbers (phone number may or may not match). [c]Matched on phone number (where the user was not the owner of the subscription or had used an unregistered prepaid card).

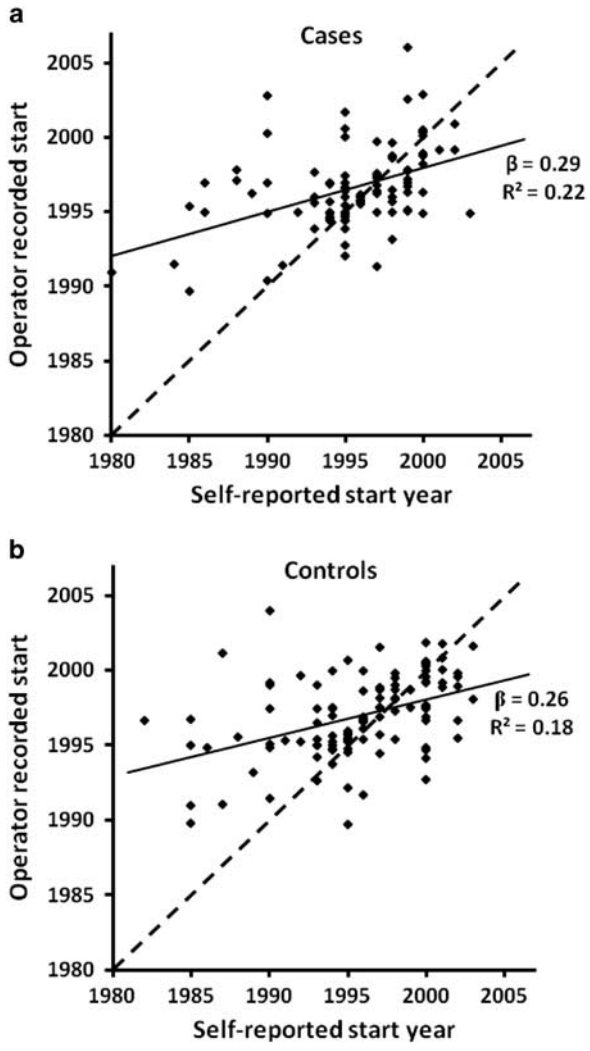


**Figure 2.** Operator start plotted against self-reported start year for cases (a) and controls (b). The solid lines depict the least squares regression lines. The dashed lines indicate equal values to illustrate how the data were actually used.

Studies validating self-reported information about number and duration of calls also found large random errors, and typically, systematic underreporting of number of calls, and overreporting of calling time.[13,19,21–27] Validation of case–control exposure information from a subset of the INTERPHONE study found no overall difference between cases and controls; however, cases tended to increase their overreporting of amount of mobile phone use for more distant time periods.[13] The agreement between categorized recalled information and actual data on number and duration of calls in the INTERPHONE validation studies (weighted kappa statistic ranged 0.45–0.50)[13,21] is similar to that observed for the start of mobile phone use in this study.

### Limitations of the Study

Retrospective use of operator-recorded data requires the ability of study participants to remember details like old mobile phone numbers, network operators and ownership of subscriptions in order to make reliable linkage between operator-recorded data and self-reported information. Therefore, it is likely that the validation of self-reported mobile phone history in this study, as well as in previous studies, is based on a selection of good reporters. Less frequent reporting of unrealistic start years (before 1987) among the participants included in the validation (5.8%) than among the rest of the study participants (8.8%) supports this assumption. A larger proportion of the answers from cases could be included in the validation compared with the controls, reflecting the fact that a smaller proportion of the controls met the level of quality of the reporting that is required to make reliable matching.

Retrospective collection of operator data also requires the study participants' permission to request data from the network operators, and that the latter are willing and able to share them. In our study, a high proportion of participants, higher among cases than controls, gave permission.

Errors in reporting of operator, phone number or ownership of the first subscription of regular use could lead to erroneous linking to subscriptions in the operator records. Reuse of phone numbers by the operators may have occurred for GSM phones, especially before September 2001, when full mobile phone number portability legislation was implemented in Sweden. However, sensitivity analyses stratified on method used for linkage of the data (by personal identification number or by phone number), revealed only small and non-statistically significant differences in the mean and variance of the error.

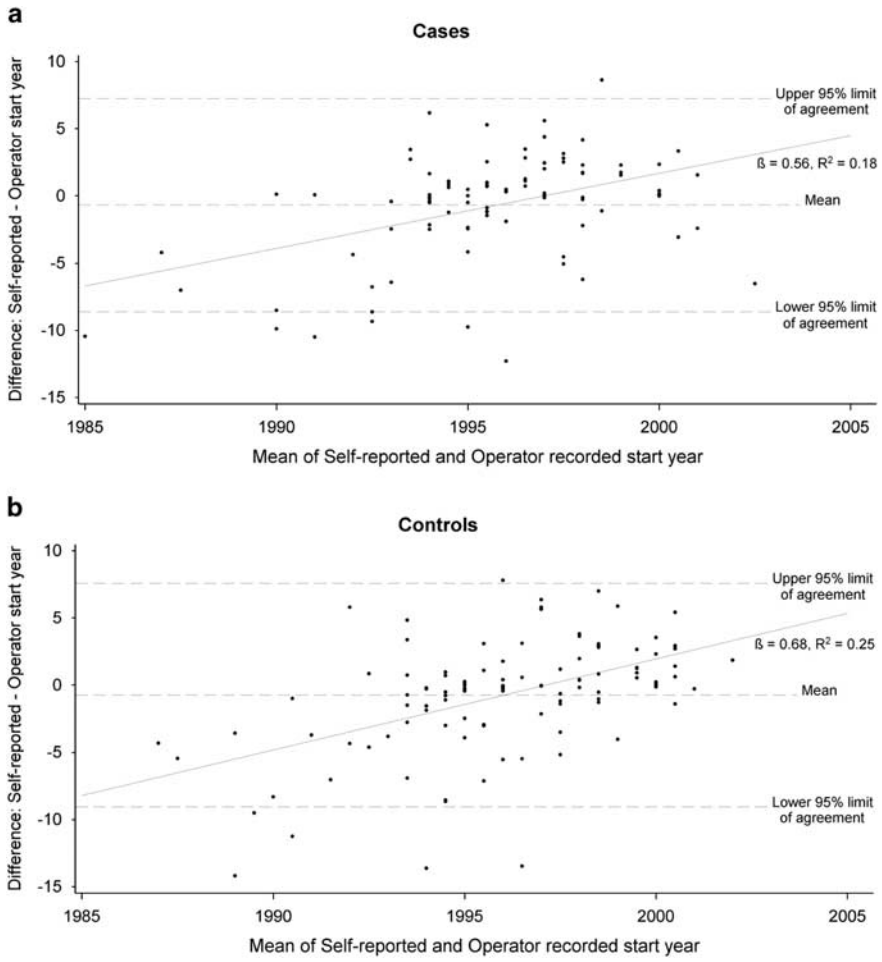


**Figure 3.** Bland-Altman plots showing the difference between self-reported start year and operator-recorded start year plotted against the mean of self-reported and operator-recorded start year for cases (a) and controls (b). The least-square fit (solid line) and the 95% limits of agreement (dashed lines) are reported in each panel.

**Table 4.** Ratio of self-reported to operator-recorded time between start year of regular use and year of filling out questionnaire.

| | *Cases* | | *Controls* | |
|---|---|---|---|---|
| | *No.* | *Ratio median (IQR)*[a] | *No.* | *Ratio median (IQR)* |
| All included in the validation | 96 | 1.00 (0.83, 1.16) | 111 | 1.00 (0.87, 1.20) |
| *By self-reported time since first use* | | | | |
| < 5 years | 25 | 0.80 (0.75, 1.00) | 33 | 0.87 (0.67, 1.00) |
| 5–9 years | 46 | 1.00 (0.91, 1.08) | 45 | 1.00 (0.92, 1.08) |
| ≥ 10 years | 25 | 1.24 (1.00, 1.58) | 33 | 1.27 (1.15, 1.64) |
| *By operator recorded time since first use* | | | | |
| < 5 years | 22 | 1.00 (1.00, 1.71) | 31 | 1.00 (0.87, 1.57) |
| 5–9 years | 52 | 1.00 (0.82, 1.09) | 62 | 1.00 (0.82, 1.10) |
| ≥10 years | 22 | 1.00 (0.92, 1.14) | 18 | 1.11 (0.81, 1.29) |

[a]IQR: interquartile range (1st, 3rd quartiles).

For the above-mentioned reasons, operator-recorded data are not likely to be a perfect gold standard when used retrospectively and errors from possible incorrectly linked data would, if present, on average add to the random error observed, whereas a selection of good reporters would decrease the observed error level. Most of the problems encountered when using operator data for retrospective validation can be avoided when data are collected prospectively and operator data could be used effectively to avoid exposure misclassification.

Lastly, an important limitation, which this study has in common with previous validation studies, was the inability to validate the self-classification into regular *vs* non-regular user. This, and previous studies, have asked follow-up questions on subscriptions, phone numbers, etc. only if the subject classified him/herself as a regular user. Considering that controls more often reported less complete information about their subscriptions, and spent less time on the questionnaire than cases, there is a possibility that controls also are less prone to regard themselves as a regular user, especially as an affirmative answer would lead to an extensive number of follow-up questions. We do not, however, have empirical data to investigate this assumption.

### Implications for the Interpretation of Studies of Mobile Phone Use and Health Outcomes

The results of this study may be generalized to case–control studies of mobile phone use and other health outcomes. However, caution is warranted when generalizing the results to studies of outcomes that may impair memory, or when other cognitive functions may be limited, for example, glioma[28] or Alzheimer's

disease and to studies that use personal interviews for data collection. The data collection method (mailed questionnaires) may also influence reporting, which should be considered when comparing to interview-based studies.

In the presence of an effect of radiofrequency exposure on disease risk, the random errors in reported start year that was observed in this study would on average dilute effect estimates between the extreme categories (long-term use *vs* non-regular use), whereas the impact on exposure–response relationships is more unpredictable — effect estimates for the intermediate categories could even be biased away from the null.[29–31]

Bias away from the null can occur if measurement errors are correlated with the true value.[32] However, although we found a strong correlation between the error and self-reported start year, the correlation between the error and the operator-recorded start year was very weak.

If recall error characteristics differ between cases and controls, estimates can be biased away from the null even in the absence of an effect.[30,31] Use of postal questionnaires allows a large variation in the effort spent to report exposure information and participants have the opportunity to consult documents like contracts or old phone bills, or to discuss their past mobile phone use with relatives and friends. The median time to fill out the questionnaire was 50% longer for cases than controls, implying a greater effort spent by the cases. Although none of the differences between the cases and controls included in the validation was statistically significant, we observed a consistent pattern of more accurate reporting and less misclassification among cases compared with controls. Such differences could lead to relative deficiency or surplus of controls in specific categories and bias effect estimates accordingly regardless of the existence of an effect. However, more accurate reporting among cases might not be expected for outcomes where impaired memory could be a complication.

We found no indications of differential systematic errors in the self-reported start year for the population that could be included in the validation. However, differential systematic recall errors among the respondents who could not be validated cannot be ruled out, especially considering the probable selection of good reporters in the validation.

Finally, reporting errors in the start of mobile phone use will also add error to the cumulative number of calls and called time measures, decreasing the accuracy of these estimates.

### Conclusion

Substantial random errors and modest systematic errors were found in the self-reported information about the start year of mobile phone use when compared with operator data. No statistically significant differences were observed between cases and controls who could be included in the validation, but a consistent pattern of better reporting and less misclassification was observed among cases compared with controls. Considering the small proportion and small numbers of cases and controls who could be validated, we cannot, however, exclude the possibility of recall bias. Should a true association between mobile phone use and disease exist, large random errors in reported start year would on average dilute risk estimates and possibly distort exposure–response patterns for time since first mobile phone use. Operator information about long-term mobile phone history is likely to be obtained for a relatively small proportion of study subjects in retrospective exposure ascertainment, and selection of cases and good reporters are expected. We recommend the use of prospectively collected data to avoid potential recall bias, and prospectively collected operator data to avoid extensive exposure misclassification of amount of phone use. There is, however, no effective measure available to reduce non-differential exposure misclassification of the start year of regular mobile phone use as most subjects will already have started to use mobile phones in future studies of health effects from radiofrequency fields emitted by mobile phones.

## CONFLICT OF INTEREST

MF is co-investigator of the COSMOS cohort study, funded by the Swedish Research Council, the Swedish Council for Working Life and Social Research, AFA Insurance and VINNOVA (The Swedish Governmental Agency for Innovation Systems). VINNOVA received funds for this purpose from TeliaSonera, Ericsson AB and Telenor. The provision of funds to the COSMOS study investigators via VINNOVA is governed by agreements that guarantees COSMOS' complete scientific independence. MF is vice chairman of the International Commission on Non-Ionizing Radiation Protection, an independent body setting guidelines for non-ionizing radiation protection. She serves as an advisor to a number of national and international public advisory and research steering groups concerning the potential health effects of exposure to non-ionizing radiation.

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