Title

Separating Interviewer and Sampling-Point Effects

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Abstract:

Data used in nationwide face-to-face surveys are always collected in multistage cluster samples. The relative homogeneity of the clusters selected in this way can lead to design effects at the sampling stage. Interviewers can further homogenize answers within the sampling points. The study presented here was designed to separate between interviewer effects and sampling-point effects using interpenetrated samples for conducting a nationwide survey on fear of crime. Even though one might, given the homogeneity of neighborhoods, assume that sampling-point effects would be especially strong for questions related to fear of crime in one's neighborhood, we found that, for most items, the interviewer was responsible for a greater share of the homogenizing effect than was spatial clustering. This result can be understood if we recognize that these questions are part of a larger class of survey questions whose subject matter is either unfamiliar to the respondent or otherwise not well anchored in the mind of the respondent. These questions permit differing interpretations to be elicited by the interviewer.

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1 Introduction

All nationwide face-to-face surveys are conducted in multistage clustered samples, which means that respondents are clustered within small geographical areas (or sampling points). The decision to use a clustered sample (and not a simple random sample) is made for organizational and financial reasons. For example, the absence of a general population register in many countries reduces many researchers' ability to use simple random samples. Sampling in several sampling stages, one of them at the level of small geographical clusters, allows for the selection of respondents without the aid of register data, for example with the help of random walk techniques. At the same time, interviewer travel expenses can be minimized. Clustered samples are therefore considered cost-efficient alternatives to simple random samples (Groves 1989, Behrens/Löffler 1999).

There is, however, a downside to the use of cluster samples: Conventional computation of standard errors and common test procedures are based on the assumption of independent and identically distributed observations, an assumption that is violated with cluster samples. One measure of the effect of the violation of this assumption is the so-called design effect, or the ratio of the variance of an estimator for a given sample design to the variance of an estimator for a simple random sample (Kish 1995, p.56). The use of simple-random-sample formulas in the computation of significance tests and confidence intervals leads to misleading results if this design effect is greater than one, in which case standard errors will be underestimated. This problem affects not only significance tests and the computation of confidence intervals for univariate statistics, but also regression analysis, analysis of variance and goodness–of-fit tests (Biemer/Trewin 1997, p. 608-624).

A number of techniques are available to estimate correct standard errors (Kish 1965, Kish/Frankel 1974, Wolter 1985, Shao 1996), among them Taylor linearization and jackknife procedures. The most intriguing way to explain the presence of design effects is, however, Kish's ANOVA model (Kish 1965, p. 162).

$$deft^2 = 1 + roh(b\text{-}1). \qquad (1)$$

In the above equation, *roh (rate of homogeneity)* represents the intra-class correlation coefficient, and *b* the number of interviews conducted within the cluster. "If the variable is distributed completely at random among the clusters, then we expect a *roh* of zero, and the design effect [1 + *roh*(*b*-

1)] =1" (Kish 1965, p.163). *Roh* is equal to 1.0 if all elements within a cluster have the same value for a given variable. Since different questions result in different answer patterns, design effects need to be calculated on a question-by-question basis.

There is an increasing recognition of the need to adjust the confidence intervals of survey variables for the *deft*. For example, in the last several years, an increasing number of statistical packages, such as SAS, SPSS or Stata have been equipped to provide correct variance estimation for different kinds of sampling designs. The size of the *deft* reported by various researchers has shown substantial variation among surveys and variables (Kish 1995, p. 60-61, O'Muircheartaigh/Campanelli 1998, p. 68). However, the rule of thumb employed in the survey literature is to assume a value for *deft* of about 1.4 (see, for example, Scheuch 1974, p. 39, Groves 1989, p. 272).

Adjusting test statistics for a design effect of 1.4 is, however, comparable to cutting the sample size almost in half, since the variance of an estimator based on the data from a complex sample has the same variance the estimator would have had a simple random sample of size *n* been adjusted by the *deft* squared.

$$n^{*}=n/deft^{2} \tag{2}$$

A larger sample size is therefore required to obtain the same precision as one would have had with a simple random sample, i.e. to compensate for the increase in standard errors incurred through the use of a cluster sample. However, reducing the costs of conducting the survey is part of the reason for a employing a cluster sample in the first place. So, the question of what steps can be taken in advance to reduce design effects naturally arises, and with it the question of the sources of design effects.

1.1 *Sources of homogeneity*

Design effects result from the relative homogeneity of respondents who belong to the same cluster. At least two mechanisms are responsible for producing this relative homogeneity. First, respondents are more homogenous within a cluster than in the population as a whole, whatever mode of data collection is used to obtain their survey answers. Second, the process of data collection itself can lead to an increased homogeneity within a cluster.

The first mechanism is termed spatial homogeneity. As a consequence of social processes such as self-segregation or imposed segregation according to income, age or ethnicity, respondents who live in the same geographical areas (sampling points) are often more similar to each other than respondents selected at random from the population as a whole (Lee/Forthofer/Lorimor 1989, p.15). There is also good reason to assume that people who live in small spatial clusters share similar attitudes because of the similar circumstances in which they live (McPherson et al. 2001). Whatever the cause of this similarity among respondents, *roh* measures the homogeneity in terms of the proportion of the total element variance that is due to group membership (Kish 1965, p. 163). If design effects are due entirely to the spatial clustering of similar people living next to each other, researchers interested in estimates with small variance may have no choice but either to give up the use of a cluster sample or to increase their sample sizes.

There is, however, a second mechanism at work in the creation of design effects, namely the data collection process. The use of a multistage cluster design involves not only the clustering of respondents, but - given the way survey research is conducted in practice - the assignment of a different interviewer to each of those respondent clusters. This interviewer may exert a further homogenizing effect within the sampling point in which he or she is active. Put somewhat differently, the interviewer could introduce an additional source of variance to the population estimators. This effect is most likely the result of the deviation of interviewer behavior from standardized procedures (Fowler/Magnione 1990, p. 28, Fowler 1991, p. 259). Interviewer technique may differ from proscribed rules in a unique way, for example with idiosyncratic interpretations of questions, incomplete reading of answer categories or incorrect probing. In addition, some interviewer characteristics can trigger social-desirability mechanisms (Schnell 1997:277).

Not all questions are equally likely to be affected (Gales/Kendall 1957, Gray 1956, Hanson/Marks 1958). Researchers have tried to address the issue of questions affected by interviewers in various ways and with varying results. Results from Hyman et al. (1954), Fellegi (1964), O'Muircheartaigh (1976), and Collins and Butcher (1982), as well as those reported in Belak and Vehovar (1995) support the hypothesis that *factual items* are less vulnerable to interviewer effects than *attitude items*. This finding was not, however, replicated by Kish (1962) and Groves/Magilavy (1986, p. 260) and O'Muircheartaigh/Campanelli (1998, p. 69). One possible explanation for this discrepancy is provided by Cannell (1954, discussed in Kish 1962), who suggested that interviewer effects occur if respondents are forced to answer questions about *unfa-*

miliar topics, especially in cases where respondents try to provide a correct or reasonable answer but they do not know which answers might be correct or reasonable. In those cases, respondents tend to use signals and help provided by the interviewer in order to gauge the appropriateness of their answers. Similarly, Hermann (1983), using the German ALLBUS 1980, found that interviewer effects were small if the item was unimportant to the interviewer and that they were large if the item was unimportant to the respondent. Interviewer effects were found for *difficult factual items* in the 1950 U.S. Census (Kish 1962, p. 96-97), where difficult factual items were defined as those that required the respondent to perform retrospective calculations or memory searches. Tilburg (1998) found large interviewer effects for questions on the respondent's personal network; these questions also belong to the group of questions that require memory search. However, data from Mangione et al. (1992) did not show larger interviewer effects for difficult items. Schnell (1997) reported that *sensitive items* that tend to evoke socially desirable answers are more likely to be affected by interviewer behavior. Bailar et al. (1977) and Fellegi (1964) showed for the National Crime Survey that items that evoke emotional reactions (for example, questions on criminal victimization) produce larger interviewer effects than questions on less emotional topics such as income or education. Gray (1956) and O'Muircheartaigh (1976) found larger interviewer effects for *open questions*, or questions for which no answer categories are provided, as those questions tend to include the interviewer in the answer process. In contrast, no support for this relationship was found subsequently by Mangione et al. (1992) or Groves and Magilavy (1986).

Given the results of the existing studies, a well-defined classification of questions into those that tend to be vulnerable to interviewer effects and those that do not is hardly possible. One reason might be that an unidimensional classification of items is not possible (see Section 2.2 below), but there may instead be a combination of factors responsible for the effects observed. For example, sensitive questions may call forth interviewer effects for a different reason than difficult estimation questions do. The inconsistency in the results may also be at least partly due to the fact that the design details of the surveys employed in the above studies are difficult to compare. Design details that might well have influenced the results but were not published for some of those studies included question format, question phrasing, interviewer training, number of interviews per interviewer, interviewer experience, and interviewer assignment. However, *one conclusion* seems to apply to most of those studies: Interviewer effects can be reduced if the interviewer has received good training, and if the use of a standardized procedure is ensured, as emphasized by Kish (1962), Groves and Magilavy (1986) or Fowler and Mangione (1990).

The strongest interviewer effects appear with incompletely standardized interview procedures (Hyman et al. 1954): If the interviewer is, for example, given latitude for decisions on the use of neutral answer categories (Collins 1980), the coding of answers (Rustemeyer 1977) or the phrasing of individual probing questions (Mangione et al. 1992). Such findings lead to the following hypotheses:

1) For the majority of questions, active interviewer involvement is responsible for most of the observed interviewer effect[2]. Active interviewer involvement is any deviation from proscribed interview protocol, such as reformulating the question or aiding in its interpretation. Interviewers may involve themselves in anticipation of an adverse reaction by the respondent or they may be prompted for help by respondent.
2) Active interviewer involvement in question clarification is more likely when additional interpretation and explanation of a question are requested by the respondent.
3) Respondents are likely to ask for clarification when
   (a) they do not understand the question wording,
   (b) they are asked to answer a factual question on a topic about which they know nothing,
   (c) ambiguous terms are used in a factual question or
   (d) the item is an attitude question and the respondents' attitudes are not salient.

As a consequence, one would assume that certain interviewer effects occur not just with a specific item type (attitude vs. factual, for example) but with a certain combination of item types or target populations and item types (for example, some item types may be problematic for certain subpopulations but not for others). The size of an interviewer effect therefore depends on interviewer characteristics, target population, question topic and question type. Here, we will concen-

[2] The interviewer effects most often described in the literature refer to a different process. These interviewer effects result from the reaction of the respondent to a perceived relationship between obvious interviewer characteristics (for example, race, age, sex or regional dialect) and the content of the question. No active interviewer involvement is required to produce such effects.

trate on the last two variables. Questions that are difficult for respondents or are perceived as such by interviewers will yield larger interviewer effects. To be more precise:

4) Larger interviewer effects are to be expected if either the topic of the question is unfamiliar or the answer is not well anchored in the mind of the respondent.

Such questions leave more room for differing interpretations to be elicited by the interviewer (Martin 1983, Fowler/Mangione 1990, Mangione et. al. 1992, Schnell 1997, Kreuter 2002).

1.2 *Interviewer – and Sampling-Point Effects*

Even though the potential of interviewers to amplify measurement errors was first addressed many years ago (Rice 1929) and there have been several warnings in recent decades about interviewer effects (Kish 1962, Hedges 1980, Hagenaars/Heinen1982, O'Muircheartaigh/Campanelli 1998), little has been done to reduce them in the practice of face-to-face surveys. This neglect of the role of the interviewer might be due to the fact that the combination of multistage cluster designs commonly used in survey practice and the interviewer assignment practices routinely employed do not allow for the determination of the relative effect sizes of the two sources.

Interviewer effects combine with the effect of spatial homogenization to produce the design effects observed for a multistage cluster sample. Given the way interviewer assignment functions in practice, these two contributions to the design effect are difficult to separate: Interviewers are usually assigned to one sampling point, and they seldom work in another sampling point unless they are employed in a large city in which interviewers can easily be sent out to help each other carry out the required number of interviews. Since interviewers generally canvas only one point and a given point is covered only by one interviewer, determining how much of the observed variance of a given estimator is due to spatial proximity or the interviewer is most often impossible (Hoag/ Allerbeck 1981, p.414, Groves 1989, p.271). Therefore, little is known about the separate contributions of the interviewer and the geographic area to the total design effect. In order to estimate the interviewer effect in geographically clustered face-to-face surveys, one must use interpenetrated samples (Bailar 1983, p.198-199). A simple version of this design would ensure that the addresses within each sampling point are randomly assigned to several interviewers working independently and that every one of these interviewers is assigned only to one sampling point (Biemer/Stokes 1985, p.159).

Only a few studies have been designed – with different kinds of interpenetrated samples – explicitly to estimate the relative impact of interviewer and sampling point on the design effect (Hansen 1961, Bailar et. al. 1977, Bailey et. al. 1978, Collins/Butcher 1982, Davis/Scott 1995, O'Muircheartaigh/Campanelli 1998). These have attempted to separate the effects of interviewer and sampling point on the variance in the answers to individual survey questions (i) observed for a given cluster ($\sigma_i^2{}_{cluster}$):

$$\sigma_i^2{}_{cluster} = \sigma_i^2{}_{Sampling\text{-}Point} + \sigma_i^2{}_{Interviewer} \qquad (2)$$

All of the above studies showed that at least part of the cluster variance (in answers to specific questions) within a given sampling point was due to the interviewer ($\sigma_i^2{}_{Interviewer}$). Knowing that interviewers play a role in producing design effects, one might ask how large this role is in survey practice.

More information on the magnitude of interviewer effects is available through from the analysis of data from a series of telephone surveys (e.g. Kish 1962, Gray 1956, Hanson/Marks 1958, Tucker 1983, Groves/Magilavy 1986, Heeb/Gmel 1999). Telephone surveys are employed for the study of interviewer effects since potential respondents from all sampling points – if there are even any defined – can easily be assigned to interviewers randomly. In the analysis of telephone survey data, respondents are clustered by interviewer. *Roh*, as it applies to interviewer effects, is answer homogeneity as the proportion of total element variance that is due to the interviewer. The average value of *roh* for these surveys is 0.01, and according to Groves (1989, p.318), values above 0.1 are seldom observed. However, a small value of *roh* in telephone surveys should not lead to the impression that interviewer effects do not lead to design effects. The average interviewer workload varies a great deal, and it is not unusual to find an interviewer workload in a telephone survey of between 60 and 70 interviews per interviewer (Tucker 1983, Groves/Magilavy 1986). Again using *Equation* (1) to compute the design effect, an interviewer workload of 70 interviews would lead to $deft^2$=1.69. Whatever the actual size of the interviewer effect for telephone interviews is, it is questionable whether the interviewer effects found with telephone surveys can be generalized to the face-to-face situation, where cost-efficiency forces survey institutes to employ cluster samples. The study presented here therefore represents an extension of the above studies in its attempt to separate the two sources of design effects for nationwide face-to-face interviews.

2 Data and method

The data presented are part of the DEFECT study on sampling errors and nonsampling errors in complex surveys[3]. It is the first nationwide sample to be carried out in Germany with an interpenetrated sampling design[4]. The same questionnaire was used to conduct five independent surveys in 160 sampling points. Four of the five surveys were conducted by professional survey institutes that are highly regarded for their good practices and reliability, while the fifth (the mail survey) was conducted by the DEFECT group itself. This design means that in each of these 160 sampling points, two face-to-face random surveys, a face-to-face survey with quota selection, a CATI random survey, and a mail survey were carried out concurrently (the use of several survey modes and sampling procedures has to do with the fact that this study was conducted as part of a methodological study with a much larger scope).

For each of the three face-to-face surveys, only one interviewer was active in each sampling point and no interviewer worked in more than one sampling point. This use of this design was intended to permit the statistical separation of interviewer and sampling-point effects, while adhering to the survey institutes' usual procedures. A cross-classified design, one in which one interviewer was assigned to more than one sampling point, would have been not only unusual but difficult to implement for a face-to-face survey. A telephone survey can be easily adapted to multi-sampling-point interviewer assignment, but the use of a cross-classified design in a face-to-face survey would be substantially more challenging. Such a design would have been reasonable only if the sampling points to which an interviewer were assigned had a substantial geographical separation, since assigning the same interviewer to neighboring points would not have allowed for a valid separation of sampling-point and interviewer effects. Assigning that interviewer to sampling

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[3] For details on the study, see Schnell/Kreuter (2000) and the project homepage (http://www.uni-konstanz.de/FuF/Verwiss/Schnell/DEFECT_home.html).

[4] To our knowledge, the only other nationwide survey that employed a design that may be considered an interpenetrating-sample design was the second wave of the British Household Panel 1991 (O'Muircheartaigh/Campanelli 1998), although the authors' description leaves some ambiguity concerning the design. As far as we understood their description, they used a subsample of 153 of their 250 PSUs to build PSU pairs or triples. However, this subsample does not appear to be a random sample of all their PSUs, because the PSU pairs/triples were required to be no more than 10 km apart. Of the 70 pools of PSUs formed in this way, a systematic sample of 35 pools was included in the interpenetrating-sample design, five of which were judged to be ineligible. Their study appears to differ from ours in several important ways: the size of the sample (35 vs. 160 PSUs), selection of PSUs (selection according to geographical proximity to other PSUs vs. random selection), and the geographical proximity of the interviewers (interviewers working independently in neighboring but different PSUs vs. in the same streets).

points in different regions would, however, have involved considerably higher travel and administrative expenses[5].

2.1 *Sampling*

The addresses of households to be contacted in these surveys were selected in a multistage procedure. In the first stage, 160 sampling points were randomly selected from a nationwide register of election districts. In the second stage, an address-random procedure was used to select households within the 160 election districts. The actual household selection was carried out by eight members of the research group, who personally visited each of the sampling points. Starting from a randomly selected address in each sampling point, the group members noted the address of every third household along the random-walk route until they had gathered 110 addresses. Of these 110 addresses, the first 64 (16*4) in each point were randomly assigned to the four random surveys mentioned above[6] using a shuffle procedure, which meant that a total of 2,560 addresses were initially sent to the survey institutes. Shortly afterward, four additional addresses were given to the institutes for each sampling point in order to replace the neutral dropouts they reported in the first round of surveys (for example, respondents who had either died or moved away since the address collection had been conducted). In some sampling points, even this number of addresses was not enough to permit a total of at least six realized interviews in all sampling points. In those cases, the institute received additional addresses for those points. However, the interviewers were committed to making at least four contact attempts at each of the original addresses before this fallback sample could be used[7].

The addresses received by the institutes were therefore household addresses, but the goal was an individual sample of the target population. That target population was defined as all German-speaking inhabitants of those households who were 18 years or older, one of whom was to be randomly selected for survey participation. The institutes themselves were responsible for this last sampling stage. In the face-to-face random surveys, the potential respondent was selected by

[5] It should be mentioned here that the participating survey institutes incurred a financial loss even in working with the sampling design actually employed.

[6] The quota survey did not employ the addresses collected during the random walk.

[7] For details see Schnell/Kreuter (2000).

the interviewer using a variant of a Kish grid. For the other two random surveys (CATI and mail), the potential respondent was selected using the last-birthday procedure.

In keeping with the desire to employ an interpenetrated sampling design, the general policy of this study was that only one interviewer should contact the households in a given point on behalf of a certain institute. In practice, this requirement could not be met by the institutes in every single sampling point[8]. The institutes were therefore allowed to assign a second interviewer to a point, but not in more than 10 percent of the sampling points. The survey institutes agreed to the stipulation that as soon as a second interviewer was assigned to a point, the first interviewer was to stop working there. This requirement, along with the procedures dictated by the original agreement, meant that at any given time there was only one interviewer from each institute working in each sampling point.

2.2 *Survey content and item classification*

The survey itself was on fear of crime. This topic was chosen for this methodological study for several reasons. First, it was intended to boost participation, since crime is a matter of at least some concern to a wide variety of people. Second, several kinds of questions can be asked on fear of crime (factual, attitudinal, sensitive, and so forth), the use of which allows for a comparison of design effects for different kinds of items. In constructing the questionnaire, we used both the well-established indicators typically used in fear of crime surveys and a set of items we developed ourselves (Kreuter 2002). All questions went through three pretest phases. In the first phase, we conducted a series of cognitive pretests to ensure that respondents understood the question wording, the use of the answer scales and the like. Unclear questions were rephrased and tested again. In the second phase, we conducted several additional rounds of pretests; in each of these rounds, five to twenty respondents filled out the complete paper-and-pencil version of the questionnaire. The questionnaire was evaluated and improved on the basis of both the behavioral coding conducted during the pretest and the answers to a number of probing questions asked af-

[8] For example, one of the interviewers refused to return to a certain sampling point after his hubcaps were stolen there during his first visit. Given the relevance of such incidents for fear of crime (the topic of the survey), sending a replacement interviewer was, of course, imperative.

terwards. In the third phase, the finalized questionnaire was tested in a large-scale telephone survey for question flow, question-order effects and completion time[9].

The final questionnaire contained 71 questions reflecting many different item types, with binary, categorical and continuous answer scales[10]. Given the fact that each question could contain multiple items, the questionnaire contained 135 items, if all filters are counted. Among them were questions on fear of crime itself, subjective victimization risk, prior primary and secondary victimization experience, and coping measures taken by the respondent, along with other questions on the respondent's housing situation, health, household composition and standard demographic characteristics.

Each of these 135 items was classified by four members of the research project into 16 item types derived from the classification scheme suggested by Mangione, Fowler and Louis (1992), which includes four dimensions: difficult/easy, sensitive/non-sensitive, factual/non-factual and open/closed. Each item was rated on all four dimensions. If the dimensions are considered individually, 84 items were classified as factual, 67 items as sensitive, 40 items as difficult, and 14 as open items. Items were classified as difficult if the respondent was asked to produce an answer that might tax his or her recall abilities (for example, the number of doctor's visits in the last three years) or if the respondent was confronted with a complicated issue about which he or she might have never thought before (for example, how often he or she had worried about victimization in the last two weeks). Items were classified as sensitive if a certain response could be seen as social desirable (for example, less fear for a fear-of-crime question) or if the question were unpleasant to answer (for example, previous victimization experience). Items were classified as factual if a check of the answers was, at least in theory, possible (for example, the presence or absence of home-security devices). Items were classified as open if no response options were read out loud to the respondent or if a numerical estimate was requested (for example, the subjective victimization probability in percent terms).

[9] A detailed documentation of the questionnaire development was created using a newly developed software tool called QDDS (questionnaire development documentation system, see Schnell/Kreuter (2001)). The application of this tool to the DEFECT questionnaire includes all versions of the questionnaire and can be viewed (in German) at http://esem.bsz-bw.de/sicher.

[10] The face-to-face interviewers in the DEFECT study were asked to answer additional questions about each respondent. Those questions are not used in the analysis presented here.

2.3 *Method*

The dependent variables in the analyses presented here are design effects (*defts*) and ratios of variances. *Deft*s are employed as a measure of the effect of using a complex sample design. For the current analysis, we used several techniques to compute the design effects: Taylor linearization, bootstrap, random groups, and jackknife procedures. The results reported here were produced using only the Taylor linearization, since the differences among the results produced by these techniques were negligible and, according to (Kish 1995, p.57), *defts* should be viewed as rough measures for large effects. For binary items, Taylor linearization provides a good approximation for design effects if they are not heavily skewed (Goldstein et al. 2000).

In a cross-classified design, interviewer and sampling-point effects could be compared by first computing design effects using the interviewer as the cluster-defining variable and then comparing those to design effects estimated using the sampling points as the cluster-defining variable. However, the DEFECT study design was not conceived of as a cross-classified model, but rather as nested hierarchical model. Therefore, a three-level model was used to estimate the relative impact of interviewer and sampling point. The respondents form the lowest hierarchical level, the interviewers the second level, and the sampling points the highest level in this model. The assumption is that the mean of the variable of interest can be estimated from a constant term k_{pi} for each interviewer within a sampling point and a random error e_{pir} for the respondents surveyed by each interviewer within every sampling point. The value for k_{pi} is estimated from a constant m and two random effects: a_p for the sampling points and b_{pi} for the interviewer, where p can take values from 1 to P (the total number of sampling points), i values from 1 to I (the total number of interviewers within any given sampling point, here a maximum of two), and r values from 1-R (the total number of respondents interviewed by the same interviewer within any given sampling point), and where a, b, e are independent of one another. The distribution of the random effects is assumed to be normal. For every non-binary item, the model specification is[11]:

$$y_{pir} = k_{pi} + e_{pir} \quad (4)$$

$$k = m + a_p + b_{pi} \quad (5)$$

[11] In the interests of clarity, we omit the item subscript in the following equations.

The model for each binary item is listed below, where *u* represents the random effect for the sampling points and *v* represents the random effect for the interviewers, and *u* and *v* are independent and *y* is conditionally independent given *u* and *v:*

$$P(y_{pir} = 1 | p_{pi}) = p_{pi} \quad (5)$$

$$logit(p_{pi}) = b + u_p + v_{pi} \quad (6)$$

A ratio that we will call R_I was computed from the resulting variance components (*a* and *b* in the linear case*;* and *u* and *v* for the binary case). R_I is the ratio of the variance component that is due to the interviewer (s_I^2) to the sum of the variance components due to the interviewer and to the sampling point ($s_P^2 + s_I^2$).

$$R_I = s_I^2 / (s_P^2 + s_I^2) \quad (7)$$

Computing R_I in this way allows for the comparison of the fraction of the variance due to the interviewer across all items.

In order to prepare the data for the analysis, several adjustments were made to the data set. First, answers were transformed for several variables. Non-ordinal categorical items with more than two categories were split up into several indicator variables and treated in the same way as all other binary variables. Ordinal attitude scales were treated as continuous. Similar procedures were used by both Davis and Scott (1995, p.42) and O'Muircheartaigh and Campanelli (1998, p.65). Second, in order avoid numerical estimation problems, highly skewed variables were excluded from the analysis; we considered binary items to be highly skewed if more than 90 percent of the answers to those items were in one of the two categories. Overall, 123 items were used for the analysis[12].

To ensure more or less equal cluster size, those sampling points were excluded in which the number of interviews conducted by either institute was fewer than six. That left 132 sampling points, in which 264 interviewers had interviewed a total of 2,280 respondents, for the analysis. Finally, only those surveys that employed exclusively the interpenetrated random-sample design

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[12] Of the original 132 variables, 14 binary variables were excluded due to skewness. Ten non-ordinal categorical variables were transformed into 86 binary indicator items, of which 58 were dropped since only a few respondents picked those answer categories, which made them highly skewed as well. Aside from the highly skewed binary items and indicators, twelve additional variables were excluded as well, two highly skewed non-categorical items, one whose answer categories unintentionally differed between the two face-to-face surveys, others because their answers were constrained by design (for example the "number of inhabitants in the sampling point") or because the answers

described above were analyzed here, which means that data from the quota and CATI surveys were excluded. For comparative purposes, data from the mail survey were analyzed as well, since any design effects found in the DEFECT mail survey can clearly be attributed to the homogenizing effect of spatial clustering. This is so because the DEFECT mail survey was conducted in the same sampling points and can, by definition, not have any interviewer effects.

Three estimation procedures were used to decompose the variance of the items from the combined face-to-face surveys into sampling point and interviewer components. First, iterative generalized least-squares algorithms were employed using MLwiN. Second, for binary variables Marginal Quasilikelihood (1-MQL) and Penalized Quasilikelihood (2-PQL), as implemented in MlwinN (Rasbash et al. 2000), were used. Third, since MQL and PQL tend to underestimate random effects in the case of smaller clusters (Rodriguez/Goldman 1995), numerical integration using adaptive Gaussian quadrature (AGQ) was used as well. AGQ is implemented in the *gllamm* module that is available for Stata (Rabe-Hesketh et al. 2002). For the items used in this analysis, the results obtained using the various techniques were very similar. The absolute difference in R_I between PQL and AGQ was below 0.15 for all but eight items, of which four did not achieve convergence with MQL/PQL. There were larger differences for the other four items; these four items were excluded from further analysis. For the remaining items, the average difference between PQL and AGQ was 0.035. A large difference was also obtained for one of continuous items depending on whether the *gllamm* or MlwiN estimation was used; the average difference for all other items was 0.03. The results presented below were obtained using the adaptive Gaussian quadrature for all 118 remaining items[13].

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were determined entirely by the interviewer contact strategy (for example, "day of the week on which the respondent was interviewed").

[13] Please contact the authors for copies of the Stata commands ("do files") used in the analysis.

3 Results

For both face-to-face surveys in the DEFECT study, the mean design effect for the 118 items in each survey is 1.39, with a standard deviation of .27 (and median values of 1.34 for one face-to-face survey and 1.36 for the other). The mean design effect computed for the pooled sample using sampling points as primary sampling units is 1.48 (median 1.43). These values correspond well to the above-mentioned rule of thumb.

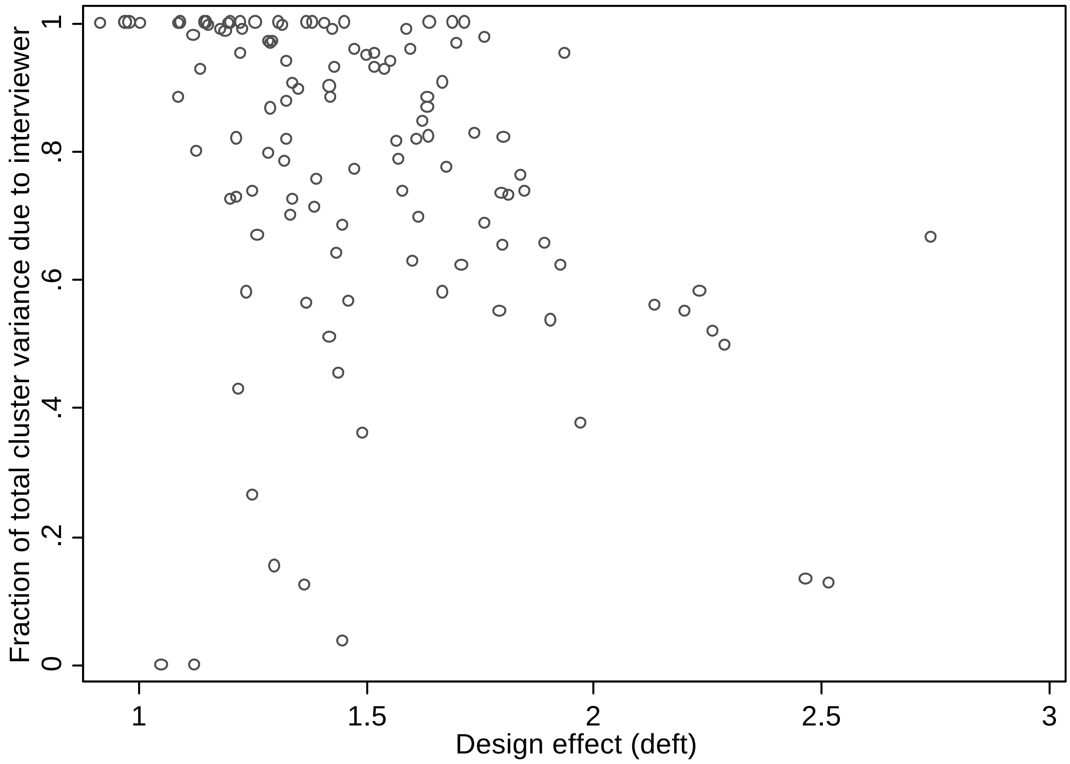


Figure 1: Fraction of interviewer variance and overall design effect for 118 items

Most of the design effects found with these data are attributable to the interviewers. After splitting up the variances obtained using a three-level hierarchical model, the relative share of the total variance due to the interviewer and sampling point combined varied around a mean of 0.77, and a median of 0.82. This means that, for those 118 items used in the DEFECT study on fear of crime, most items showed a larger relative share of interviewer-induced variance than geographical-clustering-induced variance. *Figure 1* depicts the design effects for all of the items used in the DEFECT study plotted against the interviewer share of the total variance (the variance due to both interviewer and the sampling point).

Given the usual (although mostly implicit) assumption of the importance of geographical clustering, this result is surprising. The plausibility of this result is, however, supported by evidence from two comparisons. First, we examine whether the estimated fraction of interviewer variance is higher for those items for which previous work would predict a greater susceptibility to interviewer effects. Second, we compare the size of the design effects to those estimated using the mail survey, since the DEFECT mail survey was conducted in the same sampling points and can, by definition, not have any interviewer effects.

3.1 *Assumed mechanisms for interviewer effects*

The first line of evidence concerns the theoretical plausibility of the size of the interviewer effects and design effects depicted in *Figure 1*. Items in *Figure 1* that have a very large overall design effect (we consider $deft > 2$ to be very large) are those that are closely related to the sampling point. These include items such as those on incivility (for example, the presence of abandoned houses or graffiti in the respondent's neighborhood), home ownership status and distance to the next train station[14].

Although these items share high overall design effects, the proportion of the total cluster variance that is due to the interviewer varies among the items. The large design effects observed for the home ownership indicator variables ($deft \sim 2.5$) are almost entirely attributable to the sampling point ($R_I \sim 0.13$). Incivility items, however, show a larger share of interviewer variance than sampling-point variance ($0.49 < R_I < 0.58$), which is also the case for the item on the distance to the next train station ($R_I = 0.67$). Among those items that show a very low overall design effect are questions on the number of household members or the respondent's victimization experience within the last twelve months. The (low) design effect for the former has almost no interviewer contribution. The small design effect for the latter is, however, almost entirely attributable to the interviewer.

[14] The careful reader might notice some items with design effects below one. This finding is, although empirically rare, theoretically possible (Kish 1965, p.163). The substantive explanation of such cases is, however, not straightforward. We believe that these cases result from sampling variability.

The high proportion of cluster variance due to interviewers might, at a first glance, seem surprising for an item like "distance to the next train station" since this item is a factual one that should theoretically not be susceptible to interviewer effects. It was, however, also classified as a difficult item that might require help from the interviewer, especially since no closed answer categories were provided. The gross effects of different item properties can be seen in *Figure 2*, which depicts R_I for the four item types. We consider only those items for which an overall cluster-level design effect was found, since it would not be meaningful to discuss the portion of the total cluster variance due to the interviewer in cases where the total variance is very small. We therefore excluded all items for which the overall design effect was smaller than 1.1.

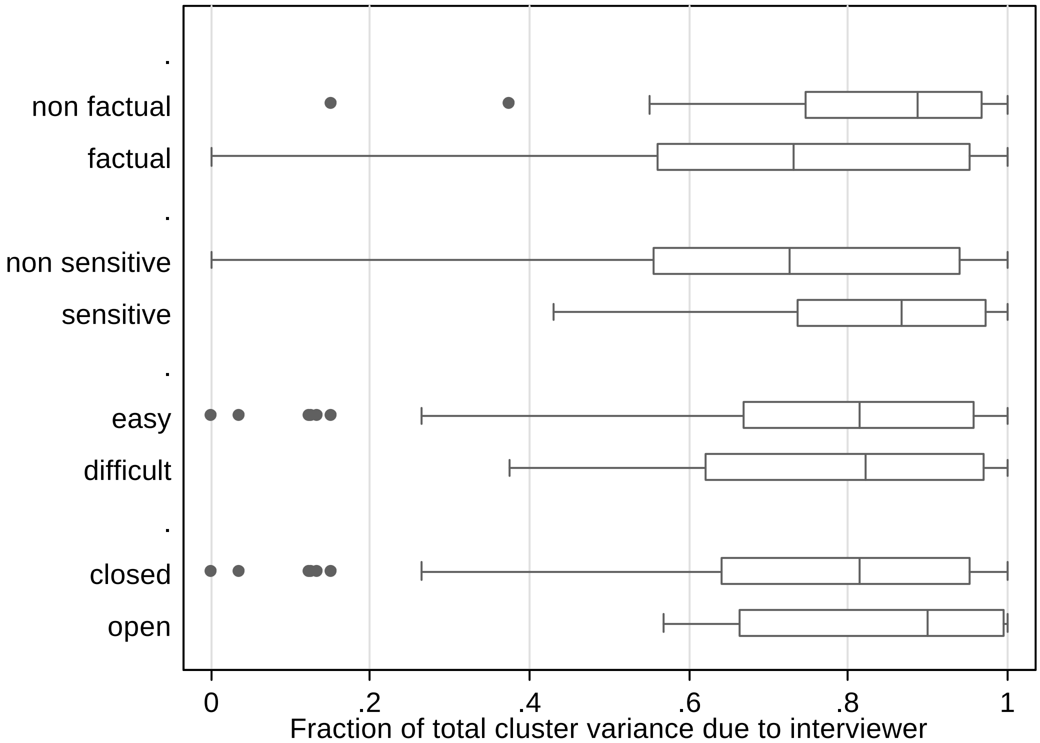


Figure 2: Box plots of interviewer variance as a fraction of variance due to both interviewers and sampling points grouped by item type.

In keeping with our hypotheses, we would expect higher portion of the total cluster variance to be due to interviewer variance for non-factual items than for factual items (a), and for items that were classified as sensitive compared to those that were classified as non-sensitive (b). We also would also expect relatively larger interviewer effects for difficult items than for easy items (c) and for items without closed answer categories (d).

Even if tests of only the first two hypotheses (a and b) yielded significant differences, the data provide support for three of the four hypotheses (see the grouped box plots in *Figure 2*)[15] :

- Sensitive items produce higher interviewer effects than non-sensitive items (mean R_I 0.84 vs. 0.69).
- Non-Factual items produce higher interviewer effects than factual items (mean R_I 0.83 vs 0.71).
- Open questions produce higher interviewer effects than closed questions (mean R_I 0.84 vs. 0.75).
- No differences in interviewer effects were found for difficult and easy items (R_I 0.78 vs. 0.75).

The number of items was not sufficient for an ANOVA with all the interaction effects of all four of the item properties (sensitivity, factuality, openness, and difficulty). Nevertheless, it should be noted that the difference between factual and non-factual items was larger among the non-sensitive questions (0.65 vs. 0.83) than among the sensitive items (0.84 vs. 0.84). Interaction effects among the various item characteristics may therefore reasonably be expected for a similar analysis of interviewer effects that employs with a larger number of items.

Despite the fact that possible interaction effects were not examined, a comparison of the results obtained here with findings from previous work on interviewer effects could be instructive. We therefore defined a simple additive index of possibly harmful item properties. This index is intended to capture the amount of a question's slackness during the interpretation process. It reflects the likelihood that the question provokes either (a) unsolicited cues from the interviewer due to his or her perception of its problematic nature, (b) requests for the interviewer's clarification or help by the respondent or (c) use by the respondent of the interviewer's non-verbal feedback for the determination of an appropriate answer.

[15] t-tests (one-sided): p<0.001, <0.001, 0.13, 0.25; Kruskal-Wallis: p=0.02, 0.01, 0.80, 0.27.

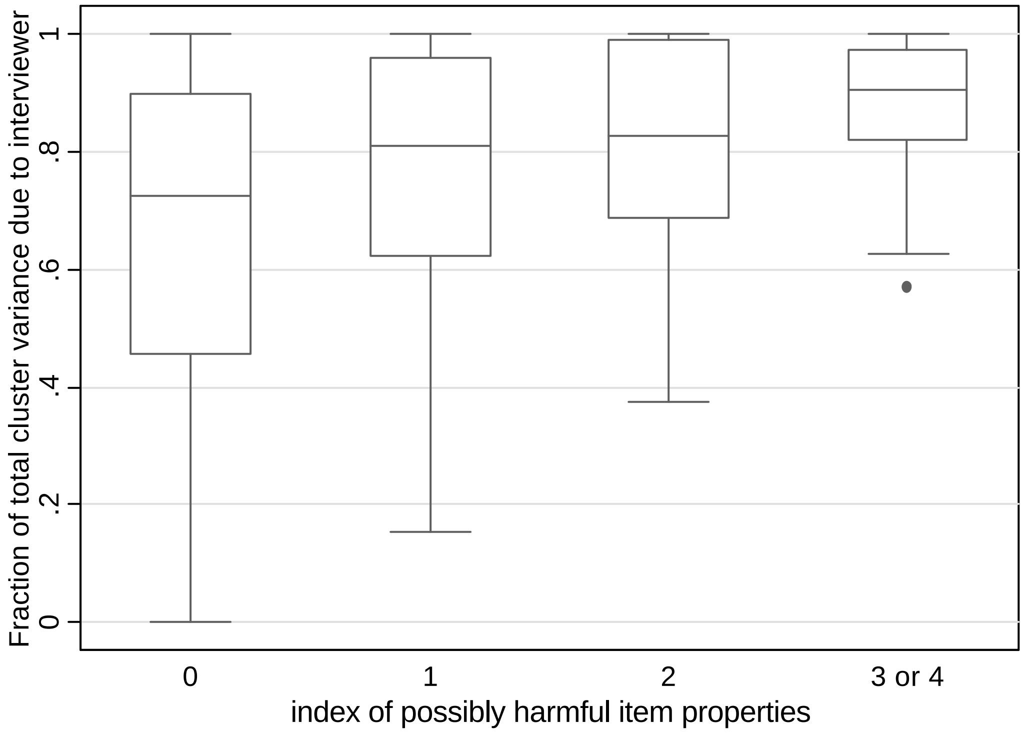


Fig. 3: Box plots of proportion of interviewer variance grouped by number of possibly harmful item properties

However, an item need not have all four properties to be susceptible to interviewer effects. However, a question with more than two possibly harmful properties may be assumed to be more susceptible to interviewer effects than a question with only one or two such properties. The lowest interviewer effects should therefore be seen for items with no potentially harmful properties, i.e. for those items that are factual, non-sensitive, closed and easy to answer. Naturally, the number of possibly harmful item properties varies between 0 and 4. Since only four items had all four of these properties, we pooled items with three or four of the properties into one category.

The fraction of the total cluster variance that is due to interviewer variance R_I varies remarkably across the item groups (see *Figure 3*). Box plots and an ANOVA show significant differences among the R_Is calculated for the four item groups in the pooled DEFECT face-to-face survey (see *Table 1).* There appears to be a steady increase of the size of interviewer effects with the number of possibly harmful item properties. Cuzick's (1985) nonparametric trend tests yields a z of 2.81.

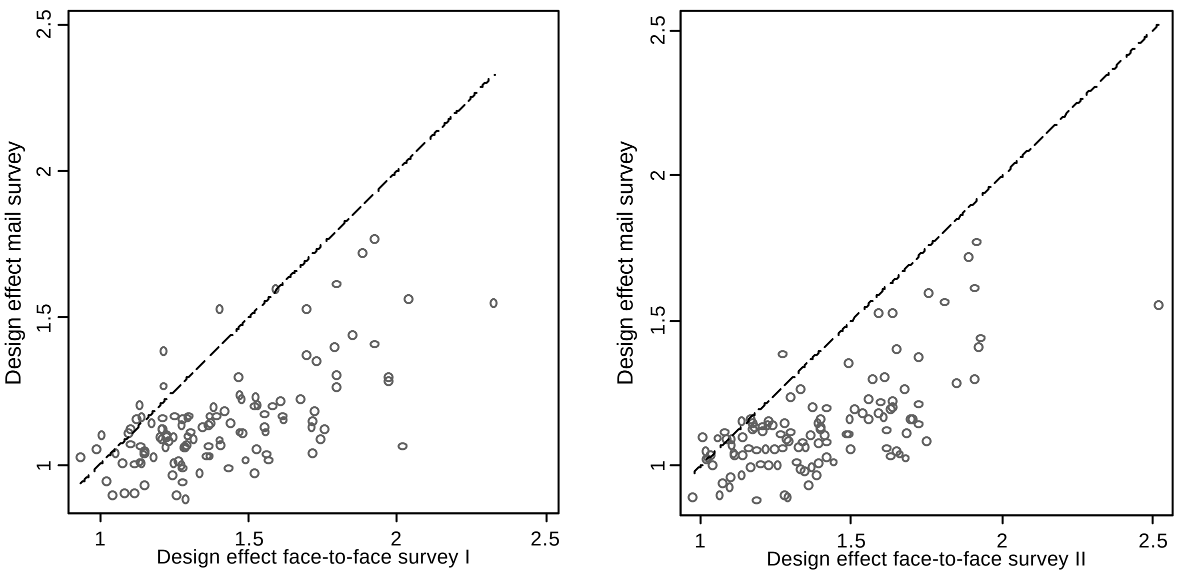
Table 1: R_I for items grouped according to presence of possibly harmful item properties (PHIP)

	DEFECT Face-to-face surveys	
Number of PHIP	*Mean R*	*N*
0	0.63	29
1	0.78	34
2	0.82	27
3	0.87	18
ANOVA results:	$F=4.95$ ($df = 3$)	
	$p=0.003$, $r^2=0.1$	

3.2 *Comparison of the face-to-face-surveys with a mail survey*

We can exploit the design of the DEFECT study for yet another check of the plausibility of the findings. The DEFECT mail survey was conducted at the same time and in the same sampling points as the face-to-face survey, using the same questionnaire[16]. This fact allows us to make direct comparisons between the size of design effects found in the DEFECT face-to-face surveys and those found in the DEFECT mail survey. Nationwide clustered mail surveys of a general-population sample are rather rare, since there are usually no cost incentive to employ a cluster design for a mail survey. However, data from a clustered-sample mail survey can be used to gain an estimate of the size of the design effects produced by spatial clustering. Design effects found in the mail survey are, by definition, solely sampling-point effects.

[16] The questionnaires actually used in the mail and face-to-face modes differed slightly in their layout. The mail-survey questionnaire also contained an introduction not included in the face-to-face questionnaires, since respondent selection within household was conducted by the interviewer in the face-to-face survey and by a household member in the mail survey.



Figures 4a and 4b: Design effects for all items in the mail survey vs. effects for the same items in each of the two face-to-face surveys

The design effects found for the mail survey have a mean of 1.1 (median 1.1) and a standard deviation of 0.17. These values are considerably lower than the ones found for both the individual face-to-face surveys and the pooled data for both face-to-face surveys. Both *Figure 4a* and *Figure 4b* display the design effects estimated for 117 items in the DEFECT mail survey against the design effects estimated for the same set of items using each of the two DEFECT face-to-face surveys[17]. In both comparisons, only very few items fall above the identity line, i.e. produce a higher design effect in the mail survey than in the face-to-face survey, and those that do are those that had small design effects in the first place. The highest design effect found for the mail survey is 1.77. Only nine mail-survey items produced a *deft* larger than 1.4, which was the average design effect size found for the face-to-face surveys[18].

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[17] The same subset of points and items was used for the analysis of the mail survey as for the analysis of the face-to-face surveys. There were, however, too few observations for one of the mail survey indicator items on occupational status for it to be used in the analysis, meaning that the comparison uses 117 items of the face-to-face 118 items.

[18] Of those nine items, three are related to the presence of incivility factors in the neighborhood (for example, the presence of graffiti), two are the home ownership indicators, one refers to the presence of motion detectors around house, two are related to questions on the closest train station, and only one item is attitudinal. The attitudinal item requires the respondents to compare the safety of their city with that of other cities. All of these items are strongly related to the geographic clustering of the sampling point, and design effects would therefore be expected .

Table 2: Design effects found for the mail survey versus both face-to-face surveys

	Design Effect Mail Survey	Face-to-Face I	Face-to-Face II
Incivility: Loitering	1.44	1.85	1.93
Safety in home city compared to other cities	1.52	1.4	1.59
How often at train station	1.53	1.7	1.64
Distance to train station	1.55	2.33	2.52
Incivility: Graffiti	1.57	2.04	1.81
Home security: Motion detector	1.59	1.6	1.76
Incivility	1.61	1.8	1.91
Home ownership status	1.72	1.89	1.89
Is the renter the main tenant?	1.77	1.93	1.92

For those nine items, the design effects computed for all three surveys are displayed in *Table 2*. The data reveal that, for both face-to-face surveys, eight of the nine items have design effects that are as high or higher than the design effects computed for those items in the mail survey. Given this result, and given the difference between the average design effect observed for the mail and face-to-face surveys over the entire item set (i.e. 1.1 versus 1.4), we can infer the influence in the face-to-face surveys of a homogenizing force acting in addition to cluster effects.

Another line of argument would hold that the cluster effects observed for mail-survey items should be independent of the item dimensions suggested above, since the effect of the item type is assumed to be exerted through the interviewer. If this logic holds, there should be no difference in the average design effect for items differentially classified according to the number of potentially harmful properties they have[19]. We ran an ANOVA for the mail survey data. Again, the analysis was done only for items that showed a considerable design effect (*deft >=1.1*), which meant that data for sixty mail-survey items were analyzed. As predicted, we found no effect for item type (see *Table 3*).

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[19] Since there are no interviewers in a mail survey, a computation of R_I has no meaning here. The design effect is a pure clustering effect. The same computation with the data from the face to face surveys would confound interviewer and cluster effects. Therefore, we don't compare defts of mail and face-to-face-surveys here directly.

Table 3: Deft for items grouped according to presence of possibly harmful item properties (PHIP) in the DEFECT Mail survey

	DEFECT Mail-Survey	
Number of PHIP	*Mean deft*	*N*
0	1.29	14
1	1.26	26
2	1.22	18
3-4	1.14	4
ANOVA results:	F=1.16 (df = 3)	
	p=0.334, r^2=0.06	

The effect of possibly harmful item properties on design effects therefore seems to be specific to the data collection mode: The size of the effect depends on the presence and behavior of an interviewer. However, since mail surveys of the general population are considered to be biased toward the more educated, there may still be other reasons to use a face-to-face survey instead of a mail-surveys.

4 Conclusions

Even though one might reasonably assume that design effects for items in a survey on fear of crime in one's neighborhood would be due mostly to the homogeneity of those neighborhoods, our analysis showed that, for most items, the interviewer was responsible for a greater share of the homogenizing effect than was spatial clustering. This somewhat surprising result can be better understood by looking at the influence of item type on interviewer effects. There were systematic influences on the size of the interviewer effects found, since different item types subject the respondent to different cognitive or emotional burdens. If an item is sensitive, non-factual, difficult or open, the question leaves more room for interpretation and is more difficult to answer, meaning that the respondent is more likely to rely on explicit or implicit help from the interviewer. These items therefore produce larger interviewer effects, especially if several of these item properties are present. Support for the large interviewer effects we found was provided by comparing design effects found for a mail survey to the design effects found for two face-to-face surveys. The design effects found for the clustered-sample mail survey are considerably lower than those found for face-to-face surveys.

Several practical conclusions can be drawn from these results. First, there is reason to argue that the design effects commonly observed in face-to-face surveys are not unavoidable. Rewriting particularly susceptible questions to reduce the interviewer's potential influence on the respondent's answers might be a worthwhile improvement, as might be reducing interviewer workload. Both measures would reduce design effects. Second, interviewer and sampling-point identifiers should be included as variables in every data set, since their inclusion would enable the computation of corrected standard errors using two different kinds of clusters. This suggestion stands in contrast to current practice, in which corrected standard errors are not estimated using the interviewer identifier as the cluster-defining variable but by using the PSU as cluster-defining variable. Third, given the large effects found for interviewers, the computation of corrected standard errors might also be necessary for the analysis of data from telephone surveys. This procedure does not appear to be a part of current practice. However, further research is needed to determine to which extent interviewer effects in telephone surveys are comparable to interviewer effects in face-to-face surveys. Those comparisons are possible with the DEFECT data and such analysis is forthcoming[20].

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[20] Preliminary datasets are available on request.

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