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An Interview with the Authors of the Book *Model Assisted Survey Sampling*

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Approximately two decades ago I was approached by Carl-Erik Särndal, Bengt Swensson, and Jan Wretman. They presented a proposal for a book project and at the time I was Head of the Research and Development Department at Statistics Sweden and Chair of our Methods Council. The Council liked the proposal very much and since two of the authors (Carl-Erik and Jan) were Statistics Sweden employees it was decided that the book project should be included in our methods development plan. Eventually the authors found other funding sources as the work progressed and they moved on to various university departments. But Statistics Sweden is very proud of being involved in the initial phases of their endeavor.

The book, *Model Assisted Survey Sampling,* was published by Springer-Verlag in 1992 and soon became a widely admired and used textbook, sometimes referred to as "The Yellow Book" or "The Big Yellow" or "The Big Yellow Book." With their book the authors set a new threshold for textbooks on survey sampling. I thought it would be interesting for readers of their book to learn about the authors' views on the creation of the book and on some current events in the survey sampling field. Their thoughts would be a nice contribution to our anniversary issue of *JOS*. Therefore I asked Phil Kott, who also is a very prominent survey sampling expert and one of JOS's Associate Editors, to ask the authors a few questions. I am very happy that Phil accepted my invitation and here is the result.

Lars Lyberg
Chief Editor

1. *Model Assisted Survey Sampling* has become one of the standard texts on survey sampling theory, if not *the* standard text. What about the Big Yellow Book are you most proud of?

The authors: Phil, we choose to reply individually to some of your questions, usually in the alphabetical order, Bengt, Carl-Erik, Jan, which happens to coincide with the order in terms of age. In the interview, we mention a number of contributors to the field; references to their work are given in the concluding bibliography.

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Bengt: I would like to summarize our contribution in these words: We succeed in bringing a unified perspective to unequal probability sampling, and in coupling it with the general outlook we call model assisted, we link survey sampling with mainstream statistics. I believe those are the reasons the book got a favorable reception around the world. It is, of course, something to be proud of. The reception exceeded by far what I had anticipated.

Carl-Erik: It is gratifying that the book proved to be "a lasting contribution," and, if you wish, "a standard text." This we could possibly hope for but not take for granted when we started out just over 20 years ago. There are many unforeseen factors in writing a book, especially a major undertaking such as this, with its nearly 700 pages. We were fortunate in that the time was ripe for the general survey sampling readership to receive the message. Some in that audience were prepared and already sympathetic; many others became convinced that the book promotes a fruitful approach for practice, supported by a palatable ideological basis. We come back later to the inspiration behind the book.

Jan: "Proud" is a strong word. Of course, I am glad that we managed to complete our big project. Among the book's contributions, I would like to single out the following:

- A more unified presentation of sampling and estimation than what I feel earlier books had to offer;
- An efficient and transparent system of notation;
- The use of (generalized) regression estimation as an important and unifying concept;
- A presentation that reflected, in a variety of ways, our own experiences from work in a national statistical agency.

2. What do you view as its most glaring weakness(es)?

The authors: Our answer should be viewed in light of the objectives that we had set from the beginning. We decided to treat some of the central issues in survey sampling and to treat those rather extensively, in a design-based inference perspective, yet with 'a modern outlook' emphasizing the use of auxiliary information in the estimation. The choice of certain issues necessarily had to be at the exclusion of others, perhaps also important ones. That some clearly important material was treated summarily or not at all in the book can be seen as a weakness. Analysis of survey data is an example of an area that we chose not to treat in detail. Small area estimation is not within the scope of the book, because model dependent, in contrast to design-based. From a teaching perspective, it would have been an advantage to have in the book, or as an accessory to it, one large (and real) population, preferably of households and individuals, for purposes of demonstrating ideas in the book. A desire to see additional end-of-chapter exercises has been expressed, notably by students with a comparatively limited background in mathematical statistics.

3. Are there significant revisions in store? If so, what will be added or removed?

The authors: It is always a difficult proposition to revise a text on an area in vigorous and constant development. Significant developments have taken place since the mid 1980's

when the contents of the book were drafted. (It finally appeared in 1992.) Important new issues have come to the forefront. They would deserve a treatment in a second edition. Despite constant encouragement from the publisher, Springer-Verlag, we have so far not made the commitment to go ahead. It is unrealistic to envisage a book even longer than the 700 pages already inside its covers. Important new material would have to enter only at the price of deleting some existing material. Issues that we feel a need to discuss include the following:

1. The fixed size πps sampling schemes known as Order Sampling, as explored by Rosén (1997) and Ohlsson (1990);
2. The extensions put forth in recent years to nonlinear assisting models for the GREG estimator, as in Breidt and Opsomer (2000) and in Lehtonen and Veijanen (1998);
3. Calibration estimators and their relation to GREG estimators;
4. More on nonresponse adjustment, in particular through the calibration approach;
5. More on simplified variance estimators and on resampling methods for variance estimation.

4. I have a gripe. What went into the decision not to name statistics after their creators? The Horvitz-Thompson estimator, for example, is not called by that name. Even more irksome, why are so many estimators not given useful names at all? We only have the variance estimator in Equation (6.6.4) rather than the weighted residual variance estimator, the π^* estimator rather than, say, the double expansion estimator. When I went to look up how the "general regression estimator" or "GREG" was defined (because I have seen it done in a number of ways, and I wanted something definitive), I was shocked to find that this common expression closely associated with your work does not appear at all.

The authors: Your points are well taken, Phil. They deserve a detailed response. The first part is our collective response; Bengt will add a few comments at the end.

One motivation for our term π estimator (rather than Horvitz-Thompson estimator, or HT estimator) was that we viewed π-inverse weighting as a general principle rather than as an aspect of the particular estimator $\sum_{k\in s} y_k/\pi_k$ that Horvitz and Thompson examine in their much cited 1952 paper. The Yellow Book puts, so to speak, all the many applications of the general principle into one big category and chooses to denote all of them by the common index π, which becomes a constant and important reminder of the fundamental role of the inclusion probabilities in the estimation process. (Nevertheless, the term Horvitz-Thompson estimator is used with an astonishing frequency in the survey sampling literature. Carl-Erik recalls a conversation, around 1975, with Dan Horvitz, where he, perhaps all too modestly, expressed his surprise at being the instigator of a 'universal concept'.) An advantage – slight perhaps, but significant – is that π is a more compact index than HT for the hundreds of π-inversely weighted quantities appearing in the formulas, and, for the many instances in the text, π estimator is a more compact name than Horvitz-Thompson estimator.

We think there are also good reasons for the name π^* estimator rather than double expansion estimator. Again, π^* estimator is, in our opinion, more telling, because it directs

attention to the important role of the inclusion probabilities. Another reason is that π^* estimator is more general than double expansion estimator. For example, if the first phase out of two phases is two-stage sampling, then the end result becomes a triple weight expansion.

We can trace the modern names generalized regression estimator and GREG through a progression over several decades, proceeding in essentially three steps:

- **Step 1.** The notions of classical survey sampling were expressed about 50 years ago in several remarkable books by authors such as Cochran, Hansen, Hurwitz and Madow, Deming, Murthy, Des Raj, and yet others. Sampling theory stood for what those two words literally signify: randomized sample selection from a finite population. Depending on the practical circumstances, the selection is done in a number of ways (albeit a fairly limited number): simple random sampling, stratified simple random sampling, single stage cluster sampling, two-stage sampling, and so on. Sampling is the primary activity; estimation is not a separate activity. It is an automatic consequence, because each type of sampling induces an estimator whose unbiasedness and variance (under the randomization defined by the fixed sampling design) is readily established, and the variance can be estimated. In the 1960s, many viewed sampling theory as a dead field of research, because one was hard pressed to invent new clever ways of randomized sample selection, beyond those already covered in the classical books. Although the ratio estimator was considered in some detail in the classical texts, the use of one or more auxiliary variables at the estimation stage was not really viewed as a promising avenue.
- **Step 2.** Around 1970, authors such as Ken Brewer and Richard Royall turned our attention to models (and only models) as a foundation for estimation and inference in surveys. Their work pointed to the possibility of having purely model-based (thus model dependent) inference about the finite population. A welcome consequence was a shift of attention away from 'sampling' on to 'estimation,' away from age-old ways of randomized sampling to relationships that may exist between the variable of interest, y, and the auxiliary variable(s).
- **Step 3.** It was easy to see that "one can have both under one umbrella," that is, combine the classical randomization outlook with a general perception of the relationship between the variable of interest, y, and the auxiliary variables. There is no need to sacrifice the design-based (or randomization-based) principles of inference. This leads to the model assisted design-based view promoted in the Yellow Book. It appealed to many, because statisticians are exposed to regression fitting already in their first statistics course, and the emphasis on model fitting is reinforced all the way through a university statistics curriculum. So, here came model assisted thinking, permitting regression ideas to be effectively married with the design-based inference paradigm, without upsetting it. For those who thought sampling theory was dead, and for interested young researchers, it offered new promise of progress: model fitting became an integral part of the classical design-based theory. Still, the classical randomization concepts remained untouched.

The vector formulation of the auxiliary variable was an 'obvious extension' that gave the design-based generalized regression estimator. The term appears in the title of the 1976 Biometrika paper (with discussion) by Cassel, Särndal, and Wretman. Despite this, as Phil notes, the term is used only sparingly in the Yellow Book. One explanation is that the concept received publicity through the book and became widely used only after its publication. A second edition would take this into account.

Cochran and his contemporaries were perhaps too conservative to take the step to multivariate auxiliary variables. Pierre Thionet at INSEE in France had thoughts along these lines in the 1950's. But the computational resources needed for extensive multiple regression fitting for large-scale statistics production were limited or absent 50 years ago. In the early 1970's, Wayne Fuller at Iowa State University explored and designed software for design-based regression analysis. The new key term became not so much *sampling finite populations* as *estimation for finite populations*. The Yellow Book expressed that new direction. The onus is always on the authors of a text, at least a good text, to express new thoughts in simple and convincing language. One of our contributions was, we believe, to make "generalized regression estimator" a "household word," a concept that every survey statistician could easily understand and comfortably use.

The acronym GREG began to be used at Statistics Canada around 1991 in the construction of the Generalized Estimation System, which took the family of generalized regression estimators as its basis. Then this convenient acronym caught on, more generally. Other software was designed to carry out generalized regression estimation, for example the Swedish product CLAN.

Bengt: Let me add a couple of comments. One is to emphasize that our choice of π instead of HT was deliberate, to stress a general principle. From books prior to ours, many students (and I myself was one of them) were led to believe that there exists a separate estimation problem and a separate estimation method for each design, even for the various single-phase sampling designs. This idea was also, unfortunately, promulgated by many university teachers in charge of the token "sampling course." It was the way that they, too, saw it. They gave a picture of the discipline that was not really false, but filled with unwarranted pedantry. This impression of mine was confirmed in talking to younger new colleagues at Statistics Sweden, and it lies behind the following lines in the Yellow Book's preface: "In supervising younger colleagues, we repeatedly found it more fruitful to stress a few important general principles than to consider every selection scheme and every estimator formula as a separate estimation problem. We emphasize a general approach."

As we noted earlier, in the 1960s many viewed sampling theory as a dead field of research. I still vividly recall, just after completing my undergraduate studies, the comment of my professor, Herman Wold at Uppsala University, when I expressed an interest in pursuing further studies in survey sampling: "Everything has already been done in that field." I was stunned. As a result I decided to accept a job offer from Statistics Sweden.

5. There were many stunning contributions to the literature by you, both individually and collectively, before Big Yellow. Two articles I particularly admire are Särndal and Swensson (*International Statistical Review 1987*) on two-phase sampling and Särndal, Swensson, and Wretman (*Biometrika 1989*) on variance estimation for the GREG. I thought the latter was heading you towards a randomization-assisted model-based approach to survey sampling because the variance estimator proposed is arguably a better measure of model variance than randomization mean squared error. Recently, I have been told that one of you, at least, has been moving in the other direction: toward a purely randomization-based approach to regression in survey sampling along the lines of the optimal difference estimator in Section 6.8 of Big Yellow. Comments?

The authors: Phil, this is another important point, because it deals with the progression of ideas. Here is our rather lengthy reply.

Bengt: A reader of the Yellow Book cannot fail to see that the fundamental outlook in regard to inference is design-based. Estimators are evaluated with respect to the (known) probability mechanism that generates the sample, and not with respect to any assumed model. The only exception is in Chapter 15, where point estimators of the population total are evaluated jointly with respect to the sampling design and the response homogeneity (RHG) model assumed to govern the response behavior.

In conversations with some readers of the Yellow Book, I have noticed a tendency to misinterpretation of 'model assisted design-based inference'. The first element, 'model assisted' is used in the book in a particular way and with a very specific meaning. We introduced it as a vehicle for the process of finding efficient estimators, as an aid for the survey statistician to utilize his or her professional opinion of the main features of the relationship existing *in the finite population* between a study variable and auxiliary variables. If this opinion is well-founded, the approach will yield an efficient estimator of a finite population total, provided a strong relationship holds in the finite population between the study variable values and the auxiliary values (possibly after an appropriate transformation of those latter values). This is expressed on page 226 as follows: "We thus assume that the scatter of the N points $\{(y_k, x_{1k}, \ldots, x_{Jk}) : k = 1, \ldots N\}$ looks *as if* it had been generated by a linear regression model, called ξ, . . . " The words *as if* are important. To further emphasize the hypothetical element and its role for the inference, Remark 6.4.1 on page 227 goes on to say: ". . . We think that the finite population looks as if it might have been generated in accordance with the model ξ. However, the assumption is never made that the population was really generated by the model ξ . . . (the regression estimator's) basic properties . . . are not dependent on whether the model ξ holds or not. Our procedures are thus *model assisted*, but they are not model dependent." It follows that the book's approach is purely randomization-based.

Admittedly, "model assisted design-based inference" is not the simplest of statistical concepts. Some have called our approach model-based. It is not. It is randomization-based. Should the risk of misinterpretation of the term "model assisted" lead us to abandon it? Carl-Erik seems to think so, in his reply which follows. I do not. I have found the concept highly useful in my teaching. It makes the subject of survey sampling more interesting and

more challenging to students. It often opens up a classroom discussion of an epistemological nature, and many students like that.

I have some difficulty accepting Carl-Erik's view (which follows) in regard to "model oblivion (the new Step 4)." Our difference of opinion is perhaps just a matter of how one should interpret 'information carried by the auxiliary variables.' But I have a feeling that the issue is not quite that simple. The basic calibration setup calls for known auxiliary vector values $\mathbf{x}_k$ for $k \in s$ and a known vector total $\mathbf{X} = \sum_U \mathbf{x}_k$. To me, these are just data. They do not carry information by themselves. They have only a *potential* to do so. Whether these data are valuable or not for estimating the total $\sum_U y_k$ of a study variable y depends on the extent to which $\mathbf{x}$ and y are related. If the survey statistician's knowledge and experience (based on similar surveys in the past, on a pilot survey, or on other evidence) tells him or her that a strong relationship exists between $\mathbf{x}$ and y in the finite population, then only does $\mathbf{x}$ become a carrier of information. The better the knowledge, the better the prospects of finding a good GREG estimator or calibration estimator. How can we benefit from this knowledge in practice? The model assisted approach offers a way to do so, and without any need to assume that the model is true. For most applications on human populations and business populations, the available social science theories are in any case not strong enough to justify such an assumption. The model serves only as a compact summary of fundamental features of relationships existing in the finite population, features which will determine the design-based properties of the chosen estimator, including its variance.

Carl-Erik: As Bengt notes and as Phil is aware, our preferred term for the Yellow Book's approach is 'model-assisted randomization-based' (or design-based). Phil is wondering about alternative future directions for the basis of survey sampling theory (not only our directions, but those of all who participate in the scientific dialogue in the field). He brings up "randomization-assisted model-based" as a name for a possible future outlook; let me call it a move to the left compared to the standpoint of the Yellow Book. Such a move could perhaps be inspired by the cited 1989 Biometrika paper. (It was selected for the IASS Jubilee Commemorative Volume of Landmark Papers in Survey Statistics, so some see it as important.) Ken Brewer, who has given much profound thought to the borderlands between "randomization" and "model," may actually like the term "randomization-assisted model-based."

These four-word constellations of terms reflect the desire to accomplish a synthesis of 'randomization aspect' and 'model aspect'. The move to the left does not exactly simplify matters. A good many sampling practitioners would probably say that the theory of inference from finite populations is already nebulous.

The opposite move is to the right, in the conservative direction, to "purely randomization-based" without the term model assisted. It brings a welcome simplification. But how make it operational, without sacrificing the ground we have gained, compared to the books of the 1950's, in our understanding of inference in sample surveys?

The word model, not the word randomization, is the root of the problem. A straight continuation of the scientific progression mentioned earlier goes from model dependence (Step 2) to model assistance (Step 3) to model oblivion (the new Step 4). This new step rids the mental process of 'model' and 'model assistance'. One can in fact be randomization-based, yet capitalize fully on the auxiliary information. Then the main building block is not

the assisting model but the information carried by the auxiliary variables. It provides input to the computation of weights, calibrated to the information, and used to weigh the observed y-data. We evaluate the merits of the resulting calibration estimators (bias, variance) in a purely randomization-based setup. The model itself is an abstraction. It contains no information. "One of us" promotes the new orientation in the new Wiley book *Estimation in Surveys with Nonresponse*, by Särndal and Lundström.

Jan: I think it is very clear that we take a design-based (or randomization-based) view of inference in survey sampling. Estimating the "model variance" of an estimator has never been one of our principal concerns.

6. The approach to survey sampling in the book and the one you appear to share personally is solidly design-based, falling short of the full model/randomization synthesis long advocated by V.P. Godambe and Ken Brewer. I have taken an even more "left-wing" view and advocated inference be based primarily on the model (*JSPI* 2005). In light of your collective reluctance to consider good model-based variance-estimating properties per se, would you renounce the weighted-residual variance estimator of your Biometrika article and Equation (6.6.4) in favor of the more traditional (6.6.11)?

The authors: Your JSPI paper thus proposes a 'randomization-assisted model-based' outlook. This search for the ideal model/randomization synthesis is an interesting phenomenon. We can also see it in the work of the persons you mention, and the Yellow Book promotes a type of synthesis. The weighted residual variance estimator, (6.6.4), has the merit of displaying good properties with respect both to the design and to the model. We certainly do not renounce it. If a user prefers, on ideological grounds, to compute (6.6.4) rather than (6.6.11) he or she may do so. Computationally, the choice is not important. Most comparisons that we have seen do not show any great numerical differences between (6.6.4) and the more traditional (6.6.11). Those seeking certain kinds of conditional inference would prefer (6.6.4).

7. What are your favorite books on survey sampling other than Big Yellow? How are they superior to your book? Inferior?

The authors: An attempt to identify 'inferior alternatives' may amount to undeserved criticism of other honest work, so we abstain from answering that part of the question. On the other hand, we can mention a number of books that we like, in part or in whole, and for different reasons. There are the older books, those that created our interest in the field, when we were young students. They were very important, although today they may be little known and little used. Then, there are modern books that we can enjoy and admire.

Bengt: Looking back, several books stand out in my memory as important and useful, in different ways, for my work and my teaching. The very first sampling book I used, in somewhat a desperate mood, was as a new employee at Statistics Sweden, fresh out of an undergraduate statistics program with almost no exposure to survey sampling. (This lack of exposure to survey sampling was often, and is still often, found in statistics programs, even in the best of universities.) My assignment was to plan a large sample survey. The book I happened to get my hands on was volume II of *Sample Survey Methods and Theory*

by Hansen, Hurwitz and Madow (1953). It did help. On several later occasions, I benefited from parts of Deming's two sampling books, *Some Theory of Sampling (1950)* and *Sample Design in Business Research* (1960), in particular the parts dealing with general planning principles and the relation between the statistician and his client. The first (to be honest, maybe the only) sampling book I ever read from cover to cover was Des Raj's *Sampling Theory* (1968); it was part of my required reading for graduate studies. It gave a solid and clear basis for my future work. For teaching purposes, my favorite book at the undergraduate level was Murthy's *Sampling Theory and Methods* (1967). I found it richer in content than Cochran's celebrated *Sampling Techniques* (1963, second edition), but occasionally I used Cochran's book as well. At present, trying to cut down on work and enjoy retirement, I look forward to reading, for mere pleasure, Ken Brewer's highly personal view expressed in *Combined Survey Sampling Inference, Weighing Basu's Elephants* (2002).

Carl-Erik: Long ago, I admired (and learned much from) the texts by Cochran, Deming and Des Raj. At this time, I like books that debate the ideological issues. The field "inference for finite populations" directly invites a conflict of ideas. This makes it interesting and challenging. On the one hand, there is the finite population. It really exists, as do the known inclusion probabilities of its elements, created by the randomized sampling scheme. On the other hand, there is the perceived relationship – unfortunately, in my opinion, it is all too easy to say it is "a model" – between the study variable y and the auxiliary vector, $\mathbf{x}$. When those two basic ingredients are brought to bear on theory, there is immediately a conflict. Some argue energetically in favor of one or the other "pure theory construct;" others want a "synthesis" of randomization and model. In the end, nobody will claim clear victory. But I enjoy reading well-prepared accounts under one or the other outlook, for example, the recent books by Brewer and by Valliant, Dorfman and Royall. It is significant and encouraging that the last twelve years or so have brought unusually many new books, some general, others devoted to a particular aspect within sample survey theory and practice. Among authors of such books are Paul Knottnerus, Sharon Lohr, Risto Lehtonen and Erkki Pahkinen, J.N.K. Rao, Mary Thompson, Steven Thompson and George Seber. (The references are listed at the end.)

Jan: I have always liked Des Raj's *Sampling Theory* (1968). It gave me a lucid introduction to the theory of sampling and estimation. And for a number of years, I have extensively used Cochran's classic *Sampling Techniques* (Third edition, 1977). Among the more recent ones, I like Sharon Lohr's *Sampling: Design and Analysis* (1999). She gives a highly readable and thorough introduction to survey sampling. It is also very much up to date. I would rather abstain from making any comparisons with the Yellow Book.

8. I myself learned survey sampling from Des Raj's *Sampling Theory*. I was surprised and delighted to learn that all of you share my admiration of it. What are your favorite recent papers on survey sampling?

The authors: Many thought-provoking papers are produced on survey sampling; a good number of them end up published in the best journals. One can feel some regret that all do not seem to leave the impact on practice that they perhaps merit. We feel that it is not easy to name one single favorite recent paper.

Bengt: Papers that would almost certainly be valuable input to work on a new edition (if we go ahead) include the following: Deville and Särndal (1992), Binder and Patak (1994), and Rosén (1997). Brewer's (2002) book would almost certainly also enrich work on a new edition.

9. More broadly, what do you see as the most promising trends in survey sampling research and practice?

The authors: That is a hard question. A "promising trend" is, one would presume, one that already shows clear signs of taking us much further along, leading us perhaps to a real breakthrough, and this within a foreseeable future. And "breakthrough" might mean something that produces a radical, resource-efficient and quality-enhancing change in the methodology of sample surveys, or of statistics production more generally.

We do perceive a promising trend – one that we are confident will continue – in the more efficient and systematic use of auxiliary information at all stages of a survey: in sampling design, in data collection, and in estimation, particularly in the presence of nonresponse.

Unquestioningly, the leading national statistical agencies set rigorous standards and produce high-quality statistical information in the best of their surveys, such as the Labor Force Survey. But at the other extreme, others do surveys (because it is all too easy) and produce numbers of appallingly bad quality, so poor that the outcome does not deserve to be called information. They produce numbers that are noninformation. Many meaningless opinion polls are conducted. A regrettable trend is that the increased use of web surveys may jeopardize the reliance on trusted concepts such as probability sampling. But because a number, that magical quantification called a number or a statistic, is produced, an unquestioning public or a group of indiscriminate users will be fooled. This is a sad trend.

Regrettably, we cannot seem to distinguish "a promising trend" that could, in the near future, yield "a clean attitude to surveys." The Yellow Book and comparable modern texts have little direct impact in that regard. They are directed to and read by a select few, a kind of "special echelon" of the wide survey community, notably those called "survey methodologists."

A thorough assimilation of the material in the Yellow Book requires a certain maturity and familiarity with mathematical and analytical reasoning. Out of the thousands of persons who make their living in the survey business, working in government or in private survey organizations or elsewhere, a fairly small percentage find the book easy reading, or 'must reading.' Still, many of these other persons are highly competent statisticians, experts perhaps in some other aspect of statistics production.

This interview has touched on alternative views on inference from surveys. You, Phil, and we are tuned in to these types of questions. They are important to us, as they are to many others who recognize inference in survey sampling as a mathematically well-structured part of the wide world of surveys. Survey methodologists do crucially important work in a statistical organization. They deserve high recognition. They can take pride in the fact that they are backed up in this effort by 'a scientific basis' that has evolved gradually for over a century. But we can only expect a fraction of all the people in official

statistics, or for that matter in the wide readership of the *Journal of Official Statistics*, to have a fully enlightened perspective on the field of inference in survey sampling.

10. What are you working on now?

The authors: Considering our status as "senior citizens" – the junior member of our team, Jan, just turned 65 – we do not feel obliged to submit to a "seven-year plan of survey sampling research." The fact that we are still "taking an interest," even a considerable interest, is perhaps just a sign of reasonably good health, despite advancing age. Phil, we wish to thank you for your thought-provoking questions.

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