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Survey Methodology

Progress in survey science and practice: yesterday-today-tomorrow

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Progress in survey science and practice: yesterday-today-tomorrow

Carl-Erik Särndal¹

Caption: “We must explain **why** science – our surest example of sound knowledge – progresses as it does, and we first must find out **how**, in fact, it does progress.” Thomas S. Kuhn (1970).

Abstract

This article confronts survey science with important notions in philosophy of science: progress, paradigm, research tradition, research programmes. The article is conceptual and exploratory, rather than mathematical/technical.

This is against a background where survey science must evolve in unfamiliar and challenging conditions. Society is changing. Survey nonresponse is high. Probability sampling surveys are in question, considered too expensive. Low cost alternative data sources – big data and others – must, in the opinion of some, be incorporated in statistics production at the national statistical offices.

A lively *research tradition* has brought progress in survey science over more than one hundred years. The article recalls some of that progress and tries to foresee how the tradition may survive and face the coming decades.

Key Words: Design-based; Model assisted; Model-based; Nonresponse; Paradigm; Probability sampling; Research tradition; Survey science.

1. Introduction

Survey science is changing. Change is nothing exceptional. Every scientific field may, at some time, find itself in a state of doubt or uncertainty, as when critical questions are pushing for answers. It is a new and unusual situation for survey science.

I use the term survey science primarily in the spirit of classical books that shaped the field, with clarifying accounts of theory and methods, once the breakthrough of *probability sampling* had happened. It led to progress that conferred high prestige on this new science. Such books are Cochran (1953), Hansen, Hurwitz and Madow (1953). In their time, they inspired countless numbers of students and research workers, focusing on sampling techniques and on estimators and their properties under different probability sampling designs. Tillé (2022), and the discussions of that article, reflect on the present state of official statistics and its future. A reader of the present article may consult numerous references given there.

It is true that survey science deals with important issues other than “accurate estimates”, such as coherence, relevance, comparability, timeliness, confidentiality, disclosure control, respondent burden and yet others. Those are not discussed in this article.

Survey science tries to advance today in a climate of unusual and challenging conditions. In recent times, these concerns have been expressed in media, in articles, at conferences. Laitila (2017) mentions several.

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(I have marked key words in bold): “Surveys based on **randomization theory** are still today a major source for the production of statistics at **National Statistical Offices**. The theory presupposes **full response**... The problem of **nonresponse** is today a major **threat to the validity**... of sample survey statistics. This problem, and the **high cost** of sample surveys, are reasons why National Statistical Offices are considering **Big Data** and other **alternative sources of data** for the production of statistics.”

The item “high cost” is striking. Scientifically superior methods will not necessarily prevail in official statistics production. A National Statistical Office (NSO) must strike a delicate and difficult balance between production cost and quality of the estimates. The NSO may settle for “less than best”, as long as other vital criteria and issues are met.

Survey science is thus called on now to deliver progress in conditions that reflect today’s reality:

- 1) High cost of probability sampling surveys.
- 2) High nonresponse rates in such surveys risk to undermine quality, give invalid estimates.
- 3) Low cost alternative (multiple) data sources exist, and NSOs seek to make progress in ways that can capitalize on those sources.
- 4) Doubts about quality: In these new conditions, what is “validity” of official statistics; how is it assured and measured?

To fully address these problems and concerns may require concepts and theories currently not available. The contents of this article are arranged in this way: Scientific revolution and **paradigms** are discussed in Sections 2 to 4. The **research tradition** in survey science is examined in Sections 5 to 9. **Progress**, in particular **cumulative progress**, in survey science is discussed in Sections 10 to 13. The **changing reality** and future prospects are topics in the concluding Sections 14 and 15.

2. Scientific revolution and paradigm shift

Paradigm is a strictly defined term in philosophy of science, Kuhn (1962). A paradigm is a set of assumptions, concepts and practices, a way for viewing reality, shared by members of a research community. It expresses a dominant way of thinking; it is a norm for scientific activity in a mature science. It defines “normal science”, setting a standard for what is legitimate contributions to the field.

Paradigm has also become a popular term, for a more or less important trend in a scientific field. The term can convey a multitude of meanings.

A time may come in the life of a paradigm when it is heaped in doubts, exposed to unresolved problems, difficulties, or even anomalies. A scientific revolution happens when an intense period of research reaches a point of development where a majority of scientists, and practitioners, feel obliged to consider it seriously,

adopt it, accept it as the new direction, both for themselves and for colleagues. “New paradigm” signifies that a scientific revolution and a *paradigm shift* have taken place, leaving a new norm for scientific activity, obliging all scientists to adhere to it.

3. Paradigm in survey science

The expression “the probability sampling paradigm” is often used in survey science. A thorough search would likely reveal a frequent presence of the expression in the literature of the last several decades. To name a paradigm after one single component of the whole survey process, the sample selection, is quite remarkable. It illustrates the importance of that particular component, and the commanding influence that probability sampling exerted for a long time on the discipline. Probability sampling became a fertile and productive driving force, a frame of reference for survey science and practice. For example, Bethlehem (2009) uses the expression in a thorough account of the rise of survey sampling.

Some protagonists see a need for a profound change, a paradigm shift. In an article about multiple data sources for estimates, Citro (2014) states: “Official statistical offices need to move from the probability sample survey paradigm for the past 75 years to a mixed model data source paradigm for the future.” It is an interesting statement, leaving it up to the reader to interpret some of the words. “Need to move” sounds, whether intended or not, as a frustration or impatience with the incumbent way of thinking. Similar signs can now be perceived elsewhere in the professional circles.

A reader may interpret the words to suggest that probability sampling is now obsolete as a technique for designating a sample. That casts an undeserved blame on probability sampling. It would be unfortunate to reject a design for a new survey just because probability sampling plays a role therein; this is certainly not intended in the cited words.

The citation may also suggest dissatisfaction with the estimation theory flowing from probability sampling, known as the design-based theory, or the randomization theory. Design-unbiased estimation with low variance served as a guideline for many years. Some now see this methodology as cumbersome or too expensive to practice.

4. A unique paradigm shift?

An incumbent paradigm exerts a powerful influence on a science. It is a norm; it can be a constraint, almost a straitjacket. There is reason to ask whether survey science has experienced, “gone through” more than one paradigm in its more than one hundred years of existence.

Although a matter of interpretation, my impression is that a paradigm shift occurred only once. It was when the scientific community, and practitioners as well, became gradually convinced, in the years 1926 to 1940, that a sample, a very small proportion of a very large population, can suffice for valid and reliable

conclusions, provided that selection from the population is carried out judiciously, more specifically by probability sampling, so that in the end, every selected unit has a known positive probability of selection attached to it. This led to the principle of inverse inclusion probability weighting, as in the unbiased estimator – in effect a class of estimators – associated with the names Horvitz and Thompson (1952).

It deserves to be called a true discovery, a century ago, that a modest size sample from a very large population can indeed suffice. Seemingly totally unrealistic, this had left the statistical community in disbelief, as late as in the last few decades of the nineteenth century, when it was still maintained that reliable numbers about the nation and its population require a complete enumeration. It took time to reach a consensus that reliable statistics could indeed be based on a sample accounting for only a very small fraction of the very large finite population. In the 1920's, important contributions were Bowley (1926) and Jensen (1926). Still more important were advances in the 1930's; in particular, Neyman (1934) stands out as a frequently cited source.

When Citro (2014) sets the life span of “the probability sampling paradigm” at 75 years, she sets the crowning of the paradigm at, very nearly, 1940. This is for good reason. That year was a milestone, a great step forward for census taking and for survey science in the United States. We read in Wikipedia (1940 overview history – U.S. Census): “One of the major innovations of the 1940 census was the use of advanced statistical techniques, including probability sampling, which had been used only on an experimental basis before. Sampling had been tested in a trial census of unemployment carried out by the Civil Works Administration in 1933-1934 and surveys of retail stores in the same decade, and an official sample survey of unemployment in 1937 that covered about 20,000 households.”

The chronology of the sampling revolution is told in the following terms and periods: Doubt, skepticism, uncertainty: 1895-1926; conquering revolution: 1926-1940; mature science with probability sampling as a dominant – although not exclusive – feature: 1940 until recently.

If only one scientific revolution occurred in survey science history, is that a sign of **strength** of this science? A sign of solid robustness of the field? Or does it suggest **weakness**, that little happened despite tremendous technological advancement in many other fields? Have there, in the opinion of some, been several scientific revolutions in survey science? Is one needed at this time, considering the conditions mentioned in the introduction? This article gives no direct answers. Its objective is to observe survey science through the lens of important concepts in philosophy of science.

5. Structures in scientific activity

Philosophers of science conclude that **structures** guide, or rule, the research activity in a field. Chalmers (1976) states: “It is clear that science is in a better position to progress if the theories have a structure that contains quite clear keys and prescriptions about their development and their extension. These structures should propose a research program.”

A paradigm is a form of structure. Other structures recognized in philosophy of science are *Research Tradition* (Laudan, 1977) and *Research Programmes* (Lakatos, 1970). Both set prescriptions, in fairly broad and flexible terms, for research activity and development in a field of science. I find Research Tradition particularly relevant and appropriate in a discussion of survey science. Thus I focus on it here. Laudan (1977) sees it as a softer, more flexible, envelope for research activity, less rigid than either Kuhn's Paradigms or Lakatos' Research Programmes.

6. Research tradition in science

A research tradition (Laudan, 1977) is a set of very general assumptions about the field of study, and about appropriate methods to investigate problems in the field. He calls them *core assumptions*.

The term *research tradition* has a temporal resonance, suggesting a duration. Decades passing, a viable research tradition will address a number of **problems** needing to be studied and resolved. In the process, **theories** emerge for investigating the problems, which may become more or less satisfactorily resolved. **Problems** and **theories** form an inseparable couple in the life of a research tradition.

Theories are typically formulated or proposed by scientists active in the field. The scientific community takes note. Theories are tested and tried. Some theories thrive and survive. Others die. Some progress is realized. Theories within a research tradition can be inconsistent rivals Laudan (1977) points out. Competing theories can co-exist in a field, different to the point of being in part contradictory. A familiar example in survey science is the contrast between design-based theory and purely model-based theory, as discussed in Section 11.

7. The research tradition in survey science

The incumbent research tradition in survey science is captured in two core assumptions:

The population assumption: There exists finite, possibly very large, collection of identifiable objects, or units, composing the target population, or the population of interest.

The sample assumption: Data are collected, or are otherwise available, on units in a comparatively small selected and designated subset, a sample; these data, together with other relevant data, are used to draw conclusions about the population.

That is my succinct formulation of the ruling research tradition in survey science. It has shown duration, has guided survey science for more than one hundred years. It sets survey science apart from most other areas of statistical science. It is a fruitful expression of structure for research activity. It is not a paradigm.

It confirms a view commonly held in National Statistical Offices, which dispose, for some populations of interest, of essentially complete registers or lists of the population units; those can serve as sampling

frames. It is a familiar starting point for researchers in the field. Young researchers are exposed to it early on. They may never question it, or reflect on its essence, for the rest of their careers. Yet it is just a structure by the human mind, open to question and criticism.

Identity and identifiability are central notions in the formulation of the research tradition. A unit has a physical or legal identity, and may have other known or apparent properties as well. Identifiability makes it possible to designate a unit to the sample. Due to nonresponse or other imperfections, data that one expected the data collection to give may be missing for some designated units.

The population units have identity, but they may or may not be identified, or listed, at the outset. But units in the selected and designated sample must be verified members of the population.

A classic example of the distinction, familiar from a basic course in survey sampling, is a survey of the unlisted households in a big country. Two stage probability sampling – geographical areas selected at the first stage, households within selected areas at the second – yields a sample of households, identified members of the population of interest. The survey taker has no idea – apart from an estimate – of how many households there are in the country. Nevertheless, an unbiased, valid estimate can be obtained for, say, the number of households in the country with three or more bathrooms, or equipped with some particular technical apparatus.

Today, in an unstructured cloud of big data, a finite population of interest may not be apparent or well identified. But in a selection from the cloud, one may be able to spot and retain data belonging to a set of units with confirmed population identity, so in principle the assumptions hold.

The research tradition has hardly changed or shifted in the roughly one hundred years of a survey science committed, since 1940, to the idea that “a small sample may suffice”, a small proportion of a population consisting of thousands or millions of units.

The formulation of the research tradition is simple, to the point of sounding trivial. But it is not redundant to spell it out here. That the tradition, as formulated, will survive is not evident. It depends, among other things, on how well the notions “identity” and “identifiability” will manifest themselves in future big unstructured data volumes.

8. The population assumption

“Finite population” is a crucially important concept. This notion of a finite number N of units, with identity, among which we observe a few, is the basis for what many consider “a beautiful theory”. It gives survey theory a unique place within statistical science, sets it apart from the common mathematical perception of sample as “iid observations on a random variable”, where iid stands for independent identically distributed.

Tracing the history of the population assumption takes us back to the life and work of Adolphe Quetelet (1796-1874), Belgian social scientist. In the 1830’s, he had a clear notion of “a collection of units with

identity”. At that time, there were no complete registers of the individuals in a country or in a region, but there were registers of births, deaths, marriages. Quetelet analyzed such data extensively. Later, the view of “finite population of interest” as one composed of households, or business firms, or schools, or hospitals, or automobiles, or trees in a forest, was an extension by modern day survey statisticians.

9. The sample assumption

The assumption reflects the contention that “a sample will suffice”. But not any sample. Its critical role is to deliver valid conclusions about the population. A poor sample gives poor results. The selection must “reflect the population” in ways that make users trust the statistics produced from it.

In a diffuse case, nothing is known about how units were recruited to the sample, only that its units belong to the finite population of interest. That information is not insignificant. Access to n observations known to be from the population of interest conveys some information. Statisticians are called on to make the best of it. But official statistics production sets high standards. Governments request valid, precise, accurate, trustworthy conclusions about the state of the nation. That takes “a scientifically structured sample”.

The sample assumption nevertheless allows a great deal of freedom in sampling the population. This has fascinated many over the years. The literature abounds with ingenious ways to sample. It resulted in an immense flow of published research, articles and books. An example is the vast array of methods capitalizing on one single but attractive idea, probability-proportional-to-size sampling. It is a collection methods that may not be very different from one another, in a global perspective. But those variations on a single theme were, or can still be, intriguing study objects or challenges for mathematically minded students.

As formulated, the sample assumption does not require probability sampling. But such sampling has been dominating in the tradition. The profession became convinced of its excellence, hence the popular term “paradigm”. Several reasons contributed to the success. One is the feeling of impartiality: everybody has a chance to be in the sample. Every member of the designated sample possesses a known positive inclusion probability, which gives a method for measuring closeness of estimates to the finite population parameters of interest.

However, the rigor of probability sampling is now seen by some as a detriment, a hindrance, unnecessarily restrictive and too expensive for modern survey conditions. If the research tradition survives, the rich possibilities in the sample assumption will continue to fascinate researchers and students. It will continue to require that a designated sample be fit for making trustworthy conclusions and estimates. However, non-probability sampling is not excluded, as long as it can deliver the required high quality sample. The research tradition has examined and used some forms of non-probability sampling in the past. Quota sampling is a prominent example; other notable names are model based sampling, expert choice sampling, purposive sampling, cut-off sampling.

10. Progress in survey science

Every science worthy of its name must deliver progress. This holds for the survey science research tradition as well. *Problems* were examined. Time passing, they were discussed, became more or less well resolved, aided by *theories* with varying degrees of success.

Much activity in the tradition focused on inference about the population via the sample drawn from it, that is, on estimation theory. Some theories were rivaling, but progress and understanding was furthered. Properties of estimators, bias, variance, mean square error (MSE), were in focus. Almost an obsession in the research tradition was “find-a-better-estimator!”. Even though only marginally “better”, it was the kind of novel evidence that earned many a student his/her Ph.D. degree in statistics

Over the years, the research tradition addressed and resolved, wholly or partially, a range of problems. In survey science, problems originate and reside (a) with the finite population (subpopulations, small areas, rare populations), and/or (b) with the sampling procedure (frame coverage, choice of sampling design, selection routines and algorithms), and/or (c) with the attributes of the data collection (modes, nonresponse and other imperfections, access to registers and auxiliary variables, recourse to “alternative sources”, such as big data).

A problem worthy of study is typically triggered at a certain point in time. It draws attention, catches the interest of researchers, rises to notoriety, inspires a research activity that may be both intense and of long duration. Many a problem originated in the sphere of the national statistical offices, often articulated by them. Much research in the tradition catered to the needs of those offices. They have, to a considerable degree, shaped the agenda for decades of research and progress in survey science, to serve national interests.

Small area estimation is an example of a problem arising from the idea of a population as a set of units with identity. Vehiculed by national statistical offices, it came to the forefront in the 1970's, by rising demands in several countries for reliable statistics for subpopulations (domains), in particular not-so-large administrative areas of the country, about which important regional decisions may nevertheless be taken. A sample survey designed to deliver statistics of high, or at least adequate, quality for the whole country will often fail to do so for smaller subpopulations, because of insufficient sample falling in those domains. But sampling enough in every domain makes survey cost prohibitive. The field of small area estimation research was thus born, one might say out of a clash between a country's need of reliable regional statistics on the one hand and the same country's unwillingness to pay for it on the other. The field became an important research agenda.

The following is an incomplete list of other problems that the research tradition came to deal with at length, over the years:

- Efficient stratification of the finite population.
- Methods for probability-proportional-to-size sampling (pps sampling).
- Permanent random number sampling theory and its use in controlling response burden.

- Locating and assessing potent auxiliary data; methods for their use.
- Statistical analysis of data collected by complex survey design.
- Survey nonresponse: To reduce it during data collection, to adjust for it afterwards.

11. Theories in survey science

Over the years, progress in the survey science research tradition was relayed by important *general theories* of inference for the finite population. They can be competing, even to some degree contradicting, views on the estimation of finite population parameters. They came to be known under names familiar to many:

- Design-based estimation (around 1920)
- Model-based estimation (around 1970)
- Model assisted estimation (around 1975)
- Small area estimation (around 1975)
- Calibration estimation (around 1985)

Dates in parenthesis are approximate starting points. Admittedly rather arbitrary is “the importance” of a stream of scientific thought before it merits the term “theory”. In any case, extensive scientific literature, and many applications, developed around each of those mentioned. There was progress, a widening understanding of the field.

A striking feature is the importance that years after 1970 seem to have. I prefer to call the last four theories, not paradigms. One should remember that “design-based” refers to the stochastic structure dictated by the probability sampling design, that model assisted estimation usually means model assisted design-based estimation. GREG (generalized regression estimator) was an early product of the concept, applied more generally later.

In his Waksberg award article, Brewer (2013) notes that the history of survey sampling has been “remarkably controversial”. He cites three occasions where new theory posed a sharp challenge to the established mode of thinking, resulting in vivid discussion and exchange, in the published literature or in other professional and scientific meeting places. The third of his examples is the debate, in the 1970’s, opposing design-based inference and model-based inference. In hindsight, this is no surprise; animated discussion, even controversy, is natural rather than exceptional in science.

Survey science is about methods. It proposes, examines and compares methods. There is no one “true theory”, one capable of ruling out alternatives as necessarily wrong or false, as might happen in an “exact” natural science.

In survey science, a researcher often resorts to examples suggesting that his/her preferred theory “is better”; more precisely, better than what a rivaling theory accomplishes under the same conditions, with similar data. But it is hard in survey science to give convincing evidence that one’s preferred theory is “superior” or “better suited” for all finite populations, for all survey circumstances.

Therefore, several alternative theories – even though to some degree contradicting – have a good chance of surviving. Exploring alternative theories has in fact been a vital part of the long research tradition in survey science, especially in the last several decades.

12. Cumulative progress

In philosophy of science, this term (see, for example, Laudan, 1977) means that new theories and methods are added within a structure, such as a research tradition, without rejecting any earlier accepted theory and methods as wrong. Problems are solved, more and more knowledge and methods are added to an already existing “store of knowledge”. There is a gain of knowledge without any corresponding loss. It may be triggered, at a certain moment in time, by the solution to a difficult problem, promptly encouraging further progress in a cumulative fashion.

Prestigious sciences evolve in an aura of cumulative progress. A mark of a successful science is that its progress is cumulative. It bears witness to a firm foundation on which continued discovery and development will build.

Examples we can admire are in physics, with the understanding of the atom’s structure, and in neuroscience, with the understanding of the human brain. As for the former, a period of more than 100 years gave progress in cumulative fashion: subatomic particles – electron, proton, neutron, meson and so on – were admirable discoveries. The world learned more and more about the composition of the atom. Cumulative progress was crowned with Nobel prize distinctions.

13. Cumulative progress in survey science

In my opinion, the research tradition in Survey Science brought, from 1926 and until recently, a long and essentially uninterrupted period of cumulative progress. Knowledge accumulated, new methods emerged, without rejecting older methods as totally wrong, albeit some of them inefficient.

It is appropriate to recognize two sub-periods: *Cumulative progress I*: 1926-1970, and *Cumulative progress II*: 1970-recently. The first sub-period, 1926-1970, contributed a stream of probability sampling designs. They became known under familiar names. Sampling could be stratified, clustered, systematic, proportional-to-size. Designs called “more complex” were in two or more stages, in two or more phases. All those were studied, extensively used in practice, especially by NSOs; they gave valid, reliable estimation. Early on, university students had to learn “one sampling design after another”, until a common

theoretical frame would cover all of them, the inverse inclusion probability weighting, $1/\pi_k$, as in the Horvitz-Thompson estimator.

The second sub-period, 1970 until recently, was a continued expansion, situated to a degree – but not exclusively – within the probability sampling framework. Today it seems almost as an overdue development of a tradition that emerged as early as in the 1920's. What generated it? It turned around two concepts, *auxiliary information* and *model*. Striking contributions to the literature in the 1970's had put a spotlight on the two concepts.

Broadly speaking, variables are auxiliary if more is known about them than just their values for the designated sample units. Researchers came to better distinguish and understand auxiliary variables, to design methods that can profit from them in estimation, such as the model assisted generalized regression estimator, GREG. The Scandinavian countries were privileged, with many high quality registers from which to extract valuable auxiliary variables.

Auxiliary variables, and their use, immediately suggests the notion of *a model* for the perceived relationship that those variables (*x*-variables) may have with the survey variables (*y*-variables). Auxiliary variables with little relationship to the survey variables are of little help, of little interest. Cumulative progress in the research tradition was propelled by those broad estimation theories built on relationship: *model based, model assisted, calibration, small area*.

Those theories widened the field, each of them elaborated in extensive scientific literature. Proponents of one or another of the theories took part in animated discussion. A certain hesitation in “conservative circles” had to be overcome until general trust was gained in methods that appeal more or less overtly to “models”. Once doubts subsided, NSOs in many countries delighted in producing more accurate official statistics, aided by auxiliary variables, as in model assisted design-based estimation, or with calibrated weighting. This could take place at no significant cost increase. It was a low cost transition, appreciated by NSOs.

14. Discussion

This article has examined the long standing research tradition in survey science. A new phase may be foreshadowed, similar in a way to the period 1895-1926: doubt, skepticism and uncertainty, but troubled at this time with concerns about high cost of accredited probability sampling surveys and about trustworthiness of surveys that may rely on inexpensive new data sources.

The research tradition was formulated in Section 7 in terms of two vital assumptions: (i) the perceived existence of a set of units with physical or legal identity, the finite population of interest; (ii) data collected for a designated subset of units, a sample, such that valid and reliable conclusions are guaranteed. Central notions are identity and identifiability of units; the latter allowing population units to be designated to a sample.

If the assumptions fail to be met in the future, a new research tradition may emerge, focused on possibly other central concepts than identity and identifiability of units in a perceived finite population. This would amount to a radical change in the orientation of the discipline. Consequences would be keenly felt in practice at NSOs and elsewhere.

Probability sampling began in the early decades of the twentieth century. It took until the 1930's for it to become a norm for survey theory and practice. Most of the following points were made in this article:

- The survey science research tradition progressed successfully for a number of decades.
- Probability sampling played an important role in this.
- Much of it took the prestigious form of cumulative progress.
- Significant advances, especially from 1970 and on, were propelled by uses of auxiliary information and by reference, in different ways, to models.
- The theory of estimation built on probability sampling requires, in principle, full response, making it in a sense a vulnerable theory.
- Response rates deteriorated, in some instances to very low levels.
- The established theory found itself compromised by high nonresponse, contributing to high data collection cost.
- Much work in the research tradition was devoted to amendment and adjustment of theory and method, especially with respect to nonresponse.
- Further significant cumulative progress may not happen under probability sampling.
- National Statistical Offices are not disposed to lend support and fund research to confine non-response to acceptable low levels; they indirectly encourage a paradigm shift.

The challenge for the future remains: To ascertain and to demonstrate, in quantifiable measures, or in convincing arguments, that the sample data – the outcome of a terminated data collection – have “desirable make-up”, a “reassuring profile and presence”, vis-a-vis the finite population of interest. This was so under probability sampling with a 100% response rate. The research tradition faces the future challenge of guaranteeing quality, to reassure users that “estimates are valid”, under new conditions, with convincing new theory. It is not a simple task.

15. The changing reality

Admittedly, it is unfair, as done in this article, to compare survey science with physics. The two sciences evolve in totally different worlds or contexts.

Survey science is not about studying a fixed reality, as when the atom is studied in physics, or when the understanding of the human brain is progressing in neuroscience. The reality of survey science is the changing world around us, the constantly evolving society we live in. It is radically different today,

compared with 80 or even 50 years ago. Methods useful and relevant 80 years ago may be impracticable or ineffective today. Society is different, so are survey conditions. Times are gone when the population at large saw it as a civil responsibility to co-operate in surveys. Nonresponse and non-participation is high, even in surveys designed to meet the nation's most important needs of reliable statistics, such as those about the labour force.

Today, private survey institutes engaged in opinion polling rely extensively on sampling from a surrogate population called a panel. It is typically claimed to be a scientifically chosen, representative selection of people in the country, a reliable "mini-population". Survey institutes sample from the panel, claiming to show society's opinion about important matters. It saves cost, it saves time, compared to sampling from the population itself.

Units recruited to the panel units may have a verified population identity, but the recruitment process is in question. Contributing to doubts are refusals to join the panel, on the part of many units invited to join; it is then not a trustworthy picture of the population.

Be that as it may; the panel technique is used, but it violates the first assumption of the research tradition formulated in Section 7 (unless the panel is a bona fide realization of a first phase of probability sampling). The panel technique attempts to face today's harsh reality, to reduce cost, to step outside an incumbent research tradition considered too demanding.

Survey science will continue to progress. But the cumulative progress brought by the present research tradition, especially in the last fifty years, may be over. The future may have to rely on radically different support.

"We need a new paradigm." That is easy to say. It does sound constructive. It does not happen instantaneously. A new norm for theory and practice rests on an implied consent, genuinely felt throughout the community of scientists and practitioners. It takes a long time to accomplish.

Today we may see precursors of scientific revolution in survey science. But it is not that existing theory failed to perform, nor that it must be abandoned because of anomalies. That can cause revolution in a prestigious natural science. But in survey science, reasons are more mundane: concerns about high cost, with respondents' unwillingness to co-operate.

If frustration continues to mount among key protagonists, such as the National Statistical Offices, then scientific revolution may take place. A new paradigm seems far away; it must be rooted in a powerful new statistical notion, one with appeal and convincing power, comparable to that which probability sampling was able to confer, in convincing and solid support of the more than century old discovery that "a small sample will suffice".

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