

# TOS

f o r u m

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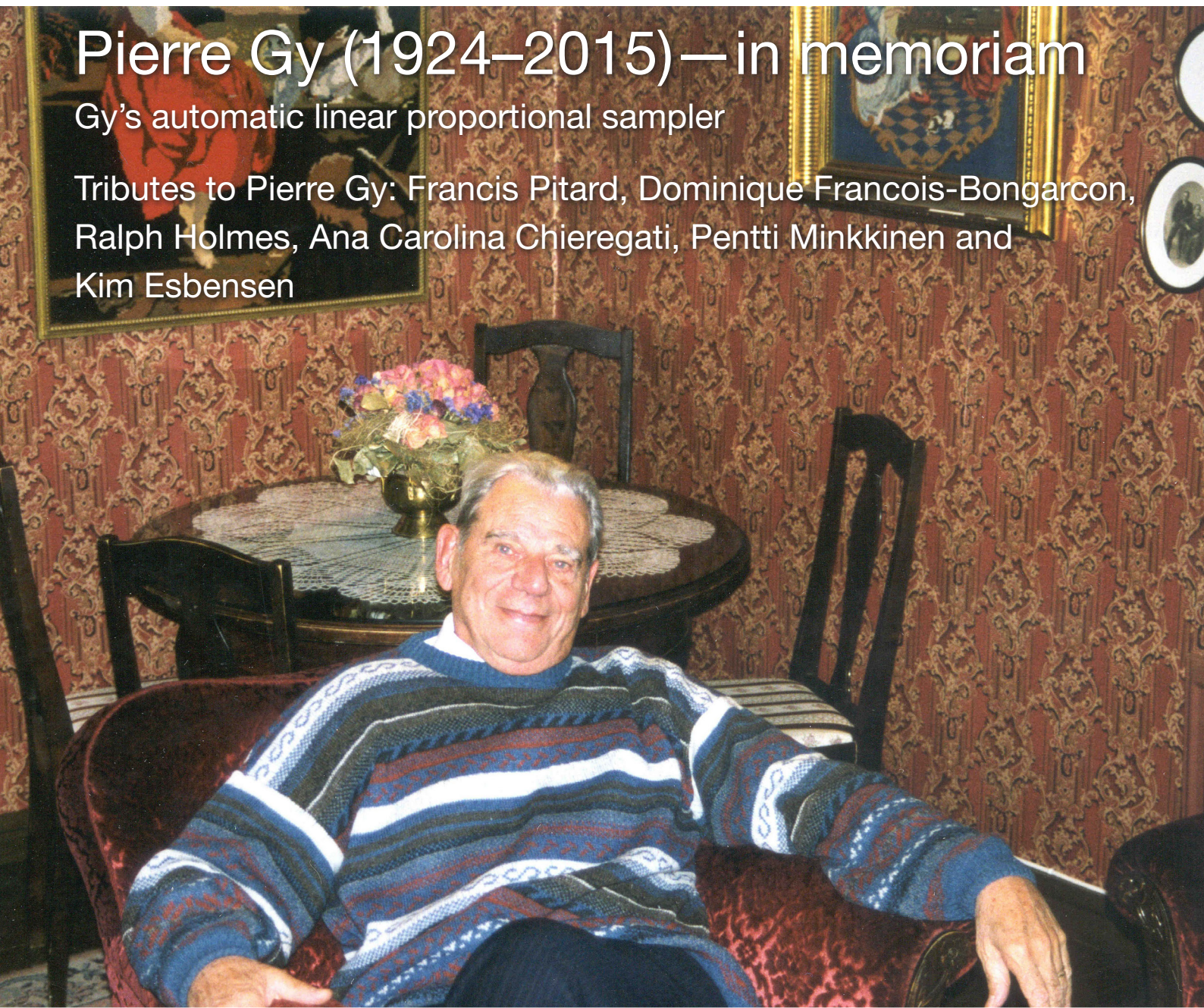
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# Pierre Gy (1924–2015)—in memoriam

## Gy's automatic linear proportional sampler

Tributes to Pierre Gy: Francis Pitard, Dominique Francois-Bongarcon, Ralph Holmes, Ana Carolina Chierigati, Pentti Minkkinen and Kim Esbensen



FORUM FOR THEORY AND PRACTICE OF REPRESENTATIVE SAMPLING (TOS)

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**Pierre Gy**

Family and science







Ana Carolina Chieregati was the last of the sampling community to meet with Pierre in Bordeaux, the day after WCSB7, 2015. [Her tribute to Pierre can be found on page 35.](#)

## Editorial

The founder of the Theory of Sampling (TOS) passed on 5 November 2015. The sampling community was shocked to the core and has been grieving since. We are only now beginning to get to grips with the fact that **Pierre Gy** is no longer with us. However, a passing is not only grief and sorrow—after a time it is also the beginning ... the beginning of a celebration of a life extraordinarily well lived, of a man in full as regards both family and his professional scientific achievements which are *sine qua non*. It is a clear obligation to our community to dedicate a full issue of *TOS forum* to his fantastic life and extraordinary achievements. In this issue is contained a wealth of evidence to his interactions with a broad spectrum of colleagues and friends, in particular in the form of personal tributes from five Pierre Gy Sampling Gold Medal awardees. We are also deeply grateful to Elsevier BV for kind permission to reprint Pierre's last five papers, originally appearing as part of the WCSB1 proceedings (2004). This is but a small selection from a scientific *oeuvre* that contains more than 250 entries. But we are confident it is a fitting excerpt because this is where Pierre Gy found occasion to write his personal history and overview of the background, gestation, development and 25 years of application of what came to be known as the Theory of Sampling. Pierre here presents a new overview of the structure of TOS based on his very long professional career and the many reactions he received during five decades. He was very happy with this opportunity to review and complete his TOS in this context. The reprints clearly show the spirit of the first-ever world conference in sampling, which was dedicated to his honour in its entirety. Please take a moment and peruse his complete bibliography, which tells its own succinct story of the period 1950–2003.

It is not an exaggeration to say that the activities of the sampling community are markedly divided: *before* and *after* WCSB1, and many of the tributes comment on this and describe the accelerated scientific and individual interactions that resulted. However, Pierre Gy himself was sadly never able

to attend any of the subsequent WCSB conferences. Never-the-less, contact was not lost; how this played out is also outlined in the tributes. This issue has been put together to illustrate as many as possible important episodes in Pierre's professional life from 2003 to 2015, for the benefit of all young(er) members of our community. The Editor thanks Phillipe Wavrer for writing a relevant paper on Pierre Gy's famous linear proportional sampler.

The last meeting with Pierre is naturally in focus, as a group of close friends and colleagues visited him at the caretaker home in Bordeaux where he spent his last years. This occasion is made all the more important because it took place at the last day of WCSB7 conference which was held on 10–12 June 2015. This meeting is well covered in this issue for obvious reasons. As it turned out one of our community, Ana Carolina Chieragati, who was unable to partake in this visit, undertook on her own volition to visit him on her way to the airport the day after. Thus history decided that Ana Carolina would be the very last of our community to meet with Pierre. Her tribute is included in this issue for this very reason. Although very brief it is without doubt the most poignant farewell on behalf of all of us who respected and loved Pierre. Ana Carolina's photos from this visit happened to capture Pierre in extraordinarily good mood and spirit, which grace this issue. The Editor is also very grateful to the Gy family for permission to include a selection of family photos.

Pierre Gy has now passed on the baton to the entire sampling community—we accept this solemnly and will work tirelessly to honour his legacy.

Kim H. Esbensen



Special Issue  
honouring  
Pierre Gy and his  
lifetime's work



## Be part of the next issue of *TOS forum*!

We welcome contributions to *TOS forum*: articles, letters, comment, news or news of PhD projects for the PhD Presentations column.

*TOS forum* Editor, Kim Esbensen, would be pleased to discuss any ideas you may have and to receive your contributions.

### EDITOR

Kim H. Esbensen (Consultant, KHE Consulting, [khe.consult@gmail.com](mailto:khe.consult@gmail.com); Adjunct Professor, Aalborg University; Adjunct Professor, Geological Survey of Denmark and Greenland (GEUS), Copenhagen, Denmark)

### PUBLISHER

Ian Michael ([ian@impublishations.com](mailto:ian@impublishations.com))

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6 Charlton Mill, Charlton, Chichester, West Sussex PO18 0HY, UK.

Tel: +44-1243-811334;

Fax: +44-1243-811711;

E-mail: [subs@impublishations.com](mailto:subs@impublishations.com)

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Editorial correspondence to Kim Esbensen, [khe.consult@gmail.com](mailto:khe.consult@gmail.com). All production correspondence should be sent to *TOS forum*, 6 Charlton Mill, Charlton, Chichester, West Sussex PO18 0HY, UK, Tel: +44(0)1243-811334, Fax: +44(0)1243-811711, e-mail: [ian@impublishations.com](mailto:ian@impublishations.com).



## 50 years of Pierre Gy's "Theory of Sampling"—WCSB1: a tribute

### Abstract

This Special Issue is dedicated to one man's distinguished achievements in science and technology: 50 years of Pierre Gy's "Theory of Sampling" (TOS); the First World Conference on Sampling and Blending (WCSB1) was dedicated to this same purpose. This tribute covers a summary of Gy's professional career with a natural focus on TOS and its relationships to fundamental as well as applied sciences and technology, and why it is timely and fitting to present Pierre Gy with the tribute of WCSB1.

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### 1. Perspective

To put this tribute in perspective, I suggest a view of the situation in one "representative" discipline, which discovered the Theory of Sampling (TOS) only a relatively short time ago—and which is currently getting to grips with the reality that much of its "data" may in fact have been generated by processes which are more or less in neglect of TOS. For heterogeneous materials, this is far from a trivial matter: from TOS' 50 years of extensive experience in the largest industry sectors of the world which routinely have to deal with reliable sampling/analysis, it has been amply demonstrated that non-representative sampling always is a very costly affair, economically as well as scientifically.

### 2. Struggling chemometrics

Chemometrics is barely 30 years old, but already there are many stories, if not legends, of how difficult it originally was breaking new ground within the various sciences in which chemometrics went to work: analytical chemistry, process technology, industrial manufacturing, engineering, geology and medicine to name but a few. Difficult, because this new holistic type of data analysis apparently competed with the traditional and well-established discipline of statistics, which was not taken lightly, but frankly also because chemometrics rocked the boat too much in many complacent fields. Many scientists, also from other sciences than chemometrics, have experienced a similar struggle for recognition in their

own career. Some have struggled much longer than others, perhaps up to a decade before a breakthrough occurred. A select few of the founding fathers and the first circles of pioneers within chemometrics have consistently contributed to the struggle for this "new discipline" for the last 25 years, more or less in a constant battle mode, when addressing ever new disciplines. Oftentimes, what kept us going was (only) the essential esprit de corps within the homeland of chemometrics. One can respectfully look to the present state of chemometrics with pride for all the efforts put in. At the end of the day when all these stories have been told, chemometricians have every reason to be very proud of what we have achieved. We can demonstrably point to the current healthy status and remarkable drive within our discipline.

### 3. TOS

Consider now a parallel story, only substitute chemometrics by sampling, the Theory of Sampling—and double the length of this period—not 25 years, but 50 years. Also, consider that you would have to do this work mainly on your own for most of this extended time...

Unusual?—To say the least.

Below are the story behind the man behind TOS and the story of the tribute to this remarkable scientist. Pierre Gy tells his own scientific story behind TOS in part IV of his five-part series of contributions to the proceedings part of this Special Issue.



#### 4. Biography: Pierre Maurice Gy

**Family:** b. Paris, July 25, 1924; s. Felix and Clemence (Gourdain) Gy; m. Sylvia Duchesne, 1946; children: Genevieve, Anne, Caroline.

**Education:** Degree in Chem. Eng. Paris Sch. Physics and Chemistry, 1946; PhD Physics, U. Nancy, 1960; PhD Math., U. Nancy, 1975.

**Memberships:** Mem. AAAS; Am. Inst. Mining Engrs. (hon.); Can. Inst. Mining and Metallurgy; N.Y. Acad. Sciences.

**Honours:** Medal, Mining and Metall. Inst. Japan, 1958; two gold medals Soc. de L'industrie Minerale, 1963, 1976, Lavoisier medal French Soc. Chemistry, 1995.

**Publications:** 9 books; 175 papers; innumerable lectures, courses.

**Professional career:** Chem. Engr. CMCF, Congo, 1946–1949; Research Engr. Minerais and Metaux., Paris, 1949–1952; from Head Mineral Processing Labs to Tech. Mgr., 1952–1962; Industrial Sampling and Blending Consultant, Cannes, 1963–present.

**Founder:** Internat. Sampling Inst., France.

**Avocations:** Photography, mountain climbing.

#### 5. From SSC6 to WCSB1

Pierre Gy was originally invited as a special guest lecturer to SSC6 (the Sixth Scandinavian Symposium on Chemometrics) in August 1999. While a contribution on the "Theory of Sampling" was planned for the SSC6 proceedings, it quickly transpired that it would not be possible to do anything even remotely close to the justice this science deserve with an average-length proceeding contribution (10–12 pages or so). Thus, the idea of a more fundamental introduction to TOS in chemometrics and related data analysis sciences was born. The lack of knowledge of TOS in this and related scientific areas was simply too great. It was decided that now was the time to present a more comprehensive introduction. The First World Conference on Sampling and Blending, WCSB1 was the result.

#### 6. 50 years anniversary introduction to TOS

A special situation now opened up. It was too much of a coincidence that TOS originated in 1950, and that the first presentation and published article appeared in 1951 and 1953, respectively. Soon, one idea followed the other, while the 50 years anniversary of TOS loomed large and suddenly very close. Knowing full well the intense workload involved, I nevertheless summoned up the courage to ask Pierre to write a new, updated introduction to the entire Theory of Sampling, complete with a comprehensive bibliography. The then 75-year young

gentleman initially balked somewhat at this suggestion, quite understandably, but after reconsidering the issue several times he came back with a scholarly opus in the form of a tutorial series which, instead of being published in the above 1999 SSC6 proceedings, you will find occupying the leading place in this proceedings issue of WCSB1 (2003). We owe Pierre a very great thank you for his willingness to undertake this hard work.

#### 7. The "autobiography" of TOS

But what originally appeared flat out impossible was to have Pierre write his own personal scientific history. Those who know him well also know that Pierre is a most generous and gracious man—he is willing to do almost anything to grant the wish of a fellow scientist or a friend. But ask him to talk of himself as a scientist and of his scientific achievements—this is where the "almost" comes in; this is where the story usually ends. He simply is extremely disinclined towards anything that even remotely perhaps could be viewed as "self-agrandissement". Nevertheless, there was no end to the sweat and toil that went into trying to convince Pierre why Science (capital S) also needed such a biography of the originator of TOS... Suffice to say that in the end the wish of the science community at large prevailed, and success for the editor came in the form of: "50 years of Sampling Theory—a personal history". This is where Pierre tells his own scientific history of TOS within the convoluted industrial, academic, professional and personal web of his life. Fascinating is but a poor qualifier. Not many scientific disciplines have had the opportunity of a similar personal introduction to an oeuvre as monumental as that of Pierre Gy's—and TOS'—contribution to the field of science and technology. You will hopefully be greatly pleased also to read this contribution in this issue.

#### 8. The significance of TOS

TOS is of the highest significance for all sciences, for technology and industry where proper sampling is on the agenda. Sadly, it has for most of its existence been largely overlooked in academia, if not totally neglected. This in no way has anything to do with the scientific content and/or the merit of TOS however, perhaps rather the opposite: For some, the mathematical language of TOS may appear somewhat difficult at first sight, while others only gradually will appreciate the full depth of the (very) practical sides of TOS, since it necessarily first must start out delineating the universal principles of correct, i.e. representative sampling. Apparently, for a select few, it may even be viewed almost as a personal affront that "data quality" (never much in focus since



this usually is considered the responsibility of “other disciplines”) may in fact justifiably be subject to a fundamental TOS-questioning coupled with a call for greater sharing of the practical sampling responsibilities between analytical chemistry, process technology, data analysis, statistics and indeed also chemometrics.

However, in the last 10 years or so, some improvement in this state of affairs has been noticeable. Pierre's own 1998 Wiley book: “Sampling for Analytical Purposes”, as well as the many current didactic efforts by an entire younger generation in this field (papers, books), have well begun to eliminate these stock objections to proper scientific attention to TOS. Today, there are relatively many types of courses taught on TOS, mostly by professional consultants and experts, but also in academia the situation now finally shows significant signs of change, even though there is much work to be done.

Primary recognition for the important work of teaching sampling, TOS, within academia in general, within chemometrics and process technology in particular goes to the spearhead efforts of Pentti Minkkinen of Lappeenranta University of Technology, Finland. He has almost completely single-handedly lifted the burden of starting teaching TOS at the university level for about 20 years now, for a long part of this time also in splendid isolation within chemometrics.

### 9. WCSB1 proceedings—also to be used outside chemometrics

WCSB1 and these proceedings can only be a relatively brief introduction to some of the most important reasons why the scientific community is finally presenting a fitting tribute to a singularly inspiring scientist's contributions—in the form of a conference dedicated exclusively to sampling. The proceedings from WCSB1 will also be used as a general introduction to the role of proper sampling within a much broader range of sciences and technological professions outside the comparatively narrow niche of chemometrics: analytical chemistry, mineral processing, process technology, engineering and technology, pharmaceutical—food, feedstock manufacturing, geology, geochemistry, clinical chemistry, medicine... These proceedings are meant to transgress traditional borders!

### 10. The man behind TOS

Before focusing on the scientific achievements of WCSB1 in the rest of this Special Issue, this tribute also presents a few of Pierre's more personal attributes, some of which perhaps could be considered to fall outside the strict scope of science and TOS, but which are never-the-less important in giving a more comprehensive picture of the man behind TOS.

### 11. The scientist



The bread-and-butter of scientific interaction: meetings, symposia, conferences ...

Pierre Gy, extreme left, was 30 years old when this picture was taken in 1954. OECC Mission (North America).

At the extreme right of picture: the Norwegian representative Prof. Magne Mortenson, Technical University of Trondheim.

For a man who has published 9 books, 175 papers, given more than 200 lectures, workshops and courses, it bears noting that Pierre Gy has never held an academic position at a university (sic), but has deliberately chosen to work mainly without the daily personal interaction of the scientific and social interaction at the workplace. There is little need to emphasise how this runs contrary to the gamut of every day conditions for the overwhelming part of most scientists. Unusual?—Indeed!

### 12. The sportsman



1975, Suisse-Italie.  
Au pied du Monte-Rosa  
4600 m.

Pierre Gy in 1975, Suisse-Italie (just turned 50).



It is only some 10 years ago that Pierre, very reluctantly, stopped taking his regular (weekly) long distance swims (10-km laps or more...) in the Mediterranean, and he last scaled the Alps in the summer of the year of 1999—as indeed he has done in almost every year of his adult life when vacationing with his wife Sylvia and family in the northern Alps.

### 13. Concluding remarks

This author considers it a privilege and a great honor to have been able to perform the academic public service of organizing, hosting and chairing WCSB1. I thank Dominique Francois-Bongarcon for our many ultra-short airport encounters and his spirited personal encouragement in this task and I especially thank Pentti Minkkinen for his role as facilitator for this Special Issue, without which we could not honor Pierre Gy for his life's work in this most appropriate manner.

### 14. The future of TOS

Pierre Gy ends his personal account in this Special Issue with the statement: "The relay is now beginning to be handed over to a younger generation of professors, engineers and other proper samplers".

To this the three of us say, most profoundly:

"May this beginning last very long indeed, Pierre—mentor and scientist extraordinaire!"



Pierre Gy with wife Sylvia at the WCSB1 banquet, August 2003.

Kim H. Esbensen  
 ACABS, Aalborg University Esbjerg, Denmark  
 E-mail address: kes@aue.auc.dk.  
 Tel.: +45 7912 7688; fax: +45 7545 3643.

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## Part IV: 50 years of sampling theory—a personal history

Pierre Gy

*Res. de Luynes, 14 Avenue Jean de Noailles, F-06400 Cannes, France*

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### Abstract

This last part, which is a rather personal history of the development of the theory of sampling, is written in the first person singular—for a reason. For a long time already, I have been asked to tell how I became interested in sampling and how I developed the theory. I don't like to speak of myself and I have hitherto refused to do so. I have always been reluctant to accept such an undertaking, at least as long as I thought that my work was not completed. It would now appear that this is no longer the case and so, when the editor requested also the present Part IV as part of the series, he originally invited me to write for the *SSC6* proceedings (see Introduction to this issue), I finally ran out of excuses and obliged (in point of fact, it took much more than a mere "request"). Upon reflection, I am very grateful for offering me this opportunity.

The development of the theory of sampling has been a solitary work from the very beginning. With the exception of the "variogram", a mathematical tool borrowed from geostatistics and Matheron [23] in 1962, I did not use any pre-existing scientific work. On the other hand, no one or no body such as university, school of mines, research organization or industry, even my own employers, *ever* asked or encouraged me to search in this direction and nobody ever paid for my research work (with an exception concerning the theory of "bed-blending", which was sponsored in 1978 by a blending equipment manufacturer—exception duly mentioned in my publications). Unusual.

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*Keywords:* Theory of sampling; Bed-blending; Binomial model

### 1. How the seed was planted

From 1946 to 1949, I worked as a Mineral Processing Engineer in a small lead mine, north of the lower course of the Congo River, in the middle of the equatorial bush that covers what was at that time the French Congo (or Congo-Brazzaville). I was in charge of the processing plant and of the laboratories. In 1947, I received a 1-week-old "cable" from Paris asking me to provide the head-office with an estimate of the average grade of a huge heap of lead concentrate of dubious quality, stored in the open since 1940, to study the possibility of its re-treatment. I discussed the question with the mine manager and I soon realized that:

- I was asked "to sample" a batch of some 200,000 tons that contained blocks weighing anything between several tons down to microgram particles.
- I knew nothing about "sampling".
- The available literature (very scarce at the equator), was mute, naive or vague at best.

- I had to *improvise*, which I did as best I could—which was not very good.

The seed was planted, but I did not realize it was the starting point of a lifetime's work (Fig. 1).

### 2. State of sampling theory, anno 1949

Back home in France in 1949, as I was in charge of a mineral-processing laboratory in Paris, our team worked on a huge variety of ores and minerals from all over the world. I soon found out that I had to solve sampling problems practically *every day*. The literature available in Paris, though more comprehensive than the one I had access to in the Congo, did nevertheless still not provide me with any satisfactory answers. All authors on sampling (few and far between) had dedicated their work and energy to answering the question "how much", i.e. "what is the minimum sample weight necessary to achieve a certain degree of reliability".





Fig. 1. 1947 (aged 23). First job, near M'Fouati lead mine, Middle Congo (200 km from Brazzaville), in the by then French Equatorial Africa.

For instance, more or less arbitrary formulas were proposed according to which the minimum sample weight had to be proportional to the *cube* of the top particle size (Brunton, 1865) or, which makes a serious difference with coarse materials, to its *square* (Richards, 1908).

Brunton's formula was based on very reasonable considerations of geometrical similarity: the idea was that, at different sizes, the same number of fragments was required. That of Richards was based on (quote) "...the fact that the quantities proposed by Brunton's formula were much larger than those accepted in practice"... "the most satisfactory rule must be based on habits acknowledged by the trade of minerals" (in this year 2000, i.e. nearly a century after Richards, the same philosophy is implemented by ISO Technical Committees)... "by adopting the rule that the sample weight should be proportional to the square (sic) of the top particle size, one should obtain figures that have every chance of being approved by sampling operators" (end of quote). For a famous M.I.T. Professor, this can hardly be called a *scientific* or a *theoretical* approach.

As far as Brunton's formula is concerned, I was worried by the fact that the *constant proportionality factor* did not allow for the other physical or mineralogical properties of the ores and minerals involved, especially their variations in grade and density.

In the 1930s, the trade of coal comprised very large tonnages, as well as huge amounts of money, which were computed on the basis of *assays* (ash, sulphur, etc.) carried out on "samples". Various teams of researchers, mostly in the UK and the USA, realizing that sampling actually generated errors that could have a financial impact, had

launched *experimental studies* with the purpose of disclosing relationships between the properties of coal (especially percentage of ash, top particle size, etc.) on the one hand, the sample mass and the sampling variance on the other. Thousands over thousands of data were compiled but no clear conclusion could be drawn and no result could be extrapolated to other minerals, which supports Albert Einstein's statement: "a theory can be checked experimentally but there is no way to derive a theory from experiments".

In the 1940s, the Mineral Processing Engineer's Bible was the "Taggart" (first edition 1927; second revised edition 1945, *John Wiley, New York*). In the latter, I found a chapter on sampling, written by Prof. Hassialis, Columbia University, New York, that included a theoretical section based on a statistical multinomial model. This model was sound but involved a very large number of parameters that were never known. For obvious reasons, it could not be *practically* implemented. Fifty-five years later, I have never met anyone who did implement it.

In 1949, the French Mining Engineer R. Duval, searching the handbooks of statistics for a ready-made solution, proposed to approximate a batch of ore with a population of black and white balls (*binomial model*) representing pure valuable mineral and pure gangue, respectively. The model attributed *the same statistical weight* to the "balls", which implied that they had *the same physical mass*. This implicit assumption was so far from reality, where fragment masses could vary in a ratio of 1 to  $10^{18}$  and where the minerals were seldom "liberated" from one another, that it was practically worthless. Its results were dangerously misleading. This triggered a reaction from me: for want of any available solution adapted to the problem, I decided to study the question from a purely *theoretical* standpoint... and the seed, planted in 1947, began to germinate in 1949.

### 3. The 1950 theoretical approach

Sampling is always necessary for a single, simple reason: in most cases and for a question of cost, analysis can of course not be carried out on the *entire bulk* of the object, the "lot  $L$ ", to be valued. The practical purpose of sampling is therefore to reduce the mass  $M_L$  of lot  $L$  to the mass  $M_S$  of the "sample  $S$ " that will represent  $L$  in further operations and ultimately in analysis; the analytical result pertaining to the entire lot  $L$  is to be estimated on sample  $S$  without altering the composition "too much". This mass reduction must be realized by *selecting* a certain number  $N_S$  of "constituents" or "elements" (fragments in the case of particulate solids) from the population of  $N_L$  elements making up the lot  $L$ . The theory deals with a single sampling stage: the reduction of a certain lot  $L$  to a certain sample  $S$ . The analysis carried out on the ultimate sample, or assay-portion,  $S$  concerns a certain component of interest,  $A$ , which is called the "critical component". The objective of the interrelated sequence

“sampling+analysis” is to *estimate* its proportion in sample  $S$ . The proportion of  $A$  in  $L$  and  $S$  is called the “critical grade”, denoted by  $a_L$  and  $a_S$ , respectively. The objective of my initial research was to study the statistical distribution of  $a_S$  and that of the “total sampling error (TSE)” (more details have been presented in Parts I, II and III above).

Specifically, the idea was to develop a mathematical model with the purpose of devising a relationship between:

1. The variance of the sampling error (a random variable),
2. The physical properties of the material being sampled, assumed to be known, and. . .
3. The lot and sample masses.

From this relationship, the minimum sample mass to be extracted from the lot in order to achieve a given degree of reproducibility (characterized by a given *sampling variance*) could be derived. I was not yet interested in the distribution mean, i.e. in the *sampling bias*: at that time, nobody was. Sampling was universally regarded as a simple *handling technique*, the tools of which were an assortment of shovels, scoops, spoons and containers: the *theoretical question* “how” had never been posed by anyone. I did feel that this point was very important however, but I was not able to deal with it until 20 years later: *the reader should know that I had to carry out my research work in my spare time only, for I was not paid by my employer to carry out this kind of research.*

The theoretical model I first devised was derived for particulate solids of mineral origin such as ores, concentrates and much later for feed to cement factories, etc. irrespective of their nominal particle size. Later again, I was also able to formulate the following further developments:

- In a first generalization step, the theory was also applicable to *solids of vegetable origin*, such as, e.g., cereals or sugar beets, as well as *solids of animal origin* such as bones imported from India and Pakistan by the gelatine industry—and indeed *any* particulate solid.
- In a second generalization step, it was applicable also to *liquids and gases*, such as those to be controlled in the chemical, pharmaceutical, oil or hydrometallurgical industries.
- More generally, with the development of environmental control, the theory was found valid also for sampling of the *rejects* of all kinds of human activities: household or industrial refuse, polluted soils, nuclear materials, etc.
- Matter is discrete, or discontinuous, by essence: with particulate solids the discontinuity appears at the scale of fragments (sizes expressed in centimeters, millimeters or micrometers). With liquids and gases, it is observed at the scale of molecules or ions (sizes expressed in Angstroms). The difference between particulate solids and liquids is thus not one of essence but rather one of scale—as far as sampling is concerned



Fig. 2. OECC Mission (Europe), 1953. Meeting on: “The beneficiation of low-grade ores”. As one of France’s two delegates (aged 29, second from right), I am lazily listening to some lecture (the memory is not quite up to the photographic documentation; I have forgotten where the meeting actually took place. . .).

of course. The general sampling model is valid irrespective of the component size(s); it would therefore appear applicable to all material “objects”, irrespective of their physical state (Fig. 2).

In the abstraction of the mathematical model of sampling, this theory seems therefore to have some form of universal validity. This point is attested by Richard Bilonick [25].

#### 4. The Formula

The 1950 sampling model assumed that the number  $N_L$  of elements (fragments) making up the lot, *a number usually very large, unknown but defined unambiguously*, was reduced, in one way or another, to a (much) smaller number  $N_S$  of fragments making up the sample  $S$ . My approach was to compute the mean and the variance of a population made of the grades  $a_S$  of *all* possible samples of  $N_S$  fragments, i.e. all combinations of  $N_L$  objects by groups of  $N_S$  units.

To remain as close to reality as possible, I had decided:

- In a first step, to take into account *all* parameters (unknown but well defined physically) characterizing *all* fragments  $F_i$ : i.e. the grades  $a_i$  and masses  $M_i$ , as well as the numbers  $N_L$  and  $N_S$  of fragments making up lot and sample respectively, and to devise strict, indisputable, mathematical relationships, based on simple algebra.
- In a second step, to introduce *simplifications* and *approximations* in order.
- In a third step, to devise *practical formulas*, approximate but easy to implement.

At the end of the first step, I had devised strict formulas for the mean and variance of the population of



“equally probable samples of  $N_S$  fragments”. With today’s notations:

$$\sigma^2(TSE) = \left[ \frac{1}{N_S} - \frac{1}{N_L} \right] \frac{\sum_{(i=1 \text{ to } N_L)} h_i^2}{N_L} \quad \text{with}$$

$$h_i = \frac{(a_i - a_L)}{a_L} \times \frac{M_i}{M_i^*} \quad (1)$$

According to its definition, the TSE is relative. The variance of any relative error is *dimensionless*. In this expression,  $h_i$  is what we today call “the contribution of  $F_i$  to the constitutional heterogeneity of  $L$ ” and  $M_i^*$  is the average mass of all  $F_i$ . The role of  $h_i$  appears of great importance: it is the link between the concept of quantified heterogeneity I introduced, and later developed, and the sampling variance. Indeed, heterogeneity lies at the root of all sampling errors: the sampling of a strictly homogeneous material would be an exact operation. The theory of homogeneity and heterogeneity was presented in its definitive form since 1975 [15–20].

This basic formula (1) involves a sum extended to the  $N_L$  values of  $a_i$  and  $M_i$  that are well defined but remain always unknown. It is strict but *cannot* be directly implemented in practice. Today, with the computing facilities at our disposal, it would for example be possible to *simulate* all kinds of distributions of  $a_i$  and  $M_i$  and to compute  $\sigma^2(TSE)$  according to Eq. (1). Theory shows that  $\sigma^2(TSE)$  as expressed in Eq. (1) is a strict *minimum* (see below: Section 6). This is why I termed the corresponding error the “fundamental sampling error (FSE)” (formerly FE).

At the end of the second and third steps, I had indeed obtained an *approximate formula* for the sampling variance, which is often referred to (by others) as “Gy’s formula” (here referred to more simply as “The Formula”).

It can be expressed as follows ( $\approx$  “approximately but practically equal to”):

$$\sigma^2(FSE) = \sim \left[ \frac{1}{M_S} - \frac{1}{M_L} \right] c \beta f g d^3$$

$$= \sim \frac{c \beta f g d^3}{M_S} \quad (\text{when } M_S \ll M_L) \quad (2)$$

- $M_L$  and  $M_S$  represent the masses of  $L$  and  $S$ , respectively (expressed in grams).
- $c$  is a “constitutional parameter”. It takes into account the average grade of the material as well as the densities of all components. It has *the physical dimension (but not the meaning)* of specific gravity (always expressed in  $\text{g/cm}^3$ ). This parameter can vary very widely. The smaller the average content, the larger the parameter  $c$ . For instance, with an alluvial gold ore containing 1 g of gold per metric ton of ore (1 g/t=1 ppm= $10^{-6}$ ), its value is *1 million times* the density of gold ( $19 \text{ g/cm}^3$ ). With the feed to a cement factory, it is only a *fraction* of the density of limestone (ca.  $2.7 \text{ g/cm}^3$ ).

- $\beta$  (or  $\lambda$  or  $l$  for certain authors) is a dimensionless “liberation parameter”, which varies between 0 and 1 according as the components are thoroughly associated—or completely liberated—from one another. The estimation of the liberation parameter is often tricky, especially with gold ores. Francois-Bongarcon proposes the expression  $\beta=(d_{lib}/d)^{1.5}$  (see Literature survey in Part V of this series).
- $f$  is a dimensionless “particle shape parameter”, also varying between 0 and 1 that, with most materials, is practically equal to 0.5 (flakes and needles are exceptions).
- $g$  is a dimensionless “size range parameter” again varying between 0 and 1. It has a general value of 0.25 with *uncalibrated* mineral populations, tending toward 0.75 with naturally *calibrated* materials such as cereals. It would equal 1.00 with, for example, high-quality bearing balls of strictly identical diameters.
- $d$ —the “top particle size” (expressed in centimeters for dimensional homogeneity) is defined as the size of the aperture of the square-mesh screen that would retain exactly 5% of the material (passing 95%). The determination of  $d$  must be very precise *as it is raised to the third power*. A quick visual estimation is not always as precise as necessary.

Thanks to simplifications and approximations which are not supposed to alter the order of magnitude of the variance, I had succeeded in transforming a sum extended to a *multitude of unknown terms* into a product of factors, which can, in most cases, be estimated with a good degree of precision. This “Formula” has seen an unexpected, but pleasing very wide use.

Formulas (1) and (2) were developed in 1950 (now more than 50 years ago), then proposed in an internal, unpublished note (Refs. [1,2], Part V). Contrary to what I recommend in the foreword to Part I of this series, I had in fact answered the second question “how much?” *before* knowing the answer to the first and foremost question “how?” As already mentioned, this fundamental question had never been clearly posed by anyone at that time and I did not answer it before the beginning of the 1970s.

## 5. Conditions of validity of The Formula [2]

The formula (2), which expresses the fundamental variance  $\sigma^2(FSE)$ , is still valid today. However, in the books I have published since 1979, a much more elegant and general demonstration has been given, which the reader finds in Part II. It is now based on the “probabilistic sampling model”, whereby each element  $U_m$  of  $L$  is submitted to the selection process with a certain selection probability,  $P_m$ . This generates the TSE. In the most general case of the probabilistic sampling model, TSE is the sum of two terms: the *correct sampling error (CSE)* and the

additional *incorrect sampling error* (ISE):

$$TSE = CSE + ISE \quad (3)$$

If the sampling is correct, i.e. if  $P_m = P = \text{constant}$ , then  $ISE = 0$  which entails:

$$TSE = CSE \quad (4)$$

If the sampling is correct, and if the elements are selected *individually and independently*, TSE and CSE boil down to the FSE:

$$TSE = CSE = FSE \quad (5)$$

It entirely falls upon the user to implement a correct sampling but except in tests or computer simulations we cannot select the elements making up the sample *individually and independently*. In practice, therefore, condition (2) is never fulfilled. The best we can do is to extract multi-elemental increments,  $I$ —i.e. *groups of neighboring elements*—with a uniform selection probability  $P$ . In this case, the existence of a distributional correlation between selected elements generates a new additional error, namely the *grouping and segregation error* (GSE), which entails:

$$TSE = CSE = FSE + GSE \quad (6)$$

When using The Formula, the reader should never forget that it is valid *only* when *both* conditions are fulfilled. It is meaningless, and dangerous, to answer the question “how much?” by means of the formula which governs the sampling *variance* only—without first answering the question “how?”, which governs the much more influential sampling *bias*.

## 6. Experimental check of theoretical results—first publications

In order to check the validity of my approach, I had, in 1950–1951, organized an experiment (described in Section 23.3 of Ref. [18]), which consisted of splitting a lot  $L$  (a few kilograms of lead ore) into 16 samples obtained at the end of 4 stages of riffle splitting (divisions into twin fractions). These 16 samples were weighed, carefully pulverized and assayed for Pb. The variance of the population of 16 results (i.e. the variance of the global estimation error, GEE) was computed; the variance of the total analytical error (TAE) was subtracted and thus a first experimental estimate of the variance of the TSE was obtained. It was several times larger than the variance computed according to The Formula, but it was of the same order of magnitude at least. This suggested that the error taken into account by the model was the *minimum of the total sampling error* TSE. For this reason, I decided to call it “fundamental sampling error” (formerly FE).

The error FSE was only *one component* of the CSE and another error, itself the sum of several components, resulted

from the fact that actual sampling did not respect the second condition assumed by the model, which generated the GSE. In addition, the ISE made their appearance. It was only much later that these errors were logically analyzed and their components identified. This experiment also showed, as an unexpected by-product, that a perfectly symmetrical riffle splitter could introduce a *sampling bias* when the sampling operator did not follow a certain number of rules, unheard of at the time, which were later formulated as an answer to the question “how?”.

The internal *standard* [2] and the results of the splitting experiment were first presented publicly at the occasion of the *Second International Mineral Processing Congress*, held in Paris in 1953 and *published* in the proceedings in 1954 [3].

My next step was to devise a certain number of charts making it easier to implement the 1950 formula. These were presented at the occasion of the *Third International Mineral Processing Congress*, at Goslar, Germany in 1955 and published in *Erzmetall* [4] and R.I.M. [5]. In the same spirit, I designed a “sampling nomogram”, a circular cardboard calculator, produced by *Minerais et Metaux* in 1956 (French [6F], English [6E] and German [6G] versions). This nomogram was presented in Japan (Bull. of the Tohoku University) in 1960 [7], the title in Japanese (Kana) can be found in the literature survey of [18]. The sampling nomogram was followed by a “sampling slide rule” operating on the same principle (French [8F] and English [8E] versions) in 1965. For technical and cost reasons, this slide rule could not be manufactured earlier.<sup>1</sup>

Back in 1956, however, I showed that the 1950 formula could easily be transposed to the case where component  $A$  was a given *size fraction* and the critical content  $a_L$  therefore corresponded to the proportion of this size fraction in lot  $L$  [9]. I obtained another formula that was first presented in a French magazine in 1956, and then under the title “The Sampling Error Committed on Size Distribution” at a Mining Congress in Jamshedpur, India in 1957; this was published in the *Indian Mining Journal* the same year [10]. I had then already shown that The Formula was also applicable to the moisture content of a lot of wet material.

It was in 1957 that I first presented the formula in English at an annual meeting of the Society of Mining Engineers of the American Institute of Mining Engineers (SME of AIME) in New Orleans, LA [11]. It was not presented in the UK—at an annual meeting of the Institution of Mining and Metallurgy (IMM) in London—until 1965 [12].

<sup>1</sup> Information to *young* readers (who studied *after* ca. 1970), who known only about electronic calculators and modern computers, students and engineers of the 1940s and 1950s calculated by means of *slide rules* based on the properties of *logarithms*, transforming an equality  $AB=CD$  into a sum of the type:

$$\log A + \log B = \log C + \log D.$$

Nomograms and sampling slide rules worked on this principle.



## 7. Sampling of flowing streams

In 1960–1962, I concentrated my attention on the problem posed by incremental sampling of flowing streams, a problem of paramount importance in mineral processing and ship-loading facilities for example. One can imagine three ways of reducing the mass of a flowing stream of particulate solids, liquids or multiphase media (e.g. pulps of finely ground minerals in water):

- Taking *the whole* of the stream during *a fraction* of the time (by means of cross-stream samplers taking increments at usually uniform intervals),
- Taking *a fraction of the stream during the whole* of the time,
- Taking *a fraction* of the stream during *a fraction* of the time.

In the mineral industries, the first method is often implemented (with exceptions) whereas in the chemical, pharmaceutical, oil, food industries, etc. the second and third methods, cheaper in the short term, are always preferred. I was conscious of the existence of two problems yet *unsolved*:

1. Cross-stream samplers should respect certain rules regarding, for instance, the cutter velocity and the cutter opening, shape and width. These rules remained to be defined scientifically, which was achieved only in 1977, after a campaign of experiments carried out on bauxite blocks [22]. But at that time I was interested in the mathematical problem posed by the second point (next paragraph).
2. The increments extracted from the stream, usually at a constant interval, are *not independent from one another*. There is a correlation between the composition of a slice of matter and the instant it passes through the sampling area. In this case the statistical laws designed for “populations of independent units” are no longer valid. It remained to develop the statistics of auto-correlated time series.

I had already collected many series of experimental data and was studying them when I heard of *Georges Matheron's* work and his recent creation of a new science called *geostatistics* [24], presented in English by *Michel David* [25]. From a theoretical standpoint, there is no difference in essence between the *spatial* correlation along drill cores, for example, and the *time* correlation along a flowing stream. It was on this occasion that I *borrowed* from Matheron the “variogram” as a function characterizing the autocorrelation of flowing streams. This opened new fields of research (later called “chronostatistics”) that I explored during the years 1961–1965. I presented my first publication on this subject under the title “Variography” at another annual meeting of SME of AIME in Denver, CO, in 1962 [24] and at the

Institution of Mining and Metallurgy (IMM) in London in 1965 [12].

## 8. First book in French—synthesis of the quantitative approach

In 1962, I felt the need (perhaps the *urge*—experienced authors will understand what I mean) to write a book gathering my experience of sampling, both as a theoretician and as a troubleshooter. I was then employed by *Minerais et Metaux* in Paris. I worked in excellent harmony with the CEO, my friend *Roger Testut*, but my time was more and more dedicated to management problems, less and less to scientific matters—the development of a sampling theory had no priority in the objectives of *Minerais et Metaux*. This left me in reality with no time to write such a book. I had to make a choice: I could not be both a manager and a scholar. Were I to stay in the first, very comfortable, position I had to abandon sampling theory. This soon came to a crossroad. I therefore opted for the second option. . . and for a *random income*.

I became a freelance *sampling consultant*, probably the first of this kind in the World, and I moved out of Paris with my family to the city of Cannes on 1st January 1963. I was now free to write my book and I started right away. Since then, soon 40 years ago, in spite of some difficult years of tightrope walking, I have never regretted this choice.

For the years to come, my time was shared between numerous forms of activity always overlapping each other in time and space: theoretical research, consulting, troubleshooting, lecturing, teaching regular courses in various schools and universities, teaching privately organized short-courses and, last but not least, writing magazine articles and books. My activities, limited to France at the beginning, led me all over the world as soon as my articles and lectures in English helped the mining and metallurgical industries realize the importance of scientific sampling. I now had the opportunity to work on practically all kinds of mineral materials, from coal or cement raw materials to diamond, gold or platinum ores by way of uranium.

So far, I had dealt only with the quantitative approach to the sampling problems where I thought I had proposed adequate solutions for both zero- and one-dimensional objects. I endeavored to gather all the results already obtained in the first of a two-volume book to be published by “Societe de l'Industrie Minerale” (SIM). This project met with more objections than I had anticipated, from one member of SIM scientific committee. The publication of the book, ready in 1965, was delayed until 1967 [13]. It was published in its original version thanks to *Lucien Vieilledent's* and *George Matheron's* friendly help. Their support was decisive in my struggle to have this book published.

This first volume was followed, in 1971, by its second part [14], in which I developed solutions to specific problems such as studies of spatial distributions, the



Fig. 3. 7th International Mineral Processing Congress, Praha, 1970 (aged 46, center). The secretary general of the congress was a friend who has asked me to help him solve a delicate problem—in the middle of the cold war: The Czech ice hockey team had just defeated the Russian team (4–3) at the end of a murderous match. The following evening, the downtown Aeroflot branch office, next door to my hotel, was destroyed. Now, you will understand my friend’s problem: At the last minute, five Russian professors demanded to present papers which had not been selected by the appropriate committee—all of them in the opening session “Crushing and Grinding”. My friend needed a chairman to take this responsibility away from him. When he asked me—I accepted (I like sports). Reluctantly, an American and an Italian professors accepted to preside together with me (witness their none-too-enthusiastic faces above). I gave each would-be speaker exactly 2 min after which I switched the microphone off. Anyway, nobody understood anything, because every sentence was first translated into Czech and secondly into the four official languages of the Congress (English, French, German, Russian). From what my friends in the room later told me, I was introduced as something like (as related by the French translation): “The President . . . of . . . France” (De Gaulle was still alive!)—and the rest was double Dutch to everyone.

sampling of coal and precious metal ores, sampling for size analysis or for moisture estimation, study of sampling errors resulting from the practical implementations of the model, etc. A large part of this book deals very *practically* with the question “how?”, but in a non-structured way. I did not yet introduce the concepts of *probabilistic* and *correct* sampling. In the meantime I had gathered an important number of references and this book contains a 769-reference literature survey (Fig. 3).

### 9. A new theoretical and practical synthesis—first definition of the concept of correct sampling

In 1972 [23], I tried for the first time ever to propose a qualitative approach to the sampling theory and to answer publicly the question “how?”, neglected so far. I presented the concepts of *probabilistic*, *non-probabilistic*, *correct* and *incorrect sampling* to the annual meeting of SIM.

- A sampling was then said to be *probabilistic* when it was based on the notion of *selection probability*. In 1979 [16], this definition was refined and its new formulation is still valid today: a sampling is said to be

probabilistic when *all* fragments have a non-zero probability of being selected.

- A sampling was then, and still is, said to be *non-probabilistic* when this condition is not fulfilled—for instance when it results from “picking”, or from a deliberate choice, by the sampling operator, of the fragments making up the sample.

Today’s reader may be sceptical but the “hammer and shovel method”, which is based on such a deliberate choice, was then described by most standards, including ISO. In 1988, i.e. sixteen years after [23] and thirteen years after the book [15], ISO proposed a text (ISO/DIS 6153) still describing the (non-probabilistic) “hammer and shovel method” for the sampling of chromium ores.

- A probabilistic sampling is then, and still is, said to be *correct*:
  - With zero-dimensional objects: when *all* fragments have a *uniform probability* to be selected.
  - With one-dimensional objects: when the density of selection probability is *uniform throughout* the one-dimensional domain occupied by lot *L*.
- A probabilistic sampling was said to be *incorrect* when the pertinent condition is not fulfilled.

The idea that sampling could be treated as a science was new and shocked some distinguished members of the audience. One of them favored a definition whereby, if sampling was at all to be thought of in terms of probability, the selection probability of each fragment had to be *proportional to its mass*. Readers may easily judge for themselves the pertinence of such a definition. *Arthur Koestler* is right when he says (in “The Sleepwalkers”): “As with contagious diseases, new ideas need long incubation periods before their effects are observed”. According to my own experience, I would say between one and two generations.

As soon as the 1971 book was published, I felt the need to write a new book. I had acquired a quarter of a century of experience as a theoretician, consultant and troubleshooter and this book was to be full of practical experience. For personal reasons, I decided to be my own publisher. The writing, typing (by a professional typist), printing and binding of the new book took about 4 years and the first copy of the book was handed over to *Roger Testut*, to whom it was dedicated, in 1975 [15]. No more than a few hundreds copies of this book were ever sold.

For the first time in a book, I was able to distinguish between the a priori selecting conditions—*on which we can act to some extent*—and the a posteriori properties of the sampling error, which *result* from the selecting conditions and which we can but observe, usually too late. This amounted to distinguishing between the possibilities of the sampling equipment and the qualities the users of this



equipment could expect or demand from manufacturers of this equipment. In short, I had to build up a logical and mathematical bridge between the selecting conditions and the sampling errors. For the first time—I had in the meantime overcome the wry opposition—the concepts of *probabilistic, non-probabilistic, correct, incorrect* sampling were presented in a book:

- *Properties of the selection process*: it can be probabilistic or non-probabilistic; if probabilistic it can be correct or incorrect.
- *Properties of the sampling errors*: these are *random variables* that can be characterized by their statistical distribution and the properties of their moments: a sampling can be *accurate or biased* (properties of the mean), *reproducible or not* (properties of the variance), *representative or not* (properties of the mean-square).

Some of the definitions used today (part I) are *slightly* different from those of 1975 but the overall philosophy of this approach was set then and has not changed since.

#### 10. First book in English—first presentation of the double Student's $t$ -Fisher test

During congresses, or on the occasions of lectures in English-speaking countries, I had been asked to write a book in English, but nobody had volunteered to translate my latest book. On the other hand, since 1974, I had been working in cooperation with *Elsevier Publishers*, who had asked me to create the “International Journal of Mineral Processing” and to become its Editor-in-Chief. They asked me to write an updated version of my 1975 book [15] in English. I accepted what was a challenge, without realizing the kind of work expected of me: *Elsevier* had asked me to provide a *camera-ready copy* of the text. This entailed that the presentation of each page had to be definitive when it left my office. Digital word processing techniques were as yet totally unavailable and my only choice, *excellent* at that time, was the well-known IBM “golf ball” typewriter, which had already been used for the typing of [15].<sup>2</sup>

I vainly tried to hire in Cannes the services of a professional typist capable of typing an *English* text full of *mathematical* expressions, of *Greek* letters and other symbols, Alas, I had to type it all myself. Due to the difficulty of correcting typing mistakes, I first had to write the entire text *by hand* and then to have it corrected for the language. It is one thing to present a 20-min lecture in a Congress where nobody remembers what you said, far less your language mistakes anyway, and quite another to write several hundred pages in a foreign, not completely familiar language.

<sup>2</sup> I still treasure this typewriter together with the collection of six golf balls I used at that time.

I decided to make a test. I would write, as best I could, what was to be an introductory chapter and submit it to an American newsman living in Cannes, whom I had met and who was willing to help me. A few mistakes considered minor by the American reader were corrected and I went on with his benediction. Since then, I have had some doubts as to the reliability of his advice. *Elsevier* had the text reviewed again and I retyped entire pages or paragraphs. The first edition of this imperfect book was available in 1979 [16]. It received a rather satisfactory review and the very decent reviewers were kind enough not to insist on the language deficiencies.

I still vividly remember the winter 1978–1979 when I worked over 10 h a day, 6.5 days a week to type the 431-page manuscript, doing nothing else. At the average rate of 3 pages/day, I spent 4 months on the typing. It very seldom snows in Cannes, but it was one of these rare snowy winters and, on Sunday afternoon, I would walk around the nearby mountains, in knee-deep snow, for a wonderful change.

The book was, for a large part, a translation from existing texts in French. Its most original feature probably was a statistical chapter presenting a *double* Student–Fisher test eliminating the risk of drawing a wrong conclusion using the alternative *single-sided* test. Many people, including authors of bias tests recommended by ISO Standards did (and still do) persistently mistake the “absence of proof of bias” (rightful conclusion of a Student–Fisher test) for the “proof of absence of bias” (wrong, *biased* conclusion of the same test). ISO standard 3086 is entitled: “Iron Ores—Experimental Methods for Checking the Bias of Sampling”. As far as I know, its latest version was published in 1986 (7 years after the publication of Ref. [16]) and it still makes the same mistake. Most standards on sampling of iron ores were revised in 1998, but this bias test 3086 was not (19 years after the publication of Ref. [16]), and is still the one on record.

Chemometricians also should be very careful with the Student–Fisher test as it is presented by these and other standards.

As early as 1981, the first edition was nearly out of print and the publishers asked me to prepare a second revised edition, which was available in 1982. The major revision concerned the statistical chapter 31, which was refined and became definitive.

#### 11. Complete textbook in French—new developments derived from sampling theory

My latest book in French [15] was 7 years old when the second edition of Ref. [16] was released. In the meantime, I had developed several ideas leading to new applications of the theory, namely:

- \* Point by point computation of the auxiliary functions of the variogram,

- \* Theory and practice of proportional sampling,
- \* Theory of bed-blending derived from the theory of sampling and industrial implementation.

On the other hand, from hundreds of missions carried out in more than 80 countries in a time span of 40 years, I had gathered a respectable amount of practical experience illustrating my theoretical conclusions and I thought it useful to publish them. The French Publisher, *Masson, Paris*, was willing to publish such a book. The latter was ready in 1988 [17].

It took me 6 years to achieve this because I had a number of new subjects to incorporate into former texts and I also had to work as a consultant and short-course teacher, my only sources of income (authors' royalties paid by publishers are ridiculously low, but having a book published by a well-known, respected publisher usually generates a certain amount of consulting work.). My troubleshooting activities had started with base metals such as lead, zinc and copper, but with the developments of the uranium industry in France and abroad, the latter had become one of my major sources of work in the 1980s. Ever since the 1950s, I had worked in close cooperation with the French "Commissariat à l'Énergie Atomique" (CEA) and, when they were created, with its mining and metallurgical subsidiaries COGEMA and COMURHEX in France, Gabon, Niger, Canada and South Africa. These companies became my major clients, as kindly recalled by my friend *Robert Bodu* in "Les Secrets des Cuves d'Attaque" [27], the history of uranium ore processing in France. I had to alternate writing, teaching and consulting. I led a busy life. . .

During these years, in addition to illustrations or refinements of the existing theory, I had also developed two new subjects, which similarly needed to be presented in a textbook. I believe that they have an enormous industrial potential:

1. Mass and volume measurement by proportional sampling,
2. Theory, and industrial implementation, of bed-blending.

## 12. Proportional sampling

In 1954, I was confronted with my first problem of "metallurgical balance reconciliation" in a group of North African lead and zinc flotation plants. A metallurgical balance is nothing other than the application of the *Lavoisier's* stoichiometric principle at the scale of an entire mineral processing plant—it can be summarized easily enough "whatever comes in must ultimately come out, one way or another". When this is not observed there per force *must* be either *measurement biases* or *unsuspected losses*—and with a single exception in 45 years of consulting, what came out was always *less* than what came in. The mine owner had observed that there was a persistent 2–3% deficit

of lead and zinc produced and he suspected shortcomings of his sampling systems (he had just read my first publication [3]). After a visit to the plants and 1 year of remote monitoring work in cooperation with the staff and a check of all measurement devices involved, I reached the conclusion that sampling was only a minor source of bias and that the biggest bias was to be attributed to the *conveyor belt scales*, in spite of the fact that they were calibrated once a day every day. In fact, the bias was due to this calibration.

Over the years, I discovered that all kinds of scales could be found operating on conveyor belts (all types of mechanical scales, nuclear scales), and they all suffered from a structural lack of reliability. It is one thing to carry out an easy electrical measurement and quite another to convert it accurately into a tonnage of ore. This opinion was reinforced when I read *Hendrik Colijn's* "Weighing and Proportioning of bulk Solids" [28]. The following is a quote of chapter 7, confirmed by *Colijn* when we later met.

The actual plant performance of belt scales, unfortunately, does not always measure up to the claims of the manufacturer or to the expectations of the operator. Instead of the ½ percent accuracy, some plant personnel have claimed that 10 percent is a more realistic figure and on a large number of installations, they may be correct.

This is true also of nuclear scales, *ibid.* (chapter 9).

When developing the theory of sampling, I had reached the mathematical, indisputable conclusion that, when sampling was carried out correctly with uniform selection probability  $P$ , the sample mass  $M_S$  was a random variable with a mean equal to  $P$  times the lot mass  $M_L$ .

$$m(M_S) = PM_L$$

In addition to this property, when the number of increments in the sample is "large enough" (which is nearly always the case) the confidence interval of  $M_S$  is very small. The sample  $S$  can be weighed,  $M_S$ , by means of conventional *static* scales (*very reliable*), with the consequence that for correct sampling, and when the uniform selection probability  $P$  can be estimated accurately, the quantity  $M_S/P$  is an excellent, unbiased estimator of the lot mass  $M_L$ .

$$M_S/P = \text{unbiased estimator of } M_L$$

According to my experience, this unbiased estimator is much more reliable than *any* that, e.g., can be obtained by means of the belt or nuclear scales available from existing specialized manufacturers. This is the basis of "proportional sampling" (PropSamp). In 1980, I recommended this new technique to the South African *Rustenburg Platinum Mines* and, since then, it has been used routinely to calibrate the nuclear scales that had been installed originally and provided unreliable results, which I have been able to check on the occasions of later visits to South Africa.



A variant of proportional sampling was implemented at the mineralurgical pilot plant of the *Bureau de Recherches Geologiques et Minières (BRGM)* of Orleans-La Source, France. It consists in implementing a proportionality ratio (the selection probability  $P$ ) that may not be known with great precision but that, *by construction, is strictly the same for all streams* (feed, concentrates, tailings). The sets of samples and lots masses are then “homothetic”, which makes it very easy to compute the metallurgical balance.

In practice, the rules of sampling correctness are always applied with a safety factor so as to make sure that the mass estimation will be as highly reliable as desired, with the consequence that this proportional sample is also “perfectly representative” of the lot. After appropriate drying and weighing, the sample is reduced and assayed for the critical component(s) in the conventional manner. The same sample thus provides all qualitative and quantitative information needed to compute the metallurgical balance of an entire plant.

*Proportional sampling* is so simple that some people would not believe its efficiency. Nowadays, simple techniques, especially when they do not use sophisticated, preferentially computer-controlled equipment, do not inspire confidence and I was often required to *prove* the adequacy of PropSamp. I was challenged to check its reliability in an existing pilot plant against a weighing system involving a 10-m<sup>3</sup> tank mounted on strain gauges, a centrifugal pump, a correct sampler and a water meter. The results of this experiment have been described in my books since 1988, e.g. chapter 29 of Ref. [18] and chapter 13 of Ref. [20]. Interestingly, instead of supposedly proving the unreliability of PropSamp, this experiment in fact helped disclose fundamental inadequacies of both the strain gauges and of the water meter, with which the pilot plant was equipped, and which had hitherto been considered to work “to everybody’s satisfaction”.

### 13. Bed-blending, derived from sampling theory

Since the 1960s, Lafarge Cements had realized the necessity of an accurate sampling of the feed to their cement kilns. Together with the sampling equipment manufacturer MINEMET-INDUSTRIE (a reincarnation of my former employer *Minerais et Metaux*), we designed and installed highly reliable sampling plants in their Cement Works.

Cement kilns, like metallurgical furnaces, are known for their severe lack of flexibility. They require to be fed with material as uniform as possible—the ultimate, very costly penalty is the loss of a kiln. To achieve this purpose, in a first step, Lafarge plants feed their raw materials to what is known as a “bed-blending system”, which ensures an imperfect form of one-dimensional homogeneity. The feed to this system is sampled in a MINEMET sampling plant coupled with an X-ray analyser capable of assaying a

sample for its major components in a few minutes. The whole system, assisted by a computer which calculates the average composition of the pile being formed, works in such a way that at the end of the constitution of a blending pile, its average composition is very well known and, when properly managed, is practically equal to the ideal feed to the kiln.

One of Lafarge subsidiaries had installed a bed-blending system manufactured by PHB-SOMERAL (now MBH) of Mulhouse, France. The blending was adequate but the technical manager of PHB had observed that the blending system did not work in agreement with Gerstel’s theory, published in 1977 [29] and generally accepted. He asked my advice in 1978 and I offered to develop a theory of bed blending, which, I realized, could easily be derived from the existing sampling theory. This new theory of *bed-blending* was developed right away and presented to PHB-SOMERAL in June 1978. This was a wonderful but, unfortunately unique experience: an equipment manufacturer wanted—and was ready to pay the services of a consultant—to understand how *his own* equipment really worked.

In order to convince potential clients, I was further asked to carry out a full-scale check of the theory, which was realized about 6 months later at the Heming Cement Works (Lorraine). Lafarge carried out the X-ray assays. To everybody’s satisfaction, these experiments showed that the new theory was in perfect agreement with experience, contrary to Gerstel’s.

This theory and the experimental check were published for the first time in 1981 [30,31] and can now be found in the books [17–20]. The Canadian mineral industries were interested and invited me to present the philosophy of blending the feed to a plant at the occasion of the 100th anniversary of the Canadian Institute of Mining and Metallurgy (CIM) in Montreal (1998) [32,33].

### 14. Complete textbook in English—presentation of proportional sampling and bed-blending

As soon as my latest book [17] in French was released, *Elsevier* asked me to write its English version. This was ready in 1992 [18] and is practically a translation of Ref. [17]. It contains nothing original worth mentioning.

### 15. Summarized versions in French and English

The voluminous 700-page textbooks [17,18] contained complete, updated mathematical demonstrations and I had to write them as reference books, but they were simply too heavy and too costly to reach a wide audience. There was a need for much shortened (on the order of 150-page) versions. My French publisher *Masson* was ready to publish it, which was achieved in 1996 [19]. My British friend *Allen*

Royle kindly offered to translate the French text into English, which I accepted with gratitude. For the first time, my work was available in *excellent* English. *John Wiley* offered to publish this book, which was released in 1998 [20]. It was well accepted by the public and a second edition was marketed in 1999. I am very grateful to “Le bon Royle”.

With this, my account has reached 50 years, indeed between one and two generations.

#### 16. What does the future hold?

First of all—always—there is the family. I am not a man to talk of my family in a public context such as this. The editor of this account, however, is very persistent. Thus, one *sample* picture from the family Gy will have to suffice (Fig. 4).

#### 17. The theory of sampling at the year 2000: 50 years—and beyond

I was originally writing this series with the intention that it was to be part of the proceedings of the SSC6 conference in 2000 (see Editors’ introduction)—in fact, I took pains to be able to finish the text on Christmas Eve 1999, which I considered an appropriate goal: Fifty years to the mark! Editorial events outside my influence later made it expedient to augment this series with several other related papers and to publish this interesting lot (*L*) *altogether* as a Special Issue. It was to be rather severely



Fig. 4. 1996, Pougnaoressse (Gard). 50th wedding anniversary with wife Sylvia, daughter Caroline and grandson Stanislas. The whole family was together, altogether 22 persons—and there are now six more great-grandchildren since then. I am not worried about the future of the Gy family. But none of them bears the name Gy, which goes back—at least—to the Norwegian Vikings in the Normandie.

delayed however.<sup>3</sup> The present amicable journal serves the same scientific community to which I have never before catered, so the delays incurred are hopefully forgiven when the result is now finally at hand.

#### 18. Is this the end of the story?

I don’t know, but the intensity of this work must soon be reduced. It may be the beginning of the end for me but certainly not for the Theory of Sampling and its applications. I am very glad to have reached beyond the mythical “Year 2000”—Now I have begun to hand the relay to a new generation of professors and engineers, students and industrialists—to all *proper samplers*...

#### Acknowledgements

I would like to mention my Scandinavian friends who have played an important role in disseminating my work in the later years. *Pentti Minkkinen*, Professor at the Lappeenranta University of Technology, Finland, was, to the best of my knowledge, the first to teach courses of the theory of sampling on a regular basis and to develop a computer program (SAMPEX) to estimate the fundamental sampling variance by means of “The Formula” as early as 1986. Thanks to Pentti, I later made the acquaintance of *Kim H. Esbensen*, then Chemometrics Professor at the Telemark Institute of Technology (HIT) in Porsgrunn, Norway. He was the enthusiastic Chairman of the *Sixth Scandinavian Symposium on Chemometrics (SSC6)*, in which capacity he invited me to present a synopsis of the Theory of Sampling at this occasion, August 15–19, 1999, and from which was developed the present tutorial series. Thanks to Kim, I made the acquaintance of the late (April 2002) Professor *Sunil de Silva*, Head of the Powder Science and Technology Research Group (POSTEC), also of HIT, who in addition invited me to present my work to the audience of the Third Symposium on the Reliable Flow of Particulate Solids (Relpowflow III), of which he was Chairman, also held at HIT, August 11–13, 1999. I thus had the opportunity to reach out to two completely new scientific communities in 1999, to my intense satisfaction.

<sup>3</sup> The author was originally writing this piece as part of a larger tutorial series with the intention to be part of the proceedings of the SSC6 conference in 1999 (due for publishing in late 2000), but it was decided to opt for a whole independent Special Issue on sampling. Sadly, only the proceedings of SSC6 made it into print, while the planned tutorial issue met with surprisingly severe, indeed hostile reactions. Various manoeuvres by highly placed non-to-TOS-interested chemometricians intervened, and the tutorial series was never published in the planned journal. It took another chemometric journal and the foresight the editors Massart and Minkkinen when accepting the proposal for the present Special Issue to eliminate this opposition [Editor’s comment].



There has been a steadily growing sampling activity in Scandinavia since, and equally pleasing. I am not worried about the future of sampling in Northern Europe.

In France, the *International Sampling Institute (ISI)* was created by a group of French Consultants in 1999 to perpetuate the national tradition of interest in sampling. Back in 1979, *Selected Annotated Titles* wrote in their review of my first book in English [16]:

The French have made a speciality of statistical applications in the earth sciences and this contribution only serves to underline their dominance.

ISI is active in organizing short courses in France (and abroad), in French and in English. Here again, the future of the sampling theory is in good hands. It is ably led by Denis Thirouin.

The newest offspring of organized sampling activity concerns the *International Sampling and Blending Forum (ISBF)*, the embryo of which was founded by two close

colleagues and friends *Kim H. Esbensen* and *Dominique Francois-Bongarcon* at an early 2001 spring day encounter in Copenhagen airport, Kastrup. ISBF will operate on the worldwide scale. ISBF will be lead by a *virtual board* of international directors; the first board was selected at WCSB1. ISBF will make it its objective to reach out primarily to the world university communities—including technical universities of course—on all matters of proper sampling, teaching, research, experimental work in collaboration with industry and other users of TOS.

It is also most appropriate here to acknowledge the active help in discussing, publishing, co-writing many of my later papers, which has been given to me by *Dominique Francois-Bongarcon*.

Last but not least, I would like to express my deep gratitude to my very good friend, to my excellent editor *Kim H. Esbensen* for the huge amount of work he fed into what primarily was a “run-of-the-pen” manuscript pile so as to transform it into a perfectly edited series of tutorial articles that, I hope, will interest many new professions.

# Sampling of discrete materials—a new introduction to the theory of sampling

## I. Qualitative approach

Pierre Gy

*Res. de Luynes, 14 Avenue Jean de Noailles, F-06400 Cannes, France*

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### Abstract

The purpose of a *theory of sampling* is to answer two questions: *How* should one select a sample?—*How much* material should be selected? Parts I (qualitative approach), II and III (quantitative approach) of this series propose answers to these two fundamental questions. These answers are not entirely new (answers have been formulated since 1950), but a scientific theory is a living structure that has to be kept up to date. At a course given in Brasilia in 1998, pointed questions were raised which suggested that the introduction to the qualitative approach had to be clarified. Part I represents the most updated introduction to theory of sampling (TOS). More than 200 scientific papers, books, lectures and courses on sampling theory—and practice—have been published or offered to the public by the author over a period of 50 years. A brief, chronological account of the development history of TOS is presented for the first time in part IV—with a comprehensive literature survey as part V.

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**Keywords:** Theory of sampling; Discrete materials; Heterogeneity; Sampling errors; Accuracy; Bias; Reproducibility; Representativity; Quality control; Quality assurance; Chemometrics

### 1. Introduction

The accuracy of many analytical data reports is a mirage because unwitting negligence and false cost consciousness have ensured that a sample of powder taken with cursory swiftness has been examined with costly precision. *Kaye*, Illinois Institute of Technology, 1967

Chemometricians process analytical data, more often than not huge amounts of data. Are these *data* reliable? If *Kaye* is right, which fully agrees with the author's extensive experience, we are entitled to have our doubts. If the data are *biased* as a consequence of systematic sampling errors, what becomes of the chemometricians' conclusions? We have every reason to be cautious that these conclusions may

be biased too. If the data are *uncertain*, for example as a consequence of high random *sampling errors* (high sampling variances), the efficiency of statistical tests will be reduced by the high residual variances. It will invariably be more difficult and/or more costly to reach safe and reliable conclusions. Few chemometricians are aware of these facts. Below it is shown that there is no such thing as a “constant sampling bias”, which is the basis for many current complacent, but false sampling understandings. This tutorial is intended to highlight that a complete theory of sampling is in fact at hand—and has been for 50 years!

The heart of the matter of proper sampling is that the question of “how much?” cannot be dissociated from the question of “how?”. Indeed, quantitative development of sampling theory assumes explicitly that a certain number of conditions have, by being respected, successfully suppressed the sampling bias. These conditions are presented in the qualitative approach in Part I.

*E-mail addresses:* [kes@auc.auc.dk](mailto:kes@auc.auc.dk), [kimhome@esenet.dk](mailto:kimhome@esenet.dk).



## Sampling of discrete materials

### II. Quantitative approach—sampling of zero-dimensional objects

Pierre Gy

*Res. de Luynes, 14 Avenue Jean de Noailles, F-06400 Cannes, France*

Received 28 August 2003; received in revised form 6 April 2004; accepted 28 May 2004

#### Abstract

Parts II and III of this series are initiated by a joint discussion of features related to the lot. Part II then delineates the central elements of the Theory of Sampling for zero-dimensional objects. It is necessary to be brief within the limited format of the present tutorial series, but all essential model rigour has been maintained. An attempt has been made to focus on the central mathematical theoretical core of TOS while also showing how this relates directly to sampling practise (materials, equipment and procedures). A highlight of the latter issue concerns experimental estimation of the Fundamental Sampling Error (FSE). Part II is also fundamental for further developments in Part III, as it presents a complete overview discussion of the basic sampling operation of the one-dimensional object as well.

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*Keywords:* Discrete; Quantitative approach; Zero-dimensional objects; Sampling of Particulate matter; Theory of Sampling

#### 1. Joint introduction of parts II and III: three-, two-, one-, zero-dimensional models

- *Strictly speaking*, all material objects, lots  $L$ , occupy a three-dimensional Cartesian space. From a practical as well as a theoretical standpoint, however, it may be useful to *represent* a physical object by a *model* of a smaller number of dimensions.
- A *three-dimensional model* alone can represent *bulky lots*  $L$ , e.g., an ore body and similar.
- *Flat objects*, such as a sheet of paper, a steel sheet, the *thickness* of which is:
  - *small* in comparison with the two dimensions of its surface,
  - *practically uniform* (with a tolerance of, say, 20%) can often be well represented by a *two-dimensional model*. From a physical and mathematical standpoint, every element of the object is represented by its projection on a plane (often horizontal). We often have occasion to work on lots  $L$ , which can be considered as practically two-dimensional.
- *Elongated objects* such as a rail, a cable or a flux of matter whose *length* is:
  - *very large* in comparison with the two dimensions of its cross-section,
  - *practically uniform* (with a tolerance of, say, 20%) can be well represented by a *one-dimensional model*. From a physical and mathematical standpoint, the lot is here represented by its projection on the axis of elongation.
- *Discrete objects* such as lots made up of a large number of unspecified units, assumed to be *independent* from one another; i.e., populations of non-ordered units can, by extension and by convention, be defined as *zero-dimensional* objects. There are two cases:
  - *Unit masses are more or less uniform* (with a tolerance of, say, 20%): Here, conventional statistics can be applied.
  - *No hypothesis of uniformity of the unit mass is made*. Conventional statistics cannot be applied. We shall here deal exclusively with this most realistic case.

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## Sampling of discrete materials

### III. Quantitative approach—sampling of one-dimensional objects

Pierre Gy\*

*Res. de Luynes, 14 Avenue Jean de Noailles, F-06400 Cannes, France*

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#### Abstract

Part III delineates the close relationships which exist between zero- and one-dimensional objects. The one-dimensional model of flowing streams of matter is presented in sufficient detail to appreciate how on this basis it is possible to perform a complete characterisation of the various heterogeneity components involved. This is achieved by the *variogram*, which forms the central one-dimensional TOS tool for practical sampling purposes. The *variogram* and its features and properties are introduced in detail. The three principal sampling selection modes of one-dimensional systems are delineated. Lastly it is explained how a *variographic experiment* allows estimation of the sampling errors involved in a particular sampling strategy.

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*Keywords:* Zero-dimensional object; One-dimensional object; Autocorrelation

#### 1. Interrelationship between zero- and one-dimensional objects

From a theoretical standpoint, the difference between zero- and one-dimensional objects concerns a difference in *internal correlation*—“autocorrelation”—and in *observation scale*.

According to the strict definition of a zero-dimensional object there is, or there should be, no correlation between its constituents. However, the concept of autocorrelation is not binary (0 or 1): it can take any value between these two limits. Autocorrelations of zero or one are practically inaccessible limits that suppose a *complete distributional homogeneity* of the material in the first case<sup>1</sup> a *complete segregation* of the constituents in the second. A very powerful analysis tool, the *variogram*, will be defined in Section 3. A variogram detects and quantifies the autocor-

relation between the compositions of the materials to be found on two points of the time axis. It shows the quantified autocorrelation as a function of the *distance* between these two points, in fact it depicts the autocorrelation for an entire *range* of inter-distances. Usually, the smaller the distance, the larger is the autocorrelation between them.

Consider a lot  $L$  which flows from time  $t=0$  to time  $t=T_L$ . On the time axis, this lot  $L$  can be broken up into a sequence of adjacent segments of uniform length  $T_I$ . Each of these segments supports a *potential increment*,  $I$ , which could be used for sampling the flow. The autocorrelation of the time series is perceptible at the scale of the total time interval  $[0, T_L]$ , but is usually imperceptible at the scale of the individual time segments.<sup>2</sup> To all practical intents and purposes, each time segment  $T_I$  can be regarded as a zero-dimensional object. Hence the practical conclusion that for our sampling purposes, a one-dimensional object  $L$  can be regarded as a time series of adjacent zero-dimensional

\* Tel.: +45 7912 7688; fax: +45 7545 3643.

<sup>1</sup> Bed-blending. The author proposed the theory and associated practice of this technique in [18]. This technique *reduces* the autocorrelation of a one-dimensional object (e.g. a flowing stream) to a *natural minimum* which is a function of the material properties.

<sup>2</sup> The great French fabulist *La Fontaine* did certainly not have the autocorrelation of a time series in mind when he wrote: *De loin c'est quelque chose mais de pres ce n'est rien* (*Seen from afar it is something but at close range it is nothing*)—but his observation fits wonderfully.

## Part V: Annotated literature compilation of Pierre Gy

Pierre Gy

Res. de Luynes, 14 Avenue Jean de Noailles, F-06400 Cannes, France

Received 22 August 2003; received in revised form 6 April 2004; accepted 28 May 2004

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### 1. Papers and Books Written by the author

[1] Formule donnant la masse minimale d'échantillon (*A formula for the minimum sample mass*) (1950) **First typewritten paper on the theory of sampling.**

[2] *Masse Minimale d'Echantillon Requisite pour Représenter un Lot de Minerai.* (Minimum sample mass required to represent a batch of ore). *Internal rules, Sté Minerais et Métaux* (1950).

[3] *Erreur Commise dans le Prélèvement d'un Echantillon sur un Lot de Minerai.* (Error committed when taking a sample from a batch of ore). *Congrès des laveries des mines métalliques françaises, Ecole des Mines de Paris* (1953), *Revue de l'Industrie Minérale, St. Etienne, France*, 36, pp. 311–345 (1954) **First public presentation and publication in France.**

[4] *Erforderliche Probemenge-Kurvetafeln* (Minimum sample mass—Graphs and Curves). Internationales Kongress für Erzaufbereitung. Third International Mineral Processing Congress, Goslar, Germany, May 1955. *Erzmetall*, 8, (1955) B 199–220. **First presentation and publication in Germany.**

[5] *Poids a Donner a un Echantillon \* Abaques d'Echantillonnage.* French edition of the former paper in German. *Revue de l'Industrie Minérale*, 1956, pp. 1–30 + 12 graphs.

[6] *Nomogramme d'Echantillonnage; Sampling Nomogram; Probenahme Nomogramm:* a cardboard circular calculator: French, English and German versions edited by Minerais et Metaux, Paris, 1956.

[7] *Presentation of "Pierre Gy's" sampling nomogram* by Prof. Wada (*Bull. of the Tohoku University*, 16, 2, 153–157, Dec. 1960), the title in Japanese can be found in the literature survey of [8]. **First publication in Japanese (translation!).**

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St. Etienne, France, 1965. Presented in *Rev. Ind. Miner.* (1965).

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[14] *L'Echantillonnage des Minerais en Vrac* (Sampling of particulate materials). *Volume 2. Revue de l'Industrie Minérale, St. Etienne, France. Numéro Spécial* (Special issue, 15 Septembre 1971).

[15] *Théorie et Pratique de l'Echantillonnage des Matières Morcelées* (Theory and practice of the sampling of particulate materials), Editions PG, Cannes, France (1975).

[16] *Sampling of Particulate Materials \* Theory and Practice.* First edition (1979) (out of print) \* Second revised edition (1982). Elsevier, Amsterdam xvii+431 pp. **First book in English.**

[17] *Hétérogénéité, Echantillonnage, Homogénéisation* (Heterogeneity, Sampling, Homogenizing). Masson, Paris, xiv+607 pp. (1988). **Complete textbook in French.**

[18] *Sampling of Heterogeneous and Dynamic Material Systems. Theories of Heterogeneity, Sampling and Homog-*



# Sampling Resources

## Sampling Columns in Spectroscopy Europe

## SPECTROSCOPY europe

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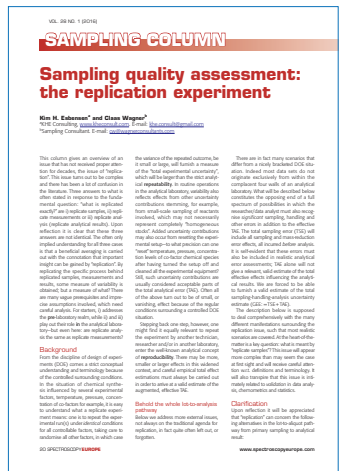
The Sampling Columns published in the free magazine, *Spectroscopy Europe*, and edited by Kim Esbensen and Claas Wagner are a valuable introduction to representative sampling and the Theory of Sampling (TOS).

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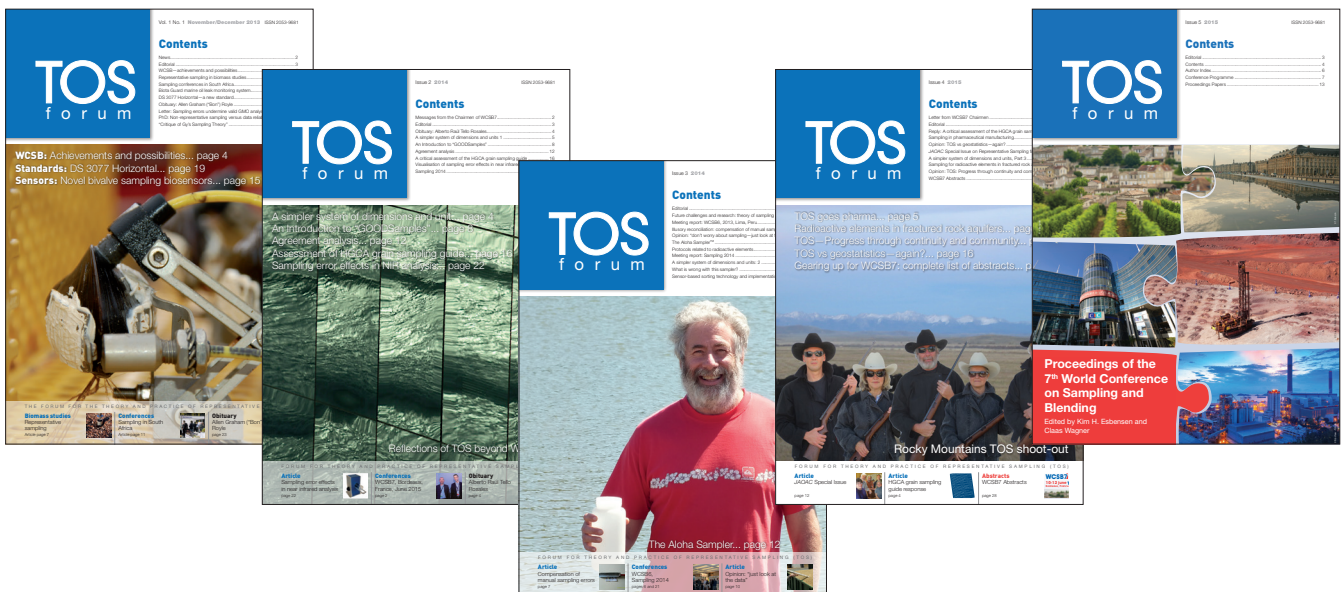
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# An automatic linear proportional sampler based on the principles of the Theory of Sampling

Philippe Wavrer

CASPEO, 3 avenue Claude Guillemin, 45060 Orléans Cedex 2, France

The MINEMET EAR 400 SL sampler is an automatic slurry sampler designed by Pierre Gy in the 1970s for the company Minemet. The EAR 400 SL respects all the principles of the Theory of Sampling. This automatic in-line cross-cut sampler is particularly adapted for the sampling of slurries, such as final tailings, flotation feeds and cyclone overflows. An automatic continuous electric chain-drive system ensures a uniform cutter velocity through the material stream. The model EAR 400 presented here (made in 1973) has three fully independent influx ports; other models with four independent input ways have also been built. It is worthy of note that this particular model was displayed at the 7<sup>th</sup> World Conference on Sampling and Blending (WCSB7), 10–12 June 2015, Bordeaux, just six months before the tragic passing of Pierre Gy in the same city.

## MINEMET EAR 400 SL sampler

According to the seminal publication by Pierre Gy (1981),<sup>1</sup> sampling is said to be “proportional” when the sample,  $S$ , extracted from a given lot,  $L$ , over a flow period of duration  $T_L$  has the following properties:

- The *qualitative characteristics* of the sample  $S$  are “representative” of those

of the lot  $L$ . This means the sampling process must be (at least) “correct”: all particles present in the stream must have the same probability to be collected. A second demand is that sampling variance is at a specified, fit-for-purpose minimum.

- The *quantitative characteristics* of the sample  $S$  are “proportional” to those of the lot  $L$  and the proportionality factor is accurately known; the sampling ratio must remain constant.

In its construction, the MINEMET EAR 400 SL sampler meets both these requirements and can therefore be considered to be a true proportional sampler. Furthermore, as three different material flows can be independently sampled, at the same time and under the same conditions, one of the major advantages of this sampler is that the sample weight ratios are an estimate of the flowrate ratios. Proportional sampling is also described in Gy’s 1988 textbook.<sup>2</sup>

## Description

The EAR 400 SL sampler consists of three main parts:

- An electrically controlled mechanism for transmitting a linear reciprocating

uniform movement to three scoops (spoons), which are the sampling tools. This mechanism is thoroughly protected by a dust- and splash-proof hood.

- Three sampling cutters, driven simultaneously with identical velocity by the above mechanism using a fixed connecting rod.
- A waterproof control box containing the electrical equipment and a timer to adjust the frequency of the sampling.

## Mechanism

The uniform cutter velocity ( $0.115\text{ms}^{-1}$ ) is ensured by a continuous electric chain-drive system, driven by a 0.75 kW electric motor associated with a speed reducer (see Figures 3 and 4). The motor is over-engineered with respect to typical loads, so as to never be slowed down even in the most irregular influx situations. Secured to the chain, a driving element engages on both sides in two slots (upper and lower) of a vertical plate (Figure 5). This one is connected to the three sampling cutters through a slide bar. The electric motor always runs in the same direction, but the slide bar (and then the sampling cutters) moves either way, depending on whether the driving element is at the top or bottom of the driving chain.



Figure 1. General view of the MINEMET EAR 400 SL slurry sampler.

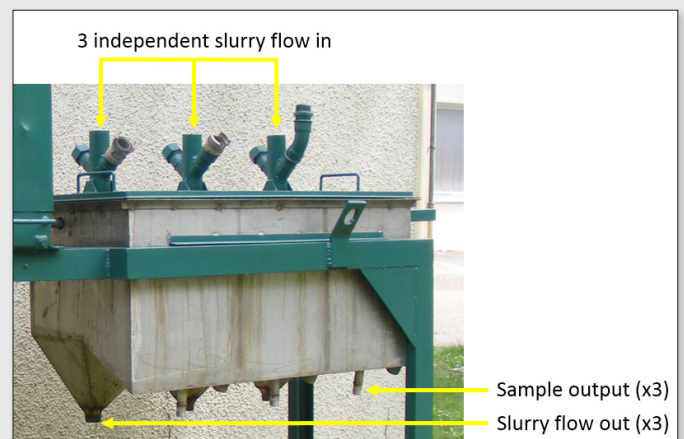


Figure 2. A linear cross-cut slurry sampler with three independent influx ports.

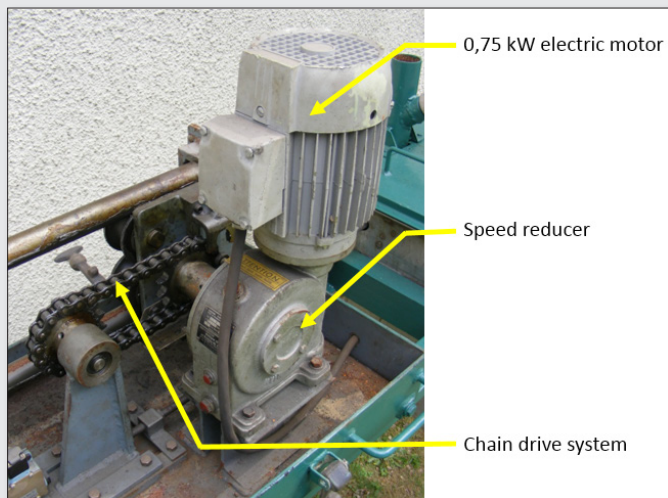


Figure 3. EAR 400 SL's automatic, continuous electric chain drive system.

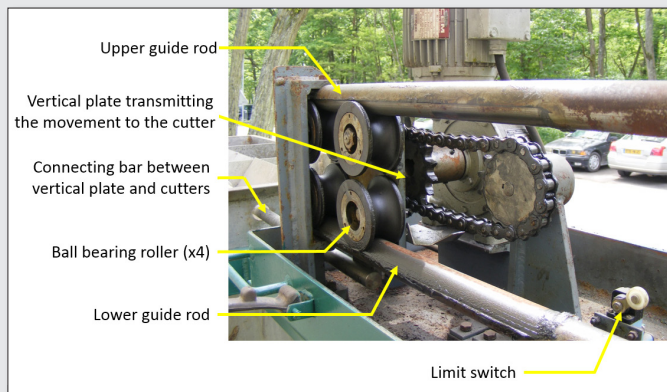


Figure 4. General view of the chain drive system. The fixed "connecting bar" transfers the drive system movement to each of the three sampling units (the most proximal one of which can be seen at the left edge of the photo).

### Sampling cutters

The shape and the opening of the sampling cutter are determined based on the material to be sampled, in order to ensure sampling correctness. Concerning the present sampler, the three sampling cutters all have a 10×88mm rectangular opening.

Due to the length of the driving chain used, the cutters work with a stroke of 280mm. This magnitude ensures that the entire inflow is sampled and that the sample cutters are parked away from the material stream between each increment.

### Control box

In addition to standard start/stop buttons, the control panel is equipped with:

- A timer for adjusting cutter travel frequency in a range from 10seconds to 45hours.
- A pulse counter, to express the number of increments extracted performed at any specific time, or aggregated at the conclusion of the sampling campaign.

The control box is completely waterproof and dust protected and can be locked once adjusted.

### Workings

Once the sampling frequency has been set via the timer and the sampler has been powered up, a normal operating cycle is as follows:

1. Start: Movement of the cutter in one direction in order to collect one increment.
2. Motion stops at ultimate travel position (farthest parking position) for a predetermined time (counted by the timer).
3. Following the pause, movement of the cutter in the opposite direction in order to take another increment.
4. Motion stops at the closest parking position for the same predetermined pausing time.
5. Following the pause interval, commencement of the next double-cycle.

As an example, if the timer is set to two minutes, after two minutes, the timer

sends an electric current pulse that starts the electric motor. The slide bar (driving the sampling three cutters) moves driven by the chain and three parallel increments are collected simultaneously. The motor is then stopped by the action of the first limit switch (see Figure 4). Two minutes later, the timer sends another electric current pulse. The electric motor starts again, always in the same direction, but the slide bar moves in the opposite direction and another increment is collected. The motor is then stopped by the action of the second limit switch. The cycle is then repeated until the STOP push button is pressed.

### Application example: gold flotation pilot plant

A similar sampler to the one presented here, but with four independent influx ports, was used on a gold flotation pilot plant in French Guyana. The objective of the pilot plant project was to evaluate the possibility of recovering gold by continuous flotation

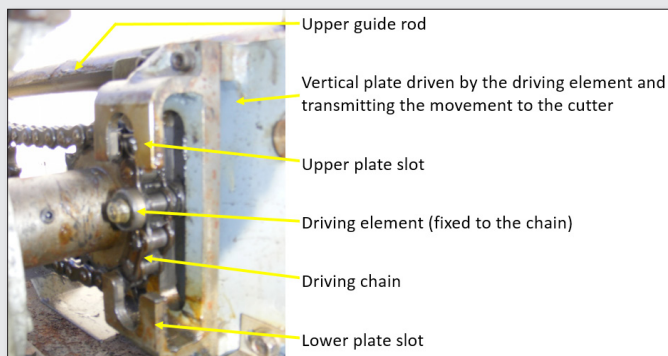


Figure 5. Detailed view of the chain-drive system.

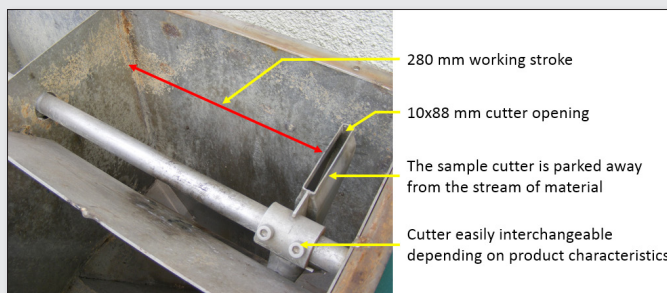


Figure 6. Sampling cutter.



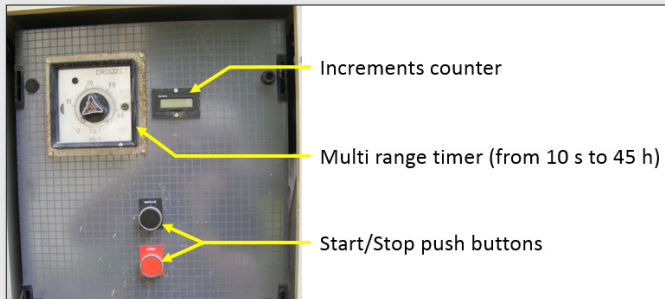


Figure 7. The 1973 control box; functionality is everything!



Figure 8. Overview of flotation pilot plant in French Guyana.

from gravity circuit tailings. In addition to the sampler, the pilot circuit included a series of flotation cells and transfer pumps for pulp. In this case, the main advantage of the sampler is to provide sample masses whose mutual ratios are equal to the influx stream flows ratios. This property is particularly interesting with the objective of calculating material balances and gold recovery from analyses made on the collected samples. Figures 8 and 9 show an overview of the flotation pilot plant with the sampler in-line between the existing industrial plant and the pilot circuit itself and a detailed view of the automatic sampler in operation.

### Significance of proportional sampling

In his theory,<sup>2-4</sup> Gy defined “proportional” sampling as a sampling process that must be not only “correct”, but also realised under conditions which allow that:

- The weight and the volume of the sample are proportional to the mass and the volume of the sampled lot,
- The proportionality factor is known with accuracy and remains constant.

With the implementation of proportional sampling, it is then possible to estimate

without any bias the mass of a given particulate material lot. To achieve that, Gy has defined:

- The “time sampling ratio”  $\tau'$  of a lot  $L$ :

$$\tau' = \frac{Q \cdot T_i}{T_L}$$

where  $T_L$  is the flowing time of the lot  $L$ ,  $Q$  is the number of increments between  $t=0$  and  $t=T_L$  and  $T_i$  is the time of one increment.

- The “mass sampling ratio”  $\tau$ :

$$\tau = \frac{M_E}{M_L}$$

where  $M_L$  is the mass of the lot  $L$  and  $M_E$  is the mass of the sample  $E$ .

According to the theory of sampling, the sample mass  $M_E$  being a random variable, when a sampling is correct (i.e. sampler correctly designed and built and correctly operated), it is then possible to write:

$$m(\tau) = \tau'$$

where  $m(\tau)$  is the mean of  $\tau$ .

That means that

$$\frac{m(M_E)}{M_L} = \tau'$$

It can therefore be deduced that

$$\frac{m(M_E)}{\tau'} = M_L$$

and

$$\frac{M_E}{\tau'} = M'_L$$

$M'_L$  is an unbiased estimator of  $M_L$ , the mass of the lot  $L$ , and can be easily calculated from the mass of the sample and the time sampling ratio.  $M_E$ , the sample mass, is usually small and can be measured by weighing the sample with a precision static balance.

The time sampling ratio  $\tau'$  can be calculated with a great accuracy from sampler technical characteristics such as the cutter velocity, the length of the driving chain or the width of the cutter opening. The calculation of  $\tau'$  will be different depending on whether the sampler is running continuously or discontinuously. In this last case, the sampling frequency has to be taken into account.

Thanks to the proportional sampling, it then becomes possible to obtain in one single operation both qualitative (contents) and quantitative (masses, volumes) information and to establish very accurate and precise metallurgical balances.



Figure 9. Simultaneous sampling of four slurry streams with mutual sampling proportionality.

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# History of the Pierre Gy Sampling Gold Medal 2003–2015

Kim H. Esbensen

KHE Consulting, [www.kheconsult.com](http://www.kheconsult.com)

Adjunct professor Geological Survey of Denmark and Greenland (GEUS), Denmark

Adjunct professor Aalborg University, Denmark

A highlight of every WCSB is the presentation of the Pierre Gy Sampling Gold Medal (PGSGM). The first award committee was elected before WCSB1, and has subsequently been augmented by the successive award recipients. An original medal design, Figure 1, stems from *Messieurs Pitard*, Esbensen and Francois-Bongarcon and was taken over by the highly effective WCSB4 committee under the inspired leadership of chairman Sean Duggan, who was also instrumental in securing funding for the minting of five medals.

The Pierre Gy Sampling Gold Medal is awarded to individuals who have made “significant contributions to teaching and dissemination of the theory and practice of sampling”. While the history of the PGSGM award presentations turned out to be convoluted and complex, it suffices to relate that the first recipient, selected with the help of Pierre Gy himself, was A.G. Royle (UK) as a sign of respect for his valuable help (teaching, translation) at a critical time in the career of Pierre Gy himself. Royle translated Gy’s famous book: *Sampling for Analytical Purposes* (1998). Most unfortunately the awardee was unable to attend WCSB1 for its presentation due to serious illness in his family. It was therefore left to the WCSB1 chairman to make the presentation at a later, more appropriate, time. This first turned out to be possible in 2010, when “Le bon Royle” (Pierre Gy’s own expression) was finally presented with his medal in his retirement home in the Cotswolds, UK, Figure 2.

After WCSB1, problems regarding the required sponsoring for the gold medal production ran into problems, which resulted in delays for the physical medal presentations at the time of the next conferences. A concerted effort finally solved the problems in time for WCSB4, when five medals could finally be minted and three could be presented simultaneously (a report of the presentation of the WCSB1 medal was also given at WCSB4). The official chronology of the PGSGM award could therefore now be



Figure 1. Pierre Gy Sampling Gold Medal (front side).

made up-to-date, and which hereafter goes into the official record in the following manner, see Figures 3 to 5.

- **A.G. Royle** (1924–2013) (WCSB1, Esbjerg, 2003) [awarded 2010]
- **P.O. Minkkinen** (WCSB2, Brisbane, 2005)
- **F.F. Pitard** (WCSB4, Cape Town, 2009)
- **D. Francois-Bongarcon** (WCSB4, Cape Town, 2009)

■ **P. Carrasco** (1950–2011) (WCSB5, Santiago de Chile, 2011) [awarded posthumously]

■ **K.H. Esbensen** (WCSB6, Lima, 2013)

■ **R. Holmes** (WCSB7, Bordeaux, 2015)

WCSB7, Bordeaux 2015 continued the by now well-established tradition and presented the seventh Pierre Gy Sampling Gold Medal to Ralph Holmes, Figure 7.

The recipient of the Gold Medal to be awarded at WCSB8, May 2017, Perth, has been selected. This will likely be the last awardee appointed in the current fashion. Since its inception, the award committee has gradually evolved by consisting of all accumulating Gold Medal recipients. Some of the members, present author included, are not overly happy with this somewhat irregular organisational arrangement. Plans are afoot to elect an independent committee at the WCSB8, Perth, in connection with establishment of a proper scientific association for our community. The Pierre Gy Gold Sampling medal will then become a committee organised by, and elected from the members of this new association.



Figure 2. Presentation of WCSB1 Pierre Gy Sampling Gold Medal to A.G. Royle. The postponed presentation (2010) took place at Mr Royle’s home in the Cotswolds, UK.





**Figure 3.** Pierre Gy Sampling Gold Medal awardees Francis Pitard (left), Pentti Minkkinen (centre) and Dominique Francois-Bongarcon (right). For logistical reasons presentation of the physical medals took place simultaneously at WCSB4, Cape Town, 2009.



**Figure 4.** A line-up of WCSB chairmen (left to right): Kim H. Esbensen (WCSB1); Ralph Holmes (WCSB2); Joao Felipe de Costa (WCSB3). At right Sean Duggan (WCSB4), facing Pedro Carrasco, designated chairman WCSB5. The 5<sup>th</sup> Pierre Gy Sampling Gold Medal was to be awarded to Pedro Carrasco, who tragically died only months before WCSB5. The Medal was presented posthumously to Pedro's children at the WCSB5 dinner by chairman Francis Pitard (Figure 5).



**Figure 5.** Presentation of the Pierre Gy Sampling Gold Medal to the family of Pedro Carrasco.



**Figure 6.** Presentation of the 6<sup>th</sup> Pierre Gy Sampling Gold Medal to Kim H. Esbensen took place, now in regular fashion, at WCSB6, Lima, Peru, 2013



**Figure 7.** Ralph Holmes with the 7<sup>th</sup> Pierre Gy Sampling Gold medal.



# Pierre Maurice Gy (1924–2015)

## Dr Pierre M. Gy's work: a 60-year revolution in the world of sampling statistics

Francis F. Pitard

14800 Tejon Street, Broomfield, CO 80023 USA. E-mail: [fpjsc@aol.com](mailto:fpjsc@aol.com)

In 1968, at MINEMET, I was introduced to Pierre's work. The intellectual level, scientific value and impeccable Cartesian approach of his work fascinated me. Immediately, I developed a passion for the Theory of Sampling, admiring the work of a man I had never met. His work could have been very useful at the French Atomic Energy Commission, where I did a lot of sampling to monitor radioactive contamination.

In 1981, we met for the first time in the USA, and it was instant friendship. I started my consulting business in 1985, in parallel with Pierre's work. My respect for Pierre's work remained unshakable, and I would like to share this with all of you who will dearly miss this remarkable mentor.

Many sampling theories were created in the past: I am familiar with most of them. None of them ever reached the depth, quality of thinking and completeness as Pierre's Theory does, dividing a complex problem into a flawless sum of smaller problems that can be solved one by one. Thank you Pierre for the Theory of Sampling, the only valid Theory of Sampling, the only one that should be part of sampling standards. But, there is much more to the man.

In a remarkable way, using Matheron's work, Pierre created a world of his own in variography as applied to metallurgical and chemical processes, leading to the new concept of Chronostatistics. It is a cousin of Geostatistics, as applied to process control and metallurgical accounting. The scientific breakthrough was awesome. The modern worlds of Statistical Quality Control, Statistical Process Control and Six Sigma offer valuable concepts to improve quality and productivity. Their statistical methodology would greatly benefit from the impressive effectiveness of Chronostatistics and its promising future. Thank you Pierre for the new world of Chronostatistics.

A direct consequence of Chronostatistics was for Pierre to enter the world of

blending. Within a few years, he was able to deliver the world of blending from paradoxical beliefs, and help that science to focus on the relevant variability. The impact on blending design, engineering and implementation was a breakthrough to manufacturers. Thank you Pierre for a logical, coherent theory of blending.

Pierre's enormous experience in sampling was the ideal ground for creating new ideas. Pierre created the concept of Proportional Sampling. It was a revolutionary idea companies took for a scientific curiosity with no practical applications. Indeed, it cannot be applied when 75% of the sampling equipment available on the world market is flawed, by design, by the way it is built, by the way it is maintained and the way it is used. Proportional Sampling is the future of good, reliable sampling equipment. It is only a matter of time before Proportional Sampling becomes a very popular way to perform a better job in sampling. Thank you Pierre for the theory of Proportional Sampling.

But, a remarkable man like Pierre would be an incomplete person without a passion for something other than sampling. We all need hobbies, as an anchor to cleanse our mind, reach serenity and at the end of the day come back to what we do best, and perform well again. My hobby is archaeology and writing novels. Pierre's passion is the tranquillity of high mountains, and the beauty of little wild flowers. The contrast between the mind of a great master in sampling, and the fragile, beautiful, little wild flower is the perfect image of Dr Pierre M. Gy's mind. If most men were given this extraordinary ability of moving, at any time, from charismatic achievement to humbleness, the entire world would be a better place today. Thank you my dear friend and mentor for combining extraordinary talent with humbleness and beauty.

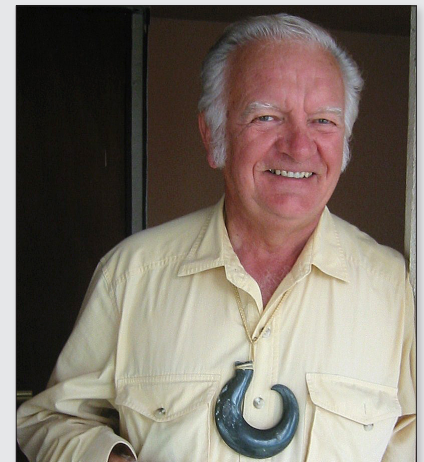
Pierre's work is superb, has enormous momentum and will inevitably conquer

many industries. Standards around the world are still either shy or resistant to the Sampling Theory, because of ignorance or because of conflicts of interest. Sixty years of patience and hard work are beginning to pay off within academic institutions. Resistance from statistical circles that are ignorant of the Theory of Sampling will soon become futile.

The very last time I saw Pierre in July 2015, as I was departing from our short visit to him in Bordeaux after the WCSB7 conference, he kissed my hand and murmured these words: "we have nice troops now!"

Indeed, his legacy will endure because of the formidable body of knowledge and talents accumulated and carried on by the World Conference on Sampling and Blending, its medallists, its technical committees, its publications and its academic and practical achievements.

On a last note for what it's worth, Dr Pierre M. Gy was a silent genius; and it's a shame that he was not recognised more by being honoured for his achievements as a "Chevalier de la Legion d'Honneur".



Dr Francis Pitard is a recipient of the prestigious Pierre M. Gy's Gold Medal for excellence in teaching and promoting the Theory of Sampling.

# Theory of Sampling and Geostatistics: a technical tribute to two geniuses

Dominique Francois-Bongarcon

Agoratek International Consultants Inc., North Vancouver, Canada. E-mail: [dfbgn2@gmail.com](mailto:dfbgn2@gmail.com)

## Introduction

The 1950s was a decisive period for the world of earth sciences: two geniuses of mathematical modelling in mining were at work simultaneously, developing theories that are now the basis for practically all we do in this and related areas. While Pierre Gy was developing modelling of microscopic scale variability and its effects on extraction of macroscopic samples of randomly selected broken material (ore, cement, food etc.), now known as the Theory of Sampling (TOS), Georges Matheron was developing models for large scale variability throughout space domains of auto-correlated variables, aka Geostatistics. Both these extraordinary scientists are no longer with us. I here want to pay tribute to both men by focusing on technical issues which bind their works together, although they actually did not regularly meet in real life (although they did at the onset of Reference 1).

As stated in a previous paper,<sup>2</sup> if the theory of the sampling of broken material (TOS) cannot be mentioned without reference to Gy's lifetime fundamental contributions, it can neither be fully understood outside of a geostatistical frame of reference.

TOS calls for some geostatistically flavoured concepts at small scale (Gy's formula), mixes with it at medium scale (sampling regime of one-dimensional flows) and is very much needed by larger scale geostatistics (data quality in view of estimation, variogram nugget effect, conditional simulations).

But these are not the only links between the two, and I will briefly mention a fundamental and more theoretical feature which is also at work, and which takes front stage when consistency is required between those two sets of tools. One that in effect joins the theory of the very small to that of the very large.

## Theory of Sampling, TOS (Gy)

Gy's formula for the relative sampling variance is the basis of numerical sampling calculations:

$$Rel.Var. = c f g \ell d^3 (1/M_s - 1/M_L) \quad (1)$$

In this expression, as we know,  $c$ ,  $g$  and  $f$  are material constants which can be known or derived from empirical characterisation of the material in question (sometimes only by non-trivial efforts though),  $d$  is the comminution nominal P95 size.

As to  $\ell$ , the liberation factor, shown recently in Reference 3 to be the ratio of any sample variance to the variance of the liberated sample with the same average number of fragments, it was earlier proposed<sup>4</sup> to be modelled as:

$$\ell = (d_i/d)^b \quad (2)$$

where  $d_i$ , which is the mineral liberation size, relates directly to the size of the coarsest grains of mineral or metal and  $b$  is an exponent between 0 and 3 that must normally be calibrated experimentally. Thus the overall exponent alpha of  $d$  in the formula is  $\alpha = 3 - b$ . This model transforms an unusable formula (1) into a workable one.

After proper experimental calibration of the set of parameters involved, using these formulations, the practical variance of any sample can be predicted with reasonable validity (as always very much dependent upon respecting all TOS' requirement for representative sampling).

Furthermore, letting  $V$  be the average fragment volume  $V$  in the lot and  $\rho$  the average density of the rock in the lot, it can be shown<sup>5</sup> that:

■ From the point of view of the practical sampling variance, a lot behaves as if actually constituted by a series of fragments all of volume  $V$ . This can at times clarify the understanding of sampling enormously but unfortunately has not received a lot of attention to date.

■ With  $Var_v = c \ell / \rho$  being the variance of the average-volume fragment, i.e. the dispersion variance, within the corresponding type of mineralisation, of a support of a known, calculable size  $V$  and with enough information on the mineralisation specifics, the effective

variance  $Var_v$  is also calculable using TOS.

## Geostatistics (Matheron)

The basis of geostatistics is Matheron's variogram curve and its modelling. Sampling errors will affect its discontinuity jump at the origin, the famous "nugget effect". It is one of the major achievements of TOS, that this contribution can be evaluated in all its components (the nugget effect consists of all incorrect—as well as all correct sampling errors, to which is added the total analytical error). This rather practical, indirect link between TOS and Geostatistics is well known and forms the basis of a powerful first understanding of the total measurement system uncertainties in practice.

Once a *valid* variogram model is available in a homogeneous domain (a *sine qua non* condition of good application of geostatistical modelling), then the dispersion variance of *any* support of known shape and dimensions throughout the domain can be predicted. The variogram of a different support than that of the data on which the original variogram curves were calculated, can also be derived theoretically.<sup>6</sup> Finally, the estimation variance (providing a precision) of any linear estimator can also be readily evaluated, *ibid*.

Thus it can be stated that geostatistics is "the science of variances", but this notably only reaches its full potential when used in synergy with TOS.

## TOS vs Geostatistics

Gy and Matheron's theories need each other, indeed they complement one-another perfectly. Taken together, they offer the sampling practitioner the complete palette of variance study tools, from very small to very large scale. They are inseparable. Each one is incomplete without the other. Together they have empowered the earth science practitioner with an incredibly efficient modelling capability. We should rightfully marvel at the fact that these two theories were developed precisely at the same

time by two different minds, independently form each other, in the 1950s.<sup>1</sup>

Now if we take a lot of broken ore from a homogeneous domain in a deposit (e.g. a copper mineralisation in a zone with only Chalcopyrite as copper mineral), we have seen that TOS allows us to calculate the dispersion variance  $Var_V$  of the average-volume fragment from appropriate empirically derived mineralogical parameters. The total sill of the variogram calculated based on this support, which depends directly from the sill of the original variogram, normally represents the dispersion variance of that support, i.e. the variance of data sample taken at random over the domain. It must therefore be equal to the variance of the average-volume fragment as predicted by TOS.

This is not a minor statement: provided domaining makes sense mineralogically, while TOS tells us what proportion of the variogram nugget effect is represented by the various sampling errors, it also tells us what the total sill of that variogram *should be*. This is the *ultimate link between TOS and Geostatistics*, and it can be paramount in terms of consistency, especially if comparisons are to be made between sampling variances and variances calculated using variograms, or if one wants an accurate derivation of the proportion of the nugget effect related to sampling errors.

If the mineralogy is well known, including  $d_t$ , but parameter “ $b$ ” in Equation (2) is not and the variogram domain is homogeneous enough, the relationship can even be used to adopt for that parameter “ $b$ ” the value that brings full consistency to the sill of the variogram, thus providing a principally new calibration method for models of liberation factor.

It is clear the two theories are in fact but two faces of the full, complete framework of modelling of uncertainty in the earth sciences, and both their authors should equally be entitled to the utmost gratitude from the scientific fields and industries who have benefitted so much from their work, as well as to the highest respect from their practitioners.

### Final, personal note

I have had the immense personal privilege of working with both Gy and Matheron and have had the singular opportunity of studying and researching their teachings at some significant depth. Recognised geniuses in their respective areas of technical research and expertise, they were both equally great human beings. They shared the same

kindness and patience, impeccable professional ethics and a common and outstanding social intellect. They will never be forgotten as individuals, no more than will their fundamental technical contributions.

Directly or indirectly, these two giants of mathematical modelling have taught me important lessons:

- Models are only models and if they can easily be invalidated, they cannot be proven, but only validated, in the long term, by the “sanction of practice” (Matheron<sup>7</sup>).
- Keep a critical mind about the underlying theories (including theirs!).
- Internal consistency of models is paramount and should never be sacrificed lest the models can grossly mislead us.

As their heirs, we all have a duty to use, disseminate, further clarify, promote and harmonise their teachings. As to the industries that have taken advantage of the two theories for some 50–70 years, it is suggested they should be more visibly grateful, and each practitioner in the industry should relentlessly fight for their further overall recognition. Indeed, how many companies in the industry would be here today was it not for their use of TOS and Geostatistics? And without the magnificent influence of these two giant mentors, how many of us would be as successful and enjoying our works, as is the case in our respective technical domains?

They have been, in turns, friends and mentors. Occasionally, they opened up on their inner feelings. I will always remember these more tender moments, when one was lamenting the cruel betrayals of professional life, discounting geostatistics as only a “social thing”, with a snarl, while insisting the only important thing he had ever written were his two mathematical books, or when the other, still shedding tears, would evoke the war and the terrible fate that had been that of persons close to him. They were compassionate but morally strong beings, and the feelings they shared were as inspiring as their technical insights. Both of them could recount situations of life where they had chosen the hard way of ethics and moral duty over the easier path. As former friends, we shall also cherish the more intimate lessons of life they had shared with us.

Pierre Gy was particularly happy when we finally launched the WCSB conferences, in his honour, as it was for him a guarantee his works would survive him, a fear he had had for very long. Figure 1 shows both of



Figure 1. The author and Pierre Gy at WCSB1, Esbjerg, 2003.

us during the happy times of WCSB1 in Esbjerg, Denmark, in 2003.

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## Pierre Gy Tribute

Ralph J. Holmes

Chief Research Scientist, CSIRO Mineral Resources

I commenced work with CSIRO (Commonwealth Scientific and Industrial Research Organisation) in Australia in November 1971, working on nuclear methods for on-stream and bulk analysis of mineral commodities, particularly iron ore. My interest in sampling grew from the need to calibrate the nuclear methods for analysis that I was developing against accurate conventional analyses, and I discovered to my dismay that the sampling and subsequent analysis of the bulk samples that I was using for calibration purposes left a lot to be desired, and the resultant analyses were not accurate enough. This led to more than 40 years involvement in promoting and developing improved methods for sampling mineral commodities such as iron ore, coal and base metal ores and concentrates, as well as improving and/or developing ISO (International Standards Organisation) sampling standards for a range of mineral commodities, drawing of course on the remarkable work by Pierre Gy in the sampling area.

I first met Pierre at the First Australian International Bulk Materials Conference in Sydney in 1982, where he presented a paper entitled "Sampling of high capacity streams". This paper was a terrific summary of the key requirements for sampling moving streams and a "watershed" event for me, convincing me that I had to learn more about Pierre Gy's sampling theory and practice as elucidated in his seminal book entitled *Sampling of Particulate Materials—Theory and Practice*. I did not meet Pierre face-to-face again until August 2003 in Esbjerg, Denmark, at the First World Conference on Sampling and Blending (WCSB1) organised by Professor Kim Esbensen, but we did correspond from time to time over the intervening years. In fact in 1988 Pierre agreed to be one of the referees for my successful CSIRO promotion case, and he encouraged me to continue work on teaching the mining industry about correct sampling practice. To assist me in this task, he provided me with a complimentary copy of his most recent book at the time entitled *Hétérogénéité Échantillonnage Homogénéisation—Ensemble cohérente*

*de théories*, which was still in French—a bit of a challenge for me to read with my limited French language skills! At Pierre's request, I also had the honour of presenting on his behalf an invited paper entitled "Theory of Sampling" at a TQM Symposium held in Melbourne, Australia, on 4–5 December 1995 that Pierre was unable to attend and present in person. Incidentally, I also received encouragement from Allan (Bon) Royle from the University of Leeds, whom I invited to a conference in Sydney as a Keynote Speaker. Bon had a strong association with Pierre Gy and played a major role in translating into English the entire French manuscript of one of Pierre's sampling books, which was a key factor in Bon being awarded the first Pierre Gy Gold Medal at WCSB1 in Esbjerg in 2003.

The task of revising ISO Standards to conform to Pierre Gy's sampling theory and correct sampling practices has been an arduous journey for me, requiring much persistence and patience. Considerable progress has been made, but there is still room for improvement. One of my first attempts at updating an antiquated ISO sampling standard to be consistent with the teachings of Pierre Gy was for coal and coke. I submitted a revised draft for discussion at the ISO/TC 27/SC 4 meeting held

in Lexington, Kentucky, in USA. However, after my introductory comments pointing out the deficiencies of the existing standard and the need to revise the Standard according to Gy's theories and practice, the committee passed a "gag" motion that Australia would no longer be heard! The committee simply did not want to hear about Gy's sampling theory and rock the boat. Anyway, the strategy eventually failed and at the next meeting two years later the participants were prepared to listen to what needed to be changed to conform to correct sampling practices. Curiously, I am now the international chair of that very same committee and the latest revision of the ISO coal and coke sampling standards are in much better shape.

While our paths did not cross in Bougainville when the copper/gold mine was still operating, we both visited Bougainville Copper to assist with improving sampling practices. One of Pierre's proposals that was implemented was to use sector cutters on the ground to sample blasthole cuttings. Our mutual friend John van der Linden, who was Chief Chemist at Bougainville Copper at the time, tells a number of stories about Pierre's adventures trekking around Bougainville Island. In fact we almost lost Pierre on Bougainville while



Pierre Gy in Bordeaux.

engrossed in photography, which was one of his hobbies together with mountain climbing. John tells the story of Pierre stepping backwards to get a better view for a photograph and accidentally stepping off the edge of a cliff. Fortunately, Pierre was able to hang on to the edge of the cliff, presumably with the assistance of some of the prolific local vegetation, and was safely rescued.

The second time I had the honour of meeting Pierre Gy was at WCSB1 in Esbjerg in 2003. It was inspirational to be in Pierre's presence at the conference and to hear his personal account of his 50 years involvement in the theory and practice of sampling. At the end of the conference, the location of the next conference was discussed and it was great to have Pierre's support for holding WCSB2 in Australia,

which I subsequently organised and chaired in association with The Australasian Institute of Mining and Metallurgy (The AusIMM). In Esbjerg I had promised a beautiful tropical environment for WCSB2 on the Queensland Sunshine Coast. However, what I had not anticipated was "tropical rain" over the whole three-day period of the conference! Still, this ensured good attendance at the conference itself. Unfortunately, Pierre was unable to make the long trip down to Australia for WCSB2. One of my treasured possessions is a copy of Pierre's book *Sampling of Particulate Materials—Theory and Practice* signed by Pierre Gy himself after the conference with the hand written message "To Ralph with congratulations on WCSB2. Best personal regards, Pierre Gy".

The last time I met Pierre was immediately after the WCSB7 conference in Bordeaux,

France, in June 2015. A small group of delegates made a short visit to the nursing home where Pierre was residing at the time. Pierre was presented with a copy of the Proceedings signed by all the authors and he engaged in conversation with those present. At the WCSB7 conference dinner, I was most honoured to receive a Pierre Gy gold medal for "Excellence in Teaching and Application of the Theory of Sampling". It was a truly humbling experience to receive an award named after such an eminent person in the sampling domain. Thank you Pierre for your encouragement over the years.

It was very sad to learn of Pierre's passing in November 2015, which was not very long after my last meeting with him in June 2015. However, Pierre leaves behind an unparalleled sampling legacy for us to continue to teach and promote.

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## Pierre Gy Tribute

Ana Carolina Chierigati

I was fortunate to meet Dr Pierre Gy shortly after WCSB7 in Bordeaux. As I was sadly not able to accompany the group that visited Pierre on the last day of the conference, I arranged to meet him alone on my way to the airport going home to Brazil.

This I will never regret—or forget. Behind the genius of the Theory of Sampling, I found a sincerely humble, kind and thoroughly warm-hearted man, as is in every way documented by the few photographs that I was able to have taken. Better than any words, they show an instant rapport between the disciple and the master, the man I will always remember and cherish. I am so thankful to all my friends who made that moment possible, allowing me to experience those few priceless minutes that filled my soul with joy. I was truly honoured to meet Pierre Gy.



Ana Carolina Chierigati with Pierre Gy in Bordeaux.

# Pierre Gy: How I discovered the Theory of Sampling and got to know a great scientist

Pentti Minkkinen

## Introduction: first encounters with sampling problems

I graduated from Helsinki University of Technology, Department of Chemistry in 1969. In analytical chemistry courses at that time hardly anything was taught about sampling, besides the phrases “the result is no better than the sample” or “the sample must be representative”. But “how to” achieve that was not explained. Coning and quartering was the only recommended sample size reduction method. Sadly, this is still the case in the majority of universities offering chemistry. However, in our final exam on analytical chemistry there was the following question: “You receive a container containing liquid, some slurry and a lump of metal (size about 1 kg) with a request to estimate the average concentration of the metallic constituents of the sample. How do you proceed with the analysis?” In my answer I proposed that if it is possible to dissolve the whole sample a good estimate of the average concentrations is obtained by analysing the resulting solution. If that was not feasible, the liquid, slurry and the metal piece should be separated and analysed separately. The metal should be sampled so that several subsamples should be drilled from it from different sides. Many years later I learned that I had described the principle of the composite sampling mode.

I met my first real-world sampling problem in 1966, when still a student and still ignorant of any theory of sampling. I had a summer job as a shift foreman in a company producing granulated super phosphate. The company was exporting this product to a customer who had a very tight quality criterion for the phosphate content. There appeared to be a quality problem. Samples taken from the stream coming from the granulator gave phosphate values that were below the specification, whereas samples taken at the storage room gave results meeting the specifications. I looked at the problem. Samples were taken manually from the

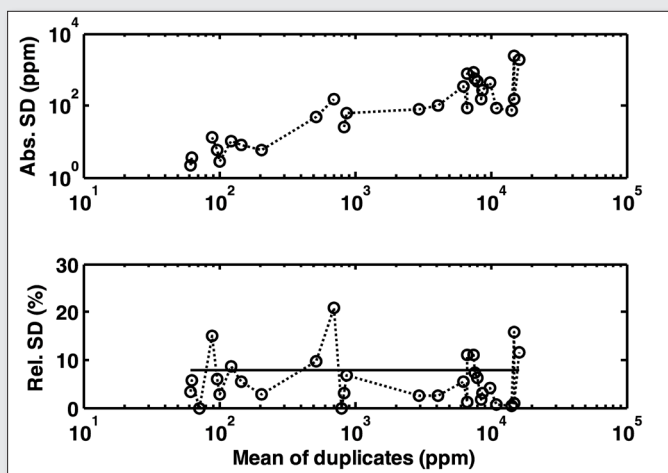
middle of the falling stream coming from the granulator with a cup having a round opening and volume of about ½ litre. I could actually observe that the fine and coarse particles in the stream were segregated and were being sampled unevenly from the middle of the stream. So I suggested that the phosphate content of the fine and coarse part were analysed separately and the result indeed showed a difference in phosphate content explaining the observed deviations. So I designed a sample cutter with a rectangle opening to be used manually as a cross-stream sampler. Samples taken with that method gave results which met the specifications.

From June 1973 to July 1974 I was working as an Associate Expert (Analytical Chemist) in a United Nation Development Program (UNDP) Mineral Exploration project in Turkey. In this project a quality control programme based on duplicate sampling was in use. This programme was used to monitor the uncertainty of both the field sampling and laboratory results. In the field, duplicate soil and sediment samples approximately 2 m apart were regularly taken and, in the laboratory, duplicate analytical samples were prepared and analysed from the laboratory samples. Reference samples and inter laboratory comparisons were also used for control purposes. The results were scrutinised but not systematically analysed. In my opinion the methods then proposed to analyse such data were not adequate for their intended use. The main problem was that the exploration samples had wide ranges of concentrations and quite obviously the standard deviations both of the samples and analyses were dependent on concentration. Consequently, the variances were dependent on the concentration level which made the assumption on normal distribution invalid and the pooling of information obtained from the duplicates difficult and very doubtful. This had to be taken into account in the subsequent data analyses.

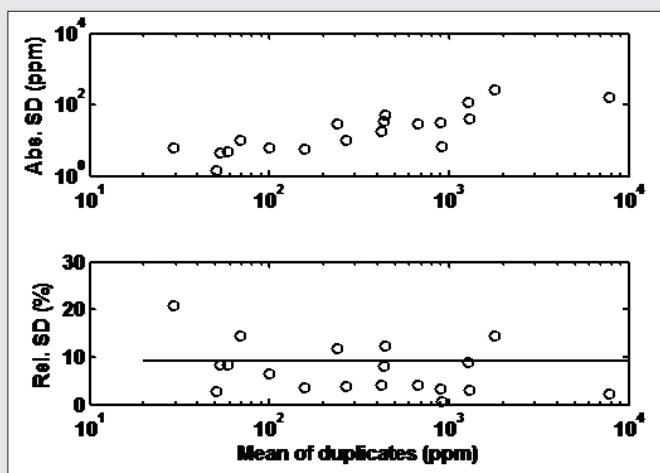
The Anatolian plateau is covered by snow in winter for a few months. As consequence, very few exploration samples arrived in the laboratory during this time. That gave me time from routine work to think if the existing quality control data could be used more effectively than up to then. I noticed that while absolute standard deviation was strongly dependent on concentration, the relative standard deviations were practically constant over a concentration range covering several order-of-magnitudes starting from concentrations approximately five times above detection limits of the analytical methods. At these concentrations the relative standard deviation estimates calculated for each duplicate and having just one degree of freedom could be pooled into one *realistic* total relative standard deviation estimate with degrees of freedom equal to the number of the duplicates. I wrote an internal report proposing this method to be used as quality control method both for the laboratory and field sampling. The report was well accepted with recommendation to adopt it in other UNDP projects. After working in another UNDP project in Egypt, I returned in August 1975 back to Finland and continued my graduate studies at Helsinki University of Technology where I wrote my graduate thesis on the use of duplicates in routine quality control. Figures 1–2 show some examples of how the method can be used in routine control. Figure 1 shows the results of atomic absorption spectrometric determinations of lead from the laboratory duplicates of soil samples covering a concentration range from 60 ppm to 17,000 ppm. As can be seen, the absolute standard deviation has a strong concentration dependence whereas the relative standard deviation now shows systematic concentration dependence. The pooled relative standard deviation was 7.8%.

The uncertainty of the primary sampling was tested by taking field samples approximately 2 m apart and submitting both to





**Figure 1.** Duplicate Pb analyses from geological soil samples. Absolute standard deviations from the duplicates (upper panel) show strong concentration dependence. Relative standard deviation estimates (lower panel) are independent on concentration, thus the pooled value (horizontal line),  $s_r(\text{pooled})=7.8\%$ , can be used over this whole concentration range as the estimate of the standard uncertainty, with a coverage factor 2, giving the expanded relative uncertainty value  $U_r=15.6\%$ .

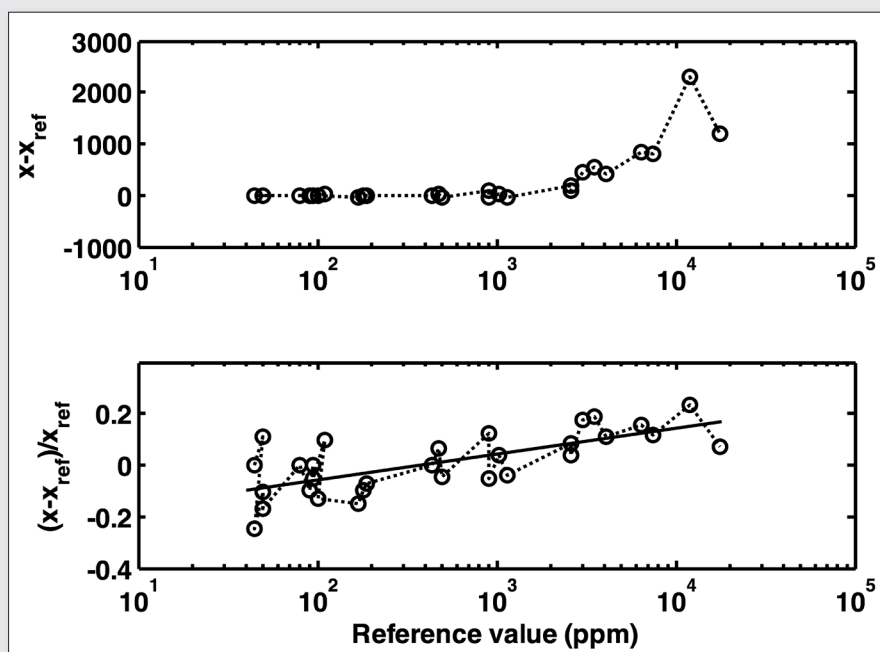


**Figure 2.** Duplicate Pb analyses from field duplicates taken about 2m apart. Absolute standard deviations from the duplicates (upper panel) show strong concentration dependence. Relative standard deviation estimates (lower panel) are independent on concentration, thus the pooled value (horizontal line),  $s_r(\text{pooled})=9.1\%$ , can be used over this whole concentration range as the estimate of total standard uncertainty, with the coverage factor 2, giving an expanded relative uncertainty value  $U_r=18.2\%$ .

analysis in laboratory. These results give the sum of the short range field sampling variance in addition to the variance of the laboratory procedure. Figure 2 shows the absolute and relative standard deviations calculated for each sample pair. As in Figure 1, the absolute standard deviation is strongly dependent on concentration, but the relative standard deviation is obviously independent on the tested concentration range. The pooled relative standard deviation estimate of the whole data set is here 9.1%. This value does not differ significantly from the laboratory duplicate results showing that laboratory sample preparation (sieving, subsampling) and analysis contribute the major part of the total variance. In retrospect, the low primary sampling error can be explained. Soil samples were dried and then sieved. The below 80 mesh (0.18 mm) fraction was used as the laboratory sample. From it 500 mg or 200 mg analytical samples were taken with a spatula, i.e. as grab samples. It is obvious, of course, that in this case segregation and fundamental sampling errors are high. For instance, at 500 ppm Pb (if lead is present as galena) the relative standard deviation of the fundamental sampling error of a 200 mg sample is about 6%. At that time nobody had any idea yet how to calculate it. Anyway, these results showed that the sampling analytical methods were adequate for mapping interesting areas.

In the third example (Figure 3) lead results from the central laboratory and the field laboratory were compared by analysing the same samples in both laboratories. In the central laboratory the metals from the geological field samples were extracted by *aqua regia* and in the field laboratory with nitric acid. By using the

central laboratory results as reference values, the absolute and relative differences from the reference values are plotted as function of the reference results. As can be seen, the field laboratory results are negatively biased at low concentration range and positively at high concentrations. The relative differences can be modelled with



**Figure 3.** Comparison of two methods of lead analysis from geological soil samples, based on absolute (upper panel) and relative (lower panel) deviations from the reference method (*aqua regia*). The relative differences of the methods can be modelled with linear regression as function of the concentration. The residual standard deviation from the regression line is 0.086 (=8.6%).

linear regression and, if necessary, the field laboratory results can be converted to be equivalent with the central laboratory results. The traditional t-test of duplicates fails in this case to detect any significant differences with these two sets of the results—also an interesting result, and still often used today to analyse data sets similar to this example.

### Getting to know about Pierre Gy's Theory of Sampling

In 1976 I got first a temporary, and later a permanent job, as Associate Professor in inorganic and analytical chemistry at the newly founded regional Lappeenranta University of Technology (LUT). The next year the first students were to start chemical engineering studies. Dr Seppo Wilska, Professor of Chemical Technology, was the first full professor appointed and the head of the new department of Chemical Technology. He also planned the curriculum of the department. He had planned that I should give a one semester course in process analytical chemistry to third year students. He stated that: "... in process analytical chemistry there is a fundamental lack of knowledge, namely on the importance of sampling. Nobody seems to be interested in sample quality and nobody is teaching this subject". Since I had shown interest in analytical quality control, he suggested that I should also start teaching *sampling*. More importantly, he indicated that there was a man who had been working with sampling and had developed a very useful theory, and equations, with clever approximations allowing the estimation of actual sampling variance from known material properties. This man, needless to say here, turned out to be Pierre Gy. Professor Wilska also gave me Pierre's first publication, written in German. In that article Pierre presented his famous equation for estimating the variance of the fundamental sampling error. I presented this equation and its applications in my first lecture series about sampling. When I received Pierre Gy's 1982 book: *Sampling of Particulate Materials—Theory and Practice* (second revised edition) the beauty of TOS really opened up to me. That was the first presentation that logically covered the whole sampling process: design and operation principles of sampling equipment, sources of sampling errors and the data analysis needed in estimating the total

sampling uncertainty. Completely new to me was also that the three different sampling modes, random, stratified and systematic, produce different variances of the lot mean, even if exactly the same number of primary samples are taken and analysed from the same lot. It took some mental wrestling to believe this, so deeply had the normal distribution rooted in my mind. Many guidelines I had read so far gave the advice always to use random sampling if there is systematic variation in the sampling target. Pierre Gy showed that the random mode leads to higher variance of the mean than the two other approaches, periodic process being the exception, if the sampling frequency is too low. The expected experimental variance is the same and independent of the sampling mode, however. Only in estimating the variance of the mean, correction for the autocorrelation due to the systematic variation depends on sampling mode. Gy also presented a practical method for estimating the correction needed for one-dimensional lots: variographic analysis. Later I reviewed, for *Chemometrics and Intelligent Laboratory Systems*, Pierre Gy's second book written in English (1992). Reading these books it became obvious that the fundamental *assumption* on randomness is hardly ever justified in planning primary sampling, and more importantly, leads to inflated and too expensive sampling plans.

Campbell *et al.*, "Sampling, sample preparation, and sampling plans for food-stuffs for mycotoxin analysis", *Pure and Applied Chemistry* 58 (1986) described an experimental study where the variances of the primary sampling and laboratory measurements were estimated as function of the mean concentration of aflatoxins in peanuts lots. That article gave  $112 \text{ mg kg}^{-1}$  for the average aflatoxin concentration in mouldy peanuts. I had an idea to compare these experimental results to the results obtained by using Pierre Gy's theory. To have the other necessary material properties I drove to the nearest shop and bought 100g of peanuts, came back to my laboratory and in half an hour I had estimates for the necessary material properties: nominal particle size, density, size distribution and shape factors. Gy's famous equation gave a variance estimate that was about half of the experimental value. As mould-producing aflatoxin is usually present in small

pockets irregularly distributed spatially in peanut lots, the grouping and segregation error most certainly also has to be taken into account. According to Gy, the grouping and segregation variance can be equal to fundamental sampling error variance. When account is taken of this theoretical result, which cost only the 100g of peanuts and half an hour's work, my small empirical study gave almost exactly the same result as the massive experimental study of Campbell *et al.*, which must have cost at least tens of thousands dollars or more. That was enough to convince me of the value of Pierre Gy's Theory of Sampling (TOS).

### Meeting Pierre Gy personally

To make calculations easier I had written a small computer program for different applications of Gy's fundamental sampling error equation. I demonstrated it at one analytical chemists' meeting. One of the commercial participants, who sold analytical instruments, stated that he absolutely *must have* this program. Often his customers complained that his analyser was not reliable, when, in his opinion the analyser was working properly—but he suspected that the real culprit was poor sampling and sample preparation. With my program, he believed he would be able to prove this. So I commercialised this little program and called it SAMPEX, with the help of friend who had a small company producing programs for process monitoring and other process applications. With audacity I sent this program to Pierre Gy for his comments. I was very pleased when he quickly answered and told that he liked it ...

The first time I had a chance to meet Pierre personally was in Graz 1994 at the EURACHEM Workshop, where he was an invited speaker. Besides sampling and analytical quality control I was also teaching *chemometrics*, applying this multivariate data analysis approach extensively in my own research. But my fellow chemometricians were mainly, indeed overwhelmingly ONLY, interested in the multivariate methods themselves (PCA and PLS) and their applications to solve complicated chemical problems. Also in this community there was little, if any, interest in data quality. Personally I had an opinion that TOS is, or at least it should be, a branch of chemometrics. I presented contributions related to TOS



Figure 4. Photo taken at Riitta Heikka's dissertation defence. Left to right: Pentti Minkinen, Pierre Gy, Riitta Heikka and Kim H. Esbensen.



Pierre Gy in Cannes.

in several chemometric conferences and in private discussions over many years. The response from my friends usually consisted in heads nodding politely with comments like: "very interesting". However, *nothing* more followed.

That probably would still be the case, even today, were it not for Pierre Gy's personal involvement. In the late 1990s I had

a graduate student, Riitta Heikka, whose doctoral thesis was concerned both with applications of chemometrics as well as sampling. She defended her thesis late in 1998. I invited two opponents to the dissertation: an already seasoned chemometrician, Professor Kim H. Esbensen and an even more seasoned sampling expert, Dr Pierre Gy (Figure 4). I had for some time had some hope that surely my friend Kim would be able to understand the importance of TOS, alas up to that time to very little avail. But when these two gentlemen of science met, officiated together and, probably most influentially, spent an entire lay-over weekend in the same hotel in my regional city of Lappeenranta where little of the cultural activities of cities like Copenhagen and Cannes were to be found (and temperatures were well below zero), Kim could not resist any more. He saw the light, converted on the spot and became a zealous disciple of Pierre Gy. That is probably my greatest contribution to promote TOS! The rest is history. Kim took the beacon and the result is a successful series of sampling conferences, this new communication platform devoted to TOS, a standard based on TOS and many publications.

### The next 15 years

After Riitta Heikka's dissertation I had occasion to meet Pierre Gy many times. It was always a distinct pleasure. The last time was as part of the small delegation, who after the WCSB7 in Bordeaux 2015, organised a meeting at his caretaking institution. He was in good mood and happy to receive visitors.

At the Graz meeting he told in his lecture that for tens of years during his consultancy all over the World, he was always asking his clients who in their company were *responsible* for proper sampling. The usual answer was: "*Err, somebody else, not in my department*". So far, he said, he had been unable to identify who that particular somebody was. He also challenged analytical chemists to take the lead in promoting TOS. Not all that much has changed in the intervening 20 years or so—or maybe it has ....

I owe Pierre the greatest debt of gratitude. At least half of my professional success both in academia and private consultancy has resulted directly from TOS. That has given me a chance to get new friends, see new interesting places and the chance to work with interesting problems and talented people. I am very proud to have been honoured as a recipient of the Pierre Gy Sampling Gold Medal. I am in fact the only recipient of both the PGSGM and the Herman Wold Gold medal in Chemometrics.

Pierre once told me that he was afraid that after his time was up, TOS would also go into oblivion. He was disappointed how slowly the analytical community reacted to his challenge, and *ditto* universities in accepting TOS into their curricula.

Friend Pierre, I assure your soul should be at rest, and in peace. You, and the TOS will never be forgotten. You will live in our memories and the acceptance of your Theory of Sampling will, eventually no matter however slowly in appearance, win ever wider acceptance and respect. We are all dedicated to work on tirelessly for this ....

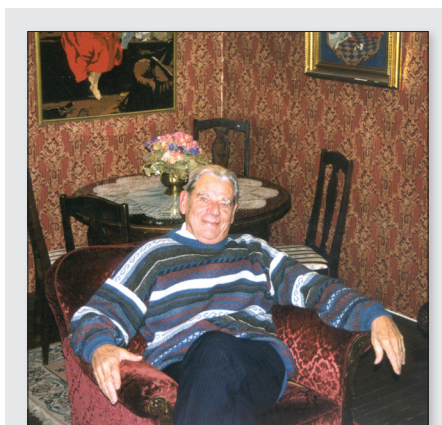


## Pierre Gy (1924–2015): a monumental scientific life

Kim H. Esbensen

**P**ierre Gy's legacy is both scientific and personal, for both community and individuals. My scientific tribute to Pierre can be found elsewhere in this issue.

I first met Pierre Gy in December 1998 when Professor Pentti Minkkinen called upon both of us as opponents at the defence for one of his PhD students, Riitta Heikka. I was the chemometric opponent while Pierre was the sampling expert *par excellence*. Aside from the serious academic duties discharged, this meeting allowed ample time for all three of us to get acquainted with one another for the first time. I had never met Pierre personally before. This meeting resulted in a 180° change in my professional career.



Pierre Gy at the Majorskaya Tea Salon, Lappeenranta, a Saturday in December 1998. My professional work was transformed forever.

reciprocal characteristic: here was also a gentleman, with a very modest attitude and demeanour (exceedingly *rare* traits in the overly brash, competitive academic world).

After this meeting I immediately started invoking TOS in my academic work at two universities (HIT, Norway and AAU, Denmark), deliberately focusing on didactic ways and means to broaden the potential impact of TOS, along with chemometrics and process analytical technology (PAT). In this context I was highly appreciative that Pierre found occasion to participate in two, very different conferences soon after: the first took place in 1999 in Porsgrunn, Norway, the second in 2003 was the inaugural WCSB1 in Esbjerg, Denmark.



In "A Personal History" paper in the WCSB1 Proceedings (reprinted in this issue), Pierre details what these events meant to him. Additional historical facts, surrounding these two events follow below.

### Norwegian sojourn: scientific and personal

In 1999 I had the honour of organising the 6<sup>th</sup> Scandinavian Symposium on Chemometrics (SSC6), in Porsgrunn, Norway.<sup>2</sup> Sampling and TOS were introduced to this field with prominence on this occasion, since Pierre had accepted my invitation as keynote lecturer. Finally, a head-on introduction of exactly what had been missing in chemometrics in its entire 40+ year history up to then! I was optimistic that most of the conference attendees would be ready to listen to the important and valuable messages offered by TOS, and would begin to consider how it could broaden the scope of data analysis in general, and of chemometrics in particular. Who better to hear it

*A personal note: after many years of continually growing dissatisfaction with geochemical, technological and industrial sampling (today, we would call this "specimenting"), accumulated over the first two decades of my professional career in chemometrics, things really came to a head. Here at last was an explanation of the reason why so many chemometric models did not work in practice (a mystery to most chemometricians until the matter of proper data quality and proper validation was introduced in this field). The Theory of Sampling*

*(TOS) explained everything that was needed to understand these enigmas and also tells everything that is needed to do something effective to resolve issues... other than just talking about them. So after half a career exactly, I realised that it was time for a change: WHAT could be more important than inducting TOS into other fields of science tormented by unrecognised sampling error effects?*<sup>1</sup>

My initial impression was that of Pierre's scientific overwhelming capacity and brilliance, which was accompanied by a rare



From a PhD defence in late 1998, Lappeenranta, Finland. The defence was a thoroughly happy affair. The three properly attired academic gentlemen met one-another for the first time.



from then the originator himself? I placed the keynote lecture between the main course and the desert at the gala dinner as a reflection of the *grandeur* of the moment! Alas, I have an admission to make here—this placement was perhaps the most blatant misjudgement in my entire academic career! Disaster followed.

As the lecture progressed a great commotion (complete with increasing noise level) could be felt (and heard) from the conference bar, which was situated just outside the open door of the dining area. Typically, such an intense chatter is considered a hallmark of a successful scientific conference—only *not* at this specific time-and-place on the programme when the lecture was only about halfway through. As matters progressed and the volume increased, I became more and more worried... more and more than a few of the delegates did not grasp the historic moment and the transformational message... quite *au contraire!* In the end I was completely mortified on behalf on the chemometric society—such disregard!

However, Pierre Gy revealed his true gentleman's attitude when trying to console me (not the other way around): "These are young kids—do not worry about them. They will either see the light later in their careers, or they will not. It is only important to *try* to turn things around, and do better—it does not matter in the eyes of history how long our endeavours take". Here I learned an essential lesson in patience and humility, and since the conference was otherwise a resounding success, we decided to move on from this historical intermezzo. Truth be told, this fiasco has been a source of much amusement ever since.

Indeed the conference also had another meaning for Pierre and his wife Sylvia, a matter of quite a different kind. Before the conference Pierre had asked me a personal favour: "Please help me locate a region, in

Norway bearing the prefix "Gy..." or any derivative thereof.... It is essential that any area so identified was a region of Norway that was home to Vikings approximately 1000 years ago". Despite my initial thought, which was to be somewhat mystified, this turned out to be an easy quest.

The photograph below, taken by Pierre himself in 1999, stems from the post-conference tour of Norway he and Sylvia enjoyed. Later he told that he had indeed found "his Gyland", the very region in southern Norway from where his family's ancestors in the Normandie originated—"just 1000 years ago". The Gy family annals are quite certain and specific regarding its Viking roots. This trip provided him a chance to close a 1000-year historic circle. He told me later how he revelled in telling his family all the details from this rare opportunity to enrich the historical family background and perspective.

Not long after this conference Pierre again visited Norway, this time for a

full-length course on TOS, with a decidedly more attuned audience from both academe and industry. From this event there are only fond memories—let history show some moments thoroughly enjoyed by Pierre of both the impact of the lectures (right, opposite) as well as the extraordinary camaraderie experienced (left, opposite).

### A return to roots and a new beginning

I changed academic affiliation in the summer of 2001 when I began a 10-year affiliation with Aalborg University, campus Esbjerg, in my native country of Denmark. My first job was to place TOS on the academic curriculum that I was responsible for. As part of that I was surprised and intrigued by the fact that there had never been any meeting or conference dedicated to the science of sampling, none, never! I decided to take a giant leap and organise the first world conference dedicated to sampling,



Gyland, Flekkefjord, Norge

This geographical family heritage was so important for Pierre that he included this photo, as well as several others from this post-conference Norwegian sojourn, in his later TOS lectures.







Pierre Gy again in Norway. He lectured with gusto and perseverance (right) and partied with even more... (left).

was held in the conventional format, but with the deliberate aim to honour one man and his monumental scientific oeuvre. It is no coincidence that the proceedings from WCSB1 were produced as a special issue of an established, peer-reviewed journal *Chemometrics and Intelligent Laboratory Systems*,<sup>3</sup> so as to be easily available for all posterity.

In 2003, Pierre agreed to write a summary of the Theory of Sampling (TOS), but only after *considerable* persuasion. As soon as it got underway, however, the opportunity completely overshadowed all his earlier reservations. In fact, this summary ended up being *five* full papers, which formed a work of 67 printed pages: three technical summaries of TOS, one complete bibliographic record and a very special “personal history”.

“Part IV: 50 years of sampling theory—a personal history” was to become seminal. This is where Pierre Gy tells his own story of the gestation, development and fruition of TOS, which took 25 years, followed by another 25 years of intense and diverse applications. It is a story that is closely intertwined with the twists and turns of his personal life that Pierre reveals as he lifts the veil of privacy a fraction for his otherwise closely guarded private and family life. In a sense, with the passing of time and with Pierre’s death in 2015, history would dictate that his “personal history” would become his scientific testament.<sup>4</sup>

WCSB1 saw a confluence of several threads of preceding individual activities and developments, in the form of a broad set of TOS-related topics being presented for the first time in a forum of colleagues and peers. This led to intense, satisfactory scientific debates, interspaced with personal presentations and inter-personal resolutions of earlier perceived differences and (what turned out to be) misunderstandings. Pierre, Dominique Francois-Bongarcon,



Very few words needed, if any—a thoroughly good time was had by all.

WCSB1 (2003). While the scientific aspects of this inaugural conference and its successive bi-annual followers are now well known and documented within the sampling community and need no space here,<sup>2</sup> the personal aspects of WCSB1 for Pierre and the sampling community are of interest to our community.

WCSB1 (2003) was, naturally, the first WCSB conference Pierre attended, and he was *en plein forme*. As were all other participants. Before the conference, professional samplers had worked as individuals for decades: the conference provided the first time they had an opportunity to meet at the same place, at the same time. Much to Pierre’s satisfaction, only a few could not, or decided not to, be present. All told, a total of 137 participants attended this inaugural world conference. The conference





and in particular Francis Pitard gave very personal testimonies from the pre-2003 history of TOS and of the then dispersed sampling community. These were eye-opening insights for all younger participants. WCSB1 was both reconciliatory and visionary, opening new doors for individual interaction and for our community to prosper from.

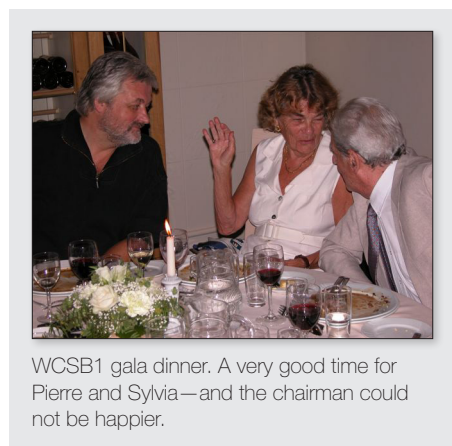
In the present context it is fitting to convey Pierre's personal reactions to WCSB1. There is perhaps no better documentation than the photograph (originally published in the WCS1 Proceedings) bearing the caption "Pierre Gy with wife Sylvia at the WCSB1 banquet, August 2003" (see page 7). It has been said a picture is worth a thousand words: indeed the smiles and the general mood say it all. His reaction to "all this attention" made the work involved in inaugurating a world conference series eminently worthwhile. Below follow a few more, mostly un-published photographs from WCSB1 that I hope succeed to convey Pierre's general pleasure and emotions.

### From conferences to Cannes and beyond

These two conferences, straddling the Millennium, helped to start inducting TOS into the data analytical community (indeed also

into the powder science and technology community, see reprinted paper by Pierre himself elsewhere in this issue). It must be admitted, however, that progress has not exactly been at dizzying speeds or had the significant impact anticipated—yet: Telling DATA ANALYSTS that their data are contaminated with a whole new class of significant uncertainties, the never-before-heard-of sampling errors, uncertainties that very likely *dominate* in magnitude, is understandably a slow process... some things require time and a willingness to take a fresh, objective view, but the start was made! It is obviously much more directly relevant to incorporate the essentials of TOS in powder science and technology.

After these events, activities for the sampling community quickly accelerated as a function of our biennial WCSB conferences—but Pierre was sadly never able to attend again. Despite this, however, contact was not lost. A small group of friends and colleagues (FFP, DFB, KHE, POM) undertook to visit Pierre in his hometown of Cannes every year if possible. This succeeded rather well. These visits became the new order of contact between Pierre and the by now rapidly growing and developing society of samplers, focused on conveying how well TOS was faring and sharing the many success stories that had quickly accumulated. It became customary at these occasions to hand over a copy of the proceedings from the preceding WCSB



WCSB1 gala dinner. A very good time for Pierre and Sylvia—and the chairman could not be happier.



WCSB1 snapshots, never-before-published. Bottom-right: the first ever official "committee" of the sampling community.





complete with signatures and greetings from a majority of the participants. These communal greetings became a vehicle for communication from the society at large to its respected originator; Pierre truly treasured this missives.

On the lighter side, a wonderful tradition developed at these meetings. At Pierre's insistence, after long walks through his beloved city and waterfront, a lunch at one of the fine seafood restaurant in Cannes was *mandatory* (always paid for by Pierre, *not negotiable*).

Nice as these culinary excursions in Cannes were, coffee was always taken afterwards on the balcony of Pierre and Sylvia's spacious apartment at Res. de Luynes, which was also the setting for his customary greeting at the beginning of any visit at which champagne and *foi gras* were *de rigeur*. There is much to say about French hospitality and traditions!

In between there also were ad hoc visits when the occasion presented itself, for example one by DFB and KHE in 2005 where Pierre insisted on giving a "guided tour" of his office (barely 10m<sup>2</sup>) in which a significant part of his scientific production took place. He a.o. proudly showed his classic IBM Electric (the famous red model with its signature exchangeable printing heads) on which two of his major books were produced. His publishers at the time demanded a flawless, print-ready copy of each of >300 pages *from the author*—no editorial or typesetting or editorial help were offered! We also noticed a personal computer (kind of) and when questioned, Pierre explained that he was "dabbling with" transferring his standard lecture presentations into PowerPoint: Pierre at this time was 80 years *young*. The audacity, drive and technical acumen of an octogenarian scientist impressed me beyond telling. Pierre became a role model in very many ways.



Walks around Cannes very often ended with the imperative oysters & Loire white wine lunch at "BRUN".

Just about five years into formally teaching TOS at university level, I certainly had not fully developed my curriculum yet, far

from, so when requested, my satisfaction reached new heights when Pierre gracefully handed over his entire lecture



Res. De Luynes, 14 Avenue Jean de Noailles, Cannes—an important place and a terrace to visit.







Pierre Gy's office in Cannes—a working place with an inspiring view.

catalogue “for inspiration, to whatever degree this might be possible”. I still treasure Pierre Gy's PowerPoint TOS course version as a historic gem from which I drew an enormous inspiration at the time.

These lectures have never been shown to anybody out of respect for a promise to Pierre, but here now a *sample* can be revealed:

[http://www.impublications.com/subs/tosf/v16/8-Proportional\\_Sampling.ppt](http://www.impublications.com/subs/tosf/v16/8-Proportional_Sampling.ppt)

It is of more than passing interest that the theme of this lecture is: “Proportional Sampling”, the TOS topic Pierre himself considered the least disseminated, which frustrated him. I have chosen to feature this topic here, especially in connection with the historical feature presented by Phillippe Wavrier: “An automatic linear proportional sampler based on the principles of the Theory of Sampling” (pages 26–28), which we hope will contribute to doing justice to Pierre's wishes.

As always, Pierre was primarily interested in getting the structure of the theory and the mathematical expressions conveyed—he was not much into incorporating many illustrations (he much favoured line drawings): “The equations tell it all”.

Well—here opinions actually diverge. I took a decidedly different point of departure from these lectures: “HOW can TOS' curriculum *also* be told in a different fashion, especially for the mathematically not-so-gifted?” This became the hallmark for all my work on the didactics of teaching TOS to a, hopefully, much broader circle of students, clients, companies and various audiences not familiar with TOS.

Our group or individual visits became more and important to us as there was always an acute realisation that sometime in the near(er) future Pierre's situation could change. But this simply increased our determination to continue providing context, information and friendship to Pierre.

Our last meeting with Pierre was at the hospice in Bordeaux where he spent his last years. Despite intense invitations from the WCSB7 committee to attend, or briefly just visit, the conference, he expressed reluctance that he did not wish to do so, with the stated concern: “I would be so embarrassed that I will perhaps not remember all the faces (and names) I *should* remember”. However, the end of the conference, there came a message from the hospice that Pierre would be prepared to see some of us the next day, in the afternoon. The meeting was set up through the tireless intervention

of Dominique Francois-Bongarcon. On this occasion it was a wonderful surprise that Pierre's daughter, Caroline, was able to join us. As it took place in the age of “smart technology” two video recordings (mobile phones) are able to document the meeting which can be downloaded from page 46.

The video recordings attest to Pierre's presence and happiness to meet with close friends and colleagues, many of which go back over much of his entire professional life. Not surprisingly Pierre also here insisted on champagne to accompany the chocolate treats we brought him (not entirely to the satisfaction of his carers, but of course this was a rare and special occasion). The degree to which he appreciated these gourmet delights is evident. The videos also highlight how much he still cherished perusing the signed copy of the WCSB7 Proceedings as well as seeing the 7<sup>th</sup> Pierre Gy Sampling Gold Medal, which had been awarded to Ralph Holmes at the conference gala dinner the evening before.

### An award of a different nature

At the risk of violating the rules of a certain well-known international institution, the following also relate to Pierre Gy's extraordinary scientific life and achievements. For a period of ten years, while in a position as





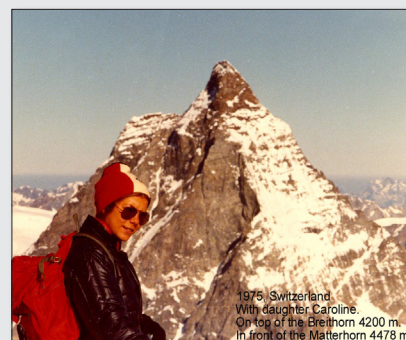
“professor of chemistry” in Scandinavia, I was regularly invited to suggest candidates for the Nobel Prize in Chemistry. One is not often in a situation in which a response to the pertinent committee feels relevant. And for understandable reasons the complete lists of candidates submitted in the office of the Nobel Committee are normally not divulged. However, in the context of our tribute to a scientific giant, it is entirely fitting to disclose that Pierre Gy was indeed suggested, several times, as a candidate for the Nobel Prize in Chemistry.

### Pierre Gy, the scientist and the family man

I was only granted to share a little over 15 years in this world together with Pierre, who died on 5 November 2015. The impact, however, feels like that from a complete professional career. I have never met a scientist of such brilliance (TOS is transcendental),



Pierre, Sylvia, grandson Stanislas and daughter Caroline.



Pierre's daughter, Caroline, in front of the Matterhorn in 1975.

character (modesty) and moral standing. The legacy of Pierre Gy, the scientist is first and foremost embodied in his monumental Theory of Sampling, the significance of which is commented in full in this issue, as well as

in many other places. For this reason I here focused on a slightly different angle of his life by conveying a view of a few, but seminal interactions between Pierre and the sampling community and to share a few more intimate moments with Pierre Gy, the man.

Pierre Gy is no longer with us. Scientifically, for me this is like I have lost a father. The sorrow is still almost unbearable, I miss him every day.

But we must (also) move forward. We shall cherish his inspirational life, now more than ever. There is a whole world out there that (still) needs to know more about TOS. What greater contribution to his legacy than doubling our efforts in this regard! And in a world characterised by senseless, indeed mindless, competition and ruthless personal ambitions, Pierre's attitude toward life was the direct opposite: collaboration and respect to the fore, endless willingness to help others... a truly monumental life!

I hope the collective efforts and the utmost respect from our entire community to honour Pierre Gy, *scientist extraordinaire*, will be able to impart but a small solace for his family.

RIP



Pierre on one of his walking tours in the Alps.





References and notes

1. There is one person in this world who strongly argues that I should have seen this light “at least 10 years earlier”. The reader need not go any further than another of the personal tributes in this special issue to latch on to this claim; which is entirely true of course.
2. K.H. Esbensen (Ed.), “Special Issue: Proceedings of the SSC6, August 1999, HIT/TF, Norway”, *J. Chemometr.* **14(5–6)**, 381–776 (2000).
3. K.H. Esbensen and P. Minkkinen (Eds), “Special Issue: 50 years of Pierre Gy’s Theory of Sampling. Proceedings: First World Conference on Sampling and Blending (WCSB1). Tutorials on Sampling: Theory and Practise”, *Chemometr. Intell. Lab. Syst.* **74(1)**, 1–236 (2004).
4. Reprints of the full proceedings issue (236 pages) are available at the Publisher’s on-line print-on-demand facility. Elsevier B.V. is owed much gratitude for their kind permission to bring six items from these proceedings into this special issue “two full-length re-reprints and four open access papers in downloadable electronic format”.
5. The Editor and the Publisher of the present issue are delighted by the very kind permission given by Elsevier B.V. to reprint this *sine qua non* historical item here.



<http://tosf.s3.amazonaws.com/Gy-1.mp4>



<http://tosf.s3.amazonaws.com/Gy-2.mp4>

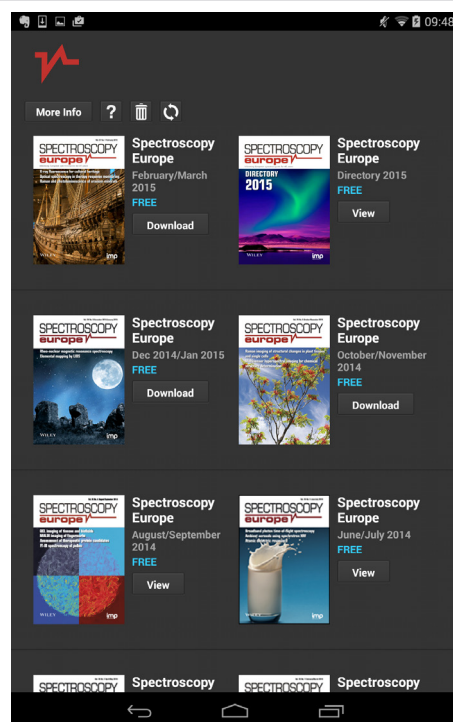
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Pierre and Sylvia Gy, Christmas 1996.



# WCSB8

## 8th World Conference on Sampling and Blending

9–11 May 2017, Perth, Western Australia



### The Conference

The 8th World Conference on Sampling and Blending (WCSB8) will be held in Perth, Australia, on 9–11 May 2017, and will be combined with the Australian sampling conference normally held every two years. The conference will be jointly organised by The Australasian Institute of Mining and Metallurgy (the AusIMM) and CSIRO, and will bring together all parties involved in sampling and blending in the mineral, pharmaceutical, food, feed, agricultural, biomass and recycling industries, including environmental monitoring. WCSB8 will provide unparalleled opportunities for updating knowledge, benchmarking sampling and QAQC practices, networking, meeting respected international sampling experts, sharing ideas and catching up on the latest developments. Sadly Pierre Gy passed away in Bordeaux, France, in November 2015, so WCSB8 will be dedicated to his memory and lifetime achievements in sampling mineral commodities.

Despite the wealth of knowledge available on correct sampling principles and practice, it is surprising how little attention and investment are dedicated to extracting representative samples for analysis. Quite often everyone appears satisfied as long as some material is collected and delivered to the laboratory for analysis. Yet, unless the samples are representative, the whole measurement process is flawed at the outset and no amount of reanalysis can fix the problem. Companies stand to lose millions of dollars in terms of poor investment decisions, wasted resources, poor product quality and reduced income.

Sampling needs to be given the critical attention it deserves ensuring that samples extracted are representative so that meaningful decisions can be made based on their analyses, which is particularly important in the mining industry where commodity prices at present are at historic low levels. WCSB8 will in particular continue on the path of covering other important industry sectors where sampling is equally important such as the cement, food, feed, agricultural and pharmaceutical industries.

### Trade Exhibition

A trade exhibition will be held at the conference venue. The exhibition will provide an excellent opportunity for companies to display their products and services to participants.

### Associated Workshops

A number of pre- and post-conference workshops will be organised on relevant topics, including chemometrics, sampling theory and practice.

**AusIMM**  
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## Call for Papers

### CRITICAL DATES



**15 September 2016 – Close of receipt of abstracts**

**14 November 2016 – Receipt of first draft of full papers**

Papers are sought on the theory and application of sampling and blending in the mineral, pharmaceutical, food, feed, agricultural, biomass and recycling industries, as well as environmental monitoring. Preference will be given to papers in the following areas:

- Tributes to Pierre Gy
- Theory of sampling and blending
- Geostatistics
- Sampling and blending mineral commodities
- Quality control and metallurgical accounting
- Sampling and quality control in the pharmaceutical and food industries
- Sampling of feed, agricultural and biomass products
- Sampling of wastes and recyclable materials
- Environmental sampling and monitoring
- New developments in sampling, sample preparation and blending equipment
- Staff training
- Development of national and international standards
- Future technologies
- Case studies

### Submission of Papers

We invite authors wishing to present a paper at WCSB8 to submit an abstract not exceeding 300 words in English to the Speakers' Portal, available via the conference website at [www.wcsb8.com](http://www.wcsb8.com).

For further information, please contact:  
Mia Wotherspoon, Coordinator, Publishing  
Telephone: +61 3 9658 6104 | Email: [mwotherspoon@ausimm.com.au](mailto:mwotherspoon@ausimm.com.au)

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Eliza Sanneman, Senior Coordinator, Events, The AusIMM  
Telephone: +61 3 9658 6105 | Facsimile: +61 3 9662 3662  
Email: [esanneman@ausimm.com.au](mailto:esanneman@ausimm.com.au)

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