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The Problematic Unity of Biometrics*

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SUMMARY. The word “biometry” may be less than 200 years old, but it can be argued that work we would now consider biometric dates back even to the pre-Christian era. The unity of biometry is attested through examples spanning two millennia, and a lesson from Francis Galton is recalled from a century ago.

KEY WORDS: Archibald Pitcairne; Asclepiades; Avicenna; Biometry; Francis Galton; Galen; John Lubbock; Karl Pearson; William Whewell.

1. Introduction

Disciplinary history is difficult in many ways, not least in the fundamental one of definition: What is the discipline? What *is* biometry? Our modern subject of biometry is amazingly diverse; so much so that the question could be raised as to whether or not it has sufficient unity to constitute a single discipline. My goal here is to take advantage of the occasion of the millennium to address this question by looking backward.

2. The Name “Biometry”

One superficial approach is to seek out the origin and diffusion of the name itself. Names are important—they convey identity and individuality to people and to scientific disciplines, but when we study histories over long periods of time they can be misleading. “Physics” today means something quite different from what the term meant 300 years ago, when it referred to medicines. What we now call physics was then called “natural philosophy,” a term that would today suggest something quite different—perhaps the opposite of “unnatural philosophy”? “Statistics” today is an altogether different subject from the “statistics” of 1800, despite the fact that there were, by that date, many books on “statistics” and the “theory of statistics,” books without any numbers or formulas, books that today might be classified as belonging to the field of political geography.

If we do look for the origin of the names “biometrics” and “biometry,” the results are mildly disappointing. Both terms only became widely used with the foundation of the journal *Biometrika* in 1901, when Francis Galton and Karl Pearson made it clear that, by biometry, they meant (and I quote from Galton’s preface to the first volume) “the application to bi-

ology of the modern methods of statistics.” To both Pearson and Galton, the most important applications were to evolution and heredity, although the scope was not restricted to these areas. But while the term biometry may have been an invention of Pearson’s, there are at least two known earlier independent inventions in English and several in other languages. Of all these documented uses, the first was in 1831, by the Master of Trinity College Cambridge, William Whewell, in correspondence with John Lubbock. Whewell wrote,

By the way there is a problem in Biometry (if you choose to call your calculations on lives by a Greek name) which may perhaps be included in something you have done, and if so I should be glad if you would point out the solution. . . . It is this: “It is said to be ascertained that to put off to a later period of life the average age of marriage, does not diminish the average number of children to a marriage. This being assumed, to find the effect on the increase of the population produced by a given retardation of the average age of marriage.” (Todhunter, 1876, 2, 134–135)

William Whewell (1794–1866) was both a philosopher and historian of science, and he seems to have, in addition, been the originator of the term “scientist.” Whewell used that word in 1834 as a gender-free description of what was previously called a “man of science,” a phrase that would have been conspicuously inappropriate to describe Mary Somerville, whose work Whewell was reviewing (Merton, 1997). But Whewell’s use of biometry in 1831 was casual, limited to calculations on lives—or, we might say, demography—and it was not published until 1876. Whewell’s correspondent, a gentleman sci-

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entist named John Lubbock (1803–1865), had coauthored a short tract on probability the year before, but otherwise is not well known in the history of the subject. He is an unlikely person to have spread Whewell's term "biometry" further (Clerke, 1921–1922). On the other hand, Lubbock's son, also named John Lubbock (1834–1913), could have conceivably been the source.

Lubbock Junior (if I may call the future Baron Avebury by that name) was a scientist with particular expertise in natural history; everyone today is doubtless familiar with one of his accomplishments—he invented the ant farm (Lubbock, 1882)! He was also a Member of Parliament (MP) and a friend and advisor on statistical matters to Charles Darwin. In 1857, while Darwin was writing his *Origin of Species*, he wrote to Lubbock, "My Dear Lubbock,—You have done me the greatest possible service in helping me to clarify my brains. If I am as muzzy on all subjects as I am on proportion and chance,—what a book I shall produce!" (Darwin, 1959, 1, 461–462). As an MP, Lubbock achieved notoriety by introducing and helping pass the Bank Holidays Act of 1871, which established a new British public holiday in August. There was wide-spread appreciation of this, which was reflected in a temporary currency of the term 'St. Lubbock's day' for the first Monday in August (Smithells, 1927). But while Lubbock Junior did survive to 1913, and he was a friend to Galton, there is no particular reason to suppose he was the source of the term's use by Pearson and Galton.

Indeed, the term "biometry" is a natural choice for anyone reaching for a way to combine measurement and biology in one name. It acquired a variety of unrelated meanings in French dictionaries in the 19th century. For example, in 1842, there was this curious definition: "Biométrie. s. f. (didact.) Art de calculer l'emploi de la vie, de manière à en tirer le parti le plus avantageux" (Barré, 1842, p. 129). What was intended, evidently, was the art of using a written hour-by-hour schedule in order to optimize an individual's day's work. An earlier use was to refer to indices of growth of animals and plants (Virey, 1833); another referred to a "science" involving the use of a "biomètre" invented by a doctor Collongues, an acoustic device that apparently generated vibrations of different pitches and purported to measure health numerically through the patient's ability to detect them (Larousse, 1867).

In English, "biometry" cropped up in the medical literature by 1875. In an article that year, a New York Doctor named Moreau Morris used the word to mean the study of the length of life and its correlates, and he explained it in an unsourced quotation (presumably from the contemporary medical literature): "This is a word whose precise derivation illustrates its intended meaning—from the Greek words *Bios*, life; *Metron*, a measure" (Morris, 1875). "Biometry" was also in use in the same sense in Italian statistics courses about that time (Wright, 1890) and no doubt elsewhere.

Pursuing the name "biometry" seems unrewarding. The history of the name neither captures the diversity of the field nor illuminates its development. I have chosen, then, to consider instead the early history of the subject as it is now conceived. That is, I will define biometry to encompass what the journal *Biometrics* publishes and has published since its humble beginnings in 1945 and to be what biometricians speak about at their professional meetings. And by looking at the

past, I want to demonstrate that the discipline (so defined) is much, much older than the name and that one may even find a sense of continuity—a sort of unity—in the field over thousands of years.

Now, attempting to cover with any degree of thoroughness a history over such a vast extent of time would do more than try the reader's patience, it would exceed my meager knowledge, and I shall make no such attempt. Rather I shall give a few short examples, tell a couple of stories, and attempt to wring a hopeful message for the new millennium from all of this. My arrangement will be loose and thematic, centering upon what I will call some of the classical problems of biometrics, problems that have occupied scientists for a very long time and continue to do so today.

3. Problem 1: Clinical Trials and the Design of Experiments

One of the most ancient of all biometric problems is that of the design of clinical trials. The *Bible's* Old Testament, in the first chapter of the Book of Daniel, tells of how the young Daniel and three compatriots were sent to the court of King Nebuchadnezzar to wait for an audience with the king. The king furnished them with a rich diet of meat and wine, and Daniel was faced with a dilemma—he and his men did not want to depart from their kosher diet, but how could they refuse to eat the king's meat without causing offense? Daniel proposed a clinical trial. Daniel asked the king's representative, Melzar,

- (12) Prove thy servants, I beseech thee, ten days; and let them give us pulse to eat, and water to drink.
- (13) Then let our countenances be looked upon before thee, and the countenance of the children that eat of the portion of the king's meat: and as thou seest, deal with thy servants.
- (14) So he consented to them in this matter, and proved them ten days.
- (15) And at the end of ten days their countenances appeared fairer and fatter in flesh than all the children which did eat the portion of the king's meat.
- (16) Thus Melzar took away the portion of their meat, and the wine that they should drink; and gave them pulse.

This account, which describes events from 606 B.C., is not adequate in all respects. What analysis led to the trial lasting only 10 days; i.e., what was the stopping rule? What would have happened if the results had been mixed rather than all in Daniel's favor; was a *t*-test employed? Exactly what measure of "fairness" was employed? The trial was clearly not randomized, but would it have qualified as a quasi-experimental design? Although Melzar is remembered today through the adoption of his name to mean "waiter" in modern Hebrew, there are sufficient unanswerable questions that we cannot enshrine Daniel with Fisher in the history of the design and analysis of experiments. Nonetheless, the trial Daniel performed does show an early awareness of the value of experimental evidence and the need for a control group. Not only did Daniel have the wit to think of this trial, Melzar also bought into it!

There is no continuous record of how these ideas were developed in the years following 606 B.C.; this was, after all, more than 2500 years before the foundation of the journal

Biometrics. But there is ample evidence in the history of science that the ideas did not vanish. For one example, consider the 11th century work of the famous Arabic doctor, scientist, and philosopher Avicenna (980–1037). His *Canon of Medicine* was the leading medical text for nearly eight centuries, and in the second volume of the *Canon*, Avicenna (whose name is also spelled Ibn Sina) listed seven rules for medical experimentation. Let me quote from a summary of Avicenna's seven rules (Crombie, 1952, 89–90).

One rule warned of the dangers of confounding: "The drug must be free from any extraneous quality; for example, we must not test the effect of water when it is heated, but wait until it has cooled down." And further, "The experimentation must be done with a simple and not a composite disease, for in the second case it would be impossible to infer from the cure what was the curing cause of the drug."

Another rule stressed the need for controls: "The drug must be tested with two contrary types of disease."

And yet another expressed the wisdom of observing the effects for many differing factor levels: "The quality of the drug must correspond to the strength of the disease. For example, there were some drugs whose 'heat' was less than the 'coldness' of certain diseases, so that they would have no effect on them. The experiment should therefore be done first with a weaker type of disease, then with diseases of gradually increasing strength."

He required careful measurement: "The time of action must be observed, so that essence and accident are not confused. For example, heated water might temporarily have a heating effect because of an acquired extraneous accident, but after a time it would return to its cold nature. The effect of the drug must be seen to occur constantly or in many cases, for if this did not happen it was an accidental effect."

And he was leery of extrapolation between species: "The experimentation must be done with the human body, for testing a drug on a lion or a horse might not prove anything about its effect on man." Imagine the problems of coping with a laboratory lion in a modern biomedical laboratory!

Avicenna's rules left a lot to be desired, but this was, after all, more than 900 years before Ronald Fisher.

Many other isolated examples of this sort could be found, but in one respect, they are all historically unsatisfactory. Indeed, the problem is that they are isolated. We do not get a chance to see how they were viewed at the time; we are not privileged to listen in on debates that might reveal how limited their understanding may have been. I shall now turn to a pair of examples—two stories—where we can listen in on the discussion, when differences of opinion existed and were aired publicly—in fact shouted publicly.

4. Problem 2: Likelihood Theory and the Evaluation of Evidence

I will now go back in time again, to the second century B.C., where I will introduce one of the truly obscure characters of the history of biometry. At this point, a reader would be excused for asking whether there are *any* characters in this history who are not obscure. I suggest that the man I am going to introduce is the most obscure famous man of our history. His name is Asclepiades the Bithynian. I would not expect any exclamations of recognition. But how obscure is he? Well, his major work is referred to as "The Lost Theory," and the only

known portrait of him is identified only by default—he was more famous than any other obscure man named Asclepiades (*Penny Cyclopaedia*, 1834; Green, 1955; Rawson, 1985, 1991; Vallance, 1990).

Asclepiades was a Greek doctor; he was born in 129 B.C. in Bithynia, now part of northwestern Turkey, and probably died in 91 B.C. in Rome. He began his career as a professor of rhetoric, but after he moved to Rome, he made the easy switch to medicine and became a renowned (and rich) doctor. His treatments departed from the teachings of Hippocrates, and they were extremely popular. He prescribed exercise, a forgiving diet, music, and plenty of wine. He may have invented the shower bath. He told people what they wanted to hear and eloquently denounced the more severe treatments of established medicine, greatly raising the reputation of Greek medicine in Roman eyes. In a word, he was a quack. But he was a quack who, given the state of medicine in his time and the common-sense nature of his prescriptions, probably did a lot of good.

Asclepiades did espouse a theory, although the content of that theory is vague due to a shortage of source material. It seems to have involved postulating the body as containing a single type of corpuscular material that conspired to reform itself to block or unblock pores. You might characterize it as assuming an unobserved "latent variable" as responsible for changes in health. My colleague Xiao-Li Meng could estimate it, but that would not guarantee its existence! Seemingly the only proof offered for this unobservable material was that it could not be seen. Asclepiades rejected any appeal to empirical medicine that was not guided by theory (his theory); he made no appeal to measurement or use of statistics.

If Asclepiades was little more than an early Greek dietary guru, why then do I dwell on him? The reason is simple—his doctrines were so attractive that, more than two centuries after his death, the great Galen of Pergamum (129–200 A.D.) felt it necessary to denounce him. Without Asclepiades, Galen might never have written what might be called the first book on biometry.

Unlike Asclepiades, Galen was a follower of Hippocrates and became one of the most influential medical philosophers of all time, principally for his works on anatomy and physiology. But one of his earliest books was written when Galen was barely 20 and is known only through a ninth century Arabic translation. The title usually given for this work is "On Medical Experience," where "experience" is to be understood to mean observation, or statistical counts, or data (Galen, c. 150 A.D.). With only slight extension, it might have been titled "On Biometry." Galen's aim in the work was to refute the doctrines of Asclepiades, which still attracted public attention. He denounced Asclepiades as boastful, arrogant, and for using distorted reasoning, but the book is not simply a polemic against a rival school. Instead, Galen presents his argument as a dialogue or debate between a dogmatist and an empiricist. The dogmatist is permitted to make as cogently as possible the better of the points that had been attributed to Asclepiades; the empiricist represented the school Galen held allegiance to.

Neither party to this debate was merely a straw man; the arguments on both sides were nuanced and substantial. The dogmatist began by arguing for the primacy of theory, claiming that data alone could not convince. The dogmatist argued

that, when it was claimed that data had convinced, it was only because the data had been found in support of an otherwise already compelling theory. To this end, the dogmatist raised the question of how the different factors were chosen for observation—how the explanatory variables were selected for a regression analysis or a clinical trial, as we might say. Why, he asked, would we consider a patient's consumption of alcohol, or sexual excesses, or diet as possible explanations for a disease, while we would never even inquire about other factors, such as whether the sick man had taken a bath, or had worn a red garment, or had read a particular book? The answer was simple: in the one case, there was abundant theory to point the way and in the other there was no theory. And hence the diagnosis of medical cause was never a purely empirical matter; it was, at bottom, a theoretical exercise.

But what of the empiricist's claim that it was still the empirical weight of evidence that convinced, the accumulation of cases pointing toward a particular cause for a disease or an efficacious cure? Nonsense, said the dogmatist. Are you to be convinced by a single case? Of course not—you will require that a relationship be seen "very many" times before being convinced. But what is "very many"? Is it 10? No, you say. Then 11? No. And I will ask again and again and you will keep saying it has not reached the required limit. Because if you do otherwise, if you say that 49 is not enough but 50 is sufficient, then "it is obvious that only by being seen a single time has it become acceptable and true" (Galen, 150, p. 97). The argument is an appealing one, and I can imagine it is still being made today by students in our classrooms or earnest advocates in congressional hearings: If $n - 1$ cases fail to produce statistical significance but the statistic crosses the line with the n th, then is not a single observation responsible for the significant finding?

What could Galen's empiricist possibly say to refute this argument? Remember, this was nearly 1800 years before the Neyman–Pearson Lemma! The empiricist's reply was long but repetitive, and it can be briefly summarized. First, as to the choice of explanatory factors or variables: If a solid theory were available, then of course this should be used. But theory is not needed, theory is not a necessary condition for the choice. Would you insist that a ship's captain not leave shore without a full understanding of the nature of the sea and winds and their elements and exact consequences at every moment (Galen, c. 150, p. 98)? In fact, in medicine, we do note all antecedent states and conditions, even those you call absurd, such as the color of the garment or the book recently read. But we do not trouble with them, in fact (Galen, c. 150, p. 151), since the memory only holds those things that are of frequent occurrence in conjunction with a disease. That is, theory could be used, but the choice of explanatory variables can be made empirically and usually is.

But what of the argument that it is impossible to be convinced empirically because a single case does not convince and any attempt to specify a number of cases that would convince amounts to saying that the addition of a single case has convinced? How can a "whole" differ in such a fundamental way from the sum of its unconvincing parts (Galen, c. 150, p. 120)? Galen argued that this amounted to what is referred to as "the fallacy of the heap," the origin and best example of the class of logical arguments called sorites. This philosophical argument is generally attributed to a fourth century

B.C. Greek, Euboulides of Miletus; however, Galen turned it to new uses. We all agree, Galen said, that there is such a thing as a heap of grain (Galen, c. 150, p. 114 ff). Yet one grain of wheat does not make a heap, nor two, nor three. Do 100, or 105? That the nature of the pile does not change from nonheap to heap with one grain does not deny the existence of a heap. Similarly, a bald man would not become luxuriantly haired with the addition of a single strand, nor does a man become bald by the removal of a single strand, yet there are bald and nonbald men who have achieved their status one hair at a time.

Galen did not provide a criterion for what would constitute a convincing heap of evidence; he was content to demonstrate existence and would leave it to the reader to recognize that heap when he saw it. It would never have occurred to him that the specification of a theory for defining and deriving criteria for heaps of evidence would be a primary occupation nearly two millennia later. At least one modern work has explicitly addressed the classical paradox. Borel (1914, pp. 122–132) gave a probabilistic resolution, a probability-based measure that would designate a pile's degree of heapness.

5. Problem 3: Probability Modeling in Biometry

When did probability and the use of likelihood become a staple of biometric thinking? In many ways, that question is better answered within a more general discussion of the history of statistics, but I will tell briefly of one early episode, in fact what may be the earliest application in science that cited the mathematical theory of probability.

The story takes place in Edinburgh in the 1690s, and it involved two Scottish doctors who were at odds in every manner conceivable—in terms of medical philosophy, in politics, and in religion. There is a full account in my recently published book, *Statistics on the Table* (Stigler, 1999, Chapter 11), and I shall only give a short sketch here. The intellectual side of the dispute was launched when Dr Archibald Pitcairne, inspired by Newton's *Principia* and excited about the prospects of solving all manner of scientific problems mathematically, proposed the use of probability in medicine.

Two of Pitcairne's "results" involved the problem of secretion—accounting for how blood distributes different substances to different organs and learning how to exploit this to cure diseases. Descartes and his followers had proposed a mechanistic scheme, suggesting that different substances were in fact differently shaped particles in the blood, and they were delivered to organs through sieves with differently shaped pores. For example, a cubical particle would fit through a square pore and a cone through a triangular pore. Pitcairne cited Christian Huygens's 1657 tract on probability and claimed that the Cartesian model would not work—that the particles were so unlikely to approach the pore at the necessary angle (all angles being considered equally likely) that practically none would pass through and the device would consequently fail.

Pitcairne also appealed to probability in prescribing treatments. He adopted the Galen–Hippocrates belief that diseases were caused by bad "humors" (substances in the body) and that the best treatment would be the one that would expel them best. And expelling meant excretion. He cited evidence that, under ordinary conditions, the proportions of amounts a

body excretes in a given time would be as follows: "Excretion by Stool be as 4, That by urine is as 16, and That thro' the Pores of the Skin as 40, or more." His advice? He effectively would assume the chance of excreting the most evil particles would be in proportion to volume excreted, and by a sort of likelihood ratio argument he would rely upon perspiration: Sweat it out!

In 1695, Pitcairne's nemesis, Dr Edward Eizat, published a brilliant satirical attack entitled *Apollo Mathematicus*. There was much of interest in this attack, but perhaps the most salient point was to call into question Pitcairne's assumption that all of the angles by which the particles could approach the pore were equally likely. If these small particles could have different shapes, why not different propensities in direction as well? He even suggested a refinement of the "different shape" hypothesis, one that would provide for a large number of different-shaped particles of about the same size but mutually exclusive in their ability to enter their matched pores. As Eizat asked, might not the particles be shaped like screws of different threading? This suggestion might be thought a vague anticipation of modern biochemistry.

In any event, Pitcairne's treatments were not particularly effective, and probability had little impact upon biometric thinking for the next 200 years.

6. Conclusion

Modern biometry was founded in the previous century with the journals *Biometrika* and *Biometrics*. Our modern science bears little resemblance to that of previous millennia—at least at a superficial level. Modern biometricians wield a technical arsenal that would have even frightened Karl Pearson, who himself was acutely aware of the effect his own arsenal had on the biologists of his day. Yet at a fundamental level, there is a constancy, a continuity, a unity to all of this, in the problems faced. Problems of how to compare, how to evaluate the weight of evidence, how to weigh the individual characteristics in the consideration of aggregated evidence for groups of individuals, how to justify the assumptions (such as the definition of equally likely cases) required for a probabilistic analysis—these problems have been recognized for 2000 years, and yet they remain with us. Only the formulations and our answers have changed. Our answers are not just more technical, they are better in every way but possibly one: the technical level we have achieved can sometimes obscure the deeper questions that were the sole preoccupation of the ancients.

I would offer two lessons from a study of this history. One is from the historical perspective: If these problems have endured but the solutions have changed, we clearly should not become complacent. Surely the solutions will change again. The second comes from Francis Galton, a founder of modern biometry, and it is closely related to the first. In 1901, Galton was 79 years old and he wrote an engaging and perceptive preface to the first issue of *Biometrika*. I digress briefly to report a prescient suggestion he made that has yet to be adopted widely. He wrote,

I hope moreover that some means may be found, through its [*Biometrika's*] efforts, of forming a manuscript library of original data. Experience has shown the advantage of occasionally rediscussing statistical conclusions, by starting from the same documents as their

author. I have begun to think that no one ought to publish biometric results, without lodging a well arranged and well bound manuscript copy of all his data, in some place where it should be accessible, under reasonable restrictions, to those who desire to verify his work. (Galton, 1901)

But it is another part of Galton's preface that I would present as conveying a useful lesson for the millennium. Let me repeat here a story he told.

I have rarely related it in conversation, fearing to give pain to some one, and I have never done so in print; neither can I find that any version of it has been published by others. But now that nearly a century has slipped past since the event, there can be no harm in digging up and bringing to light a buried but amusing historical fact.

The story was told me long, long ago, in the 'forties, by Mr George Bellas Greenough, F.R.S. I was then an eager youth fresh from college, and he an elderly man; it was as follows. In 1806–1807, when Geology was in its infancy and travelers were scarce owing to European wars, Mr Greenough and a few young friends compiled a list of questions with the view of ascertaining how far the facts of Nature might agree with the competing geological theories current in those days. Sir Joseph Banks was the President of the Royal Society at that time, an office which he exercised despotically for 43 years (1777–1820), becoming almost an autocrat over English scientific men. So it was to him that Mr Greenough and his young friends naturally went. They brought their questions and begged that copies of them might be circulated under official sanction among suitable persons, including foreign correspondents of the Royal Society. Sir Joseph was sometimes gracious in mood, frequently the reverse, and on this occasion he might be described as bearish. Not content with an emphatic "no," he dismissed them with words to the effect (in almost those very words, if my memory does not deceive me) that a few fools could ask more questions in a half an hour than wise men might answer in years. The deputation departed, ready to burst with suppressed fury, and the moment they were quit of the house, agreed to circulate the questions on their own responsibility, which considering the persons and circumstances was an act of rare audacity. Out of this impromptu coalition, aided by a multitude of elsewhere recorded circumstances, the Geological Society was evolved, with Mr Greenough as its first President. (The official account of its origin is judiciously reticent, but not inconsistent with this little piece of history. It will be found in the preface to the first volume of its *Transactions*, published in 1811.) It is not in the least my intention to insinuate that Biometry might be served by any modern authority in so rough a fashion, but I offer the anecdote as forcible evidence that a new science cannot depend on a welcome from the followers of older ones, and to confirm the former conclusion that it is advisable to establish a special Journal for Biometry. (Galton, 1901)

Galton's journal *Biometrika* and the subsequent *Biometrics* were, of course, great successes, but the lesson of his story

remains. Biometry in the coming century will surely be a different science than before; the appearance of new names such as "bioinformatics" for extensions of old ideas is an early sign of this. The record of the past suggests that, at a deep level, the problems will remain the same, and our needs will too. Like Daniel, we will still need well-conceived and executed experiments, whether the design is by the principles of Avicenna or the refinements of a later millennium. Like Pitcairne, we will need bold assumptions; like Eizat, we will need criticism of bold assumptions. We will still have dogmatists and empiricists, e.g., theoretical statisticians and those who give primacy to applications. The best biometricians will be those who can borrow from both schools, but we will continue to learn from the debate between them. Even quacks like Asclepiades may play a useful catalytic role. But we should always keep in mind the admonition from the 79-year-old Galton: not only should we be flexible, actively embracing and encouraging the questions of "young fools." We, in fact, depend upon them for the discipline's advancement—for the new solutions of a new millennium.

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RÉSUMÉ

Le mot "biométrie" doit avoir moins de 200 ans, mais on peut remarquer que le travail de biométrie que nous considérons actuellement remonte à l'ère du pré-Christianisme. L'unité de la biométrie est certifiée à travers des exemples embrassant deux millénaires, et une leçon datant d'un siècle de Francis Galton est rappelée.

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