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The Pedigree of the International Biometric Society*

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Summary. On the 60th anniversary of the International Biometric Society, a look back is taken to the view of biometry held by the first president, R. A. Fisher, as reflected in notes taken in a lecture course he gave in 1935–1936.

Biometry—the active pursuit of biological knowledge by quantitative methods.

—R. A. Fisher, 1948

The history of biometry may be traced to ancient times (Stigler, 2000), but the International Biometric Society (IBS) dates its founding only to September 1947, at the first International Biometrics Conference at Wood's Hole, Massachusetts. The Conference Organizing Committee, whose members included Chester Bliss, William G. Cochran, John von Neumann, John W. Tukey, and E. B. Wilson, met and recruited Ronald A. Fisher to join some of them in setting up a continuing organization. Together, they recommended that a membership society be formed; the recommendation was accepted by the Conference and Fisher was selected as its first President.

Much has changed since 1947: in many respects the IBS has been a resounding success, as have its conferences, and this gathering and the scientific program at this anniversary meeting are ample testimony to its intellectual and institutional prosperity. But as a byproduct of that success and the broadening of focus that came with it, there is also some unease in the biometric community, as in all of statistics. What problems and what intellectual framework for addressing them constitute the core of the Society? What limits are there on the scope of the Society? In short, what constitutes the *identity* of the Society? There are small signs of this unease even on the IBS web page, which signals that the word "Biometrics" is in danger of being hijacked by the security community to denote various methods of personal identification.

One reason for celebrating any anniversary is exactly to address such questions, in order to reconfirm or even to reestablish identity. It is intrinsic to such a celebration that a look is taken back in time, to one's historical roots. I propose to attempt this in true biometric style, by seeking after the pedigree of the Society. Most of the pedigrees considered these days by members of the Society involve the inheritance of DNA, but it may be worthwhile to start with a look at two examples of attempts made earlier, examples involving the use of more crudely determined pedigrees to portray the structure of intellectual relationships, even in nonstandard situations.

Pedigrees are surely as old as the practice of inheritance of status and property, and the use of logical tree structures goes back at least to the Greeks, as a recent investigation of Ian Hacking's (Hacking, 2007) demonstrates, although, as he shows, the story is a chequered one, with no drawn logic trees known to be preserved in Western Europe before the 9th century. Even the pedigrees of intellectual disciplines are as old as the printed book; one striking example is from a book published in 1508 by a Carthusian monk, Gregor Reisch; a book entitled *Margarita Philosophica* (the Pearl of Philosophy; Reisch, 1508) (Figure 1).

This picture might serve as a model for the pedigree of the IBS, but for two difficulties. The first problem is that modern computer graphics have not yet caught up with what was evidently available to monks 500 years ago; the second is that Reisch sets too high a standard, in concentrating on abstract disciplines rather than on individuals within them. The diagram shows the tree of the arts and sciences sprouting from the womb of Wisdom, and on the various branches are the seven liberal arts of rhetoric, grammar, and logic, and arithmetic, geometry, astronomy, and music, and the different species of philosophy (rational, natural, moral). Biometry was yet to come, but we might associate it with the physiologists lurking in the upper right-hand corner.

Let me move to an example from a more recent era, nearly a decade after Mendel was rediscovered in 1900 and there was a surge in the appearance of trees in the biological literature. In 1909, Karl Pearson began publishing a large number of standard pedigrees, as part of his work at the Galton Laboratory for National Eugenics in assembling what he called a *Treasury of Human Inheritance* (Pearson, 1909). Pearson's examples covered an amazingly wide territory. Several of these pedigree trees tracked simple biological abnormalities, such as the trait of being born with six fingers on a hand ("polydactylism"), or a condition of shortened fingers ("brachydactylism"), or a deformity called split-foot. But Pearson was bolder than most modern geneticists, and he went on to study diseases that still defy genomic mapping, such as Pulmonary

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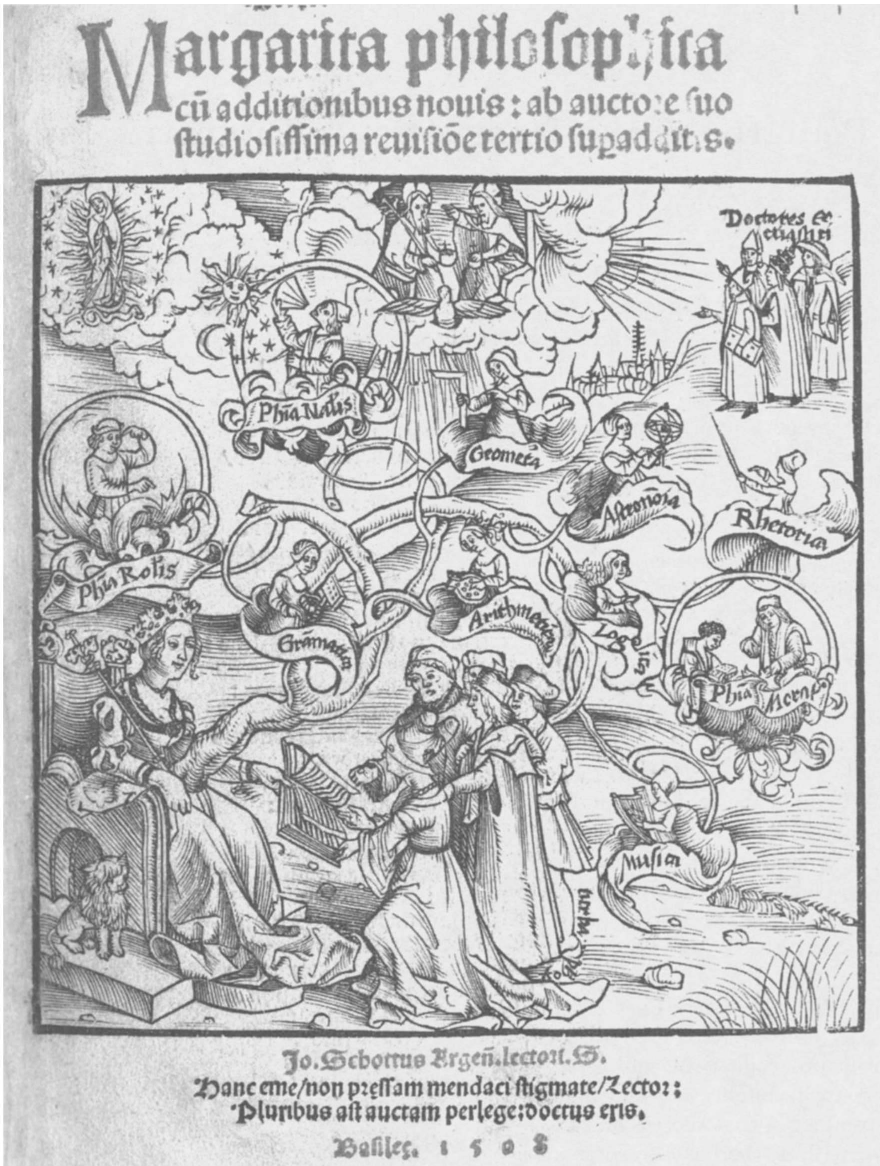

Figure 1. The title page from Gregor Reisch's *Margarita Philosophica* (The Pearl of Philosophy). The book was first published in Strassburg in 1503, but the title page shown made its initial appearance in this edition published in Basel in 1508 (Ferguson, 1930). (Reproduced with permission of the University of Chicago Library, Special Collections Research Center.)

Tuberculosis, and more complex traits such as "insanity and allied characters," where "allied characters" turns out to include alcoholism, epilepsy, tuberculosis, rheumatism, and cancer.

I looked through Pearson's vast compilations for some study that might be related to a propensity for biometry. The closest I could find were two studies, the first depicting commercial ability and liberal thought, and the second commercial and legal ability; Figure 2 shows a portion of the former. You will note that liberal thought is reflected in three flavors: "liberal in religious thought," "liberal in political thought," and a third described as "convivial, fond of good living, self-indulgent." Do any of these correspond closely to the modern IBS?

How, then, might we construct a useful pedigree for the IBS, starting with individuals? It is trivially easy to begin construction of one version of a tree, starting with Francis Galton at the top, with a line of descent to Karl Pearson. But then the task becomes more difficult: Below Pearson somewhere there will come Ronald Fisher, with a direct link then from Fisher to the IBS. But the link from Pearson to Fisher is a vexed one, and therefore Fisher's vision of what constituted Biometrics in 1947—that is, the root of the Society's identity—remains unclear. I will not attempt a full pedigree today, although I will later reveal a genetic marker that points to an important source of its identity. Instead I will tell a story about Fisher, a story that does shed light on Fisher's idea of Biometry, and so should speak to the basic goal we have in

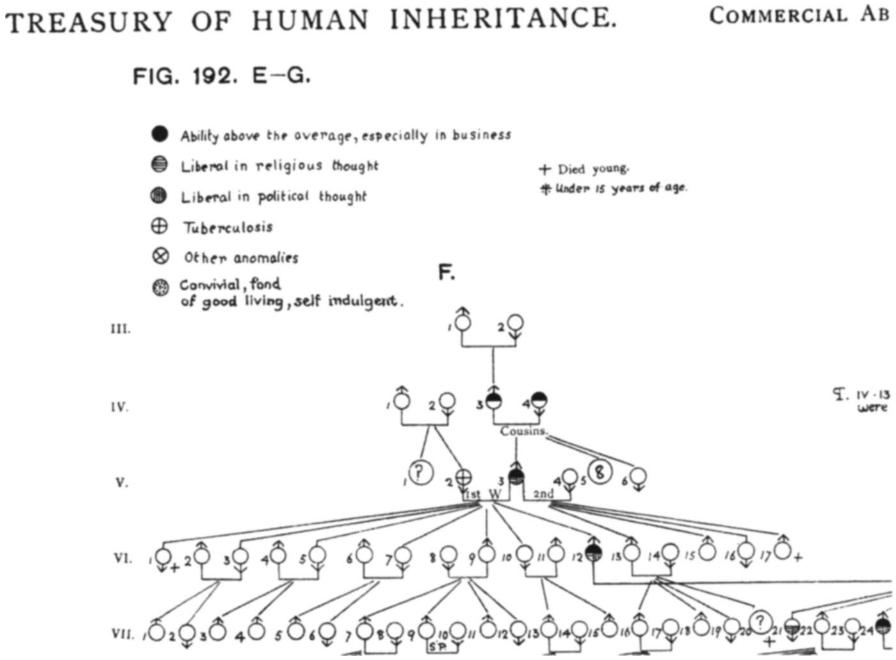

Figure 2. A portion of Plate XXIV, Figure 192, E-G ("Commercial Ability and Liberal Thought"), from Part III of *The Treasury of Human Inheritance*, first published in 1909 as Memoir IX of the Eugenics Laboratory Memoirs by the Francis Galton Laboratory for National Eugenics, under Karl Pearson's general supervision.

looking backwards. It is a story that as far as I am aware has not previously been fully told.

The story begins in 1933. In that year Karl Pearson retired as the Galton Professor of Eugenics at University College London, and the university offered the post to the one individual clearly best able to advance Pearson's agenda, Ronald Fisher. The University Provost was aware of the bad personal chemistry between Fisher and Karl Pearson, and the difficult situation this created: Karl would remain at least occasionally on the scene as Editor of *Biometrika*, Fisher would become Editor of another journal Pearson had founded, the *Annals of Eugenics*, and Karl's son Egon would also remain, teaching statistical theory while holding the post of Reader. And so the university tried to deal with the situation by splitting the Department of Applied Statistics that Karl Pearson had established into two departments; a Department of Eugenics with Fisher as Head, and a Department of Statistics with Egon Pearson as Head, occupying two adjacent floors of the same building. It brings to mind the partition of Ireland and the drawing of boundaries in the Middle East as methods of avoiding belligerency!

In May 1933, Fisher wrote to Egon Pearson in a warm spirit of cooperation, and over the next few weeks they exchanged several letters. Fisher noted that he expected some foreign students would come to him for instruction on his new statistical methods, and he wanted to ensure that this would not cause Egon a problem; Egon stated that he, unlike his father, found many of Fisher's methods congenial and expected to lecture on Fisher's methods himself. But Egon worried that if Fisher gave lectures on statistics, the roles of the departments would be muddled, and he asked Fisher to stick to instruction on biological problems for the first couple of years. On his part, though, Fisher was unwilling to foreswear lecturing on statistics when students asked, especially in connection with the biological problems that would be the principal focus of his department. Egon engaged the University Provost in the discussion, and the outcome of this negotiation was that Fisher would not *publicly announce* a series of lectures on purely statistical subjects, but Fisher could speak in his lectures upon whatever he wished. An uneasy peace was thus established, with professions of goodwill on both sides. As Fisher put it in a June 19, 1933, letter to Egon, "The division of the Department will be much laughed at, but neither of us is to blame for it."

There is an event subsequent to this division that bears upon our subject today. For the autumn term of 1935, Fisher announced a series of lectures he would give, to be held over 14 weeks at 2:30 P.M. on Thursdays, starting in autumn 1935 and apparently running into early 1936. For some time before his retirement, Karl Pearson had given series of lectures on the history of statistics, lectures that Egon would eventually collect and publish in 1979, as a book. Fisher's lectures were announced as on the history of *biometry*, and it is, I suppose, a matter of point of view whether the choice of subject was a tribute to Karl Pearson, or more like sticking a thumb in his eye. In any event the subject was nominally within the biological limits Fisher had agreed on, and definitely relevant to today's celebration. If we had a good recording of these lectures, we would have a good take on what the Founding President of the IBS took as the purview of biometry.

The lectures attracted an international audience, as Fisher had predicted. Chester Bliss and William G. Cochran, both later Presidents of the IBS, were among those in attendance. Others from the United States included Churchill Eisenhart, Paul Rider (from Washington University), and Helen M. Walker (from Teachers' College New York). What we know of the content of the lectures derives largely from an inquiry Churchill Eisenhart made in 1965. Back in 1935 Churchill had been a student spending the year in London, and in going back over his own notes from Fisher's lectures, he was puzzled at how sketchy and uninformative the notes were. He wrote to others he knew had been there, and only Cochran could provide reasonably complete notes. Cochran also gave an explanation for the lack of better records. The problem was that Fisher was a poor lecturer!

Cochran's own notes are only of medium quality, but they do give at least a record of what was covered. The lectures were far from a comprehensive survey; rather they could be described as detailed looks at five selected topics, ranging from estimating survival in the paleontologic record, to the more recent development of mathematical statistics.

Fisher's first three lectures were framed around data reported in Charles Lyell's 1830–1833 book, *Principles of Geology*. Lyell had in that book reported data on the fossil remains of mollusks from different geologic epochs within what he called the Tertiary period (the geologic period that followed the Cretaceous about 65 million years ago). Fisher introduced the data and raised the question of whether or not, based upon these data, one could infer changes in the survival rate of species over time. Presumably this was a question that would bear on changes in the fitness of species. Fisher reviewed the mathematics of life tables from De Witt through De Moivre, and on to Gompertz, and to Makeham, and he then returned to Lyell's data to conclude that the death rate of species of mollusks decreased with the passage of geologic time. Presumably this was evidence of increasing fitness.

His second topic was abruptly different. With the fourth lecture he moved to consider Gregor Mendel. After the briefest sketch of Mendel's life and his work in the 1860s, and the rediscovery of this work in 1900, Fisher dived deeply into a study of Mendel's experimental method and the data he had reported. Fisher was apparently working mostly from the volume Bateson published in 1909, presenting and discussing the translation of Mendel's original papers. Fisher's three lectures on this topic are the best-organized material in the entire series of lectures, and match quite closely the development he presented in his extraordinary paper (Fisher, 1936), "Has Mendel's work been rediscovered?" That paper is best known today for its discussion of the fact that Mendel's data fit his theory better than can be accounted for by naïve sampling models. Not just one but several dozen of the chi squares Fisher computed are too small, or so it would appear; to this day researchers are still trying to find a better explanation than Fisher's faute de mieux suggestion that "it remains a possibility among others that Mendel may have been deceived by some assistant who knew too well what was expected." The printed paper ends with a moral to the effect that different generations have misread Mendel in different ways, and includes a minor veiled reference to Karl Pearson in that regard.

With the seventh and eighth lectures Fisher shifted back in time, to look at some work of Adolphe Quetelet, from Quetelet's 1835 book "*Sur l'homme*." He discussed Quetelet's fitting of binomial distributions to grouped data on stature, and Quetelet's discussion of Laplace's rule of succession (Fisher thought Quetelet was insufficiently critical). Fisher then moved on to a detailed consideration of data Quetelet presented on growth rates and on the dates of the flowering of lilac at different locations in Europe. Cochran's notes record Fisher's judgment of Quetelet's work here, saying that "As a statistical treatment this is pottering and disappointing.... [Quetelet has neither] the data nor the enthusiasm for technique to work out a method."

With the ninth lecture, after mentioning that life tables remained a promising line for further development of statistical technique, all attempts to couch the series as concerned with biological problems were dropped. For two lectures he reviewed Gauss's work on least squares, emphasizing its relationship to inverse probability, and Fisher presented earlier work of his own, on tests of significance in harmonic analysis.

After 11 sessions, some of them punctuated by minor jibes at Karl Pearson's work, the course ended with four lectures, all revolving around work directly related to Karl Pearson. To judge from Cochran's notes, the tone throughout was respectful, but as one can imagine, at *that* time, in *that* building, even a slight hint that Pearson's work could be improved upon would have caused talk. In what I expect Fisher considered an unbiased way, he went through the development and rationale for the method of moments, not failing to note difficulties. He went to considerable lengths in showing how parabolic curves could be fit in practice using least squares and the reduction in the sum of squares for each added term found.

The final lecture in the series, the 14th, was devoted to chi square. Here too the tone was respectful, reviewing the development in Pearson's 1900 paper, after praising it as the first example of a *comprehensive* test of deviations between observations and hypothesis. Fisher did state that Pearson's original derivation of the chi-square statistic could be greatly simplified by the device (introduced in Fisher, 1922) of framing the analysis in terms of independent Poisson random variables, and he did carefully explain how estimating a parameter would reduce the degrees of freedom by one. Cochran's notes recorded no pointed references to Pearson's original error on degrees of freedom, but it is hard to believe Fisher let the occasion pass without some remark.

No doubt word of this lecture got back to the Pearsons. The precise date of this final lecture is not noted, but it was no later than early 1936. It is perhaps worth recalling that Karl Pearson died on April 27, 1936, and in the last months of his life he was working on a long paper, one that appeared posthumously, a paper that gave a spirited and unrepentant blast at Fisher's criticism of his use of chi square (Stigler, 2005).

What do these lecture notes tell us of Fisher's view of biometry? What do they tell us about our pedigree? What lessons for the future can we derive?

I would present one small message and two large ones. I mentioned earlier that I would be identifying a genetic marker to help chart the pedigree of the IBS. This marker can be thought of as conveying a small message, and it is signaled by one of the basic nucleotides that constitute DNA. I refer to Cytosine, commonly denoted "C." Whenever you see the name we gather under here today, think of Fisher, whose selection as first President by the Society's organizers was clearly intended to make a statement, a declaration of values and an affiliation with a particular school of statistical thought, and recall that it is no accident that the name chosen was *BiometriCs* as opposed to Karl Pearson's spelling of his journal, *BiometriKa*. That "C" is the genetic marker.

The IBS is in a real sense Fisher's Society, and the intention was to be different from *Biometrika*, and different from the direction of the mathematical developments of Karl and Egon Pearson and Jerzy Neyman. From the constitution of the first organizing committee to the choice of the name, there was a declaration of independence and a clear adoption of Fisherian statistics as the paramount tool for biological science. And this leads me to the two large messages.

The most important message is perhaps the one Fisher had already given to University College London in 1933 when he accepted the Galton Chair: There is no firm demarcation between biological problems and statistical theory. We should pursue statistical theory when it arises in an interesting biological problem, and explore biological problems wherever we can shed light upon them with interesting statistical theory. The second basic message is the converse of the first—that interesting biological discoveries can be missed without an understanding of statistics—not simply of textbook methods, but also of the theory that tells us when they are useful and when they can mislead.

Together these two messages assert the essential role of biometry in biological science. It is the role Fisher saw in the 1930s and 1940s, and it is the most promising role today and in the future. Fisher underscored this in his first address after assuming the Society presidency. There Fisher defined Biometry as "The active pursuit of biological knowledge by quantitative methods" (Fisher, 1948).

These messages alone could stand today as declaring the identity of the IBS, and should reassure us and provide us with a core of disciplinary values that will enable the Society to survive and thrive, even as others take up some of our methods and even the name without the requisite core understanding. The messages also carry with them an implicit uncertainty about the detail of that future. The most exciting science of tomorrow is necessarily unpredictable—otherwise we would be doing it today. New science will call for new methodology, and novel uses of old methodology. But that uncertainty is precisely what gives the future of biometry an air of excitement, precisely what will fill the journal *Biometrics* and the sessions of the International Biometric Conferences over the next 60 years.

There is possibly one final message from this reflection on the past. Karl Pearson's contributions to biometry were enormous, but he had difficulty embracing the revolutionary changes he helped inspire in Fisher's work. Fisher's brilliance sowed the seeds that led to the formation of the IBS and helped set the intellectual agenda for the past 60 years, but he himself had some rough edges that, perhaps, made the transition more fractious than was necessary. As you go forward in Montreal and beyond, keep Pearson and Fisher in mind. Be like the early path-breaking Pearson, not the older Pearson who was unable to see that he could have learned from his critics. And approach biometry as an integral part of scientific exploration, as Fisher did, but perhaps with a warmer appreciation of those who have gone before.

Acknowledgements

In addition to the references cited below, this article has drawn upon manuscripts in the collections of University College London and the University of Adelaide, as well as some in the author's possession. I am grateful to my colleague Robert J. Richards for bringing Reisch's *Margarita Philosophica* to my attention, to Alice Schreyer of our Special Collections Research Center for helping me locate and establish the pedigree of Reisch's tree, and to Ian Hacking for educating me on the history of trees.

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