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Antistigler

Unpublished

Stigler is the author of two books (1986; 1999) in which he dared to profane the memory of Gauss.

I had unavailingly criticized the first one (1993; 1999a; 1999b), but not a single person publicly supported me, whereas several statisticians, only justifying themselves by arguments *ad hominem*, urgently asked me to drop that subject. The appearance of Stigler's second book showed that they were completely wrong but the same general attitude is persisting. One of those, apparently believing that a living dog was more valuable than a dead lion, is the present President of the International Statistical Institute. But to go into detail.

1) A few years ago Stigler was elected President of that same Institute (and had served in that capacity). He is now member of the Institute's committee on history to which I was also elected (chosen?) without my previous knowledge or consent. I refused to work together with him (and with Descrosières, – of all members of the Institute, see below!).

2) A periodical (*Intern. Z. f. Geschichte u. Ethik* (!) *der Naturwissenschaften, Technik u. Medizin*, NTM) refused to consider my proposed subject, – the refutation of Stigler. The Editor politely suggested that I should apply to a statistical periodical.

3) The Gauss-Gesellschaft-Göttingen is silent and had not even answered my letter urging them to support me.

4) Healy (1995, p. 284) indirectly called Stigler the best historian of statistics of the 20th century, and Hald – yes, Hald (1998, p. xvi) even called Stigler's book (1986) *epochal*. Epochal, in spite of slandering Gauss, of humiliating Euler (below), and of its being an essay rather than THE HISTORY (!) of statistics, as Stigler had the cheek to name it.

So much is absent in THE HISTORY, – cf. my book Sheynin (2005/2009), – in spite of which it became the statisticians' Bible, that I shall extrapolate this phenomenon by reducing it with Lewis Carroll's help *ad absurdum*:

Other maps are such shapes, with their islands and capes:
But we've got our brave Captain to thank
(So the crew would protest) "That he's bought us the best –
A perfect and absolute blank!"

Stigler is regarded as a demigod. *Historia Mathematica* had published a review of his book (1999). Instead of providing its balanced account, the reviewer (an able statistician; H. M. vol. 33, No. 2, 2006) went out of his way to praise, *to worship* both the book and Stigler himself.

5) *Centaurus* rejected the manuscript of my paper (1999a) initially submitted to them since the anonymous reviewer, contrary to facts and common sense, did his damnedest to exonerate Stigler.

In addition to my papers mentioned above, I can now add two more publications (2005; 2006, see their Indices), but I ought to add several points here.

1. Stigler (1986, p. 145): *Gauss solicited reluctant testimony from friends that he had told them of the method* [of least squares, MLSq] *before* [the appearance of the Legendre memoir in] *1805*.

And in 1999, p. 322, repeating his earlier (of 1981) statement of the same ilk: *Olbers did support Gauss's claim … but only after seven years of repeated prodding by Gauss*. Grasping at straws, Stigler adds an irrelevant reference to Plackett (1972).

So what happened with Olbers? On 4.10.1809 Gauss had asked him whether he remembered that he had heard about the MLSq from him (from Gauss) in 1803 and again in 1804. Olbers apparently did not answer (or answered through a third party). On 24.1.1812 Gauss asked even more: Was Olbers prepared to confirm publicly that fact? And Olbers answered on 10.3.1812: *gern und willig* (with pleasure), and at the first opportunity. However, during 1812 – 1815 Olbers had only published a few notes on the observation of comets (*Catalogue of Scientific Literature*, Roy. Soc. London), and he therefore only fulfilled Gauss' request in 1816. (Much later Gauss, who became sick and tired of the whole dispute, mentioned that his friend had acted in good faith, but that he was nevertheless displeased by Olbers' testimony made public.)

2. Again in 1999, Stigler had deliberately omitted to mention Bessel's statement on the same subject. I discovered it while being prompted by Stigler's attitude and quoted Bessel in a paper (1993) which Stigler mentioned in 1999. Bessel's testimony, all by itself, refutes Stigler's accusation described above.

3. Stigler (1999, pp. 322 – 323) mentions von Zach, his periodical (*Monatl. Corr.*) and some material published there in 1806 – 1807 which allegedly (indirectly) proved that von Zach had not considered Gauss as the inventor of the MLSq. Stigler leaves out a review published in the same periodical in 1809 whose anonymous author (von Zach?) described the actual history of the discovery of the MLSq, see p. 191. Incidentally, I (1999a, p. 258) found von Zach's later statement in which he repeated Gauss' explanation to the effect that he, Gauss, discovered the MLSq in 1795.

4. Stigler (1986, p. 57): "It is clear […] that Legendre immediately realized the method's potential". And, on p. 146: "There is no indication that [Gauss] saw its great general potential before he learned of Legendre's work". Stigler thus denies Gauss' well-known statement that he had been applying the MLSq since 1794 or 1795, denies simply because he is inclined to dethrone Gauss and replace him by Legendre.

5. Stigler (1986, p. 143): Only Laplace saved Gauss' first justification (in 1809) of the MLSq from joining "an accumulated pile of essentially ad hoc constructions". And how about Legendre? Stigler (1986, p. 13): *For stark clarity of exposition the presentation* [by Legendre in 1805] *is unsurpassed; it must be counted as one of the clearest and most elegant introductions of a new statistical method in the history of statistics*. His work (Stgler, p. 57) revealed his "depth of understanding of his method". All this in spite of two mistakes made by Legendre and lack of any demonstration of the method. Legendre alleged that the MLSq agreed with

the minimax principle, and he mentioned errors instead of residual free terms of the initial equations. And can we believe that Stigler did not know that the Gauss' proof of 1809, which allegedly almost joined "the accumulating pile" of rubbish, had been repeated in *hundreds* of books on the treatment of observations? Was it only due to Laplace?

6. Stigler (p. 146): *Although Gauss may well have been telling the truth about his prior use of the method, he was unsuccessful in whatever attempts he made to communicate it before 1805*. The first part of the phrase was appropriate in respect to a suspected rapist, but not to Gauss. As to his "attempts", Gauss had communicated his discovery to several friends and colleagues but did not proclaim it through a public crier or by a publication in a newspaper.

Other pertinent points.

7. Stigler (1986, p. 27) denounced Euler as a mathematician who did not understand statistics. After I (1993) had refuted that pernicious statement, Stigler (1999, p. 318) declared that, in another case, Euler *was acting in the grand tradition of mathematical statistics*. He did not, however, renounce his previous opinion. More: in that second case, Euler had rejected the method of maximum likelihood, because, as he put it, the result should not change whether an outlying observation be rejected or not (read: the treatment should be such that …). Euler suggested to keep to the known and reliable method, to the mean; he had not mentioned the median although it (but not the term itself) had actually been earlier introduced by Boscovich.

8. Descrosières (1998, transl. from French) believes that Poisson had introduced the strong law of large numbers and that Gauss had derived the normal distribution as a limit of the binomial law, see my review in *Isis*, vol. 92, 2001, pp. 184 – 185. And Stigler (1999, p. 52)? He called Descrosières *a scholar of the first rank*!

9. There also, Stigler named another such high ranking scholar, Porter, and he (p. 3) also called Porter's book of 1986 *excellent*. I reviewed it (*Centaurus*, vol. 31, 1988, pp. 171 – 172) and declared an opposite opinion. In 2004 Porter published Pearson's biography, see my review in *Hist. Scientiarum*, vol. 16, 2006, pp. 206 – 209. I found there such pearls of wisdom as (p. 37) *Even mathematics has aspects that cannot be proven, such as the fourth dimension*. In my opinion, that book is barely useful.

10. In 1983, issuing from a biased stochastic supposition, Stigler declared that another author rather than Bayes had actually written the Bayes memoir. In 1999, while reprinting his 1983 paper, in spite of his sensational finding being stillborn and forgotten, Stigler got rid of its criticisms in a tiny footnote (p. 391).

11. Stigler (1986) is loath to mention his predecessors. On pp. 89 – 90 he described the De Moivre – Simpson debate forgetting to refer to me (1973a, p. 279). And on pp. 217 – 218 he discussed the once topical but then completely forgotten conclusion concerning statistics of population without citing his only possible source of information, Chuprov's letter to Markov of 10.3.1916 (Ondar 1977/1981, No. 72, pp. 84 – 85).

Long before that Stigler (1977) dwelt on Legendre's accusation of Gauss concerning number theory without naming me (1973b, p. 124, note 83).

So why does Stigler remain so popular? Because the statistical community is crassly ignorant of the history of its own discipline; because it pays absolutely no attention to the slandering of Gauss' memory (even if realizing that fact, as the reviewer for *Hist. Math*. did, see above, – I personally informed him about it in 1991, but he had known it himself); because it possesses a narrow scientific Weltanschauuung; and because the tribe of reviewers does not feel any social responsibility for their output. And of course there is a special reason: Stigler published his book (1986) when there was hardly anything pertinent except for papers in periodicals. The same happened to a lesser extent with Maistrov's book of 1974 which is still remembered!

To end my pamphlet, I quote, first, the most eminent scholar and historian of science, the late Clifford Truesdell (1984, p. 292), whom I will never forget and whose alarm bell apparently fell on deaf ears, and, second, Einstein's letter of 1933 to Gumbel, a German and later an American statistician (Einstein Archives, Hebrew Univ. of Jerusalem, 38615, in translation):

1) *No longer is learning the objective of scholarship.* […] *By definition, now, there is no learning, because truth is dismissed as an old-fashioned superstition*.

2) *Intergity is just as important as scientific merits*.

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