Karl Gauss
to
Eberhard von Zimmermann

Göttingen

May 26, 1796

Translated from the German by Peter Martinson

I must have been very much in error (and, yet, to me this appears difficult with such a free open man) if the coldness with which H. H. K[aestner] received my discovery[1] originated from a personal consideration of me, and not rather from a certain aversion against every thing new, which cannot be mistaken, however, with the greatest respect towards him. After his subsequent behavior, the circumstances in which he was in at the time appear to have contributed. Some time later, when I was with him again, he himself began with the theorem, but claimed complete certainty that the nature of the equations proved the impossibility of the matter. I put forward to him again (as for the first time), that the equation doesn't separate from the 17th, as he thought; because one knows a root from it straight away, it is only of the 16th degree, and this can resolve itself into one of the 8th, this into one of the 4th, and thus, the 2nd. This time, it seemed to completely startle him, and at once, he assumed another tone and said, if it were really true that one could construct the polygon on the plane with such ease, it would still have absolutely no usefulness. Ultimately, he went so far as to claim that the basis for the whole thing was already in his *Elements*,[2] and he had thought it not worth the trouble to develop it further. Nevertheless, that seems inconsequential to me. Since I conceded the first, and did not contradict the other, I received permission to present it in written form to him.

The matter went thus. Out of my procedure, a solution can indeed be extracted with much trouble, but this direct solution is, as he now claimed, probably unreliable. Most likely, he had already seen some of the essay that I had submitted to him. After some small criticisms, which do not affect the significance of the matter, his opinion turned out to be, that it would have no practical use, but that it would be quite a good curiosity, and would perhaps

[1] This refers to Gauss's discovery of the constructability of the Heptadecagon.

[2] *Anfangsgründe der Analysis endlicher Gr̈ossen*, the textbook Abraham Kästner wrote for Göttingen University.

procure a brighter insight into this part of mathematics, especially if I could treat the matter more generally, which is an opinion I took to heart. He said, in addition, that it was not completely new, since he has already learned that it would produce the solution to the often alluded to equation, which belief I certainly want to allow him.

I must yet say something about the progress of my actual analytical investigations. I have found an excellent treatise by *Legendre* in the 1785 *Mèmoires* of Paris, in which appears the proof of the theorem which I had long tried, in vain, to prove completely. But, the part he assumes there (je ne suppose que ce que [I assume only that] says L. G.), which I have also been lacking for almost a year, I have now found. Also, through Herr H. *Kästner's* benevolence, I now have in hand a German translation of *Lagrange's* addition to *Euler's* algebra (which you had already brought to my attention; I had not been able to get it here, and H. H. *Kästner* did not know of it), appearing for the first time this year, authored by Hrn. Hofrat *Kausler* in Württemberg. However, this book, excellent in itself, contains little that comes in conflict with my discovery. *Lagrange* had not quite understood the often mentioned theorem, much less proven it, and *Legendre's* treatment is quoted, but its merit is criticized quite wryly. It would be very useful for me to know the year the French original came out. The translator also made some additions, although of little content. Therefore, I believe I am now able to dare an elaboration of this, if a printer appears...