VNiVERSiDAD Ð SALAMANCA

FACULTAD DE FILOSOFÍA
DEPARTAMENTO DE FILOSOFÍA, LÓGICA Y ESTÉTICA

The Germanic Development of the Pre-Modern Notion of Number

From c. 1750 to Bolzano's *Rein analytischer Beweis*

Elías Fuentes Guillén

Tesis presentada para obtener el grado de
Doctor en Lógica y Filosofía de la Ciencia

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A mis padres y a mi hermana.
A Alejandrina.
A José.

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Agradecimientos

Comencé mi doctorado a finales del año 2010 y lo termino siete años después. Nada es ya lo mismo. Me fui a un país ajeno para estudiar un Máster y volver, al año siguiente, a este pedazo de tierra, donde mis padres y mi hermana. Pero no regresé: me dieron una beca y allá estaba José. Y me quedé cuatro años más, durante los cuales viví entre Salamanca y Sevilla, y entre medias en Nancy y Paris. El proyecto de tesis pasó de las prácticas matemáticas involucradas en el desarrollo de la teoría de conjuntos de Cantor a las prácticas matemáticas de quienes le precedieron y luego, a su vez, a las de quienes antecedieron a estos. Yo pasé de ser alumno a impartir clase y de sólo asistir a congresos a participar en ellos. Y en la distancia estreché lazos con mi familia y en la cercanía me hice de muy buenos amigos.

Mi último año en España, sin embargo, lo cambió todo. Cansado de la academia y sus vicios, en el verano del 2014 viajé a Praha con motivo de un congreso. Ahí, donde Bolzano y Kafka, supe que no había obtenido un apoyo para quedarme en España un año más, una posibilidad que tenía enteramente asumida. Pero, además, ocurrió lo indecible. Así que volví a Salamanca triste por lo uno y profundamente decepcionado por lo otro y fue entonces cuando descubrí que correr me hacía bien y que era bueno en ello. A partir de ese momento y hasta el último día que estuve en España corrí y volví a México y seguí corriendo. Más allá del “primer semáforo del infinito” puede que no haya nada, pero aquí ya voy en la segunda vuelta a la Tierra por su diámetro más amplio y, tras ocho meses de trabajo como si en ello se me fuera mi último aliento, presento al mundo la tesis que sucede a estas líneas.

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Introducción

La historiografía y la filosofía de la matemática usualmente describen las reformas educativas llevadas a cabo en Francia y los territorios germánicos durante la última década del siglo XVIII y la primera del siglo XIX de la siguiente manera: mientras que en Francia tales reformas se realizaron tras la Revolución Francesa, particularmente con la creación de la *École polytechnique* y la *École normale* en París en 1794 y 1795, respectivamente, las reformas germánicas comenzaron alrededor de 1810 con la fundación de la *Universität zu Berlin*.[1] Esta descripción, si bien acaso sea útil para resumir el periodo antes mencionado cuando el interés radica en lo que ocurrió antes –por ejemplo, el desarrollo del análisis matemático en el siglo XVIII– o después –por ejemplo, el desarrollo del análisis real y la teoría de conjuntos en el siglo XIX–, ha contribuido a una falta de comprensión, cuando no a una mala compresión, de lo que ocurrió en la matemática no sólo entre tales desarrollos sino además durante los propios siglos XVIII y XIX.

Primero, mientras que a inicios del siglo XIX el territorio comprendido por el Imperio Francés era muy similar a la Francia actual, la Alemania de hoy tiene poco que ver con los territorios germánicos de aquella época; esto es, mientras que en cierto sentido aquellas reformas educativas francesas tuvieron un impacto en 'una nación', no puede decirse lo mismo acerca de las reformas germánicas. Segundo, estrictamente hablando esas reformas no comenzaron en Francia en 1794 ni en los territorios germánicos en 1810, sino antes de esas fechas. Por último, así como el enfoque científico conceptual del siglo XIX (que condujo al desarrollo del análisis real matemático y la teoría conjuntista) suele ser vinculado a esas reformas germánicas, el enfoque científico utilitario (opuesto al análisis matemático del siglo XVIII) normalmente es asociado con aquellas reformas francesas. Empero, como lo evidencian tanto la tradición analítica Euleriana como la propuesta de Bernard Bolzano, tanto si aquellas reformas educativas elogiaban la utilidad de la geometría (en el caso de Francia) o un enfoque conceptual (en el caso de los territorios germánicos), lo cierto es que ni aquella tradición había conseguido librarse de sus raíces geométricas a principios de la última década del siglo XVIII, ni la propuesta de Bolzano a principios del siglo XIX puede ser considerada como el punto de partida del análisis real.

[1] Así ocurre, por ejemplo, en (Struik, 1981: 14-15), (Jahnke, 1993: 266) y (Ferreirós, 2007: 4-6).

Dejando de lado lo que ocurrió con la tradición Euleriana, en lo concerniente al análisis matemático moderno hoy en día dos autores son considerados como sus pioneros, a saber, Cauchy y Bolzano. Desde cierto punto de vista, Cauchy quizás fue "la figura más importante en el comienzo del análisis riguroso" (Grabiner, 1982: 2), mientras que las contribuciones de Bolzano –"en la fundamentación del análisis real"– acaso "no solamente [bosquejaron] los contornos generales de las nuevas matemáticas emergentes del siglo XIX, sino que además [efectuaron] el trabajo con considerable detalle" (Rusnock, 2000: 18). Aún más, a partir de cierta interpretación de la obra de ambos autores, se podría afirmar que "el logro de Cauchy fue la denominada 'aritmetización' del análisis" (Grattan-Guinness, 1970: 373), "el proyecto de colocar la teoría de la línea real sobre un fundamento sólido, aritmético, [que] habría de ser llevado adelante a lo largo del siglo XIX, en gran medida ajeno al trabajo de Bolzano" (Ewald, 1999: 226).

Sin embargo, al igual que con la descripción usual de dichas reformas educativas francesas y germánicas, hay algo erróneo en la narrativa habitual sobre las contribuciones de Bolzano al desarrollo del análisis moderno: mientras que algunos autores que mencionan o estudian los primeros trabajos matemáticos de Bolzano son cuidadosos al vincular las propuestas contenidas en esos trabajos con lo que posteriormente se llamaría "la aritmetización del análisis", así como al vincular sus resultados en esos trabajos con los de autores posteriores, en especial con Karl Weierstrass (cf. Kitcher, 1984: 191 y 263; Laugwitz, 1989: 205; Belna, 2000: 55; Moore, 2000 y 2008; Russ, 2004; Gray, 2015: 240), muchos de aquellos autores asumen ambas relaciones o al menos la segunda de ellas. De hecho, así como la emergencia de ambas lecturas anacrónicas puede ser datada, a grandes rasgos, en el Imperio Alemán (*Deutsches Reich*) durante el último tercio del siglo XIX (en el caso de quienes asumen el vínculo entre resultados particulares) y principios del siglo XX (en el caso de quienes asumen el vínculo entre propuestas generales), en ambos casos se puede señalar a ciertos matemáticos germánicos como figuras clave en la promoción de una y otra lectura, a saber, respectivamente, Karl Weierstrass y Felix Klein.

Respecto a aquella segunda lectura, Weierstrass reconocía a Bolzano como el matemático cuyo trabajo (particularmente su *Prueba puramente analítica*, de 1817)[2] fue la base para el desarrollo del llamado teorema de Bolzano-Weierstrass, teorema esencial del análisis moderno que hoy

[2] El título completo del trabajo de Bolzano es: *Rein analytischer Beweis des Lehrsatzes, dass zwischen je zwey Werthen, die ein entgegengesetztes Resultat gewähren, wenigstens eine reelle Wurzel der Gleichung liege*.

día sostiene que un conjunto infinito y limitado de números reales tiene un punto límite; testimonio de ello se encuentra en las notas de clase de Weierstrass tomadas por algunos de sus alumnos y en algunos escritos de estos y de él. Sin duda, el reconocimiento de Weierstrass contribuyó a la idea de que aquel teorema y su prueba –enunciado y ofrecida por él– fueron de alguna manera anticipados por Bolzano (cf. Weierstrass, 1868/1986: 79; 1874: 304; 1886: 59-63 y 238-239). No obstante, más allá de lo engañosa que pudo resultar la narrativa del propio Weierstrass, lo que sobre todo consolidó y propagó esa idea fue: a) la importancia atribuida por los estudiantes de Weierstrass a las referencias de éste a Bolzano y b) la lectura efectuada por esos estudiantes de la obra de este último. El "Anexo A" incluido al final de este trabajo ilustra este argumento sobre la propagación, consolidación y pervivencia de semejante lectura.

Ahora bien, en cuanto a la primera de aquellas lecturas, si bien Leopold Kronecker, por ejemplo, se refirió a la "aritmetización" (*arithmetisiren*) del análisis y de todas las disciplinas matemáticas (cf. Kronecker, 1887: 338-339),[3] su propuesta no fue exactamente la misma que la de aquella "importante tendencia matemática que", como dijo Klein en 1895, "[tuvo] a Weierstrass como su máximo exponente" (cf. Klein, 1896: 241): si bien ambas propuestas compartían la convicción en el desarrollo de las matemáticas sobre la base de la aritmética de los números naturales, Weierstrass y otros pretendieron desarrollar una teoría de los números irracionales, algo que al menos en algún momento Kronecker rechazó. Así, a partir de inicios del siglo XX fue más habitual identificar la propuesta de Weierstrass y otros bajo la designación de "aritmetización del análisis" (concebida acaso la propuesta de Kronecker como una versión restrictiva de esta), siendo incluso entonces habitual encontrar elogios hacia ella semejantes a los de Poincaré en 1900: "La matemática", dijo, "ha sido aritmetizada. [...] Uno puede decir que actualmente se ha alcanzado el rigor absoluto" (Poincaré, 1902: 120 y 122).[4] Pero fue precisamente Klein, si no el primero en identificar a Bolzano con tal aritmetización, al menos aquel cuya tal identificación se hizo eco entre matemáticos e historiadores y filósofos de las matemáticas: "Bolzano", escribió

[3] Kronecker escribió: "Y yo también creo que algún día se logrará 'aritmetizar' todo el contenido de todas estas disciplinas matemáticas, es decir, sobre la sola base del concepto de número considerado en el sentido más restringido [esto es, en el sentido de los números naturales], eliminando así las modificaciones y extensiones de este concepto, originadas principalmente por aplicaciones en geometría y mecánica" ("Und ich glaube auch, dass es dereinst gelingen wird, den gesammten Inhalt aller dieser mathematischen Disciplinen zu 'arithmetisiren', d. h. einzig und allein auf den im engsten Sinne genommenen Zahlbegriff zu gründen, also die Modificationen und Erweiterungen dieses Begriffs wieder abzustreifen, welche zumeist durch die Anwendungen auf die Geometrie und Mechanik veranlasst worden sind".).

[4] Poincaré escribió: "Il ne reste plus aujourd'hui en Analyse que des nombres entiers ou des systèmes dinis ou infinis de nombres entiers, reliés entre eux par un réseau de relations d'égalité ou d'inégalité. Les Mathématiques, comme on l'a dit, se sont arithmétisées. [...] Or, dans l'Analyse d'aujourd'hui, quand on veut se donner la peine d'être rigoureux, il n'y a plus que des syllogismes ou des appels à cette intuition du nombre pur, la seule qui ne puisse nous tromper. On peut dire qu'aujourd'hui la rigueur absolue est atteinte".

en sus *Lecturas sobre el desarrollo de las matemáticas en el siglo XIX* (*Vorlesungen über die Entwicklung der Mathematik im 19.Jahrhundert*) publicadas póstumamente en 1926-27, "es uno de los padres de la actual 'aritmetización' de nuestra ciencia" (Klein, 1826: 56).[5]

Como se muestra en el "Anexo B" de este trabajo, aún es común la inclusión de Bolzano entre los matemáticos que abogaron por la aritmetización Weierstrassiana del análisis, dos siglos después de la publicación en Praha de su *Prueba puramente analítica*. Por el contrario, el *Curso de análisis* (*Cours d'Analyse*) de Cauchy, publicado cuatro años después en París, no sólo fue más conocido durante el siglo XIX, sino que además ha sido más ampliamente estudiado desde entonces. Como consecuencia, se sostiene aquí, tanto la propuesta general, como los resultados particulares contenidos en los primeros trabajos matemáticos de Bolzano (1804-1817), continúan siendo malinterpretados: en 1817 las nociones y prácticas matemáticas de Bolzano, si bien dejaban entrever preocupaciones y características innovadoras, eran muy diferentes de las nociones y prácticas Weierstrassianas aritmetizadoras venideras, en la medida en que la obra de Bolzano aún tenía rasgos esenciales de una concepción semántico-ontológica del análisis.

De hecho, la lectura de Bolzano como un matemático de transición, esto es, uno cuyos primeros trabajos de cierta manera se orientaban hacia el terreno en el cual fue desarrollado el análisis real del siglo XIX, pero que a la vez tenía profundas raíces en concepciones y prácticas heredadas que eran comunes entre matemáticos germánicos de *c.* 1800, está estrechamente vinculada a lo dicho al principio de esta introducción sobre la narrativa sesgada de las reformas educativas germánicas a principios del siglo XIX. Tanto si se estudia cuidadosamente lo que ocurrió en las matemáticas germánicas medio siglo después de 1817 (digamos, hasta *c.* 1872), como medio siglo antes de 1804 (digamos, desde *c.* 1750), tal rol de Bolzano queda sustentado. Empero, dado que el punto de llegada de esta tesis son los trabajos de Bolzano de 1804-1817, la atención se centrará en las generaciones de matemáticos germánicos previas a Bolzano, así como en los vínculos y diferencias entre ellos y éste. Más específicamente, el interés aquí radica en la obra de los matemáticos germánicos de la segunda mitad del siglo XVIII, mismos que generalmente son pasados por alto por los matemáticos e historiadores y filósofos de la matemática.

[5] Klein escribió: "Diese Punkte wurden erst 1817 durch die Schrift von B. Bolzano 'Rein analytischer Beweis des Lehrsatzes, dass zwischen je zwey Werthen, die ein entgegengesetztes Resultat gewähren, wenigstens eine reelle Wurzel der Gleichung liege' (Prag, = Ostw. Klass. 153) erledigt, der damit auch über die späteren Entwicklungen von Cauchy hinausgegangen ist. Bolzano ist einer der Väter der eigentlichen 'Arithmetisierung' unserer Wissenschaft".

Justamente, más allá de las prácticas y los conceptos matemáticos implícitos en los primeros trabajos matemáticos de Bolzano, sus fuentes matemáticas explícitas refuerzan dos principales tesis –entrelazadas– de este trabajo. Así, mientras que hasta 1817 Bolzano cita sobre todo a matemáticos germánicos (una vasta mayoría) y franceses, y en menor medida a británicos y griegos antiguos, claramente se puede distinguir entre ellos a tres grupos de acuerdo a su conexión con cierta tradición o escuela: a) la tradición Newtoniana (Newton y, por ejemplo, John Colson, Samuel Horsley, Thomas Simpson, William Sewell y John Landen); b) la tradición francesa (Euler y, por ejemplo, Simon Antoine Jean L'Huilier, Johann Castillon, Alexis Claude Clairaut, Jean-Baptiste le Rond d'Alembert, Jean-Baptiste d'Estienne du Bourguet, François Daviet de Foncenex, Pierre-Simon Laplace, Adrien-Marie Legendre, Sylvestre François Lacroix y Joseph-Louis Lagrange); y c) la denominada escuela combinatoria de Hindenburg (Hindenburg y, por ejemplo, Heinrich August Rothe, Georg Simon Klügel y Johann Friedrich Pfaff). Empero, mientras que tales referencias de Bolzano a los autores pertenecientes a los dos primeros grupos fueron en su mayoría menciones aisladas,[6] sus referencias a Hindenburg y Klügel, así como a diversos matemáticos germánicos,[7] sobre todo de la segunda mitad del siglo XVIII y principios del siglo XIX, pone de manifiesto la importancia de estos (especialmente de aquellos inmediatamente previos) en el desarrollo de su propuesta matemática temprana.

Así, hay algo especialmente llamativo en lo concerniente a las fuentes germánicas de Bolzano: junto con Wolff (uno de los filósofos y matemáticos más influyentes del siglo XVIII) y Kant (un de los filósofos germánicos más influyentes a principios del siglo XIX), Bolzano se refiere sobre todo a matemáticos que en su mayoría ni eran seguidores de aquel filósofo, ni habían sido educados conforme a las ideas de aquel matemático. Indudablemente, esos autores germánicos a los que Bolzano se refiere con mayor frecuencia que a otros no fueron ajenos a las ideas y obras de Wolff y Kant, pero su acercamiento a las matemáticas estuvo moldeado por las obras e ideas de otros autores. Algo que, si bien dependió de la región y época en que se formaron, tuvo a sus

[6] De hecho, el grueso de ellos sólo aparecen nombrados ya sea en la lista que autores incluida al inicio del prefacio de su trabajo sobre el teorema del binomio de 1816 (cf. Bolzano, 1816: III-IV), o en la lista de autores incluida al inicio del prefacio a su primer trabajo de 1817 (cf. Bolzano, 1817B: 3 y 5).

[7] Los matemáticos germánicos cuya referencia en la obra temprana de Bolzano va más allá de una mera mención son: Christian Wolff, Abraham Kästner, Johann Schultz, Immanuel Kant (filósofo), Johann Andreas Christian Michelsen, Georg Simon Klügel, Carl Friedrich Hindenburg, Karl Christian von Langsdorf, Johann Heinrich Lambert, August Leopold Crelle, Johann Carl Friedrich Gauss, Johann Friedrich Gensichen, Ernst Platner y Ernst Gottfried Fischer. Mientras que la lista de sus fuentes germánicas la completan Bendavid, Karl Friedrich August Grashof, Franz Anton Ritter von Spaun, (Joseph-Louis Lagrange), Jakob Hermann, Leonhard Cochius, Christian Gottlieb Selle, Franz Ulrich Theodor Aepinus, Johann Andreas von Segner, Karl Scherffer, Wenceslaus Johann Gustav Karsten, Johann Friedrich Pfaff, Heinrich August Rothe, Christian Lebrecht Rösling, Mathias Metternich, Friedrich Gottlieb von Busse, Christian Friedrich Kausler, János Pasquich, Friedrich Wilhelm Jungius, Karl Christian Friedrich Krause y Gottlob Nordmann.

figuras clave en Kästner, Karsten y Segner, los profesores de matemáticas en las universidades de Göttingen y Halle. Ellos tres y sus respectivos maestros de matemáticas, en cambio, sí se formaron durante el apogeo de la influencia de Wolff, si bien, conforme a la tesis aquí defendida, no deben ser considerados como meros propagadores de las ideas de este.

Es verdad que entonces los trabajos más conocidos de Kästner, Karsten y Segner eran sus libros de texto, así como en algunos casos sus traducciones de trabajos de autores extranjeros, por lo que su influencia sobre las nuevas generaciones tuvo que ver ante todo con concepciones y prácticas matemáticas generales y no tanto con resultados o desarrollos particulares. Pero, así como Bolzano no repitió meramente lo que heredó, esos tres autores tampoco lo hicieron.[8] Más aún, así como los trabajos de esos tres matemáticos no fueron simplemente "versiones más populares y legibles [de la obra de Wolff]" (Bullynck, 2006: 4), como se suele contar, ellos paulatinamente introdujeron modificaciones significativas en sus trabajos que los alejaron cada vez más de ideas de Wolff aún presentes en los trabajos de éste de la década de 1740.

Ese último punto es relevante, dado la idea generalizada sobre el estancamiento de las matemáticas germánicas durante la segunda mitad del siglo XVIII. Mientras que a lo largo del siglo pasado se pueden encontrar menciones aisladas sobre algunos aspectos de los trabajos matemáticos de esos tres autores (cf. Cantor, 1904; Cajori, 1913 y 1923; Fellmann, 1983; Clewis, 2015), lo cierto es que existen pocos estudios enfocados –al menos parcialmente– en ellos. El trabajo de Gert Schubring (2005), así como otros trabajos recientes, entre ellos (Kleinert, 2002) y (Bullynck, 2006 y 2013), son las excepciones. Debido a su estudio detallado y extenso de las nociones de cantidad y número entre los matemáticos franceses y germánicos del siglo XVIII, el libro de Schubring ha de ser considerado la principal referencia en el tema. De hecho, muchos de los autores cuyo trabajo es abordado en el capítulo B del presente trabajo, también son estudiados por Schubring y, por ende, el análisis ofrecido aquí de dichos autores ha de ser entendido como una contribución al trabajo de él y a los otros estudios existentes.

La investigación de Schubring aborda el desarrollo del análisis matemático en los siglos XVII, XVIII y XIX, sobre todo en Francia y Alemania y enfocado en el desarrollo de los conceptos de

[8] Lo mismo puede decirse de Wolff pero ello va más allá de los alcances de este trabajo, por lo que habrá de bastar esta nota a pie de página para evitar que en este punto se atribuya el mismo error que aquí se está criticando. Así, a lo largo de este trabajo las ideas de Wolff que son mencionadas son atribuidas al año específico de la obra citada.

números negativos y cantidades infinitamente pequeñas. Como aquí, él presta especial atención a los libros de texto, dado que “ofrecen buenos indicadores respecto al horizonte conceptual de un cierto periodo y cultura” (Schubring, 2005: 7). A Bullynck, por su parte, le interesa el estado de las matemáticas Germánicas a finales del siglo XVIII y principios del XIX “en la educación elemental y superior”, así como en “las formas de mediación y comunicación que acompañaron y prepararon el camino para las revistas especializadas en matemáticas” (Bullynck, 2006 y 2013). Esta tesis comparte con ambos trabajos tales intereses y algunas elecciones metodológicas, si bien aquí el interés radica en las nociones básicas de número y cantidad durante la última parte del siglo XVIII, para así comprender mejor el surgimiento de la noción pre-moderna de número entre los matemáticos germánicos, en particular en la obra matemática temprana de Bolzano.

La comprensión de la evolución de las ideas matemáticas de Kästner, Karsten y Segner, se sostiene aquí, contribuye a su vez a comprender el panorama matemático germánico de finales del siglo XVIII e inicios del XIX. Los vínculos entre Kästner, el matemático más influyente de aquellos tres, y la escuela combinatoria de Hindenburg, así como entre ésta y Lagrange, no son fortuitos, si bien en última instancia tales proyectos eran diferentes, como lo eran sus nociones y procedimientos subyacentes. Pero, al mismo tiempo, tal comprensión de la matemática germánica a principios del siglo XIX es de suma importancia para analizar algunos aspectos clave de los trabajos matemáticos de Bolzano de 1804-1817.

Finalmente, establecidos los momentos de llegada (1817) y de partida (*c*. 1750), el territorio (las matemáticas germánicas) y la ruta a seguir (de Göttingen y Halle a Praha), así como explicadas las razones para tales elecciones, ¿dónde comenzar, en términos geográficos, esta tesis sobre el desarrollo germánico de la noción pre-moderna de número? El trabajo del agrimensor consiste precisamente no sólo en delimitar el territorio y estudiarlo, sino también en fijar un punto de partida adecuado para la óptima consecución del trabajo: “K. se detuvo durante un largo tiempo en el puente de madera que conducía desde el camino hasta la aldea, mirando lo que parecía ser un vacío” (Kafka, 2009: 5). Como consecuencia, y dada la situación política de los territorios germánicos durante ese periodo, el punto de partida estará a medio camino entre el Reino de Prussia (*Königreich Preußen*) y el Archiducado de Austria (*Erzherzogtum Österreich*), las dos partes constituyentes más importantes del Sacro Imperio Romano en ese momento (mismo que entonces comprendía la mayoría de los territorios germánicos), a saber, Silesia (*Schlesien*).

Introduction

The historiography and philosophy of mathematics usually characterize the educational reforms undertaken in France and the Germanic territories during the last decade of the 18th century and the first decade of the 19th century as follows: while in France such reforms were carried out after the French Revolution and in particular with the creation of the *École polytechnique* (in 1794) and the *École normale* (in 1795) in Paris, the Germanic reforms began around 1810 with the foundation of the *Universität zu Berlin*.[9] This characterization, although it might be useful to summarize the aforementioned period when the interest lies in what happened before –e.g. the development of mathematical analysis in 18th century– or later –e.g. the development of real analysis and set theory in 19th century–, has contributed to a lack of awareness, if not a misunderstanding, of what happened in mathematics not only in between such developments but also during the 18th and 19th centuries.

To begin with, even though at the beginning of the 19th century the territory comprised by the French Empire (*Empire Français*) was very similar to modern-day France, 21st century Germany has little to do with the Germanic territories of that time; that is, while in a sense it can be said that those French educational reforms had an impact on a 'whole nation', the same cannot be said about the Germanic. Secondly, strictly speaking those reforms did not begin in 1794 in France, nor in 1810 in the Germanic territories but in both cases before those dates. Finally, just as the scientific conceptual approach (that led to the development of real mathematical analysis and set theory) is usually linked to those Germanic reforms, the scientific utilitarian approach (opposed to 18th century mathematical analysis) is normally associated with those French reforms. However, as evidenced by both the Eulerian analytical tradition and Bernard Bolzano's proposal, whether those educational reforms praised highly the usefulness of geometry (in the case of France) or a more conceptual approach (in the case of Germanic territories), the fact is that neither that tradition had managed to rid of the geometrical roots of mathematical analysis by the beginning of last decade of the 18th century, nor Bolzano's proposal at the beginning of the 19th century can be considered the starting point of real analysis.

[9] This is the case in, for example, (Struik, 1981: 14-15), (Jahnke, 1993: 266) and (Ferreirós, 2007: 4-6).

Leaving aside what happened with the Eulerian tradition, with regard to modern mathematical analysis nowadays two authors are commonly considered as its pioneers, namely, Cauchy and Bolzano. From a certain perspective, Cauchy perhaps was "the most important figure in the initiation of rigorous analysis" (Grabiner, 1982: 2), while Bolzano's contributions –"in the foundations of real analysis"– might have "not only [sketched] the general contours of the emerging new mathematics of the nineteenth century, but also [carried] the work out in considerable detail" (Rusnock, 2000: 18). Even more, from a certain interpretation of the works of those two authors someone could state that "Cauchy's achievement was the so-called 'arithmeticisation' of analysis" (Grattan-Guinness, 1970: 373), "the project of putting the theory of the real line on a solid, arithmetical foundation [which] was to be carried forward, largely in ignorance of Bolzano's work, throughout the nineteenth century" (Ewald, 1999: 226).

Nevertheless, as with the common description of the above-mentioned French and Germanic educational reforms, there is something fundamentally wrong in the usual narrative on the contributions of Bolzano to the development of modern analysis: while some of the authors that mention or study the early mathematical works of Bolzano are wary of linking the proposals contained in those works to what would later be called the "arithmetization of analysis", as well as to link the results in them with the ones of later authors, especially Karl Weierstrass (cf. Kitcher, 1984: 191 & 263; Laugwitz, 1989: 205; Belna, 2000: 55; Moore, 2000 & 2008; Russ, 2004; Gray, 2015: 240), many of those authors either assume this second relation or both of them. Indeed, the emergence of both readings can be roughly dated during the last third of the 19th century (for those who assume that link between particular results) and the early 20th century (for those who assume that link between general proposals), and in both cases it is possible to point out certain Germanic mathematicians as key figures in the promotion of each reading, scilicet, Karl Weierstrass and Felix Klein.

Concerning the second of those readings, it is known that Weierstrass acknowledged Bolzano as the mathematician whose work (particularly his *Purely Analytic Proof*, from 1817)[10] was the basis for the development of the so-called Bolzano-Weierstrass theorem, an essential theorem of modern analysis which nowadays states that an infinite and bounded set of real numbers has

[10] The full title of Bolzano's text is: *Purely Analytic Proof of the Theorem, that between any two Values which give Results of Opposite Sign, there lies at least one real Root of the Equation* (*Rein analytischer Beweis des Lehrsatzes, dass zwischen je zwey Werthen, die ein entgegengesetztes Resultat gewähren, wenigstens eine reelle Wurzel der Gleichung liege*).

a limit point; evidence of this is found both in notes of Weierstrass' lectures taken by his students, as well as in some works of these ones and him. Undoubtedly, Weierstrass' own recognition of Bolzano contributed to the idea that the theorem –enunciated by Weierstrass– and its proof –provided by Weierstrass– were somehow anticipated by Bolzano (cf. Weierstrass, 1868/1986: 79; 1874: 304; 1886: 59-63 & 238-239). However, beyond the fact that Weierstrass' narrative was misleading, what mainly consolidated and spread that idea was: a) the importance given by the students of Weierstrass to his reference to Bolzano and b) the reading made by those students of the latter's work. The table included at the end of this work as "Annex A" illustrates this argument on the propagation, consolidation and prevalence of such a reading.

As to the first of those readings, although Leopold Kronecker, for example, talked about the "arithmetization" (*arithmetisiren*) of analysis and all mathematical disciplines (cf. Kronecker, 1887: 338-339),[11] his proposal was not entirely the same as that advocated by that "important mathematical tendency which", as Klein said in 1895, "[had] as its chief exponent Weierstrass" (cf. Klein, 1896: 241): while both proposals shared the conviction in the development of mathematics on the basis of the arithmetic of natural numbers, Weierstrass and others intended to develop a theory of irrational numbers, something that at least at some point Kronecker rejected. Thus, from the early 20th century onwards it became increasingly common to identify the proposal of Weierstrass and others under the designation of "arithmetization of analysis" (conceived in any case Kronecker's proposal as a restrictive version of this one), and to celebrate it as Poincaré did in 1900: "Mathematics", he said, "has been arithmetized. [...] One can say that nowadays absolute rigor has been achieved" (Poincaré, 1902: 120 & 122).[12] But it was Klein, if not the first author to identify Bolzano with such arithmetization, at least the first one whose identification of this mathematician with that project was echoed among mathematicians and historians and philosophers of mathematics: "Bolzano", he wrote in his *Lectures on the Development of Mathematics in the 19thcentury* (*Vorlesungen über die*

[11] Kronecker wrote: "And I also believe that someday it will be achieved to 'arithmetize' the whole content of all these mathematical disciplines, i. e. on the sole basis of the concept of number taken in the narrowest sense [that is, in the sense of natural numbers], so to strip away the modifications and extensions of this concept, which have mostly been caused by the applications in geometry and mechanics" ("Und ich glaube auch, dass es dereinst gelingen wird, den gesammten Inhalt aller dieser mathematischen Disciplinen zu 'arithmetisiren', d. h. einzig und allein auf den im engsten Sinne genommenen Zahlbegriff zu gründen, also die Modificationen und Erweiterungen dieses Begriffs wieder abzustreifen, welche zumeist durch die Anwendungen auf die Geometrie und Mechanik veranlasst worden sind.").

[12] Poincaré wrote: "Il ne reste plus aujourd'hui en Analyse que des nombres entiers ou des systèmes dinis ou infinis de nombres entiers, reliés entre eux par un réseau de relations d'égalité ou d'inégalité. Les Mathématiques, comme on l'a dit, se sont arithmétisées. [...] Or, dans l'Analyse d'aujourd'hui, quand on veut se donner la peine d'être rigoureux, il n'y a plus que des syllogismes ou des appels à cette intuition du nombre pur, la seule qui ne puisse nous tromper. On peut dire qu'aujourd'hui la rigueur absolue est atteinte."

Entwicklung der Mathematik im 19.Jahrhundert) published posthumously in 1926-27, “is one of the fathers of the current ‘arithmetization’ of our science” (Klein, 1826: 56).[13]

As shown in “Annex B” table, it is still common to include Bolzano among the mathematicians who advocated for Weierstrass’ arithmetization of analysis, despite the fact that two centuries have passed since the publication of his *Purely Analytic Proof* in Prague. On the contrary, Cauchy’s *Course of Analysis* (*Cours d’Analyse*), published four years later in Paris, not only was a much better known work during the 19th century, but also since then has been more widely studied. As a consequence, according to the thesis here defended, both the general proposal and the particular results contained in Bolzano’s early works (1804-1817) continue to be misread: by 1817 Bolzano’s mathematical notions and practices, though they hinted some ground-breaking concerns and features, were heavily deviant from later Weierstrassian arithmetizing notions and practices, insofar as Bolzano’s work still had essential traits of a semantic-ontological conception of analysis.

In fact, Bolzano’s account as a transitional mathematician, that is, as one whose early works were aimed in some way towards the land on which the 19th century real analysis was developed, yet deeply rooted in inherited views and practices which were common among Germanic mathematicians around 1800, is a thesis closely linked to the one stated at the outset on the biased narrative of Germanic educational reforms of the early 19th century. Whether it is carefully studied what happened in Germanic mathematics around half a century after 1817 (up to c. 1872), as around half a century before 1804 (since c. 1750), Bolzano’s role as a transitional author is sustained. But, since the arrival point in this thesis is precisely the work of Bolzano from 1804-1817, the focus here lies in the previous generations of Germanic mathematicians and the links and differences between them and Bolzano. More specifically, the interest rests on the Germanic mathematicians of the second half of the 18th century, those who have generally been overlooked by mathematicians and historians and philosophers of mathematics.

[13] Klein wrote: “Diese Punkte wurden erst 1817 durch die Schrift von B. Bolzano 'Rein analytischer Beweis des Lehrsatzes, dass zwischen je zwey Werthen, die ein entgegengesetztes Resultat gewähren, wenigstens eine reelle Wurzel der Gleichung liege' (Prag, = Ostw. Klass. 153) erledigt, der damit auch über die späteren Entwicklungen von Cauchy hinausgegangen ist. Bolzano ist einer der Väter der eigentlichen 'Arithmetisierung' unserer Wissenschaft.”

Precisely, beyond the mathematical concepts and practices implicit in Bolzano's early mathematical works, his explicit mathematical sources reinforce two main –intertwined– theses of this work. That way, while until 1817 he primarily quoted Germanic (a vast majority) and French mathematicians, and to a lesser extent British and ancient Greek ones, among them three groups can be clearly defined according to their connection to a certain tradition or school: a) the Newtonian tradition (Newton and, for example, John Colson, Samuel Horsley, Thomas Simpson, William Sewell, John Landen); b) the French tradition (Euler and, for example, Simon Antoine Jean L'Huilier, Johann Castillon, Alexis Claude Clairaut, Jean-Baptiste le Rond d'Alembert, Jean-Baptiste d'Estienne du Bourguet, François Daviet de Foncenex, Pierre-Simon Laplace, Adrien-Marie Legendre, Sylvestre François Lacroix, Joseph-Louis Lagrange); and c) the so-called Hindenburg's combinatorial school (Hindenburg and, for example, Heinrich August Rothe, Georg Simon Klügel, Johann Friedrich Pfaff). However, while the references by Bolzano to authors of the first groups were mostly isolated ones,[14] his references to Hindenburg and Klügel, as well as to several Germanic mathematicians (mainly from the second half of the 18th century and the early 19th century),[15] highlight the importance of these mathematicians and especially of those immediately prior to him in the development of his early mathematical proposal.

Furthermore, there is something outstanding with regard to Bolzano's Germanic sources: along with Wolff (one of the most influential philosophers and mathematicians of the 18th century) and Kant (one of the most influential Germanic philosophers by the early 19th century), Bolzano referred mainly to mathematicians who for the most part were not followers of that philosopher, nor strictly had they been educated under the ideas of that mathematician. Undoubtedly, those Germanic authors were not stranger to the ideas and works of Wolff and Kant. However, their education was mediated by the works of other auhors. This depended on the region and epoch, but the key figures during that period were Kästner, Karsten and Segner, the math teachers at the leading universities of Göttingen and Halle who, along with their math

[14] As a matter of fact, most of them are only named in the list of authors included at the beginning of his 1816 work on the binomial theorem (cf. Bolzano, 1816: III-IV) or in the one included at the beginning of his *Purely Anlytic Proof* (cf. Bolzano, 1817B: 3 & 5).

[15] The Germanic mathematicians whose mention in Bolzano's early mathematical works is not an isolated one are: Christian Wolff, Abraham Kästner, Johann Schultz, Immanuel Kant (philosopher), Johann Andreas Christian Michelsen, Georg Simon Klügel, Carl Friedrich Hindenburg, Karl Christian von Langsdorf, Johann Heinrich Lambert, August Leopold Crelle, Johann Carl Friedrich Gauss, Johann Friedrich Gensichen, Ernst Platner and Ernst Gottfried Fischer. A list (of his Germanic sources) that is completed by: Lazarus Bendavid, Karl Friedrich August Grashof, Franz Anton Ritter von Spaun, (Joseph-Louis Lagrange), Jakob Hermann, Leonhard Cochius, Christian Gottlieb Selle, Franz Ulrich Theodor Aepinus, Johann Andreas von Segner, Karl Scherffer, Wenceslaus Johann Gustav Karsten, Johann Friedrich Pfaff, Heinrich August Rothe, Christian Lebrecht Rösling, Mathias Metternich, Friedrich Gottlieb von Busse, Christian Friedrich Kausler, János Pasquich, Friedrich Wilhelm Jungius, Karl Christian Friedrich Krause and Gottlob Nordmann.

teachers, did grow during the heyday of Wolff's influence, though they were not mere propagators of this latter's ideas.

It is true, on the one hand, that by then the best known works of Kästner, Karsten and Segner were their textbooks, as well as in some cases their translations of the works of foreign authors, and so their influence on the next generations had to do primarily with general conceptions and practices and not so much with particular mathematical results or developments. But, just as Bolzano did not merely repeat what he inherited, those three mathematicians did not either.[16] Moreover, just as the works of those three mathematicians were not merely "more popular and readable version[s] [of Wolff's work]" (Bullynck, 2006: 4), as it is commonly told, they gradually introduced significant modifications in their works, which increasingly drove them away from certain ideas of Wolff that were still present in this one's works of the 1740s.

The latter point is crucial to note, given the widespread idea about the state of stagnation of Germanic mathematics during the second half of the 18th century. While isolated mentions on some relevant aspects of the mathematical works of those three authors can be found all throughout the last century (cf. Cantor, 1904; Cajori, 1913 & 1923; Fellmann, 1983; Clewis, 2015), there are few studies focused –at least partially– on them. The exception to this is the excellent work of Gert Schubring (2005), as well as some other recent works, such as (Kleinert, 2002) and (Bullynck, 2006 & 2013). Because of its extensive and detailed study of the notions of quantity and number among French and Germanic mathematicians of the 18th century, the book of Schubring must be considered the main reference in the subject. Indeed, many of the authors whose work is discussed here in chapter B are studied by Schubring and thus the analysis offered here of those authors aims to contribute to his work and the other existing studies.

Schubring's investigation treats the development of mathematical analysis mostly in France and Germany, with special focus on the development of the concepts of negative numbers and infinitely small quantities throughout the 17th, 18th and 19th centuries. Just like we do here, he pays special attention to textbooks, since "they yield good indicators as to the intended conceptual horizon of a certain period and culture" (Schubring, 2005: 7). Bullynck, on the other

[16] The same can be said about Wolff but this goes beyond the scope of this work, so this footnote shall suffice to avoid the mistake that is being criticized here. Thus, throughout this work the ideas of Wolff that are mentioned are attributed to the specific year of the quoted work.

hand, is interested in the state of Germanic mathematics in the late 18th and early 19th century, "both on a level of elementary and higher education", and in “the forms of mediation and communication that accompanied and prepared the way for specialised mathematical journals” (Bullynck, 2006 & 2013). This dissertation shares with both works such interests and some methodological options, but the focus here is placed on the basic notions of numbers and quantities during the last part of the 18th century, in order to better understand the emergence of the pre-modern notion of number among Germanic mathematicians, particularly in the early work of Bolzano.

On the one hand, understanding the evolution of the mathematical ideas and practices of Kästner, Karsten and Segner in turn contributes to a better understanding of the Germanic mathematical panorama by the end of the 18th century and the beginning of the 19th century. The links between Kästner, the most influential of those mathematicians, and the so-called Hindenburg’s combinatorial school, as well as the links between this school and Lagrange, are not fortuitous, although ultimately the project of each of them was different, as different were some of their procedures and underlying notions. Whereas, on the other hand but coupled with that, understanding the state of things in Germanic mathematics in the early 19th century is of utmost importance for a careful examination of some key aspects of Bolzano’s mathematical works of 1804-1817.

Finally, once established the arrival (1817) and departure (c. 1750) times, the territory (the Germanic mathematics) and the way forward (from Göttingen and Halle to Prague), as well as explained the reasons for such decisions, last but not least, where to begin (geographically) this thesis on the Germanic development of the pre-modern notion of number? The work of the land surveyor is not only to delimit the territory and study it, but also to set a geographical point of departure suitable for the optimal completion of his work: “K. stood on the wooden bridge leading from the road to the village for a long time, looking up at what seemed to be a void” (Kafka, 2009: 5). As a consequence, and given the political situation in the Germanic territories during that period, starting point here will be a halfway territory between the Kingdom of Prussia (*Königreich Preußen*) and the Archduchy of Austria (*Erzherzogtum Österreich*), the two most important constituent parts of the Holy Roman Empire by that time (which then comprised most of the Germanic territories), namely, Silesia (*Schlesien*).

A. The Holy Roman Empire during the second half of the 18th century

A.1. The political situation in the Empire from c. 1740 to the late 18th century

Silesia (*Schlesien*) is a region nowadays located mostly in Polish territory, between Germany, the Czech Republic and Slovakia, whose control was strongly disputed by the Kingdom of Prussia – *Königreich Preußen*– and the Archduchy of Austria –*Erzherzogtum Österreich*– by the mid-18th century. The last of the conflicts in that period involving the region has different names in historiography, depending on the combatants on which focus is placed, even though it's commonly known as the Seven Years' War. A war that took place in Europe, America, West and Southeast Africa and North, South and Southeast Asia and lasted from 1754 to 1763. A broad and complex conflict that with regard to Europe is known mostly as the Third Silesian War, having been preceded by two similar periods of war in which Silesia was also at the heart of the dispute: the first from 1740 to 1742 and the second between 1744 and 1745, both in the context of the war of the Austrian succession.

That succession to the realms of the House of Habsburg was, in fact, one of the crucial events that in one way or another would shape the future of Europe during the subsequent decades. By the mid-18thcentury, the control that the House of Habsburg had had for about 3 centuries of the throne of the Holy Roman Empire was threatened by the lack of a male heir. Leopold I had reached an agreement –*actum mutuae successionis*– in 1703 with his two sons, Joseph I and Karl VI, according to which, not having neither a son, nor having another one in the case of Leopold, their daughters would be able to inherit, precedence being given to the daughters of the first. But once in the throne after his brother's death, Karl issued a decree in 1713 by which he gave priority to his own descendants (Whaley, 2012 II: 158).

Various agreements were carried out during the following decades to ensure compliance of that decree, both with Estates of the Habsburg lands and Estates of the Reich lands, and with other

Empires, such as Spanish and Russian, as well as with the Kingdom of Prussia; agreements, some of them, that the House of Habsburg breached during the subsequent years. So while new agreements were conducted in order to preserve the dominance of a royal house whose territories and interests went beyond those of the Reich, a) “legal and institutional mechanisms were designed to keep noble families afloat”, thus ensuring their dependence (Whaley, 2012 II: 208); b) the variety of institutional forms became increasingly complex with States containing up to “five chambers with clergy, upper nobility, universities, knights, and towns represented separately” (Whaley, 2012 II: 241); c) peasants' security of tenure was increased “despite the restrictions on personal liberty, [...] the harsh treatment suffered by many [...] and the onerous fees, dues, and labour services imposed on many” (Whaley, 2012 II: 253-254); and d) while “Catholicism successfully contained popular religiosity”, its conflictive coexistence with Protestantism gradually involved a greater cooperation between them (Whaley, 2012 II: 307 & 324).

Nevertheless, Maria Theresia’s accession as ruler of the domains of the House of Habsburg after her father’s death in late 1740 was followed by the invasion of Silesia by the army of the Kingdom of Prussia, as well as the claims to other Habsburg territories launched by some Electorates of the Hole Roman Empire and by other Kingdoms (Whaley, 2012 II: 352-355). The conflict officially lasted less than a couple of years although the signing of the Treaty of Berlin between Austria and Prussia by mid-1742 did not lead to peace in some of the fronts such as Bavaria, the Electorate of Karl Albrecht, son-in-law of Joseph I and by then Holy Roman Emperor with the support of the French and Spanish Kingdoms (Bryce, 1901: 354).

Crowned in Frankfurt in early 1742 and a refugee there since then, Karl VII attempts to recover Bavaria as well as French interests in the conflict led to the latter’s formal declaration of war against Austria and Britain and eventually to the signing of the “Frankfurt Union of 22 May 1744, by which Prussia, the Palatinate, and Hessen agreed to support the beleaguered emperor” (Whaley, 2012 II: 355). This new conflict also lasted less than 2 years, signed the Treaty of Dresden in December 1745: Austria recognized the Prussian possession of Silesia and Prussia recognized the election of Francis Stephen –Maria Theresia’s consort– as Francis I, Holy Roman Emperor who succeeded Karl VII after his death early that year (cf. Whaley, 2012 II: 356).

During the following years the war against the French and Spanish kingdoms continued in Italy, the Austrian Netherlands, Canada and India, all of them lands outside the Reich. This series of conflicts, whose main adversaries were France –along with Spain, the Duchy of Modena and Reggio and the Republic of Genoa– and Britain –along with the Dutch Republic, Austria, the Kingdom of Sardinia and, passively, Russia–, ended in 1748 with the signing of the Treaty of Aix-la-Chapelle; a peace that, though feeble, lasted about ten years, until the beginning of the aforementioned Seven Years' War.

Although that new war strictly began in 1754, with clashes between the French and British settlements in North America and the consequent reactions of both kingdoms from 1755, in Europe the conflict was only developed two years later when Prussia formally allied with Britain and, in reaction, Austria and France reached an agreement known as the first *Treaty of Versailles* which was amended twice during the next couple of years (Whaley, 2012 II: 358-59). As stated before, this broad and complex conflict lasted 7 years in Europe during which: Prussia invaded Saxony; Russia joined the Austro-French coalition, followed by the Kingdom of Sweden, the Kingdom of Poland and the Grand Duchy of Lithuania; Saxony joined the anti-Prussian coalition, followed by other Germanic states such as the Palatinate and Bavaria, which led to the Reich's involvement (Burkhardt, 2012: 117); Spain intervened due to its *Pacte de Famille* with France and both invaded Portugal (ally of Great Britain); some Germanic states intervened against the Austro-French coalition, such as the Electorate of Hanover (a Germanic territory of King Georg II of Great Britain), the Principality of Brunswick-Wolfenbüttel, the Landgraviate of Hesse-Kassel, the County of Schaumburg-Lippe-Bückeburg and the Duchy of Saxe-Gotha-Altenburg (Luh, 2012: 17); "France was soon distracted by major losses in Canada and progressively withdrew her support for any action in the Reich"; "Britain too lost interest in the war after her victories against France" (Whaley, 2012 II: 360); Georg II died (in 1760), in the middle of an "increasing public disquiet at the continuation of the war and the rising costs of supporting Hanover and Prussia", and was succeeded by George III, his grandson, who focused more on the American colonies than on Hanover and the Reich (Harding, 2012: 316; Whaley, 2012 II: 360); Empress Elizabeth of Russia died, in January 1762, succeeded by her nephew Peter III, pro-Prussian Emperor who "renounced its treaty with Austria and agreed to vacate Prussian territory" under the *Treaty of Saint Petersburg* in March that year, just before his wife's –Catherine II or Catherine the Great– coup, his subsequent death in July, her coronation in

September and the revocation of the alliance between Russia and Prussia (Schumann, 2012: 512; Speelman, 2012: 520); Sweden, having considered to get out of the war since 1758, signed the peace with Prussia the same year Russia did (Åselius, 2012: 162); and, finally, Great Britain and France, as well as Austria and Prussia, began negotiating a peace agreement in 1762 which was formalized by the *Treaty of Paris* and the *Treaty of Hubertusburg* in 1763 (Whaley, 2012 II: 361-62).

By 1763, therefore, the Prussian possession of Silesia was confirmed and remained so until the abdication of Wilhelm II resulting in the abolition of the Kingdom of Prussia and the demise of the German Empire in 1918. To some extent, there was some peace in Europe during the following decades till the late 18th century. As Whaley indicates, "from 1763, both Austria and Prussia, and the Reich as a whole, were embedded in an international constellation that also guaranteed stability" (Whaley, 2012 II: 350). Nonetheless, as Schumann points out, "a variety of strategic imponderables affected military orders and diplomatic initiatives far outside the scope of the peace negotiations" (Schumann, 2012: 518). Among the military conflicts that occurred in Europe and involved at least one of its five "great powers" (i.e. Austria, Prussia, Russia, France and Britain), prior to the French Revolution and those conflicts known as the French Revolutionary Wars, most of them involved Russia (with Circassia; the Bar Confederation and as a consequence Austria, Prussia and France; the Ottoman Empire; Chechen forces; Sweden; the Commonwealth of Poland; and popular revolts as for example Pugachev's Rebellion) and only some of them directly involved the Reich, such as the Revolt of Horea, Cloșca and Crișan, the War of the Bavarian Succession (from 1778 to 1779), the Kettle War against the Republic of the Seven Netherlands of 1784, the Austro-Turkish War of 1787 and the Saxon Peasants' Revolt of 1790.

That way, such relative and regional peace, coupled with the continuity of the main European leaders between 1763 and 1792 and the development of the Enlightenment, as a matter of fact in some cases made possible the establishment of several important reforms, from financial to educational. So, while during that period Great Britain and France (under the reigns of George III and Louis XV and Louis XVI, respectively) experienced a few of those changes, Austria –and therefore the Holy Roman Empire–, Russia and Prussia experienced a number of them.

A.2. The Germanic educational reforms from c. 1760 to the late 18th century

In the beginning was the torpor. Not Napoleon (Nipperdey, 1983) nor the Reich (Winkler, 2000) or the absence of a revolution (Wehler, 1987), but as Friedrich Paulsen wrote in the early 20th century, in the beginning of the 19th century was "the torpor into which [the Austrian universities] had relapsed after the great reforms of the eighteenth century under Maria Theresia and Joseph II" (Paulsen, 1906: 51). However, while Paulsen's assertion about the Austrian universities is basically true, since with Franz II (the last Holy Roman Emperor and the first Austrian Emperor as Franz I) "many of [those] reforms were revoked and the spirit of Enlightenment almost vanished, replaced by the catholic Restoration" (Sebestik, 2014: 293), the same cannot be said of all the Germanic universities. Which means that just as it is not true that concerning education, university research and scientific development, practically nothing happened in the Germanic territories before the foundation of the *Universität zu Berlin* (or *Friedrich-Wilhelms-Universität*) in 1810, neither it is true that practically nothing happened in that territories after King's Leopold II death in 1792. In other words, it is false a) that the Germanic model of modern university –developed during the 19th century– was founded with Wilhelm Von Humboldt and the *Universität zu Berlin* (nowadays the *Humboldt-Universität zu Berlin*) and b) that before them the Germanic universities, i.e. the Germanic universities of the 18th century and early 19th, were institutions "generally ossified and decayed" (Nipperdey, 1983: 57), "in poor shape" and "stagnation" (Howard, 2006: 80-81).

Regardless of all what is meant by "Germanic", such assertions are unsustainable even if only considered in terms of language and territory. While in 1815 the German Confederation (*Deutscher Bund*) was created and during subsequent decades there was a certain unity –at least economically– of the German-speaking states, the situation was quite different in 1810. When founded the University of Berlin by Prussia, the universities in German-speaking places were mainly located in Prussia, Austria and the Confederated States of the Rhine (*États confédérés du Rhin*), a confederation of Germanic clients of the French Empire promoted by Napoleon in 1806 that lasted until 1813 (Whaley, 2012 II: 637-38). As a result, in 1810 not only there was a lack of union of German-speaking states, but, on the contrary, there was a union of some German-speaking states that for different reasons supported the French Empire against the other German-speaking states. Furthermore, as a result of the French Revolution (1789) and especially since the French Revolutionary Wars (1792), several universities located in German-speaking states were closed and although some of them were reopened, no university was inaugurated in those territories prior to the one in Berlin.

Accordingly, those who defend the thesis that the Germanic model of modern university was founded with Humboldt and the University of Berlin (hereinafter in this section "thesis A") may either refer to: a) the universities of the upcoming German Confederation of 1815 or 1820 (when the treaty to create it was concluded);[17] b) the universities of Prussia (under its control or in its territories) by 1810;[18] c) the universities under *de facto* control of the existing Germanic states by 1810;[19] d) or the universities located in territories of the existing Germanic states by 1810 (or even better by 1820);[20] e) the German-speaking universities and the ones located in German-speaking territories by 1810 or 1820;[21] f) the universities in territories of the existing Germanic states by 1820 that were part of the German Confederation by 1820 and were also part of the German Empire by 1871;[22] or g) merging the two previous aternatives, the universities located in German-speaking territories that were part of the German Confederation by 1820 and were also part of the German Empire by 1871.

[17] This would rule out Königsberg, the place where Humboldt formulated an early version of his plan to reform the Prussian educational system (cf. Humboldt, 1809 & 1809-1810), which was not the case in terms of budget (cf. Jungnickel & McCormmach, 1990: 226), constructions (being inaugurated its botanical garden and observatory between 1811 and 1813) and academic activity (Clark, 2006: 168, 177 & 448ff.), while at the same time would include Austrian universities despite the fact that with Leopold II – and, as stated before, with Franz I– "censorship regulations were strengthened after 1789" (Agnew, 2004: 97).

[18] That would rule out Paderborn, Münster, Halle and Bonn (a university that, founded in 1818, was a project of King Frederick William III of Prussia in the same vein as the universities of Berlin and Breslau), as well as all the universities located in the territories of the former Confederated States of the Rhine and some of the ones located north of future German Empire of 1871. In other words, that would mean Berlin's model is considered the "Germanic model of modern university" on the basis of its influence on a couple of Germanic universities during the 19th century (considering that Duisburg was closed 8 years later).

[19] That would rule out the universities located in the territories of the former Confederated States of the Rhine and some of the universities located northeast of future German Empire of 1871 (in both cases under French *de facto* control), but not Breslau, Duisburg, Königsberg, Prague, Cracow, Vienna, Olomouc, Graz, Pécs, Budapest, Košice, Lviv and Eperjes (under Prussian or Austrian or control). That way, Berlin's model would be considered the "Germanic model of modern university" on the basis of its influence during the 19th century on 2 Germanic universities and 10 universities in territories that by 1871 were no longer Germanic.

[20] In the first case, the universities affected by the new model would have been the ones in Prussia, Austria, Holstein and in the Confederated States of the Rhine but not the ones in some of the French north-east Departments (including Münster, Bonn and Mainz); in the second case, the ones in Prussia, Austria, Schleswig-Holstein, the former Confederated States of the Rhine and in some of the French north-east Departments. Nonetheless, the sole inclusion of the Austrian universities makes the thesis untenable.

[21] This would mean that the universities affected by the new model would have been the ones located in the territories of the member states of the German Confederation, since German was the language of central and local administrations in the Germanic states at least until the mid-19th century (Taylor, 1948: 23), but also some others like Basel in the Swiss Confederation (Zurich and Bern being founded in 1833-1834) and Tartu –Dorpat– in the Russian Empire (http://www.ut.ee/en/university/general/history).

[22] Under this position the universities affected by Berlin's model would have been the ones located in territories of the German Confederation, except those of Austria and Luxembourg, plus the eastern territories of Prussia (including Königsberg).

Leaving aside the last version of thesis A, all other are quite problematic and make untenable the thesis A. However, it seems that in fact version "g" accounts for the idea that those who defend the aforementioned thesis want to express, although it does not explain the basis for naming "Germanic" the model supposedly devised by Humboldt and implemented for the first time in Berlin. After all, if the Germanic model of modern university was inaugurated in Berlin (part of Prussia) and the universities affected by it were those that meet the conditions set by version "g" of thesis A, this basically means that model would have affected the Prussian universities (around 12) along with the universities located throughout 25 of the other 26 (without the Imperial Territory of Alsace-Lorraine) constituent territories (around 11).[23] In other words, if that version of thesis A correctly expresses its sense, it means that the modern university model called "Germanic" is the one that, designed in Prussia, should have affected the 19th century universities of Prussia and the other German constituent territories of the German Empire.

Understood thesis A in that way, why then call "Germanic" the model to which it refers instead of calling it "Prussian"?[24] The answer to this question seems related to the response to other questions such as: To what extent did Prussian model implemented in Berlin affect German universities, i.e. universities located in territories that were both part of the German Confederation and Empire, during the 19th century? To what extent it even affected Prussian universities during that century? Did the essential features of that model were really implemented for the very first time in Berlin? Was it really Humboldt who devised the Prussian model of modern university? All of them important questions here insofar as they lead to "thesis B", namely, that Germanic universities of the 18th century and early 19th were institutions "generally ossified and decayed", "in poor shape" and "stagnation".

[23] In Prussia: Paderborn (which became a seminar in 1819), Bamberg (a Lyceum since the beginning of 19th century), Duisburg (merged with Bonn in 1818), Breslau, Halle, Bonn, Greifswald, Königsberg, Marburg, Göttingen, Münster (renamed Academy in 1843), Kiel (part of Prussia since 1867), Hannover (a College/Polytechnic Institute granted the status of a university by the end of the 19th century), the *Königlich Technische Hochschule Charlottenburg* (founded in 1879 in Berlin after the merger of the *Bauakademie* and the *Königliche Gewerbeakademie*) and, of course, Berlin itself. In the other 25 constituent territories: Mainz (by the 19th century a Seminary and since 1877 a College in Hesse); Rostock (in Grand Duchy of Mecklenburg-Schwerin); Giessen (in Hesse), Freiburg and Heidelberg in Baden; Tübingen, Hohenheim (founded in 1818) and Stuttgart (founded in 1829) in Württemberg; Munich (since 1826, previously located in Ingolstadt and Landshut), Erlangen and Würzburg in Bavaria; Leipzig (in Saxony); and Jena (in Saxe-Weimar-Eisenach).

[24] Steven Turner, for example, does refer to the "Prussian model" in his works: "The Growth of Professorial Research in Prussia, 1818 to 1848-Causes and Context" (Turner, 1971), "The Prussian professoriate and the research imperative, 1790-1840" (Turner, 1981), and "Justus Liebig versus Prussian Chemistry: Reflections on Early Institute-Building in Germany" (Turner, 1982).

From what was explained above it is clear that from 1815-1820 there was some union of Germanic states that despite the vicissitudes –and its division in 1866–, when reconstituted in late 1870 it included practically all the Germanic territories of modern Germany. However, it is also clear that during the last decade of the 18th century and the first decade of the 19th century the state of Europe was quite unstable and just as territories' possession or control was constantly changing, the possession or control of some Germanic universities changed during that period and not only no Germanic university was inaugurated but rather some of them were closed, while the status of others was modified.

While that might be reason enough to consider the Germanic universities as institutions in decay over those years, what the general approach represented by authors like Nipperdey and Howard means is somewhat different. According to that consensus, the stagnation of the Germanic universities is not constrain to the last years of the 18th century, as it was not the result of that kind of external factors but primarily of inherent factors to their constitution and operation: as summarized by Howard, "their problems [...] included financial mismanagement, curricular stagnation, professorial pedantry, a decline in matriculation numbers, and a notoriously coarse and unruly student subculture that venerated drinking and duelling" (Howard, 2006: 47; cf. Nipperdey, 1983: 57).

The problem with thesis B is that it does not match –or only partially matches– certain relevant facts, nor with the historiography of late 19th century and early 20th century, whereas it entirely coincides with the predominant historiography during the 20th century which itself has widely spread "thesis A". As some authors have pointed out over the last couple of decades, both thesis seem to date back to the beginning of the 20th century: vom Bruch has pointed out the participation of other authors rather than Humboldt in the conception of Prussian model of modern university (vom Bruch, 2001); Walter Rüegg has drawn attention to the central role played by Schleiermacher in that model's development (Rüegg, 2004); Sylvia Paletschek has shown that during the 19th century Humboldt was not known as a university reformer, remaining virtually unknown –or unpublished– his programmatic texts (Paletschek, 2001); and Mitchell G. Ash has highlighted some difficulties of those thesis concerning both the association of Humboldt to the model as well as some of its essential features (Ash, 2006).

As a matter of fact, some of the characteristics of the Prussian model of modern university were not implemented for the very first time at Berlin's university and neither they were devised by Humboldt; some of them, contrary to thesis B, were initially implemented during the 18th century. However, account should be taken that those characteristics were not the exclusive or even direct result of reforms undertaken in the 18th century by Austria, Russia and Prussia but the result of a reform process that involved other kind of actors, such as "German ministers of state and avatars of the market" (Clark, 2006: 3). Undoubtedly that process was favored by some of those reforms, but above all what made it possible were other factors as the ones previously mentioned, videlicet the development of the Enlightenment, the relative and regional peace and the continuity of the main European leaders from 1763 to 1792.

Under the reigns of Friedrich II (1740-86) and Friedrich Wilhelm II (1786-97), for example, Prussia experienced some significant improvements in education mainly due to the *Königlich Preußische Generallandschulreglement* (the General School Regulations of 1763 and 1765) and the *Oberschulkollegium* (a Supreme School Council set in 1787), which in a way shaped the Kingdom's educational development over the last decades of 18th century second half. Both that new ministry and those regulations, nevertheless, were instruments capable of doing that because of social, political and economic conditions as the ones aforementioned, but also because of the several reforms instituted during those years, including the *Landratsreform* of 1766 (*Landratsinstruktion*, that is, the instruction concerning the District Administrators, "the lowest tier of the Prussian bureaucracy in the countryside" (Melton, 2002: 157)), the administrative reforms of the 1770s (which included the requirement to pass a state examination for civil service posts administered by a special commission (cf. Mueller, 1974: 191ff.; Mueller, 1981: 187; Schulze, 1999: 66)) and, last but not least, the *Allgemeines Landrecht für die Preußischen Staaten* (the General State Laws for the Prussian States commissioned since 1780 and promulgated in 1794) (cf. Fulbrook, 2004: 94).

Thus, on the one hand, the Prussian General School Regulations of the 1760s urged parents, legal guardians and rulers or authorities responsible for the education of the youth, to send children to school at the age of five at the latest and to keep them there until age 13-14 (Königlich-Preußisches, 1763: §1). However, despite being reinforced in 1765 by provisions concerning the structure and operation of the schools, as well as the studies content (Königlich-

Preußisches, 1765), those state regulations implied financial subsidy for public school but delegated the education delivery. That way, both regulations were not entirely effective during the 1760s and 1770s, partly due to the latter, in part because of the non-compulsory school attendance until 1794, the incipient instruction of bureaucracy and examination of civil servants, the initial lack of certain instruments gradually implemented throughout the following years, but also to some extent due to the introduction of a new curriculum for Gymnasiums (secondary schools) until 1781, which "to the usual staple studies of Latin, Greek and religion, added instruction in the vernacular, French, history, geography, mathematics and drawing" (Adamson, 1919: 237).

On the other hand, the creation of the Supreme School Council in 1787 as an instrument responsible for the supervision of Gymnasiums and universities education was a first step towards their control by the State. Even more, its supervision of the "appointment of all school teachers and all university instructors and professors in Prussia" (Clark, 2006: 441), further enhanced by mechanisms that guarantee the education of new generations (both at secondary level and university level), represented the first steps towards the modern university model. Thereby, among the factors that not only preceded but made possible the Prussian model of modern university can be mentioned: the university entrance examination set up in 1788 (Clark, 2006: 124ff.); the increasing development of pedagogical seminars for the training of teachers (Melton, 2002: 49ff.; Howard, 2006: 98-99), whose roots date back to mid-16th century (Clark, 2006: 159); the gradual transformation of philology seminars into research seminars since the 1760s (Kruse, 2006: 337-340; cf. Clark, 2006: 172ff.); the general establishment of a written examination or doctoral dissertation as a requirement for graduating from university and its constitution as a research work (cf. Clark, 2006: 211ff.), linked to a regulation of 1749 according to which at least 2 disputation-dissertations were required to be a lecturer and some more publications to be an extraordinary and ordinary professor (Clark, 2006: 259-260); the development of other instruments such as publications, laboratories, societies and Academies, the slow academic acceptance of "minorities" (such as women and Jews (Clark, 2006: 200)) and the fact that, in a sense, "by the mid-eighteenth century, a degree candidate had been effectively dematerialized, disembodied, and spiritualized as pure intellectual capacity" (Clark, 2006: 237). A model, it must be said, in which the philosophical system of Christian Wolff (who, recommended to the ministry by Leibniz, went to Halle to teach mathematics from 1706 to

1723, when he was ousted (cf. Clark, 2006: 269-272; Howard, 2006: 95-96)) played a crucial role after its "royal legitimation" in 1740 (Howard, 2006: 98), when he was restored by Friedrich II: it indicated the beginning of the end of scholastic philosophy, whose place was to be taken by a modern philosophy that not only denied philosophy's dependence upon theology, but above all was based on reason as all –rigorous– scientific knowledge (Paulsen, 1906: 45; cf. Clark, 2006: 283-286; Howard, 2006: 97-98).

Simultaneously, Austria and Russia also experienced significant improvements in education. Under the reign of Maria Theresia (1740-80) –and Joseph II (1764-90)–, the former, as noted by Taylor, translated the legal unit of the Habsburg lands achieved by Karl VI into practice (Taylor, 1948: 16), for which the *Böhmisch-Osterreichische Hofkanzley* (Bohemian-Austrian Court Chancellery) and the *Staatsrat* (State Council) were set in 1760 (Levy, 1988: 149). That way, the educational reforms introduced during the previous years, from the secularization and expansion of the university of Vienna in 1749 (Gnant, 2015: 87), to the establishment of some institutions "to prepare officials for the future state bureaucracy" in the late 1740s and 1750s (Beller, 2006: 91), were followed by new ones that were mainly the result of recommendations issued by the *Studien und Bücher-Zensur Hofkommission* (Commission for study and books-censorship) also established in 1760: in 1770-71 it was proposed to replace Jesuit teachers by "state-funded lay teachers" (Armour, 2012: 47; cf. Melton, 2002: 207), which was done in 1773 –with "the active co-operation of the clergy and some orders" (Holborn, 1982: 224)– after pope's suppression of the Society of Jesus; a year after the state assumed control of schools, the *Allgemeine Schulordnung* (General School Ordinance) was issued, making school attendance compulsory for all children between 6 and 12 years old (Melton, 2002: 47) and transforming the curriculum of schools and Gymnasia, which "began to emphasize such subjects as mathematics, history, geography, and German" (Holborn, 1982: 224); three types of schools were established including normal school as state certification body of tutors and school teachers (Melton, 2002: 213) whose examination was "probably modeled [...] on the French *concours*" (Clark, 2006: 245); a General School Ordinance (*Ratio educationis*) for the Kingdom of Hungary was created in 1777 (Melton, 2002: 226); "orthodox Aristotelian-Thomistic philosophy [...] was replaced with Wolff's and Leibniz's 'popular' philosophy, which was to dominate universities in the Habsburg Empire for some decades" (Wilfing, 2015: 27); publications and dissertations emerged in different languages (Evans, 2006: 136); universities were open, reopened or transformed; and, finally, in 1784 the German was decreed as "the language of state and administration" (with the exception of Belgian, Italian and Galician officials) but also the language of instruction "in all educational institutions above elementary level" instead of Latin (Armour, 2012: 53).[25]

Meanwhile in Russia, under the reign of Empress Elizabeth (1741-62) the University of Moscow was established in 1755 and a decree was issued in 1758 "insisting that potential tutors should first be examined by the Academy of Sciences in St. Petersburg and the University of Moscow to pronounce on their fitness to teach" (De Madariaga, 1979: 373). In a sense, therefore, Elizabeth continued the course started by her father [Peter III]" (Fedotova & Chigisheva, 2010: 83) and it was only until the reign of Catherine II (1762-96) when more significant reforms were implemented: in 1763 she appointed I. I. Betskoy as her main adviser on educational matters and then formed a Commission to study the previous Russian reforms, which "submitted its own recommendations for the establishment of a general system of education for all Russian orthodox subjects from the age five-six to eighteen" (De Madariaga, 1979: 373-74); in 1767 "Russian began to be used in the university" (Riasanovsky, 2000: 352); in 1768 a Sub-Commission began to work concerning primary, secondary and higher education (De Madariaga, 1979: 375-76); in 1775 was decreed the Statute on Local Government and with it "the obligation to establish schools at [provincial] level was laid on the Boards of Social Welfare" (De Madariaga, 1979: 380); a new Commission "was set up [...] in 1782 to study the various models of educational systems", the Austrian three-tier model being recommended by Aepinus (De Madariaga, 1979: 383); the same year, a Commission of National Schools was set up to organize "a national school network, [train] the teachers and [provide] the textbooks" (De Madariaga, 1979: 383-84); in 1783 a school for training teachers was opened and during the subsequent years foreign textbooks were translated, "the Commission extended its control over all existing private schools" and private and boarding schools' teachers were examined (De Madariaga, 1979: 384); measures were taken "to provide Russia with a scientific and research base outside as well as inside the Academy", such as the issue of an edict in 1783 that granted the private right to print (until 1796, in the light of French Revolution (De Madariaga, 1998: 270)) and the

[25] Nonetheless, the Germanic model of Göttingen was rejected both in 1773 and 1785: "A university commission rejected a proposal of 13 February 1773 to send Austrian students to study at Göttingen and return as future lecturers and professors. The commission rejected, in fact, the Göttingen academic model. Later, in 1785, an important Austrian, namely Swieten, received a work called *Vorschlag eines Unbekannten über Verbesserung des Universitäts-Wesen* from the emperor, who asked for Swieten's view. Concerning the suggestion again of imitating Göttingen, the minister responded that that university did not work for 'national education'" (Clark, 2006: 378).

establishment in 1765 of the Free Economic Society (Hosking, 2001: 117-18); and, finally, in 1786 "the Russian Statute of National Education was promulgated", which regulated the subjects to be taught (including geometry, mechanics and physics for high school) (De Madariaga, 1979: 385).[26]

The case of Russia in turn accounts for the emergent conception of education as a system (De Madariaga, 1979: 382, fn. 47) and highlights the educational systems considered at the time as role models in Europe, namely the Prussian and Austrian. It is true that while the Sub-Commission of 1768 studied the system of Prussia and the models of English universities and Irish schools (De Madariaga, 1979: 376), the Russian Statute "may have also been influenced by the Polish Commission of National Education, set up in 1773" (De Madariaga, 1979: 382, fn. 48). But, in the end, it was the Austrian system the one that the Commission of 1782 used as a model and it was Joseph II to whom Catherine II requested an adviser who, as soon as he arrived, acted as a member of the newly established Commission of National Schools (De Madariaga, 1979: 383).

Furthermore, that the Prussian and Austrian educational systems were the role models in Europe by the second half of the 18th century and not others, such as the French or British, can be considered conclusively proved by Diderot's own testimony in his *Essai sur les études en Russie*, which he sent to Catherine II in 1773 and which seems to have been succeeded by his *Plan d'une université pour le gouvernement de Russie ou d'une éducation publique dans toutes les sciences*, sent to her three years later (Diderot, 2013: 413; cf. De Madariaga, 1979: 380, fn. 40). There, Diderot prepends the Germanic reflection on education and the status of this latter to those in France, prepending also what was done about it by Protestants rather than by Catholics (Diderot, 2013: 415-428; cf. De Madariaga, 1979: 380). After all, significant reforms in educational matters were just about to be carried out in France in the late 18th century, as well as in Prussia the educational reforms were about to begin to consolidate, coupled with the consolidation of Kingdom of Prussia itself throughout the 19th century.

To conclude this section, however, it is worth saying something about British education during the second half of the 18th century, not because of its consideration by the Russian Sub-

[26] For an analysis of the limited effectiveness of Catherine's II reforms, cf. (De Madariaga, 1979: 393-395).

Commission of 1768, but due to the Germanic territories linked to it. Indeed, while no British educational system was developed during that period and most of elementary education was run by religious institutions –and therefore emphasis was made on religious instruction– (cf. Jewell, 1998: 96-97), the situation of British universities and Societies was quite different. Thus, in addition to Great Britain's universities and Societies (Oxford, Cambridge, St. Andrews, Glasgow, Aberdeen, King's College, Edinburgh, Marischal College, the Royal Society of London and the Royal Society of Edinburgh),[27] the Irish university and Society (Dublin and Dublin Society) and especially the university of Hanover, namely Göttingen (founded in 1734 as *Georg-August-Universität Göttingen*), must be considered, since George III, King of Great Britain (1760-1820), was as well King of Ireland and Duke and prince-elector of Brunswick-Lüneburg (Hanover).

Beyond the structural differences between British –and Scottish and Irish– and Germanic universities, being the first collegiate and the second professorial ones (Clark, 2006: 16), the difference between the university of Göttingen and all the other universities in European territories controlled by the King of Great Britain was primarily qualitative. There were undoubtedly certain qualitative distinctions among European non-continental British universities during the second half of the 18th century: for example, in Cambridge priority was given to mathematics on "traditional topics of philosophy, classics, and religion", a written examination (in English and no longer in Latin by 1772 and later extended in duration and number of moderators) "and the fetish of marking and ranking" emerged (Clark, 2006: 115), in spite of which Scottish universities enjoyed a better reputation (Hammerstein, 1996: 140; cf. Kerr, 1910).

Nonetheless, taken those differences for granted, the fact is that Göttingen a) belonged to the Electorate of Brunswick-Lüneburg which in turn belonged to the Holy Roman Empire and b) it was ruled by George III, whose position about "that horrid Electorate which has always lived on the very vitals of this poor country" was clear even before his famous accession speech to Parliament in which he declared: "Born and educated in this country, I glory in the name of Britain" (Thomas, 2002: 33). Unlike his predecessor (George II, founder of the university and the

[27] The Kingdom of Scotland united the Kingdom of England in 1707. The Scottish University of Strathclyde is not listed above since it was founded in 1796.

Königliche Gesellschaft der Wissenschaften (Göttingen Academy of Sciences)), therefore, George III had little or nothing to do with the development of that university which, according to Paulsen, was one of the two that "opened the doors of the German university to modern philosophy and science, as well as to modern enlightenment and culture", "ultimately [surpassing Halle]" (Paulsen, 1906: 44 & 47). Put another way, during that period and being –exclusively– in charge of the Privy Council of the Electorate, Göttingen not only stood out amongst British universities but also among the rest of the Germanic universities, being even the model for many of the Catholic non-Austrian universities that "after the temporary suppression of the Jesuits in 1773 [...] began, as they put it, to enlighten themselves" (Clark, 2006: 63).

Precisely, it was under the guidance of Gerlach Adolph Baron of Münchhausen, who had worked on the project of Göttingen's foundation since 1731 and was Minister of the Privy Council from 1727 to 1770 (first as responsible of educational and religious matters, then as responsible of finance and, from 1765, as head Minister of it) (Brosius, 1997: 523-24; cf. Howard, 2006: 105), that *Georg-August-Universität Göttingen* became the model of modern Germanic university;[28] modernity that, as pointed out by Howard, "is attributable to the circumstances of its founding, its statutes, and the progressive scholarly views of its professors, many of whom had previously studied at Halle" (Howard, 2006: 104). Münchhausen, in fact, regarded Halle as a model but at the same time sought to avoid this latter's pietistic orientation (Brosius, 1997: 523-24), so that, on the one hand, relevant administrative and material improvements were made and, on the other hand, important academic innovations were carried out.

Concerning the first kind of those improvements, scientific research was considered only subject to censorship by the Privy Council –and not the church– (*ibid.*; Howard, 2006: 109-110); the library, opened in 1737 and "the most modern among the modern" with three main catalogues, was constantly enhanced (as well as equipped), while open to the students (Clark, 2006: 247 & 316ff.); in 1751 the first university Observatory was founded (cf. Forbes, 1974); since 1756, the university lecture catalogue was structured "by subjects and disciplines", apparently "to

[28] Johann Lorenz von Mosheim, adviser of Münchhausen, played a key role in the development of the project: "Two years before the inauguration of the university and the formal codification of the statutes, Mosheim produced for Münchhausen several memoranda on the soon-to-be-established university. [...] These proposals from Mosheim and the lively correspondence between the two men make up an unofficial intellectual charter of the new university. While many of Mosheim's points did not make their way, verbatim, into the final statutes, their spirit thoroughly permeated the new institution" (Howard, 2006: 107-108).

facilitate ministerial paperwork" (Clark, 2006: 54-55); collaboration with the Academy of Sciences was established (cf. Howard, 2006: 112); free meals and scholarships were offered by the Göttingen seminar (Clark, 2006: 167); the university acquired the ethnographic collections of James Cook (gathered on his journeys to the Southern Pacific) and Georg Forster (who, along with his father, took part on Cook's second circumnavigation of the world), and received the donation of Baron Georg Thomas von Asch "unique ethnographica from Siberia and the Northern Pacific" (Böhme); academic acts began to be organized in dossiers (Clark, 2006: 248); nomination and appointment of the Faculty members became competence of the state, becoming the professor "privy councilor to the king" (Howard, 2006: 107); new professorships were created, such as Lichtenberg's professorship on experimental physics; and, finally, around 1763 the Council gave autonomy to the director of the seminar (by then Heyne) "to modify the seminar's structure to his own liking [and] granted him the further power to select the candidates for admission" (Clark, 2006: 166).[29]

As for the second class of innovations, renowned professors were systematically hired since, as Münchhausen wrote in a memorandum prior to the university foundation, "it [was] necessary that if the new academy should excel, its chairs must be entrusted only to the most famous and qualified men" (Howard, 2006: 107); professors wrote textbooks for their lectures (Clark, 2006, 85); the seminar for classical philology founded in 1738 gradually became the first Germanic institutional research seminar (cf. Clark, 2006: 142, 159 & 172);[30] Göttingen's review journal, the *Göttingische Zeitung* (later *Göttinger gelehrten Anzeigen*) first published in 1739, became an essential instrument for the growth and improvement of the university library and for the work at the university itself, first under the direction of Haller since 1747 and, from 1770, with Heyne as editor (cf. Clark, 2006: 323-26); and, finally, the theology faculty importance and power was attenuated in behalf of the law and philosophy faculties (Howard, 2006: 106-110). As summarized by Paulsen, throughout the second half of the 18th century Göttingen erected itself as the best and most modern Germanic university:

[29] Despite the 1736 university statutes already contemplated the "proclamation of the doctors of philosophy", the degree by then was still called the "master's degree, [...] the highest degree in philosophy", being only until many decades later when the new grade was consolidated (Clark, 2006: 194ff.).

[30] In fact, "in 1776/77 the director, Heyne, tried prevent the young student F. A. Wolf—later founding director of the seminar in Halle—from registering in the philosophy faculty as *philologiae studiosus.* Heyne entreated him instead, in his own best interests, to matriculate in the theology faculty. Wolf, however, thought otherwise" (Clark, 2006: 169).

[...] here the German counts and barons of the Holy Roman Empire studied politics and law under Schlözer and Pütter. Here Mosheim taught church history and the elegancies of pulpit diction, and J. D. Michaelis oriental languages. Here labored Albrecht von Haller and his successor Blumenbach, in their day the chief representatives of the science of man, or physical anthropology; as well as the celebrated astronomer Tobias Mayer, the brilliant physicist Lichtenberg, and the able mathematician Kästner. Finally, the newly awakened study of antiquity found its first nursery at this university; the philologists, J. M. Gesner and J. G. Heyne, to whom is due the reintroduction of Greek into the university, found a new point of view for the treatment of the classical authors: the study of the classics was no longer to be a useless erudition, nor yet an imitation of Greek and Latin models, but a living, cultural intercourse with the classical authors as the highest patterns of art and taste. This was the viewpoint of the new humanism through which the study of antiquity once more acquired a reasonable and human purpose: the cultivation of a sense and taste for the beautiful and sublime in literature. The new humanism did not stand in opposition to, but came into living reciprocal relation with contemporaneous German poetry, which was also centered at Göttingen. It is enough to mention Haller's poems, Gesner's German Society, and the *Hainbund*. (Paulsen, 1906: 47)

B. The focal points in the configuration of the Germanic mathematics during the second half of the 18th century: Göttingen and Halle

Considering that Göttingen and Halle were the leading Germanic universities during the second half of the 18th century, as well as Münchhausen's appointment criterion, a legitimate question to ask is whether mathematics teachers of both universities were also –or at least were among– the leading math teachers during that period. Which, as a consequence, entails inquiring to what extent were influential three professors among the Germanic mathematicians during those decades, namely: Johann Andreas von Segner (1704-77), the first math professor of Göttingen's university who, after the decease of Wolff in 1754 and with the assistance of Euler, became Wolff's successor at the university of Halle in 1755 (cf. Kleinert, 2002); Abraham Gotthelf Kästner (1719-1800), who arrived to Göttingen from Leipzig, where he had been teaching first as *pivatdozent* and since 1746 as extraordinary professor of mathematics (cf. Cantor, 1882); and, finally, Wenceslaus Johann Gustav Karsten (1732-87), who in 1778 occupied the position left vacant on Segner's death after teaching at the universities of Rostock and Bützow (cf. Günther, 1882).

In order to elucidate the importance and influence of those three mathematicians, however, something will be said about the authors who one might assume that exerted a greater influence on them in their student days, that is, their own math teachers. What role did their teachers play in the development of their conception of mathematics, which in turn might have shaped to a greater or lesser extent the next generations' conception? Clearly, there are differences between the university backgrounds of each of them since Segner studied at Jena in the early 1730s (by then part of the HRE's Duchy of Saxe-Eisenach), Kästner at Leipzig in the late 1730s (by then part of the HRE's Electorate of Saxony) and Karsten at Rostock (by then part of the HRE's Duchy of Mecklenburg-Schwerin, predecessor of the homonymous Grand Duchy) and Jena in the early 1750s. That is to say, they studied in different years, at different universities located in different States and under the supervision of different teachers. But, as will be shown, there are also some significant similarities concerning the concepts and procedures they employed, as well as the referential authors they quoted beyond Wolff; authors who, it must be said, in fact differed and even opposed Wolff's conception of mathematics in certain aspects as Segner, Kästner and Karsten themselves would eventually do.

B.1. Highlights on the mathematical educational background of the math teachers at Göttingen and Halle

To begin with, Segner's advisor was Georg Erhard Hamberger (1697-1755), a medical doctor who at the time taught physics and mathematics at Jena and who was linked to Wolff not only as the son of this latter's teacher, Georg Albrecht Hamberger (1662-1716), but also as a follower of some of his ideas. Erhard Hamberger, for example, shared Wolff's belief in the usefulness of mathematics' method to expand the human knowledge in various areas, including the phenomena of life; an iatrophysical –mathematical– conception[31] that can be found in his work on respiratory mechanism explained from the mere contraction of the rib muscles (Hamberger, 1749; cf. Hamberger, 1726; Uschmann, 1966: 579-580). Nonetheless, his *Elements of Physics* (*Elementa Physices*), which along with Wolff's *All kinds of useful experiments* (*Allerhand Nützliche Versuche*) was one of the most popular physics texts (where mechanics' foundations followed Leibnizian form) in the Germanic countries during the first half of the 18th century, provides some significant differences from Wolff, such as the importance given to chemistry among the parts of Physics and the emission conception of the nature of light (Hamberger, 1735).[32]

On the other hand, Kästner's advisor was Christian August Hausen (1693-1743), a mathematics professor mainly interested in the connections between mathematics and natural sciences whose textbook *Mathematics Elements* (*Elementa Matheseos*) was important throughout the second half of the 18th century. There, as Schubring has pointed out (cf. Schubring, 2005: 97-99),

[31] Iatrophysics and iatrochemistry were two "old medical systems based on mechanical principles (iatromathematics or iatrophysics) or chemistry (iatrochemistry" (Bynum, 1994: 93), "systems of medical explanation and practice that arose in the seventeenth century and that contested Galenic physiology" (Lindemann, 1999: 79).

[32] In contrast to Wolff, who defended a –corpuscular– medium conception of light (that light was an action propagated through matter), Hamberger defended a –corpuscular– emission conception (light was propagated matter) (cf. Hakfoort, 1995: 122-126; Dijksterhuis, 2005: 230).

Hausen introduced several innovative elements regarding Wolff's proposal: a) while he began his *Arithmetic Elements* with the definition of quantity as what is capable of increase and decrease, just as Wolff defined it in his *Mathematisches Lexicon* according to the usual notion at that time (Wolff, 1716: 1143), his definition of number was not the one of the latter (a collection of units or aggregate of many things of one kind (Wolff, 1710: 34; 1716: 944; cf. Euclid, 1908: 277, VII.2)) but that of "a certain quantity A, expressed by the ratio which it has to another B" (considering B the unity (cf. Newton, 1707: 2; Wolff, 1713: 21-22; 1716: 945; 1742: 24)), on the basis of his previous definitions of ratio as "the quantity of the relation of A with respect to B" and of proportion as "the identity of the ratio" (Hausen, 1734: 1-2); b) his definitions of addition (the variation of numbers by which they increase assuming other numbers), multiplication (the variation by which a number F turns into P, which is in the same ratio to F as the other number f is to the unity V), subtraction (the variation in which a given B is taken to another A) and division (by which a given A is decreased to another B, given any Q that is relative to the unit as A is relative to B) (Hausen, 1734: 3-4), not only allowed him to consider a negative difference for $A - B$ (given $B > A$) that expresses a negative quantity (*quantitas negativa*) under the condition of the determination of its oppositeness to another quantity *of the same genus* but regarded in this case as positive (Hausen, 1734: 13-16), but also allowed him to perform multiplications and divisions with negative numbers (Hausen, 1734: 19-22); and c) he introduced in his *Geometry Elements* the notion of continuity (a quantity whose parts are neighboring or connected, having them the same boundaries) as a fundamental one, just after the definition of boundary but before the one of extension (the continuity of space and bodies filling part of the space), to which the idea of homogeneity of the parts underlies (Hausen, 1734: 87).

Finally, among Karsten's teachers were Franz Ulrich Theodor Aepinus (1724-1802), at Rostock, and Joachim Georg Darjes (1714-91), at Jena. Aepinus was the same mathematician who years later would recommend to Catherine II the adoption of the Austrian educational model. He was also the author of the 1759 work *Tentamen theoriae electricitatis et magnetismi* (*Attempt at a theory of electricity and magnetism*), in which, investigating the polarization change of some crystals by changing temperature, "he conceived the idea that magnetization resulted from the redistribution within a piece of iron of a subtle magnetic fluid, different from but analogous to Franklin's electric fluid, the poles of a magnet being regions of 'plus' and 'minus' magnetic charge" (Home, 2005: 5-6; cf. Aepinus, 1759). Even though this work postdates his stay at

Rostock, his mathematical treatment of electricity and magnetism in terms of equations was, on the one hand, highly innovative and "constituted a dramatic advance toward a fully mathematized physics" (Home, 2005: 6), and, on the other hand, it was a combination of his previous interests in those topics as well as in algebraic equations.

Precisely, while teaching at Rostock, Aepinus wrote 4 mathematical works that are relevant for this section's purposes: *Commentatio mathematica de augmento sortis per anatocismum* (1747), *Demonstrationes primariarum quarundam aequationibus algebraicis competentium proprietatum* (1752), *Commentatio de notione quantitatis negativae* (1754) and *De integratione et separatione variabilium in aequationibus differentialibus, duas variabiles continentibus, commentatio* (1755). In the preliminary comment of the first one, two sources are quoted with regard to the mathematical method of the signs employed there, namely Wolff (in particular the chapter on analysis in his *Elementa matheseos universae*, "Elementa analyseos mathematicae tam finitorum quam infinitorum") and, with a special mention, Leibniz (Aepinus, 1747: 4).[33] In the second one, however, the authors mentioned at the beginning of his work as sources for those demonstrations of algebraic equations were Palmquist, Newton and Thomas Harriot (whose lack of demonstrations for negative roots of equations he pointed out),[34] while he mentioned Euler (his *Mechanica*) in the *Corollaries*. Which is not only significant because of his acquaintance of leading contemporary authors on the subject, but also due to what he stated in those corollaries: a) several paradoxes arise from the mathematics of the infinite (e.g. from the "variable quantities" and the "infinitely small or large quantities"), which makes them objectionable; b) just as the objects of geometry (point, line, surface, solid) are real beings, existing in act ("*entia realia, actu existentia*"), negative quantities are true and actual quantities ("*vera & realis*"); c) the geometer does not consider the extension body but the space in which the bodies are coextended, which does not correspond with the old and tempo-mathematical notion of continuity but in any case with the notion of contiguity (Aepinus, 1752: 14-15).[35] In his third work, the one on negative numbers whose concept he based on the one of oppositeness

[33] Aepinus's tutor at Rostock, L. F. Weiss, "was one of those mainly resposible for introducing Wolff's doctrines into Rostock", while at Jena Aepinus "spent most of his time following the lectures on mathematics, physics, and the various medical sciences of the well-known teacher G. E. Hamberger" (Home, 1979: 8-9).

[34] At least partially, this was due to the fact that Thomas Harriot's algebra book, his *Artis Analyticae Praxis* posthumously published, was "substantially rearranged" by its editor, who omitted some sections (cf. Seltman & Goulding, 2007).

[35] In his Physics, Aristotle distinguished between a "contiguous" (*echómenon*) and a "continuous" (*synechés*) thing: the former involved contact and succession, while the latter implied a unity by contact (a thing was continuous with another if the contact between their limits made them one and the same) (cf. *Phys*. 227a 8-227a 18).

(Aepinus, 1754: 11),[36] Aepinus in fact criticized the foundations of Leibniz's calculus whose conceptual difficulties, he suggested, could be overcome by conciliating the analysis of infinites with "the Archimedean method of quadratures and the method of exhaustion built upon this by Gregory of St. Vincent" (Aepinus, 1754: 4). While, in his fourth work, he criticized some methods of the analysis *infinitorum* (Aepinus, 1755: VII), in both cases resorting to contemporary sources such as Euler, Clairaut, Maupertuis and Craig, in the latter (cf. Aepinus, 1755), and Descartes, Wallis, L'Hospital, Varignon, Grandi, Newton and Fontenelle, in the former (cf. Aepinus, 1754).

As to Darjes, of whom Segner was one of his teachers while he studied at Jena, by the 1750s his initial enthusiasm for Wolff's philosophy, if had not disappeared, at least had been attenuated. Thus, his Wolffian work from 1743-44, *Elements of Metaphyscs* (*Elementa metaphysices*), was succeeded a few years later by his *Remarks on some of the teachings of Wolff's Metaphysics* (*Anmerkungen über einige Lehrsätze der Wolfischen Metaphysic*, 1748), where, among other things, he expressed himself directly even against Wolff's restricted version –to the body-soul relationship– of Leibniz's principle of sufficient reason (Darjes, 1748: 6ff.).

Darjes's mathematical works of that period, nevertheless, only show a certain detachment from Wolff's ideas, remaining close to some of his concepts and methods. That way, his 1742 *Introduction to the art of discovery or theoretico-practical logic, as analysis and dialectics are proposed* (*Introductio in artem inveniendi seu logicam theoretico-practicam, qua analytica atque dialectica proponuntur*)[37] intended to show the correct and natural way of learning, i.e. the way to truth, following the mathematical method of exposition that begins with definitions towards demonstrations which he praised in Wolff's work (Darjes, 1742: *Praefatio*); a method, however, which along with his distinction between analytical logic (or doctrine of truth) and dialectical logic (or doctrine of probability), following a common division among the Aristotelians of the 16th and 17th centuries, also accounts for his scholastic background present in his *Elementa*'s mathematical structure (cf. Darjes, 1743 & 1744; Basso, 2016: 162). Moreover, in this work Darjes a) adopted Wolff's designation of "privative thing" (*rem privativam*) for the "negative thing" $-A$ (*rem negativam*), which regarding "positive thing" $+A$ was "heterogeneous" (*res*

[36] I am grateful to the Digitale Universitätsbibliothek of Rostock, in particular to Robert Stephan, for having prioritized the digitisation process of Aepinus's text, which is now available in its institutional repository.

[37] First published in 1737 as *Die lehrende Vernunft-kunst aus der Natur des Seele in mathematischer Lehrart*, then expanded in its Latin version of 1742 and finally abridged in his 1755 *Via ad veritatem. Commoda auditoribus methodo demonstrata*.

heterogeneas), and b) considered impossible the combination of one and the other (*Ex combinatione rerum heterogenearum oritur impossibilitas*) (Darjes, 1742: 15-16), just as Wolff himself did in his *Elementa Analyseos*, where he considered ratios of heterogeneous quantities were impossible (Wolff, 1713: 247-248; 1742: 299-300).

By contrast, in his 1747 *First grounds of the whole mathematics* (*Erste Gründe der gesamten Mathematik*),[38] Darjes on the one hand disassociated himself from Wolff's general –and traditional– conception of mathematics as the "science of quantity, that is, all those things which can be magnified or diminished" (Wolff, 1716: 863), instead of which he defined mathematics as the "science of finding the quantities" (Darjes, 1747: 8),[39] in accordance to his scholastic background (cf. Darjes, 1743-44). Not being a minor modification, Darjes explained in a note that determining the number of units in the quantity belonged to mathematics but to examine the nature of the quantity, its being (the *ens*, he would say in his *Elementa*), corresponded to metaphysics (Darjes, 1747: 8).[40]

In that way, on the other hand, Darjes's division of mathematics, placing as the first part of theoretical mathematics "the art of reckoning" (*Rechen-Kunst*), allowed him to consider from the very beginning the "negative quantities" (*negative Grösse* and not, as in his 1742 work, the *rem negativam*) and operations (addition, subtraction, multiplication and division) among them (Darjes, 1747: 86-111), as well as to consider that the introduction of infinitely small and large quantities could only be done by means of ratios of finite quantities[41]: first, and unlike Wolff in 1750,[42] he considered that not only the objects of study of algebra and trigonometry belonged to general mathematics, to particular parts of theoretical mathematics and sometimes even to a particular part of practical mathematics, but also that "the manner in which the quantities are found in those sciences [algebra and trigonometry] is that which is used in *Rechen-Kunst*"

[38] The full title is: *Erste Gründe der gesamten Mathematik darinnen die Haupt-Theile sowohl der theoretischen als auch praktischen Mathematik in ihrer natürlichen Verknüpfung auf Verlangen und zum Gebrauch seiner Zuhörer entworfen*.

[39] Darjes wrote: "Diejenige Wissenschaft, in welcher man erkläret, wie der Grössen der Dinge zu erfinden, wird die Mathematik genennet. Diese ist demnach eine Wissenschaft von Erfindung der Grössen." (Darjes, 1747: 8)

[40] Darjes wrote: "Einmahl, wenn wir das Wesen der Grösse und diejenige Eigenschaften, welche daher folgen, untersuchen: Zweytens, wenn wir uns bemühen, die Anzahl der Einheiten in der Grösse zu bestimmen. Und dass die erste Abhandelung von der Grösse nicht zur Mathematik sondern zur Metaphysik gehöret." (*ibid*.)

[41] Darjes wrote: "Unendlich Grössen können wir nicht anders als bey nahe erfinden. Wenn wir demnach Grössen erfinden wollen, welche unendlich gross oder unendlich klein, so müssen wir Verhältnisse endlicher Grössen suchen, welche jene bey nahe bestimmen." (Darjes, 1747: 64)

[42] Wolff considered "negative quantities" not in his *Anfangs-Gründe der Rechen-Kunst*, but in his *Anfangs-Gründen der gemeinen Algebra*, the fourth part of the improved edition of his *Der Anfangs-Gründe Aller Mathematischen Wissenschaften Letzter* (Wolff, 1750: 1557ff.).

(Darjes, 1747: 12);[43] second, the introduction of negative quantities did not involve an ontological commitment, from his point of view, but the mere calculation with them; and, third, his work reflects how by the mid-18th century the method of ratios was preferred among some mathematicians when introducing infinitely small and large quantities due to what they considered at best a "lack of clearness" when introducing such quantities *de facto*, i.e. infinitesimals, into geometry.

B.2. The mathematical conceptions and practices of Segner, Kästner and Karsten

B.2.1. The context at the beginning of their careers as mathematicians

One year after the publication of Darje's *First grounds of the whole mathematics*, in 1748, Euler published his *Introductio in analysin infinitorum*, and later in 1755 his *Institutiones calculi differentialis*, and things slowly began to change in the Germanic territories throughout the second half of the 18th century. The works of Segner, Kästner and Karsten precisely reflect that transition, albeit in different ways and to a different extent. At least initially and explicitly or implicitly, both Segner and Kästner considered mathematics and quantity almost exactly as Wolff considered them in the corresponding dictionary entry, while Karsten adopted a different position from that of the latter but a similar one to that of Darjes.

All three, as it was mentioned before, pertained to different generations and studied and began to work as math professors not only at different times but also in different places: Segner, born in 1704, presented his mathematical dissertation *Attempt on Archimedes mirror* (*De speculis Archimedeis tentamen*) at Jena, in 1732; Kästner, born in 1719, presented his dissertation *Theory of equations roots* (*Theoria radicum in aequationibus*) at Leipzig, in 1739; and Karsten,

[43] At the end of the "First Grounds of General Mathematics", however, he stated: "Ich könnte hieraus die Natur der Differential- und Integral-Grössen erklären, weil aber befürchte, dass solches Anfängern zu schwer, so will diese Untersuchung bis zur Algebra versparen." (Darjes, 1747: 68) Which means he did consider those as parts of mathematics but simply not as parts suitable for that work's aims.

born in 1732, presented his dissertation *Inquires about the notion of Algebra and its difference from Arithmetic* (*Inquirens in notionem algebrae eiusque differentia ab arithmetica*) at Rostock, in 1755. While the dissertation's date of the latter makes patent the generational gap between him and Kästner and Segner, the dates of the dissertations of these give account of a reduction of the generational gap between them because Segner, who first qualified as a medical doctor, only began his career as a mathematician in the 1730s,[44] first at Jena, where he taught for a few years, and then at Göttingen, where he became the first professor of mathematics in 1735.

The difference concerning their educational provenance, on the other hand, is at least as significant as the temporal difference to understand the roots of their respective mathematical approaches. While towards the mid-18th century the university of Rostock was important in a regional context, the universities of Jena and Leipzig were, along with Halle, among the most important universities in the Germanic territories; the transfers of Darjes and Karsten to the university of Jena in 1731 and 1752, respectively, bear witness to this. In fact, from 1730 to 1739 Jena had the best annual average of student enrollment, with 652, ahead of Halle, with 625, and Leipzig, with 378, while Rostock had an average of 82 students per year for the same period and Göttingen an average of 185 students during its first decade, from 1734 to 1743 (cf. Eulenburg, 1904: 294-295 & 297).

However, while during the years of the 1730s that Segner was in Jena, the city and the university did not experience major problems, during Kästner's period as student and professor at Leipzig and during Karsten's period as student and professor at Rostock and Bützow, those three universities and cities went through some internal and external problems. Thus, the city of Jena, which from 1741 became part of the duchy of Saxe-Weimar, was still part of the also Ernestine duchy of Saxe-Eisenach by the time Segner studied and worked there. The Duke of Saxe-Eisenach, Wilhelm Heinrich, was in fact one of the rulers who during that period forbade the university's professors in his territory to accept offers from Münchhausen to teach at Göttingen (cf. Whaley, 2012 II: 442). And it was Jena where in 1730 it was founded a *Deutsche Gesellschaft* "on the model of [Johann Christoph] Gottsched's 'Deutsche Gesellschaft' in Leipzig", which, as seven other similar *Deutsche Gesellschaften* that were founded between 1730 and 1762, had

[44] It must be said, however, that among the works that Segner wrote before the 1730s he, for example, devoted one to prove equations with only real roots, a subject on which Kästner himself wrote a work in the 1740s.

"an anti-court animus [in] their criticism of the adoption of French language and culture, and of the princes and nobles responsible for it" (Whaley, 2012 II: 181 & 341).

In the case of Rostock and Bützow, Prussia's occupation and plunder of the Duchy of Mecklenburg-Schwerin in the Seven Years' War led to the impoverishment of this Duchy, which happened shortly after: a) the Duchy's constitution was settled (1755), b) Christian Ludwig II was succeeded by his son Friedrich II as its ruler (1756) and c) this latter decided to back Austria in that conflict (cf. Whaley, 2012 II: 534). However, neither that circumstances nor the low annual average of 65 students from 1749 to 1758 (cf. Eulenburg, 1904: 296) prevented Friedrich II from splitting the university of Rostock, a scholastic Lutheran institution of Aristotelian orientation, into two parts from 1758 until his death in 1785.[45] As a consequence, one part was located in Rostock and the other in Bützow, after Friedrich II, a Pietist supporter, appointed a Pietist professor of theology whom the university and the city itself opposed; Karsten, precisely, was one of the professors transferred to the new and small University of Bützow, of which he was rector a couple of times during the second half of the 1760s (cf. Günther, 1882).

Meanwhile, the city of Leipzig belonged to the key Electorate of Saxony, which got involved in all the three Silesian Wars, supporting Prussia in the first one and the Habsburgs in the other two. So while among the main motives of Frederick Augustus II (Elector of Saxony, King of Poland and Grand Duke of Lithuania from 1733 to 1763) for such alliances was not his personal union with Maria Josepha of Austria (the elder daughter of HRE's Emperor Joseph I and, as a consequence, the legitimate heiress to the realms of the House of Habsburg before the decree issued by his uncle in 1713) but obtaining a territorial bridge to the Polish-Lithuanian Commonwealth, at the same time that obtaining the protection of one of the European great powers closest to his Electorate (due to its vulnerability, given its geographical location), the central motive for both the Brandenburg and Austria was precisely its strategic location for their better positioning with respect to each other (cf. Whaley, 2012 II: 358).

In spite of all that, it was Leipzig where: a) between 1732 and 1754 Zedler's encyclopedia (*Großes vollständiges Universal-Lexikon aller Wissenschaften und Künste*) was published; b) Gottsched, professor at the university that by then was an enthusiastic Wolffian, founded in

[45] It was in fact Friedrich's successor, his nephew Frederick Francis I, who merged back both universities in 1789.

1731 the *Societas Conferentium* to which it would later be linked the pro-Wolff *Gesellschaft der Alethophilen*, established in Berlin (cf. Whaley, 2012 II: 339); c) Gottsched himself, after Vienna's rejection to his proposal to found a German academy of arts, founded a Society of Liberal Arts in 1752 "modelled on the *Académie Française* and the *Académie des Inscriptions et des Médailles*", foundered a few years later lacking Frederick Augustus' II patronage (Whaley, 2012 II: 182-183); and d) the university had the second best annual average of students among the Germanic universities from 1746 to 1755 (with 347), just below Halle (with 540) but above Göttingen (with 278) (cf. Eulenburg, 1904: 297).

As the latter data show, Segner studied and began his career as math teacher at a traditional Lutheran institution whose modernization process would begin during the second half of the 18th century, after c. 1750 it had gone from having the best annual average of student enrollment among the Germanic universities to not even being among the first three. On the contrary, Kästner studied and taught at a Lutheran institution whose modernization process began earlier, as a consequence of which c. 1750 it was still among the three most popular and even above the new university of Göttingen, which would eventually become the best and most modern one.

B.2.2. Their works from the early 1740s to the late 1760s

Segner taught at Göttingen for almost 20 years, throughout which he published two works that are especially relevant to the objectives pursued here, videlicet his *Sample of logic universally demonstrated* (*Specimen logicae universaliter demonstratae*), from 1740, and his *Clear and complete lectures on arithmetic and geometry* (*Deutliche und vollständige Vorlesungen über die Rechenkunst und Geometrie*), from 1747. "The idea or the notion", he wrote at the beginning of his 1740 work, "is that by which a thing is represented in the mind" (Segner, 1740: 1).[46] Therefore, ideas could be considered according to several criteria: simple or composed (as the idea of a triangle), opposite or concordant (as the ideas of figure and triangle), identical (as the

[46] Segner wrote: "Idea sive notio est id, per quod res in mente repraesentatur."

ideas of quadrilateral and quadrangle figures) or different (as the ideas of figure and triangular figure), and so on (cf. Segner, 1740: 2-10).

Among those criteria, however, there were some particularly interesting here. For Segner, given two ideas A and B, A is said to be wrapped or contained (*involvitur siue continetur*) in B if the first belongs to the ideas that compose the latter as, for example, the ideas of "extension" and "trilateral" belong to the one of triangle (cf. Segner, 1740: 3). This initially allowed Segner to compare different ideas on the basis of "containment", considering some ideas to be superior to others, and subsequently, when talking about propositions, allowed him to express those comparisons as in calculus the comparison of magnitudes was expressed: two ideas A and B are identical when they contain each other so that the relation between both is expressed $A = B$, but if A is superior it is expressed $A > B$ and, vice versa, if B is superior it is expressed B > A (cf. Segner, 1740: 5 & 71-72). Even more, that allowed him to consider the relation of "coordination" between ideas "$A \times B$" (for example, every triangle, A, is a figure, B, but only some figures are triangle) (cf. Segner, 1740: 83-84) and, having previously defined an infinite – and negative– idea (as the idea of "not-triangle", which denied the finite one of "triangle" without establishing any determination) (cf. Segner, 1740: 8), allowed him to consider relations involving infinite negative ideas, such as "$-A = -B$" (not triangle = not trilateral), "$A \times -B$" (figure and not triangle) and even "$-A > -B$" (the notion not-triangle is superior to the notion not-figure) (cf. Segner, 1740: 86-91).

It is significant that throughout Segner's calculus of ideas the only mathematical ideas that he mentioned were geometric. After all, if for him both geometry and arithmetic considered the quantity of things, although one in terms of extension and the other in terms of numbers (cf. Segner, 1747: 1 & 186), why not employing also examples from the latter? Why even choosing between restricted notions of quantities for a *calculus* for which a more general notion might have been more adequate? For example, should not he have introduced the more basic idea or notion of negative quantity, $-A$, prior to the negative version of a composed idea such as "not-triangle"? As his 1747 lectures on arithmetic and geometry show, the question is not whether he should have done so, but whether he could have done so, and he could not.

That something is expressed by a number means that it is counted (cf. Segner, 1747: 1),[47] Segner wrote at the beginning of his 1747 work. Number was thus, for him, "a notion of the way in which [things] arise from their unity" (Segner, 1747: 2),[48] a definition that, unlike that of Hausen, does recall that of Wolff as "a collection of units or aggregate of many things of one kind": since numbers arise from unit, strictly speaking there could only be two types of change with numbers, namely becoming bigger when units were augmented and smaller when they were reduced (cf. Segner, 1747: 5).[49] Which also explains why for him the numbers par excellence (*schlechthin*) were the whole numbers $(1, 2, 3, \ldots)$, from which broken numbers or fractions could be formed to express parts or portions of a thing that in turn arise from a unit (cf. Segner, 1747: 3). "To make all this [last step] even clearer", Segner said, "imagine the straight line AB which should be printed [or represented] by a number",[50] so that another straight line CD accepted at will as unit ("if one does not wish to employ one which has already been accepted in common life, such as shoes, inches, or something like that"),[51] to which the parts into which AB is divided are equal, can be aggregated to express by a broken number a portion of the straight line AB such as $\frac{2}{3}$ (cf. Segner, 1747: 4).

Together with the previous ones, however, there was also for Segner a third kind of numbers that could not be expressed by means of whole numbers, called "irrational" or even "inexpressible" numbers (*Irrationalzahlen* or *unausprechliche Zahlen*): the square root of 3, for example, could never be accurately expressed due to the endless process that its expression involved. This led to a different notion of "number" (*einem andern Begrif*), wrote Segner, since they could be regarded as a special type of fractions whose denominator was infinitely great (*unendlich gross*), i.e. enlarged without end (*ohne Ende vergrössern*), and whose represented (*darstellen*) quantities could also be expressed (*ausgedrückt*) as magnitudes in geometry (Segner, 1747: 162-163). In this way, while the use of geometry was a tool to make clearer the

[47] Segner wrote: "Wenn etwas gezehlet, oder durch eine Zahl ausgedrückt werde."
[48] Segner wrote: "Wenn wir genau reden, so verstehen wir unter diesem Worte Zahl, nicht so wohl die Dinge selbst, welche wir zehlen, als vielmehr einen Begrif von der Art und Weise, wie dieselbe aus ihrer Einheit entstehen."
[49] Segner wrote: "Eine Zahl wird grösser, wenn die Einheiten vermehret werden, welche sie ausmachen, un kleiner, wenn ihrer weniger werden. Und ausser dieser Vermehrung und Verminderung kan man mit den Zahlen keine andere Veränderung vornehmen."
[50] Segner wrote: "Alles dieses noch deutlicher einzusehen, stelle man sich die gerade Linie AB vor, welche durch eine Zahl ausgedruckt werden sol."
[51] Segner wrote: "wenn man sich nicht einer solchen bedienen will, welche bereits im gemeinen Leben dazu angenommen worden ist, dergleichen die Schuhe, Zolle, oder etwas dergleichen sind." For a detailed study of the different measures and units still accepted in common life in France, but also in the Germanic countries, during the 18th century, cf. Heilbron's work "The Measure of Enlightenment" (Heilbron, 1990).

formation of rational numbers and their discrete variation, implicit in the ideas of augmentation and reduction (*vermehrung* and *verminderung*), for irrational numbers it was not a tool but a more accurate means to express the different variation they involved, implicit in the actions of enlarge and shorten (*vergrössern* and *verkleinern*).

At first glance such remarks might not seem as important as they actually are. But, just as the endless enlargement of irrational numbers showed Segner's impossibility to consider them "numbers" in the strict sense of the idea "number", being closer to the geometric magnitudes, the paragraphs of his work on "the designation of quantities which augment and reduce each other" (*Bezeichnung der Grössen die einander vermehren oder vermindern*) show that at the time he did not consider "negative quantities" within the arithmetic framework (cf. Schubring, 2005: 132). This only happened in the second "improved edition" (*verbesserte Auflage*) of that work, in which he added a paragraph where he introduced the notion of oppositeness and with it the designations of "positive" and "negative or privative" quantities and numbers within that framework.

By 1747, Segner considered that the signs $+$ and $-$ designated the augmentation and reduction of numbers and quantities within the framework of arithmetic, but nothing more (cf. Segner, 1747: 26-27). As Darjes, Segner (Darjes' teacher at Jena) only introduced the negative numbers within a different framework from arithmetic, namely within the framework of an application of numbers for the resolution of geometric problems, a sort of unnamed analytical geometry contained in the section "Grounds for the calculation with extended quantities" (*Gründe der Berechnung ausgedehnter Grössen*) (cf. Segner, 1747: 628-629; Schubring, 2005: 132). There, Segner explained the use of signs in "algebra" or the "calculation with letters" (*Buchstaben Rechnung*) or characters to which numbers could be ascribed, and stated that quantities denoted by either the sign $+$ or $-$ were "not of a different kind" "but contrary in such a way" that together were $= 0$ or nothing (Segner, 1747: 646).[52] In other words, what changed was the criterion of denotation of the quantities and not these ones. Even further, for Segner it was from the gradual destruction of the unit 1 (*indem die Einheit nach und nach vernichtet wird, bis sie*

[52] Segner wrote: "Es bedeuten $+a$, $-a$ de Grössen von einerley Art, die einander zwar gleich, aber dergestalt zuwider sind, dass $+a$ mit $-a$ zusammen nicht $2a$ giebt, wie geschehen würde, wenn sie einander nicht zuwider wären; sondern es ist $+a$ mit $-a$ eigentlich gar nichts. [...] Denn es ist das gezeigte richtig, von was Art auch im übrigen die Grössen seyn mögen, wenn nur die Grössen, deren eine mit $+$ bezeichnet ist, nicht von einer andern Art sind, als diejenigen, vor deren Zeichen $-$ stehet."

gar nichts wird) that the quantity $-a$ arose (as happened with debts and with walking backwards); a quantity which, once established, allowed him to show the possibility of the basic ratio $1:-a$ (Segner, 1747: 651),[53] as well as to produce extended versions of the arithmetic and geometric (proportion) series:

> By a series of numbers one means a group [*Menge*] of numbers that, according to a certain law assumed arbitrarily, follow each other in an unvaried order. There are many of such laws and one can conceive several of them. Thus there are also infinite types of series of numbers. We will be satisfied with ourselves to consider the two of them among which the latter in particular will be of an indescribable usefulness. These are the arithmetic and the geometric series. (Segner, 1747: 656)[54]

That way, once quantities like $-a$ were accepted, Segner was able to produce not only the arithmetic series of quantities whose numbers were $1, 2, 3, 4, 5, \ldots$, but also the other one $-5, -4, -3, -2, -1, 0, 1, 2, 3, 4, 5, \ldots$, made up of quantities whose numbers on the one side of 0 were marked with the sign $-$ and on the other with the sign $+$ (cf. Segner, 1747: 656-657). Or, as he wrote 20 years later concerning the second of those series, a series of quantities whose numbers designated by the character $+$ were called "positive" (*Positiv*), regarded as those that "actually place something", and whose numbers designated by the character $-$ were called "negative or privative" (*Negativ oder Privativ*), regarded as those that always destroy (*vernichten*) something from the positive ones (Segner, 1767: 27).[55] A clarification, this latter, on the names of positive and negative quantities and numbers which, must be stressed, Segner only added to the arithmetic section in the second edition of his lectures after developing the idea of such arithmetic series within the framework of his sort of analytical geometry, since at that time he –just as Darjes– only considered negative numbers as adscriptions of algebraic negative quantities.

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[53] Segner wrote: "Und es entstehet also allezeit $-a$ aus der 1, indem die Einheit nach und nach vernichtet wird, bis sie gar nichts wird, und indem diejenige Grösse, welche die Einheit vernichtet hat, so dann noch weiter wächst."

[54] Segner wrote: "Man nennet aber eine Zahlreihe, eine Menge von Zahlen, welche nach einem gewissen beliebig angenommenen Gesetz in unveränderter Ordnung auf einander folgen. Es sind dergleichen Gesetze viele, und man kan sich deren immer noch mehrere vorstellen. Also giebt es auch unendliche Arten von Zahlreihen. Wir werden uns begnügen lassen, deren zwey zu betrachten, unter welchen insonderheit die letztere von unbeschreiblichem Nutzen seyn wird. Diese sind, die Arithmetische, und die Geometrische Reihe."

[55] Segner wrote: "Bey dem Gebrauch diesen Zeichen $+$ und $-$, werden die Grössen, deren Zahlen mit $+$ bezeichnet sind, als solche angesehen, die etwas würklich setzen, und man nennet sie aus diese Ursache auch Positiv. Im Gegentheil betrachtet man diejenigen, deren Zahlen das Zeichen $-$ vor sich haben, in Ausehung jener, als Negattiv oder Privativ, weil sie immer von denen, die als positiv betrachtet werden, so vieles vernichten als sie selbst betragen."

In the meantime, Kästner summited his aforementioned 1739 dissertation *Theory of equations roots* (*Theoria radicum in aequationibus*) and published several mathematical writings, such as *Aeqvationvm speciosarvm resolutio Newtoniana per series*, from 1743, *Demonstratio Theorematis Binomialis* and *Demonstratio Theorematis Harriot*, both from 1745. Their mere titles and the references to Colson, Stirling, Harriot, Euler, Fontenelle, Hausen, Segner and Stübner, in addition to Newton, Leibniz, Bernoulli and Wolff, makes evident Kästner's knowledge about some of the most recent developments of his time, while their content provides some glimpses about his conception of mathematics, quantities and numbers: he considered that the analytical method had advantages over the traditional synthetic method (Kästner, 1745A: *3);[56] for him positive and negative quantities were homogeneous, contrary to Wolff (Wolff, 1742: 26 & 300), and therefore could relate to each other (cf. Kästner, 1739: *theses*); he considered negative and imaginary quantities and numbers, as well as indefinite or infinite series, within an algebraic framework, as in the cases of his study of negative and complex roots of equations, Newton's parallelogram, the binomial theorem and the number of true and false roots in equations (cf. Kästner, 1739, 1743, 1745A & 1745B); and, within such framework, he employed new symbols, e.g. ∞ (Kästner, 1745A: 3), and new concepts, as the notion of "limit" (*limes*) (Kästner, 1739: 31; Kästner, 1745B: 3; cf. Cajori, 1923: 226).

It was, nonetheless, in his *Foundations of Mathematics* (*Mathematische Anfangsgründe*), whose first edition's volumes were published from 1758 to 1769, where Kästner provided more than suggestions about his conception of mathematics and what he considered the valid methods, procedures and core notions of that science. By that time, Segner, who Kästner had succeeded at Göttingen a couple of years earlier, was publishing the second of the five volumes that would comprise his *Mathematical courses* (*Cursis Mathematici*), published from 1756 to 1768, and Karsten, Segner's future successor at Halle, had already published several relevant mathematical works besides his 1755 dissertation. Most remarkable, by 1758 the works of both Segner and Karsten had begun to show, to a lesser or greater extent, a transition towards some of the latest mathematical developments of the time which, in the case of the latter, was intertwined with a gradual detachment from his early referential author (Darjes) and an increasing acquaintance

[56] Fourteen years later, Kästner would insist in the *Considerations* to a textbook of analytic geometry on those advantages (cf. Kästner, 1759B: *Betrachtungen* [6-7 & 13]).

about some of the most influential conceptions of mathematics among his European contemporaries.

Thus, in his 1755 dissertation and one year later in his *Elements of Universal Mathematics* (*Elementa matheseos universalis*), Karsten adopted a position different from that of Wolff but similar to that of Darjes. "To find is", he said following his teacher, "to show that from a given known some other unknown somehow becomes known", so that mathematics "is the science of finding quantities", i.e. the science of finding that "in which various homogeneous things can be distinguished from each other" (Karsten, 1756: 16, 17 & 11).[57] Even more, just as Darjes did in his *Introductio*,[58] Karsten distinguished in logic, as the doctrine of truth (Karsten, 1755: 4), between the *ars heuristica combinatoria* and the *ars inveniendi*, considering then that pure mathematics comprised the finding of quantities by "logical analysis" and elementary or sublime "calculation", which would respectively correspond to the *mathesis universalis* and the arithmetic or algebra –or mathematical analysis– (Karsten, 1756: 17). That is to say, the difference between arithmetic and algebra was for him of degree: "Arithmetic and Algebra are strictly heterogeneous cospecies of broadly understood Arithmetic" (Karsten, 1755: 6).

Accordingly, on the one hand, for Karsten, as for Darjes, the investigation of the essences of things (*essentias rerum*) corresponded to philosophy –metaphysics– (Karsten, 1755: 3-4). On the other hand, for him algebra or mathematical analysis was the "general art of reckoning", which meant that while arithmetic was the science of finding unknown numbers form given known numbers by calculation (because signs in arithmetic were especially suited to be denoted by numbers), in algebra the signs were in fact considered as universal (Karsten, 1760: 194 & 263).[59] That way, he was able to consider "negative quantities" (*quantitas negativa*) within the framework of mathematical analysis (Karsten, 1760: 271) but not "negative numbers" within the initial framework of elementary arithmetic, just as his teacher had previously done. In other

[57] Karsten wrote: "Res, in qua varia homogenea a se invicem distingui possunt, dicitur quantitas"; "Invenire est efficere, ut ex dato quodam cognito aliud quoddam, quod incognitum sumitur fiat, cogitum"; and "Mathesis est scientia inveniendi quantitates", repeting what he had already written in his 1755 dissertation: "Mathematicus itaque regulas inveniendi universales per naturam quantitatum ut magis determinet, necesse est, et huc in finem sibi format scientiam inveniendi quantitates, quae dicitur Mathesis" (Karsten, 1755: 5). Darjes wrote, concerning "to find": "Ex cogitationibus datis alias incognitas colligere dicitur invenire." (Darjes, 1742: 109)

[58] Karsten even quoted constantly the 1742 work of Darjes in his 1755 dissertation (Karsten, 1755), where he exposed for the first time his conception of logic and mathematics.

[59] Karsten wrote: "Sublimior vero arithmetica, algebra etiam seu analysis mathematica vocata signis utatur adeo universalibus; ut cujuscunque generis magnitudines possint denotare"; and "Arithmetica est scientia ex datis numeris cognitis inveniendi alios incognitos signorum ope certaque eorundem combinatione et substitutione", "quod signa in Arithmetica sint determinata et praecipue tantum numeris denotandis accommodata."

words, being "quantity" a more general concept than "number", which for him was the denotation of a "complex of several homogeneous" (Karsten, 1756: 14), he was able to consider negative quantities in the framework of general arithmetic and *only then and there*, "conceived as being born from the positive unity", he was able to consider negative numbers (*numeri negativi*): "in Analysis all the numbers either are positive or negative" (Karsten, 1760: 272).[60]

To make it even clearer, consider the example offered by Karsten himself: given two –straight– lines AB and AC, with a relation of equality $AB = AC$ but being B and C different, lines AB and AC will be in opposite direction from each other, being one indicated by the sign $+a$ and the other with the sign $-a$. However, if the quantity of one of the lines AB and AC is expressed by numbers, for example AB, and assuming any part of it AD to be the unity $= +1$ and therefore AB to be the unit increasing up to $+7$, one can likewise consider the decrease of the unity, with $AE = AD$ being E and D different and, as a consequence, with AE being the negative unity $= -1$ and $AC = -7$ (cf. Karsten, 1760: 272).

As that example also makes clear, for Karsten the essence of the quantity as such lay in the "unity", since quantity precisely consisted of a conceivable multitude of unities (cf. Karsten, 1755: 10; Karsten, 1756: 12). That way, within the arithmetic framework, an aggregate (*Menge*) of whole units would be called an integer number and an aggregate of equal parts of a unity would be called broken number or fraction, "a distinction that is not found in the nature of the numbers themselves" (Karsten, 1767: 3).[61] This seems to be the reason of the clarification on negative quantities contained in his 1768 work: the one of two quantities which is indicated by the negation of the other, he wrote, usually called negative (*negative*) or negated (*verneinte*), should properly be called "a denyingly expressed quantity" (*eine verneint ausgedrücke Grösse*) (Karsten, 1768B: 67; cf. Schubring, 2005: 136). In algebra, therefore, positively and denyingly expressed quantities, considered in themselves, were for him different but of the same kind (*von einerley Art*) considered under a common main concept of quantity (*gemeinschaftlichen Hauptbegriff*) (Karsten, 1768B: 65).

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[60] Karsten wrote: "In universa autem Analysis omnes numeri sive positivi sunt, sive negative, ex unitate positiva orti esse sunt concipiendi."

[61] Karsten wrote: "Dieser Unterscheid ist in der Natur der Zahlen selbst eigentlich nicht gegründet." That this conception of Karsten recalls that of Aristotle is not strange, taking into account his background. For Aristotle, the number was necessarily counted by addition (*Met*. 1081b 14) so that, in absolute sense, the first number was the 2 (*Phys*. 220a 27) since number was a measured plurality and 1, as the unity, was the beginning (*Met*. 1088a 4).

Nevertheless, Karsten's denomination of negative quantities as "denyingly expressed" (*verneint*), an opposed terminology to "affirmatively expressed" or "affirmed" quantities (*bejahte*), or as "opposite quantities" (*entgegengesetzte*) (Karsten, 1768B: 64), makes patent the prevalence of a geometrical –and kinetic– conception of quantity –and hence of mathematics– by the first decades of the second half of the 18th century. The title of his 1768 treatise on the possibility of logarithms of negated quantities (*Abhandlung von den Logarithmen verneinter Größen*) reaffirms this, despite the fact that throughout the work he also refers to negated quantities as "negative quantities" (*negativer Grössen*) and even as "negative numbers" (*negativen Zahl*) (cf. Karsten, 1768A: 6ff., 90, 103, etc.). After all, according to his 1760 work, it was entirely possible within the framework of mathematical analysis to employ such alternative designations: "The one of two quantities opposed against each other which is represented by the negation of the other", he said, "is called a negative quantity", even though, he insisted, one "should say more correctly: a negatively expressed quantity" (Karsten, 1768A: 8).[62]

Furthermore, not only the terminology, but the very notion of "oppositeness" on which the concept of "negative quantities" rested makes explicit that prevalence of a geometrical conception of quantity and mathematics. This was true for Karsten, at least until 1768: "For if the AE is conceived to decrease continuously, the point E flowing toward B, finally AE becomes $= 0$; if the point E now flows further toward B, it gives rise to a number which is the opposite of the unity AE, [...] which is positive, and point E coincides with B, becoming $= +7$" (Karsten, 1760: 272),[63] he said when explaining the above-mentioned example. Quantities were for him, in short, inherently variable.

So, while despite Karsten's starting point and initial reference author differed from those of Segner, the terminology, notions and methods that the former used did not differ too much from the ones used by the latter. On the other hand, precisely all that content reflects Karsten's above-mentioned transition towards contemporary conceptions and recent developments. This, in fact, is reinforced: firstly, by the mention of the authors that he praised as references on the

[62] Karsten wrote: "Diejenige von zweyten einander entgegen gesetzten Grössen, welche man durch Verneinung der ihr entgegen gesetzten anseiget, heisst eine negative Grösse. Man solte richtiger sagen: eine negative ausgedruckte Grösse."
[63] Karsten wrote: "Quodsi enim AE continuo decrescere concipiatur, puncto E versus B fluente, tandem AE evadet $= 0$, si punctum E jam ulterius versus B fluat, oritur numerus, qui oppositum ipsius unitatis AE, vel partis cujusdam ipsius aliquotae in se aliquoties continet, qui est positivus, et puncto E cum B coincidente, evadit $= +7$."

subject of negative numbers (Aepinus, Hausen, Segner and Kästner), as well as by his references to particular developments of Euler, d'Alembert and Foncenex made in the same 1768 work (cf. Karsten, 1768A); secondly, by his own testimony contained in his curriculum vitae of 1766, by which it is known that it was under under the guidance of Aepinus that he began such transition in the mid-1750s and that it was actually in 1756 when he read Euler's work on differential calculus, although it was Newton's method of ratios the one that he finally found satisfactory (Schubring, 2005: 247-248); and, thirdly, precisely by earlier examples of his work, such as his *Theory about the projections of the sphere for astronomical and geographical use* (*Theorie von den Projectionen der Kugel zum astronomischen und geographischen Gebrauch*), from 1766, where he quoted some of Lambert's procedures, and especially his 1758 work on differential and integral calculus.

To begin with, the very title of that last work of Karsten shows, like others of his works, the prevalence of a geometrical conception of quantity and mathematics: *An attempt to explain the principles of differential and integral calculus in such a way that even in this part of theoretical mathematics the old geometrical evidence can be found*.[64] As Karsten stated at the beginning of that work, for him the foundations (*Grundsätze*) of differential and integral calculus were not clearly established and all their results should therefore be obtained without the use of infinitely small quantities (cf. Karsten, 1758: 5 & 13-14), an expression for variable quantities which, following Euler, were $= 0$ (cf. Karsten, 1758: 15-16). "If the word quantity is to be taken rigorously", wrote Karsten, "then not only none of the given amounts can be $= 0$, but the result must also be a quantity and therefore not $= 0$", with 0 as "a sign of the absence of a quantity" (Karsten, 1758: 21).[65] Even more, "how can an infinite 0 be smaller than other?", he asked in reference to Euler's differential calculus, if all zeros were equal either in arithmetic or in geometric ratio (Karsten, 1758: 45).

As a consequence, instead of infinite small quantities, Karsten proposed the consideration of quantities by proportional rules in geometric terms, which meant the consideration of line segments and segments of figures –which could be interpreted numerically– according to the

[64] The German title is: *Ein Versuch, wie man die Grundsätze der Differential und Integralrechnung so vortragen könne, dass auch in diesem Theil der theoretischen Mathematik die alte geometrische Evidenz hersche*.

[65] Karsten wrote: "die 0 aber keine Grösse ist, sondern ein Zeichen von der Abwesenheit einer Grösse [...]. Soll hier das Wort Grösse nach aller Strenge genommen werden, so wird nicht nur keine von den gegebenen Grössen $= 0$ seyn können, sondern es muss auch das Resultat eine Grösse und also nicht $= 0$ seyn."

calculus of extended quantities (cf. Karsten, 1758: 11, 22, 39). So, for example, for a given triangle ABC (cf. Annex C), and established the functions for each of its straight lines, he: 1) considered the smaller inserted triangle APM and its corresponding function; 2) considered the "difference functions" for the triangle Apm originated by the extensions of P and M, Pp and Mm respectively; 3) established equalities between the proportions of the values of the initial straight lines AB and BC and the differences dx and dy for AP and PM; 4) considered the reduction of the differences for the reduction of triangle Apm towards APM without merging, that is without turning the lines Pp and $Mm = 0$; 5) reversed the tasks and took as starting point the sum of a given difference of a function or "integral f" to find the function; 6) considered, for any function, the general form $Ax^{\alpha} + Bx^{\beta} + Cx^{\gamma} + etc.$ (where x is variable, $A, B, C, etc.$ are constant quantities and the exponents can signal any number), in order to look for the difference between each term and then sum all the differences to obtain the function; 7) pointed out the importance of finding the difference for x^n or, in terms of the binomial formula, $(x + dx)^n$, whose terms in the series could extend indefinitely; 8) considered dy, the difference of the function y, = $Ax^{\alpha} + Bx^{\beta} + Cx^{\gamma} + etc.$, its expansion in series and the grouping of terms of these according to the increase of the powers of dx, having $dy = Pdx + Qdx^2 + Rdx^3 + etc.$; 9) concluded that such formula showed how, if the exponent of x was a negative (*negative Zahl*) or fractional number, dy was equal to an infinite series, while if it was a positive number the series would not be infinite but only, maybe, very large (Karsten, 1758: 22-27).

"The resolution of a big amount of mathematical problems requires that $dx = 0$", noted, however, Karsten, as required by the fundamental formulas (*Grund Formul*) of the differential and integral calculus (Karsten, 1758: 27). The point was, for him, that while some could believe no arithmetic operations could be performed with the 0, it could not be assumed, as Euler, that $2 : 1 = 0 : 0$,[66] although $n \times 0 = 0$ (cf. Karsten, 1758: 35-36). But, as he admitted, contrary to what he had previously defended in his *Elements*, scilicet that 0 could be assumed as an infinitely small quantity,[67] an infinitely small quantity was not a real one (*würkliche Grösse*), as it

[66] Euler wrote in his *Institutiones calculi differentialis*: "ratio quidem arithmetica inter binas quasque cyphras est aequalitatis, non vero ratio geometrica. Facillime hoc perspicietur ex hac proportione geometrica $2 : 1 = 0 : 0$, in qua terminus quartus est $= 0$, uti tertius. Ex natura autem proportionis, cum terminus primus duplo sit maior quam secundus, necesse est, ut & tertius duplo maior sit quam quartus" (Euler, 1755: 78).

[67] Karsten wrote: "Ich hielte die unendlich kleinen Grössen für würkliche Grössen, und sodann schienen mir alle Schwierigkeiten zu verschwinden."

was real an infinitely large (*ein wahrer und richtiger Begrif*), formed from operations with infinite numbers expressed by the sign ∞ (cf. Karsten, 1758: 39-41).

In the preamble of that work, Karsten actually quoted two authors relevant to him on the subject, namely Hausen and Segner:

> I know very well that Herr Hausen, as well as Herr von Segner, have clearly show how any quantity could be regarded as a number, although with the assumption that one could imagine that every constant quantity is composed of infinitely many infinitely small equal parts. I know that they are entitled, under these conditions, to assume about quantities in general that which flows from the concept of numbers. But I also know that Herr von Segner himself does not absolutely subscribe this presupposition. (Karsten, 1758: 10)[68]

Certainly Segner, in the VI section of his 1747 *Deutliche und vollständige Vorlesungen*, "About proportions and their equality" (*Von den Verhältnissen, und deren Gleichheit*), not only introduced the notion of "infinitely small particles" (*unendlich kleine Theilchen*), quantities that were smaller than any other that could be given or named, but he also expressed his reluctance towards such concept (cf. Segner, 1747: 333-334). "Despite these concepts are correct in themselves, it must be confessed that they cannot be approved", he wrote, adding that they lacked "the clarity which must prevail everywhere in geometry" (Segner, 1747: 334).[69] As later for Karsten, for Segner the problem was to consider those quantities as something really close to nothing that could therefore be expressed $= 0$ but still be something in itself. Instead, he expressed his adherence to the "easier and clearer" (*leichter und deutlicher*) notions of the ancient geometers for the comparison of quantities from lines and segments of them (cf. Segner, 1747: 335-336). Which in turn explains Karsten's adherence to Segner's principle according to which two bodies of the same height and bases are equal, if from the same distance from their bases equal cuts are made in them (cf. Karsten, 1758: 10-12; Segner, 1747: 384ff.): instead of postulating infinitely small quantities $= 0$ without any possible geometric reference, the consideration of segments of figures, and the proportions between their

[68] Karsten wrote: "Ich weiss es gar wohl, dass so wohl Herr Hausen, als auch der Herr von Segner deutlich gezeiget haben, wie jede Grösse als eine Zahl betrachtet werden könne, aber bey Voraussetzung dessen, dass man sich vorstellen könne, es sey eine jede stetige Grösse aus unendlich vielen unendlich kleinen gleichen Theilen zusammengesetzt. Ich weis, dass sie bey diesen Voraussetzungen berechtigt sind, was aus den Begriffen der Zahlen fliesst, von den Grössen überhaupt anzunehmen. Ich weis aber auch, dass der Herr von Segner selbst diese Vorausetzung nicht so schlechterdings billiget."
[69] Segner wrote: "So richtig aber diese Begriffe an sich sind, so muss man doch gestehen, dass sie nicht bey allen Beyfall finden. [...] Und man kan nicht in abrede seyn, dass sie sich von der Deutlichkeit, welche in der Geometrie überall herrschen soll."

decreasing differences and the values of initial figure's lines, allowed to explain those differences mathematically without the presupposition of the concept of infinite.

Almost 10 years later, in 1756, Segner published the first part of his *Mathematical courses* (*Cursus Mathematici*), "Elements of arithmetic, geometry and geometric calculation", which was followed by another four parts dedicated to the elements of *analysis finitorum* or analysis of finite quantities (part II), differential calculus (parts III-IV) and integral calculus (part V). A first part that coincided in its very structure and in its content with his 1747 work: the presentation of arithmetic, geometry and the geometric calculation, as well as the definitions of quantity, number, proportions, ratios, measures, universal mathematics –and operations– and negative quantities and numbers were practically the same as in his previous work (cf. Segner, 1756).

However, the mere subjects that nominate the other volumes that compose his *courses* constituted a novelty with respect to his previous work, reflecting a more detailed acquaintance with the mathematical developments of his contemporaries, e.g. Euler, Wolff, Hausen and Clairaut (Segner, 1758: *praefatio* [a6]). Although Segner did once again express his reluctance to the identification of infinite small quantities with 0 in the preface of the second volume (Segner, 1758: *praefatio* [b4]), he used hereinafter both negative and infinite quantities –and numbers– without further discussion not only in that volume, which dealt with literal algebra itself, but also in the volumes dedicated to the analysis of infinite quantities (both differential and integral calculus), where literal algebra was combined with special symbols for 'new' objects or processes, such as ∞, Δ and $\int$ (cf. Segner, 1758, 1761 & 1768). After all, for him: first, once introduced the negative and infinite quantities in terms of line segments and proportions, they could legitimately be used in all those other mathematical arts encompassed under the term *analysis*; second, the very beginning of an algebra-letter course was the proper place to express such disagreement before explaining all the mathematical developments on finite and infinite quantities related to it; third, the binomial theorem should be introduced within the framework of analysis of finite quantities, since initially it purely involved finite quantities denoted by letters and numbers (cf. Segner, 1758: 147ff.; cf. infra B.3.2 & C.2.2); and, fourth, the rejection of that assumption on the equality between infinitesimals and 0 was the proper thing to do since he still considered negative and infinite –big or small– quantities within a geometrical framework closely linked to curve construction problems.

During roughly the same period that Segner published his *Mathematical courses*, it was said before, the first edition of Kästner's *Foundations of Mathematics* was published: the foundations of arithmetic, geometry, trigonometry and perspective (part I), in 1758; the foundations of applied mathematics (part II), in 1759; the foundations of the analysis of finite and infinite quantities (part III, 2 volumes), in 1760 & 1761; and the foundations of higher mechanics and hydrodynamics (part IV, 2 volumes), in 1766 & 1769. There, a) Kästner defined mathematics as "the knowledge of the quantity", that is, the knowledge of "what is capable of augmentation or reduction" (Kästner, 1758: 3 & 1, respectively), following the traditional conceptions along the same lines as Wolff,[70] and b) he identified the mathematical method (whose study would correspond to logic) with the Euclidean or geometrical one and stated that it was in fact from the developments carried out by the ancient geometers that the new analysis was developed (cf. Kästner, 1758: 16, 12-13 & 4, respectively).[71]

In spite of what such general ideas might suggest, the content of Kästner's work reveals some significant differences with respect to the usual notions and procedures of his time. So, while for Kästner, as for Segner and Karsten, the whole numbers were made from units that could be divided in equal parts and, taken some of these, could be considered broken (cf. Kästner, 1758: 21-22 & 44), he explicitly considered the arithmetic corresponding to these numbers and its parts as "natural" (*natürliche Arithmetik*) (Kästner, 1758: 58). Mathematical truths, such as whole and broken numbers, as well as basic operations among them, he said, could initially be developed naturally, without instruction, being possible later to extend their arithmetic laws to comparable quantities (cf. Kästner, 1758: 10 & 59). That way, while within the framework of natural arithmetic a bigger whole number could not be subtracted from a smaller whole number, once quantities of the same kind were considered as numbers they could diminish each

[70] Kästner wrote: "Was einer Vermehrung oder Verminderung fähig ist, heitzt eine Grösse" and "die Erkenntnitz der Grösse; die mathematische Erkenntnitz liesse sich also in eine gemeine und gelehrte abtheilen". Wolff wrote: Grösse ["Quantitas"] is "alles, was sich vermehren und vermindern lässet" and mathematics is "eine Wissenschaft der Grössen, das heisset, aller derjenigen Dinge, die sich vergrössern oder verkleinern lassen." (Wolff, 1716: 1143 & 863)

[71] I am grateful to Steve Russ for sending to me a copy of Johan Blok's thesis, *Bolzano's Early Quest for A Priori Synthetic Principles. Mereological Aspects of the Analytic-Synthetic Distinction in Kant and the Early Bolzano*, which I received once I had already concluded my thesis. Though it might not be the case, a partial reading of Blok's work suggests that, throughout his work, he focuses on Kästner's mathematical method and philosophy to consider him as a follower of Wolff, without any further clarification as to the relevant differences between their mathematical notions and practices: "These textbooks stand in the tradition of the widely used textbooks by Wolff, especially insofar as the methodology and philosophy of mathematics are concerned" (Blok, 2016: 156-157; in a footnote he mentions Baasner's remark about Kästner's acquaintance of the mathematical developments of his contemporaries). The present work precisely argues that such a traditional reading of Kästner –and Segner and Karsten– shows only part of the whole picture.

other regardless their size (as assets and debts or walking forward and backwards); in which case, those quantities would be considered "opposite" (*entgegengesetzte Grössen*), one being called "positive or affirmative" (*positiv oder bejahend*) and the other "negative or denied" (*negativ oder verneinend*) (Kästner, 1758: 59; cf. Schubring, 2005: 134).

This means, first, that for Kästner, as for Segner, Karsten and others, the concept of negative quantities rested on that of "oppositeness" –of things– and, therefore, as other aspects of his work will show, his conception of quantity and mathematics was also geometrical. Which, secondly, implies that he considered that positive and negative quantities were homogeneous, as he had already stated in his 1739 dissertation: for him, negative and positive quantities were quantities of the same type whose denomination or expression (positive or negative) would depend on their relation to the others (negative or positive), so that, for example, a positive quantity could be considered as the negation of a negative one (cf. Kästner, 1758: 60).

All this in turn explains, on the one hand, his denomination of a negative quantity as a "real" one (*wirkliche Grösse*) and, on the other hand, his distinction between "absolute nothing" (*nihilum absolutum*) and "relative nothing" (*nihilum relativum*) (Kästner, 1758: 60-61).[72] Using an appellation with Cartesian roots, Kästner referred to negative –and positive– quantities as "real" inasmuch as in fact they quantify and, therefore, truly exist, being possible to locate them in the horizontal axis as line segments and, ultimately, in the Cartesian plane as extension segments. As a consequence, Kästner wrote, there is a difference between considering "nothing" in itself, without any relation, and considering it with respect to a quantity, as when a former negative quantity, for example a settled debt, is said it was a quantity "less than nothing" (*weniger als Nichts*) or less than the present nothingness that it is.

Even more, the fact that in 1758 Kästner did not state from the very beginning of his *Foundations of arithmetic* that the –inherently positive– whole numbers or that the quantities represented by positive whole numbers could –and should– be considered to constitute a specific class of numbers and quantities, seems to be closely related to both the absence of the denomination of such numbers as 'natural' and the presence of the denomination of "natural

[72] Kästner wrote: "Diese Negative, das übrig bleibt, ist eine wirkliche Grösse, nur der die als positiv betrachtet wird, entgegengesetzt. [...] An sich selbst aber ist jede verneinende Grösse mehr als Nichts, weil sie eine wirkliche Grösse ist."

arithmetic" for the operations with them. It is not that during Kästner's time the denomination of "natural numbers" was totally unusual or, so to speak, exotic, but rather that it was not entirely usual even though, on the contrary, the underlying idea of the 'naturalness' of those numbers was usual. And it is not that Kästner himself did not employ such denomination within the arithmetic framework throughout his work, because he did refer once to "the series of natural numbers" (Kästner, 1758: 144).[73]

The point is that: a) just as the initial exclusion and the unique use of the name 'natural numbers' in Kästner's 1758 work reflect the lack of consensus at the time on that denomination, a sort of historical note added to the fourth edition of that work (in which he traces back to Aristotle the idea of "the numbers of the fingers" as the "most natural" numbers and quotes a text from the early 17th century as a reference on the subject (Kästner, 1786: 27))[74] testifies, if not a growing consensus, at least an increasing use of that denomination by the 1780s; b) in addition to this, the absence of a specific name for the positive whole numbers to distinguish them from the negative whole numbers, other than that, highlights the existing reluctance towards the very notion of "negative numbers", i.e. towards their status of 'numbers': numbers, in the strict sense of the idea, were for him (as for Segner, Karsten and many others) the –positive– whole numbers, from which proper (*eigentlicher*) or rational –fractions of– numbers could be formed, while improper (*uneigentlicher*) or irrational numbers were those that could not be properly expressed by whole units or aliquote parts of the unit (cf. Kästner, 1758: 76 & 102); c) the need to explicitly call those numbers as 'positive' only arose once quantities were numerically treated, giving rise to opposite quantities that either were 'positive' or 'negative' and that, although Kästner in his 1758 work did not make it explicit and only called them "negative quantities", could be called "negative numbers" instead of "numerically negative quantities" or, as Karsten would say, "negatively expressed quantities"; and d) the negative quantities were real (as real as the positive ones) but they were not 'natural' and therefore their

[73] Kästner wrote in the last section of the part of his work dedicated to "the art of reckoning": "Wenn unter die geometrische Reihe N; die Reihe der natürlichen Zahlen so untergeschrieben wird, wie L so heisst jede Zahl in L der Logarithme der über ihr stehenden in N." He also referred to the "natürlichen Zahlen", for example, in his *Foundations*' volume on the analysis of finite quantites (Kästner, 1760: 59-60).

[74] Kästner wrote: "Nach mehr spitzfündigen Ursachen giebt er erst die natürlichste, die Zahl der Finger." Similarly, Wolff wrote in his *Die Anfangs-Gründe aller mathematischen Wissenschaften*: "Die Einheiten der Zahlen stellet man sich anfangs durch die Finger vor und verrichtet das zum addiren nöthige zehlen so lange surch sie Finger" (Wolff, 1717: 39). It must be said that the text quoted by Kästner was already mentioned in his *Foundations*' volume on the analysis of the infinite quantities (cf. 1761: *Vorrede* [15]).

arithmetic could not be considered natural but an extension of this one, which explains the specific appellative to refer to the arithmetic of numbers in the strict sense.

Kästner, as a matter of fact, introduced a specific section for the arithmetic or calculations with opposite quantities, explaining the particularities of their addition, subtraction, multiplication and division (cf. Kästner, 1758: 62ff.). That way, despite having defined numbers, as –sometimes– Wolff and others did, as an aggregate of units or things of one kind[75] and not, as Hausen –and even at some point as Wolff–, by ratios, he not only did not share the former's rejection of negative quantities, but even, in the preface to his 1758 work, criticized that Wolff had founded, "against the method" (a sort of genetic method), "the doctrine of the fractions on that of the ratios" (Kästner, 1758: *Vorrede* [6]; cf. Schubring, 2005: 134, fn. 76).[76]

Thus, although Kästner recognized the importance of Wolff's work (cf. Kästner, 1758: *Vorrede* [3-4]), he was critical too. Kästner's work cannot therefore be considered as a "more popular and readable version" (Bullynck, 2006: 4) of Wolff's work, not even in the late 1750s and much less, as will be shown, towards the last decades of the 18th century. A year later, for example, in the "Considerations" (*Betrachtungen über die Beschaffenheit und den Gebrauch des analytischen Vortrages*) that Kästner wrote at the beginning of a textbook on analytical geometry authored by Johann Michael Hube (one of Kästner's students), he quoted Descartes and Wolff as examples of important philosophers who, misled by the expression "less than nothing", "[declared] the negated quantities as false": them, as other "geometers" or mathematicians, he concluded, did not "distinguish between signs and things" (Kästner, 1759B: *Betrachtungen* [16-17]; cf. Schubring, 2005: 135, fn. 77).[77] A similar distinction to that made by Darjes and Karsten between mathematical and ontological commitment that Kästner emphasized at the beginning of the geometry section in his 1758 work, when writing about the mathematical study of extension: "It is not necessary to enter here into metaphysical

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[75] Kästner wrote: "A collection of things of one kind is called a whole number" ("Eine Menge von Dingen einer Art, heisst eine ganze Zahl") (Kästner, 1758: 21).

[76] Kästner wrote: "Dass der Freyh. v. Wolf die Lehre von den Brüchen auf die von den Verhältnissen gründet, ist ein grosser Fehler wieder die Methode, weil die grösste Menge der Verhältnisse, Brüche zu Exponenten hat." Wolff, in fact, did found the doctrine of fractions in that of ratios (cf. Wolff, 1717: 66ff.), after which he wrote: "Wenn man ein ganzes in gleiche Theile genau eintheilet und nimmet einen oder etliche Theile derselben / so nennet man es einen Bruch" (Wolff, 1717: 71).

[77] Kästner wrote: "Ist es nicht eine Demüthigung für den menschlichen Verstand dass Philosophen wie Cartes und Wolf, sich durch diesen Ausdruck, durch ein Wortspiel, haben verführen lassen, die verneinten Grössen für falsche zu erklären, sie aus der Zahl wahrer Grössen zu stossen, und wenn sie als Wurzeln von Gleichungen vorkommen, nicht in Betrachtung zu ziehen. [...] Man wird hier auf wunderbare Sätze verfallen, wenn man Zeichen und Sachen nicht unterscheidet, und jene weiter statt dieser setzt, als es erlaubt ist." As will be shown in the following section, Karsten made a similar criticism of Wolff towards the end of his days, in his 1786 work.

investigations about the space and the continuity. The concept of geometric extension is an abstract concept which remains true", he wrote, "regardless of how one presents those things" (Kästner, 1758: 157; cf. Schubring, 2005: 242).[78]

Precisely, when Kästner discussed the "law of continuity" (*Gesetz der Stetigkeit*) in his *Foundations*' volume on higher mechanics, he expressed his reluctance towards its proof by induction from the empirical: "Whether one is justified to extend [the apparent empirical evidence of the law of continuity] to everything which does not fall under our experience as well, I want to leave it to everyone's own judgment" (Kästner, 1766: 353; cf. Schubring, 2005: 183).[79] By contrast, Kästner considered that in abstract mathematics ambit, specifically in geometry, continuity should be defined in non-empirical terms and correctly deduced from concepts. So, concerning the first of these requirements, a continuous quantity was, for him as for Hausen, "one whose parts are all connected in such a way that where one finishes, the other immediately begins, and there is nothing between the one's ending and the other's beginning not belonging to this quantity" (Kästner, 1758: 157; cf. Hausen, 1734: 87).[80] That meant, as he went to explain in his book on higher mechanics, that the law of continuity both prohibited the altered thing to forthwith change from one state to another and required that each of the innumerable many intermediate states differed infinitely little from what Kästner somehow considered its next state (*jedem von seinen nächsten unendlich wenig unterschieden vor*) (cf. Kästner, 1766: 354-355).[81]

Which, the latter, in turn explains, on the one hand, Kästner's assumption of the mathematical – or geometrical– curves as inherently continuous and, on the other hand, his conception of infinitely small quantities. For him, while in geometry the law of continuity was always respected in the case of curved lines, it could not be respected in lines of rectilinear figures since

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[78] Kästner wrote: "Es ist nicht nöthig, sich hier in metaphysische Untersuchungen der Raumes und der Stetigkeit einzulassen. Der Begriff der geometrischen Ausdehnung is ein abstracter Begriff, welcher richtig bleibt, wie man auch sonst diese Dinge sich vorstellen will."

[79] Kästner wrote: "Ob man dieserwegen berechtiget sey, es auf alles zu erstrecken, was auch nicht unter unsere Erfahrung fällt, davon will ich jedem sein Urtheil überlassen." Indeed, in his autobiography, Kästner criticized Hausen's satisfaction with inductions, while , he wrote, "I was never satisfied myself before I had shown the general correctness of the same" (Schubring, 2005: 242).

[80] Kästner wrote: "Eine stetige Grösse (continuum) heisst, deren Theile alle so zusammenhängen, dass, wo einer aufhöret, gleich der andere anfängt, und zwischen des einen Ende und des andern Anfange nichts ist, das nicht zu dieser Grösse gehörte." Hausen wrote: "Continuum est, cujus partes quaevis vel conterminae sunt, vel interjectas habent ipsis & inter se conterminas."

[81] Kästner wrote: "Nun verbietet das Setzes der Stetigkeit der Sache, die verändert wird, sogleich aus jenem in diesem überzugehen. Sie muss durch einen mittlern Zustand durchgehn, der vom vorhergehenden nicht soviel unterschieden ist, als der folgende. [...] So lange die Menge dieser Zwischenzustände sich angeben lässt, so lange lässt sich auch jedes Unterschied von seinem nächsten angeben: Also muss ihre Menge grösser, als jede gegebene Menge werden, wenn diese Unterschiede verschwinden sollen, und so stellt man sich unzählich viel Zustände, jedem von seinen nächsten unendlich wenig unterschieden vor."

"no point [could] circulate the circumference of a quadrangle or triangle" (Kästner, 1766: 353-354; cf. Schubring, 2005: 185).[82] In other words, while any point on a curved line was able to move through it or any of its segments without experiencing a sudden change in its trajectory but experiencing a continuous one, the path described by the lines composing the rectilinear figures entailed those sudden changes and, as a consequence, made such continuous variability impossible.

At the same time, nonetheless, considered only a segment of a curved line or a segment of one of the lines of a rectilinear figure, that is, considered a straight or curve line, its extremes could be taken as the limits of its continuous variability and, once numerically quantified, one of those extremes could be identified with the limit of decreasing segments closer and closer to nothing or 0. Thus, just as irrational numbers could be expressed as the product of a bigger and bigger whole number and a smaller and smaller fraction of 1, as for example $\sqrt[2]{3} = 17 \cdot \frac{1}{10} = 173 \cdot \frac{1}{100} = 1732 \cdot \frac{1}{1000}$ and so on (a number which could be narrowed as much as desired, considered the closest rational numbers to it, as 1.7320508 and 1.7320509 for $\sqrt[2]{3}$) (cf. Kästner, 1758: 121-122), infinitely small quantities could be expressed as quantities of different orders that infinitely decrease, meaning infinitely small terms that gradually vanish (*verschwinder*) (cf. Kästner, 1761: 2ff.).[83] For example, he said, given the proportion $\frac{k}{u^m} : \frac{l}{u^{m+n}}$, with u infinite, the quotient of the second ratio vanishes in comparison to the quotient of the first, each constituting a different order (cf. Kästner, 1761: 5). Which, translated to differential calculus, referred to each term of a given series $(n \cdot z^{n-1}) + \left(\frac{n \cdot (n-1)}{1 \cdot 2} e \cdot z^{n-2}\right) + \left(\frac{n \cdot (n-1) \cdot (n-2)}{1 \cdot 2 \cdot 3} e^2 \cdot z^{n-3}\right) + \cdots$, in a similar way as Euler did (Kästner, 1761: 10; cf. Euler, 1755: 15).

All these considerations reaffirm that while it is true that during the 18th century a sort of degeometrization process of analysis took place in Europe, it is also true that the works of

[82] Kästner wrote: "das Gesetz der Stetigkeit in der Geometrie, wird bey krummen Linien unverbrüchlich in Acht genommen [...]. Ist es schlechterdings unmöglich, dass ein Punct seinen Weg plötzlich ändert, so kann kein Punct in dem Umfange eines Vierecks oder Dreyecks herumgehen."

[83] Concerning a couple of reflections made by Kästner on an infinitely small chord and infinitely small quantities in general, Gert Schubring wrote: "These reflections, first published in 1758, are remarkable not only for their operationalization of the concept of continuity within mathematics, but also for their clear definition of infinitely small quantities as variables having the limit null. We have not found a comparable explicitness in the contemporary reflections in France" (Schubring, 2005: 244). However, while those reflections do correspond to Kästner's volume on arithmetic and geometry, they do not correspond to the 1758 edition, nor to the one of 1764, and only appeared for the first time in later editions (cf. Kästner, 1786: 279-280), as will be discussed in the next section.

Aepinus, Darjes, Segner, Karsten and even Kästner show that, at least with regard to the Germanic states, strictly speaking it cannot be said that "curve construction problems belonged to a field of mathematical activity, flourishing from c. 1635 to c. 1750" (Bos, 2001: 10). Curves, quadratures, tangents were still at the core of those Germanic authors' works on infinite quantities analysis by c. 1770, whereby a) 20 years is a period that should not be neglected in the history of mathematics because of an approximation, as b) it should not be overlooked the relevance of the work of those authors, since they were teaching then a geometrically dependent infinite analysis not only at regionally important universities such as Rostock and Bützow, but also at Göttingen and Halle, the two main Germanic universities by that time. The whole content (terminology, concepts, procedures) of their works makes patent the prevalence of a semantic-ontological notion of quantity eminently variable and, for them, interpretable primarily in geometric terms.

B.2.3. Their works from the early 1770s to the late 18th century

Before his death, in 1777, Segner published an enhanced and improved German translation of the first volume of his *Mathematical courses*[84] in which, unlike his previous German works from 1747 and 1767, he solely used the denomination of negative (*Negativ*) quantities –and numbers– and not anymore the one of "privative" (cf. Segner, 1773: 24ff.). This modification, despite being a minor one and regardless of whether for Segner involved a modification of the underlying idea or notion, draws attention to the fact that just as there is no necessary correlation between one and the other change, neither is there between their absence.

In the case of Segner, the rest of the content of that work makes clear that such omission, although perhaps was a simplification motivated by the increasing use of the designation of "negative quantities", did not imply anything more. Thus, the aforementioned transition in his mathematical works remained constrained to the adoption of particular new developments within the framework outlined by his general conceptions of mathematics, quantities and

[84] *Foundations of arithmetic, geometry and geometric calculations* (*Anfangsgründe der Arithmetic, Geometrie und der geometrischen Berechnungen*), from 1773.

numbers. On the contrary, inasmuch as at the time the mathematical works of Karsten and Kästner also reflected a certain acquaintance about new developments and they lived a few decades longer than Segner, it is worth asking if their works from c. 1770 onwards showed, albeit slight, a change in their stance on quantities and numbers.

Karsten, for example, published a second edition of his volumes on the conceptual framework of mathematics (*Lehrbegrif der gesamten Mathematik*), an expanded and improved version of these under the title *Foundations of mathematical sciences* (*Anfangsgründe der mathematischen Wissenschaften*), an excerpt of both works (*Auszug aus den Anfangsgründen und dem Lehrbegriffe der Mathematischen Wissenschaften*) and some mathematical treatises on some of the most recent developments of the time (*Mathematische Abhandlungen*), such as the infinitely small and the negative quantities.

As it turns out from that brief description, among those works the only one that can strictly be considered "original" is the last one, of the year prior to his death, while among the rest of them the ones of which it could be expected some novelty are his *Foundations of mathematical science* and, as a consequence, its –at least partially– excerpt. In this last work, however, Karsten's previously discussed general framework was not modified: a) mathematics, arithmetic and geometry were understood as in his previous works (cf. Karsten, 1780 I: 2, 3 & 348; Karsten, 1781: 1); b) the first fundamental concepts of quantity and number were defined as before (cf. Karsten, 1780 I: 2);[85] c) –positive– whole numbers, which could be considered abstractly or concretely, were still understood as the numbers par excellence from which broken numbers were formed (cf. Karsten, 1780 I: 5); d) zero remained defined as the sign of an empty place (*leeren Stelle*) (cf. Karsten, 1780 I: 19; Karsten, 1781: 9); e) the introduction of incommensurable quantities and irrational numbers corresponded to the section on geometrical ratios, proportions and progressions (cf. Karsten, 1780 I: 185-189); f) the definition of continuous quantity was only introduced within the geometric framework (cf. Karsten, 1780 I: 351); g) in the absence of a specific part dedicated to analysis, negative quantities were introduced within the geometric framework and once again defined as "a denyingly expressed quantities" (cf. Karsten, 1780 I: 372-378; Karsten, 1781: 189); and h) infinite quantities were introduced within the

[85] Karsten wrote: "Der Begrif von dem, was eine Zahl sey, und der noch allgemeinere Begrif von einer Grösse überhaupt, gehören zu den ersten Grundbegriffen der Mathematik."

framework of general arithmetic as broken numbers with a determined number by numerator and a vanishing denominator (cf. Karsten, 1781: 195).[86]

Nevertheless, some aspects of Karsten's mathematical works since c. 1770 deserve a careful attention due to their link with his latest work's content. To begin with, it must be noticed that the notion of continuity employed by Karsten to define the transition from a positive quantity to a negative one was defined as Hausen and Kästner did: when the parts of a quantity are connected in such a way that where one ceases at the same time the next one begins without something between them which does not belong to that quantity, Karsten said, it is called a continuous quantity (cf. Karsten, 1767: 209; 1780 I: 351).[87] But, unlike those two authors, Karsten did not explicitly place the notion of continuity among the fundamental geometric ones in his works of 1767 and 1780, and he even dispensed with it in his abstract of 1781, although he considered it essential in theoretical mathematics (*theoretischen Mathematik*). As he made it clear both in his *Lehrbegriff*'s volume on further implementations of the art of reckoning (cf. Karsten, 1768B: 95)[88] and in the one on the mechanic of solid bodies (cf. Karsten, 1769B: 223; cf. Schubring, 2005: 249),[89] the inherent variability of abstract quantities depended on the so-called "law of continuity" (*Gesetz der stetigkeit*), according to which the variations of a quantity occurred bit by bit (*nach und nach*), gradually, without a sudden or abrupt (*plötzlich*) change.

Precisely, the explicit absence of that concept among the fundamental ones in those works contrasts with its inclusion among the very first mathematical notions in his *Elements of Universal Mathematics* from 1756: "That beyond which nothing more in a thing is conceived to belong to the same is called the boundary, end, limit. Parts of a quantity either are joined together by a common term, or not. If the former, quantity is called continuous; if the latter, discrete" (Karsten, 1756: 12-13).[90] Which in turn makes explicit the relevance of the notion of continuity for the development of new concepts and procedures, as he explained in one of the

[86] Karsten wrote: "Ein Bruch wird unendlich gross, wenn sein Zähler eine bestimmte Grösse hat, sein Nenner aber verschwinder."
[87] Karsten wrote: "Wenn die Theile einer Grösse so mit einander zusammenhängen, dass da, wo der eine aufhört, zugleich der andre anfängt, und zwischen beyden nichts vorhanden ist, das zu dieser Grösse nicht gehört: so heist sie eine stetige Grösse."
[88] Karsten wrote: "Wenn eine Gröze, z. E. Eine Linie AD als eine veränderliche Grösse betrachtet wird, und man setzt, sie habe sich um ein gewisses stück, z. E. Dd geändert: so nimt man in der allgemeinen Rechenkunst als eine Vorausetzung an, dass diese Veränderung nicht plötzlich geschehe."
[89] Karsten wrote: "Man stellet sich nemlich in der theoretischen Mathematik (41 §. A. R.) alle Veränderungen, die mit einer Grösse, indem sie wächst, oder abnimmt, vorgehen, als solche vor, die nicht plötzlich geschehen. [...] Man nennt diese Regel: das Gesetz der Stetigkeit."
[90] Karsten wrote: "Ultra quod nil amplius in re concipere licet ad eandem pertinens dicitur terminus, finis, limes. Partes quanti aut communi termino copulantur, aut non; si prius, quantum vocatur continuum, si posterius, discretum."

paragraphs on negative quantities in his 1780 work. There, Karsten pointed out that while the ancient geometers did not use the concept of opposite quantities (*entgegengesetzten Grössen*) and because of this they had no reason to use the addition and subtraction procedures in a general sense, the new geometers were able to apply the later developed higher arithmetic to geometry, constituting a general mathematical practice called analysis (cf. Karsten, 1780 I: 377-378).[91] Put another way, the notion of quantity could be extended to include negative ones, to which in turn arithmetical procedures, adapted, could be applied, being even possible to consider them numerically.

Not only that, but without the notions of a variable quantity and its limit, the concept of infinitely small quantities would not make sense. As Karsten wrote in 1768, in his *Lehrbegriff*'s section on the expressions of the infinite (*Von den Ausdrücken des Unendlichen*), considered a constantly decreasing quantity (*beständig abnehmende Grösse*), or a decreasing geometrical progression (cf. Karsten, 1780 I: 210), the last of all the terms or members (*ein Glied vor das unter allen das letzte*) to which it will perpetually approach until it ceases to be a quantity (*wenn sie aufhört eine Grösse zu seyn*) will be the limit (*Grenze*) 0, being called that quantity an infinitely small (*eine unendlich kleine Grösse*) (Karsten, 1768B: 88). A vanishing fraction (*verschwindender Bruch*) or an infinitely small, he added, would be for example $\frac{b}{\infty}$, which would agree with his previous stance on such quantities: inasmuch as infinitely small quantities always quantify something, they could not be identified with 0. What does not seem to agree with this is his later statement that "a fraction is infinitely large when its numerator is finite, but its denominator is infinitely small, or $= 0$", despite his explanation that once reached the limit 0 by the denominator x, the fraction $\frac{b}{x}$ must reach the limit $= \infty$, "over which it cannot grow" (Karsten, 1768B: 89-90): how else could the fraction $\frac{b}{x}$ reach the limit $= \infty$ but identifying x with 0 as an infinitely small quantity?

[91] Karsten wrote: "Die alten Geometer haben die Begriffe von den entgegen gesetzten Grössen in ihren Schriften nicht gebraucht: eben deswegen hatten sie keine Veranlassung, die Begriffe von dem, was addiren und subtrahiren heisst, in einem so sehr allgemeinen Sinn zu nehmen, wie hier die Sache ist vorstellig gemacht worden. Es sind dies Vorstellungsarten, wozu die neuern Geometer vornemlich seit der Zeit sind veranlasset worden, da man angefangen hat, die höhere Rechenkunst auf die Geometrie anzuwenden, ja beyde Wissenschaften gewissermassen auf eine solche Art mit einander zu verbinden, dass daraus eine allgemeine mathematische Erfindungskunst geworden ist, die in den folgenden Theilen dieses Lehrbuchs unter dem Nahmen der allgemeinen Mathematischen Analysis wird vorgetragen werden."

In fact, in his work of 1758 on differential and integral calculus he had already addressed the issue, explaining back then that while the concept of an infinitely large quantity as the quotient arose from the division of any number by 0 (that is $\frac{n}{0}=\infty$) was true and correct, the so-called infinitely small numbers or $\frac{n}{\infty}=0$ were not quantities at all (*es ist würklich gar keine Grösse*) (cf. Karsten, 1758: 40 & 42). For, he wrote, if one divides n by 0 and, according to the concept of division which sets $n=\frac{n}{1}=n\times 1$ or $n=n:1=n\times 1$ (cf. Karsten, 1767: 41ff.),[92] one sets $0:1=n:\frac{n}{0}$ to obtain the latter terms (1 and $\frac{n}{0}$) from the former (0 and n, respectively), just as 1 cannot be obtained from 0, neither $\frac{n}{0}$ can be obtained from n. In other words, he said, given that however large it is considered to be n, $\frac{n}{0}$ will never be obtained from it, $\frac{n}{0}$ cannot be other than an infinite number (*keine andre, als eine unendliche Zahl seyn*), namely an infinitely large. That way, assigned the sign ∞ to express an infinite number, it was obtained, firstly, $\frac{n}{0}=\infty$ and, secondly, $\frac{n}{\infty}=0$, by which, once again according to the concept of division, one could set $1:\infty=\frac{n}{\infty}:n$ to obtain the latter terms (∞ and n) from the former (1 and $\frac{n}{\infty}$, respectively). But, as in the first case, just as an infinite number could not be obtained from 1, neither n could be obtained from $\frac{n}{\infty}$, being necessarily $\frac{n}{\infty}=0$ unless, he added, that for $\frac{n}{\infty}$ it was set an arbitrarily small number ("a number as small as one would like", *eine Zahl so klein als man will*)[93] from which, on the contrary, it would be possible to obtain n. Therefore, he concluded, while by resemblance to an infinitely large number $\frac{n}{0}$, $\frac{n}{\infty}$ could be called an "infinitely small number", it should not be believed that the latter is a quantity when it is not (cf. Karsten, 1758: 40-42).

To make it clear, Karsten, as previously commented, rejected infinitely small quantities considered $=0$ or, in other terms, considered as $\frac{n}{\infty}=0$, since for him, conceived in this way, they did not express any quantity as they would express if on the contrary they were identified with an arbitrarily small quantity, in which case it would be necessary, to put it somehow, to place $\frac{n}{\infty}=$ (arbitrarily increasing small quantity), with $n=$ a finite quantity and $\infty=$ an infinitely large quantity. Precisely, Karsten's procedure based on the decrease of numerically interpreted line segments and segments of figures constituted his attempt to legitimately express those

[92] Here Karsten designates division by ":", a notation that was still common then and that goes back at least to Leibniz (cf. Leibniz GM III, p. 526; Euler, 1748: 37). That way, $3\times 2:2=3$. On the history of the various notations for division, cf. (Cajori, 1993).

[93] Karsten wrote: "Denn man setze für $\frac{n}{\infty}$ eine Zahl so klein als man will, so bleibt es noch immer möglich, dass n aus derselben entstehen kan, welches bey der 0 nur unmöglich ist."

increasingly small quantities once correctly conceptualized. Which, however, does not explain why for him an infinitely large quantity, considered as $\frac{n}{0} = \infty$, was a true and correct concept since the only way that the sign ∞ could mean an infinitely large quantity, given $n =$ (a finite quantity), would be identifying 0 with an infinitely small quantity (cf. Euler, 1755: 83), that is, to admit and use just what he intended to avoid.

His treatise on the mathematical infinite, published in his 1786 work, not only reflects that tension between the mere incorporation or in some cases the adaptation of new concepts and procedures, and the criticism or even the rejection of some of them, but also constitutes Karsten's own testimony about it. Thus, on the one hand, when explaining Euler's proposal on infinitesimals, Karsten pointed out that although by 1758 he had already thought about infinitely small quantities as he did at the time (namely in a way that could be properly explained and traced back to the method of exhaustion of the ancients), and furthermore, even though by then he already knew Newton's method of limits of ratios and thought on the possibility of considering limits of variable ratios based on Euler's differential ratios, his great respect for this latter and his little self-confidence led him to merely show how the defenders of the infinitesimals could justify them (Karsten, 1786: 93-94). The content of his previous works corroborates both his acquaintance of those authors' proposals and the development of his own, while his aforementioned curriculum vitae of 1766 not only corroborates his early dissatisfaction with Euler's infinitesimals and his preference for Newton's method of ratios, but also traces back to Aepinus both his reluctance towards infinitesimals and his conviction that the results involving them could always be obtained by means of the method of Archimedes (Schubring, 2005: 247; cf. Karsten, 1786: 48).

On the other hand, in his 1786 work Karsten elaborated more both on the acceptance of infinitely large quantities and on the rejection of infinitely small ones. Firstly, he said, while certainly dividing n into 0 equal parts seemed to be incomprehensible, the division by fractions would somehow make it viable to divide by 0: for example, considered for $\frac{1}{n}$ the values of the progression of ratios $\frac{1}{2}, \frac{1}{3}, \frac{1}{4}$, and so on, the larger n is assumed, the closer $\frac{1}{n}$ will be to 0 and, assumed n a quantity m beyond which one cannot count, that is, a quantity so large that one may never attain to enumerate but only to represent by the sign ∞, $\frac{1}{m}$ will reach the limit 0 (cf.

Karsten, 1786: 11-14). Secondly, given m, the conditional clause (*Bedingungssatz*) "if $\frac{1}{m} = 0$, then $\frac{1}{0} = \infty$" will be true, considered $\frac{1}{0}$ as the last of all constantly decreasing fractions (*den letzten unter allen beständig abnehmenden Brüchen*), whose numerator is 1 and whose denominator must be the last of all the whole numbers (*die letzte unter allen ganzen Zahlen seyn muss*) (cf. Karsten, 1786: 13-14). Thirdly, just as one can accept the idea of a line which cannot be measured because it has no end point, the idea of an infinitely large quantity $\frac{1}{0} = \infty$ must be accepted (cf. Karsten, 1786: 15-18). However, fourthly, the introduction of infinitely small quantities $= 0$ should not be accepted, at least not as it is presented by some authors who, for example, applying differential calculus to the geometry, set $x + dx = x$ and $y + dy = y$: while one is entitled to employ infinitely large quantities $= \infty$, if for infinitely small quantities $\frac{1}{\infty} = 0$, these quantities cannot be means between nothing and something (*kein Mittelding zwischen Nichts und Etwas seyn*) and, therefore, dx and dy cannot be anything other than 0 (*nichts anders als 0 sey*), whereby the use of exponents to distinguish a 0 from another one $(0, 0^2, 0^3, \ldots)$ is absurd (cf. Karsten, 1786: 23-24, 29-30 & 33-34).

Thereafter, Karsten's work continued with his own proposal, followed by a couple of sections devoted to Newton's and Euler's proposals from which, as it was said before, he actually adapted some procedures and concepts for the development of his. This, in fact, could explain why, firstly, he rejected infinitely small quantities as quantities $= 0$ in strict accordance with his conception of numbers, quantities and mathematics. But, secondly, it could explain why he accepted $\frac{1}{0} = \infty$ not being 0 an infinitely small quantity, even understood in his own way, and at the same time not being 0 strictly 0 but "the last of all the whole numbers", so that $\frac{1}{0}$ was a valid fraction, indeed "the last of all constantly decreasing fractions".

Similarly, in his treatise on negative numbers published in 1786, Karsten elaborated more on this notion although in a different sense, more focused on the recent historical development of it. So, on the one hand, he insisted on: a) the notion oppositeness as that on which the concept of negative quantities rested (cf. Karsten, 1786: 209); and b) the fact that the correct way to refer to such quantities should be as "negatively expressed quantities" (cf. Karsten, 1786: 211) since c) strictly speaking the name 'negative' was related to the assigned sign and "not to the thing itself" (*nicht auf die Sache selbst*) (cf. Karsten, 1786: 207). Even more, he pointed out, the mathematical concepts of "affirmed" and "negated" (*bejahte und verneinte*) "have gradually originated by abstraction" (Karsten, 1786: 205; cf. Schubring, 2005: 138),[94] an observation that must be framed in the historical tenor of his work.

Precisely, on the other hand, his 1786 treatise differs from the previous ones in its historical remarks. "The manner in which older algebraists explain the real nature of the numbers denoted by (–) is certainly not entirely clear as we now require from a writer who wants to explain the groundings of a mathematical science" (Karsten, 1786: 209),[95] he wrote. After which, he reviewed the conceptions of some authors of the 16th, 17th and 18th centuries, such as Styfel, Viète, Descartes, Frans van Schooten, Wallis, Newton, Claude Rabuel, Guillaume de l'Hôpital, María Gaetana Agnesi, Wolff, Euler, Maclaurin, Hausen, Segner and Aepinus (cf. Karsten, 1786: 229-249). Most of the expressions used by these mathematicians to refer to negative quantities, he said, should be understood as coinages (*Kunstwort*) for such new mathematical developments and, thus, should not be interpreted strictly (cf. Karsten, 1786: 234, 239, 241 & 246-247), while it was true that others, as Wolff, had mistakenly assumed those designations referred to the actual nature of the things (cf. Karsten, 1786: 241). Due to the authors with the correct sense of things, he even wrote, it had been possible to achieve the current state of the doctrine of negated quantities in the second half of the 18th century, in spite of which some mathematicians, especially the French, still lacked the necessary rigor observed by the Germanic when bringing forward the elements of the new developments (cf. Karsten, 1786: 249-250; cf. Schubring, 2005: 138).

Those last remarks of Karsten as a matter of fact resemble when, at the beginning of his *Foundations*' volume on arithmetic, geometry, trigonometry and perspective, Kästner wrote that the "newer teachers of geometry [were] often very deviated from the corresponding correctness in proofs", as could be said "especially [of] the French" (Kästner, 1758: 17). Which, in turn, in the general ambit of Germanic mathematicians, highlights an increasing critical

[94] Karsten wrote: "Die Begriffe davon, was man bejahte und verneinte, oder positive und negative Grössen nennt, sind nach und nach, wie die meisten wissenschaftlichen Begriffe besonders inder Mathematik, durch Abstraction entstanden."
[95] Karsten wrote: "Die Art, wie sich die ältern Algebraisten über die eigentliche Natur der mit '(—)' bezeichneten Zahlen erklären, ist freylich nicht so völlig einleuchtend, als wir es jetzt von einem Schrifsteller verlangen, der die Gründe einer mathematischen Wissenschaft erklären wil."

differentiation from their –correct– way of proceeding and, in particular, implicitly reaffirms Kästner's extensive knowledge of the works of contemporary authors by the 1760s.

Concerning the first issue, the postulated contrast between the Germanic and 'foreign' mathematical procedures (mainly French, but also English, as will be discussed later (cf. infra B.3)) rested on a specific understanding of mathematical method: for those Germanics, as Kästner summed it up, "[it had been] described by the method of Euclid and therefore [was] called geometric or Euclidean" (Kästner, 1758: 16),[96] although strictly speaking it was only inspired by Euclid but detached from his proposal. An idea that a) was already present in the work of Wolff, who stated at the very beginning of his *Anfangs-Gründe*'s first section that mathematical method starts with definitions and proceeds with proofs relying on them (Wolff, 1717: 5; cf. B.3.2),[97] and b) underlies the rejection by several Germanic mathematicians (including Segner, Karsten and Kästner) of some both procedural and conceptual new developments mainly defended by foreign authors, such as infinite small quantities, for not being –according to that conception– clearly defined or explained, nor rigorously proven.

As for the second issue, Kästner showed from his early works an extensive knowledge of the works of contemporary authors that was further increased over the years. So, while Karsten only began to discuss the work of some of the above-mentioned authors and some others like Hindenburg and Klügel in his last works (cf. Karsten, 1786 I), Kästner had already referred to several of those authors in the first edition of his *Foundations*, adding new references to them and others throughout the subsequent editions of that work, as he explicitly stated in the preface to the fifth and sixth editions (cf. Kästner, 1800: *Vorrede der 4. 5. u. 6. Auflage*), as well as in his *History of Mathematics* (*Geschichte der Mathematik*),[98] first published between 1796 and 1800. Which, however, did not change the fact that he, as Karsten, only incorporated particular concepts or procedures of the new developments of the time, as, for example, various notions of functions or, with restraints, the one of infinite small quantities, without even adopting partially the general conceptions of which these were part, such as Euler's program.

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[96] Kästner wrote: "Man hat die mathematische Methode besonders nach dem Verfahren des Eucklides abgeschildert, und sie daher die geometrische oder euklidische genannt. Schwerlich wird man sie auch recht kennen lernen; wenn man nicht diesen Schriftsteller, und solche, die ihm getreu folgen, liest."

[97] Wolff wrote: "Die Lehr-Art der Mathematicorum, das ist, die Ordnung, deren sie sich in ihrem Vortrage bedienen, fängt an von den Erklärungen, gehet fort zu den Grund-Sätzen und hiervon weiter zu den Lehr-Sätzen und Aufgaben."

[98] The whole title is *Geschichte der Mathematik seit der Wiederherstellung der Wissenschaften bis an das Ende der achtzehnten Jahrhunderts*.

That way, at least from an alternate edition of his *Foundations*' first volume published in Vienna in 1783,[99] as later in that work's fourth edition, Kästner modified and extended the paragraphs devoted to the theorem that originally stated that, in a given circle, half of a circular arc of it would not differ from its corresponding chord as much as it would the arc itself from its corresponding chord (cf. Kästner, 1758: 246; cf. Annex D). Thus, by 1783 the theorem had been replaced by several propositions expressing the content of that one, focusing in particular on the decreasing difference between the arc and the chord drawn from one to another of its ends when decreasing both arc and chord (cf. Kästner, 1783: 308; cf. Kästner, 1786: 268-288) and making, for the purposes of this work, two extremely significant additions: 1) one could call a chord "infinitely small" when it continually approached to its arc without reaching it, that is, as long as it still had a certain quantity (*so lange sie eine bestimmte Grösse hat*);[100] 2) infinitely small quantities would be precisely those that could be decreased through all the definite values and beyond any given quantity until reduced to nothing (*bis auf nichts abnehmen*) (Kästner, 1786: 279-280; cf. Schubring, 2005: 243-244).[101] This implies that, by the 1780s, Kästner considered infinitely small quantities as variables and inasmuch as they quantified and, therefore, as other Germanic authors including Segner and Karsten, he was reluctant to identify them with 0, considered this one the limit of those.

Such additions, moreover, are not only significant for their content and time, but also for their location, namely the *Foundations*' section on geometry in an alternate edition. So, with respect to the section, while it is true that even in the first edition of the work the content of the theorem of §41 suggested the consideration of the decreasing difference between chord and arc as an infinitely small quantity (cf. Kästner, 1758: 250-251), the fact is that Kästner just elaborated on that notion within the geometric framework some decades later, when those terms were more common and accepted. Infinitely small quantities, it has been pointed out,

[99] While the second edition of 1764 does not contain the mentioned modification, as it does the fourth from 1786, it is highly probable that the third edition from 1774 already contained it since a) the alternate edition published in Wien in 1783 does and b) some fragments of the *Nachricht* for the third edition mention specific changes following Euler. However, having not been able to obtain a copy of the third edition, it shall suffice with the mention of such possibility in this footnote.

[100] Kästner wrote: "In dieser Bedeutung sagt man: eine unendlich kleine Sehne, sey ihrem Bogen gleich, verliere sich in demselben, und sey um den Halbmesser vom Mittelpuncte entfernt. Man nennt hie unendlich klein einen Zustand dem sich nach (II) die Sehne beständig nähert, ihn noch nicht erreicht hat, so lange sie eine bestimmte Grösse hat, aber ihm so nahe kommen kann als man will, weil sie von einer bestimmten Grösse, durch alle kleinere, bis auf nichts abnehmen kann."

[101] Kästner wrote: "Dieses Unendlichkleine bedeutet also dass eine Grösse, durch alle bestimmte Werthe bis auf nichts abnehmen, und folglich kleiner werden kann, als jede Grösse so klein sie auch angegeben wird."

pertained to a different framework whose study was to be carried out in the third part of his work, precisely devoted to the foundations of the analysis of finite and infinite quantities.

Additionally, Vienna's edition of Kästner's *Foundations* highlights the implementation of some structural changes in his work from the 1780s by including a brief additional section, between that of arithmetic and that of geometry, included in the index under the name of "algebra" and which dealt with the arithmetic and geometric progressions (cf. Kästner, 1783: 198-204). Consequently, those topics were included in the volume on the basic parts of mathematics instead of being included in a volume on the analysis of finite quantities, to which they pertained (cf. Kästner, 1760), and they were brought together as an intermediate or transitional section between arithmetic and algebra.

The reverse structural change, scilicet not a merger but an expansion, was mentioned by Kästner himself in the prefaces to the fourth and fifth edition of the first volume of his *Foundations*. In the first one, he pointed out that the title of that new edition indicated it was the first division of the first part of the foundations of mathematics because of a new work on the art of reckoning (cf. Kästner, 1786 I: *Vorrede der vierten Auflage*). In the second one, he mentioned the inclusion of references to his recently published geometrical treatises (cf. Kästner, 1792: *Vorrede der 4. u. 5. Auflage*). That way, the first additional volume was a sequel of the section on arithmetic devoted to its applications and the two subsequent volumes of 1790-91 were the corresponding sequels of the geometric section.

Beyond what those works structurally entailed, however, their content provides evidence of those other aforementioned changes he implemented over the years, both on the notions of quantity and number, as well as concerning his reference to authors and developments of the last decades of the 18th century. To illustrate this, in his volumes on applications of geometry, for example, Kästner quoted Legendre, criticizing his procedure for calculating the surplus *w* in the angular sum of a spherical triangle by means of infinitely small twisted triangles *(unendlich wenig krumme Dreyecke*) between that and the corresponding plane triangle (cf. Kästner, 1791: 453ff.); and he criticized Johann Andreas Christian Michelsen, due to his reflections on the positivity or negativity of a circle's arcs' secants given the usual notions of positive and negative lines (*die gewöhnlichen Begriffe von positiven und negativen Linien beybehalten*) (cf. Kästner, 1790: 459-460). But, additionally to his references to those and other contemporary developments, he used the appellation “natural numbers” at the beginning of the first one to refer to the –positive– whole numbers when defining trigonal numbers (cf. Kästner, 1790: 5).

Kästner’s increasing use of the denomination of natural numbers, as a matter of fact, is amply evidenced in his volume on applications of the art of reckoning. There, for example, he stated that the simplest example of arithmetic series was the one of “whole natural affirmed numbers whose first member $= 1$ [and whose] difference also $= 1$”, adding a few lines below that Wolff’s general explanation of logarithms seemed to him unnecessary, being only necessary for them the series of natural numbers (*die Reihe der natürlichen Zahlen*) (Kästner, 1786 II: 41).[102] This was a clear –though intuitive– presentation of the notion of natural numbers (cf. Kästner, 1786 II: 83, 98), although he still replaced most of the time that denomination by that of “whole affirmed numbers” or even simply “whole numbers”, depending on the context. So, while in that work he explicitly defined the emerging notion of natural number and, in doing so, he implicitly acknowledged the modifications in both the notions of number and quantity (since the positivity of numbers only arose once considered the numerical correlate of negative quantities), he only implicitly suggested his conception on the mathematical infinite was the same as in his previous works: $y = \frac{1}{0}$ is called infinite, he wrote at one point, writing elsewhere that a sum of an infinite series of fractions was the quantity which this series, when continued to infinity, would approach as much as one wanted so that no difference could be made between it and the sum itself, as for example $\frac{1}{2} + \frac{1}{4} + \frac{1}{8} + \cdots = 1$ (cf. Kästner, 1786 II: 512 & 59, respectively).[103] After all, the proper place to study such quantities was his work on analysis.

His second 1786 work, nevertheless, was not only a continuation of his previous stances but, as his geometrical treatises, it also incorporated new developments, among which it must be mentioned here Hindenburg’s “completely new method [...] to find numbers”, whose presentation of 1776, he said, contained “a lot of new and instructional [things]” (cf. Kästner, 1786 II: 567; cf. Hindenburg, 1776). His reference to Hindenburg’s proposal, though vague,

[102] Kästner wrote: “Das einfachste Beyspiel ist die Reihe der ganzen natürlichen bejahten Zahlen, deren erstes Glied $= 1$ der Unterschied auch $= 1$ ist.”
[103] Kästner wrote: “Ich habe an diesem Exempel zeigen wollen, was man Summe einer unendlichen Reihe von Brüchen nennt. Es heisst eine Grösse, der diese Reihe, ins Unendliche fortgesetzt, so nahe kommen kann, als man will, dergestalt, dass sich kein Unterschied zwischen dieser Grösse und der Summe der Reihe angeben lässt.”

mentioned both Hindenburg's work of 1776 and that of 1778, as well as the board included in the first by means of which numbers could be displayed in a far-reaching order (cf. Kästner, 1786 II: 567-568; cf. Hindenburg, 1776: [*Beilagen*]).[104]

One of the improvements that Kästner carried out in the third edition (1794) of his *Foundations*' volume on the analysis of infinite quantities was precisely related to the section on combinations included since the first edition. There, he defined combinations as connections established between the components of a given amount or set of things (*Menge von Dingen*). For example, given the set of the first five letters of the alphabet, its combinations' table would be:

	I	II	III	IV	V
1	a	0;	0;	0;	0;
2	b	ab;	0;	0;	0;
3	c	ac; bc;	abc;	0;	0;
4	d	ad; bd; cd;	abd; acd; bcd;	abcd;	0;
5	e	ae; be; ce; de;	abe; ace; bce; ade; bde;	abce; abde; acde; bcde;	abcde.

(cf. Kästner, 1760: 397-398)

But, after proceeding to explain the formulas to calculate the possible combinations for each column (pairs, triads, etc.), as for example $\frac{5\cdot(5-1)}{1\cdot 2}$ for those letters' pairs, Kästner expanded the paragraph on their applicability for the calculation of probabilities in games (§745) and added a new one on the relation between the binomial theorem and combinations (§748). In the first case, he went on to explain how the nature of each game determined all possible cases of combinations in it, adding to the initial references in the subject (Jacob Bernoulli and de Moivre) new ones to Robert Gaeta and Gregorio Fontana, Carl Chassot de Florencourt, Johannes Nikolaus Tetens, Charles François de Bicquilley, Christian Michelsen and Georg Christoph Lichtenberg (cf. Kästner, 1794: 528-532).

[104] The board, a sort of table, consisted of a series of panels deployed in horizontal lines vertically ordered, so that when introducing in the board a sheet that contained a table of –prearrenged– numbers, the panels showed the factors of a given number (cf. Annex E). In fact, the full title of Hindenburg's work is: *Beschreibung einer ganz neuen Art, nach einem bekannten Gesetze fortgehende Zahlen, durch Abzählen oder Abmessen bequem und sicher zu finden, nebst Anwendung der Methode auf verschiedene Zahlen, besonders auf eine darnach zu fertigende Factorentafel, mit eingestreuten, die Zahlenberechnung überhaupt betreffenden Anmerkungen*.

In the second case, Kästner pointed out that, concerning the binomial theorem, combinations involved its coefficients in the case of both equations and series, although as one advanced in the series its composition became very large and the calculation of its coefficients very laborious (cf. Kästner, 1794: 535).[105] Precisely, he added, Hindenburg "[provided] great benefits to Analysis by applying combinations and skillful designations", as his works (1778, 1779 & 1781) and the ones of Hieronymus Christoph Wilhelm Eschenbach (1789), Ernst Gottfried Fischer (1792), Heinrich August Töpfer (1793) and Heinrich August Rothe (1793) would show (Kästner, 1794: 535-536). While not as vague as his mention from 1786, but still brief due to "the arrangement and limits of his book" (Kästner, 1794: 535), this new description of such recent investigations highlights the importance of combinatorial analysis among Germanic mathematicians of the late 18th century.

Additionally, as Kästner himself noted in the reminder –historical– lines of that third edition of his *Foundations of the analysis of finite quantities*,[106] not only individual researches had been added in that work, but also various applications, references and even structural changes differentiated it from the two previous editions (1760 & 1767). Among them all, however, and given the objectives of this work, the modifications that stand out are the ones Kästner made to the section on the expressions of functions by infinite series, since they sum up quite well his increasing acquaintance with new developments in the subject:

Ausdrückungen der Functionen durch unendliche Reihen		
1794	1767	1760
• After explaining that the "equation P" or simply "(P)", $0 = a + bz + cz^2 + dz^3 + \cdots$, whose exponents for the powers of z are "affirmed whole" numbers, may end or continue without end, he	Yes (cf. 313)	No (cf. 305)

[105] Kästner wrote: "Da wird immer der folgende aus den vorhergehenden bestimmt, wenn man in der Reihe weit fortgeht, wird die Zusammensetzung sehr gross, ihr Gesetz schwer zu übersehn, und des Coefficienten Berechnung sehr mühsam. Besonders empfindet man diese Schwürigkeit in der Rechnung des Unendlichen, wo Reihen häusig gebraucht werden."

[106] Kästner's lines on the history of the name of algebra go from the Arabic origin of the first syllable to Viète's and Descartes' steps towards modern algebra, explaining in between, for example, how the Spanish use of the term "Algebrista" during the 17th century as "a person who heals leg fractures and disclocations" (*ein Mann der Beinbrüche und Verrenkungen heilt*) referred to other meaning of "al-jabr", namely restoration or reduction, as a fragment of Don Quixote shows: "Als Don Quixote den Spiegelritter vom Pferde stiess, blieben des Gefallenen Rippen nicht völlig in ihrer Ordnung, und er musste sich an einem Orte aufhalten, wo ein solcher Arzt ihn wiederum zurechte brachte... llegaron à un puèblo, donde fuè ventùra hallàr à un Algebrista con quièn se curò el Sanson desgraciàdo" (Kästner, 1794: XIII). Similar lines can be found in the first volume of his *Geschichte der Mathematik* (cf. Kästner, 1796: 56ff.).

clarified that this was not the more general expression as it was $0 = A + Bx^d + Cx^{2d} + \cdots$, for an affirmed whole d, with $x^d = z$ for a fractional number and $\frac{1}{x} = y$, $0 = A + By^e + Cy^e + \cdots$, for a negated $d = -e$ (cf. 408).		
• When he explained obtaining of square root of $1 + x$ by infinite series, he introduced a few lines on the process to show how, after obtaining the largest square number contained in it, the smaller x is, the more accurate the value of the square root will be, adding that "this approximation will always be more convenient than the work of ordinary arithmetic" (cf. 410).	No (cf. 314)	No (cf. 306)
• The paragraph corresponding to the "Addition 627", which originally stated that when the coefficients in a series decrease very little, the series will consequently [converge] slowly, was entirely modified in the edition of 1767 and later improved in 1794. First, given a broken number $r = \frac{m}{n}$, with m and n whole numbers and the series $(z + e)^{\frac{m}{n}} = z^r + (A \cdot e \cdot z^{r-1}) + (B \cdot e^2 \cdot z^{r-2}) + \cdots$, according to the binomial theorem $(z + e)^m = (z^r + A \cdot e \cdot z^{r-1})^n + n \cdot (z^r + A \cdot e \cdot z^{r-1})^{n-1} \cdot Be^2 \cdot z^{r-2} \ldots$ (cf. 411-412). Second, after considering the case for negative exponent, $(1 + y)^{-r}$ for which $\frac{y}{1+y} = z$ or $\frac{z}{1-z} = y$, having $1 + y = \frac{1}{1-z}$ and $(1 + y)^{-r} = (1 - z) = r1 - rz^{r-1} \ldots$, he mentioned the works of Colson, Walz and Euler on the subject and insisted on the importance of the connection between the series and the binomial theorem since, he said, it should not be forgotten that an infinite series underlies the "finite" expressions used to obtain the quantity sought (cf. 412-416).	Partially (cf. 315-320)	No (cf. 307)

Therefore Kästner, though he did not even partially reorganize analysis according to new trends that by the late 18th century were no longer only moderately known among Germanic mathematicians as in the middle of the century, he did gradually incorporate improvements in the contents on the subject. As the comparative table suggests, this was clearer with regard to the infinite quantities, whose study strictly corresponded to his *Foundations*' second volume on analysis.

Thus, in the third edition of that work, Kästner not only added –with respect to the previous two editions (1761 & 1770)– two sections on the theory, applications and history of the calculus of variations, but also some significant modifications. For example, the original definition of infinitely small quantity as that which can be smaller than any other given quantity, decreasing infinitely or vanishing towards a certain limit, was reformulated and expanded: emphasis was made on 0 as the only limit to which an infinitely small quantity could come as close as desired, until no quantity existed anymore, since "beyond nothing it would be negated [and] would again be a quantity, only that opposed to the former, and this quantity would always grow"; and he introduced the example of an arc growing from nothing to the quadrant cosine, as well as references to his previous works (Kästner, 1799: 2-3; cf. Kästner, 1761: 2; Kästner, 1770: 2).[107]

Similar clarifications and additions can be found throughout the entire work, of which, nonetheless, solely three more will be mentioned here. First, and following the aforementioned definition, Kästner's original explanation of the exercise of finding the limit z to which a quantity u that grows infinitely approaches, with $au + b = z$, was modified since the second edition of 1770: instead of addressing the exercise from placing $x = \frac{1}{u}$ and $z = \frac{a+bx}{x}$, with x as a quantity that could decrease infinitely (something that, it must be remembered, he later pointed out could lead to mistakenly assume $\frac{n}{\infty} = 0$), he considered the process towards 1, given $\frac{au+b}{au} = 1 + \frac{b}{au}$, with $\frac{b}{au}$ decreasing infinitely and $1 + \frac{b}{au}$ approaching infinitely to 1 but still distinguished from it by a quantity e (Kästner, 1770: 3-4; Kästner, 1799: 4-5; cf. Kästner, 1761: 3).

Second, when addressing the "general proof of the binomial theorem by the differential calculus", he modified the note in which he not only mentioned Johann Bernoulli's attribution to Pascal as originator of the theorem, but also Jakob Bernoulli's statement about its connection with the doctrine of combinations, as well as the works of Clairaut, Segner and Newton on the subject (cf. Kästner, 1761: 32-33; Kästner, 1770: 35-36). In contrast, in that work's third edition,

[107] Kästner wrote: "Gränze des Abnehmens lässt sich hie keine andre denken als: Nichts. Eine Grösse die unendlich klein wird kann also der 0 so nahe kommen als man will. [...] Man betrachtet hie das Abnehmen nur bis auf Nichts, bis die Grösse nicht mehr vorhanden ist, nicht über Nichts hinaus dass sie verneint würde denn da hätte man wiederum Grösse, nur der vorigen entgegengesetzt, und diese Grösse wüchse immer."

Kästner mentioned those authors, as well as previous (scilicet Michael Stifel and Christopher Clavius) and later (such as Simon Antoine Jean L'Huilier, H. A. Rothe and Hindenburg) ones (cf. Kästner, 1799: 63-65). This, coupled with the final section's note on Hindenburg's works and the extensions gained by the analysis due to the combinatorial analysis developed by that author and others, as Tetens, Klügel and Pfaff (cf. Kästner, 1799: 79), reinforces the thesis about the importance of this proposal among Germanic mathematicians of the time.

Finally, last but not least, Kästner modified the section on the ratio of the ordinate to the sub-tangent, $MP : PT$, concretely the paragraph that previously stated that the sub-tangent and the beginning of the abscissae laid on the same or different side of the ordinate depending on whether sub-tangent and ordinate had the same or different signs (cf. Kästner, 1761: 52; Kästner, 1770: 56). As the previous paragraphs, this one sought to cope with the underlying problem to the traditional formula of the sub-tangent of a curve, $S = y\frac{dx}{dy}$, which was assumed positive as neither x nor y were considered as independent variables: although that formula was correct in general, that is, considering the line's position and the quantities involved in general terms, certain cases, as those referred in the aforementioned paragraphs, required some clarifications to account for the negativity arising within a relational framework. In other words, since that formula, though general, was not entirely correct, clarifications were made here and there to overcome its difficulties. That way, Kästner modified §83 to point out that, while a) given an increasing abscissae and a curved line distant from it, both the former and the sub-tangent would lay on one side of the ordinate, b) when the curved line approached to the abscissa line, this latter and the sub-tangent would lay on opposite sides. But he also emphasized that in calculating the value of $y \cdot \frac{dx}{dy}$ regardless of whether it is affirmed or denied (*bejaht oder verneint*), the only remark to be made was on the sub-tangent's position, indicated by its affirmed or denied value (Kästner, 1799: 87).[108]

As a matter of fact, Hindenburg published in the *Archiv der reinen und angewandten Mathematik* (of which he was editor) a letter from Klügel on the subject and some additional lines that he, the former, wrote about it. Leaving aside the details of both writings, which will be

[108] Kästner wrote: "Wenn man also nur den Werth von $y \cdot \frac{dx}{dy}$ berechnet, ohne darauf zu sehn ob dieser Werth bejaht oder verneint ist, so giebt nur beygebrachte Bemerkung, die Lage der Subtangente, welche Lage auch dadurch angedeutet wird, ob der Werth bejaht oder verneint ist."

discussed in the last section of this chapter (cf. infra B.3.2), the reason for Klügel's letter was the criticism made by Friedrich Gottlieb von Busse in his *Sub-tangent and sub-normal, tangent and normal line's formulas, amended and more carefully explained than usual*,[109] a work published in Leipzig in 1798. There, von Busse mentioned Klügel (cf. Busse, 1798: 34) among the authors who continued using the incorrect formula $S = y \cdot \frac{dx}{dy}$ instead of the correct one, namely $S = -y \cdot \frac{dx}{dy}$, which would allow to consider not only the position of the line but also its direction and avoid false results for curves as the other one did not (cf. Busse, 1798: 6-7; Schubring, 2005: 490-491). Hindenburg, however, supported Busse and in a note mentioned that Kästner himself wrote a review of that work in the *Göttingische Anzeigen von gelehrten Sachen*, recognizing Busse´s attainment (cf. Kästner, 1798: 2013-2014). Even more, Hindenburg wrote, von Busse's work was precisely the reason why Kästner made the above mentioned change to §83, although limited and still wrong for some cases (Hindenburg, 1800: 344).

What this last example makes manifest, however, not only has to do with the particular development of the sub-tangent formula but with the very evolution of Kästner's mathematical ideas. It was not only that, despite accepting that until then he and so many others had made the corresponding calculations aided by an incorrect formula, he was still reluctant to employ the correct formula or incapable to carry out all the necessary corrections in his 1799 work. The point is that Busse's formula entailed a further step in analytical geometry and ultimately a deeper change in analysis, one that, in fact, would have significantly modified his analytical framework: to consider $S = -y \cdot \frac{dx}{dy}$, instead of considering $S = y \cdot \frac{dx}{dy}$ plus its amendments, would have meant to consider algebra detached from geometry in a way that for Kästner and many of his Germanic contemporaries was not detached. For these authors, the notion of 'function', and the one of 'variable quantity' that it involved, strictly speaking were not at the core of algebraic analysis or the analysis of finite quantities, as differential and integral calculus did not constitute themselves the framework of analysis of the infinite but were developed within a more general framework of calculus outlined by the infinite large and small quantities. After all, for them the –continuous– variability of a quantity was interpretable primarily in geometric terms, as it was its limit, and so geometrical methods still determined analysis, while Busse's step required treating geometry in an algebraic way foreign to those authors.

[109] The original title is *Formulae linearum Subtangentium ac Subnormalium, Tangentium ac Normalium, et castigatae et diligentius quam fieri solet explicatae*.

Ultimately, even though in a sense the modifications carried out by those authors were in the right direction, so to speak, that step required a notion of quantity that rather than being inherently geometric, at least attempted to be inherently analytical.

Furthermore, what the passages on the combinatorial analysis and the sub-tangent formula also highlight is the emergence of new generations in the Germanic mathematical scene in the late 18th and early 19th centuries. Authors that were in some cases sympathetic to the combinatorial analysis or even involved in its development, as the mention of Rothe, Klügel and Pfaff, along with Hindenburg, shows. But also authors that were more or less attached or detached to the previous generation's notions and procedures, as the dispute between Busse and Klügel shows, among which Kästner additionally quoted Ludolf Hermann Tobiesen, Bernhard Thibaut, Carl Schulze, Karl Friedrich Hauber, (Kästner, 1799: 59, 156, 215, 239, 754, respectively), Johann Andreas Christian Michelsen, Ernst Gottfried Fischer, Hieronymus Christoph Wilhelm Eschenbach, Heinrich August Töpfer, Johann Tobias Mayer (Kästner, 1794: 530, 536, 557, respectively), Johann Carl Schulze, Mathias Metternich and Johann Ephraim Scheibel (Kästner, 1800: 343, 490, 336, respectively).

In fact, while not all of those authors but several of them studied with Kästner, they all were more or less acquainted with his work, mainly his *Foundations*' volumes, which were widely read during the second half of the 18th century (as reflected in its many editions). Such demand for these books, in contrast to the demand for the ones of any of his contemporaries, was related both to their content and external factors, such as his position as professor of mathematics at Göttingen for almost the entire second half of that century and the role he played in Germanic publications and Academies of the time. In order to clarify how influential was Kästner among those new generations of mathematicians, the last section of this work attempts to give a glimpse on the evolution of the Germanic mathematics during the last third of the 18th century and the first years of the 19th century, when Bolzano published his first mathematical work.

B.3. The impact of Kästner's mathematical ideas on subsequent generations of Germanic mathematicians

In 1771, the *Royal Academy of Sciences of Göttingen* (*Societas Regia Scientiarum Gottingensis*) published the first volume of its new scientific journal, *Novi Commentari*. This comprised works from 1769 and 1770, including one by Kästner on d'Alembert's (an "*adversarius minor*" of Bernoulli) objections to Johann Bernoulli's *Nouvelle Hydraulique* (cf. Kästner, 1771: 45-89). That same year, in a letter to d'Alembert dated on December 16, Lagrange explained to him that the reason for sending him that volume was not only that it contained some geometry *Mémoirs*, which would give him an idea of this science's state in the Germanic states, but specially because it included the aforementioned defense written by Kästner, "who", he wrote, "has a high reputation in Germany as a geometer and as a writer", adding: "you will judge how well this double reputation is founded by the mere reading of the book of which I speak to you; you will see that the author pretends to dazzle by the intellect and the pleasantry, and you will double up with laughter" (Lagrange, 1882: 222).[110]

That such animosity was towards Germanic mathematics and not exclusively towards Kästner is evidenced by both the subsequent lines of Lagrange, promising to send d'Alembert the following volumes of that journal if only to enlarge his library, as well as by d'Alembert's response, criticizing the "very weak" geometric content of the whole volume and Kästner's shallow (for its content) and ridiculous (for its form) work (Lagrange, 1882: 225).[111] An animadversion between French and Germanic mathematicians that, it was said before, was reciprocal at least during much of the second half of the 18th century. Broadly speaking, while for the first the procedures and notions of –most of– the Germanic mathematicians were retrograde and even mediocre, for these ones those of –most of– the French mathematicians were not rigorous, incorrect and even useless.

[110] Lagrange wrote: "vous jugerez combien cette double réputation est fondée par la simple lecture du Mémoire dont je vous parle; vous verrez que l'auteur y prétend aussi briller du côté de l'esprit et de la plaisanterie, et vous vous tiendrez les côtes de rire."
[111] D'Alembert wrote: "Ce Volume me paraît bien faible de Géométrie, comme à vous. La pièce de Kaestner contre moi est, ce me semble, bien mince pour le fond et surtout bien ridicule pour la forme. Je ne sais si elle vaut la peine que j'y réponde."

Leaving aside 'the form' of the dispute, while continuing to address its 'content' throughout this work, the quoted lines from Lagrange highlight other things besides the qualitative difference established between the French and Germanic mathematics. It was still comon, for example, the use of the French appellation "*géometre*" to refer to a mathematician (cf. Klügel, 1808: 624), a synonymy which is not a minor detail considered that it emphasizes (though not necessarily intentionally) the attachment of mathematics to geometry which in general endured for a few more decades and was stressed at the beginning of the 19th century, among others, by Bolzano. In addition to which, Lagrange's letter provides the testimony of a foreigner about the importance attributed to Kästner in the Germanic territories in the early 1770s. The latter, nevertheless, must be substantiated both temporally and spatially, as well as both inside and outside Göttingen: was Kästner as influential as Lagrange's testimony suggests until 1770 and, even more, how influential was he during the subsequent decades?

B.3.1. Scope and variety of Kästner's influence through his published works

By 1800 the Electorate of Brunswick-Lüneburg (Hanover), officially confirmed in 1708 and ruled in personal union with Great Britain since 1714, had acquired the surrounding territories of Saxe-Lauenburg and Bremen-Verden and had won Land Hadeln. As a result, just over a century after its creation, Hanover had unified several of the territories that previously conformed the Duchy of Brunswick-Lüneburg, with the significant exception of the Principality of Brunswick-Wolfenbüttel, which divided the Principality of Göttingen from the rest of the Electorate (cf. Whaley, 2012 II: 49). So, while the university of Göttingen, as said before, belonged to the British-Germanic Electorate of Brunswick-Lüneburg despite its geographical division, the university of Helmstedt (Protestant) belonged to the Germanic Principality of Brunswick-Wolfenbüttel, closely related to Prussia.

There, precisely, at Helmstedt, two Göttingen alumni taught mathematics and history the same year that Kästner died, namely Johann Friedrich Pfaff and Julius August Remer (cf. Zimmermann, 1889: 198), respectively. But, while the former was a student of Kästner, which is reflected in his work, the latter was not and in his *Textbook of general history for academies and gymnasiums*

(*Lehrbuch der algemeinen Geschichte für Akademien und Gymnasien*), in the section devoted to the development of mathematics from 1740 to 1800, he only mentioned Kästner for his promotion of the study of hydrodynamics, hydraulics, and hydrostatics through his textbooks, as well as for his analytical treatment of perspective in these ones (Remer, 1800: 657). By contrast, just as Remer listed Newton, Leibniz and Bernoulli as the most outstanding mathematicians of the previous period (1660-1740) (Remer, 1800: 574), when summarizing the development of mathematics during the second half of the 18th century he began commenting on the extension of analysis by "new methods", such as the ones of Lagrange (calculus of variations) and Hindenburg (combinatorial analysis), quoting then d'Alembert, Clairaut, Laplace, Condorcet, Frist, Fontana, Karsten, Klügel and Pfaff among the most remarkable analysts of the time (Remer, 1800: 657).

Remer's brief description of mathematics during the second half of the 18th century is highly significant because, in spite of not coming from a mathematician, it shows an accurate acquaintance of the overall development of that science. His reference to Germanic and French mathematicians of that period, most of them from subsequent generations to Kästner and Karsten, and his mention of both the calculus of variations and the combinatorial analysis reflects no animosity towards 'foreign' mathematical developments and also the importance of Hindenburg's proposal among Germanic mathematicians by the end of the century. While, on the other hand, his omission of Kästner among the relevant mathematicians of his time (and his sole mention due to his textbooks) provides evidence of the decline of his influence among Germanic mathematicians towards the end of the 18th century, at least with regard to the core area in mathematics.

Additionally, Remer's lines on the history of mathematics are not only important because of who he was, a professor of history who spent some time at Göttingen, what he said and when he wrote them, but also by their publication context: Helmstedt, a Germanic territory that, unlike Göttingen, was a pro-Prussian one. Was Kästner influential in the Germanic territories in general, regardless of their location or political and religious affiliation? Even more, was his influence constrained to particular issues and was he perceived more as an author of mathematical textbooks than as a mathematician?

To the northeast, at the university of Königsberg (Protestant), a territory recovered by Prussia after the Seven Years' War, two professors taught mathematics during the second half of the 18th century, scilicet Friedrich Johann Buck and Johann Friedrich Schultz. While both, as Kant, apparently used Wolff's texts for their public lectures and courses on mathematics, the latter also used Euler's texts (cf. Klemme & Kuehn, 2016: 107 & 701, respectively). However, the works of Schultz reveal as well the influence of contemporary Germanic authors and an interest in foundational issues not entirely common among Germanic mathematicians. Regarding the latter, in 1790 his book on the *Foundations of Pure Mathesis* (*Anfangsgründe der reinen Mathesis*) was published, whose "Prolegomena" were devoted to the definition, object, classification, use and method of mathematics, focusing on this last section in the correctness of mathematical methodology, i.e. the correctness of its concepts and proofs (cf. Schultz, 1790: 16-26). Concerning the former, his 1788 work on infinity, written for the 1786 Berlin Academy prize for an essay on the subject, constantly resorted to the considerations of Karsten and Kästner on the infinite large and small quantities (cf. Schultz, 1788).

On the side of cons, to the southeast, in the Germanic territories of Buda and Pest, parts of the former Kingdom of Hungary that by the end of the 18th century were under Habsburg administration but were not part of the Holy Roman Empire, Ivan Paskvić, professor of mathematics at the university Eötvös Loránd, published in 1799 a work on *Static-mechanical principles built upon the analysis of infinite quantities* (*Opuscula Statico-Mechanica Principiis Analyseos Finitorum Superstructa*) and in 1812-13 a couple of volumes on the *Foundations of the whole theoretical mathematics* (*Anfangsgründe der gesammten theoretischen Mathematik*). In both cases, Paskvić opted for the proposal of Euler –and the authors akin to it– on integral and differential calculus. Thus, in his work of 1799 he quoted the works of Newton, Leibniz and Euler, but also the ones of Michelsen, Lagrange, Lacroix and l'Huilier (cf. Paskvić, 1799: IX-XXVI), followed the structure of Euler's work (beginning by the definitions of constant and variable quantities and function) and only mentioned Kästner and Karsten as additional references in the subject (cf. Paskvić, 1799: 3ff. & XXIV-XXV, respectively). While, in his volumes of 1812-13, both in the section on "the basic principles of the theory of analytical functions" (cf. Paskvić, 1812: II Theil, 85ff.) and in the sections on "the first elements of integral and differential calculus" (cf. Paskvić, 1813: 111ff.) he clearly followed Euler's proposal; while, in the section on elementary mathematics, despite some modifications, he followed the usual Germanic presentation (cf. Paskvić, 1812).

Something similar happened in two other Germanic territories ruled by the Habsburg Monarchy, namely Praha and Vienna. Firstly, at the university of Praha the math teachers of elementary and higher mathematics, Stanislav Vydra and Franz Joseph Ritter von Gerstner, respectively, employed for their courses, according to Alena Šolcová, the works of Euler, the latter, and of Kästner, the former (Šolcová, 2010: 3 & 10). Vydra, as a matter of fact, acknowledged his debt to Kästner's *Anfangsgründe der angewandten Mathematik* at the beginning of his *Propositions from Mechanics* (*Sätze aus der Mechanik*), although he also recognized his use of the books of Wolff, Claude-François Milliet de Chales, Nicolas Louis de La Caille, Sturm, Karsten, Jakob Bernoulli, Eberhard, Walcher and Sprengel (Vydra, 1795: 3-4); concerning calculus, he relied on the lessons of Johann Bernoulli (cf. Vydra, 1783). Secondly, at the university of Vienna the professor of higher mathematics was the Jesuit Karl Scherffer, whose *Analytic Instructions* (*Institutionum Analyticarum*), even more than his *Treatise on Series* (*Abhandlung von Serien oder Reihen*, 1782), make clear such duality: the parts devoted to arithmetic and the analysis of finite quantities follow the common Germanic presentation of those subjects, as for example the algebraic-geometrical conception of negative quantities (cf. Scherffer, 1770), while his presentation of infinitesimal calculus explicitly follows Euler's proposal (cf. Scherffer, 1771 & 1772).

Kästner's works might have been highly influential among Germanic mathematicians during the first decades of the second half of the 18th century, including not only the first and fourth part of his *Foundations of mathematics* (on elementary mathematics and mechanics, respectively), but also its third part on the foundations of the analysis of finite and infinite quantities which, Conrad Heinrich Müller states, "were immediately used at the lectures in other universities" after their publication (Müller, 1904: 65). However, as those 'peripheral' examples suggest and as further ones throughout the Holy Roman Empire reaffirm, by the end of the 18th century the influence of Kästner among new Germanic mathematicians was not a matter of the political or religious circumscription of the authors who referred to him, nor it was entirely related to the content of his works quoted by them, but in many cases it was a matter of whether those authors had studied with Kästner or with someone who in turn had studied with him.

Exceptions to this are, for example, Johann Sigismund Gottfried Huth, professor of mathematics at the university of Frankfurt (Oder) and later at the Russian-Germanic university of Dorpat, and Johann Andreas Christian Michelsen, a former student at Halle and professor of mathematics at the *Berlinischen und Cöllnischen Gymnasium*. Huth, Karsten's student at Halle, recognized in the preface to his *Foundations of applied mathematics* (*Anfangsgründe der angewandten Mathematik*) the value of the textbooks of Kästner and Karsten (his "most distinguished" teachers), which, he said, at the time he still used in his courses along with the works of Klügel, Lorenz, Eberhard and Scheibel (Huth, 1789: *Vorrede* [10-11]). Michelsen, German translator of Euler's works, although he relied on Euler in his works on analysis (cf. Michelsen, 1790 I & II), he constantly stated the relevance of Kästner's and Karsten's works especially but not exclusively regarding elementary mathematical issues (Michelsen, 1786: XVI-XVII; Michelsen, 1789: 10ff.).

On the other hand, Andreas Boehm, at the university of Giessen (located in the HRE's Landgraviate of Hesse-Darmstadt), Christian Gottlieb Zimmermann in Berlin, Johann Friedrich Raupach at the Prussian-Silesian *Liegnitzer Ritterakademie*, and Christian Gottfried Ewerbeck at Halle and later at the *Akademische Gymnasium Danzig*, did not include Kästner among their main references. Boehm taught metaphysics, logic and mathematics and, as he made clear at the beginning of his *Logic* (*Logica in usum auditorii sui Ordine Scientifico*), he was a supporter of Wolff's logical-mathematical ideas (Boehm, 1762: 4; cf. Klemme & Kuehn, 2016: 86-87). Zimmermann, professor of mathematics at the *Friedrichswerdersche Gymnasium*,[112] quoted the presentations of opposite quantities of Karsten and Klügel as the more appropriate and highlighted the inclusion of Hindenburg's combinatorial analysis in his *Development of analytical principles for the initial lessons in mathematics* (cf. Zimmermann, 1805: XX-XXI & XXII-XIV);[113] Raupach, professor of mathematics, in his *Elements of algebra and analysis and their application to geometry* (*Die Elemente der Algebra und Analysis, nebst ihrer Anwendung auf die Geometrie*) followed the usual Germanic presentation in the sections on common arithmetic, while the sections on algebra and analysis were closer to 'foreign' presentations such as Euler's (cf. Raupach, 1815: IX, 63 & 121); and Ewerbeck, professor of mathematics, in his book on *The*

[112] Zimmermann was a mathematician that attended the university of Königsberg, where, according to the *Allgemeine Deutsche Biographie* entry, he became close to Schultz and Kant (Cantor, 1900: 251).
[113] The whole title is *Entwickelung analytischer Grundsätze für den ersten Unterricht in der Mathematik besonders für diejenigen welche sich ohne mündliche Anweisung darüber belehren wollen*.

Similarity between pure mathematics and logical philosophy (*De Similitudine inter Mathesin puram atque Philosophiam logicam*), quoted Segner's *Anfangsgründe* as his main mathematical reference and considered as references in the topic Lambert, Kant, Eberhard and Michelsen, among others (cf. Ewerback, 1789: 8 & 21-23).

As for other important Germanic cities, universities and professors of mathematics, Kästner was among the main references, but as in the case of Pfaff, most of those professors had studied with him. Such are the cases of Klügel, Hindenburg, Metternich, Johann Tobias Mayer, Langsdorf, Brandes, Thibaut and even Lichtenberg and Tralles. The following section will be specifically devoted to highlight the extent of Kästner's influence among some of those new Germanic mathematicians.

B.3.2. Kästner's students and the changes in Germanic mathematical conceptions and practices toward the late 18th and early 19th centuries

Georg Simon Klügel, since 1767 professor of mathematics at the university of Helmstedt and successor of Karsten at Halle in 1788, is perhaps the best-known pupil of Kästner, both for his dissertation on the theory of parallel lines, *Recension of the main endeavors to demonstrate the theory of parallel lines* (*Conatuum praecipuorum theoriam parallelarum demonstrandi recensio*) (cf. Klügel, 1763), and for his *Mathematical dictionary* (*Mathematisches Wörterbuch oder Erklärung der Begriffe, Lehrsätze, Aufgaben und Methoden der Mathematik*), this latter one of the best Germanic sources –if not the best– on the terminology used in mathematics by the beginning of the 19th century. There, Klügel defined mathematics as "the science of the forms of quantities, that is, all the ways in which a quantity is composed from others" or, in other words, the science that developed the connections among quantities, found the forms under which a quantity could be represented and taught how to find unknown quantities from known ones to which those were connected (cf. Klügel, 1808: 602-603).[114] That way, he said, in pure mathematics what was always sought were the forms of quantities (cf. Klügel, 1805: 269).[115]

[114] Klügel wrote: "Mathematik ist die Wissenschaft von den Formen der Grössen, das ist aller Arten, wie eine Grösse aus andern zusammengesetzt wird. Jede Grösse hängt con andern, wenigern oder mehrern, Grössen ab, bald auf eine einfeche, bald aus eine

Such definition of mathematics, alternative to the traditional one, was not uncommon in the early 19th century among Germanic mathematicians. For example, firstly, Bernhard Friedrich Thibaut (professor at Göttingen since 1797, first as lecturer, then as associate professor of philosophy and finally as professor of mathematics (cf. Cantor, 1894)) implicitly adopted that definition both in his *Outline of pure mathematics* (*Grundriss der reinen Mathematik zum Gebrauch bey academischen Vorlesungen abgefasst*) and in his *Outline of general arithmetic or analysis* (*Grundriss der allgemeinen Arithmetik oder Analysis zum Gebrauch bei academischen Vorlesungen entworfen*): in the former he defended that the basic concepts and rules of the combinatorial theory were "essential parts of the first principles" of pure mathematics (cf. Thibaut, 1809B: III; cf. Thibaut 1809A: 10-35), while in the latter he wrote that arithmetic dealt with numbers and laws (*Gesetzen*) of the several connections among them (cf. Thibaut, 1809A: 1). Secondly, an anonymous reviewer of Gerhard Ulrich Vieth's *Foundations of Mathematics* (*Anfangsgründe der Mathematik*) in the *Neue Leipziger Literaturzeitung* defined mathematics as "a general theory of forms", the quantity being the "most general form" and, consequently, arithmetic being the pure theory of quantities and mathematics being partly but not exclusively the theory of quantities ([Anonymous], 1808: 1291).[116]

Moreover, Heinrich Wilhelm Brandes, since 1811 professor of mathematics at the university of Breslau and professor of physics at Leipzig from 1826 thereafter (cf. Bruhns, 1876), contributed to Gehler's *Physics dictionary* (*Physikalisches Wörterbuch*) with the entry of "Mathematik", where he defined this one as "the science which compares quantities, determining from given quantities other ones according to certain given conditions" (Brandes, 1836: 1473).[117] A definition that, although at first glance may not seem entirely similar to those of others, does

verwickelte, nicht selten sehr verwickelte Art ab. Die Mathematik entwickelt den Zusammenhang der Grössen, die auf irgend eine Art mit einander verknüpft sind; sie findet die verschiedenen Formen, unter welchen eine und dieselbe Grosse dargestellt werden kann; sie lehrt, wie aus gewissen bekannten Grössen die unbekannten, welche mit ihnen in Verbindung stehen, gefunden werden, es sey ganz vollständig, oder näherungsweise."

[115] This definition can already be found in his work of 1792, the second edition of his *Anfangsgründe der Arithmetik, Geometrie und Trigonometrie*, where Klügel wrote: "Die Mathematik enthält alle Arten von Untersuchungen über die Grössen. Sie entwickelt den Zusammenhang der Grössen, die auf irgend eine Art mit einander verknüpft sind; sie findet die verschiedenen Formen, unter welchen eine und dieselbe Grösse dargestellt werden kann, und lehrt, wie aus gewissen bekannten Grössen die unbekannten, welche mit ihnen in Verbindung stehen, gefunden werden" (Klügel, 1792: 5; cf. Klügel, 1794: 48).

[116] The reviewer wrote: "Die Mathematik is allerdings auch Grössenlehre, aber nicht allein Grössenlehre; denn die reine Grössenlehre ist die Arithmetik. Die Grösse ist nur darum Gegenstand der Mathematik, weil sie die allgemeinste Form ist, endlich zu seyn, die Mathematik aber ihrer Natur nach eine allgemeine Formenlehre ist". As a matter of fact, Bolzano quoted this definition as source of inspiration for his own definition in his 1810 work.

[117] Brandes wrote: "Mathematik ist die Wissenschaft, welche Grössen vergleichen, aus gegebnen Grössen andre nach gegebnen Bedingungen bestimmen lehrt."

resemble Klügel's definition, as well as the implicit mathematical conception of Lagrange, for whom:

> In arithmetic, one seeks numbers according to given conditions between these and other numbers, and the numbers found that satisfy these conditions without preserving any trace of the operations used to form them. [While in] algebra, instead, the quantities that one seeks must be functions of given quantities, that is to say, expressions that represent the different operations that have to be carried out on these quantities to obtain the values of the quantities sought. (Lagrange, 1799: 235)[118]

Precisely, Brandes' later definition of mathematics and Lagrange's notion of 'function' highlight the link between two intertwined ways of thinking mathematical study of quantities among Germanic mathematicians by the beginning of the 19th century: on the one hand, a study focused on the forms of quantities, that is, the ways in which a quantity was composed from others (cf. Klügel, 1805: 269); on the other hand, a study focused on the functions of quantities, i.e. expressions that represented operations performed on quantities (cf. Lagrange, 1801: 10-11).

Lagrange's algebraic analysis, as a matter of fact, can be interpreted as the part of mathematics concerned with the forms of quantities in general or abstract, quite similarly to the way Klügel defined 'analysis' in his dictionary: analysis, Klügel said, was concerned with "the general presentation and development of the ways of composition of quantities by calculation" and provided general theorems and solutions, which in turn required the general type of designation of quantities and "their forms of composition" taught in the calculus of letters or algebra that also contained the most simple and common forms of reckoning or transformations (Klügel, 1803: 77-78).[119] However, while Lagrange's project was related to Euler's analytical program, which intended to place analysis as the core around which all mathematics should be reorganized, the project to which that other Germanic conception of mathematics in the early

[118] Lagrange wrote: "Dans l'arithmétique, on cherche des nombres par des conditions données entre ces nombres et d'autres nombres; et les nombres qu'on trouve satisfont à ces conditions sans conserver aucune trace des opérations qui ont servi à les former. Dans l'algèbre, au contraire, les quantités qu'on cherche doivent être des fonctions des quantités données, c'est-à-dire, des expressions qui représentent les différentes opérations qu'il faut faire sur ces quantités pour obtenir les valeurs des quantités cherchées."

[119] Klügel wrote: "Da die analytische Behandlung der Verbindungen der Grössen allgemeine Lehrsätze und Auflösungen liefert, so ist ihr eine allgemeine Bezeichnungsart der Grössen und ihrer Formen (Arten der Zusammensetzung) notwendig. Diese wird in der Buchstabenrechnung gelehrt, welche zugleich die leichtesten und gemeinsten Rechnungsarten oder Umwandlungen der Formen enthält."

19th century was linked was the placement of the theory of combinations –or combinatorial analysis– as one of the core parts of mathematics.

Inasmuch as both projects were intertwined, they shared some crucial aspects as well as differed in some others. To begin with, Germanic mathematicians of subsequent generations to Segner, Kästner and Karsten were not as reluctant as these ones to 'foreign' analytical developments, such as those carried out by Euler and several French mathematicians. So, while on the one hand some fragments written by Georg Christoph Lichtenberg, professor of experimental physics at Göttingen, show an agreement that was unusual among new Germanic generations with Kästner and previous Germanic mathematicians both terminologically (when referring to negative numbers as *verneinte*) and conceptually (when talking about foreign *mystisch algebraischen Beschwörungen*, "mystical algebraic incantations") (cf. Lichtenberg, 1994 I & II: 505 & 144, respectively), on the other hand the works of other authors such as Klügel, Metternich, Langsdorf, Pfaff, Hindenburg and Johann Tobias Mayer show quite clearly the tension between the conception they inherited and new mathematical trends to which such conception was opposed.

The case that in a sense more explicitly reflects such a tension is that of Johann Tobias Mayer, student of both Kästner and Lichtenberg and this latter's successor at Göttingen in 1799. Among his works, Mayer published in 1818 one on higher analysis (*Vollständiger Lehrbegriff der höhern Analysis*) whose opening lines criticized the "deficiency" (*Mangel*) in the usual Germanic presentation of higher analysis which he intended to improve (cf. Mayer, 1818: III-IV).[120] At the same time, however, as he himself pointed out in the preface, he followed the usual Germanic presentation of the fundamental concepts of differential calculus. That way, his presentation was not longer the one of Kästner and many of the mid-18th century Germanic mathematicians starting with what they considered to be the groundings of the infinite, namely the notions of infinitely small and large quantities: he started with the notions of function, variable and constant quantities and the classification of functions (cf. Mayer, 1818: 1ff.), as in the work of

[120] Mayer wrote: "Gegenwärtige Anleitung zur höhern Analysis, in so weit sie die Differenzial-und Integralrechnung umfasset, wird hoffentlich denen nicht unangenehm seyn, welche sich von den mancherley Kunstgriffen, womit insbesondere die Integralrechnung bereichert worden ist, vollständiger zu unterrichten wünschen, als es aus den bisher in Deutschland erschienenen Lehrbüchern jener Wissenschaft geschehen kann."

Euler and many of the French mathematicians of the second half of the 18th century, in spite of which his presentation still shared the Germanic reluctance towards infinitely small quantities.

Firstly, Mayer said, just as the concept of infinitely large rested on the possibility of a number or quantity to always grow "beyond any supposed limit", without ever being conceived as complete or perfectly defined, a quantity or a number could always be considered smaller without reaching an end, that is, he emphasized, could always be considered in a state of infinite decrease or becoming infinitely small without actually being infinitely small (cf. Mayer, 1818: 31 & 42-43).[121] Secondly, since no number could represent an infinite quantity, an infinitely large quantity was correctly represented by a symbol (∞), he wrote, although an infinitely small quantity was wrongly considered $= 0$, not being zero a quantity but the absence of quantity (cf. Mayer, 1818: 44). That way, on the one hand, the value of an infinite large quantity would not be changed with the removement or placement of a finite one, with $\infty \pm a = \infty$, though the aggregate of infinitely large quantities would give rise to an infinitely large quantity of different order, as in $\infty \cdot \infty = \infty^2$ (cf. Mayer, 1818: 38-39). While, on the other hand, the value of an infinitely small should not be reduced by subtraction so that it becomes zero "and even changes over to negative status" (*und gar in den negativen Zustand übergehen lassen könnte*), but reduced in a way that it never reaches 0, as in the expression $\frac{A}{\infty}$ for an infinitesimaly part of quantity A, which in turn made it possible to distinguish between different orders of infinitely small quantities ($\frac{1}{\infty}, \frac{1}{\infty^2}, \frac{1}{\infty^3}, \ldots$) (cf. Mayer, 1818: 44-45 & 47).[122]

Because of all that, Mayer criticized both the "greatest absurdities" (*grössten Absurditäten*) arising from the calculus of "zeros or nothings", which considered these ones as "real quantities" (*würkliche Grössen*), overruling their principles and leading to "inconsistencies and difficulties" (*Ungereimtheiten und Schwürigkeiten*) (cf. Mayer, 1818: 44), as well as the futile attempts to avoid using the term 'infinite' to refer to such perpetual increase or decrease and

[121] Mayer wrote: "Gedenkt man sich eine Grösse in einem solchen Zustande der unendlichen Abnahme, so sagt man dass sie unendlich klein werde, aber nie kann man sagen dass sie unendlich klein sey, weil dies so viel hiesse, als eine Gränze setzen, über die sie nicht noch kleiner werden könnte." This definition of infinitely small quantities recalls, for example, that of Lazare Carnot: "J'appelle *quantité infiniment petite*, toute quantité qui est considérée comme continuellement décroissante, tellement qu'elle puisse être rendue aussi petite qu'on le veut, sans qu'on soit obligé pour cela, de faire varier celles dont on cherche la relation" (Carnot, 1797/1813: 19).

[122] Mayer wrote: "Soll eine Grösse unendlich klein werden, so darf man sie nicht dergestalt abnehmen lassen, dass sie endlich Null wird (also durch Subtraction), weil man sie ja nach dieser Weise noch weiter vermindern, und gar in den negativen Zustand übergehen lassen könnte, sondern die Verminderung muss so beschaffen seyn, dass die Grösse wenn sie auch immerfort abnimmt, doch nie den völligen Nullzustand erreicht."

the "pedantic fearfulness" (*pedantische Aengstlichkeit*) with which some authors sought to avoid such decrease (cf. Mayer, 1818: 32 & VI-VII, respectively). A criticism that apparently is also found in an unpublished manuscript on the foundations of higher analysis written by Johann Georg Tralles in 1808 (former professor of mathematics at the Academy of Bern and professor at the university of Berlin at the time of his death, in 1822), in which he would have try to avoid the notions of infinitely large and small quantities "or other arrangements of these words" due to the incongruences to which they led (Schubring, 2005: 535-536).

Undoubtedly, just as there were students of Kästner who did not dedicate themselves to mathematics, as Johann Christian Polycarp Erxleben, or strictly to them, as Brandes himself, there were some others who, devoted to mathematics, did opt to employ such developments by Euler and other non-Germanic mathematicians, e.g. Johann Friedrich Pfaff, who was Klügel's successor at Helmstedt and, after this one was closed in 1810 (being part of the Germanic but Napoleonic Kingdom of Westphalia), was professor of mathematics at Halle (cf. Cantor, 1887). Pfaff's inaugural dissertation at Helmstedt clearly asserted that, in a way, it was "permissible to equal to 0 the differentials of variable quantities" (Pfaff, 1788: 5),[123] and his subsequent work on integral calculus and combinatorial analysis, while containing some references to Kästner, were more in line with Euler and Hindenburg's analytical proposals though not entirely constrained to these ones (cf. Pfaff, 1797 & 1796, respectively; cf. Dhombres, 1995).

Nonetheless, the work of several of Kästner's students that did dedicate themselves to mathematics reflects over the years the aforementioned tension. For example, among the early mathematical works of Karl Christian von Langsdorf, professor at Erlangen (by then part of Prussia) since 1798 and later at Vilniaus and Heidelberg, there were some annotations on Kästner's analysis of finite and infinite quantities (cf. Langsdorf, 1777 & 1778); but in 1802, in his *Foundations of pure elementary and higher mathematics*, although he basically followed the common Germanic presentation of the second half of the 18th century, defining mathematics as "the science of the quantities only in relation to its quantity" and introducing negative quantities as negated ones (Langsdorf, 1802: 1 & 60-62),[124] he did criticize the usual Germanic division into

[123] Pfaff wrote: "Ita, quod paradoxi speciem mentitur, differentialia quantitatum variabilium nihilo aequare, contra quantitatum certo respectu constantium differentialia pro realibus accipere licet."
[124] Langsdorf wrote: "Die Mathematik ist die Wissenschaft von den Grössen bloss in Bezug auf ihre Grösse, und heisst daher im Teutschen auch die Grössenlehre."

analysis of finite and infinite quantities as “unnecessary and not even quite correct” (*entbehrlich und nicht einmal ganz richtig*), considering more appropriate the division of mathematical analysis into arithmetic and geometric (Langsdorf, 1802: 300). That way, in his 1807 work entitled *New and more rigorous presentation of the principles of differential calculus* (*Neue und gründlichere Darstellung der Principien der Differentialrechnung*), he followed the presentation of Euler and many French mathematicians of the second half of the 18th century, beginning by the notions of function and variable and constant quantities and not anymore by the notions of infinitely small and large, even considering differentials such as $x - x$ as infinitely small quantities (cf. Langsdorf, 1807: 1 & 42).

Another case similar to that of Langsdorf, insofar as over the years his work shows a detachment from the Germanic mathematics of the mid-18th century, was that of Matthias Metternich, professor of mathematics at the former catholic university of Mainz from 1784 to 1809, when Mainz and all the other French lyceums were reorganized and he was not rehired (cf. Schubring, 2005: 506). Metternich’s work of 1783, for example, *A thorough instruction on arithmetic for beginners in public schools* (*Gründlige Anweisung zur Rechenkunst für Anfänger in öffentlichen Schulen*), was indeed not only in line with Wolff’s work but very similar to this one, from the very definition of the “art of reckoning” to the omission of negative quantities (cf. Metternich, 1783: 13ff.; Wolff, 1717: 35ff.), whose inadequacy he recognized at the beginning of the preface to his *Foundations of geometry and trigonometry* (*Anfangsgründe der Geometrie und Trigonometrie zum Gebrauche für Anfänger bei dem Unterrichte*) (cf. Metternich, 1789: I). Precisely, his 1789 book on geometry and trigonometry was already in line with the works of Kästner and Karsten, even introducing “continuity” as the very first geometric principle or axiom (*Grundsätze*) and considering therefore that a geometric body could be divided infinitely (cf. Metternich, 1789: II & 1-2).

Later, in 1808, Metternich published a work on the art of reckoning of decimal fractions and other numbers for the use “with [the] new masses and weights”, which had been set in an international meeting that took place in Paris at the end of 1798; a meeting which Tralles attended as Swiss’ delegate and whose results, the prototype standards of measurements, were presented in mid-1799 (cf. Heilbron, 1990: 234-235; Schubring, 2005: 535; Metternich, 1808). Ten years later, he published a work on theory of numbers in which, beyond beginning with an epigraph of Kästner as in his 1821 work on the divisions of triangles, he not only did consider opposite numbers (cf. Metternich, 1818: 152ff.), but he also stressed both the importance of theory of numbers and theory of combinations: while the former, he wrote, could be regarded either as a particular instruction in arithmetic or as a part "or first foundation of the whole theory of quantities (mathematics)", combinations and permutations constituted also core parts of mathematics and so he devoted to such subjects an appendix (cf. Metternich, 1818: XIII, XXIX-XXXI & 535ff.).[125]

It was in his translation of Lacroix's *Foundations of algebra*, however, first in 1811 and later in 1820, where Metternich properly introduced combinations and permutations and he positioned himself against the former's conception of negative quantities. As Schubring pointed out, by the beginning of the 19th century the official French textbook on algebra was the one of Lacroix, who although initially followed the works of Clairaut and Bézout, since 1803 turned to Carnot's proposal and hence rejected negative quantities (cf. Schubring, 2005: 411-413 & 507). Concerning the first issue, in the section in which Lacroix dealt with the general values of unknowns in first-degree equations (cf. Lacroix, 1804: 132), Metternich introduced some subsections on "the possible combinations and permutations of a given set m of things" (Metternich, 1811: 173-237), and defined forms as the various conformations of things according to connections or juxtapositions between them (cf. Metternich, 1811: 183-184).[126] A notion, this latter, which evokes the one that for several Germanic mathematicians of the time encompassed the general sense of mathematics as the science of the forms of quantities.

Similarly, Brandes, who nevertheless praised the advantages of the analysis arrangement of Euler and other French mathematicians (cf. Brandes, 1820: VII), included in his *Preparations for higher analysis* (*Vorbereitungen zur höhern Analysis*), that is, in the volume he set as a preparation for differential and integral calculus (quoting the works of Kramp and Thibaut as references in the subject), a couple of sections on permutations and combinations (cf. Brandes, 1820: V & 31-46, respectively). But, while Metternich's definition of 'forms' referred to things in

[125] Metternich wrote: "Die Zahlenlehre kann entweder als vereinzelter Unterricht im Rechnen, oder als Kapitel und erste Grundlage zu der gesammten Grössenlehre (Mathematik) betrachtet werden."

[126] Metternich wrote: "Weil bey jeder veränderten Nebeneinanderstellung dieser m Dinge eine andere Gestalt in ihrer Verbindung erscheint, so heisse man diese verschiedenen Gestalten: formen, und so kann die Aufgabe so heissen: Eine Regel zu finden, wornach man die mögliche Menge der formen angeben könne, die m Dinge, immer anders versetzt, hervorbringen können." It is curious that Metternich quoted the work of Rothe on combinatorial analysis because, he wrote, "[he had] not read Hindenburg's method" (Metternich, 1811: VII).

general, the conception outlined by Brandes of those two fundamental parts of analysis referred to ways of conformation in terms of quantities and numbers.

As for the second issue, Metternich did not suppress Lacroix's paragraphs devoted to the elucidation of the difficulty posed by equation $370 - 4x = 350$, with $-4x = -20$, in order to eliminate or rectify the quantity with the $-$ sign and thus avoid an "absurdity" (cf. Lacroix, 1804: 83, 86 & 88), but instead he added a couple of subsections on the subject in which he criticized the position of the French mathematician (cf. Metternich, 1811: 111-117). So, despite recognizing in the preface the functionality or practicality of Lacroix's textbook, Metternich stated from the very beginning that, in his opinion, Lacroix's presentation of negative numbers was incorrect (cf. Metternich, 1811: X & V, respectively). Lacroix's explanations, he said, at most could afford insight in individual results, all of which, on the contrary, should be comprised by an appropriate theory of signs (cf. Metternich, 1811: 111). That way, Metternich began by distinguishing between the absolute and the relative meaning of signs and then differentiated between the signs $+$ and – standing before quantities as expressions of either an addition or a subtraction to be performed with those quantities, and the signs $+$ and – as "algebraic signs" (*algebraischen Zeichen*) (cf. Metternich, 1811: 111-113 & 128-129; Schubring, 2005: 509).

The reluctance of Metternich towards Lacroix's conception of negative numbers additionally highlights, on the one hand, the debate around synthetic and analytical methods in mathematics and, on the other hand, the growing concern among Germanic mathematicians regarding a different idea of "correctness" of mathematical theories. Metternich's own version of the text of Lacroix was intended not only to correct those parts in which the latter was vague or wrong, but also to supplement those parts where he found gaps in the demonstrations (Metternich, 1811: IV).[127]

However, some other authors went even further, as Euler and Schultz (praised by Bolzano) did. Thibaut criticized the foundation of arithmetical theories on geometrical considerations (Thibaut, 1805: 168) and the use of "extraneous principles" that contravened the purity of

[127] Metternich wrote: "Die vielen Anmerkungen, die ich an schicklichen Stellen beygefügt habe, sollen das Vorgetragene ergänzen; damit nirgend eine Lücke in den Beweisen bleibe."

analysis (Thibaut, 1809A: IV),[128] while Lagrange, must be remembered, intended to provide a non-infinitesimalist theory of analytical functions in order to avoid external considerations to the algebraic analysis of finite quantities (Lagrange, 1797).[129] The insistence of Tobias Mayer on the abstract nature of the correct concept of infinite “that [constituted] the object of study of a whole science” (Mayer, 1818: 31), and Metternich’s statement about the correct order of the two parts of algebra, pure and applied (Metternich, 1818: XXIV), can also be mentioned as attempts in the aforementioned direction. Furthermore, Zimmermann modified the usual way of explaining the whole and the opposite quantities (with Pestalozzi as a reference and praising the works of Karsten and Klügel) in his work on the development of analytical principles (Zimmermann, 1805: VIIff.). While Michelsen quoted Kant’s idea of the construction of concepts to account for the innovations that he carried out for the sake of firmly established and well explained concepts, as those of quantity and variation (Michelsen, 1789, p. VIIff.).

Such emergence of pedagogical concerns and new methodological and foundational reflections among Germanic mathematicians do not entail that similar issues did not matter before. Inspired by Euclid but strictly detached from him, Wolff wrote in his *Mathematical vocabulary* (*Mathematisches Lexicon*): “Mathematical or geometrical method [...] means the way mathematicians, mainly geometers, arrange their thoughts from the things they want to express to others or with which they themselves deal, successively organize and connect them” (Wolff, 1816: 889-890). A method that, for him and many Germanic mathematicians of the 18th century, started with definitions (of words or nominal and of things or real)[130] and proceed, going through fundamental propositions or axioms that “flowed from those definitions” (cf. Kästner, 1758: 14),[131] to doctrine propositions or theorems and their proofs (cf. Wolff, 1717: 5-6). As Kästner emphasized, –geometric– mathematical method was “the only one that led to certainty” and that consequently guaranteed being safe from errors (cf. Kästner, 1758: 11).

[128] Thibaut wrote: “Ueberhaupt ist die Gründung arithmetischer Lehren auf geometrische Betrachtungen, welche auch zu diesem Missverstande Anlass gegeben, als völlig unwissenschaftlich zu verwerfen.Wir sollen und dürfen nicht erst, um zu erfahren, was für ein Zeichen das Product zweyer Zahlen, bekommen muss, zur Construction von Quadraten und Rechtecken unsere Zuflucht nehmen.” And: “In so fern hofft er die Form seiner Darstellung, welche in vielen Stücken von der gewöhnlichen abweichend seyn mag, in gewissem Sinne als gerechtfertigt annehmen zu dürfen. Dass es ihm angelegen gewesen sey, die Analysis von allen fremdartigen Principien zu reinigen.”

[129] The whole title of Lagrange’s 1797 work was *Théorie des fonctions analytiques, contenant les principes du calcul différentiel, dégagés de toute considération d’infiniment petits, d’evanouissans, de limites et de fluxions, et réduite à l’analyse algébrique des quantités finies*.

[130] In Wolff’s proposal real definitions are weighted over the nominal ones. For a careful study of this issue, cf. (Blok, 2016: 23-28).

[131] Kästner wrote: “Aus den Erklärungen fliessen Grundsätze (axiomata) deren Wahrheit man einsieht, sobald man sie versteht.” This feature highlights a significant difference with respect to Euclid’s method, in which axioms were not subordinated to definitions and central definitions were not the real ones but the nominal ones. There were, nonetheless, mathematicians as Darjes whose more scholastic education led to a different and richer (with more elements and more complex) approach from that of Wolff.

However, while in general for Germanic mathematicians of the mid-18th century the goal was to develop mathematics according to geometrical method, and thus developments without some kind of geometric basis were considered as wrong and discarded (as the ones carried out by Euler and several French mathematicians), one of the goals of subsequent Germanic generations was to avoid external considerations to the mathematical science in question, as in the case of geometric considerations in analysis. Surely this was not the case for all those mathematicians, as for example Johann Martin Christian Bartels (former professor of mathematics at Jena, since 1808 at the university of Kazan and from 1821 onwards at Dorpat), who in his investigations of analytical functions intended to "[follow] the methods of the Ancients" and use "the Euclidean definition of proportionality" (Lumiste, 1997: 53; cf. Bartels, 1822: 35-36).[132] But it was the case of authors like Euler and Thibaut who despite their intentions and beyond lacking entirely adequate concepts –and terminology–, they continued to base certain mathematical sciences' proofs on alien notions: Euler, who at the end of the preface to his 1755 work on differential calculus identified the correct presentation of pure analysis with the unnecessity of geometric figures (cf. Euler, 1755: XX),[133] not only presented in 1767 a geometric problem to justify the introduction of his "discontinuous" analytical functions (cf. Euler, 1767: 23), but also, as Giovanni Ferraro pointed out, when analytically characterizing the exponential function a^z, "in order to satisfy [a] geometric intuition, Euler excluded values of a which made jumps in a^z" in his 1748 work (Ferraro, 2000: 120-121; cf. Euler, 1748: 70-72); and Thibaut, who introduced the concept of opposition as a relational one through which it was possible to go from the idea of a quantity x to the idea of its opposite quantity $-x$, still posed that transition in terms of a mutual annihilation (*gegenseitig vernichten*) which, he said, could be illustrated by the examples of opposite forces and opposite paths (Thibaut, 1809B: 58).

This persistence of what, from that new approach, constituted procedural deficiencies in mathematics, linked in those cases to the geometric roots of some of the concepts and terms used, as a matter of fact underlies the tension around the negative quantities and numbers that

[132] In a letter from Bartels to Nikolaus Fuss, quoted by Ülo Lumiste, Bartels wrote: "According to my plan, this booklet had to contain all those parts of higher analysis which can be treated without the differential and integral calculus and, in a sense, serve as an introduction to them. The first part perhaps deserves some attention because I followed therein, I think, the methods of the Ancients (in the strict sense of the word). [...] I think that by using the Euclidean definition of proportionality, I have given the general proposition about functions in a clearer and more elegant way."

[133] Euler wrote: "Hic autem omnia ita intra Analyseos purae limites continentur, ut ne ulla quidem figura opus fuerit, ad omnia huius calculi praecepta explicanda."

is clearly present, precisely, in the works of Thibaut and Klügel. In the case of the former, he introduced negative numbers within arithmetic framework, in a section on "conflicting numbers" (*Von den widerstreitenden Zahlen*), and explained the "derivation" of their corresponding art of reckoning (cf. Thibaut, 1809B: 57-64); although, as he emphatically pointed out a decade earlier in his 1797 dissertation on logarithms of negative and impossible numbers, he rejected real negative logarithms (Thibaut, 1797: 21; cf. Thibaut, 1809B: 131-144).[134]

In the case of Klügel, just as in his 1767 dissertation for his appointment as professor at Helmstedt he used the formula $S = y\frac{dx}{dy}$ for the subtangent (cf. Klügel, 1767: VIII), at the end of the 18th century, as previously stated, he rejected the formula proposed by Busse, $S = -y\frac{dx}{dy}$, because, he said, while the specific cases required the consideration of the negative value, "in all analytical representations of a combination of quantities [...], quantities must be regarded absolutely and only by their quantity", that is, "as positive" (Klügel, 1800: 341; cf. Schubring, 2005: 490-491).[135] Busse's statement on the importance of discriminating positions and directions and his proposed formula to account for this, however, as both Kästner and Hindenburg themselves acknowledged, was correct (cf. Hindenburg, 1800: 343-348; cf. Kästner, 1798: 2013-2014). But in the entry "opposite quantities" of the 1805 volume of his mathematical dictionary, apropos of the formula for the tangent, Klügel actually insisted that "in the combination of quantities which are the basis for all other related combinations, all quantities must be regarded as positive" (Klügel, 1805: 108).[136] While, in the entry "subtangente" of the –posthumous– 1823 volume, no mention is made of the correction of the formula or the importance of directions for obtaining correct results (cf. Klügel, 1823: 552).

Furthermore, his reluctance towards negative quantities is reflected in several other passages of his work. To begin with, in his *Foundations of Arithmetic* (*Anfangsgründe der Arithmetik*) Klügel, similarly to the way Wolff did it, only devoted a couple of paragraphs to the "negated terms"

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[134] Thibaut wrote: "Inter nostrates: non dari reales negativorum logarithmos ex rationum conceptu derivavit Ill. Kaestnerus; refutare D'Alembertium, et eximere dubiis demonstrationes Eulerianas studuit C. Karsten; quibus e contrario evertendis, et exstruendae recentissimis temporibus operam navavit C. Michelsen." As this passage suggests and as Cajori pointed out in the early 20th century, such rejection was usual in the Germanic context of the late 18th century (cf. Cajori, 1913: 115).

[135] Klügel wrote: "Man muss bey allen analytischen Darstellungen einer Verbindung von Grössen, sie mögen geometrische oder arithmetische seyn, in demjenigen Falle, der für alle übrigen perwandten Verbindungen zum Grunde der Rechnung gelegt wird, alle Grössen als positiv ansehen, das heisst, sie absolut bloss nach ihrer Quantität betrachten."

[136] Klügel wrote: "In derjenigen Verbindung von Grössen, die zum Grunde für alle übrigen verwandten Verbindungen gelegt wird, müssen alle Grössen als positiv betrachtet werden, das heisst, ihre Symbole bezeichnen bloss die Quantität."

(*verneinte Glieder*) in the section on arithmetic and geometric progressions: if arithmetic series is considered backwards, he wrote, negated terms, indicating a defect, could be obtained (Klügel, 1792: 50-51).[137] This was consistent with his distinction between absolute and relative cases, which not only underlay his rejection of Busse's formula, but also his particular rejection of certain logarithms of negative numbers:

> The logarithm of a negated number is regarded as the same as the logarithm of the same [number] considered as positive; the negation merely refers to the case laid down in the calculation, which is transformed by changing the sign into another one. If this other relative case is considered, the negated quantity becomes positive. This has no difficulty if the logarithm is only an auxiliary mean for numerical calculation. If, however, the logarithm itself is a quantity, which occurs in a connection with some others, the possibility itself would depend on the possibility of the number whose relation to the unity is given. The $\log -xx$ [$-x^2$] is impossible, but the $\log -x$ is possible, [since] it would have to be shown that any certain condition does not allow x to be negative. (Klügel, 1795B: 481; cf. Klügel, 1792: 59)[138]

Which is not strange, if one considers what he wrote on the previous pages about how the concept of opposite quantities was unnecessary for the rules of algebra's common operations, which could be developed solely considering "absolute quantities" (cf. Klügel, 1795B: 479),[139] but it nonetheless is curious, having he previously disqualified the English for their rejection of negative quantities. For Klügel, the rejection of any case of negative quantities (*negative Grössen*) by English mathematicians exemplified the problems to which the inherent constrains to the synthetic method led, while analytic method considered several cases in a formula (cf. Klügel, 1795A: 316 & 312-313; cf. Schubring, 2005: 139).[140]

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[137] Klügel wrote: "Die Reihe ist eine steigende, wenn die Glieder, so wie sie auf einander folgen, zunehmen; eine fallende, wenn sie abnehmen. Jede steigende ist eine fallende, oder diese jene, wenn man sie rückwärts lieset. Jede Reihe besteht ihrer Natur nach aus unzählig vielen Gliedern, wovon man oft nur eine Anzahl herausnimmt. Wenn man die angeführte arithmetische rückwärts lieset, so kommt man nach der 1 auf verneinte Glieder, wie man es zu nennen pflegt, d. i. solche, die einen Defect anzeigen."

[138] Klügel wrote: "Der Logarithme einer verneinten Zahl ist einerley mit dem Logarithmen derselben als positiv betrachtet; den die Verneinung bezieht sich bloss auf den bey der Rechnung zum Grunde gelegten Fall, der durch Veränderung der Vorzeichen in einen andern ähnlichen verwandelt wird. Wenn dieser andere verwandte Fall der Rechnung untergelegt wird, so wird die verneinte Grösse positiv. Die Sache hat gar keine Schwierigkeit, wenn der Logarithme nur Hülfsmittel der numerischen Rechnung ist. Ist der Logarithme aber selbst eine Grösse, die in einer Verbindung mit andern vorkommt, so möchte die Möglichkeit desselben auf der Möglichkeit der Zahl, deren Verhältniss zur Einheit er angiebt, ankommen. Der $\log -xx$ ist unmöglich; der $\log -x$ aber möglich, es müsste denn gezeigt werden, dass irgend eine Bedingung nicht erlaubte, x negativ zu nehmen."

[139] Klügel wrote: "Die Vorschriften für die gemeinen Operationen der Buchstabenrechnung gehören zwar zu den allerersten Lehren der Analysis; es mag aber doch nicht überflüssig seyn, zu zeigen, dass sie der Begriffe von entgegengesetzten Grössen gar nich bedürfen. Die Buchstabenzeichen bedeuten in dem Folgenden bloss die absolute Quantität."

[140] Klügel wrote: "Man hat hier bloss additive und subtractive Grössen, nicht positive und negative. Es verhält sich hier gerade so, wie in der Geometrie der Alten und der ihnen nachahmenden Engländer, nach welcher in keinem Satze negative Grössen vorkommen, weil in jedem Falle bestimmt ist, was eine Summe vorkommen, weil in jedem Falle bestimmt ist, was eine Summe oder was ein Unterschied ist, und bey einem Unterschiede nie das Ganze von dem Theile abzuziehen gefordert werden kann."

Precisely, what makes Klügel's disqualification of the English rejection of negative quantities curious is not that he himself rejected negative quantities since, according to him, unlike them he did not reject in general those quantities but only confined their use to relative cases (postulating a different sort of rejection).[141] It is his argument against those mathematicians, and ancient geometers, which rests on the superiority of analytic method over the synthetic, what makes his disqualification curious; a superiority that he had already put forward in his 1767 dissertation (cf. Klügel, 1767: XV-XVI). After all, if what he praised of the analytical method was its generality in expressing the connections of quantities (cf. Klügel 1767: XV; cf. Klügel, 1795A: 313), then no solid argument could be made against the logarithms of negative numbers in general or Busse's negative formula for the subtangent, unless, of course, others were the motives for doing so, as indeed they were; motives, all of them, which can be traced back to the contravention of the prevailing geometric notion of quantity.

As Klügel himself acknowledged in the entry for "quantity" in his dictionary, while multiplicity was a concept of arithmetic origin, quantity was an inherently geometric concept (cf. Klügel, 1805: 650).[142] "Quantity", he wrote, "[is] what is composed of homogeneous parts", and thus mathematics could be regarded as the science of the form of everything that had such composition or nature in reality or in imagination, both continuous and discrete (cf. Klügel, 1805: 649-650). But, while coupled with that concept the ancient mathematicians did not develop the one of opposite quantities, he said, modern ones did so on the basis of quantity connections that entailed the consideration of all quantities as positive since their symbols only denoted their quantity (cf. Klügel, 1795A: 311 & 316).[143] That way, for Klügel "quantities" in the strict sense were the positive ones, from which, once considered their opposition, opposed quantities and therefore positive and negative quantities arose (cf. Klügel, 1805: 104).

Numbers, on the other hand, were for Klügel "representation[s] of the form of a plurality of homogeneous things" (Klügel, 1792: 7) and, as consequence, the introduction of negative numbers was justified inasmuch as they were considered representations of negative quantities.

[141] Schubring suggests that this shows a contradiction in Klügel's work, who firstly, he says, defends negative quantities and then suggests a way to avoid them (cf. Schübring, 2005: 139). Though there might be a certain contradiction in Klügel's approach, both attitudes emphasized by Schübring could be considered consistent.
[142] Klügel wrote: "Vielheit des Gleichartigen ist mit Grösse gleichbedeutend. Doch bleibt der Unterschied, dass der Begriff, Grösse, geometrischen, und Vielheit arithmetischen Ursprungs ist."
[143] Klügel wrote: "In derjenigen Verbindung von Grössen, die zum Grunde für alle übrigen verwandten Verbindungen gelegt wird, müssen alle Grössen als positiv betrachtet werden, das heisst, ihre Symbole bezeichnen bloss die Quantität."

However, while for him geometrical quantities (such as lines, surfaces and bodies) should be listed among the continuous ones, on the contrary all countable things and numbers in general should be listed among discrete quantities (Klügel, 1805: 650).[144] Which in turn reveals as well, through the notion of number and its limitations, the geometric roots of mathematics at the time: continuity was included as an essential notion in geometry (cf. Klügel, 1792: 67) and a continuous quantity was defined as one whose "parts [were] all connected that where the one [ceased] the other [began]" (Klügel, 1803: 553-554);[145] convergence or "to approximate" (*annähernd*) was firstly defined as the property of two straight lines around their intersection point and secondly as the property of a series whose terms successively became smaller (Klügel, 1803: 555); and, finally, the limit of a quantity was defined as the "[constant] quantity to which that [other] quantity can always approach more and more as a variable one" (Klügel, 1805: 646).

The rejection at the end of the 18th century and beginning of the 19th century, on the one hand, of Carnot, Lacroix and other French mathematicians of negative numbers, as well as, on the other hand, the rejection of Lagrange (an Italian mathematician who, supported by Euler and d'Alembert, succeeded the first one at the Prussian Academy of Sciences in Berlin in 1766 and later, in 1787, moved to Paris as a member of the French Academy of Sciences) and some other 'French' mathematicians of infinitely small quantities, illustrate how the tension around the notions of quantity and numbers was not something happening exclusively in the Germanic realm. Germanic mathematicians of subsequent generations to Kästner and Karsten were not able to get entirely rid of the geometric ties that were even more present in the works of their teachers, although, as these latter with regard to Wolff and their other predecessors, those new Germanic mathematicians gradually detached more from them, increasingly questioning such ties. In that mathematical context of the convergence of those two mathematical traditions, Germanic and French, but in a peculiar Germanic social context, that of the Kingdom of Bohemia in the transition from the 18th century to the 19th century, was that Bolzano, the author to whom the last chapter of this work is devoted, developed his first mathematical works.

[144] Klügel wrote: "Man unterscheider zusammenhangende, und nicht zusammenhangende Grössen (Quantitas continua et discreta). Unter jenen versteht man die geometrischen Grössen, Linien, Flächen und Körper; unter diesen alles, was aus unverbundenen Theilen besteht, als alle zählbaren Dinge, Zahlen im Allgemeinen, und jede Grösse, die als Zahl für eine Einheit betrachtet wird, wenn gleich die Einheit und die Menge der Einheiten unbestimmt gelassen wird."
[145] Klügel wrote: "Eine Grösse heisst eine stetige, ein Continuum, wenn ihre Theile alle so zusammenhängen, dass wo der einer aufhört, gleich der andre anfängt."

C. The pre-modern notion of number in the early mathematical works of Bernard Bolzano

"The founders of sects are always harmed
by the exaggerated veneration of their sectarians."
(Kästner to Bendavid, 26.04.1794)[146]

C.1. Bolzano's mathematical works of 1804 and 1810

C.1.1. Bohemia and the Austro-Germanic context in the late 18th century and the early 19th century

Most of the Germanic mathematicians of the second half of the 18th century studied so far, that is, Germanic mathematicians of subsequent generations to Segner, Kästner and Karsten that were born between c. 1750 and c. 1770, grew up in a context of relative regional peace in Central Europe, as noted in the first chapter. However, for those Germanic mathematicians who were born a decade later, the situation was very different, both because of the various effects of the French Revolution and the French Revolutionary Wars in the Germanic territories, as, later, because of the direct incidence on these territories of the series of conflicts known as the Napoleonic Wars. Moreover, those effects varied from one place to another, depending mainly on the geographical location, the specific moment and the ascription of the place in question: it was very different to be born or grow up in the Germanic Mainz before 1792 (that is, before the French intervention), than between that year and 1798 (in dispute), between this last year and 1814 (under French control) or from mid-1814's onwards (after the French withdrawal); as it was not the same to be in the Prussian Jena or Berlin in 1796, than at the end of 1806 (after the victories of Napoleon).

[146] Apropos of Kant and his followers, Kästner wrote: "Den Sectenstiftern hat allemahl die übertriebene Verehrung ihrer Sectirer geschadet" (Bendavid, 1794: [2]).

In the case of –Germanic– Austrian territories, something similar can be said, for example and depending on the moment between c. 1800 and 1815, about its western territories (as Tyrol, Brixen and Trent), its strictly speaking Austrian territories (those belonging to the Archduchy of Austria) and its northern or eastern territories (in the first case, those territories usually grouped as Lands of the Bohemian Crown; in the second case, the Kingdom of Hungary among them). But even before c. 1800, significant differences can be established between what happened in the Austrian territories following the outbreak of 'internal' French conflicts and what happened before, in contrast to what happened in other Germanic territories during the same periods.

As mentioned before, Maria Theresia ruled the territories controlled by the House of Habsburg and was Empress of the Holy Roman Empire until her death, in 1780, when her son Joseph II succeeded her *de facto*, after being co-regent of the Austrian dominions and Holy Roman Emperor since his father's death in 1765. "In a word, he is a prince of whom one can expect only great things", said Friedrich II (King of Prussia) about Joseph II at the beginning of the 1770s, "and who will get the world to talk about him, as soon as he has his elbows free" (Baumgart, 1990: 261).[147]

Bernard Bolzano was born in Prague on October 5, 1781, one year after Maria Theresia died and, therefore, one year after Joseph "had his elbows free" and began to implement reforms ranging from educational to agrarian and religious issues. Thus, for example, a week after Bolzano's birth, Joseph II "promulgated [...] a patent granting toleration to Lutherans and Calvinists, as well as regulating the status of Orthodox Christians" and reducing restrictions on Jews during the next couple of years (Agnew, 2004: 91; Scott, 1990: 169-170); and, a few weeks later, Joseph II issued a couple of patents, one to abolish serfdom and another to increase the security of the serfs and peasants on their lands (cf. Wright, 1966: 74-77). In other words, Bolzano was born in a context of constant reforms, some of them with deeper effects than others. A context in which, despite the aforementioned decree of 1784 of German as the language of instruction, administration and state (cf. supra A.2), teaching and use of other languages (as Czech) in certain publications were allowed (cf. Evans, 2006: 136), as the state

[147] Following Baumgart, Friedrich II said: "Mit einem Wort, es ist ein Fürst, von dem man nur Grosses erwarten darf und der in der Welt von sich reden machen wird, sobald er die Ellenbogen frei hat."

spending in education increased to the extent that, for example, “by 1790, as many as two-thirds of children in the Bohemian Lands were attending primary schools” (Scott, 1990: 176).

Towards the end of the 1780s, nonetheless, resistance to Joseph’s II reforms and actions increased throughout the Reich and the Habsburg domains outside it. For example, when he intended to confiscate properties of some of the bishoprics whose incumbents had died and that were located inside the Holy Roman Empire, as he had done with regard to some bishoprics outside it but within the dominions of Austria, he met with their frontal opposition, as happened with Salzburg, Regensburg and Freising around 1785 (cf. Whaley, 2012 II: 421). Something similar can be said concerning his second attempt to acquire Bavaria in the mid-1780s, which precipitated the formation in 1785 of an important but to some extent ephemeral *Fürstenbund*, a league of catholic and protestant princes led by Prussia, given “the [growing grounded] perception of the need to defend the Reich against the emperor himself” (Whaley, 2012 II: 424). Even some of his latest reform projects, such as those intended to modify the tax burden in favor of the peasants and the state, met with strong resistance not only of the lords, as many of his reforms, but also of peasants themselves (cf. Agnew, 2004: 92).

In Bohemia in particular, the resistance to the last reforms mentioned above originated largely due to the particular implementation of the previous agrarian reforms by the high commissioner designated by Joseph II to carry them out, Johann Paul von Hoyer. While in Moravia (in charge of a different high commissioner, namely Freiherr von Kaschnitz) prevailed the peasants’ secure tenure of the lands by means of the “purchase [of] hereditary leaseholds and life tenures, or, at least, to lease the land for periods of twelve to fifteen years”, in Bohemia von Hoyer “[encouraged] leases of land for periods of from three to six years”, “he did not let buildings to the serfs” and he did not strictly subscribe the abolition of serfdom (Wright, 1966: 96-98). As Wright pointed out, although the new reforms proposed for 1789 could have been profitable to peasants, at the same time that the French revolution began and Joseph II attempted to rescue his sister Queen Marie Antoinette and King Louis XVI of France, “the inauguration of [that] new law was marked”, as happened with the similar previous reforms, “by rural riots and rebellions”, in response to which Joseph II postponed its implementation until 1790 (Wright, 1966: 147).

At the beginning of 1790, four years after the death of Friedrich II, Joseph II died and, despite all the debate surrounding the government of the Empire and the perpetuation of the Empire itself, he was succeeded by his brother Leopold II (until then Grand Duke of Tuscany), who was crowned by the end of the year. “I believe that the sovereign, even the hereditary ruler, is merely the delegate of the people, for whose sake he exists [...]. I believe that the executive power belongs to the sovereign but that the legislative power belongs to the people and their representatives” (Whaley, 2012 II: 427), he wrote to his sister Marie Christine the month prior to their brother Joseph’s II death. But, since the situation that he inherited involved a series of internal conflicts to the Holy Roman Empire in general and to Austria in particular, as well as the need to position both Austria and the Reich with respect to the French internal conflict, his most immediate policies sought to tackle these issues.

To begin with, Leopold II withdrew his brother’s reforms of 1789 and showed interest in requests and complaints from the states within his domains, making some concessions to them. For example, “several long-empty traditional offices [were filled], [and he] agreed to renew the land committees for Bohemia and Moravia, give them control of the domestic fund, and consult the Estates diet in all customary matters” (Agnew, 2004: 93; cf. Scott, 1990: 185). So, while he “staged a festive coronation as king of Bohemia on September 6, 1791” (Agnew, 2004: 93), he also reverted some of the restrictions imposed on landlords and aristocrats by his brother.

On the other hand, regarding foreign policy, Leopold II ended the participation of Austria in the conflict declared in 1788 between Turkey and Russia, while he met with Friedrich Wilhelm II to begin cooperation between them. As a result of the latter, eventually the Pillnitz Conference was held and a Declaration was issued to support King Louis XVI of France, which was followed, firstly, by Leopold’s demand for “the withdrawal of the French army from the western frontier of the Reich [and] the return of all confiscated property”, secondly, by a treaty of alliance between Prussia and Austria and, finally, by the French declaration of war on Austria on April 20, 1792 (Whaley, 2012 II: 429-430; cf. Scott, 1990: 27; Agnew, 2004: 93-95).

By the end of April, however, Leopold II had already died (on March 1), being succeeded by his eldest son Franz as Holy Roman Emperor only until early July. The French declaration of war, therefore, was not on the Holy Roman Empire (then strictly without Emperor), but on the ruler of Austria and King of Bohemia and Hungary. A ruler, it must be said, who had the support of Prussia, but practically did not count on the initial support of the rest of the Germanic states. For example, while "both Bavaria and Hanover [...] argued for strict neutrality [...], Mainz and Hessen-Kassel sent small contingents" (Whaley, 2012 II: 569). That way, even though initially Austria and Prussia assumed the defense of the Germanic territories threatened by the French forces, due to the clear threat of French expansionism and after the execution of Louis XVI in early 1793, the Holy Roman Empire eventually recognized itself as a party to the conflict, though "[the] mobilization of both men and resources was, however, slow and hesitant" (Whaley, 2012 II: 574).

After the Peace of Basle (Basel), signed between the *République française* and Prussia in April 1795, and its subsequent extensions to include Spain and the Landgraviate of Hesse-Kassel, most of the Germanic territories of the north remained outside the conflict for over a decade, while those of the south continued facing France until basically 1798. As a consequence, Franz II went much further than his father in the process of counter-reforms and to the "new framework of censorship legislation designed to inhibit the spread of 'democratic principles'", agreed at the end of 1791, two bans were issued during the first half of 1793: first "against revolutionary agitators and 'Jacobins'" and, second, against "secret student societies and powers to rusticate radical students" (Whaley, 2012 II: 584; cf. Krueger, 2009: 59). These and other internal policies adopted throughout the 1790s, coupled with the socio-political particularities of the Germanic territories that formed the Holy Roman Empire, effectively ensured the infeasibility of a Germanic replica of the French revolutionary movement. In spite of which, nonetheless, over the years internal conflicts continued, there were some attempts of uprisings that sought to emulate the French and there were even a few cases in which Germanic territories declared themselves republics (cf. Whaley, 2012 II: 585-590).

The Kingdom of Bohemia, as a matter of fact, was not exempt from such internal disputes. Nevertheless, as in other parts of the Empire, just as in everyday life German was not the primary language and publications likely to be banned[148] were not always so difficult to obtain, in the scientific and intellectual spheres inside and outside the *Universitatis Carolinae*

[148] Among them were the books of Kant. These were part of the Germanic philosophy mainstream by the end of the 18th and their study in philosophy curricula was interdicted because of the threat they could pose "to civic stability within the Empire" (Lapointe, 2011: 13; cf. Lapointe and Tolley, 2014: 5; Whaley, 2012 II: 595-596; Howard, 2006: 121-129).

Ferdinandeae both Czech and those publications were present.[149] Indeed, at the end of 1791, Leopold II created a Czech language and literature chair, appointed to František Martin Pelcl, while the Bohemian Society of Sciences, approved by Joseph II in 1784 and turned into a Royal Society with the support of Leopold II in 1791, not only remained well aware of those objectionable publications, but also remained in contact with scholars and scientists abroad (cf. Krueger, 2009: 42, 60, 104-106 & 112). Furthermore, during that period, and in spite of the fact that Franz II aimed to vanish "the spirit of Enlightenment" (cf. Sebestik, 2014), Czech culture was not suppressed or isolated by the linguistic Germanization and the Habsburg policies, nor did the flow of foreign and forbidden texts stop. Evidence of this are: a) Pelcl's publications in Czech and in some cases on Czech during that decade; b) the foundation of the Society for the Patriotic Friends of the Arts in 1796; c) the activities and publications of the aforementioned Royal Society and even of the Patriotic-Economic Society (PES); as well as d) the fact that Karel Jindřich Seibt, an important figure in "the importation of works of the *Aufklärung* from Protestant Germany" (Krueger, 2009: 60, 128-129, 95-103; cf. Evans, 2006: 67, 138 & 143), was one of the most relevant figures at the University of Prague throughout the last decade of the 18th century, when he was not only professor but also dean of the Faculty of Philosophy and rector of the university. In any case, the effect of counter-reforms was gradual and not immediate.

Bolzano himself is evidence of that process, both as a student who grew up in that context and as a professor who at the beginning of his career was called to be an active part of it. He attended a Piarist gymnasium from 1791 to 1796, after which he entered the Faculty of Philosophy at the University of Praha. Over the next four years (since to the regular three years of classes an additional year was granted to him to "deepen his knowledge of mathematics and to think more about his future" (Sebestik, 2014: 292)) he took classes with Seibt, Vydra (professor of elementary mathematics and author of the first Czech algebra textbook), Vincenc Blaha, František Schmidt, Gerstner (professor of higher mathematics and member of the PES) and František Leonard Herget (professor of practical mathematics). However, in 1800 Bolzano

[149] It is worth quoting here a broad excerpt from Whaley's text: "The flow of publications continued unabated. In addition to newspapers and journals, a mass of translations of French pamphlets and commentaries kept the German public up to date. Over 2,000 works were translated between 1789 and 1799; some 1,100 texts were distributed from Leipzig between 1794 and 1798. Some 70 per cent of what was translated represented the moderate or Girondin position, which was the most congenial to the German reformist mentality. The subject matter changed as the reverses of 1794 created new perspectives. Direct commentaries on French politics were replaced by analyses of the implications of the 'failures' of the years 1792–4 and by historical analyses of the whole process since 1789. Then, the literature devoted to the theme of peace between 1795 and 1802 once more polarized opinion between moderate republican, radical democratic, and conservative writers, and changed the framework of discussion." (Whaley, 2012 II: 598)

chose to study theology, studies that lasted until 1804, and, although that same year he obtained his doctoral degree in philosophy, he also was ordained priest and, after he both entered competition for a position as professor of elementary mathematics and as professor of the science of religion at Praha, he was nominated for the second one, while Josef Ladislav Jandera got the first chair.[150]

In 1805, however, Bolzano's context, as the European context in general and the context of central Europe in particular, was changing drastically. While in 1797 France and Austria had signed first an armistice at Leoben and by the end of the year the peace Treaty of Campo Formio, France and the Reich did not achieve peace and in March 1799 France once again declared war on Austria, which along with Great Britain, Russia and the Ottoman Empire had reorganized a coalition against the former. After some initial victories of the coalition: a) in October 1799 Russia withdrew from the coalition after their defeat in the Second Battle of Zürich; b) in November of that same year a *coup d'état* usually known as the *coup* of 18 Brumaire led to the appointment of Napoleon as First Consul of France; c) the 1st of January 1801 the merging of the Kingdoms of Great Britain and Ireland established the United Kingdom of Great Britain and Ireland; d) in February of that year the Holy Roman Empire and the French Republic signed the Treaty of Lunéville, recognizing French control over the west bank of the Rhine, and just over a year later the Treaty of Amiens was signed between the United Kingdom and France, ending hostilities between both parts; e) in May 1803, after both Great Britain and France did not completely implement the agreements contained in the latter Treaty, the former resumed war against the latter, assembling a new coalition from 1804 with the Kingdom of Sweden (Swedish Pomerania), the Holy Roman Empire, Russia and the Kingdoms of Naples and Sicily; f) in May 1804 Napoleon was given the title of Emperor of France and in August Franz II was crowned as Franz I, Emperor of Austria, after the proclamation of the –First– French Empire and the Imperial Recess of 1803 (*Reichsdeputationshauptschluss*); g) at the end of 1805 France and Austria signed the Treaty of Pressburg and during the following year a brief new coalition against France that involved Prussia and Saxony was formed; and h) in July 1806 the *États confédérés du Rhin* was created and a few weeks later Franz II dissolved the Holy Roman Empire.

[150] I am grateful to Davide Crippa for sharing with me a digitalized copy of Bolzano's examination as well as his transcript of it.

Precisely, the chair of philosophy of religion to which Bolzano was appointed was one of the many set up by Franz II in a decree of early 1804, according to which those chairs were intended to improve religious instruction and, as a consequence, as summarized by Sebestik, "educate obedient citizens of the State and to eradicate the ideas of the French Lumières and the ideals of the French revolution" (Sebestik, 2014: 293). Bolzano obtained the confirmation of his position in 1807 and during the remaining years of the first decade of the 19th century he only published a single mathematical work, scilicet the first issue of his *Contributions to a Better-Grounded Presentation of Mathematics* (*Beyträge zu einer begründeteren Darstellung der Mathematik*), in addition to his 1804 work, *Considerations on some Objects of Elementary Geometry* (*Betrachtungen über einige Gegenstände der Elementargeometrie*). The following section shall precisely deal with both these works.

C.1.2. Bolzano's general conception of mathematics outlined in his works of 1804-1810

Beyond the grouping of Bolzano's works of 1804 y 1810 due to their publication during the first decade of the 19th century and for a little more than the first five years since obtaining his doctoral degree, there is a reason of content to do so. His *Considerations on some Objects of Elementary Geometry*, as he pointed out in the preface, concerned not merely some objects of elementary geometry but "the very first propositions of pure geometry" and, that way, constituted "a small sample of [his] changes" to the foundations and method of mathematics, as he wrote in the preface to his *Contributions to a Better-Grounded Presentation of Mathematics* (Bolzano, 1804: VIII; Bolzano, 1810: XIV).[151]

To begin with, for Bolzano mathematics was not the science of quantities, nor mathematical method should began with definitions. A definition, he said, inasmuch as it was a "statement of the most proximate components (two or more)" from which a concept was formed, could only be of composite concepts and, therefore, could not be the starting point in a scientific

[151] Bolzano wrote: "welche Probe doch nur die aller-ersten Sätze der reinen Geometrie betrifft" and "Zwar gab ich schon im J. 1804 eine kleine Probe meiner Veränderungen", respectively. Given the frequency with which from now on fragments of the works of Bolzano will be quoted, only in those cases in which the translation proposed here differs significantly from the English one published by Steve Russ (Russ, 2004), which is used as a basis, this will be mentioned.

exposition (cf. Bolzano, 1810: 42 & 53).[152] Instead, and in order to be able to fabricate a definition, he considered necessary, on the one hand, to have simple concepts, that is, concepts that could no longer be decomposed, and, on the other hand, to have "an insight" (*eingesehen*) on the possible and fruitful combination of some of them to produce a new one (cf. Bolzano, 1810: 44 & 53). For example, just as a straight line could be defined as an object containing "all those and only those points which lie between the two points a and b" (Bolzano, 1804: 57),[153] requiring further knowledge on what an object and a point were, mathematics defined as the science of quantities required further knowledge on the concepts of science and quantity: in the first case, a point not only was indeed a simple concept but, considered "as a mere characteristic of space which itself is not a part of [it]", could not actually be distinguishable – from what?– and thus could not constitute an object of geometrical consideration, being "the simplest object of geometrical consideration [...] a system of two points" (Bolzano, 1804: 47-48);[154] in the second case, a quantity, defined as a "(multiplicity) of things which are equal to the unity (or the measure)", could not be regarded as a simple but a composite concept (cf. Bolzano, 1804: 3-4).[155]

As a consequence, for Bolzano a correct scientific exposition should begin with simple concepts, whose understanding, he said, could be communicated through a certain kind of "arbitrary propositions" (*willkürliche Sätze*) called "circumscriptions" (*Umschreibungen*), as well as with those other arbitrary propositions by means of which a symbol was assigned to a concept, such as "the sign for addition is $+$" (cf. Bolzano, 1810: 50).[156] Concerning the latter, he stated that "semiotics [prescribed] certain rules" according to which the assignation of a sign to a concept x would not be entirely arbitrary: "the sign must be easily recognized, possess the greatest possible similarity to the concept designated, be convenient to the representation, and what is most important, it must not be in contradiction with any signs already chosen or cause any

[152] Bolzano wrote: "Die Logiker verstehen unter einer Erklärung (definitio) in dieses Wortes eigentlichstem Sinne die Angabe der nächsten (zwey oder mehreren) Bestandtheile, aus welchen ein gegebener Begriff zusammen gesetzt ist" and "Sie können offenbar nicht das Erste, womit man anfängt, seyn", respectively.
[153] Bolzano wrote: "Ein Ding, welches alle jene, und nur jene Punkte enthält, die zwischen den zwey Punkten a und b liegen, heisst eine gerade Linie zwischen a und b."
[154] Bolzano wrote: "als eines blossen Merkmals eines Raumes (semeion), das selbst kein Theil des Raumes ist" and "so ist der einfachste Gegenstand der geometrischen Betrachtung ein System zweyer Punkte."
[155] Bolzano wrote: "Grösse heisst ein Ding, insofern es angesehen wird als bestehend aus einer Anzahl (Vielheit) von Dingen, die der Einheit (oder dem Masse) gleich sind."
[156] Given the sense attributed by Bolzano to the word *Umschreibungen*, it seems more appropriate to translate it by "circumscriptions", as Rusnock (Rusnock, 2000), than by "descriptions", as Russ (Russ, 2004: 104).

ambiguity" (Bolzano, 1810: 51).[157] On the other hand, with regard to simple concepts, since they could not be properly defined insofar as definitions only concerned composite concepts, he said, they could only be explained through indications or propositions to delimit or circumscribe them: the concept of mathematical point, for example, could be explained from several propositions, such as "the point is the simple [object] in space, it is the boundary of the line and itself no part of the line, it has neither an extension in length, nor in breadth or in depth, etc." (Bolzano, 1810: 55).[158]

However, additionally to the establishment of conventions and definitions as the very first parts or elements of a proper scientific exposition, and postulates, axioms,[159] theorems and other elements as the subsequent ones, Bolzano stressed the importance of classifications to order such exposition and thus truly be a proper scientific one. To rectify the lack of "a true, natural order" in mathematical disciplines, he wrote, required "[to be] clear about all the simple concepts and axioms of these disciplines" and to know exactly which elements are needed to obtain "logically correct proof[s]" (Bolzano, 1810: 58).[160] Precisely, on the one hand, his 1810 work intended to clarify the concept of mathematics and its classification. That is, he intended to define that science and in doing so to offer some general guidelines on its order and to designate its object of study, even though at the same time he made some remarks on the mathematical method in spite of not pertaining to math, being "basically nothing else than

[157] Bolzano wrote: "Die Semiotik schreibt hier bestimmte Regeln vor. Das Zeichen muss leicht in die Augen fallen, mit dem bezeichneten Begriffe die möglichst grösste Aehnlichkeit besitzen, bequem zur Darstellung seyn, und was das Wichtigste ist, mit anden bereits gewählten Zeichen in keinem Widerspruche stehen und keine Zweydeutigkeit veranlassen." Here *Zeichen* was translated by "sign" and not "symbol" as Russ did since elsewhere Bolzano explicitly used the term "symbolische" to refer to a certain kind of expressions (Bolzano, 1810: 30-31), while in this case he used *Zeichen* to refer to an operational element.

[158] Bolzano wrote: "der Punct ist das Einfache im Raume, er ist die Grenze der Linie, und selbst kein Theil der Linie, er hat weder eine Ausdehnung in die Länge, noch in die Breite, noch in die Tiefe, u.s.w."

[159] For Bolzano, an axiom, in an "objective sense", was "a truth which we not only do not know how to prove but which in itself is unprovable" (*so müssen wir darunter eine Wahrheit verstehn, die wir nicht nur nicht zu erweisen wissen, sondern die an sich unerweislich ist*) (Bolzano, 1810: 63). That way, and considering the preeminence given by him to the definitions, his notion resembles that of Wolff, according to which "since the axioms are drawn directly from the definitions, they have no need of proof but rather their truth flows as soon as one look at those definitions" (*Weil nun die Grundsätze unmittelbahr aus den Erklärungen gezogen werden, haben sie keines Beweises nöthig, sondern ihre Warheit erhellet, so bald man die Erklärungen ansiehet, daraus sie fliessen*) (Wolff, 1717: 17; cf. Wolff, 1747: 625-626). However, in spite of this, one must be careful to link the mathematical method proposed by Bolzano with Wolff's proposal, at least with regard to that issue. While for Wolff certain principles or axioms were self-evident and therefore did not require demonstration, as Bolzano pointed out in his 1804 work (cf. Bolzano, 1804: 33), for Bolzano not only a) every proposition should be proved despite its obviousness (cf. Bolzano, 1804: II-V), but also b) where simple concepts ended and definitions began, axioms also ended and theorems began (cf. Bolzano, 1810: 96). This is because, for Bolzano, axioms were unprovable propositions (through –Barbara– syllogisms, which for him was the only valid kind of inference for propositions with simple concepts) of the kind of judgements "in which both subject and predicate [were] entirely simple concepts" (*in welchen beydes, Subject und Prädicat, ganz einfache Begriffe sind*) (Bolzano, 1810: 88-91). For a more detailed analysis of some similarities and differences between the Wolff's proposal and later works of Bolzano, cf. (Lapointe, 2011).

[160] Bolzano wrote: "In der That ist aber nichts schwerer, als diese Unordnung zu heben, und eine -nicht bloss scheinbare, sondern wahre, naturgemätze Ordnung einzuführen. Hiezu gehöret nämlich, dass man zuvor mit allen einfachen Begriffen und Grundsätzen dieser Disciplinen im Reinen sey, und bereits genau wisse, welcher Vordersätze ein jeder Grundsatz zu seinem logisch- richtigen Beweise bedürfe oder nicht bedürfe."

logic" (Bolzano, 1810: 39).[161] While, on the other hand, his 1804 work was meant to be an attempt to apply his early ideas on those issues in a particular discipline, namely geometry. However, lacking of a proper theory of the straight line, in 1804 he worked on the first propositions of the theory of triangles and parallels and only presented some thoughts to sketch out how the former could possibly be founded and proved.

What was then mathematics to Bolzano, if not the science of quantities? Throughout much of the first part of his 1810 work he not only exposed his own conception of mathematics and the various parts and disciplines that he thought comprised it, but at the same time he analyzed some contemporary conceptions. If by math is meant "the science of quantities", he said, "everything [will depend] on what one understands by the word 'quantity'" (Bolzano, 1810: 2), so that if it was defined traditionally, in the sense mentioned above, it would rule out part of the general math. On the contrary, if defined quantity as "something that exists and can be perceived by some sense" (as Franz Anton Ritter von Spaun),[162] it would either discard essential parts of mathematics, considered quantities as "only sensible objects", or it would group all the sciences, considered quantity as "every conceivable thing without exception" (Bolzano, 1810: 3).[163]

Even more, conceived in the wide sense, mathematics would be the science of all sciences, to put it some way, and therefore it would deal with things "as the freedom of God and the immortality of the soul" (Bolzano, 1810: 13),[164] which was not the case. On the other hand, conceived in the traditional sense, he argued, it would be "certainly defective and indeed too

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[161] Bolzano wrote: "Unter dieser Voraussetzung wäre nun eine Abhandlung über die mathematische Methode im Grunde nichts anders, als -Logic, und so zur Mathematik selbst gar nicht gehörig."

[162] Bolzano quoted "the anonymous author of the book *Versuch, das Studium der Mathematik durch Erläuterung einiger Grundbegriffe und durch zweckmässigere Methoden zu erleichtern. Bamberg and Würzburg*, 1805" (Bolzano, 1810: 2), an author which, as I explained in my contribution presented at the International Symposium *Bolzano in Prague 2014* (jointly organised by the Academy of Sciences of the Czech Republic (Institute of Philosophy), the International Bernard Bolzano Society and the University of Amsterdam), was Franz Anton Ritter von Spaun, an Austrian Germanic mathematician, as reflected in a copy of the book preserved by the Library of the University of Michigan and at the entry devoted to him in the *Allgemeine Deutsche Biographie* in 1893. Previously, Paula Cantù, in a paper first presented at the 2010 International Conference on Bolzano *Philosophy and Mathematics in the Work of Bernard Bolzano*, "suggest[ed] that the author, not identified until now, could well be Franz Ritter von Spaun, who after having worked for several years in the Austrian administration, was condamned for a writing that was considered politically dangerous, and from 1788 onwards, expecially during the ten years he passed in prison, devoted himself to mathematics" (Cantù, 2014: 298). Indeed, Franz Anton Ritter von Spaun left prision in 1798 and, given his political ideas and the conditions of his release, it is not strange that he opted for anonimity in many of his early 19th century works (cf. S., 1893: 69-70; Eisenmann, 1831).

[163] Bolzano wrote: "Begreiflich kömmt hiebey alles darauf an, was jemand unter dem Worte Grösse verstehe." "Im ersten Falle wäre Grösse, jedes gedenkbare Ding ohne Ausnahme; und wenn wir dann die Mathematik als die Wissenschaft der Grössen erklärten, so würden wir im Grunde alle Wissenschaften in das Gebiet dieser Einen ziehen. Im zweyten Falle dagegen wären nur sinnliche Gegenstände Grössen." Although *sinnliche Gegenstand* could be translated by "sensuous", here is preferred to translate it by "sensible", as Russ did (Russ, 2004: 91), so as not to complicate the undertanding of Bolzano's argument.

[164] Bolzano wrote: "als etwa der Freyheit, Gottes und der Unsterblichkeit der Seele."

narrow", since "the concept of quantity or of number [did] not appear in many problems of the theory of combinations (this so important part of the general mathesis)" (Bolzano, 1810: 4).[165] That way, just as he highlighted the importance of the awareness on the crucial role of combinations to form both composite concepts and the various gears of a scientific theory, and thus the theory itself, Bolzano stressed the relevance of combinations for the study of the elements of mathematical consideration.

It is true that Bolzano also drew attention to the fact that the objects of study of some particular mathematical disciplines were others than quantities and in fact were objects to which the concept of quantity was applied, as space and time (cf. Bolzano, 1810: 5). But, as a matter of fact, precisely this second argument against the usual employment of the notion of quantity in the definition of mathematics reinforced the first one: while space and time were objects of mathematical study that, although different from quantities, were quantifiable as combinations could also be, these latter were constitutive of the core part of the mathematical framework as those were not. In other words, although Bolzano said that in mathematics not everything were quantities, as combinations, space and time would exemplify, the key point was not the existence of particular objects of mathematical study as space and time, but the existence of essential objects of study other than the quantities. Just as composite concepts could not be produced without possible and fruitful –finite– combinations of simple concepts, a point could not be mathematically considered unless a second point was also considered and quantities could not be considered mathematically without combinations (finite indeed, as will be discussed in the last section (cf. infra C.2.2)).

Because of all this, Bolzano not only rejected the definition of mathematics as the science of quantity but also the interpretation of such definition as meaning "a science of those objects to which the concept of quantity is especially applicable" (Bolzano, 1810: 6).[166] If this latter was the case, he wrote, sciences as that whose object of study were the syllogisms or that other whose object of study were the categories "would in fact have to count as mathematics", unless a non-scientific criterion regarding the frequency of the applicability of the concept of quantity was

[165] Bolzano wrote: "Diese Bedeutung des Wortes Grösse vorausgesetzt, ist die gewöhnliche Erklärung der Mathematik, als einer Wissenschaft der Grössen, freylich mangelhaft, und zwar zu enge. [...] So kömmt in vielen Aufgaben der Combinationslehre (dieses so wichtigen Theiles der allgemeinen Mathesis) der Begriff der Grösse oder einer Zahl nicht einmahl vor."

[166] Bolzano wrote: "Dieses könnte vielleicht auf den Gedanken führen, die Mathematik als eine Wissenschaft von solchen Gegenständen zu erklären, auf welche der Begriff der Grösse besonders anwendbar ist."

taken into account (Bolzano, 1810: 7).[167] Instead, Bolzano proposed a definition of mathematics appropriate to the relevance he attributed to combinations: "mathematics could best be defined as a science which deals with the general laws (forms) according to which things must be regulated in their existence" (Bolzano, 1810: 11).[168] A definition, it must be stressed, similar to that proposed by all those Germanic mathematicians of the time with certain propensity for the approach of the so-called Combinatorial School led by Hindenburg.

By that definition, Bolzano intended to combine what seemed to him the best of other definitions. On the one hand, his definition, as the aforementioned interpretation of the traditional one, allowed him to consider both objects with an "objective existence independent of our consciousness" (*von unserem Bewusstseyn unabhängiges Daseyn besitzen*) and objects of thought. On the other hand, it provided him a broad enough definition to not constrict the object of mathematical study to quantities, but include also combinations, at the same time that sufficiently restrictive to differentiate its object of study from that of philosophy or metaphysics. In that sense, he wrote, definitions such as that of an anonymous reviewer, who defined mathematics as "a general theory of forms" (*allgemeine Formenlehre*),[169] or that of Kant "of pure natural science [...] as a science of the laws under which the existence of things (of phenomena) is ruled" (*als eine Wissenschaft von den Gesetzen, unter welchen das Daseyn der Dinge (der Phänomena) stehet*), could be reinterpreted according to his proposal (cf. Bolzano, 1810: 10-11 & 15). While, on the other hand, metaphysics would be distinguished from mathematics inasmuch as this one would deal "with the general conditions under which the existence of things is possible", and the former would deal with the "absolute necessity" of certain things "such as the freedom of God and the immortality of the soul" (cf. Bolzano, 1810: 11-14).[170]

Such a distinction between mathematics and philosophy was in fact fundamental for Bolzano and can be found in his writings both before and after 1810. In his autobiography, published in

[167] Bolzano wrote: "so würde man in der That alle Wissenschaften zur Mathematik zählen müssen."
[168] Bolzano wrote: "Ich denke also, dass man die Mathematik am besten als eine Wissenschaft erklären könnte, die von den allgemeinen Gesetzen (Formen) handelt, nach welchen sich die Dinge in ihrem Daseyn richten müssen."
[169] Paola Cantù wrote that "It would be interesting to verify whether the author could not be Bolzano himself" (Cantù, 2014: 298, fn.), although I agree with Johan Blok that this seems implausible (cf. Blok, 2016: 171, fn. 80).
[170] Bolzano wrote: "Mathematik und Metaphysik, die beyden Hauptbestandtheile unserer apriorischen Erkenntnisse, wären einander nach dieser Erklärung dergestalt entgegen gesetzt, dass erstere die allgemeinen Bedingungen abhandelte, unter welchen das Daseyn der Dinge möglich wird."

1836, for example, he declared that since his early years as student his pleasure in mathematics was based on its "purely speculative part", that is, he said, "what is at the same time philosophy" (Bolzano, 1836: 19),[171] while in his mathematical notebooks he wrote in 1802: "The definition of magnitude does not belong to mathematics; mathematics cannot achieve it. The definition of this concept belongs to the transcendental philosophy" (Sebestik, 2014: 293).[172] This last passage is indeed curious, considering: firstly, his criticism of Kant and the critical philosophy contained in his work of 1810, especially in the Appendix dedicated to "the Kantian theory of the construction of concepts through intuitions"; and, secondly, his own methodological proposal, exposed *in extenso* in that work but previously envisaged in his 1804 work. After all, if from his point of view the Kantian construction of concepts was not appropriate and, among other things, a strict scientific proof should not make use of alien concepts to the theory in which the thesis is to be proven, how could Bolzano simultaneously defend that the definition of the concept of quantity belonged to transcendental philosophy and not to mathematics?

In fact, his 1804 work began stating that mathematics was not only useful because of "its application to practical life", but also because of its "beneficial promotion of a thorough way of thinking" (*durch die wohlthätige Beförderung einer gründlichen Denkart liefern könne*), after which he proposed a couple of methodological rules whose application, he said, would improve speculative mathematics (Bolzano, 1804: I). The first one was that every proposition should be proved despite its obviousness and the second one that no proof should make use of concepts alien to it, just as no science should make use of concepts of a less fundamental science (cf. Bolzano, 1804: II-V). So, for example and according to his conception of geometry and his conception and classification of the mathematical sciences, as will be shown later, Bolzano defended that "all propositions about angles and ratios of straight lines to one another (in triangles) [should not be] proved by means of considerations of the plane", as the concept of motion should not be "used to prove purely geometrical truths" (Bolzano, 1804: V)[173] and

[171] Bolzano wrote: "Mein besonderes Wohlgefallen an der Mathematik beruhte also eigentlich nur auf ihrem rein speculativen Theile, oderich schätzte an ihr nur dasjenige, was zugleich Philosophie ist."

[172] I quote here the fragment as quoted by Sebestik, given my impossibility to access the original manuscript.

[173] Bolzano wrote: "dass man alle Sätze von Winkeln und Verhältnissen gerader Linien gegeneinander (in Dreiecken) mittelst Betrachtung der Ebene erweiset, wozu in den thesibus gar keine Veranlassung enthalten ist. Hieher zähle ich auch den Begriff der Bewegung, den manche Mathematiker zu Beweisen rein geometrischen Wahrheiten angewandt haben."

geometrical concepts should not be used to prove purely arithmetical truths (cf. Bolzano, 1810: 17).

Leaving aside the possibility of an incongruity in Bolzano, of which he was not exempt –as no one is–, two possible explanations seem feasible: Bolzano changed his mind from 1802 to 1804 or his opinion of 1802 somehow was consistent with what he stated in his works of 1804 and 1810. And although the first one should not be discarded, the aforementioned excerpt from his autobiography and some passages from his 1810 work suggest a way in which one could argue in favor of the second one. If mathematics and philosophy were "the two main classes of all human a priori knowledge" (*zwischen den beyden Hauptclassen aller menschlichen Erkenntnisse a priori*) (Bolzano, 1810: 8), as he agreed with critical philosophy they were, and if –pure– general mathematics (to which arithmetic, the theory of combinations and other parts belonged) dealt with hypothetical necessity, while metaphysics dealt with absolute necessity: could not one think of those general laws as a sort of absolutely necessary objects and, therefore, could not one think of the concept of quantity as belonging to philosophy or, in a sense, to the intersection of philosophy (which would define it) and mathematics (which would include it among its essential concepts)? Something that could fit with his position on mathematical method: "a work on mathematical method", he wrote, "would be basically nothing but logic and thus would not belong to mathematics itself" (*und so zur Mathematik selbst gar nicht gehö*rig) (Bolzano, 1810: 39).

Nevertheless, it must be noticed, on the one hand, that Bolzano did not explicitly address that issue in his works of 1804 and 1810. What he did was to quote the traditional definition of quantity (cf. Bolzano, 1804: 3-4; Bolzano, 1810: 4), debate some alternative conceptions in his 1810 work and state that the concept of quantity, as the one of number, belonged to –pure– general mathesis (cf. Bolzano, 1810: 4 & 17).[174] On the other hand, it should not be obviated that when Bolzano writes about transcendental and critical philosophy, as well as about various Kantian conceptions, he does so from his particular interpretation of them. That is to say, Bolzano's criticism of Kant's proposals might not always be faithful to what Kant actually said or wanted to say.

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[174] It goes beyond the scope of this work to elucidate how this all fits or does not fit with Bolzano's later projects, particularly his *Wissenschaftslehre* and his *Grössenlehre*, but even Příhonský's *Neuer Anti-Kant*, where this one wrote that, "to us", "mathematics [is] the doctrine of quantity" ("die Mathematik als eine Grössenlehre erklären") (Příhonský, 1850: 216).

All through his works of 1804 and 1810, Bolzano constantly referred to Kant, although undoubtedly it was in the second one, and especially in its Appendix, where he carefully explained the most important points of his disagreement with some of the latter's ideas. A disagreement that neither was absolute nor involved any disparagement, as he himself explicitly recognized in his 1810 work (and implicitly in the one of 1804), as Bolzano's later works to 1817 show and as Příhonský even stated in the preface to his *New Anti-Kant*, written with the collaboration of Bolzano (Příhonský, 1850: XVIff.). However, the relationship between Bolzano's proposal and that of Kant, as well as the historical evolution of criticism from the former to the latter, not only goes beyond the scope of this chapter but also divert it from its objective, insofar as only partially it is related to Bolzano's mathematical ideas. In other words, in this section all that matters is Bolzano's criticism of Kant until 1810 and to the extent that it can contribute to a better understanding of his own mathematical ideas and practices at the time.[175]

That said, Bolzano's criticism focused on the Kantian definition of mathematics: "mathematics", Kant wrote, "[is the knowledge of reason] from the construction of concepts" (Kant, 1781: 713).[176] For Bolzano, the problem with the conception of mathematics as the knowledge or

[175] As in the case of Wolff, the differences and similarities between Bolzano's proposal (and his critique of Kant's) and the Leibnizian tradition go beyond the scope of this work. A detailed study of the relationship between Bolzano's later works and Leibniz's proposal can be found in (Lapointe and Armstrong, 2014). With regard to that second relationship, however, it is worth quoting here a fragment of another work by Lapointe, inasmuch as it seems to connect with Bolzano's proposal contained in his earlier works: "A Leibnizian propositio, at least as Bolzano understood it, is 'something that can be thought, i.e. that can constitute the content of a thought' (cf. Bolzano, 1837, §21, 84, 85). But while Bolzano was ready to concede that the possibility of being thought is a property of propositions, he also assumed that the concept of a proposition did not entail the idea of this property. The concept of being 'thinkable' is not necessary for our understanding of the concept of a proposition and, hence, does not belong to its definition, assuming that it can be defined, a point on which Bolzano remains unclear (cf. Bolzano 1837, §128, 18)" (Lapointe, 2011: 167-168).

[176] Kant wrote: "Die philosophische Erkentniss ist die Vernunfterkentniß aus Begriffen, die mathematische aus der Construction der Begriffe." On the other hand, Bolzano paraphrased Kant and wrote that, for critical philosophy, "philosophische Erkenntniss, ermangelnd aller Anschauung, mit blossen discursiven Begriffen sich begnügen müsse. Sonach werde das Wesen der Mathematik am eigenthümlichsten durch die Erklärung ausgedrückt: dass sie eine Vernunftwissenschaft aus Construction der Begriffe sey (S. Kants Kritik d. r. V. S. 712)" (Bolzano, 1810: 8). Leaving aside the fact that Bolzano quoted the page where the section began and not the page in which are the lines to which he referred, it must be noticed that there is a difference between what Bolzano attributed to Kant and what Kant wrote, starting with the concept of *Vernunftwissenschaft* instead of the one of *Vernunfterkentniß*. A literal translation of the term used by Bolzano would be "science of reason", which could fit the only published English translation of which I am aware, scilicet the one of Steve Russ, who translated it simply by "science" (Russ, 1996: 182; Russ, 2004: 93). By contrast to Bolzano in 1810, Příhonský in his 1850 collaborative work with Bolzano was faithful to Kant's words and wrote that, for Kant, "[all] knowledge of reason is either from concepts or from the construction of concepts, the first one being called philosophical, the second one mathematical" ("[alle] Vernunfterkentniß ist entweder die aus Begriffen oder aus Construction der Begriffe, die erstere heisst philosophisch, die zweite mathematisch") (Příhonský, 1850: 213). Sandra Lapointe and Clinton Tolley translated Příhonský's lines in a very similar way to the translation proposed here, although instead of "knowledge of reason" they chose "cognition of reason" for *Vernunfterkentniß*, a translation that might even be more appropriate than mine, given that Tolley is said to be "a Kant scholar", which is not a minor detail (cf. Lapointe and Tolley, 2014: 138). The point is that there could be an important difference not only between "the science of the construction of concepts" and "the knowledge of reason from the construction of concepts", but also between "[the science] content with purely discursive concepts" ("blossen discursiven Begriffen sich begnügen müsse") (Bolzano, 1810: 8), as Bolzano said that Kant defined philosophy, and "the knowledge of reason from concepts", as Kant actually

science from the construction of concepts was in the claim that this required the intervention of pure or a priori intuitions ("einen Anschauung" (Kant, 1781: 718)), a concept in which he considered "there [was] already an inner contradiction" (*ein innerer Widerspruch*) (Bolzano, 1810: 9). Because, for Bolzano, intuitions were necessarily empirical and, as a consequence, intuitions or "empirical ideas" could only be involved in "empirical, perceptual or reality judgements" (cf. Bolzano, 1810: 140; cf. Lapointe, 2011: 32-33).[177] That way, while the particular judgement "some quadrilaterals *are* squares", he wrote, implied an "empirical claim" (*empirische Behauptuug*), the corresponding strictly or proper pure a priori (*rein a priorisch*) claim would be that "the concept quadrilateral *can contain* the concept of a figure with nothing but equal sides and angles" (cf. Bolzano, 1810: 112).[178]

Additionally, but related thereto, Bolzano considered equally problematic the Kantian thesis about the concept of number as one constructed in time and, consequently, the idea that the intuition of time belonged to arithmetic (cf. Bolzano, 1810: 9). A thesis, this second one, for which Bolzano gave no reference, but can be found in Kant's *Prolegomena to any future Metaphysics that will be able to come forward as Science* (*Prolegomena zu einer jeden künftigen Metaphysik, die als Wissenschaft wird auftreten können*), a book that we know Bolzano knew because, among other things, he quoted it in his work of 1804 (cf. Bolzano, 1804: 13): "Arithmetic," Kant wrote, "[constructs] its concepts of numbers by successive addition of the units in time" (Kant, 1783: 53).[179]

Bolzano agreed with Kant that mathematical truths, though not analytic but synthetic (cf. Bolzano, 1810: 146), were a priori; mathematics, it must be remembered, was for him one of

wrote in the quoted passage (while the idea of "purely discursive concepts" can be found in subsequent pages (cf. Kant, 1781: 719-722)).

[177] Bolzano wrote: "Ich bin mir nähmlich bewusst, Urtheile von der Form: 'ich nehme wahr - X', zu besitzen; und diese Urtheile nenn' ich empirische, Wahrnehmungs - oder Wirklichkeitsurtheile, und jenes X in ihnen heisse ich eine Anschauung, oder, wenn man will, eine empirische Vorstellung."

[178] Bolzano wrote: "Denn dass einige Vierecke wirklich Quadrate sind, ist so ausgedruckt, eine empirische Behauptuung; rein a priorisch kann es nur heissen: Der Begriff Viereck kann den Begriff einer Figur von lauter gleichen Seiten und Winkeln enthalten." The emphasis on both "quadrilaterals *are* squares" and "quadrilateral *can contain*" is not found in the original but here has been introduced to highlight the congruence between, on the one hand, Bolzano's distinction between those two judgements, and, on the other hand, his aforementioned definition of mathematics as a science conerned with the conditions of possibility of things and not with the proof of their existence (cf. Bolzano, 1810: 12).

[179] Kant wrote: "Arithmetik bringt selbst ihre Zahlbegriffe durch successive Hinzusetzung der Einheiten in der Zeit zu Stande." Given the context of the paragraph from which the quotation was extracted, here "bringt selbst", which could be translated by "brings itself", "achieves" or "attains", was replaced by "constructs".

the two main classes of human a priori knowledge.[180] Therefore, what he disagreed with was not the distinction "between the analytic and synthetic parts of our knowledge [...], an achievement [which Bolzano claimed had] to be attributed to Kant" (Bolzano, 1810: 135), but the introduction of the notion of "intuition" [*Anschauung*] as the ground of synthetic truths.

> I readily admit that there has to be a certain basis, which is quite different from the law of contradiction, by which the understanding connects the predicate of a synthetic judgement with the concept of the subject. But how this basis can be, and be called, intuition (and even, with a priori judgements, pure intuition) I do not find evident. (Bolzano, 1810: 138-139)[181]

While analytic truths could be showed to be true on the ground of that law, since a logical contradiction arises when its truth is denied, Bolzano rejected both the Kantian conclusion that mathematical knowledge was not purely conceptual and that synthetic truths rested on intuitions. Concerning the former, Bolzano opposed the classification of mathematics into pure and applied, instead of which he proposed the distinction between purely scientific and practical or technical mathematical exposition. While "mathematics is not concerned at all with what actually takes place but with the conditions or forms which something must have if it is to take place" (Bolzano, 1810: 32-33),[182] he said, it was undeniable that the purpose of some mathematical expositions was not "the greatest possible perfection of scientific form" but the "immediate usefulness for the needs of life" (Bolzano, 1810: 35).[183]

As for the second of such Kantian conclusions, Bolzano wrote that if, following Kant, intuitions were "representations of an individual" (*Vorstellungen von einem Individuo*), in contrast to the "representations of something general" (*Vorstellungen von etwas Allgemeinem*) or concepts, and if a "pure a priori intuition" was meant to be "an intuition which is combined with the awareness of the necessity that it must be so and not otherwise", "how could judgments which

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[180] Bolzano wrote: "there is no empirical judgement in the whole [mathematical] exposition, and [this] science is therefore a priori" (*Dann kömmt im ganzen Vortrage kein empirisches Urtheil vor, die Wissenschaft ist also apriorisch*) (Bolzano, 1810: 33).

[181] Bolzano wrote: "Ich meines Theils gebe gern so viel zu, dass es einen gewissen, vom Satz des Widerspruches ganz unterschiedenen, Grund geben müsse, aus welchem der Verstand das prädicat eines synthetischen Urtheils mit dem Begriffe des Subjects verknüpfet. Allein wie dieser Grund Anschauung, und zwar bey apriorischen Urtheilen reine Anschauung seyn und heissen könne; das finde ich nicht einleuchtend."

[182] Bolzano wrote: "Denn die Mathematik handelt ja überhaupt nicht von dem, was wirklich Statt findet, sondern von den Bedingungen oder Formen, die etwas haben muss, wenn es Statt finden soll."

[183] Bolzano wrote: "Ein solcher Vortrag unterscheidet sich von dem rein wissenschaftlichen auf eine ganz bestimmte Art durch die Verschiedenheit des Zweckes, der bey dem letzteren die möglichst grösste Vollkommenheit der wissenschaftlichen Form, und dadurch wieder die möglichst beste Uebung im richtigen Denken -bey jenem dagegen unmittelbare Brauchbarkeit für die Bedürfnisse des Lebens ist."

are absolutely certain, such as all a priori judgments, be produced by the connection with intuitions?" (Bolzano, 1810: 138 & 144).[184] Furthermore, why would mathematics, a purely conceptual knowledge, require intuitive (non-conceptual) cognitions to ground its truths? What his *Contributions* precisely intended to show was: a) that not being everything a quantity in mathematics, his definition –more appropriate than the traditional one– allowed to classify mathematics in a logical way, that is, from the pure and more general part to the individual disciplines with practical exposition (cf. Bolzano, 1810: 16); and b) that mathematical method also required some improvements in order to achieve a strictly rigorous and correct scientific exposition, that is, in order to present the "objective connection of judgments" (*objectiven Zusammenhang der Urtheile*) (cf. Bolzano, 1810: 40).

The classification proposed by Bolzano, from the general *mathesis* to the theories of time and space, both *in abstracto* (chronometry and geometry, respectively) and *in concreto* (theory of cause, for unfree things in time, and theory of motion or mechanics, for unfree things in space and time) (cf. Bolzano, 1810: 16-26), was only, therefore, a part of his argument against the disorder he perceived that prevailed at the time in mathematics. As a framework, mathematics in turn required elements that allowed its theories to be constructed, in the same way that a house requires bricks or other elements to conform its walls, and those elements (namely, conventions, definitions, axioms, theorems, etc.) as the theories required a proper order so that the walls could stand firm and not collapse. This was the other part of Bolzano's argument and in fact the one he wielded against Kant: mathematical truths had nothing to do with intuitions but rather with an objective logical –or deductive– connection between them and even between the concepts conforming them. "As a consequence of this", wrote Bolzano referring to that objective connection, "some of these judgments are the grounds of others and these are the consequences of those" (Bolzano, 1810: 40).[185] That is why, as mentioned before, for him "all propositions about angles and ratios of straight lines to one another (in triangles) [should not be] proved by means of considerations of the plane" (Bolzano, 1804: V), since the theory of plane was based on that of triangles and this one on the theory of the straight line and not vice versa (cf. Bolzano, 1804: IX).

[184] Bolzano wrote: "Wie aber können durch die Verbindung mit Anschauungen absolut gewisse Urtheile hervor gehen, dergleichen alle apriorische sind?"
[185] Bolzano wrote: "zu Folge dessen einige aus diesen Urtheilen die Gründe anderer, und diese die Folgen jener sind."

To illustrate his argument against Kant, Bolzano even provided an arithmetical example: "The propositions of arithmetic do not require the intuition of time in any manner. [...] Kant introduced the proposition $7+5=12$, instead of which we shall assume the shorter $7+2=9$ only [to have] an overview more easily" (Bolzano, 1810: 147).[186] If assumed the general proposition $a+(b+c)=(a+b)+c$, he said, which excluded any consideration of time, and given the necessary conventions and definitions, such as $1+1=2$, $7+1=8$ and $8+1=9$, one could prove the aforementioned proposition as follows:

$7+2=7+(1+1)$	Replacing 2, by definition, for $(1+1)$
$7+(1+1)=(7+1)+1$	By the general proposition $a+(b+c)=(a+b)+c$
$(7+1)+1=8+1$	Replacing $7+1$, by definition, for 8
$8+1=9$	Replacing $8+1$, by definition, for 9

(cf. Bolzano, 1810: 147)

What Bolzano attempted to make explicit through his 1804 work was precisely the existence of such an objective connection between geometrical truths: for example, given the concepts of 'identity' and 'being different', as well as the notion of a system of two points (*System zweyer Punkte*) and the concepts of distance and direction, one could form: 1) the notion of angle ("the system of two directions outgoing from one point") (*Das System zweyer aus einem Punkte ausgehender Richtungen*), 2) then the one of triangle (the system of three determined points and the three angles of the directions of every two of those points to the third) and 3) eventually the one of straight line ("an object which contains all and only those points which lie between the two points a and b") (*Ein Ding, welches alle jene, und nur jene Punkte enthält, die zwischen den zwey Punkten a und b liegen*) (cf. Bolzano, 1804: 44-57). That way, for him, it could be remedied the existing disorder in the geometry of Euclid, where all "sorts of dissimilar objects [were] dealt with in the individual theorems":

> Firstly there are triangles, but in such a way that they are already accompanied by circles which intersect at certain points; then angles, adjacent and vertex angles; then the equality of triangles; much later their

[186] Bolzano wrote: "Die Sätze der Arithmetik bedürfen der Anschauung der Zeit auf keine Weise. [...] Kant führt den Satz $7+5=12$ an; statt dessen wir, nur zu einer leichteren Uibersicht, den kürzeren $7+2=9$ annehmen wollen." For a detailed study of this discussion, cf. (Blok, 2016: 239ff.).

> similarity, which is however derived by an monstrous detour, firstly from the consideration of parallel lines, and even of the surface content [area] of triangles, etc.! (Bolzano, 1810: IX)[187]

A disorder which, he said, was linked to a faulty method (Euclid "[piled up] all his definitions at the beginning" (*alle Erklärungen gleich vorne aufhäuft*) (Bolzano, 1810: 53)), as well as to an erroneous consideration of the core objects of mathematical consideration (in this case geometrical), which, as in the case of the straight line and the plane, were properly speaking "objects of thought" (*Gedankendinge*) (cf. Bolzano, 1804: 48). Thus, he wrote in 1810, the argument that intuitions did underlie at least geometrical truths, inasmuch as geometrical objects produced an image in our imagination, was a misleading one: firstly, because such an image was not essential to the truth (the mental image of a triangle, for example, was not necessary in order to have the concept of triangle); secondly, because not even all geometrical objects could be constructed in the imagination, as could be said about the notion of straight line involved in "[the] proposition that every straight line can be extended to infinity" (*Die Satz, dass jede gerade Linie sich ins Unendliche verlängern lasse*) (cf. Bolzano, 1810: 149). Indeed, since Bolzano argued that "objects of thought" and not only objects with a concrete existence were objects of mathematical study, he spoke of "things" in general in his definition of mathematics ("a science which deals with the general laws (forms) to which things must conform in their existence"), so that with it he could refer to both "those [things] which possess an objective existence independent of our being, but also those which only exist in our imagination, [...] either as individuals (i.e. intuitions), or merely as general concepts" (Bolzano, 1810: 11-12).[188]

Nonetheless, inasmuch as his 1810 work intended to provide the guidelines for the general framework in mathematics and not to focus on any particular mathematical discipline, by the end of the first decade of the 19th century geometry was the only one about which Bolzano had published extensive lines. Despite this, he did offer some interesting brief remarks on other

[187] Bolzano wrote: "Und in der That, von was für ungleichartigen Gegenständen handeln nicht die einzelnen Lehrsätze im Euklides? Erstlich von Dreyecken, doch so, dass hier schon Kreise, die in gewissen Puncten sich schneiden, mitgenommen werden; darauf von Winkeln, von Neben-und Scheitelwinkeln; dann von der Gleichheit der Dreyecke; viel später erst von ihrer Aehnlichkeit, welche jedoch durch einen ungeheuern Umweg erst aus Betrachtung der Parallellinien, sogar der Flächeninhaltes der Dreyecke, u.s.w. hergeleitet wird!" Steve Russ translated *Scheitelwinkel* by "vertically opposed angles", which seems to illustrate quite well what Bolzano had in mind (Russ, 2004: 36).

[188] Bolzano wrote: "Unter dem Worte Dinge begreise ich hier nicht bloss solche, welche ein objectives, von unserem Bewutztseyn unabhängiges Daseyn besitzen, sondern auch solche, die bloss in unsrer Vorstellung existiren, und dieses zwar wieder entweder als Individuen (d.i. Anschauungen), oder als blosse allgemeine Begriffe."

mathematical disciplines that are worth mentioning here. Firstly, he wrote that a general theory of oppositeness should be considered as “a special appendix to the general mathesis” (*einem besondern Anhange zu der allgemeinen Mathesis*), something that resembles the hitherto usual Germanic presentation of oppositeness (cf. Bolzano, 1810: 20). Secondly, he counted among the still unclear mathematical theories not only the one about opposite quantities but also the ones on irrational and imaginary *quantities* (cf. Bolzano, 1810: V). Thirdly, he also mentioned the defective state of “higher algebra and the differential and integral calculus”, in which the “not yet [...] sufficiently explained” (*noch nicht hinlänglich aufgeklärt*) concept of infinity was used (cf. Bolzano, 1810: V & 30). Finally, he quoted “the important assertion that the function $f(x+i) = f(x) + ip + i^2q + i^3r + \cdots$ in general varies continuously with i” (*stätig verändere*) as an example of a mathematical truth which traditionally and mistakenly was “derived from geometrical consideration, namely, from the fact that a continuous curved line which intersects the abscissae-line has no smallest ordinate” (cf. Bolzano, 1810: 117).[189]

Each of those remarks, on the one hand, offers a glimpse on the state of affairs in the Germanic mathematical context of the early 19th century: negative, irrational and imaginary quantities were not yet fully accepted, infinity was still problematic and analytical proofs by geometric means were more and more repudiated as non-rigorous. Bolzano even went on to say that maybe in the future it would “be decided that the infinite or the differential [were] nothing but symbolic expressions” (*symbolischer Ausdruck*) (Bolzano, 1810: 30). On the other hand, as will be discussed (cf. infra C.2.2), his remarks on infinity and continuity in mathematics are directly linked to his subsequent mathematical works, published in the middle of the second decade of that century.

C.2. Bolzano’s mathematical works of 1816-1817

[189] Bolzano wrote: “aus einer geometrischen Betrachtung hergeleitet wird; aus dieser nähmlich, das es bey einer continuirlichen krummen Linie, die ihre Abscissenlinie schneidet, keine kleinste Ordinate gebe.” He quoted the general formula located, he said, “in Theorie des fonctions analytiques No. 14”, which Lagrange mentioned from the beginning of his work: “Considérons donc une fonction fx d’une variable quelconque x. Si à la place de x on met $x+i$, i étant une quantité quelconque indéterminée, elle deviendra $f(x+i)$; et par la théorie des séries in pourra la développer en une suite de cette forme $fx + pi + qi^2 + ri^3 + \&c.$, dans laquelle les quantités $p, q, r, \&c.$, coèfficiens des puissances de i, seront de nouvelles fonctions de x, dérivées de la fonction primitive fx, et indépendantes de la quantité i” (Lagrange, 1797: 2).

C.2.1. The context of Bolzano from c. 1810 to 1817

After the founding of the *Confédération du Rhin*, the dissolution of the Holy Roman Empire and the signing of the two peace agreements known as the Treaties of Tilsit between the French Empire and Russia and Prussia, all between July 1806 and July 1807, Britain and Sweden remained at war against France and so the fighting focused on non-Germanic territories during the last years of the 1800s (as Sweden, Portugal and Spain). The Peninsular War, however, triggered a new confrontation between Austria and France, which lasted for much of 1809 and ended with the signing of the Treaty of Schönbrunn.

The subsequent years the Peninsular War continued and in 1812, in the context of the dispute between France and Russia over controlling Poland, the former began its invasion of the second one, entering Moscow on 14 September and completely retreating three months later. Over the next year, the United Kingdom, Sweden, Prussia and Austria declared war on France, which mainly took place, in addition to the peninsular territory, in Germanic territories, with the Peninsular army crossing into France by the end of 1813 and the Allies entering Paris on March 30, 1814. As a consequence, Napoleon abdicated, the Treaties of Fontainebleau and Paris were signed and, finally, the Congress of Vienna was held from late 1814 until June 1815, resulting in the redraw of the map of Europe and the creation of the German Confederation.

While all that was happening, however, two important events took place: first, in Austria Klemens von Metternich was appointed Foreign Minister in 1809; second, in Prussia the University of Berlin was founded in 1810 "under purely national, secular auspices" (Howard, 2006: 130). By 1810, in fact, many of the Germanic universities that existed in 1789 did not exist anymore, a decrease of universities which, in the case of Prussia, was accentuated in 1807 when the Treaty of Tilsit, signed between France and Prussia, "stripped [the latter] of all its holdings west of the Elbe river [...], [holdings in which] were a number of universities: Duisburg, Halle, Paderborn, Erlangen, Erfurt, Münster, and Göttingen" (Howard, 2006: 148). But, while by 1809 the Prussian project of the University in Berlin was about to materialize, initially and largely developed by Karl Friedrich Beyme and finally credited to Napoleon's *protégé* Karl Freiherr von Stein and the person that this one recommended as head of the newly created 'Department of

Ecclesiastical Affairs and Public Education', namely, Wilhelm von Humboldt (cf. Howard, 2006: 145-153), on the contrary in Austria the intellectual and educational landscape was of closure.

Indeed, under Metternich's control of the Habsburg Monarchy's foreign policy, "the police kept a close eye on all intellectual and scholarly gatherings [...], censorship was ramped up [...] [and] reading rooms, circulating libraries, and literary reviews were all forbidden" (Krueger, 2009: 194-195). So, while in Prague in the middle of the first decade of the 19th century, on the one hand, the Polytechnical Institute was established (with Gerstner among its founders) as "an attempt to take deliberate steps to use education to shift Bohemia to the forefront of industrial nations by promoting the knowledge of industrial techniques and the practical application of science" (Krueger, 2009: 115), on the other hand Bolzano "was accused of being a 'Kantian' [...] a few months after he took up the chair of 'Science of the (Catholic) Religion'" (Lapointe and Tolley, 2014: 5; cf. Folta, 1981: 13). Furthermore, more than a decade later he was accused of heresy, dismissed by Emperor Franz I at the end of 1819 and banned from publishing and "from public scientific and clerical activities" (Lapointe, 2011: 4-5; cf. Folta, 1981: 13-14).

Before his forced retirement, however, Bolzano published three mathematical works between 1816 and 1817, one of which has been historically praised for being a pioneering contribution to the development of modern real analysis, namely, his 1817 *Purely Analytic Proof* (*Rein analytischer Beweis* (cf. Grattan-Guinness, 1970: 378; Dugac, 1986: 242; Ewald, 1996: 226; Detlefsen, 2008: 182; Bloch, 2011: 57). The following section will focus on this last work and will only discuss those other two insofar as they are related to that one.

C.2.2. Bolzano's *Rein analytischer Beweis* reconsidered

In 1816 Bolzano published a work on the binomial theorem, a theorem that, he said, was "usually quite rightly considered as one of the most important theorems in the whole analysis" and, even more, on which it could be "[said] that almost the whole of the so-called differential and integral calculus (higher analysis) [rested]" (Bolzano, 1816: III).[190] This statement, nowadays unusual, was not uncommon at the time and in fact can be found in the work of both Euler and Hindenburg. That way, Euler called that theorem a "universal theorem" (*theoremate universali*) in his *Introductio* (Euler, 1748: 55) and even the "foundation of all higher analysis" (*fundamentum constituit universae analyseis sublimioris*) in a later work (Euler, 1775: 103). Hindenburg, on the other hand, called the polynomial theorem, which could be derived from the binomial theorem, "the most important theorem of the whole analysis" (*Der polynomische Lehrsatz das wichtigste Theorem der ganzen Analysis*) (Hindenburg *et al*., 1796).

Both the Eulerian tradition and Hindenburg's combinatorial school, therefore, agreed on the importance of the binomial theorem despite the divergence of the procedures used by each to prove it. So, generally speaking, while authors linked to the French tradition advocated for the use of differentials in terms of limits (d'Alembert and others) or in terms of infinitesimals identified with zeros (Euler and others), Hindenburg's combinatorial proposal, on the contrary, intended to manipulate series in terms of whole or actual –given– finite parts. As Hindenburg wrote in 1781, "combinatorial art [teaches how] to exhibit and enumerate all the possible modes according to which several things can be arranged together, transposed [permuted] or thoroughly mixed [combined]" (Hindenburg, 1781: §I, IV).[191]

That explains the utility that according to Hindenburg's school had the combinatorial method for the analysis, conceived the "forms of the quantities" (*Formen der Grössen*) as the object of this latter (cf. Klügel, 1796: 49): "the specific enterprise of combinatorial theory", Hindenburg wrote at the beginning of his 1796 work on that independent ground-science (*selbständige Grundwissenschaft*), is "the arrangement of given elements, which is to be found in general or according to certain considerations and conditions, into an existing totality, [that is] the

[190] Bolzano wrote: "Den binomischen Lehrsatz pflegt man mit Recht als eines der wichtigsten Theoreme der ganzen Analysis zu betrachten. [...] So mögte es denn kaum übertrieben seyn zu fagen, dass fast die ganze so genannte Differential-und Integralrechnung (höhere Analysis) auf diesem Lehrsatze ruhet." The whole title of Bolzano's 1816 work is: *The Binomial Theorem, and as a Consequence from it the Polynomial Theorem, and the Series which serve for the Calculation of Logarithmic and Exponential Quantities, proved more strictly than before* (*Der binomische Lehrsatz, und als Folgerung aus ihm der polynomische, und die Reihen, die zur Berechnung der Logarithmen und Exponentialgrößen dienen, genauer als bisher erwiesen*).
[191] Hindenburg wrote: "Ars combinatoria docet exhibere atque enumerare modos possibiles omnes, secundum quos res plures propositae coniungi, transponi et permisceri possunt." Similarly, in his *Outline of the Combinations Theory and its Application to Analysis* (*Grundriss der Combinationslehre nebst Anwendung derselben auf die Analysis*), Conrad Diedrich Martin Stahl wrote that "the theory of combinations deal[s] with the composition of given things [or elements] into several totalities" ("Die Combinationslehre beschäftigt sich mit der Zusammensetzung gegebener Dinge zu mehrern Ganzen. Die gegebenen Dinge nennt sie Elemente, bezeichnet sie im allgemeinen mit Buchstaben und zeigt das Zusammensetzen derselben durch ein blosses Schreiben neben einander an") (Stahl, 1800: 1).

alteration and transformation of [...] a form into another one" (Hindenburg, 1796B: 153-154).[192] As he explained in a footnote to Klügel's text, even though analysis was sometimes called "the theory of functions", inasmuch as it was chiefly concerned with all kinds of functions, the conception of forms of quantities as the basis of analysis was appropriate "because, in general, everything that we know of the functions and their transformations can be brought back to it [that is, to the study of those forms]" (Klügel, 1796: 49, fn. a).[193]

Nevertheless, given the lack of rigor in the foundations of analysis, as Bolzano had already pointed out in 1810 (cf. Bolzano, 1810: V), in the late 18th century and early 19th century he and Lagrange, among others, attempted to remedy that situation, as the titles of their works of 1817 (1817B) and 1797, respectively, emphasize.[194] But, while Lagrange's proposal pursued an algebraic analysis rooted in Euler's program, what did Bolzano intend to do? Is it true that as early as 1817, as the usual interpretations defend, Bolzano was working on the foundations of modern real analysis and thus he can be considered as "one of the fathers of the [Weierstrassian] 'arithmetization' of [mathematics]" (cf. Klein, 1926: 56)? That is, was Bolzano really an isolated and largely ignored predecessor of the arithmetizing project whose "chief exponent" was Weierstrass? A project, it must be stressed, commonly identified with that latter's proposal because of his impact on new generations (e.g. Georg Cantor and Heinrich Heine) and his temporal preeminence over other authors (e.g. Dedekind), with whom he shared the conviction in the development of mathematics and a theory of irrational numbers on the basis of the arithmetic of natural numbers.

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[192] Hindenburg wrote: "Die, überhaupt oder nach bestimmten Rücksichten und Bedingungen, zu treffende Anordnung gegebener Elemente zu einem für sich bestehenden Ganzen, die Veränderung und Umstaltung einer gegebenen oder bereits geschaffenen Form in eine andere Gestalt, durch anderweitige Zusammensetzung, Trennung, Versetzung, Umtauschung der einzelnen oder verbundenen Elemente –dies ist das eigenthümliche Geschäft der Combinationslehre." While Klügel wrote: "Die eigentliche Analysis hat zum Gegenstande überhaupt die Formen der Grössen" (cf. Hindenburg, 1796B: 155-158).

[193] Hindenburg wrote: "Die Analysis beschäftiget sich vorzüglich mit den Functionen aller Art und ihren nach gegebenen Bedingungen erfolgenden Veränderungen; daher man sie auch zuweilen die Theorie der Functionen genannt hat. [...] Herr Prof. Klügel hat hier, zum eigentlichen Gegestande der Analysis überhaupt die Formen der Grössen, nach ihrer Entwickelung und Umwandlung in verschiedene Gestalten, angegeben; weil sich im Allgemeinen Alles, was wir von den Functionen und ihren Veränderungen wissen, darauf zurückbringen lässt. Auch Herr von Tempelhoff bezieht den grossen Nutzen der Lehre von den Functionen vornehmlich auf die Verwandlung ihrer Formen."

[194] The whole title of Bolzano's work, it was said before, is *Purely Analytic Proof of the Theorem, that between any two Values which give Results of Opposite Sign, there lies at least one real Root of the Equation*. It was in this work where Bolzano praised the work of Schultz on the groundings (*Begründung*) of pure mathematics (Bolzano, 1810: 8-9), which he had already quoted in (1804: [VII-VIII]). The whole title of Lagrange's work was: *Theory of analytic functions, containing the principles of differential calculus, free from all considerations of infinitely small, evanescents, limits and fluxions, and reduced to the algebraic analysis of finite quantities* (*Théorie des fonctions analytiques, contenant les principes du calcul différentiel, dégagés de toute considération d'infiniment petits, d'evanouissans, de limites et de fluxions, et réduite à l'analyse algébrique des quantités finies*). Also cf. (Lagrange, 1799).

Would that have been the case, it would imply that by that time he had detached himself from essential features of the Germanic mathematics of the second half of the 18th century that prevailed at the beginning of the 19th century. Indeed, to consider Bolzano's early mathematical work as a sort of anticipation of those later notions and practices would mean that, in spite of his conception of mathematics and the central role that for him had the combinatorial theory, he had different notions of quantities and numbers than those of his Germanic contemporaries, as well as different procedures for dealing with them. After all, for example, the same could be argued about Lagrange, who recognized the usefulness of Hindenburg's proposal "for the history and progress of Analysis" (Hindenburg, 1798: 370) and defended a conception of mathematics closer to that of the Germanic mathematicians of the time, but focused his proposal on "algebraic quantities".

Furthermore, while the apparent absence of an incompatibility between those two traits of Bolzano's early proposal and the groundbreaking features usually attributed to his *Purely Analytic Proof* cannot be adduced as strict evidence in favor of such a traditional interpretation, two kinds of evidence are typically used to sustain its validity. Firstly, Bolzano's own claims on the innovative nature of his works of 1816-1817: he described them as "sample[s] of a new way of developing analysis" (*als Probe einer neuen Bearbeitung der Analysis*) (Bolzano, 1816: XV; cf. Bolzano, 1817B: 20-21; Bolzano 1817A: VI-VII) and he stated that he had achieved a purely analytic proof of the intermediate value theorem (Bolzano, 1817B). In other words, he regarded his analytical procedures as inherently different from those that prevailed, which still employed geometric notions, and defended that his procedures were "purely analytic, [or] purely arithmetic, or algebraic" (*auch rein arithmetische, oder algebraische*) (Bolzano, 1817A: VI).[195]

Secondly and most importantly, however, the evidence that is normally used in favor of the traditional reading is, as it should be, eminently mathematical. As a consequence, four points of his *Purely Analytic Proof* are identified as clear signs of at least a partial anticipation of modern real analysis standpoint, namely: 1) his definition of the continuity of a function; 2) his

[195] Bolzano wrote: "Eine rein analytische (auch rein arithmetische, oder algebraische) Verrichtung heisst eine solche, zu Folge der man eine gewisse Function aus einer oder etlichen andern blos dadurch ableitet, dass man min ihnen gewisse Veränderungen und Verbindungen vornimmt, welche durch eine von der Natur der bezeichneten Grössen ganz unabhängige Regel ausgesprochen sind" (Bolzano, 1817A: VI).

[convergence] criterion; 3) his theorem about the existence of a [least] greatest bound value;[196] and 4) his formulation of the intermediate value theorem. In fact, from that perspective, in a sense the last mentioned result would be the summit of Bolzano's modernity, meaning that it would be both the ultimate expression of his innovative analytical proposal, as well as the best sample of the limits of its scope. Thus, Bolzano wrote:

> If two functions of x, fx and ϕx, vary according to the law of continuity either for all values of x or at least for all those lying between α and β, [and] furthermore if $f\alpha < \phi\alpha$ and $f\beta > \phi\beta$, then there is every time [always] a certain value of x lying between α and β for which$fx = \phi x$. (Bolzano, 1817B: 51)[197]

As John Stillwell wrote about Bolzano's proof of this theorem, in spite of being "ahead of his time", it "was incomplete, because a definition of the continuum was completely lacking in his time" (Stillwell, 2010B: 22). Or, taking into account what Johan van Benthem said, even though Bolzano's proof of the "Intermediate Value Theorem for continuous functions on $\mathbb{R}$ [stated] that, for every three distinct real numbers x, y, z, if $f(x) < z < f(y)$, then $z = f(u)$ for some u between x and y" (van Benthem, 1991: 109), the absence of an appropriate definition of the real number system resulted in a devious procedure.

Precisely, the consensus that from the late 19th century has gradually been reached on that 1817 work of Bolzano can be summarized in that it is an "epoch-making paper on the foundations of real analysis, [...] the starting point for the modern theory of the continuum" (Ewald, 1999: 225-226). On the contrary, thesis here is that a careful reading of Bolzano's first mathematical works (in particular those of 1816-1817) suggests a different interpretation of these ones. That way, accordingly to this alternative reading, although those works hinted some groundbreaking concerns and features, ultimately his mathematical notions and practices were deeply rooted in views and practices that he inherited and which were heavily deviant from later conceptions and practices of real analysis.

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[196] This theorem is usually called the "least upper bound" theorem. While the use of "bound" instead of "boundary" (which, by means of the element "ary", may indicate that it "pertains or is connected to") is taken for granted (since, in addition, although he does not use the term "Grenze" in §12, he does use it in §13), the use of "greatest" instead of "upper" is, to say the least, unusual. However, as Bolzano's corresponding fragment mentioned here (the third) will show, he refers to that quantity as "die grösste", and so translating it as "the greatest" seems to be the most appropriate thing to do given also the considerations set forth throughout this section.

[197] Bolzano wrote: "Wenn sich zwey Functionen von x, fx und ϕx, entweder für alle Werthe von x, oder doch für alle, die zwischen α und β liegen, nach dem Gesetze der Stetigkeit ändern; wenn ferner $f\alpha < \phi\alpha$ und $f\beta > \phi\beta$ ist: so gibt es jedesmahl einen gewissen zwischen α und β liegenden Werth von x, für welchen $fx = \phi x$ wird."

Consider, for example, the very first pillar on which that and the other two mentioned results of Bolzano rest, that is, his definition of a continuous function of –we would say nowadays– a one real variable. According to traditional reading, although Bolzano had already introduced a similar definition in his 1816 work (cf. Bolzano, 1816: 34), this was not articulated in the highly praised formal terms of his 1817 enunciation. That way, it was in his *Purely Analytic Proof* where he provided "the first rigorous definition of continuous function [along with Cauchy]" (Grabiner, 1981: 87), "a definition of the continuity of a real-valued function in a real interval (RB 11f.), which is substantially the same as the modern one" (Berg, 1962: 15) or "the first clear presentation of the epsilon-delta definition of continuity" (Coffa, 1991: 28). All of which, if one listens Bolzano's words with modern ears, is quite undeniable:

> According to a correct definition, by the expression that a function fx, for all values of x which lie within or outside certain limits, varies according to the law of continuity, it is understood, simply, that if x is any such value, the difference $f(x+\omega) - fx$ can be made smaller than any given quantity, if one can assume ω as small as one always wants, or it is (according to the designations we introduced in §14 of The binomial theorem etc., Prague, 1816) $f(x+\omega) = fx + \Omega$. (Bolzano, 1817B: 11-12)[198]

For Bolzano, this was a "purely analytic procedure" since, as he explained in his second work of 1817, that function could be obtained from other ones by means of changes and combinations, "expressed by a rule that is entirely independent of the nature of the designated quantities" (Bolzano, 1817A: VI).[199] This is precisely the algebraic approach that underlies his works of that time, as emphasized by the fact that he regarded the intermediate value theorem, to which he devoted his *Purely Analytic Proof*, as a proposition of the "theory of equations" (*Lehre von den Gleichungen*) (cf. Bolzano, 1817B: 3). As Steve Russ wrote, "[d]oubtless [Bolzano] would have regarded this theory of equations as part of [...] the *Analysis der endlichen Größen*, [...] but more because it [was] algebraic than because of an underlying limit concept" (Russ, 2004: 143).

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[198] Bolzano wrote: "Nach einer richtigen Erklärung nähmlich versteht man unter der Redensart, dass eine Function fx für alle Werthe von x, die inner- oder ausserhalb gewisser Grenzen liegen, nach dem Gesetze der Stetigkeit sich ändre, nur so viel, dass, wenn x irgend ein solcher Werth ist, der Unterschied $f(x+\omega) - fx$ kleiner als jede gegebene Grösse gemacht werden könne, wenn man ω so klein, als man nur immer will, annehmen kann; oder es sey (nach den Bezeichnungen, die wir im §. 14. des binomischen Lehrsatzes u. s. w. Prag 1816. eingeführt) $f(x+\omega) = fx + \Omega$."

[199] Bolzano wrote: "Eine rein analytische (auch rein arithmetische, oder algebraische) Verrichtung heisst eine solche, zu Folge der man eine gewisse Function aus einer oder etlichen andern blos dadurch ableitet, dass man mit ihnen gewisse Veränderungen und Verbindungen vornimmt, welche ducrh eine von der Natur der bezeichneten Grössen ganz unabhängige Regel ausgesprochen sind."

However, independently thereof, a question emerges as soon as one reads Bolzano's statement on the continuity of a function, namely: what does the designations ω and Ω stand for? To begin with, considered ω and Ω as "designations" (*Bezeichnungen*), and being faithful to what he said in his 1810 work, they belonged to the class of arbitrary propositions or conventions (*Classe der willkürlichen Sätze*) for concepts that did not have a proper definition (*eigentlichen Erklärung*) (Bolzano, 1810: 50 & 56). Which in turn means that what he did in §14 of his 1816 work was a) to introduce a convention according to which the symbols ω and Ω designated "a quantity which can become smaller than any given",[200] after which b) he enunciated a theorem about the "algebraic" sum or difference of a finite multitude (*endliche Menge*) of those quantities ω, which he designated as Ω (Bolzano, 1816: 15).[201]

The underlying idea to that notion, on the contrary, is not to be found in that paragraph but in the preface to that work. There, Bolzano openly criticized "the assumption of a sum of infinitely many quantities" ("infinite series") "and every attempt to calculate its value" ("calculus infinitesimalis") (Bolzano, 1816: IV). Instead, he proposed to use "the concept of those quantities, which can become smaller than any given quantity, or ([...] less properly) quantities which can become as small as one may want" (Bolzano, 1816: V).[202] This was because, as he explained, for him the concept of infinitely small quantities or infinitesimals was equivalent to the idea of a quantity that *de facto* (in-act) was smaller than any *conceivable* (*gedenkbare*) and not only any given (*gegebene*) quantity, and thus was contradictory (*widersprechend*). While, by contrast, the concept that he used of a "variable" (*veränderliche*) quantity that *could* become smaller than any *given* or *actually available* quantity was entirely correct and did not contain anything objectionable (*anstössig*) (cf. Bolzano, 1816: V). "[S]uch quantities", he even wrote, "are given very often both in space and in time" (Bolzano, 1816: V),[203] a remark that, as will be discussed below, should not be regarded as a mere far-fetched idea.

[200] Bolzano wrote: "Willkürlicher Satz. Eine Grösse zu bezeichnen, die kleiner als jede gegebene werden kann, wählen wir das Zeichen w, Ω, oder sonst ein ähnliches."
[201] Here "Menge" is translated as "multitude" and not as "set" to avoid the association of this latter term with the set theory devoloped decades later, nor as "number", as Steve Russ does (cf. Russ, 2004: 173), since "multitude", contrary to "number", seems to emphasize the primitive sense of "Menge" as a bunch of something. It is true that Bolzano refers to a *endliche Menge* and thus one could question the fact that the quantities $w, w^{(1)}, w^{(2)}$ would be better described as a "finite number" than as a "finite multitude", but that Bolzano refers to a "multitude" is shown by his own example, namely, "the quantities $w, w^{(1)}, w^{(2)}, \dots, w^{(m)}$."
[202] Bolzano wrote: "des Begriffes solcher Grössen bediene, die kleiner als jede gegebene werden können, oder (wie ich sie zur Vermeidung der Eintönigkeit zuweilen gleichfalls nenne, obwohl schon minder eigentlich) der Grössen, welche so klein werden können, als man nur immer will."
[203] Bolzano wrote: "Muss nicht vielmehr ein Jeder einsehen, dass es dergleichen Grössen im Raume sowohl als in der Zeit sehr häusig gebe?"

So far, therefore, several things should be clear about Bolzano's concept of ω: they are 'quantities'; these quantities are said not to be infinitesimals; unlike infinitesimals, it is postulated as a non-contradictory concept; unlike infinitesimals, it is assumed that it can be calculated with them; its variability underlies Bolzano's definition of the continuity of a function; and Bolzano's formulation of the difference $f(x+\omega)-fx$ closely resembles the modern Weierstrassian. How one interprets from this Bolzano's ω, nonetheless, determines much of the general interpretation of his early mathematical work, since it is usually taken for granted that they show his attempt to work with our modern real numbers (rigorously defined later by Weierstrass, Dedekind and Cantor). Moreover, the nature of quantities ω is traditionally posed in terms of a dichotomy, as if they were either numbers (i.e. Weierstrassian ε) or quantities that tend to zero in an implicitly dynamic way (i.e. Cauchy's infinitesimals). That is to say, it is traditionally assumed that those Bolzanian quantities anticipated in a strict or at least partial sense the modern ε, something that is not surprising if one considers the respective definitions of a continuous function of Cauchy and Weierstrass:

> Let $f(x)$ be a function of the variable x, and suppose that for each value of x between two given limits, the function always takes a unique [and] finite value. If, beginning with a value of x contained between these limits, we add to the variable x an infinitely small increment α, the function itself is incremented by the difference $f(x+\alpha)-f(x)$, which depends both on the new variable α and on the value of x. Given this, the function $f(x)$ is a *continuous* function of x between the [two] assigned limits [to that variable] if, for each value of x between these limits, the numerical value of the difference $f(x+\alpha)-f(x)$ decreases indefinitely with the numerical value of α. (Cauchy, 1821: 34)[204]

> If $f(x)$ is a function of x and x is a definite value, then the function will change to $f(x+h)$ when x changes to $x+h$, the difference $f(x+h)-f(x)$ being called the transformation [or "variation"] that thereby undergoes the function by changing the argument from x to $x+h$. If it is now possible to determine a limit δ for h, so that for <u>all</u> values of h whose absolute value is smaller than δ, $f(x+h)-f(x)$ will be smaller than

[204] Cauchy wrote: "Soit $f(x)$ une fonction de la variable x, et supposons que, pour chaque valeur de x intermédiaire entre deux limites donées, cette fonction admette constamment une valeur unique et finie. Si, en partant d'une valeur de x comprise entre ces limites, on attribue à la variable x un accroissement infiniment petit α, la fonction elle-même recevra pour acroissement la différence $f(x+\alpha)-f(x)$, qui dépendra en même temps de la nouvelle variable α et de la valeur de x. Cela posé, la fonction $f(x)$ sera, entre les deux limites assignées à la variable x, fonction *continue* de cette variable, si, pour chaque valeur de x intermédiaire entre ces limites, la valeur numérique de la différence $f(x+\alpha)-f(x)$ décroit indéfiniment avec celle de α." The translation, slightly modified, is from Robert E. Bradley and C. Edward Sandifer (Bradley and Sandifer, 2000: 26).

any other so small quantity ε, then one says that infinitely small changes [or "variations"] of the function correspond to infinitely small changes [or "variations"] of the argument. (Weierstrass, 1861: 2)[205]

Undoubtedly, the three definitions are very similar. But whether the one of Bolzano is at least equivalent to the one of Cauchy, or it even anticipated Weierstrass', rests on two assumptions. On the one hand, it is considered that the aforementioned definition of Weierstrass, presented during his summer course of 1861 and whose notes were taken by Hermann Schwarz, is an early presentation of the modern epsilon-delta definition of continuity, since it contains clear hints of it and replaces the intuitive idea of "tend towards" by inequalities (cf. Dugac, 1976: 7). On the other hand, it is assumed that in 1821 Cauchy somehow anticipated at least that early definition of Weierstrass. Which means, in both cases, that the explicit and immanent differences between the three definitions, as well as the differences between those definitions contained in their respective early works and the ones contained in their later works, tend to be overlooked.

Key question here is whether or not Bolzano, in spite of his terminology and the ideas underlying his formulation of the continuity of a function, in some way anticipated those other two. Therefore, insofar as this chapter is not about the development of the analytical approach of Cauchy and the Weierstrassian arithmetizing approach, for the sake of the traditional argument those two assumptions should be considered correct, even though, strictly speaking, they are untenable. After all, not only the ideas of those two mathematicians were not always –entirely– the same, but also both used expressions such as "infinitely small increment" (Cauchy) and "infinitely small changes" (Weierstrass). That way, traditional reader's stance can be summed up in the words of Hans Freudenthal: "Bolzano's and Cauchy's definitions [of the continuity of a function] are equivalent. Bolzano's is far better; it is modern (though instead of δ and ε he uses ω and Ω); the succession of the quantifiers is correct and clear" (Freudenthal, 1971: 380).

As a result of that, supporters –and promoters– of traditional interpretation of those definitions of Bolzano and Cauchy argue that these ones represent $|f(x+\omega)-f(x)|<\varepsilon$ and $|f(x+\alpha)-$

[205] Weierstrass wrote: "Ist $f(x)$ eine Funktion von x und ist x ein bestimmer Wert, so wird sich die Funktion, wenn x in $x+h$ übergeht, in $f(x+h)$ ändern; die Differenz $f(x+h)-f(x)$ nennt man die Veränderung, welche die Funktion dadurch erfährt, dass das Argument von x in $x+h$ übergeht. Ist es nun möglich, für h eine Grenze δ zu bestimmen, sodase für alle werte von h, welche ihrem absoluten Betrage nach kleiner als δ sind, $f(x+h)-f(x)$ kleiner werde als irgendeine noch so kleine Grösse ε, so sagt man, es entsprechen unendlich kleine Aenderungen des Arguments unendlich kleinen Aenderungen der Funktion."

$f(x)| < \varepsilon$, respectively (cf. Grabiner, 1981: 8-9). However, either those infinitesimalist old-fashioned expressions are left aside as manners of speaking, in which case Weierstrassian definition was not the first one since it was anticipated by the ones of Bolzano and Cauchy (both as to the way in which it was stated and used), or, if taken into account that expression (together with, for example, his carelessness on absolute values), it was not the first one and a strict modern epsilon-delta definition was yet to come. Because, it must be stressed, the possibility that Bolzano's ω are neither Weierstrassian ε nor Cauchy's α is simply discarded from the very beginning by the traditional reading, according to which either Bolzano's notions of 'quantity' and 'number' were pretty similar to the ones of Weierstrass or, at least (as the ones of Cauchy), they headed towards the latter but still had some essential kinetic traits.

Therefore, it is important to pay attention, in addition to the terminology used by Bolzano, to his mathematical practices in order to elucidate his similarities and differences with respect to later proposals. Given the objectives of this work, nonetheless, a distinction can be made between details or features of Bolzano's mathematical practices that are not so relevant to those objectives and the ones that are highly significant to them. For example, regarding the first type, a minor detail usual in his works of 1816-1817 is the lack of explicit reference to absolute values (cf. Bolzano, 1816: VI, 14-15 & 17), something that was not uncommon at the time (cf. Cauchy, 1821: 404; Cajori, 1929/1993: II, 123-124). Curiously, one of the few passages of his works of 1816-1817 in which Bolzano was explicit about that issue was at the beginning of his proof of the intermediate value theorem, where he stated that "the values of the functions fx and ϕx are to be compared with each other just as absolute quantities, that is without regard to signs" (Bolzano, 1817B: 51; for *absolute Werth*, cf. Bolzano, 1817A: XX).[206]

Additionally, among such type of details which are not so relevant here, may be mentioned: a) the differences between the approaches of Cauchy (who referred to an interval) and Bolzano (who did not) to the continuity of a function; b) the absence, in the convergence criteria of those two authors, of a distinction between pointwise and uniform convergence, which would be present in later Weierstrassian approach (cf. Weierstrass, 1876: 202); c) mathematical mistakes, as for example Bolzano's conclusion that "the binomial equation holds for no value of x which is

[206] Bolzano wrote: "Wir müssen erinnern, dass in diesem Lehrsatze die Werthe der Functionen fx und ϕx bloss ihrer absoluten Grösse nach, d. h. ohne Rücksicht auf ein Vorzeichen, oder so, als ob sie gar keine des Gegensatzes fähige Grössen wären, mit einander verglichen werden sollen."

$>$ or even only $= \pm 1$, unless at the same time n is a whole positive number or zero" (Bolzano, 1816: 53),[207] since the equation does hold for some values of n at $x = \pm 1$ (cf. Rusnock, 2000: 69); and d) the terminological differences between Bolzano's formulation of his theorem about the existence of a [least] greatest bound value and the so-called Bolzano-Weierstrass theorem.

On the contrary, the details that are highly significant here not only concern some of the results just mentioned, as for example his convergence criterion and his theorem about the existence of a [least] greatest bound value, but above all concern his notions of 'quantity' and 'number'. Bolzano, it is widely recognized even among the less moderate pens that second the traditional reading, lacked a rigorous definition of the real numbers and, therefore, within his analytical framework there was no set of real numbers, at least not in the modern sense. That is to say, in his early mathematical proposal there was no such purely mathematical domain of objects (numbers), which within subsequent frameworks would be required to be a continuous (and totally ordered and dense) but static one. And yet, did Bolzano at least have a notion of 'quantity' that somehow evokes the modern notion of number, as well as a concept of 'number' that could be considered compatible with its modern extension? That is, indeed, the key question to ask.

To begin with, within the Weierstrassian analytical framework a variable quantity x was a syntactic notion. "By a variable quantity", Weierstrass wrote in 1886, "one understands a quantity which is defined in such a way that there are infinitely many quantities corresponding to the given definition" (Weierstrass, 1886/1988: 57).[208] As he went on to explain, that meant that depending on whether in the stated numerical domain (*Gebiete*) the numbers were formed from a main unit or consisted of two main units, they would be real variable quantities or complex variable quantities, respectively (cf. *ibid*.).[209] Therefore, a variable quantity x could denote one or another type of numbers, depending on what one established in each case, so that: a) an "unlimited variable real quantity" (*unbeschränkt veränderliche reelle Grösse*) was a

[207] Bolzano wrote: "Also gilt die Binomial-gleichung für keinen Werth von x, der $>$ oder auch nur $= \pm 1$ ist, wenn nicht zugleich n eine ganze positive Zahl oder Null ist."
[208] Weierstrass wrote: "Unter einer veränderlichen Grösse versteht man eine Grösse, die so definiert ist, dass es unendlich viele Grössen gibt, die der gegebenen Definition entsprechen."
[209] Weierstrass wrote: "So z.B. bilden im Gebiete der aus einer Haupteinheit gebildeten Zahlen diejenigen Zahlen, welche Vielfache der Haupteinheit sind, veränderliche Grössen. Man kann zwischen sog. reellen und komplexen veränderlichen Grössen, welche letzteren aus zwei Haupteinheiten zusammengesetzt sind, unterscheiden; es muss aber in jedem einzelnen Falle angegeben werden, ob man die Definition auf die eine oder die andere Art von Grössen ausdehnen will."

variable quantity that could assume all real values (all values between $-\infty$ and $+\infty$) and whose domain was represented by all the points of a line (*sämmtliche Punkte*) (cf. Weierstrass, 1886/1988: 83);[210] and b) a variable quantity could assume infinitely small values (*unendlich kleine Werthe*) when among the values it was capable of, there were quantities smaller than an arbitrarily small quantity (cf. *id*.: 57).[211]

From the arithmetizing perspective of Weierstrass and later mathematicians, the set of real numbers was not only infinite in the sense of not having a last member, but also infinite inasmuch as its de facto given members could measure any given quantity and thus correspond to any ordinary quantity (cf. Gray, 2015: 253). The development of this conception, however, occurred through a process that eventually led to the replacement of the previously core notion of variable quantity by syntactic variables (i.e. a character that represents a number) and functions of a real variable (i.e. functions within the real domain). But throughout that process, while the acceptance of some emerging concepts and practices delayed more than that of others, some remnants of the pre-modern analytical framework lasted longer than others. That way, concerning the remnants, Dedekind himself denounced in the 1880s the prevalence of "foreign ideas" (*fremdartiger Vorstellungen*) in analysis, such as the "measurable quantities" (*messbaren Grössen*) (Dedekind, 1888: X; cf. Dedekind, 1872: 9) or, without going any further, the appellative "numerical quantities" (*Zahlengrössen*) used by Weierstrass and Cantor for rational and irrational numbers (cf. Cantor, 1872/1932; Weierstrass, 1878/1988), which evoked a previous understanding of mathematics as the science of discrete and continuous quantities.

As for the emergence of practices and concepts, what today is known as Bolzano-Weierstrass theorem[212] as a matter of fact provides a good example of that process and, in turn, of the mathematical anticipation attributed to Bolzano, who wrote: "If a property M does not apply to all values of a variable quantity x but does apply to all values smaller than a certain u, then there is always a quantity U which is the greatest of those of which it can be asserted that all

[210] Weierstrass wrote: "Eine unbeschränkt veränderliche reelle Grösse ist eine solche, die alle Werthe zwischen $-\infty$ und $+\infty$ annehmen kann; sämmtliche Punkte einer Geraden repräsentieren das Gebiet einer solchen Veränderlichen."

[211] Weierstrass wrote: "Wir sagen von einer veränderlichen Grösse -sei sie nun unbeschränkt oder beschränkt veränderlich-, sie könne unendlich kleine Werthe annehmen oder sie sei solcher Werthe fähig, wenn unter den Werthen, die sie annehmen kann, Grössen sind kleiner als jede beliebig klein angenommene Grösse."

[212] I am grateful to Gregory H. Moore for sending to me a copy of his article "Historians and Philosophers of Logic: Are They Compatible? The Bolzano-Weierstrass Theorem as a Case Study" in the fall of 2015, which is used here as a basis of what I explain in the following paragraphs about what is called the Bolzano-Weierstrass theorem. An advance of this was presented that same year at the *Third International Meeting of the APMP*, held in Paris at the Institut Henri Poincaré.

smaller x possess the property M" (Bolzano, 1817B: 41).[213] From the perspective of the authors that defend the traditional reading of those lines of Bolzano, contained in his *Purely Analytic Proof*, that is "the original form of the Bolzano-Weierstrass theorem" (Russ, 1980: 157; cf. Russ, 2004: 146) or, in other words, "[a] theorem like [the Bolzano-Weierstrass theorem] first proved by Bolzano" (Stillwell, 2010A: 560). Furthermore, for some of those authors that lemma of Bolzano not only "asserted M [as] a property of real numbers" (Edwards, 1979: 308) or "established the existence of a least upper bound for a bounded set of real numbers" (Kline, 1972: 953), but along with its proof it shows that Bolzano actually "proved" the Bolzano-Weierstrass theorem, even though "it was the work of Weierstrass that made it familiar to mathematicians" (Boyer, 1968: 605). And yet, was it really so?

Firstly, it must be noticed that the modern form of what is known as the Bolzano-Weierstrass theorem states that every bounded infinite set of real numbers has a limit point. Which is a pretty similar statement to that of Schoenflies of 1898: "For a set [*Menge*] P consisting of infinitely [*unbegrenzt*] many points, there is at least an accumulation point (limit point, condensation point) according to a theorem of Bolzano-Weierstrass" (Schoenflies, 1898: 185; cf. Moore, 2000: 178).[214] Which in turn is practically the same statement introduced by Georg Cantor (student of Karl Weierstrass, Leopold Kronecker and Eduard Kummer) in his 1872 work on trigonometric series (the one in which he presented his construction of the real numbers), who, when defining the concept of a limit point of a point-set (*Grenzpunkt einer Punktmenge*), wrote without naming Bolzano or Weierstrass: "a point-set consisting of an infinite number of points always has at least one limit point" (Cantor, 1872/1932: 98; cf. Moore, 2000: 176).[215]

It is beyond doubt that for Cantor and his contemporaries it was Weierstrass who initially developed that theorem, which can be found: a) in the text of his course on analytic functions

[213] This is the exact translation in (Russ, 2004: 269). Bolzano wrote: "Wenn eine Eigenschaft M nicht allen Werthen einer veränderlichen Grösse x, wohl aber allen, die kleiner sind, als ein gewisser u, zukömmt: so gibt es allemahl eine Grösse U, welche die grösste derjenigen ist, von denen behauptet werden kann, dass alle kleineren x die Eigenschaft M besitzen."

[214] Schoenflies wrote: ""Für eine aus unbegrenzt vielen Punkten bestehende Menge *P* giebt es nach einem Satz von Bolzano-K. Weierstrass mindestens eine Häufungsstelle (Grenzpunkt, Verdichtungspunkt)." Here "unbegrenzt", literally "without limit" or "without bound", has been translated by "infinite", which might be problematic despite "infinite" seems to account for the quantitative sense of the expression "unbegrenzt vielen Punkten" in a better way than "unlimited" or "unbounded" does. Schoenflies does use the expression "unendlich vielen Punkten" in his text referring to Cantor's work, and still he could be cosidering both expressions equivalent, which would sustain the use of "infinite". But "infinite" could betray the qualitative sense of Schoenflies' expression in case that, as for example Cantor and Weierstrass did sometimes, he was not requiring that the set was bounded, being the "Grenzpunkt" at infinity. Nevertheless, given that this detail, perhaps important, is not relevant for the objectives of this work, and in particular of this section, it is to be hoped that its mention in this footnote will suffice.

[215] Cantor wrote: "Darnach ist es leicht zu beweisen, dass eine aus einer unendlichen Anzahl von Punkten bestehende Punktmenge stets zum wenigsten einen Grenzpunkt hat."

held during the summer semester of 1886, in terms of a "limit place" (*Grenzstelle*) of a "manifold or variety" (*Mannigfaltigkeit*) that did not belong to this one (cf. Weierstrass, 1886/1988: 60);[216] b) in his text on the theory of single-valued analytic functions of 1876, in terms of an "essential singular place" (*wesentliche singuläre Stelle*) of a single-valued function which could be in the interior or on the limit (*Grenze*) of a "bounded realm" (*grenzten Bereichs*) under consideration (cf. Weierstrass, 1876/1895: 80); c) in the notes of his course on analytic functions held during the summer semester of 1874, in terms of a "place" (*Stelle*) in a "region [domain] of a real quantity" (*Gebiet einer reellen Grösse*) (cf. Weierstrass, 1874: 305);[217] d) in the notes of his course on analytic functions held during the summer semester of 1868, in terms of a "point" (*Punkt*) in a "limited realm" (*begrenzten Bereiche*) (cf. Weierstrass, 1868/1986: 79 & 77);[218] and e) in the notes of his course on analytic functions held in 1865-66, in terms of a "point" (*Punkt*) in a "bounded part of a plane" (*begrenzten Theileb der Eben*) which could be inside that part or on its boundary (*grenz*) (cf. Weierstrass, 1865/66: 16[B]).[219]

Even more, it is known because of a footnote that Cantor included in his second 1870 work on trigonometric series, but also due to the correspondence between Hermann Schwarz and both Cantor and Eduard Heine prior to the publication of that work, that by 1870 and among Weierstrass students: firstly, a "Weierstrass-Bolzano theorem" was identified with "upper and lower limits" (Meschkowski and Nilson, 1991: 24)[220] of what today would be called "a closed interval" of real values (Zermelo, 1932: 82);[221] and, secondly, it was recognized that this theorem had been developed by Weierstrass on the basis of a principle related in some way to Bolzano's first work of 1817. Testimony of the latter can be found, for example, both in the

[216] The term "Mannigfaltigkeit" can also be found in the notes of his winter course of 1877-78 taken by Salvatore Pincherle (who used the term "varietà" (cf. Pincherle, 1880: 60)), the notes of his course of 1878 taken by Adolf Hurwitz (cf. Weierstrass, 1878/1988) and, for example, in Cantor's series of works of 1879-1884, the one to which his *Grundlagen* belong. The term "Grenzstelle", on the contrary, is rarely found throughout his work, though it is not as exceptional as the term "Grenzpunkt". It is true that, apparently, Weierstrass never used the term "Grenzpunkt" when introducing any of his forms of what nowadays we call "the Bolzano-Weierstrass theorem", but it is not true that "[he] never used Cantor's term" (Moore, 2008: 222): it can be found, for example, in the notes by Adolf Hurwitz of 1878 (cf. Weierstrass, 1878/1988: 143) and in the text of his course of 1886 (cf. Weierstrass, 1886/1988: 72). As for the term "Grenzstelle", a certain use of it can be found in those last notes by Hurwitz (cf. Weierstrass, 1878/1988: 90, 92 & 144) and his work of 1876 (cf. Weierstrass, 1876/1895: 78-79 & 81).

[217] In these lecture notes by Georg Hettner also appears the term "Mannigfaltigkeit" (cf. Weierstrass, 1874: 313, 315, 316, 319).

[218] I am grateful to the Bibliothek des Mathematischen Instituts of the Universität Münster, in particular to Gaby Weckermann and especially to Martin Paul, for scaning the printed version of 1986 of Weierstrass text and sending me the file.

[219] I am grateful to the Universitätsbibliothek of Giessen, in particular to Olaf Schneider, for the digitisation of this notes taken by Moritz Pasch at Berlin, available in its institutional repository.

[220] In a letter to Schwarz dated March 30, 1870, Cantor wrote "dem Weierstrass-Bolzanoschen Satze von der unteren und oberen Gränze."

[221] In the footnote above mentioned, Cantor wrote in reference to a proof of Schwarz: "Dieser Beweis stützt im wesentlichen auf den Vorlesungen des Herrn Weierstrass häufig vorkommenden und bewiesenen Satz: 'Eine in einem Intervalle $(a \ldots b)$ (die Grenzen inkl.) der reellen Veränderlichen x gegebene, stetige Funktion $\varphi(x)$ *erreicht* das Maximum g der Werte, welche sie annehmen kann, zum mindesten für einen Wert x_0 der Veränderlichen, so dass $\varphi(x_0) = g$'." Cf. (Schwarz, 1890: 342).

notes of Weierstrass' summer course of 1874 by Georg Hettner, who at the bottom of the page added a reference to that work of Bolzano (cf. Weierstrass, 1874: 304), and in two letters: one from Heine to Schwarz dated March 8, 1870, in which the former refers to the latter's use of "the Bolzano-Weierstrass principle" (Dauben, 1990: 308);[222] and another one from Schwarz to Cantor dated April 1, 1870, in which the former refers to Weierstrass' developments on the basis of "Bolzano's principles" (Meschkowski, 1967: 228; cf. Meschkowski and Nilson, 1991: 24).[223]

However, as all this makes clear and as Cantor years later emphasized (cf. Cantor, 1884/1932: 212),[224] it was Bolzano's procedure of repeated bisection of the totality of values of x between two terms to obtain the least greatest value U, a procedure highly valued by Weierstrass, what later would be turn into the nowadays commonly known Bolzano-Weierstrass principle: "given a sequence of closed intervals, embedded on each other, there must be at least one point that belongs to every interval" (Ferreirós, 2007: 141). A principle, it must be said, that indeed, as Cantor wrote, was "hardly replaceable" and offered a satisfactory characterization of the continuity of the real number system, alternative to the cut property proposed by Dedekind in 1872.

Leaving aside Cantor's remark on that principle as a "very old" one whose authorship, he claimed, should not be attributed to Bolzano and Weierstrass, the fact is that even if considered the early versions of the latter of the theorem that bears the name of them two, Bolzano's 1817 theorem can not be considered equivalent to this one, not to say as an early version of it (cf. Moore, 2000). It is true that the early forms of Weierstrass' theorem (i.e. those of the 1860s), as Bolzano's theorem, do not include any explicit mention of "set", "real numbers" or "limit point", and that both use what at first glance may seem similar proof procedures. But, on the one hand, concerning the terminological issue, it should not be overlooked that in the 1860s, just as set

[222] Heine wrote: "Dagegen leugne ich nicht, dass Ihr Beweis des Hülfssatzes nach Bolzano-Weierstr. Principien, wie schön er auch ist, mir *nicht* völlig beweisend erscheint, u[nd] ich deshalb nicht zugeben kann, dass der Satz erledigt sei."

[223] Schwarz wrote: "Auch ich bekenne mich mit Dir zu der von Herrn Weierstrass in seinen Vorlesungen verfochtenen Meinung, dass man ohne die Schlussweise, welche von Herrn W. auf Bolzanoschen Principien weiter ausgebildet ist, bei vielen Untersuchungen nicht zum Ziel gelangen könne."

[224] Cantor wrote: "Ich bemerke, dass die hier angewandte Beweismethode, welche wohl schwerlich durch eine wesentlich andere ersetzt werden kann, ihrem Kerne nach sehr alt ist; in neuerer Zeit findet man sie unter anderem in gewissen zahlentheoretischen Untersuchungen bei Lagrange, Legendre und Dirichlet, in Cauchy's *Cours d'analyse* (Note troisième) und in einigen Abhandlungen von Weierstrass und Bolzano; es scheint mir daher nicht richtig, sie vorzugsweise oder ausschliesslich auf Bolzano zurückzuführen, wie solches in neuerer Zeit beliebt worden ist."

theory was just about to begin to be developed, there was no consensus on the most appropriate term for "set", for which not only the German term *Mannigfaltigkeit* was used (as Weierstrass did in 1886), but also, for example, *Menge* (cf. Cantor, 1872/1932: 97ff.) and *System* (cf. Dedekind, 1888: 2ff.).

As a matter of fact, advocates of traditional reading take for granted that, first, the terms initially used by Weierstrass in his theorem involve a conception of an interval of points as a set of real numbers and that, second, Bolzano's theorem was "at worst" an early version of the former and therefore asserted a property of real numbers at least similar to that stated by the one of Weierstrass. Which means that, according to the more common claim among those authors, what we have been taught as "Bolzano-Weierstrass theorem" deserves that name since: a) Bolzano's quantity U (the [least] greatest quantity for which all the values of a variable quantity x smaller than it posses a property M) corresponds to Weierstrass' "[limit] point" or "limit place" p; b) Bolzano's totality of values of "variable quantity x" between a certain value u (call it F) and another value $u + D$ (being D a positive quantity; call it G) corresponds to what Weierstrass called a "bounded part of plane", a "limited or bounded realm" or a "region of a real quantity", which in turn means that Bolzano referred to an interval of modern real numbers; and c) Bolzano's proof procedure of repeated bisection (of the totality of values of x between two terms to obtain a least greatest value U) corresponds to Weierstrass' proof procedure of subdivision of an interval (which he would later call "variety", i.e. 'set') of points.

In order to sustain such an argument, two assumptions must be made. The first one is that there is no essential difference between Bolzano's ω, used in his proof of that theorem, and the Weierstrassian ε, something that is a huge assumption, as said before and as will still be discussed later. The second assumption has to do with the identification of Weierstrass of "an unbounded variable magnitude, which [formed] a simple manifold", with its geometrical representation by a straight line (Weierstrass, 1886: 60);[225] an entirely valid procedure, given his satisfactory theory of real numbers, with which Bolzano would have dissented in 1817. Indeed, one of the few places throughout his 1816 work on the binomial theorem and his *Purely Analytic Proof* where he used the term "point" as identified with, traditional reading would say, the value

[225] Weierstrass wrote: "Ist x eine unbeschränkt veränderliche Grösse, die – wie man sagt – eine einfache Mannigfaltigkeit bildet und geometrisch durch eine Gerade rapräsentiert wird." Both Cantor and Dedekind stressed their right to do so in their works of 1872 (cf. Cantor, 1872/1932: 97; Dedekind, 1872: 18-19).

of a Weierstrassian real quantity,[226] was in the preface of that latter work. There, he regarded the type of proof of the intermediate value theorem that depended on geometric continuity as an invalid mathematical procedure due to the subordination of geometry to analysis (cf. Bolzano, 1817B: 6).[227] But, even if it is assumed without granting that Bolzano wanted to talk about our real numbers, there is no notion of "[limit] point" or "limit place" of a set in his theorem, as there is no repeated subdivision of an interval conceived as a set of points (nor notion of a subinterval with infinitely many points as members) in Bolzano's proof, as Moore pointed out (cf. Moore, 2000: 172-175).

Curiously, supporters of the traditional interpretation also claim, albeit from another dais, that in another sense Bolzano's procedure of repeated bisection is evidence of the modernity of his early mathematical proposal. "For Bolzano as for Cauchy," Judith V. Grabiner wrote, "the algebra of inequalities played an important role in proofs" (Grabiner, 1981: 10). Precisely, according to this argument, the outstanding nature of the analytical proposal of those authors is linked to some extent to the relevance given by both to that procedure, as shown by their convergence criteria:

> If a series of quantities $F^1x, F^2x, F^3x, \dots, F^nx, \dots, F^{n+r}x, \dots$ has the property that the difference between its *n*th term F^nx and every later one $F^{n+r}x$, no matter how far this is from that one, remains smaller than any given quantity if one has assumed *n* large enough, then there is every time a certain constant quantity, and indeed only one, which the terms of this series always approach more and to which they can come as close as one simply wants if one continues the series far enough. (Bolzano, 1817B: 35)[228]

> [For] series (1) [$u_0, u_1, u_2, \dots, u_n, u_{n+1}, \&c. \dots$] to be convergent, it is first of all necessary that the general term u_n decrease indefinitely as n increases. But this condition does not suffice, and it is also necessary that, for increasing values of n, the different sums, $u_n + u_{n+1}, u_n + u_{n+1} + u_{n+2}, \&c. \dots$., that is to say, the sums of as many of the quantities $u_n, u_{n+1}, u_{n+2}, \&c. \dots$, as we may wish, beginning with the first one, eventually

[226] Probably the only other place in those works where such identification can be found is in the §13 of his *Purely Analytic Proof*, when he exemplified his so-called "least upper bound" theorem by means of a rectangular hyperbola (cf. Bolzano, 1817B: 49-50).
[227] Bolzano wrote: "Eine jede continuirlicheLinie von einfacher Krümmung, deren Ordinaten erst positiv, dann negativ sind (oder umgekehrt), die Abscissenlinie nothwendig irgendwo in einem Puncte, der zwischen jenen Ordinaten liegt, durch schneiden müsse."
[228] Bolzano wrote: "Wenn eine Reihe von Grössen $F^1x, F^2x, F^3x, \dots, F^nx, \dots, F^{n+r}x, \dots$, von der Beschaffenheit ist, dass der Unterschied zwischen ihrem *n*ten Gliede F^nx und jedem späteren $F^{n+r}x$, sey dieses von jenem auch noch so weit entfernt, kleiner als jede gegebene Grösse verbleibt, wenn man *n* gross genug angenommen hat: so gibt es jedesmahl eine gewisse bestandige Grösse, und zwar nur eine, der sich die Glieder dieser Reihe immer mehr nähern, und der sie so nahe kommen können, als man nur will, wenn man die Reihe weit genug fortsetzt." Bolzano actually places the superscript above the corresponding term. Henceforth, whenever this is not the kind of superscript employed by Bolzano, it will be indicated.

constantly assume numerical values less than any assignable limit. Conversely, whenever these various conditions are fulfilled, the convergence of the series is guaranteed. (Cauchy, 1821: 125-126)[229]

So, even though Cauchy introduced his criterion without any attempt to prove it, it is normally quoted as evidence of his "inequality-based limit concept" the proof that he offered of the theorem known as the Root Test: supposed that $k < 1$ for which $k < U < 1$, and given the inequality $(u_n)^{\frac{1}{n}} < U$ or $u_n < U^n$, "the terms of the series $u_0, u_1, u_2, \dots, u_{n+1}, u_{n+2}, \dots$ are eventually always smaller than the corresponding terms of the geometric progression $1, U, U^2, \dots, U^n, U^{n+1}, U^{n+2}, \dots$", being the progression convergent (since $U < 1$) and concluding "*a fortiori* the convergence of series" $u_0, u_1, u_2, \dots, u_n, \dots$ with all terms positive (Cauchy, 1821: 132-133).[230]

By contrast, with regard to Bolzano's criterion, first, it is usually recognized as a clear anticipation of "Cauchy's criterion". That way: a) what Bolzano called a "certain constant quantity" is interpreted as what Cauchy calls an "assignable limit"; b) what the former denoted by $F^n x, F^{n+1}x, F^{n+2}x, \dots, F^{n+r}x$ ("the sums of the first $n, n+1, n+2, \dots, n+r$ terms" of a series, given a continuous variation) is identified with the sums referred by the latter; and c) Bolzano's ω are considered equivalent to Cauchy's α, with $f(x+\omega)$ equal to $f(x+\alpha)$.[231] But, secondly, Bolzano's proof of his criterion is frequently criticized because, it is said, he "[sought] to justify it by a reasoning which, in the absence of any arithmetic definition of real number, was and could only be a vicious circle" (Bourbaki, 1971/2007: TG IV.72; cf. Steele, 1950: 29-30; Flett, 1980: 57).[232] In other words, it is commonly pointed out that the defect of his proof is that "[he] was forced to introduce a fresh assumption the existence of a quantity X to which the terms of

[229] Cauchy wrote: "Donc, pour que la série (1) soit convergente, il est d'abord nécessaire que le terme général u_n décroisse indéfiniment, tandis que n augmente; mais cette condition ne suffit pas, et il faut encore que, pour des valeurs croissantes de n, les différentes sommes $u_n + u_{n+1}, u_n + u_{n+1} + u_{n+2}, \&c. \dots$, c'est-à-dire, les sommes es quantités $u_n, u_{n+1}, u_{n+2}, \&c. \dots$ prises, à partir de la première, en tel nombre que l'on voudra, finissent par obtenir constamment des valeurs numériques inférieures à toute limite assignable. Réciproquement, lorsque ces diverses conditions sont remplies, la convergence de la série est assurée." The translation is from Robert E. Bradley and C. Edward Sandifer (Bradley and Sandifer, 2000: 86-87).

[230] Cauchy wrote: "Par suite, il sera possible d'attribuer au nombre entier n une valeur assez considérable, pour que, n obtenant cette même valeur ou une valeur plus grande encore, on ait constamment $(u_n)^{\frac{1}{n}} < U, u_n < U^n$. Il en résulte que les termes de la série $u_0, u_1, u_2, \dots, u_{n+1}, u_{n+2}, \&c. \dots$ finiront par être toujours inférieurs aux termes correspondans de la progression géométrique $1, U, U^2, \dots, U^n, U^{n+1}, U^{n+2}, \&c. \dots$; et, comme cette progression est convergente (à cause de $U < 1$), on peut de la remarque précédente conclure *à fortiori* la convergence de la série (1)." The translation is from Robert E. Bradley and C. Edward Sandifer (Bradley and Sandifer, 2000: 91).

[231] Hourya Benis Sinaceur, for example, wrote: "Bolzano n'explique pas Cauchy, mais il peut certainement être considéré comme un précurseur de Weierstrass, même si c'est, en fait, sous l'impulsion imprimée par Cauchy et à partir de l'héritage qu'il a laissé que Weierstrass a développé sa propre manière de faire des mathématiques" (cf. Sinaceur, 1973: 111-112). For an account of Bolzano's ω as pretty similar quantities to Cauchy's α, cf. (Rusnock and Kerr-Lawson, 2005: 306, fn. 7).

[232] Nicolas Bourbaki wrote: "Énonçant clairement (avant Cauchy) le < critère de Cauchy >, il cherche à le justifier par un raisonnement qui, en l'absence de toute définition arithmétique du nombre réel, n'était et ne pouvait être qu'un cercle vicieux."

the series approach as close as we please [...], but it was precisely what he was trying to prove in the first place" (Stedall, 2008: 496).

Regardless that, it is true that Bolzano used inequalities in his proof of his convergence criterion, as well as throughout his three works of 1816-1817. Even more, they play an essential role both in that proof and in the one of his theorem of the [least] greatest bound value. In the first case, when he showed that the assumption of the constant "real quantity" (*reelle Grösse*) to which the terms of the aforementioned [sequence of] partial sums could converge, "contain[ed] nothing impossible" and could be determined as accurately as one would like, he wrote:

> But the difference $F^n x - F^{n+r} x$ always remains $< \pm d$ [a given quantity], however large r is taken. Therefore, the difference $X - F^n x = (X - F^{n+r} x) - (F^n x - F^{n+r} x)$ must also always remain $< \pm(d + \omega)$. But since for the same n this is a constant quantity, while ω can be made as small as we please by increasing r, then $X - F^n x$ must be $=$ or $< \pm d$. For if it were greater and $= \pm(d + e)$, for example, it would be impossible for the relation $d + e < d + \omega$, i.e. $e < \omega$, to hold if ω is reduced further. (Bolzano, 1817B: 36-37)[233]

In the second case, Bolzano's repeated bisection of the totality of values of x between F and G, to obtain U, precisely rested on inequalities: given a value u of the variable quantity x for which property M applies to all values smaller than it, and considered a quantity $V = u + D$ (being D positive) for which that property does not apply to all values $< V$, he asked whether M applied to all values of $x < u + \frac{D}{2^m}$ (with exponent 0 or positive integer) or not and, if so, if it applied to all values of $x < \frac{D}{2^m} + \frac{D}{2^{m+n}}$ (with exponent 0 or positive integer) and so on, until either found a value of x of the form $u + \frac{D}{2^m} + \frac{D}{2^{m+n}} + \cdots + \frac{D}{2^{m+n+\cdots+r}}$ (call it A), for which the property M applied for all values of x smaller than it, or found M applied for all values of x smaller than the value of the latter form but not to all $< u + \frac{D}{2^m} + \frac{D}{2^{m+n}} + \cdots + \frac{D}{2^{m+n+\cdots+r-1}}$ (call it form B) (cf. Bolzano, 1817B: 41-46). After which, in the second case scenario, Bolzano made the following remark: since the quantity of "form A" had the property that the change in value always remained smaller than a certain quantity (§5), such a quantity could be allowed to be U, the

[233] Bolzano wrote: "Der Unterschied $F^n x - F^{n+r} x$ bleibt aber jederzeit, so gross man auch r nehme, $< \pm d$. Also muss auch der Unterschied $X - F^n x = (X - F^{n+r} x) - (F^n x - F^{n+r} x)$ jederzeit $< \pm(d + \omega)$ verbleiben. Da aber derselbe bey einerley n eine beständige Grösse ist, ω dagegen durch die Vergrösserung von r so klein gemacht werden kann, als man nur immer will: so muss $X - F^n x =$ oder $< \pm d$ seyn. Denn wäre es grösser und z. B. $= \pm(d + e)$; so könnte unmöglich das Verhältniss $d + e < d + \omega$, d.h. $e < \omega$ bestehen, wenn man ω immer mehr verkleinert. Der wahre Werth von X ist also von dem Werthe, den das Glied $F^n x$ hat, höchstens um d verschieden; und lässt sich daher, da man d nach Belieben klein annehmen kann, so genau, als man nur immer will, bestimmen." This is the exact translation in (Russ, 2004: 267).

quantity for which property M applied for all $x < U$, as he showed through inequalities involving ω (Bolzano, 1817B: 46-48).

Once again, the most widespread opinion among the proponents of the traditional reading is that, in spite of the absence of a rigorous definition of real numbers and the flaws that it caused, Bolzano's early mathematical work unequivocally shows that he –along with Cauchy– was the "precursor of those who rigorised the calculus by means of a sophisticated limit concept and an emphasis on the arithmetic character of the key definitions" (Gray, 2015: 240; this is Gray's summary of that tendency). Furthermore, for some of those authors it shows that he was indeed a precursor of "[t]he project of putting the theory of the real line on a solid, arithmetical foundation" (Ewald, 1999: 226). Because, they stress, it is not only that he demanded pure (= *geometric-free*) analytical procedures and that he had a certain insight on modern real numbers, but also that as a result he provided some notions and procedures that anticipated some later arithmetizing ones.

And yet, the question persists whether, remaining faithful to Bolzano's notions of quantity and number, as well as to his conception of mathematics, his early mathematical work can be interpreted as proto-Weierstrassian or pre-Weierstrassian; a denomination, the latter, under which several Germanic mathematicians from the first half of the 19th century could be mentioned (e.g. Gauss, Ohm and Kronecker). In other words, the key question is whether to attribute to Bolzano the role of a mathematician that anticipated the arithmetizing project of Weierstrass and others, as well as some of his procedures and notions, or if doing so misinterprets Bolzano's work by 1817 or, even worse, contravenes his early mathematical proposal.

That said, in order to finally elucidate those core notions within Bolzano's analytical framework, and once some important features of the main results contained in his *Purely Analytic Proof* have been discussed, attention should be focused, at an 'external' level, on the connection between his *Purely Analytic Proof* and his other two works of 1816-1817, as well as between those three and the ones of 1810 and 1804. While, at an 'internal' level, attention should be paid to some crucial details of his early mathematical practices, such as: a) his distinction between quantities and numbers; b) his identification between 'real' (*reellen*) and 'actual' (*wirklichen*) quantities; c) his use of irrational quantities; d) his work with series; and e) his introduction of multitudes of infinite elements.

To begin with, it is worth remembering the difference that Bolzano established between mathematics and philosophy, for him the two main parts of human a priori knowledge: the former dealt "with the general conditions under which the existence of things becomes possible", while the latter sought to "prove a priori the reality [or 'actuality'] of certain objects" (Bolzano, 1810: 13).[234] As a consequence, "practical mathematics" dealt with "how and in what way I produce in reality an object analogous to [a] concept" (cf. Bolzano, 1810: 131-132),[235] geometry and chronometry dealt "indirectly [since they study the properties of space and time "*in abstracto*"] with the conditions to which things must conform in their existence" (Bolzano, 1810: 23)[236] and all of them were subordinated to what he called "general *mathesis*", which dealt with the laws or forms to which all things (*alle Dinge*) must conform in their existence (Bolzano, 1810: 16-17).

Mathematics for Bolzano, therefore, dealt with things that existed not only "independent of our consciousness" (*von unserem Bewutztseyn unabhängiges*), but also "in our imagination" (*in unsrer Vorstellung*); or, as he summed it up, mathematics dealt with "everything that can in general become an object of our capacity for representation" (Bolzano, 1810: 11-12).[237] However, in order to mathematically study things, quantities still played a crucial role for him. Hence his remark, in his work of 1810, that the concept of quantity (i.e. a concept of general *mathesis*) could be applied "to all objects, even to objects of thought" (*auf alle Gegenstände, selbst auf Gedankendinge*) (Bolzano, 1810: 6). That way, for example, just as the length of a wooden beam (empirical object) could be quantified geometrically if considered as a –finite– straight line (an object of thought), variable things that existed in space and time (such as, to quote some prevailing examples in the Germanic context, "gravity, force directed upwards;

[234] Bolzano wrote: "wären einander nach dieser Erklärung dergestalt entgegen gesetzt, dass erstere die allgemeinen Bedingungen abhandelte, unter welchen das Daseyn der Dinge möglich wird; die letztere dagegen versuchte, die Wirklichkeit gewisser Gegenstände [...] a priori zu beweisen."

[235] Bolzano wrote: "Wie und auf welche Weise ich ein Gegenstand analog diesem Begriffe in der Wirklichkeit darstellen lasse, gehört in die präktische Mathematik."

[236] Bolzano wrote: "Entwickeln wir also die Eigenschaften der Zeit und des Raumes *in abstracto*, und ordnen sie wissenschaftlich; so werden auch diese Wissenschaften zur Mathematik gezählet werden müssen, indem auch sie, obgleich nur mittelbarer Weise, von den Bedingungen handeln, nach welchen sich die Dinge in ihrem Daseyn richten müssen."

[237] Bolzano wrote: "alles, was überhaupt ein Gegenstand unsers Vorstellungsvermögens werden kann." In this case it is opt for Russ' translation of "Vorstellungsvermögens" as "capacity for representation" and not as "capacity for imagination" since it seems entirely more appropriate than this latter more literal translation (cf. Russ, 2004: 94).

[and] water flowing into vessel, water emanating [from it]" (Segner, 1758: 5))[238] could be quantified analytically.

A crucial trait here is that, for Bolzano, the "objects of thought" were, as redundant as it sounds, a sort of things and, thus, they were "real" in a certain way or, plainly, they were "something real" (*etwas Wirklichem*) (cf. Bolzano, 1810: 142). Lines, surfaces (cf. Bolzano, 1804: 47-48), analytical quantities, numbers and any other object of thought were therefore to be considered as real. That explains why in his *Purely Analytic Proof*, when he talked about a "real quantity" (*reellen Grösse*), he also referred to it as a quantity that "actually" existed: "for anyone who has a correct concept of quantity," he said, "the idea of an i which is the greatest of those of which it may be said that all smaller ones possess the property M, is the idea of a real, i.e. actual quantity" (Bolzano, 1817B: 23).[239] As a passage of his 1810 work suggests, real or actual quantities in a sense were opposed to what he called "symbolic expressions" (*symbolischer Ausdruck*):

> The best procedure might well be to count as higher mathesis only that in which the concept of an infinity (whether infinitely big or small), or of a differential, appears. At the present time this concept has not yet been sufficiently explained. If, in the future, it should be decided that the infinite or the differential are nothing but symbolic expressions just like $\sqrt{-1}$ and similar expressions, and if it also turns out that the method of proving truths using purely symbolic inventions is a method of proof which is indeed quite special, but is always correct and logically admissible, then I believe it would be most appropriate to continue to refer the concept of infinity, and any other equally symbolic concept, to the domain of higher mathematics. Elementary mathesis would then be that which accepts only real concepts or expressions in its exposition—higher mathesis that which also accepts purely symbolic ones. (Bolzano, 1810: 30-31)[240]

[238] Segner wrote: "Caeterum infinita sunt quantitatum genera, quarum altera refertur ad alteram, quemadmodum progressus refertur ad regressum, ascensus ad descensum, vel motus quicunque versus aliquam partem, ad motum versus partem oppositam. Tales sunt, possessiones, debita; accepta, expensa; gravitas, vis sursum directa; aqua in vas influens, aqua effluens, & aliae plurimae, quarum quaedam in sequentibus clare exponentur."

[239] Bolzano wrote: "für Jeden, der einen richtigen Begriff von Grösse hat, [...] der Gedanke eines i, welches das grösste derjenigen ist, von denen gesagt werden mag, dass alle unter ihm stehende die Eigenschaft M hesitzen, der Gedanke einer reellen d. h. wirklichen Grösse sey."

[240] Here the translation of Russ is slightly modified (cf. Russ, 2004: 100). Bolzano wrote: "Am besten dürfen wohl noch diejenigen verfahren, welche zur höheren Mathesis bestimmt nur das zählen, worin der Begriff eines Unendlichen (gleichviel ob eines unendlich Grossen oder Kleinen) oder der eines Differenzials vorkömmt. Nur ist dieser Begriff zur Stunde noch nicht hinlänglich aufgeklärt. Sollte es aber dereinst entschieden werden, dass das Unendliche, oder das Differenzial, nichts anders als ein symbolischer Ausdruck sey, gerade wie $\sqrt{-1}$, dgl.; und sollte es sich zugleich ergeben, dass die Methode, das Wahre durch bloss symbolische Erdichtungen zu beweisen, eine zwar ganz besondere, aber doch immer richtige und logisch zulässige Beweisart sey: dann, glaube ich, würde man am zweckmätzigsten verfahren, in das Gebiet der höheren Mathematik mit dem Begriffe des Unendlichen, auch jeden andern zu verweisen, der so wie er symbolisch ist. Gemeine Mathesis wäre dann jene, welche nur lauter reelle –höhere, welche auch bloss symbolische Begriffe oder Ausdrücke in ihren Vortrag aufnimmt."

On the one hand, therefore, it is worth noting that Bolzano not only rejected the mathematical knowledge of the so-called "infinitely small quantities", as stressed in his works of 1816-1817 (cf. Bolzano, 1816: IV-V; Bolzano, 1817B: 12; Bolzano, 1817A: VII-VIII), but also of the so-called "infinitely big" quantities (cf. Bolzano, 1817B: 16). This shows a consistency absent in the works of many of his Germanic predecessors and contemporaries. From his stance, both were erroneously called 'quantities' inasmuch as they could not be determined (i.e. finitely quantified). Consequently, while for many Germanic mathematicians of the second half of the 18th century one was entitled to use infinitely large quantities as $= \infty$, but not infinitely small, for Bolzano the latter was a "self-contradictory" (*selbst widersprechenden*) concept (cf. Bolzano, 1816: XI; Bolzano, 1817A: VIII) and the former stood for an undetermined value: a function $\frac{a}{b-x}$, he wrote, "does not have any determinate value [*keinen bestimmten Werth*] when $x = b$, but becomes what is called infinitely big" (Bolzano, 1817B: 15-16). Moreover, Bolzano openly criticized the introduction of infinitesimals by means of quantities that some authors "first used as divisors [but] in the end made to zero", a procedure that could "never be allowed, since it is quite possible to divide by every finite (i.e. actual) quantity, but never by a zero (i.e. by nothing)" (Bolzano, 1816: XI & XIV-XV; cf. Bolzano, 1817A: IX-X).[241]

Additionally, it should not be overlooked that, while Bolzano said that maybe in the future those infinitely small and big "quantities" could be decided to be "symbolic expressions" (or "tools", in terms alien to Bolzano), he listed among these ones what we call today "imaginary numbers", as for example Cauchy and Jandera would later do (cf. Bolzano, 1810: 30; Cauchy, 1821: iij-iv & 173ff.; Jandera, 1830: XXIX). In fact, such an understanding of imaginary expressions can be found not only at the beginning of the preface to that work, where he said that the chapter on irrational and imaginary quantities" was an ambiguous one (cf. Bolzano, 1810: V),[242] but also in his work on the binomial theorem. In that work, Bolzano introduced "imaginary expressions" a couple of times, namely, $(1+x)^{\frac{2p+1}{2q}}$ for $x > -1^m$ (Bolzano, 1816: 47) and $\frac{(-y)^{\omega}-1}{\omega}$, with ω of the form $\frac{2n+1}{m}$ and m even (Bolzano, 1816: 114). But above all, in that work he contrasted a real with an imaginary value (cf. Bolzano, 1816: 130) and, in the final note, he explained the reasons

[241] Bolzano wrote: "Sie lassen Grössen, welche sie erst als Divisoren gebraucht, am Ende Null bedeuten; welches nach meiner Meinung niemahls erlaubt seyn kann, indem es wohl möglich ist, mit jeder endlichen (d. i. wirklichen) Grösse, nie aber mit einer Null (d. i. mit Nichts) zu dividiren."

[242] Bolzano wrote: "Noch schwankender, und zum Theile mit wechselseitigen Widersprüchen erfüllt, ist das Capitel von den irrationalen und imaginären Grössen." Steve Russ translated here "Grössen" for "numbers" (Russ, 2004: 87), a modification that according to this thesis contravenes Bolzano's early mathematical proposal.

for his deliberate avoidance of the cases of series corresponding to certain expressions that involved concepts that, he said, should first be "clearly developed if one want[ed] to decide thoroughly" on those questions (*deutlich entwickelt haben, wenn man über obige Fragen gründlich entscheiden will*), such as: "imaginary expressions" (*imaginärer Ausdrücke*), "the irrationality of a quantity" (*der Irrationalität einer Grösse*), "the mathematical opposites" (*mathematischen Gegensatzes*) and "potentiation [raising to a power] of a quantity" (*der Potenzierung einer Grösse*) (Bolzano, 1816: 143-144).

Beyond the status of today's imaginary numbers within Bolzano's framework (strictly speaking were not even quantities), the inclusion of negative and irrational quantities among the unclear concepts, though might not be striking if considered his mathematical context, requires further clarification. With regard to negative quantities, his frequent use of the expression "natural numbers" (*natürlichen Zahlen*) in his 1816 work (cf. Bolzano, 1816: 41, 43, 85 & 96) implied a certain assumption of positive whole numbers as the most natural multitude of numbers and, as a consequence, a certain recognition of the opposite and –not so natural– multitude of negative numbers. However, it should be noticed that, while he did work with 'negative numbers' and he even used their corresponding German expression (cf. Bolzano, 1816: 68-69 & 76), he usually referred either to "negative quantities" or to quantities to which a negative whole numerical value was assigned (negativen ganz zähligen Grösse) (cf. Bolzano, 1816: 26 & 75).

Concerning irrational quantities, by contrast, practically nowhere in his early mathematical works is to be found the expression 'irrational number' (*irrationalen Zahl*). He only used that expression in §73 of his 1816 work, when he posed a problem in terms of "whole, fractional or even irrational number", although he immediately, in the solution, referred once again to "irrational quantities" (*irrationaler Grössen*, *Irrationalgrösse*) (Bolzano, 1816: 137). Because of that, it seems appropriate to interpret that sole reference to 'irrational number' as meaning "whole number, [and] also a fractional [or] irrational [...] quantity" (Bolzano, 1816: 2 & 60), a sort of formula that he repeats throughout his work. For Bolzano, it should be clear, irrationals were not numbers and, as that quotation suggests, neither were the fractions, at least in the strict sense of that concept, even though these latter could be determined numerically. An idea, the former, which was not strange at the time and that still lasted a few more decades: it can be found explicitly in Kant, in a letter of 1790 (cf. van Atten, 2012); Klügel (1792: 31; 1805: 94);

Thibaut (1809B: 84); Hermann Hankel, a former student of Weierstrass (1867: 59); and even in Charles Méray, who published an arithmetic theory of irrationals in 1869 (1869: 284).

Furthermore, Bolzano's mathematical practices reveal that he regarded the irrationality of a quantity as a concept that still required to be developed if one aimed to introduce it within analytical framework. As a consequence, in his early works irrational quantities are always considered indeterminate, so that fractions are used as means to determine them. This is the case in his *Purely Analytic Proof* (§8): the indeterminate sequence $0.1, 0.11, 0.111, 0.1111, \ldots$ is said to be determined if considered the –determined– fraction $\frac{1}{9}$ to which the terms approached (Bolzano, 1817B: 38). And that is also the case in his 1816 work, where the few times that he deals with irrational quantities he states that, given such a quantity, there is always a fraction which can come as close as desired to it (cf. Bolzano, 1816: 23, 76 & 137-138).

Bolzano therefore required that the objects of mathematical study (i.e. to be mathematically known) be determined, and thus determination rested on notions of 'quantity' and 'number' that, in a sense, were drawn more rigorously –but more modernly– than those of his predecessors. Strictly speaking, numbers were only the naturals, but quantities could be whole positive, fractions, negatives and irrationals. Whereas, as regards to the variable quantities, while their variability was not in question, to analytically study it not only one could not resort to geometric or kinetic notions and arguments (alien to analysis and in fact belonging to mathematical parts subordinated to it), but at the same time one could not rely on actually infinite quantities (i.e. non-determined), whose lack of determination precisely prevented their strict knowledge. For that reason, he only partially incorporated negative quantities within his analytical framework, he regarded imaginary as useful symbolic expressions and he placed irrationals at a sort of midpoint between those two concepts, not entirely banned.

Such an attempt to avoid the mathematical study of the infinite is precisely what underlies his work with determined segments of infinite series (cf. Russ, 2004: 144), his notion of a "multitude of infinite elements" and his definition of quantities ω. A reluctance towards the presence of actual infinite in mathematics that, for example, indeed is linked to his rejection of the contradictory notion of a series of consequences without a first ground (*Das Widersprechende einer Reihe von Folgen ohne ersten Grund*) (Bolzano, 1810: 69) and with his designation of concepts such as "everything which is not A" as "indefinite or infinite" (*unbestimmte oder unendliche Begriffe*) (Bolzano, 1810: 84-85).

In the first place, with regard to Bolzano's finite treatment of infinite series, both his work on the binomial theorem and his *Purely Analytic Proof* illustrate this. In his 1816 work, for example, he criticized the binomial formula $\frac{1}{11} = (1+10)^{-1} = 1 - 10 + 100 - 1000 + \cdots in\ inf.$, "a claim that frighten[ed] the common sense!", instead of which he proposed starting from the finite binomial series $1 - x + x^2 - x^3 + x^4 - \cdots \pm x^r$, which "could only be equated in a certain sense [*bloss dann in einem gewissen Sinne gleich gesetz werden könne*] to the true value" of $(1+10)^{-1} = \frac{1}{1+10} = 1 - 10 + 10^2 - 10^3 + 10^4 - \cdots \pm \frac{10^r}{1+10}$, if $10^r - \frac{10^r}{1+10}$ "can become [*werden kann*] as small as one may want" (Bolzano, 1816: VIII-IX).[243] Something that, in a way, resembles what he said at the beginning of his *Purely Analytic Proof* about how the value of a series, whose terms could be arbitrarily increased, also depended on the last term considered for the representation of that variable quantity (*veränderliche Grösse*), as in $A + Bx + Cx^2 + \cdots + Rx^r = F^r x$, different from $A + Bx + Cx^2 + \cdots + Rx^r + \cdots + Sx^{r+s} = F^{(r+s)}x$ (cf. Bolzano, 1817B: 29-30).[244]

Coupled with such a finite mathematical treatment of the infinite, however, it is worth paying attention to what Bolzano actually says in doing so. For example, it does not seem fortuitous the phrase "real and positive" (*reellen und positiven*) that he constantly uses throughout his 1816 work to refer to the value of existing expressions, such as: a) $(1+x)^{\frac{1}{m}}$, to which corresponded the –finite– binomial series $1 + \frac{1}{m}x + \frac{1}{m}\frac{\frac{1}{m}-1}{2}x^2 + \cdots + \frac{1}{m}\frac{\frac{1}{m}-1}{2} \ldots \frac{\frac{1}{m}-r+1}{r}x^r$, with which he showed the binomial equation held for every positive fractional value of a exponent $\frac{1}{m}$ (cf. Bolzano, 1816: 69); b) $(1+x)^{\frac{n}{m}}$, to which corresponded a –finite– binomial series holding for any kind of positive fractional value of the exponent (Bolzano, 1816: 74); c) $(1+x)^{-\frac{n}{m}}$, to which

[243] As noted above, Bolzano went back to this idea of how a quantity could be completely determined "in a certain sense" in his *Purely Analytic Proof*. There he explained that the series $.1, .11, .111, .1111, \ldots$ approached to the quantity $\frac{1}{9}$ as close as one always wanted, which meant that, in a certain way (*einem gewissen Wege*), such an irrational quantity could be completely determined (*völlig bestimmbar*), taken into account a rational number (cf. Bolzano, 1817B: 38).

[244] Bolzano wrote: "The value of a series therefore also depends, in addition to the law which determines the formation of its individual terms, on their number" (Der Werth einer Reihe ist daher nebst dem Gesetze, welches die Bildung ihrer einzelnen Glieder bestimmt, auch noch von ihrer Anzahl abhängig)." Regarding the formula, as in other parts of this work, the superscript r for $F^r x$ goes above F in Bolzano's notation.

corresponded a –finite– binomial series holding for all fractional and negative values of the exponent (Bolzano, 1816: 75); d) $(1+x)^i$, to which corresponded a –finite– binomial series holding for every irrational value of the exponent (Bolzano, 1816: 76-77); e) $(a+b)^n$, to which corresponded a –finite– binomial series holding for every real (*reellen*) (by which he meant a positive integer or fractional) value of the exponent n (Bolzano, 1816: 77-79). Those kind of aspects should not be overlooked precisely because, in this case, while Bolzano also uses the phrase "real and negative" (*reell und negativ*) to refer to the value of existing expressions, such as $-(+a)^x$ (Bolzano, 1816: 128, cf. 79-81), what he never does is use the word 'real' either to refer to what nowadays are called 'real numbers' or to refer to an actual particular infinite quantity.

Put another way, it is not simply that nowhere in Bolzano's early mathematical works is found an expression equivalent to "real and infinite quantity". The point is that, rather than that, what is found in those works is, on the one hand, an explicit identification of 'real quantities' as 'finite' and, on the other hand, an explicit rejection of "infinite quantities" as "real" ones. In the first case, for example, he wrote in his work of 1816 that the series $1+1+\frac{1}{1\cdot 2}+\frac{1}{1\cdot 2\cdot 3}+\cdots+\frac{1}{1\cdot 2\cdot 3\cdot \ldots \cdot r}+\Omega$ "actually express[ed] a real finite quantity, inasmuch as its value not only always remains finite, but it even approaches a certain constant quantity as much as one wants if one continues it far enough" (Bolzano, 1816: 132);[245] that is, resuming what he said in the §7 of his *Purely Analytic Proof* (the one on his convergence criterion), inasmuch as such a series approached "a real quantity" (*eine reelle Grösse*) (Bolzano, 1817B: 37). An identification, even more, that could be related to his lack of criticism to an anonymous reviewer's notion of quantity as "the most general form, to be finite" (Bolzano, 1810: 10-11),[246] a notion presented by this latter when giving the definition of mathematics quoted by Bolzano as source of inspiration for his own.

[245] Bolzano wrote: "wirklich eine reelle endliche Grösse ausdrückt, indem ihr Werth nicht nur stets endlich bleibt, sondern sich auch so sehr, als man nur will, einer gewissen beständigen Grösse nähert, wenn man sie nur weit genug fortsetzt."

[246] The anonymous reviewer wrote: "Die Grösse ist nur darum Gegenstand der Mathematik, weil sie die allgemeinste Form ist, endlich zu seyn, die Mathematik aber ihrer Natur nach eine allgemeine Formenlehre ist; und zwar Arithmetik in so fern sie die Grösse als die allgemeine Form endlicher Dinge, Geometrie, in so fern sie den Raum als die allgemeine Form der Natur, Zeitlehre, in so fern sie die allgemeine Form der Kräfte, Bewegungslehre, in so fern sie die allgemeine Form der im Raume wirkenden Kräfte betrachtet, und alle diese Formen in ihren innern, weitern Beschränkungen, ausbildet" ([Anonymous], 1808, St. LXXXI: 1291).

Nonetheless, it should be clear that, just as Bolzano did not reject in general infinity as an object of knowledge, and so metaphysics considered God (*Gott*) as an “actual object” (*wirklicher Gegenstand*) (Bolzano, 1810: 21), he was not opposed in general to infinity in mathematics. Firstly, within the geometrical framework set in his 1804 work, he explicitly considered that a straight line contained an infinite number of points and he used infinite or “indeterminate” lines in his proofs (for theorems) (cf. Bolzano, 1804: 20 & 57). Secondly, within the geometric-analytical framework set in his second work of 1817, he explicitly considered that a surface and a spatial object contained infinitely many points and he used in his solutions (for problems) multitudes (*Menge*) of infinitely many values (Bolzano, 1817A: 6, 7, 29, 31-32, 55). Finally, within the analytical framework set in his 1816 and first 1817 works, he appealed somehow to infinite terms and values through quantities ω.

Indeed, his notion of multitudes of finite or infinite of elements, clearly explained in his second work of 1817, is crucial to understand the tension in Bolzano’s early mathematical proposal. “A quantity or even any object at all”, he wrote, even if constituted by “infinitely many quantities or objects”, is not “indeterminate” if given a law or a finite multitude of laws (*endliche Menge*) by means of which all of those are determined as a multitude (cf. Bolzano, 1817A: 6). On the one hand, in a certain way the importance of such finite multitudes of laws or rules is linked to the relevance given by him to the theory of combinations, since finite combinations were essential to attain mathematical knowledge. He emphasized this, for example, among his initial remarks on mathematical method, when talking about the necessity for an insight (*eingesehen*) on the –finite– combinations (*Zusammesetzung* or *Verbindung*) of words and the concepts that these denoted to produce (*hervor bringen*) both a new and real or actual (*wirklichen*) concept and to produce judgements (Bolzano, 1810: 53 & 71). But he also emphasized that in the initial paragraphs of his 1816 work, through which he explained the combinatorial procedures underlying the binomial series (Bolzano, 1816: 4-7); series that, as shown and explained before, were ‘finitely’ treated (cf. Russ, 2004: 144).

Bolzano’s placement of the theory of combinations at the core of mathematics was not, therefore, a mere blind faith in a theory that at that time was in vogue in several of the Germanic territories. For him, such a theory was essential to mathematics. Within his geometrical framework, for example, he insisted implicitly and explicitly, both in his 1804 and in his second 1817 work, on the fact that while lines, surfaces and solids were "infinite multitudes" of points determined by a law or a finite number of laws, he worked with them in "finite" terms or, to be more precise, in terms of determined or determinable objects: "A spatial object", he said, "is a determinate or determinable object if all points of it, by a finite multitude of rules, either are actually determined or are determinable from a certain number of given points" (Bolzano, 1817A: 27-28).[247] Thus, for example, the combination of a finite number of points, e.g. two (a and b), could determine by certain rules an infinite multitude of them, namely a straight line, and a determined straight line could be considered as *one* in-act determined object (namely an infinite multitude of points) which, combined with a finite number of points outside (*ausserhalb*) it, could form *an* object constituted by infinitely many points, such as a triangle. The point is that, even in the case of a line ay prolongated indeterminately (*Unbestimmte verlängerten*) in the direction y, or in the case of an unbounded (*Unbegränzte*) line xy (that is, prolongated indefinitely in both directions x and y), as he did in §39 of his 1804 work, for Bolzano mathematical knowledge implied working directly with finite numbers of points on those lines (that is, points that determined straight line segments) and their combinations with other finite number of points outside them, forming determined lines and, together, a determined surface (cf. Bolzano, 1804: 19-21 & 24-25; Bolzano, 1817A: 6, 28-29 & 33-34).

Moreover, such a notion of "multitude" allowed Bolzano to consider not only a finite multitude of functions $fx, \bar{f}x, \bar{\bar{f}}x$, but also an "infinite multitude" of them, $fx, \bar{f}x, \bar{\bar{f}}x, \ldots$, as he explained at the beginning of his 1817 geometric-analytical work (Bolzano, 1817A: 6-7, cf. 11). And yet, despite that notion and his definition of continuity, the fact is that, by 1817, Bolzano did not conceive a quantity as an actual multitude of infinite elements (much less a number as multitude of infinite elements). That would have entailed the acceptance of actual particular infinite quantities, irrationals among them, and ultimately would have gone against his core mathematical notions, his idea of a correct mathematical procedure and his conception of mathematics.

The way Bolzano defined quantities ω as those "that can become [*werden kann*] smaller than any given quantity", and Ω as the "algebraic sum or difference" (*algebraische Summe oder*

[247] Bolzano wrote: "Ein Raumding heisst ein bestimmtes, oder bestimmbares Ding, wenn sämmtliche Puncte desselben aus einer gewissen Anzahl gegebener Puncte durch eine endliche Menge von Regeln entweder wirklich bestimmt, oder doch bestimmbar sind."

Differenz) of a "finite multitude" ("(endliche) Menge") of quantities ω, i.e. $\omega \pm \omega^{(1)} \pm \omega^{(2)} \pm \cdots \pm \omega^{(m)}$ (Bolzano, 1816: 15),[248] coupled with the finite character of his procedures and notions, proves that those quantities were neither numbers nor quantities that tended to zero in a strictly dynamic way. For him, the distinction between his quantities ω and infinitesimals was not a mere linguistic issue but one could say that, indeed, it was semantic-ontological: variable quantities ω were quantities that should not be assumed as *being* smaller than any conceivable quantity (as the strictly dynamic conception of quantities required), but as *capable of becoming* smaller than any given quantity. In a way, therefore, with those quantities Bolzano probably aimed to finitely treat the potentially infinite variability of those quantities towards zero and, thus, avoid opening the window to the actual and straightforward infinite in mathematics, unaware of the fact that the door was already open.

Beyond the degree of correctness of this thesis, which hopefully honours Bolzano's mathematical beliefs and ideas, it is undeniable that his work, given its transitional character, demands that one try to understand it from inside, reading not only between lines but lines themselves, as Detlef Laugwitz and Hans Freudenthal once said about Cauchy's work (cf. Laugwitz, 1987; Freudenthal, 1971). Calling the early Bolzano a pre-Weierstrassian mathematician and not a proto-Weierstrassian is not merely a matter of elucidating what is the most suitable expression to refer to him. It is a matter of letting go the 20th century Bolzano and at least try to place ourselves on the 'neutral ground' of time to gradually better understand the person behind the inherited version, the myth. After all, as some few authors have suggested in various ways (cf. Sinaceur, 1973; Rusnock, 2000; Russ, 2004; Gray, 2015) and paraphrasing what Moore more vividly expressed regarding, like those other authors, specific features of Bolzano's *Purely Analytic Proof*, the definitive shift from a kinetic to a static approach in real analysis was not a minor event but, on the contrary, it "was a major Gestalt shift" (Moore, 2000: 174).

[248] In the original the superscript is placed on each ω.

Conclusions

The opening lines of Dedekind's work of 1872, *Continuity and Irrational Numbers* (*Stetigkeit und irrationale Zahlen*), the one in which he presented his definition of real numbers, are devoted to explain the origin of that work: in preparing his lessons on differential calculus at the beginning of his career as a teacher (1858), he wrote, he had to rely "on geometrical evidence" "in the notion of the approximation of a variable quantity to a fixed limit value", something didactically acceptable but lacking in "scientificity" (*Wissenschaftlichkeit*) (Dedekind, 1872: 9).[249] In addition to which, sixteen years later, in the preface to *What are the numbers and what are they for?* (*Was sind und was sollen die Zahlen?*), he pointed out that, as he had already shown in his aforementioned work, the "gradual extension of the concept of number" (*die shrittweise Erweiterung der Zahlbegriffes*) could be carried out without using "foreign ideas" (*fremdartiger Vorstellungen*), such as "measurable quantities" (*messbaren Grössen*) (Dedekind, 1888: X).

Dedekind's standpoint was that of a 19th century Germanic mathematician who proposed arithmetization of analysis but arrived there independently of Weierstrass, that is, without the influence of the author who traditionally is regarded as the key figure in that project. As mentioned in the introduction, even though Kronecker, for example, also proposed a sort of arithmetization, he did not take the steps given by Dedekind, Weierstrass and many other mathematicians, including students of the latter such as Cantor and Heine. Precisely, probably due to the impact of Weierstrass on new generations, as well as due to his temporal preeminence over those other authors, the traditional way of speaking identifies the arithmetizing project with Weierstrass' proposal. But it should not be forgotten that just as the usual way of speaking of 'arithmetization' rules out some proposals that shared some key features with the Weierstrassian, it also makes blurry the differences between this latter and some others.

[249] Dedekind wrote: "Bei dem Begriffe der Annäherung einer veränderlichen Grösse an einen festen Grenzwerth und namentlich bei dem Beweise des Satzesm dass jede Grösse, welche beständig, aber nicht über alle Grenzen wächst, sich gewiss einem Grenzwerth nähern muss, nahm ich meine Zuflucht zu geometrischen Evidenzen."

That said, it is true that 'arithmetization' of analysis entailed a detachment from geometric foundations whose crucial step during the 19th century was, precisely, the abandonment of such a notion of quantity and the embracement of a purely mathematical domain of objects, namely, numbers. A domain, it must be stressed, which was required to be a given continuous one, but static. From this perspective, the set of real numbers was not only infinite in the sense of not having a last member, as for example the set of natural numbers, but also infinite inasmuch as its de facto given members could measure any given quantity and thus correspond to any ordinary quantity. That way, the core notion of variable quantity was eventually replaced by syntactic variables (i.e. a character that represents a number) and functions of a real variable (i.e. functions within the real domain), while the real domain was conceived statically and characterized as totally ordered, dense and continuous.

Nonetheless, all that was only achieved through a long process whose strict periodization simply cannot be established.[250] As denounced by Dedekind, even in the 1870s there were still in –modern– real analysis some remnants of foreign ideas to it, which can even be found, for example, in the works of Weierstrass and Cantor: during that decade, both authors still used the appellative "numerical quantities" (*Zahlengrössen*) to refer to rational and irrational numbers within a pretty modern abstract conception (cf. Cantor, 1872/1932: 97; Weierstrass, 1878/1988: 7, 8 & 40), even though it evoked a previous understanding of mathematics as the science of discrete and continuous quantities which was still present in the mid-19th century (cf. Hoffmann, 1864: 144; Ferreirós, 2007: 42). That way, the work of Weierstrass himself, the author historically regarded as the chief exponent of the proposal nowadays commonly identified with 'the arithmetization of analysis', is evidence of the aforementioned process.

More importantly for the objectives of this thesis, in a sense Bolzano's early mathematical work is also evidence of that process, though in a different way from that of Weierstrass. This means that, despite the fact that, from c. 1900 onwards, that work of Bolzano (in particular his *Purely Analytic Proof*) has been interpreted as an isolated and largely ignored antecedent of that arithmetizing project and of Weierstrassian procedures and notions, this was not the case. Instead, what a careful reading of that work seems to show is the Germanic transition towards a pre-modern notion of number, that is, a transition towards the Germanic mathematical scenario

[250] However, attempts can be made to establish a certain periodization, as is done, for example, in (Ferreirós, 2016: 216ff.).

that without specifically anticipating that later project and those later notions and procedures, preceded them. So, while all those proposals undoubtedly shared common traits, this was not because the one of Bolzano was in the line of the later ones, but rather because Weierstrass and others developed their proposals on the ground plowed by Bolzano and other mathematicians. Which is not just a matter of drawing a line to distinguish what happened before and after: Bolzano's early mathematical work was pre-Weierstrassian and not proto-Weierstrassian since it still featured traits of mathematical notions (of quantities and numbers) and practices that were heavily deviant from the Weierstrassian and later ones.

Even more, those works of Bolzano represent a sort of confluence of two core ideas that shaped Germanic mathematics during the second half of the 18th century and the beginning of the 19th century: strictly speaking, zero and infinitesimals could not be considered quantities and numbers were only the –positive– whole ones. Bolzano was very clear about this in his 1816 work on the binomial theorem: he talked about "the self-contradictory [concept] of infinitely small quantities"; he wrote that it was "possible to divide by every finite (i.e. actual) quantity, but never by a zero (i.e. by nothing)"; and he referred to an exponent n that could denote a "whole number, [and] also a fractional, irrational or negative quantity" (cf. Bolzano, 1816: XI & 2). Or, as he wrote in his *Paradoxes of the Infinite* (*Paradoxien des Unendlichen*), published posthumously in 1851: "[If] the multitude of all numbers is infinite (the set of all the so-called natural or whole) [...], then so is the multitude of quantities [...] an infinite one. [For] not only all numbers are also quantities, but [...] the fractions [...] and the so-called irrational expressions [...] denote quantities" (Bolzano, 1851: 20-21).[251]

On the one hand, it is true that Bolzano's standpoint in *Paradoxes of the Infinite* was not exactly the same as that in his works of 1804-1817 but, on the other hand, it is also true that one and the other were not exactly the same as the views of his Germanic mathematical predecessors. As explained and shown in the second chapter of this work, throughout the second half of the 18th century Germanic mathematicians were taught mathematics in a post-Wolffian context. In other words, those mathematicians grew up in a context in which, although in general Wolff's

[251] Bolzano wrote: "die Menge aller Zahlen (der sogenannten natürlichen oder ganzen [...]) unendlich sei. [...] Ist die Menge der Zahlen (nämlich der sogenannten ganzen Zahlen) unendlich: so ist um so gewisser die Menge der Größen (nach der § 6 und Wissenschaftslehre § 87 vorkommenden Erklärung) eine unendliche. Denn jener Erklärung zufolge sind nicht nur alle Zahlen zugleich auch Größen, sondern es gibt noch weit mehr Größen als Zahlen, weil auch die Brüche $\frac{1}{2}, \frac{1}{3}, \frac{2}{3}, \frac{1}{4}, \ldots$, ingleichen die sogenannten irrationalen Ausdrücke $\sqrt{2}, \sqrt[3]{2}, \ldots, \pi, e, \ldots$, Größen bezeichnen."

philosophy and mathematical method did not undergo profound modifications, some mathematical notions and practices were increasingly different from those that at least at some point –if not always– Wolff defended. Thus, while at the *Académie Royale des Sciences et Belles Lettres* of Berlin not only French was made the official language, but also the works of Euler, Johann III Bernoulli, Lambert, Lagrange and other 'foreign' mathematicians were conceived and published (in its *Histoire* and *Mémoires*), in much of the Germanic territories central notions and practices of these mathematicians were rejected.

Precisely, because of the relevance of the universities of Göttingen and Halle in shaping the Germanic mathematics of the second half of the 18th century, rather than the relevance of the Academies of Prussia, Paris and St. Petersburg, the second chapter focused on the works of Kästner, Karsten and Segner. Three authors for whom, firstly, numbers were aggregates of units and, as a consequence, numbers par excellence were the –positive– whole ones, from which rationals could be formed, although those that could be called irrational numbers, as Segner clearly stated, could never be accurately –arithmetically– expressed, while zero was not a number but a sign. The reluctance to consider negative numbers within arithmetic framework and the usual absence of the term 'natural numbers' (whose positivity would only arise once considered the negative numbers) in the works of the Germanic mathematicians of that period, were both rooted in an eminently geometric and kinetic conception of mathematics.

Evidence of the above is the fact that, while in the entry 'number' in the English *Cyclopædia* and in the French *Encyclopédie* it was stated that –positive– whole numbers or "simply numbers" were "also known as natural numbers" (Chambers, 1728: 641; Diderot, 1765: 202),[252] by contrast in the entry "*Zahl*" in Klügel's *Mathematisches Wörterbuch* the alternative appellation of "natural numbers" was not employed (Klügel *et al*., 1831: 1053ff.). This is relevant not only because of the date, but also because the division of the "whole science of numbers" into arithmetic and number theory accounts for a change that occurred during those years (*id*.: 1057; cf. Legendre, 1798; Gauss, 1801), while the notion of 'number' makes evident the prevalence of conflicts around it. Just as in his book on the groundings of arithmetic, Klügel only introduced the irrationals and negatives to say that a) the quantity of the former could only be accurately

[252] Louis de Jaucourt (D. J.) wrote: "Les nombres entiers, appelés aussi nombres naturels ou simplement nombres, sont ceux que l'on regarde comme des tous, sans supposer qu'ils soient parties d'autres nombres."

represented in geometry and b) the members (numbers) of the arithmetic progression, if read backwards, would lead to “negated” members (Klügel, 1792: 31 & 50-51), the same can be found in his dictionary (Klügel, 1805: 104ff. & 949).

Secondly, for Kästner, Karsten, Segner and many of the Germanic mathematicians of the second half of the 18th century, ‘quantity’ was that capable of increase and decrease, as –at some point– for Wolff, and therefore just as zero was not a quantity, neither were quantities the differentials and infinitesimals associated to zeros. Ultimately, one could mathematically consider the variability of a quantity but one could not ascribe to it a –finite– numerical value. As summarized by Segner when he explained the variability that led from positive to negative quantities and vice versa, there were “infinitely many types of quantities, [...] such as possessions, debts; accepted, spent; gravity, force directed upwards; water flowing into vessel, water emanating [from it], and very many other” (Segner, 1758: 5).[253] A variability, it must be stressed, that at that time Germanic mathematicians primarily interpreted in geometric terms; hence the law of continuity was introduced within the geometric framework.

Coupled with the reluctance towards those new foreign developments, however, it is worth noting two crucial aspects of Germanic mathematical practices of the time, namely, their use of irrational and infinitely large quantities. On the one hand, despite the differences between their proposals, Kästner, Karsten and Segner, like many others, conceived irrational numbers as those that could not be properly expressed by whole units or aliquote parts of the unit. As a consequence, irrationals were not numbers in the strict sense, since they were closer to geometric magnitudes, although they were referred to as ‘numbers’. On the other hand, for many Germanic mathematicians, as insistently stated by Karsten, infinitely large quantities were not problematic and so one was entitled to use them as $= \infty$ in mathematics.

By contrast, in his early mathematical works Bolzano: a) was equally reticent towards infinitely large and small quantities (cf. Bolzano, 1810: 30); b) considered that the concept of the irrationality of a quantity still had to be clearly developed (cf. Bolzano, 1816: 143-144), in spite

[253] Segner wrote: “Caeterum infinita sunt quantitatum genera, quarum altera refertur ad alteram, quemadmodum progressus refertur ad regressum, ascensus ad descensum, vel motus quicunque versus aliquam partem, ad motum versus partem oppositam. Tales sunt, possessiones, debita; accepta, expensa; gravitas, vis sursum directa; aqua in vas influens, aqua effluens, & aliae plurimae, quarum quaedam in sequentibus clare exponentur.”

of which he referred to "irrational quantities" but not to "irrational numbers";[254] and c) although he did not refer to the "commonly called" natural numbers, as he did some years later in his *Paradoxes*, he did use this expression (*natürlichen Zahlen*) by 1817 (cf. Bolzano, 1816: 41, 43, 85 & 96). Which means that, while Bolzano's mathematical terminology and practice show a step forward with regard to the basic notion of natural numbers, his general notions of quantity and number in a sense were drawn more rigorously but at the same time more modernly than the ones of his predecessors. The assumption of positive whole numbers as the most natural multiplicity of numbers entailed that there was also the –not so natural– multiplicity of negative numbers, to which he usually referred in terms of quantities with negative whole-numbered values (that is, quantities to which could be assigned numerical values). But, at the same time, such a notion of 'numbers' implied that these ones were less than the quantities, among which were the whole positive, but also the fractional, negative and irrational ones; concepts, these last two, that from his perspective still required to be clearly developed.

Furthermore, by 1817 Bolzano was reluctant towards the concept of an infinite quantity, and this included both the infinitely big and small quantities. As he wrote in 1810, it was still to be decided whether both concepts could be regarded, not as quantities, but as symbolic expressions (that is, in current terms, as tools), as they were, for example $\sqrt{-1}$ (1810) or π, e and others (1851).[255] So, while whole negative and fractional quantities could be numerically expressed, the irrationals and the infinitely small and big could not be –finitely– numerically expressed or, as he emphasized in his works of 1816-1817, infinite quantities could not be strictly determined: for example, a) while the magnitude of an infinite straight line could not be determined and therefore this one was an "indeterminate line" (1804), the magnitude of a straight line between points a and b could be determined and so he would refer to it as a "determinable line" (1817A); and b) he said that a function $\frac{a}{b-x}$ "does not have any determinate value when $x = b$, but becomes what is called infinitely big" (1817B), but spoke of a determined multitude of infinite functions $fx, \bar{f}x, \bar{\bar{f}}x, \ldots$ that varied by the law of continuity for x (1817A).

[254] As explained in the last chapter of this thesis, although it is true that Bolzano referred once to "*irrationalen Zahl*" in his mathematical works of 1804-1817 (cf. Bolzano, 1816: 137), it seems appropriate to interpret his reference to "whole, fractional or even irrational number" (§73) as meaning what he wrote at the beginning of that work, namely, "whole number, [and] also a fractional, irrational or negative quantity" (Bolzano, 1816: 2), given that in his solution to the problem stated in §73 he referred once again to "irrational quantities" (*irrationaler Grössen* and *Irrationalgrösse*).

[255] It is worth stressing that the conception as "symbolic expressions" of some of what we now consider numbers was not entirely strange at the time: as it was said before, Bolzano, Cauchy and Jandera, for example, regarded imaginary numbers as "symbolic expressions" (cf. Bolzano, 1810: 30; Cauchy, 1821: iij-iv & 173ff.; Jandera, 1830: XXIX).

His definition of a continuous function and quantities ω, definitions which closely resemble the modern ones of a continuous function of a real variable and Weierstrassian ε, and so constitute two of the pillars that have traditionally sustained much of the attributed modernity of his *Purely Analytic Proof*, should be interpreted within that framework and not within later ones (cf. C.2.2).

What Bolzano did in his mathematical works of 1816-1817 seems, therefore, consistent with his conception of mathematics and his notions of quantity and number. He defined mathematics as the science about the general laws or forms to which things must conform in their existence, because mathematical disciplines were not only purely scientific but also useful for everyday needs. To mathematically study things (i.e. objects of thought or empirical), however, quantities still played a crucial role in his system, but also their modes of composition or forms. This seems to be the reason why combinatorial theory was so appealing to him to the extent that he considered it a core part of mathematics. Even more, if one takes into account that underlying Hindenburg's program there was an aim to work mathematically in finite terms, Germanic combinatorial theory of the early 19th century fitted perfectly with Bolzano's own objectives: quantities varied but in order to study analytically their variation one could not resort to geometric or kinetic notions and arguments, since these ones were alien to analysis and in fact belonged to mathematical parts subordinated to analysis, just as one could not rely on actually infinite quantities, i.e. non determined quantities, but in any case on infinite multitudes.

That way, when Bolzano explained in the preface to his 1816 work the concept of quantities ω, he said that there was nothing objectionable in it and added that the existence of such quantities in space and time could be ascertained; a remark that does not fit with the syntactic conception of variables within the later analytical framework and indeed implies assuming space and time as continuous, something that later mathematicians would consider an axiom (cf. Cantor, 1872; Dedekind 1872). For him, variable quantities ω were not numbers (i.e. were not Weierstrassian ε), but neither were they quantities that tended to zero in a strictly dynamic way (i.e. were not Cauchy's infinitesimals, implicitly dynamic, nor Carnot's "continuously decreasing" quantities, explicitly dynamic).

In other words, quantities ω were for Bolzano variable quantities that should not be assumed as *being* smaller than any conceivable possible quantity, as the strictly dynamic conception of quantities required, but as *capable of becoming* smaller than any given quantity. Hence his distinction between “variable” and “constant” variable quantities (cf. Bolzano, 1817B: 30-31), as well as his much-criticized proof procedure of his [convergence] criterion, where: a) he first discarded to prove that the quantity X (to which a sequence of quantities [converged]) could be assumed to be variable, an hypothesis that he took for obviously true, since that way it could always be assumed as close as desired to the term of the sequence with which it was supposed to be compared; and, secondly, b) he stated that the hypothesis of X as a constant quantity also contained nothing impossible and proved that the sequence approached to such a quantity, which could be determined as accurately as pleased.

It is not only, therefore, that by 1817 Bolzano named ‘quantities’ what later would be called ‘real numbers’. For him, it must be stressed, irrational quantities were not numbers and, indeed, the irrationality of a quantity was a concept that still required to be clarified. Thus, just as he worked with finite segments of infinite series (i.e. determined segments), ultimately he worked with –indeterminate– irrational quantities by means of determined fractions (cf. Bolzano, 1816: 23, 76 & 137-138; Bolzano, 1817B: 38). Even his definition of quantities ω could be interpreted as a sort of attempt to finitely treat the potentially infinite variability of those quantities towards zero.

In that way, though some of his mathematical practices not only differ a lot from the ones of his Germanic mathematicians predecessors and contemporaries, but in fact they resemble the modern ones, the underlying ideas to those practices of Bolzano reveal how heavily deviant they were from later practices, strictly speaking. Because, while within his analytical framework a) he introduced the notion of a “multitude of infinite elements”, an object of mathematical study inasmuch as it was determined by a law or a finite number of laws, and, moreover, b) he assumed that quantities were determined by the law of continuity, c) he did not give the step to consider a quantity as an actual multitude of infinite elements. Would he have done it, he would have considered actual infinite quantities as objects of analytical study and irrational quantities would not have been problematic. Moreover, would he have done it, a further step would have still been required for Bolzano’s ω to be Weierstrassian ε, namely, to consider some ‘numbers’ as multitudes of infinite elements. But the one and the other step would have contravened not only his idea of a correct mathematical procedure, but also his notions of quantity and number.

While in 1865 Weierstrass enunciated what today is known as Bolzano-Weierstrass theorem in terms of a point in –or on the boundary of– a bounded part of a plane (cf. Weierstrass, 1865/66: 16[B]) and later in his formulation of that theorem he identified "an unbounded variable magnitude, which [formed] a simple manifold", with its geometrical representation by a straight line (Weierstrass, 1886: 60),[256] those could not have been valid procedures for Bolzano, at least according to his early mathematical proposal. As a matter of fact, one of the few places throughout his 1816 work and his *Purely Analytic Proof* where he used the term "point" as identified with, traditional reading would say, the value of a Weierstrassian real quantity,[257] was in the preface of that latter work: one of the usual types of proof of the intermediate value theorem, he wrote, depended on a geometric truth, namely, "that every continuous line of simple curvature whose ordinates are first positive [and] then negative (or vice versa), must necessarily intersect the abscissa line somewhere at a point lying between those ordinates" (Bolzano, 1817B: 6).[258] From Bolzano's perspective, this was an invalid mathematical procedure due to the subordination of geometry to analysis. But, ultimately, this also highlights how deviant were Bolzano's mathematical notions and practices from later Weierstrassian and arithmetizing ones.

Last but not least, in the transition from the kinetic approach of the 18th century, in which quantities prevailed, to the static standpoint that would be characteristic of 'arithmetized' analysis (e.g. the one of Weierstrass, Dedekind, Cantor and others), one of the crucial points was the *realization that a clear conceptual characterization of the continuity* of the real-number domain was needed. Success in arithmetizing the continuity of this domain was to become a key element in that project, perhaps even its climax, as expressed in the title of Dedekind's 1872 work, *Continuity and irrational numbers*. By contrast, it was characteristic of pre-Weierstrassian proposals, among them the early one of Bolzano, to not yet envision that project and apparently

[256] As said before, both Cantor and Dedekind stressed their right to do so in their works of 1872 (cf. Cantor, 1872/1932: 97; Dedekind, 1872: 18-19).

[257] Other place in those works where such identification can be found is in the §13 of his *Purely Analytic Proof*, when he exemplified his so-called "least upper bound" theorem by means of a rectangular hyperbola (cf. Bolzano, 1817B: 49-50).

[258] Bolzano wrote: "Eine jede continuirlicheLinie von einfacher Krümmung, deren Ordinaten erst positiv, dann negativ sind (oder umgekehrt), die Abscissenlinie nothwendig irgendwo in einem Puncte, der zwischen jenen Ordinaten liegt, durch schneiden müsse."

had no clear notion of a "theory of the continuum" (i.e. topological), nor any proposal about how to approach the elements of topology (cf. Ferreirós, 2007: 137ff.), despite Bolzano's pre-set-theoretical and pre-topological ideas. A difference that, precisely, stresses the distance between Bolzano's proposal in 1817 and, for example, the one of Cantor (who used limit points and derived sets) and Dedekind (who used his cut principle of continuity) in 1872.

And yet, once again, at the same time Bolzano appears as an important pre-Weierstrassian author and a pioneering figure, given, for example, the method of partition of intervals that he used in his *Purely Analytic Proof* and that was highly valued by Weierstrass. Once the problem of characterizing the continuity of $\mathbb{R}$ became clear, as it was not in Bolzano's early mathematical work, that method could be turned into the so-called Bolzano-Weierstrass principle, [259] according to which an infinite sequence of nested, closed intervals, always has nonvoid intersection (i.e. at least one point belongs to every interval). As it increasingly became clear, that principle offered a satisfactory characterization of the continuity of the real number system, alternative to the cut property proposed by Dedekind in 1872. Thus, for example, in 1884 Cantor emphasized the importance of that "very old" principle, which, he said, "was hardly possible to replace by an essentially different one" (Cantor, 1884/1932: 212).[260] By that time, however, the Germanic mathematical context was very different from that of Bolzano's *Purely Analytic Proof*, a work that in the end would be as widely recognized as Bolzano wanted his first works to be (cf. Bolzano, 1817B: 27), although often interpreted in a way that, according to this thesis, seems alien to what he actually proposed.

[259] This expression, used in (Ferreirós, 2007: 139-141), is quite common in mathematical terminology, even though strictly speaking it is anachronistic.

[260] Cantor wrote: "Ich bemerke, dass die hier angewandte Beweismethode, welche wohl schwerlich durch eine wesentlich andere ersetzt werden kann, ihrem Kerne nach sehr alt ist; in neuerer Zeit findet man sie unter anderem in gewissen zahlentheoretischen Untersuchungen bei Lagrange, Legendre und Dirichlet, in Cauchy's *Cours d'analyse* (Note troisième) und in einigen Abhandlungen von Weierstrass und Bolzano; es scheint mir daher nicht richtig, sie vorzugsweise oder aussechliesslich auf Bolzano zurückzuführen, wie solches in neuerer Zeit beliebt worden ist."

Conclusiones

Las líneas iniciales del trabajo de Dedekind de 1872, *Continuidad y números irracionales* (*Stetigkeit und irrationale Zahlen*), el mismo en el que presentó su definición de los números reales, están dedicadas a explicar el origen de dicho trabajo: al preparar sus lecciones sobre cálculo diferencial al inicio de su carrera como profesor (esto es, en 1858), escribió, tuvo que basarse en "evidencia geométrica" "en la noción de aproximación de una cantidad variable a un valor límite establecido", algo didácticamente aceptable pero carente de "cientificidad" (*Wissenschaftlichkeit*) (Dedekind, 1872: 9).[261] Aunado a lo cual, dieciséis años después, en el prefacio a *¿Qué son y para qué sirven los números?* (*Was sind und was sollen die Zahlen?*), él señaló que, como ya había mostrado en su trabajo antes mencionado, la "extensión gradual del concepto de número" (*die shrittweise Erweiterung der Zahlbegriffes*) se podía llevar a cabo sin emplear "ideas extrañas" (*fremdartiger Vorstellungen*), como por ejemplo la de "cantidades medibles" (*messbaren Grössen*) (Dedekind, 1888: X).

El punto de vista de Dedekind era el de un matemático germánico del siglo XIX quien propuso una aritmetización del análisis a la cual llegó independientemente de Weierstrass, esto es, sin la influencia del autor que tradicionalmente es considerado como la figura clave en dicho proyecto. Como se mencionó en la introducción, pese a que Kronecker, por ejemplo, también propuso una especie de aritmetización, él no dio los pasos dados por Dedekind, Weierstrass y otros matemáticos, incluyendo a Cantor, Heine y otros estudiantes de aquel último. Por ello, probablemente debido al impacto de Weierstrass en las nuevas generaciones, así como debido a su preeminencia temporal sobre aquellos otros autores, la narrativa tradicional identifica el proyecto aritmetizador con la propuesta de Weierstrass. Sin embargo, no se ha de olvidar que así como el modo habitual de referirse a la 'aritmetización' deja de lado algunas propuestas que compartían rasgos cruciales con la Weierstrassiana, a su vez hace borrosas las diferencias entre esta última y algunas otras.

[261] Dedekind escribió: "Bei dem Begriffe der Annäherung einer veränderlichen Grösse an einen festen Grenzwerth und namentlich bei dem Beweise des Satzesm dass jede Grösse, welche beständig, aber nicht über alle Grenzen wächst, sich gewiss einem Grenzwerth nähern muss, nahm ich meine Zuflucht zu geometrischen Evidenzen".

Dicho lo anterior, es verdad que la 'aritmetización' del análisis conllevó un desapego de los fundamentos geométricos cuyo paso crucial durante el siglo XIX fue, justamente, el abandono de tal noción de cantidad y la adopción de un dominio puramente matemático de objetos, a saber, los números. Un dominio, ha de ser recalcado, que se requería que fuera continuo pero estático. Desde esta perspectiva, el conjunto de los números reales no sólo era infinito en el sentido de que carecía de un último elemento, como por ejemplo el conjunto de los números naturales, sino también era infinito en tanto sus elementos dados de facto podían medir cualquier cantidad dada y, por ende, ser identificados con cualquiera cantidad ordinaria. De esta manera, la noción central de 'cantidad variable' fue eventualmente reemplazada por las variables sintácticas (esto es, caracteres que representan números) y funciones de una variable real (esto es, funciones en el marco del dominio real), mientras que el dominio real fue concebido estáticamente y caracterizado como totalmente ordenado, denso y continuo.

Empero, todo ello sólo fue logrado a través de un largo proceso cuya estricta periodización simplemente no puede ser establecida.[262] Como denunció Dedekind, en la década de 1870 aún existían en el análisis real –moderno– algunos remanentes de ideas ajenas a este, mismas que se pueden encontrar, por ejemplo, en los trabajos de Weierstrass y Cantor: durante dicha década, ambos autores aún empleaban la denominación "cantidades numéricas" (*Zahlengrössen*) para referirse a los números racionales e irracionales dentro de una concepción abstracta sumamente moderna (cf. Cantor, 1872/1932: 97; Weierstrass, 1878/1988: 7, 8 y 40), pese a que dicha denominación evocaba una comprensión previa de la matemática, en tanto ciencia de cantidades discretas y continuas, aún presente a mediados del siglo XIX (cf. Hoffmann, 1864: 144; Ferreirós, 2007: 42). Así, el trabajo del propio Weierstrass, el matemático que históricamente ha sido considerado como el máximo exponente de la propuesta que hoy día comúnmente se identifica con la 'aritmetización del análisis', es evidencia de aquel proceso.

Sobre todo, para los fines de esta tesis, en cierto sentido los primeros trabajos matemáticos de Bolzano a su vez son muestra de dicho proceso, si bien de una manera diferente a como lo es la obra de Weierstrass. Esto significa que, pese a que a partir de c. 1900 en adelante, aquella obra de Bolzano (en particular su *Prueba puramente analítica*) ha sido interpretada como un

[262] Sin embargo, se pueden hacer intentos por establecer una cierta periodización, como se hace, por ejemplo, en (Ferreirós, 2016: 216ss.).

antecedente aislado y ampliamente ignorado de ese proyecto aritmetizador y de nociones y procedimientos Weierstrassianos, tal lectura es incorrecta. Por el contrario, lo que parece mostrar una lectura cuidadosa de esos trabajos de Bolzano es la transición germánica hacia una noción premoderna de número, esto es, una transición hacia el escenario matemático germánico que, sin anticipar aquel proyecto y aquellas nociones y procedimientos posteriores, les antecedió. De ese modo, mientras que indudablemente todas esas propuestas comparten rasgos comunes, esto no se debe a que Bolzano estuviera en la línea de las posteriores sino, en cambio, a que Weierstrass y otros desarrollaron sus propuestas sobre la tierra trabajada por Bolzano y otros matemáticos. Lo cual no es meramente una cuestión de distinguir entre lo que ocurrió antes y después: los primeros trabajos matemáticos de Bolzano fueron pre-Weierstrassianos y no proto-Weierstrassianos, puesto que aún tenían características de nociones (de cantidad y número) y prácticas matemáticas sumamente distintas de la propuesta de Weierstrass y posteriores.

Aún más, aquellos trabajos de Bolzano representan una especie de confluencia de dos ideas medulares que moldearon las matemáticas germánicas durante la segunda mitad del siglo XVIII y comienzos del siglo XIX: estrictamente hablando, el cero y los infinitesimales no podían ser considerados cantidades y los números sólo eran los enteros –positivos–. Bolzano fue muy claro respecto a esto en su trabajo de 1816 sobre el teorema del binomio: se refirió al "[concepto] auto-contradictorio de cantidades infinitamente pequeñas"; escribió que era "posible dividir por cualquier cantidad finita (esto es, actual), pero nunca por cero (esto es, nada)"; y habló sobre un exponente n que podía denotar un "número entero, [y] también una cantidad racional, irracional o negativa" (cf. Bolzano, 1816: XI y 2). O, como escribió en *Las paradojas del infinito* (*Paradoxien des Unendlichen*), publicado póstumamente en 1851: "[Si] la multitud de todos los números es infinita (el conjunto de los llamados naturales o enteros) [...], entonces también es infinita la multitud de las cantidades [...]. [Puesto que] no sólo todos los números son también cantidades, sino que [también] [...] denotan cantidades las fracciones [...] y las denominadas expresiones irracionales" (Bolzano, 1851: 20-21).[263]

[263] Bolzano escribió: "die Menge aller Zahlen (der sogenannten natürlichen oder ganzen [...]) unendlich sei. [...] Ist die Menge der Zahlen (nämlich der sogenannten ganzen Zahlen) unendlich: so ist um so gewisser die Menge der Größen (nach der § 6 und Wissenschaftslehre § 87 vorkommenden Erklärung) eine unendliche. Denn jener Erklärung zufolge sind nicht nur alle Zahlen zugleich auch Größen, sondern es gibt noch weit mehr Größen als Zahlen, weil auch die Brüche $\frac{1}{2}, \frac{1}{3}, \frac{2}{3}, \frac{1}{4}, \ldots$, ingleichen die sogenannten irrationalen Ausdrücke $\sqrt{2}, \sqrt[3]{2}, \ldots, \pi, e, \ldots$, Größen bezeichnen".

Ciertamente, la postura de Bolzano en *Las paradojas del infinito* no era exactamente la misma que aquella en sus trabajos de 1804-1817 pero, a su vez, una y otra no eran exactamente la misma que la de sus predecesores matemáticos germánicos. Como se muestra y explica en el segundo capítulo de este trabajo, a lo largo de la segunda mitad del siglo XVIII a los matemáticos germánicos se les enseñaron las matemáticas en un contexto post-Wolffiano. En otras palabras, tales matemáticos crecieron en un contexto en el cual, si bien en general la filosofía y el método matemático de Wolff no sufrió profundas modificaciones, algunas nociones y prácticas matemáticas fueron cada vez más diferentes de aquellas que, si no siempre, al menos en algún momento defendió Wolff. Así, mientras que en la *Académie Royale des Sciences et Belles Lettres* de Berlin no sólo el francés fue establecido como el idioma oficial, sino que además ahí se concibieron y publicaron (en sus *Histoire* y *Mémoires*) trabajos de Euler, Johann III Bernoulli, Lambert, Lagrange y otros matemáticos 'extranjeros', en gran parte de los territorios germánicos se rechazaban nociones y prácticas centrales de estos matemáticos.

Precisamente, dada la relevancia de las universidades de Göttingen y Halle en la conformación de las matemáticas germánicas de la segunda mitad del siglo XVIII, no así de la Academia de Preußen, la de Paris y la de San Petersburgo, el segundo capítulo se enfocó en los trabajos de Kästner, Karsten y Segner. Tres autores para quienes, en primer lugar, los números eran agregados de unidades y, como consecuencia, los números por excelencia eran los enteros –positivos–, a partir de los cuales se podían formar los racionales, mientras que los llamados irracionales, como claramente lo señaló Segner, nunca podrían ser –aritméticamente– expresados con precisión, siendo además el cero un signo y no un número. La reticencia para considerar a los números negativos en el entramado aritmético, así como la ausencia habitual del término 'números naturales' (cuya positividad sólo emergía al considerar a los números negativos) en los trabajos de los matemáticos germánicos de la época, estaban una y otra enraizadas en una concepción eminentemente geométrica y cinética de las matemáticas.

Evidencia de lo anterior es el hecho de que, mientras que en la entrada 'número' en la *Cyclopædia* inglesa y en la *Encyclopédie* francesa se indicaba que los números enteros –positivos– o "simplemente números" "también eran conocidos como números naturales

(Chambers, 1728: 641; Diderot, 1765: 202),[264] en cambio en la entrada "*Zahl*" en el *Mathematisches Wörterbuch* de Klügel esa denominación alternativa no era usada (Klügel *et al*., 1831: 1053ss.). Esto es relevante no sólo debido a la fecha, sino también debido a que la división de "toda la ciencia de los números" en aritmética y teoría de números da cuenta de un cambio que tuvo lugar durante esos años (*id*.: 1057; cf. Legendre, 1798; Gauss, 1801), mientras que la noción de 'número' muestra la prevalecencia de conflictos en torno a ella. Como consecuencia, así como en su libro sobre los fundamentos de la aritmética Klügel sólo introdujo los irracionales y los negativos para decir que a) la cantidad de los primeros sólo se podía representar con exactitud en la geometría y b) los miembros (números) de la progresión aritmética, de ser leídos al revés, conducirían a miembros "negados" (Klügel, 1792: 31 y 50-51), lo mismo se puede encontrar en su diccionario (Klügel, 1805: 104ss. y 949).

En segundo lugar, para Kästner, Karsten, Segner y muchos otros matemáticos germánicos de la segunda mitad del siglo XVIII, 'cantidad' era aquello capaz de incremento y disminución, como –en cierta medida– lo era para Wolff, y como consecuencia, así como el cero no era una cantidad, tampoco lo eran las cantidades que los diferenciales e infinitesimales asociaban a ceros. En última instancia, uno podía considerar matemáticamente la variabilidad de una cantidad pero no adscribirle a ella un valor numérico –finito–. Como de alguna manera lo resumió Segner cuando explicó la variabilidad que conducía de una cantidad positiva a una negativa y viceversa, había "infinitamente muchos tipos de cantidades, [...] como por ejemplo las posesiones y deudas; lo recibido y lo gastado; la gravedad y la fuerza dirigida hacia arriba; el flujo del agua en un recipiente y el del agua emanando de este; y muchas otros más" (Segner, 1758: 5).[265] Una variabilidad, se ha de enfatizar, que en aquellos tiempos los matemáticos germánicos interpretaban sobre todo en términos geométricos; de ahí que la ley de continuidad fuera introducida en el entramado geométrico.

Aunado a la reticencia hacia dichos nuevos desarrollos extranjeros, sin embargo, vale la pena notar dos aspectos cruciales de las prácticas matemáticas germánicas de la época, a saber, su

[264] Louis de Jaucourt (D. J.) escribió: "Les nombres entiers, appelés aussi nombres naturels ou simplement nombres, sont ceux que l'on regarde comme des tous, sans supposer qu'ils soient parties d'autres nombres".

[265] Segner escribió: "Caeterum infinita sunt quantitatum genera, quarum altera refertur ad alteram, quemadmodum progressus refertur ad regressum, ascensus ad descensum, vel motus quicunque versus aliquam partem, ad motum versus partem oppositam. Tales sunt, possessiones, debita; accepta, expensa; gravitas, vis sursum directa; aqua in vas influens, aqua effluens, & aliae plurimae, quarum quaedam in sequentibus clare exponentur".

uso de las cantidades irracionales y de las cantidades infinitamente grandes. Por una parte, pese a las diferencias entre sus propuestas, Kästner, Karsten y Segner, como muchos otros, concebían a los números irracionales como aquellos que no podían ser expresados apropiadamente mediante unidades enteras o partes alícuotas de la unidad. Debido a ello, en sentido estricto los irracionales no eran números, puesto que ellos estaban más próximos a las magnitudes geométricas, si bien se referían a ellos como 'números'. Por otra parte, para muchos matemáticos germánicos, como Karsten insistentemente defendió, las cantidades infinitamente grandes no eran problemáticas y, por ende, uno estaba autorizado a emplearlas en matemáticas como $= \infty$.

En cambio, en sus primeros trabajos matemáticos, Bolzano: a) era reticente por igual a las cantidades infinitamente pequeñas y a las infinitamente grandes (cf. Bolzano, 1810: 30); b) consideraba que el concepto de irracionalidad de una cantidad aún debía de ser desarrollado con claridad (cf. Bolzano, 1816: 143-144), a pesar de lo cual él se refería a "cantidades irracionales", aunque no a "números irracionales";[266] y c) si bien él no se refirió a los "comúnmente llamados" números naturales, como lo hiciera años más tarde en *Las paradojas*, hacia 1817 él sí usaba esta expresión (*natürlichen Zahlen*) (cf. Bolzano, 1816: 41, 43, 85 y 96). Lo que significa que mientras la práctica y la terminología matemática de Bolzano muestran un paso adelante con respecto a la noción básica de números naturales, de cierta manera sus nociones generales de cantidad y número estaban trazadas más rigurosamente pero a la vez más modernamente que las de sus predecesores. La asunción de números enteros positivos como la multiplicidad más natural de números conllevaba que también existía la –no tan natural– multiplicidad de números negativos, a los cuales él usualmente se refería en términos de cantidades con valores numerados por enteros negativos (es decir, cantidades a las que se les podían asignar valores numéricos). Pero, al mismo tiempo, semejante noción de 'números' implicaba que estos eran menos que las cantidades, entre las cuales estaban las enteras positivas, pero también las racionales, negativas e irracionales; conceptos, estos dos últimos, que desde su perspectiva aún necesitaban ser desarrollados con claridad.

[266] Como se explicó en el último capítulo de esta tesis, si bien es cierto que Bolzano se refirió en alguna ocasión en sus trabajos de 1804-1817 a "*irrationalen Zahl*" (cf. Bolzano, 1816: 137), parece apropiado interpretar dicha referencia a "números enteros, racionales e incluso irracionales" (§73) más bien en la línea de lo que escribió al inicio de ese trabajo, a saber, "un número entero, [y] también una cantidad racional, irracional o negativa" (Bolzano, 1816: 2), dado que en su solución al problema presentado en el §73 él volvió a referirse a "cantidades irracionales" (*irrationaler Grössen* e *Irrationalgrösse*).

Aún más, hacia 1817 Bolzano era reticente respecto al concepto de una cantidad infinita, lo que incluía a las cantidades infinitamente grandes y pequeñas. Como escribió en 1810, aún estaba por ser decidido si ambos conceptos podrían ser considerados, no como cantidades, sino como expresiones simbólicas (esto es, en términos actuales, como herramientas), como lo eran, por ejemplo, $\sqrt{-1}$ (1810) o π, e y otras (1851).[267] Así, mientras que las cantidades enteras negativas y racionales podían ser expresadas numéricamente, las irracionales y las infinitamente pequeñas o grandes no podían ser expresadas numérica-finitamente, esto es, como enfatizó en sus trabajos de 1816-1817, las cantidades infinitas no podían ser estrictamente determinadas: por ejemplo, a) mientras que la magnitud de una línea recta infinita no podía ser determinada y, por ende, ésta era una "línea indeterminada" (1804), la magnitud de una línea recta entre dos puntos a y b podía ser determinada y, por lo tanto, él podía referirse a ésta como una "línea determinable" (1817A); y b) escribió que una función $\frac{a}{b-x}$ "no tiene un valor determinado cuando $x = b$, sino que se torna lo que se denomina infinitamente grande (1817B), pero se refirió a una multitud infinita determinada de funciones $fx, \bar{f}x, \bar{\bar{f}}x, \dots$ que variaba para x conforme a la ley de continuidad (1817A). Sus definiciones de una función continua y de las cantidades ω, que se asemejan mucho a la definición moderna de una función continua de una variable real y a la ε Weierstrassiana, y que por ende constituyen dos de los pilares que tradicionalmente han sustentado mucha de la modernidad atribuida a su *Prueba puramente analítica*, han de ser interpretados dentro de aquel entramado y no dentro de entramados posteriores (cf. C.2.2).

Lo que Bolzano hizo en sus trabajos matemáticos de 1816-1817 parece, por lo tanto, consistente con su concepción de las matemáticas y sus nociones de cantidad y número. Él definió la matemática como la ciencia sobre las leyes o formas generales a las que las cosas habían de ajustarse en su existencia, ya que las disciplinas matemáticas no sólo eran puramente científicas sino también útiles para necesidades del día a día. Para estudiar matemáticamente las cosas (esto es, objetos del pensamiento o empíricos), sin embargo, las cantidades aún jugaban un rol crucial en su sistema, como también lo hacían sus modos de composición o formas. Esta parece ser la razón por la que la teoría combinatoria resultaba tan atractiva para él, al grado de que él la consideraba una parte medular de la matemática. Todavía más, si uno toma en cuenta que al

[267] Vale la pena recalcar que la concepción de algunos de los que hoy día se consideran números como "expresiones simbólicas" no era enteramente extraña en la época: como se dijo antes, tanto Bolzano como Cauchy y Jandera, por ejemplo, consideraban a los números imaginarios como expresiones simbólicas (cf. Bolzano, 1810: 30; Cauchy, 1821: iij-iv y 173ss.; Jandera, 1830: XXIX).

programa de Hindenburg subyacía el objetivo de trabajar matemáticamente en términos finitos, la teoría combinatoria germánica de principios del siglo XIX encajaba perfectamente con los objetivos de Bolzano: las cantidades variaban pero para estudiar analíticamente su variación uno no podía recurrir a nociones y argumentos geométricos o cinéticos, puesto que estos eran ajenos al análisis y de hecho pertenecían a partes de la matemática subordinadas al análisis, así como uno no podía basarse en cantidades infinitas en acto, es decir, en cantidades no determinadas, sino, en cualquier caso, en multitudes infinitas.

Así, cuando en el prefacio a su trabajo de 1816 Bolzano explicó el concepto de cantidades ω, él dijo que no había nada objetable en dicho concepto y añadió que la existencia de tales cantidades en el espacio y tiempo era inapelable; una observación que no encaja con la concepción sintáctica de 'variables' en el entramado analítico posterior y que, de hecho, implica la asunción del espacio y del tiempo en tanto continuos, algo que matemáticos posteriores habrían de considerar un axioma (cf. Cantor, 1872; Dedekind 1872). Para él, las cantidades variables ω no eran números (esto es, no eran las ε Weierstrassianas), pero tampoco eran cantidades que tendían a 0 de una manera estrictamente dinámica (esto es, no eran los infinitesimales de Cauchy, implícitamente dinámicos, ni las cantidades "continuamente decrecientes" de Carnot, explícitamente dinámicas).

En otras palabras, para Bolzano las cantidades ω eran cantidades variables que no debían ser asumidas *siendo* más pequeñas que cualquier cantidad posible concebible, como lo demandaba una concepción de las cantidades estrictamente dinámica, sino que debían ser asumidas como cantidades *capaces de ser* más pequeñas que cualquier cantidad dada. De ahí su distinción entre cantidades variables "variables" y "constantes" (cf. Bolzano, 1817B: 30-31), así como su tan criticado procedimiento para probar su criterio de [convergencia], en la cual él: a) primero descartó probar que la cantidad X (en la cual [convergía] una secuencia de cantidades) podía ser asumida como variable, una hipótesis que daba por obviamente verdadera puesto que de ese modo siempre podía ser asumida tan cerca como se quisiera al término de la secuencia con el cual se suponía que era comparada; y, segundo, b) afirmó que la hipótesis de X como una cantidad constante tampoco era imposible y probó entonces que la secuencia se aproximaba a dicha cantidad, misma que podía ser determinada con la precisión que se quisiera.

No se trata simplemente, por ende, de que hacia 1817 Bolzano denominara 'cantidades' a aquellas que más tarde se llamarían 'números reales'. Para él, debe quedar claro, las cantidades irracionales no eran números y, de hecho, la irracionalidad de una cantidad era un concepto que aún necesitaba ser clarificado. Como consecuencia, así como él trabajó con segmentos finitos de series infinitas (es decir, con segmentos determinados), en última instancia también trabajó con cantidades irracionales –indeterminadas– mediante fracciones determinadas (cf. Bolzano, 1816: 23, 76 y 137-138; Bolzano, 1817B: 38). Incluso, su definición de cantidades ω podría ser interpretada como una suerte de intento por tratar de modo finito la variabilidad potencialmente infinita hacia 0 de aquellas cantidades.

Así, pese a que algunas de sus prácticas matemáticas no sólo diferían sobremanera de las de sus antecesores y contemporáneos matemáticos germánicos, sino que de hecho se asemejaban a prácticas modernas, las ideas subyacentes a aquellas prácticas de Bolzano revelan lo diferentes que estrictamente hablando eran respecto a prácticas posteriores. Porque, mientras que en su entramado analítico a) él introdujo la noción de una "multitud de infinitos elementos", un objeto de estudio matemático en tanto que estaba determinado por una ley o un número finito de leyes, y, sobre todo, b) él asumió que las cantidades estaban determinadas por la ley de continuidad, c) él no dio el paso a considerar una cantidad como una multitud actual de infinitos elementos. De haberlo hecho, él habría considerado cantidades infinitas actuales como objetos de estudio analítico y las cantidades irracionales no habrían sido problemáticas. Aún más, incluso de haberlo hecho, todavía se hubiera requerido un paso más para que las cantidades ω de Bolzano fueran ε de Weierstrass, a saber, considerar algunos 'números' como multitudes de infinitos elementos. No obstante, lo uno y lo otro habrían contravenido no sólo su idea de un procedimiento matemático correcto, sino también sus nociones de cantidad y número.

Mientras que Weierstrass en 1865 enunció lo que hoy día se conoce como el teorema de Bolzano-Weierstrass en términos de un punto en –o en la frontera de– una parte limitada de un plano (cf. Weierstrass, 1865/66: 16[B]) y, posteriormente, en su formulación de ese teorema identificó "una cantidad variable ilimitada, que [formaba] un dominio simple" con su representación geométrica de una línea recta (Weierstrass, 1886: 60),[268] ellos no habrían sido

[268] Como antes se dijo, tanto Cantor como Dedekind enfatizaron su derecho a hacer tal cosa en sus respectivos trabajos de 1872 (cf. Cantor, 1872/1932: 97; Dedekind, 1872: 18-19).

procedimientos válidos para Bolzano, al menos de acuerdo a su propuesta matemática inicial. De hecho, uno de los pocos lugares en su trabajo de 1816 y en su *Prueba puramente analítica* en el cual él usó el término "punto" en tanto identificado con, diría la lectura tradicional, el valor de una cantidad real Weierstrassiana,[269] fue en el prefacio a ese último trabajo: uno de los tipos de pruebas más comunes del teorema del valor intermedio, dijo, dependía de una verdad geométrica, a saber, "que toda línea continua de curvatura simple cuyas ordenadas eran primero positivas [y] luego negativas (o viceversa), necesariamente deben intersectar la línea abscisa en algún punto que yace entre aquellas ordenadas" (Bolzano, 1817B: 6).[270] Desde la perspectiva de Bolzano, este era un procedimiento matemático inválido debido a la subordinación de la geometría al análisis. No obstante, ultimadamente esto también pone de manifiesto cuán diferentes eran las nociones y prácticas matemáticas de Bolzano de las nociones y prácticas matemáticas Weierstrassianas y aritmetizadoras posteriores.

Por último, pero no por ello menos importante, en la transición del enfoque cinético del siglo XVIII (en el cual las cantidades prevalecían) al punto de vista estático que caracterizaría al análisis 'aritmetizado' (por ejemplo, el de Weierstrass, Dedekind, Cantor y otros), uno de los puntos cruciales fue la *comprensión de que una caracterización conceptual clara de la continuidad* del dominio de los números reales era necesaria. El éxito en aritmetizar la continuidad de este dominio se convirtió en un elemento clave de dicho proyecto, quizás incluso su clímax, como lo expresó el título del libro de Dedekind de 1872, *Continuidad y números irracionales*. Por el contrario, era característico de las propuestas pre-Weierstrassianas, entre ellas la propuesta inicial de Bolzano, no contemplar aún dicho proyecto y al parecer no tener una noción clara de una "teoría del continuo" (esto es, topológica), ni una propuesta sobre cómo abordar los elementos de la topología (cf. Ferreirós, 2007: 137ss.), pese a las ideas pre-conjuntistas y pre-topológicas de Bolzano. Una diferencia que, precisamente, subraya la distancia entre la propuesta de Bolzano en 1817 y, por ejemplo, la de Cantor (quien usó puntos límite y conjuntos derivados) y la de Dedekind (quien empleó su principio de continuidad de cortaduras) en 1872.

[269] Otro lugar en esos trabajos en el que tal identificación puede ser encontrada es en el §13 de su *Prueba puramente analítica*, cuando ejemplificó su denominado teorema por medio de una hipérbola rectangular (cf. Bolzano, 1817B: 49-50).

[270] Bolzano escribió: "Eine jede continuirlicheLinie von einfacher Krümmung, deren Ordinaten erst positiv, dann negativ sind (oder umgekehrt), die Abscissenlinie nothwendig irgendwo in einem Puncte, der zwischen jenen Ordinaten liegt, durch schneiden müsse".

Y, sin embargo, una vez más, al mismo tiempo Bolzano se revela como un autor pre-Weierstrassiano importante y como una figura pionera, considerando, por ejemplo, el método de partición de intervalos que usó en su *Prueba puramente analítica* y que fue altamente valorado por Weierstrass. Una vez que se tornó claro el problema de caracterizar la continuidad de $\mathbb{R}$, como no lo estaba en los primeros trabajos de Bolzano, aquel método pudo ser transformado en el denominado principio de Bolzano-Weierstrass,[271] conforme al cual una secuencia infinita de intervalos cerrados anidados siempre tiene una intersección no vacía (es decir, al menos un punto pertenece a todo intervalo). Como paulatinamente se volvió más claro, aquel principio permitía una caracterización satisfactoria de la continuidad del sistema de los números reales, alternativa a la propiedad de cortadura propuesta por Dedekind en 1872. De esa manera, por ejemplo, en 1884 Cantor enfatizó la importancia de ese principio "verdaderamente antiguo" que, dijo, "difícilmente podía ser reemplazado por uno esencialmente diferente" (Cantor, 1884/1932: 212).[272] Para entonces, sin embargo, el contexto matemático germánico era ya muy diferente al de la *Prueba puramente analítica* de Bolzano, obra que a la postre sería tan ampliamente reconocida como Bolzano quería que lo fueran sus primeros trabajos (cf. Bolzano, 1817B: 27), si bien muchas veces interpretada de una manera que, conforme a esta tesis, parece ajena a lo que él mismo proponía.

[271] Esta expresión, usada en (Ferreirós, 2007: 139-141), es bastante común en la terminología matemática, si bien estrictamente hablando es anacrónica.

[272] Cantor escribió: "Ich bemerke, dass die hier angewandte Beweismethode, welche wohl schwerlich durch eine wesentlich andere ersetzt werden kann, ihrem Kerne nach sehr alt ist; in neuerer Zeit findet man sie unter anderem in gewissen zahlentheoretischen Untersuchungen bei Lagrange, Legendre und Dirichlet, in Cauchy's *Cours d'analyse* (Note troisième) und in einigen Abhandlungen von Weierstrass und Bolzano; es scheint mir daher nicht richtig, sie vorzugsweise oder aussechliesslich auf Bolzano zurückzuführen, wie solches in neuerer Zeit beliebt worden ist".

Annex A

Year	Author	Reference
1817	Bolzano	“there is a certain quantity U, which is the greatest of those for which it can be true that all smaller x possess property M.” (Bolzano, 1817B: 41)
1865	Weierstrass	“If, in a bounded part of the plane, there are infinitely many points with a given property, then there is at least one point (inside that part or on its boundary) such that in every neighborhood of this point there are infinitely many points having the given property.” (translation in (Moore, 2008: 222))
1870	Cantor	“Herr Kronecker befindet sich übrigens ebenfalls im Widerspruch mit dem Weierstrass-Bolzanoschen Satze von der unteren und oberen Gränze; es wird mich dies aber nicht aufhalten, meinen Beweis zu veröffentlichen, da ich diesen Satz nicht nur für richtig sondern für das Fundament der wichtigeren mathematischen Wahrheiten halte.” (Meschkowski and Nilson, 1991: 24)
	Schwarz	“der von Herrn Weierstraß in seinen Vorlesungen verfochtenen Meinung, daß man ohne die Schlußweise, welche von Herrn W. auf Bolzanoschen Principien weiter ausgebildet ist bei vielen Untersuchungen nicht zum Ziel gelangen könne.” (Meschkowski, 1967: 228)
1872	Cantor	“Unter einem „Grenzpunkt einer Punktmenge *P*“ verstehe ich einen Punkt der Geraden von solcher Lage, daß in jeder Umgebung desselben *unendlich* viele Punkte aus *P* sich befinden, wobei es vorkommen kann, daß er außerdem selbst zu der Menge gehört. Unter „Umgebung eines Punktes“ sei aber hier ein jedes Intervall verstanden, welches den Punkt *in seinem Innern* hat. Darnach ist es leicht zu beweisen, daß eine aus einer unendlichen Anzahl von Punkten bestehende [„beschränkte“] Punktmenge

		stets zum wenigsten *einen* Grenzpunkt hat." (Cantor, 1872/1932: 98)
	Schwarz	"mit Hülfe einer von *Bolzano* ersonnenen und von Herrn *Weierstrass* weiter entwickelten Schlussweise." (Schwarz, 1872: 221, fn.)
1876	Weierstrass	"Existiren nämlich für irgend eine eindeutige Function im Innern eines begrenzten Bereichs unendlich viele ausserwesentliche singuläre Stellen, so giebt es im Innern oder an der Grenze des Bereichs wenigstens eine stelle, welche sich dadurch auszeichnet, dass in jeder Umgebung derselben von ihr verschiedene singuläre Stellen vorhanden sind, und die deshalb nothwendig eine wesentliche singuläre Stelle für die Function ist." (Weierstrass, 1876/1895: 80)
1878	Dini	"*qualunque sia il gruppo di punti G che si considera, purchè contenga un numero infinito di punti, esisterà sempre almeno un punto-limite che potrà essere o nò uno dei punti punti del gruppo*." (Dini, 1878: 16)
1880	Pincherle	"Teorema. Se in una varietà ad una dimensione si hanno infiniti posti soddisfacenti ad una definizione comune si troverà in quella varietà per lo meno un posto avente la proprietà che in qualunque suo intorno, per piccolo che si voglia prendere, esisteranno sempre infiniti posti soddisfacenti a quella definizione. [...] Il teorema del no. precedente si può generalizzare estendendolo ad una varietà di *n* dimensioni." (Pincherle, 1880: 60, 64)
1881	Stolz	"Hr. H. Schwarz bezeichnet ihn als den Urheber einer von Hrn. Weierstrass weiter entwickelten Schlussweise [...]. Die Grösse *U* heisst nach Weierstrass die *obere Grenze* aller Werthe von *x*, denen die Eigenschaft *M* zukommt." (Stolz, 1881: 55 & 58)
1898	Schoenflies	"Für eine aus unbegrenzt vielen Punkten bestehende Menge *P* giebt es nach einem Satz von Bolzano-K. Weierstrass mindestens eine Häufungsstelle (Grenzpunkt, Verdichtungspunkt)."

		(Schoenflies, 1898: 185)
1914	Hausdorff	"(Satz von Bolzano-Weierstrass). Jede beschränke unendliche Menge reeller Zahlen ["eines euklidischen Raumes"] hat mindestens einen Häufungspunkt." (Hausdorff, 1914: 258, 329)
1968	Boyer	"This theorem [Bolzano-Weierstrass theorem] was proved by Bolzano and apparently was known also to Cauchy, but it was the work of Weierstrass that made it familiar to mathematicians." (Boyer, 1968: 605)
1996	Ewald	"This lemma (the greatest lower bound principle) is the first published version of the Bolzano-Weierstrass theorem." (Ewald, 1999: 226)
2004	Russ	"the main result in RB (an early form of Bolzano– Weierstrass theorem)." (Russ, 2004: 146; cf. Russ, 1980: 157)

Annex B

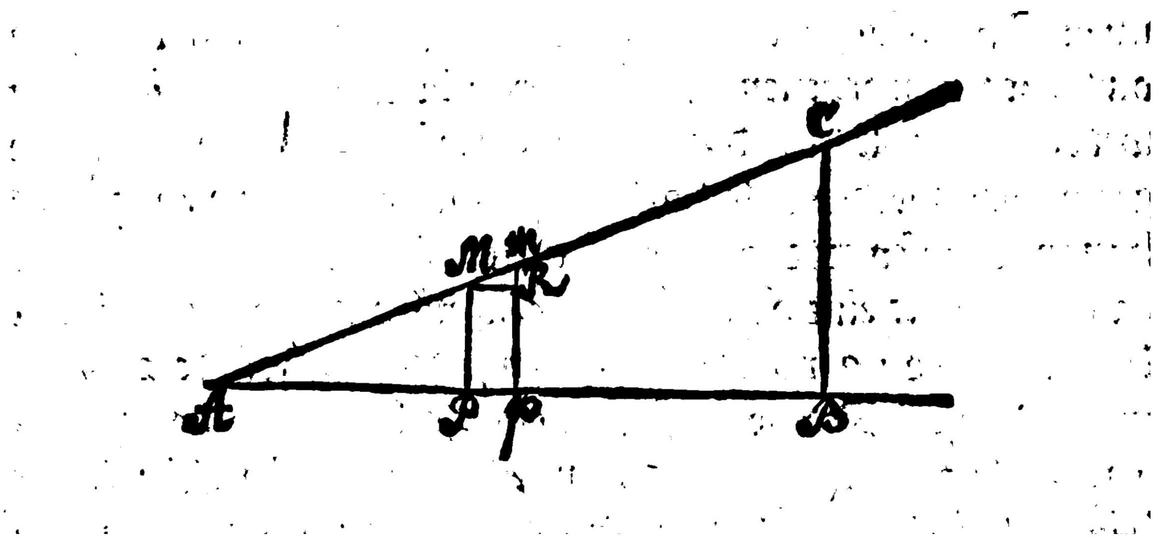
Year	Author	Reference
1900	David Hilbert	“The most suggestive and notable achievements of the last century in this field are, as it seems to me, the arithmetical formulation of the concept of the continuum in the works of Cauchy, Bolzano and Cantor, and the discovery of non-euclidean geometry.” (Hilbert, 1902: 445)
1926	Felix Klein	“Bolzano is one of the fathers of the current ‘arithmetization’ of our science.” (Klein, 1926: 56)
1956	Gottfried Martin	“Man kann den Unterschied zwischen Kant und Bolzano dahin charakterisieren, dass für Kant Kant die Axiomatisierung, und dass für Bolzano die Arithmetisierung das eigentliche Ziel gewesen ist. Man wird Felix Klein recht geben müssen, wenn er sagt: ‘Bolzano est einer der Väter der eigentlichen ‘Arithmetisierung’ unserer Wissenschaft’.” (Martin, 1956: 103)
1968	Carl B. Boyer	“for the rapid expansion of the theory of functions had been accompanied by the rigorous arithmetization of the subject from Bolzano to Weierstrass.” (Boyer, 1968: 643)
1981	Hans Niels Jahnke and Michael Otte	“Within the tendency to arithmetize mathematics, Bolzano has made an important contribution in his works concerning the quantity concept and function theory. [...] A particularly striking example is his [1817] proof of the intermediate value problem, as well as his realization that this is actually a theorem requiring proof. The necessity of proving this theorem becomes evident only from the point of vantage offered by the program of arithmetizing mathematics.” (Jahnke and Otte, 1981: 87)
1986	Pierre Dugac	“Bien que ce mémoire fût pratiquement ignoré des mathématiciens, avant la redécouverte de Bolzano, il constitue le premier pas décisif vers ce qu’on appellera plus tard

		l'arithmétisation de l'analyse." (Dugac, 1986: 242)
1991	Alberto Coffa	"As a result of Bolzano's [1817] proof, the central notions of the calculus were on their way to being 'arithmetized.' The arithmetization –or 'rigorization'– of the calculus would be completed in later years by Cauchy, Weierstrass, Cantor and Dedekind." (Coffa, 1991: 28)
1997	Michel Bourdeau	"Non content d'être ainsi un des pionniers de l'arithmétisation de l'analyse, Bolzano figure également sur l'arbre généalogique de chacune des deux écoles philosophiques qui ont dominé le vingtième siècle." (Bourdeau, 1997: 56)
2000	Paul Rusnock	"For Bolzano would not only sketch the general contours of the emerging new mathematics of the nineteenth century, but also carry the work out in considerable detail. His work in the foundations of real analysis attained results which still stand as models of rigor and sound method." (Ruscnock, 2000: 18)[273]
2006	Eckehart Köhler	"[If] Frege-Russell Logicism is rejected because of doubts about Type Theory (a variety of set theory), then the Bolzano-Weierstrass arithmetization of analysis should also be rejected for the same doubts." (Köhler, 2006: 93)
2008	Michael Detlefsen	"In the early years of the 19th century, Bolzano also articulated such an idea and applied it to the reformation of mathematics generally, and particularly to analysis. It comprised, indeed, a prime motive of his early attempts to 'arithmetize' analysis." (Detlefsen, 2008: 182)
2010	Jan Sebestik	"Two of the first Bolzano's publications have permanent interest: the *Rein analytischer Beweis* (1817) which inaugurates the arithmetization of analysis, and the *Beyträge zu einer begründeteren Darstellung der Mathematik* (1810). While the first one was noticed by Weierstrass, the second one, going against the spirit of the dominant Kantian and postkantian

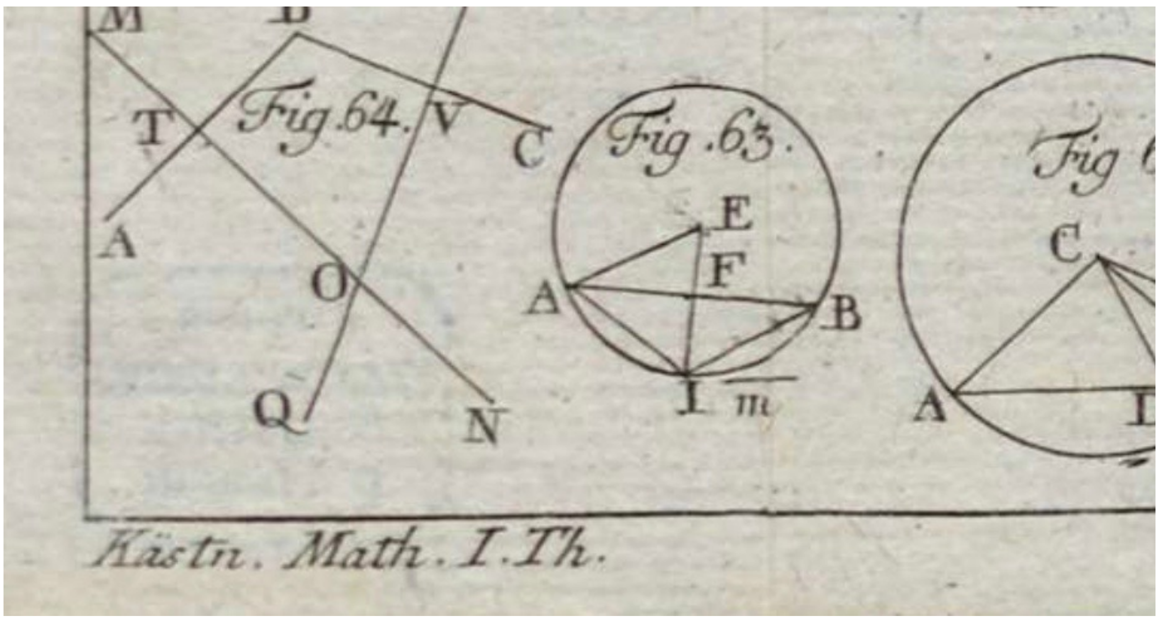
[273] Nevertheless, it must be said that, as for Rusnock's second assertion, he could have in mind Bolzano's later works when talking about "his work in the foundations of real analysis" (cf. Rusnock, 2000: 14).

		philosophy, was completely neglected." (Sebestik, 2010)
2012	Bedürftig & Murawski	"Wir unterstreichen, dass Bolzano in allen seinen Arbeiten in der Analysis als Fürsprecher der so genannten 'Arithmetisierung' der Analysis auftrat." (Bedürftig and Murawski, 2000: 66)
2016	Erich Reck	"Most directly, Dedekind's essay was tied to the arithmetization of analysis in the nineteenth century –pursued by Cauchy, Bolzano, Weierstrass, and others– which in turn was a reaction to tensions within the differential and integral calculus, introduced earlier by Newton, Leibniz, and their followers." (Reck, 2016)
2016	Michael N. Vrahatis	"Its first proofs ["Bolzano's theorem], given independently by Bolzano in 1817 and Cauchy in 1821, were crucial in the procedure of arithmetization of analysis (which was a research program in the foundations of mathematics during the second half of the 19th century)." (Vrahatis, 2016: 41)

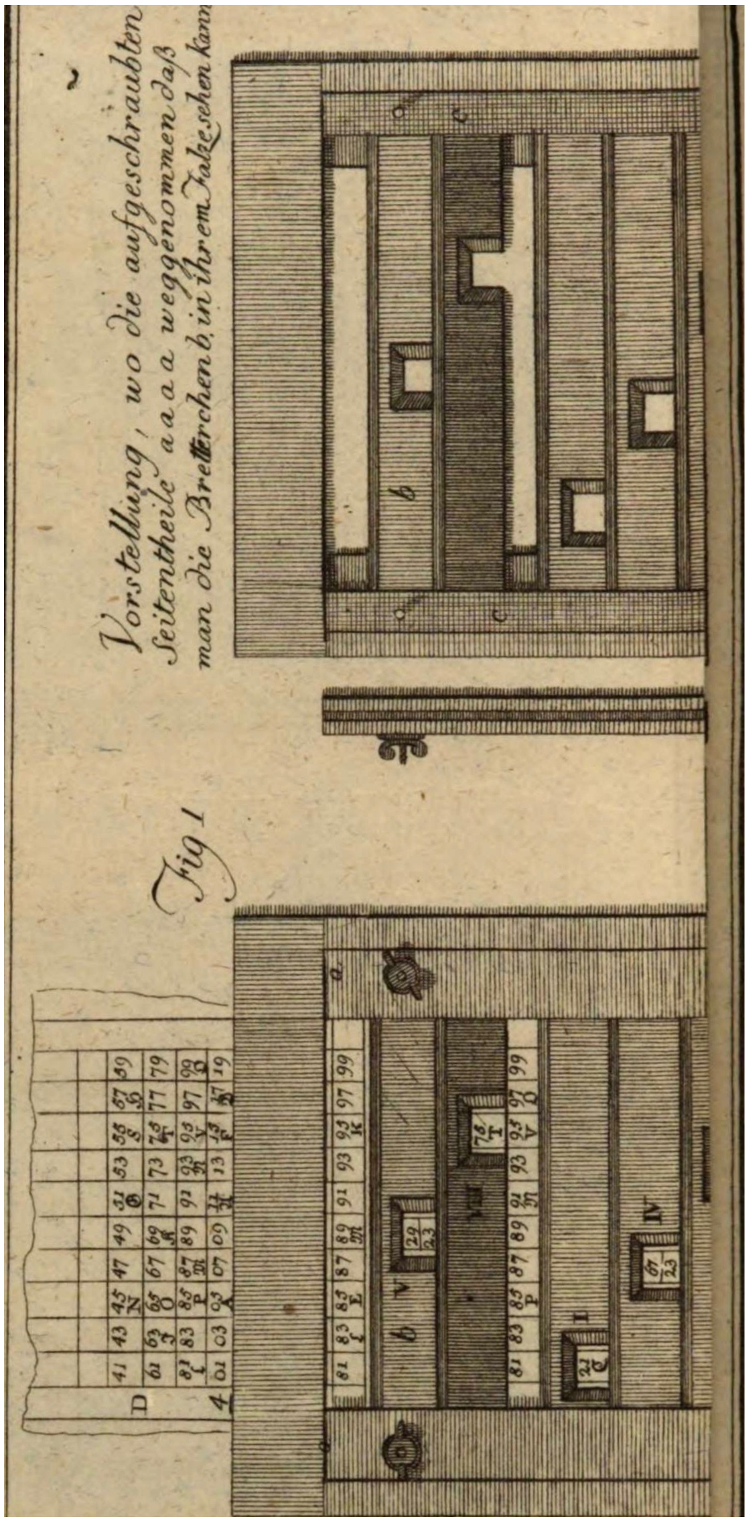
Annex C



Annex D



Annex E



Vorstellung, wo die aufgeschraubten Seitentheile a a a a weggenommen daß man die Brettrchen b in ihrem Falze sehen kann
Fig. 1

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