STUDI SU FELICE FONTANA

2

PETER K. KNOEFEL

FELICE FONTANA LIFE AND WORKS

TRENTO
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FELICE FONTANA

ENTI PROMOTORI

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To Franca

PRESENTAZIONE

Peter K. Knoefel, nato nell'Indiana il 4 agosto 1906, laureatosi in medicina alla Harvard University (Boston) nel 1931, è un autorevole professore emerito di Farmacologia, come attesta il suo invidiabile curriculum scientifico e dimostrano le sue significative ricerche soprattutto in campo tossicologico sperimentale.

Appunto dalla tossicologia nasce l'interessamento del Knoefel per la figura e l'opera di Felice Fontana. Pronubo fu il Traité sur le vénin de la vipère, *capolavoro tra i più significativi della tossicologia.*

Giunto a Firenze nel 1968, lo Knoefel, formatosi nella scienza anglosassone a sfondo neoempiristico, trovò nell'ambiente fiorentino lo stimolo irresistibile ad avvicinarsi alla cultura europea facente capo alla nostra tradizione umanistica. Di qui derivarono i suoi primi contatti con la storia della scienza nel Settecento.

«Visiting Professor» presso l'Istituto e Museo di Storia della Scienza di Firenze dal 1968, ha continuato a polarizzare le sue ricerche sulla figura e l'opera di Felice Fontana. Suo primo frutto è stata la Annotated Bibliography *(1980), apparsa come Numero 1 di questa collana, e che gli ha valso la prestigiosa nomina a membro della Roveretana Accademia degli Agiati.*

Qualche anno fa Peter Knoefel presentava a questo Comitato un voluminoso dattiloscritto contenente la bozza della ergobiografia di Felice Fontana, che viene ora pubblicata nel presente volume, la cui realizzazione è risultata in realtà più complessa di quanto si potesse supporre in un primo tempo ed ha presentato non lievi difficoltà. Comunque, la buona volontà e l'impegno posto hanno ancora una volta finito per trionfare, superando i vari ostacoli.

Finora i dati biografici reperibili sul Fontana sono relativamente scarsi e si rifanno sovente a note a sfondo divulgativo o a biografie alquanto incomplete, ancorché tuttora valide come quelle del Mangili e dell'Adami, redatte secondo determinati schemi tradizionali. Ben diverso si presenta invece il Fontana *di Knoefel, giustamente, dato che l'autore si sforza di inquadrarne la biografia e l'attività scientifica*

nell'ambito della felice collaborazione austro-toscana nel corso del Settecento.

Da questa ergobiografia traspare inoltre la particolare affezione e la particolare passione dell'Autore per il suo personaggio.

Sembra ormai giunto il momento di riscattare il Fontana – secondo soltanto allo Spallanzani – dalla minor fortuna che ha incontrato e incontra nell'arengo scientifico internazionale. La pubblicazione della presente opera nella madrelingua di Knoefel ha pertanto un duplice intento: quello di facilitare l'approccio dei potenziali lettori alla vita del Fontana, e quello di lanciare proprio da Rovereto e da Trento un messaggio su di lui che ovunque possa essere facilmente recepito. Ma pure un altro intento vi è stato: ed è stato quello di non attenuare attraverso una traduzione la peculiare vivacità, l'arguzia e il compiacimento per talune espressioni locali, anche ad impronta romantico-popolare, che appunto contraddistinguono la fatica di Knoefel, sollecitandone la lettura.

Nel congedare questo libro il Comitato confida che esso sia fonte di soddisfazione per gli Enti politico-amministrativi e per gli Enti sponsori che ne hanno assicurato la stampa, e di stimolo ad ulteriori approfondimenti e contributi sulla figura e l'opera di Felice Fontana.

IL COMITATO

PREFACE

For many years, as a pharmacologist I had known of the two volumes of Fontana, the Treatise on the Venom of the Viper, but I knew nothing else about him. When I came to live in Florence in 1968, I resolved to look further into the matter; I found more than I had anticipated. But how much I had to learn! About the Hapsburgs, the Hapsburg-Lorraine Grand Dukes of Tuscany, the Enlightenment, the Great Chain of Being, the invasion of Italy by the French, the Jacobins, the Viva Maria, *and much more. I learned of the great holdings in Florence of the Tuscan* Archivio di Stato, *the* Biblioteca Nazionale Centrale, *the* Marucelliana, *the* Riccardiana, *the* Biblioteca Medica *at Careggi. I was introduced by Laura Piccolomini, wife of Curzio Ugurgieri, author of the Life of Pio II, to Professoressa Maria Luisa Righini Bonelli, Director of the Florentine Institute and Museum of the History of Science, who gave me free access to the library and the records of its ancestor, the Royal Museum of Physics and Natural History. Also I visited the Museum* La Specola, *and was astonished by the anatomical models in wax.*

My search led me to libraries in other Italian cities: Milan, Bergamo, Venice, Pisa, Bologna, Forlì, Reggio Emilia, and Siena; also outside of Italy, to Basel, Geneva, Göttingen, Frankfurt a/M, and London. A great discovery was the Vallagarina, which has a magical attraction for me. At Rovereto I was helped much by Gina Adami, by Pio Chiusole, librarian of the Biblioteca Civica *with its many manuscripts, and by Ferrucio Trentini, President of the* Accademia Roveretana degli Agiati; *they also took me to Pomarolo. Then I went on to Trento, where I learned of the important holdings of the* Biblioteca Comunale, *with its Director Dottoressa Schlechter always being helpful.*

It became apparent that what was needed was a complete bibliography of Fontana, the existing ones being very poor. Accordingly, I wrote one, with help from many sources. When I was honored by membership in the Scientific Committee for the publication of works by and about Fontana, I submitted the bibliography for consideration of publication, which took place at Trent in 1980. From the articles I had written, I had not far to go to the Life and Works.

It has been a great experience, the search for Fontana in his time and place; eighteenth-century Italy. As I grew to appreciate his greatness, and his weakness, I could not help but feel some sympathy for him, deplorable at times though his behavior was. I ended by feeling myself to be spiritually a part of the Trentino, particularly the Vallagarina where was born in 1730 Felice Fontana, fated to be periodically forgotten and rediscovered.

The book is aimed at a wider audience than scholars and students of the period. As a colleague said of it: «he obviously wrote it for himself»; I feel there must be other ignoramuses like me, who might find Fontana's story of interest. Should someone say, there are lacunae here, I reply, remember that Voltaire wrote (more-or-less): «the surest way to boredom is to tell all».

I have given in the text, in English translation, some approximate titles of books not written in English; most inexcusably I have put them in italics. I hope that scholars will not reprove me too harshly for this lapse.

Many of the book-titles, as noted in the text, were taken from the 1807 catalog of the library of the Royal Museum of Florence. It is of the first importance to know, what were the books which Fontana knew.

Finally, O Reader, may you enjoy reading this book as I did writing it.

Vale

PETER K. KNOEFEL

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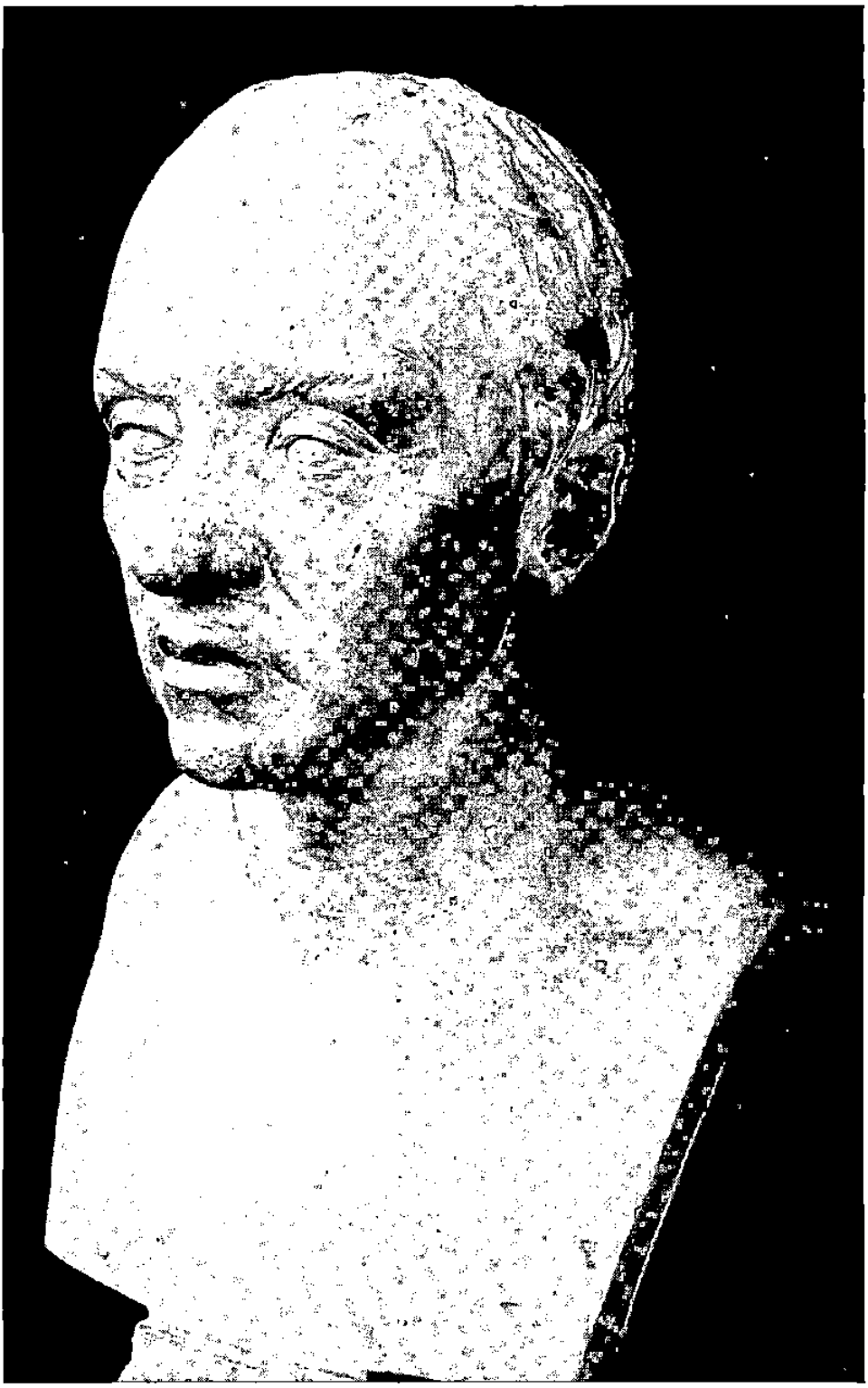

Felice Fontana. Bust in plaster at La Specola.

ARGUMENT

TWO CENTURIES OF COMMENT UPON FELICE FONTANA

Albrecht von Haller	1767	ingeniosus homo et accuratus
	1778	Ill viro cuyus summo ingenio nihil difficile est suo in vindicando vero sodali FELICI FONTANA grati animi et meritae venerationis ergo
Joseph De Lalande	1782	one of the most famous physicists, not only of Italy, but even of all Europe
Gustav of Sweden	1783	but you possess the Abbé Fontana, who is much greater than my Bergman
J.E. Dezemeiris	1834	one of the most capable experimental physiologists of the 18th century
A. Dechambre	1879	one of the most distinguished scientists of his time
Emile Dubois-Reymond (1818-1896)		shed the rays of science on all Europe
Max Neuburger	1905	one of the greatest anatomists and physiologists of the 18th century, also extremely eminent in physics and chemistry
Fielding H. Garrison	1935	pioneer in the investigation of serpent venoms... pioneer in histology... inventor of a great assembly of anatomical preparations... facile inventor of instruments... equally active as a botanist... made his mark in vegetable pathology... one of the most versatile biologists of his period

In spite of all these words of praise, Felice Fontana has largely been forgotten. Many know his treatise on the venom of the viper, some still visit the museum *La Specola* to see his anatomical models, a few praise his *Animal Physics,* but he is generally not appreciated for the many facets of his talent. To comprehend Fontana as a man and as a scientist, we must ask and answer some questions. In studying Nature, had he perception, or was it imagination? In his researches, was he pioneer or plagiarist? What were the contributions to Knowledge for which he deserves to be remembered? The nature of his character we must seek in his own writings and those of his friends and his foes.

PART I

LIFE

CHAPTER 1

FAMILY AFFAIRS, THE PLACE, THE PEOPLE

The Vallagarina - Rovereto - Pomarolo - Family Fontana

On 3 June 1730 an infant, the third son of Pietro and Elena Fontana, was baptized Gasparo Ferdinando Felice. We do not know the date of his birth, but it took place in the Casa Fontana, (Fig. 2) built by the father of Pietro, Giovanni Battista, in 1688 in the village of Pomarolo,[a] in the Vallagarina (Fig. 1), the narrow valley of the river Adige as it runs through the mountains, about 150 km south of the Brenner Pass, 20 km south of Trent, the chief city of the region known as the Trentino which lies between Italy and Austria.

Pietro Fontana (born in 1693, died between 1756 and 1777) had married in 1726 Elena Catterina Jenetti di Dambel (1704-1785), Dambel being a village in the Val di Non. She was one of sixteen children of Giovanni Pietro (born 1677), Noble Cavalier of the Holy Roman Empire, and the Noble Lady Anna Brigitta, born Groff.

The first Fontana child, a son, became a priest, dying at the age of twenty-eight. The second son, Giuseppe (1728-1788), became a much-loved physician in Rovereto, the chief town of the valley, 6 km across the river from Pomarolo. His mother thanked him in her will for having taken care of the other children after the death of their father, and many of Felice's letters were written to him. He died without issue and intestate, his estate being evaluated at 1,000 *fiorini*.[1]

The fourth son (1735-1803) was baptized Giovanni Battista Lorenzo but on becoming a priest took the name of Gregorio, by which he was

[a] This is not an uncommon sort of name; there are Pinerolo, Perarolo, and Ficarolo. Apple orchards (and vineyards) fill this intensively cultivated valley.

known subsequently. The five hundred *fiorini* allowed from his mother's dowry of 2,000 *fiorini* in order for him to enter the order of the *Scolopi* were inadequate; debts incurred by Gregorio were not settled until 1768 when Felice paid them, the first of his many beneficences to his family. Gregorio subsequently reached eminence as a mathematician, teaching at Rome, Senigallia, Bologna, Milan, and Pavia.

The fifth son, Bernardino, was born 3 January 1742. His mother spent[2] more than 300 *fiorini* to make him a cadet of a regiment of dragoons. In 1788[1] he was lieutenant in His Imperial Majesty's Guards, Vienna, but had continued to incur debts, paid by his mother[2] and by Felice, who wrote[3] from London in 1779 to his mother:

> I have written to Gregorio in Pavia in order to provide for Bernardino the post of surgeon of the regiment, which you tell me has been promised him for a long time. However, I do not believe in his sense of honor after he has dishonored himself in so many ways and deceived us so many times. Still I don't want it said that I have made him lose an opportunity. I have written to Giuseppe for the twenty *zecchini* that you ask for, which will come from Sig. Cousin Marzani, and which he will send you. As for the rest I don't know why you do not write about these needs to Gregorio, who has no pressing expenses and is much more shrewd than I. Take care not to be deceived by one who has deceived you so many times. With much affection and esteem.

Late in his life Felice wrote to Bernardino,[4] who was in trouble, attempting to advise him and discussing family financial affairs. Although Bernardino constantly sought financial help from his mother and brothers, shared in the inheritances from Giuseppe[1] and Gregorio,[5] and also took over all of Felice's estate in Florence,[6] when he died he was occupying a single room in the home of his cousins Marzani. An inventory[1] of his estate made after his death listed obligations to him of 2,000 *fiorini,* also a debt to him of 95 *zecchini,* 200 *scudi,* loaned by Felice to a colleague, Paolo Mascagni. His estate was left to his brother Francesco and family.

The sixth son, Francesco, was born 21 March 1747. After the death of his father he took up the study of surgery in which he was maintained by the family; he received in 1777 a donation of 650 *fiorini* from his mother.[2] Of his descendants we will hear again.

A daughter, Teresa, was born in 1751. She remained with her brother Giuseppe after the death of their mother, in 1803 was living in Milan. After the death of Felice (1805), reduced to living in misery, she committed suicide by throwing herself into the canal of the Naviglio, Milan.[7]

In her will,[2] Elena Fontana Jenetti testified to having had two other sons, Carlo and Francesc'Antonio, who went as soldiers to the Levant, after which no more was heard from them.

Was the family Fontana of the nobility? Many documents indicate that it was, but Felice seems not to have been so sure. One source[8] of information about the family identifies as its founder a Bartolomeo Fontana, a notary, who left Saone, a village in the Giudicarie mountains to the west of the Vallagarina to establish himself there by 1430. Another Bartolomeo, a descendant, had two sons, Sebastian who remained in Rovereto, and Stephen who moved to Pomarolo. Of the descendants of Stephen was one who married into the family Marzani of which we hear again. From another came a branch which included the Giovanni Battista who built the house in Pomarolo.

Another source of information on the family Fontana of Pomarolo is some manuscript notes[1] which remark that at the end of the fourteenth century very few families had surnames. To distinguish them it was common to add the name of habitation or occupation. These notes then mention two legal instruments of 16 November 1478 made at Castelbarco (there was a castle Castelbarco above Pomarolo) which name as witness Joannes a Fonte di Pomarolo, called in Italian Giovanni della Fontana, who could be the head of the family Fontana. These two sources of information are not necessarily in disagreement.

Festi[8] describes three coats of arms of the Noble Family De Fontana: an ancient one, one conferred by the Holy Roman Emperor Mattia in 1616, and one conferred on Felice by the Holy Roman Emperor Joseph II.

The Vallagarina had seen many invaders pour through the Brenner Pass: the Cimbri of 101 B.C. were followed by the Visigoths, the Ostrogoths, the Longobardi, and the Franks. By the 12th century there had begun a reversal of the flight of the inhabitants of the valley, which had left it deserted except for such spots as Lagaro, a fortified stronghold since Roman times. Now they returned to establish settlements such as Pomarolo, and a new political entity, the *Comun Comunale,* an agricultural community in which the produce was held for the common good.

However, these inhabitants found a new servitude when Lothair, King of Germany and Holy Roman Emperor, in 1132-6 devastated Lagaro and left it in charge of his follower Engilbert of Chostelwarch, who founded the family of Castelbarco which rapidly acquired many holdings in the valley.

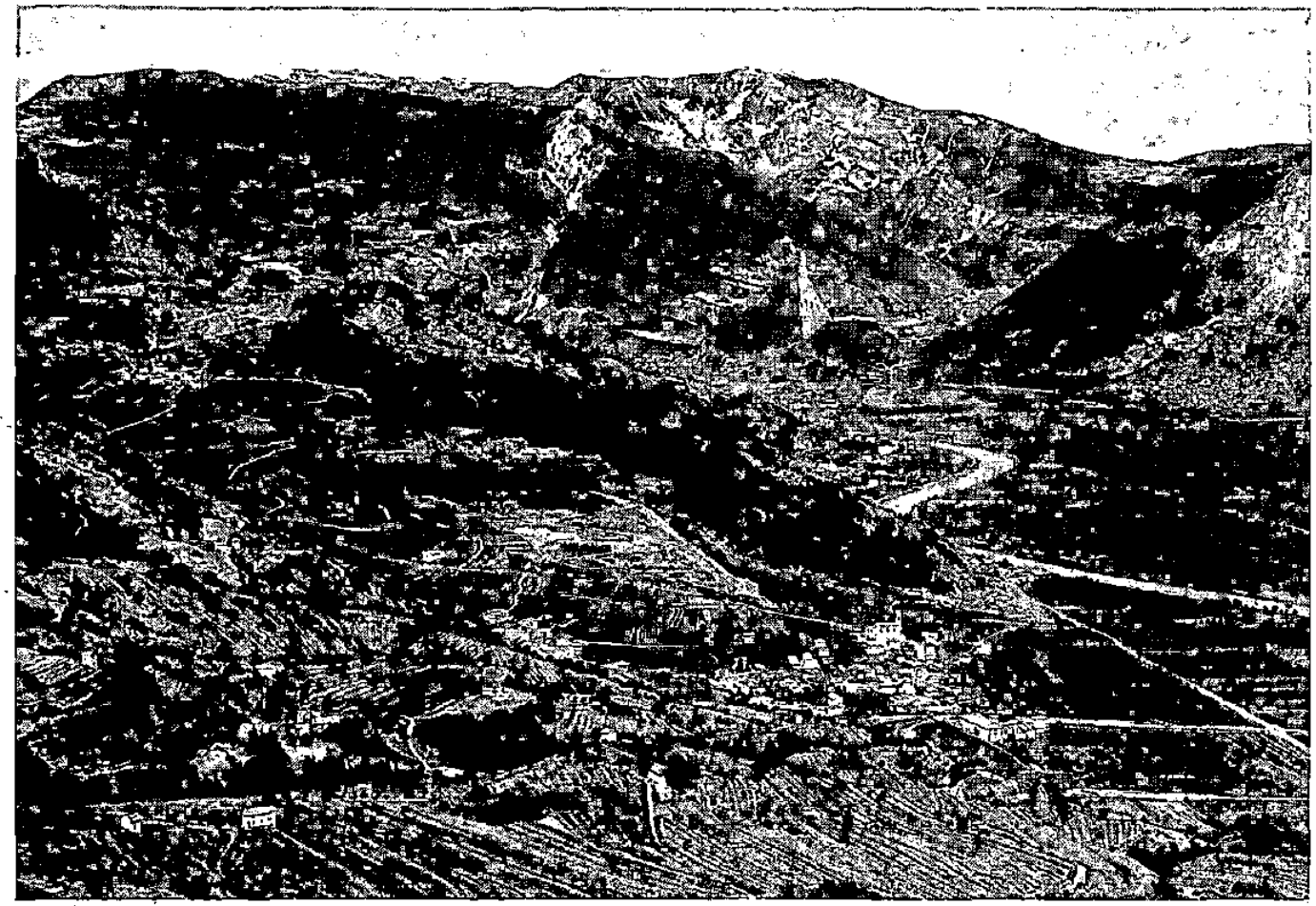

Fig. 1 – The Vallagarina. The village of Pomarolo and its church with Ghibelline tower are seen at the center of the photograph.

Fig. 2 – The Casa Fontana in Pomarolo.

Perhaps from breathing the air of the Vallagarina, they also acquired some independence of the two neighboring powers, namely the Republic of Venice, and the Prince-Bishops of Trent. The latter, with few exceptions of German origin, had been established in 1027 and by the 13th century had acquired by purchase dominion over much of the valley, which they kept until they fled before Napoleon in 1796.

By the end of the 14th century, however, a large part of the land was still the property of the *Signori* but was worked by freemen under schemes of rental or profit-sharing, protected by a civil government with statutes deriving from the 11th, 12th, and 13th centuries. Many Venetian families had moved into the Vallagarina, and it began to flourish as urban centers developed. Among these was Rovereto, which had been ceded to Venice in 1416. When war broke out between Venice and Trent in 1487, the Vallagarina was yielded by Venice to the Emperor Maximilian I of Habsburg (1459-1519). Venice was eventually and decisively defeated by Maximilian and his allies of the League of Cambrai at the battle of the Ghiara d'Adda, 4 May 1509, and on 19 October of that year Maximilian received the sworn fidelity of the Trentini. With his troops he left the Vallagarina in November, after confirming the privileges and statutes of the Roveretani and their district, covering the liberty of person, property, commerce, and the maintenance of public order.

In 1511 sericulture was introduced, and Rovereto, which was now the capital of the valley, had, by 1766, 5,319 inhabitants, 1300 of whom were employed in the industry, which shipped 117,334 pounds of silk in 1722. Politically the valley survived the passage of French troops in the War of the Spanish Succession (1701-1714), and was untouched by the Wars of the Polish Succession (1733-1735) and the Austrian Succession (1740-1748).

In 1740, Pietro Fontana, who had become a lawyer and Imperial Notary, moved his family from Pomarolo to Nogaredo or to Villa Lagarina (accounts differ), neighboring villages, for improvement of employment in his profession; in 1749 they moved to Rovereto. During his lifetime he provided for the education of his sons as best he could. On his death, his son Giuseppe became the head of the family, but it is clear that the main financial support was to come from Felice, the chief wage-earner. Although generous to his family and to many others, with the independence of character which he imbibed from the air of the valley, and manifested throughout his life, Felice Fontana came to be known as a *duro,* because of the inflexibility of his nature and the demands he made upon others.

CHAPTER 2

EDUCATION

Rovereto: Tartarotti - Parma: Belgrado - Padua: Morgagni - Bologna: Caldani - Haller

During the seventeenth century cultural life in the Trentino was somnolent.[1] Not only had the connection of the Vallagarina with Venice been broken, but any Italian influence was minor. There was no Hapsburg Grand Duke closely allied with the region. The prince-bishops of Trent had become an undistinguished lot. With the Counter-Reformation, the schools in the hands of the Company of Jesus were limited to secondary education; attempts to found a *ginnasio,* a *studium generale,* a university, had failed. The *Accademia degli Accesi* («the ignited»), founded in Trent in 1629, dying and reviving, restricted its activities to panegyrics of princes, bishops, and the saints in the worst of taste.

Towards the middle of the eighteenth century there began, not under the heavy hand of the church in Trent but in Rovereto, a reflowering of cultural life which is well exemplified by the person of Girolamo Tartarotti, who was born in Rovereto in 1706 and died there in 1761.[2] After studying at Padua, he became secretary to Cardinal Passionei;[a] later as a friend of Foscarini[b] he accompanied him as part of

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[a] Domenico Passionei (1682-1761). A love of books and culture characterized his entire life. After being Papal nuncio to Switzerland (1721) and Vienna (1730) he became prefect of the Vatican Library, and after 1738 Cardinal. Sympathetic with Jansenism and also the «philosophes», he had a liberal conception of Catholicism. He was an assiduous reander of Pascal and Quesnel, a correspondent of Montesquieu, Voltaire, and Rousseau, and had acquired 28,000 books at the time of his death.

[b] Marco Foscarini (1695-1763). Erudite historian, diplomat, and politician. In 1762 he became the fourth-from-the-last Doge of Venice.

the legation to Turin (1741-1742), then returned to Rovereto where he opened a private school in which Felice Fontana as a child was a pupil.

The figure of Tartarotti shows particularly Italian features of the eighteenth century movement known as the Enlightenment.[c] He began his literary career with a vigorous attack on scholasticism, which he felt contained something sinister and unhealthy. His *Idea of Logic, Scholastic and Modern,* published in 1731, «was an impassioned defence of modern philosophy against all the deep corruption of morality and logic which were still being disseminated by traditional philosophy, thus cutting off at the source that passionate love of truth which was the only means of defence against the powerful enchantments of scholasticism». In 1740 he published a *Dissertation on critical methods;* now he turned these against a field of superstition, to the end that his threevolume work, *On the nocturnal gathering of witches,* was printed in 1749. His studies had revealed «a whole wretched, gloomy world, so far removed from civilised society that it remained isolated and alone even in its dreams and phantasies [...] even more horrible was the persecution carried out by educated, civilised people on poor, ignorant wretches». He showed «that witchcraft did not square with the physics of his age nor with the conception men of his time had of good and evil».

However, while «the reality of witchcraft could be denied without touching on the theological problem of the devil, the same could not be said about magic». Here Tartarotti «found himself face to face with a fascinating, half-forgotten world», that of Ficino, Paracelsus, Cardano. How could its existence be denied? «Right to the end he upheld his thesis according to which magic ought to be considered a phenomenon related to philosophy and religion». He was obviously concerned to keep within the bounds of Catholic orthodoxy, and «we can see how he was stopping just at the edge of the Enlightenment but refusing to take that final step which would lead to a completely practical and rational concept of human society».

We shall see how Fontana later was to be critical of the beliefs of the past, but often stopped short of pursuing what he saw lying before him.

Another well-remembered master of Felice Fontana in Rovereto, Giambattista Graser[3] (1718-1786) who taught rhetoric at the *Ginnasio,*

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[c] The following quotations are taken from an essay[37] by Franco Venturi. See also G. Sarton.[38]

was a founding member of the *Accademia degli Agiati*. This Academy with its motto *festina lente* – its name *Agiati* is not readily translated – was founded in 1750, and was recognized by Maria Theresa in 1753.

Felice went from Rovereto to Verona, probably in 1748-49, as did many youths of the Trentino who preferred it to the alternatives of Innsbruck or Salzburg. From there he moved to Parma, probably in 1749-50, where he studied with Jacopo Belgrado.

Born in Udine in 1704, Belgrado entered the novitiate of the Company of Jesus in 1724, and taught literature at Venice; after ordination as a priest he taught the sciences at Parma from 1738 to 1763. Later rector of the College of S. Lucia in Bologna, he died in Udine in 1789. After the suppression of the Jesuit order in 1773, being in trouble he went to the Grand Ducal Court in Florence, his presence there being regarded as sufficiently important to be recorded by Horace Mann in a letter to Horace Walpole.[4] Belgrado was a member of numerous academies, published dissertations on physics, and installed an astronomical telescope at Parma. His work of greatest interest to us was on what now would be called psychophysics.[5] The conclusion seems inevitable that Felice Fontana must have been greatly influenced by Belgrado to direct his attention to animal physics.

Felice went next, probably in 1750-52, to Padua. The reigning figure there was «His Anatomical Majesty», Giambattista Morgagni. Born in Forlì in 1682, he obtained a position at Padua where he gave his first lecture in March 1712. In October 1715, on the basis of his *Adversaria anatomica* (1706) he succeeded Vesalius, Columbus, Fallopius, and Fabricius to the chair of anatomy, which he held until 1771. In 1712 he had published his *Nova institutionum medicarum idea* which put medicine on a course in which the observation of even a small fact had more value than the most attractive philosophical abstraction. This attitude will be recognized as characteristic of Felice Fontana, who remarked:

> A single experiment, one lonely observation, has often reduced to vapor whole libraries of [...] philosophical romances.

Here Fontana acquired also a knowledge of anatomy long before he came to Florence. His name is found in none of the records of the University,[6] so he was an «unofficial» student, picking and choosing what he wished to learn. His support at Padua must have come from his father; when this ran out, be returned to Rovereto. It was at this time that he worked

closely with Tartarotti, who recognized Felice's growing talent and influenced the formation of his mind. Fontana early showed an interest in natural history, which Tartarotti must have shared; he held a diploma in anatomy from Morgagni, granted to Tartarotti as a philosopher and doctor of medicine.[7]

When Fontana later left Rovereto to go to Bologna, Tartarotti expressed his great fondness for him in a letter[8] to a friend, 24 November 1755:

> My soul is embittered by the departure of my Felice Fontana, who goes to Bologna with young Partini. I have lost much; for the help he gave me in my studies I would not know where to look. He became enamored of experimental philosophy, the reason for his abandoning his country

Tartarotti later furthered Fontana's career when, through his influence with Carlo Firmian,[d] Fontana was brought to the notice of the Court in Vienna, which resulted in his appointment as professor at the university in Pisa. Nor did Fontana forget his teacher. We have three letters to him,[8] two from Bologna in 1756-57, one later from Florence in 1760, mentioning commissions from Tartarotti, with the statement: «Only death could make me lose my memory of the favors received from you, or make my devotion to you less».
The next year, from Pisa, 6 May 1761, Felice wrote[9] to his brother Giuseppe that he had spoken of Tartarotti's illness to Dr. Giovanni

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d Carlo Firmian, count (1718-1782) After studying in Trent, Innsbruck, Salzburg, and Leiden, he went to Paris where he met the «philosophes» from whom he acquired his sympathy with the liberal outlook which characterized his politics, also a little Jansenism. He returned to Salzburg where he held an academy in his home. When Francis Stephen of Lorraine ascended the throne in Vienna, Firmian was sent as minister plenipotentiary to Naples where he arranged the marriage of Ferdinand of Bourbon to Maria Carolina of Hapsburg-Lorraine, and acquired the confidence of Kaunitz, adviser to Francis Stephen and Maria Theresa. Briefly ambassador in Rome, Firmian then spent twenty-three years in Milan as minister for Lombardy. Although not a maker of Vienna's foreign policy, which was largely in the hands of Kaunitz, and being more of a maecenas than a politician, by his choice of men of merit he created an atmosphere in Milan of freedom of speaking and writing which was conducive to innovation and reform. Voltaire wrote of him to Pietro Verri: «I see by your book that the Milanese have a new face. Only one minister is needed to change a whole country. You have near you a great man [Firmian] worthy of your support».

Targioni Tozzetti, famous Florentine physician, who had prescribed a regime which should reduce Tartarotti's pain.

During this period in Rovereto, Felice read several communications to the *Accademia degli Agiati,* of which he became a member in 1753 with the academic name of Celino. On 27 December 1753 he spoke on the lanterns of the ancients, claimed to be inextinguishable.[10] The second and third parts of this discourse[11] were not given until 1755. He is said to have spoken also on gangrene and sloughing of animal tissues after freezing and thawing, and the density of gases in liquids, particularly human blood; these manuscripts have not been found. On 30 August 1754 he delivered a dissertation[10] on gunpowder, attributing its invention to Roger Bacon.

In late 1755 Fontana secured a position which he was to hold for ten years. This was as tutor to a youth, Melchiorre Partini, actually only four months younger than Felice. Melchiorre[12] was the nephew of Gian Carlo Partini (1705-65), the most illustrious member of a family eminent in Rovereto since 1444. Distinguished in his military career, Gian Carlo had adopted his nephew on the early death (1753) of the latter's father. Being of poor health, Melchiorre made his will on 23 November 1755 before departing with Felice to continue his studies in Bologna.[13, 14] Although famous for its *studium generale*[e] since the twelfth century, by the seventeenth century the foreign professors had vanished; all were local products. When reform was proposed by the Archdeacon Felice Marsili in 1689, it failed. In 1709, his brother, General Luigi Ferdinando,[f] to whom the defence of the State had been confided, made very radical proposals, including adequate salaries for professors so that there would be no need to count on their being provided by the professions such as law and medicine. Also, several chairs in the sciences were to be created. Fearing that hope to overcome the ancient system would be in vain, Marsili, with his own funds but with the support of Pope Clement XI, founded the Institute.

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[e] *Studium generale* is the ancient name of a University of Studies. In the beginning the term *Universitas magistrorum et auditorum* indicated the association or corporation of professors and students.

[f] Born in 1658 in Bologna, student at Padua and Bologna, he wrote the first scientific treatise on oceanography, *Historie Physique de la Mer.*

The Istituto Marsiliano delle Scienze was inaugurated on 13 March 1714. By 1730 it had become the center of bolognese scientific life and one of the more celebrated centers of scientific research in Italy. Its constitution stated that professors were not to give lectures but were to occupy themselves in the practice of observation and experimentation. It contained sections in astronomy, mechanics, experimental physics, natural history, and chemistry. Later others, on military architecture, anatomy, surgery, obstetrics, optics, and antiquities, were added. It was open to all who wished to learn, citizens and foreigners.

One of these foreigners was Felice Fontana. On his arrival in Bologna, one of his first acts was to write to Tartarotti and to send him some books. Four months later he wrote again.[8] Felice made no mention of his work in letters to Rovereto until 1757, when he wrote[15] that his principal study was algebra, which went easily since he had known those ABCs since his youth. Fontana's first mention of Leopoldo Marc'Antonio Caldani, who was to be his teacher, colleague, and friend, was in September 1756[9] by which time they were at work together on experiments investigating Hallerian irritability, which had become a matter of great interest and controversy in Italy; as Felice wrote later:[16]

> *Hallerian irritability* from its beginning caused many disagreements among dissenting investigators of Nature throughout Europe, and in Italy particularly such feeling and emotion that it sometimes appears to be [...] an *irritation to all Italy*.

The extent of this reaction may be judged irom the collections of Petrini[17] and Fabri[18] with their 1,345 pages of essays, which constitute only a part of the published writings on the subject. This term of *irritability* had been introduced in 1677 by Francis Glisson, who described it as a property of all human tissues. Purely metaphysical in nature, the concept had no influence on physiology until resurrected by Haller to define the property of tissues to react to a stimulus. His definitions were:[19]

> I call that part of the human body irritable, which becomes shorter on being touched; very irritable if it contracts on slight touch, and on the contrary if by a violent touch it contracts but little. I call that a sensible part of the human body, which on being touched transmits the impression of it to the soul; and in brutes, in whom the existence of a soul is not so clear, I call those parts sensible, the irritation of which occasions evident signs of pain and disquiet in the animal.

Caldani and Fontana together entered this controversy, the latter in the role of pupil, as is made clear by the Caldani-Haller correspondence;[20]

Felice wrote:[21] «I have often seen Nature speak under the hands of M. Caldani».

Fontana's part in these experiments is mentioned by Caldani in his communications to the *Accademia dell'Istituto delle Scienze* in October and November, 1756;[18] he wrote to Haller of his new colleague as «the Academician from Rovereto» on 29 February 1757;[20] in May of that year he promised to send news of their latest researches. By 17 October 1758 Haller was still hoping for communication from the two. On 13 January 1759 he received Caldani's epistle, and a month later that of Fontana, which had been transcribed by Caldani himself. Haller translated that of Fontana into French, shortened them both a bit, and printed them together in the third volume of his Mémoires (1760), which piqued[g] Caldani some what.[20] Haller had both manuscripts bound; they are now to be found in the *Biblioteca Braidense,* Milan.[22] This first labor of Fontana fixed him securely in the mind of Haller as «ingeniosus homo et accuratus»,[23] and began to establish his European reputation.

Fontana had been carrying out experiments of a different nature, but still relating to Haller's concepts. These concerned the reaction of the iris to light, as noted by changes in the size of the pupil. He demonstrated some of his findings to Giuseppe Veratti (1738-1793), professor of medicine in the *Studio,* and his wife the renowned Laura Bassi (1711-1798), who had been appointed Reader in Universal Philosophy in 1732, but was forbidden to lecture because of her sex. Instead she held classes in her home. With them Fontana continued to correspond after his departure from Bologna.[24]

After waiting in vain for Caldani to return from Venice to Bologna, Fontana went to Tuscany in the summer of 1758 expecting to study mathematics with Paolo Frisi, but apparently this expectation was not realized. Most of the year 1759 was spent in Pisa[9] still in tutoring

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g Not only did Caldani complain of the slowness of publication of his Epistle, in his letter to Haller of 28 May 1759, but also of the poor translation. He begged Haller to improve it, so as to give less chance to the adversaries for criticism. Haller replied, 29 June, attempting to mollify Caldani but the latter in his *Riflessioni fisiologiche* of 1767 (Venice, Pasquali) directed against the writings of Le Cat, used even stronger terms to complain of omissions, additions, and poor translation not only of his Epistle but also Fontana's. Fontana himself, in his *Fisica Animale* of 1775, complained that the MS of his *Dissertation epistolaire* sent to Haller by a friend (he does not say Caldani) was mutilated and badly copied.

Melchiorre Partini,[h] with whom he remained on good terms, although there had been an incident of injured feelings apparently arising from motherly preoccupation on the part of Signora Partini.[25] Fontana did not take a degree in Bologna; his name does not appear in a list of laureates.[26] It seems that in later life he may have given some to understand otherwise, as the *Memoria* of Givanni di Pedemonte of 1805, and the brief eulogy of 1805,[27] but not the eulogies of Sarchiani[28] and Mangili,[29] state that he was laureate of Bologna; the «autobiography» of Pietro Ferroni[30] speaks of: «Felice Fontana [...] native of Rovereto, recently come out with the double crown of the Studio of Padua and that of [...] Bologna». In November 1759 Fontana was in Rome[36] with his brother Gregorio at the *Collegio Nazareno* but later he was back in Pisa where he remained until May 1765, although many letters[36] during these years were written from Florence where he spent months at a time. In July 1760 he had written to Tartarotti:[8] «I breathe here under this Tuscan sky as if revived from my hypochondria, having left the tepid climate of Rome which is more fit for numbing the spirits». He had found in Pisa a sympathetic ambience, that of the University.

At the beginning of the eighteenth century the University of Pisa was neglected, but the Grand Duke Francis Stephen instituted a program of activity and progress. The library was enriched and opened to the public, the astronomical observatory was improved, as were the botanical garden, the anatomical theatre, and the museum of natural history; institutes of physics and chemistry were created. The professors mentioned by Fontana in his letters[24] were Fromond,[i] Frisi,[j] Perelli,[k] and particularly Carlo Guadagni, who first taught experimental physics at Pisa, and who received Fontana into his laboratory.

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[h] The presence in Pisa of Melchiorre Partini is shown by a letter[24] of 25 March 1759. Perini[12] wrote that Melchiorre died in Bologna. That may be, but it seems more likely to be an assumption, based on his belief that Melchiorre went to Bologna in 1755 and never returned to Rovereto.

[i] Claudio Fromond (1703-1765) professed logic (1738) and experimental chemistry (1757). He published works on optics and physics, the latter including the nature of death by suffocation and drowning.

[j] Paolo Frisi (1728-1784) professor of mathematics and ethics, later of arithmetic and algebra, was at Pisa 1755-1764. His great work was the *Cosmografia;* he also wrote eulogies of Galileo, Newton, and D'Alembert.

[k] Tommaso Perelli (1704-1783) Professor of Astronomy, in charge of land restoration, he left few writings.

Fontana had been welcomed in Pisa, probably because his *Dissertation epistolaire* had now appeared in Haller's *Mémoires*. Haller wrote to Caldani[31] that for the two articles, Caldani's and Fontana's, they could expect to receive one golden ducat per page, or copies of his *Icones anatomicae*. Caldani chose the latter, along with thirty copies of his article and ten of Fontana's. Writing[9] to Giuseppe on 20 February 1760, Felice said that he had asked Caldani to send some copies, and that the public authorities in Bologna had averred that no better work had appeared in the past thirty years.

Fontana now[24] returned to an investigation, which he had begun in Bologna, on the movements of the iris. Where he performed these experiments in Pisa is unknown; from their nature many of them may have been in his own home. In a letter of 6 June 1759 to his brother Giuseppe he wrote:[9]

> I hope that what I have written on the eyes may be much more successful,[1] both because it is entirely new and full of incredible discoveries and because it contains an infinity of observations and experiments, as well as for the reason that it [sic] is entirely logical and as systematic as any other physiological part, even the most obvious and demonstrable. [...] Padre Tosetti, as he has written me, has committed me to printing it in Rome, and in Rome it will be printed in October.

This may have been the manuscript that Fontana sent to Haller, which Haller wrote about to Caldani.[32] In fact, this work was not printed until 1765, in Pisa, with a dedication to Carlo Firmian dated 10 May 1765.

Two entirely different matters were his study of the red blood cells, his first recorded use of microscopy,[9,33] and the study of the venom of the viper, begun in 1764.[34] He also was composing some physico-mathematic dissertations including the analytical form of calculus, as well as works[9,33] on the hymen, on icterus, on the ear, the external and internal movements of the eye, and – a popular subject of the time – the sterility of the mule. These, possibly unfinished, are lost, although some fragments exist in the *manoscritti Palatini*.[33]

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[1] Earlier in this letter, he told of discussions with Haller about the language in what became his *Dissertation epistolaire* should be printed.

CHAPTER 3

THE HOUSE OF HAPSBURG, THE ENLIGHTENMENT

Hapsburgs - Hapsburg-Lorraine - Bayle - Locke - Galileo - Bacon - Boyle - Newton - Fontenelle - Voltaire - Kant

In October 1740 Charles VI, Wearer of the crowns of St. Leopold (Austria), St. Wenceslas (Bohemia), and St. Stephen (Hungary), and Holy Roman Emperor, ate a dish of mushrooms which occasioned an indigestion followed by apoplexy and death of the last male member of the direct line of the «Old Hapsburgs».[1] As the son of Leopold I (ruled 1690-1705) Charles had succeeded his older brother Joseph I as Emperor in 1711. In 1713, being without issue but desirous of preventing a struggle for the succession and ensuring the integrity of the Hapsburg dominions, he issued a *pragmatic sanction.* Altering the laws of succession, this declared as legally and politically binding on the German Hapsburg line the principle of primogeniture, here the right of sole inheritance by the eldest son, or if no sons existed, by the eldest daughter.

She was the Archduchess Maria Theresa, born in 1717, who had made a love match with Francis Stephen, Duke of Lorraine (1708-1765). For their marriage (1736) Francis Stephen had had to cede his duchy to Stanislaus I of Poland, for which in exchange he received that of Tuscany, which had been without a ruler since the death on 7 July 1737 of Gian Gastone, «the last of the Medici».

Charles had spent the rest of his life in attempts to obtain adherence to his Sanction by appeals to other European sovereigns, to the Estates of his various realms, and the Diet of the Holy Roman Empire. Largely this was in vain, as Maria Theresa on her accession in 1740 had to defend her right in the bitter struggle known as the *War of the Austrian Succession.*

In 1745 Francis Stephen was elected Holy Roman Emperor, in 1748 the Pragmatic Sanction was confirmed and the War of the Austrian Succession was ended. The House of Hapsburg-Lorraine had been founded with the birth of Joseph in 1741; the sixteen children of Francis Stephen and Maria Theresa continued the policy of the Emperor Maximilian: «let others wage war; thou fortunate Austria, marry», as is supposed to have been said by Matthias Corvinus (d. 1490), King of Hungary. But before we come to their story, we must consider the intellectual climate of their time, the phenomenon known as **THE ENLIGHTENMENT.** In December 1783 a German author asked, «Was ist Aufklärung»? (What is Enlightenment?). A year later appeared an answer, suitably enough by Immanuel Kant (1724-1804), since he, if anyone could be singled out, embodies the Enlightenment. His answer was:[2]

> Enlightenment is man's emergence from his self-imposed nonage. Nonage is the inability to use one's own understanding without another's guidance. This nonage is self-imposed if its cause lies not in lack of understanding but in indecision and lack of courage to use one's own mind without another's guidance.
> *Sapere aude:* Having the courage to use your own understanding is therefore the motto of the Enlightenment.

The first effort of the Enlightenment was to criticise. As Diderot (1713-1784) wrote in the *Encyclopédie,* «Everything must be examined, everything must be shaken up, without exception and without circumspection».[3] Kant insisted that the eighteenth century was «the age of criticism», the highest praise he could give. It is widely agreed that the representative figure of the first phase of the Enlightenment is Pierre Bayle (1647-1706), whose major enterprise was his *Historical and Critical Dictionary* (1697), in which he maintained that not doubt but dogma is the worst foe of knowledge. Bayle's greatest positive contribution was to separate morality from religion by holding that lack of religious belief does not necessarily lead to bad conduct, that conduct does not depend on metaphysical belief.

As Pierre Bayle had found safety for freedom of thought in Holland (1681), so also (1683-1689) did John Locke (1632-1704). While there he completed his *Essay on Human Understanding* to which we shall return. Again in England Locke argued for religious freedom in three essays on toleration. In his *On the Reasonableness of Christianity* he emphasized, against dogma, the ethical aspect of religion; this religious rationalism later crystallized into Deism.

The negative side of Deism is shown in the book *Christianity Not Mysterious* (John Toland, 1696) which meant just the opposite of what its title said, i.e. Christianity had become mysterious, being administered by a group of people who reserved the truth for themselves. In attacking an élite, the clergy, a seed was planted, the radical negation of a ruling class, which was to bear fruit later. On the positive side, Deists believed that there was a God; at least they would not admit that the Heavens were empty. Deism preserved the idea of obedience to a law. That law was the law of Nature so that what remained to be discovered was, What is Nature? What are its laws?

The manner in which the laws of nature must be sought was made clear in Locke's *Essay on Human Understanding,* where he began by saying, let us have done with metaphysical conjectures, let us examine what we are, do what we can do, know what we can know. Nothing is true *a priori,* there is no such thing as an innate idea; the mind is a *tabula rasa* waiting to be written on. Knowledge arises from sensation; from reflection on sense-data the mind then arrives at ideas. Here men heard the voice of Galileo and remembered the words of Bacon who had also said that we must start with observation; that the human mind apprehends things by sense-perceptions which supply matter for the reason to examine.

To some extent the scientific revolution had taken place in the previous century,[4] with a change in attitude towards the ancients, but old error dies hard; the prevailing conception of the age was that problems either could not be solved or that they had already been solved, as recorded in the great works of the past, and chiefly by Aristotle, «that excellent interpreter of nature». So the job remained for Galileo who declared that «Aristotle, in practically everything that he wrote about local motion, wrote the opposite of the truth».

In Italy the development of science next rested in the *Accademia del Cimento,*[5] active from 1657 to 1667. Its name, which may be traslated as the «Academy of Experimentation», and its motto, *provando et riprovando,* which may be traslated as «testing and retesting», indicate the nature of its activities. It was fostered by Leopold de Medici (1616-1675), brother of the reigning Grand Duke Ferdinand, who had formed the idea of liberating the human mind from the bond of Aristotelianism which was still the heart of the teaching in the Tuscan academic world. However, with the advent of Cosimo III as Grand Duke in 1670 experimental science languished in Tuscany for nearly a century.

Much of the credit for stressing experimentation as a necessary

ingredient of scientific endeavour must go to Francis Bacon's (1561-1626) *Great Instauration*.[4] Bacon was not a scientist or a patron of science; he was a prophet of science, and in agreement with the principles that he set forth, Robert Boyle (1627-1691) and Isaac Newton (1642-1727) further developed modern science. This new science was seized upon by men such as Bernard Fontenelle (1657-1757) who, as Secretary of the newly reorganized Academy of Science from 1687 to 1740, made important contributions to the popular spread of the scientific spirit with his *History of the Academy* and many eulogies of the great men of science. Later, at the château of Cirey in Lorraine, Voltaire (1694-1778) studied physics and chemistry, and with his *Elements de la philosophie de Newton* spread further European appreciation of the new science.

It is not our intention to present science as the only reforming influence of the eighteenth century or to accept entirely Gay's statement: «classicism plus science is perhaps an overly brief but far from misleading definition of the Enlightenment mind». Our purpose is to provide an appreciation of the intellectual background of the life of Felice Fontana.

It so happened that science bulked largely in the activities of the family Hapsburg-Lorraine.[6] Their interest appears to have originated in the person of Leopold, Duke of Lorraine, who, according to Voltaire's *Siècle de Louis XIV*, established at Lunéville a kind of university from which pedantry was banished and in which the young nobility were seriously taught science, instruction in experimental physics being given in laboratories furnished with marvelous apparatus. His son, Francis Stephen, acquired in 1748 the famous collection of the Chevalier Jean de Baillon who became director of his cabinet, which he visited daily and on which he spent large sums in acquiring minerals, coins, chemicals, plants, and animals. Francis Stephen's grandson Charles, later victor over Napoleon at the battle of Aspern, shared the passion for collections of natural history and built the empire's first sugar refinery in Moravia. The children of Francis Stephen, Anna, Joseph, and Peter Leopold, also shared these interests as did the latter's children: Alexander Leopold died in 1795 when an experiment exploded; John founded the «Joanneum» in Graz, with a museum, library, and scientific institute; Louis was sent to England to study technical processes with James Watt and other scientists.

What was required for this eighteenth-century transformation of the old Renaissance type of cabinet of curiosities and of art was the new

scientific method and also the comprehending interest of a sovereign, even if he were not a scientist himself in the strict sense of the word. Wandruszka[6] has identified the source and results of this interest as, firstly, the economic factor, as was realized the close connection of scientific research with improvements in agriculture and medicine, and technical and industrial progress in general. Secondly, there was a religious factor, an «enlightened» religion, quietist, Jansenist, deist, as revealed in Francis Stephen's writings. The study of Nature, God's creation, is thus a kind of religious service. So, under this family the influence of English empiricism, French encyclopedism, Jansenism, mild humanistic Christianity, the identification of God with Nature, all combined into religiosity, utilitarianism, search for the truth, knowledge of nature, and the wish to transform it, making it more pleasant to man.

The new Hapsburg-Lorraine Grand Duke and Duchess came to Florence only for three months early in 1739, after which Tuscany was governed by a regency, it president being the Marshal Botta Adorno (1688-1774), Italian but for the Tuscans another foreigner.

Some good things were accomplished during the regency by the growth of a class of men with professional competence, such as Giulio Rucellai (1702-1778), head of one of the oldest and wealthiest families of Florence but also a man of letters and a lawyer; Sallustio Bandini (1677-1760), Sienese patrician who produced a plan for the reclamation of the Maremma, a barren stretch of land in southwestern Tuscany; Pompeo Neri (1706-1776), auditor of the high court, counselor, and secretary to the Ministry of Finance, who prepared legislation for reformation of the laws of inheritance. These were some of the «new men» who were to play a large part in the Tuscany of Peter Leopold. One of the new men was Gaspare Cerati (1690-1769), a foreigner from Parma, friend and correspondent of Montesquieu and extensive traveler.[7] Appointed Rector of the University of Pisa in 1733, he spent thirty-five years there putting a program of reform and revitalization into effect, thus implementing the changes we have mentioned in Chapter 2. He saw as the task of the university the spreading of knowledge to all the people of Tuscany, to enable them to «perform honorably in every kind of employment», to «preserve and promote the more noble disciplines in this province», and to encourage «sublime geniuses» to make «ever more progress and ever new discoveries for the advantage and pleasure of all mankind». This was the ambience into which Felice Fontana entered in 1758, as we have told. His friend and benefactor, Carlo Firmian, to whom he delicated his earliest books, had recommended him as he had

his brother Gregorio. Now this resulted in Felice's first academic appointment, to the chair of logic in the University of Pisa. A letter from the Rettore Magnifico of that university, written in July 1905 to Adami,[8] stated that the appointment was conferred by H.R.H. by a decree of 10 October 1765. This presumably meant Peter Leopold, who would be referred to by that title; H.I.M. Francis Stephen had died on 18 August of that year. We are told nothing about Fontana's teaching, or if in fact he did teach at all.

It must be noted that the Enlightenment in Italy was not quite the same as it was elsewhere. This has already been touched upon in the case of Tartarotti, and will be again in connection with Carlo Antonio Martini. Valsecchi[9] has formulated the difference in this way:

> An Italian enlightenment was born, which took its place in European enlightenment as a part of the whole, but a part quite distinguished by its own character and physiognomy [...] One has heard of «common sense», a sense of concreteness, of equilibrium, as a characteristic of the national spirit, refractory to the excesses of Transalpine abstractness [...] we have remarked on a vital, immediate contact with reality among our reformers, a consciousness of our common history which is contrary to summary condemnation, to the doctrinaire stamp of the past. With us the innovatory impulse tends to conciliate, not rigidly to oppose, traditions; it is animated by an evolutionary, not revolutionary, inspiration [...] Rarely [...] has Italian thought taken the radical position of French abstract doctrine; rarely has the polemic with the past reached the extremes of Voltairean mockery and distorsion. We have had no Voltaire in Italy.

Italy turned against the old regime with its church and its doctrine of divine right, but retained its religion and adopted a program of «enlightened absolutism». This concept had as its premise that sovereignty originated in, and had its end in, the well-being of the people. The people delegated their power to the sovereign in order that he might bring about their well-being. He had all the rights because he had all the duties. All this presupposes one condition: in two words, an enlightened sovereign.

CHAPTER 4

PETER LEOPOLD, GRAND DUKE OF TUSCANY

The third male child born to Maria Theresa and Francis Stephen (5 May 1747) was named Peter in homage to the memory of the great reforming Czar of Russia, and Leopold so as not to abandon a name so traditionally Hapsburg and Lorraine (Fig. 3). The first two sons had been born while their father was only Grand Duke of Tuscany, but now since the peace of Aquisgrana (Aix, Aachen) the crowns of the monarchy were secured to the first-born, Joseph, that of Tuscany to the second-born, Charles Joseph, while to Peter Leopold fel the lot of ensuring the succession.[1]

The teacher whose influence on him was greatest was Carlo Antonio Martini, born at Revò in the Trentino in 1726. After study at Innsbruck, employment as teaching assistant in Vienna and as secretary to an emissary to Spain, he was appointed to the newly founded chair of natural law in the University of Vienna where he began to give lessons to Peter Leopold in 1758. Martini's books are full of the idealism of the Enlightenment, based on the assumptions of the innate goodness of human nature and the desirable results of education. Stressed are the duties of the Sovereign: the caring for the public benefit by adopting good laws, choosing employees and councilors on the basis of merit only, and supervising their work. On the relations between Church and State, the sovereignty of the ruler is to extend over the dignitaries of the Church. The members of the clergy and monastic orders are to be limited in number and their activities controlled. Martini finished by quoting «Render to Caesar that which is Caesar's and to God that which is God's», and «My kingdom is not of this world». If we might feel the presence of a contradiction between the strict observance of religious duties of Roman Catholic orthodoxy and the adoption of a position of interference in ecclesiastic life, with even Jansenist tendencies resulting in violent controversy with the Roman Curia, it is clear that Maria Theresa and her children were conscious of no such contradiction.

Another influence on the development of Peter Leopold was the *Encyclopédie ou Dictionnaire raisonné,* etc. which began to appear in 1751 when he was four. By 1765, when he became Grand Duke, the seventeen volumes of text were completed. Under his patronage and dedicated to him, a third edition began to be printed in Leghorn. The arousal of his interest in science especially chemistry (Fig. 4) began at an early date as is shown by his request at the age of fifteen, on the occasion of completing some examinations, that he be allowed to visit a cotton-mill in Schwechat, a suburb of Vienna.

If it might be thought that his education was overly serious, it is certain that it became more so after the death of his older brother Carl on 18 January 1761. Now it was clear that Peter Leopold was definitely destined for the throne as Grand Duke of Tuscany and his instruction was more clearly that of a prince. It was now that his tendency to hypochondria appeared, which extended to biting his nails. Even when he had become Grand Duke and a father, he wrote to his mother in 1768 to report on his health, and the condition of his nails.

His marriage being planned in Hapsburg style, a portrait of Peter Leopold was sent to Madrid in June 1763, where it was regarded favorably by Maria Luisa of Bourbon, Infanta of Spain. His progress to the throne included his travel to Frankfurt am Main in 1764 for the occasion of the coronation as King of the Romans of his brother Joseph. It was also the occasion for his father, the Emperor Francis, to present him with a document: *Instructions for my Son.* It began with the statement that he felt it superfluous to recommend to his son the fear and the love of God, but then followed a detailed description of Francis's conception of «enlightened Catholicism», which included a daily examination of the conscience in order to guard against a dangerous disproportion of the ego. This is particularly hazardous in Italy, he wrote, where they are more clever and skilled in adulation than are other peoples. There

> sagacity and discernment are more necessary than elsewhere; you will have to deal with men not only intelligent but full of half-expressed subtleties, moved often by selfish interests; persons by whom you will be well-served, however, if you are aware of and avoid such traps.[2]

The best defense was to take decisions without haste, trying to form one's opinion slowly and cautiously, trying to discover the truth.

> In that country more than elsewhere, it is necessary to counter the natural vivacity of the national character with great phlegm.[2]

Fig. 3 – Peter Leopold and Joseph II. Portrait by Pompeo Batoni, Rome, 1767.

The major theme of the Emperor was to recommend gentleness *(la dolcezza),* moderation, amiability. As God is our lord and master, gentle and good, so is gentleness a necessary virtue in governing; when men obey from personal sympathy for him who commands, they do so better than those who obey only from a sense of duty. Equally important is the virtue of politeness; a prince can never be too courteous; if someone is tempted to profit from this, there will always be someone to put the indiscreet one in his place. It is far better to be loved and appreciated than to be feared. In another part, the Emperor stressed the even greater need for these princely attributes in the conjugal state, in order to be trusted, to have a stable and serene married life. Perfect felicity is not of this world; he who seeks it will make himself ever more unhappy and dissatisfied.

Ten years later Peter Leopold was himself to write:

> Gentleness *(la dolcezza)* is most important, most precious in all human and social conditions; the virtue most to be desired; the most apt to make one loved and obeyed; that which best serves to make one useful to others and to live in peace with one's self. But no less important is charity; one must soon learn that each of us cannot do without the others and that reciprocal help is the greatest bond which unites society.[2]

On January 12, 1765 Joseph renounced the right to succession in Tuscany in favor of Peter Leopold, thus clearing the way for the marriage between Peter Leopold and the Infanta Maria Luisa. On 4 July the court left Vienna, and arrived in Innsbruck on the fifteenth of that month. Maria Luisa reached Genoa on 18 July and on 1 August Bolzano, where Peter Leopold had gone to meet her. We have no record of the impressions the young couple had of each other, but the members of the court found the bride a pleasant surprise. They returned to Innsbruck and were married there on 5 August; on the eighteenth the Emperor died unexpectedly.

Between that day and the departure of the couple for Florence on 30 August, the disconsolate mother prepared for her son some instructions, which began: «Alas, you are a sovereign». These instructions were: «General», on his behavior, which left room for improvement; «Regarding religious exercises», particularly in Tuscany where there are so many irreligious foreigners; «Specific instructions on health», which direct Lagusius (born Hasenöhrl), the new physician to the Tuscan court, to report to her every fifteen days, or daily in case of illness.

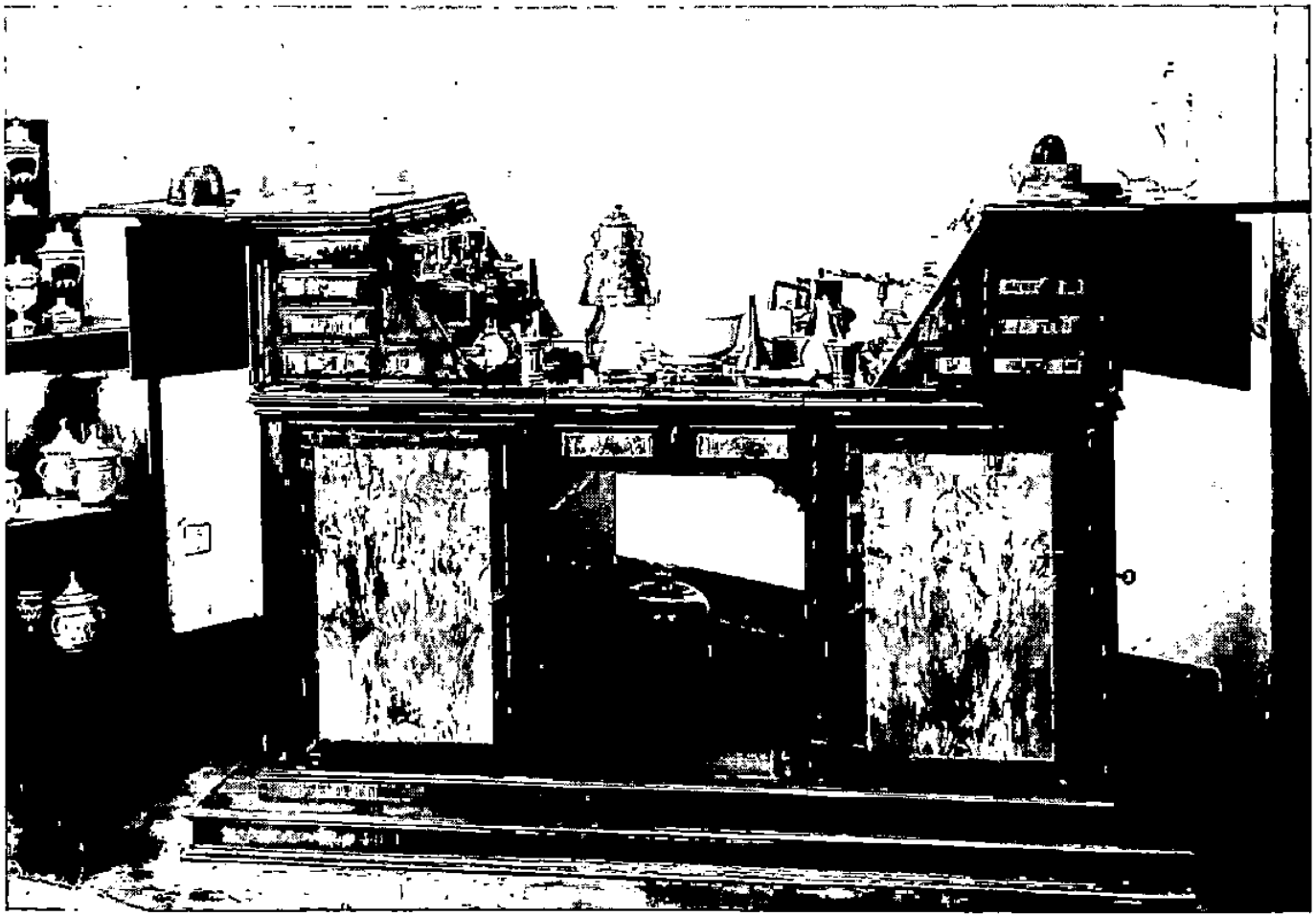

Fig. 4 – Peter Leopold's chemical bench. Now in the Institute and Museum of the History of Science of Florence.

Finally the young couple (each aged eighteen) arrived in Florence 13 September 1765 at eight in the morning, the hour having been kept secret. In spite of this a great crowd acclaimed them, having waited all night. The jubilance arose from the advent of a ruler in residence rather than *in absentia,* a ruler young, attractive, sympathetic, and cordial, representing the future full of hope instead of an old soldier representing the ugly past. Now Florence was truly a capital, of a Tuscany with about one million inhabitants; it had been for four centuries, 1200 to 1600, at the front of nearly all fields of human activity, the arts, the sciences, from politics to economy. However, in the seventeenth century, governed by the last incompetent Medici, it had fallen behind the rest of Europe. Now Florence had a new ruler who very quickly became a true Tuscan, who found the «new men» as interested in reforms as he was. These reforms are fully presented by Valsecchi,[1] by Wandruszka,[2] and by Peter Leopold himself.[3]

CHAPTER 5

FELICE FONTANA, COURT PHYSICIST

The Useful Sciences - The Great Chain of Being

On 28 April 1766 Felice Fontana wrote from Florence to his brother Giuseppe in Rovereto:[1]

> Finally I was called to the Court by H.R.H. the Grand Duke of Tuscany, who wished that I show him some of the rare microscopic animals discovered by me in stagnant waters. He kept me with him two hours and gave me every sign of Extreme Clemency and kindness. At his request, I have not yet gone to Pisa, and nearly every day I receive specific charges which he lays upon me concerning physics, in which he is strong and which he loves since he understands it; I hope to be made his personal physicist, and to have custody of the royal cabinet of instruments of experimental physics.

Gregorio Fontana wrote[2] to Graser on 2 May 1766 that Felice had been called to Florence as Experimental Physicist to the Court. According to the Rettore Magnifico of the University of Pisa in a letter[3] of 1905 to Adami, a decree of 7 November 1766 gave Felice the chair of physics in the University (which title he retained until his death) and also called him to the Cabinet of Physics in the Palazzo Pitti, the Royal Residence. It is likely that Felice both resided and worked in that palace, where the members of the *Accademia del Cimento* had performed their experiments.

The first new problem to which Felice turned was that of the rust of wheat, the cause of frequent crop failure and famine. From his publication on this subject[4] we learn that the first observations were made in June 1766, which would support Gregorio's dating of his appointment, since it is likely that his attention to this problem was called by the Grand Duke, who would be well aware of the crop failure

of 1764 and the poor harvest of 1765. Called rust on account of the reddish appearance of the leaves and stem of the plant, this was an ancient affliction:

> It is certain that the Rust is a very ancient Scourge of the greater part of the Country, cultivated by Mankind in the old World. It is known from the Holy Scriptures, that the Promised Land was not exempt from it, although very little dominated by watery meteors; and it is known from Theophrastus that Greece, and from Varro, Horace, and Virgil, that Italy were subjected to it, although more afflicted by Rain, Clouds, and Irregular Winds. A more recent testimonial that the Rust usually caused great waste in the Fields of Tuscany, and Rome, is the giving of the *Feste Rubigali,* which the King Numa, according to legend, desired to be solemnized each year on the day April 25 to his Subjects, to placate the ire of the Goddess Rubigine, or Robigine.[5]

A disastrous epidemic of 1690 had been studied by Bernardino Ramazzini (1633-1714), whose book *De morbis artificum diatriba,* Modena, 1700, was the first systematic treatment of occupational diseases. In another book, *Constitutiones epidemicae: De constitutione anni 1690* [...], Modena, 1690, «he examines at wearisome length the nature and causes of rubigo [...] he concludes that acid in the dew may account for it».[6] His discussion was warmly praised by his friend Antonio Vallisneri in his *Historia della generazione dell'uomo e degli animali,* of 1721. This emphasizes the originality of Fontana's contribution.

Fontana's work on the rust of wheat was reviewed in the *Magazzino Italiano,* pp. 237-39 of November 1767, and more extensively, with reproduction of the illustration, in the *Giornale d'Italia* pp. 169-176, 1768. It was reprinted in Naples in 1789, and an English translation by P.P. Pirone appeared as Phytopathological Classics No. 2 in 1932. The results of this publication, one of the earliest on the causal nature of rust, were: a controversy with Giovanni Targioni Tozzetti; to spread the fame of Fontana as scientist; to lead him to other studies of microscopic organisms; to little in the way of control of the pest: the wheat crop was a failure in 1766 also.

Fontana's interest in plants and microscopic organisms was to continue throughout his life. At this period, he began to take up two great themes: the nature of life and death, and the great chain of being, but he was to approach them only slowly. In general, the reasons for his concern with microscopic plants and animals were three-fold. Firstly these were objects of great interest in themselves. This is revealed by a

long footnote in the *Nuove osservazioni sopra i globetti rossi del sangue* of 1766 which is signed: *Annotaz. d'un amico,* but was probably written by Fontana himself:

> Since the year 1763 the Author of this book has set himself to examine the smallest animals, those visible to the eye as well as those which can be seen only with the microscope. In these pursuits he has carried out long and deep researches, particularly on the little eels of the generative fluid and on those little animals seen as ovules in vegetable and animal infusions. These observations he has communicated to several learned Italian friends of his.

As we know, these studies became a «useful science» when they were directed against the problem of plant diseases such as the rust of wheat.

Secondly Fontana had been led to consider the problem of life and death by his studies of the venom of the viper which he had begun in 1764, as far as we know for a practical reason, namely to examine this hazard to life in Tuscany. The problem was: when, how, and why does a living organism come to be dead. Fontana's first observations and experiments on the venom of the viper were described in a book entitled: *Ricerche fisiche sopra il veleno della vipera con alcune osservazioni sopra le anguillette del grano sperone,* published in 1767. The last paragraph (pp. XIV-XV) of the Editor's introduction reads:

> Also noted are a few points on the small eels of *grano cornuto,* or *sperone,* which may suffice to establish the true nature of these marvelous little serpents, reporting also the various thoughts of the most modern Philosophers for comparison with Our Author's observations. He reserves a longer treatment of this in another of his works, *On the animals which die and revive,* where will be seen a long series of observations he has made on this completely unknown part of organic physics.

A curiously variant edition of this work appeared, with this paragraph omitted and with no mention of the *anguillette del grano sperone* in the title. In fact the text of both versions devotes only ten lines to the *anguillette.* Both versions speak of a book to appear entitled *On the microscopic animals.* Neither this nor the one previously mentioned ever appeared. Both versions say: «see at the end the added Dissertation on the eels of wheat»; it is not there.

An organism which Fontana did discuss at greater length here was the rotifer. This microscopic animal had been written about by Leeuwenhoek in 1702, which Felice probably knew about from the work of

Henry Baker,[a] whose book in a French version was in Fontana's library and freely cited by him. Felice was fascinated by the strange working of this animal's heart; he also clarified its structure by pointing out that the mechanism at its mouth end was not a wheel as Leeuwenhoek had written but a group of cilia. In this context Fontana's interest in the rotifer came from its apparent death on being dried and its revival on being moistened, which had been described by Leeuwenhoek and by Baker. It was this phenomenon which Felice described at considerable length in the work on *Grano Sperone* or the *False Ergot.* He knew of the discovery in 1743 by John Turberville Needham,[b] announced in the *Philosophical Transactions* of the same year, as follows:

> [He] perceived a soft white substance [...] it seemed to consist [...] of longitudinal fibers bundled together without any least sign of life or motion. He dropped a globule of water on them [...] when to his surprise, these imaginary fibers, as it were, instantly separating from each other took life, moved irregularly [...] for the space of nine or ten hours [...] He states that they are a species of aquatic animals, and may be denominated worms, or serpents, which they much resemble.

Needham suggested that the blight of wheat «is frequently occasioned by the sowing of seed intermixed with blighted grains». His observations were confirmed by Henry Baker, but when they were discounted by the French biologist Aymen, the Genevan Bonnet, and the Italian Spallanzani, Needham abandoned in 1769 his position of attributing any spontaneous motility or animal nature to these filaments. However, Fontana had not given up the convictions which he had drawn from his observations described this way in 1767:

> The animal revives immediately its former movement returns to its parts, it is dead forever, as in our case, if those parts lose not only their present movement but also

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[a] Baker (1698-1774) eminent English biologist, Fellow of the Royal Society, married the youngest daughter of Daniel Defoe and grew rich from inventing a method for educating deaf-mute children. His books presenting his scientific contributions were: *Employment for the microscope,* London, 1753, and *The microscope made easy,* London, several editions.

[b] Needham (1713-1781) English Roman Catholic priest, spent most of his life on the continent. His scientific work was largely on micro-organisms; he is known generally only for his part in the controversy which arose over spontaneous generation. His *New microscopical discoveries,* London, 1745, was well-known in several translations.

the possibility of reacquiring it afterwards. Equally those microscopic eels, which are seen as dry threads in *grano sprone* or *cornuto,* recover life and movement if water moistens them, as I have seen many times with great satisfaction and pleasure; they die again on dessication. They retain always the power of revival, in fact, return from death to life solely by virtue of water bathing them.

Fontana did not comment more explicitly on the animality of the little eels in 1767, but in 1771 he returned to the subject with a letter to his friend Ferroni which was published and reprinted in the same year. This letter relates that the Dissertation, containing observations proving that the eels of *grano cornuto* were true animals, had been partially printed in 1769 but was never published. Now to the treatment of the little eels of blighted wheat was added that of the *tremella,* a saprophytic, aquatic organism of the Basidiomycetes, a subclass of the fungi thus related to the agent of the rust of wheat. A publication on the False Ergot and the Tremella was the most-often reprinted, in slightly varying forms, of Fontana's publications, appearing finally in 1803.

Thirdly the tremella was of great general interest on account of its supposed character as a «missing link», as a «plant-animal», as an example of the avidly sought-after links in the Great Chain of Being. This phrase then represented an idea,[7] «the most widely familiar conception of the general *scheme* of things», a rational and intelligible one of the pattern of the universe, «a way of predicating the constitution of the world» with «three specific, pregnant, and very curious characteristics» which «implied the nature of God».

The first of these was the principle of plenitude:

the thesis that the universe is a *plenum formarum* in which the range of conceivable diversity of *kinds* of living things is exhaustively exemplified [...] also other deductions from the assumption that no genuine potentiality of being can remain unfulfilled, that the extent and abundance of the creation must be as great as the possibility of existence and commensurate with the productive capacity of a «perfect» and inexhaustible Source [...] namely God, the creator, who is «infinitely, eternally, unchangeably and independently glorious and perfect».

The second principle was that of continuity. Aristotle maintained that Nature[8]

passes so gradually from the inanimate to the animate that their continuity renders the boundary between them indistinguishable; and there is a middle kind that belongs to both orders. [...] And the transition from plants to animals is continuous; for one might question whether some marine forms are animals or

plants, since many of them are attached to the rock and perish if they are separated from it.
If there is between two given natural species a theoretically possible intermediate type, that type must be realized – and so on *ad infinitum;* otherwise there would be gaps in the universe, and the creation would not be as full as it might be, and this would imply the inadmissible consequence that its source or author was not good.[9]

The third principle was that of unilinear graduation, or a notion of an ontological scale, a hierarchy of beings, in which living organisms are ranked according to their degree of perfection.

The result was the conception of the plan and structure of the world which, through the Middle Ages and down to the late eighteenth century, many philosophers, most men of science, and, indeed, most educated men, were to accept without question – the conception of the universe as a «Great Chain of Being», composed of [...] an infinite number of links ranging in hierarchical order from the meagerist kind of existence, which barely escapes non-existence, through «every possible» grade up to the *ens perfectissimum.*[10]

What did Fontana know of all this? His only mention of the Great Chain of Being was very brief when he waxed enthusiastic over the tremella, writing:[15]

This plant animal forms a true uniting link between the two kingdoms, animal and vegetable, that is so much sought by the philosophers and always in vain, and seems to be the last link of the great chain of animal bodies and the first of the vegetable.

The touchstone which Fontana applied to distinguish between life and death, plant or animal, was Hallerian «irritability», not so much in the sense of contractility as a property of tissue, but motility as the property of an organism. When an animal had succumbed to the venom of the viper, it no longer moved. If an organism moved it was not a plant but an animal. He quickly found these distinctions unsatisfactory; he was to change his opinion subsequently several times about the mechanism of death by poison; he first thought of the tremella as an animal because it seemed to be capable of progressive motion, but later he regarded it as a plant. In the great problem of revivification he reached his limit of exploration; after 1775 he had nothing new to say and permitted the reprinting of his old essay as late as 1803 although he did take some steps forward with his paper of 1800 on the convolvulus. However, at the time of his early studies of these problems he demonstrated his

experiments with frank enthusiasm, as he tells us many times, and as his visitors wrote. Ferber, German mineralogist, visited his laboratory 11 December 1771. Jean III Bernoulli, German astronomer, and Adolf Murray, Swedish anatomist, were visitors in this period. A later visitor was C. Dupaty (1746-1788), French magistrate and friend of Diderot. He wrote:[11]

> I should wish to study also those strange beings found in the ergot of wheat which, reduced to the last stage of drying, show all the signs of dead matter, but are still organized, living, or often can be revived. M. Fontana suggested that he make the experiment in front of me; he makes use of only one drop of water. He takes care not to let it fall on these dusty animals; it might shatter them in falling: he brings near little by little the drop of water on the point of a needle, and by degrees the little animal becomes filled with freshness; all the atoms composing it draw near to each other, bind themselves, make a whole: already there is movement; it improves, it progresses, it goes about, it lives.
>
> The inferences coming from this experiment are of the greatest importance; they throw great light on the life and death of matter. M. Fontana does not dare to write on this subject; he fears to be excommunicated. All the power of the grand duke would not save him from the consequences of excomunication, which has much power, even in Tuscany. It is not that M. Fontana's system attacks any religious dogma; but the mere word *reason* frightens Rome... M. Fontana has opened the way, new and certain, through nature's labyrinth. Unhappily for his great work, above all the proximity of Rome hinders his writing, sometimes dissuades him from thinking.

Keilin[12] discounts Dupaty's claim that fear of being excommunicated discouraged Fontana from publishing, as it disagrees with the facts: (1) Fontana had published his work on revivification ten years before Dupaty's visit; (2) as had also Spallanzani, who wrote freely of «the resurrection of killed animals».

Another field in which Fontana was active at this time was that of pneumatic chemistry. Guareschi[13] identified the beginning of Fontana's interest in the airs as his essay delivered to the *Accademia degli Agiati* in 1753 on gunpowder. This is inacceptable since another juvenile effort, *On the lamps of the ancients regarded as inextinguishable,* takes up at much greater length the chemistry of the airs. It is much more likely that his interest was that of «all the physicists of Italy» as Landriani wrote, was that pneumatic chemistry which began with the pioneer studies of Stephen Hales (1677-1761) and the discovery of Joseph Black (1728-1799) that fixed air could be expelled from a mineral, magnesium carbonate. Fontana's earliest studies of the airs were largely derivative; they were on fixed and on nitrous air and were either in error or

constituted little of an advance. His work on dephlogisticated air was of greater import and this he continued when he went to Paris and London, where he published some important contributions. While still in Florence his experiments with nitrous air led him to take up a problem of immediate practical importance, that of the salubrity of the atmosphere.

In 1770, the Grand Duke directed Fontana to install the new Franklinian lightning conductors on the Royal powder magazines throughout Tuscany. In connection with this assignment Felice first consulted Giambattista Beccaria[c] in Turin;[14] by mid-year the Florentine Fortezza da Basso had been equipped and on 3 September he wrote to nis brother Giuseppe from Leghorn, where he had been for eight days installing them, that from there he would go to Siena, Pistoia, and Arezzo to do the same.

[c] (1716-1781) Celebrated physicist, author of «Elettricismo Artificiale» which Franklin advised be translated into English.

CHAPTER 6

FROM COLLECTION OF CURIOSITIES TO MUSEUM

Accademia del Cimento - Royal Museum of Physics and Natural History

Many an eighteenth-century gentleman had as his hobby «science», which meant, was manifest by, having a «collection of curiosities». This custom had been fostered in Florence for three centuries by its ruling family, the Medici, beginning with Cosimo the Elder (1389-1464). Founder of Florence's *Accademia Platonica* and *Biblioteca Medicea Laurentiana,* Cosimo was patron of Paolo del Pozzo Toscanelli (1397-1482), mathematician, astronomer, geographer, and cartographer, who stands out as the type of scientist of that time when the humanists and artists were the chief recipients of rulers' favors.

Another Cosimo, Cosimo I (1519-1574), governed Florence for another long period (1537-1574) during which time some change in emphasis occurred, away from the more abstract sciences of the previous century. A protegé of Cosimo I, Egnazio Danti (1536-1586) was also geographer and cartographer but in addition constructed astrolabes and quadrants, which survive along with maps and armillary spheres. Cosimo I himself established a chemical-pharmaceutical laboratory in the Boboli Garden near the Palazzo Pitti where he resided.

Two years before the death of Cosimo I there was born Galileo whose activities in science, including construction of instruments, under the Medicean rule (1587-1620) of Ferdinando I and Cosimo II need no discussion here.

It has been said that the chief contribution to science of Cosimo II was his sons Ferdinand II (1610-1670) and Leopold (1617-1675). In 1642, the year of Galileo's death, Ferdinand II established an *Accademia*

medicea sperimentale but its interests were ohiefly meteorological, with instruments such as hygrometers which survive; its accomplishments pale in comparison with those of the famous *Accademia del Cimento.*

The many instruments and experiments of this group of scientists were described in a single publication of 269 pages *in folio* which appeared in September 1667, entitled *Saggi di naturali esperienze fatte nell'Accademia del Cimento* etc. Eulogizing the scientific method in the preface, it may be considered as «the first course in physics», although it is not the first book devoted to experimental research in physics, which was Robert Boyle's «New experiments physico-mechanicall» of 1660.

Many instruments remained after the dissolution of the Academy in 1669, but «although Tuscany had been in the front rank of this battle for sixty years... the publication of the *Saggi* was the last shot from the valley of the Arno. The leadership in science was moving north, and the accession of Cosimo III in 1670 ensured that it should not return».[1]

In 1780 Targioni Tozzetti wrote:[2]

> The instruments then were infinite one might say,[a] that is, all those of the engravings of the *Saggi,* and twice as many not yet published. The major part of these I saw in 1740 gathered in magnificent cabinets in a room next to the library of the Palazzo Pitti where the sessions of the *Accademia del Cimento* were held. Others were lying here and there, dispersed, or passed into other hands, and a large part Signor Vayringe, machinist of H.M., had taken home [...] After Vayringe's death [...] some were sent to Vienna, some were given to the Teresian College, some stored in the Palazzo Pitti [...] many others had been broken or carried away, and many the Cardinal Leopold himself had sent as gifts to Pope Alexander VII.

In 1776, 547 of these instruments were still in Florence, after the flood of 1966 only 223.

With the advent of the Hapsburg-Lorraine line as Grand Dukes of Tuscany, having an intense interest in the sciences as we have already noted, there was a revival of interest in collections. By a letter of 24 February 1763 the Emperor Francis Stephen ordered[16] the Marshal Botta Adorno, chief of government in Tuscany, to send him a catalog describing exactly the natural history collection in the Florentine gallery. This task was given to Targioni Tozzetti, and in 1764 two handsome volumes *in folio* were carried by Botta Adorno to the emperor.

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[a] The third edition of the *Saggi* (Florence, Tipografica Galileiana), 1841, containing Targioni Tozzetti's additions of 1780, pictures 99 pieces of apparatus.

When the idea of a museum entered Fontana's head is unknown, but it was probably soon after his appointment in 1766 as Physicist to the Court. Unfortunately we do not have the prospectus which he undoubtedly submitted to the Grand Duke; it is probably buried in the State Archives. We do know that the Palazzo Torrigiani was acquired in 1771; its occupation must have commenced immediately, since Ferber found the instruments of the *Accademia del Cimento* there, and wax models being made, in December of that year. Reconstruction also began soon, as Fontana wrote to Bettina Slop in June 1772.

In 1775 an account of the new museum appeared in print, serially in a periodical and as a pamphlet.[3] The authorship of this anonymous document is debatable. Most writers have been certain that either Fontana wrote it or it was prepared under his supervision. In any case it is full of laudatory phrases which seem fulsome to our ears. The composition of this essay is of interest; of its thirty-six pages, twenty-six are devoted to mechanical and physical instruments, nine to natural history including anatomy, and one to chemistry. The nature of the physical instruments, according to M. L. Righini Bonelli:[4]

> indicates a change of direction of the Museum which, besides being a collection of ancient instruments, became also a physics laboratory and a shop for the construction of apparatus needed for teaching science... the building activity of the Museum shop towards the end of the 18th century was very great, almost febrile, so that in a few years there could be carried to completion a collection... that, for richness and aesthetic satisfaction of the models, still commands admiration today... [Nevertheless] one must be aware... that, with few exceptions, depiction of the style of the instruments is prevalently to be found in the treatises of S'Gravesand and Nollet.

This viewpoint is not in complete accord with Bernoulli's assessment[12] or with Fontana's own accounts, either in the Essay in its Italian version[3] or its version in French[5] prepared by Argand,[b] or in his memoirs submitted

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[b] François Pierre Aimé Argand, 1755-1803, was born in Geneva but spent most of his life in France and England. He was best known for his invention of a lamp. A footnote to his version of the *Saggio* states: «This is only a very slight sketch and gives but a general and superficial idea of all that this famous Collection contains. For entering into details, a great number of drawings and plates, not permitted here, would have been required; besides, a complete description is now being prepared on the orders from H.R.H., and under the direction of M. l'Abbé Fontana; it will form several volumes *in folio* and will be the work of some years. In the meantime,

to the Grand Duke.[6] Unfortunately Fontana's descriptions of instruments of his own devising are unsatisfactory. The versions of the Essay and a few short notices on instruments in the *Antologia Romana*[7] are not certainly of his authorship and are not sufficiently illuminating. In this case we must turn to accounts of visitors to the Museum La Specola during Fontana's directorship, and to still-unpublished letters (Figs. 5, 6, 8).

The earliest of record[8] is apparently that of J.J. Ferber[c] who gave in his letter of 11 December 1771 a description of «a new Academy», i.e., La Specola.

> Abbé Felice Fontana, formerly professor of mathematics at Pisa, currently physicist and mathematician to the Grand Duke, is entrusted with arranging this establishment; perhaps he will also have its direction... Busy and intelligent as can be, Abbé Fontana has all the necessary zeal for assembling the machine: besides he is warmly encouraged by the attention which the Grand Duke gives him daily; one could not find devices, physical and otherwise, more suitable than this scientist has constructed... A young Florentine makes, under the eyes of Mr. Fontana, preparations in wax of the different parts of the human body; nothing finer can be seen.

A brief description of some of the waxes and the natural history collection follows. Ferber also noted Fontana's publications on irritability, vipers, eels of wheat, ergot, tremella, and blood cells. He wrote that the physical instruments of the *Accademia del Cimento* had been moved to the Palazzo Torrigiani, as well as books from the *Magliabecchiana* Library, which was not true, according to Bernoulli.

I thought it would not be superfluous to make known the Essay about it that was published last year in Rome by a man of letters who; having stayed in Florence had the time to examine and to study this collection with the attention it merits. The conversations that I have had with M. l'Abbé Fontana have put me in the way of making some additions where it seemed to me they were required for a better understanding; these additions I have joined with the translation in the form of notes». There are eighteen of these notes, some quite lengthy.

c Johann Jacob Ferber (1743-1790) was born in Sweden and died in Bern. He held positions at the College of Mines in Stockholm and as Professor of Physics and Natural History in Jelgava, Latvia (German Mitau) and in St. Petersburg (1783); he was a member of the Prussian Academy of Science (1790). His writings were on mineralogy and his travels, the one cited being written to Ignaz Born (1742-1794), Privy Councilor of the Exchequer at Vienna 1776-79; it was translated from the German with added notes and personal observations by Philippe Frederic, Baron de Dietrich (1748-1793) (see C.E. Petrin, «Isis» 73:545-551, 1982).

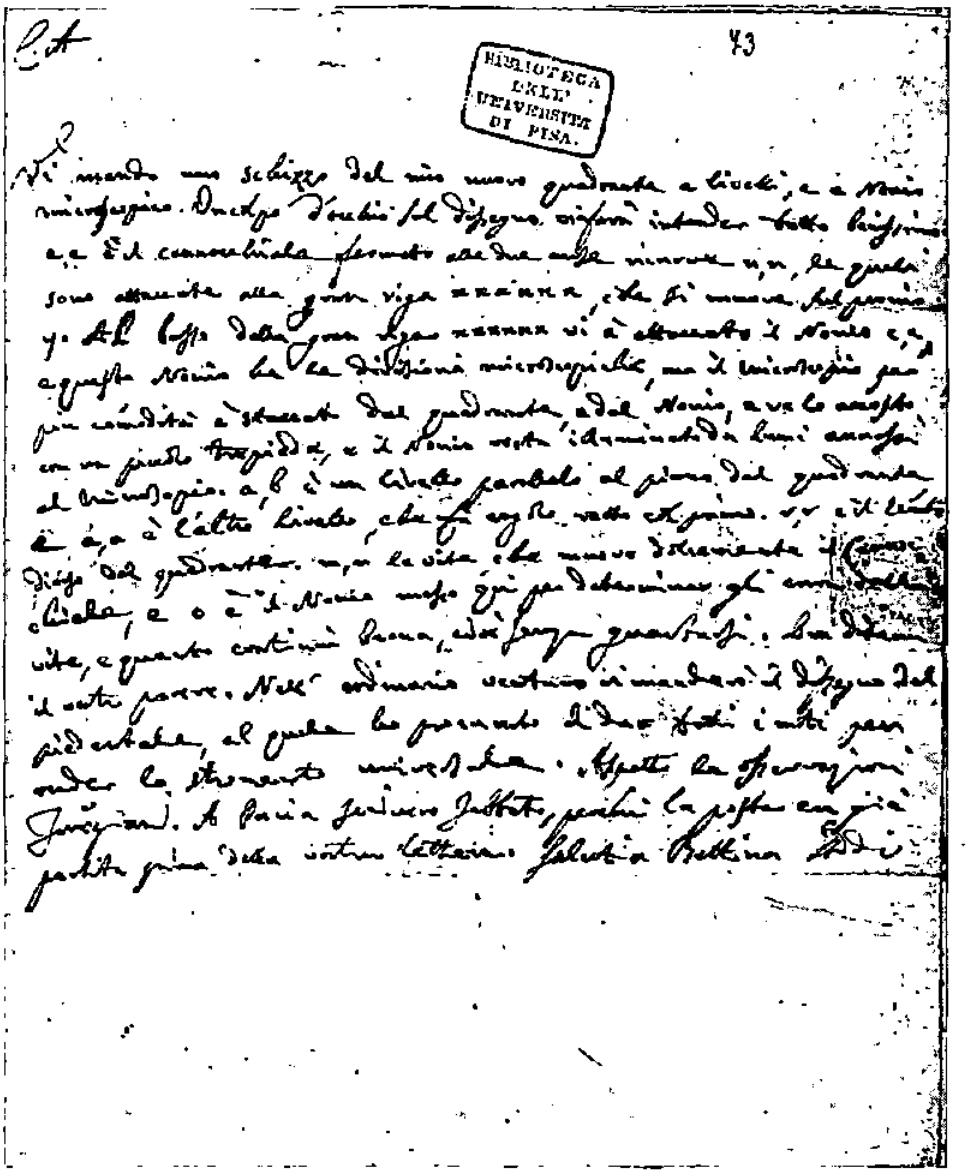

Fig. 5 – Letter of Fontana to G.A. Slop.

On his second visit to Florence in 1772 Ferber found the astronomical observatory being well built under the direction of Tommaso Perelli and G.A. Slop, and noted that some lithophytes had been found and described by Fontana. It was at this time that Fontana sent him to Pisa in care of Slop.

The next account[9] to appear (1773) was an Italian version, with additions, of a London publication[10] of 1772. The dedication to Peter Leopold, Grand Duke, mentions his establishment of a grandiose museum, with an elegant collection of physical instruments, of anatomy, natural history, and chemistry. After a preliminary discourse, the book discusses the English work, giving equivalent names in English and Italian, then mentions items developed by Fontana, including model of a machine for raising water, a sensitive thermometer, and many instruments of astronomy and physics invented by Fontana «who gave rise to one of the richest and most copious collections of natural history to be found in Europe», «Many instruments owing to the abovementioned philosopher are to be found in the Royal Museum, Florence» (Fig. 7).

Johann III Bernoulli visited Florence in early 1775, writing later about the museum[11] and its instruments.[12] He cited the *Avanzamento*[9] of 1773 and the *Saggio*[3] of 1775, contradicting Ferber's statement about the Museum library, and noting that the astronomical observatory was not yet completed. His article on the physical and astronomical instruments describes them in some detail, emphasizing Fontana's contributions to their improvement and also his originality.

Bernoulli[11] mentioned an account by another important visitor to Florence, de Lalande,[d] who was there in June 1765 as we know from a letter from Fontana to Slop. However, the lengthy description[13] seen was published in 1790 and mentions the Treatise on the Venom of the Viper of 1781. From the text, this work of Lalande's appears to have been written during the period 1781-1783. It begins:

> The Museum is an immense collection of everything relating to physics, mathematics, and natural history, formed during the last few years by the Grand Duke in the Torrigiani palace which he bought about 1772; it is to the south of the Pitti palace, as the Gallery [Uffizi] is to the north; it is the custom to say that the Palazzo Pitti is between the treasures of art and those of nature.

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[d] Joseph-Jérôme Lefrançais de Lalande (1723-1807), astronomer, was extremely well-known in his lifetime, partly because of the enormous bulk of his writings and partly because of his love of the limelight.

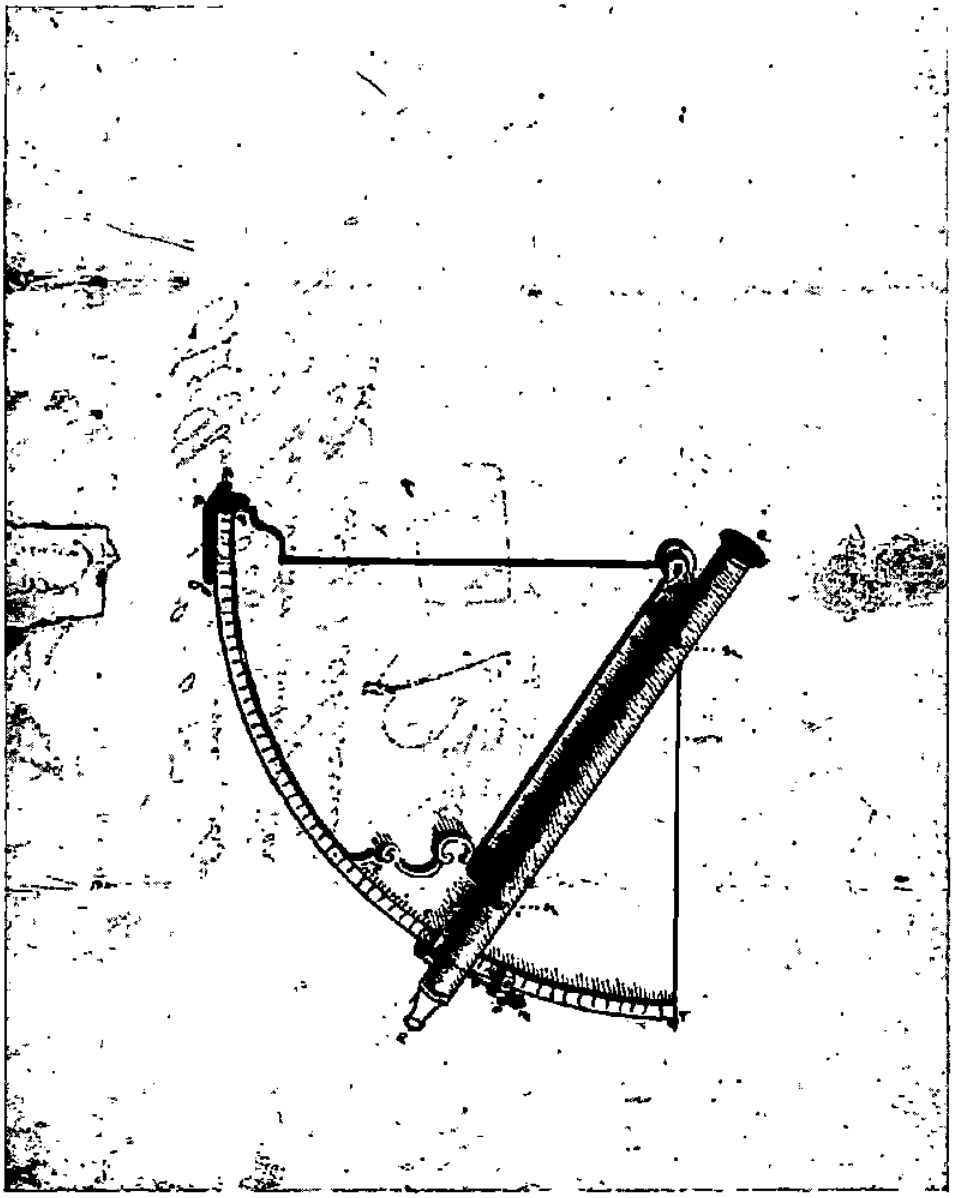

Fig. 6 – Drawing of Fontana's quadrant accompanying the letter to Slop.

M. the Abbé Felix Fontana, of Roveredo, is the one who is in charge of this collection; he has the best physical instruments made in England and in France, where he spent several years; he has had some made in Florence after his own ideas which are those of a man of genius; one may consider him as one of the most famous physicists, not only of Italy but even of all Europe.

De Lalande then described the contents of each room in some detail.

In 1785 the Museum was visited by Dupaty[c] who described his visit thus:[14]

I should like to be able to describe the collection of natural history which, for the last ten years, the grand duke has been engaged in enriching, and M. Fontana in organizing.

Fifty rooms are already full of the treasures of this collection, fifty more will be filled.

It is impossible to express the elegance, the order, the arrangement, of the suites; not only is everything to be seen, but everything is displayed, everything calls for your attention.

The cabinets of the collection reflect the mind of M. Fontana, filled with natural history.

I could never tire of going through these rooms, of wandering from kingdom to kingdom, of visiting all these different empires of nature, of examining all its treasures, of following nature endowing all organized beings with movement, giving more to these, giving a little less to others; movement which all these individuals together return to her in proportion as they have received it, faster or more slowly, in all possible forms, executing every play in the brilliant phenomenon of life.

But what has held my attention is Man. A masterly wax, perhaps more durable than bronze, offers a complete image of him in this collection. You see all the most secret parts of this complicated mechanism, first isolated, scattered, then reassembled, reunited, all ready harmoniously to fulfil in the general economy of the human body, each in its turn and at its place, its pertinent part, all ready to live.

These details fill a dozen rooms; there is not one part, so to speak, of this copy of man which has not required the sacrifice of an entire specimen of the original. This wax model has used up a thousand cadavers. What labor! What patience! But also what a splendid monument!

The emperor was so pleased with it that he has ordered a duplicate. Three years will be required to make it. I have seen them working on it.

I deeply regret not having been able to study this all-embracing model of man.

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[c] Charles-Marguerite-Jean-Baptiste Mercier Dupaty (1746-1788) was active in reform movements, particularly in penal legislation (he translated Beccaria's book). This and his impetuous character led him into being persecuted for atheism, and other troubles. In Paris for some time, he became a friend of D'Alembert. The trip to Italy of 1785 led to his book which had great influence in its time.

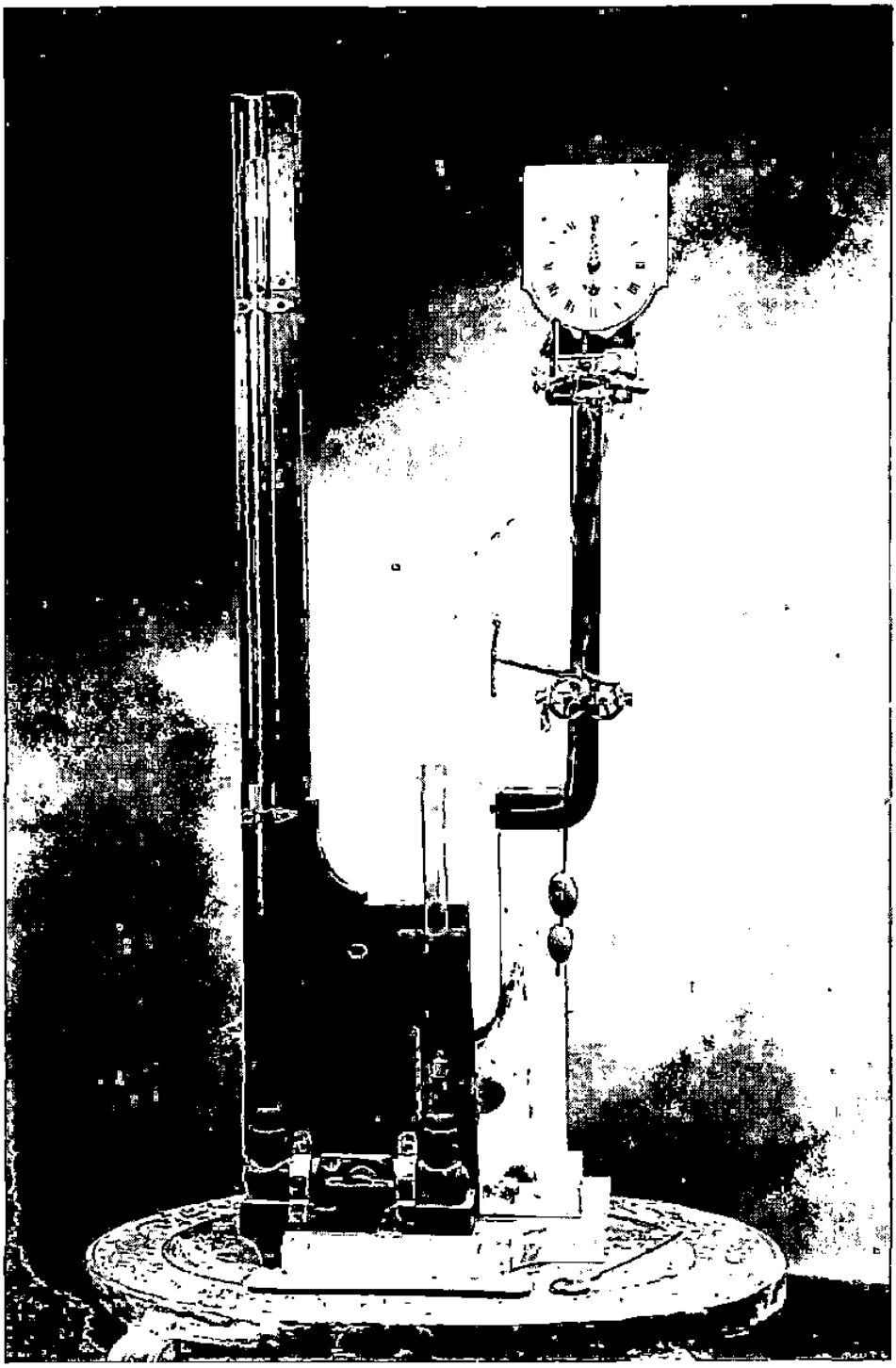

Fig. 7 – Fontana's recording barometer. Now in the Institute and Museum of the History of Science of Florence.

Some glances I have thrown at the nervous system have given a hint of some of its secrets. Philosophy has been mistaken not to dig deeper into physical man; it is there that moral man is hidden. The exterior man is only the projection of interior man.

If I could only allow thoughts to rest on so splendid a subject!

In 1790-91 a Polish traveler, Stanislaw Staszic[f] visited Florence on two occasions. He encountered Fontana on four visits which he made to the Museum. He wrote:

25 June 1790. I have been to see the collection of natural history and anatomy with was models. I saw the Abbé Fontana who showed me the physics laboratory from the beginning. There I saw the instrument for dividing astronomical devices. I saw the lens the entire circumference of which he has himself divided. In the collection I encountered only two or three people who had entered out of curiosity. Generally, ignorance reigns in this country. Science carries no weight; books in French are few; in Italian only on theology. At Florence there is no teaching of the sciences, only art, painting, sculpture, architecture, music, etc.

On 6 October Staszic visited the botanic garden with results as related on p. 288. On 24 May 1791:

I have been to the museum of natural history and physics. I found it even larger, more unusual, handsomer, more valuable, for after having traveled all over Italy I have found nothing like it. This collection and Fontana would make Florence pre-eminent as a place to live if I wished to remain in Italy.
25 May. I have been to the physics laboratory and talked for a long time with Fontana. He spoke to me of some experiments he made with the successor of Bletton[g] whom Dr. Zwinel supports. From his experiments Fontana had deduced that neither water nor any metal had any effect on him because this man had not

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[f] Stanislaw Staszic (1755-1826) is best known for his political writings. Ordained a priest, he studied in Germany and in Paris, particularly natural history with Buffon, whose *Epoques de la nature* he translated into Polish. An interest in geology resulted in a work of 1815 on the mountains of Poland and Carpathia. Back in Poland after 1791 he was active in government and took part in founding an «Association of the Friends of Science», to which he contributed many of the funds for the construction of the home of the Academy of Science. For information about Staszic, I am indebted to Antonella Ottanelli, whose thesis in Foreign Languages and Literature submitted to the *Università degli Studi,* Florence, year 1975-76, presents Staszic's memoirs.

[g] Bletton, a rhabdomantic, i.e. one who searches for springs of water or mineral deposits with a divining rod. He flourished in the second half of the eighteenth century.

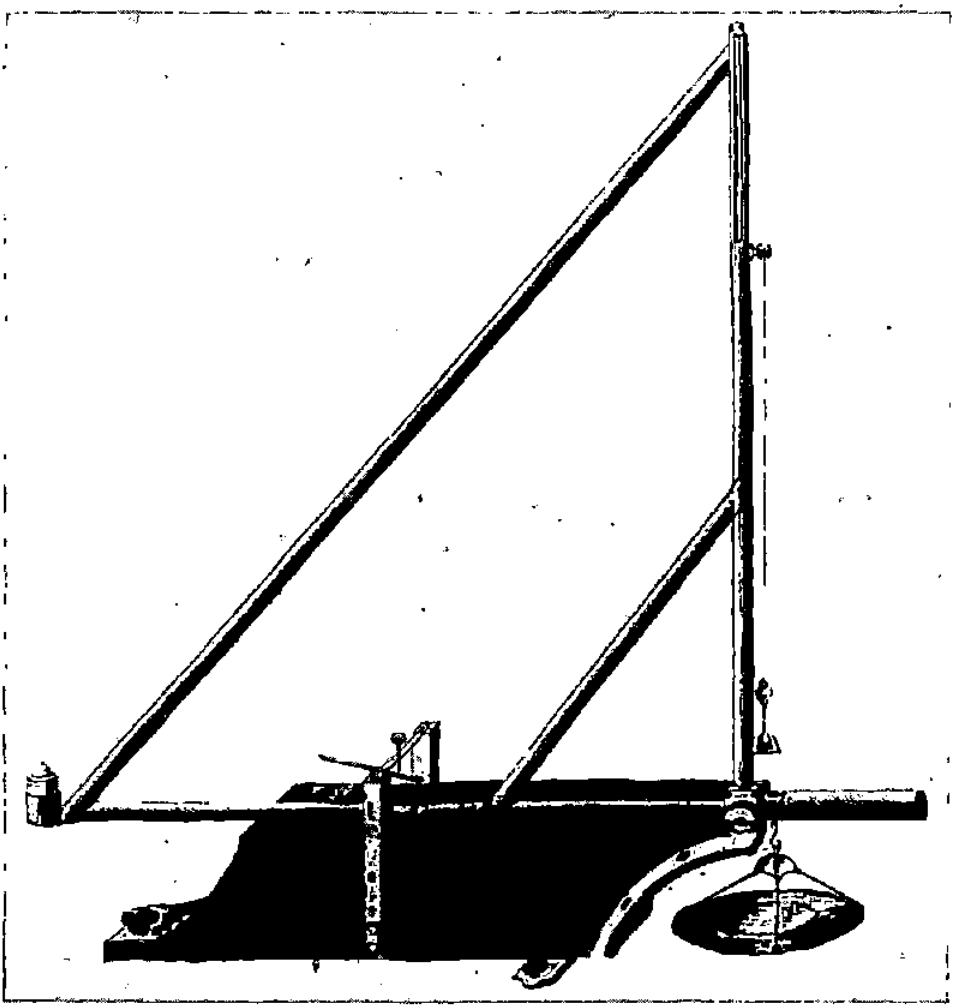

Fig. 8 – Fontana's barometer by weight. Drawing accompanying a letter to Canterzani.

found even one deposit of metal. I then saw that Fontana has some most singular ideas; he acknowledged usually in matter a certain kind of sensibility. .

In 1806 Louis Dutens[h] published a lengthy account[12] of his visit to Tuscany and Florence, relating his encounters with Horace Mann, Milord Cowper, and the Grand Duke Peter Leopold who honored him with many favors. Dutens wrote of the latter's many reforms and improvements: «among other things, he has considerably augmented the gallery, and made a collection of physics, natural history, botany, anatomy, and astronomy, which of its kind is the most comprehensive project I know of».

In order better to compare these accounts, some figures are assembled in the following table. In view of the undoubted growth of the Museum during these twenty years, and the probable «rounding-off» of values recalled from memory, the numbers seem convincing.

Number of Rooms on Display in the Museum «La Specola»

Year	Source	Total	Physics	Natural History	
				Anatomy	Total
1775	*Saggio*[3]	—	8	6	—
1775	Bernoulli[11]	25	—	8	—
1782	La Lande[13]	45	15	15	30
1785	Dupaty[14]	50	—	12	—
1786	Fontana[6]	—	—	30	50
1794	Wichelhausen[19]	—	—	20	—

The subsequent history of the Museum of Physics and Natural History may be followed in the writings of Ugo Schiff,[16] M.L. Righini Bonelli[4, 17] and M.L. Azzaroli.[18] Briefly, in 1807 La Specola became part of the *Liceo di Scienze Fisiche e Naturali* with instruction being given by six teachers, part of the *Istituto di Studi Superiori* in 1859, and part of the *Università degli Studi* in 1923. Many of the collections were moved from La Specola to various Institutes throughout Florence in 1859. In 1927 the *Istituto di Storia della Scienza* was formed and now houses the ancient instruments in the Palazzo Castellani.

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[h] Louis Dutens (1730-1812) fled Paris for England on account of religious intolerance. At Turin he was secretary and chaplain to Stuart Mackenzie, English minister there, later became chargé-d'affaires. He died in England.

CHAPTER 7

PARIS AND LONDON

Letters to Italy - Boščovič - Ingenhousz

As Fontana tells us,[a] the establishment of the Royal Museum in the Casa Torrigiani, acquired in 1771, ruined his health, so that Peter Leopold took pity on him and allowed him to travel in France and England, giving permission in 1773 according to a letter of 26 September of that year[1] from brother Gregorio to brother Giuseppe. Felice began to plan immediately, writing on 15 November 1773 to Ingenhousz:[2]

> before leaving Florence I hope to publish various works which are ready, some in press. I thank you for what you have done in order that I will not be taken for a traveling Florentine hairdresser. In another letter I will tell you my plans.

The plan presented to the Grand Duke[b] may have been the one mentioned by Adami (1905, p. XXXIV) as written 30 March 1775, which was soon approved although its extensive scope was not actually fulfilled, as Fontana visited only Paris and London.

Fontana went first, by way of Mantua where he examined the observatory, to Rovereto, intending to remain there until 15 September. He then went by way of Milan and Turin (15 December) to Paris where he arrived, accompanied by Giovanni Fabbroni, on 13 January 1776.

Our chief source of information about Fontana's life in Paris and London is his own letters, of which we have thirtyseven, from February

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[a] Pages presented in Vienna to H.M. Leopold. Florence, Archivio di Stato, I.R. Corte 119.

[b] Transcribed by Adami, *op. cit.* 1905, but not found in the archives.

1776 to September 1778. Of these, twentyfour are to Angelo Tavanti,[c] six to his cousin Marzani,[d] four to Alessandro Volta,[3] two to his brother Giuseppe,[e] and one to his mother Elena.[e] Fontana's activities in Paris are also mentioned by the Abate Raimondo Niccoli[f] in his letters[4] to Tavanti.

In Paris Fontana apparently resided in the house occupied by the Tuscan mission to the French court, although a letter to him from Maltravers[g] is addressed to the Hôtel de la Sapience, Rue de la Seine; this establishment has not been identified.

The prime reason for the travel to Paris and London was to acquire objects for the various collections in the new R. Museum. Two private collections were of particular interest in that they might be purchased *en masse.* One of these was the property of a certain Maltravers who had found it necessary to flee to Switzerland to escape his creditors in London, leaving behind him his collection in the care of his wife. On 20 June 1778 Fontana wrote to Tavanti that Maltravers had written him two pressing letters, saying that he put great confidence in Fontana's judgment, which was misplaced as he felt no competence with regard to paintings and drawings. Maltravers urged Fontana to be his guest in

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[c] Minister of Finance. These letters are in the Archivio di Stato, Florence, Miscellanea Finanze, Filze 438, 512, 529, 553.

[d] Fontana's letters to his cousins Marzani are now lost. See p. 88, note [a].

[e] Of these two letters of Felice to Giuseppe Fontana, one from Paris, 26 November 1777, is in the Biblioteca Comunale, Trent; it was published by Adami *op. cit.* (1905) and in the Nozze Rossetti-Pegoretti.[1] The other, from London 2 January 1779, and the enclosed letter of the same date to Felice's mother Elena, were copied by Giampio Adami. The last letter was published by C. Adami *op. cit.* (1905).

[f] The Abate Raimondo Niccoli was proposed as secretary of the Tuscan Legation in Paris in 1767 by the Imperial Ambassador there, Mercy-Argentau. In 1779 he returned to Florence as a pensioner, being suceeded by his nephew, Favi. He died 1780. Well-acquainted with American visitors, including Benjamin Franklin, and with Filippo Mazzei, Niccoli never acquired the title of *chargé d'affaires* nor was he widely accepted in Parisian *salons,* although he was admitted to the «Thursdays» of the Marquis de Mirabeau, Victor de Riqueti, who wrote of him at his death: «He was not a man of genius, not even a man of spirit, not even well-educated. [...] but [...] it is a loss». Niccoli had played a part in putting the French physiocrats, preeminently Mirabeau, and the Tuscan physiocrats in touch with each other. The letters Niccoli-Tavanti are in the Archivio di Stato, Florence, Misc. Finanz. 529.

[g] See below. The Maltravers letters are in the Archivio di Stato, Florence, Misc. Finanz. 438.

London in his home on Duke Street, Westminster, and also wrote at Fontana's suggestion to Tavanti accepting Fontana's judgment and urging action. Fontana wrote to Tavanti that he would examine the Maltravers collection on his arrival in London but he avoided accepting his hospitality. Later in a letter undated he wrote that he had examined the collection and recommended that the Tuscan Court not spend a great sum on a collection of things worth nothing. On 30 March 1779 he wrote that it was said that the sale of the collection had failed to reach one hundred pounds, in comparison with the debts of 20,000 pounds, and that Maltravers had been paid for the instruments he had sent to Florence, that he made no claim to be owed anything.

The other collection to be considered was in Paris, that of M. l'Espinasse, about which Fontana wrote (letter undated) to Tavanti:

> Although I saw only a part of the collection of M. L'Espinasse in the two hours we spent with him, I believe that I cannot be far wrong. I judge that at least two-thirds of those instruments are in the collection of H.R.H. One the other hand, it is true that M. L'Espinasse's instruments are in general very good, having been constructed in England. But some of them were made twenty years ago and more, and twenty years ago there were not the artists in England that there are now, nor had physics yet made the progress it has made since. It is not to be doubted that the instruments made now in London are much more perfect, and of more general use than twenty years ago. M. L'Espinasse is convinced that he has spent an enormous amount on his instruments, as he himself has told me. He is too honest a man for me to doubt his word, but it is my opinion that a good part of the great expense is due to the lack of ability of the artists of his times, and the fact that the instruments had not reached the state of perfection they are in today. Being aware of the defects, M. L'Espinasse forced himself to correct them to some degree, which he could not do without many useless and costly attempts.

Fontana wrote that Parisian workmen knew little of instruments of physics, that the physicists were obliged to get them from England if they wished to have something exact. Although prices had gone up as a result of the greatest rage in Paris, even among the women, to have unique pieces of natural history, Fontana acquired 300 pieces, also 5-600 chemicals.

Already by 5 February 1776, the date of the first letter we have of Fontana to Tavanti, he had met the Margrave of Baden-Baden, His Highness the Hereditary Prince, the Duke of Aremberg, the Duke of Ursel, Count Mercy, Vienna's ambassador, and the Duke of La Rochefou-

cauld,[h] who entertained Fontana at a dinner «where were the persons most select in Paris for their birth and learning. Spoken of constantly was the Grand Duke of Tuscany, spoken of in a kind of idolatry. I was questioned a thousand times, and a thousand times I must answer in a language not yet familiar to me. I left there full of admiration and respect».

On 10 April he wrote to Tavanti:

> I had the honor of presenting myself recently to the Count of Falkenstein, i.e. to His Imperial Majesty [Joseph II, incognito] who received me with his usual kindness and even gave me permission to see him again. [...] they speak only of him, his affability, his good qualities. I do not believe any man has ever made a greater impression on a nation. The arrival of the Emperor will set a fashion for the French.

On 23 May he wrote again to Tavanti:

> His Majesty the Emperor, after having allowed me to present myself to him several times here in Paris, yesterday morning was so kind as to have me to dine with himself. I never dared to hope for this new honor or realized that I merited it. I recognize fully the goodness, the special protection, which H.R.H. has so far shown me.

On 26 November be wrote to his brother Giuseppe:[1]

> I forgot to tell you that I have had the honor to see the Emperor several times during his visit to Paris, and that he treated me with much kindness even having me to dine with him, a favor not accorded to anyone else in France; but all this doesn't make me digest better, or sleep more, and I live as usual, unhappy, sad, and unhealthy. *Vanitas Vanitatum.*

Fontana was visited by M. Tillet,[i] Director of the Mint, who as a sign of his esteem of the Grand Duke of Tuscany wished to reveal the wet method of refining precious metals, as used in France. Tillet, believing his process to be superior, showed Fontana how they prepared gold and

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[h] Probably Louis Alexandre De La Rochefoucauld, (1743-1792), amateur scientist. Friend of Guyton de Morveau, correspondent of Torbern Bergman 1778-1782, member of the Swedish Academy of Science.

[i] Mathieu Tillet (1714-1791) lived in Paris, was a member of the Academy, was Director of the Mint in Troyes, and published memoirs on assaying.

silver, but Fontana reserved comment (letter to Tavanti of 5 April 1777) until he could compare the methods directly on his return to Florence.

Persons mentioned as those Fontana saw most often in Paris were the Duke of La Rochefoucauld, Turgot, the minister of State, and Condorcet, Secretary of the Academy of Sciences before which Fontana delivered at least one communication, on the analysis of malachite. He most likely met also in Paris Darcet and Gibelin who both played a considerable part in his scientific activities. In a letter to Tavanti of the summer of 1779 he described these as follows:

> In the great collection of works in physics, which is published in Paris by the Abbé Rozier,[j] are six dissertations of mine, and two others which will be published in a few days. These Dissertations constitute part of my work, for which I requested the favor of remaining in Paris for a year more; twenty other dissertations already finished make up the accomplishment of my work in that city, which will subsequently be published in the same collection. These twenty-eight dissertations could make two large volumes *in quarto;* a third volume *in quarto* is my new work on the venom of the viper, which is about to be published in French. In this way I have been occupied in France, where I have tried to get to know the most celebrated men of that country and to profit from their brilliance and conversation.

In fact, seventeen papers by Fontana appeared in the journal of Rozier between 1776 and 1786, in addition to a polemic note on ergot and tremella, the description of the Royal Museum, a reprint of a paper on the decomposition of water, and a review of the work on the venom of the viper. These seventeen include much of his most important work.

One acquaintance, probably made through his brother Gregorio, was Rudjer Bošković, who was in Paris at that time. The two carried on a rather hectic correspondence[5] in June and July 1777, several times Bošković writing to Fontana twice in the same day, in an effort to obtain Fontana's support for a claim of priority of invention of a geodetic optical device which Bošković had developed, before one invented by the Abbé Alexis Rochon,[k] he said. Fontana wrote to him, straight to the point:

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j Abbé Francois Rozier (1734-1793) was director of the veterinary school at Lyons, edited the *Observations sur la Physique, sur l'Histoire Naturelle et sur les Arts,* which became the *Journal de Physique* with vol. 44, 1794. He was assisted by Mongez and De La Metherie.

k Rochon, Alexis Marie de. Abbé 1741-1817. Prior of St. Martin-la-Garonne, he was librarian of the Marine Academy, curator of the Royal Physical-Optical Collection at Muette, Naval-Astronomer-Optician, director of the observatory at Brest, member of the Academy of Sciences.

I wrote you in another letter that you must read the three memoirs of the Abbé Rochon. Because the third memoir had been read to the Academy the day you gave the Minister your paper, I would assign priority to the Abbé Rochon for all that is found in that memoir, since [...] its observations had been communicated to many Academicians some months before. With much more reason I assign to the Abbé Rochon the contents of the second memoir, which had been communicated to very many of his friends long before you told me of the use of ordinary prisms to measure large angles [...] I do not speak of the first memoir of ten years earlier. I am sure that you did not know of it, as I also did not know of it, but not to have known of it is not proof that it did not exist. The day you left I came to you to say that the three memoirs existed, that you ought to see them before [...] claiming priority for the discovery. I saw then what a muddle would come of it.

And much more to the same effect, closing with the expressed hope that Boščović would come around to this point of view and be convinced of Fontana's sincere regard. This was not to Boščović's liking, which explains the latter's letter complaining of Fontana.

Fontana's hard-working habits are described by Niccoli in letters to Tavanti of April 1776 to June 1777:

there is no-one in Paris who is as strong in physics as the Abbé Fontana, and no-one who is as much of a botanist as that young man he has brought with him. Both Fontana and this young man follow the courses and the experiments in chemistry which are given here by clever men, who when Fontana is present are diminished and, notwithstanding their conceit, defer to him and listen to Fontana as to an oracle. You can't imagine how much I enjoy this, since, after all, I see that shortly we will not need to come here, as the French suppose, to recapture the sciences, which we taught them.

[...] The discoveries which he is continually making, and his astute industry in investigating the secrets of nature, make him honored in this country, and Count Belgiojoso, coming lately from London where he frequented the great scholars of that country, told me that the English themselves regard him as the foremost living European naturalist.

The Abbé Fontana works, perhaps, more than he should with his health so precarious. His experiments and discoveries on air do him honor [...] I don't know what our compatriots think of him, but I assure you that here he has a great reputation and credit, and the English scholars make much of him.

I recommended to him, as you prescribed and I constantly do for love of him, that he should take care of his health. He agreed, and accepted with gratitude our salutary advice, but I haven't much hope for it, since it is diametrically opposed to his passion for making new discoveries, which grows in him in proportion to the felicity he has in finding them. His constitution has improved in this climate, but I fear always that his experiments on new subjects with new methods will make him a martyr to his zeal, as happened recently, when there were by good fortune no consequences worse than his setting on fire his clothing and some furniture.

Fontana's health suffered in Paris, according to his own accounts. On 28 December 1777 he wrote that the weather was very raw and changeable, that for more than a month he could not leave the house without feeling the effects. On 28 February 1778 he had been confined to the house for more than two months by his poor health.

Financially he seems to have done well enough. According to Adami (1905). Fontana's proposal had a note appended, dated 15 May 1775, expressing the opinion that the annual expense would be 2,000 *scudi,* plus one-third more if he was accompanied. At another time the estimate was 1,700 *zecchini* (=3,400 *scudi*). For the trip to Paris, 350 *zecchini* were awarded. A revised estimate sent from Paris on 4 March 1776 was for 3,000 *scudi* or 1,500 *louis.* On 19 February 1776 he wrote:

> Since H.R.H. has not set my annual travel expenses, I regulate myself following your advice by keeping myself simply to the bare necessities, but it is quite true that I shall be afraid of going wrong in one way or another. As a curiosity I tell you that the cold here is so great that my fire costs five *paoli* (= one-half *scudo*) a day and I shiver all day. One pays as much for water as one pays for wine in Tuscany, and it is unhealthful to drink, so I'm compelled to mix it with wine, which even if not of good quality costs five *paoli* a bottle which weighs three florentine pounds, thus in two persons as we are, we use per day wood, wine, and water costing ten *paoli* and more.

From Fontana's letters we learn of twelve drafts of 360 *zecchini* each being received over a period of forty-two months, although there may have been others. He thus received at least one hundred *zecchini* per month, or quite likely his revised estimate of 1,700 per annum.

From his letters to his cousin, Sign. Dr. Lorenzo Marzani de Steinhoff in Villa di Nogaredo near Pomarolo, we know that Fontana's salary from Florence (as Director of the Museum, Physicist to the Court, and Professor at the University in Pisa) was sent to Marzani, as well as some money from Paris. Of this income, Fontana asked that 100 *fiorini* (33 *zecchini*) be paid yearly to his relatives, Elena his mother and Giuseppe his brother. In addition he paid certain debts of his brother Bernardino and some unusual expenses of his mother. The remainder was invested for him by Marzani until he needed and requested it later in life.

Fontana left Paris for London probably in September 1778. From a letter to Marzani, and one of Sir Joseph Banks to Giovanni Fabbroni, we know that he resided at one time at the Haymarket no. 13, but Wilson's letter places him in Bloomsbury. It appears likely that he left Paris on

bad terms with Niccoli, although Giovanni Fabbroni wrote lengthy accounts of their experiences in London to Favi, Niccoli's successor.[6] The path had been prepared for him, no doubt, by Jan Ingenhousz, so that he was able to get to work immediately, possibly in the laboratory of the Royal College of Surgeons, presided over by Cruickshank (1745-1800), although there is no evidence for this. He presented several communications to the Royal Society and continued his researches but his major effort was devoted to obtaining items for the Royal Museum, for instance «hundreds of pieces of natural history» but chiefly physical instruments, some for the observatory and some for the instruction in physics of members of the Royal Family. In this he was greatly helped by Maskelyne,[l] the Royal Astronomer. From Jesse Ramsden[m] he obtained a list of instruments proposed for the observatory.[7]
Fontana's comments to Tavanti on these were:

> The pages added here are laid out by Mr. Ramsden, who without doubt is the most capable of all. They have been approved by Mr. Maskelin, royal astronomer at Greenwich, who has agreed with all items, and believes that an observatory equipped with such instruments will be the foremost in the world.

Fontana had been commissioning instruments of physics for demonstrating the laws of physics; the final execution of these, and the completion of any astronomical instruments to be ordered and which were to be supervised by Maskelyne, were to be paid for through Ambassador Belgiojoso. Fontana had also been intrigued by a water-pumping machine of Boulton, which could, he wrote, raise 500,000 cubic feet of water one foot by burning 200 florentine pounds of ordinary wood, although here, with four such machines in operation to provide the entire city of London with water, the fuel used was «fossil carbon». Actually, he wrote, the machines could raise water to 120, 500, even 2,000 feet, and would be most useful in the Maremma where wood is so cheap.

Since there was a war on, Fontana and Fabbroni could not travel via Calais, but had to go through Ostend, leaving 15 October, passing through Amsterdam and Dijon, arriving in Florence early in 1780.

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[l] Maskelyne, Nevil (1732-1811) F.R.S. in 1758, Royal Astronomer at Greenwich from 1765 until his death.

[m] Ramsden, Jesse (1735-1800). Renowned English maker of instruments.

CHAPTER 8

RETURN TO FLORENCE

The *Annus Mirabilis* - Departure of Peter Leopold

When Fontana arrived in Florence early in 1780 he found his home in disorder. The Museum had been left in charge of Giuseppe Pigri, Professor of Mechanical Engineering we would call him; Fontana did not comment on how he found the Museum, only that Pigri had accepted personal donations, which he, Fontana, had never done.

Bringing with him the large manuscript in French on the venom of the viper, Fontana delivered it to the printer, whose name we do not know. More experiments seemed to be needed; they were performed and the account added to the work as a supplement; it appeared in 1781.

Probably at the time of his return, Fontana first saw a work by Haller.[1] Although its second volume was dedicated to Fontana in glowing terms:

I, HALLER, DEDICATE THIS WORK TO MY DISTINGUISHED COLLEAGUE FELICE FONTANA, TO THAT GREAT GENIUS FOR WHOM NOTHING IS DIFFICULT IN HIS SEARCH FOR THE TRUTH, IN TOKEN OF GRATITUDE AND IN HOMAGE.

it also criticized his *Fisica Animale* which led him to write a reply, published anonymously as *Lettera Apologetica*.[2] It gave Haller rather rough treatment and ended as follows:

> That the immortal soul of the great Haller may be at peace, I finish my letter with the words used by the Learned Editor[a] of the *Animal Physics:* «Whoever will read

[a] Fontana himself, according to Spallanzani.[20]

this work (he says), without prejudice, and with that enlightenment which is so necessary for understanding it will know immediately that irritability is no longer a vague and uncertain hypothesis as it was before but a demonstrated truth, and that the laws established by Our Author are perennial sources of understanding of the obscurities of the movements of animals».

The instruction of the Royal Family in experimental physics with the instruments arrived from England or built at La Specola began at this time. An outline of the course is given in a memoir that Fontana prepared for Peter Leopold at the time of his departure for Vienna with his son Francis.

As for Fontana's own researches, his *annus mirabilis* was 1782, when he published seven articles. Five of these were new, two being reprintings: the one on «fixed air», the other an Italian version of that in English on «inflammable air». Of the five new ones, the most important is his «Letter to Murray» which consists of 25 pages on animal respiration, eight on fixed air in the atmosphere, six on acids, five on weight changes in chemical reactions, four on the adsorption of gases by charcoal, and two on vegetables and the tremella. Other new writings were on the elasticity of airs, the physical principles of solids and liquids, on light, flame, heat and phlogiston, and his final pronouncement on the conversion of water to earth (printed twice this year).

In 1783, eight publications appeared, but only three of these were new; one on antidotes to the venom of the viper and the contents of the nervecylinder (printed twice), one on hydatids, tenia and the crystalline lens, and one on animal respiration (which is largely new). Four others were reprintings of articles published in 1782 and one other, a book entitled «Scientific Works», which reprinted seven earlier papers plus one new one on heat and thermometers.

In 1784, two reprintings of articles appeared, also a book, the «Scientific Works», with eight parts from the same book of 1783 plus the *Lettera Apologetica* of 1780 (thus admitted to be by Fontana), all of these in French. This was published in Paris by his friend Gibelin.

The seven publications of the years 1785-6 were devoted entirely to the experiments on the decomposition of water; they were no contribution to science.

In 1787 one original publication appeared, a letter to Guyton de Morveau, also saying little of consequence. However, in that year the «Scientific Works» and the paper on the rust of wheat were printed again, this time in Naples, as was an Italian version of the Treatise on the

Venom of the Viper; also there appeared a complete translation of that work into English (London) and into German (Berlin).

In the years 1788 to 1791 Fontana published nothing. One reason for this may have been his painful preoccupation with the anatomical waxes for Vienna, which he described so vividly.[3] Another reason may have been some deterioration of his character, his ability, and of the relations with those around him, including Peter Leopold. The Grand Ducal family spent the winters of 1782-3 and 1783-4 largely in Pisa, which may have contributed to an estrangement, and by 1784 more and more authority at the Museum was being entrusted to Giovanni Fabbroni.

In 1786 when the Giorgi controversy broke over Fontana's head there were overtones which indicate an antagonism towards him, ostensiby as scientist. At bottom, Desgenettes suggested,[4] it was directed against the Jansenist tendencies of Peter Leopold. It may be doubted that this endeared Fontana to the Grand Duke, nor did the legal action brought against Giorgi, which the Grand Duke eventually had to quash.

In the spring of 1787 revolt broke out, in the Low Countries under Hapsburg domination, against the policies of Joseph II. In August of that year, war was renewed between Turkey and Russia; Austria being allied with the latter would necessarily be involved, Tuscany the same. Accompanying these preoccupations was a strain in relations between Joseph and Peter Leopold; both brothers suffered ill health. In December Joseph returned from the campaigns gravely ill, but Leopold was not informed of this until January 1790. In February Joseph wrote Leopold repeatedly begging him to come to Vienna, even to become co-regent with him. On the 14th came a letter to Pisa advising that Joseph's prognosis gave no hope, whereupon Leopold appointed a Regency Council for Tuscany and went to Florence on the 17th. On the 20th a courier brought the news of Joseph's death. Not for eleven days did Leopold leave for Vienna.

> This delay has been variously interpreted and at times severely judged. Was it a sudden illness, was it a calculated plan, was it the necessity not to leave Tuscany precipitately and also to put in order his private papers in the certainty that now the Florentine sojourn might be forever finished? It is difficult to say.[5]

Numerous letters written during this time show that Leopold knew perfectly what he must do as successor to his brother. He must:

make peace with Turkey, prevent war with Prussia and Poland avoiding thus the danger of a new European war in which Italy, the King of Sardinia, the Duke of Parma who aspired to the throne of Tuscany, and finally Spain in so tense relations with Naples, would easily be involved; recover by negotations and concessions the Belgian provinces [...] take action to obtain the Imperial crown.[6]

For Tuscany, as he wrote to his sister, he thought to revoke the abolition of the secundogeniture which had been imposed upon him, to assign to his son Ferdinand the Grand Dukedom of Tuscany.

We can see why eleven days did not include an interview with Fontana, as he bitterly lamented.[7]

On the first of March Leopold left Florence. With this penultimate glimpse of him, we ask: what kind of Grand Duke had he made? What did he think of the Tuscans? What did he think of Fontana? To what extent did he influence Fontana's activities? What did the Tuscans think of him?

Leopold found himself in full accord [...] with the most important of his Tuscan Collaborators. The Tuscan reformers, predominantly jurists, who had been trained at the University of Pisa, worked along the line of the great empirical tradition of Galileo and the *Accademia del Cimento;* they were resolute adversaries of scholasticism and at the same time of cartesianism, indeed, in general of «that perennial plague of the systems» which would explain world phenomena by deduction rather than investigating them empirically.[8]

We today can grasp better the essential character of Leopold's method of governing when we realize its «experimental» nature, a method admirably suitable to that form of practical eclecticism then prevalent, open to accepting suggestions, ideas, and experiences of theorists and practicians from all countries and times, testing however their applicability. Leopold, passionate experimenter that he was, conscientiously applied to the solution of problems of collective human life, political and economic life, that same scientific method which he had well tested in laboratories and at his chemical bench. Thus he was in perfect harmony with his time, convinced to a degree rarely seen in other epochs either before or after, of the possibility of «scientific» solutions to political and economic problems.[9]

What Peter Leopold thought of the Tuscans can be gleaned from his own writings, completed shortly before he left for Vienna. Some excerpts, in translation, are:[10, 11]

The Tuscan nation, in general talented, lively, and gentle in character, is very easy to control with kindness and persuasion, severity and harshness never being needed. Serious crimes are rare [...] but continuous and frequent are the lack of

good faith, fraud, cheating on contracts, falsity, and all those kinds of offenses arising from deceit, self-interest, and bad faith which require talent, cunning, and far-sightedness, but neither firmness nor courage.

In Florence the nobility is extremely ignorant, not studying or applying themselves at all, occupied solely in idleness, without culture or education and largely devoid of honor. [...] they value solely birth [...] the nobility of Florence has always will be contrary and inimical to the government [...]

The second class, comprising townsmen, physicians, and attorneys, is the most cultivated, the most learned, and better brought up [...]

The class of so-called merchants is made up of a few bankers, who are all polite and correct [...]

The priests in Florence are excessive in number [...]

The people, the workers and all the rest of the lower classes are good, of excellent character. Generally they are prone to ways of idleness and avoidance of effort [...] The workers do not lack aptitude, ability, and skill, but do not like to exert themselves, are not assiduous in their work; they are thoughtless but want to be well paid [...]

Peter Leopold wrote about the petty officials of his court:[12]

Generally speaking the common defects of the members of the staff in Tuscany are negligence, choosing to be occupied but little with business, spending little time in the office, relying entirely on subordinates for the details of business, coming in to the office more for appearance's sake, the least and as late as they can.

On comparing these writings with the memoirs of Fontana,[3] one feels that they might have been written by the same person. About the staff at the Museum, Peter Leopold wrote:[13]

there is the Abbé Fontana, whose merit, capability, and activity are well-know; it would be desirable if he were of a nature readier to get along with other people and if, not so often changing his mind, he did not always give occasion to spend, also if he knew better how to keep subordinates and workers contented; he should be limited to the scientific side and restrained in his vast projects for buildings and continuous and costly alterations. Giovanni Fabbroni, his assistant; this one's merit, ability, honesty and zeal in service are well-known, without exceptions or room for improvement. Custodian Gagli, reliable, faithful, and honest.

Fontana's regard for Peter Leopold, as expressed in his memoirs,[14] was one of deep respect, although this he could allow to slip, as when he

wrote to a confidant[b] about Peter Leopold's knowledge of how to use a quadrant: «You make me laugh». He saw him as a ruler trying to be scientific, one whose feelings for the arts and humanities were mixed. Peter Leopold dissolved those ancient (and to some degree decrepit) academies, the *Accademia Fiorentina,* the *Accademia degli Apatisti,* and the *Accademia della Crusca,* but he formed a new Florentine academy attached to the Magliabecchian library. He dissolved the lethargic botanical society, but supported the *Georgofili.* In his travel notes he never commented on the beauty of a landscape or a work of art; if he stated that a certain place had a «fine church», it was not for its artistic, architectural value but for its acoustics favoring the exercise of its religious function. However, he founded the *Accademia delle belle arti* and promoted the causes of the Uffizi gallery and the *Loggia dei Lanzi.* Whatever may be said of Peter Leopold's own taste for art objects, it must be noted[15] that through his employment of the architect Paoletti (so reviled by Fontana[3]), the Uffizi was graced by the *Sala di Niobe,* and the Palazzo Pitti by the *Sala degli Stucchi.* Through the employment of Bencivenni Pelli and Luigi Lanzi the Uffizi was reorganized by restoration of its glories to the way we see it now, and the subject of art history given its modern style. Although he received cordially the fourteen - year - old Mozart in 1770, suggesting that he give a concert at his favorite villa, Poggio Imperiale, for which Mozart received 300 *lire,* he turned a deaf ear to proposals that Mozart join the Grand Ducal Court in 1773. Perhaps the influence of Maria Teresa was operant here; she wrote in 1771 to the Archduke Ferdinand, governor of Milan, advising him not to take into service the Mozarts as they were «useless people». Nor did Mozart find employment or help from Leopold as Emperor later in Vienna.[16]

Peter Leopold had been instructed by his parents to encourage the «useful arts» and the «useful sciences». In fact, the utility of science was in the spirit of the epoch.

> The program of exploring nature was justified, basically, by the multitude of benefits it would secure to man on the practical level. The theme of conquering the physical world, which had spurred the efforts of the Renaissance, was given a two-fold utilitarian evaluation by Descartes. First, scientific activity found immediate, personal application as both the content and purpose of what Gouhier has called «the life of the intellect». Second, the closing page of *Discours de la*

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[b] Bettina Slop. See p. 92-3.

méthode, in stressing the incalculable uses of the new physics for medicine, mechanics, and morality, broke with the verbalistic science of the Schoolmen, among other ways, by making preeminent the scruple of public utility; «Instead of that speculative philosophy that is taught in the schools, one might find a practical one», etc. The Enlightenment strove to carry out in its full sense the messianic overtone of Descartes' natural philosophy.[17]

Between Fontana and Peter Leopold, influences were mutual. However, Fontana's major interest, an intensely practical one, his work on the venom of the viper, began in Pisa in 1764, before he was appointed to the University, before he had met Peter Leopold. Fontana's major «discovery», of the gas-adsorbing power of charcoal, had then no practical or useful attribute; now it does.[18] Both Peter Leopold and Fontana loved science for its own sake but were concerned with its utility, both were children of their time.

What one Tuscan felt about Peter Leopold may be learned from Giuseppe Pelli Bencivenni who wrote in his *Efemeridi* of 1799:[19]

Yesterday evening I had occasion to speak at length with [...] one of the confidential secretaries of Leopold, about the private life of this prince, with many anecdotes about him, of interest in understanding the fundamental of his character [...]. He insisted on not being deceived. He was sharp and quick-tempered, but he knew it and controlled himself. He never tired of taking notes, etc. He had a room, «of the scandals», he called it, where he kept all the papers which informed him about his employees, his subjects, those around him. On his departure, he burned all these pages [...]. He relied greatly on chemistry for relaxation when in bad humor, he wrote down his observations; they say that after his death Lagusius had a book of them, but none who sought it could find it [...]
I have heard that he was not loved in Vienna; he always remembered Tuscany and not seldom said that his vocation had not been a happy one.
His defects were human, his virtues were more than human.

CHAPTER 9

LAST YEARS

A New Grand Duke - Death - Burial

In May 1790 Peter Leopold's family followed him to Vienna, leaving the Regency Council to govern Florence, although Peter Leopold was still Grand Duke. He neglected to inform the Florentines of his act of renunciation in favor of his son Ferdinand until February 1791, although he had signed it seven months before. He had learned of the riots which broke out in June 1790 in Pistoia, Leghorn, and in Florence, where a mob of peasants and artisans, impatient with being prevented by rising prices and lower wages from sharing in the benefits which relaxation of governmental controls had brought to production and distribution, and deprived by royal decree of spiritual sustenance through the removal of certain churchly trappings, had overturned the economic and ecclesiastical structures to which he had devoted so many years of hard work. No one gained from the riots; Peter Leopold saw many of his reforms swept away, as were the most hated men in Tuscany, two of his closest collaborators, Scipione de' Ricci, Jansenist bishop of Pistoia and Prato, symbol of his ecclesiastical reforms, and Francesco Maria Gianni, senator, councilor, and principal adviser to the Grand Duke on financial and economic problems, symbol of his economic reforms.

In April 1791 Peter Leopold (now Emperor Leopold II) and Ferdinand came to Florence; the latter was proclaimed Grand Duke 24 June, and Peter Leopold was back in Vienna by 20 July, not having granted an audience to Fontana during his visit. A memoir setting forth his grievances was composed by Fontana for the Emperor Leopold II, but it is doubtful if the latter ever saw it; by 1 March 1792 he was dead of pneumonia, his fever and chest pains having been treated by subjecting him to four bleedings and eighteen clysters in two days.

After four years of silence, Fontana began to write again in 1792. Three publications were in the form of short letters, all to Giuseppe Mangili, on adipocere,[1] the intercostal nerve,[2] and electricity as the cause of muscular contraction.[3]

His essay on generation[4] is one his most thoughtful compositions, although we do not know any details of his own experimentation on the subject. According to Mangili, who spent some time with him during the years 1792-3, Fontana's major interest was anatomy, which led Giuseppe Brambilla, younger brother of Giovanni Alessandro, to write[5] to Mangili in 1793 and 1794 proposing that he come to Florence for study; apparently nothing came of this.

Also from correspondence with Antonio Scarpa,[6] we know that anatomy occupied Fontana throughout 1792-5. Activity in the physical sciences in the Museum had generally decreased; the natural sciences, chiefly botany, were pursued by Ottaviano Targioni Tozzetti (son of Giovanni) and Attilio Zuccagni. The astronomical observatory had been completed in 1789, but an astronomer had not been nominated.

Now there appeared on the horizon the darkest cloud yet – from France. Peter Leopold had been pleased to learn of the rising republicanism there, and of the establishment of the Estates-General in 1789 when he prophesied future greatness for that country, but the increasingly radical demands of the revolutionary leaders and the problematic future of his sister Marie Antoinette changed his attitude to one of watchful waiting. The attempted flight of the royal family in 1791 spurred Leopold II to activity, but this led to declaration of war on Austria by France 20 April 1792. Tuscany under Ferdinand III attempted to remain neutral, but Britain objected violently to this, insisting that relations with France be broken, constraining Ferdinand to do so in 1793; he restored them in 1795.

With the rise of Napoleon Bonaparte came the first French invasion of Italy and in 1796 Napoleon entered Florence. Eyes alert for art treasures he wrote to Paris, «I have seen in Florence the Medici Venus, which our Museum needs, and a collection of anatomical waxes which it would be well to have». Actually Fontana had already begun work on these waxes, as we learn from letters[7] which may have come from the French ministry (they are in French). One of them read:

Florence 20 February 1788

Some of the organs represented have been cast in molds (forms). All these molds (forms) of which there is an immense number are, *in their state of perfection, in the hands of the creator of these works.* It is certain that they could not be made again

> except by the expenditure of a great deal of time and money. In fact, in order to have the plaster mold for an anatomical statue one begins by having a model made ... this operation requires about six months of daily work ... everything being guided and corrected by the Anatomist Director ... it is when the casting is done that then begins the greatest attention and the greatest trouble on the part of the Director, who needs no fewer than *two hundred cadavers* and no less than a year of work ... These manifold labors constitute a *special art for which we are entirely indebted to M. Fontana!* What is done here in a few months under his direction could not be done elsewhere *or by anyone else,* except in an infinitely longer period of time. All this great collection, which might be believed to be the work of several centuries, was made in very little time, and it is a prodigy that we owe to M. Fontana ...

Note Giovanni Fabbroni's comment[a] on this letter. In spite of differences of opinion among Fontana, Giovanni Fabbroni, and the Florentine bureaucracy (see the Fabbroni letters) it was decided that Fontana should proceed with the waxes for France, under the conditions set by him.

> His Royal Highness gives permission to Director Fontana to make use of the plaster molds and the anatomical waxes in his museum in Florence, as he made use of them for the Imperial Court.[8]

At Napoleon's request, the commission for models was extended from those in wax to those in wood.[9,10] The fate of these models was a source of great distress to Fontana. As a result of jealousy on the part of a French anatomist, the waxes destined for Paris went to Montpellier, where they are still.

For the Tuscans, the war between France and Austria began in earnest with the movement of French troops under General Gaultier from Bologna to Florence on 25 March 1799 and the establishment of civilian rule under Commissioner Reinhard. It was intimated to the Grand Duke that he leave; on 27 March he did so. On 9 April the Piazza Grand Ducale (now Piazza della Signoria) was renamed Piazza Nazionale and a «tree of liberty» erected in it. «Liberty equality and fraternity» were the rule; in fact organized expropriation took place, nominally restricted in principle to the property of the crown, not that of the nation, which enabled Fontana to preserve the contents of the Royal Museum.

Reaction against the invaders and their real or avowed principles began in May 1799 in Arezzo, where echoed the cries of «Hurrah for the Grand Duke», «Hurrah for the Emperor», «Down with the Tree of

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[a] See p. 132. The punctuation here is as in the original.

Liberty», «Death to the French». Commissioner Reinhard issued an edict on 19 May that Arezzo and Cortona were to be burned. They were not; instead an Austria-Russian coalition defeated the French under Macdonald at the battle of Trebbia, 18-20 June, and the «Viva Maria» movement took Siena, sacking the city, with the suspected Jacobin Mascagni barely escaping with his life. By July the French had abandoned Florence to its rule by the Senate, which banned the Aretini on the 5th and welcomed them on the 6th; on the seventh they entered the city.

Florentine reactionaries joined them in sending many renowned and venerated citizens to the Bargello, especially those suspected to be Jacobins; many eminent people including senators were confined to house arrest. Fear of the French, who were still at Leghorn, Pisa, and Pescia, stirred the insurgents to activity, and on the night of 8-9 July they began to sack the city. Since the effectiveness of the Senate was at a low ebb, a provisional government was established; when it was dissolved on 15 September it offered control over the «Jacobins, Jansenists, French and patriots», who were being held in prison, to the Senate. Then a «Summary Political Inquisition» was set up with the Assessor Cremani at its head. Its purpose was to discover and purge the country of those with «democratic principles», of any «patriotic infection». Recognized by the Senate, this tribunal, this «black chamber», proceeded with great fervor to persecute Jansenists and Jacobins. Out of the 32,000 who were tried in 15 months for «sympathy with the French», some were ordered to absent themselves from the country in order not to incur insults and abuse, many were thrown in prison, many put in the pillory.

> The Cavalier Felice Fontana, organizer of the Museum of Physics for Leopold I, arbitrarily arrested and mistreated by the rabble, although the Tribunal had declared a non-suit, had nevertheless to leave a land which wrongly had never shown itself hospitable to him.[11]

It is not clear that he actually did depart from Florence or Tuscany.

On 20 July the Austrians under General Klenau entered the city and restored some order; they left a Tuscany exhausted financially but hoping for the best. Instead, they got the French – again. Napoleon, returned from Egypt, became First Consul, abolished the Directorate, defeated his allied opponents at Marengo 14 June 1800, and sent his army to Florence in mid-October under General Miollis who formed a local government, the Triumvirate, and gladly threw himself into Florentine social life.

The renewal of intellectual life, in which Fontana took part, was signalled by the attempted revival of the *Accademia del Cimento*. By a decree of 27 February 1801, the Triumvirs established the Academy with an endowment of 5,000 *scudi* per year. This support was to be provided by an annual tax of 80 *scudi* on each monastery in Florence. Every ordinary member was to receive 250 *scudi* per year. The officers were: Protector – Napoleon Bonaparte, First Consul of the French Republic; President – Felice Fontana; Secretary – Pietro Ferroni. Its constitution, dated 27 February 1801 and signed by the Triumvirs, was largely written by Fontana. It begins by defining *Cimento* as scientific experimentation. Of its twenty-five articles, the first recalls the earlier Academy which became the model for those scientific academies established later, and was devoted above all to the «art of experimenting».
The all-important Article 15 declared:

> The concern of the new Academy, as with the old one, being the knowledge of the properties of matter, insofar as they may be rendered appreciable, or be demonstrated through experiment, that is to say, all experimental physics (and consequently chemistry, as it examines the composition of natural substances), may the chief objective at which the Academicians should aim always be this particular branch of useful philosophy.

The Academy was to maintain connections with other academies, libraries, universities, schools, of the sciences and the liberal arts, but should never forget that the immediate aim «must continue to be the discovery of new truths in the three natural kingdoms». Others, such as Giovanni Fabbroni, were not in sympathy with this concept, wishing for the Academy to be concerned more with public instruction.

On 16 March the Academy was inaugurated in the library of the Museum La Specola. The French government was represented by General Joachim Murat and other generals. Triumvir Chiarenti, because of his researches, was a member, as were, to mention some familiar names, Paolo Mascagni, Giovanni Fabbroni, Berthollet, Volta, Slop, Pietro Moscati, Gregorio Fontana, Gaetano Cioni, Ferdinando Giorgi (!), and twenty-two others. Pietro Ferroni spoke, dedicating the session to Napoleon, reciting the governmental decrees establishing the Academy, and delivering a discourse on the history of the *Accademia,* also on the experiences with atmospheric electricity as told in a memoir by the President, Fontana, and a proposal by Cioni for measuring, and learning the laws of, friction and traction. Ferroni closed his discourse with a discussion of progress in dynamics, demonstrating the fundamental

principles of mechanics including an outline of experiments performed with the new balances. If time had permitted, there was to have been a description of a most ingenious apparatus there at hand, invented by Fontana, for measuring the hardness of bodies.

Finally the Government was acclaimed for acting to render eternal the illustrious memory of so momentous a day by placing a marble plaque at the entrance to the Museum, on which would be sculpted:

NUOVA ACCADEMIA DEL CIMENTO
LI XVI MARZO MDCCCI

Unfortunately for this effort, on 26 March the Triumvirs resigned, having fallen out with Murat. The succeeding Quadrumvirs on 28 March annulled all that had been done before, so that in spite of the protests by Fontana and Ferroni, aided by Murat, it was decreed on 1 April that the new Academy should remain revived, but that its confirmation, its endowment, its constitution, and its members' stipends must be reserved for decision by the Sovereign. But who was the Tuscan Sovereign? Ferdinand had fled in 1799 and had renounced Tuscany when given Salzburg in 1801; the French were occupying Tuscany only through friendship.

On 28 July 1801 Murat announced to the Florentines that their new Kingdom of Etruria was to be ruled by Ludovico of Parma. Among such high-sounding words the *Nuova Accademia del Cimento* was forgotten, so that its first session was also its last.

The new King arrived in Florence 12 August 1801, but because of illness went immediately to Spain under the medical care of Attilio Zuccagni. Returning in January more ill than when he left, before his death 27 May 1803 he named his four-year-old son as his heir, with his widow as Regent.

Fontana was not idle in these last years but devoted himself chiefly to «cultivating his garden». In 1800 his article inspired by observations of a climbing plant but dealing with some fundamental problems, which is discussed in Chapter 15, was published in Paris,[12] and an English version appeared in 1802.[13] In 1803 his article on the false ergot and tremella was reprinted in Paris.[14] In 1804 and 1805 two publications appeared as «letters to Scarpa». One of these[15] reiterated his claim that movements of the iris were volutary, and added to this category respiration, sneezing, coughing, vomiting, hiccoughs, and convulsions, saying that this would be shown in articles to be published. Now he also added that he could accelerate his pulse at will, but that he still was

Fig. 9 – Fontana's death mask. At La Specola.

looking for the crucial experiment to prove that all these phenomena are dependent on consciousness *(sentimento)*. The other communication[16] reiterates in the text and lists as conclusions his claim of voluntary control over his iris and adds that he can move his ears at will. The major part of this paper describes his negative experiments on the production of heat by plants, discussed in Chapter 15. This last point also appeared in a posthumously published paper of 1805.[17] Another posthumous publication of 1806[18] gave a German translation of parts of these «letters to Scarpa».

Two papers which never were published were mentioned in seven letters to Giuseppe Mangili in Pavia.[19] Three are undated, but all appear to have been written between 10 June and 20 August 1804. They speak of a paper on physics, on the properties of matter, which he sent to Mangili by mistake and asked to have returned to him. The other paper, about his demountable anatomy in wood, he was anxious to have published as soon as possible, perhaps in Milano or as a French translation in Turin or Paris, but not under Fontana's name as it was written by a friend who, for the moment at least, did not wish his name known. Nothing more is known of these two papers. That he repeats himself in some of these letters suggests that Fontana's mental powers may have waned.

On 11 February 1805, as he was returning alone to his home, Fontana fell unconscious in the street, striking his head as he did so. A good friend, the Duke Bonelli, passing in his carriage, took him to his (Bonelli's) home, from there he was soon moved to his own home where he shortly recovered consciousness. Surrounded by friends, including Paolo Mascagni and a German anatomist, Paul Masuryni,[20] whom we have not succeeded in identifying, he made his will and requested that a post mortem examination be performed. The report of this indicates that there were two important intracranial lesions; one in the corpus striatum characteristic of apoplexy, a «vascular accident», the primary event which caused his unconsciousness and fall, resulting in the second, the tearing of a vein with a slow leaking of blood into the subdural space which was eventually the cause of death.

Adami (1905) records that he died at six in the morning of 10 March after receiving the holy sacraments and repeating the words of the dying Augustus: *plaudite amici!* For the purpose of making a death-mask, one Vincenzio Ciampi, molder, who had his own shop as a maker of casts and wax models, was summoned. He was known at the museum, having been called there in 1793 to evaluate the plaster models then on hand.[21]

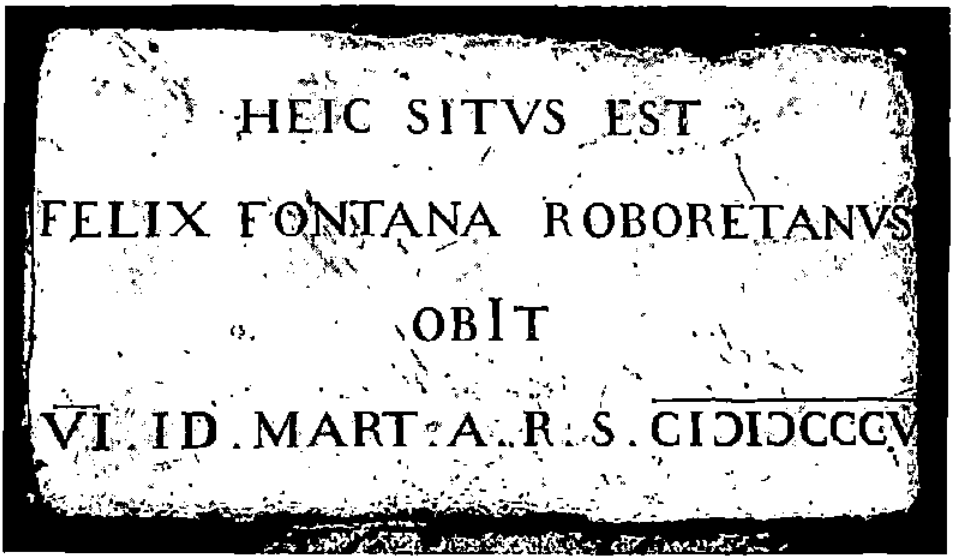

Fig. 10 – Fontana's gravestone in Santa Croce.

For some unknown reason, this mask (Fig. 9) was not paid for until 1814, as attested by an entry on a printed form[22] entitled: *Petty Expenses paid by the undersigned at the Imperial Museum of Physics and Natural History during the month of December 1814.* The entry reads:

> [Item] 27 to Gaetano Ciampi for the mask of the deceased Director Felice Fontana £ 10
>
> [signed] Luigi Baci, book-keeper
>
> De' Bardi

Gaetano Ciampi was presumably the heir of Vincenzio. De' Bardi was the director of the museum. The death-mask was found by Antonio Martelli in 1974 among the plaster molds used in making the wax models, which were still conserved at the Museum La Specola.

The body, in wooden and lead coffins, in which was placed an obituary on parchment, remained in a mortuary chapel in his home in Via della Fornace 2,[b] until services were held in his parish church, San Piero in Gattolino. It was then buried in Santa Croce, in the Chapel of the Medici, or the Novitiate, built by Michelozzo Michelozzi (1434) for Cosimo the Elder. A simple stone in the pavement, 27×50 cm., one of only two in the chapel, bears the inscription seen in Fig. 10.

[b] The location of this building has not been established.

CHAPTER 10

CONTROVERSIES

Targioni Tozzetti - Corti - Roffredi - Giorgi

Targioni Tozzetti.

Early during Fontana's residence in Tuscany, he became acquainted with Giovanni Targioni Tozzetti; their relations were good enough for Felice to consult Targioni about Tartarotti's illness. Then in 1767 appeared a book by Targioni entitled «Alimurgia», i.e., on the relief of famine. In a total of 367 pages, it devoted two and a half pages to the necessity and utility of the book, thirty-five pages to the meteorological causes of the famines which afflict Tuscany, thirty pages to chronicling these for six centuries, 100 pages of consideration of favorable storms, and 100 pages on the maladies of plants. This last chapter cites at length many ancient and modern writers on the subject, and tells how he began to examine the rust of wheat with his microscope on 11 June 1766, then enlisted the aid of Raimondo Cocchi and his microscope (of Cuff), finally the help, with many microscopes, of Carlo Alfonso Guadagni. By 19 July he had concluded that the rust was a tiny parasitic plant lying just under the epidermis of the stem and leaves of the wheat, where there are vesicles which the rust occupies; there it steals the nutriment prepared for the wheat. The rust is an organized body, not casual or formless, and shows no sign of feeling or voluntary motion, having nothing in common with Buffon's *particelle organiche viventi* or the animals discovered by Needham and Baker (the small eels). Unfortunately he found ten kinds of rust, and described numerous other plant parasites, which rather blunted the impact of his work. He made his conclusions very clear; they do not differ from Fontana's, but his illustration is less impressive.

In the same year appeared[1] what was ostensibly a review of the *Alimurgia,* but was really a virulent diatribe. The article is an attack on

the author of the *Alimurgia,* on his observations, his ability to observe, his penchant for citing the ancients, his ignorance of the moderns, his credulous belief in folk-sayings and folk-remedies; that is, all that he had written in this work.

Although the «review» was unsigned, everyone believed it to be the work of Felice Fontana; it was reprinted the next year.[2] An anonymous reply to this «maligno libello» purely polemic in nature and probably written by Targioni, entitled *Analisi e difesa della celebre opera intitolata Alimurgia,* etc., in which no names were mentioned, was printed in 1769 in Venice. These two studies of Fontana and Targioni on the rust of wheat, although quite different in style, present the same evidence and draw the same conclusions. Fontana's vituperation (if it was his) seems to have arisen from the sentiment, «How dare anyone else write on this subject»? Regardless of these bad feelings, Targioni reprinted in his own periodical publication[3] Fontana's work on «fixed air», with a long laudatory footnote relating that he (TT) had confirmed Fontana's findings.

These two works on the rust neither attracted much interest nor were they accepted by subsequent authors. Filippo Re,[a] professor of botany and agriculture at Modena, wrote in a textbook published in 1807:[4] «The most generally received theory, first put forward in 1766 by the celebrated Dr. Giovanni Targioni-Tozzetti, is that these diseases consist in the vegetation of cryptogamic plants, arising under the epidermis of corn; and according to others, also on leaves and fruits of trees; and this has been confirmed by the microscopic observations of Fontana [...] I confess that I cannot yet bring myself to adhere to it». Sir Joseph Banks, in *A short account of the cause of disease in Corn* (1805),[5] acknowledged that Felice Fontana was the first to give «an elaborate account of this mischevious weed», by describing both the uredinial and telial states. De Bary, sometimes called the «father of phytopathology», mentioned neither Fontana nor Targioni Tozzetti in his book[6] which is

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[a] In 1795 Filippo Re, then scarcely thirty years old, visited Florence. Later he wrote (*Diario di un viaggio per la Toscana,* etc., cura di C. Casali, Reggio Emilia, Off. Graff. Regg., 1928) that he was taken to the Royal Museum of Physics and Natural History by Ottaviano Targioni Tozzetti (son of Giovanni). His comments are left untranslated: «Targioni mi condusse al Gabinetto Fisico. Belle cere ma troppe [...] Fui dal Sr. Fabbroni uomo che è il più sapiente di Firenze, sebbene nato ballerino, e fattone il mestiere da ragazzo [...]. Là, fui presentato a Fontana brutto e freddissimo, ma pulito. È minore di quello che è creduto.

generally credited for bringing together convincing evidence that fungi are the causes and not the results of plant diseases.

Corti.

Michel Adanson in his *Familles des plantes,* Paris, 1763, and in the *Histoire de l'Académie Royale des Sciences,* 1770, described a microscopic organism, the Tremella, which showed a kind of movement, unequivocal, spontaneous, both from side to side, and progressive and recoiling. He did not intend to suggest that the movement was voluntary or that the organism, which he regarded as a plant, was endowed with sensibility. Various kinds of tremella were described as algae, order *oscillatoria,* by others, one being Lazzaro Spallanzani who, interested in the phenomenon but occupied with other matters, passed it along as worthy of study to his compatriot Corti, as the latter wrote to Charles Bonnet,[7a] 15 Novembre 1774.

Bonaventura Corti (1729-1813) was born near Viano in the region of Scandiano. A priest, he taught at the Collegio di S. Nazario in Reggio Emilia, succeeding Spallanzani. He published his *Osservazioni microscopiche sulla Tremella e sulla circolazione del fluido in una pianta acquaiola,* Lucca, Rocchi, in 1774. In it he described the movements of the organism, also that it «multiplied by division», and that it could be resuscitated when apparently dead from drying by being moistened. Therefore, Corti concluded, it was a «plantanimal».

Now two communications of Felice Fontana[8] had appeared in 1771 in which he had described at some length the movements of Tremella and its revivification. Not finding himself mentioned in Corti's book, he attacked him, not so much for that omission but over the second subject of the book, the circulation of the fluid in another plant, *Chara.* Spallanzani had been enthusiastic about this discovery as it confirmed his beliefs on the nature of the circulation of the blood in animals, and he encouraged Corti to draw the conclusion that the phenomena were similar.[9a]

In the *Antologia Romana* for 1775 there appeared an anonymous article (which was universally ascribed to Fontana) which began by granting Corti's discovery of the movement of fluid in certain plants. The author then describes the phenomenon as he sees it, and concludes that it is in no way a true circulation. As to his belief of the cause of this movement, he does not wish to hazard hypotheses, but it is not a little thing that had been discovered, and certain points are worthy of note.

The article continues: «Thus N.N. [«non nominato», i.e., not named] speaks of Sig. Corti, rendering justice to him and to truth at the same time. But how can he (Corti) publish a book on the Tremella and never speak of the reliable observation and discoveries made by N.N. on this plant? How can he print in 1774 that N.N. has published nothing»? On 20 May 1775 Charles Bonnet wrote to Fontana:[7b]

> Your able compatriot the Abbé Corti has written me some very curious things about the Tremella, making me suspect that this singular product is more animal than vegetable, as I replied to him.

Fontana answered Bonnet's letter on 21 June 1775:[7c]

> I foresee that should I wish, one day or another, to publish my observation with all the details of what I went into, I would run the risk of being believed a plagiarist, while it is others and not I who merit this title; it is a consideration which saddens me to the point where it could make me abandon entirely these occupations, and since you speak in your letter of the Tremella I shall take you as judge and none of the others. You will decide whether or not I have reason to complain [...]
>
> Read the pages [...] enclosed and then judge [...] The observations on *Chara* are mine but not the reflections which follow, although I believe that they are just and true.

What had Corti done? Committed plagiarism? Spallanzani wrote about him to Caldani:[9b]

> From the description, and following explanation, that Fontana gives, one sees that it is all a product of imagination [...] About the phenomena of Tremella then he complains wrongly, the physicist *duro robore natus,* when he accuses Reader Corti of plagiarism. It is true that that so-called incomparable Naturalist printed in 1771 a paper in the Gazetta di Firenze in which he touched on some phenomena of that plant, promising as usual to give a Work *of taste and unprecedented originality* (a paper which he has reprinted this year, with pages tripled because of his self-praise); but Reader Corti assures me that he had never seen it, and the truly superlative character of this man strongly commends belief. Also I can tell you that Corti had not read the brief mention by Fontana, when I suggested that he study the Tremella. Not to say that it needs but little observation to be aware that the book of Corti was drawn from nature itself. You can well imagine that he thinks not to remain idle. In these encounters it is not unsuitable to drive nail for nail.

To Bonnet Spallanzani wrote the same day:[9c]

> M. l'Abbé Corti sends you his respectful compliments, but he is a little in a bad humor. His book on Tremella and Cara has been attacked very rudely in a journal

printed at Rome called the Antologia. It was M. l'Abbé Fontana who made him this fine gift. This critique however has been repudiated by all. [...]
This physicist, otherwise very estimable, has brought upon himself this universal hatred by his sharp criticism and by the ambition he has to dominate all alone the vast kingdom of Physics and Natural History. According to his ideas, there is no one else in Italy capable of observing or experimenting. And if there appears some Production, some Discovery, which obtains approbation of the public, his maxim is to put it down, or even to make it ridiculous. That is what he has done against M. Corti. It is true that this Solipsist has never attacked me. But this is a unique instance, which I attribute less to the honorable mention which I make of him in nearly all my works, than to the close bond of friendship between his brother and me.

Actually Corti had been the first to observe this protoplasmic streaming, which he described again, answering M.N.N. but refusing to use the same kind of language.[10]

Roffredi.

Domenico Maurizio Roffredi was born and died in Turin, 1711 and 1805. Abbot of the Cistercian order in the Abbey of Casanova, he was elected a member of the Royal Academy of Sciences in Turin in 1766. He published three articles[11] on the «little worms or eels of rachitic wheat». The first one of 1775 mentioned their discovery by Needham in 1743, reaffirmed in 1745 and 1750, and the subsequent discussion by Aymen, Bonnet, Buffon, Guettard, and Spallanzani in which Needham's conclusion of the animal nature of these organism, or the possibility of their «animalization», was refuted to the extent that in 1769 Needham abandoned his original interpretation. Here and in his second paper of 1775 Roffredi submitted a great deal of evidence on the nature of these «filaments» of Needham and reaffirmed their animality. He wrote not a word about Felice Fontana.

Also in 1775. Needham sent a «letter» to Rozier's journal[12] in which he expressed his appreciation of Roffredi's papers and their particular significance to him, who had discovered the «little worms or eels» more than thirty years previously. He reproached Roffredi for the vivacious, not to say harsh, manner in which he had written about his metaphysical ideas in the *Miscellanea Taurinensia,* with language which should find no place in the memoirs of a respectable academy, but – times had changed, he forgave him because of the observations he had made.

Felice was not so forgiving. In 1776 Rozier's journal printed a «letter to the author of this work»[13] which begins by stating that Fontana's paper on the Tremella and certain little eels which had appeared there in January 1776 had not presented Fontana's illustration but one provided by the Editor which showed the true ergot rather than false ergot which was Fontana had studied. This «letter» has, in addition to its text, two sets of footnotes; one was from the «defender» of «M. de Fontana», probably Fontana himself, the other from the Editor, presumably Rozier. Thus it tends to add to the confusion already accumulated from the confounding of two kinds of «ergot». In addition to discussing Fontana *contra* Adanson on the movements of the Tremella, there is found (as a footnote by Fontana) this passage «We will not here call our observers Roffredi and Corti plagiarists; they, without suspecting that they had been anticipated, also discovered the properties of these eels and the Tremella, four years after the author [Fontana] had already published them twice in Florence, and made them known publicly to many compatriots and foreigners of all nationalities».
At the time, the guns of both were neatly spiked by Spallanzani. To Bonnet, he wrote on 29 July 1755,[9d] speaking of Fontana:

> He has had the misfortune to be accused of plagiarism in nearly all of Italy. Perhaps he is wrongly accused, but unfortunately many of his discoveries which he presents as original seem to be only simple copies. The eels of rickety wheat furnish, at least apparently, an example. The bulk of this discovery rests upon the book of the late Count Ginanni *(malattie del grano in erba)* printed more than fifteen years ago.

Again to Bonnet on 13 August 1775:[9e]

> Concerning the first inventor of the Generation of eels of rachitic wheat, you see what I have written you about it recently. The Abbé of Piedmont [Roffredi] and that of Roveredo [Fontana] appear equally guilty to me. I will not say that Count Ginanni has developed its whole story, but he at least lays the foundations, and an artless Naturalist was under the rigorous obligation to cite him, at least.

Giorgi.

The most bitter, prolonged, and fruitless controversy of Fontana's life was that with Ferdinando Giorgi, about whom we know little except that he was a socially prominent physician. Neither the first nor the last of his profession to do so, he resolved to acquire the reputation of a

scientist. Accordingly he rented laboratory space in the pharmacy of *Il Cinghiale,* which still exists, opposite the *Mercato Nuovo,* or «Straw Market», in Florence and engaged Gaetano Cioni, laureate in medicine but occupied in miscellaneous studies and translations in science, to translate into Latin the papers they were to write.

The problem that Giorgi and Cioni took on was a lively one of the day, the decomposition of water. On the basis of some clever thinking about how to obtain inflammable air from water, Lavoisier and a colleague performed and described in 1784 some experiments whereby it was produced by passing water over red-hot iron. It was this experiment that Giorgi and Cioni repeated, in the presence of many invited guests, and obtained quite different results, they said, namely the production of eminently respirable, rather than inflammable, air. Their communication appeared 1 March 1785.

Fontana had been examining the same claim, but he made two serious mistakes. The first was: although he obtained the same results as Lavoisier and Meusnier, as we have related on p. 180, he chose not to agree with their interpretation. His second mistake was to publish anonymously a short account of his work in June 1785.

Now the gage was cast, forces were marshalled, pens dipped in vitriol, and before the end 643 printed pages of pure polemic, no scientific data included, had appeared. The end came in the Court, where Fontana had taken action. The record we have[14] is incomplete but it describes the appearance of Fontana, supported by the presence of Mascagni and Giovanni Fabbroni, and others:

> After a long examination and a great many attempts the *Signore* here appearing [Fontana] thought that he could allow publication of an essay of his composition in the *Giornale Enciclopedico di Letteratura Italiana, e oltramontana* of Florence N° 11, p. 100, which was printed on 7 June 1785, although it bore the date of 1784, on account of the journal being behind one year, as happens with all journals [...] In 1785, dated 1 March, that is, much before the appearance of the abstract mentioned above, and even before the Abate Fontana appearing here began his experiments [...] there appeared in Florence as a pamphlet a Latin commentary on experiments as by Doctors Giorgi and Cioni [...] with results directly opposite to the results obtained by the Abate Fontana published much later.

It is easy to imagine the baffled judges, confronted by an action unprecedented in their experience and novel in content. The court action was eventually dropped. The Fontanas had asked for the help of Clementino Vannetti of Rovereto, member of a family close to that of

Fontana for a long time. We have a letter to him from Gregorio,[15] dated Pavia 5 August 1786:

> I imagine that you have noted the judicial action brought last year by my brother in Florence against a miserable insect who publicly calumniated him, and that you have perhaps read some anonymous pamphlets and other things printed about this matter. [...] As for the silly impostor Giorgi, he is now unmasked and condemned as a falsifier and a liar, who has feigned and invented all the experiments that he says he has done, that there is no longer in Europe any physicist who doubts it an instant. His absurd discovery of the *conversion of water into respirable air* has become a mockery even for the enemies of my brother. In short, my brother has in substance already carried away the most complete victory, and by having assailed the calumniator with such force, such impetus, with such terrible impact, and by having demonstrably unmasked him, he has gained the most sincere applause of all learned and honest men.

One reason that the point was won was because some began to get cold feet. Giorgi, as has been said, demonstrated his experiments to many people and some of them agreed to sign a statement drawn up by him, setting forth what they had seen. One of these was the Abate Giulio Perini. To him Felice wrote 18 January 1786.[16] asking for the truth about what he had seen. In a memoir of 1786 that has been attributed to Giovanni Fabbroni, Perini and others added their testimony that all had not gone in Giorgi's experiment as he had claimed, and they had previously testified. Signed pamphlets in support of Fontana or against Giorgi were published by Francesco Bartolozzi, two by Luigi Tramontani, three by Giuseppe Tofani, the learned publisher. There were anonymous pamphlets, one in verse, one comparing Giorgi with Baldassare Capra (ca. 1580-1626) plagiarizer of Galileo in 1607. Landriani's participation is related on p. 143. The final shot came from Gaetano Cioni, who had been discharged by Giorgi for disagreeing with him. Cioni wrote[17] that, although his chief employment had been in the preparation of the paper in Latin, he had been present at some of the experiments, that he and others present had pointed out the inadequacy of the apparatus but Giorgi had denied it; that Giorgi had built the eudiometer himself and that it was faulty.

Fontana had turned to his colleagues for support, not only the local ones, members of the *Accademia dei Georgofili* such as Tramontani, but to the foreign ones such as Senebier, to whom he wrote, 11 July 1786,[18] asking for his support. Apparently before this letter of Fontana's had been written, Senebier wrote to Gaetano Cioni 5 July 1786[17] praising Fontana, much as he had asked.

These events were related in 1821 by Desgenettes as follows:[19]

> All Fontana's biographers have passed over in silence an event of his life, memorable since it disturbed appreciably his rest for several years. He had just repeated the fine experiments of some French on the decomposition of water and had obtained the same results. One self-declared physicist, supported by the high society of Florence, wished to perform publicly the same experiments; he fumbled; the iron tube, supposed to lead away, in the pneumatical-chemical apparatus, the product of the operation, was burst by an unwise application of heat; atmospheric air rushed in, and the experimenter concluded that the French had decomposed nothing. Fontana had little trouble in ridiculing these experiments, but the spectators, deceived, joined together eagerly to disparage him. A motive far from the question of the decomposition of water more strongly excited his enemies' passions. Leopold had just shown, at the gates of Rome, a desire to see in operation a religious reform in his state. Three bishops, some lawyers, and some courtiers made up an opposition party, along with the rest of the Tuscan population. The enlightened populace could not see, without being scandalized, a Fontana formerly so indulgent, so free, so courageous in his religious opinions, converted all at once to the austere Christianity of the primitive Church. The proselyte being covered by the aegis of the sovereign, the physicist as the vulnerable one was the one to be pursued, in appearance. Matters which were carried so far as to result in a criminal trial gave rise to voluminous pamphlets in which nothing injurious was spared by either side. Fontana showed a redoubtable talent for the polemic and floored his adversaries, when the Authority, itself a little compromised in these procedings, put an end to the trial.

Were Giorgi and his lawyer Rivani discredited? It seems not; in the republican government of 1798-99 there were, among others, lawyer Rivani as Chief of Police and Dr. Ferdinando Giorgi as Commissioner of the Hospital of Santa Maria Nuova.[20] And in 1801, marching in the procession of members of the newly-formed, short-lived Nuova Accademia del Cimento, of which Fontana was president, who should appear but Ferdinando Giorgi.[21]

Apart from the articles of factual, i.e., experimental, content, those of Fontana's being cited in Chapter 13, there were printed and published fifteen pamphlets purely polemic in nature. There are two collections in Florence of these pamphlets, both polemic and factual, one at the *Biblioteca Medica Centrale* at Careggi, one in the *Istituto e Museo di Storia Della Scienza.* One has four, the other, five, items not in the other. The complete collection is a saddening demonstration of human malice and futile exertion.

CHAPTER 11

LEGACIES

Matters Material - Matters Spiritual - Character - Women in his Life - Position in Society - Contributions to Knowledge - Position as Scientist - Return to the Argument - Celebrations

MATTERS MATERIAL

It is difficult to treat Fontana's financial affairs for several reasons. The accounts[1] of the Museum are lacking in detail and are incomplete, the early ones giving the salaries of other dependents but not Fontana's, and they use a complex way of entering amounts. Monetary equivalents were uncertain and variable in both place and time, but the tabulation of the Appendix (p. 107) gives approximate equivalents for the units of coinage prevalent in eighteenth-century Tuscany.

When Fontana was appointed to the University of Pisa, the median annual salary of professors there was 2,000 *lire,* although one Pietro Rossi, holding the chair of logic (Fontana's appointment was also in logic), received 2,400 *lire* in 1762.[2] We have no specific information about Fontana's salary in 1765, but it is recorded as being 2,236 *lire* in 1784.[3] At that time his stipend from the Museum was 3,800 *lire per annum,* of which 1,000 *lire* was noted as pension. In January 1790,[4] i.e., when Peter Leopold was still in Tuscany, his stipend was reduced to only 2,800 *lire per annum,* remaining at that level for 1791, but by 1796,[5] it had become 3,500 *lire per annum* where it remained until his death.

The matter of pensions is far from clear. The original one, really a supplement to his salary, of 27 May 1771, signed by Peter Leopold and Angelo Tavanti,[6] was for 200 *scudi per annum.* In a record of 1803,[7] pensions are mentioned for only three dependents of the Museum: two modellers, Susini and Calenzuoli, and Giovanni Fabbroni, who was awarded 800 *lire per annum* paid in monthly installments. Since his stipend at that time was 2,800 *lire per annum,* his total payment would

have been 3,600 *lire,* greater than Fontana's stipend. However, Fontana was probably still receiving his salary from Pisa; a note in the 1805 Museum record[8] stated that his income, between stipend and pensions, was more than 4,900 *lire per annum.*

We have many records of Fontana's generosity; sums went annually to his brother Giuseppe, his mother Elena, and his sister Teresa, as did gifts to his brother Gregorio to pay debts in Rome and to his brother Bernardino to pay debts in Italy and in Vienna. His donation of valuable samples and instruments to the Museum he recorded in his memoir.[9]

Fontana also had the habit of saving, as we learn from correspondence[a] with his cousin Lorenzo Marzani and with Francesco Chiusole both from the Trentino. One letter from Marzani, dated 8 January 1783, speaks of the investment of 4,575 *fiorini,* the sum to which the interest of Fontana's holdings amounted after payment of deductions. A letter to Chiusole,[10] dated 16 December 1795, read:

> You know that Don Pietro Marzani has in his hands capital of mine [invested] at 4 percent. He writes me that he does not want to hold this year's interest and wants me to let him know some person to whom he can give it. If you were in a position to take it you would do me a great favor. It will not amount to much, because I give 100 *fiorini* a year to my sister and another 25 to my brother in Vienna, and then it is at 4 percent, as I said, so that the payment will be a small matter, perhaps less than 500 *fiorini.* Be that as it may, take what will be delivered to you, if you are willing to be bothered with it. If you are able to invest it in a bill of exchange so much the better; if not, keep it by you at my disposal.
>
> On my departure from up there you gave me reason to hope that you were in a position to keep by you something of mine; it is for this reason that I have taken the risk of giving you this trouble.
>
> I am writing to Don Pietro to make the payment to you.

This letter indicates that capital holding amounted to about 78,000 *lire.* The major accumulation had probably occurred when he was in Paris and London, where he received an allowance while his salary went to Pomarolo. Late in life he asked for his money in letters one of which[11] of 13 July 1797 to his brother Francesco read:

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[a] The Chiusole and Marzani correspondence is now, with few exceptions, lost, having disappeared during the two world wars which ravaged the Vallagarina. However, in June 1905 Giampio Adami made copies of some of Fontana's letters: ten to cousin Lorenzo Marzani, one to Felicita Marzani, one to brother Giuseppe, one to his mother. Unfortunately some to Pietro and Felicita Marzani were omitted «because they spoke exclusively of financial affairs». These drafts are now in the possession of Gina Adami, Rovereto, who kindly provided copies of them.

> It will take patience, and 1 or 2 percent will have to be paid, to get one's money. So go in my name to Sig. Francesco Chiusole, and give him a receipt for the money of mine he is holding, and send it to me through safë channels at the rate of three *lire* per *fiorino* with the abovementioned loss. So let's talk no more about the money; in this way it will all be done with. If a safe opportunity presents itself, send me the silver and clock which Sig. Chiusole is keeping for me and will deliver to you upon acknowledgment of receipt, and tell him that I will write by the next mail.

The money mentioned is presumably all of that which went to Chiusole in 1795. A letter addressed to cousin Felicita Marzani de Neidoff, probably of 9 December 1797, asking only for interest, read:

> I have written a few lines to my brother Francesco about the way for me to be paid here in Tuscany the interest on my capital which you keep to my account. I hear that the exchange in Vienna is not favorable and I will have to suffer some loss as I did last year. Also this year we must be patient but my brother will talk to you of everything on my behalf. My circumstances are such that I need this interest, which I now request of you as a favor, as soon as possible.

Many writings about Fontana have stressed the independence of his character. Casimiro Adami (1905) said: «He wrote "Non vi è di buono a questo mondo che la libertà" in a letter from Paris; and ever free his spirit seems to us». This letter asked his cousin Marzani to make annual payments to his mother and to his brother Giuseppe; it deals in fact with material affairs, is primarily concerned not with spiritual freedom, but with financial independence. The complete sentence, part of which Adami quoted, reads in translation: «Sir Cousin, there is in this world no good but freedom, and for those who are not born rich, this is acquired only through saving». Unfortunately we do not know the extent of his estate at his death. His brother Bernardino appeared in Florence, curiously enough only five days after Felice's fall in the street, claiming Felice's salary as heir of the «deceased» (he was still alive!) by virtue of a will drawn on 15 February 1805 by «Costantino Boni».[12] There was a notary, Costantino Buoni, in Florence at that time, but the file of his recorded transactions for the period 1798-1808, in the *Archivio Notarile,* Florence, does not include Felice's will. That file (Filza 17774. B21 Buoni) does, however, contain a document (Repertorio 131) of 1 February 1806 noting that on 15 February 1805 Notary Buoni drew the above-mentioned will and also recording that Bernardino was selling for 240 *scudi* property consisting of land in and near Corbignano, in S. Martino a Mensola, parish of Fiesole, on which were a villa, three laborers' houses, groves of olive, fig, and oak trees, and a stone quarry, all of which Felice

had acquired in June 1801. Bernardino was paid Felice's salary of 41+ *scudi* for the month of February, and 20+ *scudi* for March, as half of his «keep» for that month, since he «had ceased to live at 6:30 of the morning of the tenth day current».

The designation of Bernardino as Felice Fontana's sole heir by a will, which has not been found, made in 1805 during his final illness, smacks somewhat of influence, Bernardino being in Florence at the time. In two previous wills,[13] a holograph one of 20 August 1793, another not holograph but signed, of 18 October 1799, both done in Florence, all his possessions were left to his sister Teresa and Gioseffa, the wife of his brother Francesco (1793), or to his brother Gregorio, and after him to Teresa to be disposed of as she wished, but recommending the children of Francesco (1799).

From Adami's work of 1905, it might be gathered that all the papers of the Casa Fontana had been burnt, but it appears that he was speaking of the notarial archives of the family; from his work of 1930, it seems that he knew of the existence of the documents given to the Biblioteca Comunale, Trent, in 1925 by the family. They must have been taken to the Vallagarina by Bernardino, inherited by Francesco, and his descendants. They are not discussed at any length by Adami, already in 1930 seriously ill. These documents[14] include five letters of Felice Fontana to his brother Bernardino in 1804, about the financial troubles of the latter, two wills of Felice (1793 and 1799), fifteen letters to Felice including some from the French chemists de Morveau and de la Metherie, his grant of a pension in 1771, a letter of Bernardino of 1809 (see p. 264), and diplomas of membership in academies and societies as shown in the Table p. 107.

MATTERS SPIRITUAL

We began, on the first page, by quoting some laudatory comments on Felice Fontana. Granted the truth in these, why did Filippo Pacini think Fontana needed vindication?[15] Why did Giuseppe Cirincione choose that word for the title of his paper?[16] Why did Garrison[17] use that word «forgotten» in his title, echoed in 1936 by Burdin[18] and in 1966 by Di Palma?[19]

The paper of Pacini can be explained: he was a disappointed, embittered man; «Cet animal est fort méchant, quand on attaque, il se defend» was the way he began his paper, the heading of the section of Fontana being «NEMO PROPHETA IN PATRIA SUA». Pacini felt

that his own work was recognized outside of, but not in, Italy, and used the neglect of Fontana as a peg on which to hang his own grievance.

Cirincione wrote as he did for the reason that, after hearing Fontana's discovery of the structure of peripheral nerve spoken of, he could find no mention of it in current writing on general anatomy.

What prompted Garrison's essay was the «[...] eight separate Italian biographies of Fontana written and published in recent years. As an experimental physiologist, he has come into his own but latterly» that is, now rescued from undeserved oblivion.

If this book has achieved its purpose, it schould be possible to assess the verity of these statements. If Fontana needed vindication, if he was forgotten, was it because of something in the nature of his character, the nature of his contributions, or was it something else?

FONTANA'S CHARACTER

Most of what we know we learn from his own writings: his memoirs and letters, but much can be added from the letters of others such as Caldani and Spallanzani. His nature must have been what is called «difficult». His tongue was probably as abrasive as his pen, and he was naive enough to be surprised when those whom he had lashed with one or the other turned upon him. Full of zeal, he expected his workmen to be the same; as they were not, he attempted to drive them. Adami (1905) tells of having met an aged porter at the Museum who, when young, had known a porter who remembered Fontana; from the conversation Adami concluded that Fontana had been simply a rough diamond;[b] this is untenable in view of the memoirs.[9] Peter Leopold's criticism of Fontana[c] as lacking in ability to get along with others must have been well-founded. This side of his character is well revealed by the well-documented, deplorable incident of the altercation with the *spazzino* Guidetti,[61] brought up against Fontana as an example of his ill will by Giovanni Fabbroni.[62]

The Women in his Life

Women seem to have played a small part in Fontana's life. When he was twenty-six he wrote[20] from Bologna to his brother Giuseppe in Pomaro-

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[b] *burbero benefico.*

[c] See p. 63.

lo: «Find out in secret if Signora Francesca Turini has been ill in recent days. Do it secretly because I am much concerned». From London he wrote[21] to Angelo Tavanti: «If I have written something about Madame Maltravers, I have done so out of pure sensibility, and because it seems to me that her cause was better than that of her husband».
The incident related in his memoir[9] of his maid having been suborned to charge him with having violated her, if true, indicates that at least they thought him capable of such behavior. Most revelatory of his character are the letters to Bettina Slop.

Fontana's friend G.A. («Beppe») Slop married Elizabeth («Bettina») Dodsworth, of English origin, probably in 1766. According to Menestrina,[22] writing in an article about the family:

> it was not a happy choice [...] in contrast to his balance as a man and a scholar, Slop, in his choice of a partner, attached no importance to the fact that his wife was, as Perini said without a shred of chivalrous reticence, «a coarse and ugly woman». In order that qualities so repellent might be made tolerable, nature had given the Dodsworth uncommon intelligence and quickness of wit.

In the Fontana-Slop correspondence, there are sixteen letters from Fontana to Bettina. Some were written in the early days of the establishment of Fontana's home and the construction of the Museum, and some after his return to Florence in 1780. Fontana tried to advise Bettina – by scolding her unmercifully, as the following extract from a letter shows:

> Your answers are always the same, that is, never to the point [...] I tell you I don't understand how you can have 150 *scudi* to throw away out of pure vanity and caprice, when your home and your children lack the most necessary things. I tell you that when 150 *scudi* can be thrown to the devil, either it is a sign that income exceeds expenses, or that there is wrong, irregular, harmful behaviour in that same home [...] I tell you that the way to prevent Beppe's capricious spending is to make him feel the weight of the family and not to hide the expenses from him [...] I tell you that the 150 *scudi* would have been better spent on cleaning your house than on travel, as I hear it said that you live, and make Beppe and the children live, in filth [...] I know that you can't prevent Beppe's mistakes, but the fault is yours, and Beppe is right to throw away that money you tell him is left over from household expenses. He is right and you alone are wrong. When you recover your senses, I hope that you will say that I am right, but remember that it won't take long for you to repent the bad upbringing given to your family, recognized by everyone, even here, except by you and by Beppe whom you allowed to see nothing. I am sorry for your blindness, and if ever you open your eyes you will see that I was your friend, and your greatest enemy is yourself.

Fontana obviously loved the family Slop and tried to be of help to them; apparently they loved him too – when they did not find him insupportable. His last letter to Bettina that we have contains some of Fontana's bitterest remarks about friendship, and about Italians.

> I have received your letter today, and assure you that I would never have written you about anything that could displease you if you had not first given me provocation. I am already accustomed to suffering through friends, and for some time I have known that friendship is a thing unknown in Italy. It is rare everywhere, I admit, but is entirely imaginary in some countries. To my cost I have learned that nothing more easily loses friends than doing them some good [...] The burden of gratitude is too humiliating for man; he prefers to make excuses for his ingratitude rather than acknowledge himself under obligation [...] My enemies are only people I have helped, or obtained help for [...] I have never hurt anyone; on the contrary I have tried to be helpful, and have been to many who now blush to see me [...] Another source of enmity lies in the worth, whether true or false, placed on people. If, then to worth is added some fortunate turn of fate, then indeed envy is unleashed in everyone. I contend that this merit is not in me, but it is enough that it is believed to be [...] Add some good luck and that's the end of the matter. Now indeed envy and anger unite against me. These are my real faults, and I challenge you to find any others. You will find calumnies but not facts. I repeat that I have nothing to reproach myself with in my conduct, that I do not know that I have ever betrayed a friend, but I do know that I have been deceived by many [...] In the midst of all this I have true friends in France and in England [...] I challenge anyone to reckon as many, of so much merit and such probity, as I can reckon [...] I await Zanobi.[d] I am your friend and will do all that I can. I hear that Dazzi is there. Slop has not written me about it. I am sorry for him and feel real grief that he has been abandoned by the person who least should do so. That is Italian friendship for you.

Some lines strikingly recall the following passage from Diderot's *Rameau's Nephew*.

> I. To help one's friends?
> He. Vanity. Does one have friends? And if one had them, ought one to make ingrates of them? For, look at it closely, you'll see that it is nearly always one's reward for services rendered. Gratitude is a burden, and all burdens are meant to be shaken off.

More light is thrown on Fontana by a letter to him,[23] one of the few existing, from a young woman, a painter, probably French (the letter is in that language) written from Rome, 13 April (year not given), addressed

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[d] Zanobi was the brother of Tommaso Perelli, Slop's predecessor.

to him as Director of the Collection of Natural History at Via Chiara, N° 2, Florence, thus compounding confusion. Via Chiara was a segment of the present Via dei Serragli adjacent to the segment then called Via della Fornace, in which was his home at the time of his death. The letter breaks off in the middle of a page, and bears no signature. It reads in part:

> I was going to send my letter when your latest was brought to me. It has caused me such pain, my dear and good friend, I have given you much trouble, I have not weighed my words according to your feelings but according to the anguish I was suffering. Finally my weariness has worn away and I begin to be well and regain the *embonpoint* that I had lost. The solicitude that you express for me proves to me that I have done well to count on you but I fear that you have a better opinion of me than I really deserve; many women would be flattered to be able to say *«Fontana cares for me and esteems me greatly»,* but for my heart's serenity I must believe that I deserve your esteem; I am young and I have been able to please you in many ways but I think I have not yet done enough for you to place me above others. I have never sinned out of coquetry, out of caprice, but I have attributed virtues to those who did not possess them; beauty and wealth are nothing to me; but a tender heart, a compassionate soul, there are the enemies of my peace and quiet. I met M. de B... in very distressing circumstances, on the verge of losing his fortune [...] he told me his troubles so trustingly, seemed so good, so refined, that I could not deny him my regard, and his tender devotion kindled in me all the feelings of which we have seen the distressing results. A little later I realized that he was weak and fickle but my heart needed to believe him perfect and I continued to see him so; you have observed how I behaved until the last moment. In spite of all that he may be told he will be obliged at least to respect me and that is all that I want. As to the love that I felt for him it has completely disappeared; my self-regard, perhaps my judgement have made me see how ridiculous it was to love without being loved in return. I submit this charge to the scrutiny of your experience; judge me. I believe, however, that you will not censure me. My heart, my spirit, even my talent constantly renew my need to love, but this experience will I hope keep me from choosing badly. I have abandoned all love's torments for those which painting and my fate bring me [...] I am sorely tried by love of the Arts, of liberty and by the little capacity that I have to be what my nature wishes me to be; I am far from my relatives, obliged to rely sometimes on the help of my friends, and when these friends lose merit in my eyes I renounce their charity at once. I shall never blush at being poor but I should [blush] to take from one whom I do not esteem. My fortune would be made if I could have consented to sell my caresses or my love, but, my friend, I hope that you would refuse me your friendship if I were capable of that ugly game. Cultivation of the Arts requires an exterior proclaiming easy circumstances, no-one loves an impoverished painter, and a woman particularly needs to appear well-off; it is that exterior, then, which torments me. There, my friend, is the only thing which troubles my spirit, I hope that time and my work will give me the means of dispelling my anxieties; meanwhile, help me with your good advice and keep for me always a little place in

your house so that if fortune refuses me its favor I can peacefully eat a piece of bread at the side of a friend and philosopher whom I esteem; then, goodby world [...] Write me as often as you can and as will please you, to keep up my spirits with good advice, and above all promise me that should you be ill you make it possible for me to go to you, that by my care I might keep you among us, or if you must quit the world that you might have your best friend within sight until the last moment. This idea isn't cheerful but one does not live in order always to laugh and I believe that you will not be vexed with me for thinking that way.
Farewell my good friend I am moved by what I have just said and I need to end my letter in order not to become sad. Think of me and believe that at every moment our thoughts meet. I have seen M. Woodburn, she is always good but ill and very worried; we have spoken much of you, she hopes to rejoin us soon, her journey ends at Naples.
She will let me do her portrait on her return and after that you will see her again soon. [...] Farewell once more; how unhappy I am not to have you near me, we would stroll in the Villa Borghese or the Albani and I should have the pleasure of hearing you speak. A thousand regards to M. Couturier and Mlle. Rossi. Remember me to our acquaintances, farewell, farewell, your friend for the.

This letter suggests that they were together in Rome, but it could not have been during his long stay there in 1759-60, as he then had no position in Florence. From the Mangili papers,[24] we know that Fontana traveled to Rome in 1791. Even a later date is suggested by another letter[25] to Fontana written from *l'Hospice De La Salpetriere près le jardin des plantes,* Paris, on 2 September 1802 by L'Abbé De Seillans (not further identified), which reads in part:

I have only been awaiting an opportunity to pass on to you your memoir and a volume of the memoirs of the Medical Society returned to me by M. Beauvais, physician, on behalf of M. Alibert [...] Could a more favorable chance present itself than that offered me by our delightful friend Mme. Woodburn. I did not know of her arrival here until too late and her departure for Italy comes too soon; may she recover in your climate the serenity and the *embonpoint* which I found so changed; may the solicitude of your friendship heal a wound which seems very deep to me, which her long stay in Paris would surely aggravate [...] You could not give me more substantial proof of your friendship than by sending me as soon as possible after her arrival news of her journey, and by informing me as often as you can of the improvement that [...] your solicitude will have wrought in her [...] Do you still see Roussi [sic] and her non-conformist friend [amie]? Please give them my affectionate greetings; I am still very fond of them and miss them very much.

It is tempting to identify Fontana's young correspondent as the non-conformist friend of Mlle. Rossi.

Fontana's Position in Society

After moving out of the Palazzo Pitti, Fontana had at least one other home: Via della Fornace, 2, the precise location of which we do not know. It seems to have had a garden, to have housed servants, of which he apparently had several, at least one wearing livery, and to have had a stable for his horses and carriage, also his factory for making wax anatomical models. He may have been assigned other quarters for making the models for France, but he seems to have kept the first home for the rest of his life. According to Mangili and Scarpa, he appears to have moved in high social circles, but according to Dupaty:[47]

> M. Fontana is held in no esteem in Florence, especially among the nobility. It is contempt for philosophers on their part; they are not sufficiently enlightened to hate them.

Was Fontana paranoid? Was he persecuted? The answer is probably yes to both. His social standing was enough to elicit the envy of which he wrote in his memoirs. To this was added his eminence as a scientist, indicated by the universal reference to him in contemporary writings as «the famous Abbé Fontana», and also by the number of academies that elected him to membership (see p. 107).

For a time at least the constant attention paid him by the young Grand Duke must have aroused much jealousy. His zeal, his incorruptibility, his hard-working habits, all must have made his name anathema to associates and underlings. Finally, he was a foreigner, and in the pay and the confidence of another foreigner who happened to be the ruler of the country. The records indicate that he was persecuted – for all the above reasons and also because he was a thorn in the side of everyone about him. However, after his experiences it is understandable that he saw persecutors where none existed.

FONTANA'S CONTRIBUTIONS TO KNOWLEDGE

In *physics and chemistry:* Although Fontana was an ingenious experimenter and a capable manipulator, his being tied to the phlogiston theory kept him from making progress. This is not a criticism of his intellectual powers, since many other worthy men adhered to it as the best available explanation of the facts. While his discovery of the gas-adsorbing power of charcoal attracted much contemporary attention, he did nothing further with it because, as he wrote:

this way of examining the forces which the phenomena require, or to represent effects by supposing the causes which accord with them, is somewhat more mathematics than physics and tends to find more laws for effects, than causes.

His reputation probably did not stand high with all of his colleagues, although Rouelle and d'Arcet in 1778, Lavoisier and Laplace in 1783, and de la Metherie in 1785 all sent him their works.[e] Richard Kirwan repeatedly cited[26] Fontana's experiments and data on the weights of various airs. He also referred to «a very curious experiment» of «this ingenious philosopher». However, in his final defence of the phlogiston theory,[27] and in the refutation[28] of that work by Lavoisier, Berthollet, de Morveau, Fourcroy, and Monge, there is no mention of Fontana, although he had performed and published on pertinent experiments.

In *anatomy:* Fontana's discoveries in histology were regarded with suspicion in his time, as part of the general attitude towards any microscopy. Whether or not he actually saw what he described in nerve and muscle, since confirmed as correct many times, is still being debated. The most tangible relicts of his labors, the anatomical models, are still superb today, artistically surpassing others, such as those of embryonic structures of His and Ziegler[f] among later ones. Ever debated is their educational value. Today's trend in medical education is toward the use of models, not because of a shortage of cadavers, as might be thought, but because the compression of the medical curriculum leaves too little time for the dissection of a cadaver, as previously required. A study[29] of the performance in examination of separate groups of students, some using models for study and some dissecting cadavers, revealed no difference. However, the conviction is probably general that a professional anatomist must have dissected the cadaver.

In *pharmacology:* The scarcity of research on the venom of the viper between the time of Fontana (1781) and that of Weir Mitchell (1860) has been commented on.[g] Can it be that Fontana's work was so thorough that no new thoughts occurred to anyone?

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[e] These are now in the library of the *Istituto e Museo di Storia della Scienza,* Florence. They bear inscriptions to Fontana by the authors.

[f] These wax models were prepared by Friedrich Ziegler after the studies of Wilhelm His Senior (1831-1904), professor of anatomy at Basle and Leipzig. Ziegler founded the anatomical institute at the University of Freiburg i. Br. in 1852.

[g] «These studies [Redi *et. al.*] were greatly extended by Felice Fontana, whose investigation is considered the first toxicological study and the beginning of modern

In *physiology:* Fontana's writings on neuromuscular physiology drew much contemporary and subsequent comment, chiefly written in German. The first to be published was that of Girtanner[h] who wrote[30] rather patronizingly:

> I owe a great deal to the works of M. L'Abbé Fontana; often in following in the footsteps of this great philosopher, sometimes diverging from him and avoiding the errors into which he fell, I believe I found the truth.

Although not so indicated, Girtanner's works read like quotations from Fontana, whom he criticized for not performing the crucial experiments; Girtanner says he has performed experiments but he describes them very little.

In 1795 Brandis wrote[31] at length proposing to extend Fontana's laws of irritability, which he cites fully, to the entire organism. In 1797 Köllner wrote[32] of the same laws, and in 1800 Niemeyer[33] discussed in detail Fontana's work, especially on the topics of voluntary movement and restoration of irritability. It is likely that all of these men knew Fontana's writings only through Hebenstreit's excerpts; they were all physicians and drew their conclusions from observation of their patients, not from the results of their experimentation, although some of them carried on such investigation. Experiments on irritability were reported[34] by an Italian, Barzellotti, but he confined his attention to the theories of Prochaska and Girtanner, ignoring Fontana entirely.

From 1849 to 1894, Moritz Schiff[i] labored over the same problems and at times in the same place as had Fontana. He reproduced many of Fontana's arguments, but only once gave him due credit: «On this point I have the great authority of Felix Fontana».[35] In Schiff's collected works,[36] the subjects of the physiology of muscle and the innervation of

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scientific research on snake venoms. Surprisingly these studies were not substantiated by anything significant for many years, until Silas Weir Mitchell [...]» A. Devi in *Venomous Animals and their Venoms.* New York, Academic Press, 1968, I, 120.

h · Christoph Girtanner (1760-1800) was not for Fontana the most favorable promulgator of his doctrines. Living in Paris, he was an early convert to the new anti-phlogistic chemistry; later as a physician in Göttingen he freely plagiarised the work of others both in chemistry and medicine.

i Moritz Schiff (1823-1896) was professor of physiology at La Specola in Florence for 1863-1876. He investigated many aspects of physiology and showed great originality in experimentation.

the heart are treated in seven hundred pages; citations here are not of Fontana, but of contemporary workers such as Bowditch, Kronecker, and Marey, active in these fields.

FONTANA'S POSITION AS SCIENTIST

Fontana must be regarded as an extreme empiricist. As important to him as the knowledge he sought was the search for it.

He has often been labeled a follower of Galileo, but this is untenable should we accept a current view[37] of Galileo as one whose «principles were not derived from detailed observation of natural phenomena», who «appealed to thought-experiments rather than factual ones». Fontana agreed with Galileo in challenging authorities, but the use of a mathematical approach which was Galileo's point of entry into the cosmos is essentially absent from Fontana's writings; although he may have been interested in mathematics itself,[38] he rejected, for example, the hypothesis of the force of attraction changing to one of repulsion, derived from mathematics alone. He does appear as a follower of Galileo, if we accept an alternative description of the extent of his use of experiment as tool of investigation.[39]

Fontana followed William Harvey, whose works were in the 1807 library of La Specola, in opting for epigenesis and for experimental evidence as a basis for conclusions. He also made use of the kind of quantitative argument which served Harvey so well in establishing the existence of the circulation of the blood when he showed that a certain amount of venom was required to kill an animal; then he ascertained what that amount was; this he then compared with the amount of venom likely to be injected by the viper; from this he concluded that a single bite probably would not be fatal to man.

Many of the works of Robert Boyle were in the 1807 library, and Boyle's «enthusiastic espousal of the experimental as distinct from the mathematical or rational, approach to science»,[40] found approval in Fontana.

Isaac Newton's works were also familiar to Fontana; in the *Irritabilitatis legibus*[41] he quoted from the *Principles of Natural Philosophy:* «The origin of all laws of Nature is such that to the wisest judgment no vestiges of necessity are apparent. These we must seek not by invention and conjecture but by observation and experiment», While Fontana wrote of Newton's experiments and observations on light, he rarely cited explicitly the *Opticks.* Perhaps he felt that here Newton's speculative

tendency had gone too far. That work (Question 31) may have been the source of Fontana's formulation of the method of the *Treatise on the Venom of the Viper* as «analytic» rather than «synthetic».

The works of Boerhaave, Musschenbroek, s'Gravesande, and Desaguiliers were certainly familiar to Fontana; all of them rejected overspeculative hypotheses and emphasized the importance of observation and experiment, while expounding physical science without mathematics.[42] Fontana's own beliefs were expressed in the *Treatise* as follows:

> It is agreed at present that there is no other guide in a search into natural truths, than a knowledge of facts; it is only on facts that the philosopher can hope either to establish a reasonable system, or to form a sound judgment of those already established. Observation alone is capable of dissipating the mists that envelop the hidden causes of the phenomena of nature. And lastly, it is to the labours of observers that we owe the rapid progress philosophy has made in our time. (I, 1)

> Facts alone are however not sufficient to dissipate the obscurity that envelops them [natural truths]. A train of observations, without the help of a skilful hand to apply them, would be at best but the useless proof of a painful application. In the same way the most brilliant systems the rich and fertile imagination of a philosopher can supply, do not deserve the attention of naturalists, unless they are founded on good experiments. (I, 74)

> Experiment alone may conduct us through the unknown paths of nature, and may lead us to new and unexpected truths. But at the very time that man, profiting by this torch, is making bold strides towards the truth, and soars as if he meant to govern nature himself, she stops him every moment, and by discovering herself to him in part, seems afraid of being recollected; she thus continually reminds him of his weakness, and shows him that his hopes are either in vain, or confined within very narrow limits.

> Man, who assigns to comets the course they are to keep, and who fixes the time that is employed by the light in its progress from the sun to our hemisphere, is not, with all this knowledge, acquainted with the air that surrounds him, or with the fire that warms him. Such is our condition, and such is the state of human science. (I, 271)

Here he used the term «philosopher» in its «wide sense, including men learned in physical science [...] as well as those versed in the metaphysical and moral sciences [...] now chiefly confined to the latter» (OED).

When we consider writings on biological matters, it is tempting to think that Fontana may have met Diderot, although he did not mention him in his letters from Paris. He must have been aware of the *Encyclopédie,* an edition of which began appearing in 1770 under the

patronage of Peter Leopold and dedicated to him. It contained[43] a paragraph on *birth* foreshadowing Diderot's, and Fontana's, later words:

> *Naitre,* to come into the world. If a rigorous definition of this word must be given, some difficulty will perhaps be encountered. *What we are going to say is purely systematic.* Properly speaking, one is not *born* at all, one does not *die* at all; one has existed since the beginning of things, and one will exist until their completion. A spot of life grows, develops, as far as certain limit, by the successive juxtaposition of an infinity of molecules. This limit passed, it subsides, and dissolves into separate molecules which proceed to spread through the general common mass.

They both were materialists, believing natural phenomena may be explained by the properties of matter and motion. They both favored experimental science as opposed to theoretical reasoning. They both extended the Great Chain of Being to the vegetable and mineral kingdoms.

In *D'Alembert's Dream* these themes and others, which Diderot had been pondering for years, are expounded in a manner similar in many ways to Fontana's essay on *Generation.* Both ridicule the theory of preformation, both pose the questions: How can unit structures (molecules, particles) aggregate? How is an organ formed? How is an individual animal formed? How do new kinds of animals appear? How to explain the appearance of defective forms, or monsters? Both emphasize the organizing power of Nature.

This work of Diderot was not published until 1830.[44] However, Diderot read it to his friends, and he was well acquainted with Rouelle, father-in-law of Darcet, friend and colleague of Fontana. It appeared in 1782 in the *Correspondence Littéraire,* a kind of letter founded by Diderot's friend F.M. Grimm, containing much material supplied by Diderot, which went to certain crowned heads of Europe, and others. Unfortunately a complete list of subscribers has never been revealed.[45] Diderot reviewed there[46] the *Traité sur le venin de la vipère* favorably: «Our savants consider this treatise as one of the best works in physics which has appeared in a long time».

Both men were characteristic of the Age of the Enlightenment, believing in humanism and humanitarianism, the value of education, and the perfectibility of man. However, Fontana held back from being a philosopher (in today's sense) which involves the obligation to explain everything. Fontana devoted himself exclusively to matters subject to experimental investigation, well exemplifying the «scientific method»,

i.e., observation, reflection, formation of testable hypotheses, experimentation, drawing conclusions, construction of an explanatory theory.

RETURN TO THE ARGUMENT

To resolve the antitheses expressed in the Argument at the beginning of the book, we must answer – each of the contradictory statements is true. While he made many pioneering explorations, he generally omitted to mention, in his writings, those whom he did not intend to refute; in the instance of the «barometer by weight» there is a strong indication[48] that he appropriated the design without acknowledgment. He saw the «primitive nerve cylinders», the «spiral bands» of nerves, and the striations of muscle fibers, but he identified the lymphatic vessels as his imaginary «tortuous cylinders». His contributions to knowledge for which he deserves to be remembered we have set forth.

As a person, Fontana may not have been his own worst enemy, but he must have been a great exasperation to his friends. His manner no doubt antagonized many, leading them to take from him such credit as was rightly his.

Finally, in the nature of the progress of science, as his work was superseded, Fontana came to be overlooked. Unjustly forgotten and periodically rediscovered, that has been his fate.

CELEBRATIONS

While there seems often to have been a conspiracy of silence against Fontana in his adopted country, his native country, especially Pomarolo, has given repeated testimony to the reverence in which he was held.

The first commemoration of which we have a record was organized by the parish priest, Don Luigi Bolner. Born in Pergine in 1839, educated at first by the capucins of Trent, studying later in Verona, teaching there and in Venice, he came to Pomarolo in 1870. Becoming familiar with the history of the two scientist of Pomarolese origin, and perhaps wishing to provide his parishioners with something to do,[i] he

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i Instead of consuming the good wine of the region, for instance the *Marzemino di Trentino*. This wine, which recalls the Bourgueil of the Loire Valley with its fruity bouquet, was and is highly prized. Mozart traveled through the Vallagarina during all three of his visits to Italy,[60] and Don Giovanni, in the last act of the opera, instructs his servant Leporello: «Versa il Vino! Eccellente Marzimino!».

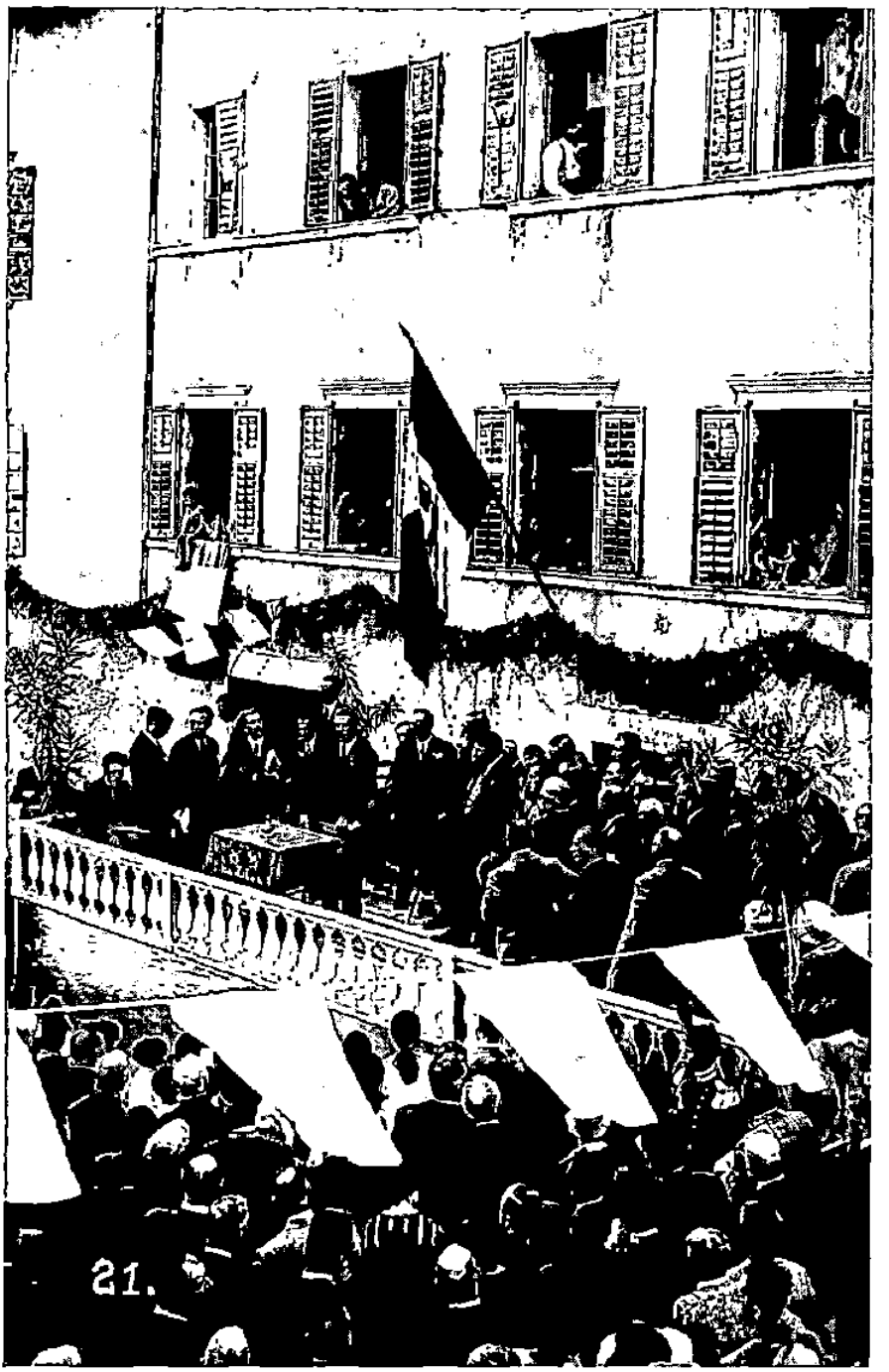

Fig. 11 – Commemoration of Felice and Gregorio Fontana at Pomarolo 13 September 1930. Standing at the left with papers under his left arm is Casimiro Adami.

founded the «Felice and Gregorio Fontana Band Society», for «the moral elevation of its members and as an Ornament to Parish and Town».

Don Luigi tells the story in his «Historical Memoirs of the Felice and Gregorio Musical Society of Pomarolo».[49] It was learned that Sig. Enrico Fontana, descendant of Felice's brother Francesco, had a medallion depicting the bust of Felice, and a lithograph of Gregorio. It was proposed by Sig. Angelo Aldrighetti, a patron member, that copies of these be sent to his brother-in-law Sig. Giulio Eckel, academician painter in Vienna, who agreed to do these portraits at no expense. When they arrived in Pomarolo 5 September 1887, they were «highly praised as masterpieces by every intelligent person»; mounted back-to-back as a *trofeo* they were carried at the head of the Band at the times of its public appearances. They are presently at the *Accademia Roveretana degli Agiati.* Reproduced as veritable many times, they are, particularly that of Felice, largely products of the artist's imagination.

Succeeding commemorations, including the two of 1905 and 1930, were largely due to the efforts of members of the family Adami, Giampio and his son Casimiro and daughter Gina. A full report of the occasion of 10 Semptember 1905 is given by the *Raccoglitore* of Rovereto for September 1905, and the *Alto Adige* (N° 209) of Trent. At that time was displayed the 117-page work[k] of Casimiro Adami, which had been printed only the night before. After performance of the Fontana March by the homonymous Band, many orations were delivered (they are recorded in these newspapers).

Again in 1930, largely owing to Casimiro Adami, a celebration (Fig. 11) was held in Pomarolo in connection with a congress of the Society for the Progress of Sciences at Bolzano and Trent.[50] At this time a bronze plaque (Fig. 12) was affixed to the façade of the *Municipio* of Pomarolo. The work of Stefano Zuech,[l] its inscription (in translation) reads:

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[k] This noble commemorative work was written in one month, according to its author. It is a mine of information. Adami also published: *Un eroe della scienza. Felice Fontana Pomarolese. Narrato ai suoi conterranei.* Rovereto, Tipografia Sant'Ilario, 1930. 74 pp.; and *Felice e Gregorio Fontana.* In: *La Scuola di Pomarolo.* 2nd ed., Trento, 1930, pp. 71-82.

[l] Born in Brez, a village in the *Dolomiti di Brenta,* in 1877, Zuech studied in Vienna, receiving in 1910 a gold medal and a state pension. After World War I he went to Trent, opening a studio in which many pieces, including commemorative medallions, were made.

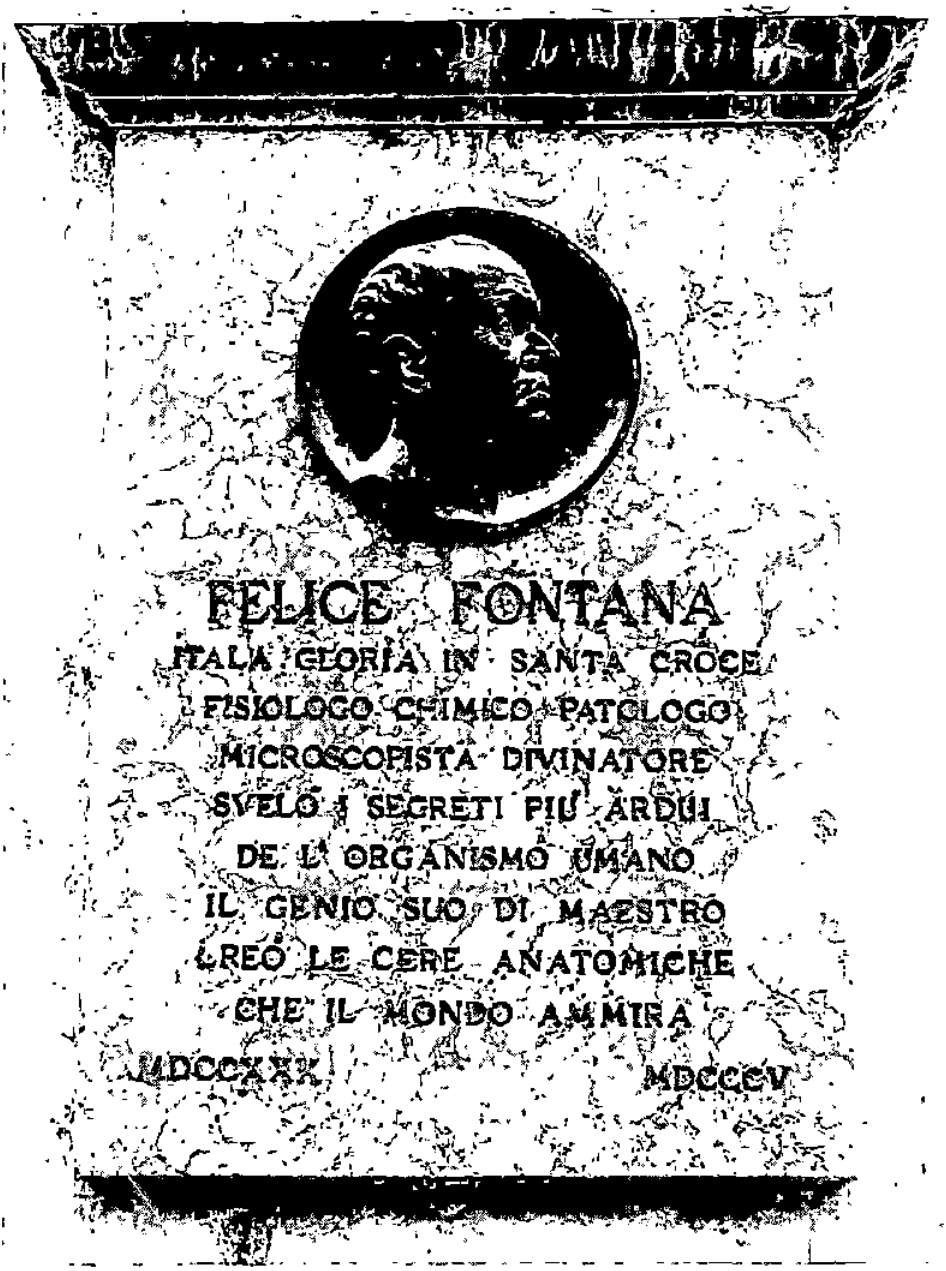

Fig. 12 – Bronze plaque by Stefano Zuech. On the façade of the Municipio of Pomarolo.

Felice Fontana glory of Italy in Santa Croce. Physiologist, chemist, pathologist, microscopist, seer, he revealed the most arduous secrets of the human organism. His masterly genius created the anatomical waxes which the world admires.

1730 1805

Since that time interest in Fontana has grown. Major studies of him have been published, notably those of Schiff,[51] Vedrani,[52,53] and Bilancioni.[54] The paper of Garrison[17] undoubtedly had a seminal influence.

Although Fontana has been spoken of as forgotten, his secondary bibliography, i.e., works about him, with its 275 items appearing between 1810 and 1980, indicates that there has always been a place, during that time, in which he was not forgotten. Most recent biographical notes on Fontana have been those of Belloni[55] and Ongaro.[56] In June 1975 the first International Congress on Ceroplastics met at La Specola in Florence. Of the addresses[57] delivered then, many dealt with Fontana and the anatomical models. Fontana's bibliography,[58] with sections on (a) Veritable Works, (b) Questioned Works, (c) False Attributions, (d) Published Letters, (e) Works on Fontana, and (f) Iconography, as well as the Fontana-Caldani correspondence,[59] have been published.

It is unlikely that many unpublished works in manuscript exist. The eighty volumes in manuscript of works and correspondance at the *Biblioteca Nazionale Centrale,* Florence, spoken of by so many biographers, prove to be largely the writings of Felice's brother, Gregorio;[38] there is evidence that this collection was formed by Felice between 1803 and 1805 and since it contains few of Felice's own works it seems probable that he had few if any unpublished manuscripts of his own at hand.

So, Felice Fontana – who rose from a needy, although not humble. family to form and direct the foremost museum of its kind at that time; who eschewed the trappings of a formal higher education but chose that instruction which made him a highly educated man; to whom nothing in Nature was foreign; who sought her secrets at first hand instead of consulting authorities; who touched on many subjects as a pioneer and enriched them; who gave more than forty years of his life to a foreign country which neither appreciated nor loved him; who devoted his efforts beyond his strength to glorifying in his way a sovereign who was aware of his value as scientist but bemoaned his grandiose ideas and expenditures; who was envied, calumniated and plagiarized on all sides – may now at last come into his own.

Table
Membership in Academies *

Year	Title
1777	The Society of Antiquities of Cassel
1779	The Royal Academy of Science and Letters, Naples
1780	The Royal Academy of Science, Upsala
1785	The Italian Society
1788	The Linnaean Society of London
1789	The Royal Academy of Science and Letters, Mantua
1792	The Royal Academy of Science, Stockolm
1795	The Society of Medicine, Surgery, and Pharmacy, Brussels
1802	The Free Society of Science, Letters, and Arts, Paris
1802	The Academy of Science, Arts, and Letters, Dijon

* Attested by diplomas, in the Biblioteca Comunale, Trent, Ms 3587. His diploma from the American Philosophical Society is not among these.

Appendix
Monetary Equivalents

To some extent the ancient usage of L.S.D., i.e., *lire, soldi,* and *denari* was retained, but other units were commonly cited. The following table gives approximate equivalents in Tuscany in the eighteenth century.

20 soldi = 1 lira = 240 denari
5 lire = 1 fiorino
7 lire = 1 scudo = 10 paoli
2 scudi = 1 zecchino

Any attempt to equate these with another system of coinage or with present-day values seems hopeless, but a European traveler[1] of the 1730s noted that a *lira* was about 6 pence (British). Cochrane[2] calculates that 1 *paolo* was about 1 dollar (U.S.A.) on the basis of a subscription to the *Novelle letterarie* costing 15 *paoli* for 52 numbers, and of a book costing then 10 1/2 *paoli* which would sell in 1970 for about 12 dollars.

1. Keysler, John George. *Travels through Germany, Bohemia, Hungary, Switzerland, Italy, and Lorrain.* Translated from the 2nd ed. of the German. London, Linde, 1757.

2. Cochrane, Eric. *Florence in the forgotten centuries* etc. Chicago, University of Chicago Press, 1973.

CHAPTER 12

FRIENDS AND ENEMIES

Americans - French and English - Swedish Chemists - Bernoulli - Boščović - Brambilla - Brunati - Caldani - Canterzani - Cocchi - Fabbroni - Ferroni - Girardi - Grisellini - Haller - Ingenhousz - Landriani - Mangili - Mascagni - Scarpa - Scopoli - Slop - Spallanzani - Targioni Tozzetti - The Verri Brothers - Volta

His courage foes, his friends
his truth proclaim.
Dryden
Absolom and Achitophel

The following pages are based as far as possible on the correspondence of or about the individuals who played some part in Fontana's life. Much of this is the letters of Fontana himself; few of the letters written to him exist now except as the copies kept by Scarpa, Bonnet, Boščović, van Swinden and Canterzani; many may have been burned along with other family archives as related by Adami.[24] What would we not give, for instance, for the letters of Bettina Slop!

Epistolary comments about Fontana, except for the exasperated accusations of Spallanzani, are often brief, such as mention of «the celebrated Abbé Fontana».

Biographical details have been added in order to show something of all the dimension of these figures.

American Friends

Fontana's relations with the U.S.A. were lively and varied; they were largely with the American Philosophical Society and the individuals

Benjamin Franklin, Thomas Jefferson, and Filippo Mazzei. Only the last, an American by association, needs introduction.

Filippo Mazzei[53] was born at Poggio a Caiano near Florence in 1730 and died in Pisa in 1816. Much of his life was spent in traveling under commissions, particularly for the state of Virginia. In 1756 he went to London which was his home for nearly eighteen years. When he visited Florence in 1765 he was banished by the Inquisition but succeeded in returning and first met Fontana in 1766. The next year, back in London, he received from him an order for Franklin stoves for the Grand Duke. He saw Franklin and decided on two of the small models; thus Peter Leopold was to have the first of these stoves in Europe. Mazzei continued to promote himself and Italo-American relations. In 1779 he signed an oath of allegiance to the State of Virginia and became its agent in Europe. His success in Tuscany was apparently qualified; he wrote familiarly, and slightingly, of the Grand Duke as «Leopold». The pushing Mazzei seems to have been made the butt of Peter Leopold's wit. On 12 May 1785 he wrote to «Sig. Giovanni Blair,[a] president of the constitutional society in America» which Mazzei had helped to organize in 1784[55e] proposing for honorary membership some Europeans: the Duc de La Rochefoucauld, Beccaria, Fontana, and Spallanzani. No more is known of this.

In 1773 Mazzei transmitted some works to the American Philosophical Society:

> I have likewise 3 pamphlets written by Mr. Fontana, who has written many more, and every one containing some discoveries, but he could not find the others at that time, and has promised to send them by the first opportunity.

These probably included the works on the *ruggine del grano* and *il veleno della vipera,* both of 1767, which are in the library of that institution. Its minutes contain documents showing that the works were sent in response to an approach made in 1771 through the influence of Benjamin Franklin

[a] This is John Blair (1732-1800), born in Williamsburg, Virginia. He was a representative of the College of William and Mary at the convention that drew up a constitution for the new Commonwealth of Virginia, and in December 1786 he was selected as one of Virginia's delegates to the constitutional convention that met in Philadelphia. In September 1789 he was appointed by President Washington an Associate Justice of the Supreme Court.

to European scientific societies, by which the American Society's *Transactions* were sent to Europe. Notice of this action appeared in the Florentine *Novelle Letterarie* and the Venetian *Giornale d'Italia.* An elaborate letter in Latin dated June 1773 to the secretaries of the American Society on behalf of Peter Leopold accompanied Fontana's books.

In 1779 Fontana and Franklin met in London; Fontana's works sent to America were known to Franklin, and those of the latter[56] were well-known to Fontana. Their chief mutual interest was certainly electricity, as Fontana had traveled through Tuscany seeing to the installation of lightning conductors and had equipped two rooms of the Royal Museum with electrical instruments including «a small model of Franklin's Lightning House».

In 1780 Thomas Jefferson wrote to Mazzei:[57a] «I have not yet sent on Fontana's works to Philadelphia, expecting the plates from you». The works probably included those on *aria fissa* and the *falso ergot e la tremella,* both of 1775, now in the Society's library, presented in the name of the author by Thomas Jefferson at the first meeting of the Society which he attended, at the «University» (of Virginia).[57b] That year Fontana was elected to membership in the Society by unanimous vote. He wrote to Humphry Marshall:[b]

> We wish to be informed if we can be supplied with any of the natural productions of America, either by barter for the productions of Italy, or at a moderate price. Quadrupeds, Birds, Insects, Worms, or Serpents, the last of which may be preserved in Spirit of wine. Minerals, Seeds and Plants, particularly that plant called *Dionea Muscipula* which is found in low marshy places in South Carolina. For such articles we shall be willing to pay the customary price or return the value of them in such plants as we are in possession of; a Catalogue of which we now send you. If any Gentleman of the Philosophical Society of Philadelphia is willing to enter on such a friendly intercourse with the Royal Museum of the grand Duke, they will please to address their Letters to Monsieur L'Abbé Fontana à Florence.

This letter, which was in French, accompanied one from Marshall's friend, Logan, who had been asked by Marshall to translate it:

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[b] Humphry Marshall (1722-1801), born in Pennsylvania, was largely self-educated. his house, which he built himself, is still standing at Marshallton; it had a conservatory for rare plants, a small observatory, and a botanic garden including many native and foreign speciments. He was the author of *Arbustum Americanum,* the *American Grove,* 1785, a list of native trees and shrubs. He was a member of the American Philosophical Society.

The above is a translation of the Letter from the abbe Fontana [...] I wish you to write to the abbe, give him such information as you may think proper and testify your desire of doing anything in your power to serve that grand institution [...]
Phila. August 26,th 1783 G.W. Logan

The next year Fontana wrote to Marshall in English:

Pisa, in Tuscany, 16th January, 1784. Sir:
It is with a great pleasure that I have received [...] your letters, and the two boxes of American plants, which you was so good to forward to us; they came almost all alive [...] I must confess to you the greatest obligation [...] your most humble and obedient servant.

At nearly the same time, Thomas Jefferson wrote to James Madison[57c]

Philadelphia May 25, 1784
[...] Boinod will receive very soon the following books which he wrote for [...] should you chuse any of them you will write to him and he will send them to you [...] Scheele's chemical observations on air and fire. Whatever has been written on air or fire by Fontana, Priestly, Ingenhouse, Black, Irvine, or Crawford.

In 1787 Jefferson[57d] sent to Europe some diplomas of membership in the American Philosophical Society, including those of Fontana, elected in 1783, Bergman in 1773, Lavoisier in 1775. Apparently Jefferson and Fontana never corresponded otherwise.[58]

FRIENDS IN FRANCE AND ENGLAND

Jean Darcet (or D'Arcet) (1725-1801) did not wish to become a magistrate like his father, but to study history, his interest being developed by Montesquieu. Although he received the degree of M.D. in Paris in 1762, he never practiced medicine, since «his ambition was in science and in happiness more than glory», but studied chemistry with Rouelle and married Rouelle's daughter. He succeeded Macquer as director of the Sèvres porcelain factory and became director of the mint. Although not active in politics, he was denounced by Robespierre as a partisan of the Duc d'Orleans but was saved by Fourcroy. Later he became a member of the Senate in which he played no large part. Cuvier said of him, «he served theory and practice at the same time». He translated into French both books of Fontana on the venom of the viper (1767 and 1781). He was the recipient of Fontana's «letter» of 1783 on hydatids, taenia, and the crystalline lens.[5a, 6b]

Marie Joseph Louis d'Albert d'Ailly, Duc de Chaulnes (1741-after 1789) published on fixed air, microcosmic salt, and phosphoric acid, 1764-1783. He was the recipient of Fontana's «letter» on animal respiration.[26a]

René-Nicolas - Dufriche Desgenettes. Born in 1762, he was deeply impressed by the lectures of Buffon and turned to the study of medicine. After much travel, particularly in Italy in 1785-89, and frequenting scientific societies, he obtained the M.D. degree at Montpellier in 1789 and published there a work on the lymphatics. Entering military servi ce he served in the *armée d'Italie* in 1793, was back in Paris in 1794. Bonaparte took him along to Egypt where, during an epidemic of what was feared to be plague, he made a heroic gesture. By plunging a knife into a supposed bubo, then into his own arm, he proved that the illness was a simple fever. Many sick soldiers were cured thereby immediately. After serving through military campaigns in Prussia, Poland, and Russia, he was Professor of Hygiene in the Faculty of Medicine, Paris, until 1833.[17]

The major writings of Desgenettes were in anatomy, notably *Sur l'utilité de l'anatomie artificielle & en particulier sur la collection de Florence, & la nécessité d'en former de semblables en France,*[21] reviewing the subject and giving one of the most extensive and detailed descriptions which we have of the Royal Museum in Florence with a closely reasoned argument for the introduction of anatomical models into the instruction of anatomy in Paris. It would seem that he must have met Fontana, whom he names, but he does not explicitly say so. However, his article in the *Dictionnaire des sciences médicales* of 1821 appears to speak from an intimate knowledge of the history of Fontana and of the operations of the museum. It corrects the erroneous statement of Cuvier in the earlier *Biographie universelle* that the wax models were made from drawings, pointing out that numerous dissections were done and that Fontana himself was a skilled anatomist. Many events of the times, even the controversy with Giorgi which earlier biographers tended to avoid discussing, are presented in detail. According to Dechambre (*Dictionnaire encyclopédique des sciences médicales,* etc., Paris, 1879, IV, 195-96), Desgenettes was Fontana's friend as well as biographer. He died in 1837.

Jean Gibelin. Born at Aix in 1744, he became curator of the library and perpetual secretary of the Academy of Sciences there. Known as a naturalist and translator of the works of Priestley, he also made

translations of Fontana's *Opuscoli scientifici,* Firenze, 1783, as *Opuscules physiques et chymiques,* Paris, 1784, and reprinted the book on nitrous and deflogisticated airs (1776) with Priestley's work *On the airs,* Paris, 1780. He was the recipient of Fontana's «letters» on the conversion of water into earth (1782) and on a supposed remedy against the venom of the viper and the contents of nerve fibers (1783). He died in 1828.[7a]

Louis Bernard Guyton de Morveau (1737-1816). Practising law, he was admitted to the Dijon Academy as an honorary member. When he criticized a chemical lecture there he was advised to confine himself to subjects with which he was familiar, whereupon he turned to the study of chemistry. As permanent secretary of the Dijon Academy from 1786 he became acquainted with the Parisian chemists and a Correspondent of the Paris Academy. He became F.R.S. in 1788. Director of several chemical industries and the Ecole Polytechnique, he wrote widely on chemistry, but held some rather peculiar ideas. He abandoned phlogiston at the right moment and embraced the new chemistry.

Enjoying a great reputation at one time, he is now almost forgotten. He was the recipient of «letters» from Fontana in 1790 and the one «to Murray» of 1783. He wrote to Bergman[27a] 10 January 1780 that Fontana had passed that way on his return to Tuscany, and that they agreed that those who loved the truth without bias or system depend much on Bergman's memoirs to establish the true principles.[26b]

Joseph Banks (1743-1820). Interested in botany and natural history, he accompanied Cook's expedition in the Endeavour, 1768-1771. On the retirement of Sir John Pringle from the presidency of the Royal Society in 1778, Banks succeeded him, was president until his death. He surely met Fontana in London; we have a letter[16b] written from Spring Grove, 25 June 1779, to Giovanni Fabbroni in the Haymarket, enclosing two letters of introduction, one to Banks's mineral agent in Derbyshire, the other to W. Gilbert «who contrived the Duke of Bridgewater's canal», opened in 1761.

Tiberius Cavallo (1749-1809). Born in Naples, the son of a physician, he went to England at an early age. In October 1775 he published his «Extraordinary Electricity of the Atmosphere observed at Islington», which was reprinted. He invented many ingenious instruments and pieces of apparatus for chemical and electrical experiments, became F.R.S. in 1779. From 1781 to 1803 he published six books on electricity and the

airs. Fontana mentioned him several times as taking part in experiments while he was in London.[4b]

Richard Kirwan (1733-1812). Born in Ireland, of English ancestry, he studied law in Poitiers and practised it without much success. Much of his chemical work was done in London, 1777-1787, after which he returned to Dublin where he was President of the Royal Irish Academy until 1812. Brilliant but eccentric, he wore his hat and coat indoors, and lived on ham and milk, because his health was poor. His reputation was international, based on his publications in chemistry. At first he defended the phlogiston theory vigorously, but abandoned it in 1791 when he failed to show that fixed air was composed of oxygen and phlogiston, but he was not enthusiastic about the new chemical theory. He cited Fontana frequently in two of his early and most important papers,[29] used his data on the density of some gases, and told of seeing Fontana demonstrate experiments in June 1779, but in a letter of 6 October 1783 he wrote to Torbern Bergman:[27b]

> there are some persons who can not or will not do anything themselves, allowing others to have the name of finding something new. It annoys me, the Count of Saluces[h] in Italy being one of this number, and the Abbé Fontana showing too a little charlatanry and being inclined the same way.[4e, 5c, 26c]

Joseph Priestley (1733-1804). To describe Priestley's life and works would go far beyond the scope of this chapter, but some parallels between him and Fontana need comment. They lived almost exactly contemporaneously; they must have been alike in many ways. It has been said that Priestley was of prodigious industry; he spoke and moved rapidly – his experiments imply great deftness of manipulation; in private converse he was vivacious; as a controversialist he was not enjoyable – he was bad-tempered and superior; his ridicule was heavy and he was fond of the phrase, «I flatter myself». He often smiled but seldom laughed, he was fond of anecdote. They both suffered invasions of their homes by a destructive rabble.

Priestley came to London once a month to attend the meetings of the Royal Society, but in 1793 he found that they were rejecting eligible members on political grounds, and withdrew. It is likely that he heard the papers of Fontana which were delivered in 1779. In his works when mentioning the Abbé Fontana's discovery of the absorptive power of charcoal for gases, he wrote that he did so with Fontana's permission so they must have corresponded, but no letters are known, except the

so-called «Letters to Dr. Priestley» as Fontana entitled his paper on the airs drawn from water. We have a letter from Priestley to Giovanni Fabbroni[16c] of 20 June 1779 but it is without interest.[4d, 5d, 26d, 32]

Franz Xaver Schwediauer (1748-1824). Austrian by birth, he became M.D. in Vienna but settled in London in 1772 as a medical practitioner, moving to Paris in 1789. Fame he acquired as a medical and pharmacological author; he edited the *Foreign Medical Review* (later called the *London Medical Journal*) which he started in 1780. A man of extensive erudition and far-reaching interests, he had a wide acquaintance among which was Fontana during his stay in London. He corresponded with Torbern Bergman until the latter's death, twenty-four letters surviving.[27c]

On 15 December 1781 Schwediauer wrote to Bergman:[27e]

> Fontana is a strictly honorable man; he has gone to Italy although not himself Italian but born in the Tyrol; his character is other than that of the detestable and diabolical Italian nation.

Benjamin Wilson (1721-1788). Known also as a portrait painter, Wilson was famous as a scientist chiefly for his electrical experiments and a treatise on electricity. He wrote to Torbern Bergman[27d] 15 March 1779. Great Russell Street, Bloomsbury, London.

> [...] the abbé Felice Fontana is most warmly of that opinion (that Torbern Bergman is the «greatest chymist in Europe»). He is in London and is my neighbour. I esteem him highly, as I think he far excells all the other philosophers who have visited this kingdom.

Wilson presented Fontana with a tourmaline of great size.

The Swedish Chemists

This was the term often used by Fontana to refer to Bergman and Scheele, both chemists of great eminence.

Torbern Bergman, 1735-1784, the son of a tax-collector, after a brilliant career as a student in «natural philosophy», became a member of the Swedish Academy in 1764 and professor of chemistry at Uppsala in 1767, having studied the subject but not having published on it. His health failed in 1769 and in 1780 he retired from active work. A chemical library, a mineral collection, and a large collection of apparatus were formed by him. Evolving his own methods which derived from his

previous mastery of the field of mathematics its precision and severity, from that of natural history its observational procedures, he made many contributions of which the most important were in qualitative and quantitative analysis, classification chemically of minerals, and elective affinity. His essays begin with complete historical introductions; that *On the Investigation of Truth* presents the experimental method as exemplified by Newton. Till the end of his life he accepted the phlogiston theory; he paid little attention to Lavoisier. A number of his students' theses and of his works were in the library of the Royal Museum; some of the latter were translated into Italian by a group of Florentines around the scholarly publisher Tofani, including Giovanni Fabbroni and Gaetano Cioni.

It seems that he carefully preserved the letters he received, but of his letters to foreign correspondents few are extant.[27] Schiff in his history of the museum[45] wrote of a visit of Bergman to Florence, but this is in error.[46] As far as we know, he and Fontana never corresponded but many others wrote to him about Fontana.[27e]

Carl Wilhelm Scheele (1742-1786), was born in Stralsund, which was then in Swedish Pomerania. He began, while working in apothecary shops in Göteborg, Malmö, and Stockholm, to study and experiment in chemistry. He got to know Gahn, Bergman's assistant, and later Bergman, to whom he owed his emergence from obscurity. While still an apothecary's assistant in 1775 he was elected to the Royal Academy of Sciences of Sweden; the «pursuit of scientific truth, unaffected alike by disappointment or success, was the one object of his brief life». It was said of him, «when it is only a question of facts, Scheele is infallible», but he held to the theory of phlogiston all his life. His experiments were well-planned and his publications were systematic; his contributions to chemistry are astonishing, in view of his limited opportunities, in both number and importance. It was he who first discovered oxygen.

His book *On Air and Fire* was in the library of the Royal Museum. It is said that he was invited by Peter Leopold to settle in Florence;[27f] no more is known of this except that he was never there.[46, 26f, g]

Johann Bernoulli member of a famous family of scientists which originally dwelt in Antwerp but, for reason of religion, fled first to Frankfurt then to Basel, Johann (Jean, Giovanni) III Bernoulli was born in Basel in 1744 and died in Berlin in 1807. A child prodigy, he received the degree of master of jurisprudence at the age of fourteen; when

twenty he was invited to reorganize the astronomical observatory of the Berlin Academy. His mathematical treatises were of little interest, but extensive travel in spite of frail health led to several accounts which had considerable cultural impact.

Bernoulli's voluminous correspondence included six letters[75] from Felice Fontana, the first of which is shown in Fig. 13 as an example of his handwriting at its most careful. It reads:

> Florence, from my home 30 March 1775
> The esteem that I have for your varied talents, and the wish to know you personally, give me the courage to write you this letter, and to offer you a place in my home for all the time that you wish to stay in Florence. I hope that you will do me this honor, and I hope that when you know me you will not think me unworthy of your friendship [...] with all esteem I am
> your servant Felice Fontana

In three subsequent letters Fontana wrote:

> 16 June 1775
> [...] By Procaccia Fensi I send you one of my books to present to the Berlin Academy. There will be some more, which you will give to whomever you please. In the same package you will find some pages for yourself, but not all. The rest I will send to Leghorn to the Abb. Serafini. I have not had time to amplify my trifles on electricity, thus I am obliged to send you them as they are. I believe that you have already received my two pages on Electrical Analysis, and the third on my published and unpublished works. Even so undeveloped a sketch is enough for the great geometer and physicist Daniel Bernoulli[c] [...] I await your letter from Basel, also on this particular.
>
> Although I am not very well, and perhaps even worse than when you left me, I will not in any case finish this month in Florence, and passing through Mantua I shall have more detailed news of that observatory and will send it to you in Basel. Trust me.
>
> Turin, 18 December 1775
> I include in this letter a page containing the [illegible] theorem of [illegible] of Leiden. Show it to the Hon. Mr. de la Grange and write him your opinion, since I will have left Tuscany. A few days after your departure from that place, I went to the Tyrol, my own country, in search of the health that I had lost, which I had little hope of regaining. Yet with the most strict regimen I finally recovered, and so well that I have been able to undertake my travel to France. Arrived in Turin I found there your very kind letter sent to me from Florence [...] Tomorrow morning I leave for Geneva and from there in a few days for Paris, where I hope you will

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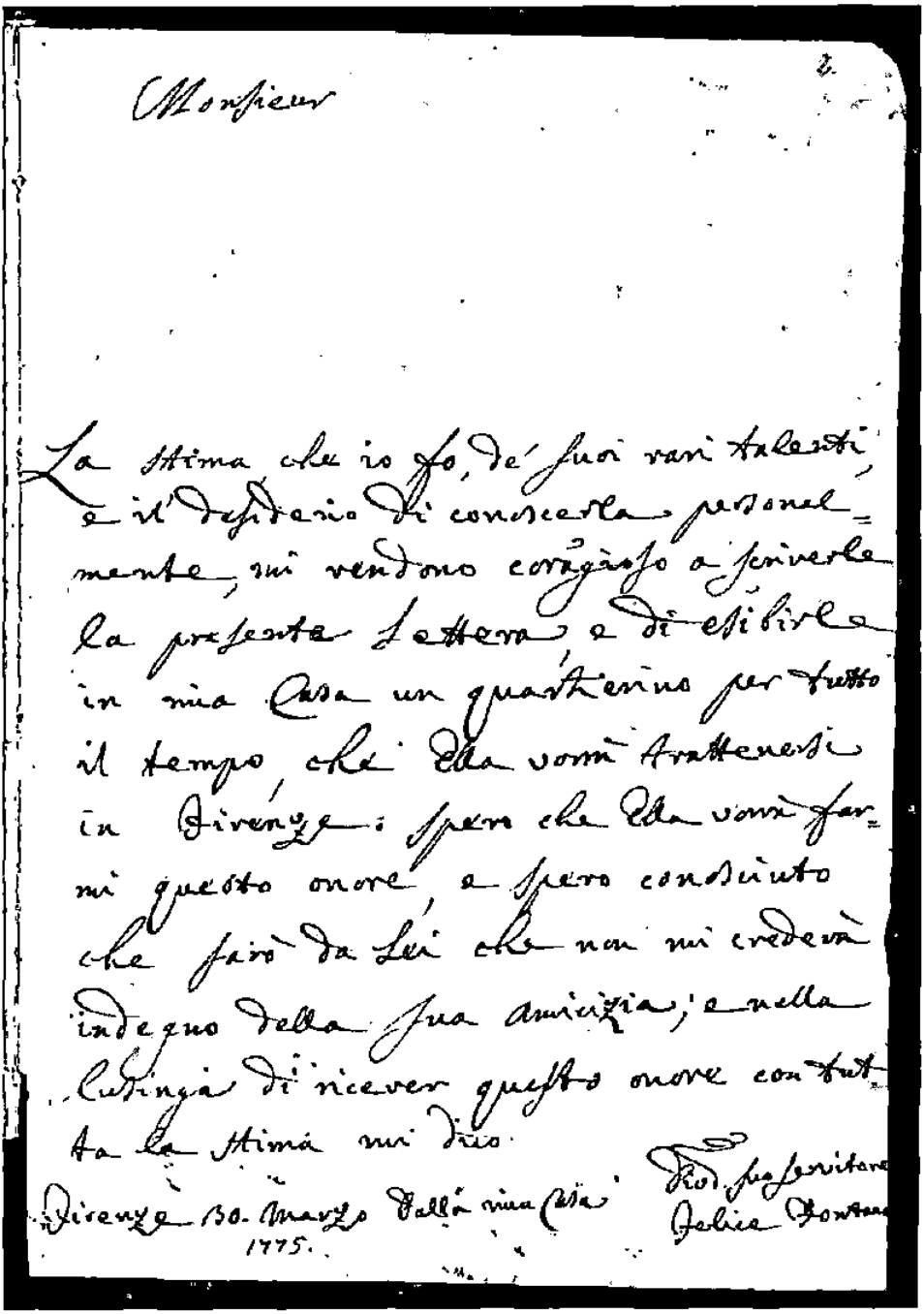
[c] Daniel Bernoulli (1700-1782), uncle of Johann III.



Monsieur

La stima che io fo, de' suoi rari talenti, e il desiderio di conoscerla personalmente, mi vendono coraggio a scriverle la presente lettera, e di esibirle in mia Casa un quartierino per tutto il tempo, che Ella vorrà trattenersi in Firenze: spero che Ella vorrà farmi questo onore, e spero conosciuto che farò da Lei che non mi crederà indegno della sua amicizia; e nella lusinga di ricever questo onore con tutta la stima mi dico

Firenze 30. Marzo 1775. Dalla mia Casa

Suo Servitore
Felice Fontana

Fig. 13 – Letter of Fontana to Johann III Bernoulli.

write to me in care of the Abbé Niccoli in charge of the Grand Ducal affairs at that Court. If you will show my pages on electricity etc. to that great man La Grange, I shall be glad to hear what he thinks of them, meanwhile I beg you to assure him that I am among his admirers. If you ever decide to publish something on your travels to Italy, you would do me a great favor to say with your usual sincerity what you saw in the collection, and particularly about my collection of pendulum clocks, moveable quadrants, and the micrometer objective, etc., and about the observatory, since they write me that young Cassini,[d] after having found everything perfect when I was in Florence, subsequently won over by my enemies, has spoken badly, or at least not very well, of it. This matter is of great concern to me, and only a Bernoulli can do me what justice I may merit. I await your letter in Paris, meanwhile I am your true friend.

Paris, 21 January 1776

I have been in Paris for eight days and I am more sure than ever, as I wrote you from Turin, that young Cassini, after my departure from Florence, has tried to discredit me in the mind of my Sovereign, and has chiefly attacked my astronomical pendulum and my mobile quadrant with all its divisions, eighteen inches in radius and all mounted in brass [...]

I beg you in all earnestness to get something into print about these two instruments of mine, which I believe superior to any others we have. Thus I ask you to speak of my microscopic objective, and my dividers, which you have seen. It seems to me also that it might be said that I have overcome all the difficulties in measuring distance, and that with a single change I have remedied the unequal thrust of the screws, the lost motion, and the uneven pressure of [illegible] in the filar micrometer which made it of little use, and that instrument suspect.

I wish that as soon as possible you could speak of these things of mine in some of your letters on Italy, or better in the volume of the Atti of Berlin closest to being printed [...]

In two later letters from Paris in February and April of 1776, Fontana wrote more about the intrigues of Cassini and a great deal about correction of the pendulum, a filar micrometer for telescopes, and friendly relations with Lagrange and Lalande. Bernoulli proved to be a true friend, publishing an article[76] about Fontana's new machine for inscribing instruments, improved over that of Chaulnes, pendulums corrected for temperature changes, better spirit levels, astronomical quadrants, and new micrometers. The article refers to the publications

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[d] This must have been Jacques Dominique Cassini, Comte de Thury, fourth-generation member of a family of astronomers, italian in origin but established in France. He was born in Paris in 1748, had published in 1776 only an account of a voyage to investigate marine monsters. He was made a member of the Paris Academy of Sciences in 1779. Later he became known as a maker of maps.

Avanzamento dell'Arte delle Manifatture e del Commercio, Florence 1773, and the *Saggio del real Gabinetto* etc., Rome 1775. He also gave a description of the Royal Museum in a report of his visit to Florence.[77]

Rudjer Bošković. Born in Dubrovnik in 1711, he died in Milan in 1787. He was «the last polymath to figure in an important way in the history of science, his career was something of an anachronism and something of an enigma». Croatian by birth, he became a Jesuit, was drawn to Italy in 1725. In 1735 he began to study Newton and in 1754 published his textbook *Elementa universae matheseos.* Falling out of sympathy with his ecclesiastical superiors he went to Paris in 1759, on to London in 1760 where he met Franklin. He was made F.R.S. in 1761. After many more travels he was back in Pavia in 1763, concentrating on optics and the improvement of telescopic lenses. The Company of Jesus being suppressed in 1773, he went to Paris again, working on optics and astronomy. There too disputes occurred, one with Laplace over determining the path of a comet, another with the Abbé Alexis de Rochon over priorities in the invention of a type of micrometer and megameter consisting of pairs of rotating prisms, which device became important in the design of rotating telemeters. His correspondence with Fontana on this matter is discussed in Chapter 6.

Giovanni Alessandro Brambilla (1728-1800), was born at San Zenone (Pavia). After studying medicine, particularly surgery, at the University of Pavia he entered the Imperial Austrian army as surgeon and was promoted to major after submitting a dissertation to Vienna. The fortunate outcome of two operations, one on Lacy, commandant of his regiment, drew attention to him, and in 1763 he became surgeon of the imperial guard. As surgeon to Peter Leopold after 1764, he would have gone to Tuscany with him in 1765 but Joseph II kept him in Vienna. He became counselor to Joseph on matters of public health and many improvements in hospitals, jails, asylums for the young, the aged, and pregnant women, and military sanitation are attributable to Brambilla. He suggested Frank and Scarpa for professorial chairs at Pavia; when Joseph had acquired full power after the death of Maria Theresa, Brambilla improved the teaching of surgery in the medical school and was influential in the founding of the military medico-surgical school, the «Josephinum», which was inaugurated in 1785 with Brambilla's address on the superiority of surgery to medicine. He published books particularly on the history of surgery. Joseph gave him the fief of Carpiano in

1784, to which he retired in 1795 with full emolument from Francis II, his position having become difficult after the death of Joseph in 1790. After the battle of Marengo he went to Pavia, where he died.

Peter Leopold wrote of Brambilla as one of the persons most necessary to get away from Joseph because of his evil influence, especially with «low people», i.e., as procurer, in fact getting him everything he wanted, including praise in the press; that he meddled in everything, told everything, criticized everyone, and was very dangerous.[10] Pietro Verri[9c] called Brambilla his good friend. Gorani, a traveller in Italy in the 1700s, wrote[11] most disparagingly about him.

Giovanni Francesco Brunati. Born to a noble family of Rovereto in 1723, he studied law at Innsbruck, Bologna, and Padova. Francis Stephen designated him agent of the Austrian Embassy to the Vatican, 8 June 1751, secretary in 1758. Confirmation came from Joseph II in 1784 and Francis II in 1794. He died in Rovereto in 1806.

One of his friends was Felice Fontana, five letters from whom are in the Rovereto collection.[12] They go from April 1768 to June 1770. Among the subjects mentioned is: F. sends B. money to settle brother Gregorio's debts.

Leopoldo Marc'Antonio Caldani (1725-1813) was born in Bologna, studied medicine there, and became doctor of philosophy and medicine in October, 1750. While he was assistant at the *Ospedale di S. Maria delle Morte* he pursued his studies in anatomy and was appointed professor of medical practice in 1755. From 1756 he and Haller corresponded frequently until the latter's death in 1777. The jealousy and intrigues of Caldani's colleagues drove him from Bologna; put in charge of anatomy also there in 1759-60, he resigned and went to Venice. He had spent some of 1758 with Morgagni in Padova, where he now was appointed professor of the theory of medicine in 1764, although he did not actually accede to the chair until 1765, on the death of its occupant. The appointment was made with the condition that Caldani would succeed Morgagni as professor of anatomy, who had strongly contested it as he wished to be followed by one of his own pupils, particularly Michele Girardi. From 1773-1805 Caldani held the chair, producing his *Institutiones pathologicae* (Padova 1772), *Institutiones physiologicae* (Padova 1773), and *Institutiones anatomicae* (ibid., 1787-91).

It was mainly owing to the work of Caldani that the powerful resistance to Haller's doctrine in Italy was overcome. Many experiments

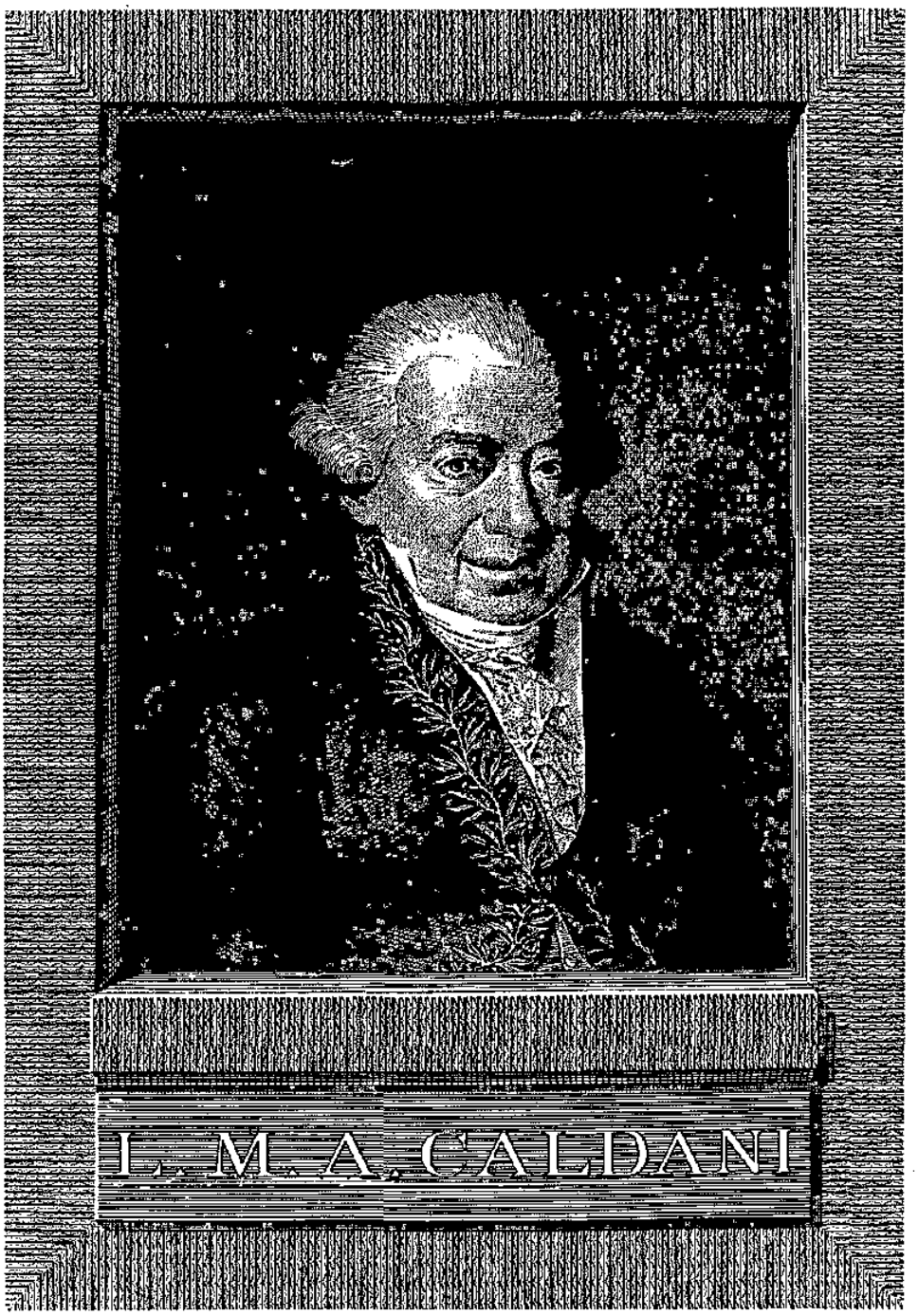

Fig. 14 – Portrait of L.M.A. Caldani.

were performed and many papers written about them. Caldani was a pioneer in the use of electrical stimulation, unfortunately only through the use of the Leiden jar. It has been remarked, what would he not have discovered if he had had an induction coil! Unfortunately he was too much inclined to disregard an observation if it did not fit into a certain theoretical scheme, especially if the scheme had been advanced by Haller. Caldani repeatedly observed that the internal surface of the *dura mater* was sensitive to mechanical stimuli, but this being denied and explained away by Haller, Caldani agreed.

With others Caldani was not so agreeable – his character was apparently highly colored: one biographer wrote that he «expressed himself with that flaming heat of certain characters from Bologna»; another that he saw things through eyes «green as bile». His friends Haller and Somis (Torinese physician who served as forwarding agent) tried to ameliorate his character; recorded in his favor is his retiring in good order, i.e., renouncing priority without polemics, when he had discovered the acqueduct of the inner ear before Cotugno but the latter published first.

The relations between Caldani (Fig. 14) and Fontana were not entirely amicable. Caldani was not pleased when their two letters appeared side by side in the third volume of Haller's memoir of 1760 on irritability. Justly or not, Caldani was irritated by what he believed was Fontana's delay in returning a manuscript. On 28 May 1774 Caldani wrote to Haller[13a] that he corresponded with friends in Bologna, but not in Florence, adding that Fontana had broken off friendly relations with him, for which he was thankful.

No soothing influence was provided by Lazzaro Spallanzani who wrote to Caldani 23 June 1775:[14a]

> From your note, I feel that you know him [Felice] well enough, that you do not have good relations with him, as surely no one who loves truth and honesty could have. Still, some reason, such as his giving you his works, might be a cause for you to remain his friend, as it seems you once were. So I wish that, in the answer you make me, you sincerely open your heart to me, as I have to you; you can be sure that I will not reveal the secret to a living soul. If you still should be his friend, you will have no difficulty in telling me so; or if you abhor his way of operating, as does all Italy, as well as Florence. In the second case, I will explain myself more clearly to you, and give you some confidences, which will not displease you, indeed will not be unimportant to you. I am all yours, Lazzaro Spallanzani

Spallanzani wrote again a month later:[14b]

[it] should not be a matter of indifference to you to see a little bit humiliated someone who, depreciating everyone, spares not even yourself. I speak of your Physiology, which, in spite of its universal acceptance by the experts, and its supreme usefulness to the young, has been treated by him as a puerile thing. I have heard this from some of my students, now practising at the Santa Maria Hospital in Florence, who took their medical degrees at Pavia. That's how it is: in Italy he wants to be the only one – in Physiology as in Physics and Natural History. The Scolopian brother thinks in the same way. You, I, and others could go on printing until Judgment Day with no danger of his reading half a page of it. Works in Physiology that do not come from the pen of his brother he calls laughable. But I take care to give him none of mine.

Only two weeks after this, Fontana wrote to Caldani:[15a]

Here in Rovereto I have found your Physiology, which I have read with pleasure. It is fine, and well written; here and there are gleams of genius. Your nerve force – *vis nervea,* could be very fine but it is not developed; perhaps it lacks some more principles, which you will see treated in the fourth Volume of my Animal Physics, in good time.

Not entirely soothing words either, they were followed by a long discussion on the iris. Fontana was then on his way to Paris, but he must have heard more in the meantime, perhaps from Caldani himself. He wrote again two weeks later from Rovereto:[15b]

Your letter has made me realize that you have been thinking badly of me, and you would be entirely right if I were in the wrong. I see that you have been listening to gossip that someone has picked up perhaps in Tuscany against me, perhaps someone no friend to me, perhaps even less to you. Whoever carries such tales between friends can't be an honest man, and I'm sure that it wasn't an honest one who told you that I had adjudged your Pathology *[sic]* as a *puerile thing.* I never said it nor could I say it, because I do not believe it inferior to any other, and thus I told Sig. Lagusius, chief physician of the Grand Duke of Tuscany, to read it; to learn this you have only to write him and ask him how I have always spoken of you. You can be sure then that he who told you such a lie is a wicked man, a scoundrel; I beg you to confront him with me when I come to Padua and then you will see, perhaps it will be found out where he picked up such a thing. I would say more if I could recall; nor do I remember seeing any of your pupils in recent years, but I might be wrong.

The letter proceeds with another long discussion of the iris, but whereas the previous letter was signed: «in all friendship, your affectionate friend Felice Fontana», the last one ends only: «yours, Fontana». The correspondence did not cease; we have letters of Fontana to Caldani of

1793 and 1794[15c] about anatomical models of the heart and the ear which are promised to Caldani. Both are signed, «most affectionate friend, F. Fontana».

Sebastiano Canterzani (1734-1819). Born and dying in Bologna, Canterzani was laureate at twenty-two, professor (of astronomy) at twentysix. As secretary and later president of the *Istituto Marsiliano,* ultimately Senator, he was a well-known figure, a member of many academies and societies. He corresponded with the American Philosophical Society, as did Fontana. Modest and sober in everything, he was heavy in manner but not rude. He left behind him many publications and eight children.

In the university library at Bologna, there are forty-one letters from Felice Fontana, with their replies, from October 1769 to August 1801, almost all of them short, of one page. Fontana appears to have been the agent for many of his friends, such as Slop (many times), Pietro Ferroni, and various French and Germans, in sending their works to Canterzani, either for publication or for suggestions, also arranging for the foreigners to meet the scientists of Bologna. Until 1775, when the letters stop while Fontana was in Paris and London, they tell of the many instruments and machines that Fontana had invented and had built, or mention the works that he is sending. In 1784-85 they are frantically seeking help in finding modellers in wax, in 1786 they send memoirs about the controversy with Giorgi. In the last letter, Fontana wrote that their long friendship and his great esteem for Canterzani led him to write on the occasion of a visit to Florence of the Bolognese physicist Aldini, nephew of Galvani. The letters, while not as intimate as are those to Slop, are evidence of a deep and abiding friendship.

Raimondo Cocchi. Born in 1735 into the learned ambience of the home in Florence of his father, Antonio, physician and scholar, Raimondo began to study medicine at the age of nineteen, but his progress was so rapid that he became laureate in 1757. He assisted his father at first but cared little for medical practice, or for teaching anatomy, so that in 1771 he resigned from his appointment as professor of anatomy at S. Maria Nuova (since 1758). He enjoyed collecting and literature, was made royal antiquary and custodian of the royal cabinet of gems by Francis Stephen and on the death of Canon Querci in 1773 Cocchi became director of the Uffizi. He wrote about a trip to Corsica and some «physical-anatomical lessons» (Leghorn 1775). Suffering from pulmonary tuberculosis after the age of thirty, he died in 1775.

Many biographers speak of Cocchi as a friend of Fontana, whose only mention of him in a letter is in one of 9 April 1764 from Pisa to L. M-A. Caldani:[16]

> Immediately on receiving your writings I will read and study them without loss of time, but so far they have not reached me. Cocchi never arrived in Pisa, although he said and wrote that he was coming; I had to write to him to consign them to someone reliable. I believe it would be useless for him to arrive without the Hallerian principles.

This sounds as though Caldani had given these writings to Cocchi, whom he probably knew as they were both concerned about the problem of the iris, to give to Fontana. These «Hallerian principles» were probably the «fourth letter» about which Caldani wrote to Haller 16 February 1765:[13b]

> My fourth letter would have been out for a year, if our friend Fontana had not done me the disfavor of slowing it up. More than a year has passed that he has had it in order to review certain passages which concern him; he has never returned it. I am very indignant with him, the more so in that every time he has sent me something to be reviewed, I have always abandoned all my affairs to serve him.

Giovanni Fabbroni. Our first glimpse of him comes from Filippo Mazzei *(q.v.),* apparently in 1766, although the date cannot be fixed with any certainty. Mazzei wrote:[55a]

> While I was provisioning and preparing what I wished to take with me, there came to find me one morning two youths, unknown to me, who wished to come with me in America, whereas I thought that they could find suitable employment [in Italy]. One was Giovanni Fabbroni, who was not yet fifteen, whose appearance pleased me mightily, and I was surprised at all the knowledge he had acquired at that age, and even more at the solidity of his reasoning. The other was a certain Ab. Zaccagni, whose age I did not learn but I supposed him to have about two years more than Fabbroni (although smaller in size), and I could not say about his talents, because I spoke little with him. Concerning their wish, I said that I could not give them anything definite before being some time in that country. Going out with them as they were leaving, I gave Fabbroni to understand that I would like to see him without his companion. When I saw him again, I said that Mr. Thomas Jefferson, Virginian, with a name of great scholarship, excellence in science and in law, who had learned our language by himself without ever having heard it spoken, would be very pleased to have the company of a young educated Tuscan, etc. but wishing to send a load of wheat to Leghorn as soon as I arrived in Virginia, I thought it more convenient for him to await further news from me, and when that would be satisfactory to him, as I hoped, he would be able to find me on the same ship.

> As for his companion, I told him that I didn't care to meddle with him, and he understood that I had no favorable opinion of him; however Fabbroni's good heart did not allow him to accept this; eight years later he told me that he was always the victim, on other occasions and for the same reason.

The date not being known, this reference to eight years later and the occasions which are mentioned are also unclear. Mazzei wrote in another place[55b] that Fabbroni told him that he was proposed for employment in the Royal Museum «by the Ab. Fontana, great physicist, who (for jealousy) later became his fierce enemy and tried, but uselessly, to damage him in every way possible».

Our second glimpse comes from Fabbroni himself, who related[62] that he was presented to the Abate Fontana in 1769 by his teacher of botany, the Abate Giovanni Lapi, because he had found a microscopic plant called *tremella* for which everyone was looking. This account continues with the statement that Fabbroni then began to assist Fontana, on a regular basis but without stipend, after the purchase by the Grand Duke of the palazzo Torrigiani in 1771. In 1773 he began to receive ten *scudi* per month; from 1775 to 1780 he was with Fontana in France and England; in 1784 he was made subdirector of the Museum. In 1789 by being appointed Superintendent of Budget he obtained some control over finances.

Details differ in the various accounts of Fabbroni's life.[2k, 5h, 63] That in Tipaldo is one of the most hagiographic in that universally laudatory compilation. There many of the contributions of Fontana (who did not himself rate a biography), such as the development of the museum, the authorship of the 1775 *Saggio* describing it, the instruction in physics of the Royal Family, the preservation of the treasures of the museum from the predatory invaders, are taken from him and given to Fabbroni.

It appears that he was born in 1752 of a family originally of the Romagna but established in Florence (or Pistoia) in the late fourteenth century. His mother was born Werner in Heidelberg; her relative, Canon Saidingelt, directed Giovanni's early education. He then studied mathematics and natural science at the Hospital of Santa Maria Nuova and the Accademia di Belle Arti. Accompanying Fontana to Paris and London, he subsequently was given more and more responsibility in the Royal Museum, becoming its Director on the death of Fontana but only for one year, being driven out by jealousy and intrigue according to Tipaldo. In 1798 he was sent to Paris as Tuscan representative to

Fig. 15 – Portrait bust of Giovanni Fabbroni.

consider laws concerning new weights and measures; in 1800 he was Director of the Mint; in 1805 Sanitary Commissioner at Leghorn; in 1815 honorary professor in the University of Pisa; in 1816 Director of Granducal Mines; he held other important posts (Fig. 15). His publications numbered more than eighty in several fields including chemistry for which he is best known, although Partington[26] discounts them. To Cochrane[64] Fabbroni appears as «one of the brightest of the younger Florentine political economists».

His success in life was undoubtedly aided greatly by his marriage to Teresa, adopted daughter of Giuseppe Bencivenni Pelli (1729-1803), a figure of considerable interest himself.[1b] Director of the Uffizi Gallery from 1775-1799, his history of it, *Saggio Istorico della Reale Galleria di Firenze,* 1779, was only one of his scholarly productions. He adopted Teresa in 1770 on the death of her parents, when she was seven. The rhapsodical account of her life in Tipaldo[2l] tells how the Fabbroni Pelli home was a meetingplace of the «important men» of Florence, Italy, and Europe. The Grand Duke Peter Leopold gave his name to their son, born 25 September 1785. The years 1810-11 they spent together in Paris, Teresa dying shortly after their return to Florence. Giovanni remarried, died in 1822.

Fabbroni's character is indicated in part by the cordial relations that he established and maintained with numerous foreigners, shown by the Fabbroni papers at the American Philosophical Society, Philadelphia, and elsewhere. Among them are numerous letters from eminent contemporary figures, some showing that already, in Paris with Fontana, Fabbroni was preparing for his reception in England. On 12 July 1778 R. Bošković wrote to Dr. Shepperd:[74]

> Although I am not sure that this will find you in England, since you must leave on a voyage, still I hand it over to a youth who comes there; he is Sig. Fabbroni, an Italian; if he finds you it will matter much to me that he receives all possible attention. He is highly talented and has an amiable manner. I take a great interest in him and beg you to help him in every way. He travels with Sig. Ab. Fontana, with whom I have had occasion here to be highly displeased, for the past year I have had nothing to do with him, but for his companion who acts with probity I am interested in all that might be of use to him.

In England Sir Joseph Banks *(q.v.)* wrote to Fabbroni while he was there to offer introductions, and also in December 1781 he wrote to him in Florence about shipment of seeds. Other Englishmen to write to

Fabbroni in Florence, 1785-88, were J.R. Forster,[e] J.H. Magellan,[f] and John Wedgwood.[g] Perhaps the most impressive instance of correspondence was that of Richard Kirwan *(q.v.)* who wrote 28 January and 4 February 1780 (soon after Fabbroni's departure) about recent discoveries of Priestley, Higgins, Black, and Herbert of Vienna, and on 15 May 1786 about his own work. Most important was a letter of 1 October 1791 in which he wrote:

> Mr. Tenant of our Society has decomposed fixed air into oxygen and carbon by heating powdered marble with phosphorus. He obtained phosphate calx and carbon. You will agree that (in my opinion) one can no longer support phlogiston, which you prophesied would soon be universally abandoned!

Kirwan had already written to Berthollet on 26 January 1791[26h] that he was converted to Lavoisier's views, and had announced his abandonment of the phlogiston theory to Crell in that year, but his writing also to Fabbroni to this end shows his regard for him. In spite of Kirwan's detection of some degree of charlatanry in Fontana, he expressed his esteem for him in these letters.

Somewhat surprisingly, Fontana's long-time friends and collaborators, Jan Ingenhousz (27 December 1784 and 2 April 1789) and L. M-A. Caldani (22 August 1794), wrote to Fabbroni about Fontana, the former sending his respects, the latter a characteristic letter:

> It would surprise me if Director Fontana came to Abano without showing himself. All the more because either coming from Roveredo, his home land, or directly from Florence, he would necessarily have to pass through Padua if he wished to go on to Abano. When I see him he whose name and remarkable merit are well-known to me, he will be especially well cared for, such being the duty of one who professes great esteem at every meeting.

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[e] John Reinhold Forster, F.R.S., was born in West Prussia in 1729, died in Halle in 1798. He was a professor in the Warrington Academy (where Priestley taught) in 1776-77. According to Partington[26] he was aided by the Freemasons but unpopular in all quarters.

[f] J.J. Magellan (Magalhaens or Magelhaens) (1722-1790) was an Augustinian prior in Lisbon, went to England in 1764. He wrote on physics, particularly on instruments.

[g] John Wedgwood, son of Josiah, F.R.S., founder of the Etruria pottery.

[h] Giuseppe Angelo, Conte di Saluzzo (1734-1810) published numerous papers on pneumatic and other chemistry, particularly on gunpowder.

It appears that Fabbroni served as Fontana's secretary at the latter's request; also he more readily established and maintained good personal relations, as well as being esteemed as a scientist in his own right. He kept up as well a correspondence with Thomas Jefferson.[57]

Fabbroni's relations with Fontana are less readily assessed. The anonymous eulogy of 1805[65] does not mention Fabbroni, nor does that of Sarchiani[66] in 1810. Mangili[67] wrote in 1812:

> He [Fontana] was many times embittered (as he related to me when I had the chance to live with him as disciple and as his collaborator), he was embittered, I say, many times by the ingratitude of those on whom he had himself heaped benefits

and Mangili's notes[68] show the complaining and backbiting prevalent among those persons connected with the Royal Museum. Schiff[45] drew the conclusion that «Fabbroni, who had a character entirely different, was not always in agreement with Fontana and took no great part in the development of the museum». Zobi,[69] recording the testimony of Assessor Cremani, who was chosen in 1799 to purge Tuscany of «patriots and Jacobins», wrote:

> Cremàni testified to the Senate, and reported besides that the discredit of Fontana with the populace depended on the animosity of Giovanni Fabbroni and Ferdinando Giorgi, his persecutors.

Regarding the preparation of anatomical models, Fabbroni repeatedly denied Fontana his support. In a memoir[70] of 1788, written in French, probably by the French envoy to Tuscany, La Flotte, there is a summary of the methods of preparation of the wax models that is highly laudatory of Fontana. At the end is this comment in Italian, in Fabbroni's handwriting:

> Without stopping to refute many falsities stated here, I conclude that it is all aimed at securing exclusive commissions, showing on one hand how [the work] is easy, on the other how difficult.

In 1792, Fontana submitted a series of queries[70] to Bartolini, Administrator of Affairs of the Crown; he asked if, in preparing the waxes for France, he was permitted to use the plaster molds already made, to borrow waxes and utensils from the Royal Museum (giving receipts for them), to employ the artists at the Museum at the customary hours and

prices, «as was permitted him for Vienna». Fabbroni wrote a long letter[70] to Bartolini, early in 1793; the following are some excerpts:

> I have found no existing orders in these archives to prove, as has been assumed, that the Director was legitimately entitled to use the molds and the waxes of the R.M. for the Imperial Court of Vienna; much less that receipts and guarantees were required of him; no one at that time being authorized to do so, he was guided entirely by his own free will, he gave away everything he could. The permission, in the terms requested [by Fontana] is too sweeping: it is disorganized: and exposes the Museum, the country, and the Court itself, to inconvenience and annoyance. All was done but nothing was conceded – for the Vienna commission.

In a lengthy letter[71] of 22 November 1793 to the Grand Duke Ferdinand, Fabbroni blasted the competence of Mascagni, Fontana's anatomist colleague, then praised another, Tommaso Bonicoli, a dissector employed in the Royal Museum:

> I would be of the opinion then that Y.R.H. [...] should comply with the request of the Director, according to him formally that which was within his authority, that is, to seek the judgment of an expert, but enjoining him to consult in writing dissector Bonicoli, not famous but really understanding anatomy to its full extent. If he for some reason were incapable of giving a consultation [...], he might recommend applying to Dr. Alessandro Bicchierai [...] Equally suitable would be Professor Nannaioni [...] or Royal Surgeon Santini.

These consultations, if they were required, must have been galling indeed to Fontana. On 26 April 1794 Fabbroni wrote again[72] to the Grand Duke about Fontana's proposed «anatomy in wood»; excerpts from this letter follow.

> The Director was pleased to make an anatomy in wood: a new subject and of signal expense to the R.M.; he expresses his desire and immediately the Royal Treasury opens without limit.

Fabbroni then listed the many items needed: the knives, the cadavers, where these must come from, and continued:

> Certainly the Director has no less love for the royal interests than for his own glory; he never would have made such costly copies to be carved with difficulty in wood, if he could have executed them more easily in clay or wax. The project of making another half-man in wood to demonstrate the origins of the nerves must be deemed useless, these being seen, or at least they should be perfectly seen, in the preparations in wax.

What was Fontana's attitude to Fabbroni? We know that at times he complained of him. However, it appears that he continued to support and encourage him. We have nine letters[74] of Fontana to Fabbroni, four from 1781-1799, five undated. On 9 July 1781 he wrote:

> I have received your letter, and can do nothing other than praise all that which you have done to the advantage of this R.M., and all that you propose to do. You are familiar enough with its contents to know what is lacking [...] I rely entirely on your discretion [...] Act with prudence; if you are not coming with Fensi, write me. I am your most affectionate friend.

On 23 March 1793, Fontana wrote:

> I was sorry to learn of your indisposition, and hope that it is nothing of consequence. Avoid exposing yourself to the cold, and don't come to the Museum until you are completely cured.

Similar letters were written in 1798 and later. Zobi, writing[69] on the short-lived revival of the *Accademia del Cimento* in 1801 and its constitution, formulated by Fontana and emphasizing experimental science, said:

> Giovanni Fabbroni took the occasion to criticize the constitution compiled by Fontana, accusing him flatly of promoting the pure and simple reestablishment of the old Academy. While the criticism arose from jealousy, Fontana, always benevolent to Fabbroni, praised him to the Triumvirate, had him admitted [to the Academy] as ordinary member, and proposed him for the Commission of the Magona [the ironworks].

Yet Fabbroni did not shirk all obligations to the Director and his family. In acknowledgment, Teresa not knowing that her brother had died on the tenth of that month, wrote to him on 16 March 1805,[74] as follows:

> Only now have I received here in Milan the letter you honored me by writing on the 16 February; it was forwarded from Roveredo [...] I have no words to express to you my appreciation of your reporting to me the sad news of the accident to my brother Felice on the tenth of February [...] I cannot thank you enough for the kindness shown me by a man as great as you, and call myself fortunate if in addition to the kindness you have shown me [...] you would investigate the terms of his will, as well as keeping me informed of the progress of his illness.

All in all, the relationship of the two men resembles that defined for all time by Sophocles in *Oedipus King of Thebes:*[73]

Long ago
Apollo spake a doom, that I...
with mine own hand spill
my father's blood...

Pietro Ferroni. Spending his life in Florence, 1744-1825, he studied at the Nazarene College in Rome, later at Pisa, chiefly in mathematics, which he taught to the budding architects and engineers at the *Studio Fiorentino* and at the *Istituto dei Nobili* from 1770. Three years later appointments as professor at the *Studio Pisano* and as Royal Mathematician were added. Undertaking many practical assignments as well as abstract analyses, in France and Tuscany, he was superintendent of rivers, member of the commissions for new weights and measures, and for a new survey of Tuscany. He was active in the formation of the New Accademia del Cimento, and a close friend of the brothers Felice and Gregorio Fontana. His «autobiography», in the Biblioteca Moreniana, Florence, Acquisiti Diversi Filza 53, Inserto I°, gives many glimpses of the Tuscan Court.

Michele Girardi (1731-1797), was educated at Brescia and at Padua where he was the pupil of Dal Covolo and of Morgagni, becoming assistant to the latter. In 1770 he was called to the chair of anatomy at Parma. He published often and was a member of many academies; his early death was much lamented by the people of Parma.

His first memoir was inscribed to Fontana; it was on the anatomy of the testicle, in contradiction of John Hunter but supported by Wrisberg. Girardi prepared a discourse on the «intercostal nerve» which was to have been delivered at the beginning of a course of study which he never gave. It expounded at length the work of Fontana on the subject and intended to present pertinent applications to the theory and practice of medicine.

According to Quérard[22b] it was badly in error; a corrected edition was published by Desgenettes in Paris in 1792.[2f]

Francesco Griselini. Two activities of Griselini, born in 1717, are most pertinent for us. Peter Leopold had encouraged new ventures in agriculture; one of these was the culture of kohlrabi *(cavolo-rapa)* from which Griselini extracted an edible oil. The memoir he published was dedicated to Peter Leopold. The *Accademia dei Georgofili* having recommended that he be given an academic honor, that and a gold medal

were awarded to him by Peter Leopold, the letter of presentation being by Felice Fontana:[23]

> Most Illustrious Sir and Worthy Master:
>
> His Royal Highness the Grand Duke of Tuscany, my Sovereign, who recognizes true scholars and rewards their virtues, renders justice also to you, who hold a notable place among them. He has consigned to me to send to you as a sign of his approbation of the book on the kohlrabi which you have dedicated to him, a gold medal which you will receive from Mr. Christopher Weber to whom it will be sent this evening by Mr. Cosimo Siries, director of the «Pietre Dure» Workshop of the Royal Gallery in Florence. I rejoice with you in seeing you singled out with such a generous act of a Philosopher-Sovereign, and assure you of the great pleasure I feel.
>
> With the greatest esteem I am
>
> Your devoted servant
>
> 2 October 1772 Felice Fontana

The Pope also gave Griselini a gold medal for this work.

For some time Fontana had known Griselini, who founded in 1764 the *Giornale d'Italia.* It printed, on interesting subjects of the time in natural history and agriculture, original articles, reviews, and letters, including many of his own works on forms of marine life. In 1766 Fontana wrote[24] to Griselini that he was sending a «little book» of his on microscopic observations [of the blood] as a mark of respect. He mentioned also some works on microscopic animals and the venom of the viper. If any of these «little things» were worthy of Griselini's fine journal, Fontana would be so pleased to «see them sketched by your masterly pen». A review of that «little book» did appear in 1766, also a lengthy one in 1768 of the work on the rust of grain, apparently sent in by Matani, professor of medicine at Pisa, and one by Matani himself on Fontana's work on fungi. In 1769 a long review of Fontana's first book on the venom of the viper and his note on some microscopic animals appeared. In 1779 there was a review of the book on nitrous and deflogisticated airs, in 1782 a reprinting of the article on inflammable air.

Griselini died in 1783.[3b,25]

Albrecht von Haller. No more than a nod in his direction can be given here. Living from 1708 to 1777, he was the master physiologist of his time. A most encyclopedic author and experimenter, leading in Bern his life of varied activity as public health authority, nearly «Lord High Everything Else», he still carried on perhaps the most gigantic correspondence in the history of science. His *De partibus corporis humani sensilibus*

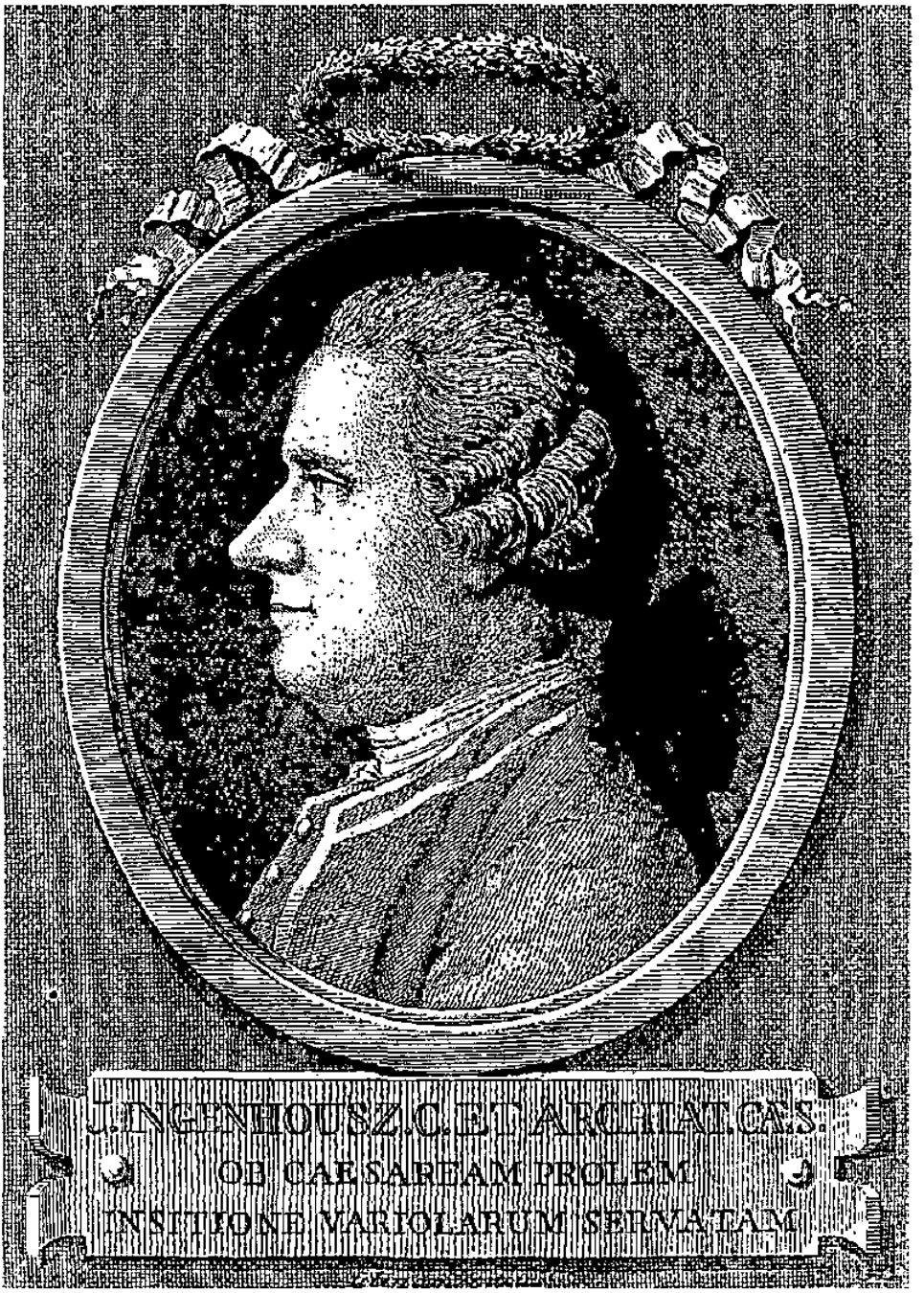

Fig. 16 – Jan Ingenhousz. Line engraving by D. Cuneo, Rome, 1769.

et irritabilibus, Gôttingen 1753, and *Mémoire sur les parties sensibles et irritables du corps animal,* Lausanne, 1756-60, contain his, and some of his supporters' works on the subject of «irritability».

Haller died according to his lights, shortly after the Emperor Joseph II had visited him on his way home to Vienna after his trip to Paris. With his finger on his pulse, he remarked to the friend at the bedside, «The artery no longer beats».

Haller was most kind to Fontana, sending him his publications,[13] and dedicating to him a volume of what might be called the second edition of his Physiology.[28] Haller had been curious as to Fontana's title of *Abate.* Caldani answered[13c] that Felice was not a religious, not a priest, but wore the habit as was commonly done in Italy; in fact Caldani's father wore his habit at his own wedding.

When Fontana left Florence to start for Paris, he wrote to Caldani[15d] that he was taking one or two pieces of anatomical models for Haller, at Peter Leopold's order. Although he did pass through Geneva to meet Bonnet, he did not go to Bern to see Haller, as the latter wrote sadly to Caldani.[13d] Fontana had dedicated his Animal Physics to Haller, with permission, and sent him a copy, but Haller was uncertain of its value; at least he wrote to Caldani[13e] that he wished to hear his opinion of it. Eventually he criticised parts of it in his work mentioned above.[28] Fontana probably did not see the criticism until he returned to Florence in 1780, by which time Haller was dead, but he replied to it with some acerbity in his anonymous *Lettera Apologetica.*

Jan Ingenhousz (1730-1799). Born in Breda, the second son of a leather merchant, he became M.D. at Louvain in 1753, then studied at Leyden, anatomy with Albinus and physics with Musschenbroek. Home again, he drew the interest of a visitor, Sir John Pringle, Royal Physician and President of the Royal Society, who persuaded him to come to Britain; in 1764 he went to Edinburgh and London where he became acquainted with the Hunters, the elder Monro, Priestley, and Franklin. During the controversy over inoculation against smallpox, Ingenhousz and one associate inoculated more than 700 persons, acquiring fully the skill. In 1768 George III sent him to Vienna where, after preliminary trial on 200 children, he inoculated members of the royal family. Maria Theresa made him court physician with a life-long annual income of 5,000 *gulden* (Fig. 16). He then went in 1769 to Florence and successfully inoculated Peter Leopold, at which time he met Fontana through the kindness of Laura Bassi.[30] Ingenhousz spent the years 1771-1789 between Paris,

Vienna and London, collaborating with Fontana on the study of various problems.

He is best known for his work on photosynthesis which was motivated by Priestley's experiments, but the discovery belongs to him, Priestley and Senebier having only vague ideas about it before Ingenhousz.

The genial nature of this man comes through the print even now. A prominent figure in London scientific circles (F.R.S. 1769), he was always willing to show his experiments to his friends, was especially considerate to young people, and was noted for his simple and kindly disposition. In his publications he honorably gave full credit to Fontana for his contributions. His manner was not to everyone's taste. When he was introducing a foreign scientist at Sir Joseph Banks's, Henry Cavendish found him so pompous that he bolted from the room. More comment came from Pietro Verri who wrote[9a] to his brother Alessandro in 1768:

> At Vienna the English Dr. Inghenhousz has come to inoculate; already he has done it sixty times, four under the eyes of the sovereigns [...] The English inoculate in the arm, not in the hand. They have profited by a thousand little *scudi*.

Pietro later wrote:[9b] «I have met Ingenhousz; to me he seems extremely cold; *la petite célébrité d'inoculateur* says Gatti, and he is right». Again Verri:[9c] «That inoculator Ingenhousz wished to see me, but I excused myself; I saw him by chance and found him strangely distant». Maria Theresa was disappointed in him for not becoming a courtier; she wrote to Joseph II 14 May 1772 that he had changed, «very mannered after his travels».[4f, 5e, 26e, 31]

Marsilio Landriani. The year of his birth is variously given as 1746 and 1751; the year of his death as not later than 1816. The little known of the life of Landriani is enough to envision him as a typical figure of the Establishment, as indeed he became in 1794 at Vienna, where he remained for the rest of his life. Desirous of the acquaintance of the right people, anxious to behave correctly, by amiability he made a modest ability go a long way. Born of a noble family, he began to teach physics in the *Ginnasio di Brera* in Milan in 1776, and was charged by the government with making extended trips abroad (1782-88) to observe foreign industries, on which he reported at length.[33] In 1790 as adviser to the government he established the Veterinary School of Milan. He was

made a member of the Treasury Office. Going to Vienna he was sent on a diplomatic mission to Dresden lasting until 1794.

He began his scientific career as collaborator with Pietro Moscati[i] in studying pneumatic chemistry which he wrote about as «having become the occupation of all the physicists of Europe». When he visited Paris, Lavoisier designed and performed an experiment «to convince Cav. Landriani».[38] On learning[39] from Joseph Priestley in 1779 of Fontana's discovery of the absorptive power of charcoal for gases, he went to Turin, where with Morozzo[j] he repeated the experiments. His publications included: *opuscoli fisici-chimici,* 1781; *Déscription d'une machine propre à élever l'eau* etc., 1782; *Dell'utilità dei conduttori elettrici,* 1784. Attracting greatest interest, and translated into German, was his book, *Ricerche fisiche intorno alla salubrità dell'aria,* Milan, Marelli; 1775. Dedicated to Firmian, it quotes Franklin, Hales, Priestley, and Fontana, the last three on the behavior of *aria nitrosa* (nitric oxide). After describing his apparatus for carrying out the reaction with nitric oxide to measure the salubrity of the air, he wrote:

> Barely having finished my researches, there came to me kindly from the celebrated physicist of H.R.H. the Grand Duke of Tuscany his new book, in which he described his machines. I read it avidly and realized that he had preceded me, but I wished to make clear my originality. The celebrated physicist with unusual kindness sent me some clarifications which he intends to publish as the second part of his work on *aria nitrosa,* also two letters appreciating my communication to him and assuring me that no-one could take away from me the glory of my original invention; he will be the first to render me justice, not claiming any priority.

In the introduction to the book, he had written:

> Then many friends to whom I had shown it [the instrument], particularly Prof. Moscati, advised me to make it known publicly. However, news having arrived

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i Pietro Moscati (1719-1824). While still very young he was named professor. In 1772 he qualified as surgeon at the *Ospedale Maggiore* there, became its director in 1785. Later he was removed on charges of maladministration, which he showed to be unjustified. When Napoleon entered Milan, the position was restored to Moscati, who had demonstrated democratic ideas. In 1797 he was made a member of the executive council of the Cisalpine Republic, dismissed the next year he returned to the hospital.

j Carlo Lodovico Morozzo (1744-1804). A general in the army of Piedmont, he published on pneumatic chemistry, animal colors, and the absorption of gases by charcoal. He was one of the first in Italy to accept the new chemistry.

from Florence that the renowned Abb. Fontana had devised and constructed seven of them, as he had written to Moscati, I temporized, dismayed by the fertility of his inventions, until they appeared, and I pondered more and more over this matter. As he readily makes discoveries in this new science, so I still could do so, as I soon told Moscati, who was pleased to take part by sending drawings and descriptions of my instrument to the aforementioned Sig. Fontana: and he strongly encouraged me to continue with it.

There is no doubt that Landriani and Fontana independently pursued this subject. Landriani's choice of a name for the instrument, eudiometer, gained more acceptance, even with Fontana, than did Fontana's name of evaerometer, which he continued to use for some time but eventually abandoned. Examples of both instruments were in Priestley's laboratory in Birmingham.[37]

Antagonism between the two «pneumatic chemists» remained; the choice by the Verri brothers of whom to support rested on Fontana. Pietro wrote[9d] on 23 August 1775:

Don Marsilio Landriani, son of the famous Don Giuseppe, his admirer, has published a dissertation on his instrument for measuring the salubrity of the air. We are not yet agreed on defining the most healthy [air] as that which contains more or less fixed air [carbon dioxide] – and they already devise the dimensions of salubrity! I fear that the century of Montesquieu, Helvetius, D'Alembert, etc. has passed; now trifles appear with great acclaim.

Alessandro answered[9e] 30 August 1775:

I have a sort of aversion for various novelties in physics, among them these for fixed air; thus I would relish very little the dissertation of Don Marsilio. Davy[k] died in a simple experiment with fixed air; after that it doesn't take much to convert me. Besides, much of what I read on this subject seems charlatanry.

Note that Alessandro was ready to condemn Landriani's book before he had seen it. Both brothers seemed to be fixed on associating Landriani with fixed air for no good reason. Alessandro was also sceptical of Benjamin Franklin's claim that oil would quiet troubled waters. He added later[9f] on 16 March 1776 that he had now read Landriani's book although he found it hard to finish. He made many strong criticisms, calling it speculative and lacking in experimental support, drawing too

[k] Unidentified. Humphry Davy died in 1829.

many conclusions from a few facts. He found it charlatanry for Landriani to write that he had fully resolved one of the most important matters of the chemistry of the air, then to say that Priestley had already done so. Pietro replied[9g] on 23 March 1776:

> The abate Fontana showed me little esteem for our Landriani, as indicated by his firm belief that the experiments claimed by him had never been done.

He returned to the same subject[9h] on 10 July 1776:

> Don Marsilio Landriani has been made Reader in experimental physics with three thousand lire as salary and a separate grant for a trip in Tuscany. You see how well fixed air pays [...] This Landriani for me is a youth of greatly limited ability but with chemical and literary quackery to the point of being ridiculous. He speaks of himself as a genius of discovery and invention, but give him a little push and he goes crazy. I know that the abate Fontana emphatically believes that the eudiometer Landriani described was not made, and that the experiments on the heat of different rays separated by a prism were not only false as to conclusions and contrary to the facts shown, but were not done, as being impossible with the method Landriani said he used.

As there is a strong doubt that all of the instruments described by Fontana in 1775[34] were ever actually built, it seems likely here that he was attributing his own sins to Landriani. We have no information on the experiments with light, about which Pietro wrote again[9i] on 30 September 1778, that Fontana, on his way to France, had passed that way and had demonstrated to Verri the points that he had made.

On 21 September 1776 Alessandro had written:[9j]

> The big wind just blew into town.[1] Don Marsilio Landriani is here. Attentions from an author one does not esteem are received in desperation [...] He might be praised, but then one is rather entangled and sweats blood.

Pietro's final shot at Landriani (and Moscati) was written[9k] on 26 February 1780:

> Then fixed air [...] last summer our two physicistchemists, Moscati and Landriani, were at the baths. It came into their heads to collect some flatus which rose through the water in a fine great bubble. They analyzed it; they found that it

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[1] Literally: unexpectedly the fixed air has arrived in Rome.

was inflammable; they cried: Discovery! Discovery! Everyone knows that such an air comes from places where there is fermentation. It was thought to make an economical lamp with such a substance. Why not think of lighting a city at night!

Landriani re-entered Fontana's life briefly in 1785-86 in connection with the Giorgi controversy. We know of this through Landriani's letters to Francesco Bartolozzi, printed in a pamphlet in support of Fontana.[35] On 22 June he wrote that they were well aware in Milan of the recent experiments of Lavoisier on the decomposition of water; that they [he, Moscati, and Volta] had tried to repeat them without success; that he had sent the «ridiculous» report of Giorgi and Cioni, with the addition of his own observations, to Paris. On 18 March, 15 April, and 18 May 1786, he wrote to the same purpose to Bartolozzi and also to Cioni, adding that he had corresponded with Giorgi, that he had never told Giorgi that he was of his opinion, but had urged him to change his apparatus and make more experiments, after which Giorgi wrote him no more. In one of these letters he wrote that he was sure that Fontana was right in what he had written to Ingenhousz. Landriani ended by writing, 24 May 1786, in a letter apparently to the Court[41] concerning the case, Fontana *vs.* Giorgi, that he had been surprised to see his letter to Bartolozzi appear in print; that his silence in this matter should have made it understood that he was far from entering into this dispute which had always seemed ridiculous and scandalous to him; that he felt Bartolozzi's publication to be an abuse of confidence and that he had asked him not to write anymore to him about it.

All that was apparently enough to prove to Fontana that he was not being supported by Landriani; he wrote to Senebier[36] on 11 July 1786: «I send you also a reply to a letter of Landriani, my old enemy, which portrays well his character».[5,40]

Giuseppe Mangili. The only person formally to be identified as Fontana's pupil, Mangili was born in 1767 «where there are mills on the brook Sonna», that is, near Bergamo. He became a priest but used the title of *Abate*. In 1786 he was professor in the *Liceo Latina* in the public schools of Bergamo but renounced that employment in 1790 to go to Pavia, where he studied under Spallanzani, Volta, Scarpa, Brusati (physicist), and Mascheroni (poet). Several stays in Florence were made for study with Fontana, apparently in 1791, 1792, and 1793. Fontana's testimonial for him, dated Florence 2 July 1794, reads:

Abate Mangili of Bergamo, a person of spotless probity and varied talents, was pleased to spend more than a year in my house in Florence; for all this time we have worked together in the most useful sciences, but principally in anatomy, where his able assistance has been of great help to me. Thus I must designate him as one of the great, rendering him justice not from simple friendship but really as the truth, as with pleasure I declare here in writing, signed by me

Felice Fontana

In March 1799 Mangili was appointed professor of natural history at Pavia, succeeding Spallanzani. He was made Rector of the University in 1801, and was politically active. He died 13 March 1829.

Mangili's papers, including correspondence, lecture notes, and much biographical material, are in the Biblioteca Civica, Bergamo. A compulsive note-taker, he made long lists of the names of those he met on his frequent travels and kept detailed records of expenditures, his own and those of others with whom he traveled, for instance to Naples in 1791 with the «distinguished professors» Fontana, Baldinotti, and Mascheroni. His papers contain several long analyses of Fontana's «venom of the viper», on which subject he worked himself, publishing in 1809. In papers distinguished by little information in many many words, he reported that large doses of venom by mouth were innocuous to birds and that ammonia and cherrylaurel water had antidotal value.

Mangili's «autobiographical» notes (filza 79.R.6) are of value especially for presenting a picture of Fontana's active social life not given elsewhere. He wrote how together they strolled in the Boboli garden, visited cafés in which they took chocolate and met many ladies, saw an exhibit of paintings, went hunting for pheasant, and were received by the Royal Family (of the Grand Duke Ferdinand) several times. The notes for the years after 1792 consist exclusively of official university and governmental documents. The letters on Fontana to Mangili of 1804 are discussed on p. 74.

The «official» eulogy of Fontana by Mangili was delivered in Pavia 11 November 1812, i.e. seven years after Fontana's death. It was declared to be a result of the «courteous admission of myself to his most intimate friendship; his broad teaching gave me the stimulus to forge my career in the study of nature». Printed (Stamperia Reale, Milan) in 1813, it has served as a major source of information for subsequent biographers.

Paolo Mascagni. Born in Pomarance near Volterra in 1755, he studied anatomy so well that he succeeded his teacher Tabarrani as professor at

PAOLO MASCAGNI

Fig. 17 – Paolo Mascagni.

Siena in 1787. On the advent of the French in Tuscany and the departure of the Grand Duke Ferdinand III, Mascagni became a member of the local government. In January of 1799, the Aretine rebels of the Viva Maria entered Siena, dragged Mascagni from his home into the Piazza del Campo, where he was saved from the scaffold by his workmen. Under the charge of irreligion and jacobinism he was kept in prison until June 1800, after the French victory at Marengo; when the University of Pisa, which had been closed by Ferdinand in 1799, reopened, Mascagni was made Reader in anatomy and continued preparing his works (Fig. 17). During the kingdom of Etruria suspicion of jacobins was revived but through the efforts of Mons. Angelo Fabroni, Rector of the University of Pisa, Mascagni was kept there and made professor of anatomy in 1801, with the added obligation to teach as Director of Anatomy at the hospital of S. Maria Nuova in Florence. His most famous works were: *Anatomy for the use of sculptors and painters,* Florence 1816, *Lymphatic vessels of the human body,* 1787. His *Universal Anatomy,* for which he had had prepared illustrations, appeared only after his death in 1815. It was entrusted to his prosector, Francesco Antonmarchi, Napoleons's physician at St. Helena, who issued in 1819. Mascagni's family, disgusted with Antonmarchi's methods, issued the work again in 1823-1832; some of the plates were later plagiarized in work purporting to be Antonmarchi's.

Mascagni also verified the presence of boric acid discovered in lagoons between Siena and Volterra. His procedure for recovering it was published in 1779; the production was highly profitable to the State treasury.

Mascagni's contribution to the preparation of the anatomical models in wax was no doubt considerable, but of uncertain quality. Documents[18] relate that Fontana sent Giovanni Fabbroni to look over the models that had been prepared for Vienna. He found numerous anatomical errors, but while he was there Mascagni came in and flew into a rage at such interference. He was restrained by some employees, who later gave Fabbroni their attestations to the circumstances. Dissector Tommaso Bonicoli was asked to evaluate the accuracy of the waxes; he gave an opinion that they could be corrected. Fabbroni subsequently wrote[70] through channels that Mascagni may have known much about the lymphatic system, but little about the rest of anatomy.

When Mascagni took charge of the subject at S. Maria Nuova he took Bonicoli with him but the latter, who had been ill for some time, committed suicide in 1802. Mascagni then proposed Filippo Uccelli, pupil of Bonicoli and dissector at Pisa, to succeed him. Uccelli, having

been arrested in 1799 as a francophile, was objected to by the Bourbon government, but Mascagni insisted and prevailed.

He apparently was a faithful friend to Fontana; he borrowed money from him, and was present at his death-bed.[2c, 5b, 19b]

Antonio Scarpa (1752-1832). Scarpa was born at Motta del Friuli, in the north of Italy. At Padua he was torn between the study of mathematics or medicine and surgery. Electing the latter, he was well regarded by Morgagni and assisted the professor of obstetrics Calza in preparing anatomical models. Invited to join the faculty at Modena, he later afterward traveled in France and England. In Paris he became acquainted with Vic d'Azyr, the anatomist, and Wenzel Senior, expert operator for cataract, about whom he wrote:[42a] «Mr. Wenzel, the celebrated oculist, did me the honor of advising me by note every time he operated».

In London in 1781 he met the Hunters, Cruickshank, and Pott. In 1782 he was in Montpellier. Back in Modena, he was offered through Brambilla, whom he had met in Paris in 1781, the chair of anatomy at Pavia, which he accepted. During his first year he traveled with Volta to Vienna, was made professor also of surgery. Napoleon made him his surgical consultant. In 1804, age began to tell on him; he died at 80, having become a great miser in his last years.

He published twelve works in anatomy, especially of the nervous system, and six in surgery. He was deeply involved in the movement against Spallanzani which left him bitter against his colleague in Pavia, Gregorio Fontana. However, in 1786 he wrote[42b] glowingly to Gregorio about his visit to Florence:

> 16 September 1786... Together with Canon Volta, I must send you my thanks for the many and repeated kindnesses that your brother was so good as to grant us during our sojourn here. We have spent the greater part of the time with him, now admiring the vast extent of his knowledge, now examining closely the Cabinet of Physics, which is surely one of the most interesting Collections, or better said, is unique in Europe. I cannot express my amazement on seeing the rooms of anatomy. The beauty of the work, united with scrupulous accuracy and precision, the really professional arrangement of the pieces, the ease of understanding them by anyone, makes them such a rare and useful whole as to surpass by far the imagination of an artist. Would you believe it? There I found my latest work on the olfactory mechanism so well done in wax that it seemed I had under my eyes the same part of the cadaver from which I drew my figures. I confess to you that I never would have believed that a mass of the finest nerves could be executed in wax with such precision. Your brother makes no piece of wax without the cadaver

present, nor does he trust the illustrations of men however great; that makes this Collection precious and renders the greatest honor to him who has directed it. I also had the pleasure of comparing with the microscopy the beautiful representations of the structure of nerves; this alone would have made my trip worthwhile. The Canon occupied himself the whole week with examining the part of the Cabinet which deals with Natural History, and he is not yet satiated with looking. The kindness of your brother has been so great that, beyond suffering patiently our presence for whole mornings, he has favored us with outings in his carriage, and making us acquainted with some lovely and cultured ladies. Before knowing him personally I esteemed him as a man of letters, now I have proof that he is most amiable in his ways.

Scarpa, wishing to have examples of the anatomical waxes for Pavia, sent Fontana in a letter of 2 August 1792[42b] a request from the Treasury in Milan for a statement of costs so that payment might be ordered. Fontana had submitted to the Grand Ducal Court in Florence in December 1791[43] a memoir on the preparation of anatomical waxes for Pavia, in which he emphasized the importance of Scarpa's coming to Florence in order to select the particular models which he wished to have duplicated, or to state what new ones he desired to have.

In letters, one of 1792[42b] and three of 1795,[42c,d,e] Scarpa wrote to Fontana much about the latter's views on the intercostal (sympathetic) nerves, and the cranial and sacral nerves, with which he agreed; on the seminal vesicles; on the innervation of muscle; on the retina; on animals once believed to be without a nervous system; on the phenomena of animal electricity as being propounded by Galvani and Volta. We do not have Fontana's side of this correspondence but Scarpa's letters make it clear that Felice was actively investigating the problems mentioned during those years and that his mind was still a lively one.

We have two letters from Fontana to Scarpa of 1804 and 1805, on the movements of the iris and of the ears, on the production of heat by plants, and their colors and sensitivity, which are matters partly old and partly new, partly ingenious and partly – eccentric. These were his last publications.[28]

Giovanni Antonio Scopoli. Born at Cavalese in the Val di Fiemme near Trent in 1723, Scopoli studied medicine and allied sciences at Innsbruck, was laureate in 1743. He continued his studies in Trent and Graz, then went to Vienna where he took the degree in medicine again. Van Swieten made him chief physician at a «squalid village» in Carniola, later professor of mineralogy at Schemnitz. During these periods, 1753-69, he

Fig. 18 – G.A. Slop.

published works on the nervous system, on botany, fossils, and crystallography; he translated Macquer's chemical dictionary. In 1776 he took the chair of botany and chemistry at Pavia, where he directed a chemical laboratory and the botanical garden. His habit of working hard drew considerable enmity. Giuseppe Frank, professor of medicine at Pavia, wrote of him[8] that as a scientist he emulated Linnaeus, so he naturally gave Spallanzani a good looking-over when he came to Pavia and did not hide his feelings. Scopoli took part, along with Gregorio Fontana and Scarpa, in the accusation against Spallanzani of removing items from the museum at Pavia, which led to a break between the two. Frank continued:

> the enemies of Scopoli had a country physician send him a vial of alcohol containing what was apparently a worm, said to have been expelled after an attack of colic, according to the label. Scopoli examined it scrupulously and, finding that it did not correspond to any known intestinal worm, described it as a new species with the name of *Physis intestinalis*. He had it drawn and engraved, dedicating the figure to Sir Joseph Banks *[q.v.]*. Then Scopoli's enemies published that the so-called worm was none other than the esophagus of a chick, well prepared. Scopoli frankly confessed his error, but remained deeply affected by the nasty trick, in spite of the kindness of the German naturalists who wrote excusing him, citing various examples of great men who had made similar mistakes. However, much gossip about it, from the same source as the «worm» had come, and domestic difficulties, ruined Scopoli's health. He was suddenly stricken blind in one eye [...] as a precursor to the apoplexy which carried him off a few months later (1788).

Capparoni[8a] attributes this trick to Spallanzani himself, who had a good collection of intestinal worms, saying he showed here, rather than Christian charity, the belief in «an eye for an eye – a tooth for a tooth». Tipaldo[2c] lists among Spallanzani's works, *Lettere al Signore Scopoli* (anonymous), Zoopoli (Pavia) 1788, which recite the story of the fraudulent worm.[2d]

Giuseppe Antonio Slop (1740-1808). Born at Cadine in the mountains near Trent, he was not inclined to the priesthood which was his father's wish, but went, after the school at Trent, to Pisa where he was laureate in law in 1762. Not cut out for that, he devoted himself to the study of mathematics and astronomy. Back in Pisa as assistant to Tommaso Perelli in 1765, he became professor extraordinary of astronomy in 1775 and succeeded Perelli in mathematics in 1780 (Fig. 18). His publications were many: six volumes of *Observations* over a thirty-year period, treatises on comets, on the planets Uranus and Juno. A

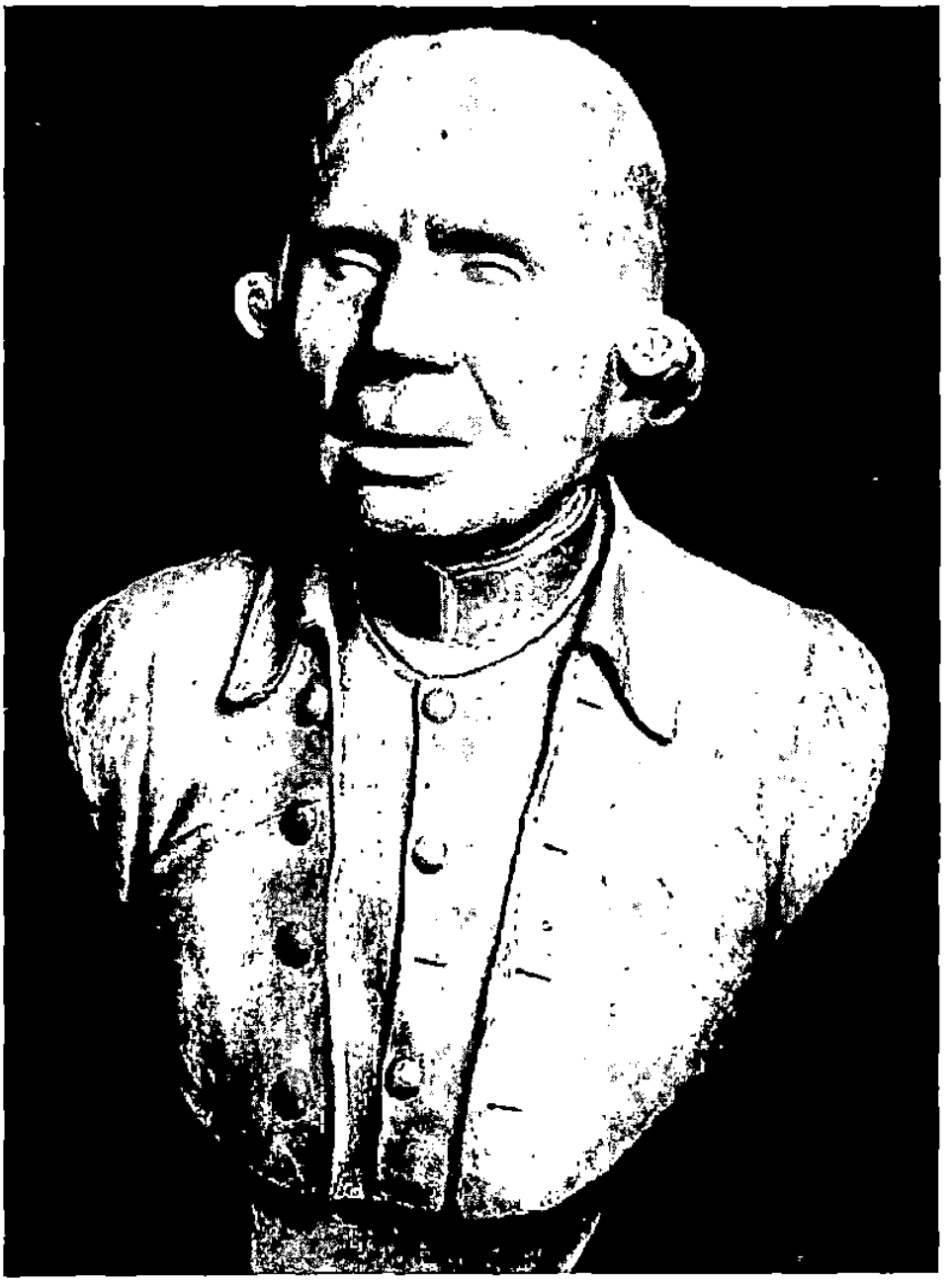

Fig. 19 – Lazzaro Spallanzani. Portrait bust in the Ospedale Psichiatrico di Reggio Emilia.

member of numerous academies, he was made Baron of Agnano by Peter Leopold in 1786.

His copious correspondence (at the University Library, Pisa) contains in Ms. 166, Vol. II, Inserts 7 and 8, 181 letters from Felice Fontana, 92 undated, 89 from 20 January 1764 to 12 January 1804. These letters are from a few words to several pages in length, on pieces of paper of all shapes and sizes, many being used for making notes and calculations apparently by Slop. Among the many names mentioned are those of Brambilla, Boščović, Cotugno, Monsignor Angelo Fabroni (for whom copies of Felice's publications were sent), Ferroni, Lagusius (many times), Maskelyne (English astronomer), Perelli, Pignotti, and Volta. Bernoulli is spoken of as «a true friend». Ferber, «a crazy writer», «my friend», is mentioned. «My friend» Murray, professor of anatomy at Uppsala, is «a great physician, great anatomist, great naturalist, and also a chemist. In Pisa he should see the city and meet some of the university professors».

The tone of these letters is always intimate; he wrote about his felt boots, also that «you will do me a real favor if you send my five shirts as I need them very much». Fontana's relations with the family Slop are presented at greater length in Chapter 11.

Lazzaro Spallanzani (1729-1799). Born in Scandiano, near Modena, he first studied law, then natural history. He took the chair of logic at Reggio Emilia in 1754, moved in 1760 to Modena, in 1769 to Pavia where he spent the rest of his life (Fig. 19). He traveled much in Italy, and to Constantinople in 1785 whence he returned twenty-one months later. His contributions to science, mostly to aspects of animal physiology, are numerous, important, and wellknown.

His relations with the Fontana brothers were thorny. They began by an exchange of friendly letters with Felice in May and July of 1766, but subsequently Spallanzani became annoyed[47] with Felice's habit of promising but not delivering works on subjects in which Spallanzani, or his associates such as Corti, was interested. He wrote to Bonnet 6 June 1767:[14c]

> The microscopic animalcules of M. Fontana have not yet been printed. Who knows when they will be? He is a philosopher who is involved in too many things. Beyond the chief occupation of teaching experimental physics to the Grand Duke of Tuscany, he takes on too many projects at the same time.

His regard for Fontana's ability continued to find expression (to Bonnet, 11 October 1768[14d]): «In all Italy, outside of the Abbé Fontana I know of no-one who has capability in these studies». As did his exasperation (to Bonnet, 20 August 1769[14e]): «Fontana is so occupied with the Cabinet of Machines for the Grand Duke of Tuscany that he has no leisure to follow his own observations».

After Spallanzani went to Pavia, where Gregorio Fontana was also professor, the antipathy grew. A letter to Caldani of 23 June 1775 reads:[14f]

> Abate Felice, Gregorio [Fontana] says, is the only one in Europe, let alone Italy, who is really supreme in physics, natural history, and physiology. Padre Gregorio is the top mathematician of our century – so he thinks. Confidentially, he is a bigwig who makes me itch to sharpen my pen against Felice, unmasking the humbuggery. But I don't want to be the attacker, but rather the attacked, and my work which will be printed here soon will surely move that solipsist to attack me, if not otherwise, surreptitiously in some journal.

Spallanzani now managed to add to his laments charges of plagiarism while disavowing (at least to Bonnet) that he was doing so. A letter to Caldani of 29 July 1775 reads:[14b]

> In the matter of Natural History I keep a record, not so short, of plagiarisms, spread as discoveries by that dear rogue, discoveries as usual *entirely original and unique*. You could in any case (and I flatter myself that you would have no difficulty) accumulate a list of plagiarisms committed in Physiology and Anatomy, since also here I would believe him to be consistent. In any case Doctor Scarpa assured me the other day in Modena that his work on the epididymis (which I have not seen) was lifted entirely from one of Haller. Was he truthful? I don't know if you have seen the small book on Animal Physics recently come out. You will find a *Notice from the Editor* the language of which is perfectly recognizable, that might be said to be written indecently, so great is the excess of the Author in praising himself. I assure you that I know no-one like him.

The charge of plagiarism over the work on the testicle is readily refuted. In Fontana's publication[48] the work of Haller is fully cited as his own original added studies are presented. Haller reviewed this publication[49] without making any suggestion that his own work had been «lifted».

To Bonnet, Spallanzani wrote 29 July 1775:[14g]

> One of the fine discoveries of the Florentine naturalist *sur les vipéres,* is that the poison of these small serpents is innocuous for the vipers themselves; this discovery had been made by M. Vallisnieri; he speaks of it at length in his Works

in folio [...] You know that my book entitled: *Saggio di osservazioni microscopiche* appeared in 1765. M. Abbé Fontana, to whom I gave this book, published a year later a memoir *sur les globules rouges du sang* in which there was a long note (pp. 14, 15) where one saw as a footnote the statements of most of my chapters. In this note he promised someday a work on the Animalcules of Infusions. He made no mention of my dissertation [...] I would not dare however with all that to accuse him of plagiarism. I know all his merit in pursuing Physics, Natural History, and besides, experimental discoveries are not rare among different observers, without their knowing the works of others. What seems a little likely here is that the writers whose works have contained the discoveries of the Abbé Fontana are too well-known, and the naturalist may even cite them in his works, when he finds it appropriate to refute them.

As for the charge concerning Vallisnieri, Benedicenti[30] relates that Vallisnieri's «nymph Egeria» was the most cultivated woman of the age, speaking English, French, German, Spanish, and Latin, reading Greek, and Arabic; Hebrew, and Chinese were not unknown to her. In the home of this learned lady, he made many experiments on vipers. One day, finding some of them dead in a chest because they had been bitten by the others, he undertook some researches to see if the venom of the viper would be poisonous for vipers themselves. The results were inconclusive. For further discussion of this point see p. 300.

In 1776, Spallanzani himself was guilty of a form of plagiarism, in his book[51b] on animal and vegetable physics. Here he discussed spermatozoa, the molds which might belong to the vegetable kingdom or might approach the mineral, or might be the link between the vegetable and animal kingdoms. He mentions the rotifers, the tremella, the eels of rachitic wheat and of vinegar, their revivification and their means of reproduction. He cites Leeuwenhoek, Buffon, Bonnet, Needham, Baker, and Roffredi, but Fontana is not mentioned. It is true that Fontana's articles on the eels of false ergot and the tremella which appeared in 1771 were not full-scale publications, such a paper not appearing until 1775, but Spallanzani could hardly have been unaware of his work.

As to the footnote on microscopic animals of infusions, Spallanzani's dissertation of 1765[51a] does deal with somewhat the same material as was mentioned in the footnote, but it is certainly not set forth in the same way in the two places. Furthermore Spallanzani wrote[14h] to Fontana on 25 May 1766 as follows:

The reading of the most sagacious work of *V.S.Ill.ma* on the red globules of the blood has given me the same pleasure as did that on the movements of the iris. With one as with the other I have greatly admired the solidity of the arguments,

the fine judgment, the nobility of invention in the experiments and their felicitous execution. But in addition this last one has given me the courage to send you these pages of mine, something I would not have dared to do before. The reason for it was your very erudite note in which you allude to the plan and development of the book you are about to bring out on the microscopic animalcules. I feel an undescribable pleasure in seeing that we two, without one knowing about the other, find ourselves in agreement on the greater part of the problem.

Is that how one writes to another whom he intends to charge with plagiarism? There is a certain amount of truth in Spallanzani's charge that Fontana was prone not to cite another unless he proceded to refute him. As to Spallanzani's statement to Bonnet, 29 March 1776:[14i]

I am pleased that you should have sampled the conversation of the learned Abbé Fontana. Naturally the inspection of the famous *eels* would have been to your taste. It would have been greatly to be desired that you had been equally satisfied with his metaphysics; but that was very difficult on account of his way of thinking. He is a materialist by profession, and that has contributed not a little to the kind of execration that he draws from nearly all of Italy. At Geneva the learned Abbé was happier than at Milan or Turin, where he is thought to be a charlatan.

It is not clear what significance Spallanzani placed on the word «materialist». If he meant that Fontana dispensed with metaphysics, he was correct.

The epithet charlatan was freely applied in the eighteenth century, when disagreement was likely in the new sciences. At times it also failed to be applied when it might justly have been. Giuseppe Frank, Spallanzani's colleague at Pavia, related[32] the occasion of the visit in 1791 of the physicist Thouvenel and his servant Pennet to Pavia, where they gave a demonstration of waterdivining by which the credulous Spallanzani was taken in. After he wrote about it glowingly to others, he learned that in Florence the trick had been exposed by Fontana who caught Pennet the night before planting pieces of metal which would cause the physical phenomenon that Thouvenet would claim to indicate the divination. Frank learned of the Florentine experience through a letter from his friend William Thomson, an English physician, who wrote that he had always regarded Spallanzani as a «shameless impostor». Fontana wrote some of this story to his sister Teresa on 4 June 1794.[33]

Relations between Spallanzani and the Fontana family had degenerated badly after the affair of the natural history museum in Spallanzani's home. In the letter of Antonio Scarpa to Gregorio Fontana of 16 September 1786[42b] it is related that the Canon Volta (this is not

Alessandro the physicist) had gone to Scandiano under another name and found in Spallanzani's Cabinet more than a thousand pieces taken from the Cabinet in Pavia, still bearing the labels affixed by Volta. While Spallanzani was eventually exonerated, a formal charge had been laid through the efforts of Scarpa, Volta, Scopoli, and Gregorio Fontana, the last confessing his error in having originated the drawing up of the charge in a letter of 10 August 1787 to the Royal Imperial Council:[8b]

> I receive with the most profound submission and respect the notification of the sovereign disposition, the justice and wisdom of which I revere and always will revere. A false light of untruth had imposed itself on my short intellect and seduced my poor judgment; the illusion now dissipated I acknowledge my unintentional error and deplore my blindness.

The involvement of the family Fontana in the affair of the museum included even the brother Bernardino, as related by Spallanzani:[54]

> When I found myself on the return voyage to Italy I knew nothing of all that, but had the news only in Vienna; indeed the whole city was full of it by means of a long letter which Lieutenant Fontana readily said he had received from his brother the professor and which he showed to everyone.

Unsurprisingly Spallanzani wrote on 3 May 1793:[14i] «Oh what an egoist, an envious solipsist, is Fontana! And that whole Roveretan family»![2h]

Giovanni Targioni Tozzetti. The son of Benedetto Targioni and Cecilia Tozzetti, he was born in Florence in 1712. As a boy he studied botany, mineralogy, and paleontology and amassed a collection of 50,000 specimens. At the University of Pisa he was laureate in medicine and appointed Reader Extraordinary in 1734. Returning to Florence he practised medicine with his father and studied mathematics and Greek. In 1737 Pier Antonio Micheli, renowned botanist, professor and director of the Royal Garden of Simples, with whom Giovanni had been studying, died and was succeeded by his pupil. In 1739 Targioni was made Prefect of the Magliabecchiana library of 40,000 books and 1,100 manuscripts, which he catalogued, and stored much of it in his mind. Being charged with the disposition of the collection and manuscripts of Micheli he offered them for sale; when no buyer appeared, he acquired them himself for 1,384 *scudi* and the pledge to publish them; this was finally redeemed only in 1858 by Targioni's son, who included his father's note:

> My earnings are these: fifty-three *scudi* from lecturing, fifty-five as librarian, twenty-four from the Garden, and about sixty from medicine [...] I can't make money in improper or unbecoming ways [...] From 17 August 1738 when I bought the Micheli collection and took on the burden of it I have not had an hour's peace. Up to now I have spent a youth so full of tribulations that I would not wish it on my worst enemy [he was then twenty-six]. I must constantly pursue my roles as physician, librarian, botanist, all unrelated to each other. With all that, I abominate the servile life of a physician and do nothing to draw myself into it, and yet such is the kindness of some, and the esteem in which they hold me, that they want in every way to believe me a physician and to be treated by me. I would have some reason to be glad of this if it left me more time for my studies, because I practise medicine with complete propriety.

When in 1763 the Emperor Francis Stephen ordered Marshal Botta Adorno to prepare an annotated catalogue of the items in the Florentine collection, it was Targioni who was given the job, which he executed. Of his many works, particularly notable were: *Account of travels in Tuscany to observe its natural products and its monuments,* first edition, 6 vols., 1751-54, second edition 12 vols., 1768-79; *Notes on the growth of the physical sciences* in Tuscany in sixty years of the seventeenth century, 3 vols. in 4°, 1780. He also preserved in copies a part of the records of the experiments of the *Accademia del Cimento,* the originals subsequently being lost. He died in 1783.

After Fontana's appearance on the scene, Targioni's name no longer appears in connection with the museum; their relations must have been distant – at times they were certainly strained. Targioni's contribution to the cultural life of Florence was great but he would not have developed the museum from the Cabinet of Curiosities as did Fontana.

Pietro and Alessandro Verri. The correspondence[9] of these brothers, taking place from 1767 to 1782 between Milan and Rome, provides vivid glimpses of the individuals of the Italy of the period. Pietro (1728-1797) rebelled against his father who was a functionary of the Austrian government and, after military service, began to write. With Beccaria, Frisi, Carli, and others the «*Società de' Pugni*» was founded as a Milanese colony of the «encyclopedists beyond the Alps». Pietro created the periodicial «*Il Caffè*», the voice of reform, and edited it during its existence, after which he was in and out of public life. His objective was to put reform on a scientific basis, but he avoided the abstract rationalism and exaggeration of the French *philosophes.* While preoccupied with economics, he took a stand against all demagogues, extremists, and anticlericals. His wish was to conciliate liberty with authority in a

foreseen future of Italian unity. His judgments of men are often harsh – as Valsecchi[79] wrote, they were highly subjective, they were polemic.

Alessandro (1741-1816) studied literature in Rome, wrote a history of the city from Romulus to 1761, but it was never published. He studied jurisprudence and in 1764 became a member of a commission to draw up a new contract for the tax farm. He also experimented in chemistry.

They seemed to know Fontana well and to approve of him. Pietro wrote[9l] in October 1769:

> The Abate Fontana [Felice] brother of the Scolopian [Gregorio] gave him the highly confidential news that it had already been decided in Vienna four months ago that Firmian, Daverio, and Carli would be removed; the difficulty is to find a niche for the first. The one who wrote this is intimate with the Grand Duke [the passage is in cypher].

Alessandro wrote[9m] in December 1772:

> A vender of barometers from Como sells a powerful newly-invented electrical machine [described further]; it must be the invention of the Abate Fontana.

Pietro wrote[9n] in November 1775:

> Yesterday I went to see the Brera observatory and was enchanted by the beauty and rationality of the building, the convenience for all purposes, and the abundance and perfection of the instruments. The Abate Fontana was equally enchanted with it; he spoke of you with great esteem.

Alessandro Volta (1745-1827). Born at Como, made professor of experimental physics at Pavia in 1778, and Rector of the University in 1785, Volta became one of the most highly honored figures of his time for his discoveries and writings, which need not be described here. His *Epistolario*[60] gives five letters from Fontana, three in 1778, one in 1779, one in 1786, chiefly on subjects of pneumatic chemistry and eudiometry. One from Paris in 1778 gives many details of Fontana's experiments, then:

> Your last letter makes me suspect that you may have the idea that I claim your discoveries, or those of others [...] I wished to write all this in order that you may see that it is not I who fishes in the dark or that I appropriate the things of others. The duty of an honorable man thus satisfied, it remains only for me to beg you not to trouble yourself with writing me about your friends' matters. If you wish to

continue to write to me, you will do me a real favor and I will receive your letters in London for where I leave in a few days. I will learn with pleasure about that which you have published but not that which you have not yet published, because I do not wish to arouse jealousy on the part of anyone. It is for three years that I have worked on the airs; nothing is likelier than that we should meet on that path. But I learn to my cost that one must hurry if one wishes not to pass for a plagiarist even if one is the originator.

Fontana was not entirely candid, however; the same letter ended with:

Arrived in London I shall write you of my experiments with a known material [i.e., charcoal] which absorbs air completely and changes its nature. Mr. Magellan who was here a few days ago wished to see these experiments of mine with that substance, and he was astonished, as you showed yourself to be in your letter. But first I await your reply at London, then I will answer. If you think of any way I may serve you in that country, do not spare me; I assure you of all my respect and esteem.

Volta had already written from Como, 8 Juli 1775, to Marsilio Landriani, for whom he apparently had friendlier feelings than he did for Fontana:

I have also seen [...] the memoir of the Abate Fontana, and I feel that at bottom he wishes to usurp the glory of the fine instrument for measuring the salubrity of the air that you had already conceived and constructed before him.

Volta was justly if sharply critical of Fontana's book on nitrous and dephlogisticated airs in a letter of 18 April 1777 to Barletti, professor at Pavia and famous for his studies of electricity:

I also have read, although too hurriedly, the work of the Ab. Fontana on nitrous air. He has found little new there, beyond what to me seems unlikely, much less demonstrated [...] I believe then that he boosts Fontana to set up as a rival to Priestley. Imagine that! I do not speak of the first volume of this clever Englishman; I speak of the second which has more than a hundred pages, each of which is worth the whole book of the Ab. Fontana.

However, in his numerous writings[61] Volta did give full notice of and credit for Fontana's works, particularly in pneumatic chemistry and eudiometry, although he believed his own eudiometer to be superior.

In his letter to Volta of 1786, at the time of the Giorgi controversy, Fontana wrote: «A great misfortune to encounter mountebanks who divert us from the straight path. That is my fate».

PART II

WORKS

CHAPTER 13

AIR, WATER, EARTH, FIRE

Fixed Air - Salubrity of the Air - Nitrous and Dephlogisticated Airs - The Air from Saltpetre - Airs from Waters - On the Elasticity of Aeriform Fluids - The Density of an Air - Evaporation of Fluids - On Water into Earth - On the Decomposition of Water - On Malachite - On Sulfur - The Adsorption of Airs by Charcoal - On Gold - Felice Fontana as Physicist - Felice Fontana as Chemist - Phlogiston

These four elements, those substances into which all things can be decomposed, which are not themselves capable of being divided into others, various amounts of which are contained in every real thing, but which never appear in a pure form, as taken by Aristotle from Empedocles, formed the theoretical basis of thought about matter until late in the eighteenth century. By that time, as theories go, they had undergone some change. Cardano (1501-1576) had eliminated fire as a form of motion rather than a substance, Van Helmont (1579-1644) had eliminated earth. Gassendi (1592-1655) had resolved them into atoms, Boyle (1627-1691) had defined them:[1]

> I mean by elements, as those Chymists that speak plainest do by their principles, certain Primitive and Simple, or perfectly unmingled bodies; which not being made of any other bodies, or of one another, are the Ingredients of which all those called perfectly mixed bodies [meaning chemical compounds] are immediately compounded, and into which they are ultimately resolved.

Unfortunately Boyle gave no list of what the considered to be the elements.[a] While he «never wrote the "chemical philosophy" he once

[a] In fact Boyle finally lost any conviction that such things as elements existed. See M. Boas, «Isis» 41:261, 1950.

contemplated, he did introduce a spirit, a point of view, and a method of attack that could permit chemistry to develop along scientific lines, as distinct from purely alchemical lines». He used rigorous experimental methods, clearly described.

By the eighteenth century, the chemist had become fully operational, rather than theoretical; as Macquer[b] wrote, there was reason to think that the four classic elements were perhaps not the most simple elements of matter, but «since experience has taught us that we cannot by our senses discover the principles of which they are composed, we may most reasonably consider them as simple homogeneous bodies, and the principles of the rest».[2] Into this rather Newtonian frame, Fontana fit. His approach was experimental, and he applied himself to many physical and chemical problems. Since there is no single unifying theme in his writings on these subjects, they are taken up individually in a more or less chronological order, but still are conveniently grouped under the headings of the classical four elements.

AIR

Although Macquer adopted Van Helmont's name *gas,* Fontana followed the lead of Priestley, who saw no occasion to use it, but instead wrote of *airs.*

FIXED AIR. Fontana's article[3] on fixed air[c] (carbon dioxide) begins with mention of «modern physicists», with citation of Priestley's «beautiful experiments and observations», also that Bergman had written to Priestley that this *aerial acid,* as he called it, turns blue litmus red, also that Hey (a surgeon in Leeds) had found that it did not behave as an acid in changing the color of syrup of violets, as do sulfuric and other strong acids.

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[b] Macquer was an excellent teacher and the author of a text-book (*Elemens de Chymie Theorique,* Paris, Didot, 1749) which became very popular, and the first dictionary of chemistry (Paris, 1766, 2nd Ed. 1778). Both were translated into many other languages.

[c] This name was applied by Joseph Black, who had prepared and described it in 1756. An extensive study of fixed air by Cavendish appeared in 1766. Neither mentioned its acidic properties. Its existence in nature had been recognized by Van Helmont in 1648.

Although Priestley had stated that fixed air was acid in itself, albeit a weak one, Fontana devoted his paper to confirming his suspicion based on observation that water containing fixed air dissolves soap poorly, curdles with it, does not foam, also precipitates with lead acetate – that the acidity is due to the sulfuric acid used to generate the fixed air (all false reasoning). His experiments all supported his conviction, that the sulfuric acid forms a solution in the fixed air so that they move together, with the acid property residing in the former, not the latter. This belief was soon refuted by Bewley.[4]

He then passed to his confirmation of Priestley's discovery that the passage of an electric spark through a tube containing atmospheric air and a litmus solution will turn it red, i.e. an acid is generated. From this he was led into fanciful speculations, based on the phlogiston theory, on noxious and salubrious airs.

The Royal Society of London has a manuscript (A.P.4.8) which is a translation of this work into English, probably prepared when its publication in the *Philosophical Transactions* was being considered. The judgment was apparently adverse, as it never appeared there.

Fontana seems seldom to have used the word element. Athough he had no clear conception of the composition of matter, he had a grasp of the existence of a single form of matter in the three physical states of solid, liquid, and vaporous. Now his mind turned to other «useful» objectives, such as: «to improve the air which we breathe. That is a subject of equal interest to all: even sovereigns who like others share its use».

THE SALUBRITY OF THE AIR. An idea of the medical thinking on the subject at that time and place comes from a work by the eminent physician Giovanni Targioni Tozzetti.[5] In writing of the severe epidemic of 1756 of a fever with a high rate of mortality (in one locality with about 8,050 inhabitants, there were 2,797 cases, 566 of whom died), he concluded that the cause was:

> the pestiferous exhalations from the stagnant waters of a brook, more active and poisonous on account of the excessive heat and dryness of the season, combined with exhalation, no less pestiferous, of the marshes and swamps.

Predisposing causes were the sickly disposition of the inhabitants manifested in cachexia and obstruction of the viscera, bad food and drinking water, although these occur frequently without pestilence

following. All these matters are discussed fully with copious quotations on all points from Hippocrates on. He notes that Boissier de Sauvage[d] had suggested: nerve juice, an electric material, unites with the lymph. If the nerve juice is lost, the animal falls into languor; perhaps those vapors which destroy the elasticity of the air might destroy the electric material.

Targioni concludes from the statements of the authorities that to be healthy and live long it is necessary to choose a good and salubrious air and avoid the bad. Among bad airs, those contaminated by the putrid exhalations of stagnant waters are the worst. Water which moves does not putrefy. However, stagnation does not produce an epidemic every year, so also necessary are much heat and a dry season. Targioni had some doubts as to his conclusion about the cause of epidemics:

> this same situation occurs unfortunately in our time in the Valdinievole through the serious derangement of the course of its waters; but today the true and striking cause of pestilence is not the same as it was in 1318, when they made no ponds [...] or sought the cause in them with the microscope.

His remedy was: control the drainage of the land. For treatment the physician needs to know whether these pestiferous exhalations have contaminated the blood or the gastric juice. Drugs used were *china china,* spirits of arsenic, and aconite.

As Fontana had promised in the previous paper, he published in the same year a description[6] of instruments to measure the salubrity of the atmosphere in which he followed the lead of Priestley, who had constructed a similar device.[7] Their use depended on a reaction of «nitrous air» (nitric oxide). This substance had been prepared by Boyle[8] in 1660 although he was not aware of it. It was prepared again[9] by Hales[e] from Walton pyrites (iron sulfide) and nitric acid but to no advantage; as Priestley wrote, «nitrous air obtruded itself upon Dr.

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[d] François Boissier de Sauvage de la Croix (1706-1767), French physician, was professor of medicine and botany at Montpellier. The citation was: *Dissertazione come l'aria con le sue diverse qualità operi sul corpo umano,* with annotations by Saverio Manetti, (professor of botany). The original, on the cause of fevers, was printed in Geneva in 1744.

[e] Stephen Hales (1677-1761), F.R.S., «perpetual curate» of Teddington, Middlesex, published two books: the *Vegetable Staticks* cited in which he described many experiments in pneumatic chemistry with apparatus he had invented, and *Statical Essays, containing haemastaticks.*

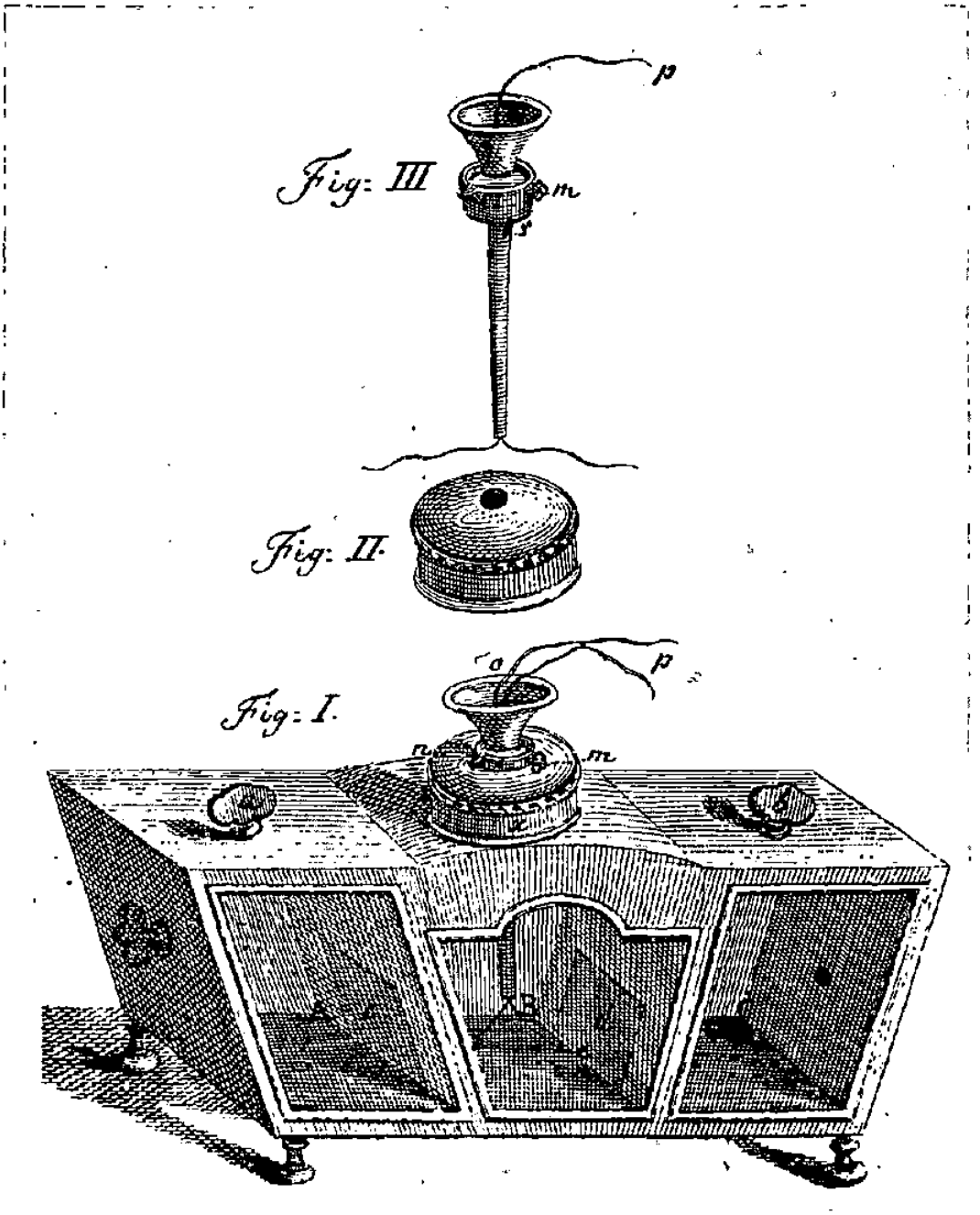

Fig. 20 – Fontana's Eudiometer, Model II.

Hales», who had written that it «expanded with great heat and fume into a space equal to 200 cubick inches, and in a little time it condensed into its former space, and then absorbed 85 cubick inches of air». This experiment stuck in Priestley's mind and

> happening to mention this subject to the Hon. Mr. Cavendish, when I was in London in the spring of the year 1772, he said that he did not imagine but that other kinds of pyrites might answer as well as that which Dr. Hales had made use of, and that probably the red appearance of the mixture depended upon the spirit of nitre only [...] Thus encouraged, I tried different metals in spirit of nitre [...] I found what I wanted, and a great deal more.

Priestley[10] named the new air nitrous air and gave it a thorough study, noting that this colorless air on mixing with common air developed reddish fumes which persisted for a long time over mercury but quickly disappeared on the addition of water, which also augmented the diminution in volume which occurred. After saturation of common air with nitrous air, adding more made an equal addition in volume, but no red fumes, no visible effect. The common air diminished in volume by 1/5 to 1/4 (atmospheric air is 21% oxygen). It was also found remarkable that, on whatever account air is unfit for respiration, this same test was equally applicable. This is untrue, but it was a sufficient lead for Fontana; his book begins with some clear statements, as usual with him; after crediting Priestley with the discovery of nitrous air, he listed the desirable features of such an intrument, the difficulties in its use, and its possible utility. Fontana then showed eight instruments, four of them depending for quantitation on the weighing of mercury. Two of these were of glass, resembling the device at the left of Fig. 21; two were wooden boxes with glass inserts in the walls, to be used similarly. (Fig. 20) Four instruments were volumetric, three with a vertical glass tube connected at the top with one or two chambers for the airs to be mixed by a stop-cock, another of similar form but with the function of the stop-cock supplanted by manipulation of the mercury column within. The eighth instrument, shown in Fig. 21, performed the reaction in the vessel to the left, measured the residual volume of gas in the horizontal tube, displacing mercury. It is difficult to imagine the successful operation of this last instrument; in fact the construction and use of all these instruments may be doubted. They were all to be used with mercury, no water being present, but Fontana made no mention of the results differing from those in the presence of water; actually no results of analysis are given in his book. He did use some kind of such an analytical

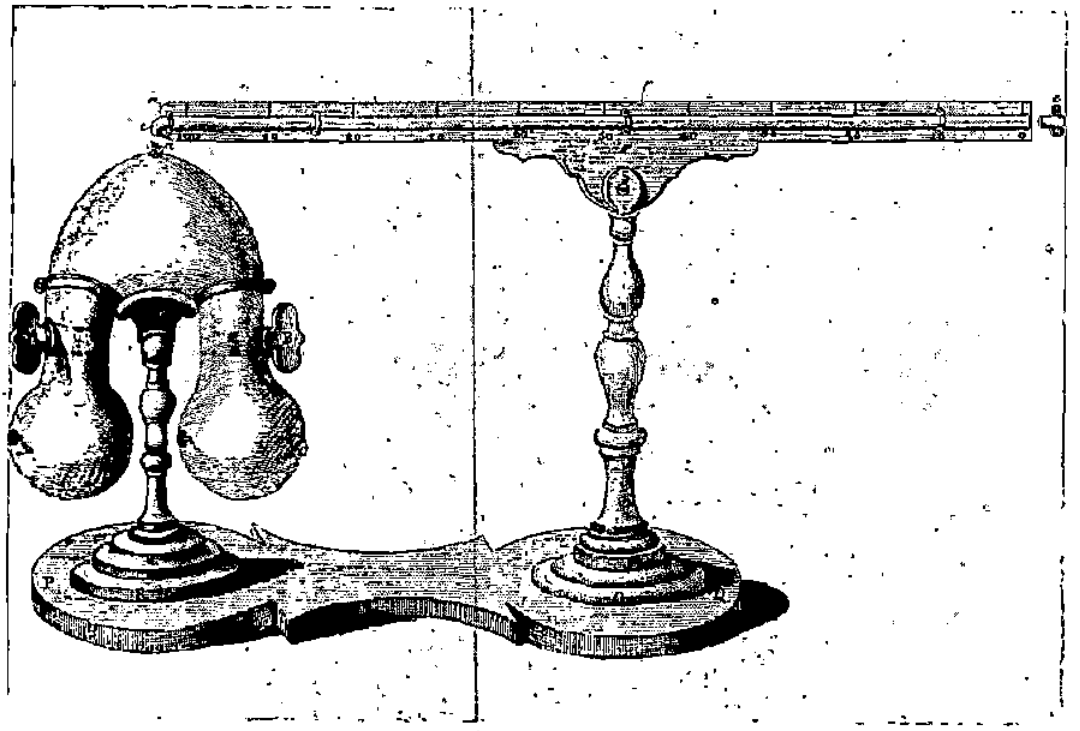

Fig. 21 – Fontana's Eudiometer, Model VIII.

instrument throughout his career, including his last investigation; by then he was using Landriani's term *eudiometer* although he had wished that it be called *evaerometer*. His findings are obscured by the adoption of a cumbersome way of recording results, which he varied from time to time.

The most precise description of «Fontana's eudiometer» that we have came from Ingenhousz.[11] He wrote:

> The method [of eudiometry] was the same which the celebrated Abbé Fontana makes use of now and which he himself has not yet given an account to the public. As I had no right either to claim the invention of his method, or to anticipate the publication of it without his leave. I have asked his consent on this head. He agreed to my request very readily, gave me his notes to consult, and even permitted me to get his instruments engraved, for which purpose he allowed me to make use of his drawings. As he had already shewn me his method of examining the different kinds of air in regard to their degree of salubrity, or fitness for respiration, when I was with him in Paris in the beginning of the summer of 1777, and as I have since he rejoined me in London, 1778, seen a very great number of like experiments, on purpose to imitate his method of examining air, which I found so accurate, that, in ten experiments made one after the other with the same kind of air the result differed seldom above 1/500; that is to say, that the remaining bulk of the three measures of nitrous air, which he joins one after another to the two measures of atmospheric air, is so alike in the various experiments made with the same common air, that the difference will seldom amount to more than 1/500 of the whole; which accuracy in exploring the degree of goodness of respirable air surpasses the exactness of judging the degree of heat and cold by the thermometer of Reaumur.
>
> The Abbé has, since I saw him at Paris, changed somewhat his instruments and method of using them, or rather corrected them a little; but they remain still materially the same as they were before.

The instrument Ingenhousz showed is seen in Fig. 22. He stressed the importance of adopting a standard procedure for all steps. He calculated the parts of air destroyed by subtracting the quantity existing in the tube at the end from the amount employed; with 2 volumes of common air and 5 of nitrous air, the difference lay between 1.03 and 1.09, usually 1.08.

Whence came this instrument? It resembles none of those described in 1775 by Fontana. Did Fontana neglect to mention his earlier publication to Ingenhousz, if he was in fact unaware of it? Had Fontana realized that his instruments of 1775 were of little value, and then by 1777 returned to the design described by Ingenhousz, which actually was quite similar to the original one of Priestley? The last seems the most likely, and is supported by a footnote added by Argand in his French version, with notes, of the essay[64] describing the new Royal Museum in Florence. The text to which the footnote referred read

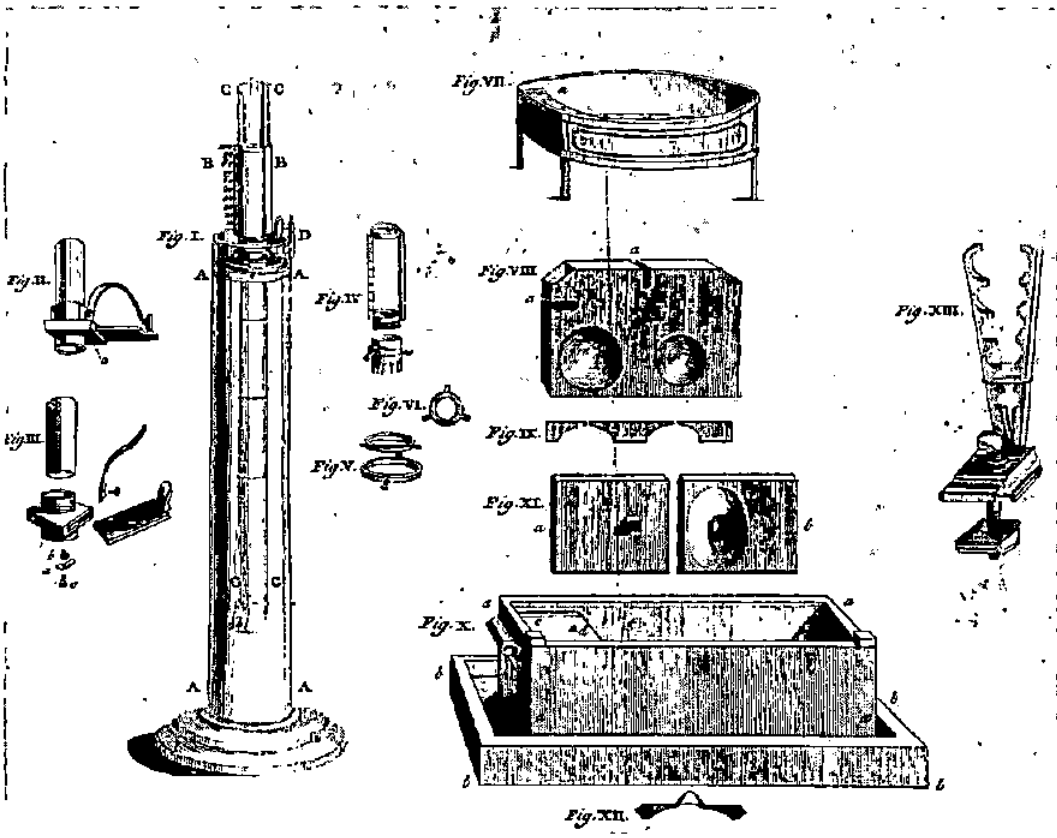

Fig. 22 – Fontana's Eudiometer according to Ingenhousz.

he [Fontana] has invented two small instruments for measuring the degree of salubrity of the air in relation to respiration, or what is the same thing, the reciprocal penetration of nitrous and respirable airs; one of these instruments makes this known by a difference in weight, the other by the difference in the height of measurements.

The footnote read

Fontana has since invented, and worked out in Italy, eight different instruments to measure the degree of salubrity of airs; but as he found them defective in some respects, he has substituted for them two others much more exact, which he will announce in a work that he is now writing on the subject, where he will give the method of using them, and ways to avoid all error.

This work never appeared, except as represented in the articles of Ingenhousz.[11, 17]

A subsequent study of nitrous air eudiometry was made by Cavendish,[12] who found some variations in results depending upon the procedure. Nevertheless, he reported: «The highest test that I ever observed was 1.100, the lowest 1.068, the mean 1.081...», or the same as reported by Ingenhousz.

Others who found Fontana's eudiometer useful were Luz[13] and Scherer.[14] Senebier at first[15] criticized it, but later[16] wrote that it was excellent. Ingenhousz reiterated its superiority.[17] However the nitrous air eudiometer was superseded by Volta's invention,[18] 1777-1790, of an instrument using inflammable air (hydrogen) as the reagent, it being more convenient to use.

Contemporary and subsequent research on the salubrity of the air with the nitrous air eudiometer led to conclusions markedly at variance. Priestley and Sigaud de la Fond[26] saw little difference in the results obtained in different places, but Ingenhousz found that the air at the sea coast was better than ordinary air, and Landriani, after making a «eudiometrical tour» of Italy, reported that the air on mountain-tops was better than that in valleys.

Fontana wrote as follows in 1778:[30]

I have not the least hesitation in asserting that the experiments made to ascertain the salubrity of the atmospherical air in various places, in different countries and situations, mentioned by several authors, are not to be depended upon; because the method they used was far from being exact, the elements or ingredients for the experiment were unknown and uncertain, and the results very different from one another. When all the errors are corrected it will be found that the difference between the air of one country and that of another, at different times, is much less

than what is commonly believed, and that the great differences found by various observers are owing to the fallacious effects of uncertain methods. This I advance from experience: for when I was in the same error, I found very great differences between the results of the experiments of this nature which ought to have similar; which diversities I attributed to myself rather than to the method I then used... we clearly see, how little the experiments hitherto published about the differences of common air are to be depended upon. In general I find that the air changes from one time to another; so that the differences between them are far greater that those of the airs of different countries, or different heights [...] I could not perceive any particular change of health, or facility of breathing, arising from those changes of the salubrity of the atmospherical air; and I am informed that no particular diseases appeared which could indicate any remarkable change of air... I do not mean to deny the existence of certain kinds of noxious air in some particular places; but only say, that in general the air is good everywhere and that the small differences are not to be feared so much as some people would make us believe... I would not have any body suppose, that I think it of little importance to know the goodness of the atmospherical air, and the changes it undergoes. On the contrary, I believe it to be a useful inquiry for mankind, because we do not yet know how far one kind of air more than another may contribute to a perfect state of health; nor at what time small differences may become very considerable, when one continues to breathe that same kind of air for whole years, especially in some kind of diseases.

It is passages such as these which have given rise to the statements that Fontana demonstrated the constancy of the composition of the atmosphere. We must also admit that his assertions on salubrity of atmospheric air were judicious and are now being appreciated.

NITROUS AIR AND DEPHLOGISTICATED AIR. In the first part of this small book[19] Fontana presents experiments done in Florence at the time of preparation of the work on instruments for measuring the salubrity of the air. Fontana knew Priestley's study of nitrous air, and confirmed many of his results. He states that it is colorless, that it is most active when it is generated vigorously, in large bubbles (which is true), but also when the bubbles have a reddish color, which is misleading since the reddish color is that of the reaction product with oxygen. He described the reaction with atmospheric and dephlogisticated airs correctly, as being more rapid and complete when water is present. He noted that it is not acid itself, in confirmation of the Duc de Chaulnes' finding that it did not turn litmus red, nor did it have an acid taste on the tongue, but that it formed nitric acid on reaction with air and water as shown by its attacking metallic silver. It precipitated lime-water, but not in the same way as did fixed air. He concluded at first that it was compounded of nitric acid, iron in firm combination in the form of *terra marziale* (iron

oxide), and phlogiston, but the conclusion that iron was present was weakened when he found that it could be prepared with non-ferrous metals. The phlogiston phlogisticated common air or dephlogisticated air, but the goodness of these could be regenerated by shaking with water.

In short, most of his experiments were well-conceived and well done, but constituted only a modest advance over those of Priestley, and left considerable uncertainty in his mind.

The second part of the book, on dephlogisticated air, led Fontana into hot water. Scheele had discovered oxygen, which he called *aer vitriolicus,* later fire-air, between 1770 and 1773, but that did not become known until his book on *Air and Fire* appeared in 1777. Priestley independently discovered oxygen, which he named dephlogisticated air, in 1774 and published on it in 1775, about which Fontana must have known. When Felice arrived in Paris in early 1776, Lavoisier had already rediscovered oxygen and was occupied in rewriting his communications to make it appear that he had done so without any knowledge obtained from Priestley, or from Bayen (1725-1798), who had performed similar experiments. Fontana cited these three in this book of 1776, in which he described his careful quantitative experiments on the preparation of mercuric oxide and its decomposition to give dephlogisticated air and metallic mercury. He wrote:

> The atmosphere is a respirable fluid, which contains suspended and in solution an infinity of foreign bodies which ceaselessly arise from the earth, and combine in a thousand ways. It is also certain that there is in the atmosphere either nitric acid itself, or at least the elements and principles from which it is formed [...] the dephlogisticated air that one draws from this substance in the state of a calx [mercuric oxide] comes from the nitric acid of the atmosphere, or better said, from the principal constituents of that acid.

He added a note that he could draw dephlogisticated air from various substances; Gibelin testified in the reprinting of this 1776 work in 1780 that he had assisted with these experiments in Fontana's home in September 1776. Fontana left a note on his new experiments with Condorcet, secretary of the Academy of Sciences, in November 1777 but this remained unpublished.

Priestley wrote in 1777 of those quantitative experiments of Fontana's in which he recovered all the liquid mercury used to form the oxide, after its regeneration with the loss of dephlogisticated air, saying that Lavoisier's experiment was the same, «and which of them made it the first does not appear». There is no real reason to believe that

Fontana's contribution influenced the experiments of Priestley or Lavoisier concerning oxygen, and no proof that he anticipated them. It is clear that he conceived and performed successful and pertinent quantitative experiments and had a grasp of their significance – up to the point at which phlogiston intruded. This work received much attention, being reprinted, translated into German,[20] repeatedly reviewed,[21-24] and cited.[25,26]

THE AIR FROM SALTPETRE. There is a manuscript[27] of Fontana's entitled «Experiences sur l'air qui se devouluppe du nitre». It was published[28] in 1778 as part of a communication: «Experiences sur l'alcali fixe végetale, & sur l'alcali minéral», with the explanation that experiments on the decomposition of nitre had as their only objective the examination of fixed alkali...», which is for us of less interest; this paper has not received much attention. Felice quotes, as previous observers of the decomposition by heat of nitre, Pott[f] and Gaillard (unknown to us), who did not collect or describe the air emitted, also Hales, who measured the volume of air given off but missed discovering it to be oxygen, and Berthollet[g] who collected the gas, washed it in water, and showed that a flame burned much brighter in it than in common air. Fontana was certainly unaware of the earlier similar demonstrations of Borrichius[29] in 1680 and Scheele (published in 1777).

In his own experiments he heated saltpetre in a retort, collected and analyzed the emitted gas and the residue. The gas he showed to be dephlogisticated air by the brightness of a flame in it, and its test with his nitrous air eudiometer. He concluded that nitre was a salt of fixed alkali and nitric acid, the latter composed of dephlogisticated air and phlogi-

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f Johann Heinrich Pott (1692-1777), pupil of Stahl, originator of the phlogiston theory, on which Pott wrote at length. He was a man of great learning and wide reading, as well as practical industry. Commissioned by the King of Prussia to discover the secret of Meissen porcelain, he is said to have made over 30,000 experiments.

g Claude Louis Berthollet (1748-1822), Savoyard. Student first of medicine, then chemistry, member of the Academy of Sciences and the Royal Society, he had an eminent career. His grasp of physical chemistry made him one of the founders of that science. At first defending the phlogiston theory, he was the first prominent French chemist to adopt Lavoisier's views. His memoir on the decomposition of nitre, which Fontana learned of from d'Arcet who heard it read in 1778, did not appear in print except as reported by Lavoisier.

ston. He knew of no other substance which yielded so much dephlogisticated air, one pound giving 12,800 cubic inches. On calculating the cost of breathing this air by man, which might be useful as a remedy, he found it too dear, but he began to think of alternatives, which led him to invent the closed-system oxygen-breathing apparatus described by Ingen Housz (p. 201).

AIRS FROM WATERS. This study[30] of Fontana's has no great content; its chief interest lies in the demonstration that oxygen is more soluble in water than is nitrogen:

> water, after it has been boiled for a long time, absorbs in forty days about one fourteenth of its own bulk of dephlogisticated air, when in the same length of time it does not absorb more than one twenty-eighth of common air.

This is in close agreement with more recent information on comparative solubility. He also wrote:

> By means of pure water, especially distilled water, common air may be changed to dephlogisticated air, that is, into air much more salubrious than the best common air which we breathe; and this, for what I know, is the only means of meliorating common air.

Priestley had remarked[10] that continual agitation in a trough of water «never failed to restore any kind of noxious air on which I have tried it». This was a delusion.

ON THE ELASTICITY OF AERIFORM FLUIDS. Boyle's famous experiment, «Touching the Spring of the Air», which led him in 1661 to formulate the law named after him, that the volume of a gas is inversely proportional to the pressure, was well-known to Fontana, as no doubt were the many subsequent confirmations. Many observations had been made with common air, but not with the «factitious airs» that were now known. Mayow had made some rough comparisons of the elasticity of some factitious airs with that of common air in 1674, but it is doubtful if Fontana was aware of them.

He did his experiments[31] with twelve gases, in an apparatus that had two graduated vertical glass tubes closed at the top and immersed in mercury at the bottom. Equal amounts of airs, always common air in one tube and a factitious one in the other, were introduced, then the pressure on the mercury was raised with his compression machine. Thus

comparative elasticities or compressibilities, at pressure above atmospheric and at the same temperature, were obtained. He mentioned but not in detail experiments at less than atmospheric pressure.

All the factitious airs were more compressible than atmospheric air, but did not differ significantly among themselves. Fontana found it interesting that so many fluids, so different, observed the same law of expansion and contraction, and thought there must be a physical force in nature, of a principle as yet unknown, by which the particles separate and return by the same law, as seen.

THE DENSITY OF AN AIR. As an addendum to another paper,[28] Fontana showed how to measure the absolute weight of an air with a method which he pointed out was one of general application. It rested on the measurement of the volume of an air (carbon dioxide) liberated on heating zinc carbonate, which he chose for its ease of manipulation; by weighing the solid before and after heating, he obtained the weight of the air. The method is really that of Cavendish[32] who was the first to show that different airs have different densities; he is cited by Felice, as is the Duc de Chaulnes, who had weighed known volumes of fixed air which requires an excellent balance and presents other problems.

Fontana himself made many determinations of the specific gravity of airs with this last method, which he never published. The book of Cavallo[h] speaks of these experiments, showing the apparatus (Fig. X of plate III) and giving a table on p. 772 of Fontana's results for ten elastic fluids.

WATER

EVAPORATION OF FLUIDS. This communication[33] of Fontana's and its experiments were based on an observation that water in a closed vessel did not disappear when heated, although it did so when the vessel

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h Tiberius Cavallo: *A treatise on the nature and properties of air and other permanently elastic fluids, to which is prefixed an introduction to chemistry,* London, 1781. Cavallo was associated with Fontana in many of the experiments done in London, in fact he was the one who picked him up from the floor when he lost consciousness while breathing hydrogen. This book, described at length in the *Critical Review* for January 1782, is worthy of note; although his own contributions to knowledge may not have been great, he presented a useful compendium of the labors and thought of Fontana particularly, also of Bergman, Lavoisier, Priestley, Scheele, and many others of the time.

was open. Suspecting that water «was not susceptible to being dissolved as vapor in unrenewed air», Fontana performed many curious experiments. One type was with that highly volatile fluid, ether. Two flasks were sealed together by their necks; into one, ether was introduced by means of a small tube subsequently sealed off. The flask containing ether was heated; the other was cooled. No ether passed from the hot flask into the cool one (although they sometimes exploded). A similar result was obtained with water, although if a very small hole was left in the top, of the recipient flask, water distilled into it. Similarly, if water was put in one flask, and the very hygroscopic sulfuric acid in the other, and the system sealed, no water passed into the acid. Apparently Fontana believed that air was necessarily present, and renewed, for volatilization or distillation of fluids to occur. He gave no discussion of the general opinion on this subject, except in the first sentence (in part the first sentence above) where he added parenthetically: «a phenomenon in some aspect known by the *Philosophes*». This may be a reference to the theory[34] of the state of water vapor in the atmosphere, that it is not physically mixed with the air but in a state of solution. This theory was favored by Berthollet, and by de Saussure,[i] who thought that evaporation was less in a vacuum than in air.

ON WATER INTO EARTH. As Fontana wrote, the problem of the conversion of water into earth is most ancient; even the Greeks discussed it. Partington cites those who believed that they had converted water into earth, from finding a slight earthy residue after evaporating distilled water to dryness in a glass vessel, as: Van Helmont (1579-1644), Newton (1642-1727), and Wallerius (1709-1785). Boyle (1627-1691) was sceptical. Borrichius (1626-1690) «found that water distilled ten times still left a white earth on evaporation in a glass retort, and reported that Edmund Dickinson[j] at Oxford had obtained the same result after a hundred distillations». Almost alone, Boerhaave (1668-1738) «denied the conversion of water into earth; the small residue which he found he thought was dust from the air, saying: men often overlook things which

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i Nicholas Theodore de Saussure (1767-1845), is known chiefly for his contributions to the understanding of photosynthesis.

j Edmund Dickinson, (1624-1707), physician to Charles II and James II. He claimed to know the secret of the philosopher's stone, and labored for many years on *stercus humanum,* from which he extracted metals.

mix themselves unexpectedly in chemical operations». Fontana mentioned some of these, also Le Roi who published his experiments in 1773; Le Roi disputed the conversion on the basis that salts in water would distill with it, so that the two could not be separated by distillation.

Fontana then described the communication of Lavoisier, which also appeared in 1773; he heated water, which had been distilled eight times, in a closed and sealed vessel called a pelican, which permitted return of the distillate, for 101 days. The white solid which had formed was collected and weighed, and compared with the loss in weight of the pelican. Lavoisier concluded that this «earth» had been dissolved by the water from the glass vessel and had not come from the water itself. In 1774 a paper by De Machy (1728-1803) appeared (described by Fontana) with the conclusion that the problem was not resolved, that conversion may have occurred.

Fontana wrote[35] that he could not hope to resolve the problem in the time he could remain in Paris, and that his object was to observe what happened to the air in the sealed flask which he had eight months to study. He used flasks of glass and of crystal, putting in water, or sulfuric, nitric, hydrochloric or acetic acids. He weighed the flasks and contents before and after heating, analyzed the air and the solid which formed. His results were indecisive.

In 1781, he returned to this problem and concluded[36] from his new experiments that the earth of silicious nature found in flasks after 14 months of heating came from the flasks, not from the water.

ON THE DECOMPOSITION OF WATER. Contemplation of Fontana's papers on this subject is painful; they contributed nothing to science; they show him refusing to face the significance of his own experiments, and wilfully performing others leading away from a great new truth; they also recall his involvement in a vicious controversy.

In 1784 Meusnier and Lavoisier[37] published a memoir of their experiments on the decomposition of water, a small but important bit of the intense research into the composition of water which in turn played a large part in the acceptance of the new chemistry. Curiously enough, one of Lavoisier's leads to this work had been Fontana's own finding that inflammable air was produced on quenching flaming charcoal in water. Lavoisier wrote later:[38]

> «I then [in 1783] observed that if water is really compounded, as announced, by the combustion of two airs, the union of the oxygen principle with the inflammable

aqueous principle [hydrogen], it could not be decomposed so as to obtain one of its principles separately without presenting to the other a substance with which it has more affinity».

To do this, they ran water slowly into a copper tube containing iron which was heated to redness. The other end of the tube led to a vessel to entrap water then to a gas-collecting bottle. The gas was found to be inflammable and detonated with dephlogisticated air; it was identical with that formed with some metals and sulfuric acid, i.e. it was hydrogen. The iron was changed to a friable substance which did not differ from martial ethiops, i.e. iron oxide.

Priestley confirmed these findings.[39] So also did Fontana,[40] with identical results. However, his conclusion was «not proven». It is only fair to state that Priestley among others was also not convinced[39] by these experiments:

I was a long time of opinion that his conclusion [Lavoisier's] was just, and that the inflammable air was really furnished by the water being decomposed in the process, but though I continued to be of this opinion for some time, the frequent repetition of the experiments [...] satisfied me at length that the inflammable air came from [...] the iron.

However, Fontana regressed, so to speak. As described in his «Two Letters to Ingenhousz»,[41] he repeated the experiments at ever lower temperatures, with negative results, or as he put it, «water to vapor and vapor to water, or the physics of the kitchen – only you have seen me now turned into a cook». Fontana illustrated his apparatus and described one experiment, lasting about two hours and at a temperature of about 180° C, in a «Letter to Lorgna».[42] He reiterated his conviction that water gave nothing but water.

EARTH

ON MALACHITE. In May, 1778, Fontana read a communication to the Academy of Sciences in Paris[43] on the analysis of malachite. He may have chosen this material because it had been described by Biringuccio,[k] early

[k] Biringuccio (1480-1538). Little is known of his life. His book, *De la Pirotechnia* appearing in 1540 dealt with metallurgy, applied chemistry, gunpowder, military arts, and fireworks. It was essentially practical and described a number of minerals, also the separation of gold and silver.

Italian chemist. Fontana showed that it contained copper, but not sulfur or arsenic. On heating it yielded water and fixed air (carbon dioxide). His assay for copper as the oxide gave 77%; the theoretical value is 71%. Malachite is $Cu_2 (OH)_2 (CO_3)$.

ON SULFUR. Fontana described the curious behavior of sulfur on heating, but as he did so in an article on another subject,[33] it received little attention. The phenomenon was: solid sulfur on heating first liquefied then coagulated; on cooling it passed again through these phases. Baumé had made a somewhat similar observation[44] but added that if the coagulum is quickly cooled in water it becomes soft, like leather. He also found that it would crystallize if cooled slowly.

ADSORPTION OF AIRS BY CHARCOAL. These experiments of Fontana in which he made what might be identified as a true discovery, that is, of a new phenomenon of nature, were made during his earliest days in Paris.[45] Apparently his first observation was to hold a piece of charcoal in a flame until it was glowing, then introduce it through mercury into a vessel containing some air, which he saw disappear «down to the last atom». He performed numerous experiments using a glass tube over mercury, which, by being changed between vertical and nearly horizontal, permitted contained airs to be adsorbed by, or expelled from, the charcoal introduced into them. He varied the quality of the charcoal, the temperature of the activating flame, and the quality of the air, showing that phlogisticated air (nitrogen), dephlogisticated air (oxygen) fixed air (carbon dioxide), and inflammable air (hydrogen) were all adsorbed, to different extents. He characterized the air expelled from charcoal by its solubility in water, its inflammability, and its behaviour in his «nitrous air eudiometer». He also showed that charcoal quenched in water liberated «inflammable air» (carbon monoxide), while charcoal immersed in water after being flamed and cooled liberated nitrogen and carbon dioxide. His interpretation was that «these effects are derived from the phlogistic principle that is in the carbon».

Fontana was only moderately interested in his new discovery, as is indicated by his not publishing on it until 5 years later. However, he demonstrated it freely to visitors to his laboratory, as was his wont. He undoubtedly wrote about it to his correspondents, although we have seen no letter of his about it. Word of his discovery spread. Priestley learned of it, confirmed it, wrote of it to Landriani[46] and in his published works.[47] Volta wrote of it to Senebier.[48] Landriani went to Turin where,

with Morozzo[l] he repeated the experiment and confirmed the observation. Morozzo took up the study of the phenomenon[49] and, in his first publication commented with some pique that he had been unable to find any publication of Fontana's on the subject, although everyone seemed to know of the discovery. By the time of his second publication, he had seen Fontana's paper of 1782; he confirmed, in general, Felice's findings and extended them.

Somewhat similar observations, that charcoal took up gases reversibly, had been made independently, by Priestley,[50] Scheele,[51] and Bryan Higgins.[52] Although Fontana did not continue to study the adsorptive power of charcoal, others did. Lowitz[m] found that charcoal would remove many materials from solution, which he spoke of as its «dephlogisticating power», he having suspected that it had a strong attraction for phlogiston; this was part of Fontana's explanation of his observations.

For many years charcoal has been widely employed in the food and beverage industry. Its use in the chemical laboratory, in treating the products of organic reactions before crystallization, is a commonplace. More recently the uses of charcoal have expanded, particularly in the control of pollution of air and water. In the U.S.A. the production of decolorizing and water purification grades of activated carbon fluctuated between 160 and 180 million pounds per year for the past decade, but is estimated to have been 200 million pounds in 1974, and is projected to be 350 million pounds in 1985.[53] Fontana would find this of great interest.

ON GOLD. What we have on Fontana's experiments as part of his activity as Director of the mint is only a summary[54] prepared for the Grand Duke. While its principles are valid it would not be enough to

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[l] Carlo Lodovico Morozzo also wrote on respiration in plants and animals.

[m] Johann Tobias Lowitz (1757-1804). Born in Göttingen, died in St. Petersburg, where he had been Director of the Royal Apothecary. His book, *On Making Foul waters Drinkable,* etc. was Vol. II, Aus d. Abh. d. ökon. Gesellschaft, Petersburg, 1790, has not been seen. It was noticed in the «Giornale Fisico-Medico», 1792, IV, 253-4, which printed an article of Lowitz on the subject in 1793, III, 48-75. A translation appeared as: *Per conservare l'acqua dolce sul mare e render bevibile la corrotta,* Firenze, Pagani, 1792, 26 pp., 175×120 mm. Translations appeared in French also: *Mémoire sur le Charbon Végétale* of Kehls, «Obs. Phys.» 42: 250-261, 1793 which mentions Fontana and Lowitz; *Nouvelles observations,* etc. of Lowitz, *idem.* 42: 456-461, 1793, in which he said that the discovery belonged to him. See also Partington, *Op. cit.* III, 585-9.

guide an assayer in his daily work. In an abridged description of the Royal Museum,[55] there is a footnote which gives a little more detail on Fontana's methods of assay, saying that he would publish it in time (he never did).
The memoir gives the principles of detecting platinum in gold, and of refining gold (which will still contain two *grani ideali*[n] of silver) and silver (so that no more than one third of a *grano ideale*[o] of gold remains).

In two articles[56] entitled *General Principles of the Solidity and Fluidity of Bodies* and *On Light, Flame, Heat and Phlogiston,* Fontana discussed the physical states of matter, the force of gravity and the forces between particles of matter, and light, flame, heat, and phlogiston as if they were substances.

In an unpublished manuscript (*Biblioteca Nazionale Centrale.* Ms Palatino 1197, Vol. 74, cc 91[r]-95[v]) he wrote on the molecular properties of matter, apparently drawing his considerations from the writings of Keill,[p] thus eventually from Isaac Newton. Here he mentions[q] hydrogen and caloric, which suggests that the manuscript was composed after 1787.

Felice Fontana as Physicist

In physics Fontana is of interest not so much for his own original contributions as for his position at the point of change from Newtonian to post-Newtonian thought. With celestial mechanics Fontana concerned

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[n] One *grano ideale* was one-fifth of a carat. Therefore two *grani ideali* meant $2 \times 1/5 \times 1/24 = 1/60$.

[o] 1/360.

[p] John Keill (1671-1712), mathematician who applied Newtonian principles to chemistry. The citation is (probably) to «Phil. Trans.» 1708, 26: 97-110 (not seen). There he laid down «principles, as the foundation of all physics, viz. (1) a vacuum, (2) the divisibility of quantity and infinitum, (3) the attraction of matter... Keill assumed 'another power in matter', but diminishing in a greater ratio than the inverse square of the distance, by which all the other particles of matter mutually attract one another». Partington, *op. cit.* II, 478.

[q] According to Partington, *op. cit.* III, 31 and 448, the names *hidrogéne* and *calorique* were introduced by Guyton de Morveau in 1787 (read 2 May to the Académie des Sciences, published in *Nomenclature Chimique etc.,* Paris, Cuchet, 1787. The second edition of this work, published in 1789, seen by us, cites «calorique», the material of heat, on p. 32, and «hydrogéne, c'est-à-dire engendrant l'eau» on p. 35.

himself little except for the construction of instruments, since he had small interest in astronomy and anyway Newton's application of the universal law of gravitation had swept the Republic of Letters. Many of Newton's works, in several languages, were in the library of 1807 but only the *Principle* is quoted by Fontana, and then only the necessity of observation and experiment; the *Opticks* is *rarely* mentioned by him. In terrestrial mechanics Newton's laws were undoubtedly taught; we have an outline of the course in experimental physics which he gave to the Royal Family.

Memoir[r] from: Various Points for His Majesty the Emperor relating to the Education of the Archduke Francis

H.R.H. the Archduke Francis has taken a course in experimental physics, which has lasted three years. It was sought in the entire course to confirm with experiments the theory of physics, this being presented in the way best to be understood. The course would have been completed if it had been continued for a few more months; for that reason the treatment of fire, light, electricity, and magnetism was barely by allusion to the principal features. The rest of physics can be considered as fully treated, at least the parts susceptible to experimentation, as materials permitted.

The treatises of physics which H.R.H. has studied are the following: the general properties of matter, the laws of motion and of equilibrium, which make up statics; mechanics, and hydrostatics; treatises on air and water, on fire, light, electricity, and magnetism; meteorology was rapidly outlined; of astronomy little or nothing was mentioned.

Felice Fontana

Fontana's interest in machines and instruments was great, as indicated in the descriptions of the Royal Museum, certainly written at least in part by him; and in the brief notes to the «Antologia Romana».[57]

Fontana's more formal activities in physics, especially mechanics, were probably similar to those presented in the works of VANS' GRAVESANDE (1688-1742), whose *Introduction a la Philosophie,* 1 vol.,

[r] This memoir, not in Fontana's script, formed part of a document entitled «Punti fissati con l'imperatore nel febbraio 1784» (Points agreed upon with the Emperor in February 1784), written by Peter Leopold to describe the agreement reached between the brothers, Joseph II and himself, as to the eventual succession to the throne in Vienna of Leopold's first-born son, Francis. The document is: Haus-, Hof-, und Staatsarchiv, Familienarchiv Sammelbände, Kart 15, «Punti diversi di S.M. l'Imperatore relativamente all'educazione dell'Arciduca Francesco», fol. 77r, in the Oesterreiches Staatsarchiv, Vienna.

and *Physices elementa mathematica, experimentis confirmata, sive Introductio ad Philosophiam Newtonianam,* 2 vols., 1720-21; PETRUS VAN MUSSCHENBROEK (1692-1761) whose *Cour de physique,* 3 vols., *Physicae experimentales et geometricae... dissertationes,* 1 vol., 1729, *Tentamina experimentorum,* 1 vol.; and JEAN THÉOPHILE DESAGULIERS (1683-1744) whose *Cours de physigue expérimentelle* were all in the 1807 library; all stressed demontration by experiment.

All Fontana's works, and undoubtedly his thoughts, upheld the empiric approach to which he was temperamentally suited. This is made explicit in his comment on his experiments with charcoal, that «this way of examining forces which the phenomena require, or to represent effects by supposing the causes which accord with them, is somewhat more mathematics than physics and tends to find more laws for effects, than causes». Of course this is pure doctrine of the Newton who wrote: «I have not been able to discover the cause of those properties of gravity from phenomena [...] to us it is enough that gravity does really exist, and acts according to the laws which we have explained». Fontana's position is stated in his work on the elasticity of aeriform fluids, in which he found it interesting that so many fluids observed the same law and thought there must be a physical force in nature to explain the phenomena, as if the caloric and kinetic theories of gases had never received and were not receiving any attention.[58, 59, 60]

When it came to consideration of phenomena on the microscale, to which Newton had clearly thought to extend the principles which he had discovered: «The laws (and properties) of all bodies on which it is possible to institute experiments, are laws (and properties) of all bodies whatsoever»,[61] Fontana parted company with Newton and his followers; he rejected the theory that repulsive and attractive powers were the essence of matter. However, he did at least in later life agree with Newton and his followers such as Keill, although his thoughts on the nature of intermolecular forces do not seem to have been clearly formulated. Nor did he have precise ideas of the nature of heat and light; while he regarded them as material, his late memoirs suggest that experimentally he had found them imponderable.

In physics he seems to have been of the opinion voiced by Peter Gay:[62]

> The irresistible propulsion of modern scientific inquiry was towards positivism, toward the elimination of metaphysics, and the clean separation of facts and values, foreshadowed by Bacon, implied by Newton, triumphantly announced by Hume,

taken for granted by the leading scientists o the late eighteenth century. Scientific thinking exacted the stripping away of theological, metaphysical, esthetic, and ethical admixtures that had been a constituent part of science since the Greeks.

Felice Fontana as Chemist

It has been suggested[63] that: «Perhaps from the beginning Fontana gave no great importance to the arrangement of a chemical laboratory since the Grand Duke had a private one of his own». It is true that the *Saggio* of 1775[64] describing the new Royal Museum stated:

> We will pass over in silence that part of the physics of bodies which is called chemistry, on which the collection has an excellent beginning and good materials, so that it promises to become soon one of the foremost in Europe in this part also.

Fontana may have deferred to some extent to the Grand Duke in not touching certain fields himself, but his early publications in pneumatic chemistry and those from Paris, where he devoted himself chiefly to chemical experimentation, show that he was active in chemical research; in fact one of his most important publications, the «Letter to Murray» of 1782, largely deals with chemical problems.

How good a chemist was he? His works attracted a considerable amount of favorable attention from contemporary souces,[21, 22, 25, 26, 65, 66] but some were unfavorable.[67]

If we compare Fontana and Priestley as chemists, we find that Priestley was not a good chemist, as he admitted himself:[68]

> My knowledge of chymistry is very imperfect [...] The *Facts* appear to me to be rather extraordinary. You must help me to explain them, for I am a bad theorist. I have made many other new observations, but they are chiefly of a chymical nature, and not worth making the subject of a letter.

Schofield[68] concludes:

> Priestley may not have been a chemist at all; no one really knows what a good chemist was, in the eighteenthcentury sense.

Equally Fontana stressed observations rather than theory, but he realized that he had not been a good chemist either; at least he wrote no more papers on chemical subjects after 1786, while Priestley, continuing to write them throughout his life, was looking for a theory, as his late works

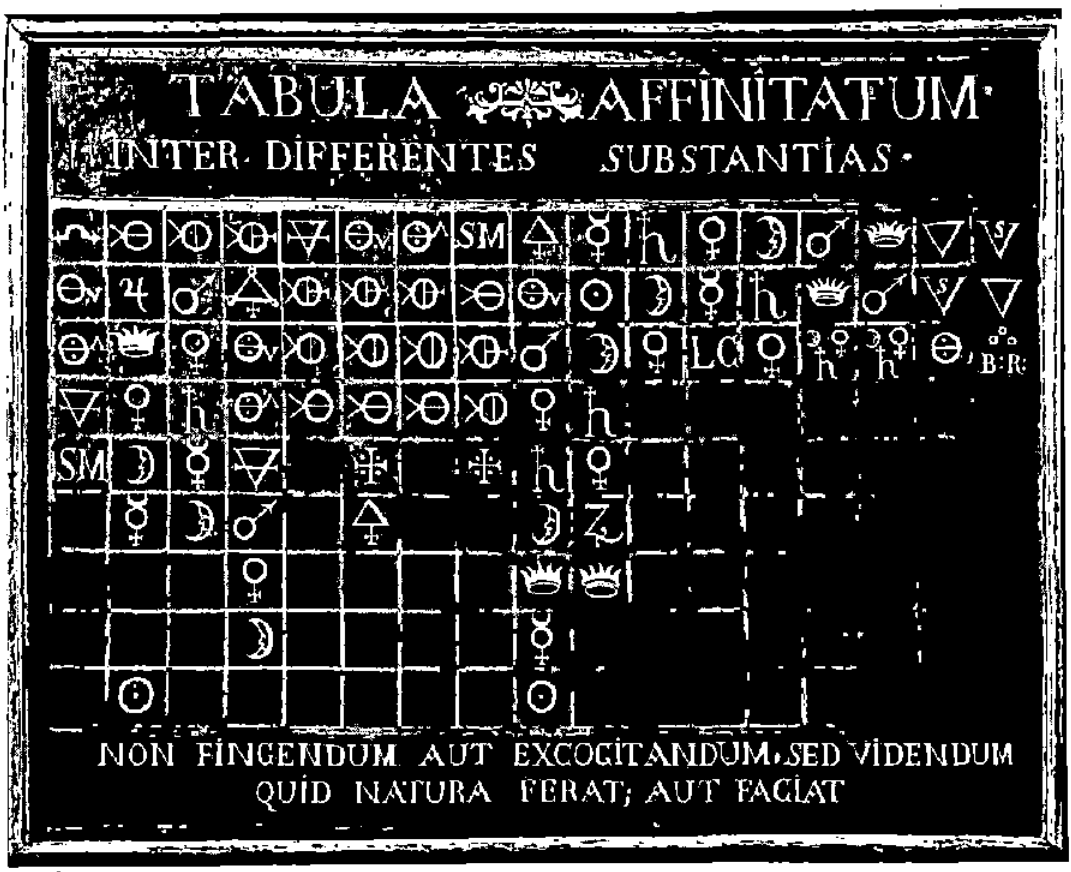

Fig. 23 – 18th-century chart of chemical affinities. In the Institute and Museum of the History of Science of Florence.

showed, but he chose to develop it on the basis of the writings of Newton and Boščovič, which proved to be largely sterile as far as contribution to the development of chemistry is concerned. We have seen that Fontana rejected the purely gravitational approach of Newton and followers; instead he turned to the more empirical approach to chemistry as exemplified by the work of the «Swedish chemists», as he called Bergman and Scheele, whose works were in the 1807 library and were cited frequently. Bergman's *Opuscula Physica et Chemica,* Upsala 1779-, were translated as four volumes in Italian in Florence in 1787-8 and his *Traité des affinités chymiques ou attractions electives* of 1788, the French version of the Latin of 1775, was in the library. The English translation of this work[69] begins with Newton's principle of gravitation, but here in chemistry by contrast, attraction is by contiguity, a tendency of only small particles that «scarce reaches beyond contact, whereas remote attraction extends to the great masses of matter in the immensity of space [...] [and] seems to be regulated by different laws». Bergman distinguished several species of chemical attraction: *attraction of aggregation* where there is only an increase in mass, the nature of the bodies remaining the same, *attraction of composition* (i.e. of solution or fusion, as to whether it is in the wet or dry way); when it takes place among these respectively, to the occlusion of one, it is said to be a *single elective attraction;* when between two compounds, each consisting of only two proximate species, which are exchanged in consequence of mixture, it is entitled *double attraction.* Bergman particularly considered the two last species, with results which he presented in tables in a manner which, as he wrote, stemmed from that of Geoffroy (1672-1731), exhibited first in 1718, but Bergman's tables are much more elaborate, having fifty-nine columns as compared with Geoffroy's sixteen. Fig. 23 shows a table of affinities believed to have been prepared in the Royal Museum in Fontana's time.[70] It is actually identical with that of Geoffroy except for the last column.

Here Fontana followed the example of Bergman rather than that of Lavoisier who wrote in his elementary treatise (with Guyton de Morveau, an Italian translation of 1792 being in the 1807 library):

> I do not permit myself to include in this work the part of chemistry most apt, perhaps, to become an exact science some day; that is, the one which treats of chemical affinity, or elective attraction. Geoffroy and many others have already combined a multitude of specific facts which are ready to be placed but the principal data are lacking, or at least those which we have are not yet precise enough, nor certain enough to form the fundamental basis on which such an important part of chemistry must be raised.

Phlogiston

Fontana had chosen the path suitable to his temperament; the path of empiricism. In theory he strayed, as also did Priestley, Bergman, Scheele, and many others, in the direction of using, to explain phenomena not otherwise explicable, that fugitive thing phlogiston, for which purpose it was well-suited.

What was phlogiston? Since it differed for nearly all those who wrote about it, that is difficult to answer; although it has been written about at great length,[71,72] every aspect is to some degree controversial. The name was applied by Stahl[s] to Becher's «oily earth» *(terra pin guis)* which was the principle of inflammability, expelled on combustion. It was not fire itself, but the true motivating agent of fire; when uncombined it was nothing like fire but volatilized in invisible particles, whence it produced simply heat which is an invisible and much divided fire. It possessed a subtlety and expansion which rendered it unrecognizable to the senses to such an extent that no means served to detect, attract, or collect it. For Stahl it was not a material substance.

Later it underwent an infinity of changes. In 1777 Scheele wrote «since phlogiston is a substance (which always supposes some weight)». It was repeatedly shown that metals on being calcined increase in weight but supposedly lose phlogiston, that a calx is converted to the metal (losing weight) supposedly by gaining phlogiston; then phlogiston must have the property of levity; i.e. relative levity as when tying corks to a body may make it float in water, or absolute levity, i.e. a negative weight.

This theory, in various metamorphoses, was accepted by the giants of eighteenth-century science such as Priestley, Bergman, Scheele and Cavendish; Macquer never abandoned it although he felt its end was near; Joseph Black never had faith in it and ultimately abandoned it; Kirwan strongly supported it, later gave it up. It was finally dealt the

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[s] Georg Ernst Stahl (1660-1734), court physician to the Duke of Saxe- Weimar and one of the outstanding chemists of the eighteenth century. He kept his chemistry and medicine entirely separate, chemistry being dangerous to medicine, throwing no light on the vital functions. According to Partington (*op. cit.* II, 654) he was «proud, morose, atrabilious, a Pietist [...] he showed contempt for all who differed from his views and reacted violently to criticism. These qualities [...] greatly enhanced his reputation [...] he studiously avoided every form of expression and thought which he did not believe to be peculiar to himself [...] he regarded his ideas at least in part due to divine inspiration and the common herd could have no inkling of them».

death-blow by Lavoisier, although it died a lingering death partly because he kept it in his new theory under the disguise of the name caloric.

Its persistence has been excused on the basis that it, and only it (whatever it was), provided an explanation for many observations not otherwise explicable. The adherence to it on the part of Fontana and many others has been justified[73] with the claim that it had heuristic value. It may be preferable to have any theory rather than no theory, but the evidence is that the theory of phlogiston was a hindrance to progress in understanding, that it was not a stimulus to research, that chemistry advanced in spite of it, and that the truth was not appreciated until it had been swept away with the Aristotelian elements.[71, 74]

It is plain to see, from Fontana's papers on respiration particularly, that he achieved nothing of value by invoking phlogiston. It is plain from his paper on Light, Flame, Heat, and Phlogiston that he found little inspiration in the last, yet he never openly rejected it, simply stopped mentioning it.

CHAPTER 14

ANIMAL PHYSICS

Epistolary Dissertation - The Laws of Irritability - Animal Physics - Animal Respiration - Breathing Hydrogen - Breathing Oxygen - Movements of the Iris - Visual Accommodation - Apologetic Letter - Felice Fontana as Physiologist

In the 18th century the term physics was comprehensive, including what would now be called physics, as well as chemistry, and biology. It signified «the science of nature». While Fontana's appointment in Florence was that of Physicist to the Court, we would say rather, Scientist. The work he projected on the grandest scale, although never completed, was called Animal Physics, and his earliest printed work was on physiology, entitled simply: *Epistolary Dissertation.*[1] It is dated Bologna, 23 May 1757. Nominally addressed to the Reverend Father Urbain Tosetti, reader in philosophy and mathematics at the Nazarene College in Rome, a fellow Hallerian, the first line addresses the real recipient, Haller, as Reverend Father. It deals with the claims of Tommaso Laghi[a] in a highly polemic tone but with much experimental data and with arguments that Haller apparently found convincing,

[a] Tommaso Laghi (1709-1764) professor of medicine at Bologna. See: Vincenzo Busacchi, *Atti e Memorie dell'Accademia di Storia dell'Arte Sanitaria,* in: «Rassegna di Clin. Ter. e Sci. Aff.», Anno XXXIV, Fasc. I, 1935. Laghi was virulently antihallerian. Haller wrote of him (9 February 1759): «I have felt in this Laghi a spirit more malignant than I would have believed in». About vol. III of his Mémoires in which this dissertation[1] appeared, Haller wrote: «I do not believe there will be a reply» Laghi later (1762) attempted to ingratiate himself with Haller.

although Fontana himself was dissatisfied since he returned later to take up the same problems.

Of the three chief points discussed, the first is that of sensibility. Fontana adduced many experiments in support of his conclusions, but his position is weakened by two considerations. The subjective nature of the experiments is shown by Haller's description of the procedure.[2]

> I took living animals of different kind, and different ages and after laying bare that part which I wanted to examine [...] I examined attentively, whether upon touching, cutting, burning, or lacerating the part, the animal seemed disquieted, made a noise, struggled, or pulled back the wounded limb, if the part were convulsed, or if nothing of all this happened.

Fontana added:[3]

> experiments [may be] conclusive, provided that they are subject to necessary precautions [...] It often happens that animals give loud cries, even before the skin is touched, or when they are fastened to the table. Others, irritated by the incision they have suffered, never cease to lament.

The other objection is the impression one receives of a pre-conceived idea being followed. Both pro-and anti-Hallerians[4] believed that the presence of nerves in a structure was necessary for it to be sensible. Accordingly, the anti-Hallerians found nerves in those structures they claimed were sensible; in these same structures, which the pro-Hallerians claimed to be insensible, *they* found no nerves.[b]

The second point on which Fontana attacked Laghi was the latter's identification of «animal spirits» with electricity. Ever since Galen (131-201), the prevailing theory of muscular contraction was that it was caused by the animal spirits which were formed in the brain from vital spirits and then traveled down the nerves. Now, attention had been called to the effect of electricity on the human body by experiments with the Leyden jar, such as those of the Abbé Nollet.[c] This instrument

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[b] On Italy *pro* and *contra* Haller, see G. Ongaro.[4]

[c] Jean-Antoine Nollet, 1700-1770, the «savant à la mode» of the 18th century. Recognizing his ability, Louis XV appointed him to the chair of experimental physics at the College of Navarre. Member of the Académie des Sciences and the Royal Society, he published *Leçons de physique expérimentale,* Paris 1743-64, and *Programme d'un cours de physique expérimentale...,* Paris 1738.

permitted the use of far stronger shocks than had been possible with the older electrical machines. The Abbé discharged one of them through 180 of the King's guards at Versailles in the presence of the King, who was greatly amused. Inflamed by the success of this experiment, Nollet performed it on the entire membership of a Carthusian monastery, who, holding hands, formed a line just over a mile in length, and upon discharge of the jar all gave a sudden spring at the same instant, and felt the shock equally.[5]

Laghi was not the first or the only one to make this identification, but Haller had briefly rejected it,[6] and Caldani and Fontana took up the cudgels. Fontana's first criticism was adapted from Beccaria's[d] theory of electricity, which required that its effects be produced by flow from a body in which it is in a greater quantity into one in which there is less. Thus Fontana:[7]

> the electrical substance must be in the nerves and muscles in an inequal proportion, since no movement will result if the quantity is equal; that appears to be the case since nerve and muscle are electrifiable by communication [that is, they are conductors] [...] Even supposing that the electrical fluid, produced by the animal spirits, runs along the nerve, how can it be that only one muscle moves at will, and that many muscles do not contract at the same time? How to prevent the electrical vapor from expanding into all the muscles besides the nerve, nearer the brain?

He wrote that one might propose an answer: that the animal spirits are embedded in a resin, but that the phenomenon of electrical stimulation shows only that electricity is a powerful stimulus to muscle or nerve and does not prove that muscle normally contracts because of electricity:

> One must take care not to esteem beyond the truth this superior force of electrical stimulation, not to conclude that electricity is the cause of the movement in a muscle which it excites. A muscle which no stimulation can make move has not lost for all that its irritability. One need only divide it into bits; each will be irritable at least for a while. That persuades me that the superior exciting force of electricity resides only in the facility with which the spark penetrates all the muscle, and evokes the irritability of the innermost fibers, which other stimuli would not have disturbed. As of now one could not draw a conclusion on the identity of the electrical substance and the animal spirits [...] one could reach that only on

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[d] Giovanni Battista (or Giambattista) Beccaria (1716-1781), professor of physics at Turin from 1748. Published *Dell'elettricismo artificiale e naturale,* Torino 1753, corresponded on the subject with Franklin, Priestley and others. Not to be confused with J.B. Beccari (1682-1766) of Bologna.

demonstrating that the irritability of the [muscle] fiber depends on the electric substance and that this substance is the effective cause of muscular contraction: a demonstration that no one has ever offered, that no one will ever try to offer, perhaps.

Subsequently, in the 1770s, as a result of study of the torpedo fish, the ability of that animal to deliver a shock was appreciated as evidence for the existence of animal electricity. In 1781, Fontana reconsidered:[8]

The pretended very great velocity of the nervous fluid seems to be contradicted by that inert viscous fluid or matter with which the primitive nervous cylinders appear filled.[e] [...] In a word, we are not only ignorant of muscular motion, but we cannot even imagine any way to explain it, and we shall apparently be driven to have recourse to some other principle; that principle, if it be not common electricity, may be something, however, very analogous to it. The electrical gymnotus and torpedo, if they do not render the thing very probable, make it at least possible, and this principle may be believed to follow the most common laws of electricity. It may likewise be more modified in the nerves than in the torpedo or gymnotus. The nerves should be the organs destined to conduct the fluid, and perhaps also to excite it, but there everything yet remains to be done. We must first assure ourselves by certain experiments, whether there is really an electrical principle in the contracting muscles; we must determine the laws that this fluid observes in the human body; and after all it will yet remain to be known what it is that excites this principle, and how it is excited. How many things are left in an uncertain state, to posterity.

His final statement on the role of electricity in the excitations of skeletal muscle by nerve followed the demonstration of Galvani. Fontana wrote in 1792:[9]

Shortly there will appear my work on the *new principle of muscular movement* discovered in Bologna by the clever Professor Galvani where I show that one cannot reasonably doubt that electricity has nothing to do with this new principle which, whatever it might be, never causes ordinary natural muscular movement in the animal. Thus this obscure principle is reduced to a beautiful phenomenon whose uses and nature remain to be determined in the future.

This work never appeared, but Fontana must have been aware of the dissertation[10] of a great admirer of his, Francesco Chiarenti, who

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[e] The existence of these primitive cylinders and their contents had first been demonstrated by Fontana. See p. 234-6.

concluded that in the nerves exists the cause of motion of the muscles, that the so-called nervous fluid does not reside continuously in a free state in the nerves, but only when they are irritated, that then it flows, draining from the orifices of the nerves, reaching the muscles, that this fluid was contained within the corpuscles that Fontana had described in nerve fibers.

THE LAWS OF IRRITABILITY. Fontana published two works with this title. They are identical except that the second[11] added three more laws to the two of the first.[12] Both have a section on the inefficacy of the animal spirits in causing muscular contraction. Both are dated Florence, 28 October 1765; both are dedicated to the physician of the Grand Duke Peter Leopold, Lagusius, who had Greco-Latinized his name of Hasenöhrl on the recommendation of Van Swieten, physician to the Court in Vienna. These articles have been translated into English[13] with an accompanying essay on Fontana's life and the significance of his work.

The five laws of irritability given by Fontana are:

I. For each contraction of the [muscle] fiber, a new stimulus is always necessary to awaken its irritability.
II. Irritability is not always constant, but only after a time returns to the muscle, according to its nature and state.
III. Muscle a long time contracted loses its irritability.
IV. Muscle stretched or compressed for a long time loses its irritability.
V. Muscle which remains relaxed for a long time loses its irritability.

After elaborating these points, Fontana proceeded to show why the animal spirits could not be the effecting cause of muscular movement. In his dedication he wrote: «The reward of my effort on this problem has been to read the animal spirits out of office forever...» However, they do seem to creep back into discussion; they were accustomed to being driven out. Steno (Niels Stensen) (1638-1686) had written[14] in 1667:

> Many people talk of animal spirits, the more subtle part of the blood, the juice of the nerves – but these are mere words signifying nothing.

Francis Glisson (1597-1677) wrote[14] in 1677:

> I come to the second important reason proving animal spirits not to be the immediate cause of sense and movement [...] Therefore one must conclude that the

fibers are shortened by their own vital motion, and have no need of plentiful afflux of animal or vital spirits.

These matters received more attention in Felice's *Animal Physics* which appeared in 1775.[15]

Animal Physics. With permission which he had requested,[16] he dedicated this book to Haller. It repeats the previously presented laws of irritability, etc., and adds a great deal. In 1785,[17] a translation into German of selected passages from this book was published, which contributed to its influence through being used as a textbook by students of medicine.[18] Many of Hebenstreit's excerpts, and passages from the *Epistolary Dissertation* have been elegantly translated by H.E. Hoff.[19]

A question which confronted Fontana after the statement of his laws was, how can there be a sustained contraction of muscles as in a voluntary effort? He answered by referring to his observation that in a muscle contracting a long time he saw «a constant twitching alternating with tiny and fleeting relaxations». Thus, «its irritability in every instant is renewed and exhausted».

Another problem relating to irritability arose from the beat of the heart. Here Whytt[f] pointed out its relaxation in diastole despite the continued presence in its chambers of blood, which according to Haller was the stimulus to contraction. Fontana replied by distinguishing between «exciting» and «effective» causes. He dispensed with the Aristotelian «final cause», drawing this comment from Voltaire (letter to Monsieur Aubert, 1776[20]):

> It is true that the old invalid believes a little in final causes, but Cicero and Spinoza believed too. Monsieur l'Abbé Fontana can believe what he wishes, but he must have some indulgence for final causes, and wait like the rest of us. We greatly respect causes which are final, even those which are not. I am in my bed, I am born to suffer, that is my final cause. My nature is to pardon those who make me ridiculous with caricatures, even to love them. My nature is to love philosophers whatever nature they have.

Continuing his argument, Fontana turned to his analogy:

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[f] Robert Whytt (1714-1766), Edinburgh neurologist, whose thoughts on the circulation appeared in *Physiological Essays*, Edinburgh, 1755. A French translation (Paris 1759) was in Fontana's library.

The contractile energy of the entire muscle can surpass that of the stimulus. It is thus that a tiny spark ignites a great mass of gunpowder, the energy of which is prodigious. This spark could hardly move a pebble, while the air imprisoned in an infinity of grains of powder in developing its elastic power, upsets boulders. The spark is not the cause of this enormous effort, which greatly exceeds it in force, it is only the exciting cause, which liberates in the powder the energy of an agent which is enclosed within it.

The needle that pricks the heart does what the spark does: it excites only a single fiber, a supposition I wish to admit, although it might affect a thousand, but it forces the entire muscle into a complete systole: because the pricked fiber in contracting stretches and sets in motion other neighboring fibers, and in this manner unites in a single contraction the maximum effort of all fibers insofar as they are capable by virtue of their irritable nature.

This passage bears a notable resemblance to Bowditch's statement of the «all-or-none principle» in 1871.

Performing experiments controverting the claim that the beat of the heart was due to animal spirits arriving by way of the nerves, Fontana stimulated the spinal cord and the «cardiac nerves». It is generally supposed that by the last is meant the vagus nerves. He reported that the irritation of the nerves to the heart neither accelerated nor restored its movements. This failure to observe an effect may have resulted from the stimulus that he gave to these iterative nerves not being repetitive. He concluded that: «there is in the heart another cause of movement than in voluntary muscles».

He continued:

One has therefore no alternative but to explain the fact that the heart relaxes in spite of constant stimulation according to the basic principles of animal irritability established in Part I. The longer and the stronger a muscle remains contracted in voluntary motion, the more it loses its own irritability, and relaxes finally involuntarily, although we attempt to increase its supply of nerve fluid.

External stimuli evoke no response in the contracted or just relaxing muscle fibers, because the heart is not then irritable, as it was a moment before, and it has not yet reached the stage of irritability which is necessary for a new response.

Hoff[19] concluded from Fontana's writings that

1. The refractory period of the heart was discovered by Felice Fontana.
2. Fontana developed the general doctrine that rhythmic activity in the heart and many other organs depends on the existence of the refractory period.

Animal Respiration. Fontana's first paper on this subject,[21] which combined observations in physiology and pharmacology and arose from

his interest in the airs, was read at the Royal Society in London on 11 March 1779. It resolved the disagreement between Priestley and Scheele on the poisonous properties of inflammable air (hydrogen), recently prepared and described by Cavendish. According to Priestley this air caused the death of animals breathing it as readily as did fixed air (carbon dioxide), although he added the remark, as he did with all his newly discovered airs, that it became as respirable as common air after being shaken with water. Scheele claimed that inflammable air was good and innocuous to breathe, even by himself.

Fontana, supposing that they both might be right but that some circumstance not attended to might explain the contradiction, collected inflammable air, made from zinc or iron and sulfuric acid, over mercury and found that birds introduced into it succumbed in a few minutes, even if the air had been passed through water or had been shaken with it, although long shaking did appear to decompose it so that it became in some measure respirable and less inflammable.

Two possible modes of this toxic action, namely action on the nose or the skin of the body, were ruled out by forcing animals, including quadrupeds, to breathe it through the mouth or directly into the trachea, without exposure of the skin. He then proceeded to breathe it himself, in measured amounts collected in bladders, controlling the progressive change in nature of the air with his evaerometer (see p. 170). When he rebreathed a small volume, say 80 cu.in., he could breathe it 34 times with impunity, although it was plain that this air was worse than common air. However, when 250 cu.in. was breathed, even the second inspiration was oppressive. Then when he began to breathe from a larger volume after a violent expiration, he lost consciousness.

Fontana drew the correct conclusion in explaining that a small volume of inflammable air is diluted with the residual air[8] from the lungs which, although partially phlogisticated, is still diminished by nitrous air (i.e., contains oxygen), while a large volume of inflammable air rebreathed is less so diluted. Thus inflammable air may be breathed with impunity although it is not such a kind of air as can by itself alone be directly useful for respiration. It might be said to be useful to give the lungs their necessary expansion, in that it can be breathed for a longer

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[8] The existence of pulmonary residual air had been recognized by Giovanni Alfonso Borelli (1606-1679) in *De Motu Animalium,* Rome, 1680-81, Vol. II, Ch. VII, Prop. 94.

time than can expired air. This last observation may be correct but the explanation is not; expired air becomes unbearable because it contains carbon dioxide, while hydrogen becomes irrespirable through anoxia.

He felt a sensation of levity and facility of breathing with hydrogen, which he explained as due to the greater levity of hydrogen as compared with common air.

Fontana next wrote[22] on animal respiration in the «Letter to Murray», dated 20 October 1781, which was elicited by the *Opuscoli Scientifici* of Bergman, to whom along with Scheele he refers as the Swedish chemists. Bergman had proved, Fontana says, with experiments on animals in a closed space and with blood, that the lung does not phlogisticate the inspired air but on the contrary removes phlogiston from it.

Bergman's first proof was that the air breathed by animals in a closed space was not diminished in volume; as everyone knows, phlogiston decreases respirable air, so the animals did not contribute phlogiston. To this argument, Fontana opposed the results of experiments, 37 on mice, 452 on birds, 179 on small guinea-pigs and rabbits in common air; when they were dead, the volume of the air had decreased by 1/30-1/23: the longer the animals lived the more the volume was decreased; in dephlogisticated air (oxygen) the decrease was even greater. He stated that the decrease would have been even greater if the experiments had been done over water, which would have absorbed the fixed air which was in amount greater than the observed decrease in volume over mercury; no figures are given. How he meant this to show that the animals contributed phlogiston from their lungs is not clear, unless the action of phlogiston to decrease the volume is accepted.

Bergman's proof regarded as most direct was that air on being shaken with blood did not decrease in volume, but was vitiated since a candle would not burn in it; thus the air had lost its phlogiston to the blood. Fontana now related his experiments with common and dephlogisticated airs over mercury, shaken with blood (which ran in torrents from a castrated ram) in an apparatus all warmed to body temperature. The remaining air was examined with his evaerometer (eudiometer in the French version). It was vitiated and decreased in volume when measured over water. The experiment was repeated with measurement of the volume of air first over mercury (slight increase in volume) then over water (decrease in volume), showing that contact with blood tends to decrease the volume of air and its goodness, and to increase its volume by adding fixed air. He said that these results were new and unexpected,

though conforming to those obtained in London in 1778 and 1779, but that this was not the place to go into details, which he reserved for a treatise on animal respiration.

This section of the work continues with a polemic against Landriani who concluded that the fixed air issuing from the lungs had been formed from the phlogiston. Fontana reaffirmed his beliefs that the blood furnished fixed air, regretting that he could not measure the contribution by the whole animal, and that blood vitiated air by phlogisticating it; he also repeated and confirmed his previous experiments.

The section finishes with descriptions of many more experiments on himself with inflammable air and phlogisticated air, restating his conviction that these are innocuous as such although they do not support life.

Fontana's «Letter to the Duc de Chaulnes»[23] was brought about by comment of Pilatre de Rozier on the experiments with inflammable air. Fontana recommended that his papers be consulted, briefly citing their contents. He then gave a summary of his experiments and opinions on animal respiration: the manuscript from which this was drawn was one entitled «Science de l'Air». In the first section on airs in general are five pages on respiration in which it is stated:

> savants in different times and places have conceived and supported quite diverse, even opposed [hypotheses]. The first, and perhaps the most ancient, was that air must contain some unknown principle or other which is quite essential even indispensable to the support of life, for that reason called *pabulum vite* [sic]. That was however only a name which designated a very obscure thing conceived to explain a phenomenon even more obscure.

Dismissing this hypothesis which it would be useless to stop and oppose, and others such as the elasticity of the air being essential, also some frivolous proposals, he proceeded to his essay.

Through twenty-four paragraphs, beginning with the truth, accepted as demonstrable, that atmospheric air is an indispensable necessity for animal life, he considered previous hypotheses and adduced new evidence. Fontana pointed out correctly that there were two different kinds of airs other than atmospheric, one consisted of *inflammable air* (hydrogen) and *phlogisticated air* (nitrogen) which would not support life but are themselves innocent of any noxious principle; the other kind included *fixed air* (carbon dioxide) and other airs, noxious because they attack and derange the vital organs. He described what we would call the anesthetic action of carbon dioxide. Fontana dismissed as unfounded the

hypothesis of a vital principle in atmospheric air, and concluded that the function of respiration was to strip the blood of phlogiston. Why an excess of phlogiston might kill an animal he confessed he did not know, but his opinion was that it diminished muscular irritability.

Perhaps the most noted contribution of Fontana to respiratory matters was published not by him but by his friend and collaborator Ingenhousz[24] as follows:

> «When this book was entirely printed, and nothing but the latter end of the preface was unfinished, I was informed by my friend the Abbé Fontana that he discovered a few days ago a new method of procuring to a sick person the benefit of breathing any quantity of dephlogisticated air at a cheap rate [...] Abbé Fontana found that an animal breathing in either common or dephlogisticated air renders it unfit for respiration by communicating to it a considerable portion of fixed air, which is generated in our body, and thrown out by the lungs as excrementitious. This fixed air is easily absorbed by shaking it with common water, but infinitely more readily by the contact with quick-lime water. He fills one of the large receivers of an air-pump, which are very wide at the upper extremity half-full of dephlogisticated air extracted from nitre, so that it may contain about 500 cubic inches of this air, which will serve for breathing during half an hour. The manner of drawing this air of the receiver is by thrusting a bended glass tube under the receiver (when this is floating in water, in which it is supported by its peculiar bulky form), reaching into the air itself, and keeping the other extremity in the mouth, thus drawing this air into the lungs, and breathing it out in the same tube. This air returning from the lungs is infected with fixed air, which being immediately absorbed by the contact with the lime-water, the dephlogisticated air is restored very near to its former purity.
>
> We consume, by each inspiration, about 30 cubic inches of air; and thus, allowing 15 respirations for a minute, we consume each minute 450 cubic inches of air. The Abbé Fontana found that the dephlogisticated air being, after each respiration, purified again by the lime-water, will remain good about thirty times as long as it would when breathed in the ordinary way; and that thus the quantity of dephlogisticated air necessary for one minute will now serve for breathing during half an hour, and thus the expenses will be thirty times less.

Ingenhousz calculated the expense of this oxygen therapy for 12 hours to be less than one shilling! He proposed[25] its trial with sick people, but nothing seems to have come of it.

Priestley had already written to suggest the use of oxygen in medicine for diseases of the lungs; «hitherto only two mice and myself have had the privilege of breathing it».

Experiments on respiration of a different sort were made by Fontana in 1780. We know of these from some notes in his own hand,[26] which bear at the end attestation of Valerio Gioson Fontana, son of

Felice's brother Francesco, made 29 April 1824. Here is one note dated 11 April 1780.

> Four medium-sized salamanders were put into a vessel in which there were five cubic inches of air and twenty cubic inches of water over mercury. At the beginning the salamanders went here and there in the vessel, but more often they stayed at the bottom. After some time it appeared that they remained at the top where the air was, but after three hours one saw that they could no longer descend by any effort they could make, that the water pushed them up. Having examined them closely I found that they had become larger than before, much swollen. In three hours the air had been reduced to one-half; in a few more hours it was diminished a little more. One salamander was at the bottom of the water, dead; the others were at the top, alive, and could not descend to or stay at the bottom. The air on being shaken with water was not sensibly decreased, with nitrous air it was diminished nearly not at all, and it extinguished a light.

This experiment with salamanders was repeated with the same results. The same experiments were done with lemons, a water plant *(scopa acquajola),* apples, artichokes, asparagus, and potatoes, with results similar to those obtained with the salamanders. Experiments with gum of opoponax, powdered storax, oils of beaver and musk, and almond lotion gave negative results. As a test for carbon dioxide, in addition to shaking with water, Fontana used frequently the crystallization of potassium bicarbonate from «oil of tartar» (deliquescent potassium carbonate).

Although others had placed animals in closed chambers before, and observed, for instance, that at the time of death of the animal, the air would extinguish a flame, these experiments of Fontana's, with their demonstration of the disappearance of the salubrity of the air (oxygen) by means of his eudiometer and the production of carbon dioxide, constitute an advance. Why he did not pursue them further we do not know.

Some years later Spallanzani was to carry out similar experiments. These were his «swan song», and were published only posthumously.[27] According to a letter[28] to Senebier of 16 February 1797, he had begun to think about studying respiration two years before. In all there were five volumes printed on the subject, including more than seventeen monographs on the respiration of plants and many animal species, in a system resembling that Fontana's, with the use of eudiometers of Fontana or one employing hot phosphorous as the reagent, and in some cases using lime water to absorb the fixed air. Spallanzani's studies were incomparably more extensive than those of Fontana, and his writing has a more modern sound, as he speaks of oxygen, carbon dioxide, and *gaz azotico*

(nitrogen). He explained that «the luminous theory of Lavoisier (and followers) has served me as convoy».

Movements of the Iris. The point of departure of this work[29] was the existence of the iris, a body which moved in response to light but not to being irritated, behavior quite different from that more familiar motile tissue, skeletal muscle.

The lack of response of the iris to mechanical or chemical stimuli had been demonstrated by Haller (*Dissertation sur la Sensibilité,* 1755) but it had been pointed out that proof of the inefficacy of light as stimulus was lacking.

Fontana confirmed Haller's observations that needle pricks, and, as he also found, electric sparks, delivered to the iris did not elicit contraction. The remainder of his book is devoted to (1) why iris contracts, and (2) how it contracts. He first prepared devices which permitted him to illuminate only the iris or only the interior of the eye. Movement of the iris occurred, in the second case, not in the first, so he concluded that the movement resulted from light falling on the retina. His conclusions were applied to observations made on disease of the visual system, as was his practice throughout this work.

In his second chapter, on why the pupil changes its size in varying degrees of illumination, he pointed out that no fibers of the optic nerve go to the iris; that Mariotte's hypothesis that the iris, being part of the choroid, moves because the choroid is the light-sensitive part of the organ, was false; that Morgagni's supposition of transmission of mechanical vibrations from optic nerve to iris had already been disposed of by Haller.

Now he wished to know the natural state of the iris. That, he concluded, was the form which it took in sleep, when, as he saw in his cat and in an 18-month-old infant, the pupil is greatly constricted when the iris is distended.

Proceeding to his third chapter, on why the pupil changes its size, on why it is smaller when the retina is illuminated, he concluded that movements of the iris were voluntary. This conclusion was drawn chiefly from observations again on his faithful cat; for instance sudden application of a bright light caused evident pain and struggling, and constriction of the pupil. Later, the cat tranquil in the same illumination, the pupil was normal in size. In other ways, the size of the pupil was noted to be quite independent of the amount of light, particularly when the cat was attempting to see small objects clearly. Thus the true

argument is that the movements of the iris are voluntary, a result of the animal's sensation of vision, or what is called the sentient principle.

To dispel all shadow of doubt, Fontana wished to compare the movements of the iris in the two eyes. In his cat and in himself, he saw that varying the illumination of only one eye produced similar changes in both pupils. Thus it is not the light; the two eyes are not connected with each other; it is an internal influence which governs both eyes; this is purely the will.

As he now said, it is not enough to demonstrate the eternal verities, one must resolve the difficulties which might with some appearance of justice be raised. He was led to write that, since infancy, one makes a habit of changing the size of the pupil in order to see better; that suffices without knowing the physics involved. Are such movements, though habitual, any the less voluntary? In pursuing his argument, he ended by maintaining that breathing and sneezing are also voluntary acts. We shall be content with his general statement:

> I use these words, *free movement, voluntary movement, sentient principle,* to conform to usage, and I mean by these words nothing other than that arousal of a sensation in the brain precedes muscular movement; I leave to others the trouble of determining by lofty researches the exact meaning of those words; it bothers me very little what interpretation might be given them, on condition that it will always be true that the pupils change as by the laws indicated, that the natural phenomenon is indisputable.

Chapter V deals briefly with how the iris moves. The existence of a cirular and a radial muscle, decribed by Ruysch and affirmed by Winslow, had been denied by Haller, Zinn,[h] Ferrein,[i] Morgagni, and Fontana himself. Haller had proposed as explantion of the movement an afflux of humors. With some qualifications, this was accepted by Fontana, but there is a strong suspicion of adherence to a principle, that is: movement is most characteristic of muscle, which is irritable – it moves on being stimulated (mechanically, chemically, electrically, or by light) – therefore the iris is not a muscle.

The Italian version of this work appeared in 1765, but even 10 years later Fontana was not satisfied with it, as can be seen from his

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[h] Johann Gottfried Zinn (1727-1759) was a pupil of Haller at Göttingen, where after 1753 he was Professor of Medicine.

[i] Antoine Ferrein (1693-1769) carried out anatomical research while professor of medicine and surgery in Paris.

letters to Caldani.[30,41] However, he permitted a closely equivalent French version to be printed in 1777. In these letters of 12 August and 2 September 1775 Fontana repeated the same arguments, cited the same experiments, and reaffirmed their findings. He proposed that they collaborate in writing on these problems, in the form of letters which would be inserted in the second volume of his *Animal Physics,* but nothing came of this. These letters seem to suggest that the hypothesis of the afflux of humors was regarded by Fontana as Caldani's rather that Haller's. Fontana was prepared to accept it but not very enthusiastically. His language throughout is that of the book of 1765.

Fontana did not write again on this subject until a year before his death, when he sent two letters to Scarpa[31] in which he wrote that he was of the same opinion still, that movement of the iris was voluntary, so also respiration and sneezing, to which he now added coughing, vomiting, hiccoughs and convulsions, control of the heart rate, and movement of the ears. For long he had sought Bacon's crucial experiment as direct and convincing proof. He now reported that, in himself, he could at will change the size of his pupils, his pulse rate – and move his ears! These phenomena he had demonstrated to Tompson, celebrated English anatomist and naturalist then living in Naples, Professor Tommasini, physiologist, of Parma, and his colleague Mascagni, and he invited Scarpa to come and see for himself.

Many of the explanations of the movements of the iris proposed in antiquity are presented by Caverni.[32] For example, Fabricius *(De oculo)* was persuaded by lack of muscles that the motion of the pupil was not voluntary but caused in another way, by affluent humors, as Galen taught, which would produce opposing effects; the constriction and dilation of the iris resembles the systole and diastole of the heart, or better the flaccidity and turgescence of the *corpora cavernosa.*[i]

The eighteenth-century discussion of the anatomy and physiology of the iris, with the observations made and theories developed, with resulting controversy and changing opinion, is thoroughly presented by Mazzolini.[42]

Visual Accomodation. Only in an unpublished fragment[33] do we have Fontana's thoughts on this subject. All of the points considered there may be found in Haller's *Elementa physiologiae* of 1769.[34] Since Fontana

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i This might be termed the cockeyed theory.

certainly knew that work he may have used it in preparing these notes; in a letter, however, to his brother Giuseppe in 1764[35] he mentioned having nearly finished an «operetta» on the internal and external movements of the eye.

Lettera Apologetica. There is little doubt that this work[36] was written by Fontana, although he speaks of himself in the third person, as «Our Author». Its tone is highly polemic throughout; it deals with six points.

The first is a rather futile discussion of whether the hearts of cold-blooded animals shorten or lengthen in systole, or really whether the apex of the ventricle approaches or recedes from the base. The description of the arrangement of the muscle fibers in the heart had been given clearly, with the statement: «Hence these fibers not only compress and diminish the intraventricular cavity, whenever they contract on both sides, but they also bring the apex nearer the base [..]» in the book by Richard Lower, *Tractatus de Corde,* 1669, which was in the library of the Royal Museum.

The second point was whether or not the heart (the ventricles are meant) empties itself of blood in systole. Haller had written that it did, Fontana states correctly that it does not. He also wrote that he had given for the first time the correct explanation of the observation that the hearts of cold-blooded animals show an increased red color in diastole, which is that the walls are transparent and the red color is that of the blood.

The third point was whether or not any blood returns from the arterial system to the left ventricle in diastole, Haller having concluded no because it was not reasonable. Fontana pointed out that it did, some from the coronary vessels and some from around the semilunar valve.

On the fourth point, whether the heart is the most irritable of muscles, Haller had said yes, since its nerves are exposed continuously to the irritating action of the blood. Fontana denied this on the basis that nerves of the heart are not concerned in the maintenance of the motion of the heart or its excitation. It is true that the heart does not tire, he said, but neither do the diaphragm nor the intestines.

The fifth point maintained that Hallerian irritability had not been effectively defended by the Hallerians against the criticism of de Haen, particularly in connection with the phenomenon of the heart relaxing event though it had been given an added external stimulus. This was explained by Our Author by means of the laws of irritability which he discovered.

Point six moves to the attack, exclaiming how many errors would have been avoided in the new Physiology of Mr. Haller if he had known better the laws of irritability as set forth by Our Author. Then he would not have cited as authority what Sig. Caldani with Our Author had published in Bologna on Hallerian sensibility and irritability,[k] when neither one knew then what Our Author had discovered later in Tuscany.

The work then closes with a paean of self-praise.

Felice Fontana as Physiologist

Fontana's writings on the peripheral neuromuscular systems of skeletal and cardiac muscle are amazingly percipient and could well be used by today's students. In his *Animal Physics* he finally emancipated himself from Hallerian concepts and saw matters for himself. He was close to today's conception of the manner in which nerve causes excitation of muscle. As for the problem of the nature of sustained voluntary muscular contraction, whether or not there is some kind of «rotation» of activity, it is not settled yet.[37] However Fontana seems strangely uninterested in the reflex nature of muscular contraction and bodily movement. Delineation of that phenomenon had to await an understanding of the anatomical structures that might carry it out; then even the existence of separate sensory and motor nerve fibers was not clear. Already Fernel, Descartes, and Willis had adumbrated such a functioning system, but agreement as to its demonstrable existence was many years in coming.[38]

ROBERT WHYTT,[l] had invoked (1751)[39] a *sentient principle* in explaining the contractility of muscle. Fontana did not resort to that, but when Whytt demonstrated that the central nervous system was essential for neuromuscular response,[40] but then contented himself with writing of the presence there of a sentient principle, this time Fontana was satisfied.

JOHN AUGUSTUS UNZER (1727-1799) in his *Philosophical View of the Human Body (1750), Outline of a System treating of the Sentiency of*

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[k] This must refer to the communications of Caldani which may be found in Fabri, 1757-9.

[l] The library catalogue of 1807 does not list Whytt's 1751 essay but Fontana was obviously familiar with it.

the Animal Organism (1768), and his Principles of Physiology[m] (1771) had written on the animal machine and *sentient forces* or the *vis nervosa* but Fontana was probably unaware of, and would have been uninterested in, his writings. The name of Unzer's pupil GEORGE PROCHASKA (1749-1820) was known to him through his book *Controversae quaestiones physiologiae,* and his studies of nerve and muscle (1778-79) received high praise. *His Dissertation on the Functions of the Nervous System*[m] of 1784 with its depiction of the *sensorium commune,* related to the writings of Unzer and Willis, was probably not known to Fontana.

Fontana's treatment of the movements of the iris owed a great deal to Whytt's book[39] even to the language being often almost the same, on both matters of what is now called the «light reflex» and on changes in pupil size not directly related to the amount of light falling on the retina. They held opposing opinions on the presence of muscle in the iris, the role of humors in changing the size of the pupil, and the natural state of the pupil. They agreed on many of the phenomena to be observed and on the interpretation of the involvement of the central nervous system, whether through a sentient principle or action of the will.

Fontana's observations on the heart have a modern ring. He discovered the refractory period and showed how the rhythmic activity depends on it. He was aware of the «all-or-none» phenomenon. He missed discovering the influence of nerves on the heart, probably because his methods of stimulating them were inadequate.

Turning to Fontana on respiration, why was he so wrong? The answer seems inescapable, that he chose the road of the phlogistians rather than that of the anti-phlogistians. He probably had heard Lavoisier address the *Académie des Sciences* on 3 May 1777, as he was then in Paris. Lavoisier reported that the air remaining in a closed chamber after respiration by a sparrow extinguished a candle and precipitated lime-water, concluding that in respiration, as in combustion, «true air» was removed and converted into «chalky acid». He mentioned repeatedly Priestley's idea that respiration was a phlogistication of air so that it becomes irrespirable; he did not at that time openly attack the phlogiston theory. Priestley had attacked the theory of a *pabulum vitae* and attributed death of animals in a closed space to want of discharge of the phlogistic matter.

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[m] The Sydenham Society, London, 1851.

Haller's authority also disposed Felice to dismiss the theory of the «vital principle of the ancients», meaning, in particular, John Mayow.[n] Haller had written on Mayow: in his *Primae Linae Physiologiae,* Göttingen, 1751, where he said that there is nitre in the air but it is of no use in respiration; in his *Elementae Physiologiae Corporis Humanis,* Lausanne, 1757-66 (a copy of which he sent to Fontana) where there are numerous mentions of Mayow and his theory of aerial nitre which Haller found doubtful. Haller's own view of the use of respiration was that the function of the lungs was to expel the noxious air contained in them. According to Partington, Haller did not understand Mayow, and presented in his own survey of the subject (respiration) an uncritical and dreary picture.

Fontana's own argument, which he said destroyed the hypothesis of a vital principle, is feeble and contradicted by his own experience. Late in the eighteenth century when the antiphlogistic theory was becoming accepted, and Mayow's work appreciated, Fontana was no longer interested in respiration. Wrong though he could be, his contributions, leaving apart phlogiston, still were appreciable.

One of Fontana's major errors was his conclusion that the *dura mater* was insensible. Was this a matter of looking and not seeing, or did he perform his experiments in such a way as to exclude the demonstration of sensibility? In taking this position he was one of many, and the influence on him of Haller, his leader, and of Caldani, his master who himself changed his opinion on this subject because Haller said so, must have been strong. Although Fontana never recanted, he may have realized his mistake later and have stored resentment as he showed when, after he had escaped from Haller's dominance, he replied with so much acerbity in his *Lettera Apologetica* to Haller's criticism of his *Fisica Animale.*

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[n] John Mayow (1641-1679) follower if not actual associate of Willis, Hooke, and Boyle. In his *Tractatus Duo Quorum prior agit De Respiratione,* etc. of 1668, again in *Tractatus Quinque* of 1674, he described experiments in support of his theory: «With respect then to the use of respiration, it may be affirmed that an aerial something whatever it may be, essential to life, passes into the mass of blood. And this air, driven out of the lungs, these particles being drained from it, is no longer fit for breathing again» (Partington, *Op. cit.,* III, 609).

CHAPTER 15

PLANTS AND ANIMALS

Rust and Blight of Wheat - Tremella - Convulvulus - Vegetable Heat - Hydatids and Tenia - Generation

The Rust of Wheat. Fontana's work[1] on the rust of wheat is dedicated to van Swieten. Its preface «renounces the glory» of writing five hundred pages of quotations from ancient and modern authors, since he respects too much the «Century of Philosophy and of Reason» to favor the pedants who maintain that all queries may be found answered in Virgil and Homer. He then cites Buffon, «It is by well reasoned and prolonged experiments that one forces nature to disclose her secrets [...]»

Some earlier theories on how the rust ruins the grain are mentioned, e.g., the statement of Ginanni and others that insects are found in the grain; that the eggs of animals destroy it; that drops of water focus the sun's rays to burn, scorch and consume, as Galileo proposed; or that a poisonous humor is dispersed in the air. Also cited is what was the predominant theory for 150 years, that the rust is a product of a disease of the plant itself, as Hales had suggested. During that time J.P. de Tournefort (1656-1708) and others classified plant diseases under the influence of Linnaeus, paying little attention to any external causative agent.

Fontana proceded to examine the powder of the rust, both removed from the plant and *in situ,* with the naked eye, with lenses, and with the compound microscope of Cuff. He gives a detailed description in each case, and an excellent illustration (Fig. 24). His observations are succeeded by hypotheses and further observations and experiments, in the modern manner.

When the isolated bodies of the rust were moistened with water, they moved but only briefly which suggested that they were minute

plants; if they were animals movement would continue, even accelerate. After many observations, Fontana concluded that

> It is certain that the black rust, as well as the red rust, of grain consists of real plants, even though they must probably be placed in the imperfect group, so called by botanists because of the lack of many parts found in the more common plants [...] M. Guettard, in an excellent memoir included in the Proceedings of the Paris Academy of Science, 1756, separates parasitic plants into two groups, the true and the false. He called false plants those which attach themselves to others, forming ulcers in which they live without causing damage and offense to the principal plant; for example, fungi, lichens, and many other plants that climb or grow on the [host] plants. The true parasite is the one that attaches itself to healthy and undamaged plants, which forms lesions and wounds on them by introducing its roots or absorbing papillae into them and living at their expense; for example [...] the mistletoe. It appears that our rust plants should be placed between these two types of parasites, since it seems certain, on the one hand, that they attach themselves to the grain plant only when there is a rupturing of vessels and a diffusing of humors, and that, on the other hand, they feed and nourish themselves at the expense of the stalk and leaves of the grain plant; thus they would be called semi-parasites.

Fontana did not realize that the black rust and red rust which he saw were the same organism (named *Puccinia graminis* by Persoon in 1801).

He made one incidental observation, that farmers who cut their grain at the first sign of rust, even while it was still green, had obtained yields that, although mediocre, were still incomparably higher than the yields of neighbors who, under similar circumstances, had waited and had cut their grain at the regular harvest time. He suggested that that practice be followed up. Otherwise he fell back on a familiar theme:

> A long and well-understood series of observations on the rust of grain, made by an intelligent observer, might bring out more important lights not only on the nature of the disease but, also, on the nature and economy of the parasitic plants that produce it. Above all, it is necessary to observe the beginning of the disease, to examine closely the condition of the plant, the germination and development of the parasitic plant, and, finally, the real nature, the origin and causes of the rust. It will then be less difficult to find remedies that will enable us to exclude from our wheat fields such a terrible scourge, which impoverishes and starves us.

The Blight of Wheat. The next disease of wheat to be studied by Fontana[2] was the blight, called by him *Grano cornuto,* or *Sprone,* which he also called *False Ergot,* recognizing that it differed from the true ergot, named *Secale luxurians* by Bauhin (modern name, *Claviceps purpurea*), although he was not always careful to include the word «false» so that

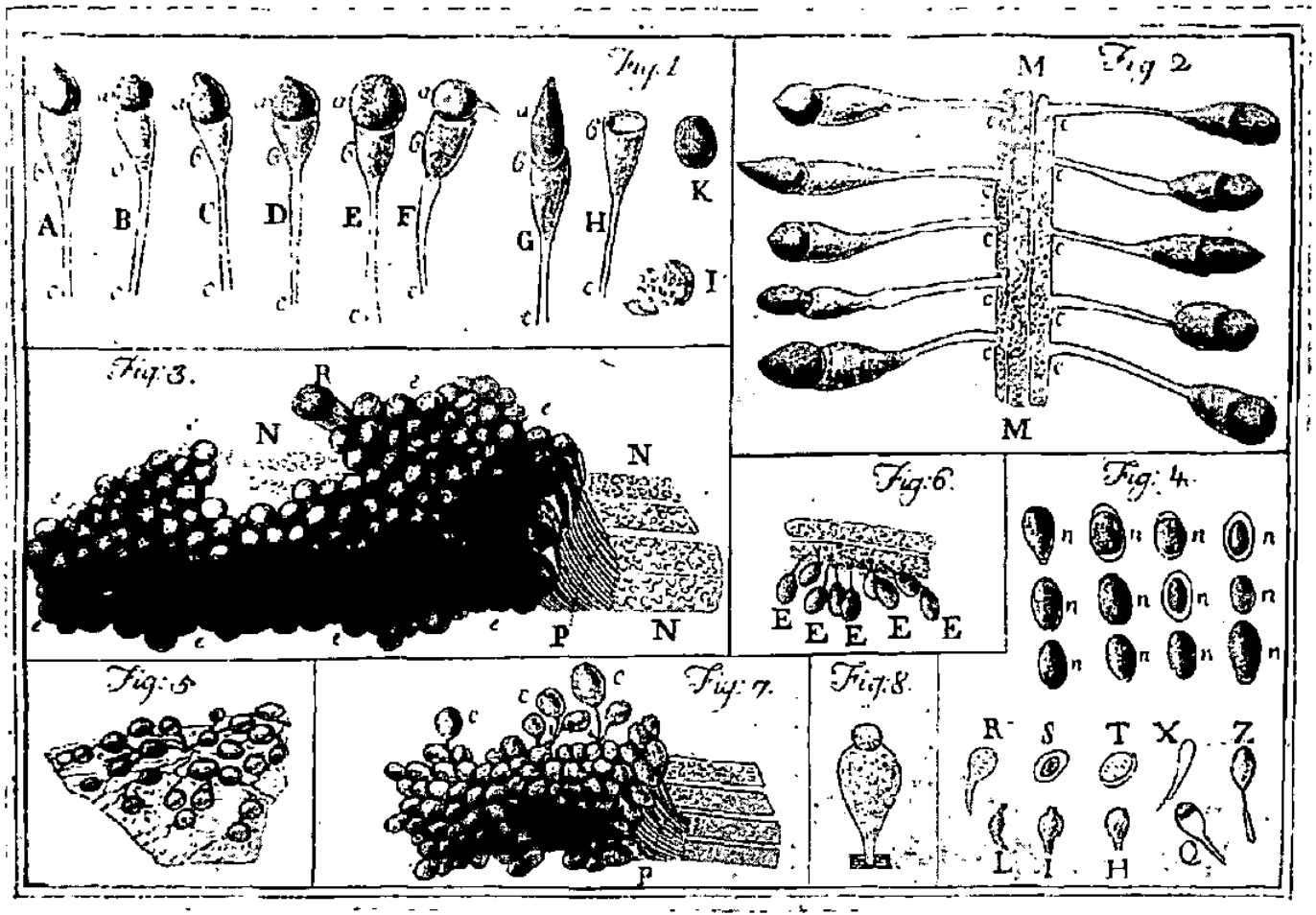

Fig. 24 – *Puccinia graminis* according to Fontana.

confusion of the two eventually resulted. These «small eels» which he saw in the blighted wheat had been discovered in 1743 by Needham. What Fontana found in his studies was:

(1) that wheat becomes infected with eel-worms when healthy and blighted grains are sown together;
(2) that in the early stages of infection the blighted grain contains a few sexually mature eel-worms;
(3) that the females, which are much larger than the males, produce a great number of eggs from which emerge the young eel-worms;
(4) that while males and females die and disintegrate the larvae remain within the blighted grain, ultimately forming the white mass of dried eel-worms discovered by Needham;
(5) that the blighted grain is not the true grain but a gall, similar to plant galls produced by insects.

In his articles of 1775 and 1776 Fontana cited Needham, Buffon, Aymen, Bonnet, but not Baker or Spallanzani, nor, most unfortunately, Francesco Ginanni who had described these organisms in 1759, so that when the controversy with Roffredi arose, as is related in Chapter 10, Fontana's position was not a strong one. Perhaps for this reason, his promised work on «the life and apparent death of animals», describing the revivification of these small eels, never appeared, and he must be recorded as being only one of several who found them of interest in seeking for the distinction between plants and animals, and the nature of life and death.

The subsequent history of the study of this nematode organism, *Anguina* or *Anguillulina tritici,* including Spallazani's about-face, and the problem of anabiosis, or cryptobiosis, is presented by Keilin,[3] Van Gundy,[4] and Mazzolini.[5]

The Tremella. In the same series of papers Fontana described his observations on an acquatic microscopic organism, the tremella.

Exactly what organism Fontana observed is not clear, but it was a member of the *Tremellacea,* belonging to the *basidiomyceti,* the second sub-class of the *micomyceti,* of the genus *Fungi,* comprising forty species of cosmopolitan saprophytes.

Described in 1759-63 by the French botanist Adanson, it was regarded by him as a plant; although it manifested a certain type of motility, it was not thought to move progressively or to be endowed with feeling. Fontana believed that he saw progression from place to place, and other movements not described by Adanson; these movements he

showed not to result from movement in the watery medium or from currents of air above it.

Fontana concluded that tremella was endowed with feeling, although no evidence was presented. This organism could also be dried and apparently be dead, but be revived by being moistened, which was consonant with animality. In spite of his observation that tremella multiplied by simply separating off a portion of a filament, Fontana concluded that it was animal but composed of a mass of plants, thus was another example of a link between the two kingdoms. The extent to which his conception of the nature of this organism was influenced by his past experience with Hallerian «irritability» is stressed by Mazzolini.[5]

Convulvulus. This paper[6] which Fontana wrote late in life reveals him as a new Candide, gone to cultivate his garden. He begins with the *ipomaea hispida,* and other plants of the *convulvulus* type, which have the property of twisting themselves around bodies with which they come into contact. This is not because they are formed of spiral fibers, or to any peculiar mechanism or to local circumstances, but is the effect of a principle of sensation and life, such as he saw years before in the tremella.

Life and sensation are sometimes obscured in uncommon animals, which may lack organs of sensation such as hearing, sight, taste, and smell, and we cannot prove that plants feel, although we cannot refuse then this power. Motion, perhaps, is the most certain character by which animals are distinguished, not ascribable to mere mechanism, but necessarily produced by sensation. Vital movements become almost imperceptible in plants, although Fontana had described them in the tremella, which he now seems to regard as a plant. These plants have little if any analogy with other plants, which is why he began his discussion with consideration of the *convolvuli,* plants large and well-known to everybody, as it is much to be wished that the qualities of life and sensation should be extended to all organized beings.

To encourage others, who will no doubt finish what he has sketched out, he wrote, he says a few words about two properties common to animals and vegetables – generation and respiration.

Plants and animals have an absolute need of air, of oxygen (the first time he has written this word) which contains the vital principle. Then the phenomenon of revivification is discussed, and the analogy between plants and animals in this regard is founded on their equal need of oxygen.

Generation forms the second analogy; the sexual organs and the manner of reproducing themselves are common to the two great families of organized beings. The grand act of generation is executed by a principle of sensation which regulates and directs it. Although sensation may abandon plants as soon as the end of nature is accomplished, so do different kinds of animals die when they have ensured the reproduction of the species.

It is thus highly probable that plants as well as animals enjoy a principle of life and sensation, whatever be the nature of that principle. Surprisingly, nature grants to vegetables a force and energy which it seems to have refused to animals, namely the property, not only of decomposing every kind of bodies, but of collecting the elements to form new compounds. Animals are destitute of this prosperty, or at least enjoy it only in a weak degree, he wrote.

Movement may remain, even though sensibility has been greatly altered, as is discussed at length with the example of the tortoise, which may life for five or six months with no change being observed in its movements or habits after the brains have been scooped out. A great variety of movements may be observed in the decapitated house-fly – not to be regarded as contemptible. «Nothing is contemptible in the eyes of the philosopher; everything in nature is great and wonderful». The closing sentences of the work are:

> These truths and several others are explained and illustrated in a work which I intend to publish, on the life, death, and sensibility of animals. This work, several fragments of which have been communicated for more than twenty years past to different learned men in Europe, will be enriched with above 200 engravings, and will form two large volumes in quarto. I have need only of a few moments of philosophic leisure to put the last hand to it; but, can I hope for this after having been persecuted with as much injustice as barbarity?

Needless to say, the work never appeared.

On the Heat of Vegetables. This work, Fontana's last,[7] sought to answer the question: do vegetable have their own natural heat in life, as do the animals called warm-blooded. John Hunter was the first to examine the question, with observations on trees; he concluded in the affirmative. Bonnet and de Saussure re-examined the question with negative results, but felt that the decisive experiments had not yet been done.

The difficulty seen by Fontana was separate the heat of the plant from the ever-changing heat of the earth. His chosen experimental

conditions, his delicate thermometers, his testing of the air with indicators, with his hygrometer and his eudiometer, the plants used, are fully described; his results in great number (more than 4,600) leave no doubt of the conclusion that plants do not produce any degree of heat – except – the mushroom called in Tuscany the *fungo porcino,* which constantly raised the mercury in the thermometer by one-half degree centigrade. It may be possible that other plants do the same; he will continue the investigation.

Hydatids and Tenia. Fontana's venture into the study of animal parasites is described in a communication on several subjects.[8] Its origin was a disease of sheep called in Italy *la pazzia,* or madness. In fifteen of these animals which had ceased to eat, staggered around, and eventually succumbed, he found in the brain a vesicle, filled with a transparent fluid, on the side of the brain opposite to the side of the body on which they fell to the ground. Considering the gross appearance of the vesicle, he began to suspect that a similar lesion might be found in man, and he was told by a practising physician that he had found them in various persons dying mad.

Now he recalled that when he was in Paris he had seen great numbers of vesicles, or hydatids, in the intestinal mesentery and omentum of wild rabbits, these proving to be true animals but as they had already been described by Pallas[a] in his Zootomia, he thought it would be superfluous to publish anything on them. The suspicion grew that these two kinds of vesicles, or hydatids, might be similar.

First Fontana re-examined in detail the abdominal hydatids, to which he had assigned no cause of illness. The rabbits in Paris, in excellent health, had had three to four hundred, each the size of a bean. Of the Florentine sheep, eighteen to twenty had had much larger abdominal vesicles, up to two inches in length, covered with membranous tissue, full of limpid humor. The hydatids in these did not move progressively but their cuticle showed fluctuating, wave-like movements as in a storm at sea. These organisms, of animal nature as described by

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[a] Peter Simon Pallas (1714-1811) born and died in Berlin. He became Doctor of Medicine with a dissertation on intestinal worms, insisting that they were parasites of external origin, rather than being formed by spontaneous generation or from humors. His travels in Russia and Siberia made him one of the most eminent explorers of the century. The work mentioned is possibly his *Spicilegia zoologica,* Berlin, 1767-80. Several of his works were in the library of 1807.

Pallas, led him to suspect, by analogy, that the hydatids of the brain might be similar, but in this he was disappointed. Instead, he found the vesicles to contain, in the fluid but sometimes attached to the wall, a great number of oviform granules, with microscopic structure resembling that of the abdominal hydatids, obviously animal in nature.

Fontana had discovered the cause of the *pazzia* of sheep and the nature of the similar disease, so terrible and humiliating to man. The hydatids of the abdomen, described by Tifone,[b] Hoffman,[c] and Pallas, had been recognized as animals, but no one, as far as Fontana knew, had spoken of those of the brain or connected them with any illness. Now the physician might have a better idea of them and apply suitable remedies.

At this point in the version printed in the *Raccolta Ferrarese* there is a footnote reading:

> I am told by a friend at this moment that there was published in Germany a work on this illness of sheep, speaking of the same illness and the same cause, but not having seen it I can say nothing. However I am more than persuaded that the countrymen and the butchers know this illness better than do the philosophers, because their interest is greater, but the observations of ignorant persons are always rough and formless; it falls to the philosophers to give them the life they deserve. If I have been preceded, nothing is easier in a century with so many observers; it will still be profitable that I had confirmed the other's discovery, and to give a chance to others after me to verify what might be disagreed upon.

This presumably refers to the work of Leske, *Von dem Drehen der Schafe,* Leipzig, 1790, not seen by us but related[9] to have been the first recognition of the parasitic nature of the disease.

Fontana proceeded to discuss another kind of animal called tenia which is related to the hydatids of the brain and belly. This similarity is only in the animal's head, the rest of it being quite different. The common beliefs, that each section of tenia can become a new worm, and that each worm consists of a mass of distinct individuals tied together, are mistaken. The animal is oviparous, the most mature eggs coming from the more terminal segments at the tail. He had found these worms implanted among the intestinal villi, attached so strongly that the worm would break on being pulled, leaving the head still attached.

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[b] Not identified.

[c] Probably Friedrich Hoffman (1660-1742), physician and chemist. His works were in the library of 1807.

The animals that Fontana had observed so correctly were members of the cestodes, the flat-worms. The adult form are now known as *Taenia solium* and *Taenia saginata,* the pork and beef tapeworms; they had been described but not distinguished by Linnaeus in 1758, nor by Pallas in 1781, who called them both *Taenia cucurbitina,* a name often used for them in Italian. The larval forms spoken of by Fontana are *Cysticercus pisiformis,* seen in the rabbit, where they may reach extraordinary accumulations; *Coenurus cerebralis* found in sheep, in which the large vesicle varying in size from pea to turkey egg contains the scolexes which are 4-5 mm. in lenght; *Cysticercus cellulosae,* seen in man, is the larval form of *Taenia solium.*[10]

Generation. In 1792, Cambiagi, Royal Printer to Ferdinand III, who ruled Tuscany as Grand Duke, the third member of the House of Hapsburg-Lorraine to do so, produced a pamphlet of forty-seven pages entitled *Letter to a Friend on the Process of Development.*[d] It was anonymous, perhaps because its author espoused the cause of the epigenesists, and «by 1786, indeed that viewpoint [preformation theory] was so orthodox that Senebier [...] could treat the epigenesists as no better than atheists».[11]
Its author was Felice Fontana, who had already been in trouble[12] with Tuscan ecclesiastical circles on account of the Jansenist tendencies of the Grand Duke Peter Leopold.

The article on generation is undoubtedly Fontana's; the style is completely his. An example, bound with a number of such pamphlets, is in the *Biblioteca Medica Centrale,* Careggi (segnatura 314.2), apparently collected by Attilio Zuccagni (1754-1807) who was prefect of the botanical garden of the Royal Museum from 1782 to 1805. It bears the inscription in his hand «del Sig. Felice Fontana». The work was cited by Cuvier (1769-1832)[13] as *Principes Raisonnés sur la Génération,* «Fontana's last work», which it was not. The term «generation» referring to the coming into existence of new individual organism, plant and animal, was much written about in the eighteenth century. The term itself subsequen-

[d] The title in Italian is: *Lettera ad un amico sopra il sistema degli sviluppi.* A literal translation might be: Letter to a friend on the system of development. It is clear from the text that by development was meant the theory of preformation. The phrase was used frequently in translations from the works of Maupertuis and would have been familiar to Italian scientists.

tly fell out of use, although the study of the subject continued to be pursued as it had since antiquity. Exactly how much investigative experimentation Fontana carried out on it is not clear. In 1782 he wrote[14] to his friend Gibelin:

> I have worked much on the *incubation of eggs,* to discover the successive formation and development of the parts of the embryo and I have found new things capable of shedding a great light on the profund mystery of generation. The drawings that I have made and colored number more than one hundred.

This work never appeared, as happened with so many of Fontana's promised contributions.[15]

The problem of generation had been considered by Aristotle,[16] who stated in the clearest terms two rival theories (later to be named preformation and epigenesis) and decided on the latter, holding that the future generation is present only potentially in the embryo.

Although Fontana's essay[17] on generation is not free from ambiguity, it gives a good, often eloquent summing-up of the situation at the time,[18] well exemplifies his style and methods. It is further of interest in its departure from the authority of Haller and its contradiction of Spallanzani. He may have represented Bonnet's writings unfairly, although he refrained from attacking them in his usual way. As Glass[19] puts it:

> What is often overlooked today is the fact that Bonnet was profoundly right in holding as senseless the view that «that unity, that organic whole one calls an animal», can arise from an amorphous semen, and in affirming that a gel *(glu)* which appears to become organized in development must possess some organization to start with: [...] Every genesis, in other words, must issue from a «predetermination», a «primordial design». [...] As so often in the history of scientific ideas, the ultimate truth includes those antithetical ideas over which earlier men were embroiled in controversy.

CHAPTER 16

ANATOMY

Macro Scale - Testicle - Intercostal Nerve - Ciliary Canal - Micro Scale - Red Corpuscles - White Corpuscles - Nerves - Brain and Retina - Reproduction of Nerves - Muscular Substance - The Crystalline Lens - On the Gluten of Eels - Felice Fontana as Anatomist

Macro Scale.

Fontana «himself handled the scalpel with great dexterity, but it was hardly for more than to study the innermost structures of parts, always with the eye armed with a strong lens, or with recourse to the microscope», according to Desgenettes. Mangili related how Fontana's «passion for science was so untiring that many times, his frugal meal barely finished, he had brought to the same dining table human skulls, trunks, and members when he needed to make the most acute anatomical examination of them». Fontana himself tells us that he acquired a knowledge of anatomy long before he came to Tuscany; presumably this happened in Padua, the home of many great anatomists.

His first recorded anatomical investigation was of the inner ear, in which he examined the claims of Domenico Cotugno (1736-1822) who had recently (1761) described for the first time the acqueducts. From a letter of September 1763 from Fontana to L.M.-A. Caldani[1] we know that Fontana confirmed Cotugno's findings in general but did not agree with him entirely. That Fontana's interest in, and work on, the anatomy of the inner ear continued is shown by letters of Cotugno and Caldani.[1] From the gushing letter that Fontana wrote to Cotugno,[2] in June 1765, we learn that he considered Cotugno as «something more than human», that he «called himself happy to live in a century in which lived a Cotugno», but the occasion of writing this letter was the dispatch to Cotugno of Fontana's book on the iris.

TESTICLE. Fontana's first publication in anatomy appeared in 1767 in the appendix to the third volume of the *Atti dell'Accademia delle Scienze di Siena detta de' fisio-critici,* which was dedicated to Peter Leopold, Grand Duke of Tuscany. Nominally a letter addressed to the predecessor and teacher of Mascagni at Siena, it bore as the title: Observations on the Human Testicle made by the Abate Felice Fontana, nobleman of Rovereto and Reader in the University of Pisa [...] to Pietro Tabarrani, Professor of Anatomy in the *Studio* of Siena.

The off-prints of this article were provided with a halftitle, Letter on the Epididymis. (Siena, Bindi, 1767). This example of synecdoche may have been furnished by the printer Bindi, but as the greater part of the work is about the epididymis, and as Fontana referred to it in that way, he may have suggested the half-title himself. It is an excellent description of the epididymis as thought of today.

INTERCOSTAL NERVE. A late work in anatomy of Fontana appeared[3] as a brief note in a letter to his pupil Giuseppe Mangili, who published it. Fontana wrote that the intercostal nerves do not arise from the sixth pair of cranial nerves but run together with them in a common sheath with no interconnections, between cavernous sinus and the orbit. «Here for more clarity is a sketch of the sheath of the sixth nerve», he wrote. This sketch does not appear in the paper, which continues with a footnote by Mangili:

> Our author, as he writes me in one of his letters (says Sig. Abb. Mangili), has never claimed originality in speaking of the intercostal, but only of all those observations which others had published; among those he wanted chiefly to mention those published many years ago by the famous Professor of Padova, Sig. Comparetti; he is well content to have confirmed others or to have added his own new facts. I add that his drawings seen by me and shown to his friends on this nerve are such that they show how much was his own and how much of others about the origins and branching of that nerve cranially before the superior cervical ganglion; it was precisely these which convinced the two clever anatomists Girardi and Malacarne. Meanwhile it is certain that, in anatomical works published in the years before, not only is the origin of the intercostal from the sixth pair not denied, but just the opposite is generally found: confutation of the contrary opinion; thus in few or no cases were observations published before by others, as anatomists and all the Universities sustained the origin of the intercostal from the sixth, impugning the contrary opinions. Our author, by only sending them his drawings, succeeded in persuading and convincing them that the intercostal certainly did not arise from the sixth pair of nerves.

If, as Mangili wrote, Fontana's efforts convinced the Italian anatomists, they dispelled the confusion of centuries which had arisen partly from differences in the numbering of the cranial nerves. Thus Galen and Mondino after him had included the abducens nerve (VI), identified as a separate cranial nerve by Eustachius only in 1552, with the oculomotor (III) as the «second pair», and the glossopharyngeal (IX), vagus (X), and spinal accessory (XI) cranial nerves together with the structure now called the sympathetic trunk, as the «sixth pair». Vesalius seems to have followed this scheme; he also mentioned the intercostal nerves, «which augment the offshoots of the sixth pair of nerves of the brain extending to the roots of the ribs»,[4] although it is not clear if Vesalius meant by the intercostal nerves the structures given that name by Thomas Willis in 1664,[5] but which have been called the «grand sympathetic» since the appearance of the four-volume work on anatomy of Winslow[a] (1669-1760) in a four-volume French edition in 1733, in English in 1733-34.

Eustachius (1524-1574) made an accurate study of the cranial nerves, although his plates, completed in 1552, were not printed until 1714. However he attributed the origin of the sympathetic trunk to the abducens nerve which he had discovered, and in this he was followed by Thomas Willis (1621-1675) and Raymond Vieussens (1641-1716). Both these anatomists (Willis in 1664, Vieussens in 1685) described the fifth and sixth cranial nerves, twigs of which were believed to form the «intercostal nerve» (as named by Willis; our sympathetic trunk), rationalizing the theory of Willis of the function of this nerve in providing reciprocity of responses in the different parts of the body, in that animal spirits having their source in the cerebellum were distributed throughout the body providing a «concomitance between actions and involuntary passions».[6]

These views were refuted by François Pourfour du Petit (1664-1741), French ophthalmic surgeon, who began to doubt the claims

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[a] James Benignus Winslow, *An anatomical exposition of the structure of the human body*. Transl. from the French original by G. Douglas, 4th Ed., London, 1756. Section VI, a description of the nerves. P. 94, Item 361: «These nerves, as I have said, are commonly called Intercostales, though this name does not agree either with their situation, or with the extent of their course, as we shall presently see; and therefore I believe the name of Sympathetic Majores, or Maximi, will be more proper, because of their frequent communications with almost all the other principal nerves of the body».

of Willis and Vieussens in 1705, publishing in 1727[6] his own anatomical studies and, more convincingly, his physiological experiments describing the ocular changes on section of the sympathetic trunk in the neck. Petit believed that the anatomical observations excluded the possibility of animal spirits flowing from the cranial nerves into the intercostal extensions, that the intercostal was attached in the opposite direction, and that his experiments convincingly showed that the energy transmitted to the eyes by the intercostal nerves was through fibers which extended from the trunk of the body upwards into the head.

In 1791 there appeared a book on the intercostal nerve[7] by Michele Girardi, dedicated to «Viro illustri Felici Fontana, Matheseos, Anatomes, ac UNIVERSAE HISTORIAE NATURALIS PERITISSIMO PRAESTANTISSIMARUM ACADEMIARUM», which reviewed the anatomy of these nerves as presented by earlier workers and in his own studies, with the explanation that, in spite of the experiments and observations of Petit which had been confirmed by Winslow, the general opinion of anatomists was the ancient one, that the origins of the intercostal nerves lay in the fifth and sixth cranial nerves. Girardi wrote that the work of Petit had «fallen into oblivion», while Fontana «Had recalled and demonstrated it in an obvious manner by new facts, observed with that sagacity which characterizes him». Girardi also followed Fontana's letter[3] with a laudatory note[8] describing the work as very fine.

According to Quérard (1829)[9] who mentions the «Prolusio» (Florence) of Girardi and its Paris editions:

> This dissertation, intended for delivery on the opening of a course of lectures but not so given, is a commentary on an extensive and exact work of Fontana, presented by Girardi with happy applications to the theory and practice of medicine. The early edition was highly incorrect, but Baron Desgenettes had printed in Paris a very elegant and correct edition in 1792 and gave a comprehensive extract of it in the *Journal de Physique* [...] in the same year».

This was in vol. *41*, pp. 174-184, to the length of approx. 5,500 words, whereas the original (in Latin) had about 5,000 words; the two seem to be not greatly different. A footnote on p. 179 resembles very closely the «letter» published by Mangili, suggesting that Fontana had sent a copy at the same time to Desgenettes.

Girardi wrote also about anatomical observations of Fontana of which we have no record otherwise, dealing with the glossopharyngeal nerves, and also Fontana's opinion that the spinal medulla was an organ of sensation, independently of the brain. Fontana was said to have

Fig. 25 – Drawing accompanying Wax Model N°. 702, Museum of La Specola, Florence.

demonstrated to several individuals over a period of twenty years his experiments showing that animals without brain, without head, could walk, jump, swim, climb, breathe, turn in various ways, protect themselves, take fright, amuse themselves, be angry, finally could have feelings and judgement as before.

Although the last line of the text of Fontana's letter mentions a sketch, none accompanied the published paper, so we have little idea of the drawings by which he is said by Mangili to have convinced his contemporaries of the true origin of the intercostal nerves. However, there is in the Museum of La Specola in Florence one of the wax models which bears the number 702 and the title of «The Maxillary Artery». It is labeled: «A dissected head and neck which shows the cervical nerves and maxillary artery, etc. A work of Clemente Susini and his assistants under the direction of Doctor Tomm. Bonicoli, between the years 1775 and 1791».

The related drawing (Fig. 25) is poor in quality in comparison with many of the others accompanying the models, but the angle at which the «roots of the intercostal nerve» join the cranial nerves, an important part of Petit's and apparently Fontana's argument, is well shown.

Another Italian anatomist ready to be convinced was Scarpa. He wrote[10] to Fontana 2 August 1792:

> Send me the drawing of the branches of the intercostal within the cranium in order that I can also be by way of verifying your observations. We are already in agreement that too much has been attributed to the brain, that among anatomists this going up and down of the nerves has been until now too arbitrary. If, then, the soft nerves of the superior cervical ganglion, following a principle, ascend around the external carotid, there would be no room for surprise if some of these soft nerves ascended also around the internal carotid, to unite with the fifth and to follow the branches of the intracerebral carotid.

CILIARY CANAL. One of the best-known works of Fontana in anatomy, because it led to the eponymic designation of a structure in the human eye, was his description of a canal in the eye of the ox. It appeared (Fig. 26) in his treatise, *Le Venin de la Vipère,* Florence, 1781, in the form of a letter, dated 1778, to Adolphus Murray, Professor of Anatomy at Upsala. It read:[11]

> I enclose you three designs [...] which I thought sufficient to bring to your recollection the idea of the new canal I have discovered in the eye, and which I had the pleasure to show you when you passed through Florence.

It appears that the visit was paid in February 1775, from letters written by Fontana to his friend Slop in Pisa. One, undated, said:

> Mr. Murray, professor of anatomy at Upsala in Sweden, will give you my letter. He will spend some days in Pisa to see the city and meet some professors of the University. He is a great physician, great anatomist, great naturalist, and also a chemist, thus you will know immediately what persons to introduce him to. I beg you to take every kind of care of him, because he is my great friend whom I esteem most highly.

Another, dated only 4 March, said:

> Tell me how you found Murray in Pisa, and what he said of it.

A third, undated, said:

> Murray was very pleased with you [...] You will be glad to see Bernoulli here [...] Padre Serati will send you six copies of my pamphlet on Ergot and Tremella [which appeared in 1775].

From Fontana's letters to Bernoulli, we know that the latter was in Florence early in 1775. The letter to Murray continues:

> I send you the drawing of this new canal of the eye, not because I wish you, as you tell me you are desirous of doing, to publish it in the acts of the academy at Upsala, but simply because you ask it of me [...] Do what you will with it for I am totally indifferent (Fig. 26).

The preface of the French editor of the Treatise on the Venom of the Viper stated:[11]

> To complete this work, I have judged it necessary to add a description of a new canal of the eye, discovered by our author more than eighteen years ago, and which he has never published. I have taken this description from a letter dated from London, which he wrote at the end of the year 1779, to Mr. Adolphus Murray, a celebrated professor of anatomy at Upsala; and have given the part of our author's letter that relates to this subject, in his own words.
>
> We cannot avoid being surprised at the little value our author places on his own discoveries, whilst any other anatomist, however celebrated, would have hastened to publish them. At the end of eighteen years, he scarcely permits this new canal he has discovered to be announced in a few lines in one of his works; at the same time that it has been demonstrated at Vienna for upwards of ten years, in the common courses of anatomy, to the professors of which it was probably communicated by M. Brambilla, surgeon to the Emperor, and director of the

military hospitals. Our author showed this canal to M. Brambilla, when he accompanied his Imperial Majesty, in his travels in Italy.

Although our author has never published this so long discovered canal, he has, however, shown it to a great number of his friends, and to several other persons. – M. Murray, the professor of anatomy at Upsal, in a letter he addressed to our author some time ago, informs him, that the description of his new canal of the eye has been inserted in the last volume of the Acts of the Academy at Upsal. *In ultimo tomo* (he writes) *descriptio canalis a te detecti extat.* – This canal was shown by our author to the Swedish professor, when this last, on his coming to Italy, made some stay at Florence. At his return to Sweden, he addressed our author at Paris, where he then was, to procure the drawing and description of the canal, which he was desirous of publishing in the Acts of the Swedish Academy, for the advancement of anatomy, and for the good of his countrymen. The drawing and description were sent from Paris, but were lost on the road. Our author sent him a fresh copy of them from London, but I do not know whether they ever reached him. I have inserted the figures and description at the end of this work, with the copy of the letter that accompanied them.

Fontana's letter continues:

You must certainly have perceived when you were here how little I valued this discovery, now grown out of date with me; I say *discovery,* since you will have it called so [...] I will not advance imaginary hypotheses, nor simple probabilities.

Murray did publish at Upsala a long article[12] on the anatomy of the ciliary region of the eye, full of encomiums of Fontana and containing the following:

Fig. 26 – Part of Plate VII from the *Traité sur le venin de la vipere* etc., Florence 1781. Translation of Fontana's caption from the *Trattato del veleno della vipera* etc., Naples 1787: Fig. III represents one of the eighth pair of nerves of a rabbit which had been sectioned twenty-nine days previously, shown about three times the natural size. Letters *r, r,* indicate the point of reunion. Fig. IV is a repetition of Fig. III enlarged the better to show the spiral bands. Fig. VIII represents roughly the anterior half of the bulb of the eye, as seen from the concave side. The letter *n,* indicates the sclera, *m* the ciliary body, *e* the ciliary processes, *c* the uvea, *a* the pupil. Fig. IX shows one-half of the previous figure; *r, m, m, o,* indicate the new circular canal of the eye cut at *m, m,* the raised edges of which are indicated by *r, o. a* is the pupil, *r* the sclera. Fig. X is one-half of the previous figure, with the ciliary body and the new canal *o* half detached from the rest. *r* is the sclera stripped from the choroid, *c* the groove where the ciliary body is attached to the transparent cornea. The letters *e, o, f,* denote a membranous structure formed by the coincidence of the choroid *e,* the ciliary ligament *o,* and the uvea *f.* Letter *e* denotes that part of the ligament which is attached in the groove *c.*

Fig. VIII.
Fig. II.
Fig. III
Fig. IV
Fig. I

The new ciliary canal

To this canal I shall give the name of Fontana, as his due. Felice Fontana, a most perceptive and erudite man, physicist to the Archduke of Austria and Etruria, the most Serene Prince Peter Leopold, was the first to discover it, at Florence in 1774 [...] When I was in Florence three years ago, he first demonstrated this canal to me, asking me to prepare a description of it for communication to the world of learning. This task, entrusted to me by that illustrious man, I now endeavor to carry out.

In 1804 Kieser of Göttingen pointed out[13] that such a structure did not appear in the human eye; however certain spaces in the trabecular meshwork are often referred to as the spaces of Fontana.

From letters[14] of Scarpa to Fontana of August and November, 1795, we learn that Fontana still had a lively interest in the anatomy of the cranial and spinal nerves. A fragment of description of the anatomy of the inguinal region in Fontana's hand remains in manuscript.[15]

Micro Scale

By the nineteenth century, the microscope had fallen under a cloud for several reasons. The compound microscopes used in the eighteenth century with their uncorrected lens systems and low numerical apertures suffered from a high degree of chromatic and spherical aberration. With the magnification often employed (as much as 500) the final images must have been confused and optical artefacts were prominent, so that observers completed a great deal with their imaginations and twisted their imperfect observations into accordance with their theories. The instrument and its users fell into disrepute, as expressed by Bichat[16] in 1801; he considered a microscope to be:

a kind of agent from which physiology and anatomy seem to me to have never derived a great help, because when one looks into its confusion, each sees in his own way and pursues what concerns him.

By the middle of the eighteenth century the microscope had largely come to serve as an instrument of entertainment.

Felice Fontana was aware of the problems of microscopy, discussing «microscopical errors; and consequences deduced from microscopical observations» in «Le venin de la vipère» (1781). He wrote:[11]

A simple bare observation does not admit a full confidence, even although it be made by a celebrated observer, because it tacitly supposes that there is a necessary

and exclusive agreement betwixt the image represented by the microscope, and the real exterior object; which is not always the case [...] There is a great difference betwixt a microscopical observation and a microscopical experiment [...] It is absolutely necessary to analyse the observation itself; that is to say, to make a microscopical experiment capable of assuring us, that the object is in reality such as the microscope has figured it to us [...] Truth is simple, error is infinite.

RED CORPUSCLES. Many important studies with the microscope were made by Fontana, the first to be published[17] being entitled *New Observations on the Red Globules of the Blood.*

Of all the human tissues at which the microscope has been directed, blood has undoubtedly been the most frequently examined, since it is readily available in a fresh state and requires no complicated handling. Microscopy of the blood was a common pursuit, also for Fontana, although he gives as the reason for publishing this *coserella* (little thing), as he called it, the recent appearance of a work[18] by a «learned Italian philosopher», whom he does not name but who was in fact the Neapolitan histologist Giovanni Maria Della Torre (1710-1782). He had examined the blood of man and other animals, of all ages, constitutions, and conditions of health, by lightly compressing a few drops between two plates of mica and examining it with simple spherical lenses at magnifications of 42-1,920 times. Della Torre concluded that the corpuscles were not spherical but rings, or «doughnuts», composed of one to six bags, or sacks, of thin membranes full of lymph.

Fontana repeated these observations, with the same kind of lens and with the compound microscope, on whole blood or blood mixed with water held between two plates of mica. He saw the same sights (Fig. 27) as had Della Torre, but by varying the illumination, he convinced himself that the bright spot in the center of the circular cells was not an open space, or hole, as Della Torre had concluded, that the cells were not rings, or doughnuts, but spheroids, which is what they probably were, having swollen from taking up water; it is likely that Fontana's observations were largely on blood plus added water, this dilution making it easier to get a thin layer of what he called «the molecules of the blood» and also preventing clotting. Thus Fontana made the same error as had hundreds of others, adding water which caused the cells to swell into spheres, as may be gathered from the name «globules», an error which long persisted.

Leeuwenhoek and others, as Fontana mentioned, who described the red blood cells long before, had also written how these cells were deformed into cylindrical shapes when they had to pass through a small

vascular channel *in vivo*. When Fontana looked at small blood vessels in the mesentery of the frog or other cold-blooded animals, he saw that the cells may pass through the vessel in single file, but were not deformed, although this change in shape was seen on passage through the pulmonary vessels, which he described at some length.

Fontana believed these globules consisted of a viscid oleogelatinous material, with there being no basis to believe that they were surrounded by a thin skin or membrane, as many people had thought. His evidence against the belief was that after flattening the cells to four or five times their normal diameter by pressure on the mica, then releasing it, the cells returned to their previous spheroid shape. If there were a skin, it would certainly have been ruptured by this treatment, he thought. Also, if the cells were let stand long enough the separate ones would unite in a homogeneous glutinous uniformity, which would not occur if there were membranes, which are «children of hypotheses fabricated in the arm-chair». Also, there are spherical globules in milk, fats, and chyle, which no one believes to be covered with a membrane.[19]

A review of this work promptly appeared in Griselini's journal.[20]

At about this time, William Hewson (1739-1774) was making his studies of the red blood cells, which he described to the Royal Society of London in 1776.[21] Hewson used some of the same lenses used by Della Torre which had a greater magnifying power but were much inferior in distinctness to those of Hewson. By diluting the blood with blood serum, rather than with water, Hewson avoided the uptake of water and swelling of the cells leading to a spheroid shape, and he saw that the cells were actually flat, not globular, and without a central perforation. If water were added dropwise, the shape changed to spheroidal. If a solution of a neutral salt of the proper concentration was used as a diluent, no change in shape was seen. Alkali and acid in any concentration changed the shape. Hewson concluded that «the use of those salts which enter into the composition of the blood is probably to preserve the flat shape» of the blood corpuscles. He saw no change in shape when the corpuscles moved with difficulty in blood vessels which would admit only single corpuscles. The current opinion, that the normal shape of the erythrocyte is that of a biconcave disk, was not so stated until 1827, by T. Hodgkin and J.J. Lister.

White Corpuscles. The white cells of the blood were seen by Fontana, not in the circulating blood but in pus. He wrote:[22]

I have examined them [pus samples] of various natures; some well tolerated, which the physicians call laudable, some of gangrene, some cancerous. I never happened to see there particles, or motile ovules although I looked for them immediately on collection from the sores. I saw nothing other than very many droplets somewhat varying in size, or molecules tending to spheroidal shape, which floated in a transparent humor. It would seem very strange, however, not easily believed, if the indisputable fact did not show them in natural pus and if one did not see in the living animal those ovules, or little microscopic animals which are discerned constantly in animal substance put into water to rot. It would also be desirable that an observant philosopher should examine attentively the nature of those molecules which are seen in pus since after long observation one might end by determining if those molecules and corpuscles of natural pus are the globules of blood, or loosened and partly broken-down fat: which truth could then serve admirably to distinguish various kinds of pus; that from sores, and laceration ofvessels, or that coming from transudation of serum without rupture of vessels; that would be of very great usefulness in practical medicine.

The identification of pus corpuscles with the leucocytes of the circulating blood would wait until Addison (1843)[23] and Waller (1846).[24]

Nerves. When Fontana's *Treatise on the Venom of the Viper* appeared in 1781, it contained a section of ninety-six pages on «Observations on the Primitive Structure of the Animal Body»; the word *primitive* here was undoubtedly meant in the sense of primary, or not derived from other structures. Many have wondered why this treatise on poisons should be such a miscellany. There were probably two reasons: one, that Fontana decided not to wait for completion of his work, «my Microscopic Observations, about which I have spoken with you many times», as he wrote to his friend Gibelin.[25] The other reason he told to his friend de Saussure[b] when he wrote[26] 23 March 1781:

My work *on the poisons* [...] goes on apace, and I flatter myself that I shall be able to send you a copy before the end of June. It will be in two volumes in 4°. I have included in it some other notes to give the work more variety [...]

At the end you will find experiments and observations on the subject of nerves. I flatter myself that they will not entirely displease you. I do not doubt my knowledge now of the primary organic elements of the nerve and other animal structures previously unknown or poorly known.

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[b] Horace-Bénédict de Saussure (1740-1799). Physician, geologist, professor of natural history and philosophy, Geneva, 1762-1786. Known for his Alpine travel, ascent of Mont Blanc, 1787. Father of Nicolas Théodore.

In twenty-six pages of the *Treatise,* on observations on the structure of the nerves, made at London in 1779, Fontana related his discovery of the ultimate structural unit of nerves, the axis cylinder and its sheath. Haller had confessed that nothing but conjecture could be drawn from Leeuwenhoek's earlier observations; Fontana felt that two others who deserved notice for their previous observations on nerves were Della Torre and Prochaska, although they agreed that nerves are aligned assemblies of globules, a common error of the times that persisted[23] even in the writings in 1820 of Everard Home and in 1823 of Milne Edwards.[c]

Fontana, arriving in London, learned of the studies on nerves of Monro who claimed to have found there a system of convoluted fibers, of which more later. Fontana attempted, without success, to correspond with Monro, then proceeded cautiously to make his own observations on nerves in the living animal, separating them with needles under lenses and illumination of varying strength. He saw alternating dark and light-colored bands, regular and irregular, at various angles to each other, at times crossing, at times seeming to form a spiral. At first persuaded that the bands were not an optical illusion, he continued his examination after stripping nerves of their «cellular (i.e., connective tissue) membrane». With a very strong lens, the bands were no longer visible; instead he saw parallel winding fibers. Diversifying the circumstances as much as possible, he was convinced that the bands were caused by the winding fibers themselves. Continued observation for several days enabled him to see clearly and discover the truth.

> I now employed a lens of a middling strength, and threw a light upon the object in such a way as to distinguish clearly the winding, wavy fibers; but without seeing the spiral bands. Without touching object or lens, I simply turned the mirror a little, and so directed the light on the object, that at length the spiral bands appeared very distinctly [...] Thus I could at pleasure produce this double appearance, of bands and fibers, by only throwing more or less light on the object [...] It was no longer possible to doubt the reality of the observations; that is to say, that the bands were not real but apparent; and on the contrary, that the winding fibers were not apparent but real.[11]

---

[c] Henry Milne Edwards (1800-1885). Born at Bruges, lived in Paris. Zoologist and comparative anatomist. According to him, every organ in the body was made up of globules, nearly all of which were uniformly 1/300 of a millimetre in diameter. Usually these globules were circles produced by optical interference. See reference 35, p. 8.

These observations and experiments of Fontana have recently been fully confirmed by Zanobio[27] and by Clarke and Bearn;[28] the latter list all those making the same observations since Fontana, and conclude that the waning of interest in this detail of structure was a result of the use of preserved rather than fresh material; they suggest that this «nerve fiber zigzag is one of the mechanisms which allows the nerve to tolerate moderate stretching without damage».
Fontana's narrative continues:[11]

After having thus surmounted this first difficulty, and assured myself that a nerve presents to the view a great number of winding fibers, of which it is formed, I proceeded to farther researches. I wished to know what the primitive structure of the nerves is, that is to say, whether it is composed of channels or simple threads; whether it merely consists of globules, or contains a non-organic, irregular, spongy matter. This research is as important as difficult, since it tends to nothing less than the fixing, once for all, the ideas of anatomists on the nature of the nerves; that is to say, on the structure of the organ of motion and sensation in animals. They have disputed for more than three thousand years, from Hippocrates down to Albinus, from the Greeks to the moderns, and seem during this time to have done nothing more than multiply doubts and hypotheses.

Without being very sanguine in my hopes of discovering the first principle of nerves, I have undertaken the investigation with ardour, persuaded that the knowledge I have of their winding fibers, must be extremely useful to me in so difficult a search.

I began my observations on a very small nerve, which I had stripped of the cellular [i.e., connective tissue] membrane. I observed the winding fibers narrowly with a very strong lens, and determined the size of them. This done, I divided the nerve toward its extremity in a longitudinal direction, by means of a very sharp needle, and divided the parts or threads of it, separating one from the other. I immersed the nerve in water, in which the threads floated. After several useless attempts, and several observations either suspicious or inconstant, I at length succeeded in finding many very small cylinders, more or less transparent, seemingly composed of a pellicle, and partly filled with a transparent, gelatinous humour, and with small unequal globules, or bodies.

* * *

The primitive construction of the nerves is as follows: a nerve is formed by a great number of transparent, homogeneous, uniform, very simple cylinders. These cylinders seem composed of a very fine uniform tunic, filled, as far as the eye can judge, with a transparent, gelatinous humour, not soluble in water. Each of these cylinders receives a cover in the form of an external sheath, which is composed of an immense number of winding threads. A very great number of transparent cylinders form together an almost invisible nerve, presenting the exterior appearance of white bands; and several of these nerves united, form the larger nerves seen in animals.

I am fully convinced by my own observations, repeated a great number of times with the same success, that the cylinders I have described, are the simple and first organical elements of nerves, for I have never been able to divide them farther, whatever trials I made with the sharpest-pointed needles. I could easily tear and rend them here and there, but they always remained simple as before. I could strip them of their sheaths, and separate the winding cylinders of which these were formed, although they were very small. The primitive nervous cylinder then appeared transparent, homogeneous, and every where of equal diameter. We see by this how much even the best anatomists were in general mistaken, when they maintained that the nerves were divided and subdivided without end, without there being any hope of ever seeing, or coming at a knowledge of, their first threads, or first organical elements.

Fontana then wrote that he reserved the examination of the matter of which the nerves are composed, or filled. This he must have done quite soon, as he wrote about it to his friend Gibelin, which letter[25] was published in 1783 in Italian, in 1784 in French. Two pages of manuscript, not in Fontana's hand, cc 27r-28r, Vol. 75, Ms Palatino 1197, Biblioteca Nazionale Centrale, Florence, are in French and resemble closely but are not identical with the section of the letter to Gibelin describing the contents of the nerve cylinders which appeared in 1784 in Rozier's journal. This contribution of Fontana is discussed more fully below.

Brain and Retina. Fontana wrote:[11]

After having examined the structure of the nerves, and their first organic elements, order requires me to turn my attention to the brain, whence they draw their principal origin. We know that the brain is composed of two substances, named cortical and medullary, which are distinguished by their respective color.

I have already related the various opinions of authors as to the structure of the brain. Some believe its substance to be altogether vascular, and others will have it not to be so. There are some who suppose it simply composed of blood vessels [Ruysch]; others on the contrary believe it formed of vessels much smaller again than these.

Malpighi believed the brain, as well as all the other viscera in the body destined for particular secretions, to be glandular. Of the more modern observers, some believe it to be formed of simple globules [Leeuwenhoek, Della Torre, Prochaska], others of a non-organical and spongy pulp [...] the medullary substance of the brain is not a simple collection of venous and arterial vessels [...] it is not simply formed of spheroidal globules or corpuscles; but... it is an organized, particular substance, composed of irregular cylinders, or transparent canals, which fold as the intestines do, and which I shall call the *intestinal substance,* on account of the shape in which it is seen.

[The cortical substance] seemed to be formed of an irregular web, granated here and there, which I should have supposed a cellular membrane, if I had trusted to a simple inspection. Besides this substance, were very small, irregular, transparent, spheroidal corpuscles, which seemed filled with a gelatinous humor, and which were not soluble in water; they were smaller than those I had observed in the medullary substance, but in every other respect like them [...] The observations I have since made have more and more convinced me, that the structure of the cortical substance of the brain, is as I have described it and that it does not differ sensibly from the medullary substance, although their color makes them appear so distinct.

Fontana also recorded in his *Treatise* many observations on the retina. Clarke and O'Malley[29] give as their opinion that he did not advance beyond previous descriptions of these structures. Rasmussen[30] stated that the rods and cones of the retina were described by Treviranus (1776-1837) in 1835, but that Fontana probably saw nerve cells in the retina fifty years previously and Leeuwenhoek may have seen them by 1674.

Reproduction of the Nerves[11] (Fig. 26). Rasmussen wrote:[30]

Observations on the structure of severed nerves date [...] [from] those of Fontana, who had failed to get nerves to reunite with real nerve tissue until he restudied the subject under Cruickshank at London during 1778-79. Two years previously Cruickshank had reported the healing of cut human nerves and had a demonstration preparation of a dog's vagus nerve (vago-sympathetic trunk?) from which about one inch had been removed but which appeared fully restored, although the material bridging the gap seemed somewhat different from the rest of the nerve (to Fontana). In Fontana's experiments the sciatic nerves of rabbits even thirty days after section, while apparently united to gross appearance, failed to contain true nerve fibers in the scar upon microscopic observation. But with the vagi, both after simple section and after removal of a segment, he thought he could identify real nerve fibers in the tissues bridging the gap [he also noted that the spiral bands reappeared], although he could not follow particular nerve fibers through the entire defect [...] He was not sure that all cut nerves would return to normal though he fully realized that functional restoration was evidence of the existence of nerve fibers through the scar. He failed to get regeneration when a segment was removed but replaced in the reverse direction.

Muscular Substance. Bowman[31] wrote his classical description of striated muscle: «A more common opinion is that these threads are bundles of beaded fibrillas, whose beads being placed side by side, cause the appearance of transverse lines, a view which was first entertained by

Fontana, although his claims to it have been often overlooked. Of the transverse striae [...] Fontana was the first to give what has seemed to me the correct explanation of their nature».

The Crystalline Lens. Fontana published three times[32] on this subject, in a letter to his friend Darcet. The form in the *Opuscoli Scelti* is the most brief; here he described his observations with the microscope, best made on young animals such as nursing rats before the eyes have opened, or on newly-hatched chicks. He saw the lens to be composed of small, solid, transparent cylinders, arched but parallel to each other. Thus he described what we see now as rows of columnar epithelial cells which are transformed into the lens fibers, each one a hexagonal prism. These cylinders were seen to be bound to his tortuous cylinders present in abundance, for which he suggested the hypothesis that they constituted the basic origin of lymphatic vessels.

In the same letter in the *Raccolta Ferrarese* a long postscript is found, supporting his hypothesis by analogy with the intestinal villi, of similar dimensions, which serve the function of absorbing the chyle. The postscript continues with the added hypothesis that the tortuous cylinders also serve for transport of airs and vapors; this addition is omitted from the version published the following year in Rozier's journal.

Fig. 27 – From the *Traité sur le venin de la vipere* etc., Florence 1781, Plate I, lower section only. Translation of Fontana's caption from the *Trattato del veleno della vipera* etc., Naples 1787: In Fig. 1, *mm* is part of a single hair; brown spots are seen in the center; all the surface appears to be covered with tiny tortuous cylinders, to some extent parallel to each other. Fig. 2 shows a small segment of the same hair which has been compressed strongly between an iron plate and a glass slide. Fig. 3 is a fragment of the above in which are seen very tiny globules detached from the tortuous cylinders. Fig. 4 is another fragment of the same which, having been soaked in water, appears as a transparent irregular pellicle as seen in Fig. 5. Fig. 6 shows the globules of transpiration. Fig. 7 is a blood globule, seen with the same lens as used for Fig. 6. Fig. 8 shows an amassing of the globules which make up the gluten of the skin of eels. They appear as if they were vesicles filled with infinitely small globules. Fig. 9 is a repetition of the same mass of globules of Fig. 8, allowed to dry out somewhat. Within each globule, variously situated, is a tiny body. Fig. 10 shows one of the globules of Fig. 9 which had a central ovoid body also spotted at its center. Beside it is placed body *c*, which is one of the blood globules for comparison of size. The drawings for the plates of the *Treatise on the venom of the viper* were done by Giovanni Fabbroni, according to Fontana (*Trattato, op. cit.*, 1787, vol. I, p. 174, footnote *a*).

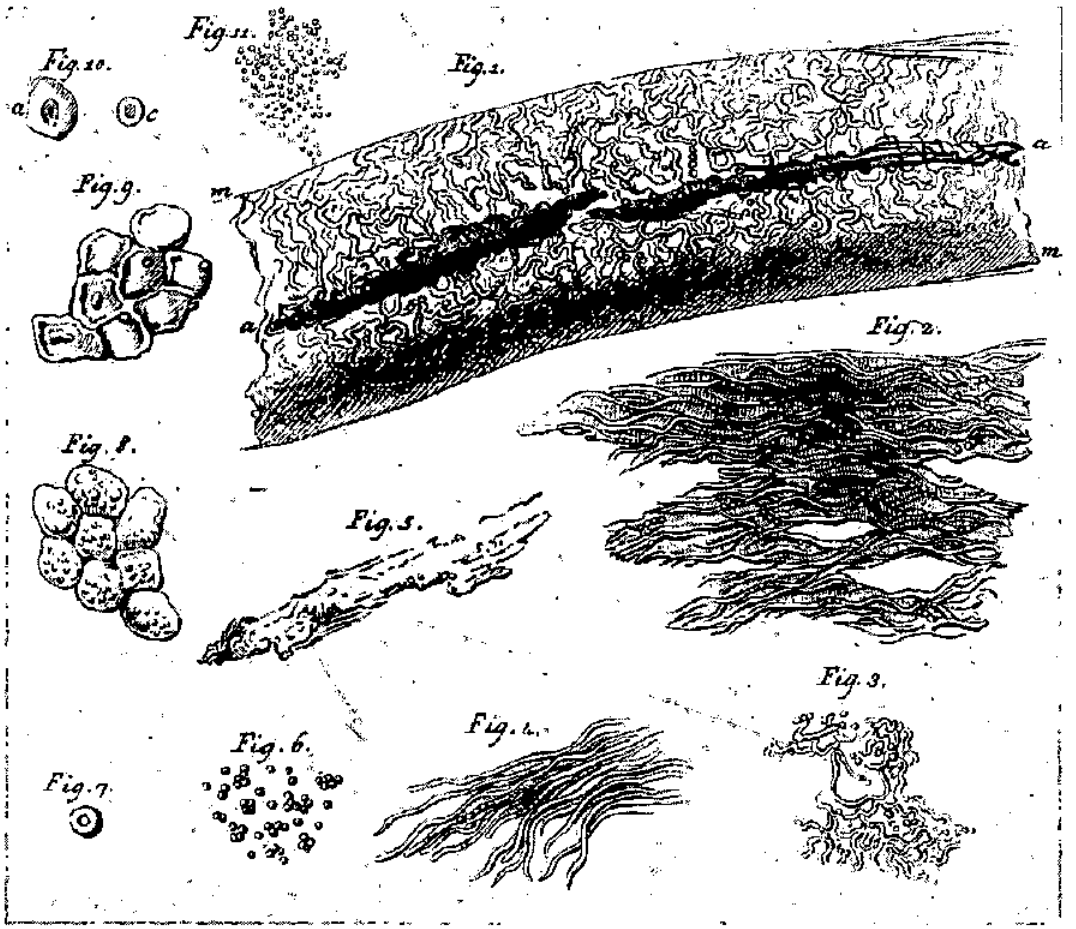
Fig. 10.
a
c
Fig. 11.
Fig. 1.
a
m
m
a
Fig. 9.
Fig. 2.
Fig. 8.
Fig. 5.
Fig. 3.
Fig. 4.
Fig. 6.
Fig. 7.

On the gluten of eels

On pages 295-96 of Vol. II of the Skinner translation of the *Treatise,* is the following:

> Being desirous of examining the gluten of the skin of eels, I had several brought me of different sizes, and found, after taking it in a very small quantity, and diluting it a little in water, that it was formed of uniform irregular bladders, filled with very minute spheroidal corpuscles, as they are described by Fig. 8 [in our Fig. 27].

Subsequent comments on Fontana's observations have been various. A classic study in modern times of the nucleolus was that of Montgomery.[33] It stated, «Fontana (1781) was the first to figure the nucleolus in the nucleus [...]»; Montgomery was in fact not citing Fontana himself but J. B. Carnoy, *Le biologie cellulaire,* Lierre, 1884, which we have not seen. Adamstone and Taylor[34] repeated this statement. Hughes[35] wrote:

> The first observation of the nucleus in an adult animal cell, other than in a blood corpuscle, was made in 1781 by Felix Fontana, who, in a book mainly on the viper and its venom, includes at the end a section on miscellaneous observations with a microscope, among which is one on the slime from the skin of an eel. Within this substance he saw globules which were epithelial cells, and inside them again he saw an oviform body, the nucleus. A spot *(une tache)* within this may well have been the nucleolus.

Hughes also wrote that recognition of the nucleus of the plant cell was established in 1833 by Robert Brown (1773-1858); also that in 1836 Valentin (1810-1883) suggested that the arrangement of cells and nuclei in the choroid plexus compared closely with that in plants; in 1839 he named the nuclear body the nucleolus.

Moller and Westergaard,[36] learning with surprise of Fontana's work, raised the question, do nucleoli really exist in the mucous cells of the skin of the eel? With fixation of such material and with phase contrast microscopy they found a clearly affirmative answer, concluding that Fontana did give a correct and the first illustration of the nucleolus, and that his primitive microscope may have given some phase contrast effect. They added that Fontana was the first also to describe nuclei in cells other than the erythrocyte, where a nucleus had been located by Leeuwenhoek and numerous followers, leading to much confusion until E.H. Weber (1795-1878) pointed out in 1841 that while the erythrocytes of some animals are nucleated, those of mammals are not.

Garrison Morton[37] assigns the discovery of the cell nucleus to Robert Brown in 1831, and the description of the nucleolus to Rudolph Wagner (1805-1835) in 1835.

Fontana's descriptions and illustrations are primitive, but the structures are there and he saw and noted them; testimony at least to his perception.

FELICE FONTANA AS ANATOMIST

Had Felice Fontana worked only as anatomist, his contributions would have entitled him to a place in the first rank of eighteenth-century scientists. His merit was recognized by his senior Haller, his junior Girardi, and succeeding anatomists of the nineteenth century. His publications in gross anatomy are few, as his labors were for the purpose of creating the anatomical models, which remain as testimony to his clear vision and detailed understanding of human anatomy. His published discoveries were in the field of microscopic anatomy, the most important of which were those on nerves.

His first sight, with the unaided eye, of the structure of nerves was of the spiral band, his explanation of the nature of which being fully confirmed by Zanobio[27] and Clarke and Bearn.[28] The latter wrote: «We would emphasize the significance of Fontana's analysis of the spirals which is worthy of very high praise». They cite subsequent mention of them, as by Alexander Monro *secundus* (1732-1817) in 1783 (he did not mention Fontana); Sömmering (1755-1830) who saw the bands and cited Fontana in 1783; Michaelis who in 1785 did not see them himself but called them the «spiral bands of Fontana»; Arnemann, 1787, who confirmed Fontana's findings and reviewed the writings on the subject; Hyrtl (1810-1894) who called them «Fontana's spiral line» in 1853. From 1810 to 1860 the spiral nerve bands were frequently referred to, then mention of them became rare, probably because the simple methods of observation of fresh tissue, such as Fontana used, were no longer common.

As for the discovery of the primary unit, the nerve fiber, opinions have varied. One reason for caution on the part of other investigators has been the long period of authoritative but mistaken opinions on the subject. Haller in his *Primae linae physiologiae* (1747) did not dismiss the ancient scheme of the hollow nerve, suitable for the conduction of animal spirits, nebulous though they might be. In 1702 Mead «recorded his suspicion that they [hollow nerves] conveyed the poison of the viper [...]

to the brain [...] Once again the hollow- or porous-nerve concept was used for a hypothesis that eventually would be disproved (by Fontana in 1781) by means of a series of conclusive experiments [...] But there were two investigations in the eighteenth century, the results of which were readily available to all, which pointed to the future».[38] In 1717 Leeuwenhoek saw and illustrated the single myelinated nerve fiber, the center of which (the axis cylinder or axon) he took to be hollow. According to Liddell,[39] Leeuwenhoek thought in 1722 that nerve fibers were composed of globules arranged in a line. «Of greater significance,[38] was the second discovery, made by Fontana in 1779. Like Leeuwenhoek he observed the ultimate nerve fiber, but unlike his Dutch predecessor he realized that he had done so».[38]

Fontana was right, nowhere more so than in his description of the contents of the nerve cylinder in a communication amazingly similar to that of J.Z. Young in 1934,[40] as has been pointed out by Hoff.[41] Young wrote:

> The giant axons provide very suitable material for study of the living nerves. No definite neurofibrils could be seen in them, but under the highest powers of the microscope a very fine longitudinal striation could be detected. This appears to be due to the presence of large numbers of exceedingly small granules, almost at the limit of visibility, which are oriented in rows along the fiber. The rows are not straight but wavy, making an inward curve, for instance, where a sheath nucleus projects into the axon.
>
> After a giant fiber has been cut the contents flow out from the end of the connective tissue sheath. This remarkable process continues for many minutes as the axon pours slowly from the tube, which itself collapses like a deflated balloon. The substance which emerges forms an irregular mass at the cut surface, not mixing with the sea water unless disturbed. The fine granules can be seeen to lose their orientation as the liquid emerges, so that no trace of fibrillar arrangement remains in the resulting mass.

Fontana had written (Hoff's translation):

> Having decomposed a very small nervelet into its ultimate nervous filaments, composed of different primitive nervous cylinders, of which I have spoken all along in my book, I succeeded in stripping from the last internal envelope, or tortuous filaments, several of these primitive nervous cylinders; they were transparent, homogeneous, not empty, and as I had found them on other occasions.
>
> The idea came to me to place them between two crystals, arranged in such a way that I could make them approach one another, to the point of touching their opposing faces, and to compress completely the material placed between them. I can bring a very strong lens close to the upper crystal, which is the thinner, and this

lens serves me to observe what happens as the glasses approach each other by insensible degrees and compress the objects. This little instrument, which I devised and had made several years ago, is of a very great utility in the most delicate microscopic observations, and I owe to it many important truths that I shall not have known without its help, as will be seen one day in my Microscopic Observations.

Liddel has written:[39]

Observations such as these were made then and for some time afterwards by smearing the semi-fluid tissue on the glass microscope slide and pressing down of cover-slips [he is speaking of the observations on brain of Rolando in 1809]. Mechanical devices called «Compressoria», which were thus used for the pressing-down of cover-slips, became general in the nineteenth century.

He did not mention the origin of this practice, which may have rested with Fontana. Fontana continued:

Examining thus a little mass of different primitive nervous cylinders with my instrument, I noticed that in proportion as I approximated the two sheets of crystal, there ran out of these crushed filaments a glutinous, elastic, transparent material, which the water in which the filaments bathe could in no way dissolve. When two or more neighboring cylinders were compressed together, the glutinous materials of the one did not mix at all with those of the other, although these different materials were mutually compressed, and the one pushed aside the other.

Continuing to compress the cylinders more and more, I came to notice that the glutinous material decomposed itself into little round grains of a diameter four or five times less than a red blood globule. I saw on this occasion that many of the little grains flowed with great celerity in the center of the primitive cylinders, and ran out at the cut ends of these cylinders. In this state, the water of the slide transported them with great rapidity from one place to another, and they did not reunite to form the glutinous material. This glutinous material, examined with the strongest lenses, at the moment when they [the grains] leave the primitive nervous cylinders, seemed to be formed of granular filaments, tenacious and elastic, which the water could neither dissolve nor separate. I confirmed this observation several times on several kinds of nerves, in several animals, and even in man, so that I do not fear to certify to its verity. It is thus necessary at present to consider the nervous cylinders as true canals, containing an elastic, glutinous, graniform material; that at least is where observation leads us.

Clarke and O'Malley[29] have criticized this description; «Fontana had no idea of its precise origin or of its function, for he naturally interpreted it in terms of the then current concept of animal spirits». This is manifestly unjustified in view of Fontana's paragraph in this paper:

Thus the existence of these imaginary animal spirits flowing from one part to another does not seem to be compatible with the observations that I have just reported, and the vibration of nerves is contrary to experience and to the structure of the nerve itself

as well as his avowed intention to read animal spirits out of existence.

A subsequent investigator of the structure of nerves, not far removed from the time of Fontana, was Robert Remak (1815-1865), cited as follows:[39]

Remak's oft-quoted work in 1838 represented a thesis for the University of Berlin entitled «Observationes anatomicae de systematis nevrosi structura». Fontana, as Remak recalls, had seen as early as 1781 that «primitive» nerve fibers had two parts – the external part being knotted and the internal part being composed of a special membrane which was transparent, homogeneous, and filled with a gelatinous fluid. This latter observation was one of the first of its kind, and was important and so often to be repeated by later authors because it showed that no canal could exist inside nerve fibers along which Galen's «animal spirits» might pass to activate the muscles. So from that time the ancient belief began to totter.

In 1842 another thesis was presented to the University of Berlin, this time by H.L.F. Helmholtz:[39]

Fibers, he found, are clear cylinders with a simple external membrane and have liquid contents which exude when the nerve is compressed; the observation which, it seems, could hardly be mentioned too frequently.

Pacini (1812-1883), moved by the disrepute into which Fontana's writings had fallen in his own country, and for other reasons which will be discussed later, published[42] a long defense in 1848, but the article is purely polemic.

By 1852, Kölliker (1817-1905) had become a leading authority on the finer structure of the nervous system. His *Manual of Human Histology,* appearing in many editions and languages, was cautious about the discovery of the nerve fiber:[38]

[the primitive nerve tubes of fibers consist of] entirely distinct component structures, viz. of a delicate coat and a viscid fluid, in the center of which is a soft but elastic fiber. The coat or sheath of the nerve fibers (limiting membrane of Valentin) is excessively delicate, flexible but elastic, perfectly structureless and transparent membrane. Within the structureless sheath lies the nerve medulla or pulp («medullary sheath» of Purkinje and Rosenthal, «white substance» of Schwann, 1838) in the form of a cylindrical tube and exactly surrounding the

central fiber... The central or axis fiber of the nerve tubes («cylinder axis» of Purkinje and Rosenthal, «primitive band» of Remak, though perhaps first seen by Fontana) is a cylindrical or slightly flattened filament which, entire and unaltered, is as little recognizable as the sheath, being surrounded by the pulp and possessing the same refractive power, whilst it comes readily into view when the film is torn or treated with various reagents.

In 1890 Cirincione reviewed the writings on the subject of the nerve fiber before and after Fontana, and defended his discovery. He added a consideration:[43]

One unfamiliar with the drawings of that period will smile seeing Fontana's figures; instead I find them admirable. To convince one of this, two things suffice: one, that they represent unstained preparations in water, probably without a cover-slip and observed at great magnifications disproportionate to the power of the lenses; the other, that they were made by a person of little competence.

Fontana himself had written:[11]

It is not possible to meet with an engraver who can express with the graving tool, all the little indeterminate strokes, which characterize the truth of the object, and which are not neglected by the person who at once delineates the object, and observes it with the microscope.

In 1958, J.H. Scharf wrote[44] in the authoritative Möllendorf Handbook: «the era of neurohistology begins with the discovery of the nerve fiber by Fontana». By 1968, Clarke and O'Malley[29] expressed some doubt: «It has been claimed by Zanobio and Brazier that Fontana described the axis cylinder and the enveloping myelin sheath; however, it is difficult to be certain that this was so». However, it seems that Fontana did see and describe the structure of nerves as clearly as any of these authorities, more clearly than most, and before them.

In the section «The Primitive Winding Cylinders of the Animal Body, or on the Cellular Membrane» of his *Treatise* (ref. 11, vol. II, pp. 272-76), Fontana related his most seriously erroneous venture into microscopy (Fig. 27). He wrote:

The primitive winding cylinders I have discovered in the cellular membrane[d] of the nerves, tendons, and muscles, are of all the parts or organs that I know of, in the animal body, the smallest.

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[d] For this we would say, areolar connective tissue.

We first observed them in the external cellular membrane of the nerves, tendons, and muscles. I have since shown how they may with ease be found in all the cellular membranes of these organs; so that their whole cellular structure is a web of winding canals.

We already see that according to this, a great part of the animal solids is composed of winding cylinders; it remains to examine whether these cylinders are found in other parts.

It will be sufficient to say that I have found the whole cellular substance to be formed of winding cylinders, in whatever part of the body it is met with.

All the cellular substance, the sacculi, and the vesicles which contain the fat, are a web of these winding cylinders. In a word, I know of no part in the body which has a cellular membrane, and does not present these winding cylinders.

One general purpose of these winding cylinders may be, if they are really vessels, to nourish the parts in which they are found, or which are surrounded by them. According to this hypothesis, they may perhaps serve for the nutrition of the primitive cylinders; as well nervous, as tendinous and fleshy. But there is another purpose, still more noble, and perhaps equally important, to be ascribed to them; the principle functions of life may even depend on them: the smallest changes made in these organs, may cause the utmost disorder in the animal economy.

My experiments on poisons have shown that they bring about the death of an animal in an unknown way, and we seem to need the discovery of a principle, an organ in short, on which these poisons act. Who knows but this principle, this organ, may consist in the winding cylinders we have observed? But what can be expected from an insensible substance, and one on which poisons do not seem to have any action?

Zanobio[45,46] has examined this situation searchingly. He shows how Thomas Willis (1674-75), Alexander Monro *secundus* (1783), Paolo Mascagni (posthumously 1819-1821), and Joseph Berres (1796-1844) all represented in their works the same filamento-reticular structures described by Fontana, in an amazingly similar manner. He has repeated these observations with modern techniques, and has concluded that these images «have no structural significance [...] [are] not an expression of structures really existant, but due to complex optical phenomena», that is, they are an optical illusion.

Majno and Joris[47] have repeated these observations, noting that Fontana represented fat cells both with and without the artefact of the reticular image.

Zanobio also discusses the frequent observation, under many circumstances, of globules, an error of which Fontana was not free (Fig. 27).

Perhaps the greatest oversight of Fontana as histologist was his failure to recognize smooth muscle. We saw that he (and many others) overlooked the ciliary muscle and denied the presence of muscle fibers in

the iris. In his *Animal Physics* (Chapter III, Sections 20 and 21) he had discussed intestinal motility and the presence of circular and longitudinal muscular coats; in fact as early as 1756 he had observed the intense motility of esophagus, stomach, and intestine of animals soon after death, which he interpreted as a convulsive phenomenon. He also discussed there the motility of the uterus. The principle of Hallerian irritability required that these organs be muscular, but Fontana could not be expected to see the cells of smooth muscle, minuscule in comparison with those of striated muscle, in his unstained preparations. The nature of smooth muscle was not to be clarified until 1847-48 when R.A. Kölliker wrote:[48]

> The elements of the smooth muscles were formerly universally regarded as elongated bands containing many nuclei, which were supposed to be developed by the coalescence of numerous mutually applied cells. In 1847 I showed that this is not the case; that, on the other hand, the elements of these muscles are only modified simple cells; and at the same time demonstrated, that these contractile fiber-cells occur wherever contractile connective tissue had previously been assumed to exist, and also, that they are to be found in many localities in which their presence had not been suspected. These views [...] are now universally confirmed [...]

CHAPTER 17

ANATOMICAL MODELS

In Wax - In Wood - The Waxes for Vienna - Some Responses to the Waxes - The Models for France - Other Anatomical Waxes from La Specola

> We are firmly and Tuscanly convinced, even at the cost of seeming archaic, that scientific research is neither in contrast nor should be detached from artistic work and that, to paraphrase Leonardo, «man is not worthy of praise if he doesn't tend to be universal».[a]

What more effective testimony to the universal nature of Felice Fontana's genius than the anatomical models?[1,2,3,38]

Every advance in science and art is built on its antecedents; in this case they were numerous. Images, waxen and other-wise, have been with us since antiquity.[4] Their history has been closely, although by no means exclusively, linked with the city of Florence, so that their coming to be Fontana's major occupation is understandable.

Among the oldest images were those offered to the Gods, in propitiation, begging a favor, or in thanksgiving. These were made of many materials but often of wax, it being easy to cast, to color, and to carve into a semblance of human flesh; These *ex voto* offerings, called «bôti» in Tuscany, covered the walls and hung from the ceiling of the church of S.S. Annunziata in Florence until the sixteenth century, so densely that the first impression on entering was of a cemetery rather

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[a] From the address of welcome to the First International Congress on Ceroplastics in Science and Art. by Benedetto Lanza, Professor of General Biology; Director, Museo Zoologico «La Specola» of the University, Florence.

than a church. Nearby were many shops of the «image-makers» or «wax-workers»; an archbishop of Florence in 1533 was advised to stay away from them, as he was so «thin, light, yellow, and empty».

The Italian artists of the Rinascimento became interested in wax models for several reasons. First, they wished to know more of anatomy, second, wax was easily worked, third it made possible certain techniques such as that of *cire perdu.* Leonardo, Michelangelo, and Raffaello all practised dissection for the study of anatomy, and Michelangelo is outstanding for the number of wax models which he left.[5] His «notomia» which disappeared in the eighteenth century was probably the prototype which inspired later Florentines.[6]

Numerous examples of anatomical waxes were the *écorchés,* or «muscle-men», and the finest of these[7] was that of «Cigoli», dated 1598. Lodovico Cardi (1559-1613),[8] called Cigoli after his birthplace in Tuscany, was a pupil of A. Allori who kept jars of pickled human extremities in his studio, and of Ostilio Ricci who introduced the young Galileo to mathematics. Allori's pupils were critical of the tendency to exaggerated departure from exact representation, and they abandoned purely decorative art. They turned to wax models, appreciating the difficulties of trying to use deteriorating pieces of actual anatomy.

The next forerunner was a strange figure, Gaetano Giulio Zummo.[9] Born at Syracuse in 1656, perhaps the son of a slave, he received a simple education and was apprenticed to an artist. Reaching Naples in 1691, there he created his three most famous wax-works, figuring the plague. These repellent depictions of death and decomposition, correctly modelled anatomically, only tolerable because of their miniature size, drew the notice of Cosimo III de' Medici who sponsored him in Florence where he executed two similar pieces, one depicting The Syphilis, the other The Triumph of Time. By 1695 Zummo had moved to Genova where he was associated until 1700 with Guillaume Desnoues for the purpose of executing, showing, and selling, anatomies in wax. Moving then to Paris where he had great success, he changed his name to Zumbo as more sonorous; he died in 1701. While in Florence, Zummo had executed two anatomical studies of the human head. These, one of which is in Florence, one in Paris, show his skill in representing anatomy. His other creations, now at La Specola in Florence, were works of art notable, not as necrophilia or romantic decadence, but as symbols of the pathetic, tragic drama of human existence.[9]

Before this time, it had been realized that figures in wax could be exhibited to the public for profit, beginning at least as early as 1611

when Michel Bourdin sued François de Bechefer for damage done to his wax portrait of the murdered Henri IV.[10] An extraordinary success was achieved by a Swiss, Christoph Creutz (Philippe Curtius), who began to exhibit waxen figures in Paris around 1783; he taught his art to his niece Marie Grosholz (1760-1850) who as Madame Tussaud took her museum in 1802 to London. As she had learned by modelling the still-warm heads of the governor of the Bastille and the victims of the machine of Dr. Guillotin, her success was assured; her museum has been the prototype.[10, 11]

What is the fascination of these repellent objects? Is it the «ambiguity of lifelike figures [...] [that is] responsible for the uneasy feeling [...] verging on repulsion and disgust experienced by the average onlooker»?[12] Does that «creepy halflife which all wax-works share [proceed] from our conviction that they are uneasily aware of their own mere waxiness»?[13] As we observe them, do we suspect that they are looking at us?

These sentiments appear not to have troubled the makers of models purely for the science of anatomy. The first attempts at providing material for study made use of actual anatomical specimens prepared and preserved by injections,[14] as carried to a high degree of perfection (with methods kept secret) by Frederik Ruysch (1638-1731) in Amsterdam. As noted before, such preparations were unsatisfactory, as they deteriorated. In Bologna, the artist Ercole Lelli (1702-1766) molded muscles from «tow impregnated with wax, mustard, and turpentine» and laid them on a real human skeleton. Further development of models in wax and clay by Lelli, Giovanni Manzolini (1700-1755) and his wife Anna Morandi Manzolini (1716-1774) was encouraged by Giovan Antonio Galli (1708-1782), Bolognese lecturer in surgery and obstetrics.[15] Of these models, which were used by Galvani for teaching anatomy, about 105 survive today.

Galli's collection was seen in Bologna in August 1770 by Giuseppe Galletti, teacher of surgery and obstetrics at the hospital of S. Maria Nuova in Florence. Returning to Florence with the desire to emulate Galli, he found a sculptor, Giuseppe Ferrini, Livornese, who made for him several models of obstetrical interest in wax and clay. By the end of 1771, Ferrini had moved to the R. Museum of Physics and Natural History and was making wax models for Fontana. It was probably he whom Ferber saw at work.

So began an industry of anatomical waxes which, according to much testimony, was the creation of Fontana, although many dissectors,

modellers, and artists were employed, notably Clemente Susini (1754-1814) to whom are attributed the most excellent models. When the Museum opened in 1775,[16] there were six rooms containing anatomical waxes; three full-size statues, 137 cases holding 486 preparations, 208 colored drawings, and 177 pages of descriptions. Today[17] there are seven rooms (about 700 m^2) containing 19 life-size bodies, and 543 cases holding more than 1400 models, each case with one or more water-colors and as many as thirty pages of description (Fig. 28-32).

The Waxes for Vienna. On his first visit to Italy in 1769, the Emperor Joseph II saw the anatomical waxes in Bologna; those which he saw later in his brother's museum in Florence instilled in him the great desire to have his own collection in Vienna. The painful sequel is told in Fontana's memoir. Apparently some of these waxes were made at the Museum, some in his home. At last, in 1786[b] 1,192 pieces went to Vienna by mule back; there were sixteen large figures and 353 cases of smaller ones. Subsequently two more shipments were made, one of 1788 amounting to sixteen cases each containing two to six cabinets.[19] Fontana complained in 1789 that this shipment had never been paid for, but he still sent a third that year.[20] The preparation of these waxes seems to have been complicated by the interference of Mascagni, according to Giovanni Fabbroni.

The cost to Joseph II is said to have amounted to more than 30,000 fiorini; but an accountant at the R. Museum felt that he had received more than he had paid for. For his labors, Fontana received a title and a snuff-box.

These waxes, except for a few sent to Budapest,[21] have been housed in the Josephinum, constructed in 1785 for the School of Military Medicine. They suffered repeated damage from 1797 to 1961, but in 1962-66 they were magnificently restored, made possible by the receipt of 15,000,000 schillings from the State and a gift of L St. 5,000 from the Wellcome Trust.[22] Kept in a large room in a beautiful building, they make a powerful impact on the visitor.

Some Responses to the Waxes. Much has been written about the quality of these wax models. Scarpa, himself an anatomist, was one of the first to

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[b] Perhaps spurred on by Joseph's letters to Peter Leopold.[18]

Fig. 28 – The «Reclining Venus», life-sized model in wax. At La Specola.

Fig. 29 – The same, with the outermost front layer reemoved.

Fig. 30 – The same, with the second layer removed.

Fig. 31 – Life-sized male figure in wax. At La Specola.

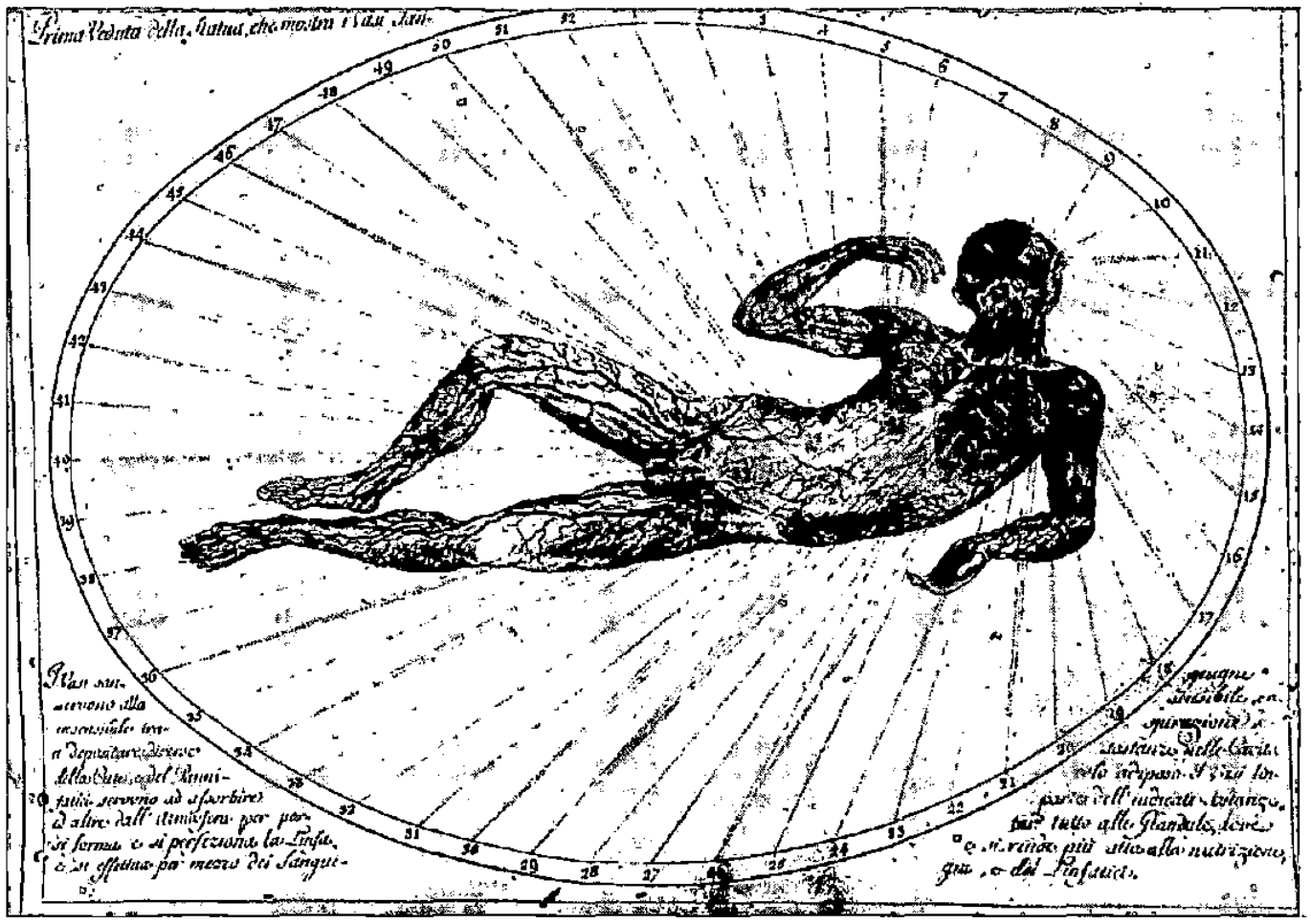

Fig. 32 – Drawing accompanying the same.

praise them, in glowing terms. Adolf Murray, the Upsala anatomist who visited Florence, wrote about them:[23]

> In fact, who, on entering the museum of this most puissant Grand Duke, established in the Palazzo Torrigiani in Florence, which includes a wide range of all natural history and physics, will not admire its richness, orderliness, and elegance? But above all the student of anatomy will delight in finding in this temple of Minerva, exceeding all expectation, many parts of the body reproduced in wax. He who studies the anatomical preparations of Ercole Lelli, Manzolini of Bologna, Verniani[c] of Luis, Biheron[d] of Paris, swarming with errors, has every right to condemn and deplore this method of imitation of nature but – he will change his opinion when he examines the preparations made in Florence. Not only, in fact, will he note in them supreme elegance and clarity, but also that they are put together with complete accuracy, according to nature itself as shown in the figures of Haller, Albinus, Meckel,[e] Zinn, Neubauer[e] and others.

The most extended analysis of the «artificial anatomy» of that time was made by Fontana's friend Desgenettes.[27] His discussion began with antiquity (Hippocrates), and continued with Leonardo, Raffaello, Lelli, Houdon, Desnoues, Galli, Biheron, Pinson, Laumonier, W. Hunter, and Curtius. He wrote:

> The comprehensive collection of anatomical waxes of Florence [which he enumerated and described in detail] is infinitely superior in every possible respect to all the other collections spread throughout the rest of Europe.
> The Florentine anatomical waxes cannot be reproached with having copied nature altered and disfigured by sickness and decay. Everything has been taken into account, and those who thought they could criticize them because they do not have the hue of the cadaver are wrong in looking for dead and corrupted nature where the aim was to portray it in the healthy living state.

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[c] Unknown.

[d] Marie-Catherine Biheron (1719-1786).[24] Her father, a Parisian apothecary, she lost at an early age. The redoubtable Madamoiselle Basseporte[25] gave her the idea, seemingly so unsuitable for a young lady, of «artificial anatomies», i.e., wax reproductions of the human body. To this work she devoted 47 years, exhibiting her products to the public and to the Academy of Sciences, eventually selling some of them to Catherine of Russia, with the help of Diderot. «So life-like were the representations that the Scottish Surgeon Sir John Pringle is said to have remarked that all they lacked in being natural was the stink».[26]

[e] Johann Friedrich I (1724-1774), his son Philipp Friedrich Theodore (1756-1803), and his grandsons Johann Friedrich II (1781-1833) and August Albrecht (1790-1829) were all eminent anatomists.

Although Desgenettes had not seen the collection since 1789, he now (1793) knew that it had been enlarged and was in favor of having a similar collection placed in each of the nine state schools proposed to the National Assembly by Condorcet, these to be copies of that in Florence and to be prepared there.

Goethe knew the Florentine waxes and thought highly of them, so much that in *Wilhelm Meister's Wanderjahre,* III, *3,* 18-32 he «raised plastic anatomy to the status of protagonist».[2] On 4 February 1852 he wrote[28] to Beuth in Berlin rejoicing that a new institute had been established, recalling his earlier novel, and adding (much more besides):

> Plastic anatomy is the subject; in Florence it has been carried on at a high level for a long time, but nowhere else can be as successfully undertaken as there, where science, art, taste, and technique are by nature fully active. Should the promotion of such a place in Berlin be proposed [but] not immediately... send an anatomist, a sculptor, a modeller in plaster to Florence, for there this special art is taught.

Thomas Jefferson Hogg (1792-1862) wrote in 1827:

> The true wonder of Florence is the collection of anatomical models in wax, at the Spegola; their number, beauty and perfection are astonishing. All the various systems of the animal economy are exhibited with perfect accuracy, as large as life, are shown as well by the most careful dissection. Nothing shocks the senses; on the contrary, a certain beauty of imitation and gracefulness in displaying and disposing the part and limbs, is even pleasing.

Not all responses to the anatomical waxes have been favorable. An unusual one[29] was that of Lady Blessington:[f]

> I entered the Gabinetto Fisico today, and though I only remained a few minutes in time I carried away a sense of loathing that has not yet left me. Surely some restriction should exist to preclude men and women from examining these models together! I entered with a female companion only, but retreated when I observed men and women; some of them, too, young ones, contemplating objects which, although highly useful for scientific purposes, are certainly of a character unfit for

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[f] Marguerite, Countess of Blessington (1789-1849) was born in Tipperary. Leaving the Captain Farmer to whom she had been forcibly married by her parents, she went to London where she met and married John Gardner, first Earl of Blessington. After his death she supported herself by writing, the «Idler in Italy» being her most successful work. In 1849 she fled to Paris with Alfred Count d'Orsay with whom she had long been associated.[30]

> this promiscuous exhibition. It is meet that we should know that we are fearfully and wonderfully created; but not that we should witness the disgusting details of the animal economy in all its hideous and appalling nakedness and truth. What a lesson for personal vanity does this exhibition convey! Yet probably few view it in this light. For me, I hold that its fearful images will recur to my memory when I behold some creature, in the zenith of youth and beauty, who almost believes that she is not formed of the perilous stuff so shockingly delineated in the Gabinetto Fisico.

Schiff wrote:[31]

> Also in Florence in times not too remote a professor's judgment was: any preparation for examinations by studying the waxes at the Porta Romana [La Specola] will mean knowing nothing about anatomy.

Garrison[32] quoted J.S. Billings (Surgeon General, U.S.A.) as saying that Fontana's waxes were «beautiful to look at, but inaccurate and of little scientific value».

This last conclusion is clearly debatable, but leaving it aside, it can be said incontrovertibly that the waxes provide a look at the anatomical concepts of that time. A personal example is the discovery of Wax Model No. 702 at La Specola. It is entitled «The Maxillary Artery», more fully, «Dissected Head and Neck Showing the Cranial Nerves and the Maxillary Artery. A work of Clemente Susini and his helpers, under the direction of Dr. Tomm. Bonicoli between 1775 and 1791». Fig. 26 shows the accompanying watercolor. The interest lies in the depiction of the cephalic distribution of the so-called Intercostal Nerve. Fontana was given much credit by Girardi and Mangili for convincing Italian antomists that this nerve did not arise from the Vth and VIth cranial nerves, but came from the trunk to join them. The Fontana-Mangili publication wanted an illustration, which is provided by the model and drawing: they demonstrate Fontana's concept.

The Models for France. By 1790, Fontana was devoting himself entirely to anatomy and the models; he may have felt that his papers on the decomposition of water and his part in the controversy with Giorgi showed him no longer shining as physicist. For some years, since 1786, he had grown dissatisfied with wax models, for reasons which he clearly explained in a memoir. He conceived the idea that a lifesize human figure with the parts made of wood could be made demountable, so that the relationships of the parts could better be seen. To this end he engaged wood carvers and obtained exotic kinds of wood from the world over. By

1788 the wax models were essentially complete and he turned, in spite of objections by Giovanni Fabbroni, to making wooden models in earnest. These were copied from actual dissections of cadavers, as the waxes had been. Many were required; in the year 1793, 177 «pieces» of cadavers were brought to the Museum, carried by the sweeper in rush baskets. The work did not go as rapidly as with the waxes; a detailed list of the pieces of anatomy in wood, finished and unfinished, for the year 1797 described only eighteen, which included two statues, three busts, and other regional representations. Of these eighteen, only seven are now to be found at La Specola.[16]

When Napoleon Bonaparte visited the studio in 1796, he saw not only the waxes but had demonstrated to him the demountable wooden statue, a copy of which he added to his order. Fontana's execution of these orders met with many difficulties. These originated not in Paris, but in Florence. In Paris Desgenettes labored mightily to bring there the Florentine waxes, submitting memoirs to the committee on public instruction of the National Convention and to the Minister of the Interior.[27] Cubières delivered a discourse on 5 June 1799 to the Society of Sciences,[33] describing the statue in wood and announcing that a copy should arrive in Paris soon.

Many Florentines cared little either for Fontana or for the models for France. The anatomy in wood had little support from Giovanni Fabbroni, who worked against Fontana also in Paris, where he was sent in 1798 as part of a Commission on Weights and Measures. However, it is noteworthy that the French in Tuscany had great sympathy for Fontana. On 28 January 1801, Belleville, French Consul at Leghorn, wrote to Talleyrand:[34]

> By fortunate chance, the Aretini who laid waste Fontana's home did not enter at all the room with the forty boxes of anatomies under glass belonging to the Republic... M. Fontana is also working on the anatomical statue, but it is not yet finished... Eighteen months of the most cruel emprisonment have suspended his labors, dispersed his pupils, enfeebled his health, and destroyed a competence acquired in fifty years of study.
>
> ...I offered to give M. Fontana the sum of 3,000 francs to help the workers and pupils whom he employs to carry on his labors, but I could see that I offended his sensibility; the Tuscan government has provided for the most urgent needs and I thought that before insisting I should await your orders. I presented M. Fontana to General Murat. If the First Consul deems it suitable to make M. Fontana an offer of either a supplement to the 4,800 francs which he has received for the 40 cases of anatomy, or *on account against the cost of the statue,* or as an advance for the continuation of his work, this last form might be most suitable, along with a

creditable letter to this scientist, all the more sensitive for being unhappy, I draw to your attention, Citizen Minister, that the 4,800 francs paid for the 40 cases of anatomy are only the price of the labor [...] in seeking to turn to the advantage of the Republic the consciousness harbored by M. Fontana of the ingratitude of the Tuscans he honors and who have treated him so cruelly (he still bears the scars of the irons which these men dared load upon the hands that for 60 years have rendered such service to mankind), I offered him a new fatherland; I assured him that he would find in the First Consul a benevolent friend, in all French artists an adoptive family eager with wholly respectful and kindly solicitude to console him for his long sufferings. But he replied that he was 65 (really he was 71) —; that he loved his art passionately, but that his sight is no longer so good, and that his enfeebled health would not permit him to work from the cadaver as readily and for as long a time as copying in wax. However I do not think it would be difficult after his work is finished (that is to say the statue in wood) to induce him to come and train some pupils in France and I dare to think that this acquisition would still be an important victory for the Republic and creditable to the government. I beg you Citizen Minister to send me your orders for the continuation of M. Fontana's work; it would also be gratifying to me to be able to convey to him on the part of the First Consul a sign of satisfaction and some means of encouragement.

Six days later Belleville wrote again to Talleyrand forwarding notes from Fontana, also saying that the works on models in wax and wood had recommenced, that the cost would not exceed 24,000 francs per year, and that Fontana requested that he be granted a title by the French government which would protect him from further prosecution. Fontana's note reads:

Your letter of the 19 Nivose year IX gave me an agreeable surprise. It proves to me that the conqueror of Italy has not completely forgotten me, nor the commissions, he gave me on his first arrival in Florence; he asked of me, at that time, the execution of two works of great use to mankind, one an anatomical collection in wax similar to that which I had made for the Museum in Florence; the second a demountable statue in wood, composed of 3,000 pieces with some millions of smaller ones.

In the presence of Miot, French Minister in Tuscany, the hero of Italy ordered the funding of this work.

It is true that on the arrival of Commissioner Saliceti I was paid [...] for the antomical waxworks which I had begun under the Constitutional King.

[...] The sum paid was about 200 louis. This money served only to reimburse me for advances.

The work done was packed in 40 cases, filled with anatomical models; these cases are in my home, always at the disposition of the First Consul. If it had been possible I would have sent them overland to Paris; the passage by sea appeared to me too dangerous. It was the only way to avoid a heavy expense and of being sure that everything would arrive without damage. I have just taken up again my works

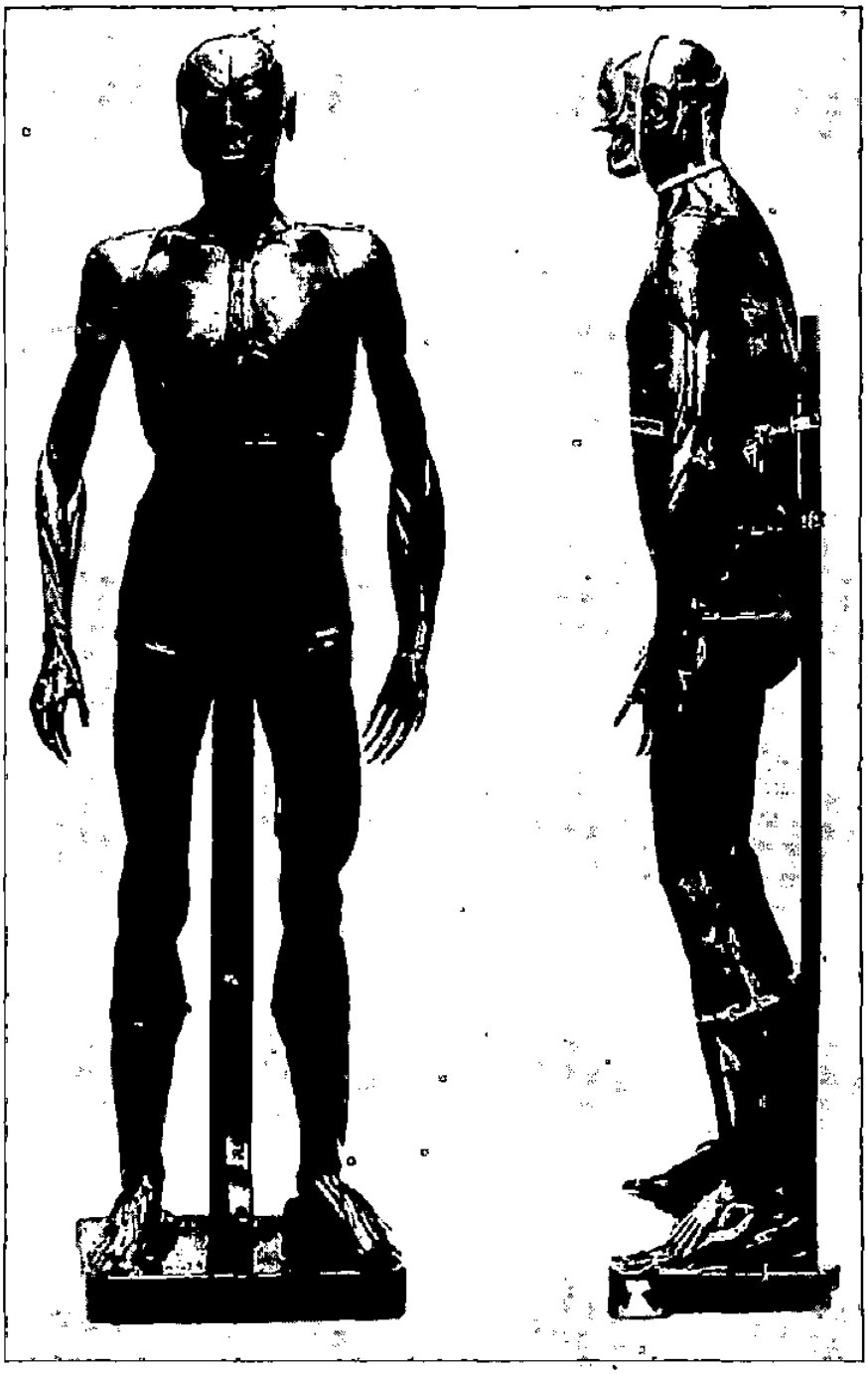

Fig. 33 – Life-sized statue, demountable model in wood. At the Musée d'Histoire de la Medecine, Paris.

in wax, as well as the demountable statue in wood which the First Consul requested of me at the time of his passage through Florence, a request renewed now by the Foreign Minister Talleyrand, according to what you tell me in your letter.

I wish nothing more eagerly than to support the broad views of France's first Magistrate, and to be able to be useful to that great Nation as far as my age and my health permit.

Fontana

In 1802 the wax models were dispatched for Paris; unhappily for Fontana they never reached it. In Rouen was Jean-Baptiste Laumonier[33] (1749-1818), a competing maker of waxen anatomical models some of which he had sent to Paris. There the Assembly of the School of Medicine commended the exactitude, the natural attitudes, and the color of Laumonier's waxen models shown to them, and «finally the absence of draperies and décor comparable to those at Florence, Vienna, and Pavia». Accordingly, Fontana's waxes went to Montpellier, where they are today in the school of medicine.

A statue in wood (Fig. 33) did reach Paris, where it is today at the Museum of the History of Medicine.[34] It can hardly be regarded as either a work of art or a successful anatomy.

On Fontana's death in 1805, his home containing many anatomical models was sealed, we learn from a letter of François de Beauharnais. French plenipotentiary Minister in Tuscany.[34] The wooden statue now in Paris may have been sent there at that time, but the rest became the property of Fontana's heir, his brother Bernardino, who tried to sell them to Russia but apparently failed.[16] On 9 April 1814 Pietro Ferroni wrote[36] from Florence to Bernardino offering 400 *scudi* for «Both the museums», presumably the remaining anatomical models, but nothing more is known of this; thirty days later Bernardino was dead.

Other Anatomical Waxes from La Specola. Giovanni Fabbroni wrote of Fontana that he wished «to obtain orders [for waxes] for all the courts in the universe [...] many preparations in wax were given, at the expense of the Crown, to Russia, to Spain [...] to many individuals in Sweden; and finally some were intended for the king of France [...] he gave away everything he could». From a letter to Caldani[37] of 2 September 1775 we learn that Murray petitioned the Grand Duke for an «anatomical preparation» when he was in Florence, and obtained it as a gift. Fontana wrote that this was the only case of an anatomical wax from the collection being given to a third person, but then he added that he was

taking with him «a piece or two designated for Haller, by the sovereign's orders». Castaldi[1] mentions waxes going to Lausanne, Cairo, and «Chauleston» *(sic)*. Nothing is known of the existence of any of these. He also listed the Italian cities of Torino, Pisa, Bologna, Pavia, Genova, Siena, and Cagliari as having waxes from *La Specola*.

In 1979 a *magnum opus* appeared from *La Specola*. This extraordinary work[38] contains 682 photographs of all the anatomical waxes there, sixty-two of these in color. The text includes essays on the history of the museum and of ceroplastics with technical details of their preparation, biographical notes on Fontana, Mascagni, Susini, Zumbo, and Pietro Leopoldo.

CHAPTER 18

POISONS

Venom of the Viper - Antidotes to the Venom of the Viper - If the Bite of the Viper is Naturally Mortal to Man? - Cherry-Laurel - Toxicodendron - Tobacco - Opium - Curare. Fontana as Pharmacologist - In General

Fontana began his studies of venomous vipers in 1764 in Pisa and published his first work[1] on them in Lucca in 1767, but subsequently he returned to the subject many times and performed more experiments, as well he might, since he had raised such difficult questions as: how does the venom bring about the death of an animal? At what point is an animal dead, rather than alive? What is death? What is life? Searching for answers to these questions led him down other paths in biology, such as the study of animals which die and revive (anabiosis).

In the preface to the first book he promised another one which in 1781 finally appeared;[2] it is Fontana's best-known work. The circumstances were that the first book had been translated into French by his friend Darcet but was not published. When Fontana reached Paris in 1776 Darcet gave him this translation. Later a pamphlet by Sage[a] appeared which contradicted Fontana's earlier writings, so that he made additional experiments. While in London he studied other poisons such as that of the Ticunas (curare), the cherry-laurel, nicotine, and opium. To the description of these experiments were added accounts of anatomical and

[a] Balthazar Georges Sage (1740-1824), apothecary, professor of assaying at the Paris Mint, founder and director of the Ecole des Mines, held peculiar ideas based on the phlogiston theory. Among his many publications were two works on the Volatile Ammoniacal Fluid Alkali, Paris, 1778.

physiological observations, for reasons of his own,[b] and all was published in a French translation in Florence in 1781, the name of the printer not being given. The arrangement of material in these two handsomely-printed volumes is inconvenient for the reader; in Volume I the first part is a translation of the 1767 book; the second and third parts describe new experiments with the venom, probably performed in Florence and Paris in 1767-1778. Volume II begins with the fourth part which speaks of antidotes against the venom and other treatments of individuals bitten, an appendix on experiments done in London in 1778-9, treatises on other poisons, anatomy and physiology, and supplement added after the rest had been printed which deals again with all the poisons; the editor advises the reader to read its parts jointly with the other sections on the same subjects. At the end are ten plates of illustration with explanations of the same.

Fontana was aware that his *Treatise* might be subject to criticism. He wrote:[5, II, 72]

> I feel that I have been too prolix. I might have been less so, and perhaps even more clear, had I followed the synthetick, instead of the analytical method. I preferred the latter, presenting my experiments in the order in which I made them. I have not even dreaded disclosing my errours, and the shewing how often I have been obliged to begin over again. The analytical method is certainly neither the shortest nor the most favourable to a writer; but it is the most certain, the most luminous, and the only one which leads immediately to a discovery. It inspires the reader with confidence, shows in what way the naturalist has searched into nature, and in what way she has answered to his researches. His faults are at the same time observed, the efforts he makes to come at the truth, and the difficulty of attaining it.

From a letter to Bettina Slop it appears that the work was printed at his own expense, which he expected to recover.

In 1785 E.B.G. Hebenstreit[c] published a volume in German[3] containing 110 pages of excerpts from the 1781 work. In 1787 an Italian version of the 1781 work appeared[4] in Naples, with the same plates. The

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[b] He wished to introduce some variety, he wrote to H.B. De Saussure. In this chapter, the superscripts of numerals which are applied to the quotations from Skinner's translation of the Treatise are to be read as follows: 5, II, 72 means page 72 of Volume II of Reference N° 5.

[c] Ernest Benjamin Gottlieb Hebenstreit (1758-1802) was assistant professor of general medicine, anatomy, and surgery in Leipzig, and edited several volumes of the complete works of Torbern Bergman and of Scheele, Leipzig 1787-1790.

language of Part I here differs from that of the Lucca version of 1767; either it was an extensive revision or a retranslation from the French. Joseph Skinner, Navy surgeon and member of the corporation of surgeons of London, made a faithful translation of the French version of 1781 into English, which was published in 1787[5] and again in 1796.[6] Also in 1787, a translation of the entire 1781 work into German appeared.[7]

In 1782-3, a long review appeared[8] of the works on the viper of 1767 and 1781, and the 1780 article[9] on the Ticunas. Although it is often attributed to Fontana, the style is not his and it was not written by him. Reviews of the 1767 volume appeared in 1768,[10] 1769,[11] and 1780.[12] The 1781 work was reviewed in great detail in 1783,[13] in 1784,[14] and in 1784-5,[15] this last by his friend Gibelin. In 1787 the English translation of 1787 received additional notice.[16,17]

It can readily be seen that Fontana's works on the venom of the viper did not lack interest and attention. What had he done? As he saw his work in progress he wrote to his brother Giuseppe from Paris on 26 November 1777:[18]

> My work on the airs has suffered a delay of nearly three months on account of a book appearing here on the use of the *volatile alkaline fluid* [ammonia] in many illnesses, and even in that caused by the venom of the viper. I had been bringing out in French my Italian work on the *venom of the viper* when out came this book, full of miracles and resurrections, the product of a chemist, believed here to be a great chemist.[a] It maintains that the poison is an acid, that blood coagulates in it, that it is a mass of salts that kills like an acid, and that those bitten are cured with alkali which takes up the acid salts of the poison... I had to retrace my steps to examine anew this matter; without this no one would have believed the experiments reported in my book. I do not regret having done this new work, as I have found new and interesting things, and I flatter myself to have carried this subject as far as man could carry it; so that my new work will be perfect of its kind and will leave nothing more to be desired. There you will find that a true specific against the venom of the viper is an impossibility, and at the same time you will see that the bite of the viper is not to be feared as much as it has been until now, that scarcely two men in a hundred would die, even if untreated.
>
> There I deal with, then, more than thirty very burning questions that until now have not been touched upon by anyone, that perhaps would not even have been believed susceptible to proof, within man's capacity; I finish by showing that the venom of the viper is a true gum, since it has all the qualities of such, and as it is formed in an animal, it is an *animal gum;* which amounts to a real discovery, since it is the first and only animal gum known to naturalists until now.
>
> I have economized neither on experiments nor animals. The experiments exceed four thousand. I have made use of six hundred vipers, and have had bitten more than a thousand animals. Not any scientific subject has ever been treated

more extravagantly with experiments by anyone at any time; still when you will have read it you will say that there is none too much, and perhaps I have not done enough. I give only the results. The record of experiments would amount to several volumes *in quarto,* and would be no more instructive. All this work cost me two months. Printing will begin in December, publication will be in March.

In fact, it took another three years.

Another statement on the size of the work came from the Editor, who perhaps was Gibelin; he wrote in the Preface:[5, I, XV] «But what confidence ought not an author to inspire us with, who, after having said, *I have made more than 6000 experiments; I have had more than 4000 animals bit; I have employed upwards of 3000 vipers;* finds no difficulty in adding, *I may have been mistaken, and it is almost impossible that I have not been mistaken*».

Fontana's general plan of research and the *Treatise* was as follows:[5, I, 124-6]

For my part, I think it is the duty of the latest observer, not only to repeat faithfully the anteriour experiments that contradict his, but likewise to give his own in such a way, that they cannot leave the shadow of a doubt in the mind of the reader. Without this proviso, he will lose the aim he proposed to himself in writing – that of being believed; which he will not deserve, although he may, by accident, have said the truth.

There are three principal methods of avoiding this inconvenience, which perpetuates errours, and still keeps us in a very dangerous scepticism.

The first is, to multiply the experiments exceedingly. It is almost impossible, in repeating them so many times, that fortuitous cases do not occur to vary them, and that the final result of so many of them is not certain and constant.

The second is, to vary them in a thousand ways, changing the circumstances as the nature and species of them may require, and giving them all the precision and simplicity they are capable of. This method supposes much greater talent and genius in the observer than the first, and there are few of these, even among the most skilful, who can boast of having invariably put it in practice.

The third method is, not only to succeed in making experiments, decisive by their number, variety, and simplicity; but likewise to attain to a discovery of the source of the errours that others have fallen into.

It is a fault, then, in those who write the last, not to enter into a very minute detail of their experiments, and to endeavour to demonstrate their superiority and exactness, in comparison with those of their predecessors. It, however, is particularly incumbent on them to trace the origin of errours, and to show how the former observers have been deceived. Without this, all their labour is a pure loss, and they are by no means worthy of confidence.

As was Fontana's wont, and as he wrote in the first paragraph of his general plan, he cited earlier writers only to refute them. For mention of

some of them, he relied on the book of Brogiani,[19] a professor of anatomy at Pisa and thus his colleague, who had «written a treatise full of erudition, on the venoms of animals».

The most eminent of Fontana's predecessors writing on the viper was Francesco Redi, Aretino, 1626-1698. Redi made a departure from previous works on the viper such as those by Abati[20] and Severino,[21] whom he quoted. They did make some points about the poisoning, such as its not arising from puncture by the tail or the tongue, but rather by the fangs, but they concentrated on the use of the viper as a medicine. In addition to Redi's two works[22, 23] on the viper, his *Experiments on the Generation of Insects* (1668), which gave strong evidence against spontaneous generation, his parasitology, *Observations on living animals to be found on other living animals* (1684), and his *Medical Consultations* showed his empiric scientific and medical interests as a member of the *Accademia del Cimento* and physician to Cosimo III. However, «he actually revealed himself to be, if not baroque, in any case a man of the seventeenth century [...] an outstanding example of how in his time culture was still formally humanistic; science was part of philosophy; and was placed on the same level as history more or less anecdotal, and literature».[24] Thus his own work on the viper is described between long Greek and Latin quotations from Nicander, Pliny, Dante, and Petrarch.

Some of his most telling points were made in his letter to the French investigators.[23] These were:

1. The viper kills a dove, a cock, a turkey, a squirrel, a dormouse, and other small animals and birds more readily than a large animal, such as a sheep, a deer, a horse, a bull; indeed these animals that are larger and have tough hides very many times are not killed.

2. Death follows earlier or later according to the size of the animal and the spot bitten in particular whether the place is a tissue thickly or sparsely furnished with vein and arteries, and whether the veins and arteries are fine or large.

3. If much blood flows from the bite of the viper, it sometimes happens that the animal not only does not die, but is not even much hurt.

4. Again, it frequently happens that an animal wounded by a viper suffers from the poison the fiercest attack which brings it close to death, and yet it does not die but recovers without the help of treatment, simply by an act of nature.

5. Those animals into which the yellow liquid penetrates from the bite of the viper die somewhat more quickly than those receiving it artificially as it was drawn from the sheath of the viper's fangs.

6. The act of making the above-mentioned liquor penetrate into the wound needs to be highly accurate, since if the wound is narrow it hardly penetrates, and if it is wide, it cannot help but bleed; and the blood usually flushes out the poison.

The venom was shown not to be poisonous if swallowed. Redi told how one Jacopo Sozzi of Popiglio «took one of the largest, liveliest, and angriest vipers and made him spurt into half a glass of wine not only all the liquor that the sheaths held but also all the foam and saliva that this excited, provoked, overpowered, beaten serpent could shoot forth; he drank that wine as if it had been so much pearly julep. And the next day, with three vipers twisted together, he played again the same game, without a fear in the world; and he was right...» Of course some believed that Sozzi had taken just before, although he denied it, some extraordinary magic potion, perhaps that of Mithradates, which counteracted the venom.

Redi showed that by allowing a viper to bite a chicken several times, it discharged the poisonous liquor so that a second chicken now bitten did not die. Another important passage in his work is the following:

> It seems to me that you expect me to give you some learned, subtle, and well-pondered discourse, speaking on what way the viperinepoison does away with life, ushering death into the body; if it introduces its working by an occult power not discerned by human intelligence; or whether, arrived at the heart, expelling the calorific atoms, it cools and freezes it completely, or whether, increasing and energizing these same atoms, it heats it to excess and dries it up, and quite disperses and destroys the spirits, or whether it deprives it of feeling; or prodding it with painful punctures brings it about that the blood, returning too copiously to the heart, chokes it; or whether it impedes the movement of the heart itself, making the blood congeal in one or the other cavity in such a way that it can no longer contract and dilate; or whether it makes the blood coagulate not only in the cavities of the heart but even clot in all the veins.

Redi's conclusions were challenged and his experiments contradicted by a trio who deserve additional comment.

Moses Charas (1618-1698), learned physician and pharmacist, was Royal Demonstrator of Chemistry at the *Jardin des Plantes* in Paris when the revocation of the Edict of Nantes drove him in 1685 to exile in England and Holland. Later he was called to the Madrid court of Charles II, who was ill. Having said one day that the Spanish vipers were poisonous, he was thrown into prison as a heretic because the archbishop had exorcised the venom from all vipers born and bred in that happy

state. However, the Inquisition, discreet as always, liberated him after four months with the requirement that he abjure, which he did but was quick to leave a land where, if the vipers were not poisonous, bites from another indigenous breed of animal were.

Pierre Michon Bourdelot (1610-1685) studied medicine at Paris under Gassendi. Attached to the Prince de Condé, Governor of Burgundy, then to the Duc d'Enghien, he was not recognized for his efforts in pacifying the Fronde, whereupon he went to Sweden. At the court of Queen Christina he gained so much favor for his clowning and his sure instinct as courtier that he became known in France, to which he was recalled in 1653. Provided with the Abbey of Massay in Berry, he quickly dissipated all its resources, then was called into the service of Condé who returned from exile in 1659. He acquired great celebrity for his treatment of the gout; his *Histoire de la musique et de ses effets* (1715) and his *Histoire générale de la danse sacrée et profane* (1732) were pulished by his nephews after his death.

Alexander More (1616-1670), son of a Scotchman, studied theology in Geneva and became a celebrated protestant minister, publishing many sermons. His banter and haughtiness of manner, also his merit and influence, elicited the jealousy of his colleagues, and he traveled much, being honored by the Grand Duke of Tuscany. He was always in trouble of one kind or another.

These three met in the home of Charas and carried out experiments with vipers. They wrote[25] that, contrary to Redi, the poison of the viper was not the yellow liquid but the spirits formed by the enraged serpent. Redi replied that perhaps French vipers differed from Italian vipers, but that he was sure of his results, and suggested that they perform more experiments.

Fontana disposed of Redi's erroneous conclusions about the path traveled by the venom by describing correctly the mechanism; the vesicle where the venom is formed, its duct, and the canal within the fangs, with apertures at the base and the tip, from which the venom emerges. He denied Redi's contention that normally the venom flowed between the fang and its sheath, but admitted that this might happen if the canal were obstructed, as by dried venom. He disposed of Charas's claim that the poison was not contained in the yellow liquid but in the spirits from the enraged viper with several experiments. He allowed the serpent to bite frequently until all the venom had been spent. Then the serpent was tormented until it became enraged and its mouth full of foam, but its bite was now innocuous. He held open the mouths of enraged vipers so that

they could not bite and collected on bits of cotton the foamy saliva. These on introduction into wounds had no effect. He collected the yellow liquid from the heads of vipers which had been decapitated while tranquil; this venom was fatal on being introduced into wounds. Finally, he removed the viper's two vesicles containing the venom; its bites were now innocuous.

A later investigator of venomous serpents was Richard Mead (1673-1754), who proposed[26] that the venom contained salts which he saw in dried preparations as sharp-pointed crystals which exerted a corrosive action, decomposing the cells of the blood. Fontana established that the venom was not an acid, not alkaline, not a crystalline salt. In hot water it dissolved instead of coagulating, as did «animal lymph» or white of egg; it was precipitated by alcohol; therefore he likened it to gum arabic, saying that it should be considered as an animal gum.

In Fontana's first book, of 1767, he wrote that he was led by the earlier claims of Mead and others to believe that serpents were poisonous to their own species, or to themselves. However, from his own experiments he concluded that they were not, as was known also of spiders, scorpions, and the fresh water polyp. He now asked, may the venom be innocent to other species? He found that leeches, snails, slugs, asps, adders, and turtles were not injured by the venom, but frogs, eels, and lizards were killed quickly. Warm blooded animals of various species universally were killed.

The last chapter of this first book, dealing with the cause of death in animals that have been poisoned by the viper, begins by quoting the passage from Redi which we give on p. 272. Fontana added,[5, I, 75] «Other authors, bolder than he, are not afraid of exposing their sentiments, whether badly or well founded. Before I propose mine, I think it necessary to relate the most reasonable opinions that have been held by naturalists, as well ancient as modern, on the subject». One of these could well have been Nicholas Lemery[d] who wrote in his Course of Chemistry:[27]

> After having related the opinions of others, it may not be amiss to shew my own. I conceive, that what they call the Venom of *Vipers* consists only in the abundance of acid volatile Salts, which this *Animal* throws out violently when it

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[d] Nicholas Lemery (1645-1715), apothecary, physician, and professor of chemistry. His *Cours de Chymie,* first published in 1675, went through many editions and was translated into Latin, English, German, Dutch, Italian, and Spanish.

bites. And these Salts, passing into the Veins and Arteries, do coagulate the blood, and hinder both its Circulation and the Course of the Animal Spirits, even as it happens when an Acid Liquor is syring'd into a Vein [...] [In] the Person bitten [...] Death at last follows, because the Blood is still sharpening and congealing more and more, the Passage of the Spirits is entirely stopped, and there can be no more circulation, without which none can live. If after Death you open the veins of the dead Body, you shall find some Blood more liquid than usual, and other Blood thick or curdled, which may easily be explicated; for when one Part of the Blood curdles, it separates from the Serosity.

Fontana disposed of this hypothesis, and that of Mead, with the statement that this acid, or these salts, do not exist in the venom. Also, death occurs in too short a time for it to result from a «universal inflammation» as had been proposed, or from a «universal spasm» which is not apparent. Nor are to be seen the «organic particles» of Buffon, nor obstruction of the biliary tract and jaundice.

Fontana was impressed by this consideration:

I could not comprehend how creatures with cold blood, such as the frog, are soon destroyed by this poison, whilst they survive for so long a time the loss of the heart, intestines, and other viscera, and even that of the brain and head.[5, I, 89] [He was led to suspect an action in destroying the irritability of the muscular fibers by *analogy* with the effects of opium and mephitic vapors, then he] procured fifty of the strongest and largest frogs I could meet with. I preferred these animals, because they are livelier than others; because they die with greater difficulty; because they are more irritable; and lastly because their muscles contract even several days after they are dead.[5, I, 97-9]

He had each one bitten by a viper, or introduced the venom himself by hand. Some showed inflammation at the site, but many died without that, becoming «motionless and paralytic»; irritation of nerves or muscles produced no contraction, although the heart continued its action.

Thus then it appears, that animals die of the bite of the viper, from their fibres losing that irritability, which is the grand principle, both of voluntary and involuntary motions...[5, I, 100] The flesh softens much sooner than usual, to such a degree as to crumble at the least touch [...] the venom of the viper destroys irritability by conveying a putrefactive principle into the flesh.[5, I, 101]
But since it is certain that the voluntary motions of the muscles of animals with cold blood do not depend more on the circulation of humors, than does the irritability of the fibres, which seems to be the source and principle of life and motion in the animal, it follows that, in animals, life consists in the action of their muscles and parts, for the moment this motion ceases, the animal ceases likewise to live [...][5, I, 111]

Here Fontana introduced a discussion of his observations on the microscopic eels which can be dessicated then revived by moistening with water.

> It is evident, then, that neither of the numerous hypotheses naturalists have invented... explain in a reasonable way the death of animals poisoned by the viper; but that its venom kills in no other way, than by destroying the principle of motion [...][5, I, 114]
>
> I call every animal dead in which there are no longer any exteriour signs by which we can say that it lives...[5, I, 115] A principle of corruption penetrates into, and spreads itself in, the solids and fluids, relaxes and decomposes the muscular fibres, and causes them to lose the power of contractility. It is to this general law of putrefaction, then, and to this universal principle of dissolution and death, that the entire action of the venom of the viper on organized bodies is reduced [...][5, I, 116]

The reprinted work of 1767 ends here with the comment that so he thought at that time, but that then he was ignorant of the properties of the poisons Ticunas and the Cherry Laurel. He promised a new work on *Microscopical Observations,* but it never appeared.

Fontana now began to reconsider all the questions he had previously raised, reëxamining even the anatomy of the viper and the physical-chemical properties of the venom, he confirmed his earlier findings. He decided that it was equally important to examine the effects of repeated bites in the same part, and the action of the venom on different parts of the same animal. In the latter study, it appeared that more consistent results were obtained by manually introducing the fang and compressing the vesicle holding the venom, than to allow an animal to be bitten. He incidentally supported to some degree Charas's contention that the bite of an enraged viper was more dangerous, by reasons of greater penetration of the fangs, longer duration of bite, and most important, delivery of a larger amount of venom. He concluded that:

> an animal dies sooner when bit a certain number of times in two parts, than when the same number of bites is confined to one... in this case the single part is subject to a much more violent external [local] disease[5, I, 166]

Fontana believed it to be important to examine the action of the venom on different parts of the body because, these organs and parts being differently organized, the actions on them must also differ. Accordingly, he performed an immense number of experiments, with results of which he did not make very much, with certain exceptions to be discussed later. Fontana now wished[5, I, 277]

to enquire into the quantity of this venom, that is necessary to kill an animal of a certain size. This enquiry, very curious in itself, cannot be but of some use in the practice of medicine, particularly in cautioning us against thinking the danger greater than it really is, when any one of our own species has the misfortune to be bit by this animal.

It was necessary to measure very small quantities of the venom, which he achieved by procuring a scoop slightly more than one millimeter in diameter, made from a glass capillary tube, which he plunged ten times into three grains of venom held on the pan of a balance. The loss in weight of the venom was 1/100 of a grain. Elsewhere[5, I, 245] he wrote that «a cubick inch of water weighs about 373 grains», or 16.4 grams, so that one grain for Fontana was about 44 milligrams, and one scoop of venom was about 0.044 milligrams. At first the scoop of venom was introduced by way of an incision into the leg muscle of sparrows, but the results were so variable that he changed to using pigeons. The results were as follows, expressed in mortality ratios, i.e., the number of pigeons dying over the number used.

Number of scoops	Milligrams of venom	Mortality Ratio
1	0.044	0/7, 1/6, 1/12
2	0.088	2/12, 4/12
4	0.176	9/12

It appears that the LD_{50}, or dose killing one-half of a group of pigeons receiving the same dose, would be three scoops or about 0.13 mg. If we take the weight of the pigeons to be six ounces[5, I, 286] the LD_{50} in milligrams per kilogram body weight was 0.8, which is the same value for the LD_{50} of the venom of *Vipera berus* on intraperitoneal injection into mice.[28] The «minimal lethal dose» of the venom of *Vipera aspis* in pigeons is reported[63] to be 1.25 mg per kg. These results of Fontana's were viewed with some suspicion by Earles,[29] who has made an extensive study of the *Treatise,* but the coincidence of these values is striking. Fontana went on to write[5, I, 286-7]

The quantity of venom it will require to kill a large animal, an ox for instance, supposing it to weigh 750 lb. will be about twelve grains; and it will require nearly two grains and a half to kill a man, supposing him to weigh the fifth part of what an ox weighs, that is to say 150 lb.

It is true that this calculation takes for granted some new hypotheses more or less probable, but of which neither is unlikely. A sufficient number of experiments

are wanted, to render them either absolute truths, or susceptible of some restrictions.

The first hypothesis supposed here is, that the venom of the viper acts on an animal in proportion to the quantity. There is nothing unreasonable in believing this to be the case, since if a very small portion of venom is capable of deranging the economy of an animal *to a certain point,* a greater dose of it ought to produce a greater derangement, a more violent disease. Besides we have seen that animals bit several times by one viper or by several, die sooner than those that are only bit once by a single viper; and we know that a viper which bites several times, introduces fresh venom into the part at each bite.

The second hypothesis is, that the disorder produced in the animal economy by the venom of the viper, is less in proportion, or rather that the power of the animal to resist the action of the venom, is greater in proportion, as the animal is larger. This is generally so, although there may be exceptions to this law, that may prevent its being rigorously the case.

The third hypothesis is, that from the effects produced in an animal of one species, we may argue as to the effects produced in an animal of another species; that is to say, from birds to quadrupeds. This argument is drawn from a simple analogy; but this analogy is at the same time formed betwixt animals with warm blood, and it may therefore be deemed of some weight.

Now granting that a viper of a middle size has in its vesicles two grains in weight of venom, it will require the venom of six vipers to kill an ox, and nearly of two to kill a man.

But if we reflect that a viper which bites, does not leave itself without venom; that at each bite, at least for the first three or four, it may bring about the death of an animal with almost the same facility; it will not appear altogether unlikely, that it may perhaps require twenty vipers, each biting only once, to kill an ox, and five or six, with the same restriction, to kill a man.

The next question Fontana attempted to answer was, what is the time required for the effects of the venom to be visible. His first experiment gave results new and seemingly paradoxical. If the venom was introduced into legs of animals immediately after their amputation from the body, or if they remained connected with the body only by the femur, no local phenomenon of disease resulted, indicating that the noxious action is not through simple mechanical motion, or a chemical effect such as from the contact of an acid with an alkali. The lack of any local response when the leg was amputated immediately after bite confirmed this. Varying the length of time between bite and amputation showed that the local action was not instantaneous, but required from fifteen to twenty seconds for becoming sensible.

Next, he asked may the animal die from a disease independent of that which discovers itself in the part which has been bit. By amputating bitten legs at various times after the bite he showed «that a mortal disease is communicated to the animal in a very little time; and that it

dies, independently of the local disease, by an interiour derangement, which the venom has already communicated to its whole system».[5, I, 299] He found that if more than twenty seconds time was allowed before amputation, the animals died. Also, he observed that there could be a marked extravasation of humors at a great distance from the part that had been bitten.

Fontana now examined the action of the venom on blood freshly drawn from animals; his reason for doing so was not made clear – it may have been because Mead had done so. He observed that the blood remained in the natural state, not remarking that this behavior, that is, failure to coagulate, was quite unnatural. He wrote «Nothing appears more natural than the deducing from these particulars, that the venom of the viper has no action on the blood of the animal that receives the bite».[5, I, 313] However, he determined to try a new kind of experiment, partly analogous, but «more direct and more simple», namely the introduction of venom into a vein in the animal, the jugular vein in rabbits, by means of a glass syringe, which procedure he described carefully. Scarcely had the venom begun to enter the vein when the rabbits shrieked, exhibited violent convulsions, and died in a few minutes. On examination, the blood in the large vessels and the heart was found black and coagulated. Extravasated blood was seen in the heart, lungs, muscles, and intestines. Connective tissues contained black, extravasated fluid and blood. The pericardium was distended with pinkish fluid. The problem now confronting him was:

> But in what way are we now to reconcile the immediate action of the venom of the viper on the blood, when it is injected by the veins and the inactivity of this same venom, not only on the parts of an animal recently cut off, but likewise on those that have remained in an entire state, and united with the animal, during a period of 15 or 20 seconds after it has been introduced.[5, I, 327]

Fontana speculated on the presence or absence of «living principle» in blood, but never answered this question, only concluding that the circumstances, like the effects, were different, that something exists in one case that is not met with in the other.

He turned to examining the effects of the venom on the nerves, never having lost sight of the principle of sensation which appeared to him to be acted on. In many experiments on frogs and rabbits, he exposed peripheral nerves, ligated and unligated, divided and undivided, the spinal cord, and the brain, to the venom. He injected venom into those animals in which the spinal cord had been divided, or the brain

removed. He concluded that the venom is altogether innocent to the nerves, and that the nerves in no way contribute to the disease of the venom of the viper.

These observations led Fontana to reëxamine the action of the venom on shed blood. He then realized the significance of the previous observations, now confirmed, and wrote:[5, I, 378]

> Here then the venom not only does not produce its ordinary effect on the blood, namely that of coagulating it, but produces an altogether contrary effect namely that of keeping it in a dissolved state and so preventing the coagulation which would otherwise take place.

Fontana now had to prepare a new chapter on the cause of death of animals bit by the viper. It read:[5, I, 400-409]

> When I wrote the first part of the present work I was of the opinion that the venom of the viper attacked the irritability in an immediate way, and that the animal died from the loss of irritability in the fibres. But I did not then know, that the venom of the viper has no action on the nerves, and that when it is introduced into the blood, it kills an animal in a few instants. This hypothesis ought now to be partly modified. It is not that in effect the irritability is not diminished, in the animal that has been bit, and that it is not even destroyed in a little time, but that this is rather an effect than a cause, and it is a consequence of the change caused in the blood by the venom, rather than an effect of the venom on the muscular fibres. The disease resides solely in the humours, and the stoppage of these humours in their natural course, occasions the death of the animal.
>
> We say in effect, that an animal is dead, when it is no longer sensible, and we say, that it is no longer sensible, when there are no longer in its organs, the signs, the external motions, that indicate sensation. The moment these motions cease, we say that an animal is dead.
>
> I therefore do not see, why a certain obscure motion may not subsist in the organs of an animal, which may not come within the reach of the evidence of our senses.
>
> But is there any method by which we can assure ourselves of the total immobility of the organs of an animal, in which the humours are still in a fluid state? I cannot conceive any one. I know of only two states of an animal, that can make us certain of its being really dead. One of these is the total putrefaction of its organs; the other, the absolute dessication of its humours. The blood coagulates in the vessels of an animal bit by the viper, and the animal itself is in a state of death. The blood, changed by the venom, corrupts and destroys the organs of animals, and renders the least suspicion of life altogether improbable. It is true that, in proportion as the circulation of the blood stops in the vessels, and as the death of the animal approaches, we likewise see a perceptible diminution of the sensibility, but this does not yet demonstrate to us that the nerves are either changed, or have received an injury.

Such is the death of animals with warm blood bit by the viper, but in cold animals, it is not exactly the same... The death of the animal then follows, but happens much later than in animals with warm blood, because the principle of life is not so intimately connected with the circulation of the humours.

Antidotes to the Venom of the Viper. In Volume I of the *Treatise,* Fontana devoted twenty pages to a description of his experiments in sparrows, pigeons, fowls, guinea pigs, rabbits, cats, dogs, and frogs, either bit by vipers or subjected to manual insertion of their fangs, either left untreated or given by mouth the volatile fluid alkali, i.e., ammonium hydroxide. He concluded that the latter had no antidotal value, and often increased the disease. In Chapter I of Volume II, he returned to the study of this substance, showing that it had no value when applied to the point of injection of the venom, or even mixed with it. Other antidotes tried were various acids, alkalies, neutral fats and oils, also antimony tartrate as emetic, cantharides, cinchona bark, theriac, and viper fat itself. None had any antidotal value. Scarification, cauterization, and electricity were valueless, as were the application of leeches, or sucking the wound. Amputation of the bitten part saved the animal if performed within a few seconds after the bite. In numerous experiments, application of a ligature prevented the communication of the venom to the system and saved the animals, although the local disease occurred, even to a greater extent.

In the Appendix, Volume II, experiments performed in London in March-May 1779 with the highly touted cobra stones, claimed to be effective by the Abbé Tecmeyer, were described. This substance, said to consist of calcined hartshorn (ammonium carbonate), Fontana found of no value as an antidote, as were numerous other substances tested at this time.

By the time of the experiments on poisons described in the Supplement (Vol. II, pp. 319-395), Fontana had examined the antidotal value of lunar caustic (silver nitrate), which was found to render innocent the venom when mixed with it. Fontana «flattered himself with having at length discovered a certain remedy against the bite of the viper», but asked whether it unites with the venom as acids do with alkalies, or by crisping the blood-vessels, does it prevent the poison from insinuating itself into the blood? At first he had felt a real satisfaction, flattering himself that labors had been crowned with great success, as he found that, in birds, application of the lunar caustic by scarification might destroy the effects of the venom; however some of the birds died, probably, he felt, because the antidote failed to reach the envenomed

parts. Further experiments confirmed its antidotal value, but much remained to be done in applying it to advantage.

If the Bite of the Viper is Naturally Mortal to Man? This is the title of the last chapter of the main body of the *Treatise* (Vol. II, pp. 34-75). Fontana concluded:[5, II, 34]

> And here I am not afraid to advance freely that the bite of the viper is not absolutely mortal to man, and that those have been mistaken who have regarded the disease caused by the viper as one of the most dangerous, and from which it is impossible to recover.

His first argument is that the smaller the species of animal which he caused to be bitten and the smaller the individual in a single species, the more likely was death to result.

> [...] five bites from three vipers were not suficient to kill a dog that weighed nearly sixty pounds. Now let it be noticed that a man is about three times as big as the dog of which I speak, and let any one judge if a viper can kill him with a single bite![5, II, 36]

The second argument is that in all the countries in which Fontana could obtain information he found that:

> there is no kind of substance or medicine, that some one has not tried in this disease. What is very certain is, that with all this none have died... A complaint yielding to all these remedies, even to those of opposite nature, can never be a dangerous one.[5, II, 37]

He had heard of two persons who were said to have died from having neglected to take remedies. Of one, no futher information could be obtained; the other died after twenty days, with a gangrened arm. Fontana defended his extension of argument frim animal to man by other authorities, as well as his own observations. His final summing-up was:[5, II, 46]

> Although I have not had the good fortune to discover a certain specifick against the bite of the viper, I have however the pleasure to assure the public, that this bite is not so dangerous as has been hitherto universally believed, and that in an instance where a person may have the misfortune to be bit, life should not be despaired of, even though no remedy be applied.
>
> If I have demonstrated the inutility of a remedy which was believed a certain one, if I have destroyed the hope of discovering a specifick against the viper's venom, I have at least the consolation of having subdued the frightful idea that has been entertained, that the bite of the viper is usually mortal.
>
> I am of the opinion, that of 100 men bit, each of them once by a single viper, either in the foot or in the hand, parts which are usually exposed to the bite of this

animal, not one of them would probably die, even though they should make use of no remedy.

After having seen the effects of the viper's bite on eight different kinds of animals, as well those with warm blood, as those with cold; after having had more than a thousand of them bit in so many different parts of the body, by several vipers, and that repeatedly, I believe no one will deem the opinion I have delivered a rash one; an opinion which even becomes the necessary consequence of what we have hitherto observed.

Ticunas, Woorara, or Curare

Arrived in London, Fontana received from William Heberden,[e] F.R.S., a number of American arrows, well preserved and impregnated with poison, also a pot labeled: «Indian poison, brought from the banks of the river of the Amazons, by Don Pedro Maldonado». Much had been written about this poison, and Fontana's first effort was to dispel the error that had been spread, that the very smell of it was noxious, as written by Gomara in 1554, according to Benedicenti,[30] or as in La Condamine's story that «this poison is prepared by women condemned to death; and they determine its having attained its point of perfection, when the vapor it emits, during the boiling, kills the person who attends it». Fontana found that the smoke or vapor from it did no harm to a pigeon, nor to him nauseous and disagreeable though it was. He found it to be, however, a very strong poison, more active on intramuscular application than on subcutaneous administration, weaker still on application to the scarified skin, and although reported to be innocent when swallowed (as he found on making rabbits swallow two to six grains – up to 265 milligrams!), it did kill some pigeons, rabbits, and guinea pigs if given in large amounts, from which he concluded:

> I deduce from this as an established fact, that the American poison taken internally, is a poison, but that a considerable quantity of it is required to kill even a small animal.[5, II, 105]

He determined the fatal dose on intramuscular injection, finding it to be 1/100 of a grain (0.44 mg) in pigeons. He generalized that if the material

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[e] William Heberden (1710-1801). By Dr. Johnson, whom he attended in his last illness, he was called «ultimus Romanorum», «the last of our great physicians». A monument to his scholarship was his *Essay on Mithridatum and Theriaca* (1745) which was instrumental in banishing many curious concoctions from the pharmacopeia.

is applied to a region where there are few blood vessels, «the complaint either does not communicate itself, or is not mortal».

Fontana found the poison itself (which he called Ticunas after the name of the South American tribe producing it) was soluble in water, not acid, alkaline, or crystalline, had a bitter taste, was decomposed by strong acids, was innocuous on the conjunctival mucous membrane. Its action was slower than that of the venom of the viper and could be prevented by amputating the part receiving it. Fontana first concluded that it was innocent in cold-blooded animals, but revised his belief after experimenting with frogs, eels, and vipers.[5, II, 342] Salt, sugar, and lunar caustic had no antidotal effect.

As for the mechanism of its action, he usually observed a symptom which effectually seemed to demonstrate that the disease produced by the ticunas was purely nervous. These symptoms were convulsions, fainting, total loss of strength and motion. The animal, if it survived, was as well in a few minutes as before, not appearing to have suffered at all. «This is exactly what happens in diseases called nervous, the attack of which is frequently very sudden».

Fontana now examined the effects of this American poison on blood drawn from animals; except for preventing coagulation it had none. Now, since experiments like those with the venom of the viper showed that the poison had no effect on nerves when applied to them, or produced any sensible change in the economy, Fontana concluded, from the sudden death on injection, with the poison scarcely having time to reach the heart, that the poison acts on the blood; «this humour contains a very active subtil, and volatile principle [...] highly essential to life [...] particularly acted upon by the poison [although] these are mere conjectures, more or less probable, and not demonstrated by experiment. We must stick to certain facts, in whatever way we explain them; it is certain then that the ticunas does not act on the nerves, that it acts altogether on the blood».

Fontana defended himself thus:

> Someone may object, that probably the viper's venom and the American poison, act only on the extremities of the nerves, and on that account are innocent when applied to the trunks of them. But everything serves as an objection to those who busy themselves in imagining difficulties; and it is very rare to find two things in every respect alike. For my part, it appears to me, that the internal substance of the great nerves, is the same as that of their extremities; and that the former are subject to pain as well as the latter. I do not create hypotheses which are not confirmed by facts.[5, II, 137]

But after doing more experiments, he wrote:[5, II, 142]

> It cannot be doubted, but the ticunas attacks the principle of irritability in the muscles, although it has no action on that of the heart.

Fontana's writings on ticunas, or curare, appeared in the Philosophical Transactions[9] and in the *Treatise*. He obtained similar results with the pot-curare, and the poisoned arrows.

The Cherry-laurel

The reason given by Fontana for studying the cherry-laurel was as follows:[5, II, 344]

> this oil, whether given internally or applied to the wounds of animals, is one of the most terrible and deadly poisons known. This important truth will, I hope, destroy once for all the abuse that has crept into several places in Italy, of selling the oil of the cherry-laurel publicly in the shops, to all those who ask for it. It is easy to see how dangerous this custom may be to society; and this danger is increased again by the method adopted of selling it. It is usually masked under the title of *Essence of bitter Almonds,* and is found under this name in the printed lists of distillers, who sell it with other essences, oils, and liquors, that may be drunk with the greatest impunity. Nay, what is more, this dangerous poison enters into the composition of a liquor for the use of the publick, which is made and sold without any ceremony; and to prevent all suspicion of the true nature of this poisonous mixture, it is vended under the title of *Liquor of bitter Almonds,* or of *Peach-flowers,* and is even put into milk, and into ragouts. It is true that a small quantity of this poison is employed in them, and that these liquors are not drank like wine and water; but a poison is always a poison; and we do not besides know but it may be hurtful when used a long time, although in very small quantities; and whether it may not give a disposition to certain diseases. I have heard some persons say, that when swallowed it must be an excellent cordial, which its truly agreeable and aromatick smell may easily induce them to believe.

The poisonous nature of members of the *Rosaceae* had been known since antiquity. According to Benedicenti[30] an Egyptian papyrus records the lethal use of *Prunus persica* (peach pits) as punishment for revealing the nature of the secret medicaments of the priests. The toxicity of laurel leaves was mentioned by Strabo, Apuleius, and Pliny. Bitter almonds *(Amygdala amara)* were given to a swan, doves, and a cat by Wepfer.[f]

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[f] Johann Jacob Wepfer (1620-1695), physician to the Duke of Würtemburg, the Margrave of Baden, and the Kürfürst of Pfals.

The rapidly fatal outcome he attributed to weakening of the heart. T. Madden of Dublin wrote in the Phil. Trans.[31] in 1731 that laurel-water was one of the most dangerous poisons. He had observed two fatalities in September 1728 resulting from drinking about five teaspoons of the volatile oil of *Prunus laurocerasus* Linn., and had heard of another case. He prepared an oil by steam distillation of the leaves, clarifying it by pressing it through a flannel bag; this he gave to sixteen dogs. If more than one ounce were given, they died in convulsions; if less, convulsions occurred but the dogs survived, as is seen in human epilepsy. Similar experiments were reported in the *Phil. Trans.*[32] for 1731 by Cromwell Mortimer,[8] well-known to Fontana. He prepared a steam-distillate, fractions of which he gave to dogs. He concluded that this water from a vegetable was equally mortal with the bite of the rattlesnake and more quick in its operation than any mineral poison. John Rutty reported[33] the death in convulsions of an 18-year-old girl after taking less than two spoonfuls of the water of laurel.

Fontana mentioned none of these previous works. Following his principles, he made a variety of preparations of the cherry-laurel and performed various experiments. His derivatives were called water, oil, spirit, extract, phlegm, and empyreumatical oil, none being clearly defined but differing in the mode of preparation, such as distillation with and without added water and with heating on the water-bath or with a flame. These processes were often repeated several times, as for example when the steam-distillate was redistilled over fresh leaves (cohobation); also the dry distillate was treated with decrepitated sea-salt, which would remove any water which had been present in the leaves. All these preparations were varying mixtures of benzyl alcohol, benzaldehyde, benzaldehyde cyanohydrin, benzoic acid, and hydrocyanic acid. Their toxicity being conferred on them by the last named substance alone, and its concentration varying in the preparations, they also varied in their toxicity. He did state clearly that the poisonous quality was unrelated to the pungent smell (which is due to the benzaldehyde).

Fontana gave these products to a variety of cold and warmblooded animals by various routes of administration. After a certain-sized dose, all these animals died after having violent convulsions. With his method of local poisoning, he found that this poison rendered the heart motionless,

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8 Corresponding secretary of the Royal Society.

and nerves unexcitable at the point of application. Its toxic effects were not counteracted by silver nitrate.

Although Fontana promised[5, II, 145] to speak of bitter almonds, he did not do so, and he presented no data on the comparative toxicity. As both bitter almond and cherry-laurel contain cyanogenic glycosides of similar structure and properties, the substitution of one for the other should make little difference. However, Fontana wrote[5, II, 345] apparently after succeeding in convincing the Grand Duke to ban the practice:

> Tuscany owes to a phylosophick sovereign, the knowledge of this pretended *oil of bitter almonds,* and the advantage of being secured from the abuse that may be made of it.

Toxicodendron[h]

Fontana began his brief essay by writing:[5, II, 181-4]

> I was desirous of making a series of experiments on the Toxicodendron, which the most celebrated writers have in general regarded as a very strong poison, although some modern naturalists have not found it so to certain animals.

Who these writers were is uncertain. It is not likely that Fontana was aware of Captain John Smith's earliest account in English of 1609:[34, 35]

> The poisonous weed, being in shape but little different from our English ivie; but being touched causeth redness, itchings, and lastly blysters, the which, howsoever, after a while they pass away of themselves without further harme; yet because for the time they are somewhat painefull, and in aspect dangerous, it hath gotten itself an ill name, although questionless of noe very ill nature.

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[h] The taxonomy and nomenclature of these members of the family Anacardiaceae have been in a state of confusion; however, they are immunologically equivalent. Of (1) poison ivy, (2) poison oak, and (3) poison sumach, now called (1) *Rhus radicans,* (2) *Rhus toxicodendron,* and (3) *Rhus vernix,* none are native to Europe. We do not know which plant Fontana studied. The herbarium of the University *Istituto di Botanica,* Florence, has specimens of *Rhus radicans* placed in the collection by Micheli (1677-1737) himself, of *Rhus toxicodendron* noted to have been growing in the *Giardino dei Semplici* in 1866, and of *Rhus vernix,* so labelled, but this last is probably *R. verniciosa,* the oriental species. It seems likely that an *albero del veleno,* or *tossicodendro,* as it is called in Antonio Targioni-Tozzetti's (grandson of Giovanni T-T) *Scelta di Piante Officinali,* Firenze, Tofani, 1824. was cultivated in Florence before Fontana's time.

He may have been familiar with Peter Kalm's account, published in Sweden in 1753-61, and as an English translation in 1770-1; he was more likely to have read that of Dudley of 1721.[36] We do not know who were the «modern naturalists» who found the plant innocuous to animals.

Fontana's observations on its effects closely resemble those of Dudley and Kalm, and subsequent studies as reviewed by Rostenberg[34] and Kligman.[35] They were also described by a Polish traveler, Stanislaw Staszic,[i] who visited Florence in 1790 and 1791:

> 6 October (1790) Passing through the Florentine botanical garden, I came upon a small, scanty, unimpressive shrub, but since I did not know it, I wished to feel it and strip off some leaves so as to be acquainted better with it. At that moment I heard the voice of Fontana. Grasping my hand, he said: «Stop, Sir; do not touch it because it can make you ill». That plant of the genus *Rhus* contains a poison so strong that a man, urinating in the garden, because he had pulled some branches from it, was covered with pustules nine days later, and had a fever for several days. The phenomenon was particularly apparent on the face. The pustules ran for two weeks, then dried. Fontana, amazed and not wishing to believe that the effect was caused by the plant, had himself broken off a branch. The same thing happened. In nine days he was full of pustules, especially on the face, and had a fever and headache for several days. This illness lasted two weeks. At the same period the following year, the other man and he had similar pustules although they had not again touched the shrub. The poison appears as a white juice, so strong that if it barely touches the body, by the ninth day the illness has spread throughout the whole system. After explaining what might happen to him, Fontana as an experiment directed a man to break off a sprig so that the sap spread over his palm which was very tough and callous; there was no effect. The poison is terribly strong. I broke off a sprig but carefully so as not to be touched by the sap. I held it im my bare hands but noticed no spillage nor the slightest change.

Fontana administered the sap and an extract of it to animals by mouth, to the bared skin, and to wound made in skin and muscles; no effect was noted.

Tobacco

The «oil of tobacco» studied by Fontana,[5, II, 185] probably a distillate containing not a great amount of nicotine in view of its low toxicity, was applied to the muscle of pigeons. Vomiting was the most constant effect observed.

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[i] See the thesis of Antonella Ottanelli, University of Florence, 1976.

Opium

Galen prescribed opium in accord with his theories of disease and drugs, the latter ordinarily undergoing change in the body and exerting an action through an elementary quality of hot, cold, dry or moist. Opium, being cold in the fourth degree, cannot be modified by the body, thus retains its extreme coldness, causes sleep or stupor, and in high dosage kills. These tenets were still being debated at the middle of the seventeenth century.

Robert Boyle, believing that pharmacology must be experimental described two experiments that became classic as demonstrations for centuries. A cat, instead of being tranquillized by opium, became so wild that the porter was obliged to kill it. A dog, after intravenous injection of opium, fell into narcosis so deep that its tongue hung out and its extremities were motionless. Taken into fresh air and whipped into action, it escaped death.

Thomas Willis[37] wrote that opium causes sleep because it acts on the cerebral cortex, there depressing the formation of vital spirits which preside over sensation and voluntary movements, leaving intact the natural and vital faculties, although these are fatally destroyed by a large dose. He stated that the analgesic action of opium is not peripheral but central, on pain perception in the cerebral cortex.

Richard Mead[26] believed that opium causes sleep because it produces cerebral vasodilation, as occurs after a large meal.

In the eighteenth century, the experimental approach to the pharmacology of opium developed in the hands of Boerhaave, Whytt, and Haller. Particular attention was paid to the heart, Whytt finding that its irritability or moving power was lessened, Haller finding no change in heart rate in the whole animal. All agreed that other muscular activity was reduced. Monro found that opium weakened the power of muscles including the heart, but Haller was ever vague about the nervous fluid and the influence of narcotics on the irritability of nerves. Cullen took up some Hallerian ideas, although he had little faith in animal experimentation, regarding its results as uncertain and contradictory. He considered that drugs act on the organism either as depressants or stimulants. Opium, which he believed had a depressant action throughout all phases, reduced the tension of the solid parts of the brain. In this he was not followed by his pupil John Brown who felt otherwise, to the extent that his pupils honored him with a bust inscribed, «Opium, me hercle, non sedat».

Fontana's first comment on opium appeared on pp. 210-2 of his Epistolary Dissertation of 1757.

> Opium, says M. Laghi, destroys the irritability of the intestines, that seems to prove that this quality depends on nerves. That reasoning should not conclude, at least it is not proved, that opium acts precisely on the nerves when it destroys the irritability. For if it acted on the *gluten* itself, it could destroy irritability without any change in the nerves. M. Laghi has again supposed, he has regarded as demonstrated, that which is in question, that the muscular movement which opium abolishes comes from nerves. I have made some experiments which perhaps will not be useless.

Opening the lower belly of frogs to expose the crural nerves, Fontana applied to them several drops of laudanum or alcoholic solution of opium. With the latter, stimulation of the nerves or the spinal cords failed to cause contraction of the leg muscles after about two minutes. After laudanum, the stimulated nerves continued to elicit contraction of the muscles for two hours in spite of repeated application of the laudanum. Fontana concluded that opium had scarcely any influence on the exposed nerves, that the abolition of the response in the one group was due to the alcohol rather than the opium. Further experiments with opium in almond oil or in water, and alcohol alone, confirmed these findings, which seemed to make it likely that opium brought by the blood changed the [muscle] fiber itself, or the *gluten,* the proposed site of irritability. On p. 234, he wrote: «Opium, besides, in my experiments and those of M. de Haller, abolishes the irritability of muscles, but not that of the heart». In his *Ricerche sopra il veleno della vipera* of 1767, Fontana continued to discuss the action of opium.

> That opium causes convulsions is owing, in my opinion, to its destroying at different times and in an irregular way, the irritability of the muscular fibres.[5, I, 81]
> But what will they reply to the example of opium? it kills by weakening, by even destroying, the irritability of the [muscular] fibres. If the virulence of this vegetable juice resides essentially in its gummy and resinous part, will they likewise suppose the existence of salts there?[5, I, 103]
> It appears very certain, that the venom of this animal [viper] differs but little from opium as to its effects, and that its mode of action on the fibres comes very near to that of this vegetable juice. Both of them excite violent convulsions and vomitings. Each of them conveys a universal debility into the organs. They render the muscle paralytic, make the animal heavy, and finally bring on a speedy, death, by destroying the irritability of the fibres. The heart alone in both cases still preserves its irritable quality for some time after the death of the other parts.[5, I 114]

In the 1781 *Treatise on the Venom of the Viper,* Chapter II in Part IV, on «considerations on the nerves in disease» (p. 199), he wrote:

> I am not ignorant of its having been asserted, that opium, when it is applied directly to the nerves, produces a palsy of the muscles; but I recalled to have seen some years ago, a disorder of this kind clearly occasioned by the spirits of wine in which I dissolved the opium, since it did not ensue when I tried it dissolved in water. This circumstance appears to me so interesting, that I shall not fail to repeat the experiments at my leisure, particularly as the authority of Monro, who has found the contrary, may be opposed to mine.

Monro described in 1783[38] some experiments with opium which he had communicated to the Philosophical Society of Edinburgh and published in 1771.

> When I poured a solution of opium in water under the skin of the leg of a frog, the muscles to the surface of which it was applied, were very soon deprived of their power of contraction. In like manner, when I poured this solution into the cavity of the heart by opening the vena cava, the heart was almost instantly deprived of its power of motion, whether the experiment was performed on it fixed in its place or cut out of the body. I opened the thorax of a living frog, and then tied or cut its aorta so as to put a stop to the circulation of its blood. I then opened the vena cava, and poured the solution of opium into the heart, and found not only that this organ was instantly deprived of its powers of action, but that, in a few minutes, the most distant muscles of the limbs were extremely weakened. Yet this weakness was not owing to the want of circulation, for the frog could jump about for more than an hour after the heart was cut out. In the first of these two experiments we observed the supposed *vis insita* destroyed by the opium, in the latter the *vis nervea;* for it is evident that the limbs were affected by the sympathy of the brain and of the nervous system in general, with the nerves of the heart.

Note that an aqueous solution of opium was used.

Back in Florence, Fontana wrote in the Supplement to the *Treatise*[5, II, 360] first reviewing his own, Haller's, and Whytt's experiments, that he wished to repeat and extend his own, to give greater certainty. He gave aqueous and alcoholic solutions of opium to cold and warm-blooded animals. *Guinea pigs,* after oral, rectal, intraperitoneal, and subcutaneous injections, became motionless and died. After intraperitoneal injection particularly, convulsions were seen.

> It is a fact, then, confirmed by all my experiments, that opium when simply dissolved in water, kills animals with warm blood; although it is besides true, that

> when dissolved in spirit of wine, its effects are much quicker and more violent; but they then proceed, in a great part however, from the spirit itself, since we have seen that this spirit is alone capable of producing all these effects, and that it even produces more violent ones, and that more expeditiously.[5, II, 363-4]

He then did experiments on turtles, frogs, and leeches. The last were killed in about the same time in alcohol, alcoholic opium, and aqueous opium. Immersion of one-half of a leech deadened that one-half only! *Turtles* were given the same three solutions orally, rectally, and subcutaneously. He wrote that opium in water acts violently, even to kill, but is trifling (sic) as compared with alcohol! The turtle heart was exposed *in situ* and solutions dripped upon it, but Fontana felt that this was uncertain, as the vessels might carry the drugs in the circulation, deranging other viscera, or the blood. Therefore he removed the heart, applied solutions, and concluded that opium in water has no immediate action on the heart of turtles. To hundreds of *frogs,* he gave the same solutions orally, subcutaneously, to hearts excised and *in situ,* to the crural nerves *in situ,* detached from the cord, excised with the muscles, and to the exposed brain. He concluded that he could not find that opium had wrought any greater change on the nerves than simply water.

> I can conceive nothing more decisive and more certain than the series of experiments I have just related, from which it seems of necessity to follow, that the circulation of blood and humours in the machine is the vehicle for opium, and that without this circulation it would have no action on the living body.[5, II, 383]

On intravenous injection into *rabbits,* after about eight drops of the aqueous solution, loss of posture occurred. About forty drops caused death instantly.

> When once it is received into the vessels, I do not see how it can communicate itself in an immediate way to any of the nerves, since anatomy assures us, that the inner membrane of the blood-vessels is not furnished with what can properly be called nerves, and although this should even be the case, opium, on touching a nerve, does not act on it in any way [...][5, II, 285]

Fontana favored an action of opium on the blood, by analogy with his observations with the venom of the viper, the Ticunas, and the cherry-laurel.

Venom of the Viper. Contemporary opinion of these works was largely favorable. The *Giornale d'Italia* said:[11]

> Two years have gradually run their course since, a learned professor at the University of Pisa having conveyed to us an idea of that distinguished work concerning Natural History, we imparted it to the public. Now we shall give an adequate extract from that same work, with all the greater pleasure in that it will be seen how much Mr. Fontana has, by means of experiment and observation, elaborated upon this important subject and to what extent it should engage the attention of every educated person.

Presentation of the work was extensive,[10, 11] particularly in the *Riflessioni*[8] which contain many laudatory phrases.

British comment on the work, included in lengthy relation of the findings, was commendatory, if cautious.

> To the merits of Fontana we have often borne a willing testimony; and of this work, we have given a very ample account [...][17] It is enough to observe, in general, that this volume is chiefly composed of decisive and well conducted experiments; that the reflexions and arguments are clear, accurate, and judicious, and that the author has greatly extended the bounds of physiology.[13]
> Under this head [on the cause of death] we have much nice disquisition...[14] He supports these views with much ingenuity; but if we had room for our own observations we could show, that M. Fontana has been in the light of truth without obtaining it...[13]
> M. Fontana, who is perhaps too partial to experiments, may, we apprehend, have made his conclusions rather hastily.[16] Our author very justly observes that we have hitherto consulted books, rather than nature; and have joined in repeating fancies, till we have imagined them to be real.[13]
> We can only repeat what we already have said, that this work is in general accurate and sensible; though confined to one subject, it conveys much curious information on several parts of the animal economy.[13]

Fontana's French friend and editor Gibelin analyzed the work in detail,[15] commenting that «M. Fontana combined the diligence and sagacity which characterize him». He pointed out Fontana's fairness in citing the work on theriac, the letter of Galen to Piso.[39] Fontana's excerpt from it reads:[5, I, 67]

> There are men who under the pretence of possessing an antidote, have themselves bit by vipers; they previously give them a certain paste,[i] which stops up the holes

[i] *Offa* (Latin) or *focaccia* (Italian), a dense bread.

in their teeth, and thus renders their bites ineffectual, to the great astonishment of the spectators, who are ignorant of the method employed by these people to conceal their imposture.

Gibelin related the following anecdote:

M. Coste, the scholar and translator of Mead, reports an observation which will not be out of place here. A physician, as well as his father, also a physician, was bitten by a viper. As all too often happens among physicians, these gentlemen were not at all in accord on the theory of the illness caused by the venom of the viper, or on the treatment indicated. Consequently each treated himself by his own method; both recovered, both were right. These gentlemen took no heed that when everyone is right, it is possible that everyone is wrong.

Gibelin found distressing what Fontana had pointed out, that

by an unlucky but necessary mechanism, the same tooth at once conveys the poison into the animal the viper bites, and into the aliments it feeds on. I am very far from thinking that all this singular mechanism [...] has been expressly made for the destruction of other living creatures. The venomous liquor with which it is provided is perhaps necessary to its digestion, and I shall show that it singularly disposes the flesh on which the viper feeds, to a speedy putrefaction.[5, I, 63]

Indeed Fontana expounds at length on this point, where as to facts he has been supported by the demonstration of numerous proteolytic enzymes in the venom, and as to interpretation by the general acceptance of his concept. The presumed salivary function of the serpent's venomous glands early instigated comparison of venom with the digestive ferments,[40] particularly ptyalin, pepsin, and trypsin.[41, 42]

The severest criticism of Fontana's work, largely unfair as we shall show, came from Orfila,[k] whose opus on toxicology[43] first appeared in 1814 and went through many editions in various languages. It listed the physical and chemical properties of the venom, all taken from Fontana but without mention of him; later he is cited as the source of information of many aspects of its actions. Orfila cited a work of Paulet, *Observations sur le Vipère de Fontainebleau,* 1805, as establishing that the bite of this viper can be fatal, contrary to Fontana's assertion.

Orfila quoted Mangili on two points he made against Fontana. «Professor Mangili has recently undertaken a series of experiments to

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[k] Mathieu Joseph Bonaventura Orfila (1787-1853), born at Mahon, Minorca, studied in Valencia, Barcelona, and Madrid, later became nationalized in France and professor of legal medicine and chemistry in Paris. After sacrificing 4,000 dogs to the investigation of poisons, he published his general toxicology and other works.

ascertain, 1° if the venom of the viper, introduced into the stomach, can cause death as Fontana has proposed; 2° if it no longer exerts its toxic properties after having been dried and kept for nine months, as the same author reported. As one may judge from the following facts, both of these assertions are erroneous». Mangili had reported[44] experiments in which the fluid venom of 3-10 vipers had been placed in the stomachs of blackbirds, ravens, and pigeons with no effect, and that the dried venom stored for 14-26 months was still active.

The first claim touches on a weak point. For three reasons, namely his observations that very large doses of curare by mouth could be fatal,[5, II, 104-5] that conjunctival instillation of venom into the eyes of pigeons produced inflammation[5, II, 320] Fontana suspected[5, II, 106] that the viper's venom itself might kill animals when swallowed plentifully,[5, II, 321] although his servant, Jacques Benvenuti, a native of Tirol, had swallowed the venom without harm.[5, I, 56] Fontana made this experiment:[5, II, 321]

> I cut off the heads of eight vipers, and received the venom expressed from them, in a tea-spoon; it was almost full, and might contain thirty drops or more. I introduced the whole of this into the aesophagus of a pigeon that had fasted for eight hours. In less than a minute it became very weak, two minutes after it began to reel, and at lenght fell on its side in violent convulsions, dying in less than six minutes. The beak, aesophagus, and crop, to the very gullet, were inflamed and livid, and the blood was blacker than usual; these parts were so discolored, that they seemed to tend to gangrene and sphacelus. It can no longer then be doubted but that the venom of the viper, contrary to what Redi and many other famous observers after him have written, is a violent poison.

Why Fontana put so much faith in this single experiment is not clear.

As to the second point, Mitchell has commented:[45]

> the learned Abbé distinctly states that viper poison is active after being preserved for several years [...] this is not the only instance when the learned Abbé has been misquoted and misunderstood. Few authors of such merit as Fontana have had so little justice at the hands of those who have followed them, and this remark applied not only to his work on the viper, but to his researches on Ticunas and to many other labors, many of the results of which have been assiduously rediscovered by more modern observers.

Orfila also wrote:

> However, Fontana claims, from a very great number of experiments, that these preparations [ammoniacal] used internally and externally are more injurious than

> the reptiles. M. Everard Home[l] contests the opinion of those who believe that ammonia and *eau de luce* (mixture of oil of amber, alcohol, and ammonia, sometimes with balm of Gilead added), are specifics against the bites of serpents. We are of the opinion of this celebrated surgeon, but we think that the volatile alkali cannot be harmful, and even that it might be considered as a medicament very useful in favoring sweating.

Ammonium salts acting internally can be dangerous, and no therapeutic value against venom is now recognized. Orfila, who recommended as antidote the local application of ammonia and oil, and ammonia by mouth, was wrong. He also remarked:

> Fontana has concluded from his experiments that caustic potash was a specific against viper bite; an assertion which cannot be sustained if one reflects on the way in which this alkali acts; as a matter of fact its action is analagous to that of all other caustics.

As a matter of fact, Fontana made no such statement; only that: «Alkaline salts, caustic and not caustic, whether mineral, vegetable, or animal, had the same effect [i.e., none]».

Orfila may have had in mind Fontana's experiences with lunar caustic, another matter entirely. Orfila also commented in this way on another statement of Fontana's:

> Warm water baths diminish the dangers, which stop on prompt amputation of the part bitten. Finally, ligatures sometimes guarantee mishaps. The experiments that Russel[m] has made with the snakes of India far from confirm this assertion of Fontana's, to wit, that injury is stopped by amputation of the part bitten.

What Fontana actually wrote was:[5, II, 10]

> I made repeated experiments by dipping the part that had been bit in various fluids. The plunging into very warm water, and keeping it there, appeared to me to be truly advantageous. The pain was evidently lessened, the inflammation abated, and the color less livid and changed.

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[l] Brother-in-law of John Hunter, Home consigned himself to oblivion by burning Hunter's manuscripts after using them as groundwork for sundry lectures and other alleged scientific contributions of his own devising (Garrison).

[m] Patrick Russel (1727-1805) author of *Account of Indian Serpents,* 4 vols., 1796-1809.

Fontana's experiments on amputation we have already discussed. His conclusions seem cautious and well-judged.

In 1849 Fontana's *Treatise* was read attentively by Claude Bernard (1813-1878), then holding a special chair created for him at the Sorbonne.[46] Bernard was much impressed and influenced by Fontana's techniques; he was to write later «Localized poisonings, as Fontana and J. Müller have already used, constitute a valuable means of physiological analysis». In general, Bernard's method of research was very close to that of Fontana, described by him as the «analytical method». The chief point of which Bernard was to depart from Fontana's opinions was that of the viper being innocuous to its own species. He wrote in his notebook:

> This chapter [of Fontana's] is filled with false considerations, dangerous as a scientific philosophy. Combat this claimed difference in animal organization which kills one and spares the other with the same substance. His experiments with leeches prove nothing. He assumes that only warm-blooded animals perish from the venom of the viper. This is not precise at all.

Fontana made no such assumption. Bernard later wrote:

> One hears of hedgehogs which are not poisoned by prussic acid, the goat which eats belladonna, sheep which accept enormous doses of arsenic, toads not themselves poisoned by their venom... All these things are false as explanation. Because if one admitted that, science would be impossible... Thus an experimenter who has injected a toad with its venom without effect or a goat with belladonna with no result would say: that is logical; yes, but there are facts which one cannot believe because the mind is certain of their being otherwise. I have not been able to believe that about the toad. I would have had, if I had not overcome it, to give my resignation as physiologist.

Bernard blamed Fontana for not having observed his animals long enough. As Mitchell says,[45] this is not true. Fontana watched his vipers bitten by other vipers for periods as long as seven days.

The next eminent investigator of the venom of the viper was S. Weir Mitchell.[n] Working with the American rattlesnake, *Crotalus,* he confirmed many of Fontana's findings,[45] but did not materially advance the understanding of the cause of death from the venom.

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[n] Silas Weir Mitchell (1829-1914) studied in Paris and became the leading American neurologist of his time. Besides his medical monographs, his poetry and novels give him a place in history.

Fontana is credited with first describing the venom gland.[47] He was the first to describe the course of the venom through the fang, although as he pointed out himself, others knew of it previously. He confirmed Redi that the venom must be introduced into the body, by puncture or injection, but this is said[48] to have been known to Pliny. Fontana recanted from this position as the result of a single experiment. he confirmed Redi in identifying the yellow liquor, not the saliva, as the venom. Redi had implied that the effect of the bite was related to the size of the animal, but Fontana was much more explicit about this, and showed that the effect is related to the size of the dose of venom per unit of body weight, and he determined with considerable accuracy the size of the fatal dose.

On the probability of a fatal outcome in man from a bite of a viper, Fontana may have underestimated it. Phisalix[49] quotes Viaud-Grand-Marais on mortality of fourteen per cent (50 deaths in 370 cases) for the Loire-Inférieur and the Vendée. Orfila quoted Paulet as establishing that the bite of the viper of Fontainebleau could be fatal. Brown[50] calculated that the bite of *V. berus* contained only 0.03 lethal doses for man, by extension from data in experimental animals; the probability of death from this dose cannot be calculated from this figure alone.

Boquet[51] states without giving evidence, «Occasionally death rapidly follows *Vipera aspis* bite when the venom has been directly injected into a small vessel». Gitter and de Vries[52] say, «The most complete information has been compiled by the Swedish Public Health Service. It is estimated that a total of about 1300 bites by *Vipera berus* occur each year in Sweden; about 12% of the victims are hospitalized. The entire death rate is very low; from 1915 to 1944, only fifteen of the 4736 hospitalized snake-bitten patients died, a mortality of 0.3%». This agrees with Fontana's estimate of less than one per cent.

In Britain, adder bites are uncommon and when they do occur poisoning is often trivial; nevertheless severe poisoning sometimes results and on rare occasions it is fatal. In a series of 95 cases, reported over the past 100 years, 14 deaths occurred, but the total number of adder bite victims treated in British hospitals is unknown, and it would be grossly misleading to deduce a mortality rate from these figures.

In Italy deaths recorded from poison of animal origin (Statistica cause di morte, Roma, Libreria dello Stato) were, in twenty-five of the years between 1895 and 1923, 382 in number from bite of a viper or other poisonous animal. In eight of the years 1961-1970, there were

recorded in Italy (Annuario di Statistiche Sanitarie, Roma) sixty-six deaths from «bites and punctures of poisonous animals and insects».

Fontana's uncertainty as to the cause of death extends to the present day. He proposed first that it resulted from loss of muscular irritability. Meldrum[56] wrote: «In the first major study of paralysis by snake venoms, Fontana showed, in many mammalian and avian species, that nerve conduction is not affected by venom, but that the direct excitability of skeletal muscle is abolished». He abandoned this explanation, recognizing that it resulted from over-emphasis of his experiments with frogs. Today, from experiments described by Brown[50] and Russell,[41] it would seem likely that, as Fontana suggested, loss of peripheral neuromuscular irritability, and also of voluntary muscular activity through impairment of the central nervous system, are secondary effects; they result from circulatory insufficiency following loss of blood from the circulation.

About half the cases of human adder bite seen by a physician give him cause for real anxiety.[53] In severe poisoning persistent or recurring shock is the main feature, as shown by weakness, sweating, thirst, collapse, confusion, semiconsciousness or loss of consciousness, coldness, cyanosis, absent pulse, and low or unrecordable blood pressure.[54] Apart from the depression of consciousness and dilated pupils no abnormal neurological signs are recorded. Generalized bleeding is common. Most patients respond satisfactorily to simple symptomatic treatment, but fatalities have occurred because the victim was thought to have only slight poisoning. A ligature may delay absorption of venom into the general circulation. Zagreb antivenom is effective against *Vipera berus* bite in monkeys,[55] is indicated in man in persistent or recurrent shock, and should be considered to minimize morbidity from local effects.[54]

The other explanation of the cause of death which Fontana advanced was the coagulation of the blood *in situ*. A special case of the result of intravenous injection, it seems unlikely that this is the cause of death from a bite, or injection other than into a vein. However, the observation has led to a long series of investigations with Fontana receiving a considerable amount of credit. Thus:

> This group of experiments (on coagulation of the blood) could be considered to be a logical continuation of those initiated in 1769 [sic] by Fontana.[57]
> Apparently the first well documented oservations of the effect of viper venom on the blood were made by Fontana.[58]
> The hypothesis (transient afibrinogemia) was the direct result of a venom which, like thrombin, clots fibrinogen [...] was suggested by Fontana in 1787 [sic].[59]
> Fontana was the first to describe snake venom as coagulants and anticoagulants.[60]

These statements contain a small amount of the truth, which is set forth by Phisalix.[61] Fontana himself quoted Redi, the passage which we give on page 272. He also was no doubt aware of Lemery's statement, which must have been generally known:

> We must consider two sorts of effects in poisons; the one does coagulate the blood by degrees, as that of the *Viper,* the *tarantula,* scorpion, hemlock, woolfs-bane, *etc.* and whereas these do hinder the motion of the spirits by the coagulation, the animal falls into convulsions, and dies soon after; much after the same manner as it happens when some acid liquor is syrenged into a vein or artery.

It should be realized that Fontana did not, himself, describe the anticoagulant action of venom *in vivo,* but only *in vitro,* when he simply confirmed the observations of Mead. Fontana quoted[5, II, 39] Geoffroy and Hunauld, who wrote in 1737 that they had had bitten several pigeons, chickens, cats, a goose, a turkey cock, and eight dogs; they observed, not coagulation in the blood but on the contrary, every sign of fluidity. The same observation was made in 1854 by Brainard, in 1860 by Mitchell.

Phisalix pointed out that whether coagulant or anticoagulant action is seen depends on the species of animal bitten (or providing the blood for experimentation *in vitro*), the species of serpent, the temperature, and the concentration of the venom (or the dose *in vivo*).

Perhaps the subject of greatest controversy has been Fontana's aphorism that the venom of the viper is not at all a poison for its own species. This has been revised by Phisalix to read: the venom of the viper is not a poison for its own species, *under the natural conditions of injection.* The immunity thus turns out to be, not absolute, but of a quantitative nature. In the following Table are shown some fatal doses of the venom of *V. aspis* (our averages).

Fatal doses of the venom of *V. aspis*
milligrams per kilogram body weight
adapted from Phisalix[63]

Vipera aspis, grass-snake	1,111
Frog, eel, salamander, toad, crocodile	26
Dog, rabbit, rat, guinea pig	2
Pigeon, chicken, buzzard	1

Fontana himself had qualified his aphorism by writing:[5, I, 272]

> When I say that a substance is venomous to an animal, I mean to express, that it produces in it very violent disorders, although it is only introduced into its body in a small quantity.

Untouched by the centuries-old myths[64, 78] attendant on the animal that seduced Eve, bit Cleopatra, and accompanied Aesklepios as a symbol of the healing power of nature, Fontana showed that the poisonous nature of the venom of the viper could be subjected to investigation by experiment.° He was justly proud of having proceeded methodically, and if he seems to have been preoccupied with details, it is because he adopted the «analytick method» in presenting his results. If he did not achieve a new synthesis, noone else has either. In 1968, Boquet[65] wrote: «The mechanism of the biochemical reactions through which snake venoms cause death is not as yet known».

Curare. An account of Fontana's article in the *Philosophical Transactions* appearing in the *Critical Review*[66] merely retailed the observations; that in the *Monthly Review*[67] paid particular attention to Fontana's attack on the viewpoint that the disease produced by the viper and the American poison is to be attributed to an alteration of the nerves. The reviewer also wrote:

> We cannot, without both feeling and communicating pain, dwell long on these cruel experiments made with the American and other poisons applied, in various modes of torture, to rabbits, pigeons, and other animals.

Claud Bernard wrote about curare in 1845 that «nothing was known about the mode of physiological action of this substance».[68] From his point of view he was correct; Fontana had provided much information about the properties of this poison for which Bernard gave him small credit, but – he was so near and yet so far from the demonstration which Bernard was to make, that the site of action was not nerve, not muscle, but the «neuromuscular junction». As explanation this is tenuous enough so that Bernard, lacking any concept of how the nerve brought about the excitation of the muscle, was still far from having the last say.

Cherry-laurel. At the end of Fontana's Second Tract on the Cherry-Laurel he wrote:

° These seem intolerably cruel today.

> So that after all these experiments, notwithstanding that they have been greatly varied and multiplied, we are ignorant in what the poison of the leaves of the cherry-laurel really consists; we are ignorant of the mechanical effects of this poison; and we are ignorant on what part of an animal it acts, when it causes its death.

In his Supplement[5, II, 359] he added, after describing the results of his experiments with intravenous injection of the cherry-laurel:

> It is then beyond doubt that the spirit of the cherry-laurel itself, given in sufficient doses, and rendered more active by re-distillations, is a violent poison when introduced into the blood by the jugular vein, and that it kills instantly [...] It is not only absurd to have recourse to the nerves to explain the action of the poison in these cases, but this imaginary hypothesis is quite superfluous, since its violent effects on the blood are so very evident.

Fontana's conclusions were not accepted, according to Cullen,[P] who wrote:

> The tendency of the lauro-cerasus is plainly to destroy the mobility of the nervous power, and thereby the vital principle... it is only necessary to add, that when the lauro-cerasus is given in such a manner as instantly to kill, without occasioning almost any other disorder, there is not any change in the state of the blood to be perceived, and upon that occasion, that the death is owing to an operation on the blood, I believe there is no other physiologist in Europe, except the Abbé Fontana, who can imagine.

Benedicenti wrote that Cullen misquoted Fontana; this is discussed further below.

Toxicodendron. Orfila[43, I, 725-32] noted that Fontana's observations on himself and in experimental animals had been confirmed by Gowan and Amoreux, but that Lavini, while confirming the lack of effect in animals, had seen a reaction in himself appearing twenty-five days later! Kligman[35] states that 24 to 48 hours is the usual latency, although it may be as long as 12 days. Repeated exposure of Lavini seems likely.

P William Cullen (1712-1790), a pupil of Monro *primus,* occupied the chairs of medicine and chemistry at Glasgow and Edinburgh. He held that life was a function of nervous energy, muscle a continuation of nerve, and disease mainly nervous disorder.

In comparison with the earlier reports of Kalm and Dudley, Fontana appreciated, as they did, that not all human beings are sensitive to *Rhus* when tested. He failed to appreciate that sensitivity may be developed by repeated exposure. Dudley wrote: «A second thing to be remarked of the Poyson-Wood is, that it has this effect only on some particular Persons and Constitutions». Kalm wrote: [...] «it is noxious to some people yet does not in the least effect others [...] though a person be secured against the power of the poison for some time, yet, that in length of time, he may be affected with it, as well as people of a weaker constitution». Fontana attributed the insensitivity of his gardeners to poison ivy to their hands being extremely callous. Apparently he did not repeat the application to the skin of his guinea pigs, which can produce sensitization.[70]

Opium. It cannot be said that the 18th century experimental analysis of the site and nature of the action of opium in the animal body contributed much understanding. Although Willis and Cullen located its most important actions in the brain, Whytt, Haller, Fontana, and Monro all directed their observations to peripheral neuromuscular systems. Whytt resorted to vague statements, writing:[71]

> Opium, which is remarkable for its power of impairing or destroying the sensibility of all the parts of the body, also lessens or suspends the irritability or moving power of the muscles.

He concluded from his experiments that the heart is not exempt from this action, contrary to Haller, and decided that opium does not produce its effects by entering the blood and being conveyed to the brain, since it destroyed the sensibility and moving power equally well in frogs deprived of the heart.[72]

Monro *tertius,* writing[73] about the experiments of his father,[74] said:

> It is evident, that, in those several experiments, the poisons act upon the *muscles by the medium of the nerves,* and not by the mechanism of the blood, as Fontana had supposed.

In General

In Fontana's work with the airs, or gases, he showed clearly that pharmacologically they could be separated into two classes: the one exemplified by hydrogen and nitrogen which had no toxic action

although they did not support life, the other exemplified by carbon dioxide which had in addition a toxic action of its own. Otherwise Fontana's reputation as pharmacologist rests on his *Treatise* on the venom of the viper and the American poisons, largely on his studies of the venom.

Most generally formulated, his contribution was the demonstration that the problem could be examined experimentally, and the arrival at the conviction that the results were applicable to man. He proceeded in a systematic way, following his principles of multiplicity of experiments under a variety of circumstances in different animals. Specifically his contribution may be divided into those largely original with him, and those not so.

The original demonstrations are perhaps six in number. (1) He clearly and correctly described the anatomy of the envenoming mechanism. (2) He showed that the venomous principle was not acid, not alkaline, not a salt, but what he called an «animal gum». He could scarcely have come closer to characterizing it, when Mulder was not to apply the name «protein», suggested to him by Berzelius, until fifty-seven years later, even then having chosen «a field of study of extreme difficulty – for the time an impossible once».[75] (3) He pointed out that the effect was directly related to the dose, and that there was a minimal lethal dose, the size of which he measured accurately. (4) he showed that the dose should be calculated per unit of body weight. Points three and four had been got at by Redi, but not clearly conceived and stated. (5) He was aware of what we now would call the enzymatic activity of venom, the digestive action of which he recognized might be the function of the venom in the viper itself. (6) He introduced the analytical use of local poisoning to identify the action on particular organs.

It is of interest that in Fontana's own opinion, his third and fourth points, and his conviction that results in one animal species could be extended as applicable to another, including man, were really hypotheses.

Points made by Fontana that were not original with him must include the action of venom on blood, in view of the statements of Redi, Lemery, Geoffroy and Hunauld.

Fontana did not quote, and he may not have been aware of, the experiments of the *Accademia del Cimento* on the venom of the viper. These were probably performed by Francesco Redi; they remained unpublished until 1780.[76] The complete note, in translation, read:

28 June 1660. To ascertain what might be the effect of the venom of the viper, two cockerels were allowed to be bitten. Opened up immediately after death they showed clotting of the blood in the cardiac ventricles and the vena cava; notable bleeding was not seen on dissection. Copious bleeding and no clotting was found in another cockerel which was strangled.

Early in the afternoon of the same day, before the Grand Duke, other animals: tortoises, foxes, and cockerels were opened after being bitten by vipers, and in all some clotting of the blood seems to have been found. To the same end a castrated ram was allowed to be bitten; upon its death three hours after the poison, there was no doubt of the evident coagulation. In another ram killed by drowning, through its head being held under water, the blood was fluid to the highest degree, and was seen to spurt forth violently when the vena cava was opened. It remains to be cleared up whether the clotting in the first might be attributable to the three hours of suffering a difficult death, in which there might have been cooling, therefore it was decided to have two other rams die, one from the bite of the viper, the other from a drink of arsenic, this diluted, however, enough that it worked in the same time as the venom of the viper. The result of this will be noted in its proper place.

Fontana's point that the venom must enter the body, but was comparatively less active when introduced into a relatively avascular area, had been made by Redi. Fontana here reversed his belief on the basis of one experiment; he claimed that what we would regard as enormous doses of both venom and curare were active by mouth. His demonstration that it was not the saliva that was poisonous was in confirmation of Redi.

Fontana's failures included the nonidentification of the nature of the fatal action of venom. At first laying it to an effect on nerves, he performed many experiments; how he thought about these is not easy to grasp. He and others were still undoubtedly under the influence of Hallerian irritability, and the only place in which this could readily be studied was in the peripheral skeletal neuromuscular system. It seems likely that he saw these experiments as providing a model for the action of the venom, rather than having any concept of a way of poisoning the whole animal by application of a substance to a nerve, in the way that we believe rabies virus behaves. He obtained positive results in finding that alcohol blocks conduction in nerve, and that the cherry-laurel blocks excitation at the point of application. He obtained negative results supporting his conclusions that the venom and opium are without action on nerves.

These observations led him to identify the blood as the site of lethal action. He wrote:[5, II, 388]

> Let not the suddenness of the effects of opium, and the sensible diminution of its weight [?], be brought in favour of the nerves, against the blood; since it has been seen that the venom of the viper, the Ticunas, and the Cherry-laurel, when injected into the jugular vein, act instantly, so as to occasion death, even when employed in very small quantities; and since it has been found by experience that the action or effect of these poisons is wrought on the blood, and not on the nerves.

At times, Fontana was not quite clear about the role of the blood; he added:[5, II, 394]

> It seems to follow from all these particulars, that opium does not act immediately on the nerves, but has need of the circulation of the blood and its humours, in exercising itself on animals.

It is also difficult to understand how Fontana could state that the action of opium was trifling in comparison with that of alcohol, in view of his awareness of the importance of dosage. Mead was nearer the truth on this point when he wrote:[77]

> the action of opium is very analogous to that of other volatile spirits, only that a small portion of it has a force equal to a greater quantity of most of them.

An important contribution was the sound demonstration that all reputed antidotes to the venom were valueless, even silver nitrate which, although it inactivated the venom, could not be usefully applied.

Fontana's last sentence in the main body of his writing on the venom of the viper was:[5, II, 75]

> Now, that a basis of experiments, and of determined facts, is formed, on the bite of the viper, the naturalist will continue his researches with more ease, and will present them with more brevity.

Fontana's French editor, in introducing the supplement, wrote:[5, II, 318]

> The learned and impartial reader will readily agree with me, that this work cannot fail of forming an epoch in experimental philosophy.

PART III

DOCUMENTS

Sections A, B, and C are translated from the autograph manuscripts found in the *Archivio di Stato,* Florence, I.E. Corte, Filza 119. Just preceding them is this note: «Summary Protocol of Affairs of the Department of the Crown and Court, Conference held in Pisa before H.R.H., 28 and 30 November, 1 and 2 December, 1791. Three memoirs of the Director Fontana containing: first, defence against imputations made openly against him by several persons; second, accusations against various individuals; third, a number of inconclusive facts. Reply by orders of the Sovereign, after the Visto [seen] has been applied». Then follow consecutively the three memoirs in Fontana's script. Only A bears at the end the seal and signatures as shown. C has been shortened as indicated by the omission of passages repeated from A and B.

The report of the necropsy translated in D is in Ms. 910 of the *Biblioteca Comunale,* Trent.

Of the abbreviations, in section A H.M. and H.I.M. refer to His Imperial Majesty Joseph II, King of Bohemia and Hungary. Emperor of the Holy Roman Empire. H.R.H. refers to Peter Leopold, Archduke of Austria and Grand Duke of Tuscany. In section B H.I.M. or H.M. refers to Joseph II from 1765 until 20 February 1790, from then until 2 March 1792 it meant Peter Leopold as Leopold II. After 7 March 1791 H.R.H. referred to Ferdinand III, except when he was in exile from 27 March 1799 until 17 September 1814. R.M. refers to the royal Museum of Physics and Natural History in Florence.

The quotations of Peter Leopold on Tuscany and the Tuscans are from his «Descrizione dei principali impiegati nei vari dipartimenti ed uffizi del Granducato» in Section I, Volume I of his Relazioni sul Governo della Toscana, Arnaldo Salvestrini, Editor, Firenze, Olschki, 1969.

A

Memoir of the Professor Cavalier Fontana on the Anatomical Preparations for Vienna

A new source of worry for me was the commission of anatomical waxes for the Royal Court of Vienna. God alone knows the obstacles and difficulties I raised in order not to take it on. By itself it was beyond my strength, and being occupied at the R. Museum I did not even see the thing as possible; it would require stopping the work of the Museum, exposing myself weak and in uncertain health to a thousand exertions, to the greatest dangers, and almost to certain death. I tried to gain the support of young Brambilla,[a] who was pressing me, without any response even when I invoked the name of His Majesty. My arguments, my prayers, the risk of ruining everything, the loss of my health, even of my life itself, were of no avail on him, nor were heard my supplications to H.I.M. himself, either in Florence or in Pisa; useless was my proposal to H.M. to train some artists, taught the work by me, and send them to Vienna so that the waxes might be made under the direction of Brambilla. I even brought in Dr. Lagusius[b] to dissuade somehow young Brambilla from further insisting, and I myself begged him to make Brambilla believe in the impossibility of the outcome, and even my inability and unwillingness to undertake that work. I was so well served

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[a] See p. 121-2.

[b] Johann Georg Hasenöhrl changed his surname to its equivalent on the advice of van Swieten, whose professional training and moral attitude Lagusius unfortunately lacked. Although he occupied nominally the same position in Tuscany as did van Swieten in Vienna, the Grand Duchess could not abide him and chose as her obstetrician Giuseppe Vespa. Lagusius accompanied Peter Leopold to Vienna in 1790; his poor diagnosis and treatment of the Emperor's illness in 1792 may have played a part in the fatal outcome. Wandruszka (*op. cit.* 1968, p. 114) cites an anonymous letter about him.

by Lagusius that Brambilla painted him to me the next day as an envious man and my capital enemy, Brambilla certainly not knowing that Lagusius spoke for my good. So all was useless, and in the end I had to obey the orders of my Sovereign.

All the most flattering promises that had been made me by young Brambilla were then confirmed in the name of H.M., also directly by H.M. himself in Pisa; I was assured of his special protection and even a change in my status, the work being finished. Only four years were given me to do all the work required, which was then eight statues and two hundred cabinets of anatomical waxes, which could hardly have been done well in eight years; a little later to my great surprise and pain there were ordered from me sixteen statues and four hundred cabinets, all in the same four years, without regard for the possibility of the commission or for my health, and I was forced to accept all or be persecuted and ruined, as was given me implicitly to understand through the Imperial Resident Weigl.[c]

Howerver I knew from past experience how dangerous it was to administer money, even in the name of the Sovereign, and to what calumny would be exposed the most honest and zealous man; thus to avoid at least that kind of persecution I obtained after much resistance and obstruction an agreement that the charge should go to the Imperial Resident, who then became my greatest enemy and persecutor, most unjust and infamous because I wished not to be corrupted by him but only wished to keep my zealous attachment to my Sovereign, whom I had the honor to serve for many years, to whom I owed everything, even my existence in Tuscany.

Weigl began his new incumbency by showing me a thousand courtesies and favors, by inviting me often to his home and his dinners, revealing to me his power and influence over H.I.M., and the effective means that he said he had, through friendly and close relationship with the secretaries who had accompanied their Sovereign in Italy, whom he wished me to know in person. Then he spoke of my bad luck in Tuscany, saying that I was neither loved in the country or esteemed by the Court. He said that I would be hindered more in my future work for Vienna than I had been in Florence, since he knew that it was not favored by

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[c] Joseph Weigl, who had been secretary of the Austrian legation, succeeded Joseph Wilczek as representative in 1773. He remained in charge of Austrian affairs into the period of Francis as Emperor (1792-1835).

those who ruled in Tuscany. He asked me later if I did not believe it a better decision to do the work in Vienna rather than in Florence, that he would find the way to adjust everything to my satisfaction, honor, and interest, but seeing no disposition in me to do so, he offered me in the name of H.I.M. himself carte blanche, and he told me he had orders to grant all requests and conditions which I might ask for my security and for my future status; he suggested to me that after four or five years at the most I would have from H.M. an honorable pension, so that I could live anywhere I wished without any expense, with honorific title. But all was in vain; I reminded him of my duties and obligations, the benefits and signs of good will that I had received from my Sovereign; that I had served H.I.M. in everything but not at the cost of ingratitude or my dishonor; thus finished the conversation between the two of us which left me agitated and full of fear, since he ended by telling me openly that *I would soon be sorry.*

Reassured however by my conscience, and convinced of the clemency and justice of H.I.M., I calmed myself little by little, and I threw myself into giving satisfaction in my new incumbency. Sixteen workers were trained by me, drawn one by one from nothing with such exertion and labor as cannot be expressed in words, and I began my anatomical work with the greatest possible zeal and attention. Some time later Weigl was informed by my dissector Semenzi – whose friend and secret confidant he had become, and who was at the head of a dastardly plot – that there was a lack of cadavers for the work for Vienna, that they were made to disappear on purpose from the hospital of S. Maria Nuova. Weigl then came to me in person, shouting at me as if the fault were mine, reproving me in the harshest terms for not having gone to Vienna, *where I would have found,* he said, *every convenience, and not these Florentine muffs.*[d] So passed several weeks with no harmony between us, with reproofs and mockery on his part, every time he saw me or I had to go to see him. Finally came the decisive day, fatal for my quiet and my rest, the injury and pain of which I still feel. Weigl appeared at my home in a great fury and angrily told me that he had discovered from the conversations of Covoni, Serrati, and of H.R.H. himself, that a plot had been formed to deliver no cadavers, the aim of it all being to impede the work for the Court in Vienna. He remarked that he was resolved to write of it to H.I.M., to inform him of everything,

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[d] *veggini,* literally hand-warmers.

and he advised me again to *agree to travel to Vienna, to leave forever the Tuscan Court, where I had nothing to hope for and all to fear, and where I would be poor, disparaged, calumniated, as I had been in the past, all of which was known even to H.I.M. himself, who for this reason offered me his sovereign protection and favor.* Finally he begged me to write with him to H.I.M. at Vienna, to confirm that in Florence there was a lack of cadavers for the preparation of the anatomical waxes ordered to the satisfaction and with the approval of him who ruled Tuscany, that it was in every way expedient to make them in Vienna if H.I.M. wished to see them finished. But since he saw that I opposed all this, that I was resolved to risk everything rather than fail in my duty, than to commit an infamous act unworthy of a good subject and man of honor, he had recourse to the final argument, which he thought would be convincing and would persuade me, that H.R.H. himself had spoken ill of me and my nature, that he had told him that I would not long remain in harmony with him as I had not with anyone else of my acquaintance; he remarked that no honorable man would ever do well in Tuscany, and that my zeal and assiduity in forming the R. Museum would never produce for me anything other than persecution and scorn in a land where all was corruption, cabals, and cliques; that from the past I should be able to judge the future, and that he was not unaware of the wrongs done me, the calumnies fomented against me, from whom they least should come. But seeing me obstinate in my resolution he stopped me from answering immediately as I wished and as I had begun to do; at all costs he wished me to think about it for a few days. In fact, after three days he came to me, saying frankly that he had already written the whole story to Vienna, and that, foreseeing my insane obstinacy, as he called my duty, he abandoned me entirely to my bitter fate, and that I would surely regret it but too late for me. I answered immediately and without any fear, that let him do anything he believed himself capable of doing against me, never and again never would he be able to induce me to dishonor myself by failing in my duty, no matter what evil might befall me, and that whatever might be my Sovereign's opinion of my person, I was conscious of not having been wanting, that my innocence would one day be recognized.

I know not how to speak of all the things Weigl told me, or if all he told me of Vienna, of promises made me, and of H.R.H., might be true, exaggerated, or false; I know well that he wrote to Vienna with little regard for the Person who should be most respected in Tuscany; I know well that he wrote ill of me, that he accused me of connivance with my

Sovereign, of cabals planned to evade more easily the Imperial commission I had accepted, he said for political reasons only, and that at any cost I must wash my hands of the work if H.I.M. himself wished to see it finished; and that in fact a little later H.I.M. himself wrote to my Sovereign that he should send to Vienna my dissector Semenzi with all his assistants, who he made Vienna believe were his own pupils, although they were trained by me with so much anxiety and toil; and from this painful era Weigl began my blackest persecution, not sparing cabals and intrigues, to ruin me in Florence and in Vienna, and in writing and in talk to expose me to the greatest mortification.

In fact it was not long until he found a way to sink money in corrupting even the sweepers at S. Maria Nuova, so that they would bring to him at night, not only the cadavers destined for the anatomical models that I made for Vienna in my home, but even those which should have served for the R. Museum itself. With the power of gold and gain he corrupted my dissector Semenzi in order that everything I did in my home for H.I.M. should go badly, and he induced Semenzi to execute anatomical preparation in another house, and secretly. By the power of gold he lured away the greater part of the pupils trained by me for the waxes commissioned by H.I.M. who worked in my home, leading them with money to work secretly under Semenzi, and then it was that the rest of my workers were corrupted; won over by Weigl they rebelled openly against me, they denied wanting to continue the work at the prices set by themselves, they denied in public prints any wish to work for me, at hours and on the days agreed upon as fixed in writing, neither did they pay any attention to their work, all threatening to leave me, since they were assured of protection, and greater advantages *from someone who could command me, they said.* Nor was Weigl content to spread confusion, disorder, and discord only among my workers for Vienna, but he brought corruption even among the wax-workers of the R. Museum itself, by bribery inducing Susini to work for him secretly and at night, along with others of the R. Museum; so that in a short time I was aware that no attention was paid to making anatomical waxes for H.R.H. by the artists of the Museum, that subordination was ended and that to all my representations and prayers the only response was defiance; the threat of further humiliations and troubles was so painful to me in this state that for a few days I considered fleeing from Tuscany and escaping forever from such persecutions.

I wished first though to inform H.R.H. himself, and to hear his final intentions and orders, always for me sacred and respected. I had the

opportunity to be content and consoled and he had the clemency to render justice to my zeal, to my innocence. H.R.H. discovered in a few days the daring plots against me, and found they even extended into foreign countries against their own laws.

Even with that the persecution of me by Weigl did not end. He tried everything to ruin me in Vienna; by powerful means which he had through the private secretariat he tried to throw a thousand suspicions onto me in the mind of Cavalier Brambilla, such that a thousand unpleasantnesses intervened during my dedication of all the five years that I worked for the Imperial Court. The artists already won over by him through promises for their own interest showed no more zeal for me and the Imperial works, and my prayers and threats served only to leave me derided and ignored. They were paid, although irregularly, by Weigl, thus he alone was regarded by them as their principal hope of receiving rewards and flattery; in time these were the two great means of keeping them bound and dependent, finally these people encouraged and moved obviously by the spirit of gain took part in the blackest calumnies against me to put me in a bad light not only at the Court of Vienna, but even with my own Sovereign. In this way were my zeal and my innocence rewarded and thus is treated a man of honor, with the most atrocious and unjust insults, opprobrium, and calumnies. I cannot say how surprising was a sovereign order from Vienna in favor of the artists who had served the Court so badly, who took from me books, untensils, waxes, models, instruments, in short all that is necessary to make the anatomical waxes, and at the same time I was ordered by Cavalier Brambilla in the name of H.I.M. to conclude the work, and finish the things still lacking, demanding at the same time the results and destroying the means. Then the order itself was so confused as to allow fifteen different interpretations, as later Abate Covi showed Brambilla himself. Meanwhile I was threatened with recourse to the Florentine courts of law by various groups of artists who interpreted it in different ways, each one at its own will and to its own advantage, in spite of possible quite opposite interpretations, and when I asked Weigl for clarification, only to know the wishes of H.I.M., I received in reply only sarcasm, invective, and threats; nor did it help me to write repeatedly to Cav. Brambilla in Vienna, whose responses were none other than humiliations and insolence. Willingly I pass over this part of my troubles, which were not entirely unknown to my Sovereign, to whom I showed all the orders and the letters, which were the reason why with his protection I could present a carefully thought-out Memoir to H.I.M., in which I related the

story of my life, of my labors over the five years spent on the anatomical work in his Royal Service. This memoir must be in the hands of Brambilla, who since wrote me that it had been consigned to him by His Majesty, who had read it, saying that he had seen with great surprise how much I had suffered, how much I had been persecuted unjustly without his knowledge. The writer would be lucky if he could now flatter himself that in some quiet moment his Sovereign had glanced at his pages; then he would judge my state, how much I really had suffered and done for the Court of Vienna. Nor have I said, here in my notes, all that I have done and could justly have said and that I would say, and that I could prove if there had been assigned to me a person or a judge to hear me. I know well that no man in this world would carry zeal and assiduity in his work, without self-interest, farther than I surely have done; but my zeal has made me a thousand enemies, my assiduity has occasioned four deadly illnesses; of the attack on my Sovereign, the persecution, the unconcern for the loss of my own property, and the poverty, this last is the only part which might be unknown to my Sovereign, although not to my enemies; thus I believe I should say a few words about it.

I believed from the time in which I was obliged to work for the Court of Vienna, that to remove me from the possibility of suspicion of poor administration it sufficed that all payments go through other hands than mine, hence Weigl was charged with them by H.I.M. himself. Such were my precautions against calumny and envy: I am unfortunately an example of this, for while I am being maligned unjustly, my affairs suffer and I see my own money thrown away.

When it came to taking a house for the anatomical work for Vienna, Weigl promised me, as was just, that I should be indemnified by him on order of H.I.M. for any expense that I should incur for reason of that work, and that I should be burdened with nothing in the new home provided that I myself went to live there in order to be closer to the artists. How he kept his word will be seen below, from the principal expenses of the Imperial service being borne by me. In the first removal of furniture from my home to the new one, I spent twelve *scudi;* my stove, valued at twenty *scudi,* was broken into pieces and I was obliged to spend twenty-six *scudi* on a new one. In the second and last move from the Imperial house to my own, I spent thirty-four *scudi,* because I had to pay with my own money for the transportation of all the Imperial utensils intended as gifts for the artists; in this move they broke a stove valued at twenty-six *scudi,* and I had to buy another for twelve *scudi.*

Obliged to live in the house provided by Weigl, I was forced to put in, if I wished not to die of the cold, a very high stovepipe to connect the stove with the chimney; I had to open two walls, put in a door, a corridor and a brick roof so that the stove could be used, spending more than eighty *scudi*. I spent another twenty *scudi* on a fireplace and had to make two rooms habitable and decent, which cost me thirty *scudi*. In the course of five years, for painting walls, repairing roofs, setting in order the common rooms, and other things, I spent more than fifty *scudi*. There was a garden with the house; to keep it properly and cover it in winter, I spent yearly thirty-five *scudi*, and I was obliged to do it in order to pick a few lemons. The money spent by me for the work for Vienna alone reached many hundreds of *scudi*, for which I was never reimbursed by anyone. I pass over what I paid monthly to my servants in the course of five years' work for that Court; on account of the putrid cadavers, the domestics would stay only if their salaries were raised; I pass over the wear-and-tear of my books, my clothing, my furniture, my repeated expensive illnesses; it would be fair to state that I spent more than a thousand *scudi* of my own for having served the Imperial Court.

I will not mention my suffering, my toil, for five years in a row; he who could expose his own health to so many dangers for all this time, he who was found at death's door four times in five years, must have been moved by passions other than self-interest. If self-interest only and not the honor of serving the Royal Family had animated me to do this work, I would have asked of Cav. Brambilla, when in the name of H.I.M. he asked me what were my claims, the work being finished and sent to Vienna, I would have been able to ask in all justice no less than ten thousand *zecchini*. Nor do I believe that one can judge from my efforts or in terms of recompense for the time I have spent, but rather from the quality and the quantity of the work done by me, and I am sure that if they wished to copy in Vienna the waxes made by me, they will not copy them in twenty years.

It is true that a diploma and a snuff-box have been bestowed on me, but it is also true that Brambilla, in whose hands my affairs and my fortune rested, as he told me himself many times, a few years before had granted the diploma of Baron of the Empire to the oculist Wincel[e] for having treated a few cataracts in the Vienna hospital. But I am more than

[e] This may have been the senior Wenzel of Paris, a great operator for cataract of this time, visited there by Scarpa.

contented with what H.I.M. has done for me, though I may hope for nothing from my enemies who have tried to limit the Royal Generosity. I have sought more honor than profit in my work, and in serving the Imperial Court I have served my Sovereign.

My enemies have tried with endless cabals and on every occasion to injure me, even to assailing my honor in Vienna, Tuscany, and elsewhere; I have many documents to bring to light which suffice to justify my conduct, and to show the calumnies of others. And if until now it has been permitted, or at least tolerated, that the guilty, the calumniator, offend with impunity, injure unjustly, it will also be permitted that the innocent shall defend himself and uncover calumny, and I like to hope that my Sovereign will permit such defence in the public prints, defence that is founded on natural rights.

H.R.H. has ordered to be affixed – «Seen».
Granted the 30 November 1791

[Signed] Luigi Bartolini[f] Huart[g]

SEAL

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[f] Luigi Bartolini Baldelli became a court chamberlain in 1767. In 1789 he was one of those designated to formulate a division of the property of the Crown from that of the State. In 1790 Bartolini as Senator was named as one of the provisory regents by Peter Leopold, who wrote of him: «Senator Bartolini, general administrator of the property of the Crown, is honest, able, and capable, full of zeal and careful attention to serving well, very active and precise, needing only to be restrained in his facility of proposing expensive works with luxury beyond the need and in his disposition to favor the persons with whom he deals». When in 1800 Ferdinand III in exile appointed his Regents for Tuscany, Bartolini was one of them.

[g] Carlo Huart, secretary to Bartolini, was «excellent, superlatively faithful and honest, exact, and truly a gentleman», according to Peter Leopold.

B

Memoir of Cavalier Felice Fontana in his Justification

My enemies have never neglected any opportunity to humiliate me, and to humiliate me even in public, in the most innocent matters where there would seem to be no chance to do so. For example, Galluzzi,[a] my old enemy, from pure envy and hate, without any good reason known to me, he being censor of publications found in a writing of Dr. Tramontani[b] in my defence in the unfortunate and unjust trial of Giorgi, the word, speaking of me, *Preceptor* of Two Old Tuscan Princes, and he angrily cancelled those words in the presence of the errand-boy of the printer Cambiagi, saying openly that I never had been anything in Tuscany, and he thus combined disparagement with injury, Galluzzi, if he was authorized to do so, about which I don't know, could have cancelled that innocent expression of Tramontani, previously unknown to me, without making me ridiculous and odious throughout the city as Cambiagi's errand-boy has imprudently done.

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[a] Riguccio Galluzzi, of a Volterran family, studied law and theology at Pisa, and taught moral philosophy at the Florentine *Accademia dei Nobili*. With the help of Pompeo Neri he rose through the bureaucracy, becoming censor in 1769 and counselor in 1784. Active in developing the *Archivio di Stato* founded by Peter Leopold in 1778, he wrote on request the *History of the Grand Duchy of Tuscany under the House of the Medici* which went through several editions and translations, pleasing Peter Leopold if not everyone. Apparently his hand as censor was gentle, almost never making a prohibition. Peter Leopold wrote of him: «talented, capable, a good business man, equivocal in character, an intriguer, dangerous, with many connections; in various business matters he has shown a tendency to go against orders, in accord with his views and his friendships; in his surveillance of the press he is too ready to favor individuals».

[b] Luigi Tramontani, lawyer, philosopher, naturalist, and sociologist, member of many academies. Although not ambitious, he wrote many erudite dissertation in mineralogy and natural history, and came to the defence of Fontana in the controversy with Giorgi.

Monsignor Fabroni,[c] my enemy ever since he went to Paris, only, I believe, because he knew me incapable of humbling myself with anybody and of wasting my time in paying him court, in the annual rolls published by the University of Pisa, where I too am Professor, to show Europe that I am nothing in Tuscany, always in naming me put down the single word *addictus* at the R. Museum of Florence, with nothing more, denying me, even though it has many times been pointed out to him by some friends, even the simple title of dissector, with which my Sovereign has also honored me, thus placing me with all the sweepers and servants of the place who are also *addetti* in the R. Museum, and he has always spoken and written of me in the same tone in Paris, in Tuscany and outside of Tuscany.

So many wrongs and injustices multiplied in so many different ways against me by my enemies have never been able to change my heart, naturally inclined to do good to others. A proof of that is Gagli[d] himself, whom I have never failed to help in his urgent needs, with my own money especially when he had to account every year to the auditors for the money he had spent. Still, even after all this, even after having endangered my health so many times, after having renounced the comforts of life, honors and riches, as I shall tell presently, I was calumniated, humiliated, depreciated, even in the eyes of my Sovereign, whom I have served faithfully. Arrived in Florence from Pisa in the month of April to go to Vienna, he received and consoled everyone; only to me was it denied even to throw myself at his feet and weep for my misfortunes. My enemies knew it and glorified in it in public cafés, in the R.M., and to humiliate me further went so far as to say that not one of many was turned away, even those not employees; the more to mortify me they said that even the Abate Frati among others had an audience. My Sovereign can punish me, although I am innocent, but my enemies

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[c] Angelo Fabroni, monsignor (1732-1803). He was inclined to Jansenism, against the Jesuits. Coming to Tuscany in 1767, he was made Rector of the University of Pisa. He wrote twenty-nine works including many biographies and eulogies, and traveled much in Europe. Peter Leopold wrote of him: «man of talent, much knowledge and capability, but of a character and manner so unreliable and imprudent that he makes himself hated by all his subordinates without knowing why; on account of his connections and correspondents in foreign countries it is very dangerous to admit him to intimacy, he being of dubious sincerity».

[d] Peter Leopold wrote: «Custodian Luigi Gagli, reliable, faithful and honest man».

have no right to make a mockery of me at the R.M., to render me useless in my position, for which I am still being paid by my Sovereign.

I would never finish were I to relate the mortifications, the troubles, the wrongs and calumnies from my enemies during the whole time that I had the honor to serve my Sovereign in the R.M.; the more I showed zeal and willigness, the greater were the obstacles and opposition prepared for me, and it was very clear that they sought nothing less than to disgust me and make me abandon the task I had undertaken to organize a Museum in Tuscany, that they were moved by envy and malice against a foreigner, vices so natural to this nation. Among them were chiefly two; one was the late Piombanti, secretary of the Royal Industries, the other the surviving Paoletti.[e] My Sovereign decided to have an observatory; charged with it were Paoletti, Piombanti, and I as the only one for the astronomical part. Struggle as I might with Paoletti, even enlisting Piombanti, I never could induce him to make either a model or a plan of the two rooms destined for celestial observations. I wrote to him on the use of them and the instruments but in vain; obstinately he built the observatory without plans, but he made a thousand shameful errors, ending by saying that I had been the cause of all his mistakes, even in memoranda to my Sovereign, after I had protested against what he was doing because he could do nothing well in complete ignorance of the first elements of the science, which he boasted about knowing along with Piombanti, who knew no more than he. Both agreed to throw the blame for their mistakes on me, doing all they could to oppose the progress of the R.M., even hindering me from illuminating with English-style lanterns the five rooms where it could not be done with windows, doing this with such reasoning in physics as to make the most ignorant of men laugh, even unblushingly maintaining it in front of my Sovereign, who had the clemency and sufferance to listen in the presence of Giovanni Fabbroni.

From them I learned for the first time the fine art of deceit with the greatest safety and success, or how to deceive according to the circumstances. Paoletti, when he was alone with me, complained about

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[e] Gaspare Paoletti architect and engineer. He was responsible for the redecoration of the favorite villa of Peter Leopold, at Poggio Imperiale; for the construction of a new ballroom, the *Sala degli Stucchi* in the Palazzo Pitti; for the *Sala di Niobe* in the Uffizi. Peter Leopold wrote of him: «very skilful, honest, and capable, but slow, not easily satisfied, and extravagant».

Piombanti being domineering, ignorant, wanting to do everything but knowing how to do nothing. Piombanti complained of the negligence and arrogance of Paoletti, who wanted to do nothing but boasted of doing everything; certainly they knew each other well. Each one proposed separately that I ally myself with him but to pretend outwardly to be enemies, and opposed; otherwise, each one said, nothing could be approved in Tuscany. In this way alone they supported each other and their schemes. I saw then with what kind of person I had to deal; I refused to join with either one of them but the usual consequences were a thousand difficulties they made me, even to the stolid insolence of Piombanti forbidding me to clean the spots off the brick pavement of the R.M., if by chance there were any from water or spit; it came even to my being mute and obedient, seeing myself condemned at the same time by my Sovereign for negligence and uncleanliness, for not keeping clean those rooms which I was prevented from keeping clean. In short, all the harm that could be done the Museum by one or the other was done, only to contest my proposals; it is enough to point out that the plans for the rooms for mineralogy having had to be done over, they advised me not to do them at all although they would not be a major expense, or to commit the incongruity of putting in stairs from the cactus rooms to the rooms of minerals. The monstrosity of two different levels in the same building, which all the foreigners noticed, is only partly corrected in those places by new and elegant means arranged by me.

Confronted by so many obstacles, my ardor for the R.M. never faltered a moment. Of the anatomy in wax, it being the most troublesome work of my life as the repeated deadly illnesses that I have suffered working on cadavers, surely prove, I dare to say that it is truly an endeavor of the human spirit, and when I consider how in a little time I have made thirty-six statues, and more than one thousand cabinets of anatomical preparations in detail, more than three thousand colored drawings, and about eighty volumes *in quarto* of explanations of them, it still seems now to be a dream, almost miraculous. So it is considered, the anatomical collection of the R.M., by all foreigners of understanding, *if not by the malignant ignorant Tuscans.* I am sure that what is found in the R.M. will be imitated by no one, in no time, in no place, at least with the same extent, detail, exactness, and perfection, for the reason that it is nearly impossible to find another man so daring and fervent as to be willing to expose his health to a thousand ills and risks, who would be willing to spend his life among cadavers and putrefaction, who would expose himself as a target of persecution, cabals of envy, as happened to

me, who would serve as an example to others, now that much is known outside of Tuscany of the persecution and wrongs done to me in this country.

It will be said by my enemies that I owe much to my present dissector, because he cuts up the cadavers following my orders. But whatever professor of anatomy has not used a dissector at least for preparing the cadavers? Will it be said from this that the work and discoveries published by the professors come from the dissectors? Then there will be no more like Mascagni and the lymphatics, Morgagni, Albini,[f] Scarpa; there will be only copyists, plagiarizing their dissectors. That is what they said in the past from ignorant envy. There is no one, not even in Tuscany, who does not know that in the early days of the R.M. the anatomical waxes were made for two years running without dissectors and when, to lighten my work at least in part, I took on a few dissectors.

I had to train them from nothing, as they were entirely ignorant of the fine and delicate anatomy of the human body; the same dissector that I have now certainly knew then not the thousandth part of what he knows now, nor can I call it entirely his fault since in the public schools of anatomy in Tuscany and Florence little is taken up, in the University and hospital courses is shown only about one-thousandth part of human anatomy; I must not deny that the current dissector has learned to use hand and eye in getting something out of a dissection, but I do not know that I have ever seen a man who detests his profession more; he is so lazy that he even keeps in the R.M. for his comfort a bed, on which he throws himself the moment I leave the dissecting room, and doesn't leave it except for shame when I return; he never considers my wishes. As for dissection while I'm there and push him he works, but just as I look away he either does nothing or does it badly, and even distracts the other workers with chatter and joking. With such persons I must form the R.M., and I leave it for consideration whether I owe anything to such a man. If he remains at the R.M., it is only from necessity, and because otherwise he would make all the cadavers from the hospital disappear, as he always did before he worked here; and it is also for this that I am forced to use him. If there is anyone else at the R.M. with ability, I believe that it is all owing to me, to my labors, my direction, to the

[f] Bernhard Siegfried Albinus (1697-1779), professor of anatomy, medicine, and surgery at the University of Leiden. His anatomical atlases are renowned.

presence of my endeavors in every kind of study and science; to be convinced it is enough to consider such a person in his origins, who he was, what he knew, to see that to train such a man was a labor extraneous to my greater occupations; I well recall the bloodiest criticism, satire, and sarcasm launched against me at the outset, in my undertaking to form from nothing one for whom others had no hope, and they blaming me openly for having chosen someone without education or learning, for which I cared not at all, because I have never asked from men other than obedience, for success in my most difficult enterprises. If gratitude and acknowledgement were regarded as virtues in Florence I would not now need to speak of this. I do not deny that I have drawn some help from another of this place, but that person was formed in exactly the same spot; he was nothing when I took him in and trained him, he seeing my work, but he always had and still has need of my continued assistance and direction; then his aptitude as concerns the Museum does not go beyond the nomenclature of Natural History which is only the thousandth part of the works of the R.M. created by me. After my brief absences for illnesses, I have found either nothing done or everything badly done or in need of correction. Then it takes more to build up the Museum than to put in only two hours a morning, less all holidays, and to leave the place on every call from possible or regular patients. Such assistance barely amounts to six hours a week, twenty-four a month, or ten days a year as he was absent two months a year as a rule. With such persons ten centuries would scarcely suffice to bring the R.M. to its present state. The time that I put in was very different from that of those who work under my direction. I have had no vacation in the twentyfive years I have served in Tuscany, nor am I aware of having been away from the Museum one whole day, while the daily absences of other employees have been continuous nor have they omitted in any year to take the usual vacations, as allowed, in truth, in all the departments of Tuscany, and by me alone not taken in order not to prejudice the progress of the Museum.

Meanwhile it is certain that everything – even in the least distressing which in the Museum required great attention and study, all that which might have the slightest shade of the disagreeable, or be likely to make enemies – all that was left to me and was not dealt with by others, not by Zuccagni,[8] not by Fabbroni. It is true that one time only

[8] Attilio Zuccagni (1754-1807). Physician, prefect of the botanical garden in Boboli and assistant to Fontana, 1782-1805, he accompanied Lodovico of Parma, King of

Fabbroni, moved by pity I believe, for the troubled and sad life that I led in the service of the Museum, offered on his own to relieve me in view of my vanished health, to take upon himself, if I were willing, some of the many charges of the place, to relieve me of my labors, at least in part. I thanked him and accepted the offer; since he knew drawing much better than I, I asked him to attend to the drawings of the anatomical waxes, certainly the smallest and least interesting part of the work in the Museum, which would cost him little effort. But I required him to be responsible to me as I must be to my Sovereign, not only regarding the accuracy of the drawings but also in the quantity of the work, as the painters were paid by the month.

A week of his incumbency had not passed which was the last as well as the first, before he came to me all dejected; he told me openly that he renounced it as he saw he could not do it without estranging the people of the place, without finding himself in danger of a grave accident, adding as proof that Varroni, one of the artists, had gone so far in showing him his error as to call him *breed of scoundrel, disgraceful theatre dancer,* and even to threaten to kick him. It was no news to me, because I knew those who incited them to rebel, to oppose me: with such persons however I had to spend twenty and more years of my life in Florence, and with such persons I was able to bring near to completion an undertaking that perhaps no one else could have completed in a century with optimal and zealous employees and able artists, but I leave for consideration my labors and troubles, and what I had to bear in all that time. God only knows, who sees into the heart of man, and from him and in him I hope to find some day that rest and justice which men have denied me.

A great part of the calumny and persecution that pursued me through the long course of twenty years derived from my zeal for the good service of my Sovereign, and from the spirit of economy I vigorously observed in the R.M. I sought perfection in all and every work I underook, and at the same time I wanted it done with maximal saving. I don't deny that this was my chief vanity; I wished it to be seen that much was done at little expense, all done well, perfectly, while I knew that elsewhere with great expense very little was done and that badly. For my vanity I have been punished, drawing to myself hate and

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Etruria, on his trip to Spain from 1801 to 1803. During the regency of Maria Luisa of Parma he became professor of mineralogy and zoology in the museum, trasformed into the *Liceo.*

calumny from all the artists and all employees of the place. I could not tolerate slow and negligent work, thus I was obliged at every moment to start scolding again. I know I have done much damage to my health and my maligned honor, but on the other hand I have done my duty, I have nothing to reproach myself with, and to this conduct of mine alone is due the greatest, perhaps the most useful establishment for the sciences of this century. All that I have spent to make it I can justify, and I can show the greatest economy and saving in my purchases. In order to spend little for the R.M. I risked everything of my own; health, inconvenience, unhappiness. The physical, astronomical, and chemical instruments functioned in rooms so poorly adapted, so small, so ill-kept, although they are improved since then, that foreigners were surprised to see them, and couldn't see how it was done; it is enough to say that I cast in the courtyard more than forty wax statues and more than seven hundred major pieces of anatomy in wax, all of which had to be done by me in the greatest winter cold as well as in the greatest summer heat, under heavy rain as well as in the strongest winds. The most exposed was I, and I exposed myself to everything, both out of zeal and duty.

But it is true that whatever I asked for was denied me by the Secretariat of Works, where Paoletti, my enemy because I saw through him and did not spare him when my duty and the advantage of the R.M. were concerned, had indisposed the secretaries towards me with calumny and cabal, according to his natural way of behaving.

The waxes for Vienna being finished, I was ordered by rescript from the Royal Finances not to exceed thirty-two thousand *lire* in annual expenses for the R.M.; if any extraordinary work or acquisition were needed, I was to inform the Sovereign and obtain permission. I immediately complied with the orders received, which I am certain never to have violated in any way, although the waxes for the R.M. were not finished, only those for Vienna; but since it was found that expenses had exceeded the fixed amount in the new fittings for the R.M., this excess of expenditure was held, as usual without giving me a hearing, to be a contravention on my part of the orders of my Sovereign; resolutions were taken mortifying to me, which I had never deserved. If before condemning me without a hearing, my supposed disobedience had been communicated to me, my innocence and compliance with the orders would readily have been seen. When I was commanded to restrict myself to the budget of thirty-two thousand *lire,* a great many acquisitions and commitments had been ordered by sovereign command for a long time before, particularly in London, in Sweden, in England... among these

were all the metals and ebony for the *Gran Cerchio Astronomico,* twelve feet in diameter, for the Royal Observatory, and the acquisition of the collection of shells of the Swedish professor Modeer.[h] All these acquisitions and many others of great value unfortunately fell behind, and the payment for them accumulated in the time of the restricted budget, as they were not received on account of war or other accidental delays, but it was believed that I had disobeyed the always just orders of my Sovereign.

I had always been so far from making superfluous expenditures or spending against orders and intentions of my Sovereign that I have even avoided on purpose examining the offers of natural products made to the R.M. on many occasions, and sufficient proof is the last acquisition of the insects of the German Vidman, which I did not want or even wish to see before it was expressly ordered by my Sovereign. I could have reduced the price by about one-half, while the generosity of the Sovereign paid the double; this too was acquired not at my proposal but notwithstanding was presented as usual as my exceeding the fixed budget, and I was mortified on the other hand for my discretion in not having wanted or even wished to see that collection without a sovereign order; acting or not acting, I always was guilty. The same may be said of the collection of insects of the Abate Ranieri of Turin, which I saw only on express orders and for which also was paid the double of the price set by me. It is true, I don't deny, that I proposed the acquisition of the Swedish conchology, but I did so because I saw that it was being offered, in consideration of the scarcity of money in that country then at war, at the third of its real value, and because I saw from the catalog sent me that with it the collection of the R.M. would be complete; it lacked many hundreds of uncommon pieces. In fact, after the collection was purchased, more than two thousand shells were selected from it; the remainder brought at least twice the price of the collection; all the more so as Modeer my friend and correspondent added as a gift about a thousand shells only in friendship for me. If I had acquired for myself that collection of shells, as I even told my Sovereign I would certainly have done in case the R.M. had not wished to have it, I should have undoubtedly profited by more than a thousand *zecchini* by selling it elsewhere. Meanwhile I assure you that I have the pleasure of being able to say to my Sovereign that the

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[h] Adolf Modeer (1738-1799). Engineer, member of the Academy of Science of Stockholm, he wrote on mineralogy.

conchology room of the R.M.; is the most complete in Europe, that there are more than three hundred new species in that room not described in print.

My economy for the R.M. has often ruined my own, and I have sacrificed a good part of my salary in its favor. All the letters and parcels, of which there were many, which arrived daily sent by post and by Procacci and Vetturali from my friends and correspondents in different countries have been paid for over many years with my own money, although they contained things for the Museum, and only for a few years back have I benefited by postal franking. The R.M. has always lacked rooms suitable for testing physical instruments, filling barometers, thermometers, and such things. It is closed on holidays and at night, thus I have always tried to have a house much larger than would have sufficed for me alone; I have worked at home for the R.M.; day and night, using my wood, coal, oil, charcoal, cloths, vessels... and it was precisely in my home and for the service of the R.M. that I suffered four mercurial salivations, which I barely survived after suffering the most atrocious pains.

I know that I am discredited by my enemies for having proposed a new building in place of the present one in Casa Torrigiani. My zeal for the glory of my Sovereign and for the public benefit may have deceived me, I don't deny, but certainly I have acted in good faith. In my travels in France and England I found no convenient building such as is needed for a collection of natural history. The British Museum itself, which was built by plan to house natural products, is not worth much; it is such that it obliges scholars and foreigners to pass rapidly from room to room, and successively be scrutinized by those persons concerned with the security of the items. This gives great inconvenience to those who preside and is of no use to those who wish to take advantage of it. Impassioned as I am about my studies and about the superb collection of the R.M. which I believe unique in the world, I still believe the glory of my Sovereign and the public benefit to rest in a new building that would mark a new epoch in the century and render immortal to the most remote posterity the name of the Great Maecenas who had made it. It seemed to me that any addition to the building where it was must be costly and would make the R.M. less useful for the public, that it would be entirely impossible, by reason of the shelves already made, to complete the various missing branches of natural history by way of news shelves proportionate to the rooms and the material they contain. I did not see how it was possible, nor do I see even now, how two new reptile and serpent rooms, for

example, could be put between the present serpent room and the insect room, and how three other insect rooms could be put between the present insect room and the next containing the zoöphytes, and two others of zoöphytes between that of the zoöphytes and the shell and seed collections, many of which are lacking to complete those branches of natural history. The same might be said of minerals and fossils; it was for this which I begged my Sovereign from the beginning, that I made my design of a new building, so that he might have it examined by expert and honest persons, like Fantoni, Salvetti,[i] Fossombroni,[j] Ferroni, and the Count of Oghenwar. In good faith I believed that such a building might not only be useful but even necessary. So I sought to be judged by people capable of judgement but not of being deceived. In fact my plan assures the R.M. of a roster which does not necessitate the service of a great number of useless, bothersome, dangerous, irresponsible guards; it can accept any new acquisition in natural history without at all disturbing the order that nature demands of her products, it can do so by reason of its great simplicity and with less expense that the additions, incongruous and never enough, that will need to be made to the old building, if the natural history of the R.M. is to be completed.

They wished to accuse me of throwing away the money of the R.M. on obscure and useless things, aiming principally at the anatomy in wood, as superfluous after the waxes I had made. This accusation comes not only from ignorance, but even more from malice and envy, as have other accusations against me. However, I believed I knew anatomy, even before I knew Tuscany, and it is still believed that I know it by other professors at least those who are not Tuscans. I believed furthermore that I knew natural history, physics, and chemistry since that time, as I believed anyone who had made since that time discoveries in all those sciences knew at least as much about them as a professor, inasmuch as not all professors are capable of making discoveries; in fact, few do, as proof might be found in the University of Pisa and the Studios of Florence, where not one professor for more than a century has been able

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i Salvetti, first engineer in the Office of Tithes, was «a man of sufficient ability, but slow and easy-going, wishing to complete his jobs with too much luxury and magnificence», according to Peter Leopold.

j Vittorio Fossombroni (1754-1844). Minister of War and foreign Minister in the Grand Ducal Court, he was a hydraulic engineer and author of works on the Val di Chiana (1789) and the Maremma.

to boast of the smallest discovery of any kind. How then can I have proposed anything useless or superfluous?

Perhaps the work is not for me so difficult and laborious that it might be believed that I wished to undertake it for mere diversion? I still know of nothing in the R.M. more difficult, and at the same time more useful. Public utility, always unfortunately one of my passions, has made me overcome the greatest difficulties, and when the anatomy in wood, not far from complete, is finished, Europe will judge its usefulness; ignorance, envy, and calumny will then be stilled. I do not deny that the waxes I made are highly important for the science of anatomy, but they are so many disconnected parts or members of the human body that do not and cannot form a whole, which do not give an idea of the whole, which do not show the relations, the sites, the positions, the connections, either among themselves or with the entire body; the waxes, in short, are the wheels and springs of a dismantled watch, separate, divided, not in place, which will never give a perfect idea of the entire watch, its uses, the interplay of one piece with another, the mechanisms, finally, which bring about their movements, until each part is united with the others so that it can be seen how, from them, the watch results. My opinion is that one could more readily do without the waxes than the demountable anatomy in wood, that a student who could make more progress in six months with the waxes than in six years with cadavers, would learn more in six weeks with the anatomy in wood. Taking apart an entire man piece by piece and then putting him back together again as he was is the easiest and surely the most useful exercise for understanding the highly organized mechanism of the human body; all this which can be done in hours with the anatomy in wood will never be done with that in wax, much less with the cadaver, where if we wish to examine any part or organ we are forced to separate and remove all that covers it, or is near it, and thus all the relations are lost. This is not only my feeling, but that of all those who came to Florence this year and last year, who are in a position to judge because they understand anatomy, but have no interest in calumny. It is a real sorrow for me to be obliged to speak of myself and my affairs, but when modesty and the facts themselves, which are evident to all, are not enough, if not to acquire merit in Tuscany, at least for defence against envy and calumny, one must speak, speak of one's self after twenty-four years of silent patience; one must defend one's honor and the truth in spite of appearing vain and selfpraising, because it is just and necessary in such a case. I know then no greater enterprise of this century or one more useful for the advance of science than the R.M. in

Florence of Peter Leopold; I know no other undertaking as great and at the same time as difficult to have been finished in a shorter time. I know no other undertaking so vast and perfect accomplished with so little expense and so few means as the Museum has. It includes all the sciences, and all of nature is represented there in great detail and clarity. Physics, chemistry, astronomy, natural history, are seen there in their greatest splendor and domain. The greater part of the physical and chemical instruments are newly invented, the others all more or less perfected; the meteorological room is all new, unique in the world; the eight chief instruments which record all the atmospheric changes for twenty-four hours of the day are as new as they are astounding, and useful. It was not possible, nor would they have believed it possible, for the physicists to arrive, with instruments, at establishing the laws of atmospheric irregularities; with new mechanisms invented and executed by me, in ten years' observations there will easily be discovered as yet unforeseen phenomena and cycles.

The three machines to divide astronomical circles, strips of metal, plates of crystal, are new, perfect, most useful, and the first invented in Europe. Four perfect barometers, entirely new for measuring the heights of mountains and subterranean depths are of my invention; before my four only that of M. de Luc[k] was known, less certain and perfect than mine. The engine for compressing all elastic, non-elastic, and aeriform fluids and liquids is all mine; with its use alone can be discovered the most exact laws of compression of all those substances, laws not yet found but so useful and desired by the physicists.

The two apparatuses for igniting inflammable and dephlogisticated airs, for the crystallization of red-hot iron, for the decomposition of water, are new, exact, and decisive. The series of balances is the most complete part of the R.M., and the most useful for physics and chemistry. Here all is new and the precision is such as foreign physicists think quite impossible; experience was necessary to convince those who came through Florence. With them it was possible for me alone to establish new points and principles in physics such as the weight of light, fire, and latent heat, and to discover an infinity of errors in the works of the most illustrious experimenters of this century.

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[k] Jean André Deluc (1727-1817). Although professor in Göttingen, he spent most of his life in England; he wrote many papers on cosmology, geology, and physics.

Within a few weeks astronomy will be enriched by my newly invented parallax-instrument which permits the most delicate and important observations; it will serve very well for measuring terrestrial gradients in Tuscany, most useful for the geography of a country, and the physics of the earth, which is anxiously awaited in the observatory. As soon as I have finished the Astronomical Great Circle, twelve feet in diameter, the Florentine observatory will be the first in Europe for the superiority of its instruments. Rejected in England by the most excellent artists even at a price of two thousand Sterling, on account of its great size and extreme difficulty of execution, this immense circle will yet be made by me in Florence if God grants me health, with ignorant artists, at the moderate expense of five hundred Sterling at most, and I flatter myself that it will be perfect.

I omit mentioning a great number of other instruments, either entirely new or perfected by me, that I shall describe to the public advantage at a better time.

The anatomy in the R.M. is already near ultimation, is such as to seem to all truly expert anatomists to exceed human powers. As has been said it comprises more than sixteen complete life-sized statues, and more than five hundred cases of detailed pieces, numbering more than one thousand, of which more than two hundred are whole busts or half-statues. There is nothing known of the human body that cannot be found displayed here to be seen and considered. The elegance, the exactness, the refinement, and precision of the work make it unique, probably forever. Everything to be seen of anatomy in the R.M. is entirely new; new and very useful is the system of colored drawings, now exceeding one thousand five hundred; the explanations of these already fill more than twenty volumes *in quarto*. The cabinets are also of new invention; nothing better can be imagined for elegance, convenience, or utility. By means of a simple mechanism the statues revolve, and are seen through the glass on all four sides, so that each statue takes the place of four, a great saving in work and expense. I have had to overcome hundreds and hundreds of difficulties to find the arrangement of the various parts in colored waxes, with the nodose structure of the lymphatic vessels, with the iron armatures of the statues, and most of all with anatomical precision and exactness and with the devising of the most instructive yet natural positions of the human body. Difficulties have been infinite, and even now it seems to me a dream that I was able to overcome all of them to the point that it seems nothing is left to be desired. When one considers that I have had to use ignorant, idle,

malicious artists, obstinate and lazy dissectors barely aware of even the gross features of anatomy, that I have had to use putrid cadavers in miserable places in no way suitable for such a great undertaking, it will be agreed that to have accomplished so much in so little time smacks of the miraculous.

Not only was I occupied with the two great anatomical collections for Florence and for Vienna, but at the same time also with all the other branches of natural history, each of which I sought not only to enlarge by procuring materials from all countries, from friends and correspondents, but also to make everything elegant and new, in short, such as is found in no European cabinet. Entirely new is the arrangement of birds in natural trees with leaves, labeled by name, preserved by an equally new means, as are the fishes, with burning sulfur in warm weather. The very arrangement of the shelves of fishes inclined in a way that places everything under the observer's eye is something to be found in no other place. The jars of reptiles, snakes, zoöphytes, and an infinity of other animals, are arranged by me with a circular duct full of amalgam over each jar so that any evaporation of alcohol is impeded, resulting in a great saving of expense and the surest preservation of the animals. Entirely new is the room of seeds, woods, dried fruits, and all those products derived from vegetables by chemistry, as entirely new are the shelves I devised on which they are placed. The two rooms of cactuses, fruits, and funguses cast in wax are unique in Europe. Elegance and lifelikeness clearly shine forth in these three great collections of vegetable substances. This work is most difficult and long drawn-out, requiring the closest attention, in order to avoid mistakes, from the one who directs it. All the plants are shown in states of fruit and of flower, the latter also broken down for the use of botanists; they are largely most lifelike, something entirely lacking in all plant collections and herbariums as it cannot be preserved in drying them. The very vases in which they are placed could not be more elegant and simple, but not expensive, and are sought after and bought for their own country from the Ginori factory by the most illustrious Personages passing through Florence. The room of insects is equally new and elegant; the system I contrived for preserving them and changing their locations as needed could not be more infallible and more convenient. Never does any insect perish in the R.M., while in other collections a great many are lost yearly, and when new species of insects are found, as happens every year, they can be put in place instantly, when with the methods of classification used by the others it is done with difficulty, and badly. But what is more important than

anything else in the R.M., and what distinguishes it from all others so far known, is not only that every branch of natural history is classified there according to the methods of the best naturalists of the century – so far not done in any other museum in Europe – but even more useful, and entirely new, is that in addition to the names indicating the species and the more notable varieties, in each room is found, in fixed frames, and easily legible, the nomenclature necessary for understanding everything contained in that room, and in suitable cases, the general divisions of the branch of natural history contained in that room, with the genera in prominent characters and with the corresponding examples alongside, so that the student, without need of professor or book, can by himself become a great naturalist, a great anatomist, with the greatest facility and in very little time. Such advantages are found only in the R.M. of Florence, and it is for this that it is the only one useful to science and to the public.

However, during the time when I was completely occupied in creating the R.M., full of thoughts of complying with my Sovereign's wishes, and of making myself useful to the public, I neglected my own interests and my private economy, sacrificing everything to my ruling passion. My indifference, I would say almost my contempt, for money and for getting rich, which is the spirit and mainspring in the actions of the common herd, which forms the major part of humanity, has been so great in me that in the course of twenty-five consecutive years, in which I have published new works in print, I never have written a memorandum in order to have the usual raise of twenty *scudi* per year so far denied not one of the professors of Pisa, even living in Florence as in the case of Dr. Ferroni. As my works printed at various times are more than twenty, the proceeds from these alone would bring me annually four hundred *scudi;* more than the current provision which I draw from that Studio; this loss only at a mean value of two hundred *scudi* per year, for twentyfive years amounts to five thousand *scudi.* It is then true that I have renounced this five thousand *scudi* voluntarily and freely and that currently I renounce annually four hundred *scudi* that I could rightly ask and could hope to obtain by the clemency of my Sovereign. If now one were to calculate what of my own I have given to the R.M. and consider what I have voluntarily renounced and could justly demand, enough would be found to constitute an estate more than mediocre for a private person as I am, certainly such as to have today need of no one.

Meanwhile I can take pride in having been the first and only one to dare to beg my Sovereign not to permit tips in the R.M., as it was

customary to collect in all the Royal houses in Tuscany from time immemorial. As everyone knows, the Gallery [the Uffizi] brought in up to eight hundred *scudi* a year in tips to the late canon Querci.[l] The R.M. was frequented much more than the Gallery, and such money according to custom could justifiably belong to me. I renounce it spontaneously, and thus saved the decorum of the place and the greatness of the Sovereign who does not allow profit to be reaped from curiosity, or that instruction should be at the expense of those who love it and seek it. This was found to be so proper and decorous, that a little later all tipping was stopped by law all over Tuscany; there is still hatred on the part of the departments for me, recognized as the source of this reform. H.I.M. Joseph II came to Italy a little later and honored the R.M. with his presence. The next day he sent his secretary to my home with sixty ungheri, which he declared to be *for me only and not for the R.M.* I had the delicacy to take them immediately to my Sovereign, in order to dispose of them according to his wishes, I being excluded, in order that my enemies at the R.M. could not say that the law was not observed when my interest was concerned; my petition was granted so that everyone in the place had something excepting only myself.

The Elector Palatine came to the museum with the late Governor of Leghorn, who offered me in the name of the Elector two hundred *zecchini* which I instantly refused, as I refused a valuable golden box of greater value which he wished to substitute for the money to make the gift more honorable. The King of Sweden also offered me a golden box, which I also refused and which the Gallery accepted without any scruple.

I pass over a large number of gifts offered me by Great Personages on the occasion of their coming and which I refused. There were reigning Princes, for whom it seemed that an exception might be made, as in fact the Gallery did in many such cases and as did Dr. Pigri,[m] interim director of the R.M. in my absence, when H.R.H. the Archduchess Christine came to see this place. When the R.M. acquired through me the Viennese collection of minerals, their proprietor sent me as a gift, through Banker Orsi, a gold repeater watch which I refused, but a similar one was not

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[l] Querci was brought from Rome in 1769 to be Director of the Uffizi. He died in 1773.

[m] Giuseppe Pigri. In charge of mechanics at La Specola, in 1784 he became professor of that subject at the *Accademia delle Belle Arti.*

refused by another person, which drew admiration and laughter from Orsi.

On my return from London and Paris, my Sovereign, always merciful and good to me, ordered Banker Bussoni through the late Councilor Tavanti[n] to reimburse me with two hundred *zecchini* for my trip to Florence. Instead of using them I brought the check back to Florence and put it in the hands of Tavanti himself; this money was money of my Sovereign and was given to me; I could dispose of it and no one could accuse me if I had collected it, still I renounced it and I renounced it freely.

If ambition, if greed for gold had been my passion I should not have returned to Tuscany, I would no longer be in Florence. Turgot and Tronchet, Ministers of the King of France, offered me various rich and honorific literary post which I steadily refused, and I forced the late Abate Niccoli to confer with the Ministers mentioned because he was threatening to write against me to Florence, saying that I sought employment in Paris, when on the contrary I had refused all the posts offered me. Niccoli had become my enemy a little before, because I had not been willing to join with him in *sub rosa* trickery against my Sovereign, when we made a trip together to Rouen.

In Florence again I refused offers to go to Russia not much different from those which would have taken me to Vienna, in the service of H.I.M. Joseph II. Everything was subordinated to the wish to serve my Sovereign, and the zeal I have always had for the R.M. which I considered my own child, the fruit of so much toil and sweat. He who could sacrifice his own peace and tranquillity, he who had sacrificed even his own health, exposing himself to a thousand deaths for so many years, he who had placed before his private glory the honor of serving his Sovereign and the R.M., when if he had only worked for himself in the course of so many years would certainly have made discoveries in physics, as he had made before being employed in Tuscany, he, it is quite natural to believe, would sacrifice for the advantage of the R.M. all the rest, so much less important, like money and his own interest. In fact,

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[n] Angelo Tavanti (1714-1782). In 1746 he became secretary of the Finance Council, in 1770 its director, presiding over its meetings in the absence of the Grand Duke, as he also did over the weekly meetings of General Administrators, reporting twice weekly to the Grand Duke. A member of the Council of State, he was active in abolishing the Tax Farm. His work as liberal and physiocrat was crowned by th adoption of the new liberalizing Customs Laws and Tariff of 1781.

during the early times of the R.M. I presented it with a black flint stone with natural imprint of a superb white head so beautiful and unusual that I refused for it a diamond ring worth fifty *louis* offered me by a Russian general. It is true that it cost me only half a *paolo* but that does not alter the fact that it was a unique and remarkable stone worth fifty *louis* which I presented to the R.M. About six years ago, a religious offered me privately, for twenty *zecchini,* one hundred and fifty pieces of mineral from Elba, of such beauty that I was offered, before I bought it, one thousand two hundred *lire* by a Frenchman for only two pieces. I refused that offer and instead gave the religious six *zecchini* for the entire number of one hundred and fifty pieces on behalf of the R.M.

The Elector of Bavaria gave me three years ago a superb magnetic compass worth at least twenty and more *zecchini;* I put it immediately among the instruments of the Cabinet, where it is to be found at present.

Four superb pieces of the regulus of cobalt were a gift of a Polish Cavalier, and I put them in the R.M. among the minerals; various *occhi di mondo* of considerable value I gave to the R.M., some bought by me, some presents from friends. I presented the R.M. with a superb green tourmaline of Brazil, given to me in London. Above all the other things I gave to the R.M., one deserves recall here, a tourmaline of Ceylon, the most beautiful known, priceless because of its enormous size which makes it unique in the world, for which one could ask some thousands of *zecchini.* Both were given to me in London by by friend the excellent physicist Mr. Wilson.

I omit mention of a portable barometer of Ramsden, valued at twenty-two *zecchini,* bought by me in London; a complete assortment for making experiments with factitious airs with three eudiometers of my invention, value sixty *zecchini;* more than seven hundred items of natural history bought by me in my travels to Paris and London; a collection of chemical products bought by me in Paris; an electrical machine with appliances; and many copies of my works presented by me to numerous European naturalists in exchange for items of natural history given by me to the R.M.

All these things were mine, either bought with my money or given to me, and now they have long been in the R.M. There are to be found a great many valuable *scientific books,* presented to me over many years by my friends and men of letters, many anatomical works, of great value because full of engravings; these I have verified for the waxwork of the R.M., and at present there are still in the hands of the artists the plates of

the anatomical works of Senac, Santorini,[o] Scarpa, and Haller... torn and dirty from careless use by waxworkers.

Similarly I presented the R.M. with an instrument of mine made in London for measuring the velocity and force of the wind, valued at fifteen *zecchini,* and a silver compass worth six zecchini bought in London. Also to be found there are two hygrometers of mine and an electrometer I invented and had built in London, with two achromatic telescopes, worth 25 *zecchini.* It is not long since I had Dr. Zuccagni send three large medals worth ten *zecchini* to M. Modeer in Stockholm to acquire the Trapp collection from Sweden and the zeolites from Iceland, thus to fill some wants still in the R.M., with no expense to itself.

A very great number of choice and varied items of natural history presented to me by various friends and foreign naturalists are also to be found in the R.M.; among these are two very fine pieces of *spato ponderosa aerato* of England, *lapis ponderosus* of Bohemia, *molibdena* of Germany, and the *green sand* of the desert of Atacama, given me a few months ago by the naturalist-traveler M. Dombey of Lyons. This last product is very rare and so valuable that the famous Bergman never was able to have more than two grains in weight to analyze, and now the R.M. has pounds of it given by me out of pure zeal. The things given freely by me to the R.M. amount to several thousands of *scudi*... And yet it will be believed that he who gives thousands to the R.M. would rob it of trifles, since the entire value of the pearls and pottery hardly reaches twenty *zecchini.* No, that would not be believed even by the fiercest of my enemies, my most able and most corrupt calumniators; even if I were such a one I would no longer be a thief, a scoundrel, but really a fool, a madman to be shut up forever, to whom even in such a state should rightly be given the more than twenty *zecchini* for what he gave to the R.M. I challenge now my enemies most determined on calumniating me to prove if there is one single fact told falsely or altered in all my pages here. I wish for nothing more ardently than that they should be examined with all rigor and impartiality, and nothing will give me greater pleasure and justify better my works, my conduct, than to order informed and honest persons to examine the things I have done, that is, the anatomy, physics, astronomy, chemistry, natural history, that are found in the R.M.

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[o] Giandomenico Santorini, 1681-1737. After study in Bologna, Padua, and Pisa, he became professor of anatomy at Padua in 1703. His textbook for naval surgeons appeared in a Greek translation in 1745. His anatomical tables were published after his death by his pupil Michele Girardi.

I am sure that they will say much more than what I have had to say in a few lines, forced by calumny to justify myself. I hope that as a matter of justice orders will be given to make a catalog of things donated by me to the R.M.,[p] and as much as the personnel of the place are against me as enemies, and so charged will remain such for me, even in shameful impudence they will not dare deny as being mine a great number of things of great value and extraordinary rarity.

This can suffice to justify my honor regarding my Sovereign, but not before the public which demands proof and documents, and indeed proofs and documents are already prepared to be brought forth, and my honor will be saved. The fault will not be mine, if the true character of some people is made known, of some who have, either secretly or openly, calumniated me. This is a matter of the honor which no man can abandon, it involves a necessary defence which the public demands of me, which my friends not completely uninformed demand, it concerns the unmasking of the false, the revelation of the truth.

What I have noted in these pages is more than sufficient to give judgement of me and my conduct, and I am sure that my august Sovereign who made his Tuscan subjects happy, who is doing the same now over much of Europe, will not abandon and deny justice to one of his subjects who believes he has deserved it from him, and from his State.

Here end the pages sent by me to Vienna to H.I.M. [Leopold], who gave me hope by word given to my brother by way of Lagusius and Störck, that once H.M. reached Florence he would see that justice was done me, and I would be consoled. I had reason also to hope for it on his arrival here because he had the kindness to tell me that he would speak of my affairs to my new Sovereign [Grand Duke Ferdinand]. The multiplicity of affairs all more important than mine will surely have made him forget me and my matters, but there always remains to me the consolation of being able to lay my arguments before my new just and enlightened Sovereign.

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p As noted in Section 90, *R. Museo Filza di Negozi dell'Anno 1792* (in the Museum of the History of Science, Florence), such a catalog was compiled by Bonicoli, Fabbroni, and Zuccagni; it listed the books, instruments, and natural products of many sorts given to the R.M. by Fontana. Although Fontana wrote that he did not wish to be paid but wanted only to contribute to the R.M., he signed (20 November 1789) a final quittance of any claim, upon the receipt of 5,344 *lire*.

I still believe it indispensable to point out here that when H.M. the reigning Emperor [Leopold] appointed me to his Royal Service I had no obligation or responsibility other than that of having to carry out experiments in physics for him; as we would have received too late the instruments from England, the only country where they know well how to make them, I did not decline to have made in Florence under my direction a certain number of instruments only for the private use of H.M. Hence for me this was an additional temporary burden, willingly accepted although not as an obligation, simply a trial since I did not even know then if I would ever be able to make instruments in a country where others before me had not succeeded, although they had tried repeatedly.

All the rest of the instruments for astronomy, chemistry, physics, all of the natural history, all the anatomical waxes... were made by me out of pure zeal, and to earn credit for myself; it was for this reason that the late Emperor [Joseph II], being informed of my undertaking and obligations in Tuscany, asked me if I would take upon myself the extraordinary and special commission of the anatomical waxes for the hospital of Vienna; he promised me protection and advantages for my efforts when the work was finished. I must state again in truth alone, not in a spirit of self-interest of which, after all that has been mentioned above, I can not even be suspected, and because my new Sovereign is aware of it, that for eighteen years and more I have had no increase in honorarium in the face of so much work done by me for the R.M.; without obligation but purely for zeal, while all those in the R.M. have had repeated increases in wages, which has given occasion to my enemies to denigrate me at home and abroad.

Equally I never refused to earn the favor of my Sovereign on the many occasions when I thought to be useful to him, neglecting the least important, even that of the Mint, where I reestablished the art of setting the quality of fine metals, an art which had degenerated and had been lost in Tuscany, with the danger of the gold coined in Florence being discredited, as I have already said above. It will be enough for me to say what great good fortune I had to be able to demonstrate the natural sciences in the course of many years to my present Sovereign [Ferdinand], and this has been certainly the most happy time for me of my life in Tuscany.

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Pages Presented in Vienna to H.M. the Emperor Leopold

Since the moment when I first had the fortune and the honor to be able to consecrate myself entirely to the service of a Philosopher Sovereign destined by Heaven to make his subjects in Tuscany happy, I proposed to myself as a basis and guide for my actions in my future life the most constant zeal and firm fidelity – these alone have been the incentives of all my enterprises and labors in the service of my Sovereign, and I flatter myself never to have abandoned them a moment in facing obstacles encountered, and the persecutions suffered through the long course of twenty-four years. Only to the zeal which has always animated me is attributable after all an establishment in Tuscany which seems beyong human strength, if one regards the shortness of the time, the perfection of the work, the resistance and attacks encountered through envy and calumny, the enmity, the ignorance, the moderate outlay for the immense quantity of valuable materials gathered together in that place.

The very nature of my employment, and my status as a foreigner in Tuscany alone must have brought upon me envy and enemies; the firmness of my character must have embittered and irritated them, and my incorruptibility in the face of whatever trial, must have led them to contrive everything to ruin me. All this had to happen, in fact did happen, but all this has not so far been able to lessen my zeal and my faithfulness to my Sovereign or my work, which will last as long as my life lasts.

When my Sovereign charged me with making for him a collection of Physics and Natural History, I purposed to create something which would be a monument worthy of the Great Maecenas who ordered it, which would be at the same time a major benefit to the human sciences. I knew the defects, the imperfections, the uselessness, I will even go so far as to say absolute, of all the other collections in Europe which, not serving the public good must be considered as contributing little or no

glory to the Sovereigns who had them made, although many years and whole treasuries had gone into them.

It amounted then, following the views of my Sovereign, to doing something so great that the like had never been done before in Europe, to do it in a country which lacked even the first elements to embody at least the basic ideas, to do it where there was no artist capable of beginning the work, where there never had been correspondence and connections with foreign parts, so necessary to get the prime materials for this great work. Finally it was a matter of doing all this in a few years, of doing it in Tuscany, where it was barely possible to find in Pisa an animal skeleton, a collection of rough and imperfect fragments, much less any real physical instruments, or moreover any work by a professor for fifty years and more. It is true that I foresaw all these difficulties, but my zeal for my new job, my love of science, my desire to acquire merit with my Sovereign, made me surmount everything, and I set to the enterprise with all the courage of which I am capable.

Thus full of great zeal I set myself to train artists for making instruments, but immediately I was made aware that, lacking the most elementary notions of the art, of drawing, of arithmetic, or reading, they could come to nothing; nothing even mediocre would I ever be able to achieve with them. Then I resolved to use them as one would a hammer, a file, that is to consider them simply as tools, and to make them do everything without understanding anything; I leave to anyone to consider the immense and endless labor necessarily required to control and govern such persons, and what trouble and time even the simplest machine needed. My patience and labor have been incredibly great; to myself even today it seems impossible that I could have done so much. At the same time that I was occupied in making physical, chemical, and astronomical instruments, I set myself to work on natural history and finally anatomy, which alone would have occupied many of the most hard-working and capable persons known in Europe. Zeal, love of science, made me overcome all the most arduous difficulties, so that I could, in only a few years and with no assistance, lay the foundation of a museum which already showed that it would become the first in Europe.

All these beginnings took place before my journey to France and England, and not without obstacles and persecution through envy and calumny; in some ways I myself was the cause of them, on account of my too-great zeal in wishing to do too much, and to do it at the least possible expense. It was natural that in a country so indolent and slothful for so long that I should have as many enemies, as were those whom I obliged

to work more than was their custom, and at wages limited, though fair. I was, then, calumniated, persecuted continually by them, and my Sovereign had many times to hear complaints against me, which even came to accusations of disloyalty and false use of money. However, all these accusations were secret, concealed, nearly always without the accuser being named. Fearfully I saw the calumny, almost never the calumniator, nor was I ever permitted to defend myself, or to prove the other's calumny before the public, before the Tribunals, as I asked many times, and as it has never been denied even to the most guilty. Fortunately for me, and it happened almost as by a miracle, I was permitted, although only a single time, to justify myself before a Tribunal against the atrocious calumnies, in documents hidden from me but reaching the Throne, from a certain Chinter, cabinet-maker, who accused me of withholding wages, of giving him heavy and unfair work, of money owed and not paid. The affair ended quickly in the Tribunal with a public sentence declaring Chinter to be a calumniator and a thief at the same time; only the clemency and extraordinary goodness of my Sovereign freed him from the jail which he deserved, but which served for nothing but to encourage others to calumny.

If it had been permitted me every time to defend and justify myself before the Tribunal when accused, as it was permitted this time to my surprise, my Sovereign would easily have recognized my innocence, my enemies, and the spirit animating them against me. Chinter himself confessed on his death-bed that he had been incited to calumniate me by two persons in the service of the Sovereign, who had promised him money and protection. A terrible school even for the most enlightened and zealous Sovereigns, to find themselves exposed every moment of rule to deceit, and so often to confound the innocent with the guilty.

It is impossible to believe how far the bad faith and thieving spirit of Florentine artists will go. If the experience of twenty-four years had not proved it to me a thousand times in hundreds of them I could not believe it even possible; unfortunately that's the way they are. It is quite incredible, what they have done to deceive me, to lead me to betray my Sovereign in the price of their work, in things bought for the R.M. presents, entreaties, scheming, they tried everything, and it will tell the whole story if I put down in a few words what happened to me a few months after I began to have work done for the R.M.

The late secretary Piombanti recommended for the R.M., as a most honest man, a person who for some time had served in the secretariat of the Royal Workshops, where he was the head. He assured me that I

should be most content with him as he had been, for the long time that he had tried him. Not three months had passed when he brought me a bill for items supplied to the R.M. of about one hundred thirty *scudi,* which after many arguments I reduced to one hundred two. I paid him in coins of ten *paoli* and he left me the receipt for one hundred two *scudi.* On leaving the room he left on the table where he had been paid pieces of ten *paoli,* I judged about twenty of them at least. I called him back, thinking he had made an error, telling him that he had forgotten some money on the table; he answered me smilingly that *he knew it, that it was for me.* I said with surprise that he knew me not very well, that he should take his money at once, and never appear again in the R.M. He did so, taking his money, but saying in his own defence as he left *that he thought he was master of his money, could give it to whom he wished, and that the receipt guaranteed me* against any risk; he added moreover *that in other departments serving the same sovereign he had not found that delicacy, and that I didn't know how to turn things to my advantage.* I dismissed him angrily and immediately informed Piombanti of what had happened to me, complaining that the had recommended to me a scoundrel and thief. He tried every way to quiet me, and made me swear that *I would not speak of it to anyone, particularly the Sovereign, because, he said, for too much zeal I had made a thousand enemies, and would finally be abandoned even by the Sovereign himself because of accusations and calumnies, as had already happened to others.*

Then I saw for the first time the danger to which I was exposed in the commissioning of the artists, and with what infamous and skilful trickery one could cheat one's own Sovereign, without the swindle ever being found out, and even disclosed before the Tribunal by the swindler all accusations against him would fail, the corrupt would be declared innocent since he had the receipt for his surety.

On that occasion I learned for the first time in Florence about this subtly malicious trick and I saw it used, unfortunately, in the service of the Sovereign. All that I have now revealed is true; the person who tried to bribe me still lives and is still in the secretariat of the Workshops, it is not unlikely that he himself would admit to the deed as reported, perhaps because he still thinks it nothing wrong, but customary, it being commonly observed that the most dishonest deeds that have always been done, and that everyone does, are no longer believed really bad. However that may be, this is certain; in such a way any means of exposing the guilty is foiled, and the accused can always defend himself

before the Tribunal with the receipt in his hand, and the accuser is punished for calumny.

However, if the disburser often dismisses the sellers, if he no longer makes use of them and the artists, that is sure proof that there is no connivance, agreement, or deceit among them, because it is naturally not in the interest of the disburser to part with such people, so useful to himself. I have found myself continually in this situation, because as soon as I was aware of being tricked or of unfair and falsified prices, at that moment I stopped using them, which is why I was forced to dismiss so often such people as both thieves and slackers. The result was that all turned against me and became my enemies and calumniators. I did not fear them, as I was innocent; I did not use them because it was not in the interest of the R.M. to do so.

Such conduct on my part, irreproachable and honest, drew enemies from everywhere who did all they could to make me odious, suspect, and wretched. There is no trick which they did not use against me, even to the nobility of the country being influenced partly by the complaints of the artists dismissed by me; partly by the hatred of a foreigner who occupied a place which they believed should go to a Tuscan. Chief among those who persecuted me only from envy was Niccolini, at that time commissioner of Santa Maria Nuova. He, taking it ill that I should use cadavers from the hospital, and envious at seeing a permanent school of anatomy formed outside of that place and his domain, did everything to prevent my using the cadavers, and seeing that even with very few the work in wax of the R.M. was carried on, he finally went so far as to accuse me of lack of respect, the misuse of too many cadavers, of arrogance on the part of my dissector while I knew nothing of all this, had never said anything to my dissector later brought to trial who also was found innocent; I was forced to be content with some remnants of cadavers that Niccolini out of charity deigned to allow me for use by the R.M.; I asked then for a hearing, and learned very soon where the guilt lay and was persuaded by Dr. Lagusius to *let it all be forgotten and to speak no more about it, H.R.H. being sufficiently aware of my innocence.* Then the fact was learned at the hearing that of ten dying in the hospital, the cadavers of at least eight were buried untouched by the anatomist's knife, in this to do harm to me the work of the R.M., which was something desired and ordered by my Sovereign, was slowed down. In a few words, on the one hand I was pressed to make the work on the anatomical waxes proceed without any delay and to answer for their perfection, on the other I was deprived of any means of doing so. Such

has been my situation for many years [...] God knows if I have ever spared my health for a single moment in doing my duty; I worked day and night, without rest, or vacation [...].

Before me it had been tried to make instruments of physics in Rome, Naples, Bologna, and Padua, but with no success. Professor Guadagni,[a] using freely the riches of Lord Cowper,[b] could make only a few miserable ridiculous childish instruments of no use which had to be thrown away, and he had to bring everything from London at great expense, as also the Universities of Pavia and Milan had to do. With patience and at the same time with great saving, I was able in a few years to make so many and so perfect as to be sufficient to give a complete course; many of these are entirely new and of my own invention [...] At the same time I did not refuse any of the various commissions given me by the government [...] I will mention the improvement of the art of assaying precious metals [...].

So many and so varied labors, combined with mercurial salivations [...] finally ruined my health, so that my Sovereign took pity on me and gave me permission for a journey in France and England [...] My journey of several years was no more than a continuous study, with perpetual instruction through conversations with the most celebrated philosophers and artists, and with working day and night in my rooms on physics and chemistry. [...] The result of the experiments was seventeen dissertations published in Paris and London, and a much greater work on experiments in animal physics which I published soon after arrival in Florence, two large volumes *in quarto*. [...] Back in Tuscany I devoted myself entirely to bring to completion the museum of physics, chemistry, astronomy and natural history. [...]

As the payment of the artists [...] distracted me from more important things, [...] I begged my Sovereign to relieve me of this; the charge was given to Custodian Gagli. In this way I could apply myself entirely to the progress of the many branchess of the R.M.; in a few years Florence saw the birth of a Botanical Garden, one of the richest and best planned as any in Europe [...]

Meanwhile Custodian Gagli exercised his new incumbency with so little attention and zeal that I could not at all be satisfied, nor hide my

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[a] See p. 16.

[b] George Nassau Clavering, Third Earl Cowper, spent 1760 to 1789 in Florence, playing a large part in its cultural life. He was a member of the *Accademia della Crusca* and other academies, and advised Peter Leopold on musical matters.

displeasure and disapproval. Infinite were the reasons which made me suspect his conduct, either with regard to good and faithful service to the R.M. or to me personally, whom he made out to be too stern and demanding. I do not deny that often I reproved him for negligence, for lack of attention and zeal for the R.M., but more than anything else for ill-advised purchases, at prices more or less altered to the prejudice of the institution, but in the face of all this I never failed him in his needs, and in spite of the counsel of all the employees of the R.M. I did not omit giving him some of my own money nearly every month; I did this for many years in order that he could pay the artists, who appealed to me to be paid for months past because he owed them, although he had received tha money from the treasury, and held thousands of *lire* in advance from month to month; often it happened that he had not spent twenty *lire* for the R.M., but asked me for money as he had none then. It was easy to see that he had used it for his own needs, and if I had listened to the voices of those who are now his friends, who protect and defend him because they see him triumphing in everything even over justice, I would have had to let him fall, be found a thief, but it is so far from my nature to do harm to anyone that then and there I tried to help him as I thought he needed help, taking from my own money whatever sums he needed, often more than one thousand *lire* at a time, so that often I was obliged to borrow on my furniture to give to him. Then with every annual audit I gave him even greater sums of money, so that either I borrowed from my friends or pawned things, especially when I saw him in danger of legal prosecution and being found guilty, and although I was advised by people at the Museum to abandon him, as my enemy and calumniator, and although it was pointed out to me that the least ill that could come to me was to lose the money that I had loaned, I never wished to do so hoping always that he would behave better in the future. I was not unaware that he was really one of my principal enemies, whether because I would not put up with his deficiencies prejudicial to the R.M., or whether because, as he said, he was forced to speak ill of me and calumniate me if he did not wish to lose his job; however all this did not make me change my ways nor did I worry about having him as enemy, as I knew that in no way could he really injure me.

But finally came the time when it would be delinquent of me to be silent any longer, nor could I be silent without offending my honor, without failing in my duty to the R.M., and my Sovereign. Old Gori, a man of known probity, together with Matteo Matteucci, both instrument makers for many years in the R.M., came to me complaining that Gagli

against the prevailing orders of the Sovereign, had not paid them for the whole past month and that by violent threats he had obtained from them a false receipt for the whole payment; they handed me a petition in which they asked for justice against such violence and for their wages, threatening to go to Commissioner Cangini,[c] and the Sovereign himself. I did everything to placate them, and so that Gagli should not be accused of theft and violence at the Tribunal which would have been his ruin, I promised to take care of the matter myself and pay them from my own pocket, if they were not promptly paid by Gagli. Considering however that I had put my signature, or the *Vidit* on their receipts, as I thought was fair and honest because when Gagli gave them to me to be signed, I believed then that the payment had been made, I began to fear that sooner or later, the trick being discovered, my name and my honor should be compromised, and it even could be suspected that I put my *Vidit* in connivance with Gagli to our common advantage. I wished then to have a legal opinion and was advised on no account to put my *Vidit* again on any receipt which could be false and incriminating. Uncertain however as to proceed, I went to Councilor Schmidweiller,[d] and told him everything in confidence, but he told me Gagli's crime was great and that to keep silent was equally a crime for me, thus he wished that I would give him the attestation of Gori and Matteucci concerning the false and extorted receipt from Gagli and he asked for my deputation in which I asked to be exempted in the future from putting the *Vidit* on Gagli's

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[c] Cangini, in the police, «does not lack talent, capability, and activity, but he is of equivocal character, insincere, prone to inopportune undertakings and pursuits, inclined toward favoritisms and also animosities, is untruthful and inclined to intrigue [...]» according to Peter Leopold.

[d] Ludwig Dithmar von Schmidweiller was Secretary for Finance in 1781, became Minister in 1782 on the death of Tavanti. In 1782, as Councillor of State he opposed the adoption of a constitution by Tuscany. He retained his position in the reorganization of the Council of State in 1789, and with others was charged to formulate a division of the property of the Crown and that of the State. He was named a member of the Provisory Regency on the departure of Peter Leopold in 1790. The latter's opinion of him was: «Councillor Schmidweiller is of tested honesty, is full of zeal and attachment to the service, assiduous and of good will, unsparing of effort, but is of mediocre talent, weak, with little spirit and courage, is timid, readily lets anyone give suggestions in all matters, with the best of intentions defers to the opinions of others, seeks advice and counsel of dubious and equivocal persons by whom he is misled, in a word lets himself be deceived; not firm or based on principle, fearful of everyone, wishing to please everyone, he is not able to take a firm, decisive, and resolute stand».

papers; this I obtained from my Sovereign with Schmidweiller's help; then the placing of the *Vidit* was given to Fabbroni without involving him in anything, a favor which I had never been able to obtain. I cannot say how Gagli learned about this, but I know well that he knew it all, and from that moment his hatred for me became fury and he boasted that he would find ways to ruin me, threatening me with the names of Ricci,[e] Stefani...[f] Then I saw at the R.M. something I had not seen before, as Gagli who had always in the past depicted me as an infamous man and not to be trusted, suddenly became a confidant, friend, protégé, and every moment in which I was not at the R.M., he remained among people or closed in his room in lengthy secret dialogues, surely for fear of being persecuted by Ricci, Stefani... and such people. The accusation against Gagli, moreover, was of the most serious nature, as it concerned theft, violence, disloyalty in service, and betrayal of trust; why then not look into it, whether true or false? If false, why not punish the calumniators in such serious matters? But what interest could Gori and Matteucci have in a false accusation, when their receipt cleared Gagli, and accused them of calumny, so that it was they who would be obliged to prove the accusation, the non-payment, in an uncertain and dangerous confrontation with Gagli himself? I will never believe that old Gori, a man of regular habits and conduct, would for no good reason commit such a shocking crime as an accusation which if false would necessarily lead to his punishment [...] but the fact is that a little later Gagli paid Gori and Matteucci secretly the money which was owing to them; thus was this affair stilled by my enemies, outraging the truth, punishing my zeal and devotion to duty [...]

My successor, meanwhile put on his guard by my misfortunes and the dangers I ran, arranged (as I said before) that the *Vidit* could be granted without any responsibility, or requirement to ascertain if the payments will have been made by Gagli or not; in short the granting of the *Vidit* became a pure formality, of no use to the R.M.; something which had been denied to me.

The peak of my misfortune and worries was reached a little later with the theft of the pearls. My ceaseless occupation and uninterrupted work gave me little time to go up to the rooms of natural history, where

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[e] A Giovanni Ricci was in the foreign ministry, «honest but with little ability or talent» according to Peter Leopold.

[f] A Gaetano Stefani, clerk in the Secretariat, accompanied Peter Leopold and Francis to Vienna in 1784.

Zuccagni had been working for some months. One morning, Zuccagni had me go up to the room where was the cabinet of precious stones, in which room he had worked for many days, and he showed me that all those stones which decorated the vault, or chapel, of the cabinet had been removed. He remarked that the theft had happened at night, because the day before he had worked in exactly this room, and had observed that the pearls of the vault were all there. In examining the cabinet I noticed that also missing were all those stones at the top, and I asked him what he could tell me of those. He answered that for about six months and more he noticed that every day one was missing, and that for six months back he had told Gagli about it almost every day, and ordered him to advise me of the daily thefts from the cabinet. Surprised by all this, I immediately called Gagli into the presence of Zuccagni, and asked him why he had not advised me of the daily disappearance of the pearls, and had not followed Zuccagni's orders. He replied, all red in the face and confused [...], that he had always forgotten about it. Zuccagni put all this in writing for his justification, in order that I might communicate it through the proper channels, which I did immediately, adding my representations for Schmidweiller. I certainly will not stoop here to vindicate myself, although I was aware that Gagli and calumniators of his ilk had tried to cast suspicion on me. Innocence needs no excuses, a man of honor never lowers or abases himself. This thing is so clear in itself as to leave no room for suspicion. It was a matter of about five hundred removed one by one with suitable instruments since all were fastened together by metal chains. The work was long, and it hardly seems possible that a whole night would be sufficient to detach so many pearls with none being found on the floor by Zuccagni. A ladder was needed to reach to the top of the cabinet; none was found in the room by Zuccagni. Who does not see that such a theft was an inside job, and who alone has every opportunity and security to do it? But how ever could Gagli forget for six months running to speak to me about the lower pearls of the cabinet, when Zuccagni advised him every day that pearls were disappearing and ordered him every day to speak to me about it? No one will ever be persuaded of that. Exposition of the facts alone shows clearly who is criminal, who is innocent, still everything was buried in a mysterious silence, and no one wished to hear of it even Zuccagni.

Finally it appeared that Heaven wished for once to put a stop to so much iniquity, with the theft of the English pottery, which happened a little later. Fabbroni advised me that it had been noticed that various ceramic pieces, that is, many plates and a number of latticework baskets

were being stolen. I hastened to examine them, and it seemed to me that a good part was indeed missing. I questioned secretly, after many others, Bicchierai the doorkeeper, who told me that he knew nothing of it, but had hopes of knowing something shortly; in fact after two days he told me he was nearly certain to have discovered the thief, but to make sure he wanted, if possible, to have a plate like those stolen. He took it from me secretly, and two days later gave me a paper written in his own hand in which he related that the stolen plates were in the home of Gagli, that he kept them in a closed chest under his own bed, taking them out only on occasions of giving a dinner when friends were invited; that not only the plates but also the latticework baskets were in that chest, that he had learned it all from Gagli's servant. He added that I should inform the proper authorities, or otherwise he would feel obliged by duty and conscience to resort to the Commissioner, or Senator Bartolini, or to the Sovereign himself. I tried to quiet him, and forbade him for the moment to speak of it to anyone.

For me the author of the theft was clear, because Bicchierai surely did not know that among the English ceramics there were some latticework baskets, and that exactly those were missing from the R.M., as the woman did not know that the latticework baskets in Gagli's chest belonged to the R.M. Here I confess, however, my weakness; I weighed for a long time whether I should be silent or speak, remembering past misfortunes, also I wished in this case to hear the opinion of some Minister, some man of the law; I was advised by all to speak as a matter of obligation and honor. It was pointed out to me that the accusation was such that in a few moments it could and should be verified, because the material evidence should be found in the chest under Gagli's bed, that beside the servant to testify there were all those who had been entertained at dinner by Gagli, whom the servant knew. I went then to Senator Bartolini, revealed my disquiet, and asked his advice. He told me my duty demanded that I not be silent, that I should bring him Bicchierai's accusing paper, that in less than four hours the ceramics would be found, and also the guilty one. All this I did, but again this time the affair ended by going against me. Gagli learned immediately from his usual protectors of Bicchierai's accusal against him, and ended by saying that the plates were in my house and that of Bicchierai, and people like Ricci, Stefani... were for him because they were my enemies and with such support he laughed at everyone.

I confess that a blow so horrible, so unjust, I had not expected this time, where the truth was so easily discovered, and I did not believe that

there could exist on the earth men so villainous and perverse. I confess that I do not understand how a matter so important to the honor of so many persons, so many innocents, so easy of discovery, should have been suppressed to my prejudice and damage only. I confess I do not understand why the infamous originator of so many crimes should not for once have been discovered. I confess I do not understand why, at least believing me guilty, Bicchierai, the servant, and I myself were not called up in private. The confrontation, with individual questioning of the three persons separately would have uncovered at least the calumny if not the thief, but because all fell out as usual against me, Bartolini would do nothing; in his only answer to my repeated and vivid entreaties he shrugged his shoulders and went away. Who knowing who was the guilty unfortunate one would not wish to search that chest of Gagli's? Only that could have declared him guilty. So a guilty person had to be spared only for the purpose of punishing an innocent one.

To so many misfortunes and vexations my enemies sought to add another, of a new kind, nothing concerning my work or duty but with the sole object of my dishonor, by making me appear to my Sovereign and to the public as a vile seducer, a destroyer of reputations and other people's integrity. I was accused by a maid, seduced by one of my servants whom my enemies had suborned, of having violated her by force and deceit; the crime began to be believed true by Commissioner Cangini, on the calumny of a shameless woman against the word of a man of honor. By a Minister of Justice I was exhorted to appease the slanderer quickly with money, as the lesser evil, if I did not wish to be punished and publicly disgraced. My innocence did not save me; I was required to appear as a criminal and violator before a Minister who was not unaware of the infamous past life of the woman, whose seducer, my servant, was a secret spy of three magistrates. My enemies, knowing of the accusation, spread it through the R.M. and throughout the land; Gagli declared everywhere that I was an infamous seducer. A miracle was needed to save me from all these scoundrels; it was a miracle for a calumniator to accuse himself of calumny and to declare my innocence before the same judges who had heard and believed the accusation, which as calumny should be punished according to the laws; and yet in a few days the villain, moved by fear of Hell, of his own accord declared my innocence to the Commissioner, perhaps to his regret, and confessed to having calumniated me at the instigation of others and for money. All Florence knew of imputed crime, of the money I was forced to pay the woman to save myself from punishment, but few or none knew of my proven innocence, of the

confessed and convicted calumniators. Meanwhile Gagli continued to triumph over me at the R.M., making them believe that I had not been punished publicly by the law inasmuch as I had thrown myself at the feet of my Sovereign to avoid punishment; so I remain, covered with dishonor and infamy, even in the eyes of my subordinates at the R.M., as a necessary consequence of calumny unpunished. I do not believe that there has been another man in the world more calumniated, more persecuted, and who has deserved it less than I have. If I had been heard every time that I was accused by my enemies, if it had been permitted me to clear myself in a court of justice, if a formal suit had been brought against me as required by the laws of all countries, there would surely have been recognized my innocence, the calumny of others and the underhanded and treacherous ways of those who have always tried to bring about my ruin with secret accusations – infallible sign of calumny and duplicity.

D

Account of the Dissection of the Cadaver of the late Felice Fontana, Professor of Physics and Director of the Royal Museum of Florence

On submission to anatomical dissection, as soon as the skin of the head was incised, there appeared an ecchymosis between the connective tissue and the aponeurotic sheath in the occipital region, exactly in the place where appeared the vestiges of the injury reported from the fall. Next, the skull was examined attentively, externally and internally, and nothing was found that merited attention, neither in the part struck, nor in the part corresponding. Examined, the dura mater showed an unnatural color with blood vessels much swollen; from the lateral part over the hemispheres on being opened there issued some quantity of bloody serum which lay between the dura mater and the arachnoid, occupying the entire space. The arachnoid was much infiltrated with lymph in its entirety, as if there had been a true hydrops from it and the pia mater. All of the convolutions and anfractuosities of the cerebrum and the cerebellum were very turgid, also the medulla oblongata, the blood vessels, arteries and veins, of the pia mater, and particularly all the other parts of the posterior lobes of the cerebrum in their inferior part. Subsequently examined in the substance of the cerebrum, both in the cortical and medullary parts, the blood vessels were very full of blood, so much that the medullary substance of the cerebrum had taken a reddish color. The corpus callosum was in a normal state. As for the first ventricles, there was a very small extravasation of bloody serum in the left ventricle, none in the right. The choroid plexus in all its length was in the natural state. There was no extravasation in the third ventricle.

The optic nerves had suffered no alteration, but in the corpus striatum, the left part on its surface showed a substance like clotted blood of a reddish color, and examined more closely there was visible the remains of an extravasation, which in all its upper part had passed to

suppuration, which extended nearly the half of its substance. The right appeared to be normal. There was no extravasation in the fourth ventricle. Similar engorgement of the blood vessels in cortical and medullary parts of the brain was found elsewhere in the section of the cranium, but no more. Drawn from the cavity of the thorax, the lungs were examined first of all, and found to be much inflamed, particularly the left, and the vessels were much engorged. Both thoracic cavities showed a slightly bloody serous fluid. The heart was of normal size, but extremely flaccid and empty of blood, so that compression with the fingers divided it as if putrified. Finally passing to the examination of the abdomen, we found much gas in the intestine, a portion of which was inflamed. The stomach contained a little fluid matter; particularly at the part near the liver ist internal membrane was gangrenous. The liver was livid and inflamed in all its upper external part, particularly the lobe concave toward the stomach. The diaphragm was also inflamed near its junction with the falciform ligament of the liver. The gallbladder was full of dark-colored bile. The spleen was also much inflamed, flaccid, and very small. The kidneys and the bladder were normal.

[Signed] Dott. Gaetano Palloni
Dott. Flamminio Piselli
G. Fabre, Dr. en méd.

[Undated]

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Florence P.K.K.

* * *

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ABBREVIATIONS OF NAMES OF PERIODICALS

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Archeion – Archivio di Storia della Scienza. Roma. 1919-1943.

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Giornale di Firenze – Giornale di Firenze. Opera periodica che ha per primo oggetto la conservazione del corpo umano. Firenze. 1769-1771.

Giornale di Medicina – Giornale di Medicina. Venezia. 1763-1777.

Giornale d'Italia – Giornale d'Italia. Spettante alla Scienza Naturale, e principalmente all'Agricoltura, alle Arti, ed al Commercio. Venezia. 1764-1776.

Giornale Fis.-Med. – Giornale Fisico-Medico, ossia Raccolta di Osservazioni sopra la Fisica, Matematica, Chimica, Storia Naturale, Medicina, Chirurgia, Arti e Agricoltura. Pavia, Comini. 1792-1796.

Jour. de Physique – see Obs. Phys.

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Chapter 18

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