ΣΥΖΗΤΗΣΙΣ

Just like Astronomers Do: Building Hypotheses in Giorgio Baglivi's Medicine

di

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Abstract: Thomas Sydenham's view on methodology in the preface to *Observationes medicae* (1676) is traditionally considered one of Giorgio Baglivi's main sources for the reform of medical practice outlined in *De praxi medica* (1696). This is the case for two crucial aspects: the recovery of observation in medicine and the conception of "natural history of disease". However, Sydenham and Baglivi have different opinions about the role of hypotheses in medical practice. While recognising the main critical issues about their use, Baglivi tries to provide physicians with some useful instructions for building them: just like astronomers do, hypotheses should be founded on a strong empirical evidence and rejected once they are in discordance with Nature and thus unable to properly explain phenomena. This paper will explore this pivotal aspect of Baglivi's methodology.

Keywords: Giorgio Baglivi, Thomas Sydenham, Experience, Natural History, Hypothesis

Abstract: La metodologia di Sydenham descritta nella prefazione alle *Observationes medicae* (1676) è tradizionalmente considerata una delle principali fonti usate da Giorgio Baglivi per la sua riforma della medicina pratica delineata nel *De praxi medica* (1696). Questo vale soprattutto per due aspetti essenziali: il recupero dell'osservazione in medicina e la concezione della "storia naturale della malattia". Tuttavia, Sydenham e Baglivi hanno opinioni diverse in merito al ruolo delle ipotesi nella medicina pratica. Pur riconoscendo le principali criticità nel loro uso, Baglivi tenta di fornire ai medici alcune utili istruzioni per la loro formulazione: proprio come fanno gli astronomi, le ipotesi dovrebbero essere fondate su una solida evidenza empirica, da rigettare una volta entrate in disaccordo con la natura e quindi divenute incapaci di spiegare correttamente i fenomeni. Questo contributo approfondirà questo aspetto fondamentale della metodologia di Baglivi.

Keywords: Giorgio Baglivi, Thomas Sydenham, esperienza, storia naturale, ipotesi

Introduction: Baglivi and the Reform of Medical Practice

In 1696, the Croatian physician Giorgio Baglivi (1668-1707), professor of anatomy and surgery at Sapienza University (*Studium Urbis*), published his first work *De praxi medica*, a treatise about medical practice[1]. Differently from what we generally expect from the genre of *practica medicinae*[2], however, Baglivi does not provide here a "head-to-toe" (*a*

[1] On Giorgio Baglivi's life and works, see M. Salomon, *Giorgio Baglivi und seine Zeit. Ein Beitrag zur Geschichte der Medicin im 17. Jahrhundert*, Hirschwald, Berlin 1889; F. Scalzi, *Giorgio Baglivi. Altre notizie biografiche ricavate da un epistolario inedito e dalla sua opera*, «Gazzetta medica di Roma» 15 (1889), pp. 457-470, 529-546, 553-561; Id., *Giorgio Baglivi e il suo tempo*, «Lo Spallanzani» 7-8 (1889), pp. 321-337; M. D. Grmek, *Osservazioni sulla vita, opera ed importanza storica di Giorgio Baglivi*, in *Atti del 14. Congresso internazionale di storia della medicina, Roma-Salerno, 13-20 settembre 1954*, Guerra e Belli, Roma 1960, pp. 423-437; Id., *La vita e l'opera di Giorgio Baglivi medico raguseo e leccese (1668-1707)*, in G. Cimino-U. Sanzo-G. Sava (eds.), *Il nucleo filosofico della scienza*, Congedo, Galatina 1991, pp. 93-111. See also the proceedings of the conference *Alle origini della biologia medica. Giorgio Baglivi tra le due sponde dell'Adriatico*, published as a special issue in «Medicina nei secoli» 12/1 (2000). On Baglivi and medicine in Rome, see M. Conforti-S. De Renzi, *Sapere anatomico negli ospedali romani: Formazione dei chirurghi e pratiche sperimentali (1620-1720)*, in A. Romano (ed.), *Rome et la science moderne: Entre Renaissance et Lumières*, Publications de l'École française de Rome, Rome 2009, pp. 433-472. A pivotal source for Baglivi's biography is the correspondence: see D. Schullian (ed.), *The Baglivi Correspondence from the Library of William Osler*, Cornell University Press, Ithaca-London 1974 (hereafter: Osler); A. Toscano (ed.), *Carteggio, 1679-1704: conservato nella Waller Collection presso la University Library Carolina Rediviva di Uppsala*, L.S. Olschki, Firenze 1999 (hereafter: Waller); F. Di Trocchio-G. Guerrieri-E. De Simone (eds.), *Carteggi di Giorgio Baglivi: Fondi Osler e Magliabechi (1677-1706)*, Milella, Lecce 1999. In this paper passages of *De praxi medica* are quoted from the first edition: Giorgio Baglivi, *De praxi medica ad priscam observandi rationem revocanda. Libri duo. Accedunt Dissertationes novae*, typis Dominici Antonii Herculis, sumptibus Caesaretti, Romae 1696 (hereafter: *PM*). Other quotations are from G. Baglivi, *Opera omnia medico-practica, et anatomica*, sumptibus Anisson, & Joannis Posuel, Lugduni 1704 (hereafter: Opera 1704).

[2] For an account of how the genre of *practica medicinae* developed, see J. Coste, *La Médicine pratique et ses genres littéraires en France à l'époque moderne*, available at http://www.bium.univ-paris5.fr/histmed/medica/medpratique.htm (23.05.2019): Coste's analysis is restricted to the French collection, notably at the Bibliothèque Nationale de France (BNF). See also A. Wear, *Explorations in Renaissance Writings on the Practice of Medicine*, in A. Wear-R. K. French-I. M. Lonie (eds.), *The Medical Renaissance of the Sixteenth Century*, Cambridge University Press, Cambridge 1985, pp. 118-145; I. Maclean, *Logic, Signs and Nature in the Renaissance*, Cambridge University Press, Cambridge 2001.

capite ad calcem) description of diseases[3], but rather he focuses his attention on the method of medicine, particularly on the main problems that are supposed to weaken its reliability. Following a Baconian perspective, he firstly tries to identify the *idola* of medical practice, i.e. those malpractices affecting medical formation and profession; secondly, he outlines a new method for making accurate observations and good clinical inferences, which implies the construction of "natural histories of diseases" – an adaptation of Bacon's *historiae naturales* to medicine.

According to Baglivi, the main reason for the crisis of medicine is to be found in the fact that physicians have gradually neglected the importance of direct observations and bedside experiences due to the spread of rational medical systems with scant consideration of Nature. While recognising the limits of rationalism, however, Baglivi is also perfectly aware of the risks of mere empiricism in medicine, which states that knowledge comes only or primarily from sensory experience. In both cases, rationalism and empiricism suffer from the absence of a «methodus experiundi», meaning a procedure for processing the information provided by the senses, in order to appropriately analyse experience. Such a misleading interpretation of nature explains the dissemination of numerous medical theories grounded on false hypotheses – namely, Van Helmont's chemical medicine, Gilbert's magnetism, Mayow's nitroaerial theory, acid-alkaline theory, or Doläus's cardimelech and microcosmetor principles, for example – by which physicians claim to define and heal diseases[4]. Similarly, empiricists base their clinical judgments (on both diagnosis and therapeutics) entirely on experience, but without any filter, so that their *ratio experiundi*, as rationalists reply, is

[3] In fact, Baglivi provides an account of diseases in a separate section of aphorisms, after describing (bk. I, ch. 9) the sixth "impediment" to the progress of medicine, that is the «intermissum studium tractandi de morbis aphoristice». However, although this section may be used for clinical purposes (and it has been used indeed: see, for instance, *Maladies traduites du latin de Baglivi... par M.G. D'Aignan*, chez la Veuve Delaguette..., Paris 1757, which provides a French translation of these aphorisms), this is not the main aim of Baglivi's work. Rather, these aphorisms serve as a reference model for the style physicians should use in their medical reports. See *PM* I, 9, §1, p. 50: «Sed quomodo solidae, diutiusque repetitae morborum observationes stylo brevi, & aphoristico exponendae sint, ex morbis mox recensendis, & per Patientem in Xenodochiis Italiae factam observationem examinatis aperte constabit».

[4] On these theories, see A. G. Debus, *The Chemical Philosophy*, Dover Publications, Mineola, New York 2002; A. Clericuzio, *Elements, Principles and Corpuscles: A Study of Atomism and Chemistry in the Seventeenth Century*, Kluwer, Dordrecht 2000.

«stupidam, erraticam, non repetitam, in intellectu non fermentatam»[5], which inevitably draws false conclusions.

De praxi medica provides a possible alternative to these two opposing views. Another form of experience can be pursued, which is mediated by reason, and therefore able to properly question nature and lead to the knowledge of the morbid state, thanks also to the direct intervention on nature, according to the Baconian idea of *natura constricta et vexata*[6]. Thus, Baglivi's medical reform searches for a balance between reason and observation, because experience without reason is not able to manage the complexity of the living being:

> Quod spectat ad peculiarem cujuslibet morbi curationem, arbitror illam raro feliciter cessuram, nisi ratio observationi adjungatur. Mille namque morborum causae, varia aegrorum temperamenta, aetates, sexus, vitae genera, climata diversae naturae, variae annorum constitutiones, & varia semper influentes; innumera denique alia, quae ad producendos, fovendosque morbos concurrunt, ita interdum certam constantemque morbi, & suorum symptomatum naturam perturbant, ut difficile sit veritatem investigare, nisi complexus horum omnium sagaci rationis usu perpendatur, & illustretur[7].

This perspective follows the same combination of experimental and rational faculties represented by the action of "bees", according to the well-known Baconian metaphor in *Novum Organum* I, 95:

> Qui tractaverunt Scientias aut Empirici, aut Dogmatici fuerunt. Empirici, formicae more, congerunt tantum & utuntur; Rationales, aranearum more, telas ex se conficiunt; Apis vero ratio media est, quae materiam ex floribus horti et agri elicit, sed tamen eam propria facultate vertit & digerit[8].

This image perfectly fits also the idea of physician promoted by Baglivi

[5] *PM* II, 2, §1, p. 155.
[6] See, for instance, Baglivi's experiments with blistering drugs: L. Tonetti, *Corpus fasciculus fibrarum: Teoria della fibra e pratica medica nel De praxi medica di Giorgio Baglivi*, «Physis. Rivista Internazionale di Storia della Scienza» 51/1-2 n.s. (2016), pp. 379-392.
[7] *PM* I, 2, §12, pp. 11-12.
[8] *Novum Organum* I, 95, in G. Rees-M. Wakely (eds.), *The Oxford Francis Bacon* (hereafter OFB), vol. XI, Clarendon Press, Oxford 2004, p. 152. See P. Rossi, *Ants, Spiders, Epistemologists*, in M. Fattori (ed.), *Francis Bacon: Terminologia e Fortuna nel XVII Secolo*, Edizioni dell'Ateneo, Rome 1984, pp. 245-260.

in his work:

> Formica colligit & utitur, ut faciunt Empirici, qui hinc inde experimenta venantur, iisdemque nec observatione repetita confirmatis, nec dilucido examinatis ratiocinio, paulo post indiscriminatim utuntur. Aranea ex se omnia fila educit, neque ullam à particularibus materiem petit, ita faciunt Medici speculativi, ac mere sophistici. Apis denique caeteris se melius gerit: haec indigesta è floribus mella colligit, deinde in viscerum cellulis concoquit, maturat, iisdemque tandiu insudat, donec ad integram perfectionem perduxerit. Hoc genus Medicorum apis aemulum desideratur in Arte nostra [...][9].

Therefore, what physicians need is not mere experience, but a very new approach to Nature enabling them to perform "qualified" observations. Baglivi's new method represents exactly that kind of approach that makes it possible to combine the role of experience with reason, that is with those cognitive processes necessary to infer clinical principles and "practical axioms" suitable for medical practice.

It is worth noting that the English physician Thomas Sydenham (1624-1689), core advocate of empiricism in medicine, is one of Baglivi's main sources: undoubtedly, Sydenham's ideas on methodology have influenced *De praxi medica*, especially on these two crucial aspects, 1) the recovery of observation in medicine according to a Hippocratic perspective, and 2) the collection of "natural histories of disease"[10]. However, in some respects, these two positions seem to differ with regard to the problem of hypotheses in medicine: unlike Sydenham, Baglivi devotes an entire chapter to this issue, providing some instructions for building reliable and proper hypotheses.

This paper will explore this pivotal aspect of Baglivi's methodology. In the first part, I will describe Sydenham's instructions for compiling

[9] *PM* I, 12, §5, pp. 104-5. Baglivi will further address the main issues of rational medicine, by the image of the "spider". Cf. *PM* I, 7, §10, pp. 41-42: «Medici itaque valde litterati, Philosophiis, & Theoriis plusquam par est addicti, & ad instar araneae ab indigestis cogitationibus sapientiam perpetuo educentes, nunquam boni Practici evadent, nisi diuturno praxeos usui, & exercitationi omnino se subjecerint».

[10] On Thomas Sydenham, see K. Dewhurst, *Dr. Thomas Sydenham (1624-1689): His Life and Original Writings*, Wellcome Historical Medical Library, London 1966. See also: A. Cunningham, *Thomas Sydenham: Epidemics, Experiment and the 'Good Old Cause'*, in R. French-A. Cunningham (eds.), *The Medical Revolution of the Seventeenth Century*, Cambridge University Press, Cambridge 1989, pp. 175-177.

natural histories of diseases, provided in the preface to *Observationes medicae* (1676), a tentative "discourse on method" of medicine which shows a strict dependence from Locke. Then, I will analyse Baglivi's method (§2) and his view about hypotheses' role in medicine (§3). Finally, in §4, I will focus on William Cole (1635-1716), a rational physician, friend of Sydenham and Locke, but also one of Baglivi's correspondents. Interestingly, he addressed this issue in a letter to Baglivi after the publication of *De praxi medica*. His observations will allow us to further clarify Baglivi's position.

1. *Sydenham's Natural History of Disease*

Thomas Sydenham's views on clinical methodology are given in the thirty-five pages of the preface to *Observationes medicae* (1676), a large description of epidemics and fevers occurred in London between 1661 and 1675. In fact, this is the third edition of a previous work, *Methodus curandi febres* (1666), whose main aim was that of providing practical methods in order to both identify the species of fevers and find the best way to treat them, without relying on any hypotheses about the body or the disease itself.

However, although the term "hypothesis" is used almost inconsistently – as usually happens at that time – such approach does not seem to disregard suppositions. Interestingly, at the very beginning of the first chapter about continued fevers, Sydenham considered it necessary to reveal the two main assumptions on which his method of healing was based, in order to prove it reasonable and fully justified[11]. He supposed that 1) fevers are the effort of Nature to remove disease,

[11] T. Sydenham, *Methodus curandi febres propriis observationibus superstructa*, edited by G. G. Meynell, Winterdown Books, Folkestone 1987, p. 17: «Quaenam a me in Febrium continuarum medela observata est methodus, quo luculentius patescat, non abs re futurum arbitror de Principiis, e quibus Praxis nostra enascitur, pauca quaedam praemittere. Id quod eo libentius facio, ut palam fiat Therapiam nostram non esse prorsus Empirice institutam, sed ejusmodi quae solidis rationum fulcris innitantur, aut saltem nobis inniti visa sit». However, Meynell says (p. 229, note 1), this paragraph should not be too literally interpreted: «Presumably the 'solid foundation of reason' was, to him, the body of hypotheses that follow immediately in para.1-5 which he regarded as derived from his clinical experience». Meynell's edition reproduces the Latin text of the 1666 and 1668 editions with the English translation by R. G. Latham (1848). See also K. D. Keele, *The Sydenham-Boyle Theory of Morbific Particles*, «Medical History» 18 (1974), pp. 240-248.

by evacuating the impurities in the blood or making disease conversions (*successiones morborum*) possible; 2) the best treatment is the one that tempers the blood commotion. However, despite Sydenham's emphasis on his personal manifest experience, a tacit knowledge may be anyway unconsciously implied: it could be, for instance, a certain conception of Nature, or a certain definition of blood composition, or some idea about the morbid mechanism that is supposed to be involved in fevers. It is not obvious to determine if such "hypotheses" are really subservient to experience or rather they are the result of some *a priori* knowledge, which may correspondingly influence the way of observing and interpreting the nature. Three main problems are at issue here: 1) to what extent and manner hypotheses are related to experience and observation; 2) to what extent physicians employ them consciously; 3) to what extent hypotheses prove necessary for clinical practice.

The close collaboration with John Locke in 1660s may have helped Sydenham explore more deeply these issues[12]. The same themes that will be developed in the preface of 1676 are already given in some medical papers in Locke's hand amongst the Shaftesbury Papers now preserved at the Public Record Office in London[13]. Besides sharing same interests and values, these unpublished manuscripts, now attributed

[12] On Sydenham-Locke collaboration, see: G. G. Meynell, *Sydenham, Locke and Sydenham's De peste sive febre pestilentiali*, «Medical History» 36 (1993), pp. 330-332; Id., *John Locke and the Preface to Thomas Sydenham's Observationes Medicae*, «Medical History» 50 (2006), pp. 93-110; J. C. Walmsley, *Sydenham and the Development of Locke's Natural Philosophy*, «British Journal for the History of Philosophy» 16/1 (2008), pp. 65-83.

[13] The transcription of *Anatomia* (P.R.O. File 30/24/47/2) is given in K. Dewhurst, *Locke and Sydenham on the Teaching of Anatomy*, «Medical History» 2 (1958), pp. 3-8. The transcription of *De arte medica/Ars medica* is given in A. G. Gibson, *The Physician's Art: An Attempt to Expand John Locke's Fragment "De arte medica"*, Clarendon Press, Oxford 1933, pp. 13-26. Walmsley offers a new revised version of both manuscripts in his PhD dissertation: J. C. Walmsley, *John Locke's Natural Philosophy (1632-1671)*, Thesis (Ph.D.), King's College, London 1998, pp. 221-231, 232-239. Their attribution to Locke is a much-debated issue: see G. G. Meynell, *Locke as the Author of Anatomia and De arte medica*, «Locke Newsletter» 25 (1994), pp. 65-73; P. Anstey-J. Burrows, *John Locke, Thomas Sydenham, and the Authorship of Two Medical Essays*, «The Electronic British Library Journal» 3 (2009), pp. 1-42. On Locke as a "physician", see K. Dewhurst, *John Locke (1632-1704): Physician and Philosopher: A Medical Biography; with an Edition of the Medical Notes in his Journals*, The Wellcome Historical Medical Library, London 1963. See also C. Crignon, *Locke médecin: manuscrits sur l'art medical*, Classiques Garnier, Paris 2016.

to Locke, show the evolution of that methodological approach to medicine which, although already partially presented in *Methodus*, will be fully outlined only in *Observationes medicae*.

In the short paper *Anatomia*, for example, Locke argues against the role of anatomical knowledge in medicine. No improvement in medical practice follows the development of anatomy, because it is impossible in fact to grasp and penetrate the hidden structure and functioning of the body by dissection. But even if it were possible, it would be of no use to the physician. A good disease treatment does not depend on a good anatomical knowledge:

> But that anatomie is like to afford any great improvemts [in]*to* the practise of physic or assist a man in the findeing out & establishing a true method I have reason to doubt: All that Anatomie can doe is only to shew us the grosse & sensible parts of the body, or the vapid and dead juices. all wch, after the most diligent search will be noe more able to direct a physitian how to cure a disease than how to make a man, for to remedy the [eff]*de*fects of a part whose organica{...} constitution & that texture whereby it operates he cannot possibly know is alike hard as to make a part wch he knows not how is made. now it is certaine & beyond controversy that nature perform all her operations in the body by parts soe minute. & in sensible that I thinke noe body will ever hope or pretend even by the assistance of glasses or any other invention to come to a sight of them [...][14].

What allows medicine to improve is only experience, that means natural history («only from history & the advantage of a [seriou] diligent observation of *these* diseases»), performed at the bedside without knives or magnifying tools, just as a gardener who «may by his art & observation be able to ripen meliorali{...} & preserve his fruit without examining, what kindes of juices fibres pores &c are to be found in the roots barke or body of the tree»[15].

If *Anatomia* is concerned with the impossibility to detect the operations of nature, say to grasp the hidden causes of things, *De arte medica* instead explores further the reason of this inability (strictly related to the idea of a both non-understandable God and creation), by accusing the learned physicians of disseminating fanciful hypotheses due to

[14] PRO 30/24/47/2 f. 31r. I quote from Walmsley's edition.
[15] f. 31v. On Locke's conception of "natural history", see P. Anstey, *Locke, Bacon and Natural History*, «Early Science and Medicine» 7/1 (2002), pp. 65-92.

their vain attempt to penetrate the essences of diseases and, as a result, of preventing medicine from progress.

> [...] I think I may confidently affirme, that those hypothesis w^{ch} tied the long & elaborate discourses of the ancientts & suffered not their enquirys to extend them selves any farther then how the phenomena of diseases might be explaind by these doctrines & the rules of practise accommodated to the received principles has at last but confined & narrowed men thoughts, amused their understanding with fine but uselesse speculations, & diverted their enquiries from the true & advantageous knowledge of things[16].

The same scant consideration for the use of hypotheses, except for their function of aid to memory, can also be found in the so-called "Smallpox Fragment" by Locke (1670), probably the sketch for the preface of a work by Sydenham on smallpox, which however never appeared:

> But tis but ostentation & losse of time to lay downe hypothesis wch are many times false always uncertain & make a show to enquire into the essences of things & pretend to shew the way & manner of their observacon things that we cannot know being beyond the information of our senses or the reach of our understanding & therefor with very little advantage pretend to them. Hypothesisos serveing after the thing is discovered very well for helps to our memorry but very seldom are sound & sure enough without experience to warrant our practise or lead us into the right way of operacon[17].

In the second and third editions of *Methodus*, published respectively in 1668 and 1676, Sydenham provided a revised version of his previous work, that is practically doubled in pages: from 156 pages of the first edition, to 218 of the second one, and 425 of the third one. Such change does not involve only the content structure, being evidently fuelled with new material from clinical observations, but concerns also a different approach to medicine, more pessimistic and critical about the effective capabilities of human knowledge. This new attitude is particularly evident from the preface to the third edition, in

[16] PRO 30/24/47/2 f. 51r.
[17] For this transciption, see P. Romanell, *Locke and Sydenham: A Fragment on Smallpox (1670)*, «Bulletin of the History of Medicine» 32/4 (1958), pp. 293-321: 295.

which Sydenham for the first time clearly outlines his methodology. Scholars have in depth examined Locke's influence on these introductory pages[18].

Development and progress in medicine are made possible only by natural history. By «historia» Sydenham means a disease description that is «graphica & naturalis», namely that represents phaenomena as appear to our eyes, without any unnecessary information, according to what proposed by Bacon:

> Sane morbos crasse depingere satis obvium est; atqui Historiam eorum ita conscribere, ut evitetur Censura, quam *Clariss. Verulamius* in nonnullos ejusmodi Promissores vibravit, longe majoris est negotii: *Satis scimus* (inquit vir Nobiliss.) *haberi Historiam Naturalem, mole amplam, varietate gratam, diligentia saepius curiosam: Atamen si quis ex ea fabulas, & authorum citationes, & inanes controversias, Philologiam denique & ornamenta eximat (quae ad convivales sermones, hominumque doctorum Noctes potius, quam ad instituendam Philosophiam sint accommodata) ad nil magni res recidet. Longe profecto abest ab ea Historia quam animo metimur*[19].

Four rules should guide the compilation of natural histories:

1. Physicians should classify diseases by reducing them to certain and defined species, in the same way that botanists build their phytology;
2. Physicians should abandon any theoretical hypothesis and, like painters, should draw a picture as accurate as possible of the disease;
3. Physicians should be able to distinguish constant and purely adventitious features within diseases;
4. Physicians should be able to identify the relationship between the diseases and the season of the year in which they arise more frequently.

Independently of Locke's influence, Sydenham's view about hypoth-

[18] See note 12.
[19] T. Sydenham, *Observationes medicae circa morborum acutorum historiam et curationem*, Typis A.C. Impensis Gualteri Kettelby, Londini 1676, pp. ar-v, emphasis in the original. Sydenham quotes from Bacon's *De Augmentis Scientiarum*, book II, ch. 3. Cfr. *Descriptio globi intellectualis*, ch. 3, in OFB VI, pp. 104-107. The same passage is quoted also by Baglivi.

eses, as represented in rule two, is the result of a more general consideration about the search for causes in medicine, which is already highlighted in *Methodus*. What explains the absence of a theoretical structure in his exposition is the impossibility, for him, to identify the causes responsible for the morbid condition. Anyway, even if this were the case, such knowledge would not be necessary for therapy.

2. *Baglivi's Methodology*

After his death in 1689, Thomas Sydenham's reputation dramatically changed, since his methodology was gradually considered an inspiring means to return to the early Hippocratism due to the disdain of speculations and the promotion of bedside experiences. Peter Anstey has recently argued for Locke's role in establishing Sydenham's myth as the "English Hippocrates"[20]. Interestingly, Baglivi was among the first to support this view, by celebrating Sydenham as «artis nostrae ornator, & ornamentum, qui sepositis opinionum commentis ad observationes prorsus se dedit, & a prima aetate ad extremum usque senium cum natura cohabitavit»[21], «Vir magni nominis»[22], «doctissimum»[23], «diligentissimus post Hippocratem Observator»[24]. Such new approach to medicine was so widespread that, for instance, Oronzio Rizzo, asking Baglivi for a medical advice, wrote: «Si degni dunque, e come ordina V.S. al modo di Sydenam [*sic*], *intendere nude, et sine* filosofia l'historia del mio male [...]»[25]. In a sense, Baglivi is thus perceived as embodying that "modo di Sydenham", that way of interpreting medicine as something entirely free of conjectures and theoretical speculations (*sine filosofia*). Similarly, even one of Sydenham's supporters, the English physician Walter Harris (1647-1732), considered Baglivi's medicine compatible with what advocated by Sydenham: «Tu vero, insignissime Domine, signis uspiam, expectationem nostram suscitas, et quae Sydenhamius noster voluit, efflagivit aut conatus est, ex te merito speramus, et ex principiis tantis perfectionem aliquam in difficillima Praxeos provin-

[20] P. Anstey, *The Creation of the English Hippocrates*, «Medical History» 55/4 (2011) pp. 457-478.
[21] Opera 1704, p. 130.
[22] Ivi, p. 138.
[23] Ivi, p. 207.
[24] Ivi, p. 222.
[25] Rizzo's letter to G. Baglivi, 13 October 1699, in Waller, no. 22, p. 72.

cia, nobis promittimus»[26]. However, the steps towards the definition of a new method for medical practice are more complicated in Baglivi. The same applies also to the way Sydenham's methodology has been then implemented in *De praxi medica*.

What does "to observe" really mean? How do physicians accomplish "qualified" observations, i.e. make experiences that prove to be reliable sources of information from which principles and operative axioms can be inferred? Hippocrates, while being a model for the physician-observer, did not arrange any form of methodology or procedure: just like architects, Baglivi said, the Hippocratics «pro talibus perficiendis operibus scalas, trabes, funes, & innumera alia aedificandi instrumenta; opere absoluto omnia submovent: unde posteri licet aedificiorum magnificentiam admirentur, ignorant tamen eisdem perficiendis adhibita instrumenta»[27]. Thus, Baglivi believed that those means or methods (*vias*), that «olim ab Hippocrate in usu forsan habitas ad promovendam perficiendamque Medicinam per observationes, historiam & praecepta»[28], could be found in Baconian methodology[29].

Baglivi's method for natural histories of diseases consists of four different but strictly interrelated steps:

1. *acquisitio*;
2. *dispositio*;
3. *maturatio ac digestio*;
4. *abstractio praeceptorum*.

In the first step, data recording (*acquisitio*), physicians should only collect observations, without adding any comment, supposition or rhetorical device. Baglivi, like Sydenham, quotes the aforementioned

[26] Harris's letter to G. Baglivi, 8-19 April 1701, in Waller, no. 57, p. 132. A version of this letter is given also in Opera 1704, p. 658.
[27] *PM* II, 3, §1, p. 161.
[28] *Ibidem*.
[29] See J. Boucher, *De l'influence du baconisme sur les sciences en général et la médecine en particulier*, Labé, Paris 1851; G. Dell'Anna, *Giorgio Baglivi e la «Medendi methodus»: una rilettura dell'empirismo baconiano*, in L. Conti (ed.), *Medicina e biologia nella rivoluzione scientifica*, Edizioni Porziuncola, Santa Maria degli Angeli-Assisi 1990, pp. 272-288; M. Vidal, *Giorgio Baglivi tra osservazione clinica e speculazioni iatromeccaniche*, «Atti del centro ricerche storiche di Rovigno» 20 (1990), pp. 133-214; Ead., *The* methodus medendi *Innovation in Giorgio Baglivi's Work*, «Medicina nei secoli» 12/1 (2000), pp. 171-190; R. K. French, *Medicine Before Science: The Business of Medicine from the Middle Ages to the Enlightenment*, Cambridge University Press, Cambridge 2003, pp. 207-212.

well-known passage from Bacon to explain precisely the way that physicians should follow when preliminary dealing with diseases:

> Satis scimus, inquit, haberi historiam naturalem varietate gratam, diligentia saepius curiosam; si quis tamen ex ea fabulas, & antiquitatem, Auctorum citationes, inanes controversias, superstitionem, philologiam denique & ornamenta eximat (quae ad convivales sermones, hominumque Doctorum noctes potius quam ad instituendam Philosophiam sunt accomodata) ad nil magni res recidet[30].

This implies that everything they see must be recorded, even if considered meaningless or useless. Any judgments or inferences from sensory impressions are not allowed, even when evidences for discarding them are compelling.

In the second step, data organization (*dispositio*), physician should classify and organize data sets in order to obtain refined information assets that can be effectively processed. Data preparation consists in gathering, combining and structuring the "raw" data according to labels or categories of sorts (*articula inquisitionis*). So, for instance, data concerning a disease will be divided and organized in diagnostic and prognostic signs, constant or inconstant signs, causes, constitutions, symptoms (occurring continuously over a period of time or not), unfortunate events associated with indications or remedies.

Only in the third step, data elaboration (*maturatio ac digestio*), data are finally accurately processed, in order to carefully examine all the information that may be dubious and ambiguous or eliminate those false. Notes and comments, like Bacon's *monita*, are now allowed. This operation of data refinement, that seems to imitate Bacon's negative method of exclusion (*reiectio*), is subject to a new form of induction, just like in *NO*. In providing a definition of induction, Baglivi clearly refers to Bacon (*Verulamio teste*), as also results from the comparison between the two following passages:

[30] *PM* II, 3, §2, p. 163.

Bacon, *Distributio operis*

Inductionem enim censemus eam esse demonstrandi formam, quae Sensum tuetur, & Naturam premit, & Operibus imminet ac fere immiscetur. [...] At in forma ipsa quoque Inductionis, & iudicio quod per eam fit, opus longe maximum movemus. Ea enim de qua Dialectici loquuntur, quae procedit per Enumerationem simplicem, puerile quiddam est, & precario concludit, & periculo ab instantia contradictoria exponitur, & consueta tantum intuetur, nec exitum reperit. Atqui opus est ad Scientias Inductionis forma tali, quae experientiam solvat, & separet, & per exclusiones ac reiectiones debitas necessario concludat[31].

Baglivi, *De praxi medica*

Inductio namque quae fit per simplicem enumerationem nullis additis cautionibus rebus dubiis, & analogiam habentibus cum phaenomenis alterius morbi sub cujus specie illudunt; vel rejectionibus falsarum, & omnino incostantium, imperfecte concludit. Contra inductio laudata est Verulamio teste, quaedam demonstrandi forma, quae sensum tuetur, mentem illustrat ac perficit in conclusionibus recte deducendis, naturae imminet, ac fere immiscetur[32].

However, Baglivi provides us with a rather naive interpretation of Bacon's method, in which data processing is greatly simplified, ending (in the fourth step) with some gradual generalization from the collection of particulars, which leads to the derivation of axioms or "practical aphorisms", i.e. those precepts that should guide medical practice.

> Observator postquam in copiosa observationum sylva sat superque se exercitaverit, & Abecedarium naturae morborum optime didicerit, non debet ad maxime generalia advolare via compendiaria, & praecipiti, ad naturam impervia, disputationibusque proclivi; sed ascendendo, & descendendo, massam particularium sufficienter penetrando, sensim denique & continenter ad eadem pervenire, ab iisque postea propositiones medias & axiomata deducere[33].

A question immediately arises: how can physicians guarantee data

[31] OFB XI, pp. 30-33.
[32] *PM* II, 3, §4, p. 166.
[33] *PM* II, 3, §6, p. 167.

quality and completeness in order to get inductive inferences that are «sine fallaciis»? Baglivi would reply that both the collection of observations and the inference process are the result of a collaborative activity in which physicians work together in order to manage the amount of data created and achieve as much qualified information as possible. This would be possible thanks to "practical academies" aimed at systematically collecting observations.

However, generalization is not clearly explained, and it is therefore difficult to understand how this process of abstraction of practical axioms should really be.

3. *Baglivi's Requisites for Good Hypotheses*

In a letter to the Swiss physician Jean-Jacques Manget (1652-1742) – who was at that time involved in the design of the *Bibliotheca medico-practica* and in the re-edition of the *Bibliotheca anatomica* – Baglivi suggested paying more attention to the definition of a medical practice totally free of hypotheses, as Sydenham has shown in his works[34]. Remarkably, Manget replied that he has always avoided making hypotheses in his own descriptions of diseases, with the sole exception of those of Thomas Willis, which he decided to include in his *Bibliotheca* for their accuracy and clarity, despite being them however founded too much on conjectures. He gave Baglivi also an account of how each disease would have been described:

> Per me itaque, aut subinde tantorum Virorum opera, postquam morbum aliquem delineavi eiusque curationem tradidi, varias alias curationes e selectioribus Practicis Autoribus, tum Galenicis, tum Chimicis, tum, si ita loqui licet, mixtis, petitas exhibeo, illisque consilia, consiliis Observationes, observationibus anatomicas inspectiones, distincto ac proprio quaeque loco superaddo[35].

In his correspondence with Manget, Baglivi – working on a book on surgery that unfortunately never appeared – repeatedly emphasizes

[34] G. Baglivi's letter to J.-J. Manget, 1 August 1693, in Osler, no. 51, p. 112: «postpositis hypothesum figmentis et nugis [...], quae omnium votis hodie expetitur, eiusque fontes nuper aperuit immortalis ille Sydenhamius toti Italiae perquam charissimus».
[35] J.-J. Manget's letter to G. Baglivi, 17/27 September 1693, in Osler, no. 53, pp. 116-121, p. 118.

the need to create an apparatus of observations derived from manifest experience, without relying on any hypothesis, as Hippocrates himself realised with his «divina opera». Baglivi's disdain of hypotheses, however, is not yet supported by strong arguments. *De praxi medica*, in this sense, offers us a more pondered view on this issue.

Ch. XII in book I – whose title is «Methodus ad Tyrones de Morborum hypothesi recte construenda» – does not prohibit the use of hypotheses, but rather it is intended to provide inexperienced physicians with some requisites to form valuable and long-lasting ones. Thus, while having the same pessimistic attitude as Sydenham towards the possibility to grasp the essences of diseases, Baglivi does not exclude hypotheses at all, but believes that only those produced by mere speculation are definitely vain and harmful. Hypotheses do not precede but follow by necessity observation. In other words, the practice of natural histories, as the only way to achieve a "qualified" experience, is a necessary condition for the formulation of any hypothesis.

Interestingly, Baglivi recommends that physicians behave the same way as astronomers when formulating hypotheses. This comparison paves the way for a remarkable correlation between astronomy' and medicine's methodology, even if in so different fields. Astronomers, he says, proceed first with an accurate collection of data and only then formulate theories or hypotheses, by which predicting and calculating the motions of the stars and, in general, making sense of the phenomena observed. This is exactly what physicians should do: inferring hypotheses directly from nature, by preventing however the errors of the empiricists thanks to the compilation of natural histories of disease, that only ensures experience be qualified and, thus, able to be processed.

Such comparison may be further explored, by focusing on the debate on the epistemic status of astronomical hypotheses begun with the earliest reception of Copernicus's planetary heliocentric system[36], particularly between those supporting a conventionalist or a realist interpretation of it[37]. Conventionalism in astronomy is concerning with the attention that some mathematicians, particularly German

[36] See P. D. Omodeo, *Perfection of the World and Mathematics in Late Sixteenth-Century Copernican Cosmologies*, in J. D. Fleming (ed.), *The Invention of Discovery, 1500-1700*, Ashgate, Farnham 2011, pp. 93-108; Id., *Copernicus in the Cultural Debates of the Renaissance. Reception, Legacy, Transformation*, Brill, Leiden 2014.

[37] These categories – Nicholas Jardine says – should be avoided since they may be anachronistic if applied to the early modern astronomy. However, I will use them here only to simplify a very broad and complex debate.

scholars at the University of Wittenberg, paid to the problem of model predictability and empirical adequacy, independently from the search for the causal explanation of physical reality. In other words, Copernicus's system was shared to the extent it can better predict phenomena, say, for instance, the angular position of a planet. But other claims, such as those about the motion of the earth, were severely questioned or minimized as mere mathematical hypotheses, which are useful for making predictions but are not supposed to have any ontological implication, being inevitably in conflict with Aristotle's physics or the Bible. This interpretation fosters numerous arguments about the geometrical equivalence of models[38].

A very different view, a realist one, was defended instead by Kepler, who addressed the problem of the status of astronomical hypotheses in a dispute with Nicolaus Reimers Baer (1551-1600), also known as Ursus[39]. In 1588, in his work *Fundamentum astronomicum*, Ursus proposed a geo-heliocentric model very similar to the one outlined by Tycho Brahe in *De mundi aetherei recentioribus phaenomenis*, which probably was actually ready in 1587, but appeared at Hveen just the following year. Tycho's account of geoheliocentrism puts the planets (Mercury, Venus, Mars, Jupiter, and Saturn) orbiting the Sun, which in turn – together with the Moon and the fixed stars – moves about a motionless Earth. Ursus's model is the same, except for admitting a daily rotation (a single motion from West to East) of the Earth and correcting Mars's orbit. Helisaeus Roeslin (1545-1616) proposed a further variant in 1597. Tycho accused both of plagiarism in a letter to Cristoph Rothmann[40].

[38] R. S. Westman, *The Melanchthon Circle, Rheticus, and the Wittenberg Interpretation of the Copernican Theory*, «Isis» 66/2 (1975), pp. 164-193.
[39] On this dispute, see N. Jardine (ed. and trans.), *The Birth of History and Philosophy of Science: Kepler's "A Defence of Tycho Against Ursus"*, Cambridge University Press, Cambridge 1984. See also, the French edition: N. Jardine-A. P. Segonds, *La guerre des astronomes. La querelle au sujet de l'origine du système géo-héliocentrique à la fin du XVIe siècle. Volume II/2 – Le Contra Ursum de Jean Kepler*, Les Belles Lettres, Paris 2008. Jardine's analysis of Tycho/Kepler-Ursus dispute focuses particularly on the epistemological issues. See also: G. Cifoletti, *La nuova edizione di «Apologia pro Tychone contra Ursum» di Keplero: Teoria e storia delle ipotesi astronomiche*, «Rivista di Storia della Filosofia» 42/3 (1987), pp. 465-480; R. Martens, *Kepler's Philosophy and the New Astronomy*, Princeton University Press, Princeton and Oxford 2000, ch. 3; J. D. Serrano, *Trying Ursus: A Reappraisal of the Tycho-Ursus Priority Dispute*, «Journal for the History of Astronomy» 44/1 (2013), pp. 17-46.
[40] On Tychonic world system, see C. J. Schofield, *Tychonic and Semi-Tychonic World*

The controversy over the birth and the development of the geo-heliocentric model dealt also with the nature of hypotheses. In his harsh reply to Tycho, within the *Tractatus de astronomicis hypothesibus*, Ursus defended a "sceptical" interpretation of hypotheses: these would be mere inventions which are aimed only at fostering observations and predicting phenomena[41]. Since they are constructions, they cannot be true, nor really represent the system of the world:

> HYPOTHESIS, SEU FICTITIA SUPPOSITIO, est efficta Delineatio quorundam imaginariorum circulorum imaginariae formae systematis Mundani, observandis motibus coelestibus accomodata, atque ob servandos salvandosque motus coelestium corporum, eorundemque calculum exprimendum, efficta, assumpta, introductaque. Dico effictam Delineationem imaginariae (non verae ac genuinae, eam namque scire non possumus) formae systematis Mundani, non ipsius systematis; sed ejus formae talis, qualem imaginando mente concipimus, mentisque conceptu circumferimus[42].

Kepler was forced to intervene in the dispute, in order not only to defend Tycho, but also to save his own reputation: in *De astronomicis hypothesibus* Ursus had reported a letter by Kepler who looked at him with great admiration.

Kepler's reply is given in an unfinished manuscript *Apologia pro Tychone contra Ursum* (composed between October 1600 and April 1601), which however appeared for the first time only in 1858, within the 19th-century critical edition of Kepler's *Opera omnia* (1858-1871) by C. Frisch[43]. This text proves to be a valuable means to reconstruct the dispute over the epistemic status of hypotheses in early modern astronomy.

Chapter 1 addresses exactly this question: «Quid sit Hypothesis Astronomica»[44]. In refuting Ursus' arguments, Kepler traces the history

Systems, Arno Press, New York 1981, particularly pp. 50f. Tycho's starting point is explaining the comet of 1577. On Ursus' claim to priority of discovery, see *ibid*., pp. 108f.

[41] Moreover, Ursus maintains that Tycho's model – which centres the fixed stars upon the Earth and admits a motion of the Sun about the Earth – reproduces the same version originally proposed by Apollonius of Perga. It's quite clear why Tycho is particularly concerned about refuting Ursus' claim as soon as possible.

[42] Quoted in N. Jardine-A. P. Segonds, *La guerre des astronomes*, cit., pp. 402-403.

[43] N. Jardine (ed. and trans.), *The Birth of History and Philosophy of Science*, cit. I quote from this edition: hereafter, *Contra Ursum*. See also the French edition: N. Jardine-A-P Segonds, *La guerre des astronome*, cit.

[44] Kepler provides a preliminary general definition of "hypothesis": «Non statim cum cum ipsa caeli observandi consuetudine natus est mos iste, ut quam quisque

of the concept of "hypothesis", which originated from geometry, by maintaining that at least three different meanings are possible:

1. as to geometry, hypotheses are those axioms or postulates supposed to be self-evident and universally accepted, on which geometers base their demonstrations;
2. as to logic – precisely, Aristotle's theory of demonstration – hypotheses are the premises of a syllogism;
3. finally, as to astronomy, hypotheses represent a) the empirical data achieved by observation on which the demonstration is grounded; b) the general conceptions about the planetary system, from which the explanations of celestial phenomena are derived.

Kepler reverses Ursus's thesis, by claiming that astronomical hypotheses must be "true", being this a necessary condition for making true conclusions too. It is impossible, if not by mistake or chance, that a true conclusion follows from false premises. This is the case, even if there seems to be a variety of hypotheses able to explain the same phenomena. In fact, different hypotheses cannot lead to the same conclusion, if not apparently. And even if this happened, there would be some physical differences to be considered in the conclusions. If, for instance, Kepler says, Tycho got the same calculations as Copernicus from his hypotheses, however, the conclusions would be very different, because he would not accept the immensity of fixed stars admitted by Copernicus. Therefore: «Ita conclusione mutata, Hypotheses varias existere necesse est. Inconsideratus vero aliquis, ad solos numeros respiciens, idem ex varijs hypothesibus adeoque verum ex falsis sequi existimabit»[45].

Remarkably, Kepler realised, moreover, the difference between the geometrical and physical level of hypotheses: «Nam si in Geometricis duarum hypothesium conclusiones coincidant, in physicis tamen quaelibet habebit suam peculiarem appendicem»[46]. What underlies scepticism is then exactly the confusion between these two different levels. For example, orbits are something completely different from their geometrical constructions (say, concentric-plus-epicycle model or eccentric-circle model).

philosophorum ex intuitu caeli, de mundi dispositione concepisset opinionem, ea nomen aliquod haberet, *Hypothesisque* diceretur». *Contra Ursum*, I, 264r, p. 87.
[45] Ivi, 267v, p. 90.
[46] Ivi, 268r, p. 90.

The fact that geocentrism and heliocentrism – so, two contradictory hypotheses – are able to explain the same phenomena does not imply that a true conclusion could derive from both a false and true hypothesis. These models properly work because of what they are in common, i.e. to the extent they both assume the separation between the heaven and the earth. This means that:

> Omnis in astronomia conclusio non nisi ab uno et eodem medio perficitur, et uniformem praemittit hypothesin: etsi illa a seipsa differat, quatenus extra hanc demonstrationem consideratur. Et vicissim, quaelibet hypothesis, si accurate consideremus, propriam nec ulli alij hypothesi communem penitus producit conclusionem[47].

Conventionalists like Ursus confuse the different levels (geometrical, logical, and physical) implied in the formulation of the hypotheses and, consequently, are patently absurd when admitting that the astronomical hypotheses can be only false. As summarized by Serrano, the main aim of Kepler's reply is refuting two pivotal arguments of Ursus's scepticism about hypotheses: 1) the empirical equivalence (different hypotheses are deemed equally able to predict the same phenomena) and 2) insufficiency of evidence (evidences are not a sufficient condition to prove the truth of a hypothesis)[48].

It is hard to say if and how much Baglivi really dealt with these issues, but the fact that, as we will see, he distinguished between Tychonic and semi-Tychonic world systems shows a certain, even perhaps approximate, knowledge of the debate on the astronomical hypotheses[49]. This may depend on his close connection with Jesuit science, as a result of the "Galileo's affair", or rather on his probable familiarity with Bacon's works. Astronomical models, such as the Ptolemaic and the Copernican systems, are indeed considered by Bacon mere mathematical constructions that do not say anything about reality or the causal factors involved, serving rather as convenient tools for making predictions and calculations[50]. And since

[47] Ivi, 268v-269r, p. 92.
[48] Serrano, *op. cit.*, p. 21.
[49] On Tychonic and semi-Tychonic world systems, see Schofield, *op. cit.*
[50] See S. Ducheyne, *The Status of Hypothesis and Theory*, in P. Anstey (ed.), *Oxford Handbook of British Philosophy in the Seventeenth Century*, Oxford University Press, Oxford 2013, pp. 169-191.

they are mere constructions, without any ontological implication, it is always possible to think of alternative models that can be equally compatible with phenomena, provided they are likewise able to "save" them. Baglivi seems to adopt the same "conventionalist/instrumentalist" interpretation of hypothesis in medicine.

Interestingly, Baglivi realizes that one of astronomy's main concerns is the dissemination of different and opposing models, all equally able to explain the same phenomena. At stake here is the difference between the physical and mathematical level and the possibility to combine them in order to explain reality. As we have seen, how can be possible, for instance, to accept Copernicus's calculations and tables without sharing his heliocentric system? Baglivi claims that the existence of different astronomical models, such as the Ptolemaic, Copernican, Tychonic and Semi-Tychonic world systems, does not prove the "way of hypothesis" be as so useless and weak as to justify its rejection in astronomy. It is not properly a problem of underdetermination. That these systems, although so different from each other, can equally explain celestial phenomena and achieve the purpose for which they have been formulated, i.e. measuring the motion of planets or predicting astronomical phenomena, is due to the fact that they all are founded on the same observations, meaning that they all share a common basis data:

> [...] Quilibet Astronomorum, quamvis peculiarem astrorum theoriam, suam quisque animo conceperit; singulorum tamen theoriae ab una eademque corporum coelestium constanti observatione prodierunt; nam quicquid observavit unus, observavit etiam & alter[51].

Thus, there can be different models because numerous ways of interpreting reality are possible, but data and observations on which these interpretations are built – if correctly carried out by the practise of natural histories – should be the same for everyone. This is what assure their applicability to reality. According to Baglivi, the same applies to medicine. Therefore, when formulating hypotheses about diseases, physicians should follow astronomers:

> Hoc efficient, si prius idioma didicerint, quo natura loquitur, & modos quibus eadem exprimitur, id est longam in observandis

[51] *PM* I, 12, §7, p. 106.

> juvantium, & laedentium eventibus exercitationem. Postquam igitur per diuturnas observationes innotuerit tale quid in aliquo morbo frequenter, ac perpetuo tali modo succedere, tunc mens assurgat ad formandam hypothesim, eamque stabiliendam super praedictos constantes naturae motus in hoc, aliove morbo tum producendo, tum sanando[52].

Clinical hypotheses are mere heuristic "tools", whose main aim is not to provide a description of the diseases, but to make sense of them – «rationem reddere phaenomenon in morbis apparentium» – so that the indications for treatment can more easily be inferred. Since they are constructions of the mind, hypotheses should be modified once they stop properly representing reality. The goal of a good physician is then to formulate as much as possible strong and long-lasting hypotheses, that are representative of what really happens in nature. This obviously depends on the degree of accuracy of the observations and on some necessary requisites, which can be so resumed:

1. First of all, clinical hypotheses should be the result of accurate observations by means of the compilation of natural histories of diseases;
2. Hypotheses should be abandoned not only when they "move away" from nature, but also when they no longer get the expected results;
3. Finally, good hypotheses are grounded also upon some general and self-evident principles – namely, figure and motion.

This explains why so different hypotheses, provided they meet these rules, will have the same effect in a disease treatment, just like in astronomical systems.

4. *On Hypotheses in Medicine: William Cole's Letters to Locke and Baglivi*

Contrary to a widely and common interpretation of Locke – which is well represented by Laudan and Farr, for example[53] – Anstey has

[52] *PM* I, 12, §8, p. 107.
[53] See L. Laudan, *The Nature and Sources of Locke's Views on Hypotheses*, «Journal of the History of Ideas» 28/2 (1967), pp. 211-223; J. Farr, *The Way of Hypotheses: Locke on Method*, «Journal of the History of Ideas» 48/1 (1987), pp. 51-72.

recently argued for the importance of natural history in Locke's natural philosophy, having hypotheses (and analogical reasoning) only a secondary role[54]. Hypotheses indeed would serve rather as mere aids to memory. As we have seen in §1, Locke's manuscripts, during the close collaboration with Sydenham, seem to support this view.

The same issue is addressed in the correspondence with William Cole (1635-1716), an English physician that, although being close to the Sydenham's circle, did not share the same view on hypotheses. Indicatively, Haller refers to him as «jatromathematicus & hypothesium inventor» in his *Bibliotheca medicinae practicae*[55]. Cole's first work, *De Secretione Animali* (1674), tries to explain secretion in mechanical terms, following a completely conjectural and even more speculative approach. It leaves no doubt, moreover, the fact that the term "hypothesis" clearly appears in the title of a work on fevers: *Novae Hypotheseos ad explicanda febrium intermittentium symptomata...Hypotyposis* (1694)[56].

On 11 June 1690, Cole wrote to Locke, replying to an earlier letter that unfortunately has been lost but that probably was also concerned with the problem of hypothesis in medicine. After having reassured Locke about the state of his health, Cole specified his conception of hypothesis that most likely reflected that of his correspondent:

> And now to looke back a little to the former part of your letter. As I thinke no Hypothesis allowable which goes not upon such (mechanicall) grounds as the subject is, without straining, in an easy and obvious chaine of thoughts, capable to beare; so all ought, as much as is possible, to be eyther built on Experiment, or be tryed by it; and if that confirme them not, when it can come in to be consulted, they ought to be slighted. But, since every thing has a reason of its being so or so, I thinke that Physitian is very much to blame who will content himselfe, without particularly enquiring into it (so farre as the matter will beare, and his Talent goes) to advise medcines upon generall rules, which can never square to all cases, the circumstances and complication of diseases and symptoms being so various, and thence a variation of Indications so necessary; so that tis requisite a man dig deepe to lay a good

[54] P. Anstey, *John Locke and Natural Philosophy*, Oxford University Press, Oxford 2011, ch. 4.
[55] A. von Haller, *Bibliotheca medicinae practicae qua scripta ad partem medicinaæ practicam fascientia a rerum initiis recensentur*, Tomus III: ab anno 1648 ad a. 1685, apud Em. Haller, Bernæ; apud Joh. Schweighauser, Basileæ 1779, p. 362.
[56] On William Cole, see J. F. Payne, *Cole, William (1635-1716)*, in *Dictionary of National Biography*, Smith, Elder, & Co., London 1887, vol. 11, pp. 277-278.

> foundation to built a judgment on; and, according to the Maxim, Dolus latet in Universalibus[57].

Two aspects are worth highlighting. First of all, hypotheses should be grounded on a strong empirical evidence by performing experiments: when this does not happen, they should be rejected. Secondly, physicians should be advised against recommending remedies based on too general rules. A high level of generality cannot cover all cases, due to the variation of diseases and symptoms, and the individual characteristics predisposing each patient. A good empirical foundation is required for building valuable clinical judgments.

Interestingly, nine years later, Cole took up the same issue in a letter to Baglivi dated 8 August 1699, by commenting on the claims about hypotheses in *De praxi medica*, namely in the preface and the twelfth chapter of the first book[58]. While recognising some limits of hypothetical reasoning, Cole accused Baglivi of having too brutally treated those physicians using it in their work. In fact, the effort of formulating hypotheses «tum pro eruendo morborum, quandocunque in praxi contigerint, [...], tum pro prognosi et curatione apposite ad rem praesentem instituendis»[59], once performed the necessary observations, cannot be condemned. Rather, for Cole it may be even more dangerous for a physician to act on the basis of inaccurate and incomplete observations, without being guided by a research hypothesis:

> Quin, pace Doctissimi, mihique multum celebrandi Viri, interrogare liceat, num observationes ex re praesenti habitae, nullaque vel non satis plena, circumstantiarum, ut pote temporis morbi, aetatis, constitutionis, et (quale quid in ipso magno Hippocrate quandoque desideratur) notatione, chartis mandata, lectores hypothesibus et methodo destitutos in errores ducere possint[60].

Cole believes that the hypotheses built on mechanistic principles, of which he himself makes use in his works, are more likely to better respond to the morbid phenomena.

Baglivi replied to these objections months later, in a letter dated

[57] W. Cole's letter to J. Locke, 11 June 1690, in E. S. De Beer (ed.), *The Correspondence of John Locke*, 8 voll., Clarendon Press, Oxford 1979, vol. 4, letter no. 1299, pp. 89-99, p. 91.
[58] W. Cole's letter to G. Baglivi, 8 August 1699, in Waller, no. 20, pp. 67-70.
[59] Ivi, p. 68.
[60] *Ibidem*.

4 January 1700, explaining that the use of hypotheses is necessary and legitimate only when is founded «super geometrico mechanicas regulas»[61]:

> Verum qui librum attente pervoluat, inveniet me caput peculiare de hypotesi recte construenda conscripsiste; et inter hypotheses, quae unquam excogitatae antea sint, vel impostenum essent produturae, principem locum tribuisse hypothesi super geometrico mechanicas regulas stabilitae; quam non temporis livor, non hominum turbulentum Ingenium delere poterunt, sed adinstar Ignis externa vi coacti, [...] cum impetu depressa exurget, et ad longam durabit posteritatem. Ecquid enim Vir Celeb.me mathematice certius? Ecquid solidus medicina, mechanica methodo explicata?[62]

Baglivi is evidently referring to those self-evident principles, such as figure and motion, which by definition do not need to be proved and which represent a reliable basis for clinical hypotheses. Thus, Cole has nothing to worry about: «Te igitur quoniam omnes mechanicorum in re medica principem salutant, puto meum libellum offendere non potuisse cogitationes tuas, quas in aureis libri de secretione animali prodidisti»[63].

Conclusions: «Medicina prima» comes first!

It might seem hard to understand how Baglivi's attack on the dissemination of abstract systems of medicine could be at the same time compatible with a some form of hypothetical reasoning, even if in a instrumentalist perspective. But Baglivi's view on hypotheses depends on a more important distinction – which remarkably characterizes his medicine – that is the difference between «medicina prima» and «medicina secunda»[64].

«Medicina prima» means an accurate description of the morbid

[61] G. Baglivi's letter to W. Cole, 4 January 1700, in Waller, no. 28, p. 85.
[62] Ivi, pp. 85-86.
[63] Ivi, p. 86.
[64] On this distinction, see R. Andrault, *What Does it Mean to Be an Empiricist in Medicine? Baglivi's Praxis Medica (1696)*, in A.-L Rey-S. Bodenmann (eds.), *What Does it Mean to Be an 18th Century Empiricist? Construction and Circulation of a Pluralistic Concept*, Springer, Cham 2018, pp. 169-188.

state, which is the first step in the cognitive processes involved in clinical decisions. The "descriptive level" precedes any support from other disciplines similar or external to medicine, i.e. from all branches of knowledge that complete medical education and represent the so-called «medicina secunda». But it takes priority over any matter, because it implies the practice of *historia naturalis*, which is a «scientia sui generis» deriving its principles exclusively from nature, without depending on anything else.

For Baglivi, just as for Bacon, natural (and experimental) history is not only a means for organizing data, but constitutes also the "matter" – therefore, it comes "first" – for induction. However, although it is independent from the other sciences, «medicina prima» cannot by itself produce any clinical judgments and determine the most effective curative indications to treat diseases. It needs the support of the other sciences. Similarly, it needs also the formulation of hypotheses, provided they are well formed, in order to better process sensory data.

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