MAX-PLANCK-INSTITUT FÜR WISSENSCHAFTSGESCHICHTE
Max Planck Institute for the History of Science

2002

PREPRINT 222

Conference

A Cultural History of Heredity I: 17th and 18th Centuries

ISSN 0948-9444

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Introduction

This volume assembles some of the contributions to the workshop "A Cultural History of Heredity I: Seventeenth and Eighteenth Centuries" which took place at the Max-Planck-Institute for the History of Science MAY 24-26, 2001.

The workshop was the first in a series of workshops dedicated to the cultural history of heredity. There are a number of histories of genetics written from the perspective of history of ideas. François Jacob's *La logique du vivant* (1970; engl. transl. 1973) and Robert Olby's *Origins of Mendelism* (1966, 2nd ed. 1985) have certainly set lasting standards in this field. There are also some sophisticated, far from whiggish histories written from a disciplinary perspective, like Hans Stubbes *Kurze Geschichte der Genetik* (1963; engl. transl. 1972), Leslie Clarence Dunn's *A short history of genetics* (1965), and Elof Axel Carlson's *The gene: a critical history* (1966), to name just a few. What is missing, however, is a comprehensive study that embraces the cultural history of heredity by presenting the knowledge of heredity in its broader practical and historical contexts.

In our project we wish to focus on the scientific and technological procedures, in which the knowledge of heredity was materially anchored and by which it affected other cultural domains. Such a project will be content neither with conventional history of ideas nor with mere social history. It will rather explore the various practices, standards, and architectures of hereditary knowledge and the "spaces" which they formed by their respective historical conjunctions. "Heredity", under this perspective, is more than the scientific discipline "genetics". The project is less about the history of a science than about the history of a broader knowledge regime, in which a naturalistic conception of heredity developed historically that today affects all domains of society. This knowledge regime dates back to the social illusions and illuminations of the Enlightenment. What does it mean that nature determines history so that it appears as if history could be controlled by nature? And: are we today, with the advent of gene technologies, witnessing the end of a deterministic world view, or are we confronted with its definite restoration?

A project like this is vitally dependent on the participation of experts from a broad range of disciplines, covering cultural history in its various subdomains of science, technology, medicine, politics, economy, law, literature, and art. It will be pursued in a series of workshops, each focussing on a loosely defined "epoch" characterised by a certain development in hereditary knowledge.

The first workshop, organized by *Hans-Jörg Rheinberger*, *Peter McLaughlin* and *Staffan Müller-Wille*, concentrated on the late seventeenth and the eighteenth century and assembled historians of science, medicine, politics and literature from the United States, Mexiko, Germany, Switzerland, and Italy. Four poapers presented at this workshop make up this volume. Discussion during the workshop turned around two main questions: 1), if a concept of heredity existed at all in this period; and 2), in how far 18th century theories of generation were guided by empirical experience.

In regard to the first question, several contributions could show that there was no general concept of heredity underlying the discourse of the life sciences (Fantini, Terrall). However, there did exist some isolated, well-defined and sometimes, especially in breeding and medicine, highly localised fields structured by the recognition of hereditary transmission of differential characters in the 18th century – the definition of specific difference in natural history (Müller-Wille), the explanation of hereditary diseases in pathology (Lopez-Beltran), political organisation of colonial societies according to racial characteristics (Mazzolini), and the application of hybridisation in plant (Ratcliff).

In regard to the second question, the workshop disclosed a rich spectrum of theoretical approaches to generation in the 18th century and made clear that this diversity is only insufficiently captured by the conventional dichotomy of preformation vs. epigenesis (Fantini, Terrall). This spectrum, however, was rather determined by different positions in regard to the politics and poetics of production, both experimental and social, than by a secured and well-defined domain of empirical data (Müller-Sievers, Roe, Terrall).

Future workshops in the series follow a very rough chronological order. Their thematic structure will be developed from the results reached in the preceding workshop. The folllowing list of planned workshops, therefore, indicates in a very preliminary way how the project will proceed:

Eighteenth to Nineteenth Century: Heredity Becomes Central (January 10-12, 2003)

This workshop will focus on the period demarcated by the publication of Kant's *Von den verschiedenen Rassen der Menschen* 1775 and Darwin's *The Variation of Animals and Plants under Domestication* 1868. This "epoch" in the cultural history of heredity was characterised by a decisive development at the end of which stood the establishment of "inheritance" as one of the central problems of the life sciences. Parallel to this shift one can observe conceptual displacements: Heredity began to be conceptualised as a relation between parental and filial dispositions, rather than between over-all constitutions. Accordingly, theory formation began to revolve around the production and combination of traits within a species rather than around its over-all morphology. Finally, adaptation (or degeneration) under changing conditions, rather than a pre-established balance of nature, became its general framework. Four fields have preliminarily been identified in which these displacements took place: natural history, breeding research, medicine (including psychiatry), and anthropology.

Nineteenth to Twentieth Century: Heredity Becomes Exact

This workshop will cover the period from Galton's *Hereditary Genius* to the formulation of a thoroughly mathematical populations genetics in the 1930s. Population thinking and statistics replace the taxonomic regime of race and character, that previously provided the conceptual framework for hereditary knowledge. The historical background for this development can be seen in the social transformations triggered by industrialisation and concurrent eugenic visions of regulating and controlling populations, including their notorious 20th century versions.

Early Twentieth Century: Heredity Becomes Molecular

Molecular genetics did not immediately result from the developments characterised in the previous workshop, but from a plurality of methodological achievements in other biological domains, few of which had to do with genetics. The result of these conjunctions, however, was a further naturalisation of hereditary thought with the "cracking" of the genetic code. Heredity was brought down from the structural level of populations and genetic traits to a material level of molecules capable of being “read”, “copied”, “translated”, and “transmitted”, be it in human bodies, be it in laboratories. The question of genetic determination and control thus gained renewed prominence after the demise of eugenics.

Late Twentieth Century: Heredity Becomes Technological

The field of heredity was again thoroughly transformed with the advent of “gene technologies”. Potentially all areas of social life are affected by these practices, and their application in medical and industrial contexts open up perspectives whose limits are typically not foreseeable. Gene patenting, DNA-fingerprinting, gene therapy, and cloning are areas hotly debated in this regard. For the discipline, moreover, the question arises how future research will be oriented after the sequencing of large genomes has become routine work. With the *longue durée* historical perspective provided by the previous workshops we hope to gain innovative, even surprising outlooks on these problems of the day.

Hans-Jörg Rheinberger
Peter McLaughlin
Staffan Müller-Wille

Cabbage, Tulips, Ethiopians – "Experiments" in Early Modern Heredity

Staffan Müller-Wille

0. Introduction

Genealogy is the oldest logic. Not only do the key concepts of ancient logic – *γενοσ* and *ειδοσ* – have genealogical connotations, but even the way in which these concepts were seen to relate deductively was modelled on genealogy: *ειδοσ* and *γενοσ* relate in the same way "as Agamemnon is an Atride, a Pelopide, a Tantalide, and finally of Zeus", as it says in Porphyry's *Isagoge* to Aristotle's *Categoria*.[1] Some identity persists in descending genealogies, and thus things can be inferred for individuals by retracing their origin (*αρχη* or *principium*, again a term with a genealogical connotation) through the chain of their antecedents.[2]

And yet, the old world, as is well known, was a world full of monstrous births, strange transmutations and unnatural copulations. Though Aristotle, e. g., repeatedly maintained that "a man is generated by a man",[3] he equally conceded the possibility that "two animals different in species produce offspring which differs in species; for instance a dog differs in species from a lion, and the offspring of a male dog and a female lion is different in species".[4] And still in 1690 we see no one less than John Locke resuming in his *Essay concerning human understanding* that he "once saw a Creature that was the Issue of a Cat and a Rat, and had the plain marks of both about it."[5] As fantastic as this promiscuity of nature – in which every combination seemed as plausible as possible – may seem to modern eyes, it rested on a rational ground: The foundation for the similarity among parents and offspring was provided by the recurrence of similar physiological and climatic conditions during procreation and development, which, inversely, meant that any deviation from this ordinary course of things – e. g. mesalliances as the ones referred to by Aristotle and Locke – would produce as deviant results. "All things are governed by law" is the conventional translation for the opening sentence of a Hippocratic tract *De genitura*.[6] Yet, it is worthwhile to consult its Renaissance Latin translation: "*Lex quidem omnia corroborat*" – "Law strenghtens" – the original indeed has "*κρατυνει*", which signifies both strengthen and govern – "everything", with "law (*νομοσ*)" – as the commentator Girolamo Mercuriale carefully noted –

1 Porphyrius (1887), 2b1-4, p. 6. The full text in its Latin translation by Boethius (ibid, p. 31) is: "ea vero quae sunt ante specialissima usque ad generalissimum ascendentia et genera dicuntur et species et subalterna genera, ut Agamemnon Atrides et Pelopides et Tantalides et ultimum Jovis." Cf. Porphyrius (1998), I 1-3, II 9.

2 Cf. Heinrich (1981), pp. 98-100, also Gayon and Wunenburger (1995), p. 8: "[...] la valorisation des filiations historiques, des ascendances et descendances d'un être, a longtemps servi de vecteur quasi unique d'intellegibilité canonique".

3 E. g. Aristotle *De gen. anim.* 735a20. Cf. Lesky (1950), p. 139, for further references and discussion.

4 Aristotle *De gen. anim.* 747b33-36. Aristotle refers to this as an "abstract argument" in the discussion of mules, which are infertile, yet, as he himself states (ibid 748a16), he gave a wealth of examples for such "hybrids" in both *De generatione* and *Historia animalium*. Cf. Zirkle (1935), pp. 15-17.

5 (Locke ([1690] 1975), p. 451, §23. For a rich account on such "hybrids" through history see Zirkle (1935), for iconographical evidence also Daston and Park (1998).

6 E.g. Lloyd (1978), p. 317. Cf. the French translation – "La loi gouverne tout" – in Hippocrates (1851), p. 471.

meaning “customs, pasture, region, class (*instituta, pascua, regionem, classem*)” in the dialect of the Hippocratic texts.[7] Genealogy was seen to consist less in relations resulting from the lawful transmission and redistribution of some hereditary material but rather in the local persistence of economical, political and social configurations, in the persistence of a “fabric”– as Claude Lévi-Straus has formulated it – “in which warp and filling yarn correspond to localities and tribes”.[8] In this context, heredity was at once something trivial and precarious: It was stabilised and reinforced by the persistence of its own, municipal bounds (local rules of marriage and residence), and yet remained infinitely open to disturbances by illicit transgressions of these bounds.[9]

It is, I presume, behind this background that we must see the achievements of the 18th century in regard to the formation of a modern concept of heredity. Peter Bowler has rightly argued, I think, that one important condition had to be fulfilled, before one could speak of such a concept: a clear distinction had to be drawn “between the transmission of characters from one generation to the next and the process by which the characters are produced in the growing organism”.[10] As an indication of how difficult it is, indeed, to see this, I take it that one 18th century theory of organic reproduction, in which this precondition seems to have been fulfilled – if only to a certain degree, which is the very topic of this paper – has consistently been underrated, if not simply overlooked, by historians of 18th century theories of organic reproduction.[11] I speak of the theory of organic reproduction of Carolus Linnaeus.

1. Laws of generation

One of the reasons for the neglect of this theory by modern historians may be the place and the form in which it first appeared: Linnaeus laid out his theory not – as one might expect – in a coherent tract on the physiology of generation, but in a set of short aphorisms introducing the first editions of his famous taxonomic works, the *Systema naturae* of 1735 and the *Genera plantarum* of 1736.[12] Moreover, it did not enter the scene as an independent account on the mechanisms of propagation, but was contained in the definition of a central category of Linnaean taxonomy: the species. Let us first have a close look at this definition:

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7 Hippocrates (1588), p. 10 & 15. Cf. Stubbe (1965), pp. 18-21. I thank Volkmar Schüller and Friedrich Steinle from the Max-Planck-Institute for the History of Science, Berlin, for discussing this passage with me.

8 Lévi-Strauss ([1962] 1969), p. 97.

9 Cfr. ch. 2 “La logique des classifications totémiques” in Lévi-Strauss (1962), pp. 48-99, who explicitly subsumes “the naturalists and hermetics of antiquity and the middle ages: Galen, Plinius, Hermes Trismegistos, Albert the Great” (ibid, p.57) under his analysis.

10 Bowler (1989), p. 6.

11 Lippmann (1933), pp. 49, 63 & 78, Cole (1930), pp. 18 & 137 and Roger ([1963] 1993), p. 322 mention Linnaeus only in passing. Ritterbush (1964), pp.101-103, though discussing Linnaeus’ theory of (plant) reproduction at length, tries to side him in the debate between “ovists” and “pollenists”, an opposition Linnaeus discarded, as we shall see. Delaporte (1983), pp. 124-127 discusses the anthropomorphism of Linnaeus’ theory of reproduction, but otherwise misses its idiosyncrasies. Interestingly, and rightly so, as we shall see, Linnaeus is compeletely missing in François Duchesneau voluminous study of 18th century physiology.

12 Far from reducing the impact of Linnaeus’ theory of reproduction, this place rather had the effect that it “swept all of Europe and North America”. Cf. Farley (1982), p. 5.

> There are as many Species as different forms produced by the Infinite Being in the beginning. Which forms afterwards produce more, but always similar forms according to inherent laws of generation; so that there are not more Species now than came into being in the beginning. Hence, there are as many Species as different forms or structures of Plants occurring today, those rejected which place or accident exhibits to be less different (varieties).[13]

This short, and in its brevity so concise, passage has been endlessly quoted as indicating Linnaeus's adherence to a "typological" species concept.[14] And yet, as a close reading reveals, it does not so much tell us, *what* species are, but rather *how many* there are *on the assumption of certain "laws of generation"*. If we analyse Linnaeus's species definition with regard to these laws, three peculiar *absences* come to the fore, which on the one hand set off this theory from contemporary theories of generation and on the other hand indicate its propensity towards the modern concept of heredity:

1. The subjects of Linnaeus's "laws of generation" are not substances – seminal principles, moules interieures or the like – but "forms or *structures*". "*Structura*" – in contrast to the much more wider term "*forma*", which the definition uses synonymously – means something highly specific in Linnaeus's terminology: It designates the totality of characters differentiating the members of a species from all other species according to "all its parts [...] in four dimensions: number, form, proportion, position".[15] The similarity relation posited among parents and offspring "according to inherent laws of generation" is one of mere structural analogy, not of substantial contiguity. There are two peculiar consequences to this: First, that the traditional "three tier" set-up of the problem of heredity – heredity of generic, sexual and individual traits – does not play, as far as I can see, the least role in Linnaeus's writings on organic reproduction.[16] Sexual and individual differences – ontogenetic as well as intraspecific differences, e. g. in the number of certain organs – simply collapse with generic differences as they become differences within one and the same structural whole.[17] And second – as is also clear from the stress put on the number, rather than the essence, of species –, that the order reigning between species is not a contiguous "scale of being" but a simple juxtaposition of discrete entities, without overlaps or intermediates.[18]

2. The second absence within Linnaeus's theory of reproduction is the absence of "physiology". If one follows the "laws of generation" referred to by the definition, up to their full explication in the

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13 Linné (1737), Ratio operis §5, [p.2]: "*Species* tot sunt, quot diversas formas ab initio produxit Infinitum Ens; quæ deinde formae secundum generationis inditas leges produxere plures, at sibi semper similes, ut Species nunc nobis non sint plures, quam quæ fuere ab initio. Ergo Species tot sunt, quot diversæ formæ seu structuræ Plantarum, rejectis istis, quas locus vel casus parum differentes (Varietates) exhibuit, hodienum occurrunt."

14 For the locus classicus of this interpretation see Mayr (1957).

15 Linnaeus (1736), §92, p.11. Cfr. ibid, § 326, p.32.

16 Olby (1966), p. 1, describes this set-up, which, e. g., was still shared by Buffon, as one of the main obstacles on the way to the modern (Mendelina) concept of heredity.

17 Thus Linnaeus designated sexual dimorphism as a "natural variety (varietas naturalis)"; Cf. Linnaeus (1736), § 308, p. 30. A particular good example for the collapse of generic, sexual, and individual differences is provided by the description of the "structure" of the genus *Urtica* in the *Genera plantarum*, where one line says: "Female flowers, either in one or two different plant individuals". (Linnaeus 1737), p. 283. This collapse is why Linnaeus was prepared to accept "constant varieties (varietates constantes)" – i. e. distinct and constantly reproducing forms within one and the same species –, in later writings on the reproduction of plants (see Linnaeus ([1755] 1788), pp. 380-383). I will come back to that in the section on "Ethiopians".

very first two paragraphs of the *Systema naturae* of 1735, it becomes evident, that the substrate of the structural similarity posited between ancestors and descendants is made up of purely genealogical relations. All in all, these two paragraphs formulate three basic laws: a) that "individual living beings are propagated by the egg (*viventia singula ex ovo propagari*)"; b) that "each egg produces offspring similar to the parents (*omne ovum producere sobolem parenti simillimam*)"; and c) that "individuals are multiplied by generation (*ex generatione multiplicantur individua*)".[19] Translated into negative statements, the first two "laws" reject any possibility for spontaneous generation or transmutation: No generation can occur outside the context of genealogical relations and no generation can transgress the bounds of this context. The only change that does occur is one in the number of individuals. That "*ovum*", the fertilised egg, does not represent a physiological unit (in the sense, e. g., in which it does in William Harvey's work), but simply that entity which mediates generations of parents and offspring, can be inferred from another paragraph of the introduction to the *Systema naturae*, where Linnaeus states that "natural bodies are made up of elements, but in a way inexplicable except for creation and laws of generation."[20] Consequently, Linnaeus did not side himself in the debate between animalculists/pollenists and ovists, but rejected both versions of preformation with the (basically genealogical) argument, that both parents, male and female, enter into the procreative act to leave traces in the offspring (which, of course, was known to preformationists) and that otherwise "it remains an obscure matter, now and once, how generation or fertilisation happen".[21]
3. The third absence in Linnaeus's theory of organic reproduction is explicit in his formulation of a species concept: "[T]hose [structures are to be] rejected which place or accident exhibit to be less different (varieties)." Though formulated in a rather awkward way, the message is clear: Linnaeus draws a distinction between structural differences distinguishing organisms on a generic (species) level and obeying the laws of generation, and structural differences distinguishing organisms on the level of individuals and being due to local, accidental causes – "climate, soil, heat, wind", as a later formulation of this distinction in the *Philosophia botanica* (1751) has it.[22] In modern words: Linnaeus drew a distinction between nature and nurture, the latter's effects being excluded from the realm of "laws of generation". Accordingly, and in stark contrast to contemporary theories of generation, Linnaeus's theory of reproduction, even in its later, more elaborated versions, does not show the least trace of the age old theories of pangenesis and "inheritance of acquired characters".[23]

I take all three abstractions – the absence of substance, the absence of physiology, and the absence of environment – to be hallmarks of the distinction of transmission and development

[18] That there are no intermediates between species and, by extension, genera is frequently stressed in Linnaeus's *Critica botanica*; see, e. g. Linnaeus (1737a), §224, p. 29. As is well known, Linnaeus rejected the "scala" in favour of the map as representing relations among generic entities (cf. Linnaeus (1751), §77, p. 27). For a discussion see Rheinberger (1986) and Müller-Wille (1999), pp. 89-97. Barsanti (1995), p. 35, considers the map, instead of the scale, to be the "image plus apte à acceullir une logique de filiation".

[19] Linnaeus (1735), §§1-2, [p. 1].

[20] Ibid, §7, [p. 1]: "Naturalia illa ex elementis constructa, licet modo, praeter creationem & leges generationis, inexplicabili." For Harveys concept of "ovum" as a fundamental physiological unit, which, e. g., also could effect spontaneous generation of living beings, see Jacob (1970), pp. 63/64.

[21] Linnaeus ([1746] 1749), p. 347: Quomodo fiat generatio, vel fecundatio, innumerae sententiae Physiologorum fuerunt; sed aeque ac olim obscura res est." Cfr. ibid, p. 349.

[22] Linnaeus (1751), § 158, p. 100.

which according to Bowler was vital for the formation of the modern (Mendelian) concept of heredity, with the important limitation, however, that in Linnaeus's theory of organic reproduction "transmission" did not consist in the redistribution of independent characters, but in the universal persistence of a totality of characters, i. e. of "structure" (I will come back to this in the end of my paper). To be sure, also, Linnaeus later supplemented his theory with a physiological model of generation, which, interestingly, however, hypostatised the abstractions inherent to the earlier formulation of "laws of generation": According to this model, all organisms consist of two, antagonistic substances: the inner "pith (*medulla*)", which is propagated via the maternal line, and has a "power of infinite multiplication" (in animals, this substance corresponds to the nervous system). And the outer "bark (*cortex*)", which is propagated via the paternal line and has the power to attract and conduct nutriments, thus nourishing and protecting the medulla, and thus controlling its growth. The medulla, to put it shortly, is a merely propagative, the *cortex* a merely distributive substance, and it is their balanced, yet inherently antagonistic interaction through which life is maintained and develops.[24] One of the earliest formulations of this physiological model in the *Philosophia botanica* 1751 shows, how closely it was tied to Linnaeus's peculiar theory of generation:

> The root [containing the pith] extends infinitely, until the integuments [i. e. the bark] break up at the top to form the flower, and the seeds develop as the continuation and the utmost end of vegetation. This seed falls down, sprouts, and sort of continues the plant at a different place. Thus similar offspring is produced as the tree produces the branch, the branch the bud, and the bud the plant; therefore the generation of plants is a continuation.[25]

As much as some of the assumptions within Linnaeus's theory of reproduction seem to be evident in the context of modern biology – as the rejection of spontaneous generation and transmutation, or the distinction of environmentally induced variation and "genetic" determination – at their time they were very strong claims. The strength of these claims, however, stood in a peculiar contrast to their empirical foundation. In the *Fundamenta botanica* of 1736, e. g., the sentence "*Omne vivum ex ovo*" is just said to be "repeatedly asserted by reason and experience and confirmed by the cotyledons".[26] If this already seems to be a rather weak, arbitrary, and ad hoc substantiation of the claim in question, even more so do Linnaeus's references to "experiences (*experimenta*)" in his first published essay on the reproduction of organisms, the

[23] See Zirkle (1946) for rich material of the occurrence of these beliefs in the eighteenth century. Linnaeus did speculate sometimes, that certain plant species had resulted from the prolonged exposition of other species to a different climate or to intense cultivation (cfr. Ramsbottom 1938). This was also the basis for his attempts to "acculturate" exotic plants like tea to the climate of Sweden (cf. Körner 1994). Linnaeus remained undecided, however, if these changes were not reversible, and thus just special cases of "varieties" (e. g. Linnaeus (1737a), §316, p. 255).

[24] See, e. g., Linnaeus ([1759] 1763). For a detailed discussion of this theory see Stevens and Cullen (1990).

[25] Linnaeus (1751), § 157, p. 99: "Radix extenditur in herbam inque infinitum, usque dum apice rumpantur integumenta in florem, formantque semen contiguum, ultimum terminum vegetationis; Hoc semen cadit, prognascitur, & in diverso loco quasi plantam continuat; hinc simillimam sobolem producit, uti Arbor ramum, Ramus gemmam, Gemma herbam; ergo Continuatio est generatio plantarum." Ibid, §79, p. 37, identifies the extending root with the medullar, and the integuments with the cortical substance.

[26] Linnaeus (1736), §135, p. 16: "Omne vegetabile ex ovo (134) provenire dictitat ratio & experientia, confirmant cotyledones."

Sponsalia plantarum of 1746: To modern eyes they just appear as a potpourri of chance observations, poorly designed experiments, just-so stories and circular reasonings.[27] Yet, a closer look at these empirical references is necessary to identify a historical space for the formation of Linnaeus's "laws of generation" and to reach a better understanding of their position in the cultural history of heredity.

2. Ethiopians

The bulk of the *Sponsalia plantarum* tries to identify the sexual organs of plants in analogy to animals, and employs morphological arguments for this aim. Among this, however, we find a few, which refer to what we today would call hereditary phenomena. One of them is especially intriguing, as it appeals to an experiential background, that was shared by the learned since antiquity: the "blended" character of children resulting from interracial "crossings" among humans of African and European origin.[28] Yet, Linnaeus did not leave it at a general reference to this experiential background, but quoted a particular example from literature in the following words:

> "A common Ethiopian detained in the Copenhagen prison and kindled with love for a maid, secretly slept with her. Pregnant from that, and after due time of bearing, was brought forth a child of male sex, which resembled the mother in the whiteness of skin all over the body, only the darker penis showing the kind of its father." Barthol. cent. 4. obs. 5. Which all evinces, that the beginnings of the coming fetus by no means lie hidden in one sex only.[29]

That Linnaeus chose to quote this instance, rather than to report a general experience, has certainly to do with the "empirical weight" gained by the authority of its source: The quote is from the *Historiarum anatomicarum rariorum centuria* which Thomas Bartholin (1606-1680), influential professor of medicine and theology at Copenhagen university, published in four volumes 1656 and 1657, each volume containing a *centuria* (a hundred) of "histories". This format, also used by Bartholin in his *Epistolarum medicinalium à doctis vel ad doctos scriptarum* 1663-1667, appears rather strange to modern eyes: Completely unrelated observations of various medical interest – dissection reports, observations of unusual phenomena, cures and receipts, clinical cases – follow each other without any attempt at a systematisation. The headings of the first ten histories – including the one quoted by Linnaeus – of the fourth *centuria* may illustrate this:

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27 Cf. Sachs (1875), p. 104-106, whose reading of the *Sponsalia plantarum* has long influenced the picture of Linnaeus as a poor empirist.

28 That such "crossings" were common in the Mediterranean of Antiquity is clear, and thus we find frequent references to them in the writings of ancient natural philosophy (e. g. in Aristotle, cf. Stubbe (1965), p. 21). How continuous these contacts remained even in Central Europe of the Middle Ages is discussed at length in Martin (1993).

29 Linnaeus ([1746] 1749), p. 349: "‚*Æthiops* cerdo in ergastulo Hafniensi detentus, amore puellæ servæ accensus, clanculum illam compressit. Gravida inde, legitimo partus tempore enixa est prolem virilis sexus, quæ matrem universo corpore cutis candore referebat, solus vero penis paternum genus nigrore commonstravit. Barthol. cent. 4. obs. 5.‘ Quæ omnia, rudimentum futuri fœtus neutiquam in uno tantum sexu delitescere, evincunt."

I *Anatome Civettae* (Dissection of a civet cat)

II *Prolapsus uteri cum urinae incontinentia* (Prolapsus with incontinence)

III *Convulsiones paralyticae* (Paralytic convulsions)

IV *Unicornu Groenlandicum* (Greenlandian unicorn)

V *Ex Aethiope natus* (Birth from an Ethiopian)

VI *Anatome Hominis sanis* (Dissection of a healthy man)

VII *Epilepsia ex vermibus* (Epilepsy from worms)

VIII *Calculus ex scroto suppuratu* (Stone gathered from a scrotum)

IX *Emplastra magna* (Large plasters)

X *Anatomi Monachi* (Dissection of a monk)

This "aphoristic" make-up was not used in deficiency of a system or theory, as Lorraine Daston has argued, but rather to avoid the pitfalls of speculation, to immunise the gathering of observations from rash conjecture and system building to enhance their "facticity".[30] Yet this very format entailed a difficulty in regard to the empirical foundation of Linnaeus's theory of generation, which comes to the fore, if we compare Linnaeus's quote with the original text of Bartholin:

> A common Ethiopian detained in the Copenhagen prison and kindled with love for a maid, secretly slept with her. Pregnant from that, and after due time of bearing, was brought forth a child of male sex, which resembled the mother in the whiteness of skin all over the body, only the darker penis showing the kind of its father, which several people eye-witnessed and wondered at. I assign this wholly to the imagination of the mother, which seizing the desired part with a fixed and vigorous mind, impressed its colour on the offspring. Of mixed colour are children otherwise brought forth from an Ethiopian and a white, which often shows us, how both sexes have their separate commands over generation.[31]

What Linnaeus leaves out in his quote is what is interesting to Bartholin, while what Linnaeus tries to prove by his quote is what Bartholin takes for granted, as something known anyway. Bartholin concentrates on those features of the case reported – the fact that the colours do not blend in the child, as is usual, but remain seperated – which single it out as a singular case. And consequently, he is looking for singular circumstances in this case (the passion of the mixed couple, which, as it seems, was looked upon as something unusual by Bartholin) in search for an explanation. Only by leaving out this argument, and by reformulating the general conclusion, could Linnaeus adapt Bartholin's observation to his theory of generation in which a combination of structural

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30 Cf. Daston (1998).

31 Bartholinus (1657), pp. 220-221: "Æthiops Cerdo in Ergastulo Hafniensi detentus, amore puellæ servæ accensus, clanculum illam compressit. Gravida inde legitimo partus tempore enixa est prolem virilis sexus, qvæ matrem universo corpore cutis candore referebat, solis verò penis paternum genus nigrore prodebat, qvod oculati testes plures & viderunt & mirati sunt. Imaginationi matris id universum assigno, qvæ partem vehementis desideratam animo fixo comprehendens ejusdem colorem fœtui impressit. Mixti |221| aliàs coloris solent fœetus ex Æthiope & alba procreari, qvod sæpe nobis visum, si qvidem uterq; sexus divisum in generatione imperium habet."

differences in the offspring rather was to be expected than posed a problem, and in which such developmental causes as “maternal imagination” did notably not play a role. Quite in line with this, the problem that Linnaeus saw in the well known case of “Ethiopians” was not to explain that and how their skin colour persisted, but rather that they possessed a distinctive character – black colour – that remained constant even under varying climatic and geographic conditions, while they nevertheless doubtlessly belonged to the same species as other humans: “Who would deny that the Ethiopian is of the same species as we humans”, as it says in a paragraph discussing difficulties in distinguishing varieties from species in the *Critica botanica* (1737), “and yet the Ethiopian brings forth black children in our countries.”[32]

What this shows, is that something more than isolated observations of hereditary phenomena had to enter the scene before Linnaeus’ could formulate his “laws of generation”. These laws, as we saw, transcended the bounds of individual parentage, and only in hindsight could the numerous isolated instances of inheritance reported in medicine and natural history appear to Linnaeus as approving his laws. What is missing is an empirical context in which the hereditary relations constitutive of Linnaeus’ theory of organic reproduction were actively implemented to reach beyond the exceptional and the ordinary course of things. The problem of the historical formation of this theory is one of synthesis, of how to knit together hereditary phenomena to form a network of relations rather than a number of individual cases.[33]

3. Tulips

For understandable reasons, this problem of synthesis could not be solved in anthropology. A much better candidate for that would have been plant and animal breeding, and the *Sponsalia plantarum* indeed abound with examples taken from this ancient realm of technology. A particular interesting example, as it expressly referred to an “experiment”, is the following:

> TULIP. Delightful is this horticultural experiment: If someone perchance rejoices in completely red tulips and tears out the anthers from some flower before the pollen is scattered, and afterwards takes a tulip with a white flower and sprinkles the other, red one’s stigma with its anthers; and finally puts the ripe seeds in their own bed, he will obtain in this bed some red, some white, and for the greatest part two coloured flowers, no less than variously coloured offspring is produced from two animals of different colour.[34]

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32 Linnaeus (1737a), §316, p. 255: “Quis neget æthiopes esse ejusdem speciei ac nos homines, tamen æthiops nigros procreat infantes, in nostra terra”. This problem of “constant varieties” became more and more pressing for Linnaeus, resulting in the development of a theory of hybridization to account for the rise of new species and varieties from 1751 on. Cf. Müller-Wille (1998). For Linnaeus’s classification of human races according to skin colour, which he already presented in his *Systema naturae* of 1735, see Sloan (1995).

33 The “aphoristic” style of Bartholin resembles that of the “consilia”, a collection of court cases which was posthumously added to Paolo Zacchias’ *Quaestiones medico-legales* 1621-1650. This latter publication contained, among other topics, a systematic exploration of ancient and early modern theories about “the similarity and dissimilarity of children (*De similitudine & dissimilitudine natorum*)”, reaching the conclusion that both male and female have “equal potentials” in the “process of generation” (see Bajada (1988), pp. 23-60, who calls the *consilia* a collection of “experiments”; ibid. p. 31). This forensic context, in which Bartholin was active too (see *Dansk Biografisk Leksikon*, Copenhagen 1979, vol. 1, pp. 475-6) and which may very well have been a source of “synthesis” of individual cases, could not be explored in this paper.

The "experiment" Linnaeus recounts here came from a well known historical background: Not only had the trade in tulip varieties – after their introduction to Europe by the Turks in the early sixteenth century, Matthias L'Obel already could include illustrations of 20 different sorts in his *Kruydtboeck* of 1581 – been responsible for the first emergence of widespread, public option trading and ensuing market crash in the Dutch "tulip craze" 1633-1637;[35] this trade also sparked off an intense activity of breeding new varieties of tulips which survived the "craze".

But far from praising this activity as giving insights into the "laws of generation", we see Linnaeus complaining about it elsewhere, namely in his *Critica botanica,* curiously enough invoking just the example of tulips:

> The prime reason why [our] precursors came out with wrong species names only consisted in that they refused to distinguish natural characters and parts, or certain ones from sportive ones. As they accepted all characters, accidental and natural ones alike, they erected new species from the most insignificant character, from whence so much confusion, such a barbarity of names, such an accumulation of wrong species, that it were easier to clean the stable of Augias, than that of Botany. [...]
>
> Certainly, if each character would equally constitute new species, there would be no wiser and accurate Botanists among mortals than those FLOWER-LOVERS, who each year in tulips, primroses, anemones, daffodils and hyacinths alone present to the curious thousands [of plants] unknown to the Botanists, and hence new species. [However:] The Omnipotent Builder of Creation stood off from work on the seventh day, so that there is no new creation each day, but a continuous multiplication of things once created. He created one human, as the Holy Scripture teaches: but if the smallest character were enough, there would stand out thousands of human species today; there stand out namely [those] with white, red, black, doggish [?] hair; with white, pink, brown, black face; with erect, short, curved, snub, aquiline nose; there stand out giants and pygmies, fat and thin, straight and bowed, leprosic [?] and lame people etc. etc. Yet who would ever lightheadedly [?] call them different species? You see, therefore we assume certain characters, and look for the deceptive ones, which lead astray and do not change the thing. [... e. g.], Tournefort counts 93 Tulips (where there is only one) and 63 hyacinths (were there are only two), and no less do others sport in others.[36]

But not only do such "flower-lovers (*anthophili*)" as Tournefort – who in fact was counted among the greatest botanists in Linnaeus's time – burden botany with trivial distinctions and wrong species; Linnaeus even believed that they were to count as no botanists at all:

> Flower-lovers and Botanists have the same objects in varieties, however, with the difference, that the Flower-lover enters the scene, where the Botanist leaves it. The latter, sort of weary of it, sets an end to the work; the former, vigorous, begins to build that he may reach the stars.[37]

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34 Linnaeus ([1746] 1749), p. 370: "TULIPA. Jucundum est horticulturæ experimentum: si forsan rubris tantum gaudeat Tulipis, in flore aliquo antheras omnes decerpat ante pollinis dispersionem, assumat deinde Tulipam flore albo hujusque antheris stigma alterius rubræ aspergat6; maturis deinde seminibus, eadem in areolam propriam projiciat, & in hac areola flores reportabit, alios rubros, alios albos, bicolores plerosque ceteros, haud secus ac ex duobus animalibus diversi coloris, fœtus variis decoratus coloribus producitur." Examples from animal breeding – mules, hens, dogs and sheep – are listed on p. 349.

35 Cf. Jessen (1864), pp. 256-257, Kulischer ([1929] 1988), pp. 319-320, Zirkle (1935), p. 88.

And, as a comment to his own, consequent dictum, that the "botanist does not care for those fortuitious monstrosities and varieties (*casuales monstroisitates varietatesque [...] non curat Botanicus*)", Linnaeus added in obvious allusion to his species concept:

> The number of species of Botanists remains the same, now or in the future, as when they poured out of the hand of the Omnipotent Creator. Of the Flower-lovers, however, new and different ones are produced each day from the species (as the Botanists call them), and descended from these they finally ruin them. To the former, therefore, [i. e. the species of Botanists], there have been set certain limits by nature, which cannot be transgressed; in the latter, however, there is an infinite sport of nature without end; the former's species come from the all-wise hand of the Omnipotent, the latter's varieties from the sport of nature, especially under the auspices of the gardeners. From hence the greatest difference between Botanists and Flower-lovers.[38]

I have quoted Linnaeus here at length, because it is not easy to see, why Linnaeus so polemically criticised the "*Anthophili*", who, after all, seem to have provided him with a "delightful experiment" to back his theory of organic reproduction. It is not a difference in "scientificity", as the mentioning of Tournefort as one of the proponents of the "*Anthophili*" proves.[39] It is equally not a difference in the realm of objects studied by Botanists and flower-lovers respectively: Varieties are said to be as much a topic for botanists as they are for Flower-lovers. The best way to describe the divide raised between botanists and flower-lovers by Linnaeus is to say that it is a difference of aspect: While the botanist is interested in *limits* inherent to the "continuous multiplication of things once created ", the flower-lover concentrates on quite the opposite, namely change and variety – the " infinite sport of nature without end" – and the *means to effect* these, notably by gardening technologies.[40]

That this is not just nit-picking reasoning to reach some social distinction for botanists, but has important consequences on the level of theorising can be confirmed by a glance at the content of seventeenth and early eighteenth century horticultural literature: Far away from expounding something like "laws of generation" it indulges in the multifarious means of effecting the

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36 Linnaeus (1737a), § 259, p. 152-155: "Primaria causa, cur nomina specifica Antecessorum fallacia evaserint, sola in eo consistit, quod partes & notas naturales, ac certas, a ludicris distinguere recusarint. Cum autem assumserint omnes notas, accidentales & naturales indifferenter, indeque constituerint ob minimum notam, novam speciem, orta fuit tanta confusio, tanta nominum barbaries, tanta specierum falsarum accumulatio, ut facilius stabulum Augias purgare, quam Botanicen. [...].
Certe si omnis nota indifferens novam constituat speciem, nulli mortalium Botanici Sapientiores & acutiores ANTHOPHILIS, qui in solis *Tulipis, Primulis, Anemonibus, Narcissis, Hyacinthis* omni anno aliquot millia Botanicis ignotas, novas proinde, species ostendunt curiosis. Desistebat ab opere Creationis Omnipotens Conditor die septima, nec nova creatio omni die, sed continuata multiplicatio creatorum. Hominem creavit unicum, dictiante S. Scriptura: at si minima nota sufficiat, vel mille hominum species hodie prostant; prostant enim capillis albis, rubris, nigris, canis; facie alba, rosea, fusca, nigra; naso erecto, brevi, inflexo, simo, aquilino; prostanbt gigantes, pygmæi, obesi, macilenti, erecti, incurvi, tophosi, claudi &c. &c. sed quis leviter ianus hos distinctas diceret species. En itaque assumamus notas certas, & inquiramus notas fallaces, quae seducunt, nec variant rem, [...].
Tournefortius Tulipas 93. (ubi una est) & Hyacinthis 63. (ubi duo sint) numerat, nec minus sæpe in aliis alii luxuriarunt."

37 Ibid, § 306, p. 238: "Anthophilorum & Botanicorum in Varietatibus objecta eadem sunt, ea tamen cum differentia, ut Anthophilus incipiat scenam, ubi Botanicus definit; hic, dum lassus quasi, finem operi imponit; ille, vegetus struere incipit, ut astra petat."

"transmutation" of plants, like soil preparation, watering, application of heat and manure, grafting, transplantation etc. Thus we find Laurembergius setting out the following general principle in the first book of his *Horticultura* 1631:

> [...] for the prosperity and flourishing of gardens; for the growth and augmentation of fruits, flowers, and vegetables, we wish for two things: [...]: These are a benign sky and a fecund earth. The sky is the father of everything sown; the earth the mother.[41]

This is as far away as can be from the principles expounded in Linnaeus's species definition. And it is clear, that, with such principles, the transmutation, or degeneration, and the spontaneous generation of plants would make perfect sense. And even though doubts in transmutation should grow towards the turn to the eighteenth century;[42] and even though artificial pollination should more and more raise the interest of authors on gardening, the situation in horticultural literature seems to have remained basically the same up to the time of Linnaeus, i. e. artificial pollination should remain one among many means to meliorate plants, and it should basically be thought of in analogy to technological processes.[43] And still in 1773, we see that Joh. Chr. Fabricius, one of the more famous of Linnaeus's students, was able to announce similar principles as his teacher in a chapter on "Gartenbau":[44]

> Die unzähligen Abänderungen der Gewächse beobachtet der Gärtner. Wir verlangen die besten unter denselben, und man muß mehr auf die Cultur der Abänderung, als die Art selbst sehen. [...]. Die Ursachen dieser vielen Abänderungen untersucht der Gärtner nach dem Boden, dem Clima, der Cultur und vielleicht der Zeugung (generatio hybrida).

The distinction of horticulture as a basically technological discipline does of course not mean that the knowledge accumulated on the basis of horticultural practices was of no significance for Linnaeus's theory of (plant) reproduction. Quite the contrary is true: In the *Philosophia botanica* Linnaeus explicitely stated that "culture is the mother of so many varieties, and thus also the best mean of examining varieties."[45] After all, he was, as director of the Uppsala university garden, also

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38 Ibid, § 310, p. 245-246: "Species omnes Botanicorum eodem numero, quo hoc vel futuro tempore existent, ab Omnipotentis Creatoris manu profluxerunt: Anthophilorum autem a Speciebus (Botanicis dictis) omni die novæ & diversæ prognascuntur, & prognatae in priores tandem ruunt. Illis itaque impositi sunt limites a natura certi, ultra quos progredi nequeat; in his vero lusus infiniti naturae absque fine; Illorum species Sapientissima a Manu Omnipotentis, horum Varietates a Ludente natura, sub auspiciis præsertim Hortulanorum, prodiere. Hinc differentia inter Botanicum & Anthophilum maxima."

39 In his *Genera plantarum* Linnaeus even declared his dependance on Tournefort, by stating that he understood "no one but Tournefort and his school" (Linnaeus (1737b), § 11, [p. 6]).

40 Cf. Linnaeus (1739), which is discussed in detail in Müller-Wille (1999), pp. 151-155.

41 Laurembergius ([1631] o.J.), lib. I, cap. i, § 7, p. 38: "[...] ad hortorum prosperitatem, florentemq; constitutionem; ad fructum, florum, olerum felicem proventum & incrementa, duo adesse optamus [...]. Ea sunt Coelum benignum, & terra foecunda. Coelum satorum omnium Pater est. Terra mater." In the foreword it says in ragard to wine "tot genera, tot species, tot deliciae, quot regiones, quot oppida & urbes", something any wine *afficiando* would wholeheartedly agree to even today.

42 See, e. g., Sharrock (1660), pp. 28-32, who, however, concludes at one instance that "it was reason we should believe the report [on a transmutation] of good artists in matters of their own faculty". In Rudbeck jun. (1686), who was one of the teachers of Linnaeus, we find Laurembergius principles still expounded (p. 5) and spontaneous generation admitted (p. 8ff.). Rich material on the belief in "degeneration" till the end of the seventeenth century see Zirkle (1935), pp. 61-88.

very much himself involved in gardening.[46] Yet it shows, again, that something else had to enter the scene apart from the *mere production* of variety for the formation of his theory.[47]

4. Cabbage

> CABBAGE. "Baal (Rich.), gardener from Brainford, had collected an enormous amount of seeds from flowered cabbage [cauliflower?] in his garden and sold it to most of the gardeners living in the suburbs of London. But these, after having sown these seeds with care in fat earth, produced common, long-leafed cabbage, wherefore they lamented to have been betrayed and summoned the aforesaid Baal before the Westminster court, who was condemned by the decision of the judge not only to repay the price to them, but also to restore the waste of time and loss of land use." Raj. hist. I. p. 42. The deed is not to be ascribed to that gardener Baal, but to an impregnation of his better cabbage by the common cabbage. Therefore, if someone possesses that better cabbage, he should not let it flower on the same bed with another one, so that the better one is not fecundated with pollen from the lower and the lower one is generated from the seeds.[48]

This third example for empirical references in Linnaeus's *Sponsalia plantarum* also stems from horticulture, yet with a decisive, additional shift in comparison with the case of the tulips: The "criminal case" reported does not only encompass the local production of varieties (in which Baal seems to have been quite successful) but also their circulation. Its geographical localisation – *in suburbanis Londini* – is revealing in this respect: It was near this large city and – some decades earlier – near the cities of Holland that from the 1650ies onwards and due to population growth in the urban centers crop production changed from subsistence to commercial production, involving a separation of production and consumption and a consequent interposition of trade mechanisms.[49] For this trade – either in the products themselves, or in seeds as an important means of production – it was vital, of course, that the products exchanged did not change due to their transport from one locality to the other. Baals failure to guarantee this for his seed variety – he did notably not fail in the production of this variety as such – was responsible for his trial and – most probably – subsequent ruin.[50]

There is something else noteworthy about the reference to Baal's fate: As in the case of the Copenhagen Ethiopian discussed before, Linnaeus quotes from literature and adds an explanation

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43 Thus we see Richard Bradley, one of the "fathers" of plant sexuality and probably the source for Linnaeus's report on the "delightful experiment" with tulips (cf. Roberts ([1929] 1965), pp. 65/66), speak of fertilisation as a "mixing of qualities" and a possible source of "adulteration". Well into the 17th century, plant sexuality was generally denied, and even the fig, on which articial pollination had been used since Babylonian times, was regarded either as exceptional or as only analogically sexual (thus Laurembergius; cf. Prest (1988), pp. 81-84).

44 Fabricius (1773), p. 45.

45 Linnaeus (1751), § 316, p. 247: "Cultura tot Varietatum mater, optima quoque Varietatum examinatrix est."

46 Accordingly, he knew the horticultural literature very well; see Stearn (1976).

47 A close study of seventeenth and early eigteenth century literature on gardening and animal husbandry is, I believe, the great lacunae in the history of biology, which I, for the purposes of this paper, could of course not fill. The two most important publications on this topic are Zirkle (1935) and Roberts ([1929] 1965), which, however, because of their interest in the prehistory of Mendelism, exclusively focus on early accounts of "hybridisation". Henrey (1975), though voluminous and highly informative, rather serves bibliographical puposes.

to the case in question that rests on (and thus supposedly validates) his own theory of (plant) reproduction. And again, we can observe that this addition is contrary to the context of the original source: The quote is from a book John Ray published in 1686 and which bore the monstrous title *Historia plantarum Species hactenus editas aliasque insuper multas noviter inventas & descriptas complectens : in qua agitur primò De Plantis in genere, Earúmque Partibus, Accidentibus & Differentiis; Deinde Genera omnia tum summa tum subalterna ad species usque infimas, Notis suis certis & characteristicis definita, Methodo Naturae vestigiis insistente disponuntur, Species singulae accurate describuntur, obscura illustrantur, omissa supplentur, superflua resecantur, Synonyma necessaria adjiciuntur; Vires denique & usus recepti compendiò traduntur* (not surprisingly Linnaeus should refer to this work as one of the few "universal" plant histories in his *Bibliotheca botanica*[51]). This title already indicates, that *Brassica florida* belonged to a context much more systematic than that of the Copenhagen Ethiopian, and it occupied a central position within this context: Other than Linnaeus, Ray added the case of *Brassica florida* under the heading "About the transmutation of species among plants (*De Specierum in Plantis transmutatione*)" as a problematic case to a chapter, which discusses the "so called specific differences of plants (De specifica (ut vocant) Plantarum differentia)", thus revealing the *translation of plants* as a relevant field of empirical evidence for his discussion of "specific differences". Interestingly, a central passage of this discussion reveals some close resemblances to Linnaeus species definition:

> As, namely, the difference in sex in animals is not enough to argue for a difference in species, because both sexes originate from the same kind of seed and not rarely from the same parents, although they differ from each other in many and insignificant properties; [...]: so, also, there is no surer sign for a specific conformity than that plants originate from the same seed, be it individually or specifically. For what differs in species perpetually serves its species and not does this [species] originate from that seed [of another species] or vice versa.
>
> Therefore I propose not to hold for different species of plants those that differ by the colour [...] of the flower alone. [...].
>
> [Because these varieties] can be brought about by art and display, not less than by repeated translation from one place to the other and by irrigation with water tinged with some colour. [...] Laurembergius, a worthy and truthful man, wrote in his Horticultura that he experienced [this] often in pinks [...].[52]

48 Linnaeus ([1746] 1749), p. 370: "BRASSICA. *Baal* (Rich.) hortulanus Brainfordensis, ingentem copiam seminis Brassicæ floridæ in horto suo collectam, hortulanis quam plurimis in suburbanis Londini degentibus vendidit; At hi cum summa cura eadem semina terræ pingui commiserunt, brassicas longifolias vulgares ipsis produxere, quare se fraudatos queruntur, & prædicto Baal litem intendunt in foro Vestmonasteriensi, qui ex sententia judicum condemnatus est, non solum ut ipsis pecunias restitueret, sed jacturam temporis, & amissum terræ usum fructum resarciret. Raj. hist. I. p. 42. Facinus hoc hortulano Baal non adscribendum est, sed impræagnationi Brassicæ ejus optimæ a Brassica vulgari factæ. Quare si quis Brassicam possideat optimam, eandem cum alia in eadem areola florescere non sinat, ne præstantior vilioris polline fecundutur, & ex seminibus vilior generetur."

49 Slicher van Bath (1963), pp. 14/15, Grigg (1982), pp. 102ff. Grigg also notes a steep rise in the number of books published on agriculture in the sebventeenth century (ibid., p. 159). The distinction of the spheres of production/consumption and circulation is reflected in Sharrock (1660), p.3: "The end of the Artist is to Propagate and Improve".

50 On the emergence of seed trade see Webber (1968).

51 Cf. Linnaeus (1751), § 12, p. 10.

In contrast to Linnaeus's axiomatic species definition this formulation of a criterion for "specific conformity (*convenientia specifica*)" reveals its experiential basis and a peculiar shift in regard to it: While Ray's formulation already foreshadows the three "absences" we observed in Linnaeus – absence of substance, physiology and environment[53]– Laurembergius used the experiences Ray drew upon in quite the opposite direction:

> The diverse regions and parts of the earth are imbued with diverse liquids of diverse properties, with which the plant adorns itself, and as it draws new food who wonders that it also acquires a new nature because of the subordination and mutual proportion between that which nourishes and that which is nourished? Thus we see white flowers change into red, yellow, blue on dousing with liquid tinged with these colours, which attracted by the roots bring a similar shape to the flowers.[54]

Behind this peculiar shift lay two decades of botanical activity, in which Ray concentrated on two projects: cataloguing plant species both indigenous *and* exotic to England - with the *Catalogus plantarum circa Cantabrigiam nascentium* (1660) and the *Methodus plantarum nova* (1682) demarkating this project – and experimental studies into plant embryology and physiology. The aim of the first project was announced in the 1660 Catalogus as asking "by which similarity and by which characters [any unknown plant] coincides with its congeners (*ex similitudine, & notis quibus cum congeneribus conveniret*)"[55]. The experiments, carried out in collaboration with Francis Willoughby and Martin Lister, regarded the "motion of sap in trees" and operated by "bleeding" various trees to determine the flow directions of the "sap" under various conditions.[56] Parallel to that, Ray studied the structure of the seed plant, reaching the distinction of mono- and dicotyledons.[57]

Both cataloguing and experimenting on plants depended on a highly specific locality: the botanic garden, in which on the one hand plants from all over the world were collected and

52 Ray (1686), lib. I, cap. xx, p. 40: "Sicut enim in Animalibus sexuum distnctio non sufficit ad speciei diviersitatem arguendam, quia sexusa uterque ex eodfem speciei semine, eisdémque non rarò parentibus orìtur, quamvis multis & insignibus Accidentibus inter se differant; [...]: sic pariter in plantis convenientiæ specificæ non aliud certius indicium est quàm ex semine ajusdem plantæ seu in specie seu in individuo oriri. nam quæ specie differunt speciem suam perpetu' servant, neque hæc ab illius semine oritur, aut vice versa.
Hinc pro distinctis plantarum seciebus non habendas censeo,
1. Quæ solo floris colore [...] differunt. [...]
Denique arte & mangonio induci possunt, nimirum translatione iterata de loco in locum, & irrigitaione aquà colore aliquo imbutà. Nam P. Laurembergius, vir fide dignus, Horticult. cap. 28. Sect. 3. se in Caryophyllis sæpius expertum scribit [...]."

53 Ray ended his unpublished "discourse on the specific differences of plants" with the following words, which clearly foreshadowed Linnaeus's species definition: "By this way of sowing ["in rich soil"] may new varieties of flowers and fruits be still produced ad infinitum, which affords me with another argument to prove them not specifically distinct; the number of species being in nature certain and determinate, as is generally acknowledged by philosophers and might be proved also by divine authority, God having finished his works of creation, that is, consummated the number of species in six days" (Ray 1674), p. 173.

54 Laurembergius ([1631] o.J.), cap. XIII, § vi, p. 77: "Diversae autem regiones & telluris partes diversis sunt imbutae humoribus, diversis proporietatibus, & novo cibo ali assuscit, quid mirum si & novam acquirat naturam, propter illam quae est inter alitum & alens subordinatium & proportionem mutuam? Ita videmus flosculos albos permutari in rubros, flavos, ceruleos, affusione humoris his coloribus imbuti; qui per radices attractus similem florem ideam conciliat."

55 Ray (1660), p. 5.

compared (Ray mainly acquired seeds for his garden commercially from London),[58] and in which, on the other hand, plant specimens were produced under controlled and – for reason of experimental studies – manipulable conditions.[59] Though Ray, in contrast to Linnaeus, remained inconclusive and tentative about his findings in both projects,[60] it is here that we can identify a space in which hereditary relations could be observed to transcend local conditions: the botanic garden – with its peculiar double nature of a site for collection and experimentation– installed the same separation of production/consumption and circulation that emerged in commercial horticulture in the course of the 17th century,[61] and it was thus the site that could provide the material space for the abstractions expressed in Linnaeus's "laws of generation". And thus we can see Ray expressing the same "structuralist" approach to species as Linnaeus:[62]

> It is to be observed, however, that the distinct propagation by seed is not by itself that which constitutes the essential or specific difference, or that in which it [i. e. the essential or specific difference] consists, but its sign or indication alone.

5. Conclusion: Linnaeus's concept of heredity

By looking at three empirical references adduced by Linnaeus to back his theory of organic reproduction and by retracing some of their background in 17th century medicine, horticulture and natural history, I hope to have provided some evidence that observations of hereditary phenomena occupied a much more systematic space than is usually acknowledged by the history of science,[63] and that the organisation of this space – in the case of plants – hinged upon a separation of plant reproduction and circulation instituted by botanical gardens.

With this background in mind it is worthwhile to return to Linnaeus concept of "heredity" as expressed in his species definition. It is striking that Linnaeus – at least to my knowledge – never

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56 Willoughby and Ray ([1669] 1928); cf. Ray (1928), pp. 45-47.

57 Ray (1674).

58 See Raven (1942), p. 109. In respect to this collecting practice it is interesting to see, that Ray quoted – apart from Laurambergius – another author of a gardening book, Johann Baptista Ferrarius, who directed his book not to gardeners, but to "rei florae scriptoribus" (see Ferrarius (1638), p. 13) and included a "law of sowing flowers (lex floris serendi)" and a "law of propagating flowers (lex floris propagandi" in this book. The first "law" consisted, among other things, in prescriptions of how to produce a "paper garden (hortus papyraceus)" representing the garden and guranteeing the identification of the plants sown out over time. In regard to the second "law", Ferrarius stated, that "two things are necessary in the affairs of the propagator, namely that he augments the domestic [plants] and that he acquired foreign ones (duplex in propagatore industria requiritur, ut flores domesticos augeat, externosque conquirat)" (ibid. p. 294), and that for this "trade in flowers (florum commercia) was necessary.

59 The importance of the garden was emphasised by Ray in a letter to James Petiver in 1701 when complaining of the loss of access to gardens: "Since I came to prosecute the work [i. e. the second edition of Rays Methodus, which appeared in 1703] in good earnest, I have been in no case to travel to visit gardens, and to see plants growing, flowering, and seeding. Dried specimens, figures, descriptions, and names of plants, is all I have had to work by, so that I must needs be liable to commit a thousand mistakes."

60 Ray called the report he sent to the Royal Society in 1674 on plant embryology and physiology "inchoate & imperfect" (see Ray (1928), p. 68). Likewise, he warned the reader of his *Methodus*, not to "believe that it [i.e. his "universal method"] extends so far as to signify all plant genera over the whole world (Quod Methodum nostram generalem apello, cave credas me vocare hanc extendere ad omnia per totum terrarum orbem nascentia plantarum genera significanda.)" (Ray 1682), pp. 7/8).

used this or similar concepts in the context of organic reproduction. I think there was a good reason for this: If the "structure" characterising organisms as belonging to certain species consists in nothing but properties which identify and differentiate organisms "universally", i. e. irrespective of any physiological and environmental contiguity, there is no need to postulate any mechanism of heredity. The different "structures" postulated in Linnaeus species concept just persist through time, or rather time-lessly. In a way Linnaeus stood halfway between the traditional and the modern concept of heredity, by maintaining persistence as the mechanism responsible for the similarity among parents and offspring and at the same time reducing that which persists to universal relations of formal identity and difference.[64]

For a historical moment therefore we see heredity vanishing as a "biological" concept. And yet, this moment inadvertently provided a decisive precondition for the emergence of modern heredity: The taxonomically defined species of Linnaeus was to serve as the substrate for the "transmission of characters from one generation to the next" characterising the Mendelian concept of heredity (thus Mendel spoke of plant species as the "Träger" of his experiments).[65] The role of taxonomy for the history of heredity still awaits its analysis.[66]

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61 Cf. Stearn (1961) and Wijnands (1988) for the development of exchange relations among European botanical gardens during the seventeenth century.

62 Ray (1686), p. 42: "Notandum tamen distinctam propagationem ex semine non esse illud ipsum quod constituit differentiam essentialem seu specificam, sive in quo illa consistit, sed ejus signum seu indicium tantúm."
Sloan (1972) has connected John Rays well known scepticism about knowing the "essences" of plants with Lockean philosophy. Though Ray and Locke probably knew each other (they became fellows of the Royal Society the same day; see Ray (1928), p. 44), I doubt this interpretation. Rays scepticism concerned the possibility to get a knowledge of taxonomic criteria through physiological studies, not the possibility to determine taxonomic criteria as such. Locke, on the contrary, explicitely rejected the criterion of "constant" reproduction in his Essay concerning human understanding (see above, n. 5). Mary Slaughter has also situated Rays scientific projects in a "philosophical" context, namely the search for an "universal language" by John Wilkins, a context, to which Ray – in hindsight – referred himself (Ray 1703, p. 5). While I think, that, beyond this, Rays attempts have to be seen in the much more specific context of physiology and natural history, I share Slaughters general conclusion: "The isolation of these variables [i. e. structure, sign, essence] or the decomposition of the organism into these elements permits organic form to be reconstituted or retranslated into a linear language, into a series of successively ordered elements which constitute a taxonomy" (Slaughter 1982, pp. 9/10).

63 Cf., e. g., Ritterbush (1964), p. vii.

64 In this respect, Linnaeus' theory of organic reproduction showed striking resemblances to the 18th century concept of the "genealogical state", see Paulson Eigen (2000) and Müller-Wille (2000).

65 Mendel (1866), p. 5.

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[66] Contributions in that direction are Barsanti (1995), p. 35, who maintained "une priorité de classification sur la pense généalogique", and Gayon (1995), who described the transition from a concept of heredity as "force" to a concept of heredity as "structure" as the main development in the formation of 20th century heredity.

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Speculation and Experiment in Enlightenment Life Sciences

Mary Terrall

Although, as Carlos López-Beltrán has argued convincingly, heredity was not a coherently theorized concept until the 1830s, the inheritance of particular traits entered into quite a range of discussions about the nature and organization of living matter in the Enlightenment. Since the scientific community was not yet parsed strictly by discipline or profession, it is not surprising that these discussions easily crossed lines that later would become more impermeable. In addition to the writings of physicians on the cause (or even existence) of hereditary diseases, hereditary phenomena from animal breeding and from observations of human families made their way into in works of various genres that addressed the vexed question of generation.[1] Precisely because biology was not yet constituted as a discipline, this topic was open to interpretation, speculation and experimentation by a range of writers including philosophically-minded materialists (or crypto-materialists) as well as practicing physicians, anatomists, and observers of animal behavior. Inheritance of traits, whether abnormal or not, was one kind of evidence mustered in the discourse about generation and the origin of organization. Much of this discourse remained speculative, but some writers drew on empirical evidence to buttress their conjectures about unseen forces and hidden mysteries. Ideas and experiments related to the passing of traits from parent to offspring thus belong in a cultural context that includes a shifting and fluid discourse about the nature of life.

The obscurity of the principles governing generation and organization meant that the means for uncovering them were as fraught as the elusive principles themselves. Turning to the *Encyclopédie* article on generation, for example, we find first a description of the mechanics of copulation, and then the reflection that the really interesting part of the process is not mechanical, but "physical." The physical lies at a more fundamental level than the mechanical, apparently, and "nature employs the most secret means, the least available to the senses, to put fertilization into operation." Ironically, the most sensual of phenomena masks a process beyond the reach of empirical investigation.

> This mystery has always excited the curiosity of *physiciens* and has led them to conduct so many investigations in order to penetrate it, so many experiments in an effort to take nature in the act; ... they have imagined so many different systems, which have destroyed each other successively, ... without resulting in more light on the subject. On the contrary it seems that the veil behind which nature hides herself is essentially impenetrable to the eyes of the subtlest mind, and that the cause of the formation of animals must be ranked among first causes, like that of motion and gravity, of which we will never be able to know anything but the effects.[2]

Diderot, too, in his article "Animal" referred to the ordered succession of generations by which species preserve themselves as "the greatest marvel". He went on to rhapsodize, "The machine is

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1 On the medical discourse about heredity López-Beltrán (1995); see also López-Beltrán (1994).
2 Aumont (1757), p. 568.

finished, and the hours strike under the eye of the clockmaker. But among the sequences of the mechanism, we must admit that this faculty of animals and vegetables to produce their kind, ... this procreative power that operates perpetually ... is for us ... a mystery whose depths it seems we will not be allowed to sound."[3] Here Diderot articulates the tension between predictable clockwork mechanism and the "procreative power" essential to life. The sense of mystery, and the recalcitrance of the problem, pervaded discussion of generation and hereditary phenomena in this period. If we are to understand the efforts of 18th-century thinkers to explore this mystery, we need to keep this in view. Otherwise we risk reading these writers as simply groping towards 19th-century biology, an approach that ignores the complexity of their efforts and the meanings attached to them by their readers.

The suspicion that the depths of the mystery might remain beyond the reach of human understanding did not keep people from trying to sound them. By the mid-18th century, whenever the question of how to understand the generation of living forms came up, it brought along related philosophical, theological and methodological freight. Empirical evidence came from anatomical observations going back to Harvey, Malpighi, Leeuwenhoek and others working in the previous century. By the time the volumes of the *Encyclopédie* started to appear, in 1751, the ramifications of the problem of generation extended across several disciplines, and involved overlapping sets of medical men, academic anatomists, experimenters, natural historians and philosophers. It was not a problem that found a ready solution and no consensus was reached. Some were willing, even eager, to peer beyond the visible (behind the veil, as it were) to speculate about fundamental forces and principles; others recoiled with something like repugnance. The interplay between eager speculation and prudent restraint characterized this discursive terrain, and gave an edge of danger, a whiff of suspicion, to theories of generation that went beyond the mechanical. I look here at competing theories and interpretations in the context of related debates about method, and especially about how speculation (or "conjecture," in the terminology of the day) was interconnected with experiment and observation of organisms. Claims about method – what was allowable or legitimate – played into hotly contested questions that stretched the physical toward the metaphysical.

Defining a science of life – setting parameters for how to go about investigating organic phenomena like generation – was inseparable from the question of the nature of life itself, which inevitably shaded toward claims with moral or religious implications. The investigation of inherited traits also entailed reflection on the properties of matter, not to mention the role of God in the universe. In the spectrum of discourse about generation, and the boundary between living and inert, we find conflict not just about content of theories, or interpretations of experiments, but about what is thinkable, what is dangerous, what is threatening or liberating. The disjuncture between the mechanical and the living inspired attention to generation and inheritance, and especially the arguments against the pre-existence of germs, whose "development" was only a

[3] "La machine est faite, & les heures se marquent sous l'oeil de l'horloger. Mais entre les suites du mécanisme, il faut convenir que cette faculté de produire son semblable qui réside dans les animaux and dans les végétaux, ... cette vertu procréatrice qui s'excerce perpétuellement sans se détruire jamais, est pour nous ... un mystere dont il semble qu'il ne nous est pas permis de sonder la profondeur." (Diderot 1751).

mechanical unfolding of forms already organized. Getting behind the mechanical, for some at least, meant going beyond the visible and rehabilitating conjecture, or speculation.

Theories of generation

Debates about generation revolved around two sets of oppositions: female eggs and male sperm (what is contribution of each sex?), and pre-existence and epigenesis.[4] Pre-existence theories, the main contenders in the aftermath of microscopical/anatomical work at the end of the 17th century, located a fully organized creature in the germ, whether in egg or sperm. The organism was assumed to be contained in sub-microscopic particles, only just beyond the reach of microscope-enhanced vision. The origin of organization was, then, referred back to God, and therefore not open to scientific investigation. Any alternative to this way of thinking had to assign a cause (whether physical or metaphysical) responsible for getting unorganized material particles organized into a functioning animal. Epigenesis, for example, assigned responsibility for the process of organization to properties or forces associated with elements of organic matter. Organisms formed from unorganized matter, rather than unfolding or expanding according to pre-ordained plan. The problem was how to get at the non-mechanical aspect of the process without raising other sorts of philosophical obscurity. Suffice it to say that recent developments in physics (gravity and electricity) and chemistry (selective affinities driving reactions) made it conceivable to associate forces with matter, in very un-Cartesian ways.

In spite of the obvious conceptual difficulties (all future organisms had to be present either in Eve or in Adam; or else seeds have to be created directly by God in each generation) pre-existence was compelling because it avoided the problem of self-acting matter. This was not a question that could be settled definitively by a crucial experiment, and there was a remarkable lack of consensus about how to explain even the most mundane phenomena of inheritance, like the resemblance of children and parents. Nevertheless, observational and experimental evidence was continually brought into the discussion, and certain phenomena became touchstones for the debate.

One of these was the freshwater polyp. This tiny organism's capacity not just to propagate by budding, but to regenerate whole organisms from pieces of itself, or to grow several heads where one had been, captured the imagination of naturalists, philosophers and their readers alike. It made an appearance in virtually every text that touched on the origin of life, the relation of soul to body, or generation. The polyp became emblematic of the mysterious capacities of organized matter, and by virtue of its stranger-than-fiction quality, inspired some people to speculate about what it might mean for a science of life. Abraham Trembley, though, who first noticed these "little organized bodies," undertook to show that, however unusual they might appear, they followed normal patterns just like any other species. He identified their feeding habits, their normal mode of reproduction by budding and their response to being cut in all conceivable ways.[5] His focused, almost obsessive, effort was an attempt to contain the startling nature of his discovery, and to avoid possible implications for thinking about the properties of matter, and the relation between body and soul. Others were not so timid. Buffon argued, for example, that in polyps "each part contains a whole," and he went on to argue that these organisms imply the existence of a pervasive

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4 The standard source is Roger (1971).
5 Trembley (1744). See also Dawson (1987), Vartanian (1950).

multitude of organic particles from which organisms form and develop. It takes thousands or millions of them to form a single germ. In polyps, according to Buffon, organization means a simple repetition of the same “form;” more complex organisms more complex combinations of organized parts.[6]

Maupertuis described the polyp as a “Hydra more marvelous than that of the fable,” with its ability to regenerate a tail from a severed head and vice versa. “What are we to think of this strange kind of generation? of this principle of life spread throughout each part of the animal? Are these animals anything other than collections of embryos just ready to develop, as soon as they are allowed? Or do they reproduce by unknown means all that the mutilated parts are missing? Would Nature, which in all other animals attached pleasure to the act by which they multiply, cause these [creatures] to feel some kind of sensual delight when they are cut into pieces?”[7] For Maupertuis, this “principle of life spread throughout each part” was the organizing force driving the process of generation. In higher organisms, he speculated, parts of the body send organic elements to the reproductive organs, where they collect and combine by virtue of selective organizing forces, analogous to chemical affinities. The mixing of male and female fluids is essential, as evidenced by the resemblance of children to both parents; neither sex is privileged. At the elemental level, on this view, matter must be self-organizing; whatever the forces are, they must be intimately associated with material elements.

Looking at how different people responded to strange phenomena like polyps, we see uncertainty, not only around how to interpret unexpected observations, but about whether they are even open to interpretation. Where were the limits of intelligibility? And just how far could speculation – based on the visible, but extending beyond it– safely go? Diderot, to take one example, drifted easily (and quite consciously) from the evidence of natural history to a vision of nature’s activity going well beyond empirically verifiable sights. “It seems that nature varies the same mechanism in an infinity of different ways. ... When we consider the animal kingdom, ... wouldn’t we willingly believe that there never has been more than one original animal, prototype of all animals, in which nature only lengthened, shortened, transformed, multiplied, or obliterated certain organs?”[8] The prototype is more than an ideal – it’s a presumed ancestor, dating back to a moment before the proliferation of natural forms. Diderot calls his suggestion a “philosophical conjecture,” bringing the imagination to bear on solid evidence from natural history. From the indefinite past, and the malleable form of the original prototype, he ventured into the sub-microscopic realm of organic molecules and proposed “muffled sensibility” as a property of all matter, the property that made all those variations in form possible. This force drives molecules to seek situations of stable equilibrium, by an “automatic restlessness [*inquiétude automate*].”[9]

Diderot’s primary interest was not in a theory of generation as such, but a theory of matter, which was in turn tied to a theory of life. He embedded the theory in reflections on method, arguing for a combination of exploration and synthesis, experiment and interpretation. The philosopher seeks new knowledge in the same way that molecules seek their places, by a kind of

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6 Buffon (1954), p. 239. “Il y a dans la Nature une infinité de parties organiques actuellement existantes, vivantes, et dont la substance est la même que celle des êtres organisés.”
7 Maupertuis (1980), pp. 107/108.
8 Diderot ([1754] 1981), Pensée XII, pp. 36/37.
9 Ibid., Pensée LI, pp. 84/85.

restless touching and retouching, trial and error, a "*tatonnement*" like that of a blind man's stick. The ideal method involves moving back and forth from sense impressions to reflection, from experiment to theory, in a kind of oscillatory exploration: "C'est le travail de l'abeille."[10] It's productive, useful, like making honey, but it's also synthetic, and physical, or empirical. Any position, whether material or philosophical, is always either in flux or in a state of dynamic equilibrium, and potentially unstable. Diderot's *Pensées sur l'interpretation de la nature* (1754) is a defense of "conjecture" as a productive method; the interpreter of nature starts where the senses leave off. "He draws, from the order of things, abstract and general conclusions that are for him just as evident as sensible and particular truths."[11]

Diderot developed his radical defense of "philosophical conjecture" or "*esprit de divination*" in dialogue with the books and experiments of Buffon and Maupertuis. Toward the beginning of his investigation of reproduction, Buffon declared that "the living and animate, instead of being a metaphysical aspect [*un degré métaphysique*] of creatures, is a physical property of matter."[12] Organic molecules are not yet organisms – they cannot reproduce themselves – but they are the material of life, the matter from which organisms build themselves. In collaboration with the English microscopist John Turberville Needham, who had earlier found teeming animalcules in the milt of squid, Buffon examined fluids taken from the reproductive organs of dogs and other animals. They found "moving bodies," many with tails, in both male and female fluids, and decided that these apparently ubiquitous entities must be organic elements that combined to produce "germs" that in turn combined to generate new organisms. From the modern point of view, these experiments involved considerable confusion about what was actually seen.[13] But for Buffon and Needham, their observations of seminal fluids implied first of all that mammalian eggs did not exist, which they used as evidence against the preexistence of organized germs, and secondly that male and female functioned symmetrically in reproduction. After further experiments with infusions of seeds and meat, in which moving particles appeared spontaneously, they concluded that organic particles could be found throughout nature, and not just in reproductive organs. They claimed to have actually seen the building blocks of living organisms through their microscope.

The next question was how to understand the organization of these elements into functioning, living organisms. Buffon's organic molecules come together through the action of "penetrating forces" that guide them into "internal molds" where they take on the appropriate structure and form of body parts. "In the same way that we can make molds by which we give to the exterior of bodies whatever shape we please, let us suppose that Nature can make molds by which she gives not only the external shape, but also the internal form, would this not be a means by which reproduction could be effected?"[14] This notoriously obscure formulation was ridiculed and misunderstood from its inception, though on careful consideration it seems that Buffon was self-consciously searching for an analytic tool to give him a way of talking about "hidden mysteries"

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[10] Ibid., p. 34.
[11] Pensée LVI, p. 88.
[12] Buffon, *Histoire Naturelle*, vol. ii, In Buffon (1954), p. 238.
[13] On Needham, see Roger (1971), pp. 424/520; Roe (1983). For the experiments on seminal fluids and the microscope they used, see Roger (1997), pp. 140/145 and Sloane (1992). Sloane concludes that Buffon was very likely seeing bacteria and cell fragments in Brownian motion.

without retreating into agnosticism. I want to emphasize the interpretive power of analogy for Buffon. He did not claim that physical, three-dimensional molds hover in the sex organs, waiting to receive matter exactly as metal molds do. "Nature can have these internal molds, which we will never have, just as she has the qualities of gravity, which in effect penetrate to the interior; the supposition of these molds is therefore founded on good analogies."[15] If our senses were not limited to the surfaces of things, we might be able to conceive of internal molds more immediately. As it is, Buffon tells us, we have no reason to assume that Nature cannot employ means that are beyond our direct intuition. In fact, this may be the only way to know about natural processes, and Buffon invoked the authority of Newton to support the analogy between gravity and internal molds:

> I have admitted in my explanation of development and reproduction first the accepted principles of mechanics, then that of the penetrating force of gravity that we must accept, and by analogy I thought I could say that there were other penetrating forces that act on organic bodies, as experience assures us. I have proved by facts that matter tends to organize itself, and that there are an infinite number of organic particles. I have thus done nothing but generalize from observations, without having advanced anything contrary to mechanical principles.[16]

The internal molds are conceptually slippery because they sometimes seem to operate as forces, by analogy to gravity, and sometimes as constraining structures, by analogy to the sculptor's molds. Buffon wrestled with how to combine a quasi-mechanical explanation of forces, shapes and motions with a notion of active organic matter that resisted this kind of explanation. He tried to escape the limitations of the human senses, while simultaneously drawing on empirical evidence. Experiment and observation were crucial components of his theory of life – but he insisted that the limitations of the human senses made it necessary to bring into play other means, if complex phenomena were to be understood. Opponents attacked him at precisely this point, unwilling to make the analogical leap; his most outspoken defender, Charles Joseph Panckoucke, remarked that we would need a sixth or even a seventh sense, in order to be able to witness or understand internal molds more immediately.[17] This attitude threatened deists like Réaumur and Pluche, who explicitly pursued a descriptive approach restricted to visible surfaces, and avoided mysteries and metaphysics.

Following his collaboration with Buffon, Needham continued his microscopical work. His experiments with infusions of animal and plant material, as well as observations of seminal fluids

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14 De la même façon que nous pouvons faire des moules par lesquels nous donnons à l'extérieure des corps telle figure qu'il nous plaît, supposons que la Nature puisse faire des moules par lesquels elle donne non seulement la figure extérieure, mais aussi la forme intérieure, ne seroit-ce pas un moyen par lequel la reproduction pourroit être opérée?" (Buffon 1954, p. 243).

15 "Ces moules intérieures, que nous n'aurons jamais, la Nature peut les avoir, comme elle a les qualités de la pesanteur, qui en effet pénètrent à l'intérieur; la supposition de ces moules est donc fondée sur de bonnes analogies." (Ibid., p. 244).

16 "J'ai admis dans mon explication du développement et de la reproduction, d'abord les principes mécaniques reçus, ensuite celui de la force pénétrante de la pesanteur qu'on est obligé de recevoir, et par analogie j'ai cru pouvoir dire qu'il y avoit d'autres forces pénétrantes qui s'exercoient dans les corps organisez, comme l'expérience nous assure. J'ai prouvé par des faits que la matiere tend à s'organiser, et qu'il existe un nombre infini de parties organiques, je n'ai donc fait que généraliser des observations, sans avoir rien avancé de contraire aux principes mécaniques." (Ibid., p. 254).

17 Panckoucke (1761).

of animals, led him to insist upon the existence of “a new Class of Beings,” creatures he saw come to life in his infusions. These “microscopical Animals ... furnish a key to the Generation of all others.” Infusions of seeds and meat gravy produced microscopic “moving Globules” after sitting for a few days. Based on many observations, he decided that all organic substances, animal and plant alike, had “vegetative Powers.”[18] I won’t stop to elaborate on Needham’s interpretation of what he saw; he appears here simply to show the centrality of his surprising experiments for those of his contemporaries worried about making a science of life.

Maupertuis was not convinced by Needham’s talk of epigenesis through vegetation, but the array of new observations of infusoria inspired his own conjectures. Writing to Buffon, Maupertuis commented, “I have just been reading Needham’s book. What disorder, what a reasoner! But it also contains plenty of marvels. It’s too bad that such a man wants to make systems, and that a maker of bricks wants to be an architect.”[19] In the same vein, he wrote to La Condamine: “Have you read Needham’s book? What are we to think? What a new universe! What a shame that a man who observes so well reasons so poorly! After reading his book, my mind was so dizzy [*étourdi*] from all the ideas it presented to me that I had to go to bed like an invalid, and I have not yet completely recovered from the upheaval that this reading put me in. I hope when this tumult calms down a bit to take up again the thread of some meditations that I have begun some time since on this subject, and see if it is possible to pull out something reasonable from it.”[20] This passage captures the contemporary sense, experienced bodily by the reader in this case, of the inexplicability and astounding novelty of Needham’s observations, not to mention the dissatisfaction with his theoretical excursions. The exciting prospect of a “new world” was tempered by frustration at how to make sense of it. The ultimate question remained unsolved, according to Maupertuis, regardless of the status of Buffon’s and Needham’s microscopic bodies. “Even if material organic parts of the bodies of animals had been found, it [still] would not have fundamentally explained generation: because the formation of these organic particles would [also] need to be explained. It is too bad that those who do the experiments hardly attempt these speculations, and those who do the speculating are devoid of experiments.”[21] In his writings on generation (none of them in the form of a systematic treatise), he attempted to bridge this gap between speculation and experiment, by opening up the boundaries of both categories.

Maupertuis used evidence from microscopy, animal breeding and inheritance of human traits to argue that matter must have more properties than impenetrability, extension and inertia. While gravity and chemical affinities could account for many kinds of phenomena, organization required properties “of another order than those we call physical. ... We must have recourse to some principle of intelligence, to something similar to what we call desire, aversion, memory.”[22]

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18 Needham (1748).

19 Maupertuis to Buffon, 1 September 1750, Saint-Malo Municipal Archives.

20 Maupertuis to La Condamine, 24 August 1750, Saint-Malo Municipal Archives.

21 Quand on n’auroit trouvé des parties organiques materiaux des corps des animaux ce ne seroit pas avoir expliqué primordialement la generation: car il faudroit expliquer la formation de ces parties organiques elles memes. C’est domage que ceux qui font des experiences ne s’elevent queres à ces speculations, et que ceux qui font ces speculations soyent denués des experiences. (Ibid.).

22 “Il faut avoir recours à quelque principe d’intelligence, à quelque chose semblable à ce que nous appellons désir, aversion, memoire.” (Maupertuis, *Systême de la nature*, In Maupertuis (1756), vol. ii, p. 147).

This “principle of intelligence” resides in matter, down to its smallest parts. In effect, the elements of matter have special organic affinities analogous to psychic entities: desires, aversions, perceptions, habits and memories. As for generation, both male and female contribute material to the embryo, which forms from a mixture of the seminal fluids. Desire and aversion direct the organic elements to their places, and memory provides a link (albeit immaterial) between elements and comparable particles in the parent organism. Each one “retains a kind of memory (*souvenir*) of its previous situation and will resume it whenever it can, in order to form the same part in the fetus.”[23] The same process produces individual variations and congenital defects. The original cause of excesses or deficiencies could be strictly accidental, but their effects may then be perpetuated through normal generation. Once a trait was established, “the particles become accustomed to their locations, which makes them place themselves similarly [in succeeding generations].”[24] Once again, we see a mix of speculation and empirical evidence – a theory of self-directing matter required more than observation, because it could not be seen directly.

Responses to Epigenesis

The works of Buffon, Maupertuis, and Needham were widely read, cited, admired and challenged. A number of writers regarded these theories as dangerous, morally and theologically, as well as problematic from the anatomical point of view (especially on the question of whether male and female fluids actually mix in the uterus). Haller, for example, couched his objections to Buffon in the language of anatomical expertise. He argued, point by point, that Buffon just was not a competent anatomist, and that his theoretical conclusions were not well grounded in observation. “I have opened, without preconception or prejudice, the bodies of hundreds and hundreds of women, old and young,” he boasted, but rarely encountered the glandular bodies where Buffon located the female seminal fluid. Internal molds (being invisible) were even more problematic for Haller; he argued against them not on the basis of his fundamental incredulity about the process, but as an anatomist who knew from experience that no two individuals really resemble each other. “It is anatomy that has taught me the troublesome truth” that no two individuals have exactly the same layout of arteries, nerves, veins, muscles and bones. Therefore, since no two individuals truly resemble each other, to build a theory of heredity on resemblance is to start from false premisses.[25] This may seem a perverse argument, but it is built on Haller’s sense of himself as the consummate anatomist and also reflects the divide separating medical practitioners from more philosophical writers like Buffon.[26]

Réaumur also objected to giving matter the capacity to order itself through the action of attractive forces, and he likewise objected to speculating about unseen causes and forces. Even supposing that male and female fluids mix to form the germ, how can we imagine that order will emerge from the chaos of the mixture? “Who is the agent which is to disentangle and clear this

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23 “Mais chacun extrait de la partie semblable à celle qu’il doit former, conserve une espece de souvenir de son ancienne situation; et l’ira reprendre toutes les fois qu’il le pourra, pour former dans le foetus la même partie.” (Ibid., p. 158).
24 “L’habitude de la situation des parties dans le premier individu les fait se replacer de la même manière.” (Ibid., pp. 160/61).
25 Haller (1751), quotations on pp. 48, 32.
26 See López-Beltrán (1995).

chaos, to sort the several parts which are to come together, to construct organs with them, ... in short, to finish that germ...? We must not expect ... that the bare action of a gentle heat can ever be capable of producing such a work, a work infinitely more complicated than any repeating watch can possibly be."[27] To speculate, to turn the imagination loose, was tantamount to irreligion for Réaumur. Design and knowledge could not reside in matter, however fashionable attractive forces might be. "In order to arrive at the formation of so complicated a piece of work, it is not enough to have multiplied and varied the laws of attraction at pleasure; one must besides attribute the most complete stock of knowledge to that attraction."[28] To avoid this sort of trouble, Réaumur was content to refrain from theorizing altogether.

Réaumur's protégé Lelarge de Lignac, a Jesuit amateur of natural history, went further and accused the epigenesists of materialism. Buffon, he charged, relied on chance as an explanation: "animals make themselves from what he calls living elements, equally appropriate for entering into the formation of animals and vegetables."[29] Lignac was incensed by every aspect of Buffon's book, from its content to its style, pointing to its subversion of morals along with what he considered its absurd use of attractive forces. He denounced the excesses of Buffon's style ("worthy of a modern novel") used to reinforce heterodox accounts of the origin of human life and the order of the solar system. These critiques share a squeamishness about active matter, chance, sexual license, modern style: all compounded together. Natural history is safe; argument from design is useful; epigenesis is materialist, and therefore scary.

Anxieties about self-directing matter were not limited to priests like Lignac; Voltaire also found pre-existence (in the sperm) the only possible explanation for generation, even though he recognized that the implied infinite regress is counter-intuitive and must remain a "mystery." He preferred the mystery to the threatening implications of active matter. Voltaire stubbornly misunderstood Maupertuis's and Buffon's analogical use of forces, in order to more easily caricature epigenesis. Maupertuis, he complained, "has supposed that, in the fertile principles of man and woman mixed together, the left leg of the fetus unerringly attracts the right leg; that an eye attracts an eye leaving the nose between them, that a lung is attracted by the other lobe, etc."[30] Voltaire here found himself on the same side of the fence as Lignac, the Cartesian Jesuit, since they shared a conviction that God alone could understand the first principles behind phenomena. What was left of divine design if matter could organize itself? Voltaire and Lignac agreed that epigenesis implied a disturbing lack of orderly causality; "a man could be born from a clod of earth just like an eel from a bit of flour paste. Besides, this ridiculous system would lead obviously to atheism. ... Needham's microscope appears to be the laboratory of atheists."[31]

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27 Réaumur ([1749] 1751), p. 457.

28 Ibid., p. 463.

29 "Dans son ouvrage tout s'opere fortuitement; les animaux meme se composent d'élémens qu'il appelle vivans et egalement propre à entrer dans la construction des animaux et des végétaux. Il est vrai qu'il met l'efficace de l'attraction à la place du hazard d'Epicure..." (Lelarge de Lignac 1751).

30 "Il [l'auteur] a pretendu que, dans les principes feconds de l'homme et de la femme meles ensemble, la jambe gauche du foetus attire la jambe droite sans se méprendre; qu'un oeil attire un oeil en laissant le nez entre deux, qu'un poumon est attiré par l'autre lobe, etc." (Voltaire 1752, p. 446).

31 Si des animaux naissaient sans germe, il n'y aurait plus de cause de la generation: un homme pourrait naitre d'une motte de terre tout aussi bien qu'une anguille d'un morceau de pate. Ce systeme ridicule menerait d'ailleurs visiblement a l'atheisme. ... Le microscope de Needham passa pour être le laboratoire des athées." Voltaire pamphlet against Needham, 1765; cited Mazzolini and Roe (1986), p. 82.

Animal Breeding

I now turn to another kind of experiment that contributed to the debates about life and how to investigate the origin of organization, namely breeding experiments. Domesticated animals had been bred for selected characteristics for centuries, by farmers, by pet fanciers, and by dealers in animals for the luxury market. These breeders didn't worry about what caused certain traits to be inherited with a certain frequency. But in the 18th century hybrids and domestic animals entered the discourse about generation alongside Needham's microscopic animals, and the inheritance of visible traits became evidence for the mixing of contributions from male and female parents. In breeding practices, and in thought experiments about hypothetical crosses, we can see how the specialist discourse of life science (coming out of anatomy and microscopy) intersected with polite culture, specifically in the cultivation of pets and other domestic or farm animals, like horses and fowl, and also in literature written for this same genteel readership.

Maupertuis himself kept quite a menagerie of pets (as did Buffon and Réaumur), some of which he bred systematically, looking for hereditary patterns. When he came across a rare combination of colors in the coat of a female dog, for example, he attempted to perpetuate the trait; after four litters a male puppy with identical markings was born.[32] This dog eventually went on to father yet another with the distinctive coloring, showing that the trait was not sex linked. Another example came from a human family in Berlin, some of whom were born with extra fingers: he recorded the genealogy of this family for three generations with names of individuals, their spouses, and their children, and the occurrence of extra digits among them. The trait occurred frequently enough to show that it was transmitted bilaterally, and that marriage to a five-fingered spouse affected the frequency of its appearance in the offspring. These examples seem prosaic, and not particularly startling; but they opened up questions about the meaning of individual variations for a science of life – especially the analytic value of examining populations over time – and about the relation of individuals to their species (or races). To show that the sixth finger was inherited rather than accidental, Maupertuis estimated its probability of appearing at random in consecutive generations of one family. The probability decreases by the same factor for each generation, to the point where the chances of three generations of the same family producing such individuals at random would be impossibly high; "numbers so large that the certainty of the best demonstrated things in Physics does not approach these probabilities."[33]

Breeding also raised questions about agency: could manipulation by human breeders produce new species? How malleable were living forms? And how could human efforts to breed new animals illuminate natural processes? Maupertuis understood the perpetuation of naturally occurring variations, like albinism, by analogy to selective breeding practices: "Whether one takes this whiteness [of the albino's skin] for an illness or accident, it will never be anything but a

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32 Formey recalled making his way fearfully through Maupertuis' s menagerie when he went to visit. "Il étoit dangereux quelquefois de passer a travers le plupart de ces animaux, par lesquels on étoit attaqués Je craignois surtout beaucoup les chiens Islandois." These were the dogs used in Maupertuis's breeding experiments. (Formey 1789, vol. i, pp. 218/219). Many letters attest to Maupertuis's trading in dogs with his correspondents. The dog-breeding experiments are described in Maupertuis, "Sur la génération des animaux," *Lettres de M. de Maupertuis,* in Maupertuis (1756) vol. ii, pp. 310/311. See also Hoffheimer (1982).

33 Ibid., p. 310.

hereditary variation that reinforces itself or erases itself over the course of generations."[34] Similarly for polydactyly: "By such repeated marriages, it would probably die out, and it would perpetuate itself through marriages where the trait was common to both sexes."[35] Varieties occurring by chance could be perpetuated through cultivation, just as particular traits are preserved in animals by systematically selecting for them. Over time, cultivation could cause varieties to solidify into a separate species that would be stable enough to perpetuate itself.

In his most speculative mode, Maupertuis suggested that naturalists might use the collections of animals in menageries to explore heredity through hybridization, even to the point of crossing animals that would never mate in nature. "The efforts of a hardworking and enlightened naturalist would result in plenty of curiosities of this type, by causing animals of different species to lose their natural repugnance for each other, through education, habit and need." And for animals that couldn't be pressured to mate, he imagined the enlightened naturalist developing techniques for artificial insemination to create new "marvels": "We might see from these unions plenty of monsters, of new animals, perhaps even entire species that nature has not yet produced."[36] These suggestions were meant to push beyond the limits of current knowledge into the unknown, the possible, the marvelous, not just by looking at nature, but by manipulating it. Maupertuis implied that much of what we don't know is only mysterious because it hasn't yet been carefully (or imaginatively) investigated. Reflecting on the regenerative capacity of polyps and lizards (who regrow their tails), he asked, "Is it probable that this marvelous property belongs only to the small number of animals we know about? ... perhaps it only depends on the method for separating the parts of other animals to see them reproduce themselves [in the same way]."[37]

Maupertuis's projects along these lines remained in the realm of the hypothetical, of course. I would point out, though, that interspecies crossing carried a definite flavor of the fashionably transgressive and risqué. An extreme example, that undoubtedly draws on contemporary discussions about breeding, is found in Diderot's *D'Alembert's Dream,* where the characters imagine the production of a hybrid race of "goat-footed men" to work as servants: they would be "vigoureuse, intelligente, infatigable et véloce."[38] Mlle de l'Espinasse only realizes belatedly that such a breed would require people to mate with goats; this causes her to withdraw her approval for the scheme. This fantasy, intended to shock as well as to amuse, is an extreme example of the resonance of epigenetic discourse with social concerns, cast in the enlightened medical framework articulated by Diderot's version of the vitalist doctor Bordeu.

Even Réaumur, a denizen of salon culture though not a freethinker by any means, wrote about the "philosophical amusements of the barnyard," giving detailed voyeuristic accounts of the amorous adventures of a duck and a rooster and, more engaging still, of a rabbit and a hen. These animals, which Réaumur had kept under observation in his house for a time, had entertained "the whole of Paris," and prompted, naturally enough, speculation about the outcome of the inter-species union: "It was the general wish, as well as my own, that it might have procured us chickens covered with hair, or rabbits clothed with feathers."[39]

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34 Maupertuis (1980), p. 138.
35 Maupertuis (1756), vol. ii, p. 308.
36 Maupertuis, *Lettre sur le progrès des sciences* (Berlin 1752), in Maupertuis (1756), vol. ii, pp. 420/421.
37 Ibid., pp. 421-422.
38 Diderot, *Le Rêve de d'Alembert*, in Diderot (1964), p. 383.

A furry chicken or a feathered rabbit would have called into question Réaumur's commitment to preformed germs, so it's perhaps just as well that the hen's eggs never hatched. Réaumur did note in his work on poultry that chickens could provide clues to understanding inheritance, through selective breeding for readily observable variations like extra claws. In fact, he described breeding programs to enlist the efforts of "those who love to find in their poultry-yards amusements conducive to the progress of natural knowledge."[40] He claimed to have done such systematic crosses with his own chickens, but coyly refused to disclose the results. His readers continued to send him material, some of which made it into the second edition of the book. Réaumur's impulse to collect stories and specimens of hybrids, as well as to proceed with his own breeding experiments, brought him up against recalcitrant evidence for bilateral inheritance. He managed to avoid the troubling conclusion, however, by focussing on the "curiosities" themselves – including wingless chickens, a North African beast of burden resulting from crossing an ass with a cow, and even six-fingered people. This last example, which corroborated Maupertuis's account of the Berlin family, was provided by an informant in Malta with a genealogy over several generations. Réaumur quoted the report in full with no comment other than "it does not seem favorable to the preexistence of germs."[41]

Buffon also devoted substantial effort to breeding across species (wolves and dogs, for example), not so much to show how traits were passed from generation to generation, but to map out relations among species. His programs for breeding agricultural animals (within species) were intended to counter what he saw as the natural degeneration of animal forms if left to themselves. "In order to have beautiful horses, good dogs, etc., it is necessary to give foreign males to the native females, and reciprocally to the native males, foreign females; failing that, animals will degenerate ... In mixing the races, and above all in renewing them constantly with foreign races, the form seems to perfect itself, and Nature seems to revive herself."[42] Nature is flexible, with the potential to be molded by environmental factors, including human intervention. Buffon argued that people can use the forces of nature to shape animals to their purposes. Breeding for specific traits (as in pet dogs, for example) required constant attention, however, to avoid reversion to the original forms.

The Paris physician Charles Augustin Vandermonde incorporated Buffon's theory of organic molecules into a system for improving the human species through application of principles of nutrition, education, hygiene – and breeding. Vandermonde played on contemporary concerns about declining population and degeneration by prescribing healthy practices for everything from the choice of mate to engendering vigorous offspring to the care of pregnant women and the feeding and education of children. Since, following Buffon, both male and female contribute equally to the offspring, they must be well matched to complement each other. Just as animal stock is strengthened by crossing with natives of other regions, he says, humans benefit from mating with people from different climates, who will necessarily have different constitutions. When people move from the provinces to the city and intermarry with urban dwellers, the result is

39 Réaumur ([1749] 1751), p. 457.
40 Ibid., p. 467.
41 This discussion about crosses in chickens was added to the second edition of Réaumur's book. (Réaumur, *Art de faire éclorre* ..., 2nd ed. (Paris 1751), p. 376.)
42 Quoted from Spary (2000), pp. 113/114.

improvement in strength and health of the city stock. Further, he implies that human agency could easily be put to work improving its own species, by analogy to animal breeders who “create new races of dogs, cats, horses.” “We can easily see that one could perfect animals by varying them in different ways. Why should we not work also on the human species? It would be just as possible, ... in combining all our rules, to embellish men, as it is routine for an able sculptor to cause a model of beautiful nature to emerge from a block of marble.”[43] Here the breeder appears as an artist, molding active matter to do his bidding. Vandermonde’s book reads as a handbook for healthy living; he assumes his readers will take an interst in shaping their own progeny. While he does not indulge in fantasies about cross-species mating, his rules for choosing suitable partners from other climates dictate a kind of subtle hybridization, or at least mixing. The program rests on a mechanics of heredity inspired by Buffon’s organic molecules, whereby specific traits materialize in oscillating spiral-shaped particles that wind together selectively to make up the germ of the offspring. These operations at the submicroscopic level, well beyond the reach of direct observation, locate macroscopic properties firmly in the stuff of organic matter with its resilience, activity, vibrations and self-propulsion. Vandermonde adapted Buffon’s version of active matter to his own more medical vision of prescriptions for human betterment where heredity is key.

In the realm of satire, boundaries can be tested with even greater abandon. An anonymous pamphlet published in Paris in 1772 tells the “true and marvelous story of a “lynx-girl” who could see into solid objects. “I take the thing from an Englishman, and those gentlemen do not pass as very gullible, nor are they prone to immediately crying ‘Miracle!’” The author discusses the odd trait of the lynx-girl by analogy to hydroscopes, people with the ability to find water underground by seeing through the earth. He presumed that the trait was heritable, and therefore could be perpetuated by breeding. Inspired by Maupertuis’s speculations about cross-breeding, he goes on to suggest that the Royal Society and the Paris Academy of Sciences really ought to preside over (and pay the expenses of) the marriage and breeding of this girl, in order to produce more such gifted lynx-people. “There is no need to mention what advantages would result from a lynx race, for the good of humanity; what light [lumières], what vision, what insight, these living telescopes, born in the sanctuary and under the auspices of physics, could communicate to savants, the authors and the cause of their existence!”[44] He went on to calculate how long it would take for the trait to multiply in subsequent generations, and how useful these people, bred in academies would be for police work, for uncovering court intrigues, and so on.

Conclusion

I have tried to lay out the landscape of discourse about heredity and generation to highlight resonances with other claims – about active matter, about the origin of organization, about progress and change, and also about method. We are not surprised to see that experiments played an essential role, often as provocation for conjectural interpretation and rarely as conclusive or indisputable. While Haller, who has only come into my story as a critic of Buffon, implied that everything could be sorted out by multiplying careful and professional observations of embryos, a number of French writers at mid-century addressed the question of how to accommodate

43 Vandermonde (1756), p. 155.
44 *Histoire véritable et merveilleuse d’une jeune angloise...* (1772) p. 58.

method to mystery, and to the limits of human senses. If we are to understand their attempts to lay the foundations of a science of life, we should consider the role of hypothetical experiments, as well as conjectures and queries, and how all these theoretical modes interact with actual experiments and observation. When Réaumur, for example, imagines crosses designed to determine the location of the germ, but then refrains from reporting his results, does this count as experiment? How is this different from Diderot's goat-men or Maupertuis's hypothetical hybrids between exotic African jungle animals? Or Vandermonde's perfected humans? If we examine these speculative experiments alongside the breeding programs that traced abnormalities through generations of dogs or birds, we can read them as part of a program to get below the surface to general laws of nature and of life. Maupertuis articulated this desideratum in his critique of natural history (and here he had Réaumur in mind as the contemporary exemplar): "All our treatises on animals, even the most methodical, form nothing but paintings pleasing to the eye. To make natural history into a true science [véritable science] we would have to apply ourselves to researches that would allow us to know, not the particular figure of such and such an animal, but the general processes of nature in her production and preservation."[45] This comment is partly about designing experiments to ask the right questions, but it also points to a willingness to interpret them. Even d'Alembert addressed the question of the role of conjecture in his discussion of experiment in the *Encyclopédie*, where he says, "When I proscribe from physics the mania for explanations, I am very far from proscribing that spirit of conjecture, which at once timid and enlightened sometimes leads to discoveries: ... that spirit of analogy, whose wise strength pierces beyond that which nature seems to want to show, and foresees facts before having seen them."[46] For many thinkers at mid-century seeking access to what Buffon called "the hidden means that nature might be employing for the generation of creatures," the complementary use of conjecture and experiment was the only viable method.

Department of History, University of California, Los Angeles

[45] "Tous ces traités des animaux que nous avons, les plus méthodiques meme, ne forment que des Tableaux agréables à la vue: pour faire de l'histoire naturelle une véritable science, il faudroit qu'on s'appliquat à des recherches qui nous fissent connoitre, non la figure particulière de tel ou tel animal, mais les procédés généraux de la nature dans sa production & sa conservation." Maupertuis, *Lettre sur le progrès des sciences*, p. 418.

[46] "...quand je proscris de la Physique la manie des explications, je suis bien éloigné d'en proscrire cet esprit de conjecture, qui tout-a la-fois timide & éclairé conduit quelquefois à des découvertes, pourvu qu'il se donne pour ce qu'il est, jusqu'à ce qu'il soit arrivé à la découverte réelle: cet esprit d'analogie, dont la sage hardiesse perce au dela de ce que la nature semble vouloir montrer, & prévoit les faits, avant que de les avoir vus." D'Alembert, "Experimental," in Diderot and Alembert (1751-1765).

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Duchesne's Strawberries: Between Grower's Practices and Academic Knowledge

Marc J. Ratcliff

INTRODUCTION

This paper studies the history of eighteenth century hybridization and genealogy while looking at the interaction of growers' practices and theoretical botany. I discuss a case-study based on the discovery of a new vegetable species during the second part of the eighteenth-century. Antoine Duchesne, a Paris gardener and botanist, discovered an undescribed form of strawberry in 1766. While a certain work was done by James L. Larson on Koelreuter and Linnaeus' experiments on hybridization, the secondary literature did not pay a lot of attention to Duchesne although the story displays a much revealing example of the strategy a scholar elaborated to deal with new species. Indeed, the interest of this discovery lies at least in the fact that Duchesne attempted to understand this new production of "nature and art", the new sort of strawberry, thanks to a model labeled genealogical tree. Thus through Duchesne's work, hybridization crosses genealogy. The main epistemological and historical points are to understand what does genealogy mean, and to what extent is this a sign of a modern issue of genealogical tree as defined later by Darwin, Haeckel and de Vries and, eventually, what are the influences that shaped this story.

After a brief description of the discovery reported by Duchesne, I focus on what did historiography with this story. Nineteenth-century biologists and twentieth-century historians made various uses of Duchesne's strawberries. When using Duchesne's species, nineteenth-century evolutionists show outspoken ideas yet different from that of the historians of biology and classification. The historians of classification and evolutionism have slightly discussed Duchesne, who was neglected by the tradition of the history of the biological thought as outlined by Jacques Roger, Elizabeth Gasking or John Farley. Yet, the recent comments on Duchesne placed contradictory interpretations on the meaning of the genealogy. My paper aims partly at bringing more data on what does "genealogy" and genealogical classification mean for a breeder and botanist in the second part of the eighteenth-century.

I examine the several factors that determine this story in order to explain where does such a conception of genealogy come from. Putting Duchesne's ideas in context thus means to investigate on his working milieu, the continent of practitioners: breeder, gardeners, growers, florists, a world close to that of cattle farmers. Two problems may need investigation. First, what are the current and shared practices of hybridization in the agricultural and "domestic economy" milieu which could have influenced Duchesne's experiments and conception? Second, what are the differences – of object, practices and conceptions – between this milieu and the botanical academic milieu which discusses similar questions. One shall examine the methods of hybridization used by growers and florists, before attempting to define the borders between botanists and breeder-growers-florists. This shall bring us to more precise definitions of the terms *species* and *varieties* according to the "professional" contexts using these concepts. Indeed, it seems that the definition varied according to the use each community made of it, though certain concepts were shared by botanists and practitioners.

Another problem relates to fixity, history and genealogy. First, in relation to species and classifications, the status of time was at stake in the debates on varieties and species. A fixed species is conceived as being out of time, while an organism suddenly appearing raises the question of time and of a constancy of species. This issue was much discussed by the classical botanists such as John Ray, Robert Morison, Jean Marchant and Linnaeus, and Duchesne did not evade it. Second, time is also related to history, and the use Linnaeus, and Duchesne made of the history of botanical species was new in contrast to the descriptive tradition of botany. Including time in the history of botany shows a new way of reading history, which relates to the changes from a narrative to a temporal history. Third the use of genealogy in Duchesne seems to have been influenced by human genealogy and I shall use context clues consistent with this interpretation. All this should enhance our view of the origin and function of the genealogical tree and classification in Duchesne, while putting greater emphasis on a synthetic moment that made the field of practitioners and botanists crossing each other.

1. Constructing the discovery

In 1763, a young Paris grower and botanist, Antoine-Nicolas Duchesne (1747-1827) discovers an unknown kind of strawberries in his Versailles garden. Unknown with regard to its morphology, this plant had not been described by previous botanists. The main morphological characteristic of the plant was its monophylla leaves, with only one lobe, while leaves of common strawberries contain three lobes (fig. 1).

Fig. 1 *Fragaria Monophylla* by S. Edwards. From *The Botanical Magazine* 2 (1788).

At the age of seventeen, in July 1764, Duchesne who had published a small handbook of botany, “personally presented King Louis XV with a pot of strawberries.”[1] Thanks to a particular hybridization, the result was spectacular and the King decided to patronise him. “He authorized Duchesne to raise more F. chiloensis in the royal kitchen garden at Versailles and to collect all varieties of strawberries known in Europe for the Trianon garden.”[2] Such a high patron opened him many doors. Tutored by Bernard de Jussieu, who had helped him from the beginning, he wrote to Haller, to Linnaeus, to other French and European naturalists to ask for seeds and specimens.

After he made the plant reproducing from budding, Duchesne collected the seeds obtained which he sowed in 1764. Two weeks after, the first strawberries appeared and he was much astonished to find that they had reproduced from the seeds. This was indeed one of the features of a stable species, constant in time, and the plant was yet undescribed. Duchesne dispatched the seeds obtained and asked the scholars to sow them in order to repeat the experiment. Sowed in other places the seeds stubbornly reproduced the same plant. On this series of fact, Duchesne launched a two years research that led him to publish his 1766 *Histoire naturelle des Fraisiers*. Still non extant for the botanist community, the plant had to be at least baptised. It was named, the year after Duchesne’s book, *Fragaria monophylla*, the latter term being not used by Duchesne. Linnaeus was, at this time, at the apex of his career, for he had been recently elected a foreign member of the Paris *Académie des Sciences*.[3] In *Philosophia botanica*, Linnaeus had prescribed strict rules that enabled only “orthodox Botanists” to give the “right name” to a plant.[4] This secured him and a few other privileged botanists against amateurs, while himself and other emergent professionals shared, among other, the power of naming plants. In the 1767 twelfth edition of *Systema Naturae*,[5] Linnaeus thus named the new plant *F. monophylla*, a name later currently used, for instance by Abbé Rozier in his entry “strawberry” of his 1782 *Cours Complet d’agriculture*.

The only plate in Duchesne’s book illustrates a “genealogy of strawberries” (fig. 2) in which the author classifies ten kinds of strawberries thanks to a genealogical key.[6]

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1 Darrow (1966), ch. 5, section “Duchesne and his work.” (I quote Darrow from an Internet version without pagination).

2 Darrow (1966), ch. 5, section “Duchesne and his work.”

3 Larson (1994), p. 17.

4 Linnaeus (1751), §211. Orthodox Botanists were those “who established their method on a real base, such as fructification” (Ibid, § 26), i.e. a few botanists among whom was Linnaeus.

5 Linnaeus, Systema Naturae (1767) II, 349.

6 Duchesne (1766a), p. 228, pl. I.

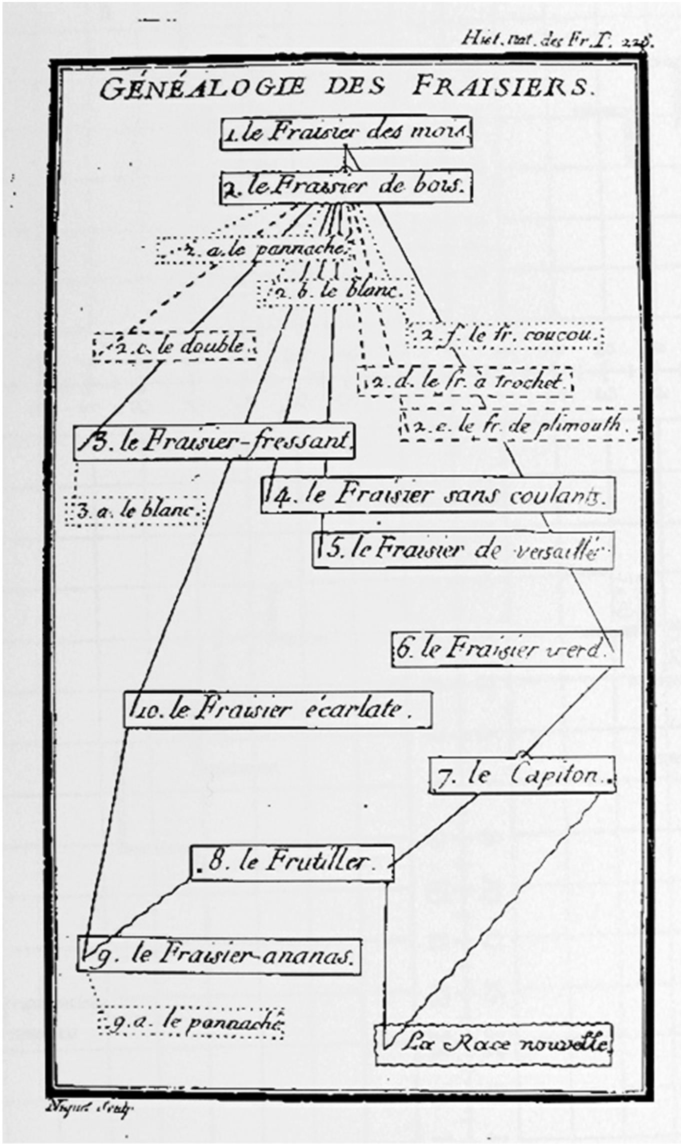

Fig. 2 Genealogy of the strawberries according to Duchesne.

2. DUCHESNE'S STRAWBERRIES IN THE HISTORIOGRAPHY OF SCIENCE

On the basis of the genealogical plate, scientists and historians have regarded Duchesne as an early representative of mutationism and evolutionism, while other historians have rejected the presence of a concept of genealogy in the book.

The relationship between the discovery of new species in the eighteenth-century and the framework of heredity and generation was poorly discussed by historians. Jacques Roger, Peter Bowler, John Farley, Walter Bernardi, Roselyne Rey who have studied the interplay between generation, species, and sometimes, heredity, have mainly looked for a theoretical debate of ideas that included Maupertuis, Diderot, Buffon and Bonnet's speculations.[7] Notably much work has been dedicated to the debates on teratological cases that illustrate both imaginary and detailed conceptions of heredity, for instance in the works of Jean-Louis Fischer and of Javier Moscoso. But, the theory-oriented approach was much interested in animal and human field, and it has notably set apart the influence practitioners had on the debate over heredity and new species. On the other hand, the historians of eighteenth-century botany and natural history have focused more on Linnaeus and Jussieu's works. Yet the question of new species has been mainly discussed through the example of plant hybridization for Linnaeus on the Peloria, Adanson's conceptions, Koelreuter's hybridization, and Lamarck's view.[8]

One has to look to another field – the history of classification, evolutionism, and breeding – to find a place for Duchesne's strawberries in the history and historiography of science. Historians of breeding have no problems in declaring that Duchesne's work represents the starting point of the modern strawberry.[9] Certain scientists have discussed the relevance of the species itself *Fragaria monophylla* to the issue of mutation and new species, while historians have emphasised Duchesne's *conception* of species, fixism or mutationism, grounded on his genealogical classification. Nineteenth-century scientists in particular have constructed a historical narrative that includes Duchesne's study into a whiggish history. Discovering *Fragaria* and the why and how of new species was less examined than the so-called anticipation of modern evolutionism based on the genealogical tree. Alphonse de Candolle (1806-1893), a Swiss botanist who wrote one of the first sociological studies of science, is among the first to quote Duchesne as a forerunner of Darwin's genealogical definition of species.[10] On the other hand, Hugo de Vries (1848-1935), who defended a mutationist version of evolutionism in his 1900 work on the mutation of species and varieties, had included *Fragaria monophylla* among various examples of mutations remarked by previous botanists. He noticed that a similar example of *F. monophylla* was represented in a painting by Holbein (1495-1573). De Vries' was an argument showing that the same mutation had occurred in various places and times.[11] He used it as a rationale useful to his scientific programme, assigning the value of a proof to a historical example. This use seems to be different from that of Candolle who regarded Duchesne's model as an anticipation of evolution, for De Vries did not comment on Duchesne's genealogical tree. Still, not much time later, his French translator, L. Blaringhem, published a book on mutationism in which he developed the right same example. However he did not content himself to discuss the presence of *F. monophylla* as a mutation vouched for historically by several authors, but considered Duchesne to have been a supporter of mutable species. To Blaringhem, Duchesne's questions "show that he had, at least since 1766, the very precise notion of the sudden transformation of forms (...) he refused to believe in the fixity of species".[12] This was closer to Candolle than to De Vries's thought.

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7 Roger (1963), Bowler (1973), Farley (1977), Bernardi (1986) and Rey (1989) have usually not much discussed botanical works, and have almost neglected topical authors for the question of the new species such as Marchant, Linnaeus, Adanson, Gleditsch, Koelreuter and Duchesne.

8 Stafleu (1971), pp. 261/62, 305/06; on Koelreuter, see Larson (1994).

9 Darrow (1966), ch. 6, section "Breeding in France 1770 to 1900."

10 De Candolle (1881).

11 De Vries (1909), p. 378.

12 Blaringhem (1911), p. 8.

Later this interpretation of Duchesne became topical in Emile Guyénot's 1941 standard French handbook of history of biology. Guyénot, himself a biologist, followed Blaringhem's reconstruction, but was disturbed by the use Duchesne had made of the term race, and added in a footnote: "the author uses frequently the term race with the meaning of species, and the term species with that of genus".[13] And Guyénot quoted Duchesne, who wrote "thus there are new [species]".[14] Eventually, through recent literature other options brought up concerning not Duchesne's conception of species, but his ideas on genealogy. Pascal Tassy notably devoted a book to the issue of the historical construction and to the function of the genealogical tree in modern biology. He considered Duchesne was the first to make use of a temporal genealogy for a classification of living organisms.[15] On the other hand, Giulio Barsanti's study of the images and representations of classification up until the time of Darwin highlights another conception of Duchesne's genealogical tree. Barsanti regarded Duchesne's use of a genealogical tree as a misconception. For Barsanti, Duchesne's is not a genealogical tree, it has no bough, no bifurcation and intermediary species, and is furthermore turned over.[16] According to Barsanti, Duchesne's use of a genealogy stems from misunderstanding this notion, treated in a obscure and "misterioso" way.[17]

It appears thus that secondary literature offers several opinions on the particular meaning of the "new species" for Duchesne. In front of so opposed ideas from historians of cladism and of classification, one shall look at the sources.

If the secondary literature is not outdated, there is nevertheless no study on Duchesne that discusses its apparition in the second half of the 18th century – what means the publication of a whole book on a new strawberries in 1766? More particularly, I could not find studies on the origins, contexts, social network, sources of inspiration and continuation of this enterprise. Such a work is a preliminary historical investigation before explaining Duchesne's conceptions, and their influence on the realm he addressed. In order to understand Duchesne's enterprise, I shall follow several lines and hypothesis:

a. Duchesne's work stems from a *hybridization* of two particular "traditions": the grower and breeder's practices, and the academic botany. I thus examine both traditions, show their relationships, in order to understand what were their respective contributions to Duchesne's enterprise.
b. Once identified the origin of certain practices and ideas, it will be more easy to analyse and display Duchesne's conception of genealogy, species, race, heredity, etc., in order to characterise his own contribution.
c. I shall address the problem of the impact this enterprise had on the second half of the eighteenth-century practices and knowledge of natural history, notably on two points: what became the new strawberries, and what was Duchesne's impact on heredity and new species.

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13 Guyénot (1941), p. 376n.
14 Guyénot (1941), p. 376; Duchesne (1766), p. 13.
15 Tassy (1991), p. 26. "avec la publication d'une véritable généalogie d'êtres vivants qui se veut comme telle, à la manière des fraisiers de Duchesne, apparaît la notion du temps (...)." Barsanti (1992), p. 85.
16 Barsanti (1992), p. 84.
17 Ibid., pp. 84/85.

3. Botany and Practitioners in the mid Eighteenth-Century

3.1 Grower and breeder's practices: morphological changes of plants

G. M. Darrow has given a comprehensive account of the history and breeding of strawberries in England. Among the wide variety of growers and breeders' practices, I shall characterise those affecting morphological changes of plant during the years of Duchesne's enterprise. These practices established a common background for growers, florists, breeders – and cattle farmers – from the seventeenth-century onwards; they had been progressively improved along several treatises on husbandry, gardening and agriculture. At the time of Duchesne, the leading publication providing information on empirical practices in France was *Journal économique*. A monthly journal created in 1751 by a group of people favoring practical economics and agricultural topic, it was partly controlled by the *Académie des Sciences* through the well-disposed censorship of Jean-Etienne Guettard.[18] The *Journal économique* excluded non-applied scientific research, and served as a link between applied knowledge and civil society, for subjects such as natural history, domestic economy, law, public health, navy, geography, technology, and, of course gardening, growing and breeding.

The growers of the mid eighteenth-century distinguished several methods that caused morphological changes in a plant or an animal. One of the concrete problems was, for the florists, to transform a specimen with simple flowers into a specimen with double flowers, or in other words, to double or even triple the number of petals. The simple flowered and non cultivated specimens had been described by botanists with the minimum number of petals. However, sustained by adequate cultivation, they could be converted into double flowered specimens, with a great increase in value for the rare ones. This was not just a joke invented by a few gardeners. In the seventeenth-century and early eighteenth-century, the price of certain rare tulips was so high that the Dutch government published a law regulating the market, to avoid certain people losing everything for a couple of flowers... In the mid eighteenth-century the German and Dutch florists and growers distributed catalogues of their best specimens of carnations and other flowers all over Europe.

Several methods were used by these practitioners to transform a flower from simple to double. Soil transplantation was one of these methods, and especially frequent transplantation from a soil to another without blooming was regarded as being much successful. Such method echoed that of the cattle farmers. When a sheep flock was seen as "degenerating", being too meagre and small – "like rabbits"! – farmers imported a more than three-years old strong ram from another region to cover the sheep. An experiment was said to have been carried out by a farmer in Limousin who used the latter method, and, in about three years, "multiplied by three the income of the wool".[19] While the growers and florist had in mind to transform the shape of flowers, the cattle farmers experimented on the selection of domestic animals with the aim of "improving the species."[20]

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18 In the history of the relationship between Paris *Académie des Sciences* and French societies directed to utilitarian practices, the first *Société des Arts* created in the 1720s in Paris failed apparently because of rivalries with the *Académies des Sciences* (see Hahn (1971), pp. 109/10). However this situation was not reproduced with the *Journal économique* and Guettard seems to have been well-disposed toward the group of people that led it.

19 Puismarais (1754), p. 74

Certain horticulturists wondered if it was possible to "treat the young trees in the same manner as the flowers, in order to change the structure of their organs, and create, so to speak, double species".[21] Notably, there was a list of "good species" in which transformations were easier to carry out: anemone, auricula, carnation, crocuses, daisy, larspur, hepatica, primrose, tulip, violet and wallflower. It was also currently admitted that if a double flower ceased to be cared for, it returned to the state of simple flower.

The second example of changes in plants was well-known in the eighteenth-century, and concerns the transmission of colors in tulip and other flowers. Indeed, "when cutting all the stamens of a red tulip before the emission of dust, and when powdering the stigma of this same tulip with the dust of the stamen of a white tulip", the results was the following: "the seeds obtained were red for a part, white for another part and red and white for the last part". However no quantification of the result was observed or reported by eighteenth-century scholars. This example was reported by Adanson who presented it as the result of a basic hybridization of varieties of the same species.[22] Perhaps because it was a too much known example – which is what Adanson says – careful experiments on hybridization are not frequently reported in *Journal économique*, contrary to their place in academic *Mémoires*, like those by Koelreuter and Spallanzani. However, hybridization was certainly used by growers and breeders. Yet, to a large extent based on tacit knowledge, heredity was embodied in various practices. Except the word *degeneration*, I could not find other particular names (such as "atavism", "reversion", "heredity" used in the nineteenth-century) referring to the return to the natural condition of the specimen. The language for hybridization is not particularly technical.

The positive impact of human art on nature was a common model by which florists, growers and cattle farmers understood their own practices of transforming the sorts and species.[23] As shown by historians of gardening, a shared project of the practitioners was to push nature out of its limits, including morphology and calendar rhythms. In the case of plants, not only the morphological properties of the organisms were affected, but even their blooming season. Indeed certain methods displayed in *Journal économique* made particular flowers (hyacinth and others) blooming at a precise date, to serve for private parties and Christian festivals.[24] However, such positive impact of art on nature was limited by teratological considerations, notably when produced by man hands. Grafting was a technique used to change either the morphology or color of certain species, for instance an anonymous was able to produce "green or yellow roses", thanks to a graft.[25] In several papers, authors reported various grafting "experiments," through which a horned cock was obtained, or a pear-apple, or seedless cherries, etc.[26] For some of these productions, the authors discussed the question of constancy, yet they usually regarded them as monstrous productions with no possibility of sexual reproduction.

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[20] Anonymous E (1758).
[21] Anonymous I (1761), p. 172.
[22] Adanson (1763), p. cxiii.
[23] Anonymous F (1758), p. 500.
[24] Anonymous G (1760), p. 335.
[25] Anonymous E (1758), p. 170.
[26] Anonymous D (1757); Anonymous H (1761); Anonymous I (1761).

To understand Duchesne's enterprise, it is necessary to remind that he knew a great deal of the breeder, florist and grower's practices. Notably he quoted several authors of this "tradition" and his hundred page of domestic economy section shows that he was himself a grower. This will help us to establish the contribution of the grower's tradition to his own enterprise. For instance, Duchesne knew well a town particularly renowned in *Journal économique* as an important essay and market place for breeders and florists: Montreuil. The Montreuil community of growers was famous for its products supported by horticultural and agricultural experiments. This small town close to Paris was especially known for its practices of selection of new races of fruits, notably peaches and strawberries, which dated back to the first part of the seventeenth-century.[27] Duchesne had taken much of his domestic economy section from "the practices of the grower traders of the town of the Wood [from Boulogne?] and of Montreuil".[28]

As a synthesis of the grower and breeder's practices, it is worth saying that one could hardly identify a single tradition of gardening and breeding in the Ancien Régime. The know-how depended on the country, and on the type of plant or animal breeding implemented. Silkworm was present in Italy and France since the Renaissance, while breeding flowers occupied much place in the Dutch Republic and in certain areas of Germany. Moreover, new trends were developed for gardening, for instance the expansion of the English home gardening between 1750 and 1850.[29] Thus it is perhaps better to speak of the diversity of grower and breeders' applied practices, opposed to the monolithic system of the botanical tradition. Yet, the diversity among practitioners had nevertheless a unity which was confined geographically to particular places. Indeed Paris was, during the eighteenth-century and up until the middle of the nineteenth-century, a city surrounded by many small towns which produced goods and especially vegetables. They fed the capital bringing the goods to the Halles. There are scenes of the Parisian life of *les Halles* described by Balzac that show the wide circulation of vegetable goods in Paris during the first half of the nineteenth century. From the seventeenth to the eighteenth-century, many of these surrounding villages were material to the training of growers, breeders and practitioners as well. Rozier's *Cours complet d'agriculture* reports that young apprentice florists complied with masters from many of these towns which had a specialised production of vegetables. Montreuil was specialised in producing fruits and particularly peaches, while other such as Montmorency, Bagnolet, Vincennes, Charonne, etc. cultivated specialty fruits. This environment gave its bedrock to a localised tradition of practitioners as opposed to the international tradition of botany. The regional tradition was destroyed before the end of the nineteenth century with the growth of Paris and the railways that facilitated transport of goods from other parts of France and developed other markets.

3.2 Varieties and new species between botanists and practitioners

Even criticised by Linnaeus since the 1730s as a tower of Babel, the botanical tradition was certainly much more united than were the florists and growers, who were usually organised in local guilds. Botanists worked within an international network. Indeed the methodological rules

27 Schabot (1758), pp. 75/76; Rozier (1787), p. 56.
28 Duchesne (1766a), p. x.
29 See Lustig (2000).

and skills of communication, notably the international use of Latin, enabled a network of scholars stretched over Europe and the world to collect plants and information, to give plants a place – in any classification or in the *Systema naturae* – and to spread botanical information. As well, an important difference between botanists and growers is that the botanical tradition kept its memory alive, listed hundred of names, while among the growers and practitioners, such a relation to memory was much less vivid. They wrote papers in journals, treatises, dictionaries of gardening and *Traité d'Agriculture*, but except for a few heroes such as Thomas Fairchild (1667-1729) or Olivier de Serres (1539-1619), the vast majority remained actually anonymous until the second half of the eighteenth-century.

The new species question was also an affair of controlling geographical areas. However it was a type of control different from the strategies and know-how of the practitioners. When *Peloria* was reported to Linnaeus, when *Fragaria Monophylla* appeared in Versailles, both were considered undescribed species, which is not saying they had a recent origin. Adanson and other travelers described hundreds of new species during their travels out of Europe, but they did not consider them as *new* species. There are actually two ways of conceiving the novelty of a species, which depended much on the extant and previous control of an area by botanists. In an area already checked by a centuries-long exploration, like Europe, any undescribed species should appear as a new one, which was of course not the case for species abroad. The new species problem reveals us something of the geographical control by botanists, a territory into which worked nevertheless also the practitioners.

It is known that Linnaeus' specific project was to unify his "army of botanists" thanks to institutional means as well as guideline books such as *Critica botanica* and *Philosophia botanica*.[30] In these books he notably formalised the botanical rules, for nomenclature and the morphological language, addressed to the community of the botanists. Franz Stafleu, Jean-Marc Drouin and Pascal Duris have stressed both the sociological model supplied by Linnaeus for botanists and his social impact on the formation of a new type of scientific society at the end of the eighteenth-century. Moreover, Linnaeus was also very attentive in working economically, reducing the number of species while eliminating varieties from the botanist's realm. Indeed, while, in 1623, Bauhin had described about 6000 species, Ray 18'500, Linnaeus reduced the number to 7000. The barrier towards varieties he raised pushed dramatically the study of variations back to nature and put it in the hands of the florists and growers, a fracture that strengthened the distinction between the two professions.[31] Furthermore this impetus helped to forge the botanical memory and identity for it extended what John Ray said half a century before, that botanists should not be interested in varieties. The economic way of treating botany by Linnaeus entailed the elimination of sub-species, but, when discarding varieties, Ray and Linnaeus had touched a level other than organising the botanist's duties. They actually blocked any access to "sources of variation" and avoided bringing confusion between things that vary (varieties) and things that, as a rule, do not vary: species and genera.[32] Rejecting the varieties out of botany was a sort of political and theological act that eliminated variation from the botanist's cabinet, whose role was to lock up the myth of the fixity of the species – or genus. Thus, the world of growers and breeders, whose role

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30 Stafleu (1971).
31 Linnaeus (1751).

was to play with varieties, sorts, hybrids, etc., was clearly set apart from *Systema Naturae*.

Four events appeared in the story that disturbed this decision of Linnaeus. First, the definition of species by Ray himself was a sort of genealogical definition. Second, the fixist system was seriously challenged by several plants that did not obey to Linnaeus' rules, such as *Mercurialis* from Marchant, *Peloria* from Rudbeck and Linnaeus himself, and *Delphinium* from Gmelin. Third, certain botanists, such as Michel Adanson, considered that varieties shall not be neglected by botanists, a position that appealed for a massive work of re-classification Adanson was not able to carry out alone. It nevertheless gave birth to the latter "evolutionary" theory.[33] Fourth, Duchesne's strawberries are a kind of return of the repressed voice of the florists, breeders and growers, who acquire their meaning, partly from being placed at the exact cross border which Linnaeus wanted to remain a no man's land. Duchesne pushed further the exploration of this no man's land. He had published, at the age of seventeen, his 1764 *Manuel de botanique*, that used both French and Latin Linnaean nomenclature, and dealt only with the utility of plants for feeding, medicine, arts and decoration.[34] In *Histoire naturelle des fraisiers*, the true border was the question of the *constancy*: Botanist worked with species and rejected varieties which were not constant. This was clearly stated by a botanist such as Chrétien Guillaume Lamoignon de Malesherbes in the early 1750s: "All these alleged species [pears, peaches, carnation, tulips] degenerate when one wants to multiply them from seeds; consequently they are only varieties for botanists".[35]

It is a question whether the disciplinary boundaries of botany were strong enough to make someone considering himself a botanist even working on varieties, such as Adanson or Duchesne. Especially, many voices gathered at this time to agree with Linnaeus, but what about the botanists who defied the "taboo"? As noticed, Duchesne was a grower, as his discussion on Montreuil and other clues testify, but he was also a botanist, son of a grower and botanist. One shall stress Duchesne's botanical skills, close to those of Linnaeus: use of Latin name, morphological description of species, use of classification, historical knowledge, quotation of botanists, correction of "bad description", etc. Duchesne followed a similar method to that of the botanical tradition: a systematical and historical survey, for a particular species, of its name, description, morphological characters, differences, reproduction, cultivation, synonymy, authors, geography, to which remarks were added. Such a schema is as old as the Renaissance botany and natural history. Other works show that he was deeply rooted in natural history tradition. The strawberries

32 It is not clear to me what is the main *fixed* unity in the eighteenth-century botanical tradition, either the genus, the species or both. Such that the relationship of genus and species to nature was considered to be not arbitrary, while classes and orders could vary according to the system and methods of classification used. The diversity of systems of classification, that touched on orders, classes, section and other subdivisions, should not, as a rule, touch on the genera. In a text written in 1749 as a general criticism toward Buffon's first two volumes of *Histoire Naturelle,* Malesherbes (*sur l'histoire naturelle*, I, 43) defined well the role of the various systems, "which are not made in order to *establish* the genera, but only to *order* [ranger] the genera already established" (Malesherbes ([1798] 1971), I, pp. 115/116).

33 Adanson claimed his evolutionary conception in his 1763 *Familles des Plantes* He indeed admitted thus the possibility of creation of new species: "one could perhaps apply these examples to a number of insects, shellfish and worms, which would demonstrate the possibility of the mutations [mutations] or of the creations of new species in animals, as it seems proved that there are new species in plants showing to be not immutable" (Adanson (1763), p. clxii). See also Atran (1993), p. 306.

34 Duchesne (1764), p. xv.

35 Malesherbes ([1798] 1971), I, p. 31.

particularly lead him to correspond with Linnaeus, Monti in Bologna, Allioni in Turin, Haller in Bern, other European botanists and collectors, and he acknowledged Bernard de Jussieu for the classification used in *Histoire naturelle des fraisiers*.[36] The classification of the strawberries in the Rosacea family was used in *Jardin de Trianon* and Jussieu had authorized Duchesne to publish an abridgment of this family.[37]

The management of variety and variation by the botanical tradition and by grower-breeders reveals much of the tensions among botanists. Adanson remarked that "modern botanists do no agree with these changes, which are, to speak properly, only varieties more characterised".[38] This led him to criticise the definition of species proposed by Buffon: defining the species as the product of a sexual generation worked for superior animals and vegetables. But, what about the other species that reproduced through parthenogenesis, budding, section, and hermaphroditism? "What about those whose individual produce varieties that change at each generation, or that will be fixed during several generation?"[39] In term of Linnean botany, such a question did not have a lot of meaning, or perhaps better, it was cast out by the Linnean rules. Indeed, since varieties should be the object of practitioners such as gardeners and florists, they were excluded from the botanical discipline, as written in *Philosophia botanica*.[40] Still, Linnaeus cast doubts on the constancy of the species during a decade after he had described the Peloria. Of course, the practitioners had not waited the *Philosophia botanica* to work on varieties and on the various methods of generation. The local tradition of the practitioners probably was enough unified to work with certain routines on varieties and races. Still, much probably, there was nothing such as a systematical search for new varieties appearing through the techniques of hybridization, although several example vouch for the existence of the latter practice. Duchesne was to treat much of these problems in several remarks of *Histoire naturelle des fraisiers*.

4. Duchesne's Enterprise: Culture, History, and Genealogy.

In his book, Duchesne first gives a historical survey on strawberries, including the changes due to the climate and other causes. Then he describes the various species of strawberries, placed within their family (Rosacea) and compared with other genera of the family. Duchesne thus begins to study each particular race, including morphological differences and history, "notifying at the same time what race seems the older, and concerning the new ones I indicate those among which they came from".[41] This is followed by a summary of the characters particular to each race together with their genealogy. The section on domestic economy of strawberries includes reflections on art, ornamental, gardening, growers' practices, harmful insects, recipes, remedies, etc.[42] *Histoire naturelle des fraisiers* contains a second part, distinguished by a separate pagination. Duchesne

36 Duchesne (1766a), pp. vii-viii.
37 Antoine-Laurent de Jussieu who worked with his uncle Bernard published his major work *Genera plantarum* (1789), a foundation for the natural method, see Stevens (1994).
38 Adanson (1763), p. clxii. "les botanistes modernes, ne conviennent pas de ces changements qui cependant ne sont, à proprement parler, que des variétés seulement plus marquées."
39 Adanson (1763), p. clxiii.
40 Linnaeus (1751), § 306: "Botanists have nothing to sort out about varieties."
41 Duchesne (1766a), p. ix: "avertissant en même temps quelles races paroissent les plus anciennes, et indiquant pour les nouvelles celles dont elles ont pris naissance".
42 Duchesne's plan takes place at pages viii-x.

added there a hundred pages containing five "particular remarks", intended to develop "certain interesting points, too much weighted with details [which] I could not discuss as much as they deserved".[43] The five remarks deal respectively with the question of the constancy of the race, the definition of varieties, species and genus, the existence of hermaphrodite strawberries, the comparison between hybrids in plants and animals, and the synonymy.[44]

4.1 Of species, races and constancy

The question of the limits between the species, the variety and the race is topical both to understand the relationship between botany and practitioners, as well as Duchesne's enterprise. In his first remark, he addresses the influence of the method of generation (sexual or budding) on the constancy of a race. Commenting on a passage from Miller's *Gardener's Dictionary*, Duchesne emphasised that only the sexual reproduction allowed certain races and varieties to appear and to become constant, while budding and even grafting never give birth to a species, because the plant obtained was an extension of the same specimen. Such that it could not change. For these plants, the second generation – thus the new specimen – does not usually produce seeds.[45] This distinction, used by practitioners, could be useful to understand the limits between species and varieties. On these limits, the second remark focused stressing the status of the new strawberries: "is it a species? Then there are new species. Is it a variety? Thus how many varieties are there in other genera, considered to be species?".[46]

Through these questions, Duchesne opened the door to a redefinition of the species that expected to bring the fixed in harmony with the variable. As we saw above, several historians have considered that, for Duchesne, the race was a species, or that he had changed one term for the other. Nothing is less sure. Actually Duchesne adopted the botanical definitions of his time and was sharply clear on the distinction between species and race, the first being fix and the second mutable.[47] Duchesne himself criticised the usual confusion between race and species, taking the example of the human being.[48] Once established the necessity to eliminate the confusion, yet a second problem emerged, i.e. to distinguish the meaning of constancy for a species and for a variety. Indeed, constancy was a property of the species, and how could one deal with constancy for a race? The botanists had carefully erased the problem of the constancy of the race from their field of vision.

It was the discovery of *F. Monophylla* which obliged Duchesne to rethink the story as a whole and led him write that "cultivation and other accidental causes do not produce new species, but they cause, in certain individuals, some changes which, being persistent through their posterity, make new races".[49] Once admitted that new races could originate from stables species, the questions were two: first, how to secure the distinction between the race and the species; second,

43 Duchesne (1766b), p. 1.
44 Duchesne (1766a), p. xi-xii.
45 Duchesne (1766b), pp. 5/6, 8.
46 Ibid., 13.
47 Duchesne (1766a), p. 135; Idem (1766b), p. 14. The second remark comments on the "distinction one must establish of the fixed and invariable characters of the species, from the slight and changing differences of the Races; on the Constancy of the one and the Mutability of the others."
48 Duchesne (1766b), pp. 18/19.
49 Ibid., p. 21.

what is the relationship between the parent species and the race offspring? For the first problem, cross-breeding of various species allowed to decide which were the species, but it was a too long method – and it could raise particular difficulties – such that Duchesne reverted to consideration on an adequate morphological description. But a new plant – such as the *Peloria* – and new races such as *F. Monophylla* raised the unknown problem, and helped to cross the borders of the botanical world to penetrate into that of the practitioners, which made the system of the fixed species overturning. The cause of such a "mess", Duchesne saw it in the fact that many specific characters were not the true one.

Thus, what had been built on the *Peloria* – making hybridization a model to understand the origin of many new species by Linnaeus – was made "in small" with *F. monophylla*, raising attention to the conceptions and practices of growers and breeders. It was actually the practitioners, growers and cattle farmers, who employed the category of "constant variety", usually termed race. Buffon had used it in *Histoire Naturelle des animaux*, and, for Duchesne, this term needed "to be introduced into that of the vegetables".[50] The field in-between the classical botany and the grower's practices could be defined through this notion, because botanists maintained the fatal confusion where practitioners had eliminated it: "following Ray's axiom, since one can not label variety a constant race, they are named species".[51] Being both a botanist and a grower, Duchesne could easily admit constancy for certain types of strawberries and thus land on his feet, considering "all the strawberries as a species distinct from all the others, and each strawberry, taken particularly, as a race or a variety. [The reasoning] led me to look for their genealogy".[52]

4.2 A new reading of history?

What kind of influence did the botanical tradition have on Duchesne's enterprise? What were the routines and mental habits of a botanist in the second half of the eighteenth-century? Although there was a tendency to use vernacular languages for publication, botanists still knew Latin, used it for morphology and nomenclature of plants, as well as classification. They also made reference to a two to three centuries year old tradition. Which means that an eighteenth-century botanist had a particular relation to memory and, going back to the Renaissance, referred to names and morphological descriptions of plants recorded by previous botanists. Such relationship to history in the botanical tradition was much distinct from that of another important tradition, the experimental tradition, which takes its roots in the second half of the seventeenth-century.[53] And indeed, when one looks after the historical perception demonstrated by eighteenth-century experimentalists, they usually – with a few exceptions[54]– do not refer to works before the 1660-1680s. The line that separates the "Ancients" from the "Moderns" is roughly situated at the end or

[50] Ibid., p. 18.
[51] Ibid., p. 26.
[52] Ibid., p. 14.
[53] Many historians have studied the social, rhetorical and cognitive novelty both for physical and natural sciences; see Dear (1985), Shapin and Schaffer (1985), Bernardi (1986) and Licoppe (1994)."
[54] For instance certain Italian scholars such as Neapolitan (Bovi 1769, Cavolini 1785) still quote Aristotle, Galen or Pliny the Elder. But it is not the case of many experimentalists in the Center and North of Italy, such as Felice Fontana, Lazzaro Spallanzani, Bonaventura Corti, Maurizio Roffredi, Scarpa, who worked during the second half of the eighteenth-century.

the second half of the seventeenth-century, corresponding with the birth of Academies and the famous *querelle des anciens et des modernes*.

Such a line of separation did not exist in the botanical and natural history tradition. To speak only of the botanical tradition, although there are controversies of historians on the boundaries between the herbalist tradition and the botanical tradition,[55] at least, eighteenth-century botanists has in mind to consider *everything* of the tradition. Indeed Linnaeus' classification of botanists, his *Bibliotheca botanica*, and Haller's *Bibliotheca botanica* left place for Renaissance authors. Similarly when Adanson speaks of the botanical iconography, he begun his inventory with Corbichon and Cuba in 1482, and Leonicenus in 1491.[56] Of course, the plan for systematically surveying the ancient literature was both critical and cumulative. Critical in order to identify and separate copyists from creators; for instance among the 70'000 engravings of plants Adanson had identified, he regarded only 10'000 as original species, 60'000 were copies of the originals, and only 2000 to 3000 were "good" according to him.[57] The aim was also to establish a cumulative selection of data to avoid loosing information on authors, plants, figures, names, descriptions, classifications, etc.

While the reading of history was a duty in the botanical tradition, it was probably seldom carried out in the way Duchesne established in his book. An important point of rupture in Duchesne's thought was his temporal interpretation of a historical material. Linnaeus, who did it for the Peloria, probably influenced him.[58] Duchesne conferred a historical meaning to the observations on strawberries made by the botanists since Antiquity. An example of his method of reading the botanical history will make clear what I mean by temporal interpretation of history: "*In 1665*, Colbert (...) asked for the catalogue of the King's garden (..) They included the double strawberry. *At around the same time*, Morison also describes it (...) Father Barrelier, *who died in 1673*, drew it (..) it has been engraved *since*, and published *in 1714*. (...). Zanoni, *at the same time*, drew also it (...); it is in his botanical history published *in 1675* (..). It appears in Furetière's dictionary, *from 1690* (...) M. Haller quotes a description of it (..) in the memoir of Breslau, *July 1722*".[59] Much of Duchesne's reading of the history of strawberries is characterised by a focus on context, dates and places, and by a frequent use of temporal connectors.

This temporal history supplied Duchesne's with the historical ground for his genealogical conception, as showed in the following example: "From Plymouth where our strawberry seems to be born, it arrived in Leyde, then in Paris, and eventually in Bologna, where Zanoni saw it in 1675. It also stayed in England, its native country, for Ray said in 1686, he had cultivated it during several years in Cambridge. But since that time no one saw it".[60] Everything thus pushed Duchesne to reshape the relationships among races into a classification that would include the multiple pressures of time. The genealogical tree was the main and probably the most efficient model available for the job.

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55 See Atran (1993).
56 Adanson (1763), pp. lxxxi, cxlii.
57 Ibid., p. cxlii.
58 I'm indebted to Staffan Müller-Wille for this information.
59 Duchesne (1766a), pp. 77/79. My italics.
60 Ibid., p. 104.

4.3 From genealogy to experiment on new races

Duchesne's use of genealogy was probably enriched also by a socio-cultural influence, notably the use of the nobiliary conception and language. For instance even the term race seldom appears in the papers of the *Journal économique*,[61] which usually prefers to use "sort". "Souche" i.e. stock, stump, is used by Marquis de Puismarais to qualify sheep; he also employs the term alliance.[62] Other terms used by Duchesne point clearly at their origin in the language of nobility and human heredity. "Head of the race" was used by Buffon; "Offspring" (*postérité*) is frequent in Duchesne; "Head of the tree", "branch of the same house", "genealogical tree", "genealogy", "family", belong to a vocabulary borrowed to the nobiliary conception of heredity.[63] Duchesne had been among the first, after Adanson, to generalise the term family in his *Manuel de Botanique*. Another interesting clue for the relationship between hybridization and noble genealogy, is that many specialty fruits, and especially strawberries, received noble names: Duke of Kent's Scarlet, Comte de Paris, Princesse Royale, Duc de Malakoff were, among others, names given to new varieties of strawberries during the second part of the nineteenth century.[64] I see three rationale that could account for that. First, the awareness of a true genealogical origin was a shared knowledge in the breeding milieu, yet it did not necessarily vouch for the presence of an evolutionary issue. Second, the mixture supposed by the process of hybridization could be exorcised through a noble name. Third, a patron could be thanked through this habits.

Still the genealogical tree of Duchesne (fig. 2) owns its proper logic, which makes it a particular model for understanding the descent from a race of strawberries to another. It is organised according to certain rules and contains symbols that obey to them. The first distinction Duchesne made was to separate races that were constant through the seeds (bigger rectangles), from varieties or monstrous specimens resulting from these constant races (smaller and dotted line rectangles). Such that the genealogical descent was stopped with the varieties but could theoretically last with a possible constant race. The second rule was to place the head of all races on the top of the tree, quite similarly to the way used for a human being genealogical tree. Consequently, the younger races are on the bottom. The third rule relates to the proximity relation among several races. The three races 3, 4 and 5, close to each other "have several common characters" which make possible reducing them to a common root. The places of the other strawberries were organised according to morphological and ecological similarities, and of course, genealogical descent. The 7th comes from the 6th, the 10th from the 2d, and the 6th from the first. The kind of lines used in the tree had also a signification, the normal line designates a descent without hybridization, and the wavy line a hybridization: the 9th, *F. Ananassa* (fig. 3) was suspected to come from a crossbreeding of 8 and 10,[65] and a new race was expected to come from the crossbreeding of 7 and 8.

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61 Anonymous H (1761), p. 122.

62 Puismarais (1754), p. 70.

63 "Souche": Duchesne (1766a), p. 224. "Chef de race": idem (1766b), pp. 23, 47, 63. "Tête de l'arbre", "branches de la même maison," "Arbre généalogique": idem (1766a), pp. 214, 221. "Famille", a botanical term much used by Adanson and Jussieu is also represented in Duchesne's work.

64 Darrow (1966), ch. 6, "Breeding in France 1770 to 1900."

65 Indeed, modern botanists such as Darrow considers that Duchesne "must be credited as the first to identify F. chiloensis x F. virginiana as the origin of the modern strawberry" (Darrow (1966), ch. 6). In the plate, F. chiloensis is *frutiller* (8) F. virginiana is *fraisier écarlate* (10) and the modern strawberry (F. ananassa) is *fraisier-ananas* (9).

Fig. 3 *Fragaria Ananassa* from Rozier's *Cours d'agriculture*.

However, if it established a temporal relationship from the origin (top) to the last races (bottom), the time in the genealogical tree was not quantified precisely, even if Duchesne had proposed many hints in his historical reconstruction of strawberries that could help to do so. He was able to date the appearance of certain strawberries, and which were the older to have been described by the ancients. This helped him to establish *Fragaria semperflorens* as the head of the line.

In addressing the questions of genealogy and new constant races, Duchesne proved also to be consistent with his main methodology and his numerous appeals for naturalists to use empirical means. Indeed, his genealogical investigation had an important counterpart in his experimentation. Duchesne actually carried out himself experiments of hybridization, with the scope of testing the constant race issue. Such that *Histoire naturelle des fraisiers* discusses two new races and not only one. The first race was accidental and named *strawberry from Versailles* (*F. monophylla*) by Duchesne, who managed to reproduce it through seeds. But the second race was the result of an experimentation. Having received a female *Frutiller*, without a male of the race, Duchesne placed it in the house of the male *Capiton*, to which it resembled. A week after, the *Frutillier* gave a fruit. Duchesnes tried to repeat the experiment, and, thanks to the help of people from the *Jardin du Roi*, they obtained four *Frutiller* pollinated by the *Capiton*. The seeds were given to several botanists, such as Jussieu and Richard, who sowed them at the time the printing press was running on Duchesne's book. Other scholars, such as Le Monnier, and Le Normand had tried to cross the *Frutiller* with *F. Silvestris*, but with no success.

On this point, *Histoire naturelle des fraisiers* closes on a disappointing end, and the fate of the current hybridization is not reported. However, the practitioners conserved its memory. Thirty years later, Abbé Rozier and a society of people's *Cours complet d'agriculture* solved the question when showing, at the entry "Strawberries", the genealogical tree of Duchesne, with the new race which was now undoubtedly constant (fig. 4) and identified to the Scarlet strawberry from Bath.[66] Indeed the genealogy contains the new name for the race number 11, and another race was added by the author (number 12). Duchesne's experiment seems to have left, at least in the late eighteenth-century, some offspring.

[66] Rozier (1787), p. 50, pl. II.

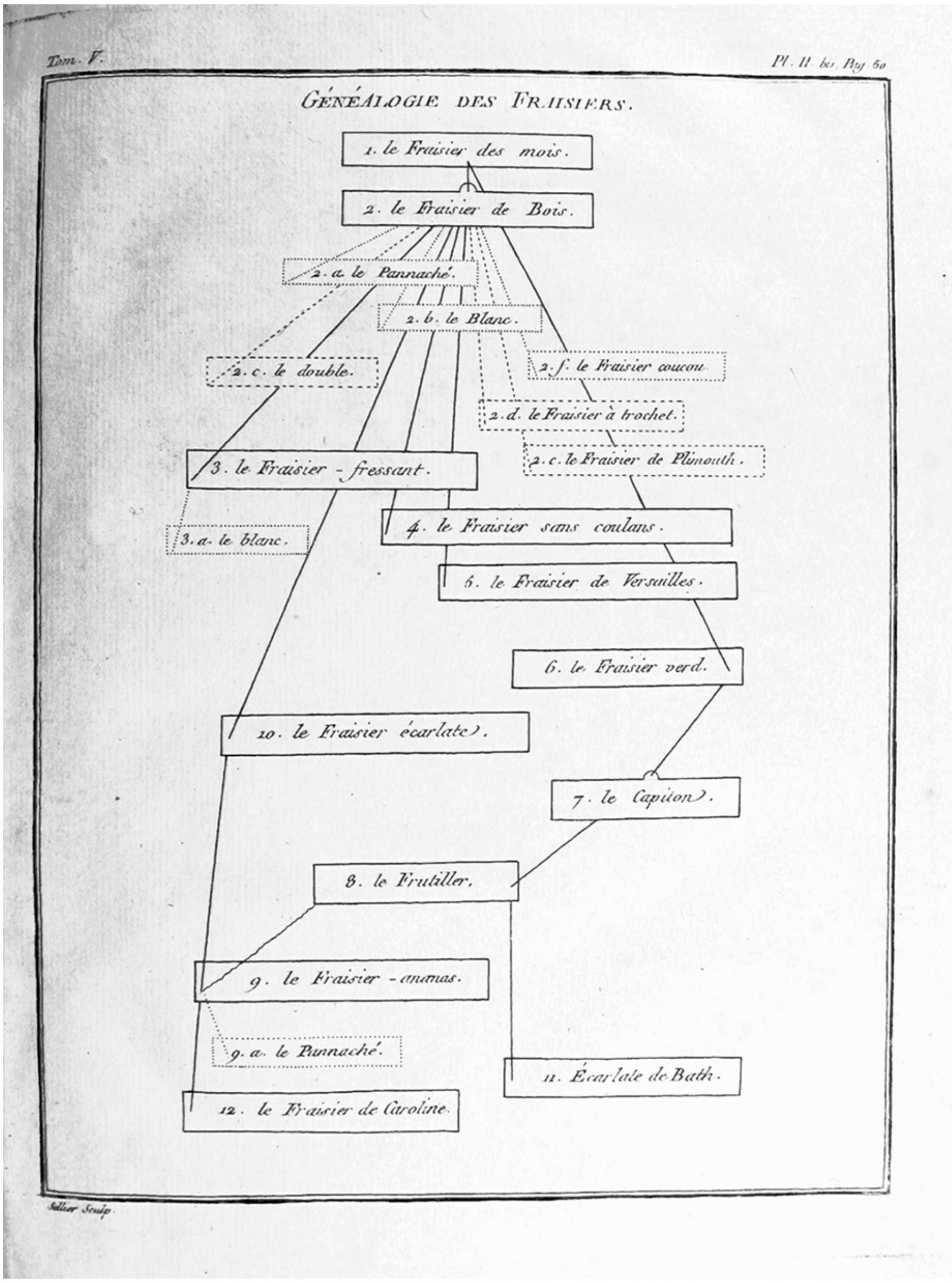

Fig. 4 Genealogy of the strawberries in Rozier's *Cours d'agriculture*.

Yet, in the 1780s, after Duchesne's discovery was diffused and adapt to the knowledge of practitioners and botanists, the difference were still important between them, notably on the concept of species. Rozier remarked that the botanists distinguished only three species of fragaria, (*semperflorens, muricata* et *sterilis*). While, on the other hand, practitioners gave to the definition of species a larger extension, because they considered the constant varieties to be species, and labeled "variety" the sorts that derive from these "species". It seems thus that two models for the species co-existed in France at the end of the eighteenth century, which served the rather different interests of two distinct communities, the practitioners and the botanists. Yet it was, during this time, in the restricted context of the practitioners that the genealogical model emerged.

CONCLUSION

Various influences brought thus Duchesne to use the genealogical tree. In addition to the empirical approach to hybridization used by Marchant, Réaumur, Buffon, Sprengel, Koelreuter, Adanson and Gmelin, Duchesne developed a proper methodology. In order to explore the no man's land of the "constant races" in-between botany and the practitioners, he systematically examined the history of botany-gardening, looking for the historical meaning of what means describing a plant, in term of human activity and history. In this respect, Duchesne cultivated a temporal history as opposed to a narrative history, that circulated, for instance through Buffon's *Histoire naturelle*. The account of nature according to a temporal scheme was nevertheless the method used later by Darwin, Haeckel and De Vries. Duchesne's attractive attempts of explanation such as the relationships between race, species and genealogy, his genealogical tree and use of a historical methodology, explain perhaps why certain historians of science tried to acclimatise his strawberries into their sometimes heroic history of science without practitioners.

This story thus tells us the entry of a skilled practitioner into the no man's land left unexplored in-between the two traditions, botany and breeding, and their hybridization that created a new field which crossed each border. *Practices of heredity* took a shape through secret know-how and hybridization cultivated by breeders, who demonstrate no particular quest for the understanding of these influences of art on nature. The double education of Duchesne drew him to look for a theoretical and schematic account of novelty according both to the current language and concepts used by breeders and to that of botany. Relevant to his problem, a nobiliary custom of illustrating genealogy was available by which he designed the first genealogical account of species in transformation. The tensions between tacit and explicit knowledge, morphological and genealogical definitions of the species, and each tradition, allowed to bring into focus new questions and are perhaps symptoms of an emergent discourse about heredity.

Institut d'histoire de la médecine et de la santé
Université de Genève

Acknowledgements

This paper is a improved version of a paper presented at a workshop on the cultural history of heredity at the MPIWG, Berlin. I want to thank Hans-Jörg Rheinberger, Peter McLaughlin, Staffan Müller-Wille and the participants to the MPI workshop for the discussion of this text.

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Natural Things and Non-natural Things. The Boundaries of the Hereditary in the 18th Century

Carlos López-Beltrán

1.

Hereditary transmission of bodily (physical) and behavioral (moral) features from parents to offspring became an independent subject of scientific theorizing only in the mid-decades of the nineteenth century. Only then did it become clear that the questions around the stability of species and the question about the contingent similarities responsible for family resemblance were tightly intermingled, and that both could be explained in a unified way. The problem of how transmission is effected became central issue for biologists.[1] Until then the issue of the conservation of type within a genealogical line was considered a major biological problem, whereas the phenomenology of hereditary communication of accidental traits within the lineages was normally seen as of secondary importance, except in a few local contexts, such as among animal and plant breeders and particularly among some physiologists and medical men, whose interests guided them towards considering the possibility of a regularity in the transmission of traits from ancestors to offspring within a genealogical line.

Elsewhere I defended the view that the appearance of the word *Heredity*, after the decade of the 1830's, first in French and then in other European languages, signals the turning point, as the shift from an adjectival use (in which there always was a peculiarity that was called *hereditary*) to the nominal use (*Hérédité*) reveals a growing awareness of the existence of a causal pathway responsible for observed regularities, and irregularities.[2]

I have also contended that before that period there existed among medical men and naturalists a set of accepted phenomena associated with the use of the adjective *hereditary* which were explained (or explained away) in different fashions during the different epochs.

A rather modest tradition of paying attention to how hereditary accidents are communicated through lineages can be followed from Antiquity to modern times. One can find within it a growing number of descriptions, and of attempted explanations of how the phenomenom occurs and why it can be responsible for similarities of physical and moral characters within such lineages. Some well known examples are Aristotle's mixed (cultural-hereditarian) explanation of the "Longheads",[3] Aristotle's complex physiological (dynamic) explanation of resemblance of the offspring to both parents.[4] Hippocratic and Galenic appeals to dual-seminal models of reproduction in order to account for resemblances in general.[5] Medieval astrological accounts of children's peculiarities of feature. Paracelsian influential dualistic theory, in which the

1 López-Beltrán (1992), Idem. (1994), Churchill (1987).
2 López-Beltrán (1992), Idem. (1994).
3 See Glacken (1967).
4 See Coles (1995).
5 See Boylan (1984); Idem. (1986); Jacob (1970); Lloyd (1983); Coles (1995).

imagination could induce both resemblances and monstrosities[6]. The medical attempts (after the Renaissance) to account for hereditary transmission of disease within family lines under different theoretical frameworks.[7]

In 1775 I. Kant proposed an analytical distinction that can be of use in our descriptions. He suggested that hereditary variations should be called "resemblances" if "they agree with their derivations" (the offspring "takes after" one or both parents, or some ancestor), and be called degenerations (or "expeciations" according to one translator) if they moved away from the norm in such a way "that the original stem-formation cannot be restored".[8] By the mid-eighteenth century a more or less stable set of phenomena were usually included in this peculiar category of the hereditary. Be it *resemblances* or *degenerations*, the fact that inessential peculiarities of feature not shared by all the members of a species or lineage, managed to be re-produced with some fidelity in the descendants, was considered a stumbling block for descriptive and explanatory systems in both Natural History and Physiology. It can be said that a special relationship was forged during the 17th and 18th century between such set of hereditary phenomena and the theoretical models of generation that were vehemently discussed. Resemblance to both parents, the mixed peculiarities of mules, the origins and transmission of monstrosity, etc. became a probing, evidential ground for the dispute.

What I call the *hereditary* constituted thus, from the mid decades of the eighteenth century on, a set of heterogeneous phenomena that anyone had to "save" in order to consolidate his view on generation. Chamber´s Dictionary (1738) for instance, in his entry for "Generation" mentions that Sir John Floyer "starts a difficulty which seems to press equally against each system (ovism and animalculism), taken singly"... the fact that mules partake of the characteristics of both horse and ass, and that the defenders of both systems capriciously choose the characters that favor their view as the important ones for the determination of the origin of the foetus, having the characters conveyed by the opposing sex as secondary.[9] When Diderot was preparing in the 1750´s his *Élemens de Physiologie*, he assigned a special weight the hereditary facts for the evaluation of the several systems of generation he intended to describe. The difficulty that preformationist views had in dealing with "maladies héréditaires; resemblance des parens; mules et mulets qui engendrent" was particularly highlighted by him in those notes.[10] At the beginning of his *Considérations sur les Corps Organisés*, Charles Bonnet posed some of the challenges his preformationist stance had to face:

> Si les germes sont contenus originairement dans les ovaires de la femelle, et si la matiere séminale n'est qu'une espece de fluide nourricier, destiné à devenir le principe du développement, d'où viennent les divers trait de resemblance des enfans avec ceux qui leur ont donné le jour? Pourquoi les Monstres? Comment se forment les Mulets?[11]

6 See for instance Glacken (1967) and Radl (1930).
7 See López-Beltrán (1992).
8 See Kant (1775) In Chukwade (1997). Chukwade uses the term "expeciation" to translate Kant's expression which covers similar semantical grounds as Buffon's "dégénération". Though Kant's appeal to the *original stem-formation* shows, I believe, an attempt to a generality influenced by Bonnet and Blumenbach that Buffon´s concepts lacked, I will keep "degeneration" as the adequate translation.
9 See "Generation" in *Chamber's Dictionary*, vol. I, 2nd. edition (1738).
10 See "Génération", chapter XXIV. in Diderot ([1875] 1964), pp. 182–185.
11 C.Bonnet ([1778] 1985), p. 31.

In his *Primae Linae Physiologiae*, written in the period when he still sided with epigenesis, Haller outlined the facts that pushed him *necessarily* in that direction. That organisms, especially hybrids, resemble both parents, simply "rules out" he wrote, any possibility that the new being is preformed in one or the other parent.[12]

Diderot, in his adjudicator's stance, knew well that even if the dual seminal (pangenetical)[13] models could account with more ease for the hereditary transmission of features by both parents, they had serious problems of their own when facing actual anatomical observations, and detailed physiological questioning. In his *Élemens de Physiologie* he writes "Dans ce sisteme (pangenesis) placenta, et envelopes impossibles a expliquer". This kind of criticism was of course made forcefully by Haller and Bonnet against Buffon.[14]

An important difference to point out concerns the character of the empirical facts that posed problems for each competing approach to generation. While detailed observation of the organs of generation, and of the development of the embryo, backed strongly preformationist (specially ovist) positions, dual seminal (pangenetic) accounts were favored by what may be called "genealogical" observations (or pedigree following); that is the following of patterns of similitude and difference within lineages. While the first set of facts depend on a focus on single individual development, the second set imply a higher level, comparative perspective, that needs the observation of many individuals belonging to several generations. The kind of features that, due to curiosity or a special interest, were followed through the generations varied widely. From very vague family resemblances to precise weird characters like an extra digit, a spectacular mole or a snub nose, or on the pathological side from general tendencies to ill health to precise ailments that develop in exactly the same manner at a particular age.

The "genealogical" approach to evidence and observation, I believe, opens up the possibility for setting external limits to physiological speculation,[15] in contrast with the interior limits set by dissection and microscopy. The gathering of convincing cases of hereditary transmission of a wide range of different features, the progressive closure of different causal avenues for dealing with them,[16] was one main theme of 18th century debates around generation. Important aspects of Bonnet's increasingly sophisticated ovism (in which he strategically adopts several explanatory resources from the rival dual seminal theories) were no doubt a consequence of the strains put on preformationism by hereditary facts.

It was not however an all powerful set of facts. The *hereditary* was an unstable domain, plagued with irregularities and exceptions. Not everybody agreed to grant it some reality. The fact of hereditary transmission itself could be put into question. When reviewing Buffon's theory of generation, Haller considered that the hereditary posed a challenge important enough, to go to the

12 Haller collected a series of hereditary facts in his miscelaneous collection *Similitude Parentum*. In this period he was convinced that only a new formation could acccount for them. See Roe (1981), p. 25 and Guyenot (1957), p. 295.

13 Whereas when discussing generation theories the main opposition between systems seems to be between preformation and succesion (say, epigenesis), in the case of hereditary transmission a crucial issue is the origin of the "information" concerning details (snub noses), and the opposition preformation vs. dual semen (or seed) is more relevant.

14 See Haller (1752).

15 By considering for instance the changing sets of conditions under which herediatary transmission occurs, when and with what regularity some features are preserved within a lineage.

16 For example explaining them away by ascribing them to chance ocurrences or calling them irrelevant.

extreme of denying its reality. “I prefer simply to deny to Mr. Buffon that offspring resemble their parents... the offspring are no longer images of their parents” (“et le reste de l’édifice tomberá de lui même”). Haller bases his belief in the greater number of exceptions than positive cases, and especially on the fact that internally (anatomically) there is never a shared pattern of nerves or veins between parents and offspring.[17] When commenting this extreme denial by Haller, Duchesneau writes that it illustrates with particular clarity the “individual” character of generation, in the eyes of the savants of the first half of the 18th century.[18] I believe this statement deserves some clarification. Rather than “individual” the act of generation was considered the result of natural (or divine) laws that produced each individual separately. This made the consideration of genealogical links rather irrelevant for all the fundamental issues. What however remained under dispute was if generation was to be given responsibility for all the features, both general and paticular, of the individual, or only for the former. The latter option left the door open for the all things individual, accidental, to be affected by influences from both external and ancestral (hereditary) influences, somehow independently from how generation occurred.

A denial similar to Haller’s was made in 1747 by the French physician Antoine Louis in a notorious discussion on hereditary disease.[19] What Louis was keen in establishing there is the impossibility that any anatomical (“solid”) feature of the parents can serve as the origin of a similar feature in the offspring. Any similitude has to be due to common, external causes. What is singular, individual, is the acquisition of peculiarities by each new being. Generation is entirely another question. About the possibility of hereditary disease Louis writes:

> les desordres de l’oeconomie animale doivent s’acquerir particuliérement par chaque homme: toutes les maladies seront individuelles puisqu’elles doivent être postérieures a la formation des germes qui n’ont reçu aucune alteration dans leur principe.[20]

Causal boundaries of the hereditary were drawn in a very different manner than the one we have become habituated after the instauration of our nature-nurture distinction. The explanation of individual bodily peculiarities was still closely linked to complex and open-ended medical notion such as constitution, temperament, etc. where an interaction between external and internal elements was responsible for idiosyncratic features of form and (dis)function.

The restrictive view that Haller and Louis (in their different projects) put forward for the notion of hereditary transmission of individual peculiarities somehow strangles (squeezes) the possibility of such transmission by closing the gap between two causally independent domains. The internal of the generation (or reproduction) of the germ (the first formation), and the external, circumstantial, influences, on the body. Among other things, what was being blocked by their arguments is the possibility that external influences (climate, nutrition, etc.) became somehow integrated into the lineages and eventually adopted in a non-accidental manner. For such authors, there is no conceivable way in which accidental variations of any kind could be

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17 Haller (1752), p. 32. The English translation by Phillip Sloan (“Reflections on the theory of generation of Mr. Buffon”, p. 318) insists that the resemblances whose existence Haller is denying are “exact” replications, but that modification is not to be found in the French.
18 Duchesneau (1982), footnote 132, p. 539.
19 For a discussion of this dissertation see López-Beltrán (1994).
20 Louis (1749), p. 35.

inherited from parents to offspring with any regularity. Even if for instance family resemblances were caused by a transmission of peculiar features from parents to offspring, they would be unimportant. For them the hereditary belonged to the domain of anecdote.

2.

A particularly suitable and surprising approximation to the conceptual frame under which hereditary matters were conceived within the medical tradition in the 18th century is given by the Galenic distinction between *natural things* and *non-natural things*. The boundary between body and environment, between physiology and milieu, for 18th century physicians can be neatly outlined with these concepts. Arnulfe D'Aumont writing for the *Encyclopédie* defined the (six) non-natural things|

> on appelle donc choses non-naturelles (d'aprés Galien[21]) celles qui en composent pas notre nature ou notre etre, mais dont l'économie animale éprouve des grands effects, des grands changémens, des grandes altérations.[22]

The list of the six non-naturals is somehow surprising for a modern eye: "l'Air, les Alimens, le Travail et le Repos, le Somneil et la Veille, les Excrétions retenues ou évacuées, et les Passions de l'Ame".[23] Galenists contrast these external factors with those called the (seven) natural things, and which are essential part by nature of the individual's bodily constitution:

> les élemens, les tempéramens, les parties, les humeurs, les esprits, les facultés et les actions: ce sont celles qui concurrent à former le physique de notre être.

The coherence, logical and explanatory power of this conceptual framework has been studied and discussed in several papers by William Coleman. He writes for instance that in the 18th century "the doctrine of the non-naturals provided a concise, flexible, and widely accepted framework for articulating the primary demands imposed by the conditions of existence upon men and women who sought seriously to preserve their physical well-being". (...)"The non-naturals became an integral part of a new and largely secular moral order."[24]

I believe we can also deploy such conceptual frame to focus on the way the boundary between internal and external determinations of bodily features and constitution were delineated in the period, in order to raise the question of the permeability, and , in some sense, the fluidity, between the internal (natural) and the external (non-natural) actions.

The ancient notion of the body as both a product of a mixture of humors (*crasis, temperament*) and a constant subject to multiple humoral influences played an important role in this kind of issues. Questions linked to the individuality of temperament, and to the possible explanatory role

[21] Galen inspired the concept of the six non–naturals, but did not really coin it himself. From a few suggestions he gave about which external influences were important to check in order to promote a healthy life, his followers ended up establishing a narrow list of six determinant factors. For historical studies of the concept see Nyebil (1971), Rather (1968), D'Aumont (1765), Nutton (1971).

[22] D'Aumont (1765).

[23] See Louis (1747), p. 18; D'Aumont (1765).

[24] Coleman (1974), p. 406. See also Idem. (1984).

that geography ("airs, waters, and places") and genealogy (family, tribe, nation) could play on some of its aspects. Taking Antoine Louis' analysis again as an example of a radical view, he insisted in the extreme individuality of the body:

> Le tempérament des enfans qui naissent d'un même pere, et d'une même mere est presque tojours différent; les un son bilieux, les autres sanguins; les uns son guais, les autres sérieux, pésans: ces différences d'humeur, de caractere et d'inclination dans les freres et soeurs, sont des suites de la différence des tempéramens; et elle depend peut-être moins de la constitution primitive ou radicale, qui paroît devoir être la même dans tous les enfans; que d'une diposition acquise par la combinaison infiniment variée de toutes les choses extérieures.[25]

Among the external influences Louis mentions are the weather at birth, the suffering during birth, the amount of blood in vessels at birth, the quality of the nurse's milk, the thickness of the air that was breathed during the first hours, etc. ("on ne finiroit à faire l'enumeration"). The earlier external influences have the more lasting effects on the individual's temperament. Future illnesses (or dispositions to them) are often acquired at very early stages. Louis was adamant that this acquisitions (pathological or not) were not however transmitted to the next generation

> (si la) diversité des tempéramens n'est point héréditaire, comment les maladies qui ont les suites pourroient-elles se transmettre par les parens[26] (...) les variations décident donc rien en faveur de la question des maladies héréditaires, puis quelles en vienent d'un principe interne et des dispositions inhérentes et immuables; mais qu'elles dependant uniquement des choses non-naturelles qui sont toutes extérieures[27]

As I have shown elsewhere,[28] most of Louis' critics, a few decades later, focused their attack on his "unbelievable" argument against the reality of hereditary transmission on what they saw as the false assumption that temperaments are all in all individual, secondary and accidental. A considerable proportion of medics believed that the possibility of transmitting bodily peculiarities through the family line was just impossible to deny. Contrary to Haller's and Louis' attitude, they claimed, facts should be given precedence over theory.

To understand the differences on this issue among 18th century physicians an important "theoretical" division should be considered (one that to my knowledge has not been sufficiently discussed). The one which separated solidists from humoralits. The latter is of course the older tradition, and is responsible for the main conceptual scheme for Hippocratic-Galenic medicine.[29] Its rationale for a unity and diversity of human bodily and spiritual propensities turns around the balance of its fluid (humoral) constituents and their relation to the environment.[30] As Vivian Nutton wrote "the advantages of this logical scheme can best be seen in the development within humoralism of the theory of the six non-naturals".[31].Solidism is a medical outgrowth of modern

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25 Louis (1747), p. 35.
26 Ibid., p. 37.
27 Ibid., pp. 74/75. Further on he writes: "Les hommes sont soumis a cette regle generale comme les plantes et les animaux, leur caractère et leur tempérament dependent d'une infinie des choses extérieures qui peuvent être variées a l'infini: c'est une verité recinue en médecine."
28 López-Beltrán (1994).
29 About the history of these medical doctrines and its links to hereditary disease see Portal (1808), and Adams (1814).

mechanicism. It is tied to the search for mechanical, structural causes within the body. Theoretical stances like iatromechanics and the physiology of fibers are expressions of its main contention: disease and/or normality should be found in the physical properties of organization. Lesions (not a bad mixture of humors) are the causes of diseases.

Over the issue of the permeability of the boundary body and environment these two theoretical positions worked under different presuppositions. If under a mechanical stance the solid parts (fibers, tissues, organs) are given causal (etiological) primacy over the fluid portions in the body,[32] then it becomes (as Louis argues) much less likely that external presences (air, efluvia, waters, food, climate in general) could *irreversibly* affect the body's properties; specially if the origin of the main features of the solids (organization) is thought of as preformed. In other words, the solid parts of each new being , ultimate bearers of all functional responsibility, will not receive any kind of regular, permanent influence, physiologically normal, from the equivalent solid parts of its ancestors; nothing acts at the first formation of the new being that deserves the name of heredity. On the other hand, if primacy is given to humoral mixtures (and if solids are thought of as product of a solidification or condensation of humors after fecundation), then the possibility arises that at the first formation of the new being its organizational plan acquires "original" alterations due to the peculiarities (the qualities) of the materials contributed by the parental seeds. It also becomes possible that those alterations acquire some "permanence" within the lineage, so that any causal influence that can find its way, through air, water, food, etc., and that dramatically changes a person's humoral balance will alter his physical constitution. This will happen because such alteration in turn will affect the next generation's reproductive humors like semen, blood, milk.[33]

In the context of hereditary traits (*resemblances, degenerations*) then, I believe that the distinction between naturals and non-naturals, and the conception of their interactions, can be illuminating. A traditional explanatory resource for variation within biological species, especially in the human case, is the appeal to external influences. The climatic (geographic) explanation is ancient.[34] During the 18th century the action of the sun, the air, the water, etc. was again given a central role in the explanation of diversity of bodily constitution (and of *moeurs*) by important theorists as

30 As Vivian Nutton defines it: "Humoralism is a system of medicine that considers illness to be the result of some disturbance in the natural balance of the humours, within the body as a whole or within one particular part. It stresses the unity of the body and the strong interaction between mental and physical processes. It is at one and the same time highly individualistic, for each person and each bodily part has their own natural humoral composition (also known as *krasis*, mixture, or temperament), and universal, for the range of variation is limited and the same patterns of illness (diseases) can be seen to occur in many individuals. The natural balance of health is always pecarious, for it is constantly subject to potentially harmful influences from one's diet, life-style and environment." "Humoralism" in Porter and Bynum (1993), p. 281.

31 Nutton (1993), p. 288.

32 Louis writes "l'action des fibres plus ou moins forte et vigoureuse, façonne et modifie différemment les humeurs de notre corps; ces huneurs agissent suivant leur quantité sur les solides dans lequels elles sont contenues, et elles en determinent diversement les actions: de-là viennent les complexions particulieres qui mettent tant de différence entre les hommes, tant par rapport aux dispositions du corps qu'aux caracteres de l'esprit." Louis (1747), pp. 37/38.

33 The capacity that one is willing to concede to a humor was a point in dispute. Haller was very critical of the way humoralists were willing to accept that amorfous fluids could by themselves produce any sensible order.

34 Its locus classicus is the Hippocratical *Airs, Waters, Places.*

Arbuthnot, Falconer, Montesquieu, Buffon, etc.[35] This external origin of difference was often combined with discussions concerning resemblances and *degenerations*. What could be called the "staying power" of externally induced alterations was often a point of both empirical and theoretical discussion. Competing systems (or models) of generation had to adapt themselves to collected facts and consensus opinion around this issue.

It will be of interest to notice that conceptions of hereditary transmission associated with French vitalism tended to side with a more permeable view of the link between body and environment. This open position, that saw in (complex) equilibria between internal and external elements of the body the only plausible realistic approach to health (and to natural history) allowed for the eventual conception of a Hygienic approach to both health and the physical betterment of humanity. A crucial difference between French and British hereditarianism in the course of the 19th century was due exactly to this permeability allowed by the French. As Coleman clearly showed, the notion of the *non-naturals* was the antecedent to the hygienic programs in the post-Napoleonic years. Elizabeth Williams has followed brilliantly the influence of vitalism throughout the 19th century approaches to the link between body and environment by French physicians.[36]

Humoralists on the whole were prone to what we could call a holistic view of the links between the body and the environment, and found it conceivable that a kind of a flux (or flow) of externally induced characters could travel (trough physiological means) within the family lines and conform a peculiar bodily heritage for families, tribes, races. On the other hand, solidists (à la Louis), given that no possible route of influence can be imagined that connect the parent's initial, solid bodily frame, with that of its infant's, and that every single case of variation and resemblance can be explained (away) using external factors, hereditary attributions become just a "façon de parler".

What I want to discuss then is the question of the permeability of the (possible) hereditary causal routes to the external factors during the 18th century. The task should be to show how the two levels of analysis, the influence of external factors on bodily constitution, and the hereditary influence of the parents bodily constitution on the bodily constitution of the child could be (and were) coordinated by different authors with different viewpoints.

The specific discussion around the hereditary transmission of disease in the frame of eighteenth century views of physiology and generation provides a privileged access to the evasive conceptualization of the possible routes through which the environment could affect, in dramatic and more or less permanent fashion, the constitution of individuals and lineages.

3.

It seems to me that a useful way to frame the discussion of heredity in the 18th century is to be found in Peter McLaughlin's proposal[37] to make a "distinction between the two types of inheritance – the law-like transmission of species form and the contingent disturbance or supplementation of this transmission by individual traits." I believe however that such distinction should be made in another fashion, and more care should be taken in the choice of words. For the

35 See relevant chapters in Glacken (1967).
36 See Coleman (1984), Williams (1994).
37 McLaughlin (2000).

most, during the eighteenth century, what McLaughlin refers to as the first type of inheritance was considered outside the domain of the hereditary. Generally speaking, all theoretical positions about generation did not refer to inheritance when dealing with a lawful transmission of form; that is a notion that was adopted well into the 19th century. Authors in the 18th century, both preformationists and succesionists,[38] thought that the transmission of form from generation to generation was due to a constant, invariable cause (or fact) that gave the species its fixity and stability, and which had nothing to do with the contingencies of genealogy. Both the idea of the preexistence of the germ, and that of a lawful (successive) production of the new being driven by a mould, or by a generative force, or some other epigenetic principle, shared the notion of a basic common structure for each species, over which the singular, accidental characters of the ancestors had no permanent influence. A shared attitude towards hereditary characteristics (the peculiarities of individual, family, or major lineages) was thus to confine them to the domain of the accidental. While accepted and discussed in both Hippocratic treatises and Aristotle's works, the facts of hereditary transmission of physical and moral resemblance, deformation and disease, were seen both as undeniable (though bothersome) factual givens that had to be explained away. Perhaps only in the medical tradition, with its preoccupation with the singularity of disease and patients had more room for thinking about the peculiarities of individual temperaments and their transmission through the family line. For that period the distinction that MacLaughlin is aiming at should in my view be refered to as that between type vs. accident, and only the latter should be seen as linked to the hereditary.

The type-accident distinction allows for a careful following of the place that the hereditary received in different theoretical frameworks. For instance, Hippocratic-Galenic medics were content with the fact that dual seminal, pangenetic, solidification of fluids, model could among other explanatory virtues, account for the facts of hereditary recurrences. At the same time, the more theoretically sophisticated model of generation devised by Aristotle was seen to generate some puzzles difficult to sort out in regards to this kind of facts.[39] There is in fact an ongoing discussion among Aristotelian scholars about how to understand his proposal of explaining (away) in his scheme, the hereditary fact of resemblance to both parents, and sometimes to previous ancestors.

As is well known, in Aristotle's scheme of generation male seed was responsible for form and female matter for individual peculiarities.[40] In an illuminating recent piece, Andrew Coles provides a complex physiological analysis of the origin and function of the male seed (semen) in Aristotle, so that the main difficulties that arise with hereditary facts are overcome. Contrary to widespread belief, according to Coles it is by rescuing the Cnidian (Hippocratic) notion of pangenetic origin of the semen that he manages to do the trick: "It is in Aristotle's conception of

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38 Succesionist, as I said, is a label that includes both epigenetists and the partisans af dual seminal rapid formation of the germ or first rudiment.

39 At least since Empedocles, for anybody in the business of giving an account of human, (or animal) generation, the paradoxes of *the hereditary* were a serious stumbling block: Aristotle's view of the male seed as the only causal contributor to the form (shape) of the body of the offspring had to by-pass the empirical evidence of female transmitted characteristics (resemblances, mules). The most convincing account of of the irregular mixtures of resemblances to both parents was given by dual seminal theories.

40 The question then (as Sharples poses it) is, if it is supposed that the father imparts form and nothing else, why snub-nosed fathers tend to have snub-nosed children. See Sharples (1985).

the physiological origins of semen, and more particularly of its hereditary properties, that the links between his biology and that of the Cnidian's are the closest".[41] Apparently, the view that the (male) semen is the product of a special process of separation from the blood, and that in its passing through all the different parts of the father's body, it acquires particular kinds of secondary movements (*dunamis*) which can be responsible for carrying on the offspring hereditary resemblances. The result from the (part by part) struggle of these paternal *dunamis* with similar movements that oppose them and that come in the mother's generative matter defines the side that the offspring's different parts are going to take after.[42]

Aristotle's acute perception of the thorny causal problems that hereditary resemblances posited was progressively diluted by his interpreters, so the aspects of his system specifically designed for coping with them seem to have been lost. Galen for instance was convinced that there was no way in which Aristotle could account for female transmitted resemblances, and for that reason made a forceful argument in favor of the existence of a female seed (semen), with equivalent physiological powers to transmit hereditary peculiarities.[43]

Harvey believed he was being Aristotelian when he limited the influence of material physiology to the action of male spirits in the production (conception) of form in the new being, and pushed out the hereditary influence (resemblances, etc.) to the Paracelsian domain of the action of the mother's imagination.[44]

As McLaughlin points out, the conflict between pangenetic (dual seminal) models of generation (with their hereditary support) and the epistemological (and theological) demands that promoted preformationist views an the 17th century produced different attempts at "mixing" the virtues of both accounts, as Aristotle had apparently done.

At the beginning of the 18th century Bourguet proposed one such synthetic model. A preformed germ affected after fecundation by a pangenetic, dual seminal influence, responsible for all things hereditary.

As has been said, Haller first used the hereditary as a weapon for epigenesis, and later changed sides, denying emphatically its reality when criticizing Buffon.

Bonnet eventually adopted and deepened Bourguet's strategy separating clearly the origin of form (in the germ) from the external origins of resemblances[45] which were due to the incorporation (*intussusception*) of accidents in the nutritional growth-development process where (pangenetically gathered) particles from both origins could play a role. Although succesionist authors like Maupertuis and Buffon insisted that the manner of the (new) production of the germ in each fecundation is important, several authors (including McLaughlin) have argued that there is a common structure between their hypothesis and elaborate preformationism such as Bonnet's. More than a hundred years ago, in an insightful historical essay called "Evolution in Biology"(1878), T.H. Huxley arrived at the conclusion that if we set aside around the time and

[41] Coles (1995), p. 50.
[42] See Coles (1995), pp. 70/76.
[43] Coles (1995), p. 76, Boylan (1984).
[44] See Harvey ([1651] 1981).
[45] Though in his later versions of his theory of generation Bonnet came closer and closer to a quasi-epigenetical scheme, in which organised structure is not actually present in the preformed germ but somehow "preprogrammed". See Huxley ([1878] 1896), and Bonnet ([1769] 1985).

manner of the production of the germ (Harvey's rudiment), most 18th century theorists of generation, preformationists or succesionists, supported some form of the doctrine of evolution or development, which considers a dual phase in the production of the new being's body; first the creation or reproduction of the basic structure in the germ, and second the growth and development of this rudiment.[46] Huxley sees in Bonnet's description of such process the common "exemplar" for the period. For the case of the hen's egg, Bonnet in his *Considérations* ... states that:

> fecundation and incubation simply cause the germ to absorb nutritious matters, which are deposited in the interstices of the elementary structures of which the miniature chick, or germ, is made up. The consequence of this intussusceptive growth is the "development" or "evolution" of the germ into the visible bird. Thus an organized individual is a composite body consisting of the original, or *elementary* parts and of the matters which have been associated with them by the aid of nutrition; so that if these matters could be extracted from the individual, it would, so to speak, become concentrated in a point, and would thus be restored to its primitive condition of a germ.[47]

Further modifications to his notion of germ made Bonnet's position even more general and inclusive.[48]

> Ce mot (germe) en designéra pas seulement un corps organisé reduit en petit, il désignera encore toute espèce de préformation originelle dont un tout organique peut résulter comme de son principe immédiat[49]

Huxley's conlusion is eloquent:

> But thus defined, the germ is neither more nor less than the "particule genitalis" of Aristotle, or the "primodium vegetale" or "ovum" of Harvey; and the "evolution" of such germ would not be distinguishable from "epigenesis".[50]

So even something as obtuse as Buffon's "moule interieur" could somehow become equivalent to Bonnet's generalized "germ". Notwithstanding important subtleties that stand in the way of Huxley's coarse integration of these concepts, he is no doubt making an important point. Most 18th century theorists shared the idea of a deep conceptual hiatus between the explanation of organization (taxonomic similarities), and that of accidental individual peculiarities (variations, resemblances).

For our purposes, the similarity Huxley sees between the generation models of the kind Bonnet-Haller, and of the kind Buffon-Maupertuis, turns around the fact that both deliver a germ that suffers a process of growth and transformation under the influence of a fluid milieu that

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46 Huxley added that even Cuvier, in following century, adopted a very similar scheme. Huxley ([1878] 1896), p. 190.

47 Ibid., p. 191.

48 Huxley writes: "Bonnet ... in his later writings and at length ... admits that the germ need not be the actual miniature of the organism, but that it may be merely a "original preformation" capable of producing the latter." (Huxley [1878] 1896, p. 193. he quotes from Bonnet, 1769, X., ch. ii).

49 Bonnet (1769), X, ch. ii. quoted by Huxley ([1878] 1896), p. 193.

50 Huxley ([1878] 1896), p. 193.

provides the elements that become *incorporated*. This milieu is responsible for hereditary influence. The origin, qualities and manner of action becomes the question for hereditary analysis. The typological, essential, permanent features of organization are identified with the solid frame, whereas accidental, ephemeral features come into play through humoral (fluid) influences. The dialectics of solid-humoral interaction have a crucial role in the split between structural stability and individual deviations. The solid organization (given for instance by the germ or driven by a solidifying force) receives constant influences, and matter from the fluid environment. These alter and shape the bodily frame.

Aggregation or intussuseption of particles of different origins (paternal, maternal, external) are processes indifferent with regards to the hereditary route. Both a succesionist and a preformationist account of generation can equally explain (away) hereditary transmission through the incorporation of elements during growth or development.

There is an important difference to keep in mind about the consequences of the acceptance of a preformationist account and that of a succesionist account of the origin of the germ (or the "first formation"). Although some hereditary facts can in both cases be accounted for through external supplements[51] to the first formation during "evolution", or growth, the preservation of a clear-cut split between the (causal) origin of form and the (causal) origin of resemblances or *degenerations* becomes more problematic for the succesionist. In a Maupertuis-Buffon kind of model, for instance, some of the more dramatic, important hereditary variations (*degenerations*) could up to a point be incorporated into the essential genealogical sequence. Six-digitism, or other *degenerations*, become susceptible of being transmitted or "copied" by the generation process[52] responsible for the first formation in a similar fashion as the more basic characteristics of the species. In fact, for some defendants of the preformation, like Haller, this vagueness about the limits of the individual (resemblances) and the formal (type) became one of the main reasons for their opposition to succesionist schemes. At the same time the failure of preformation to make sense (by itself) of preexistence within genealogical lines of accidental features made the subsequent appeal to a supplementary dual pangenetic influence methodologically suspicious.[53] Again, the permeability or impermeability of the specific form to external, accidental influences is one of the issues at stake within these discussions.

Though it must be remembered that hereditary facts could, during the 18th century, still have alternative non-physiological explanations (v.gr. a constancy or repetition of some external climatic influences or an appeal to the action of imagination or other "mind over matter" interventions) the search for a regular, stable physiological source was increasingly seen as the only sensible explanatory strategy. For such a strategy one important question turned around the origin, kind, and qualities of the material particles that were incorporated (through generation, nutrition, development and growth) into the different parts of the body. The notion of a strict ontological boundary between organic and inorganic particles, as proposed for instance by

51 Dual seminal, plus maternal blood, plus other sources of nourishment.

52 For example through the modified *moule interieur*, or Maupertuis' material memory. There seems to be evidence both for a possibility of modification of the moule interieur, and for its inalterability in Buffon's writings. See Aréchiga (1996).

53 In his 1878 work Huxley concludes that it was only with the careful observation of development of the chick by C.F. Wolff that the speculative impasse ended and embriology could take a progressive route.

Buffon, provided another boundary to take into account. The access of external influences to the peculiar bodily features of each individual could clearly be limited by such considerations. Climatic, nutritional and other sources of bodily variation, captured eloquently by the label of the *non-naturals* can have very different capacities to alter the constitution according to the continuity or discontinuity postulated between bodily constituents and external matter (air, water, places, etc.). The medical frame of natural and non-natural, and of solid vs. humoral influences was the main available resource for coping with these questions. It was undoubtedly at the root of Buffon's speculations concerning the influence of climate and food in the production of degenerations (*degenerations*), which begin as individual variations and are progressively generalized within a lineage as they are hereditarily transmitted ("like diseases are communicated from fathers or mothers to the children").[54]

A further question, not often formulated clearly is the "staying power" (which makes them more or less permanent or ephemeral) of the peculiarities incorporated by environmental and nutritional routes, within the genealogical succesions. This question can become crucial in a succesionist account, if there is a dilution of the difference between individual and specific organization.[55] The question presented itself to Maupertuis and Buffon, and also to Blumenbach. They all allowed for a semi–permanent transformation of the type due to the conservation of accidental variation within the lineage.[56] It is probably in the extensive discussions of Buffon on the notion of degeneration that the intricacies of this issue began to be sorted out. The belief in a strict and widespread correlation between climate and bodily temperament in human groups was heavily questioned in the course of the 18th century. The tenacity or the feebleness (i.e. differences in "staying power") of accidental racial features had both evidence and testimonies in their favor.[57]The issue of establishing how and when external influence on the physical characteristics became "rooted"[58] in a lineage acquired a progressive importance after Buffon's work.[59] This French author always maintained that a limit existed to the amount and kind of variation accepted by the interior mould.[60]

Kant for instance believed in the feebleness of accidental acquisitions. He writes:

> Gradually and at last the constitution of the soil (moisture or drought), and food, also, induce a hereditary difference or strain among animals of one and the same stock and race, especially in stature, proportion of limbs, and also in the temperament; which later hybridizes when mixed with another kind: but on another soil and in the presence of other food (even without alteration of the climate) disappears but in a few generations. [61]

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54 See Buffon ([1749] 1971). See also Aréchiga (1996), p. 74.

55 The process of using the complete bodily constitution or temperament of a parent as an original pattern or mould for the production of the new being (at the moment its first formation) has even induced a paradoxical talk of the conservation of an individual type(!): See Duchesneau (1982), p. 539, and López-Beltrán (1992), chapter V, and Borie (1985).

56 John Hunter arrived at a similar view, influenced by Blumenbach. See López-Beltrán (1992), chapter IV.

57 "such is the difference of this effects – writes Blumenbach – (some) are preserved unimpaired by a sort of tenacious constancy through long series of generations, or by some power of change withdraw themselves again in a short space of time". (Blumenbach [1795] 1865).

58 He metaphor of depth, of rootedness, taken I believe from iatrochemical speculations of the seed or material cause of disease, was frequently invoqued in this context.

59 See Roger (1989), Aréchiga (1996), Glacken (1967).

60 Blumenbach himself wavered in front of the evidence. Compare Blumenbach ([1775 and 1795] 1865). See also López-Beltrán (1992), chapter IV.

Blumenbach wavered in front of the evidence. After first having also dismissed the view that what Buffon called degenerations could become a stable hereditary part of an animal's (or a person's) constitution, he came to accept what he called "hereditary peculiarities of animals from diseased temperaments":

> An hereditary disposition to disease would seem at first sight rather to belong to pathology than to natural history of animals. But when the matter is more carefully looked into, it is plain that in more ways than one it has something to do with those causes of degeneration we are concerned with.[62]

Blumenbach later writes that when some of these (constitutional diseases or disorders)...

> are propagated by hereditary causes for a long series of generations it shades sensibly away into a sort of second nature and in some species of animals gives rise to peculiar and constant varieties.[63]

4.

Blumenbach's analogy between hereditary disease and hereditary variation, and his blurring of the distinction is to my mind revealing, in that within medical tradition (as I said) there existed a frame that allowed a way of dealing with the issues of the link between the body (temperament, constitution) and the environment. A complex, and mutually dependent set of causal factors could be considered as working over the same product (the body) in such a way that the final result could not be attributed wholly to one or another particular influence. The notions of crasis (mixture), fluidity, individuality, allowed the imagination to both conceive a given outcome as determined and caused, and as complex product of an un-analyzable and variable set. On the other hand the distinction between the *natural things* (bodily frame, temperament) and the *non-natural things* (that affect and change the former) allowed for a separation of causal spaces, that is however very different from the modern nature-nurture split we have become habituated to. The fact for instance that passions, dreams, and other psychological elements were placed on the same grounds as the air, or food, or water, reveals a very different conception of the boundaries, and of the causal dependencies that are implied. Organisms (bodies) were not seen diachronically as we see them, that is as being in a dialectical sequence of expression of a hereditary information within succesive environments, but were rather seen as embedded in, open to and intimately dependent on its physical surroundings. The complex web ob influences that acted over each individual (human) body was a main preoccupation of the medical tradition, and the physical and moral inheritance that was passed on from parents to offspring through generation was only a small portion of such web.

The *natural things* (humors, elements, temperament) can be equaled for the sake of our story with the bodily elements that are put in place by typical generation. That is to say, those which constituted the germ, and/or the preordered set of elements that get together to form the

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61 Kant (1775), in Chukwudi (1997).
62 Blumenbach ([1795] 1865), p. 202.
63 Ibid., p. 259.

organized new individual are product of the *natural things*.

The *non-naturals* (air, food, dreams, passions, etc.) are the set of "external" influences that somehow come into contact with, and/or determinantly, affect the body as it grows, lives and decays. From a superficial analysis the two sets of causes are disconnetced. They were treated for instance in two different Hippocratic treatises.[64] But of course the link between them is powerful, and as Arnulfe D'Aumont writes, it is when these interactions (*natural/non-natural*) goes wrong that the *praeter-natural* (diseases) appear.[65] As we saw in Antoine Louis' argument above, the six non-naturals can readily play a part in the discussion concerning external causes of degeneration, and the "staying power" they acquire according to their origin and moment of influence.[66] The possibility that external influences could become more or less permanent through generation within a genealogical line was seen as more or less open by differently oriented physicians.

A privileged access to how these matters were dealt with, and into the details of physiology and etiology, during the18th century is gained when we look at discussions of hereditary disease.[67] Through them we can clarify the different consequences for the phenomenology of the hereditary that medical men could see if they were to accept certain openness (permeability) or closure to external, accidental influences at the moment of the first formation. According to different physiological views, different boundaries could be drawn. The question can again be posed: are accidental hereditary bodily features induced by the *six non-naturals*(?) and are these transformations of such an order that the family lineage is affected in a permanent way? I will attempt in the following paragraphs a short summary of several positions that can be found among physicians during the second half of the 18th century.

A strict solidist position can readily drive to an anti-hereditary stance. We have seen that with Louis.[68] The impossibility of the incorporation (at the required depth) of anything external or accidental into the body's frame (i.e. the original germ). This is Louis' main argument. If diseases have a solidist *root*, any lesion would have to be local, isolated, and its effects would end with the death of the bearer. In contrast a humoralist view incorporates the hereditary to a choreography of internal and external influences. Within them there is no clear-cut separation between external and internal humors. The hereditary cause does not have a special ontological status. It is more a consequence (a "side effect") of the flux of succesive generations irregularly acquiring (not only through generation) physical (and moral) peculiarities and transmitting them (through generation) to their offspring. At the most, the adjective "hereditary" refers to a possible route of transmission of influences that may also operate through other routes.

An important theoretical development occurs among medical men when committed solidists, uncomfortable with humoral, proteic explanations of hereditary disease, nevertheless accept the

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64 "Airs, Waters, Places" and "The Nature of Man".

65 D'Aumont (1765).

66 The division we described between humoralists and solidist becomes relevant. In an humoralist view the balance among humors is crucial, and there is a continuous flow of materials, mixtures, in and out of the body, carried by air and liquids and food. The solid parts of the body are always changing according to these interactions. In a solidist view the root of the disease is in a lesion or malformation at the level of the organs or solid parts. It is the solids and their properties who master a control the humors.

67 I have made a thorough study of the disputes around this issue that happened in France between 1745 and 1810. See López-Beltrán (1996).

68 Louis himself locates the thrust of his antihereditary argument in the older work of Luis Mercado, where apparently all solid to solid influence is also denied. See Louis (1749), footnote, p. 14.

reality of the fact of hereditary transmission, on the base of the accumulated evidence provided by the accumulation of cases were features, traits and diseases were seen to travel through lineages. A new kind of speculation about the possibility of solid to solid hereditary influence is then developed, and as a consequence a distinction between properly hereditary transmission and parallel congenital influences is proposed.[69].

We could also summarize the different "models" of hereditary disease transmission according to which notion of generation is presupposed by physicians. Under preformation: the germ is invaded by the "seed" of disease,[70] and its humoral balance is disturbed. Or it is damaged by an external influence (v.gr. a fluid) during its development *and* the alteration can then be somehow transmitted to the following generation through parental semen (pangenetically for instance). The transmission then is not necessary and is avoidable. In a clear-cut succesionist account something analogous can happen. After the "first formation", heritable alterations can be suffered. Humors are responsible for idiosyncracies. There is not an important etiological distinction between receiving the evil action for the first time in the lineage (by an environmental cause) or receiving it through the liquids of the parents during growth and development, or even during nursing.

Towards the end of the 18th century a very different notion of hereditary transmission is adumbrated by solidists that is only possible in a succesionist scheme. A form or other of a copying mechanism is proposed as responsible for producing, each generation, the solid parts of the new being.[71] The latter can be framed (as for Buffon and Maupertuis) under the analogy of crystallization, or not. The (solid) bodies of the parents are taken to be the *source* (or mould) that is used as a base for the new production of organization in the offspring. Both essential and non-essential features can be thus candidates for copying. The hereditary act (transmission) occurs always at the moment of the "first formation" of solid parts (the production of the germ). Bodily variations due to external influence can become hereditary only if they originate at the moment of the "first formation". Besides, not every feature is transmitted identically. There is rather a different tendency among them to be transmitted according to their properties. Essential features are transmitted with more constancy, and accidental features have less chance to be copied. Similarities have the way more open than degenerations. The older an accidental feature has been running in the family the stronger will its propensity to be communicated due to a sort of rootedness it has developed. What we have been calling the "staying power" of a variation is thus linked to its propensity to be carried along to posterior generations by the "copying" mechanism. Variation within the offspring is thus explained. The bodily organization of the new being is a version of the parent's organization, in which both essential and accidental features are copied from them. Even deformities can be transmitted if their origin is an accident at the moment of the first formation. What can be called a "frozen accident" explanation of hereditary transmission of accidental variation due to external causes. This kind of thinking can be traced to the model of Maupertuis' generation theory and was used by John Hunter and his followers. The clear-cut distinction it aims at establishing between hereditary and non-hereditary transmission within lineages was used by French medics, and also by Hunter and his followers, to establish a difference

69 For a detailed discussion of this conceptual innovation, and its consequences in both French and British discussions of the hereditary see López-Beltrán (1992), idem. (1996).

70 In a iatrochemical wording.

71 See Pujol (1802), Prichard (1813) and idem. (1824).

between congenital and connate transmission of disease.[72] What is often discussed is, I repeat, if the depth of the incorporation of the hereditary variations into the normal flux driven by the reproduction mechanism. Usually, among medics, the question attended to is how, if in any way, can hereditary disease be eliminated. The answer to such question reflects the view held by the physician.[73]

5. (Conclusions)

We can talk of the existence during the 18th century of two kinds of limits or boundaries to hereditary transmission. The "channelization" of hereditary influences through the physiological "line of production" of the new beings can be seen, during that period as being limited by an internal and an external boundary. The internal boundary is given by the separation between the re-production (generation after generation) of the specific form, and the acquisition and transmission of peculiar accidents of constitution first by individuals and later by lineages. Both preformationist and epigenist views of generation accepted the challenge of explaining (away) the facts of hereditary links between parents and offspring. And in both their positions a tight boundary was placed in order to preserve the integrity and stability of the form. The accidents could not be incorporated *definitively* into the essential organizational features or *form*. Each new being when first constituted as a germ was free from accidental variations produced by external influences, including hereditary ones.

The external boundary is given by the physiological facts of growth, development, nutrition. The amount of isolation they were thought to have with respect to the physical milieu made the outcome of growth and development (the mature individual's temperament) more or less variable, and the depth of the changes could also vary. The belief that only those modifications of temperament by the *non-naturals* that could find their way to the flux of pangenetic chain of transmission was shared by many. The mystery of course was how this could actually take place. What exactly had to be modified by external influences? The mould or the particles? The bodily features of the parent and the particles that integrated them. Buffon, Blumenbach and other 18th century thinkers conceived mechanisms by which physical modifications induced by the environment could be preserved hereditarily and give rise to varieties within species. The reversibility by external influences was always an option, as was the idea of betterment through hygienic measures.[74] The vitalist program in France preserved this attitude throughout the 19th century, allowing the notion of hereditary determination of physical and moral characters to be balanced by a hygienist counterpart.

Though the notion of hereditary transmission was restricted and had a minor importance in the 18th century. The breaching of what I just called of the internal boundary is responsible for the hardening of heredity, and its progressive transformation during the 19th century into a central cause in physiological thought. The permanence and transcendence of "accidental" variations needed a new conceptualization of the hereditary. Towards the turn of the century (18th to 19th), as can be seen in Blumenbach's changing views, and more prominently in

72 See Pagès (1798), Pujol (1802), and Adams (1814).
73 See López-Beltrán (1996).
74 Changing the climate, the food, the way of life.

Lamarck's use of hereditary (and in a sense "accidental") modifications as the source of species transformation, the narrow boundaries between which hereditary transmission could be conceived to occur, became progressively breached, and blurred. The outcome of this opening was that external climatic and psychological causes could be now seen to affect and modify in the long run the essential form of organisms, and of their descendants. Deeply embedded in the organism's constitution, some *degenerations* with considerable staying power could be conceived as witnesses to the power of both environment and heredity to shape bodies (and minds). Resemblances, *degenerations* and the transmission of form through generation could be now seen as having similar causal dependencies.

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List of Authors

Dr. Staffan Müller-Wille
Max Planck Institute for the History of Science
Wilhemstraße 44
10117 Berlin, Germany
smuewi@mpiwg-berlin.mpg.de

Prof. Mary Terrall
Harvard University
Department of the History of Science
Science Center 235
Cambridge, MA 02138, USA
terrall@history.ucla.edu

Dr. Marc J. Ratcliff
Université de Genève
Faculté des Sciences - Faculté des Lettres
Historie et Philosophie des Sciences
10, avenue Jules Crosnier
CH-1206 Genève
ratclifm@mail.lettres.unige.ch

Prof. Carlos López Beltran
Instituto de Investigaciones Filosóficas
Universidad Nacional Autónoma de México
Ciudad Universitaria
Coyoacan D.F. 04500, México
lbeltran@servidor.unam.mx