

Domesticating the Wild Type: A Historical
Investigation of the Role of the Domestic-Wild
Divide in Scientific Knowledge Production

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Acknowledgements

This thesis started life as a relatively diminutive 20,000 word MA dissertation on the origins of the wild type concept in classical genetics. It was then introduced into an artificially open PhD environment where, subjected to continual bombardment by all manner of unusual academic influences not found in its original habitation, it mutated (or should I say degenerated?) and dramatically expanded, requiring rigorous selection in order to be trimmed into shape and fully domesticated. As it is, it's still something of a monster, but one I find slightly less terrifying now that it's at least partially been tamed. Certainly, it's not the cute little mogwai it started as, but through a process of continual re-engineering, I think I've at least managed to find and remove most of the gremlins.

Joking aside, this has been a difficult project which has taken longer than hoped and has taken more twists and turns than I could have expected. It's certainly been no linear evolution, but rather a thicket of branching enquiries, many of which have ended up being dead ends. I certainly could not have managed through if it had not been for the support of others. I will start by thanking my uncle Garth for funding the costs of my fees, as well as his secretary Mandy and my uncle and aunt Earle and Eve, who helped arrange the necessary transactions. I would then like to extend thanks to all the staff and faculty at Byrne House for helping create such a congenial and invigorating environment for research. This of course includes my supervisors Staffan and Sabina, who have been incredibly patient over the years with what has often been an ill-disciplined and sprawling project, as well as my mentor Nigel. I am also indebted to my fellow graduate students, especially Ann-Sophie, David, Jean, Jim, Jo, Lou, Kai, Mila, Nick and Trijsje, as well as post-doc and research fellow colleagues such as Berris, Chris, Dan, Daniele and Ginny, who together helped make Egenis a welcoming community of friends as well as minds (this list is far from exhaustive!). Further gratitude for their loving care goes to my immediate family, Ben, Zsuzsa, Marcus and Patti, who have always believed in my potential even when I have been less optimistic. But the one person I owe most to, and without whose undiminished and loving support I could not have done without, is my wife Kay, and it is to her that I dedicate this thesis.

Abstract

This thesis focuses on the role and historical development of strategies of experimental domestication in scientific knowledge production, with a particular focus on the function of the laboratory strains known as 'wild types' in the model organism systems of classical genetics, where they play the role of standing in for the 'natural' instance of the species so that variation may be measured. As part of establishing how lab wild types came to assume this role, I have situated them within a much longer historical trajectory that tracks how changes in the manner that European intellectual traditions conceptualised the domestic-wild divide were linked to the development of new forms of scientific domestication and knowledge production. These new developments required that existing domesticating practices be intensified, expanded and analogised in order to better control, capture and comprehend 'wild' nature. My first two chapters introduce the domestic-wild divide by discussing both contemporary and ancient interpretations of it. In my third and fourth chapter, I explore the roots of the knowledge regime of European scientific domestication. I highlight Francis Bacon's campaign to use knowledge of domesticating practices to restore human dominion, before showing how Linnaeus later re-conceptualised the natural economy as an autonomous order and original order, with domestication reinterpreted as an artful transformation of nature requiring human maintenance to prevent reversion to its wild 'natural state'. I identify this idea of the wild as original and the domestic as derivative and artificially maintained as the basis of the original wild type concept. In my fifth chapter, I discuss Darwin's attempt to unite the domestic and wild under common laws of variation and selection, including his argument that reversion was simply a product of a return to ancestral conditions of existence. I observe that Darwin's theory of variation was problematic for the effort to bring wild nature under controlled conditions for study, so in my sixth and seventh chapters discuss how this difficulty was resolved, first by experimental naturalists both before and after Darwin who utilised vivaria and microscopes to bring pieces of nature indoors, and then by Weismann and Galton's sequestration of heredity, which helped persuade scientists that domestication was not in itself a cause of germinal variation. In my eighth and ninth chapter, I detail how sequestration led the early Mendelians de Vries and Bateson to assume that wild types could be brought into the lab from nature and purified into true-breeding strains. I discuss their differing atomist and interactionist perspectives on wild type, with de Vries favouring 'elementary species' as units of nature, whereas Bateson held wild types and mutants to

represent normal and abnormal forms of the species respectively. In my last chapter, I cover the replacement of Bateson's interactionist genetics by the reductionist genetics of the Morgan group and argue that this led to a disintegration of wild types into their component genes. I conclude with a discussion of what wild type strains in classical genetics were meant to be representative of, and end by establishing that whilst these strains may not wholly be representative of their species, they are nonetheless useful tools for scientific knowledge production.

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Introduction – Introducing Wild Types and the Domestic-Wild Divide

What are Wild Types?

The term ‘wild type’ is generally used in genetics to describe individuals deemed ‘normal’ or typical of their species and also to refer to the ‘normal’ alleles of genes associated with these typical individuals. But more concretely wild type is what the standard lab strains of experimental organisms in genetics are commonly referred to as. The role these wild type strains play in model organism systems in genetics is one of acting as a control against which deviation is measured. As the philosopher Rachel Ankeny has observed, without first establishing a ‘wild type’ “it is not possible to have a ‘norm’ against which ‘abnormal’ (or more precisely, that which is variant) can be compared”. Selection of a wild type is therefore “the first step in the underlying strategy [of classical genetics]”.¹ The necessity of undertaking this first step was recognised early on in genetics. A ‘Report of the Committee on Genetic form and Nomenclature’ from 1921, written up by the mouse geneticist C.C. Little, comments on how “In most animals and plants it is convenient to settle on a standard type, preferably the wild type, when this is known. The effects of the various genetic factors are in general to be measured by the departure from type which they bring about”. It was further noted that “This recommendation involves no real departure from the system now in use by most geneticists”.² The importance of the wild type’s role as an instrument against which to measure variation in the laboratory has therefore been acknowledged both by early 20th century geneticists and some of today’s philosophers of science. It has commonly been emphasised that the historical selection of original model organism species and stocks often had a strongly arbitrary element where the principle concern was readiness to hand, familiarity with life history and known tolerance of captive conditions. This is reflected in the fact that most species used as model organisms in genetics have a prior history of domestication or commensal association with humans. The original stocks on which ‘wild type’ strains were founded were moreover usually locally sourced.³ But, as indicated by the 1921 Committee’s statement that the standard selected for the lab should be “preferably the

1 Rachel A. Ankeny, ‘Wormy Logic: Model Organisms as Case-based Reasoning’, In: Angela N.H. Creager, Elizabeth Lunbeck & M. Norton Wise (Eds.), *Science Without Laws: Model Systems, Cases, Exemplary Narratives*, Duke University Press Books, 2007, pp. 49-50.

2 Clarence Cook Little, ‘Report of the Committee on Genetic form and Nomenclature’, *The American Naturalist*, Vol. 55, No. 637, 1921, p. 176.

3 Rachel A. Ankeny and Sabina Leonelli, ‘What’s So Special About Model Organisms’, *Studies in History and Philosophy of Science*, Vol. 42, 2011, p. 314; & Robert E. Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Life*, The University of Chicago Press, 1994, pp. 19-28.

wild type, when this is known”, there has also historically been a parallel effort to align the laboratory standard organism with some kind of natural standard. In many cases, I believe this appeal to nature reflected an underlying belief in the real existence of typical forms in nature, i.e. actually wild ‘wild types’. I believe that many researchers in early genetics thought inbred domesticated strains a means of studying these theorised natural typical forms under more tractable circumstances. This thesis will seek to establish the historical context of these beliefs by tracing developments in the long history of concepts of the domestic-wild divide.

It should be emphasised that lab wild types are in truth anything but wild or typical. They constitute a specialised class of domesticated organisms, removed from free living in wild or commensal spaces into controlled and artificially uniform laboratory spaces. Here, they are isolated from the natural populations from which they were derived and are forced to adapt to a manmade ecosystem of standardised nutriment, temperature and housing. Moreover, making lab strains useful for breeding experiments typically requires that they be selectively inbred and standardised, as the more homogeneous the strain, the more distinctly underlying recessive mutations are expressed and the easier these mutations are to spot against this standardised background. Given that all this has been known for some time, the claim that lab strains can be representative of a natural and/or typical form of the species has been surprisingly enduring in genetics textbooks. For instance, Griffiths et al in the 1999 edition of the still widely used *Modern Genetic Analysis* define the wild type as “the type observed in the wild, in other words, in nature”.⁴ Other genetics textbooks today do admittedly present a more sceptical account of wild type. Guttman et al’s *Genetics: the Code of Life* (2011) cautions that “the term [wild type] is useful only for certain experimental organisms: for ordinary human characteristics, such as eye color or blood type, no one allele can be considered the wild-type. And wild populations carry several alleles for many genes”.⁵ The problem of utilising standard varieties to represent the species in the lab is here outlined, namely that they cannot be properly representative of the full extent of variation in the wild. This has led to some overt criticism of the very use of the term ‘wild type’. Biologist Ken Weiss for example writes on the blog *The Mermaid’s Tale* that the use of the term ‘wild type’

4 Anthony J.F. Griffiths, William M. Gelbart, Jeffrey H. Miller, & Richard C. Lewontin, *Modern Genetic Analysis*, W.H. Freeman and Company, 1999, p. 15.

5 Burton Guttman, Anthony Griffiths, David Suzuki & Tara Cullis, *Genetics: the Code of Life*, New York: The Rosen Publishing Group, Inc., 2011, p. 128.

in genetics originated out of “a very poor understanding of evolution” and that “If we really want to understand Nature, we should wean ourselves from habits that can be very, if subtly misleading”. These bad habits include what he regards as outmoded terminology that confuses students. Weiss suggests that other terms such as ‘reference’, ‘baseline’ or ‘control’ be preferred, concluding by stating that “to keep from being wildly misled, don’t walk on the wild-type side!”.⁶

My aim in writing this thesis is not to advocate terminological reform. Moreover, criticism of using standardised lab strains as representatives of the ‘normal’ instance of the species is not new or unfashionable. Even those classical genetics researchers who endorsed the term commonly acknowledged that the lab environment was far from naturalistic and the selectively bred strains employed were imperfect representatives of the real wild types assumed to exist in nature. There was also discomfort about whether human interventions such as isolating inbred lineages were representative of evolutionary phenomena in natural populations. What interests me is that despite these known divergences between nature and the lab, classical geneticists generally believed themselves justified in extrapolating to nature knowledge produced in the lab. I believe this can only be understood by placing early genetics in the context of 19th and early 20th century evolutionary theory. What we see from looking back is that the term ‘wild type’ had a prior history before entering the laboratory, and that there has been a long history of using domestic organisms to stand in for the wild and natural, both in abstract argument and concrete experiment. I believe understanding this history can help us understand how it came to be that lab wild types played the role they did in classical genetics and what presumptions about the relationship between the domestic and wild, artificial and natural this research strategy depended on for epistemic legitimacy.

The major purpose of this thesis will be to explain how the wild type concept originated and how it came to be applied as a label to domesticated strains of laboratory model organisms. In order to pursue this goal, I must first establish the broader historical context within which the processes and practices responsible developed. I will be adopting a ‘funnelling’ model of the development of the wild type concept similar to the ‘hourglass’ model first applied by Barahona, Suárez and Rheinberger to the history of heredity and then further developed by

6 Ken Weiss, 'Walk on the wild-type side', *The Mermaid's Tale* (Blog), Thursday, March 21st, 2013. <http://ecodevoevo.blogspot.co.uk/2013/03/walk-on-wild-type-side.html>. Accessed 24/07/2014.

Müller-Wille and Rheinberger.⁷ As in the hourglass model, I will move from the disparate cultural and epistemic worlds from which the elements of and conditions for the wild type concept were drawn to the crystallisation of an idea of wild type itself and its concrete instantiation within the novel experimental systems of the genetics laboratory (“experimental system” is Rheinberger's term for “the smallest integral working units of research”). I will moreover show how in this development the wild type concept was modified to fit into new theoretical frameworks and experimental contexts. I will treat the wild type lab lineages as the end product of this process. They are, in Rheinberger's terminology, the “technical object” (a concrete part of the experimental conditions) which the wild type as “epistemic thing” (the vaguely defined material object of research) eventually condensed into, and it was as a technical object which wild types survived as once the wild-type-as-epistemic-thing disintegrated into and was superseded by the gene-as-epistemic-thing.⁸ Whilst this will be the end-point of my enquiry, it will be processes and practices of *domestication* which will provide the overriding theme and framework of the thesis.

Müller-Wille and Rheinberger speak of modern ideas of heredity as the product of a “knowledge regime” that produces and structures the “epistemic space” necessary to allow for conceptual development and is also in turn reproduced and restructured in its interactions with this space. I will argue that what I call 'scientific domestication' can be interpreted in similar terms (by 'scientific domestication', I mean the use of domesticating practices to discipline space and its inhabitants for the primary purpose of systematic naturalistic knowledge production). Müller-Wille and Rheinberger borrow the term 'knowledge regime' (*regime de savoir* in the original French) from Dominic Pestre. A knowledge regime is defined as deriving from separate heterogeneous contexts and as characterised by an interaction, not a separation, between scientific knowledge production and other cultural domains, including politics, the arts and technology. Embracing this concept allows the historian to “move beyond disciplinary perspectives on science while continuing to focus on its long-term development”. Pestre's knowledge regime shares similarities with Michel Foucault's notion of the ‘power/knowledge regime’, which makes the bolder claim that

7 Ana Barahona, Edna Suárez and Hans-Jörg Rheinberger, 'The Hereditary Hourglass: Narrowing and Expanding the Domain of Heredity', In: *The Hereditary Hourglass, Genetics and Epigenetics, 1868-2000*, Max Planck Preprint 392, 2010, pp. 5-12; & Staffan Müller-Wille and Hans Jörg Rheinberger, *A Cultural History of Heredity*, Chicago and London: The University of Chicago Press, 2012, pp. ix-xii.

8 Hans-Jörg Rheinberger, *Towards a History of Epistemic Things: Synthesizing Proteins in the Test Tube*, Stanford University Press, 1997, pp. 24-31 & 219-220.

knowledge production and power politics not only interact but are furthermore interdependent, as seen in Foucault's assertion that “there is no power relation without the correlative constitution of a field of knowledge, nor any knowledge that does not presuppose and constitute at the same time power relations”.⁹ My understanding of scientific domestication as a knowledge regime embraces both Pestre's emphasis on multiple, heterogeneous and interacting sources (as will be apparent in the diverse range of actors and arenas I will call upon in my narrative) and Foucault's argument for the interdependence of knowledge production and power relations (as is vividly evident in the interdependence of human power over the wild on naturalistic knowledge).

'Epistemic space' is a concept originating in Rheinberger's earlier writings, where it first appears as “*space of representation*”. He asserts that “the experimental scientist... creates a space of representation through the graphematical concatenation which represents the epistemic thing as a kind of ‘writing’”, i.e. new epistemic space is opened up through the activity of representing epistemic things as inscriptions (what Rheinberger, following Derrida, calls “graphemes”), and may furthermore be expanded through the development of second-order representations such as models and theories.¹⁰ This space is material, not ideal, and is concretely produced by the processes and practices which shape the construction and operation of a particular experimental system. But because experimental systems are open-ended, being in Rheinberger's words “vehicles for materializing questions” and not merely providing answers, the development of an epistemic space also feeds back to influence the differential reproduction of the knowledge regime that lies behind the processes and practices of experimentation.¹¹

To extend Rheinberger's ideas to the present case, I argue that the numerous experimental systems which were associated with the knowledge regime of scientific domestication contributed to the opening of a new epistemic space allowing both for novel representations of the domestic-wild divide and for new forms of experimental systems to be devised. This

9 Staffan Müller-Wille and Hans Jörg Rheinberger, *A Cultural History of Heredity*, Chicago and London: The University of Chicago Press, 2012, pp. x-xii; & Michel Foucault, *Discipline and Punish: The Birth of the Prison*, Alan Sheridan (Trans.), New York: Vintage Books, 1995, pp. 27-28.

10 Hans-Jörg Rheinberger, 'Experiment, Difference and Writing: I. Tracing Protein Synthesis', *Studies in History and Philosophy of Science*, Vol. 23, No. 2, 1992, p. 308.

11 Hans-Jörg Rheinberger, *Towards a History of Epistemic Things: Synthesizing Proteins in the Test Tube*, Stanford University Press, 1997, p. 28.

growth in the epistemic space of scientific domestication was also correlated with a growth in its ecological space, as technical developments in experimental systems opened up more and more of the world's wild and inaccessible spaces to intervention, disciplining and exploitation. In this manner the knowledge regime of scientific domestication expanded into geographical space as well as representational space. This growth in space was fundamental not only to the growth of knowledge but also of power, for as Foucault asserts space is “fundamental in... any exercise of power”.¹² The expansion of scientific domestication would thus manifest as a form of colonial encroachment upon 'wild' territory, including both external 'wilds' (e.g. 'wildernesses' inhabited by non-European 'others') and internal 'wilds' (e.g. such partially domesticated places as gardens and fields, where intensification could be applied, and the previously unseen, untamed world of the microscopic).

In establishing how the knowledge regime of scientific domestication eventually produced the wild type, I will move from epistemic space (representations of the domestic and wild) via epistemic thing (the heterogeneous entities and processes called or associated with ‘wild type’ prior to the establishment of standardised lab lineages) to technical object (standardised lab lineages called ‘wild type’ that form part of the experimental infrastructure of genetic investigations). This necessitates moving back through history to a period in time (the late 17th and 18th century) when ‘wild type’ had yet to make itself visible. This will enable me to show what initial practices and processes were at play in early modern scientific utilisation of the sphere of domestication and what subsequent developments in the life sciences and in broader industrial Western society led to the extension and augmentation of domestication in both ecological and representational space, eventually producing the technical object known as ‘wild type’. My main focus will be the history of evolutionary theory and classical genetics. I do not cover events after c. 1950, including highly important developments in practices of scientific domestication such as the molecularisation of genetics, genetic modification and cloning technologies, and genomic sequencing. Nevertheless, I believe that much of what I discuss regarding the role of scientific domestication in the development of lab wild types as tools for knowledge production will remain relevant for more recent developments in the life sciences. One area I believe my historical study will have particular philosophical utility for is the study of contemporary model organism systems, especially

¹² Michel Foucault, 'Space, Knowledge, and Power', Christian Hubert (Trans.), In: *The Foucault Reader*, Paul Rabinow (Ed.), New York: Pantheon Books, 1984, p. 252.

those in genetics. Historical and philosophical research on model organisms has been pioneered by Kohler (*Drosophila*), Rader (*Mus*), Ankeny (*C. elegans*), Leonelli (*Arabidopsis*), etc., but whilst they have treated domestication as an important theme, attention to this topic has usually been ancillary to the main focus, namely exploring the representational role of model organisms in scientific knowledge production.¹³ By examining domestication in greater depth, I will seek to highlight the particular representational difficulties that it generates, particularly the problem of artifice and 'de-naturing', and how practitioners have sought to circumvent such obstacles through material and ideological practices.

The principal claim of the thesis will be that lab 'wild types' were the outcome of a long-term historical trajectory in the life sciences whereby scientific knowledge and technique was produced and extended through the expansion of the sphere of domestication into new physical and representational spaces, and through the intensification of domesticating disciplinary regimes within already extant domestic space. I will illustrate developments in practices and processes of domestication by focusing on strategies and technologies of *intensification*, *encapsulation* and *analogy*, which were utilised respectively to impose greater domestic discipline in existing domesticated spaces, to capture and transfer fragments of wild nature into the domestic sphere, and to apply the logic and language of domestication to 'wild' spaces and in doing so make them more intelligible to human understanding and open to future expedient interventions (note the power of such domesticating metaphors such as 'economy of nature' and 'natural selection').

Tied in to these developments were three key changes in conceptions of the domestic-wild divide which provide the foundation for the logic of the early 20th scientific strategy of using standardised laboratory strains as stand-ins for nature. The first of these was a move from viewing the domestic as original and wildness as a derived 'fallen' state, an idea associated with the Biblical Eden narrative, towards the growing acceptance among 18th and 19th century natural historians that wildness was a primitive state and domestication an artful product of

13 Robert E. Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Life*, The University of Chicago Press, 1994; Karen Ann Rader, *Making Mice: Standardizing Animals for American Biomedical Research, 1900-1955*, Princeton University Press, 2004; Rachel A. Ankeny, 'The Conqueror Worm: An Historical and Philosophical Examination of the Use of the Nematode *C. elegans* as a Model Organism', Thesis (PhD): The University of Pittsburgh, 1997; Sabina Leonelli, 'Weed for Thought: Using *Arabidopsis thaliana* to Understand Plant biology', Thesis (PhD): The University of Amsterdam, 2007.

human endeavour, an 'improving' of nature and not an Edenic restoration. As the 19th century progressed, new technologies of encapsulation such as the vivarium and microscope encouraged the idea that nature could be brought indoors, that wild behaviours and ecologies could be transplanted and maintained within conditions of domestic comfort and convenience. The idea that domestication could be a useful site for studying 'wild' processes usually hidden in nature's tumult and heterogeneity was also encouraged by Charles Darwin's drawing of analogies between gradual natural evolutionary processes and the artificial selection practiced by breeders. This assertion that human art paralleled natural processes, but on a more accessible timescale, helped justify the use of domesticating practices in scientific knowledge production against the charge that they produced creatures and conditions not viable nor visible in wild nature. Finally, late in the 19th century scientific interpretations of inheritance and variation became more hereditarian, with less influence being attributed to the external environment in shaping organismic development and evolution. This would encourage the idea that organisms brought into the lab would not genetically degenerate despite the great change in conditions and that consequently lineages could be brought into the wild and purified into their component types so that in this way the 'normal' type of the species, the 'wild type', could be properly identified and analysed free from the environmental disturbances and genetic intermingling found in wild nature.

To summarise my thesis' structure: my first chapter will consist in a literature review of how the domestic-wild divide has been conceptualised in contemporary debates in the humanities and will seek to establish what the implications these various ways of interpreting domestication and the wild have for attempts to utilise domestication for scientific knowledge production. I will then in my second chapter look at the long-term development of concepts of the domestic and wild from Aristotle to the 17th century 'Scientific Revolution'. My third chapter will focus on gardens and fields as hybrid zones between domestic and wild, and will discuss how this hybridity lent itself to exploitation for the purposes of scientific knowledge production. I in particular highlight the role gardens played in the thinking of Francis Bacon and in his proposed restorationist programme of scientific domestication for the purposes of extending human empire. In Chapter 4, I look at how the naturalist Carl Linnaeus sought to utilise experimental taxonomy as part of a Baconian programme of national improvement through scientific domestication. I furthermore highlight how Linnaeus demarcates the

natural and national economies as parallel states with their own particular order and means of fulfilling their inhabitants' ends, but with the natural economy regarded as original. This would contribute to the idea of wild types as ancestors of domestic varieties and as the normative form of the species within the natural economy. Chapter 5 will begin by establishing when the term 'wild type' started to appear in English language scientific literature (the earliest I have found in 1823), before then establishing the three major notions of wild type – genealogical, comparative and normative – which are already evident in these early references. I will then concentrate on how Darwin helped overturn the assumption that variation under domestication and in the wild differed in kind by drawing analogies between the selective role of animate and inanimate forces in the natural economy and of human breeders in the domestic economy. My sixth chapter will then turn to exploring how the mid-19th century growth in the laboratory life sciences was founded on the earlier success of utilising new inventions such as the Wardian case, aquarium and microscope in order to establish what I have called 'indoor natures'. These indoor natures represented the creation of a novel form of domestication that better enabled living specimens to be brought in from the wild and kept alive for study in the laboratory. I examine how problematic it actually was to emulate nature indoors and how this led to the establishment of conventional forms of practices that were held to be 'naturalistic'. I also look at how, with the professionalisation of the lab life sciences, there came to be an increasing demand for exactitude, which led to greater standardisation and the increased homogenisation of laboratory life into single-species cultures. Chapter 7 returns to discussing evolutionary theory, in particular the late 19th century problem of variation, which Darwin attempted to resolve by assuming variation in the conditions of existence would correlate with organismic variation. I discuss how this claim was problematised for lack of a mechanism of heredity and variation, and how it was rejected by Galton and Weismann, who both more or less sequestered the germ-line from influence by somatic changes. Galton and Weismann offered different solutions to how evolutionary novelty might arise, Galton favouring internally driven qualitative saltational variation, Weismann preferring a limited variability ultimately driven by external influences, out of which variation an all-mighty natural selection could arrange Darwin's 'endless forms most beautiful'. In my eighth chapter, I explore how de Vries and Bateson, assuming the sequestration of the germ-line, sought to determine the internal mechanisms that drove variation through a combination of experimental domestications and field studies,

culminating in de Vries' mutation theory and Bateson's adoption of Mendelism. In the following chapter, I examine how the rediscovered work of Mendel (whose connection to garden-based traditions of experimental scientific domestication I cover in Chapter 4) helped shape both de Vries and Bateson's conceptualisation of wild types (especially Bateson's). I furthermore investigate in both Chapter 8 and 9 how the assumptions and theories of heredity and variation held by de Vries, Bateson and other 'early Mendelians' led to their concluding that wild types were natural entities which could be domesticated in the lab and purified without their capacity to stand-in for the natural form of the species being compromised. In Chapter 10, I discuss how the limitations of treating wild types as holistic products of genetic interaction would eventually lead to the Morgan group adopting a genocentric reductionism which would disintegrate wild types into their component wild type genes, at both considerable epistemic gain and representational cost. In my conclusion, I interrogate how it was that wild types in classical genetics were supposed to act as representatives of nature in the laboratory, how this was important for lab genetics' claim to produce knowledge that could be extrapolated to nature and what actions the Morgan group took to defend its role. Finally, I discuss Dobzhansky's challenge to the very validity of using any single domesticated lab strain as a representative for wild nature's bewildering diversity, before concluding with an assessment of how the 'wild type' scientific domesticating strategy both has serious limitations but has nonetheless been highly epistemically productive.

In summary, the principal achievements of my thesis are to provide wild type with a previously undisclosed *longue durée* history and through establishing this narrative give needed context to the otherwise strange claims of early 20th century classical genetics regarding the use of laboratory strains as stand-ins for nature. I establish this context through outlining some of the major material, social and intellectual conditions, relations and practices which would contribute to the origin and development of the knowledge regime of scientific domestication, thus enabling the opening up of the epistemic space within which the wild type concept would crystallise. I furthermore show how scientific strategies for knowledge production developed novel domesticating practices to better capture and interrogate living nature but faced the continual difficulty of distinguishing natural characteristics from experiment artefacts. I discuss an array of both technical and theoretical strategies used by researchers to circumvent this problem of artifice, and in doing so show

how these innovations fed back into the development of new technologies and theories. I furthermore follow the broad development of ideas of wildness and domesticity in western natural science and show how these both obstructed and enabled efforts to bring the wild into controlled conditions and moreover that the interpretation of organisms as wild or domestic was often central to epistemic and social controversies among scientists.

Chapter 1 – The Domestic and the Wild

Introducing Domestication

There is a rich literature on the subject of domestication in both the human sciences and the life sciences. In philosophy generally, debates regarding domestication have traditionally surfaced most prominently in ethics, especially bioethics, environmental ethics and rights theory. These have principally focused on the moral status of higher animals, i.e. whether humans have a right to use animals for food, clothing, scientific research (medical or otherwise), labour, etc., and what political protections, freedoms and privileges animals should be accorded in a just society (the moral status and rights of plants, other ‘non-sentient’ organisms, and of ecosystems and other superorganic assemblies have for the most part attained much less mainstream prominence). Much of the attention that the peculiar forms of domestication experimentally practiced in the life sciences have received has been linked to these debates on animal moral status and what ethical limitations should be imposed on research. I would argue that these ethical enquiries are mostly concerned with the *conditions* of domestication, i.e. with the nature of particular domestic regimes in a particular place and time, usually the here-and-now, and the quality of life experience of those organisms living under such cultures of domestication. I, on the other hand, am concerned in this thesis principally with how processes and practices of domestication have historically been conceived and how they have been conscripted and manipulated for the purposes of scientific knowledge production. By *processes* of domestication I refer to the more or less individuated constituents/inhabitants of a region/space undergoing domestication, including individual organisms, lineages of organisms, landscapes, machines, etc., which are analysed in terms of their undergoing change through time (becoming more domestic, reverting towards the ‘wild’, remaining in dynamic stasis, etc.), and also the human practitioners and their technical assemblages and tools (including other symbiotic species that act as allies in domesticating practice, [an example would be the role of the sheepdog in the practice of domesticating sheep]), which also are altered through time by their interactions with the entities they attempt to order and control (and are therefore themselves also being domesticated). By *practices* of domestication, I refer to those activities which act to promote domestication, e.g. enclosure (the archetypal example being house-building), regulation (including standardising and streamlining as part of fitting an organism into a disciplinary schema), and manipulation

(including experimental intervention, physiological modification and behavioural manipulation).

Defining and Delimiting Domestication – the Domus

A deceptively simple place to start when trying to arrive at a definition of domestication that can incorporate these different aspects is to look at its etymology. The English word ‘domestication’ comes from the Latin ‘domesticus’, which is derivative of ‘domus’, the Latin term for ‘house’.¹⁴ The domestic-wild dichotomy can here be connected to the pair indoor-outdoor, which in the modern life sciences has come to be heavily associated with the distinction between lab and field. But it should be noted that the original Latin ‘domus’ included anything within the walls of the house, including gardens (*horti*) and unroofed courtyards.¹⁵ The domestic may therefore be open to the elements. Because the Roman ‘domus’ typically referred to a town house, as opposed to the rural villa, an association between domestic-wild and town-country (*urbs* and *rus*) can also be drawn. But the current idea of there being a rigid demarcation between town and country is a thoroughly modern idea born out a combination of the ‘de-ruralization’ of towns in the industrial age and the accompanying romanticisation of the countryside as an unspoiled place of calm and innocence.¹⁶ Though the Romans too were guilty of idyllising country life (e.g. the pastoral poems of Horace), they did not draw a strict border between town and country. There was a clear core to the *urbs*, this being the *continentia aedificia*, the ‘built-up area’, or (as Nicholas Purcell translates), ‘buildings with no space in between’. But as Purcell explains “it would not... have occurred to [Roman] people to say, ‘Here is the edge of the *continentia aedificia*, the *rus* starts here’”. The boundary was more a continuum characterised by the extreme between on the one hand the claustrophobic, constricted and busy spaces of the *continentia aedificia* and on the other the open and empty terrain of the countryside. The *rus* was moreover by no means necessarily ‘wild’ as we would imagine it, for it included the ‘tame’ countryside of farmers’ fields and pastures, areas we would associate with domestication. There was a wild *rus* of the ‘solitudines’ – woods, mountains and other uncivilised ‘empty spaces’.

14 James Donald (Ed.), *Chamber's Etymological Dictionary of the English Language*, London & Edinburgh: W. & R. Chambers, 1872, p. 138.

15 John R. Clarke, *The Houses of the Roman Empire, 100 B.C.-A.D. 250: Ritual, Space, and Decoration*, University of California Press, 1991, p. 12.

16 Keith Thomas, *Man and the Natural World: Changing Attitudes in England 1500-1800*, Allen Lane, 1983, pp. 249-250.

sacred groves, were also regarded as ‘solitudines’. Much of the Roman *rus* was therefore “much more cheery at least in its idealized form than the woods, stones, and swamps of the real wild”.¹⁷ The idea of a strictly demarcated domestic and wild is therefore modern and not inherent in the original Latin concept of ‘domesticus’, which furthermore could not be simply equated with ideas of the indoor and the town as contrasted with the outdoor and the country.

Part of the ambiguity in the Latin term ‘domesticus’ lies in ambiguities in its parent term, ‘domus’. Whilst ‘domus’ could simply refer to the house, it could also be used as a synonym for the household – i.e. the family (*familia*), which for the Romans included live-in tenants and slaves (the word ‘familia’ indeed descends from the older ‘famulus’, ‘slave’) – and for the broader kinship group associated with the major household lineage. It was also used as a metonym for the (usually male) head of the household, the *paterfamilias*, who was also often referred to as the *dominus*, meaning ‘master’ or ‘slave-owner’. The *dominus* exercised control over a *dominium*, which developed under the Empire into a legal meaning as absolute private property. ‘Domus’ was with some regularity used as a synonym for ‘dominium’. But as with the English word ‘property’, houses were only one of many things that could be regarded as part of a *dominium*. Indeed, it has been argued that, as with *familia*, the Roman notion of *dominium* derived from slave-keeping, i.e. from the social relations found in the household, and was therefore not directly related to the idea of the *domus* as a wall-bound space. The extension of *dominium* from its original application in master-slave social relations to its legal application to all things regarded ‘property’ was therefore a legal extension of the recognised authority of the *dominus* over the household *familia* to non-human organisms and inanimate objects. It did not, in other words, derive from the idea of the *domus*-as-physical-house as a container of domestic things, and these things as being property on account of being part of the larger property of the house. The extension of this logic from human-human relations to human-animal/plant and human-object relations occurred, Graeber (after Patterson) notes, through the increasing treatment of human property, i.e. slaves, as *res*, ‘things’. He thus observes that “In creating a notion of *dominium*... what Roman jurists were doing first of all was taking a principle of domestic authority, of absolute power over people, defining some of those people (slaves) as things, and then extending the logic that originally

17 Nicholas Purcell, ‘Town in Country and Country in Town’, In: *Ancient Roman Villa Gardens*, Elisabeth Blair MacDougall (Ed.), *Dumbarton Oaks Colloquium on the History of Landscape Architecture* (10th: 1984), 1987, pp. 189-202.

applied to slaves to geese, chariots, barns, jewelry boxes, and so forth-that is, to every other sort of thing that the law had anything to do with".¹⁸

From what has been said, it can be seen that the *domus*, even in its original Latin meaning, was never a neutral space determined by simply being bounded by walls but rather a social space produced through human-human, human-animal/plant and human-object relations of hierarchy, domination and ownership. This observation that the walls of a house are less important in bounding space than the activities within those walls is supported by the analysis of social space offered by the geographer Henri Lefebvre. Walls, he observes, are really "glorified screens", and when stripped away it becomes apparent that what really constitutes a house is its acting as a nexus for the convergence of mobile energies. The identity of a household is as a "machine analogous to an active body", not as 'immovable property'.¹⁹ Whilst the energies and information Lefebvre mainly invokes are facets of the modern age such as electricity, water, television signals, etc., I believe this treatment of the house as an active nexus for domestic activities rather than simply a passive container can be more broadly applied to households in non-industrial eras and places, not least as it enables one to distinguish between the ordered domesticity of an active inhabited household and the lack of such order or human activity found in an abandoned building. Further support for this view is found in what Tim Ingold calls the 'dwelling perspective' in anthropology. This is contrasted with the older 'building' perspective, which argued that "worlds are made before they are lived in". Ingold selects Peter Wilson's 1988 *Domestication of the Human Species* as a typical statement of this view. Wilson argues the building of permanent houses to have marked the point in human social evolution when a sharp delineation was formed between hunter-gatherers, who "create for themselves only the flimsiest architectural context, and only the faintest line divides their living space from nature", and sedentary agriculturalists who permanently inhabit an "architecturally modified environment". The dwelling perspective takes inspiration from the philosopher Martin Heidegger's contrary claim that "*Only if we are capable of dwelling, only then can we build*". Unlike the building perspective, the dwelling perspective does not assume the necessity of a rational design that is then executed

18 Richard P. Saller, 'Familia, Domus, and the Roman Conception of the Family', *Phoenix*, Vol. 38, No. 4, 1984, pp 342-343; & David Graeber, *Debt: The First 5,000 Years*, First Melville House Printing, 2011, pp. 199-201.

19 Henri Lefebvre, *The Production of Space*, Donald Nicholson-Smith (Trans.), Blackwell Publishing, 1991, pp. 92-93.

to produce a house but instead stresses the continual activity of organisms, human and non-human, to modify their environments in order to make themselves more at home. House-building is therefore a derivative form of this generally organismic tendency to attempt to domesticate the environment, not an initial form. Ingold therefore concludes that the distinction between the use of a house by human beings and of a tree by woodland animals is a relative not an absolute one, for in both cases they act as a place of dwelling and are shaped by the interactions of the organisms which inhabit them. A house can therefore metaphorically be seen as an organism with a life history; the more dominated by human interactions, the more of a built domestic environment it is, but when non-human interactions predominate, it may be regarded as becoming an increasingly wild dwelling. I believe it to be a logical extension of Ingold's arguments to see the domesticating tendency as ultimately an aspect of the more general evolutionary phenomenon of niche construction. This accords with the fact that (a) the construction of houses better enables the perpetuation and spread of human lineages and culture by providing shelter from the elements including in climates often too hostile for continual outdoor existence, as well as storage of food and other resources for times of scarcity and defence against predation; (b) houses are part of the inherited environment of offspring and therefore continue to benefit survival and reproduction; (c) like all constructed environments, houses require continual maintenance to sustain and optimise their utility – this is one of the grounds of Ingold's critique of the 'building perspective', as it ignores the fact that building "is a process that is continually going on, for as long as people dwell in an environment. It does not begin here, with a pre-formed plan, and end there, with a finished artefact".²⁰ Domestication is therefore not an established state but rather a continual dialogue and dialectic between the *domus* and the wild.

Defining & Delimiting Domestication – Nature & the Wild

Having looked for clues to defining domestication in its etymological origins and its relationship with the house and household, the *domus*, I now turn to attempts to define domestication in terms of what it is to be contrasted with. In Western discourse, domestication is most typically contrasted with the 'wild', which is typically represented as

20 Tim Ingold, 'Building, Dwelling, Living: How Animals and People Make Themselves at Home in the World', *The Perception of the Environment: Essays on Livelihood, Dwelling and Skill*, London and New York: Routledge, 2000, pp. 172-188.

being prior to domestication and without domestic space. The 'wild' is also often conflated with 'nature', and as a consequence the domestic is widely associated with nature's other many antonyms – e.g. society, culture, art, civilisation, technology, etc. The treatment of the domestic-wild and society-nature as synonymous has been especially widespread. It has, however, come under criticism from anthropologists studying some traditional non-Western cultures where it is claimed that a notion of a domestic-wild distinction exists without an associated and parallel society-nature dichotomy. The Mount Hageners of New Guinea, for instance, have conceptions of *mbo* and *rømi* that have certain parallels with society-nature but do not denote a society carved out of nature but rather the distinction between what is inside and what is outside the limits of human concern and care. This distinction does not necessarily coincide with the limits of human physical transformation of the local landscape.²¹

That 'nature' is one of Western epistemology's most nebulous categories is further unhelpful if its meaning is to be used to clarify that of 'wild'. The amorphous character of conceptions of 'nature' is long-standing – already in Aristotle, seven meanings of the Greek equivalent term 'physis' are identified.²² In the 17th century, Robert Boyle attempted to clarify use of the word by rejecting those interpretations (eight identified in total) he believed leaned themselves to pantheist or atheist cosmologies. His efforts, however, were largely in vain.²³ There is far too little space here to go into detail regarding the many, many interpretations of nature identified by scholars. I will, however, highlight two philosophers' taxonomies of nature which are useful for the purposes of discussing the domestic-wild dichotomy. Neither of these taxonomies is especially detailed, unlike Aristotle and Boyle's, but they have the advantage of reducing the variety of conceptions into two contrasting classes and of demonstrating long-term continuities in ideas of art and nature. The first taxonomy I will introduce is that identified by R.G. Collingwood in his 1945 *Idea of Nature*. This is the distinction between *physis* and *kosmos*, the two ideas of nature Collingwood claims to have been most prominent amongst the ancient Greeks. Of the two, *physis* is the older idea of

21 Marilyn Strathern, 'No Nature, No Culture: the Hagen Case', In: *Nature, Culture and Gender*, Carol MacCormack and Marilyn Strathern (Eds.), Cambridge: Cambridge University Press, 1980, pp. 174-219.

22 R.G. Collingwood, *The Idea of Nature*, Oxford University Press, 1945, pp. 80-82.

23 Ian Hacking, 'Almost Zero' (Review: 'The Veil of Isis: An Essay on the History of the Idea of Nature' by Pierre Hadot), *The London Review of Books*, Vol. 29, No. 9, 2007, pp. 29-30; & Robert Boyle, *A Free Enquiry into the Vulgarly Received Notion of Nature*, Edward B. Davis and Michael Hunter (Eds.), Cambridge University Press, 1996, pp. 3-4.

nature and denotes that which is “the internal source of a thing’s behaviour”, whereas the later *kosmos* refers to “the sum total or aggregate of natural things”.²⁴

I will return to *kosmos* shortly, but will first focus on *phusis*. Originally, *phusis* had quite a different meaning, Pierre Hadot asserting that it was only in the later 5th and 4th centuries BC that *phusis* became strongly associated with individual constitution and essence. Beforehand, *phusis* was principally used to refer to generative processes and also their productions. This idea of *phusis* was associated with a Pre-Socratic philosophy that saw nature and its beings as mere composites of processes of generation and decay. This view is exemplified in Empedocles' assertion that “There is absolutely no birth [*phusis*] for all mortal things, nor end, in detested death, but there is only mixture and distinction of mixed-up things, and this is what men call *phusis*”. It was the Sophists and Hippocraticians who would later reinterpret *phusis* as primarily related to constitution, in the latter case particularly related to the constitution of the human body. Plato and Aristotle then abstracted as 'essence' this constitutive notion of *phusis* and attributed to this essence the generative power to produce specific forms and their natural behaviours.²⁵ *Phusis* in this manner changed from principally referring to processes of growth to principally referring to that which lay behind growth and determined constitution and behaviour. Moreover, by distinguishing between processes and the power to produce them, philosophers such as Aristotle could also distinguish between 'natural' behaviour/constitution (i.e. when an entity behaves only according to its internal source of behaviour) and 'forced' behaviour/constitution (i.e. when an entity's natural behaviour is constrained or compelled by external forces).²⁶ Making this distinction, as shall be seen, was key to Aristotle's account of art and nature, and through this influenced his interpretation of the domestic and the wild.

That *phusis* increasingly came to refer to individual inner natures, as opposed to collective processes of generation and corruption, may explain why *kosmos* emerged as an alternative term to capture the sense of nature as the sum of natural entities, which *phusis* originally had covered but had since diverged from. By adopting these two distinct notions of nature, Aristotelians were able to treat nature as both the totality of all processes (*kosmos*) and as a

24 R.G. Collingwood, *The Idea of Nature*, Oxford University Press, 1945, pp. 43-45.

25 Pierre Hadot, *The Veil of Isis: An Essay on the History of the Idea of Nature*, Michael Chase (Trans.), The Belknap Press of Harvard University Press, 2006, pp. 7-19.

26 R.G. Collingwood, *The Idea of Nature*, Oxford University Press, 1945, pp. 43-45.

particular process/entity interacting with other natural influences. This twofold interpretation of nature as both external totality and internal inclination has remained highly influential in Western thought, despite efforts to reduce this dualistic picture to a uniform monism. It is for instance visible in the presentation of the wild both as everything spatially that lies beyond domestication and as the inherent natural instinct and constitution within organisms which must be modified in order to tame and control them even once they are brought into domestic space (an inner wildness that may nonetheless remain latent awaiting reactivation). The idea of natural things as directed by an internal inclination would come to be challenged from the 17th century onwards by mechanistic worldviews which denied nature an agency independent of God and instead presented motion as due to the inertial conservation of momentum and natural interactions as the mere transference of momentum between entities. But nonetheless the opposition between forced and natural behaviour found in the Greek idea of nature as *phusis* survived in the vast majority of mechanistic cosmologies in the form of an opposition between nature's order and the human capacity for intervention and artifice. The endurance of this opposition between nature as all-encompassing and nature as distinct from art is apparent in the second taxonomy of nature I have selected, that of John Stuart Mill. Mill identifies as nature's two principal meanings a first sense by which 'nature' means "all the powers existing in either the outer or the inner world and everything which takes place by means of those powers", and a second sense by which "it means, not everything which happens, but only what takes place without the agency, or without the voluntary and intentional agency, of man".²⁷ I thus contend that whilst much else about the concept of nature has been changeable between ancient and modern times, this twofold conception of nature as both containing and distinct from the world of human artifice has been remarkably enduring. And I believe that it is the ambiguous place of art within nature which has leant itself to the ambiguous status of domestication and domestic organisms.

Controlling the Wild

Having established domestication's ambiguous place in nature, I now move on to examining the wild's ambiguous place within domestication. Whilst it is true that 'wildness' is often an admired personal characteristic, especially in association with male virility and creative lack of restraint, and is also occasionally sought as a trait in domestic animals, e.g. by some

²⁷ John Stuart Mill, 'On Nature', *Nature, The Utility of Religion and Theism*, Watts & Co., 1904, p. 9.

breeders of semi-feral horses such as the Icelandic breed, it otherwise largely has negative connotations in a domestic setting. In particular, the wild has associations with the ‘savage’, an English term that originates from the French word for ‘wild’, ‘sauvage’ (the common translation of ‘wild type’ into French is incidentally ‘type sauvage’ which is moreover likely older than the English term).²⁸ However, due to its applications to hostile animals and ‘barbarian’ peoples encountered during European colonial expansion, it today has acquired strong associations with anti-social behaviour, e.g. violence, rudeness, lewdness and ferocity. This last association with ferocity is reflected in discourses on domestic organisms by the prevalence of the term ‘feral’. Derived from the Latin for ‘fierce’, *ferox*, ‘feral’ has come to be applied to plants and (especially) animals which have moved from domestication back into nature, in the process reverting also to their ancestors’ fierce behaviour and unfriendly disposition to humans. The fierce and unfriendly behaviour of many wild organisms also contributes to another of the wild’s set of associations, namely with that which is untamed, unbounded and uncontrolled. The wild in this context is commonly conceived as representing a threat to the social and moral order of domestication.

The idea of the wild as disturbing the domestic order appears to have been common throughout human history and across many cultures. It has been theorised to have deep roots in the origins of the domesticating process. The post-processualist archaeologist Ian Hodder has for instance argued that the advent of sedentism and Neolithic agriculture in the Near East and south-east Europe was marked by the development of a symbolism of the house (*domus*), which was viewed as a site of structure, stability and history, reinforced by practices such as the interring of the dead beneath the floors. This symbolism of the house was contrasted with a symbolism of the wild (*agrios*), which gave meaning to the security offered by the house by being presented as a region of danger and disorder. The power of the *domus* according to Hodder lay in its capacity to domesticate, either through the actual bringing in and taming of wild organisms or through symbolic taming, e.g. through placing representations of wild animals in alcoves. As Hodder summarises, “Domestication involved creating the wild as ‘other’, and establishing the domestic, the house as the structured, the stable, the long term. As wild plants and animals were brought in and domesticated according

28 E.g. see: *Nouveau Cours Complet D'Agriculture: Théorique et Pratique (Tome Onzième)*, Paris: Chez Deterville, 1809, p. 355, where the *sanglier* (wild boar) is stated to be “C'est le type sauvage du cochon domestique” (wild type of the domestic pig).

to a practical logic, so symbols of the wild were created and tamed symbolically. The symbols of the wild were controlled within a cultural metaphor within the house”.²⁹ Hodder thus here emphasises the role that the house can play as a disciplinary space, where animals and plants can be brought in and made to obey the moral and social order of the *domus* – ‘house trained’ as it were. He furthermore allies his work with that of Foucault’s analysis of the prison in *Discipline and Punish*, describing the *domus* as “a power-knowledge-truth network” linking “idea, economy and social relations of dominance through the control of production, reproduction and exchange”, in the process “creating ‘docile’ bodies in settled villages”.³⁰

Hodder admits differences between the Neolithic domesticating process and the early modern prison system as analysed by Foucault in that the former occurred through accidental historical circumstance rather than an intentional plot by dominant groups. But it appears clear that when he is speaking about the accidental as opposed to intentional production of ‘docile bodies’, he is principally discussing human-human relations. His conception of human-animal/plant relations, on the other hand, is much more one of the intentional effort of humans to subjugate non-human organisms within domestic space either physically or symbolically. Nevertheless, it is clear that the unwritten rules which shape the social space of a household, imposing domesticating discipline on a nexus of otherwise unstable relations, affect animals and plants just as they do people. Because non-humans do not normally comprehend these human-made conventions, their encounter with the domestic order is usually in the form of conflict. For this reason, non-humans are usually perceived by domestic inhabitants as a source of disorder requiring expulsion to protect the *domus*. For a non-human to be accepted and successfully brought within the *domus* requires that they come to learn their proper place within the household moral economy – they must be house trained. It is no coincidence that the exemplary case of house training is teaching animals to defecate in a particular assigned place, for the effort to control dirt, as Mary Douglas observes in *Purity and Danger*, is a common manifestation of human efforts to impose a moral order on domestic space. Dirt, she quips, “is essentially disorder”, i.e. part of the ‘wild’ that must be kept out of the *domus*. There is, she continues, “no such thing as absolute dirt”, as dirt is

29 Ian Hodder, 'The Domestication of Europe', In: *Theory and Practice in Archaeology*, London and New York: Routledge, 1995, pp. 208-210.

30 Ian Hodder, 'Towards a Coherent Archaeology', In: *Theory and Practice in Archaeology*, London and New York: Routledge, 1995, p. 151.

“matter out of place” that therefore “offends against order”. Consequently “Eliminating [dirt] is not a negative movement, but a positive effort to organise the environment”, i.e. to enforce an underlying conceived moral order towards which all members of the household and community, be they human, animal, plant or mineral, must learn to accommodate their behaviour if they are to be tolerated.³¹ In Douglas as in Hodder, the ‘wild’, in this case ‘dirt’, is presented as a source of disorder which threatens to undermine the social and moral order of the *domus*.

Hodder notes that he does not “see the *domus/agrios* opposition as universal”, stating that his characterisation of it best fits the Neolithic Near East and south-east Europe, whereas different regimes of domestication were prevalent in central and northern Europe, where the *domus* was centred on megalithic monuments and tombs respectively. “The *domus*”, Hodder summarises, “was given different local meanings”.³² The particular hostility to the *wild/agrios* of the Near East/south-east European model of the *domus*, as well as its perceived power to domesticate through ‘bringing in’ the wild physically or symbolically, was therefore, the argument goes, also a local manifestation. But it seems clear to me that Hodder thinks this particular ancient model for demarcating wild and domestic was ultimately more successful than rival schemas and has down the line been more influential on modern European perceptions. I believe that Hodder ties the spread of the *domus-agrios* opposition to the spread across Europe over millennia of the kind of agrarian economies that had originally appeared to its south-east. Hodder is thus treating the Neolithic Near East as a model of what Europe would become, and by inferring from the archaeological remains the kind of culture this society produced is attempting to trace the roots of ‘civilised’ European thought. There is certainly room to criticise Hodder’s overly speculative account. His treatment of the domestic as a feminine space and the wild as a masculine one depends, Tringham notes, on assuming generic gender relations, i.e. those found in mid-20th century industrial Europe and America, to hold across space and time in the Neolithic Near East.³³ Hodder himself has admitted that much of the logic of his own thinking about domestication

31 Mary Douglas, *Purity and Danger: An Analysis of Concepts of Pollution and Taboo*, London and New York: Routledge, 1984, pp. 2 & 36.

32 Ian Hodder, ‘The Domestication of Europe’, In: *Theory and Practice in Archaeology*, London and New York: Routledge, 1995, p. 216.

33 Ruth E. Tringham, ‘Households with Faces: The Challenge of Gender in Prehistoric Architectural Remains’, In: *Engendering Archaeology: Women in Prehistory*, Joan M. Gero and Margaret W. Conkey (Eds.), Blackwell Publishers: Oxford, 1991, pp. 93-131.

was led by terminological associations, stating that if he had focused on the Greek idea of the *oikos* instead of the Roman idea of the *domus*, then a host of different associated terms would have likely come into consideration, e.g. economy, ecology, etc., and the *domus-agrios*/domestic-wild distinction may have escaped attention.³⁴ Davis picks up on this point, observing that what Hodder really seems to have in mind is not the *domus-agrios* distinction but rather the ancient Greek idea of the *oikos-polis* distinction, i.e. the contrast not between domestic and wild but rather between “two differing species of domesticity”.³⁵

It is probably the case that Hodder, in his effort to reconstruct past cultural norms, has imposed a 20th century idea of a sharp demarcation of the domestic-wild onto Neolithic peoples only recently transitioned into sedentism. Nonetheless, there is a broader lesson which can be drawn from archaeological and anthropological studies of prehistoric and ‘primitive’ contemporary agrarian societies, namely that the idea of a sharp divide between domestic and wild is very much the creation of a domesticated mind. Nerissa Russell thus observes that “[W]hen ancient peoples domesticated plants and animals, among other things they created a category of the Wild. The Wild cannot exist until there is a Domestic”. She goes further to state that “Although not every society may stress the wild/domestic distinction, most with domestic animals regard this as important”.³⁶ The extent to which a marked divide is observed between domestic and wild is therefore indicative of the socio-economic mode of life predominant within a particular culture and, moreover, of the extent to which processes and practices of domestication have advanced in that culture. More sedentary and technological cultures have long recognised the role of domestication in creating their societies, Kay Anderson observing that there is an extensive Western scholarly tradition which uses the capacity to domesticate as a benchmark distinguishing the ‘civilised’ world from that of ‘wild’ barbarism.³⁷ Human domestication has moreover been suggested by Helen Leach to have been responsible for physiological changes analogous to those that occur in domesticated animals, e.g. gracilisation, initial decline in body size, and shortening

34 Ian Hodder, 'Towards a Coherent Archaeology', In: *Theory and Practice in Archaeology*, London and New York: Routledge, 1995, p. 151.

35 Whitney Davis, *Replications: Archaeology, Art History, Psychoanalysis*, The Pennsylvania State University Press, 1996, pp. 123-125.

36 Nerissa Russell, 'The Wild Side of Animal Domestication', *Society & Animals*, Vol. 10, No. 3, 2002, pp. 295-296.

37 Kay Anderson, 'A walk on the wild side: a critical geography of domestication', *Progress in Human Geography*, Vol. 21, No. 4, 1997, pp. 467-473.

of the jaw.³⁸ The domestic-wild divide has thus served to demarcate bodies as domestic as well as domesticate minds.

The sharpness of demarcation perceived between the domestic and wild (and more broadly between the artificial and natural) in our contemporary globalised society, compared even to the cultural perceptions of 17th century Europeans, is evidence of just how far such processes and practices of domestication have advanced in recent centuries. The perceived gap between nature and culture, which has long dogged scientific efforts to study nature naturalistically, has therefore been widened with each successful scientific domestication, for the more scientific artifice and technique has advanced, the greater the apparent disparity between scientific technical assemblages and ‘free’ nature. But it has also been the social effects of this development of technology, e.g. the shift from dependence on animals to dependence on machines associated with the move from peasant economies to industrial ones, the mass urbanisation which accompanied it and the increasingly indoor and built-up nature of the work environment, which have also perhaps made the inhabitants of developed nations even more domesticated and removed from the wild than before, or at the very least made the demarcation appear much more radical and real.³⁹ The history of the widening of the domestic-wild dichotomy is therefore in some ways the history of civilisation and its discontents.

Relations of Domestication

But it would be a mistake to conflate domestication with agrarian civilisation. Whilst the processes of domestication and related technological developments associated with sedentary agriculture and animal husbandry have tended to exacerbate the apparent divide between wild and domestic, domesticating practices in themselves have deeper roots in human history. As we have noted above, if we adopt the ‘dwelling perspective’ advocated by Ingold, then domesticating looks increasingly like a form of niche construction based on ancestral practices which precede building. This implies that not only are domesticating practices not exclusively the preserve of sedentary peoples but also that they are shared in some form or another with non-human animals. Ingold for instance points to the tendency of the great apes

38 Helen M. Leach, 'Human Domestication Reconsidered', *Current Anthropology*, Vol. 44, No. 3, 2003, pp. 349-368.

39 Keith Thomas, *Man and the Natural World: Changing Attitudes in England 1500-1800*, Allen Lane, 1983, pp. 181-182 & 242-287.

to construct nightly 'nests' as an example of non-human relatives shaping the environment to better shelter themselves from the elements. There is admittedly a significant difference between these temporary manipulations of the immediate environment and human modifications. But the key technology to humanity's greater capacity for niche construction is arguably not house-building but rather fire. The hearth played a central role in the majority of pre-industrial households as provider of light, heat and power (especially the power to process raw food). Given how long humanity has had some control of fire (estimated by some archaeologists to date back 1,400,000 years), it could be argued that the house was built around the hearth rather than the hearth placed in the house. The domestication of fire may therefore be regarded as humanity's original and most influential domestication, and one which preceded permanent settlements by hundreds of thousands of years. Johan Goudsblom has on this basis argued that the 'dawn of civilization' should be associated not with Neolithic agrarianisation or early modern industrialisation but rather with control of fire.⁴⁰ The use of fire for niche construction by pre-settled peoples is perhaps most conspicuous in the formation of the Great Plains of North America, large tracts of which are increasingly believed to have become established as a result of the deliberate burning by Native Americans of woodland and scrub, among other things in order to promote the grasslands favoured by their main food animal, the bison.⁴¹

Treating domestication purely in terms of its spatial/social elements, its relations to the technologies of the hearth and of house-building and its development out of pre-existing evolutionary tendencies towards niche construction would be to ignore its most intriguing aspect, namely interspecies relations and symbiosis. Whilst fire certainly allowed humans to transform the landscape, better utilise food resources, defend themselves against hostile animals and colonise colder climates, the relation between humanity and fire, though certainly one where human projection of personality and supernatural power to this natural force has been historically important, has not had either the reciprocal emotive element commonly found between domestic animals and humans or the mutually altering co-dependence found between humans and food plant and animals that has characterised agricultural and pastoral lifestyles. Fire certainly impacted on human evolution, but the brute

40 Johan Goudsblom, 'The Civilizing Process and the Domestication of Fire', *Journal of World History*, Vol. 3, No. 1, 1992, p. 1-11.

41 Stephen J. Pyne, *Fire in America: A Cultural History of Wildland and Rural Fire*, Princeton, New Jersey: Princeton University Press, 1982, pp. 66-122.

facts of its physics and chemistry have not evolved as a result of this interaction. On the other hand, domestic and commensal species have over time undergone dramatic alterations compared to their known wild types, to the extent that some domestic varieties are completely dependent on human care for their existence. This is even truer of some experimental varieties, particularly those deliberately engineered as disease or deficiency models, e.g. gene 'knockout' mice. Equally, the vast majority of human beings alive today are dependent almost entirely on domesticated food sources, especially crops such as wheat and rice. Most animal protein is also sourced from domestic stock such as chickens, cattle, pigs, sheep and goats, and that proportion sourced from wild animal protein, e.g. fish and game, is decreasing per head as wild stocks are put under increasing pressure by exploitation. Domestic human-animal/plant relations (and those with other life forms such as fungi, e.g. yeast, and microbes) have in this manner arguably shaped human evolution as much as technological advances such as control of fire.

It should be emphasised that the origins and nature of interspecific relations under domestication are heterogeneous. The argument for an inherent connection between domestication and sedentary agriculture is, for instance, problematised by the fact that humanity's oldest animal partnership, namely with the wolf/dog, may pre-date the Neolithic agricultural revolution by tens of millennia (though domestication dates around 15,000 BP are generally considered more orthodox).⁴² It is similarly conceivable that the human domestication of herd animals such as cattle, sheep and goats may have been initiated prior to the move to sedentism as part of nomadic practices of following and directing wild herds with the assistance of dogs.⁴³ This may be one motivation for reforming the terminology of domestication, e.g. by adopting alternative terms that do not imply sedentism such as 'human-animal relations' or 'cultural control'. Another strategy is to analyse domestication into more homogeneous categories, as Ingold does when he suggests that three distinct and independent elements of animal domestication can be discerned, namely taming (bringing into household), herding (treatment of animals as property) and breeding (control of reproduction).⁴⁴ However, I believe the *domus* an important enough site in scientific domesticating practices that I think it would not benefit my analysis to adopt this alternative terminology. I also believe the

42 Mikhail V. Sablin and Gennady A. Khlopachev, 'The Earliest Ice Age Dogs: Evidence from Eliseevichi 1', *Current Anthropology*, Vol. 43, No. 5, 2002, pp. 795-799.

43 Nerissa Russell, 'The Wild Side of Animal Domestication', *Society & Animals*, Vol. 10, No. 3, 2002, p. 289.

44 *Ibid.*, pp. 290-292.

alternative terms have their limitations ('human-animal relations' neglects domestic relations between humans and plants, fungi, microbes, etc., whereas 'cultural control' slants too heavily towards narratives of human domination). Additionally, whilst I find Ingold's analysis useful, not least because it is very true that the various forms of domestication do not necessarily coincide, it should nonetheless be kept in mind that all three elements are closely integrated in most 'typical' cases of domestication, as well as in the majority of those experimental forms of scientific domestication which are the focus of this thesis.

Whilst human-dog relations certainly pre-date agriculture, for most other animal species the human move to sedentism was a definite condition enabling their domestication. This is particularly true of those species that originally entered the domestic sphere as commensals. These species were likely first lured into houses by left over human food waste, e.g. pigeons, or by the rodent prey this bounty also attracted, e.g. cats. Commensal rodents such as mice and rats themselves eventually became domesticates and came to play an important role in the story of scientific domestication. But other commensal animals, whilst strongly associated with human habitations, have by and large not been truly domesticated due to their lack of perceived aesthetic appeal or utility when tamed, e.g. the house sparrow.⁴⁵ There are also species which are part-time commensals, e.g. birds such as swallows and martins that have used human houses for summer nesting for thousands of years (in some cases, such as the American purple martin, a species has become heavily dependent for its survival on such commensal nesting) but which migrate to wilder regions in the winter.⁴⁶ Whilst commensals commonly enter the domestic sphere voluntarily, other species more clearly require being forcibly introduced. This is especially evident in those species which may be tamed but which do not or rarely reproduce in captivity, e.g. elephants.⁴⁷ For these species, adaptation to domestic conditions is limited by the need to replenish each generation with new stock taken

45 Robert E. Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Life*, The University of Chicago Press, 1994, pp. 9-10; & Carlos A. Driscoll, David W. Macdonald and Stephen J. O'Brien, 'From Wild Animals to Domestic Pets, an Evolutionary View of Domestication', *Proceedings of the National Academy of Sciences of the United States of America*, Vol. 106, Supplement 1: In the Light of Evolution III: Two Centuries of Darwin, 2009, pp. 9974-9977.

46 For details on the purple martin's heavy dependence on artificial nest sites in the eastern United States, see: Robin Doughty and Rob Fergus, *The Purple Martin*, University of Texas Press, 2002.

47 On elephants, see for instance: William Lawrence, *Lectures on Comparative Anatomy, Physiology, Zoology, and the Natural history of Man*, London: John Taylor, 1840, pp. 178-9; & Carlos A. Driscoll, David W. Macdonald and Stephen J. O'Brien, 'From Wild Animals to Domestic Pets, an Evolutionary View of Domestication', *Proceedings of the National Academy of Sciences of the United States of America*, Vol. 106, Supplement 1: In the Light of Evolution III: Two Centuries of Darwin, 2009, p. 9972.

from the wild. Capture is, however, not always permanent, as seen in the example of semi-feral breeds such as the Camargue horse and cattle, which traditionally were only rounded-up for recording and medical purposes or when their use for mounts, bullfighting or meat was desired.⁴⁸ Adding to the heterogeneity of animal domestication are zoo animals, originally captured from the wild for entertainment and aesthetic purposes but today increasingly repurposed (despite their common tameness) as ambassadors for wild nature. In the case of endangered species, zoo stock usually act as a breeding pool for the species, with the common intent of using them to repopulate areas of the wild where their species has been extirpated – necessitating, of course, the re-wilding of captive-bred stock, a process often as difficult as domestication.⁴⁹

Plants are in many ways even more amenable to domestication than animals as, rooted in place, their movements are more easily controlled. Furthermore, their embryos (seeds) can be gathered and stored until needed or transported to where needed. Their sexual reproductive process, once understood, is relatively easy to intervene in, though, with the possible exception of date palms (based on a contested interpretation of Assyrian pictorial reliefs), it was not until the 18th century that artificial pollination became widely utilised.⁵⁰ Plants also often reproduce asexually, which can be exploited by human breeders to perpetuate clonal lineages with desired features. Admittedly, there are problems faced domesticating plants that do not apply to animal domestication, e.g. fertilisation of cultivars by wild pollen borne on the wind or by insects, as well as the difficulty of controlling competition from weeds. But for much of the history of human agriculture the problem of admixture was simply treated through 'roguing', i.e. selecting out undesirable strains and not perpetuating them.⁵¹ It was only with the 19th century turn towards attempting to create 'pure' pedigree strains that admixture became perceived as a significant obstacle to domestication.

48 Bonnie L. Hendricks, *International Encyclopedia of Horse Breeds*, University of Oklahoma Press, 2007, pp. 95-6.

49 Virginia Morell, 'Into the Wild: Reintroduced Animals Face Daunting Odds', *Science*, Vol. 320, No. 5877, 2008, pp. 742-743; & Katherine Ellison, 'Into the Wild', *Frontiers in Ecology and the Environment*, Vol. 6, No. 2, 2008, p. 112.

50 For the claim that the ancient Assyrians practised artificial pollination of date palms see: Barbara Nevling Porter, 'Sacred Trees, Date Palms, and the Royal Persona of Ashurnasirpal II', *Journal of Near Eastern Studies*, Vol. 52, No. 2, 1993, pp. 129-139.

51 Berris Charnley, 'Agricultural Science, Plant Breeding and the Emergence of a Mendelian System in Britain, 1880-1930', Thesis (PhD): The University of Leeds, 2011, pp. 85-88.

As with animals, there are many different degrees of domestication found in plants. Numerous species can be regarded as commensals which benefit from anthropogenically created environments. Grasses, for example, commonly take over areas cleared by fires started by humans. Some of these commensal grasses, e.g. wheat, became major human cultivars. But in other cereal species it appears that anthropogenic alteration of the landscape principally followed as opposed to preceded exploitation, e.g. rice, which to be fully domesticated required substantial modifications such as deliberate flooding of fields.⁵² Other grass species were originally domesticated unintentionally, e.g. rye and oats, which started as 'weeds' in wheat and barley fields but through a process known as crop mimicry adapted to resemble cultivars (thus avoiding being weeded out), thereby acquiring desirable domestic properties, e.g. large seed heads, and eventually were selected as crops in themselves.⁵³ Whilst of enormous importance as a source of human nutrition, cereals compose only a fraction of domesticated food plants, other important groups including legumes, root and leaf vegetables, herbs, berries, and fruit and nut trees. Added to these are plants domesticated for pharmacological (including psychoactive), forage, clothing and ornamental purposes. Not to be forgotten are those species of domestic organism which are neither animal nor plant – fungi and microbes, some only recently deliberately co-opted for human utility, e.g. *E. coli*, others of which have a much longer history of cultivation and exploitation, e.g. baker's and brewer's yeast.

Amongst this great variety of domesticated species is a vast diversity of histories and ecologies of domestication. Generally, it is much easier to make sense of this diversity if we treat domestication as a continuum that in many places intersects with the wild rather than being austere demarcated from nature. At one end of this continuum are truly wild organisms living in 'natural environs'. At the boundaries of domestication are those organisms which accidentally or occasionally associate with the human sphere, e.g. wild animals that sometimes venture into human settlements in search of food. Within the domestic zone but not deliberately domesticated are commensal species, some of which, e.g. the house sparrow, are now almost wholly adapted to living in built environments, whereas others have

52 For comparison of the differing difficulties of domesticating rice, wheat and barley see: Dorian Q. Fuller and Ling Qin, 'Water management and labour in the origins and dispersal of Asian rice', *World Archaeology*, Vol. 41, No. 1, 2009, pp. 88-111; & Dorian Q. Fuller, Robin G. Allaby and Chris Stevens 'Domestication as innovation: the entanglement of techniques, technology and chance in the domestication of cereal crops', *World Archaeology*, Vol. 42, No. 1, 2010, pp. 13-28.

53 Spencer C.H. Barrett, 'Crop Mimicry in Weeds', *Economic Botany*, Vol. 37, 1983, pp. 255-282.

developed distinct urban and wild ecotypes, e.g. the European red fox, or, like the previously mentioned swallows and martins, periodically migrate between domestic and wild zones.⁵⁴ Further in are casual domesticates, such as those animals which are relatively tame and may elicit food and affection from their human hosts but which have freedom to wander from the *domus* and usually do not have their breeding controlled. This is for example the status of cats, dogs and pigs in some non-Western and traditional Western rural cultures. More domesticated are those animals and plants which are treated as property, for unlike the previous class they are considered by human society as tied to a particular *domus*. This does not necessitate restriction within the boundaries of that *domus*, for there are cases like the Camargue horses and cattle where, whilst they are allowed to live in a semi-feral state, individual animals are nonetheless considered to have particular owners (this is usually tracked through such forms of marking as branding and tagging). Where ownership coincides with restriction of movement (or, in the case of plants, treatment as an extension of the physical property – as highlighted in neighbourhood disputes over overhanging trees), domestication is no doubt more complete (though restriction within the bounds of property can coincide with semi-natural lifestyles, as in the case of game animals restricted within spacious reserves). Domestication, however, has long been argued to be most complete when taming, ownership and restriction of movement coincide with human control over reproduction (this was particularly the view, as shall be shown, in Darwin's time). Again, control over reproduction does not necessitate these other aspects of domestication. For example, the reproduction of endangered species in the wild is today regularly manipulated, e.g. transplanting new stock into isolated areas to promote outbreeding, artificial animal insemination, manually fertilising wild specimens of plant individuals too isolated to naturally cross, etc.⁵⁵ These techniques, however, represent technologies first mastered on more familiar domestic organisms that have since been extended to work with free-living species. In the case of these familiar domestic organisms, reproductive control was typically preceded by prior taming and restriction of movement and/or establishment of ownership.

54 P. Wandeler, S.M. Funk, C.R. Largiadèr, S. Gloor and U. Breitenmoser, 'The city-fox phenomenon: genetic consequences of a recent colonization of urban habitat', *Molecular Ecology*, Vol. 12, No. 3, 2003, pp. 647-656.

55 See, for example: S. Shivaji, S.D. Kholkute, S.K. Verma, et al., 'Conservation of wild animals by assisted reproduction and molecular marker technology', *Indian Journal of Experimental Biology*, Vol. 41, 2003, pp. 710-723, & University Of Arizona, 'Arizona Biologists Help Restore Mauna Kea Silversword, One Of Hawaii's Most Critically Endangered Plants', *Science Daily*, 10 March 1998. <www.sciencedaily.com/releases/1998/03/980310080030.htm>.

The form of reproductive control classically envisaged as completing domestication by 19th century theorists such as Darwin was methodical selection. As we shall see, however, developments in genetics would in the 20th century led to even more stringent and meticulous means of control over breeding, e.g. Mendelian lineage breeding, and later discoveries in molecular genetics would initiate a new biotechnological age in which direct manipulation of the genome, e.g. the insertion or deletion of specific genes, became commonplace and precise. The 20th century thus saw a novel extension of the scope of domestication beyond its previous bounds. I will not be discussing all of these developments, but will dwell on the particular quandary of how the highly domesticated lab 'wild types' in classical genetics were still believed useful stand-ins for nature.

That domestication is a continuum does not imply we should abandon the domestic-wild dichotomy. Domestication, for one thing, does track real changes in the physiology, heredity and behaviour of organisms that causes them to differ from ancestors and relatives living in 'wild' (i.e. negligibly anthropogenic) ecologies. It is moreover far too culturally embedded as a means of making value demarcations between organisms to be easily abandoned. One strategy for accommodating this lack of boundedness between domestic and wild is to recognise it not as a stark and static contrast but rather an interactive dialectic. This approach is supported by the anthropologist Laura Rival, who maintains that domestication should be understood as “a historical and ecological process involving the dialectical interaction of humans and other species over long periods of time”.⁵⁶ I think this an attractive approach, especially as it allows us to recognise that domestication shapes both the domesticated and the domesticator. This still leaves open whether demarcating the domestic from the wild should be a matter of subjective judgement or if there are salient markers that can serve as more objective criteria. Nerissa Russell has advocated treating the transformation of animals into property as representing “a quantum shift in human-animal relations that we cannot ignore, a difference not only of degree but also of kind”. This distinction does not, however, directly relate to practices of control, but instead more to changes in human social relations, namely the move from treating commensal organisms as a common resource to that of regarding particular individuals or groups as owned by particular individuals.⁵⁷ This accords

⁵⁶ Laura M. Rival, 'Introduction: South America', In: *The Cambridge Encyclopedia of Hunters and Gatherers*, Richard B. Lee and Richard Daly (Eds.), Cambridge University press, 1999, p. 80.

⁵⁷ Nerissa Russell, 'The Wild Side of Animal Domestication', *Society & Animals*, Vol. 10, No. 3, 2002, pp. 291 & 294.

with Lefebvre's account whereby the organisation of social space is determined by relations of production and property, and that therefore the establishment of domestic space and its imposition on wild nature can be understood as corresponding to a move from the treatment of nature, wild and tame, as a communal resource to that of treating nature as a resource to be divided up and demarcated in space according to socially determined property claims.⁵⁸ Whilst this socio-economic account of the origins of domestication has definite merit, it faces the difficulty, as Russell admits, that the move from a communal to a property-based economic model “is unlikely to correspond with the appearance of morphological change in animals”, i.e. it does not leave obvious traces in the archaeological record, and this limits the use of this criterion for determining historical points of domestication.⁵⁹

It may further be objected that Russell's socio-economic definition of domestication is overly anthropocentric, as it does not take into account the impact of domestication on other organisms, only human social treatment of these non-human domesticates. This tendency of many accounts of domestication to be 'one-sided' has been criticised by those who wish to present domestication as more a form of symbiosis than a human domination of nature. Donna Haraway for instance condemns the tendency of many analysts to treat humans as the only actors in domestic interspecies relations, complaining that this attitude “reduce[s] other organisms to the lived status of being merely raw material or tools”. By presenting domestication as animal instrumentalisation, a false picture is drawn whereby only wild animals can be ends in themselves – even pets are assumed to be “affectional slaves” wrenched out of their original nature. Haraway particularly protests the presentation of domestication by animal advocates as an “original sin separating human beings from nature”, instead preferring the view that domestication is a form of engagement with nature, a “multispecies sociality”. Recognising that domestic non-humans often benefit from these interrelations is, she believes, not only truer to history but more useful a set of background assumptions when determining how to counter human brutality to animals.⁶⁰ Terence O' Connor has in parallel argued for a move away from models of domestication which emphasise human intentional domination to ones which instead treat it as “a form of

58 Henri Lefebvre, *The Production of Space*, Donald Nicholson-Smith (Trans.), Blackwell Publishing, 1991, p. 83.

59 Nerissa Russell, 'The Wild Side of Animal Domestication', *Society & Animals*, Vol. 10, No. 3, 2002, p. 294.

60 Donna J. Haraway, *When Species Meet*, Minneapolis and London: University of Minnesota Press, 2008, pp. 206-207.

behavioural coevolution, by which mutualistic and commensal relationships developed between people and other species, either because the interaction was to the benefit of both species, or because it was beneficial to one and neutral (or at least not strongly detrimental) to the other". He believes it was natural selection, not some inherent human will to dominate, which was responsible for the success of these relationships, and that human-animal relations parallel animal-animal relations such as those between ants and aphids.⁶¹ Even human domination in human-plant relations may be overstated. Laura Rival points to the fact that domestication usually "presupposes dependence on [organisms] whose growth is much faster relative to human growth and maturation processes", and that the term is therefore difficult to apply to long-lived plants, e.g. fruit trees.⁶² Ingold on this basis remarks that in the case of an ancient tree utilised by generations of humans it seems "more appropriate to say that it has played its part in the domestication *of* humans rather than having been domesticated *by* them". Ingold furthermore critiques the argument that domestication is rooted in the human imposition of socio-economic norms of production and property on nature as "embedded in a grand narrative of the human transcendence of nature" based on the equation of production with 'making', an idea which in its modern form he traces to Marx and Engel's theory of labour. Against this narrative, Ingold asserts that "the farmer or herdsman does not *make* crops or livestock, but rather serves to set up certain conditions of development within which plants and animals take on their particular forms and behavioural dispositions. We are dealing, in a word, with processes of *growth*". He overall concludes that what is represented "under the rubric of domestication, as a transcendence and transformation of nature may be more a reflection of an increasing reliance on plants and animals that, by comparison with humans, are relatively fast-growing and short-lived".⁶³

Additional support against the claim that domestication is a one-sided human transcendence of nature, as opposed to a symmetrical interaction, has come from environmental ethics, specifically with the turn towards critical assessment of the 'wilderness' concept in the 1990s. The notion of wildernesses as pristine natural landscapes which require the absence of human beings in order to survive has been a pivotal idea in modern conservation biology, especially

61 Terence P. O' Connor, 'Working at Relationships: Another Look at Animal Domestication', *Antiquity*, Vol. 71, No. 271, 1997, pp. 149-156.

62 Laura Rival, 'The Growth of Family Trees: Understanding Huaorani Perceptions of the Forest', *Man*, New Series, Vol. 28, No. 4, 1993, p. 648.

63 Tim Ingold, 'Making things, growing plants, raising animals and bringing up children', *The Perception of the Environment: Essays on Livelihood, Dwelling and Skill*, London and New York: Routledge, 2000, pp. 77-86.

in the United States. Critics such as William Cronon have however pointed out that many so-called American wildernesses, e.g. Yellowstone National Park, have in fact been strongly shaped by human influences for thousands of years. Indeed, in order for many US National Parks to be created, it was first necessary to expel and bar from them the Native American tribes which had traditionally lived, hunted or foraged in the area. Other 'undesirable' species, such as wolves, were exterminated by ecologically naïve park managers keen to promote the numbers of iconic herbivores such as bison and moose. These wildernesses are therefore not unaltered natural landscapes but have rather been a deliberate creation of one human cultural and political order (the America of European settlers) destroying another (Native American societies) and reshaping the local environment according to its own perceived needs (identified by Cronon as a mixture of nostalgic desire to preserve a 'vanishing frontier' and a Romantic belief in nature as the home of the sublime). The 'wilderness experience' has moreover long been carefully stage-managed so as to best ensure the impression of nature's awesome power without the dangers and disappointments of unmanaged nature (hidden fences keep animals and visitors apart, whilst feeding stations keep animals in view). Additionally, even though wolves were reintroduced to Yellowstone in the 1990s, the park's displaced Native American tribes remain unwelcome. Cronon ultimately concludes that behind their 'beguiling' mask of naturalness, wildernesses are essentially unnatural, tamed and impoverished ecosystems which represent not a pristine nature but rather “the reflection of our own unexamined longings and desires”.⁶⁴

Cronon believes the troubling character of wilderness reflects problematic assumptions about nature and culture, the wild and the domestic. He emphasises that it is a mistake to think that “wilderness can be the solution to our culture’s problematic relationships with the nonhuman world”, because wilderness takes us back to 'the wrong nature', i.e. an imagined and idealised nature that ignores the anthropogenic character of the pre-Columbian American environment. Cronon caustically comments that “The dream of an unworked natural landscape is very much the fantasy of people who have never themselves had to work the land to make a living... Only people whose relation to the land was already alienated could hold up wilderness as a model for human life in nature, for the romantic ideology of wilderness leaves

64 William Cronon, 'The Trouble with Wilderness; or, getting Back to the Wrong Nature', In: *Uncommon Ground: Rethinking the Human Place in Nature*, William Cronon (Ed.), New York: W. W. Norton & Co., 1995, pp. 69-90.

precisely nowhere for human beings actually to make their living from the land”. By positioning humankind outside of nature, the wilderness ideal denies the legitimacy, however benevolent and mutually beneficial, of the domestic interspecies symbiosis which has been necessary for the development of civilisation, and prevents humanity from finding a home in nature. This desire for wilderness nature to be 'unpeopled' has not only had tragic consequences for those disempowered peoples who traditionally used 'wild' lands but has also removed the keystone species in those ecosystems with anthropogenic origins. A wilderness without human inhabitants is therefore not only one with a false history but also a dramatically altered ecology. Cronon believes that recognising the positive role of traditional land-users in maintaining 'natural' habitats can help lead us away from the unhelpful dualism of pristine vs. degraded towards embracing “the full continuum of a natural landscape that is also cultural”. Additionally, he dismisses the claim that wildness is inherently fragile, asserting that it can “can be found anywhere”, for it is not passive and withdrawing from human contact but is rather an active aspect of nature in continual dialogue with human art and culture.⁶⁵ Robert E. Kohler, following Cronon, has similarly spoken of the need to recognise ambiguity and interpenetration between domestic and wild. Instead of an abrupt divide between domestic and wild, civilisation and wilderness, we must instead recognise that “Landscapes are technologies”, and “technological workplaces have natural histories”.⁶⁶ By recognising the heterogeneity and hybridity of many 'wild' and 'domestic' spaces, we are better able to understand both how these differing orders interact and how organisms may move between and be affected by them. Furthermore, if we accept Cronon and Kohler's assertion that wildness is found even under the most exactingly domestic conditions, this offers significant insight into how it is that domestic spaces and domesticating practices can be used for naturalistic knowledge production, a subject I will now discuss.

Domestication and Scientific Knowledge Production

This issue of whether domestication is an aspect of human domination of nature or rather of human symbiosis with nature is not just one for animal and environmental ethics. There are highly divergent implications for the use of domestication for scientific knowledge production depending on whether we interpret non-human organisms are mere passive

⁶⁵ Ibid.

⁶⁶ Robert E. Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Life*, The University of Chicago Press, 1994, pp. 9-11.

subjects or active collaborators in experimentation. Experimental organisms are often spoken of as 'tools' or 'instruments' manipulated by scientists much in the same way as inanimate apparatuses are. This language is particularly applied to selectively inbred laboratory strains and transgenic organisms.⁶⁷ Taken literally, such terminology denies agency to the laboratory organism. This suggests either that human art has reduced them to slaves robbed of their wild instincts or else that they are so restricted within the parameters of the experiments as to be allowed no freedom of action. But a contrary tradition has emerged in some of the literature on experimental organisms which emphasises the co-evolution of experimental organisms and their experimenters. One of the best known examples is Kohler's study of the use of *Drosophila* in classical genetics. Fruit flies, he maintains, were “active players in the relationship with experimental biologists”; they were “not just molded like putty... and put through their paces”, for even once domesticated and thoroughly standardised, “they had the capacity to change and frustrate drosophilists' plans and change the purposes for which they had originally been brought into the lab”. He therefore concludes that the relationship between *Drosophila* and 'drosophilists' should be seen not as one of scientific domination over nature but rather as “an interactive and evolving symbiosis within the special ecological spaces of experimental laboratories”. Kohler's argument is that the success of *Drosophila* as an experimental model cannot be made sense of if we assume that it was the intentions of experimenters which were the leading influence in knowledge production. Instead, Kohler maintains, it is often the experimental organism which leads the researcher through its combination of useful attributes and its readiness to adapt to the unnatural laboratory ecosystem. The experimental organism is therefore effectively a collaborator in successful scientific knowledge production in a manner in which a purpose-built machine is not.⁶⁸

Kohler was not the first to make such arguments. One study he drew on was Bruno Latour's 1988 *Pasteurization of France*. In this book, the traditional narrative of Louis Pasteur as a man of genius utilising the new science of microbiology to eradicate such threats to public health as anthrax and rabies is reinterpreted in terms of Pasteur being merely the head of an army of allies he recruited. These allies included both human agents such as hygienists, stock farmers and government ministers, and non-human agents, including the microbes

67 Marcel Weber, *Philosophy of Experimental Biology*, Cambridge University Press, 2005, pp. 170-172.

68 Robert E. Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Life*, The University of Chicago Press, 1994, p. 19.

themselves. It was only by offering microbes a salubrious arena for growth in the lab that they could be cultured and therefore made visible, and it was this capacity to reveal the role of microbes in social interrelations and offer strategies for removing their interference that gave the Pasteurians power through appearing indispensable.⁶⁹ Latour thus insisted that even such lowly organisms as bacteria must be treated as active agents which human researchers must recruit and mollify if they are to be co-operative experimental participants, and which moreover play an active role as allies in the success of experimental research programmes.

Questions may be raised as to whether we can apply symbiotic models in which we treat human researchers and experimental organisms as collaborators operating on a level plane of agency. Can we really, for instance apply Ingold's model of production as the promotion of processes of growth to transgenic lab organisms, e.g. if we paraphrase Ingold to say that “the work of the [scientific investigator] does not *make* [transgenic organisms]”, it merely “serves to set up certain conditions of development within which plants and animals take on their particular forms and behavioural dispositions”. Surely there is a difference here between manipulating or regulating the environmental conditions necessary for development and intervening in the germ-line so as to produce novel potentialities of development? Yes, these interventions do not make organisms into literal tools – as Marcel Weber observes “Even a *Drosophila* that has been bred for hundreds of generations, or a genetically engineered fly, is not an artifact – it is still a living creature”.⁷⁰ But on the other hand the power of the modern scientific investigator to deliberately alter both an organism's conditions of existence and its constitution surely in such cases outweighs the power of experimental organisms to make demands of experimenters. The organism, it is true, can be 'uncooperative', e.g. by failing to mate in captivity, being inclined to sickness or dying prematurely. But whilst such non-compliance may frustrate the experimenter, at the end of the day they can always select or manufacture another stock which will better meet their demands, whereas the experimental organism usually cannot choose to select a different custodian. There is therefore good reason to believe that whilst lab organisms should not be unduly denied agency, this does not imply their agency operates on a level playing field with the experimenter, and moreover that the laboratory is commonly more an arena of human domination than of mutual symbiosis. This

69 Bruno Latour, *The Pasteurization of France*, Alan Sheridan and John Law (Trans.), Harvard University Press, 1988, pp. 38-39, 71-72 & 82.

70 Marcel Weber, *Philosophy of Experimental Biology*, Cambridge University Press, 2005, p. 171.

would, nevertheless, be to ignore one of Latour's major claims, which is precisely that the power of the laboratory is that it is a site where the human investigator is stronger than nature, and that outside of this controlled environment it is usually nature which has the upper hand. Microbes in 'wild' conditions are invisible, uncooperative and dangerous; only in the lab can they be made visible, compliant and harmless.⁷¹ The laboratory, and domestication more broadly, are therefore special instances of human domination as opposed to indicative of our general relationship with nature (which in many cases domineers over us), and this domination is never total (despite efforts at closure, the lab remains an entropic system that must be continually reinforced against the decay of order, the intrusion of the wild and the reversion of the domesticated). Moreover, it is only once nature is made to co-operate, whether through carrot or stick, that it can become part of an experimental system productive of scientific knowledge.

That nature needs to be made co-operative does not imply that the nature of this co-operation (including co-operation in knowledge production) cannot have a strong element of mutualism. This is particularly true in multi-generational experiments such as were carried out in classical genetics, which required that organisms flourish enough under lab conditions to reproduce and remain viable over the generations so that the potentially debilitating effects of inbreeding and interventions such as irradiation would not lead to die-offs distorting the Mendelian ratios required to track genetic ratios. This demand for flourishing, for example, led the Morgan group to abandon its original method of mass culturing fruit flies, which decreased survival rates (especially for sought-after deleterious mutants) due to competition for food and overcrowding, and to instead breed pairs of flies in separate jars.⁷² So whilst these *Drosophila*, like Pasteur's microbes, were essentially prisoners of the lab, they nevertheless exercised a power over their human masters in that they failed to prove especially useful for detailed knowledge production if their demands for space and food were not met. There are certainly therefore benefits for a species that can adapt to domestication in the lab, as experimentalists will not only shield them from predation and the vicissitudes of nature but also provide them with food and space to flourish and perpetuate their kind. The penalties of loss of freedom of movement, growing dependency on human intervention and

71 Bruno Latour, *The Pasteurization of France*, Alan Sheridan and John Law (Trans.), Harvard University Press, 1988, pp. 73-74.

72 Robert E. Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Life*, The University of Chicago Press, 1994, p. 77.

vulnerability to a researcher's experimental whims are real but only likely to be fatal to a whole lab lineage if the research programme is a failure, is therefore junked and the stocks euthanised, or if the defensive boundaries of the experiment are compromised (drosophilists, Kohler records, lost numerous lab stocks through heating failures in winter and predation by mites and mice).⁷³ Otherwise, the lab is an excellent ecological niche that allows an adaptive species free rein to feed and reproduce without competition from other species or fluctuations in conditions and resources.

Kohler's major claim, however, is not just that laboratory organisms can enjoy privileges if they are able to adapt to the initially austere laboratory environment, but rather that they can also direct research through their utility (or lack of it) for particular experiments, their having unusual biological features that lend themselves to particular modes of enquiry and through their greater or lesser tolerance to being introduced into new experimental set-ups. That avenues for scientific research are constrained or enabled by the particular features of a laboratory species is widely acknowledged. But there is a dispute between those who claim the utility of an organism in a particular experimental system is determined principally by shrewd selection by practitioners and those who would instead argue that the epistemic productivity of particular model systems is in the main an outcome of interactions between experimenter and experimental subject. Is it, Weber poses, a matter of selecting the right tools for the right job or of co-construction of the experimental system by organism and practitioner?⁷⁴ Certainly, the selection of specific species for lab work is commonly directed by particular requirements of experiment. In some cases, model organisms have been deliberately selected as major foci of research based on specific attributes lending themselves to a pre-planned research programme. The nematode *C. elegans*, for instance, was deliberately selected by Sydney Brenner as a model for the study of neuronal development due to its possessing such salient features as a small fixed number of adult neurons (302, compared to c. 100,000 in *Drosophila*) and the capacity to reproduce both through hermaphroditic self-fertilisation and sexually (allowing both the establishment of clonal lineages and their modification through crossing).⁷⁵ But choice for a particular purpose does not guarantee a species any longevity as an experimental organism, for as Richard Burian

73 Ibid., pp . 82-83.

74 Marcel Weber, *Philosophy of Experimental Biology*, Cambridge University Press, 2005, pp. 176-179.

75 Sydney Brenner, 'The Genetics of *Caenorhabditis elegans*', *Genetics*, Vol. 77, No. 1, 1974, p. 72.

comments “even when some organism is "the" right one for a theoretical job, its rightness is temporary and more or less local or regional”. Moreover, when the choice of organism is not correct, it has a clear impact on the direction of research. Burian points to examples such as Hugo de Vries' selection of *Oenothera*, which provided plentiful data for his theory of mutationism but ultimately turned out to be misleading, and Theodor Boveri's choice of *Ascaris*, a nematode which appeared to show chromosomal disintegration in somatic cells, lending support to August Weismann's erroneous mosaic theory of development. Therefore it is not simply a matter of a researcher making an astute original choice, for an organism will often have hidden features that may aid or hinder research that are only revealed once it has been experimentalised.⁷⁶ *Drosophila* is a classic case of an experimental organism with useful hidden traits that were not discovered until long after its domestication in the lab. These include, for example, the banding of its giant larval salivary chromosomes, utilised in cytological mapping of genes, which was only discovered by Theophilus Painter as late as 1933.⁷⁷ This capacity of experimental organisms to surprise and direct researchers down fruitful (and less fruitful) avenues supports a significant role for co-construction, as opposed to top-down design, as determining the epistemic productivity of organisms in experimental systems. If knowledge production using experimental organisms is regarded as co-construction, I believe this offers corroboration for the view that scientific domestication can be symbiotic as well as dominating. If we view symbiosis as the dynamic and productive interaction of two natures, human and non-human, as opposed to the domineering imposition on passive nature of human art, this may offer conceptual support for the naturalistic nature of knowledge produced through domestication.

That the value of an organism for a particular job is not inherently obvious or indicative of its long term experimental value should not preclude us from observing that there are nonetheless a number of more general demands of laboratory life that have affected the kinds of organisms laboratory biologists have tended to select, leading to inherent biases in life science experimental systems. Weber lists five such general characteristics affecting model organism suitability: inclination to breed in captivity; generation time; visibility and viability of mutations if used for genetic analysis; “peculiar features” the biologist may exploit (e.g.

76 Richard M. Burian, 'How the Choice of Experimental Organism Matters: Epistemological Reflections on an Aspect of Biological Practice', *Journal of the History of Biology*, Vol. 26, No. 2, 1993, pp. 352-353.

77 William Wimsatt, 'False Models as Means to Truer Theories', *Re-engineering Philosophy for Limited Beings: Piecewise Approximations to Reality*, Harvard University Press, 2007, p. 113.

the large size of squid nerve cells, which make neurophysiological measurement much easier); and the pre-existence of established experimental resources and knowledge which allow researchers to overcome the disadvantages of a particular model.⁷⁸ Bolker adds to this list the features of high developmental rate and uniformity (canalisation) as important for developmental biologists.⁷⁹ Ankeny and Leonelli also note that most model organisms in genetics share features such as “small physical and genomic sizes... high fertility rates, and often high mutation rates or high susceptibility to simple techniques for genetic modification”.⁸⁰ That these general demands of laboratory domestication can be problematic is highlighted particularly by Bolker. She points, for instance, to the fact that the very properties that make *C. elegans* an excellent model system, e.g. early determination of cell lineages, are precisely what make it atypical as a nematode. Selection for developmental canalisation has moreover been responsible for “a disproportionately deterministic view of development”, and choosing organisms with short generation times and rapid life cycles introduces other biases into model systems, such as a predisposition towards paedomorphism, progenesis, loss of larval stages, miniaturisation, morphological and genetic simplifications or losses, and novel evolutionary adaptations to this faster and usually smaller-scale mode of existence.⁸¹ There are therefore systematic issues with the kinds of organisms that large-scale laboratory experimentation usually selects for, as the resulting model species are less than representative of the diversity found in nature.

I wish to introduce a further constraint that the demands of the laboratory place on the selection of experimental organisms, namely the requirement that they can be domesticated. Some attributes relevant to laboratory domestication have already been discussed, namely, ability to breed prolifically in captivity, speedy development and early sexual maturation (especially in genetics, which favours a rapid turnover of generations), and small size (important given most laboratories are not roomy spaces). To these may be added: a lack of dietary specialism (meaning biologists can maintain stocks using cheap, readily available food stuffs; furthermore, difficulties determining diet have been a major obstacle to domesticating most microbes); a sociable or socially adaptive behavioural phenotype (this

78 Marcel Weber, *Philosophy of Experimental Biology*, Cambridge University Press, 2005, p. 177.

79 Jessica Bolker, 'Model systems in developmental biology', *BioEssays*, Vol. 17, No. 5, 1995, pp. 451-452.

80 Rachel A. Ankeny and Sabina Leonelli, 'What's So Special About Model Organisms', *Studies in History and Philosophy of Science*, Vol. 42, 2011, p. 314; & Robert E. Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Life*, The University of Chicago Press, 1994, p. 316.

81 Jessica Bolker, 'Model systems in developmental biology', *BioEssays*, Vol. 17, No. 5, 1995, pp. 451-452.

allows for mass culturing without violent competition, and therefore more stock per unit of lab space); robustness in health and disease resistance even when inbred and mass cultured; the ability to tolerate the otherwise egregious interventions imposed by experimentation, e.g. e.g. amputations, transplantations, heating, freezing, irradiation; ease of control of reproduction (both to prevent unsupervised crossing within the lab and admixture from without, e.g. by wind-borne pollen); preferably not to be dangerous to researchers (compare lab strains of *E. coli* used for pure genetics with pathogenic ones); and a lack of fear or awareness of their human retainers (this allows for 'natural' behaviour). Many of these pre-adaptations to lab life are the same or similar to the pre-adaptations necessary for general commensal life, and this is one reason why the vast majority of organisms chosen for experimental purposes have had some prior history of domestication or commensalism (*Drosophila melanogaster*, as Kohler establishes, is a highly commensal species).⁸² This leads us to ask to what extent these laboratory pre-adapted species can act as representatives for nature in general given their shared unusual characteristics. Furthermore, the restrictions and modifications imposed on lab organisms are often extreme even compared to those constraints placed on traditional domesticates. Factory farming can be considered a similarly intense form of domestication but its purpose, the maximal production of meat, eggs and dairy at minimal cost, is usually starkly distinct from the primary aims of lab work, namely the production of naturalistic knowledge. How much, it may be asked, can commensal species tell us about wild species adapted to minimally anthropogenic ecosystems, and further still how much can hyper-domesticated lab strains particularly selected for their adaptivity to the lab environment tell us about their free-living commensal relatives, never mind relatives and other species in 'wild' nature?

I am not here suggesting that lab strains, including those called 'wild type', cannot act as stand-ins for nature. The fact is that they have proven highly epistemically productive precisely through taking on this role, and much of the earlier knowledge generated in the lab has ultimately been vindicated by later studies in the field. But domestication is nonetheless selective in the kind of nature it can allow to enter its space, and once within it, that nature is to a greater or lesser degree 'de-natured' and 'de-wilded'. This transformation is far from total – it is the subtlety of many changes that tends to mislead observers into thinking such

⁸² Robert E. Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Life*, The University of Chicago Press, 1994, p. 19-22.

alterations 'natural'. Researchers must therefore determine which features of the organism are altered by domestication, which 'wild' elements remain stable throughout and whether their experimental interventions have natural analogies in order to establish which aspects of lab produced knowledge may be extrapolated to nature at large and which must be treated as likely artefacts of the laboratory ecosystem (this task has parallels with that of determining which characteristics of a model organism are broadly homologous and which are idiosyncratic, see Kellogg and Shaffer for details).⁸³

The overall goal of domesticating the wild in the laboratory is to integrate it into an epistemically productive experimental system but to still retain enough 'wildness' to ensure the knowledge produced is naturalistic. By 'wildness' I do not mean some esoteric essential property but rather refer to those very real behavioural, physiological and constitutional differences that exist between 'wild types' in nature and their domesticated relations. These differences have been recognised for a long time and have generally been assumed to have something to do with human art and custodianship. A major question for natural historians has therefore been how to subject wild nature to art without compromising its integrity. Differing strategies of scientifically addressing nature have developed based on varying assumptions regarding how to study the wild, some emphasising passive observation, others active intervention, others steering a middle course. In the following chapters, I will examine some of the history of Western ideas about the domestic-wild divide and show how different interpretations of this distinction were linked to differing epistemologies of nature. I will furthermore show how these differing epistemologies privileged particular domestic spaces as sites of scientific knowledge production, but also how the development of new sites and techniques of domestication influenced changes in epistemology and the understanding of wild nature. Following historical trajectories, I will show how the idea of wild type developed first as contrastive to domestic types and then how it later became perceived as transferable into the domestic sphere, an idea that grew in strength as heredity became increasingly conceived as dominated by internal processes shielded from outside environmental influences. This apparently contradictory transposition of wild into the domestic realm of the lab was not, however, without its critics. I will conclude by discussing the representative role 'wild types' played once domesticated within the classical genetics laboratory, specifically to

83 Elizabeth A. Kellogg and H. Bradley Shaffer, 'Model Organisms in Evolutionary Studies', *Systematic Biology*, Vol. 42, No. 4, 1993, pp. 409-414.

what extent they could be considered to 'stand in' for nature and whether the strategy of laboratory domestication has led to a distorted interpretation of wild nature.

Chapter 2 – The Domestic-Wild Divide in Pre-Modern & Early Modern Western

Thought

Aristotle on the Domestic-Wild Divide

I now move to looking at historical interpretations of the domestic-wild distinction in Western thought in detail. I will begin with our earliest major source, Aristotle, not with the purpose of complete comprehensiveness but rather as a useful means of establishing a baseline theory of the domestic and wild against which the differential development of later conceptions can be tracked. Aristotle's main contributions to Western discourse on the domestic and wild are found in his later 4th century BC work *History of Animals*. Aristotle did not treat the dichotomy of domestic-wild as absolute, noting that there was a great deal of variation in how wild or tameable animals were – “some creatures are tame and some are wild: some are at all times tame, as man and the mule; others are at all times savage, as the leopard and the wolf; and some creatures can be rapidly tamed, as the elephant”. He further observed that domestic-wild was not useful for the taxonomic purpose of categorising species of animal as “whenever a race of animals is found domesticated, the same is always to be found in a wild condition; as we find to be the case with horses, kine, swine, (men), sheep, goats, and dogs”.⁸⁴ Aristotle therefore recognised that domesticated species necessarily had wild types from which they were descended and did not originally come into being as domestic (with the exception of mules).⁸⁵ This has importance for the origins of the wild type concept in that Aristotle rejected the common tendency of folk taxonomies to use the domestic-wild dichotomy as a means to distinguish species.⁸⁶ Aristotle's contribution was thus to recognise that domestication was a process whereby originally wild animals and plants entered into the *domus*, either by force or of their own accord, and became tame. This can be contrasted with the later views of many medieval and early modern Christian naturalists who held that all creatures had originally been created tame and subservient to Adam, and that the fierce nature of wild animals, the existence of pests and parasites and the fecundity of thorn and thistle were all punitive effects of the Fall.⁸⁷

84 Aristotle, ‘History of Animals’, D’Arcy Wentworth Thomas (Trans.), In: *The Works of Aristotle*, William David Ross and J.A. Smith (Eds.), Oxford at the Clarendon Press, 1908-1931, p. 1395.

85 This claim is supported by Isager and Skydsgaard (“Domestic animals were originally wild species tamed by man. Aristotle is fully aware of this”). See: Signe Isager and Jens Erik Skydsgaard, *Ancient Greek Agriculture: An Introduction*, London and New York: Routledge, 1995, p. 85.

86 Keith Thomas, *Man and the Natural World: Changing Attitudes in England 1500-1800*, Allen Lane, 1983, p. 56.

87 *Ibid.*, pp. 17-18.

Aristotle used one main term, 'agrios', to refer to 'wild' animals (he also applied it to the 'savage' cannibal human tribes held to exist in the far corners of the world).⁸⁸ By contrast, he used two quite distinct Greek terms to refer to domestic animals. The more inclusive 'synanthropeuómena' is translated by Dalby as 'animals symbiotic with man'.⁸⁹ Isager and Skydsgaard caution that it "should probably not be taken to indicate domesticated animals in general... but as a designation for animals which live together with man", noting that it includes not only traditional domesticates such as pigs and dogs but also birds that breed in human dwellings and insects that winter in them. They believe Aristotle's 'hemeros' (translated as 'tame') better fits our modern notion of 'domesticated'; this term was also applied by Aristotle's student and successor Theophrastus to cultivated plants.⁹⁰ But it should be noted that Aristotle attributed most of the important behavioural and physiological differences between wild and domestic stock not to the effects of taming but rather to the conditions of synanthropeuómena, i.e. of what would now be termed commensalism. One particular effect of commensalism that Aristotle noted was on reproductive behaviour. He observed that "Many animals time the season of intercourse with a view to the right nurture subsequently of their young". This had the effect that in the wild most land animals tended to breed in spring, when the scarcity of winter was giving way to the greater abundance of food found in summer. He contrasted the behaviour of wild animals with that of human beings and domestic animals, which paired and bred "at all seasons... owing to the shelter and good feeding they enjoy". This change in reproductive behaviour was found not only in traditional domesticates such as dogs and pigs but also in "those birds that breed frequently" (e.g. the pigeons, fowls and songbirds that sheltered in human houses and fed on domestic grain and leftovers).⁹¹

88 Helen M. Leach, 'Selection and the Unforeseen Consequences of Domestication', In: *Where the Wild Things Are Now: Domestication Reconsidered*, Rebecca Cassidy and Molly Mullin (Eds.), Oxford/New York: Berg, 2007, p. 94.

89 Andrew Dalby, *Siren Feasts: A History of Food and Gastronomy in Greece*, Routledge, 1996, p. 60.

90 Signe Isager and Jens Erik Skydsgaard, *Ancient Greek Agriculture: An Introduction*, London and New York: Routledge, 1995, p. 85; & Helen M. Leach, 'Selection and the Unforeseen Consequences of Domestication', In: *Where the Wild Things Are Now: Domestication Reconsidered*, Rebecca Cassidy and Molly Mullin (Eds.), Oxford/New York: Berg, 2007, p. 94.

91 Aristotle, 'History of Animals', D'Arcy Wentworth Thomas (Trans.), In: *The Works of Aristotle*, William David Ross and J.A. Smith (Eds.), Oxford at the Clarendon Press, 1908-1931, p. 1547.

Aristotle was well aware of the effects that taming ('hemeros') could also have on shaping the domestic organism. He makes many comments, for instance, on how the control of breeding can be used to ensure the production of healthy desirable offspring. He discourages, for instance, the use of fat individuals as breeding stock, believing that they "whether male or female, are more or less unfitted for breeding purposes" (this was due to his belief that seminal fluids were converted into fat in overweight individuals, resulting in a loss of vigour).⁹² Among his other advice on the care and characteristics of domestic animals, Aristotle also discusses what ages they are best bred at (e.g. "the Laconian hound of either sex is fit for breeding purposes when eight months old", how to strategically promote fruitful copulation ("The boar is most capable after a good feed, and with the first sow it mounts"), how to accelerate maturity ("The breeding faculties of the younger horses may be stimulated beyond their years if they be supplied with good feeding in abundance"), what seasons produce the best offspring ("with regard to the seasons of the year, the litter [of piglets] is the best that comes at the beginning of winter; and the summer litter the poorest"), and even how to influence the sex of offspring ("if [ewes] submit to the male when north winds are blowing, they are apt to bear males; if when south winds are blowing, females").⁹³

The main empirical source for Aristotle's advice on breeding was likely the testimony of local farmers and pastoralists. It is not clear to what extent Aristotle himself observed the reproductive behaviour of domestic animals but he was evidently less than critical regarding some of the more folkloric breeding customs he recorded. But what is evident in all these examples is the idea that whatever efforts were made by the breeder to ensure the production of the best possible stock, there was no believed means to improve stock beyond the existing standard. Human taming could only maximise potential, never improve on nature, and in order to effect optimal stock realisation breeders were often dependent on harnessing largely uncontrollable natural forces such as winds and seasons. Much of this attitude could be traced to the limitations to breeding knowledge and technique in this period. But Aristotle also incorporated this idea of species, whether domestic or wild, as generally unchanging in optimal form and range of variation into his theories of generation and of technology. Regarding the former, he believed the developmental process to consist in the

92 Ibid., pp. 1490 & 1557; & Aristotle, 'On the Generation of Animals', Arthur Platt (Trans.), In: *The Works of Aristotle*, William David Ross and J.A. Smith (Eds.), Oxford at the Clarendon Press, 1908-1931, p. 2038.

93 Aristotle, 'History of Animals', D'Arcy Wentworth Thomas (Trans.), In: *The Works of Aristotle*, William David Ross and J.A. Smith (Eds.), Oxford at the Clarendon Press, 1908-1931, pp. 1557-8 & 1629-1635.

male seed's actively forming the embryo out of passive matter provided by the female. The male seed thus acts as the efficient cause and the female womb as the material. The seed is able to act in an orderly manner because it contains something of the male's essence. This essence provides the formal cause, directing the male seed to attempt to replicate the father out of the material of the mother.⁹⁴ Variation was therefore conceived by Aristotle as a consequence of the failure of the efficient cause (the male seed) to meet the demands of the formal cause (the essence), this often being attributed to the inadequacy of the material cause (the female womb) or due to environmental perturbations. The best a breeder could therefore do would be to choose good parents, provide good food and shelter, and protect against undesirable environmental perturbations during copulation and pregnancy, with the hope that offspring would therefore be of the father's quality. Improvement beyond optimal realisation of the father's essence was not expected.

Aristotle's ideas on generation were heavily linked to his ideas on technology. His view of the relationship between art ('techné') and nature was that art best attained its results by helping nature achieve its own ends, as opposed to trying to go against nature. Aristotle held all things in the universe to operate under a final cause, or 'telos', that they tend towards as an end, and when no other force is acting upon them apart from their own nature, things will move towards this end. He for example explained the upward movement of fire and the downward movement of stones in terms of their having separate natures with contrary states of rest, the final end of stones being the centre of the earth, that of fire being the heavens.⁹⁵ For Aristotle, art operated through either mimicking nature, i.e. by following its course, or through perfecting it, i.e. removing obstacles in its course. An example of mimetic art is theatre, which imitates aspects of real life but exaggerates them either for tragic or comic effect.⁹⁶ An example of perfective art is medicine, which seeks to remove obstacles to health when nature cannot do so on its own (in Galen's later words, art acts as the servant of nature).⁹⁷ These two strategies of art were not viewed as exclusive, as seen in Aristotle's

94 Staffan Müller-Wille and Hans Jörg Rheinberger, *A Cultural History of Heredity*, Chicago and London: The University of Chicago Press, 2012, pp. 24-5 & Aristotle, 'On the Generation of Animals', In: *The Works of Aristotle*, William David Ross and J.A. Smith (Eds.), Oxford at the Clarendon Press, 1908-1931, p. 2009.

95 Aristotle, 'Physics', R. P. Hardie and R. K. Gaye (Trans.), In: *The Works of Aristotle*, William David Ross and J.A. Smith (Eds.), Oxford at the Clarendon Press, 1908-1931, pp. 677 & 742.

96 Aristotle, 'Poetics', S.H. Butcher (Trans.), In: *The Works of Aristotle*, William David Ross and J.A. Smith (Eds.), Oxford at the Clarendon Press, 1908-1931, pp. 3308-3322.

97 William R. Newman, *Promethean Ambitions: Alchemy and the Quest to Perfect Nature*, Chicago and London: The University of Chicago Press, 2004, p. 17.

stating that “generally art partly completes what nature cannot bring to a finish, and partly imitates her”.⁹⁸ Overall, Aristotle was not of the view that all artful or experimental intervention went against nature, a frequent misinterpretation that William R. Newman has called the ‘noninterventionist fallacy’.⁹⁹ That intervention could be natural in Aristotle’s worldview can be seen from his statement that “when fire or earth is moved by something the motion is violent when it is unnatural, *and natural when it brings to actuality the proper activities that they potentially possess*” [emphasis mine].¹⁰⁰ Nevertheless, it must be recognised that Aristotle did place significant limitations on artifice by holding the view that it must either follow or clear nature’s course to succeed. Artifice which attempted to go against nature was inherently fragile in that once human intervention was removed, nature would once more take its course and the artefact would rapidly decay back into its natural state. This model of the natural behaviour of things has been dubbed by Elliott Sober “the Natural State Model”, based as it is on the notion that all things have a natural state or trajectory from which they only deviate if under the influence of other bodies and to which they revert when this external influence is removed.¹⁰¹

The relevant question with regards to domestication is whether Aristotle believed it to be a perfective process or rather one which was contrary to nature. As identified before, he does not suggest domestication to be an original non-derived state for any non-hybrid species. Before humans tamed them, all animals were wild. Nonetheless, as previously noted, Aristotle does observe some animals to be much more easily tamed than others, suggesting he made a distinction between the eminently tameable and the incorrigibly wild. This suggestion is further supported by statements in the *Politics*. Here Aristotle appears to claim that the extent to which animals are tameable or remain wild relates to something in their original nature and to their natural purpose. In particular, he affirms that “other animals exist for the sake of man, the tame for use and food, the wild, if not all at least the greater part of them, for

98 Aristotle, ‘Physics’, R. P. Hardie and R. K. Gaye (Trans.), In: *The Works of Aristotle*, William David Ross and J.A. Smith (Eds.), Oxford at the Clarendon Press, 1908-1931, p. 648; & William R. Newman, *Promethean Ambitions: Alchemy and the Quest to Perfect Nature*, Chicago and London: The University of Chicago Press, 2004, p. 17.

99 William R. Newman, *Promethean Ambitions: Alchemy and the Quest to Perfect Nature*, Chicago and London: The University of Chicago Press, 2004, p. 238.

100 Aristotle, ‘Physics’, R. P. Hardie and R. K. Gaye (Trans.), In: *The Works of Aristotle*, William David Ross and J.A. Smith (Eds.), Oxford at the Clarendon Press, 1908-1931, p. 813.

101 Elliot Sober, ‘Evolution, population thinking, and essentialism’, *Philosophy of Science*, Vol. 47, No. 3, 1980, p. 360.

food, and for the provision of clothing and various instruments”. Where evidence of human utility is not obvious, he maintains that “if nature makes nothing incomplete, and nothing in vain, the inference must be that she has made all animals for the sake of man”. The natural purpose of animal life is thus assumed to be to serve human needs. All non-human life must in some way serve human interests, for if it were otherwise, it would be without purpose, and in Aristotle’s view nothing in nature is without purpose, *ergo* there are no non-human species which do not exist for humanity’s sake. As to domestication, Aristotle states that “tame animals have a better nature than wild”, and are also “are better off when they are ruled by man; for then they are preserved”.¹⁰² The implication is that whilst all animals were originally wild, they have come into being only in order that they might one day provide utility to humankind, and therefore domestication is a perfective art that completes nature’s ends and fulfils the purpose for which tameable animals have come into existence for.

The radical anthropocentrism displayed here is difficult to understand without being placed in the context of what Aristotle is principally arguing for in the *Politics*, namely the existence of natural slavery. Specifically, Aristotle draws an analogy between the human use of animals and the use of slaves by their masters, observing that “the use made of slaves and of tame animals is not very different; for both with their bodies minister to the needs of life”. Indeed, he goes as far as to refer to the ox as “the poor man’s slave”. Moreover, just as he argues that there are tameable animals made better by domestication, he asserts there to be humans (namely non-Greek barbarians) who are naturally servile and therefore best flourish as slaves (the servile character in moreover hereditary, thus justifying lineage slavery).¹⁰³ Along with aligning the naturalness of animal and slave domesticity, Aristotle also justifies the practice of capturing slaves by analogy with the exploitation of wild animals. “The art of acquiring slaves” is stated to be “a species of hunting or war”, and the art of war is argued to be “a natural art of acquisition”, as just as wild animals ought to be hunted for human benefit, so should “men... intended by nature to be governed” be forced to submit, Aristotle going as far as to state that “war of such a kind is naturally just”.¹⁰⁴ Aristotle’s justification of natural slavery thus rests on an analogy with the claimed natural right of humankind to domesticate

102 Aristotle, ‘Politics’, Benjamin Jowett (Trans.), In: *The Works of Aristotle*, William David Ross and J.A. Smith (Eds.), Oxford at the Clarendon Press, 1908-1931, pp. 2796-2802.

103 Pierre Pellegrin, ‘Natural Slavery’, E. Zoli Filotas (Trans.), In: *The Cambridge Companion to Aristotle’s Politics*, Marguerite Deslauriers and Pierre Destrée (Eds.), Cambridge University Press, 2013, pp. 100-7.

104 Aristotle, ‘Politics’, Benjamin Jowett (Trans.), In: *The Works of Aristotle*, William David Ross and J.A. Smith (Eds.), Oxford at the Clarendon Press, 1908-1931, pp. 2790-2802.

and exploit animals. He sought to equate those who would question the naturalness of slavery with those who would question the naturalness of human dominion over nature.

That Aristotle sought to bolster the institution of slavery by arguing it to be analogous to 'natural' human-animal relations reflects the fact that whilst slavery was ubiquitous in his time it was not without its critics. The sophist Alcidas had argued against the Spartan oppression of their serfs that "Nature has made no man a slave".¹⁰⁵ In his play *Hecuba*, the tragedian Euripides had described slavery as "an evil" based on force and violence which "endures what is not right".¹⁰⁶ By comparison, the idea of a natural human right of dominion over other species appears to have been taken as given by most Mediterranean ancients. There were dissenting voices. The Pythagoreans, believing in metempsychosis, considered all living things kindred and refused to eat fellow ensouled beings.¹⁰⁷ Porphyry, a 3rd century AD Neoplatonist, would later advocate Pythagorean-style vegetarianism, ridiculing anthropocentric claims of human dominion by asserting that if pigs were created to be eaten by men, then men must equally be made for the repast of crocodiles. Porphyry's arguments would later be drawn on by early modern critics of the notion of natural human dominion such as Thomas Hobbes and Bernard Mandeville.¹⁰⁸ Atomism was another ancient philosophical movement which denied human dominion, although in their case it was based on a rejection of the idea of a world created for human benefit. The 1st century BC Roman writer Lucretius would thus assert that to say "*the gods* designed to arrange *all* this noble fabric of the world for the sake of men... is to be guilty of *the utmost* folly", not least as nature was full of "a terrible brood of wild beasts *and monsters*, hostile to the human race".¹⁰⁹ Lucretius thus rejected the Aristotelian claim that animals were 'intended by nature to be governed', and therefore also the argument for domestication as a perfective art. Porphyry and Lucretius' views on the domestic and wild were, however, minority opinions in the ancient world, and whilst their arguments would be resurrected in the late medieval period

105 G.B. Kerford, *The Sophistic Movement*, Cambridge University Press, 1981, p. 156.

106 Stephen G. Daitz, 'Concepts of Freedom and Slavery in Euripides' *Hecuba*', *Hermes*, 99. Bd., H. 2, 1971, p. 225.

107 W.K.C. Guthrie, *A History of Greek Philosophy, Volume I: The Earlier Presocratics and the Pythagoreans*, Cambridge University Press, 1962, p. 186-187.

108 Keith Thomas, *Man and the Natural World: Changing Attitudes in England 1500-1800*, Allen Lane, 1983, pp. 166 & 171; & Bernard de Mandeville, *The Fable of the Bees: or Private Virtues, Publick Benefits*, 3rd Ed., London: J. Tonson, 1724, pp. 190-197.

109 Lucretius, *On the Nature of Things*, John Selby Watson (Trans.), London: Henry G. Bohn, York Street, Covent Garden, 1851, pp. 199-202.

they would until then be largely neglected by a generally anthropocentric Christian orthodoxy. It is probably a fair assessment therefore that a belief in an inherent dominion of humankind over nature was the majority view among philosophers in the ancient and early medieval world.

Scholastic Aristotelianism, Human Dominion & the Theological Debate over Final Causes

Aristotle's *oeuvre* came back into wide circulation in Western Europe in the 12th and 13th century. His works encountered initial difficulties due to the perceived paganism of the views found in them but eventually came to dominate university curricula and late medieval scholastic philosophy, not least thanks to Thomas Aquinas' work reconciling Aristotle with Christian doctrine. However, despite Aristotelianism's domination of pedagogy, there remained a divide in the Church between the supporters of Thomism (e.g. the Dominicans and later the Jesuits) and opponents such as the Franciscans.¹¹⁰ In the 14th century, nominalism emerged as the major theological opposition to Aristotelianism. This philosophy had its roots in translations of the Islamic Mutakallimun scholars such as the 11th century writer Al-Ghazali. These Muslim thinkers had initiated an earlier critique of Aristotle, rejecting his doctrine of independently existing essences and necessary causal relations on the grounds that it imposed unwarranted restrictions on the omnipotence of God. In its place they adopted an occasionalist atomist metaphysics which held God to be responsible for the creation and continued existence of each atom. The Mutakallimun thus insisted that all natural relations were contingent on the will of God and that therefore no final ends of nature could be inferred from observation, as to know nature's final ends would require knowledge of the mind of God.¹¹¹ The arguments of these Arab and Persian philosophers were taken up and further developed by Christian scholastics such as John Duns Scotus and William of Ockham, both of whom held that the uniformities of nature were dependent on the will of God and could be circumvented should He choose. They also thus denied the independent reality of essences and the necessity of natural phenomena. Duns Scotus in particular held that only 'aptitudinal unions' of phenomena not necessary relations of truth – i.e. that x *can*

110 Ralph McInerney and John O' Callaghan, 'Saint Thomas Aquinas', *The Stanford Encyclopedia of Philosophy* (Summer 2014 Edition), Edward N. Zalta (ed.), URL=
<<http://plato.stanford.edu/archives/sum2014/entries/aquinas/>>.

111 Binyamin Abrahamov, 'Al-Ghazālī's Theory of Causality', *Studia Islamica*, No. 67, 1988, pp. 75-98, & William J. Courtenay, 'The Critique on Natural Causality in the Mutakallimun and Nominalism', *The Harvard Theological Review*, Vol. 66, No. 1, 1973, pp. 77-94.

be y, but not x *must* be y – could be inferred from observation.¹¹² The nominalists, like the Mutakallimun, thus insisted on the contingency of nature and therefore denied that the final ends of natural entities could be inductively determined. Challenges to Aristotle's teleological method and cosmology therefore considerably predated the 17th century repudiation of scholastic Aristotelianism by experimental and mechanist natural philosophers.

Given these doubts about the capability of human beings to infer the ends of nature through observation alone, Christian natural philosophers often preferred to turn to scripture for clues as to God's intentions for the world. The main Biblical book relevant to discussion of domestication was Genesis, which gave an account of the Creation in which Eden was a garden paradise purposefully made for Adam and Eve, in which all species of animal and plant were originally found. These creatures had been created by God not for themselves but rather for the use and service of human beings. In origin, all animals had been tame and co-existed with humans harmoniously. It was only after the Fall that they became wild and unwilling to serve, although God had allowed those animals most useful to humans – e.g., the ox, ass and horse – to remain tameable. Their tendency toward obedience was consequently treated as a natural, not acquired, instinct.¹¹³ Domestication was therefore according to the scriptural Christian worldview a restorative art whereby organisms were returned towards their original ends, as opposed to the Aristotelian view that animals and plants were originally wild but could through domestication be perfected if this accorded with their nature. The scriptural view of human dominion also differed from Aristotle's, being if anything even more anthropocentric. Aristotle believed nature to serve humankind on the grounds that nature does nothing in vain. But whilst humanity was nature's crowning glory, humankind also owed its existence to nature. By contrast, the Christian view held that nature had been co-created by God alongside human beings and deliberately fashioned with their ends in mind. Adam was moreover held to have been granted dominion and stewardship over all creatures by divine decree, whereas for Aristotle such human dominion was instead implicit in the order of nature. In summary, there was an inherent conflict between Aristotle and Christian scripture regarding the nature of domestication, the former viewing it as a perfecting art, the latter as a restorative one. As in most cases when scriptural doctrine

112 John Losee, *A Historical Introduction to the Philosophy of Science*, 3rd Ed., Oxford/New York: Oxford University Press, 1993, pp. 33-35 & 39-41.

113 Keith Thomas, *Man and the Natural World: Changing Attitudes in England 1500-1800*, Allen Lane, 1983, pp. 17-19.

conflicted with pagan philosophy, the Christian view of domestication tended to win out amongst most medieval Aristotelians.

The 17th Century Challenge to Aristotelianism

From what has been said, it can be seen that the ‘Aristotelianism’ challenged by 17th century mechanist and experimental philosophers was a hybrid set of worldviews synthesising Greek texts arguing that knowledge of final causes could be attained through observation of nature with Abrahamic scriptures that tended to promote the view that nature’s ends could only truly be known through scriptural revelation. In the resolution of this conflict, the scriptural view of non-human organisms as originally domestic and domestication as restorative tended to win out over Aristotle’s own view of wildness as original but domestication as perfective. The Aristotelian view of nature was also one already challenged from within scholasticism by nominalist doubts concerning the possibility of inferring final causes. The principal novelty of many of the 17th century attacks on Aristotelianism was their use of arguments from sciences such as mechanics and astronomy to critique teleological speculations. The empirical and theoretical grounds for these arguments were not themselves wholly novel. As observed by McLaughlin, Aristotle’s demarcation between forced and natural motions had been overcome in practical mechanics long before theoreticians provided justification for the elimination of the distinction.¹¹⁴ There also existed the long-standing issue for ‘Natural State’ style Aristotelian physics of the problem of projectiles, namely what it was that resisted the natural inclination of a stone to move towards the centre of the Earth when thrown. To resolve this problem, the 14th century scholastics Jean Buridan and Nicole Oresme developed a theory of impetus, whereby projectiles continue to move because they have an internal power imparted to them by their mover. It was this paradigm through which Galileo interpreted physical phenomena.¹¹⁵

Galileo’s significant modification was to suggest that impetus was the principal form of motion, in opposition to Aristotle’s view of a distinction between internal natural sources of motion and external interfering ones. The motive for this move was largely practical. External sources of motion could be subjected to mathematical analysis and prediction;

114 Peter McLaughlin, ‘Mechanism and the Clockwork Universe’ [Draft], 2010, pp. 11-14.

115 Thomas Kuhn, *The Structure of Scientific Revolutions*, Chicago and London: The University of Chicago Press, 1996, pp. 119-120.

internal sources could not. When Galileo found that he could make accurate predictions concerning the trajectory of falling bodies such as cannonballs and pendulums without needing to postulate the effects of internal sources of motion, this led him to believe it to be unnecessary to take such hypothetical forces into account. He did not completely abandon the Aristotelian doctrine of final ends but he rejected the explanatory utility of postulating them.¹¹⁶ Galileo thus sought to explain the trajectories of natural motion without speculating on the internal natures of entities and their final ends. To do so required rejecting the idea that it was important to distinguish between natural and forced motions to make accurate predictions. The implication was that natural entities could be treated as if they did not have internal sources of motion or final ends towards which they moved. It was a short step from this 'as if' position to one rejecting internal sources of motion and final causes. The downstream effect of this teleological agnosticism would be a practical move from thinking about whether domestication was perfective or restorative to investigating whether domesticated organisms could be *improved*.

I shall not go into detail regarding the Copernican challenge to the Aristotelian Ptolemaic system, as this only relates indirectly to the issue of domestic and wild nature. I will, however, note that the downfall of both Ptolemy's original geocentric system and Tycho Brahe's geoheliocentric system significantly undermined confidence in the Aristotelian cosmology, its teleology-based theories of elemental behaviour and its demarcation between incorruptible celestial and mutable earthly realms. This opened the way for alternative materialist theories of universal composition whilst simultaneously reinforcing the suspicion against postulating final causes as a means of explaining natural phenomena.¹¹⁷

A final contributing factor to the rejection of teleological methods of explanation were theological arguments, which continued to play a central role in 17th century anti-Aristotelian reasoning as they had in previous scholastic disputes. For instance, Francis Bacon's famed denouncement of final causes as 'barren virgins' should not be taken to imply he denied their existence. Rather, he believed Aristotle's error was to think that such final ends could be determined through logic as opposed to theology. Bacon by contrast held that God, not

116 John Losee, *A Historical Introduction to the Philosophy of Science*, 3rd Ed., Oxford/New York: Oxford University Press, 1993, pp. 55-57.

117 For details see: Thomas Kuhn, *The Copernican Revolution: Planetary Astronomy in the Development of Western Thought*, Cambridge (Mass.) and London: Harvard University Press, 1957.

nature, was “the fountain of final causes”, and that only revelation can teach us the true ends of nature.¹¹⁸ Similarly, René Descartes, like Bacon a critic of teleological supposition, criticised Aristotelians for being “so arrogant as to suppose that we can share in God’s plans”. There is some continuity between the arguments of scholastic anti-Aristotelians and of 17th century natural philosophers, namely the shared belief that final causes exist but the denial that they could be known without divine help. Theology, however, also remained a tool for those who wished to defend the human ability to infer final causes from observation and experiment. Robert Boyle, for example, held that “all Consideration of *Final Causes* is not to be Banish’d from Natural Philosophy: but *that* ’tis rather Allowable, and in some Cases Commendable, to Observe and Argue from the Manifest Uses of Things, that the Author of Nature Pre-ordain’d those ends and uses”.¹¹⁹ Boyle's disagreement with Bacon and Descartes on teleology related to his differing views on how to attain knowledge of God. Descartes had insisted that knowledge of God’s existence and power could only be attained through abstract rational introspection on clear and distinct ideas, which unlike the observations of the senses were not subject to doubt. Bacon, on the other hand, held that human knowledge, whilst vast in potential, was limited to created things and could not apprehend the transcendent uncreated essence of God and his true will.¹²⁰ In contrast to these views, Boyle held that knowledge of God’s intentions could be derived both from traditional scripture, the Book of Revelation, and the ‘Book of Nature’.¹²¹

The ‘two books’ argument not only defended teleological supposition but also supplied a means to justify experimental philosophy as a form of natural theology. It thus offered a defence against criticisms from those who, like Margaret Cavendish, accused Boyle and his colleagues in the Royal Society of being addicted to “unprofitable arts” and of acting like “boys that play with watery bubbles or fling dust into each other’s eyes”.¹²² This physico-theological argument would also be used by Boyle and his supporters to argue for human

118 Francis Bacon, 'De Augmentaris Scientiarum', James Spedding (Trans.), In: *The Philosophical Works of Francis Bacon*, John M. Robertson (Ed.), Routledge Revivals, 2011, pp. 423 & 472.

119 Daniel Garber, 'Physics and Foundations', In: *The Cambridge History of Science, Volume 3: Early Modern Science*, Katharine Park and Lorraine Daston (Eds.), Cambridge University Press, 2006, p. 64.

120 Steven Matthews, *Theology and Science in the Thought of Francis Bacon*, Great Britain: Ashgate, 2008, p. 66.

121 Rose-Mary Sargent, *The Diffident Naturalist: Robert Boyle and the Philosophy of Experiment*, University of Chicago Press, 1995, pp. 109-130.

122 Margaret Cavendish, *Observations upon Experimental Philosophy*, Eileen O' Neill (Ed.), Cambridge University Press, 2003, p. 52.

dominion and to justify the experimental use of animals. A professed believer in “the empire of man over the inferior creatures”, Boyle, like most of his contemporaries, believed the Book of God to have in Genesis sanctioned the stewardship of humankind over the rest of nature.¹²³ But Boyle also believed the Book of Nature to demonstrate human dominion over nature, as can be seen from his stating that it would be “no great presumption to conceive, that the rest of the creatures were made for man, since he alone of the visible world is able to enjoy, use, and relish many of the other creatures, and to discern the omniscience, almightiness, and goodness of their author in them”.¹²⁴ To reject human dominion was therefore not only to challenge scriptural authority but also to turn away from the revelation of God's work in nature. Boyle believed it imperative that a good Christian natural philosopher be prepared to take all reasonable measures to interpret nature, for this was not only in the interest of humankind's betterment but was moreover part of God's plan for human fulfilment and salvation. As animals had been placed in the world for humanity's sake and not their own, experimenting on them, whether by starving them of air or vivisectioning them, was justified so long as it was in line with the greater cause of bringing humankind closer to God. Boyle did recognise animal suffering and did not approve of unwarranted cruelty, not least as to misuse animals was to misuse God's creation and was therefore blasphemous.¹²⁵ But in practice he endorsed animal experimentation so long as it was purposeful. For Boyle, therefore, the use of severe domesticating practices and extreme organismic interventions had a twofold justification of not only being within the right of humankind as granted in Genesis but also as a responsibility of Christians as a means of approaching God through knowledge of His creation. Wild nature, as what was yet to be made useful or understood, was thus doubly repugnant as evidence of both humankind's wasteful disuse of its dominion and ignorance and distance from God.

Hobbes' Rejection of Dominion & Domestication as Natural

Boyle's defence of animal experimentation on the basis of divinely gifted dominion did not go unchallenged. Thomas Hobbes' hostility to Boyle's experimental programme is well-known and has been discussed in detail elsewhere. Most focus, however, has centred on the

¹²³ Keith Thomas, *Man and the Natural World: Changing Attitudes in England 1500-1800*, Allen Lane, 1983, p. 22.

¹²⁴ Anita Guerrini, 'The Ethics of Animal Experimentation in Seventeenth-Century England', *Journal of the History of Ideas*, Vol. 50, No. 3, 1989, pp. 396-397.

¹²⁵ Malcolm R. Oster, 'The "Beame of Diuinity": Animal suffering in the Early Thought of Robert Boyle', *The British Journal for the History of Science*, Vol. 22, No. 2, 1989, p. 172.

conflict between Boyle's believed experimental demonstration of the vacuum and Hobbes' plenist critique of this claim. Equally stark differences, though, existed between Boyle and Hobbes' interpretations of human dominion and of the domestic-wild relationship. These differences arose out of the same initial disagreements on nature and politics which Shapin and Schaffer have shown to have driven the dispute over the existence of the vacuum.¹²⁶

At the root of these differences was Hobbes' heterodox interpretation of human nature, natural ontology and scriptural authority. Genesis would have it, as we have seen, that the first humans were horticulturalists already inclined towards the habits and morals of civilised life. This claim was reinforced by the belief, promoted by Francis Bacon and theologians such as Lancelot Andrewes, that Adam was a natural philosopher with an inherent knowledge of agriculture and husbandry (as will be discussed in more detail later on).¹²⁷ The Fall was assumed to have clouded some of this original knowledge of nature but it was usually assumed that humanity had retained its knowledge of agriculture and its inclination for sedentism (Adam and Eve were thrown out to work the land, not to hunt and gather, and their son Cain was a farmer). Hobbes, however, was led to question this claim of an original (relatively) civil and settled human nature on the basis of claims that there existed 'savage peoples', such as those found in the Americas, that had "no government at all" and lived in a "brutish manner", lacking all social ties save those of kinship. Hobbes' perception of an underlying human inclination to barbarism was reinforced by the descent of the Three Kingdoms of England, Scotland and Ireland into civil war, which forced him into exile in Paris in 1640. In 1651, Hobbes published *Leviathan*, a political tract both responding to the past dozen years' political crises and presenting a theory of governance aimed at negating the factors that Hobbes saw as responsible for the conflict. As part of this, Hobbes introduced the claim that human culture and civilisation had not directly descended from Eden but instead emerged from a barbaric 'state of nature' in which life was "solitary, poor, nasty, brutish, and short". Humankind was not born civilised but had instead needed to be forcibly domesticated. This had required the imposition on men of "a common power to keep them all in awe", namely the disciplining authority of an absolute sovereign.¹²⁸

126 Steven Shapin and Simon Schaffer, *Leviathan and the Airpump: Hobbes, Boyle, and the Experimental Life*, Princeton and Oxford: Princeton University Press, 2011.

127 Steven Matthews, *Theology and Science in the Thought of Francis Bacon*, Great Britain: Ashgate, 2008, 59-63.

128 Thomas Hobbes, *Leviathan*, J.C.A. Gaskin (Ed.), Oxford University Press, 1996, pp. 82-86.

Hobbes suggested that such sovereigns had first been put in place not by God but through the formation of commonwealths by diverse tribal parties making covenant, who then selected one of their number to act as leader. Such covenants formed when individuals recognised that the best means for ensuring peace and the preservation of life was to establish a civil society, which required a sovereign as lawmaker and enforcer. These comings together were not due to natural human inclination or inherent capacity to recognise civil society as a good. Hobbes thus rejected the Biblical claim that humans have an intrinsic ability to distinguish good from evil. Instead, he claimed that only 'the right of nature' could be recognised by moral reason, namely "the liberty each man hath to use his own power as he will himself for the preservation of his own nature; that is to say, of his own life; and consequently, of doing anything which, in his own judgement and reason, he shall conceive to be the aptest means thereunto". Conflict thus ensued because of the overlapping needs and desires of different individuals, passions Hobbes saw as "in themselves no sin". The 'state of nature' was not one of fallen grace nor due to inherent human evil but rather a product of the absence of earthly law-giving. Earthly law was necessary, Hobbes believed, to establish a moral order, as even avaricious human desires and acts were not sinful "till they know a law that forbids them; which till laws be made they cannot know, nor can any law be made till they have agreed upon the person that shall make it".¹²⁹ Hobbes further insisted that this law must be unitary. The populace must for instance be protected against the clergy's use of 'priestcraft', the manipulation of doctrine to pursue power, by granting the sovereign sole authority over establishing the correct interpretation of scripture.¹³⁰ Permitting another power space to grow would threaten sedition against the sovereign and descent into civil war.¹³¹

In sum, human 'savagery' was for Hobbes the natural state and civil society an artefact derived not from innate moral capabilities but rather through narrow but rational human self-interest. Hobbes' commonwealth, his Leviathan or "artificial man", was therefore a unified "body politic" only so much as it was held together by the weak ties of covenant and forcibly by the power of the sovereign, the artificial man's "artificial soul".¹³² Hobbes further

129 Ibid.

130 Steven Shapin and Simon Schaffer, *Leviathan and the Airpump: Hobbes, Boyle, and the Experimental Life*, Princeton and Oxford: Princeton University Press, 2011, pp. 103-104.

131 Thomas Hobbes, *Leviathan*, J.C.A. Gaskin (Ed.), Oxford University Press, 1996, p. 7.

132 Ibid.

extended his naturalistic account of morality and civil society to incorporate an alternative account of the domestication of non-human animals which rejected the idea that they were created tame. This too was motivated by his rejection of a differentiation between spiritual and temporal government, as embodied in institutions of church and state, in favour of a unitary sovereignty, for the religious argument that human control over domestic creatures rested on a divine mandate was for Hobbes another example of churchmen deceiving people into 'seeing double' as a means of defending their claim to spiritual government.¹³³ Just as Hobbes denied the legitimacy of a spiritual government independent of temporal government, so he also denied a theological source for human dominion over nature. Against those who insisted on human dominion over nature as God-given, Hobbes asked "what consisteth the dominion of man over a lion or a bear?" It could not be an obligation of promise or debt, as such beasts had no sense of debt or duty, nor could it be based on a divine command to obey, as beasts lacked the reason to comprehend the word of God. In lieu of a theological argument for human dominion over nature, Hobbes offered a naturalistic account. Humans had dominion over bears and lions because whilst the latter were individually physically stronger, humans were more powerful due to their great artifice and intellect. But this was a relative rather than absolute dominion; whilst humans usually had dominion over beasts, in such circumstances as when a hungry lion met an unarmed man in the desert, then the lion had dominion (Hobbes here echoes Porphyry). Based on this relativity, Hobbes argued that relations between man and beast could not properly be considered relations of dominion, which were absolute and based on covenant, e.g. the dominion of God over Man (established originally through the Jewish covenant and then extended to all humankind by Christ) and the dominion of Sovereign over Society. Instead, he regarded these as relations of "hostility", and applied this logic even to the keeping of livestock, "for we keep them only to labour, and to be killed and devoured by us; so that lions and bears would be as good masters to them as we are". Relations of hostility had no base in a divinely granted right of dominion or a natural state of domestication, the implication being that animals had originally existed wild in a state of nature prior to domestication. In the state of nature, only the right of nature applied, so animals therefore owed humanity no subjugation. Hobbes did however allow that humans could justify their hostile domination over animals based on the right of nature, i.e. domesticating useful animals to provide sustenance and clothing, and killing pests and

¹³³ Steven Shapin and Simon Schaffer, *Leviathan and the Airpump: Hobbes, Boyle, and the Experimental Life*, Princeton and Oxford: Princeton University Press, 2011, pp. 92-99.

predators to preserve and protect their own lives.¹³⁴ But humans could never have true dominion over other animals as “To make covenants with brute beasts is impossible, because not understanding our speech, they understand not, nor accept of any translation of right, nor can translate any right to another: and without mutual acceptation, there is no covenant”¹³⁵

The Dividing Line of Reason – New Secular Accounts of Dominion

By holding domestication to be a hostile as opposed to divinely sanctioned relationship, Hobbes contradicted Boyle's belief that scientific domestication was both a right and a responsibility of Christian natural philosophers. If the suffering of non-human organisms were to be justified, it could only be on grounds of the right of nature, i.e. on material human need. Furthermore, because Hobbes denied the possibility of using nature as a second book, maintaining that only scripture offered access to knowledge of God, there was subsequently no justification for animal suffering as a means to religious knowledge.¹³⁶ Hobbes would therefore, in rejecting the two books' argument, restrict scientific domestication's legitimate remit to more narrowly utilitarian projects. But whilst Hobbes's deflationary account of dominion both naturalised domestication and undermined theological justifications for its crueller applications, he continued to see human mentality and society as comprehensively differing from that of other animals in a number of important ways. Like most of his contemporaries, Hobbes distinguished humans from beasts by their possession of reason, an idea that went back to Aristotle. Hobbes did not, however, consider the mental divide between man and beast to be wholly drastic and discrete, arguing for animals to possess some basic imagination and understanding and even going as far as to state that beasts were commonly superior in their prudence compared to human children.¹³⁷ Hobbes moreover emphasised that the possession of reason was as troublesome as it was helpful for humans, asserting that “whereas by the benefit of words and ratiocination they exceed brute beasts in knowledge; by the incommodities that accompany the same they exceed them also in errors. For true and false are things not incident to beasts, because they adhere to propositions and language; nor have they ratiocination, whereby to multiply one untruth by another, as men

134 Thomas Hobbes, *The English Works of Thomas Hobbes, Volume 5*, William Molesworth (Ed.), London: John Bohn, 1841, pp. 187-188; & Keith Thomas, *Man and the Natural World: Changing Attitudes in England 1500-1800*, Allen Lane, 1983, p. 171.

135 Thomas Hobbes, *Leviathan*, J.C.A. Gaskin (Ed.), Oxford University Press, 1996, p. 92.

136 Steven Shapin and Simon Schaffer, *Leviathan and the Airpump: Hobbes, Boyle, and the Experimental Life*, Princeton and Oxford: Princeton University Press, 2011, pp. 103-104.

137 *Ibid.*, pp. 15-19.

have”.¹³⁸ The ‘war of all against all’ was not, Hobbes believed, inherent in a state of nature – he did not anticipate Darwin’s ‘struggle for life’. Instead, Hobbes interpreted the fact that many animals, e.g. bees and ants, were able to live sociably as evidence for the existence of ‘natural agreement’ among such species, i.e. that they were instinctually co-operative. He also did not see animals as driven to competition in the same manner as humans since “men are continually in competition for honour and dignity, which these creatures are not”. Furthermore, the fact that animals lacked reason was to their benefit in the sense that they “do not see... any fault in the administration of their common business: whereas amongst men there are very many that think themselves wiser and abler to govern the public better than the rest, and these strive to reform and innovate, one this way, another that way; and thereby bring it into distraction and civil war”.¹³⁹ Because animals lacked reason, they thus also lacked politics, the great source of dissension which made the imposition of sovereignty necessary. Animals therefore co-operated as they could not find cause to do otherwise, and animal sociability was thus by nature harmonious. But the converse deficiency animals suffered due to their lack of reason was their inability to act artfully including, importantly, an inability to form artificial covenants and so establish civil societies. Whilst reason brought men to strife, it also allowed them to see the benefits of allegiance and of submission to a sovereign, and by this means they were able to unite their strength and exercise far greater power over nature than any other animal. It was thus this capacity for artful civilisation, for what might be viewed in the context of this thesis as the human capability for self-domestication, which set human beings apart from the rest of nature.

Other philosophers saw the divide in rationality between humankind and nature to be far more stark. Mechanistic philosophy had already concluded, contra to Aristotelian doctrine, that the activity of non-living things such as stones and fire was non-purposive. Hobbes was part of this revolutionary new worldview. But he did not extend this anti-teleological perspective on nature to the behaviour of non-human organisms. Other mechanists however did make this step, the most influential of whom was René Descartes. It should be emphasised that Descartes did not make this move towards mechanising living nature blindly. As Gideon Manning has shown, Descartes, whilst better known for his theoretical and

138 Thomas Hobbes, *Elements of Law, Natural and Politic*, J.C.A. Gaskin (Ed.), Oxford University Press, 1994, pp. 38-39.

139 Thomas Hobbes, *Leviathan*, J.C.A. Gaskin (Ed.), Oxford University Press, 1996, p. 113.

mathematical labours, also engaged in physiological studies, including dissecting animal brains.¹⁴⁰ Descartes was also influenced by William Harvey's *De Motu Cordis*, a work which rejected earlier ideas of the heart as imbuing blood with immaterial animal spirits in favour of the view that it was a mere 'pump' for its circulation.¹⁴¹ Under the influence both of Harvey and his own experience, Descartes came to see the animal body as essentially a mechanistic contraption of pumps and cables. Knowing that automata could be crafted which simulated purposeful behaviour yet were lifeless, he further claimed that the activities of the 'beast machine' similarly did not require animation by an underlying intelligence. Organic bodies were only differentiated from manmade machines by their complexity, which was enabled by their being crafted by an infinitely more talented designer, namely God. Treating organism as machines and denying animal mentality, Descartes presented non-human nature as uniformly material, composed solely of *res extensa*, a corporeal substance the essence of which was its being extended in space. Human beings were set apart from nature by being uniquely composed of both an extended body and an incorporeal mind composed of a second substance, the *res cogitans* or 'thinking thing'. This rendered them sole possessors of reason in the mortal world and also the only living beings able to feel pain, Descartes believing pain required understanding to be felt. This claim was taken by many of Descartes' followers to imply that humans had no cause for qualms about animal experimentation and similar such interventions, the screams of vivisected animals being equated with the noises made by malfunctioning machines.¹⁴²

Descartes' substance dualism offered a new secular justification of human dominion, for if material nature was essentially mindless and unfeeling, then there was no cause not to treat the rest of living nature in purely instrumental terms. As John Ray observed, "those philosophers indeed, who hold man to be the only creature in this sublunary world, endued with sense and perception, and that all other animals are mere machines or puppets, have some reason to think that all things here below were made for man". This shows that whilst

140 Gideon Manning, 'Descartes and the Bologna Affair', *The British Journal for the History of Science*, Vol. 47, No. 1, pp. 5-6.

141 Harold J. Cook, 'Medicine', In: *The Cambridge History of Science, Vol. 3: Early Modern Science*, Katharine Park and Lorraine Daston (Eds.), Cambridge University Press, 2008, pp. 425-427.

142 Malcolm R. Oster, 'The 'Beame of Diuinity': Animal suffering in the Early Thought of Robert Boyle', *The British Journal for the History of Science*, Vol. 22, No. 2, 1989, p. 152; Anita Guerrini, 'The Ethics of Animal Experimentation in Seventeenth-Century England', *Journal of the History of Ideas*, Vol. 50, No. 3, 1989, pp. 391-392.

much has been made of how the 17th century's 'scientific revolution' de-centred the Earth and humankind within the universe, the rejection of Aristotelianism and scriptural authority did not significantly alter broad perceptions of the relationship between humanity and other species. If anything, the gradual replacement of scripturally grounded accounts of dominion with secular ones based on the distinctiveness of human reason tended to lend itself to a more aggressively exploitative attitude to nature. For whereas Christian tradition held that God-given dominion also necessitated responsibilities of stewardship over created nature, secular accounts stressed that since nature was not ensouled, human instrumental reason was free to do as it desired without moral qualms. Ray himself rejected this implicit anthropocentrism and sought to use natural theology to prove that other creatures not only behaved purposefully but also had independent ends which circumscribed ethical limits to the human right of dominion (see my later discussion of Ray's influence on Linnaeus' idea of the economy of nature).¹⁴³

Ray has often been seen as part of a losing cause ultimately subsumed by a mechanistic and instrumentalising rationalist interpretation of nature. The 17th century 'scientific revolution' therefore in the 20th century became a target for critics of modernity who traced its genesis back to this era and its thinking. The German philosopher Martin Heidegger for instance saw in the rejection of Aristotelianism and subsequent expansion of human technologies of domestication a move towards treating nature as a mere 'standing-reserve' having no more worth than as reserve potential for future industrial consumption.¹⁴⁴ The feminist philosopher Carolyn Merchant has moreover interpreted the shift from an organic to a mechanistic view of nature as being a move from treating nature as a nurturing mother, and therefore being culturally constrained not to slay, wound or mutilate her, to treating nature as "a system of dead, inert particles moved by external... forces". This "death of nature" enabled its rational control by allowing it to be treated in instrumental terms as if it were a lifeless machine. To domesticate nature through domination therefore according to Merchant required first to 'kill' nature and reduce it into malleable fragments. This attitude has, she argues, persisted in 20th

143 John Hedley Brooke, 'Wise Men Nowadays Think Otherwise': John Ray, Natural Theology and the Meanings of Anthropocentrism', *Notes and Records of the Royal Society of London*, Vol. 54, No. 2, 2000, pp. 207-208.

144 Martin Heidegger, 'The Question Concerning Technology', *The Question Concerning Technology and Other Essays*, William Lovitt (Trans.), New York/London/Toronto/Sydney: Harper Perennial, 1977, pp. 3-35.

century physics, which “still views the world in terms of fundamental particles”, rather than as an animated whole.¹⁴⁵

The control extended over nature by 17th century natural philosophy was also extended over human society, which was increasingly seen, as posited by Hobbes, as a mechanistic artefact decomposable into human individuals, who themselves were re-conceived in economic terms as components pieces of the societal machine. This social aspect of mechanistic modernity was highlighted by Max Horkheimer and Theodor Adorno of the Frankfurt school of critical theory, who traced the roots of mid-20th century totalitarianism to this 17th century embrace of instrumental rationality and its emphasis on controlling society through rational planning and instrumental hierarchies. “The history of man's efforts to subjugate nature”, Horkheimer observes, “is also the history of man's subjugation by man”.¹⁴⁶ This argument would therefore have it that advances in human dominion over nature through the extension of technologies of domestication have also further contributed to a stultifying and alienating human domestication characterised by disenchantment, commodification of human productive capabilities and restricted freedom.

These modern critical accounts of the wider impact of 17th century mechanistic and rationalist philosophies and their associated justifications of dominion over nature and society are by and large grounded in an interpretation of domestication, both non-human and human, as characterised by domination by human art and technology. As discussed above, whilst I certainly accept the argument that many particular cases of domestication are characterised by human dominance over other organisms (and over other humans, if we are going to consider human slavery a particularly egregious instance of domestication), I nonetheless argued that the picture is more complex in that many cases of domestication are more characterised by relations of symbiosis, and that in some cases it is humans that follow non-human actors into establishing such relations rather than *vice versa*. These specific instances of symbiosis do not in any way excuse the destructive impact of the instrumental treatment of nature on the wild environment or on domestic organism welfare. It also does not negate the negative effects of technological controls on human liberty. But acknowledging the

145 Carolyn Merchant, 'The Death of Nature', *Environmental Philosophy: From Animal Rights to Radical Ecology*, Michael Zimmerman et al (Eds.), Prentice-Hall, 1993, pp. 269-278.

146 Max Horkheimer, *Eclipse of Reason*, New York: Oxford University Press, 1947, p. 72.

coexistence in the domestic sphere of relations of domination and symbiosis allows us to recognise the importance of interspecies relations in the development of modern human enterprises such as the sciences. It also allows us to recognise that artifice and technology is as much part of our evolving human nature that enables new capacities for action (in doing so disrupting previous dominant social structures), as it is a restrictive force binding individuals in place in the social fabric. So much as the domestic-wild dichotomy is best treated as a dialectic, so too should domination-symbiosis and technology-nature. Acknowledging the dynamism of these relations allows us to move away from the view of modern human-nature/art-nature relations as characterised by the destruction or neutralising of nature and the forcible imprinting of human intentions and artifice on its yielding dead surface towards a perspective which acknowledges that such domineering and destructive relations are sometimes the case but that more symbiotic and mutually beneficial relationships are equally often prevalent. In scientific knowledge production there has particularly been a tension between more crudely interventionist approaches to the study of nature and those dependent on mutualist relations between the investigator and their object. This tension has been especially visible in the life sciences. My argument shall be that hybrid domestic zones where human-nature relations have been neither wholly characterised by an effort to bracket a disinterested observer from observed wild nature or an effort to strictly impose human order on nature have been key sites for such symbiotic and epistemically productive relations. I will now move to investigating such sites by beginning with what I consider were the original hybrid zones where the intermingling of domestic and wild was exploited by natural philosophers as a means of accessing nature under relatively controlled conditions, namely the garden and the field.

Chapter 3 – Gardens and Fields: Baconian Natural Philosophy and the Role of Hybrid Spaces for Scientific Knowledge Production & the Restoration of Human Dominion

Gardens and Fields as Important Sites of Early Modern Natural Historical Research & Knowledge Production

In his *Novum Organum*, Francis Bacon sought to promote a hybrid natural philosophy. He claimed that “Those who have handled sciences have been either men of experiment or men of dogmas”. He compared the men of experiment to the ant, for “they only collect and use”, whereas “the reasoners resemble spiders, who make cobwebs out of their own substance”. Bacon advocated “a middle course”, that of the bee which “gathers its material from the flowers of the garden and of the field, but transforms and digests it by a power of its own”. True philosophy, Bacon remarked, “neither relies solely or chiefly on the powers of the mind, nor does it take the matter which it gathers from natural history and mechanical experiments and lay it up in the memory whole, as it finds it, but lays it up in the understanding altered and digested”.¹⁴⁷ Most philosophical analysis of Bacon’s statement has focused on his desire to develop a hybrid mode of enquiry joining together the power of the experimental and rational faculties.¹⁴⁸ I wish, however, to direct attention not so much to the mode of enquiry Bacon champions as the sites suggested, i.e. the garden and field (by ‘field’ I take to be implied the cultivated farm field or the managed pastoral meadow). The reference to these sites of enquiry may be taken as simply part of Bacon’s metaphorical analogy between the bee and natural philosopher’s modes of existence. But this would be to ignore the importance already ascribed in Bacon’s time to gardens and fields as spaces for natural historical enquiry and knowledge production, as I will now attempt to show.

I will shortly return to considering Bacon's specific ideas about gardens and fields. But first I will ask: what might be the specific attraction of gardens and fields as sites of scientific enquiry? One element was certainly their ease of access (‘nature on the doorstep’) for

147 Francis Bacon, 'Novum Organum', *The Philosophical Works of Francis Bacon*, Vol. I, Robert Leslie Ellis and James Spedding (Trans.), John M. Robertson (Ed.), Routledge Revivals, 2011, p. 288.

148 For useful overviews of traditional philosophical interpretations of Bacon's metaphor of the insects, see: Madeline M. Muntersbjorn, 'Francis Bacon's Philosophy of Science: *Machina intellectus* and *Forma indita*', *Philosophy of Science*, Vol. 70, No. 5, Proceedings of the 2002 Biennial Meeting of The Philosophy of Science Association Part I: Contributed Papers Edited by Sandra D. Mitchell, 2003, pp. 1137-1148; & Ian Hacking, *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science*, Cambridge University Press, 1983, pp. 246-249.

naturalists operating in an era when travel aboard to wilder natures was often limited, expensive and dangerous. Even more proximate European 'wildernesses', such as mountains, forests and tundras, were widely feared as dismal abodes of savage animals, uncultured tribes, outlaws, and hostile elemental and spiritual forces.¹⁴⁹ Scientific investigation within the domestic-wild milieu of garden and field was not hampered by the dangers, real and imagined, of the wilderness, and the natural entities within it, being partly or wholly domesticated, were easier to control than the unfriendly beasts of the wild. This is not to deny the importance of expeditionary naturalism in this period. Abroad, three of the most substantial scientific projects of the 16th and 17th centuries were the colonial surveys of 'wild' Brazil, Mexico and Virginia.¹⁵⁰ Closer to home, botanists climbed mountains in search of new plant species (as Edward Llwyd did in Snowdonia), and exhorted medical students to enter deep into the forest in their search for nature's remedies (as Caspar Hoffmann did in his and Ludwig Jungermann's 1615 *Catalogus Plantarum, quae circa Altorfium Noricum*).¹⁵¹

The attraction of gardens and fields was thus not simply one of mere proximity. It was rather their capacity as hybrid spaces to bring exotic species fetched from the far-off 'wild' Indies by explorers and traders into a familiar and local European domesticity. As Daniela Bleichmar comments, "The great majority of Europeans came into contact with the New World in Europe: colonial science was often enacted at home, not abroad".¹⁵² It was in European botanical gardens and aristocratic menageries that many exotic plant and animal species were first properly studied as living beings as opposed to preserved specimens. These institutions acted as nurseries in which foreign species could be contained and kept alive so that their possible utility as a source of food, materials, beauty or entertainment could be determined, either experimentally or based on reports of their use by the inhabitants of their place of origin. Once processed, the newly domesticated exotics could then be disseminated to other gardens and fields based on their discovered uses. Pharmaceutical plants went to physic

149 Keith Thomas, *Man and the Natural World: Changing Attitudes in England 1500-1800*, Allen Lane, 1983, pp. 194-196 & 258.

150 Paula Findlen, 'Courting Nature', In: *Cultures of Natural History*, N. Jardine, J.A. Secord and E.C. Spary (Eds.), Cambridge University Press, 1996, pp. 71-73.

151 Keith Thomas, *Man and the Natural World: Changing Attitudes in England 1500-1800*, Allen Lane, 1983, p. 258; & Alix Cooper, *Inventing the Indigenous: Local Knowledge and Natural History in Early Modern Europe*, pp. Cambridge University Press, 2007, p. 64.

152 Daniela Bleichmar, 'Books, Bodies, and Fields: Sixteenth-Century Transatlantic Encounters with New World *Materia Medica*', In: *Colonial Botany: Science, Commerce, and Politics in the Early Modern World*, Londa Schiebinger and Claudia Swan (Eds.), University of Pennsylvania Press, 2005, p. 83.

gardens, aesthetically pleasing ones to ornamental gardens; some exotic animals became popular household pets, e.g. parrots and guinea pigs, whereas others went to the farm, e.g. turkeys, where they joined a growing bonanza of new European crops, e.g. potatoes, tomatoes, maize and tobacco. All in all, there was plenty of material to keep a institutionally well-connected naturalist very busy even if they were restricted to their home country. The extent of this influx of new species was immense, especially in terms of plants species – Thomas notes that in the years between 1500 and 1839 the number of kinds of cultivated plant in Britain increased from around 200 to 18,000, much of it driven by the exponential growth in imports of exotic ornamental flower species.¹⁵³

Gardens and fields not only acted as processing points for incoming new species but also played a significant role as sites of metonymy where the global order of nature was catalogued through the collection and ordering of living specimens. This effort to create a microcosm of world nature within the garden was most evident in the physic gardens attached to university medical faculties. These research institutions were first established in mid-16th century Italy as living repositories of medicinally valuable species for medical students to study and use. They were originally organised on the basis of usage classifications such as that developed in the 1st century physician Dioscorides' *De materia medica*. But the continual influx of new species from the Indies that had no place in these classical taxonomies led to a perceived need for a change in organisation, so that from the 1590s onward plants were increasingly ordered on the basis of geographical origin. When, for instance, Carolus Clusius reorganised the Leiden garden (founded in 1577), he divided it into four quadrants, each representing the four continents, and planted them with corresponding species. This new schema was better able to incorporate the continual intrusion of new species. But, as Paula Findlen observes, it also contributed towards the move to organising physic gardens “as a microcosm of the world”.¹⁵⁴ Gardens became maps of phytogeography, with individual species as representatives of their continent's floral diversity. The physic garden in this way came to represent one of the many means by which domestic spaces have been utilised as means of bringing the wild under controlled conditions, imposing an artificial order upon it

153 Keith Thomas, *Man and the Natural World: Changing Attitudes in England 1500-1800*, Allen Lane, 1983, p. 226.

154 Paula Findlen, 'Anatomy Theaters, Botanical Gardens, and Natural History Collections', *The Cambridge History of Science, Vol. 3: Early Modern Science*, Katharine Park and Lorraine Daston (Eds.), Cambridge University Press, 2008, pp. 280-283.

and reducing its variability whilst retaining and in some cases magnifying the representative quality of domestic specimens as stand-ins for 'nature'. The metonymic use of exotic specimens to stand in for newly encountered or conquered territories was not limited to plants – Harriet Ritvo, for instance, has noted that there was a long tradition of using wild animals captured from outlying lands as “tokens of political submission”. Originally kept in private menageries, from the late 17th century these animals were increasingly made available for public view, where they operated as stand-ins for “the spread of [European] commercial influence throughout the globe”. It moreover became common to demonstrate the penetration of European power and intellect into virgin territories by bringing back a novel exotic specimen by which to awe spectators back home.¹⁵⁵

As well as offering accommodation for and access to exotic species, gardens and fields were also of considerable value due to the ingress they offered to local natures. Many amateur naturalists lacked expeditionary or institutional access to the exotic, so instead sought to study nature in more familiar places, collecting specimens in such domesticated or half-wild spaces as pastoral fields, coppiced forests, orchards, deer parks, seashores, streams and rivers, and even in their own backyards. By mass collecting oddities and anecdotes, non-elite naturalists were able to emulate the aristocratic culture of curiosity despite their lack of access to the exotic and far-fetched.¹⁵⁶ They were aided in this exploration of local nature by the existence of an extensive corpus of folk knowledge, practices and traditions which could be gathered and conscripted for the purposes of natural history. Skilled lay husbandry and cultivation was often far better informed regarding particular facets of domesticated nature than ancient writers and educated scholars – regarding apiculture, one Jacobean authority remarked that “every silly woman” was ready to deride the “learned ignorance” of those who had learnt of bee-keeping through reading Aristotle and Pliny.¹⁵⁷ Through conversing with and learning from these lay practitioners, enterprising naturalists who lacked institutional affiliations and could not afford travel overseas were nonetheless able to pursue the study of nature and claim to add to novel erudition by documenting and rigorously testing such folk knowledge. Gardens and fields additionally were important sources of material for the kinds

155 Harriet Ritvo, *The Animal Estate: The English and Other Creatures in the Victorian Age*, 1987, Harvard University Press, 1987, pp. 206-207.

156 Katie Whitaker, 'The Culture of Curiosity', In: *Cultures of Natural History*, N. Jardine, J.A. Secord and E.C. Spary (Eds.), Cambridge University Press, 1996, pp. 75-90.

157 Keith Thomas, *Man and the Natural World: Changing Attitudes in England 1500-1800*, Allen Lane, 1983, p. 73.

of experimental enquiry Bacon and his followers were promoting, there being an ample supply of domestic and commensal animals and plants which could be brought indoors and subjected to dissection, vivisection, temperature changes, chemical treatments, etc., so as to further the understanding of the effects of art on living nature. The near outdoors also furnished much of the material for later 17th century microscopic investigations into the previously invisible world of tissue structures, insect anatomy and animalcules. Furthermore, as shall be discussed in detail later, the garden and field were the central sites for experimental interventions in animal and plant reproduction, be they experimental hybridisations (both between different breeds and different species), experimental selection for particular characteristics, experimental inbreeding, or other empirical efforts to influence offspring type, e.g. experiments in provision of different animal forage and accommodation.

The local nature of garden and field ultimately proved as important an exotic frontier for scientific enquiry as the 'wildernesses' of the Indies, despite being otherwise highly familiar and domesticated. Alix Cooper observes there to have been an increased interest in local nature in the sixteenth and seventeenth centuries, which was marked by the growth in popularity of local natural histories and handbooks documenting the uses of indigenous flora and fauna.¹⁵⁸ There is moreover a significant parallel between how European explorers and colonialists utilised non-European folk knowledge to better control and understand the exotic natures of the New World and how scholarly Europeans exploited the wisdom of their own lay countrymen/women to extend knowledge of local natures. Londa Schiebinger for instance notes that “The process of collecting information from women 'root cutters', old women, or a particularly successful woman healer was strikingly similar to that of prospecting abroad [e.g. in the West Indies]”.¹⁵⁹ This gender dynamic of men conscripting knowledge from female practitioners has also been observed to have occurred in other areas science, e.g. reproductive medicine.¹⁶⁰ We may thus speak of an internally directed scientific exploration and colonisation of domestic European nature, principally focused on gardens and fields, which responded to and complemented the externally directed secular and scientific exploration and

158 Alix Cooper, *Inventing the Indigenous: Local Knowledge and Natural History in Early Modern Europe*, pp. Cambridge University Press, 2007, p. 21-22.

159 Londa L. Schiebinger, *Plants and Empire: Colonial Bioprospecting in the Atlantic World*, Harvard University Press, 2007, pp. 96-97.

160 Carolyn Merchant, 'The Scientific Revolution and *The Death of Nature*', *Isis*, Vol. 97, No. 3, 2006, pp. 514-515.

colonisation of 'wild' nature in the New World (as well as those wilderness regions at Europe's edges, e.g. Lapland and Siberia). Of course, much of the New World was not truly wild and much of folk knowledge about domestic natures (whether European or non-European, male or female) was detailed and accurate enough to suit its particular practical purposes without requiring significant embellishment. What the New World lacked was 'the right kind' of domestication (Christian European civilisation), and what the practitioners of folk knowledges lacked was the credibility of being part of and knowing the social niceties necessary to negotiate the gentlemanly circles within which 'proper' natural historical discourse was carried out.¹⁶¹ Women particularly, even those of a gentrified upbringing, were excluded from these circles, typically on the grounds of sexist claims that they lacked the necessary intelligence or objectivity for scientific pursuits (though there were a few notable instances, such as the natural philosopher Margaret Cavendish, of early modern women who did manage to enter such male scientific spaces as the Royal Society).¹⁶²

To further discredit folk knowledge, much emphasis was also placed on the 'vulgar errors' and godless superstitions that natural historians perceived it as shot through with. Such calumny was utilised to discredit folk knowers and therefore allowed the useful parts of their knowledge to be conscripted by scholarly naturalists without risking the accusation that such naturalists were dependent on external expertise. One means by which naturalists claimed superior expertise, especially those following Bacon's hybrid epistemology, was through experimentally testing folk wisdoms. The proof or disproof of such beliefs was claimed as contributing to scientific knowledge (whereas the original folk assertion, whether true or not, was not a contribution so long as it was 'untested'). Another self-ascribed source of expertise was the claimed objectivity of naturalists, for unlike folk knowers who supposedly learnt about nature almost accidentally whilst pursuing other practical ends, e.g. agriculture, it was claimed that true naturalists pursued nature for its own sake and therefore the knowledge they produced was not compromised by the influence of extraneous interests. Through this 'boundary work', as Thomas Gieryn calls it, the early modern natural scientific community was able to establish firm boundaries between its 'tested' knowledge and associated expertise

161 Steven Shapin, *Never Pure: Historical Studies of Science as if It Was Produced by People with Bodies, Situated in Time, Space, Culture, and Society, and Struggling for Credibility and Authority*, John Hopkins University Press, 2010, pp. 17-31.

162 Londa Schiebinger, *The Mind Has No Sex?: Women in the Origins of Modern Science*, Harvard University Press, 1991, pp. 37-65; & Naomi Oreskes, 'Objectivity or Heroism? On the Invisibility of Women in Science' *Osiris*, Vol. 11, Science in the Field, 1996, pp. 87-113.

and the 'unreliable' folk knowledges of women, laypeople and non-Europeans.¹⁶³ In particular, it was the experimental and apparently disinterested nature of the domesticating practices employed by naturalists which set them apart and provided grounds for their unique credibility. One of the first places where such practices were employed was in the sphere of the already domesticated, for whilst gardens and fields had already been brought under human stewardship, they had yet to be brought under the stewardship of science for the purpose of scientific knowledge production.

Gardens and Fields as Hybrid Spaces

As I have shown, gardens and fields were important sites for 17th century natural historical research, providing both local access to the more familiar, more domesticated nature of Europe and places for taming and ordering exotic specimens transplanted from the 'wilder' natures of the Indies. The utility of these sites was thus that they contained enough of the wild to provide access to nature and enough of the domestic to allow control and study of the untamed and unknown. I therefore assert that it is the inherent hybridity of gardens and fields as spaces where the wild and the domestic border and intermingle which made them particularly suitable for early modern scientific investigations into nature. They are exemplary instances of what Michel Foucault has described as a 'milieu', a heterogeneous space where a conjunction of the natural and artificial is formed by the attempts of human governmental reason to impose order on (and domesticate) unstable geographies and populations. Whilst Foucault principally applied this concept to biopolitical efforts to control civil populaces, it can equally be applied to human efforts to order and control nature in spaces such as gardens and fields, spaces, as we will see below, that Foucault recognised as exhibiting hybridity/heterogeneity.¹⁶⁴

As I will discuss, Bacon and other 17th century philosophers differed from us in their interpretation of the heterogeneous nature of gardens due to differing presumptions about the domestic-wild relationship, much of which was grounded in their understanding of scripture. This does not, however, detract from the argument I am making that the material (as opposed

163 Thomas F. Gieryn, *Cultural Boundaries of Science: Credibility on the Line*, Chicago & London: The University of Chicago Press, 1999, pp. 15-18.

164 Michel Foucault, *Security, Territory, Population: Lectures at the Collège de France, 1977-78*, Michel Senellart (Ed.), Graham Burchell (Trans.), Palgrave Macmillan, 2007, pp. 35-36; & Marco Altamirano, 'Three Concepts for Crossing the Nature-Artifice Divide: Technology, Milieu, and Machine', *Foucault Studies*, No. 17, 2014, pp. 17-18.

to conceptual) hybridity of gardens and fields significantly lent itself to natural historical investigations. These particular milieus were not alone in making such a contribution to science. For example, the lagoon at Lesbos which formed a major site for many of Aristotle's natural historical investigations can certainly be seen as such a milieu, where human efforts to control and harvest the sea meet with the continual influx of new water and new life through the lagoon's opening to the Mediterranean. But as it would be significantly beyond the bounds of this chapter to consider the role of domestic-wild milieus other than gardens and fields in early modern natural historical enquiry, and as it was from gardens and fields that breeding experiments moved into the laboratory, leading eventually to genetics and lab 'wild types', I will not here consider these other hybrid spaces in detail.

It may be contested that there is nothing special about the heterogeneous character of gardens and fields as sites of scientific investigation. Hybridity, it has been contended by Latour, has been a ubiquitous consequence of what he labels 'the Modern Constitution', this being the idea that developed around the time of the 'Scientific Revolution' that nature and society are distinct spheres of existence. Latour contends that any attempt to discern a 'pure' nature or culture inevitably results in the proliferation of hybrid nature-cultures that are 'impure' and take of both worldly aspects.¹⁶⁵ I have already discussed in detail one facet of this ascribed division between the natural and social, namely that between wild and domestic, and have concluded that it is best to treat these as an interpenetrating continuum or as a dialectic. I nevertheless maintained that it is still useful to treat domestic and wild as distinct categories because the differences between organisms lying at either extreme of the continuum are stark and significantly impact on their tractability/co-operability as participants in scientific knowledge production. Similarly, I will maintain that whilst Latour is correct that the idea of pure nature or pure society is fatuous, there are nonetheless spaces which are more hybrid and heterogeneous than others, and one of their characteristics is the ease they offer of movement between domestic and wild. This claim finds support in Foucault's assertion of the garden as "perhaps the oldest example" of the species of heterotopia he terms the 'contradictory site'. 'Heterotopia' is Foucault's term for sites which exhibit non-hegemonic conditions, i.e. which lack the uniform order of a utopia or dystopia and are instead characterised by the coexistence and interaction of different orders and beings. A 'contradictory site' is a

¹⁶⁵ Bruno Latour, *We Have Never been Modern*, Catherine Porter (Trans.), Harvard University Press, 1993, pp. 1-48.

heterotopia which “juxtapos[es] in a single real place several spaces, several sites that are in themselves incompatible”. Foucault here cites the traditional Persian design of the garden as a sacred rectangular enclosure that is at once “the smallest parcel of the world” and also stands in for the four quarters of the world.¹⁶⁶

By emphasising that the garden can be both a self-contained fragment of the world and stand in for the world beyond, Foucault here points to the simultaneous closure and openness of gardens (and also enclosed fields). Their openness is exploited by commensal species to move from and between wild and domestic but their enclosure at the same time keeps out much of wild nature beyond. The extent of openness and closure is also something humans can manipulate – a garden can be allowed to 'run wild', even 'return to nature', but it can also be rigorously ordered and maintained through effort and design. The garden is therefore a bounded space that is at least partially open to the wild but the extent of this openness can be increased or diminished depending on the intensity of domesticating practices within it. This partial exposure to the wild can be conceived in terms of gardens as lying along the domestic's frontier with nature. The frontier, as Sarah Franklin notes, is “a familiar but strange concept” encompassing in it both the character of being a border between states and also “open-ended, unknown terrain”. The frontier, in other words, is both a barrier to movement from one disciplinary regime to another, representing as it does the transition into an often hostile world of different rules and actors, and an opening to possible freedom from the rules and relations of one's home sphere. Crossing frontiers therefore offers both adversity and opportunity. Crossing the domestic-wild frontier is, for example, fatal to many animals and plants, especially those which are either seen as pests or as food. But those which survive culling and predation can do very well in this particular niche. The dual open/closed nature of the frontier is, I maintain, related to its heterogeneity. That the edge of frontiers are heterogeneous regions is noted by Franklin, who describes them as “zone[s] of hybridity, in which settlement and domestication are entangled with the unknown or wild elements—a mix of agencies, entities, and forces”.¹⁶⁷ It is this heterotopic entanglement which allows wild organisms to enter frontier spaces and transition into the domestic, for there is enough wildness in these hybrid spaces for wild organisms to survive but enough also of the domestic

166 Michel Foucault, 'Of Other Spaces: Utopias and Heterotopias', Jay Miskowiec (Trans.), *Diacritics*, Vol. 16, No. 1, 1986, pp. 22-27.

167 Sarah Franklin, *Biological Relatives: IVF, Stem Cells, and the Future of Kinship*, Duke University Press, 2013, pp. 259.

to allow for familiarisation, adaptation (e.g. the loss of fear of humans) and enticement deeper into the domestic zone by resource opportunities.

Returning now to consideration of the early 17th century, was this hybridity of gardens and fields properly recognised at this time? The idea of a stark divide between nature and society that Latour sees as characterising the modern mindset, and which is pre-requisite for recognising hybridity, certainly did not exist in the same form in this period. This is not least due to the subsequent distancing of the domestic from the wild in Western society effected through the advance of technologies and practices of domestication, which has sharpened the perceived divide between art and nature, technology and biology, town and country, etc.¹⁶⁸ In a pre-industrial age in which human technical power over nature was not as advanced, the wild appeared closer at hand and the domestic more fragile to incursions from without. The perceived boundary between domestic and wild was therefore fuzzier and more mobile. But the differing perception of this boundary was also conditioned by a difference in worldview. Contemporary Christian theology believed the human world and nature as principally interacting not through material relations but rather through spiritual and symbolic ones. Animals and plants were embodiments and exemplars of human virtues and vices, created by God to reinforce in nature the messages of correct and false conduct found in the Bible. This allegorical value of nature had been celebrated in medieval bestiaries, which claimed, for example, that the burden-carrying camel “signifies the humility of Christ, who bears all our sins” whereas the 'blind' and burrowing mole was “the symbol of heretics and false Christians who... lack the light of true knowledge and devote themselves to earthly deeds”.¹⁶⁹ Early modern folk beliefs and superstitions were also based on a supposition that nature was a mirror of the human world, in which portents of human events and fortunes could be read. The two worlds were connected by analogical and symbolic relations of sympathy and antipathy, systems of resemblance Foucault has described as “the prose of the world”.¹⁷⁰

Whilst wild nature and organisms could offer good fortune or moral lessons, more often it was seen as a source or sign of physical or moral danger. The invasion of towns by wild

168 Keith Thomas, *Man and the Natural World: Changing Attitudes in England 1500-1800*, Allen Lane, 1983, pp. 242-286.

169 Richard Barber (Trans.), *Bestiary: Being an English Version of the Bodleian Library, Oxford M.S. Bodley 764*, The Folio Society, 1992, pp. 96-111.

170 Michel Foucault, *The Order of Things: An Archaeology of the Human Sciences*, Routledge Classics, 2002, pp. 19-33.

birds, for instance, was seen as a sign of ill omen, and the bringing of certain wild plants into households was thought a source of mortal danger. Both superstitions reflect fear of the incursion of a chaotic nature into domestic order.¹⁷¹ As William Cronon reminds us, wild nature was originally regarded as Satan's Home, a place of adversity and exile, long before late 18th century romantics re-conceived it as God's Temple.¹⁷² Christian orthodoxy, after all, held that whilst Eden, the original Garden, had been a place of harmony, nature had since fallen into corruption as a consequence of human sin. Nature was not alone in being tarnished, for the battle to tame and purify the animal ferocity and elemental hostility of the wild was for Christians a mere external manifestation of a more fundamental interior conflict, that of taming sinful urges and purifying the soul. The domestic and wild, society and nature, were therefore connected in that they were both arenas for the battle between God and Satan.

From what I have discussed, it can be seen that the modern idea of a distinct nature and society was difficult to fit into a Christian and folk religious worldview wherein both wild nature and human society were thought corrupted by sin and both were considered connected by symbols and analogies built into the world by God. If a 'pure' nature or society could exist, it would not be concurrently – a pure nature had existed in Eden at the beginning of time and a pure society would exist in Jerusalem at its end, but through the course of history the external vicissitudes of nature would be thoroughly entangled with the internal struggles of the human soul between the rule of God and Satan. It would require the stripping back of this system of symbolic associations and the development of the idea of nature and society as independent orders in order for the domestic-wild divide to harden. This would begin to happen in the 17th century as the old scholastic natural history, grounded in reverence for Biblical and classical text, came into increasing disfavour among those naturalists favouring alternative forms of investigation into nature. Francis Bacon would be among those leading calls for renewed naturalistic enquiry, and as I will show the garden and field would prove key sites in his philosophical programme for scientific knowledge production.

171 Keith Thomas, *Man and the Natural World: Changing Attitudes in England 1500-1800*, Allen Lane, 1983, pp. 56-57 & 73-81.

172 William Cronon, 'The Trouble with Wilderness; or, getting Back to the Wrong Nature', In: *Uncommon Ground: Rethinking the Human Place in Nature*, William Cronon (Ed.), New York: W. W. Norton & Co., 1995, pp. 71-72.

Francis Bacon in the Garden

Francis Bacon was a great enthusiast of gardens. He inherited from his father Nicholas both an extensive garden at Gorhambury House extending to nearly twenty seven acres and a passion for garden design. The latter he would put into practice at neighbouring Verulam House, where he designed a renowned water garden in 1608.¹⁷³ Bacon furthermore later addressed garden design theoretically in his 1625 essay 'Of Gardens'. This was a companion to his piece on architecture, 'Of Building', and the two essays can together be interpreted as Bacon's manifesto on the ideal aristocratic *domus*.¹⁷⁴ One use of formal gardens was as a form of conspicuous consumption allowing aristocrats to show off the resources at their disposal, and this was likely one of the purposes of Bacon's gardens. This use of gardens for display is apparent in his description of their offering 'embellishment' to the "gross handy-works" of buildings. But Bacon also believed his gardens to serve a higher purpose, namely that of providing "refreshment to the spirits of man". This refreshment was for Bacon at once aesthetic, therapeutic and moral. The production of refreshment was dependent on the judicious design of a garden and appropriate maintenance. Bacon speaks of how through a "royal ordering" there may be gardens "for all the months in the year, in which severally things of beauty may be then in season". This ordering consisted in the prudent selection and planting of species with differing seasons of flowering and fruiting so as to ensure that at every time of the year there were some plants perfuming and prettifying the garden. Bacon similarly saw judicious design and domestic maintenance as a means to prevent ugliness, repulsion and disease. He insists, for instance, that aviaries "have living plants and bushes set in them... [so] that no foulness appear in the floor of the aviary", and lambasts pools as "full of flies and frogs" and so making gardens "unwholesome". Fountains, he asserts, "are a great beauty and refreshment", but must be designed so that their water "be never by rest discoloured, green or red or the like, or gather any mossiness or putrefaction". He further recommended they "be cleaned every day by hand".¹⁷⁵ Bacon's horror of the unsightly and unwholesome was not just aesthetic – it was widely believed in his time that dirt and bad air

173 Hassell Smith, 'The gardens of Sir Nicholas and Sir Francis Bacon: an enigma resolved and a mind explored', In: *Religion, Culture and Society in Early Modern Britain: Essays in Honour of Patrick Collinson*, Anthony Fletcher and Peter Roberts (Eds.), Cambridge University Press, 1994, pp. 125-139.

174 Paula Henderson, 'Sir Francis Bacon's Essay 'Of Gardens' in Context', *Garden History*, Vol. 36, No. 1, 2008, p. 64.

175 Francis Bacon, 'Of Gardens', *The Essayes or Councils, Civill and Morall*, London: John Haviland, 1625, pp. 266-279.

were responsible for sickness.¹⁷⁶ But we can also make sense of Bacon's attitude to these forms of contamination and corruption in terms of his treating them as manifestations of disorder (see my earlier discussion of Douglas' analysis of 'dirt'), as a threatening incursion by the wild into the sanctuary of the *domus*. I suggest Bacon's attempts to control ugliness and decay can therefore be interpreted as an effort to keep out or contain the wild.

However, Bacon was not wholly opposed to wildness in his garden. This can be seen in *Of Gardens*, where he asserts that one particular area of his ideal garden, the heath or 'desert', is "to be framed, as much as may be, to a natural wildness".¹⁷⁷ This suggests that whilst Bacon wanted to keep out those elements of the wild deleterious to the domestic order of the garden, he also saw there to be aspects of the wild that were useful and/or delightful, and which therefore he sought to safely incorporate into his horticultural idyll. This duality of the Baconian garden, which is both a space for disciplining nature into domestic usefulness and for cultivating desirable wild aspects, suggests that he recognised something of the hybrid nature of gardens as sites lying on the frontier of wild and domestic, with the role of the human gardener being to monitor and control traffic between these two realms through the greater or lesser imposition of practices of opening and closure. But it is perhaps worthwhile not getting too carried away with the claim of Bacon as a friend of the wild. Whilst less rigorously disciplined than the rest of the garden, the heath was still a purposely planted patch sown with domesticated species of flower, berry and herb. It was not created to be admired at a distance, and otherwise left to its own devices, but rather developed to produce saleable goods and thus serve the direct economic interests of the master of the *domus*. Indeed, Bacon is recorded in 1608 as ascertaining that the sale of rose petals, fruit and herbs harvested from his Gorhambury heath would prove a useful source of revenue for helping alleviate his long-term money problems.¹⁷⁸ Furthermore, whatever the extent to which Bacon aspired to emulate the wild in his garden, he continued to desire strict control over it, and the end result was a rather tame wildness, one whose bushes were "kept with cutting, that they grow not out of

176 Richard Serjeantson, 'Natural Knowledge in the *New Atlantis*', In: *Francis Bacon's New Atlantis: New Interdisciplinary Essays*, Bronwen Price (Ed.), Manchester and New York: Manchester University Press, 2002, p. 91.

177 Francis Bacon, 'Of Gardens', *The Essayes or Counsells, Civill and Morall*, London: John Haviland, 1625, p. 267.

178 Hassell Smith, 'The gardens of Sir Nicholas and Sir Francis Bacon: an enigma resolved and a mind explored', In: *Religion, Culture and Society in Early Modern Britain: Essays in Honour of Patrick Collinson*, Anthony Fletcher and Peter Roberts (Eds.), Cambridge University Press, 1994, p. 139.

course".¹⁷⁹ The heath can therefore in a sense be seen as something of a 'folly' version of wildness, with the appearance of free-ranging growth but any real possibility of disorder being constrained to protect the overall orderly beauty, utility and moral economy of the garden.

Bacon's ambivalent attitude to wildness in his garden can be traced to the placing of gardens within his broader philosophical and religious worldview. As I have already discussed, gardens have a long history of symbolic importance in the Abrahamic religions as metonymic stand-ins for a lost Paradise. Because gardens were believed to have this residual connection to Eden, they were perceived as sites of special access to the divine. Bacon consciously refers to this idea of gardens as containing something of Eden when he begins *Of Gardens* by stating that "God Almighty first planted a garden", implicitly linking the refreshment offered by gardens with the spiritual renewal engendered by divine contact.¹⁸⁰ But Bacon's reverence of gardens likely also had roots in his family as well as his religion. His father Nicholas had been a great believer in the spiritually edifying power of gardens, and it was he who originally developed Gorhambury's grounds. Nicholas' fancy for garden design stemmed particularly from an interest in ancient pagan philosophy, especially the writings of Stoics such as Seneca the Younger. Seneca had argued for the garden as not only a means to self-sufficiency but as also a place where one could live virtuously according to God's law as embodied in the natural order. Hassell Smith sees Nicholas' Gorhambury as a manifestation of his Christianised Neo-Stoicism, as is visible in the integration of gardening, horticulture and agriculture so as to offer the full range of activities required for Stoic intellectual, moral and physical refreshment.¹⁸¹ Father and son thus shared a common interest in garden design and horticulture. Both moreover appear to have agreed that the *general* end of gardening was to promote beauty, health and moral order. There were, however, significant differences of opinion between Nicholas and Francis as to the *specific* purposes of gardens. For Nicholas the Neo-Stoic, the gardener attained the aesthetic, therapeutic and moral ends of their activity through obedience to nature, and thus obedience to God. This attitude was grounded in the Stoic belief in nature as created in perfect harmony with human needs, with the attendant

179 Francis Bacon, 'Of Gardens', *The Essayes or Counsells, Civill and Morall*, London: John Haviand, 1625, p. 277.

180 Ibid., p. 266.

181 Hassell Smith, 'The gardens of Sir Nicholas and Sir Francis Bacon: an enigma resolved and a mind explored', In: *Religion, Culture and Society in Early Modern Britain: Essays in Honour of Patrick Collinson*, Anthony Fletcher and Peter Roberts (Eds.), Cambridge University Press, 1994, p. 151.

claim that it was only failure to live according to nature due to human delusion and greed which caused it to rebel against human domestication.¹⁸² Francis, on the other hand, attributed such “reluctation”, as he termed it, to the effects of the Fall and did not think merely following nature would better humanity's lot. Instead, he believed scientific knowledge and artifice must be utilised in order to not only understand nature but also to manipulate it and exploit it for the ends of human empire. It was this active technical manipulation of natural law, not mere passive obedience, which would best secure human happiness and prosperity. Nature, Francis insisted, was to be obeyed only so that it could be commanded.¹⁸³

Francis Bacon's differing attitude towards reverence for nature is best explained in terms of his beliefs concerning human dominion (I here base my account largely on Steven Matthews' excellent analysis of Bacon's theological ideas). Like most Western Christians, he believed that the 'reluctation' of nature (its resistance to human efforts to control it) was part of the punishment dealt humankind as a result of original sin. But whereas orthodox views of sin and sacred history professed that the full obedience of nature would only be restored with the full restoration of grace at the end of time, Bacon instead maintained the unusual belief that just as Christians could work their way back towards grace through faith and piety, so natural philosophers could work towards restoring nature's obedience through knowledge and art. As part of this claim, he asserted that just as Adam had possessed full grace in the Garden, so he had also possessed all the requisite knowledge to dominate nature. This was in keeping with humanity's original ascribed role in the garden, namely to mediate between God and his creation through contemplating nature and experimentally intervening in it. Humankind, in other words, was created as a means of God's engagement and apprehension of his own creation – Bacon speaks of God as having “framed the mind of man as a glass capable of the image of the universal world, joyning to receive the signature thereof as the eye is of light” – and were granted the necessary art and wisdom to pursue this role. Adam was in Bacon's view the first natural philosopher, and his work was that of knowledge production. In particular Bacon comments that Adam's first act performed in Paradise “consisted of the two summary parts of knowledge: the view of creatures, and the imposition of names”. Through observation, Adam determined the proclivities of creatures and by naming them identified

182 Dirk Baltzly, "Stoicism", *The Stanford Encyclopedia of Philosophy* (Spring 2014 Edition), Edward N. Zalta (ed.), URL = <<http://plato.stanford.edu/archives/spr2014/entries/stoicism/>>.

183 Steven Matthews, *Theology and Science in the Thought of Francis Bacon*, Great Britain: Ashgate, 2008, pp. 57-58; & Peter McLaughlin, 'Mechanism and the Clockwork Universe' [Draft], 2010, p. 11.

and assigned them their place in nature's hierarchy. This original identification and assignment was perfect due to Adam's perfect knowledge of creation given by God. What was lost in the Fall was not only nature's submission to human dominion, which necessitated that humankind labour for its existence, but also this original perfect knowledge, without which human ability to exercise dominion over nature was also much reduced.¹⁸⁴

The idea of Adam as a working natural philosopher, whilst perhaps seeming odd to us contemporaries, influenced as we are by later Enlightenment discourses which equated Paradise with the primitive, e.g. Rousseau's interpretation of the State of Nature, had precedent in the writings of Church fathers such as Basil the Great and Gregory of Nyssa, for whom the designated role of humanity was to behold, comprehend and help order God's creation. Where Bacon proved more heterodox was in his espousing the idea that the knowledge lost in the Fall could be recovered and used to restore human dominion. This contravened the common feeling that humankind and nature had continued to decline after the Fall (as evidenced, for instance, by the decreasing longevity of humankind after Methuselah), and that grace and dominion could only be restored through faith and the intervention of God through the medium of Christ. There was also the issue of knowledge being thought more part of the problem than the solution, since the Fall was often attributed to human acquisition of knowledge of good and evil through the eating of forbidden fruit. The two claims that only faith and divine aid could reverse the Fall and that original sin had principally consisted in an over-reaching for knowledge were moreover both endorsed by a majority of contemporary English divines, especially those of a Calvinist stripe. Bacon, who had himself grown up under his mother's Puritanical Calvinism, had in his 20s moved towards less radical forms of English Protestantism that maintained a more conciliatory attitude towards Church tradition. He particularly objected to Calvin's doctrines of predestination and total depravity, which maintained that after the Fall humanity no longer possessed free will and was incapable of incorrupt thought and knowledge. Against these doctrines, which precluded the possibility of naturalistic knowledge being used to restore human dominion, Bacon employed the writings of Church Fathers such as Augustine and Irenaeus in order to support his claim that humans had free will and could therefore re-approach God through faith and reason. He furthermore argued that the original sin had not

¹⁸⁴ Steven Matthews, *Theology and Science in the Thought of Francis Bacon*, Great Britain: Ashgate, 2008, pp. 58-63.

been desire for knowledge but prideful disobedience of God. In this he followed Lancelot Andrewes, a proto-High Anglican theologian and friend of Bacon, the two of whom Matthews has shown to have undoubtedly mutually influenced each other. Andrewes, like Bacon, maintained that Adam had been given all requisite natural knowledge in the beginning and therefore did not lack knowledge of good and evil, but that Satan had deceived Eve and himself into thinking that in forbidding them the fruit of the Tree of Knowledge God was hiding additional knowledge from them, and that it was their hubris in disobeying this divine dictat which saw them expelled from the Garden and this original knowledge lost.¹⁸⁵

In place of pessimistic narratives of decline, Bacon presented his own optimistic restoration myth. As mentioned, the traditional Christian 'Restoration Narrative' (as Merchant refers to such Western cultural myths) stressed that only a combination of human faith and divine providence could restore humanity to an Edenic state (domesticating practices, whilst praised as restoring nature to its proper place, were thought incapable of undoing the damage of the Fall).¹⁸⁶ Bacon challenged this by claiming that knowledge was a necessary accompaniment to faith, for it was only through knowledge that Adam's dominion over nature could be recovered. He for instance comments in *Novum Organum* that “man by the fall fell at the same time from his state of innocency and from his dominion over creation. Both of these losses however can even in this life be in some part repaired; the former by religion and faith, the latter by arts and sciences”. Bacon furthermore maintained that God had ordained the means so that humankind could re-attain its lost dominion, and that the time for such a recovery was now at hand. To evidence the first claim, he pointed to Genesis 3:19 – “In the sweat of thy face shalt thou eat bread”. Whilst ordinarily interpreted as part of the curse of the Fall, Bacon alternatively suggested that this passage's meaning was that “creation was not by the curse made altogether and forever a rebel” but rather that God had granted as a 'charter' the provision that humankind could better their existence and proximity to the divine through the twin means of religious faith and scientific art. To back his assertion that the restoration of Adam's knowledge was imminent, Bacon interpreted Daniel 12:4 – “Many shall go to and fro, and knowledge shall be increased” – as a prophecy establishing that towards the end of the secular age there would be a great growth in scientific knowledge, and he moreover

185 Steven Matthews, *Theology and Science in the Thought of Francis Bacon*, Great Britain: Ashgate, 2008, pp. 61-8.

186 Carolyn Merchant, *Reinventing Eden: The Fate of Nature in Western Culture*, New York/London: Routledge, 2003, pp. 10-61.

inferred that the advent of the Age of European Exploration had signalled the beginning of this process. Bacon can here be seen as reinterpreting existing millenarian Protestant ideas that the Reformation signalled the beginning of the sacred millennium of saintly rule on Earth prophesised in Revelations in order to claim the coming of a similarly prefigured sacred millennium of scientific knowledge. He believed that for this millennium to come to pass would require a grand reform of education and scientific learning, an *Instauratio Magna*, or 'Great Instauration'. Through rejecting unworldly scholasticism and undirected fact-collecting in favour of a hybrid and rigorously inductive natural philosophy, Bacon believed human dominion could be reattained and humanity returned to, or even possibly made to surpass, its prelapsarian exalted state.¹⁸⁷

Evidence of how Bacon believed the arts and science could help restore human dominion is found in his depiction of the scientific utopia of Bensalem in his late work *The New Atlantis*. The guide to Salomon's House describes how:

“We have... large and various orchards and gardens... And we make (by art) in the same orchards and gardens trees and flowers to come earlier or later than their seasons, and to come up and bear more speedily than by their natural course they do. We make them also by art greater much than their nature, and their fruit greater and sweeter and of differing taste, smell, colour and figure, from their nature... We have also parks and enclosures of all sorts of beasts and birds... By art likewise we make them greater or taller than their kind is, and contrariwise dwarf them, and stay their growth; we make them more fruitful and bearing than their kind is, and contrariwise barren and not generative. Also we make them differ in colour, shape, activity, many ways”.¹⁸⁸

Scientific knowledge and art is here shown as restoring dominion by allowing for greater control and modification of useful animals and plants. Enclosed gardens and fields operate as a principal site for such artful interventions and experiments. The major difference between Bensalem and Bacon's own society was not that different kinds of site or organism were

187 Steven Matthews, *Theology and Science in the Thought of Francis Bacon*, Great Britain: Ashgate, 2008, pp. 1-99.

188 Francis Bacon, 'The New Atlantis', In: *The Works of Lord Bacon: Vol. I*, London: William Ball, 1838, pp. 213.

incorporated into domesticating knowledge/power regimes but principally that greater knowledge of artifice allowed for an intensification of domesticating practices to allow for the fine-tuned manipulation of nature and “the relief of man’s estate”.¹⁸⁹ Whilst the fulfilment of much of Bensalem's promise awaited scientific advances that would yet be centuries in the making, the basic foundation for its establishment would be already existing gardening and farming design and practice. What the new inductive natural philosophy would allow, Bacon hoped, was the development of knowledge and practices that would enable a tightening of disciplinary controls on nature, which he believed could counteract the 'reluctation' of a natural world rendered rebellious by the corruption of sin.¹⁹⁰

Bacon's Influence on Conceptions of Domestication: Long and Short-Term

Overall, what is distinctive in Bacon's natural philosophy is this idea of the improvement of nature through rationalisation, control and intensification via the means of expanding and systematising useful naturalistic knowledge. This notion is not found in traditional Christian recovery narratives, which stressed that only providence could directly better the lot of the faithful as only God could undo the wilding of nature and the fall from grace. Against this, Bacon maintained that God had provided the means for humanity to help raise itself back to an uplifted state. This would require the practical development and application of human knowledge and art towards the end of subduing and domesticating nature's wildness. Rather than await Christ's return to see nature made to bend to human need, an experimental natural philosophy would enable humankind to accommodate nature to its requirements without direct divine assistance. This innovative element of Bacon's philosophy has not always been appreciated, perhaps because, unlike his illustrious contemporaries Galileo and Descartes, Bacon did not make any significant scientific discoveries or inventions. He has subsequently been portrayed more as propagandist than direct contributor to the sciences, and his philosophical contributions have been downplayed by such influential historians as Alexander Koyré and Thomas Kuhn.¹⁹¹ Where Bacon has generally received credit has been for his call for natural philosophers to collaborate in order to advance knowledge and human

189 Carolyn Merchant, *Reinventing Eden: The Fate of Nature in Western Culture*, New York/London: Routledge, 2003, p. 72.

190 Steven Matthews, *Theology and Science in the Thought of Francis Bacon*, Great Britain: Ashgate, 2008, p. 61.

191 John Losee, *A Historical Introduction to the Philosophy of Science*, 3rd Ed., Oxford/New York: Oxford University Press, 1993, pp. 64-65; & Thomas, Kuhn, *The Structure of Scientific Revolutions*, Chicago and London: The University of Chicago Press, 1996, p. 16.

empire, as opposed to practising the petty one-upmanship of traditional scholars. This idea of large-scale collaboration, as modelled on the governing scientific society of Bensalem, is broadly recognised as relatively novel and as a founding inspiration for the Royal Society and other early modern European scientific circles.¹⁹²

Bacon's contribution to philosophy of domestication has been less recognised in general history of science, but has received considerable negative attention from those philosophers and historians critical of modernity and its treatment of nature. Most notably, the ecofeminist Carolyn Merchant presents Bacon in her 1980 *Death of Nature* as having “fashioned a new ethic sanctioning the exploitation of nature”, asserting that he “treats nature as a female to be tortured through mechanical inventions, strongly suggest[ing] the interrogations of the witch trials and the mechanical devices used to torture witches”. She here points to a statement where Bacon approvingly compares the enquiry into nature's secrets with his master James I's interrogation of witches, particularly noting the ugly sexual connotations of a passage where Bacon states that a man should not “make scruple of entering and penetrating into these holes and corners, when the inquisition of truth is his whole objective”.¹⁹³ From this and other passages, Merchant concludes that Bacon's epistemic project constitutes a rape of a female nature for the purpose of advancing male knowledge. She sees the production of “docile” new domestic forms of beast and bud at Bensalem as part of this assault, their creation being based on the brute reordering of matter to create a subservient, artificial and ultimately dead nature.¹⁹⁴ The natural world is not the only victim of this domestication by brute force – women and the lower classes are also in Bensalem forced to conform to the domesticating regime of the House of Salomon, without significantly sharing in its fruits.¹⁹⁵

Merchant offers necessary qualification to the supposed universal aims of the Baconian project of scientific domestication, namely by showing that what was purported to be for the

192 Edgar Zilsel, 'The Sociological Roots of Science', *Social Studies of Science*, Vol. 30, No. 6, 2000, pp. 944-945; Lorraine Daston and Katherine Park, *Wonders and the Order of Nature: 1150-1750*, New York: Zone Books, 1998, pp. 220-231; & Steven Shapin, *Never Pure: Historical Studies of Science as if It Was Produced by People with Bodies, Situated in Time, Space, Culture, and Society, and Struggling for Credibility and Authority*, John Hopkins University Press, 2010, pp. 160-164.

193 Carolyn Merchant, *The Death of Nature: Women, Ecology and the Scientific Revolution*, HarperOne (Reprint), 1990, pp. 164-168.

194 Carolyn Merchant, *Reinventing Eden: The Fate of Nature in Western Culture*, New York/London: Routledge, 2003, p. 72.

195 Carolyn Merchant, *The Death of Nature: Women, Ecology and the Scientific Revolution*, HarperOne (Reprint), 1990, pp. 172-178.

advancement of human empire was too often skewed towards the advancement of Protestant male mercantile and aristocratic interests. She is also persuasive in her argument that Bacon's instrumentalism at times encouraged a domineering and destructive attitude towards wild nature. The more general claim for the knowledge regime of scientific domestication as animated by a Baconian spirit of hostile domination towards nature is, however, more difficult to sustain. This is not least because Bacon does not always speak of the natural philosopher's relationship with nature as one of domination. He for instance argues for the need to establish "a chaste and lawful marriage between Mind and Nature" and describes man as "but the servant and interpreter of nature". Science must therefore be responsive to and engage with nature as well as seek to control it. Evelyn Fox Keller interprets this as evidence in Bacon's thought of a "sexual dialectic" between aggressive and erotic science, rape and seductive conquest.¹⁹⁶ She does not see this dialectic as peculiar to Bacon, asserting that the differences between ideas of science as "dominating" and as "conversing with" nature are not primarily differences between epochs or the sexes, but rather represent "a dual theme played out in the work of *all scientists, in all ages*" (emphasis mine). Keller regards Bacon's ideological contributions to science as contributing to dominating tendencies, but she believes "neither science nor individuals are totally bound by ideology", finding the history of science characterised by "thematic pluralism". It is notable that her favourite example of a scientist engaged in a conversant approach to nature is the maize geneticist Barbara McClintock, i.e. a researcher working in a field of stringent scientific domestication, who speaks of a need in her work for "a feeling for the organism".¹⁹⁷ This acknowledgement of the openness of scientific domestication to conversational and symbiotic approaches to nature better fits my own impression that knowledge production under scientific domestication is broadly characterised by co-construction than does Merchant's claim that Bacon's 'new ethic of exploitation' initiated a unswerving turn towards domineering and destructive scientific approaches to nature. I furthermore believe the knowledge regime of scientific domestication too heterogeneous in its range of contributing historical practices, processes and discourses for its trajectory to be deduced by merely tracking the influence of the Baconian recovery narrative, which whilst influential has been just one of many threads. I therefore conclude that whatever Bacon's ideological influence on attitudes to nature (which was not

196 Evelyn Fox Keller, *Reflections on Gender and Science: Tenth Anniversary Edition*, Yale University Press, 1995, pp. 33-42.

197 Evelyn Fox Keller, 'Feminism and Science', *Signs*, Vol. 7, No. 3, 1982, pp. 599-602.

inconsiderable), it has certainly not been enough to foreclose the co-construction of naturalistic knowledge through sympathetic and symbiotic approaches to non-human organisms under scientific domestication.

Bacon's more proximate influence on 17th century thinking concerning domestication has been less appreciated than his contribution to medium and longer term trends. This is in part a result of historians of early modern English science concentrating on achievements in the physical and mathematical sciences, especially those attained after the founding of the Royal Society in 1660 by members such as Boyle, Hooke and Newton (as Müller-Wille notes “It is the mathematical and experimental sciences, rather than natural history, that are usually seen in connection with Francis Bacon’s methodological reform of science, in particular, and the Scientific Revolution in general”).¹⁹⁸ But a survey of one of the Royal Society's precursors, the Hartlib Circle, reveals a widespread and explicitly Baconian interest in improved technologies of domestication and rationalisation of current practice in both Britain and Continental Europe. The Circle was an international correspondence network established by the London-based German intelligencer Samuel Hartlib for the purposes of promoting scientific enquiry, Protestant religion and educational reform.¹⁹⁹ Hartlib himself had many interests but one of the most prominent was agricultural reform, and in this he appears to have been directly inspired by the Baconian utopia of Bensalem. Scientific knowledge would, Hartlib believed, unlock the inherent wealth of nature set there by providence. He for instance asserts that “our *Native Countrey*, hath in its bowels an (even almost) infinite, and inexhaustible treasure”, which required only ingenuity, industry and God's benevolent will to uncover.²⁰⁰ Among his more concrete proposals, Hartlib forwarded that exhausted soils could be enriched without need of manure by determining which grasses best helped barren land recover and sowing them, a project he thought would be aided by the importation of new species from abroad, especially from the New World. He furthermore believed that English agriculture could be improved through learning from the practices of other cultures, and to

198 Staffan Müller-Wille, 'History Redoubled: The Synthesis of Facts in Linnaean Natural History', *Philosophies of Technology: Francis Bacon and his Contemporaries*, Claus Zittel, Gisela Engel, Romano Nanni and Nicole C. Karafyllis (Eds.), Leiden/Boston: Brill, 2008, p. 515.

199 Oana Matei, 'Gabriel Plattes, Hartlib Circle and the Interest for Husbandry in the Seventeenth Century England', *Prolegomena*, Vol. 11, No. 2 2012, pp. 209-210.

200 Carl Wennerlind, 'Money: Hartlibian Political Economy and the New Culture of Credit', In: *Mercantilism Reimagined: Political Economy in Early Modern Britain and its Empire*, Philip J. Stern & Carl Wennerlind (Eds.), Oxford University Press, 2014, p. 78.

this end implored “Ingenious Gentlemen and Merchants, who travel beyond the Sea, to take notice of the Husbandry of those parts”.²⁰¹ Hartlib also from his own observations recommended that English farmers move from the use of generalist cattle stock to the model of specialist dairy and beef stock predominant in the Netherlands.²⁰² Hartlib was not alone in this Baconian enthusiasm for improvement, or in his belief in the providential restoration of human mastery of nature's spoils. His colleague Gabriel Plattes echoed Bacon's restoration narrative when he spoke of making “this Countrey the Paradise of the World”, and another Hartlibian, William Petty, asserted England to be underpopulated, and that with the appropriate reform of husbandry (such as the enclosure of common land) it could support twice its current populace.²⁰³ Other Harlibians, such as John Beale and Ralph Austen, wrote works on such matters of domestication as improving orchard care, forestry and beekeeping.²⁰⁴

This wealth of interest shows us that not only were Bacon's proposals concerning the intensification of practices of domestication widely and well received, but also that there were early serious and programmatic attempts to emulate the ideal of Bensalem established in the *New Atlantis*. These early efforts at improvement have not received anything of the attention given to the similar efforts a century later of improvers such as Thomas Coke, Arthur Young, William Marshall and Robert Bakewell. In part this may be put down to the failure of most Hartlibian improvers to move beyond words and attain permanent achievements in the field. But their efforts have also been overshadowed due to the early 20th century identification of the mid-18th century as the beginning of an 'Agricultural Revolution', with the aforementioned improvers being depicted as the “Great Men” responsible by such historians as Lord Ernle and W.H.R. Curtler. Subsequent scholarship has shown that this declaration of a revolutionary break in British agriculture is over-egged, with practices such

201 Fredrik Albritton Jonsson, 'Natural History and Improvement: The Case of Tobacco', In: *Mercantilism Reimagined: Political Economy in Early Modern Britain and its Empire*, Philip J. Stern & Carl Wennerlind (Eds.), Oxford University Press, 2014, p. 120.

202 Nicholas Russell, *Like Engend'ring Like: Heredity and Animal Breeding in Early Modern England*, Cambridge University Press, 1986, p. 132.

203 Fredrik Albritton Jonsson, 'Natural History and Improvement: The Case of Tobacco', In: *Mercantilism Reimagined: Political Economy in Early Modern Britain and its Empire*, Philip J. Stern & Carl Wennerlind (Eds.), Oxford University Press, 2014, p. 120.; & David McNally, *Political Economy and the Rise of Capitalism: A Reinterpretation*, Berkeley/London: The University of California Press, 1988, p. 48.

204 Joan Thirsk, 'Agricultural Innovations and their Diffusion', In: *Chapters from the Agrarian History of England and Wales: Volume 3, Agricultural Change: Policy and Practice, 1500-1750*, Joan Thirsk (Ed.), Cambridge University Press, 1990, pp. 263-319.

as enclosure, the use of new forage crops and even selective breeding having strong pre-18th century roots. There was therefore more continuity between mid-17th century Hartlibian Baconianism and the mid-18th century improvement movement than is commonly held.²⁰⁵ I will further demonstrate this continuity by establishing the intellectual debt to Bacon (including a garden-centred practice and pursuit of an Edenic ideal) owed by one such mid-18th century improver, the naturalist Carl Linnaeus. I will also show, however, how Linnaeus reinterpreted Bacon's restorationism in the light of his belief in the autonomy of natural and human economies, in particular his view that the human economy emerged from the natural, and will seek to demonstrate that this demarcation of nature from society and the reinterpretation of domestication as transformative, not restorative, would contribute to the development of the idea of wild types as original, normative units of nature and of domestic varieties as artificial, unstable and inclined to revert.

²⁰⁵ Lord Ernle, *English Farming Past and Present*, 5th Ed., London/New York/Toronto: Longmans, Green and Co., 1936, p. 149; & Mark Overton, *Agricultural Revolution in England: The Transformation of the Agrarian Economy 1500-1850*, Cambridge University Press, 1996, p. 3.

Chapter 4 – Gardens of Knowledge, Economies of the Domestic and Wild:

Linnaeus and the Origins of the Wild Type Concept

Linnaeus as a Baconian.

Francis Bacon spoke of gardens and fields as significant spaces for naturalistic investigation but did not himself significantly develop such spaces into organised sites for scientific knowledge production. But the potential highlighted by Bacon of using such hybrid spaces in order to draw in wild nature and then interrogate it through regimes of disciplining domestication was seen by other natural philosophers, who sought, like the Hartlibians, to realise Bensalem. The naturalist who in many ways would do most to realise the Baconian idea of gardens as sites of scientific domestication would be the 18th century Swedish botanist Carl Linné, better known as Linnaeus. His realisation of the power of scientific domestication to determine plant species would moreover significantly contribute to the idea that the variability of wild species was best understood by bringing them into controlled and uniform conditions, a notion that would set an important precedent for subsequent efforts to understand the nature of variation across the domestic-wild divide.

Linnaeus has not always been recognised as a Baconian, in part due to erroneous characterisations of him by Arthur Cain and Ernst Mayr, who presented him as a latter-day scholastic working from formal logic as opposed to experience. Cain himself came later to realise that Linnaeus was no mere “intellectual caddis-worm, going about in a case of incongruous bits and pieces of philosophy” but rather instead that Linnaeus had a holistic worldview that, whilst derived from various philosophical sources, was ultimately bound together and reinforced by experience.²⁰⁶ Linnaeus' empiricism and his indebtedness to Bacon have both been further established by Staffan Müller-Wille. He points out that not only is Bacon one of only two authorities Linnaeus quotes on scientific method (the other, Herman Boerhaave, was furthermore a well-known Baconian), but moreover that signs of Bacon's influence can be found in Linnaeus' aphoristic writing style and numerous instances of

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Mary Winsor, 'Cain on Linnaeus: The Scientist-Historian as Unanalysed Entity', *Studies in History and Philosophy of Biological and Biomedical Sciences*, Vol. 32, No. 2, 2001, pp. 239-254.

paraphrasing Bacon's words. Furthermore, Linnaeus in 1746 referenced Bacon's account of Adam's naming the animals of Eden, asserting that for such names to be retained required that Adam observe the “distinct characters or traits by which to distinguish species”, requiring he therefore “make zoology his business”. Müller-Wille interprets this statement as evidence of Linnaeus' Baconian empiricism “in that it shows Linnaeus' vision of a natural history proceeding by the observation of individual instances, rather than *a priori* reasoning [as claimed by the younger Cain and Mayr]”.²⁰⁷ Müller-Wille elsewhere speaks of Linnaeus' taxonomic system as “inductive and empirical, although disciplined... by a complex apparatus of methodological, terminological, and nomenclatorial conventions”.²⁰⁸ Linnaeus, in other words, was not a naïve empiricist nor an abstract reasoner, but rather a systematic collector and classifier, and his practice was that of a good Baconian bee, gathering in specimens from far-flung corners of the wild and domestic and analysing and synthesising said specimens both materially under controlled and homogeneous experimental conditions (principally the soil of the botanical garden), as well as abstractly through the use of paper tools such as lists, tables and diagrams.²⁰⁹

The Economy of Nature

Before getting to Linnaeus' Baconian practices of scientific domestication, it is first useful to consider a less observed Baconian influence which significantly shaped Linnaeus' interpretation of the wild and domestic, namely the idea of an 'economy of nature' existing alongside the human economy. Whilst this concept has other older roots (see Egerton's lengthy discussion of the origins of the idea of 'balance' in nature), I believe that a key influence in shaping the version developed by Linnaeus was Francis Bacon's doctrine of God as author of nature “by links and subordinate degrees”.²¹⁰ This was a theodicy developed by Bacon in response to Calvin's doctrine of predestination, which he felt implied God to be responsible for evil in the world. Bacon held instead that whilst God was the original source

207 Staffan Müller-Wille, 'History Redoubled: The Synthesis of Facts in Linnaean Natural History', *Philosophies of Technology: Francis Bacon and his Contemporaries*, Claus Zittel, Gisela Engel, Romano Nanni and Nicole C. Karafyllis (Eds.), Leiden/Boston: Brill, 2008, pp. 519-521.

208 Staffan Müller-Wille, 'Collection and Collation: Theory and Practice of Linnaean Botany', *Studies in History and Philosophy of Biological and Biomedical Sciences*, Vol. 38, No. 3, 2007, p. 559.

209 For details on Linnaeus' paper tools, which I will not cover here in any detail, see: Staffan Müller-Wille and Isabelle Charmantier, 'Natural History and Information overload: The Case of Linnaeus', Vol. 43, No. 1, *Studies in History and Philosophy of Biological and Biomedical Sciences*, 2012, pp. 4-15.

210 Frank N. Egerton, 'Changing Concepts of the Balance of Nature', *The Quarterly Review of Biology*, Vol. 48, No. 2, 1973, pp. 322-350; & Steven Matthews, *Theology and Science in the Thought of Francis Bacon*, Great Britain: Ashgate, 2008, p. 34.

of universal motion, he otherwise manipulated events only through the arrangement of long chains of causation. Whilst the world was ordered in such a way that history would unfold in a divinely designed manner and reach its desired ultimate ends, human beings nevertheless were able to select between proximately good and evil outcomes. Bacon thus claimed that God did not purposefully cause evil and that whilst sacred history was pre-ordained, humans still possessed free will.²¹¹ A further consequence of this view was that, by asserting nature to only be indirectly contingent on God's will, Bacon buttressed his claim that nature had an independent and regular order which could be determined through observation and experiment and manipulated for human benefit. If, on the other hand, nature was directly contingent on God, confirmation of its order would require knowledge of the mind of God. Bacon held such direct knowledge to be impossible, as he thought God transcendent.²¹² Making God an indirect actor in the world was therefore both a theodicy and a guarantor of the possibility of independently attaining knowledge of the world through empirical study.

The treatment of God as transcendent and indirectly maintaining nature (as opposed to the occasionalist belief in God as immanent and directly sustaining the world) would spur the growth of natural theology as an attempt to understand the world in terms of God's indirect governance of it. This governance would be interpreted using metaphors from the administration of earthly states, such as ideas of divine dispensation, natural law and natural economy. 'Economy'/'Oeconomy' is derived from '*oikos*', the Greek equivalent of the Latin '*domus*', and originally referred to household management. During the 16th and 17th centuries, it began to be used to refer to the political administration of the resources of a state, i.e. the societal *domus*. When adopted by natural theology 'economy' came to refer to the wise manner in which God designed the world and matched means to ends.²¹³ This application of the term 'economy' to nature can be interpreted as an attempt to cognitively domesticate the wild by imposing the logic and language of household management upon it, a strategy of domestication by analogy. In particular, by imposing on 'feminine' nature the order of the

211 Jürgen Klein, "Francis Bacon", *The Stanford Encyclopedia of Philosophy* (Summer 2015 Edition), Edward N. Zalta (Ed.), URL= <<http://plato.stanford.edu/archives/sum2015/entries/francis-bacon/>>. ; & Steven Matthews, *Theology and Science in the Thought of Francis Bacon*, Great Britain: Ashgate, 2008, pp. 34-35.

212 Steven Matthews, *Theology and Science in the Thought of Francis Bacon*, Great Britain: Ashgate, 2008, pp. 55-56 & 68.

213 Donald Worster, *Nature's Economy: A History of Ecological Ideas*, Cambridge University Press, 1985, p. 37; & Trevor Pearce, "'A great complication of circumstances' – Darwin and the economy of nature", *Journal of the History of Biology*, Vol. 43, No. 3, p. 497.

domus, its subordination to God was emphasised. As the Father, God occupied a role in the natural economy analogous to the *paterfamilias* within the domestic economy, to whom Nature as wife and mother was subservient. Therefore, even when God acted through nature, nature was not of itself to be credited with true authority and agency. This domesticating metaphor was used to argue against 'animist' beliefs that nature possessed agency and independently perpetuated its own order. This view, termed by Robert Boyle "the vulgar notion of nature", was condemned as propagating the erroneous idea that nature was a 'semi-deity' and collaborator in God's creation.²¹⁴ The use of 'economy', by drawing on the analogy with the order of the *domus*, therefore allowed God to act indirectly in the world without undermining His authority over nature.

The first natural philosopher to speak explicitly of an 'economy of nature' was Kenelm Digby, in whose work the term appears in 1644. Whilst he "cannot properly be called a Baconian in method", Digby was heavily influenced, Betty Jo Dobbs asserts, by Bacon's theories of matter and motion, and it seems likely that the idea of nature as indirectly contingent on God via long causal chains was transmitted as part of this take-up of Baconian physical theory.²¹⁵ This Baconian influence was incorporated into an eclectic mechanistic cosmology that has been termed an "Aristotelian atomism", a synthesis of scholastic metaphysics and the newer systems of Descartes, Hobbes and Gassendi.²¹⁶ Digby therefore retained Aristotle's natural/violent motion distinction whilst simultaneously denying the original basis for this differentiation, namely the idea of natural things as having inherent motion. This left Digby with a problem of "how bodies that of themselves have no propension unto any determinate place do never the less move constantly and perpetually", i.e. how entities might have a natural motion and orderly disposition without having an inherent source of directed motion. Digby believed the resolution to this apparent contradiction lay in understanding "the economy of nature that hath set on foot due and plain causes to produce known effects". He theorised that the cosmic order was sustained by the Sun, established by God as the constant and perpetual engine of universal motion, which through its operations continually powered the movement and natural cycles of the Earth and heavens. It was God's fashioning of the

214 Elizabeth Potter, *Gender and Boyle's Law of Gases*, Indiana University Press, 2001, pp. 124-129.

215 Betty Jo Dobbs, 'Studies in the Natural Philosophy of Sir Kenelm Digby', *Ambix*, Vol. 18, No. 1, 1971, p. 13.

216 Paul S. MacDonald, 'Introduction', *Kenelm Digby's Two Treatises*, Paul S. MacDonald (Ed.). The Gresham Press, 2013, pp. 31-33.

universe in a manner that allowed for perpetual existence and motion without requiring divine intervention which Digby considered the major aspect of nature's economy.²¹⁷

Digby also extended his discussion of nature's economy and God's design to living creatures. Defending Descartes' 'beast-machine', he asserted that whilst the “admirable economy of some living creatures” may lead observers to conclude they possess reason, when their nature and operations was reflected upon it became clear that they were merely “material instruments of a rational agent working through them, from whose orderly prescriptions they have not power to swerve in the least circumstance”. Rather than ascribe agency to nature and natural things, Digby would have us instead “look with reverence and duty upon the immensity of that provident Architect out of whose hands these masterpieces issue”. Digby's Architect, like Bacon's God, operated indirectly in the world through composing and ordering chains of causation. God is for instance stated to make “a chain of a thousand or of a million links” as easily as one alone, and “the whole economy of [God's] actions” is said to consist of “nothing else but a production of material effects by a due ranging and ordering of material causes”.²¹⁸ We thus see here in Digby, as in Bacon, a presentation of God as a transcendent creator operating indirectly in nature through long causal chains.

The idea of a well-ordered cosmos designed by God in such an economic fashion as to run autonomously and indefinitely was a common theme in in 17th and early 18th century natural philosophy. This was commonly conjoined, as seen in Digby, with the belief in a purely mechanical nature lacking agency, with the individual and collective behaviour of non-human organisms dismissed as unthinking and deterministic. This position would be challenged towards the end of the 17th century by John Ray, who instead argued living creatures to contain within them an “intelligent Plastick Nature” that was both responsible for their growth and development and allowed them free agency (see also his previously discussed and related critique of anthropocentrism). Ray derived this notion of 'plastic nature' from the Cambridge Platonist Henry More, who had, against purely mechanical interpretations of nature, proposed that God was assisted in his labours by an *Anima Mundi* or 'world spirit' (More would subsequently be one of the main targets of Boyle's critique of 'animism'). Ray

217 Kenelm Digby, *Kenelm Digby's Two Treatises*, Paul S. MacDonald (Ed.). The Gresham Press, 2013, pp. 135-143.

218 Kenelm Digby, *Kenelm Digby's Two Treatises*, Paul S. MacDonald (Ed.). The Gresham Press, 2013, p. 407.

further developed More's animism in order to argue that whilst nature was an economy divinely designed on mechanistic principles, its creatures nonetheless possessed agency and could therefore proximately influence nature's course.²¹⁹

Ray's model of nature as a mechanistic economy of organisms with agency was likely the major influence for Linnaeus' economy of nature. Like Ray, his “special hero” (Koerner), Linnaeus believed that, unlike brute matter, “Animals move of their own accord, simply by willing”.²²⁰ Because of this belief in animal agency, Linnaeus had little problem in declaring Man too to be “an animal”, and furthermore condemned the Cartesian supposition that the human body is “a little clockwork, a machine”.²²¹ Even plants, which Linnaeus believed to lack sensation, are described in metaphorical terms that suggest liveliness and agency – they “imbibe nourishment through *bibulous* roots, breathe by *quivering* leaves [and] *celebrate* their nuptials in a genial metamorphosis” (emphasis mine).²²² That Linnaeus endorsed Ray's belief in 'plastick nature' should not preclude us from recognising that his model of the natural economy was very much machine-like. The difference is that unlike 17th century mechanists such as Digby he did not model nature on clockwork but instead on a different kind of mechanism, namely the legislative and organisational machinery of the state. Lisbet Rausing asserts that “What we regard as nature’s checks and balances and feedback loops, Linnaeus imagined as the visible and heavy hand of the state”, with the dominant metaphor in both his natural and national economics being that of “state violence” as a means of maintaining order.²²³ This interpretation of 'economy' is in many ways closer to the original political sense than Digby's, in that nature, like society, is conceived of as a collective of agents which it is the role of authority to organise and police. This state-like interpretation of the natural order is reflected in another of Linnaeus' terms for it, namely *politia naturae*, or the 'polity of nature'.

219 Donald Worster, *Nature's Economy: A History of Ecological Ideas*, Cambridge University Press, 1985, pp. 41-43; & John Henry, 'Henry More', *The Stanford Encyclopedia of Philosophy* (Fall 2012 Edition), Edward N. Zalta (ed.), URL = <<http://plato.stanford.edu/archives/fall2012/entries/henry-more/>>.

220 Lisbet Koerner [née Rausing], *Linnaeus: Nature and Nation*, Harvard University Press, 1999, p. 82; Carl Linnaeus, *Nemesis Divina*, M.J. Petry (Ed./Trans.), Dordrecht/Boston/London: Kluwer Academic Publishers, 2001, p. 90.

221 Lisbet Koerner [née Rausing], *Linnaeus: Nature and Nation*, Harvard University Press, 1999, pp. 74-75.

222 Carl Linnaeus, 'System of Nature (1735)', In: *The Enlightenment: A Sourcebook and Reader*, Paul Hyland, Olga Gomez and Francesca Greensides (Eds.), London: Routledge, 2003, p. 104.

223 Lisbet Rausing, 'Underwriting the Oeconomy: Linnaeus on Nature and Mind', *History of Political Economy*, Vol. 35, Annual Supplement, 2003, pp. 186-187.

As part of this notion of the natural economy as a state-like mechanism, Linnaeus embraced an alternative interpretation of the Baconian idea of indirect divine action through long causal chains. He begins by describing the economy of nature as “the all-wise disposition of the Creator in relation to natural things, by which they are fitted to produce general ends, and reciprocal uses”. But he goes on to state that all things in nature are “so connected, so chained together, that they all aim at the same end, *and to this end a vast number of intermediate ends are subservient* [emphasis mine]”.²²⁴ The difference between this natural economy and that of Digby was this necessity of subordinating intermediate ends. Digby, as quoted above, believed non-human creatures mere 'material instruments' lacking in agency, means to the ends of God's creation, not ends in themselves. Linnaeus, on the other hand, recognised not only the agency of non-human creatures but also that the natural economy served their ends as much as it served human ones. Linnaeus was still anthropocentric in much of his basic analysis, asserting that ultimately nature's order was “intended by the Creator for the sake of man”, a common sentiment for his time.²²⁵ But he rejected a linear hierarchy of the subordination of ends, stating that he could discover no reason to suppose that plants were created for the sake of animals or that animal prey had been created for predators' sake. Linnaeus instead counter-intuitively concluded that predation principally served the purposes of plants by maintaining “a due proportion among vegetables”, consuming the superfluous and useless, removing disease and impurities, and helping disseminate seeds.²²⁶ Linnaeus thus recognised both the interdependence of trophic levels for a stable ecology and the cyclical nature of nutritional exchange between them. He stressed that even “Man who turns everything to his needs” could still serve as “food of the beast or bird or fish of prey or of the worm and the earth”.²²⁷

Linnaeus believed that the interconnection and cyclical interdependence of nature was not only arranged to serve the proximate ends of non-human creatures but also helped perpetuate the natural order as a whole through dynamically maintaining equilibrium. As established by Müller-Wille, there are two major dimensions to Linnaeus' economy of nature – the

224 Carl Linnaeus, ‘The Economy of Nature’, In: *Miscellaneous Tracts Relating to Natural History, Husbandry and Physick to which is added the Calendar of Flora*, 2nd Edition, Benjamin Stillingfleet (Ed. & Trans.), London: R. and J. Dodsley/S. Baker/T. Paine, 1762, pp. 39-40.

225 Ibid., p. 121.

226 Carl Linnaeus, ‘On the Police of Nature’, In: *Select Dissertations from the Amœnitates Academicæ, Vol. I*, F.J. Brand (Trans.), London: Robinson & Robson, 1781, pp. 137-138.

227 Margaret Schabas, *The Natural Origins of Economics*, The University of Chicago Press, 2005, p. 30.

continued series and the nexus. The series is the diachronic perpetuation of species through reproduction and the inheritance by offspring of their parents' species-specific hereditary character and ecological niche (what Linnaeus calls an 'office' or 'station'). The nexus, on the other hand, is the synchronic network of ecological relations between species.²²⁸ These relations have been so ordered by God that all species are able to continually maximally flourish within their own office without overexploiting their food sources or impinging on the offices of other species, with 'due proportions' being maintained through a combination of predation, disease and reproductive prudence (e.g. longer-lived organisms propagate more slowly so as not to overwhelm the world with their numbers). The state-like nature of these arrangements is emphasised by Linnaeus' use of analogies of human social hierarchy. Discussing the plant kingdom, he speaks of mosses as “poor laborious *peasants*”, grasses as yeomanry, herbs as gentry, and trees as the nobility. Each order moreover carries out parallel social roles, with mosses modifying infertile tracts of land to enable other plants to occupy them, grasses performing the role of a downtrodden citizenry, herbs through their luxuriance and splendour giving “dignity to the vegetable community”, and trees as protecting their “fellow-citizens” from heat, cold and storms. Plant predators such as insects are meanwhile termed “ministers of nature”, policing vegetable growth and curbing excess.²²⁹ It is this idea of nature's economy as a parallel state and independent order lying alongside and preceding the human economy which would from the mid-18th century onward become key to the idea of the wild as an original state and domestication as a derived one. This understanding of the domestic-wild divide was an essential pre-requisite for the idea of the wild type as the original and natural form of the species.

Linnaeus on the Human Economy and the Possibility of Improvement

Linnaeus' economy of nature serves both the purposes of natural history and of theology. It speculates how, with a pre-designed ordering of things, nature might be so arranged that once started it may perpetuate its order indefinitely without need of continual corrective interventions. It further posits that nature has an independent order that operates according to pre-established natural laws and towards the end of all agents' general benefit. Attaining this

228 Staffan Müller-Wille, 'Figures of Inheritance, 1650-1850' *Heredity Produced: At the Crossroads of Biology, Politics, and Culture, 1500-1870*, Staffan Müller-Wille & Hans-Jörg Rheinberger (eds.), The MIT Press, 2007, p. 185.

229 Carl Linnaeus, 'On the Police of Nature', In: *Select Dissertations from the Amœnitates Academicæ, Vol. I*, F.J. Brand (Trans.), London: Robinson & Robson, 1781, pp. 134-135, & 138.

greater good is shown to come at a price, for in order “to perpetuate the established course of nature in a continued series”, living creatures must “constantly be employed in producing individuals”, “contribute and lend a helping hand to preserve every species”, and die, the destruction of one organism always being “subservient to the restitution of another”.²³⁰

Linnaeus is thus also constructing a theodicy to explain natural evil as not incompatible with a benevolent Creator. Even human disease and war are suggested to some extent to be part of the great balancing act of natural economy, for humankind as an animal species is not exempt from the force of this “general institution”.²³¹ But Linnaeus did not believe humankind to be wholly subsumed into nature, nor did he fail to acknowledge that there were evils of deprivation and poverty which his theodicy of a natural economy did little to answer for. Conventionally, these evils were attributed to the Fall, as punishment for human hubris. But Linnaeus became otherwise convinced, believing that not only had human economy derived from the natural economy but moreover that there still peoples who retained traces of “man's natural and almost unspoilt state”. He thought himself to have encountered such a prelapsarian people in the nomadic Sami, whom Linnaeus spent time amongst on his expedition to Lapland in 1732. He described the Sami as living in harmony with their environment, judiciously exploiting its sparse resources to meet all their needs, and to lack the vices and diseases found amongst propertied peoples. The existence of people still living in an 'Edenic' state challenged the Christian belief that original sin applied universally. Against this, Linnaeus instead suggested that the cause of evil inclinations was cultural – “we are taught by parents and upbringing and conversation to desire unnatural things”. Human deprivation, meanwhile, was a consequence of the loss of knowledge of nature among civilised peoples. Reattaining this knowledge and applying good economic principles could, Linnaeus thought, be a means to eliminating misery. One means towards societal moral and economic improvement would be therefore to treat “the Lapp [as] our teacher”, in particular to emulate their wise utilisation of local resources and lack of unwarranted desire for imported goods. Whilst Linnaeus would not advocate that Swedes take up a nomadic life like the Sami, he would nonetheless utilise 'the Lapp' as an exemplar of good economic prudence and a partial model for Sweden's own effort to become self-sufficient.²³²

230 Carl Linnaeus, ‘The Economy of Nature’, In: *Miscellaneous Tracts Relating to Natural History, Husbandry and Physick to which is added the Calendar of Flora*, 2nd Edition, Benjamin Stillingfleet (Ed. & Trans.), London: R. and J. Dodsley/S. Baker/T. Paine, 1762, pp. 39-40.

231 Carl Linnaeus, ‘On the Police of Nature’, In: *Select Dissertations from the Amœnitates Academicæ, Vol. I*, F.J. Brand (Trans.), London: Robinson & Robson, 1781, p. 159.

232 Lisbet Koerner [née Rausing], *Linnaeus: Nature and Nation*, Harvard University Press, 1999, pp. 56-81.

As Koerner has shown, the goal of national self-sufficiency was at the core of Linnaeus' cameralist economic beliefs. Cameralism was a school of 17th century German fiscal theory which sought to improve agriculture and manufacturing through a combination of innovation and protectionism. It was a politically conservative doctrine tied to the interests of the aristocratic elite, but one which stressed the importance of commitment to the nation, rational oversight and the integration of the social order. Its idea of the social order as an integrated, interdependent but hierarchical whole clearly left its imprint in Linnaeus' conception of the natural order. But there was for Linnaeus a clear difference between natural and social order, namely that this rational integration was never as fully achieved in society as it was in nature. In nature, harmony had been pre-established by God. Human society, once it emerged out of nature's economy, had on the other hand to struggle to attain concord. But Linnaeus believed that God, being benevolent, had granted humankind both the cognitive and technical ability and the natural resources necessary to attain the end of prosperity. He was highly optimistic of the possibilities for improvement, remarking that "The most savage wilderness, where hardly a sparrow can feed itself, can through good economics become the most wonderful land".²³³ We here again hear echoes of Bacon, who in his *Novum Organum* had commented that the great differences between "the most civilized provinces of Europe and the wildest and most barbarous districts of New India . . . come not from the soil, not from climate, not from race, but from the arts".²³⁴ The similarity in sentiments may in part be due to the influence of Hartlibian economics on cameralism. But I believe it also reflects a more general Baconian influence on Linnaeus' natural philosophy and theology, particularly the idea that it is the expansion of human knowledge and technology, especially technologies of domestication, which will serve to raise humankind from its current state.

I shall now, drawing on Koerner, briefly summarise how Linnaeus' apparently disinterested research programme of scientific collection and classification was in fact linked to highly concrete projects aimed at furthering the ends of the economy and well-being of the Swedish nation. During Linnaeus' youth, Sweden had lost a substantial land empire during the Great Northern War of 1700-1721, which had also seen tens of thousands of Swedish civilians die

²³³ Ibid., pp. 1-3 & 102.

²³⁴ Francis Bacon, 'Novum Organum', *The Philosophical Works of Francis Bacon*, Vol. I, Robert Leslie Ellis and James Spedding (Trans.), John M. Robertson (Ed.), Routledge Revivals, 2011, p. 221.

from famine. It was this loss of territory and threat of famine that Linnaean economics would seek to compensate against and combat. One aspect of Sweden's privations that Linnaeus especially concentrated on was its unequal allocation of natural resources. This had become particularly apparent in an age of mercantilism that had brought valuable new commodities to Europe from Asia, Africa and the Americas, e.g. spices, textiles and stimulants such as tea, sugar and coffee. Adam Smith would later in the 18th century advocate free trade as a means by which countries could mutually compensate for their deficiencies through the exchange of goods. Another possible avenue for accessing overseas resources was colonialism. Linnaeus however rejected both these avenues. Colonial expansion would depend on Swedish strength of arms and Linnaeus thought the country too weak to defeat the military might of the great Asian empires or significantly challenge the imperialist efforts of the major European powers in the New World and Africa. On the issue of trade, Linnaeus followed the popular mercantilist dictum that it was parasitic. This was based on the belief that it was necessary to keep a positive trade balance, measured in terms of metal bullion. There was a particular worry that European gold and silver was being lost to Asia due to a substantial trade imbalance. Linnaeus therefore advocated import substitution as the best means to halt this outflow of bullion and make Sweden self-sufficient. Import substitution would be effected through three main strategies – increasing domestic production through rationalistic improvement programmes, developing native equivalents to substitute for foreign imports, and acclimatising non-native species to Swedish environmental conditions. Linnaeus had particular hope that Lapland could be Sweden's West Indies, offering plentiful land for the expansion of traditional agriculture, an array of Arctic flora that could be domesticated and studied for their potential as substitutes (he for instance developed a 'Lapp tea' from the shrub *Linnaea borealis*), and space for experimental plantations of acclimatised foreign species. All of these strategies would require the use of such hybrid spaces as botanical gardens and experimental plots and pastures, and would succeed or fail based on the ability of extant technologies of domestication to control and modify plants and animals and their conditions of existence.²³⁵

Linnaeus' Baconian belief that the Arctic wilderness could be remade into a productive and domesticated landscape may appear naïve, but his faith that human technological innovation

235 Lisbet Koerner [née Rausing], *Linnaeus: Nature and Nation*, Harvard University Press, 1999, pp. 1-11 & 56-81.

could dramatically modify animals, plants and the environment had some grounding in recent successes in Sweden and elsewhere in Europe. One of the most notable achievements had been Jonas Alströmer's successful establishment of Spanish merino sheep in Sweden, beginning in 1723. He did this by crossing the transplanted stock with local varieties, selecting offspring that maintained a merino wool coat and then crossing them with local stock, repeated over several generations. Using this method, later known as 'grading up', he was able to produce a Swedish merino breed that could survive in their harsh new climate without degenerating and losing their valued merino wool. Linnaeus, as a friend of Alströmer's, was well-informed about this acclimatisation project and acted as one of its major advocates.²³⁶ Linnaeus' own acclimatisation efforts with plants were less successful – his failed efforts trying to grow tea in his experimental gardens were particularly disappointing – but his initial optimism was similarly underwritten by the long-standing success of establishing plants like tobacco, potatoes and rhubarb as northern European crops.²³⁷

On the Reclamation of Names and the Transformation of Nature

Linnaeus' ambitious acclimatisation experiments have been ignored by most historians, who have tended to focus on what is seen as his positive contribution to science, namely his taxonomic work (including, of course, his introduction of binomial nomenclature). But the work of Koerner (née Rausing) and Müller-Wille have helped show that Linnaeus' taxonomic labours were an integral part of his broader project of seeking to increase knowledge of nature for the practical purposes of economic improvement. Koerner argues, for instance, that Linnaeus' use of binomials originated as “a stopgap measure to make his students into a more efficient support staff in his project of national self-sufficiency”.²³⁸ Müller-Wille and Charmantier have meanwhile shown that determining “[t]he taxonomic proximity of genera... clearly guided Linnaeus in speculations about domestic substitutes”. They give the example of Linnaeus' efforts to develop silk production in Sweden. Because the silkworm's favoured food source, mulberry trees, did not fare well in cold climates, Linnaeus explored possible substitutes, including, among other things, nettles. This was based on his (spurious) belief in the affinity between the mulberry genus *Morus* and the nettle genus *Urtica*, which was

236 Roger J. Wood, ‘The Sheep Breeders’ View of Heredity (1723-1843)’, In: *A Cultural History of Heredity II: 18th and 19th Centuries*, Max Planck Preprint No. 247, 2003, pp. 25-26.

237 Lisbet Koerner [née Rausing], *Linnaeus: Nature and Nation*, Harvard University Press, 1999, pp. 113-139.

238 *Ibid.*, pp. 6-7.

grounded in their reproductive similarities and Linnaeus' use of the sexual system of classification.²³⁹ This and other examples show that Linnaean taxonomy was not classification for its own sake but was rather motivated by pragmatic concerns such as being used to identify domestic substitutes, based on the presumption that related species would have similar useful properties.

It is admittedly one thing to demonstrate that Linnaeus believed taxonomy to have useful local applications and quite another to establish that he thought the ultimate end of naturalistic classification a supremely practical one. Depictions of Linnaeus as a scholastic logician have certainly aimed to dismiss the pragmatic orientation of his research programme, as well as to discredit the experimental nature of his work. This of course ignores the fact that Linnaean scientific practices owe far more arguably to the practical efforts of 16th and 17th century European naturalists to experimentally domesticate exotic plants and animals than to medieval Aristotelianism. I shall shortly discuss the experimental nature of Linnaeus' determination of species and how this would help contribute to the nascent concept of wild types. But I shall first establish what I believe Linnaeus saw as the ultimate end of classification, namely the reclamation of nature's names.

Linnaeus, as established, was heavily influenced by Francis Bacon, evidence of which is found in his scientific practice, theories of nature and theology. He furthermore adhered to the popular analogy, also found in Bacon, of gardens as an attempt to re-create Eden – he refers to the botanical garden as a 'Hortus paradisi' ('paradise garden'). Müller-Wille has linked Linnaeus' idea of botany as a paradisiacal practice with his creationist and fixist account of species. Linnaeus asserted in his 1737 *Genera Plantarum* that “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)”.²⁴⁰ By gathering specimens from across the world and distinguishing species from varieties, Linnaeus believed the

239 Staffan Müller-Wille and Isabelle Charmantier, 'Natural History and Information overload: The Case of Linnaeus', Vol. 43, No. 1, *Studies in History and Philosophy of Biological and Biomedical Sciences*, 2012, pp. 11-14.

240 Staffan Müller-Wille, 'Gardens of Paradise', *Endeavour*, Vol. 25, No. 2, 2001, p. 51.

original order of creation could be determined. But there is a notable difference between Linnaeus and Bacon's restorationist projects, one grounded in their divergent beliefs about nature's economy and the nature of improvement. As established, Bacon believed the natural world as fallen into reluctance as punishment for human hubris, and thought scientific domestication to restore its former obedience. Linnaeus, on the other hand, thought the economy of nature as not fallen but instead a well-ordered system designed to equitably benefit all its constituents. This included human beings, who when self-sufficient and ecologically mindful (like the Sami) did not suffer the evils of malfunctioning human economy. Human misery only escalated with the move to a propertied society, the loss of the nomad's naturalistic knowledge and prudence, and the development of culturally transmitted harmful inclinations (such as a want for exotic goods). Because of this different understanding of natural economy, Linnaeus' conception of improvement also subtly differed from Bacon's, for whereas Bacon thought improvement was constituted by the restoration of a docile and tame Edenic natural order (an original domestication), Linnaeus instead believed that Eden's order was that of the (wild but wisely governed) economy of nature. He did not, however, advocate a wholesale adoption of more naturalistic ways of living as a means of restoring Eden. What Linnaeus instead sought to restore was the knowledge of nature's names and uses that was found among 'Edenic' peoples such as the Sami and which he, like Bacon, assumed to be possessed by Adam. The work of taxonomy was the reclamation of these original names and the taxonomist was thus a 'second Adam' restoring nature's proper names.²⁴¹

Linnaean knowledge of nature's economy could be utilised to improve the human economy by emulating what was best in nature, but with the ultimate end of better controlling nature for the promotion of the political power and societal well-being of the nation. For Linnaeus, knowledge of nature thus offers both a model for the state and acts as a means to further the state's ends through domesticating the natural environment and its creatures. There are of course here echoes of Bacon's assertion that nature is best commanded through utilising knowledge of its own laws, but Linnaeus' divergent belief that nature's economy is autonomous and free of reluctance entails that improvement is not a matter of restored Edenic domesticity but rather one of transformative utilisation of nature's untapped resources.

241 Peter Harrison, 'Linnaeus as a Second Adam? Taxonomy and the Religious Vocation', *Zygon*, Vol. 44, No. 4, 2009, pp. 879-891.

This characterisation of improvement as a process by which an emergent human economy uses knowledge of nature to transform and domesticate an original and wild natural order would become increasingly influential as the 18th century progressed. Precedents for this view can be found in Aristotle and Hobbes (see above), but it was Linnaeus who was the great scientific populariser of the idea of natural economy as original, autonomous and well-ordered and human economy as derived, artfully technological and transformative. It was these presuppositions which were necessary for the modern concept of the domestic-wild divide to emerge and for the idea of wild types as ancestral to domestic varieties to develop.

Linnaeus' Experimental Programme of Reduction: Successes and Challenges

In order for nature to be transformed, nature need first be understood through the reclamation of its original names. Reclaiming names required the determination of species, in particular through distinguishing species from variety. As noted, Linnaeus adhered to a fixist notion of species whereby no new forms had arisen since creation. He believed species had spread outwards from Eden, which he theorised in his 1744 essay *On the Increase of the Habitable Earth* to have been located on a mountainous tropical island that had over time expanded to become the continents of today.²⁴² Each species had descended from a “procreative unit” created in Eden, an original pair for species with separate sexes and an individual for hermaphroditic species. Descent from this original reproductive unit occurred through ‘continued generation’, the passing down of the original unit’s typical properties to its progeny.²⁴³ This may be contrasted with the belief in generation as occurring through ‘fresh creation’, or ‘epigenesis’, advocated by Renaissance Aristotelians, for whom generation was a fallible process requiring divine guidance to ensure its success, the parents having no direct role in procreation aside from that of supplying ingredients for the embryo. Hereditary resemblances were explained as being as due to a combination of shared physical properties and environmental circumstances but were thought a secondary rather than necessary effect of the process.²⁴⁴ For Linnaeus, by contrast, the reproduction of the parent and their species’ kind occurred through ‘laws of generation’, these being laws of nature as eternal and

242 Staffan Müller-Wille, ‘Gardens of Paradise’, *Endeavour*, Vol. 25, No. 2, 2001, p. 51.

243 Staffan Müller-Wille and Hans Jörg Rheinberger, *A Cultural History of Heredity*, Chicago and London: The University of Chicago Press, 2012, pp. 31-33.

244 François Jacob, *The Logic of Life: A History of Heredity*, Betty E. Spillmann (Trans.), Princeton University Press, 1973, p. 23.

infallible as Newton's physical laws.²⁴⁵ Under constant environmental conditions, this would imply that parent and offspring should be identical within a species. Linnaeus recognised this not be the case, that nature tended towards variation rather than uniformity, but considered there to be good reason to believe that much of this variability was surface deep.

John Ray had previously established that in some plants species numerous variations in colour and number of flower could be triggered simply through differing regimes of soil and cultivation. Ray had responded to this discovery by in 1686 asserting that the classification of species should only use "distinguishing features that perpetuate themselves in propagation from seed", for "no matter what variations occur in the individuals or the species, if they spring from the seed of one and the same plant, they are accidental variations and not such as to distinguish a species".²⁴⁶ Linnaeus followed Ray in treating species as characterised by their constantly inherited traits and in believing that inconstant traits were accidental products of environmental influences. This assumption would ground Linnaeus' programme of experimental taxonomy, for it suggested that the best means to determine species and thus reclaim nature's names would be to negate the interference of environmental influences. Linnaeus sought to achieve this by sowing the seeds of dubious species in the uniform soils of his botanical garden (a widespread existing practice but one which had not previously been seen as essential to the classificatory process). Since species were assumed to be visibly and constantly distinct across all environments, any loss of distinguishing specific traits when subjected to "cultivation in various soils" was the mark of a mere variety, a product of "climate, soil, heat wind etc... reduced in changed soil".²⁴⁷ I will refer to this strategy of determining species through transplantation as 'reduction'.

It can be seen that Linnaeus assumed variation to be caused by external forces affecting the expression of an inherited type through interfering in its developmental process. Reduction was assumed to work by removing varieties to a uniform environment where they would develop under the same conditions. The idea of reduction to the species type has similarities with the later idea of reversion to the wild type. This is no coincidence, as both are based on a

245 Ron Amundson, *The Changing Role of the Embryo in Evolutionary Thought: Roots of Evo-Devo*, Cambridge University Press, 2005, p. 40.

246 Ernst Mayr, 'Illiger and the Biological Species Concept', *Journal of the History of Biology*, Vol. 1, No. 2, 1968, p. 165.

247 Staffan Müller-Wille, 'Gardens of Paradise', *Endeavour*, Vol. 25, No. 2, 2001, p. 53.

similar model of species and variation grounded in the idea of the species as what is constantly transmitted across the generations and variation as the product of environmental interference in the development of individual instantiations of the species. This can be considered a form of what Sober calls the 'Natural State Model' (see my earlier discussion of Aristotle for its roots).²⁴⁸ Natural state models were central to fixist theories of species such as that of Linnaeus, for they offered a means of explaining how species could at once be constant and be seen to vary in nature. But this theory of variation would be challenged in the course of the Linnaean programme of experimental taxonomy.

Reduction can be considered a form of scientific domestication ancestral to the lab-based strategy of negating locality that Shapin terms 'placelessness' (discussed in later chapters).²⁴⁹ Applied systematically, it became a fruitful strategy for scientific knowledge production, allowing for true species to be distinguished from varieties and the natural order to be better uncovered. Linnaeus hoped that reduction could establish how it was that the immense variation seen in nature had arisen from a relatively small number of species created in Eden. He further anticipated that knowledge of how climate produced varieties might be utilised to help acclimatise foreign species in Sweden. Initially, Linnaeus' programme of experimentally determining species was very successful. He managed, for instance, to cut Ray's 18,500 described plant species down to a mere 7,000.²⁵⁰ But reduction would soon show its limitations and furthermore would problematise the assumptions upon which Linnaeus' paradigm was grounded. Firstly, Linnaeus' original downward revision of species numbers was undone by a continual influx of new specimens, driven by expanded chains of scientific exchange and correspondence and a growth in collecting activity at home and abroad, especially in the tropics.²⁵¹ This made it clear that the number of species in the world was beyond Linnaeus' initial imagining, it being less clear how they could all fit on one original island Eden. The strange distribution of many genera, such as the unique and often bizarre

248 Elliot Sober, 'Evolution, population thinking, and essentialism', *Philosophy of Science*, Vol. 47, No. 3, 1980, p. 360.

249 Steven Shapin, *Never Pure: Historical Studies of Science as if It Was Produced by People with Bodies, Situated in Time, Space, Culture, and Society, and Struggling for Credibility and Authority*, John Hopkins University Press, 2010, p. 57.

250 Marc J. Ratcliff, 'Duchesne's Strawberries: Between Growers' Practices and Academic Knowledge', *Heredity Produced: At the Crossroads of Biology, Politics, and Culture, 1500-1870*, Staffan Müller-Wille & Hans-Jörg Rheinberger (eds.), The MIT Press, 2007, pp. 211.

251 Staffan Müller-Wille and Isabelle Charmantier, 'Natural History and Information overload: The Case of Linnaeus', Vol. 43, No. 1, *Studies in History and Philosophy of Biological and Biomedical Sciences*, 2012, p. 5.

megafauna of Australasia, moreover led to some philosophers speculating that there may have been multiple sites of creation, an idea which was also associated with polygenic theories of race. Voltaire, for instance, speculated that there might have been individual creations on each continent, that “the same providence which placed men in Norway, planted some also in America and under the Antarctic circle, in the same manner as it planted trees and made grass grow there”.²⁵²

Polygenic creation, whilst challenging the Biblical Eden narrative, did not admittedly undermine Linnaean species fixism. There were, however, other naturalists inclined to suggest that species may be mutable as a means of allowing for growth in number from an original few. One of the more radical theories forwarded was the Comte de Buffon's degeneration hypothesis. Buffon assumed, like Linnaeus, that the character of species was hereditarian in nature. He believed species constancy due to the passing down from parent to offspring of a ‘moule interieur’ (‘interior mould’), a biological property responsible for the transmission of type, the generation and growth of individuals, and the regeneration of lost parts.²⁵³ Each species had its own individual ‘mould’, which acted as a ‘memory’ organising matter to produce offspring in the image of their parents, enabling the species’ continued reproduction.²⁵⁴ Where Buffon differed from Linnaeus was in asserting that climatically induced variation could become permanent and constantly transmitted, as not only the external characters but also the internal constitution of the organism could thus be affected. Buffon’s differing view of constant variation was in part motivated by a very different notion of what constituted species, as he considered capacity to interbreed and produce fertile offspring as distinctive of species difference, a view under which varieties that reproduced themselves constantly but which also could interbreed were considered as part of a larger species. Buffon still considered species to be “the sole beings of Nature, perpetual beings, as ancient and as permanent as it is”, but did not consider them as united by their constancy in the present but rather by their historical connectedness through descent. Species thus were real and everlasting units of Nature but with the capacity to degenerate into races which

252 François-Marie Voltaire, ‘Of the Different Races of Men’ from *The Philosophy of History* [1765], In: *The Idea of Race*, Robert Bernasconi and Tommy L. Lott (eds.), Indianapolis: Hackett Publishing Company, 2000, p. 6.

253 Peter McLaughlin, ‘Mechanism and the Clockwork Universe’ [Draft], 2010, p. 19.

254 François Jacob, *The Logic of Life: A History of Heredity*, Betty E. Spillmann (Trans.), Princeton University Press, 1973, p. 80.

reproduced themselves constantly.²⁵⁵ Buffon's degeneration theory was but one part of a radically materialist cosmogony which went much further than even Linnaeus had in offering a mechanistic and secular account for the natural economy. For Buffon, God the creator existed but had merely established a general cosmic order. The Earth itself came into being later, as a fragment of the Sun thrown out by a comet, which had then cooled to its present temperature over 70,000 years. Life had originated in the hotter conditions of the early Earth not through direct divine creation but instead through spontaneous generation. It was to survive the planet's cooling and colonise a variety of climates that species degenerated from a few original forms into the multitude seen today.²⁵⁶ The appeal of ecological theories which saw the economy of nature as emerging out of a larger cosmic order with minimal direct divine intervention (as opposed to being designedly established by God in Eden), would increase towards the end of the 18th century, particularly as evidence accumulated for the ancient age of the Earth and the ubiquity of past extinctions. But I shall leave discussion of further developments in these strains of ecological thought for my next chapter.

It was not only the deluge of new species and competition from alternative theories of natural economy which caused Linnaeus' research programme difficulties, for reduction also raised its own problems. A particular challenge was the taxonomic place of so-called 'constant varieties'. These were varieties derived from known species but which did not reduce when transplanted into different soils. As early as 1744, Linnaeus accepted one of these, the 'monstrous' toadflax variety *Peloria*, as a species, in doing so explicitly accepting the possibility of new species arising through degeneration and undermining his model of the distinction between species and varieties.²⁵⁷ Whilst *Peloria* eventually proved sterile, reproducing vegetatively, other constant varieties – e.g. Antoine-Nicolas Duchesne's strawberry, *Fragaria monophylla*, discovered in 1763 – were fertile and reproduced constantly from seed in varying environmental conditions.²⁵⁸ Here, the interfering cause preventing reduction could not be externally found in the earth or sky and therefore, it was

255 Phillip R. Sloan, 'Buffon, German Biology, and the Historical Interpretation of Biological Species', *The British Journal for the History of Science*, Vol. 12, No. 2, 1979, pp. 117-119.

256 Peter J. Bowler, *Evolution: The History of an Idea*, University of California Press, 2009, pp. 27-81.

257 Å. Gustafsson, 'Linnaeus' *Peloria*: The history of a monster', *TAG Theoretical and Applied Genetics*, 1979, Volume 54, Number 6, pp. 242-243.

258 Marc J. Ratcliff, 'Duchesne's Strawberries: Between Growers' Practices and Academic Knowledge', In: *Heredity Produced: At the Crossroads of Biology, Politics, and Culture, 1500-1870*, Staffan Müller-Wille & Hans-Jörg Rheinberger (eds.), The MIT Press, 2007, pp. 205-215.

inferred, had to be internal, an aspect of the organism's individual and inherited constitution. Intraspecific variation could thus not always be dismissed as an environmental effect.

In addition to the problem of constant varieties, doubts were being raised in Linnaeus' time regarding the effectiveness of transplantation as a means of causing reduction. This strategy was certainly successful for many plants – indeed many of Linnaeus' methods would be re-utilised by taxonomists in the 20th century (see for instance the work of his latter-day countryman, Göte Turesson).²⁵⁹ However, other variations long supposed to be environmental in origin proved to endure obstinately following translocation. The most notable of these was human skin colour, the variations of which were thought due to the differing effects of temperature and humidity, but which it had long been noted did not appear significantly affected by removal to new climates. As François Bernier observed already in 1684, “if a black African pair be transported to a cold country, their children are just as black, and so are all their descendants until they come to marry with white women”.²⁶⁰ Consequently, when Immanuel Kant developed his theories on racial derivation, he would suppose that, whilst the cause of racial differences was due to adaptation to environmental conditions, once this initial plasticity was used up such changes had become permanent and irreversible.²⁶¹

Such Buffonian style degeneration theories faced their own problems, for there were other variations that very much did appear affected by changes to living conditions. There were, for instance, numerous cases reported of European animals gone wild in overseas colonies reacquiring ancestral characteristics. The Spanish naturalist Felix de Azara, writing in the 1790s, asserted that of the many wild horses encountered in South America's La Plata region “I have never seen one of any color but chestnut”. Non-chestnut horses invariably turned out to be recently escaped domestic animals. He concluded that it seemed likely that “the horses that have become feral have regained not only the habits, disposition, and shapes of the type... but also the color”.²⁶² Azara believed his observations refuted Buffon's degeneration

259 Robert E. Kohler, *Landscapes and Labscapes: Exploring the Lab-Field Border in Biology*, The University of Chicago Press, 2002, pp. 163-172.

260 François Bernier, ‘A New Division of the Earth’ [1684], In: *The Idea of Race*, Robert Bernasconi and Tommy L. Lott (eds.), Indianapolis: Hackett Publishing Company, 2000, p. 2.

261 Immanuel Kant, ‘Of the Different Human Races’ [1777], In: *The Idea of Race*, Robert Bernasconi and Tommy L. Lott (eds.), Indianapolis: Hackett Publishing Company, 2000, p. 19.

262 Barbara G. Beddall, ‘“Un Naturalista Original”: Don Félix de Azara, 1746-1821’, *Journal of the History of Biology*, Vol. 8, No. 1, 1975, pp. 48-49.

theory by showing, as Linnaeus had claimed, that variation from the type was an impermanent effect of environmental influences. But because of the constancy of racial variations and of spontaneously appearing 'constant varieties' (later termed more commonly termed 'sports'), there was now a strong inclination to believe that variation, at least when produced by nature, was commonly permanent. Azara's observations therefore did not greatly stymie the belief that species might degenerate, but did cast doubt on whether variation under domestication was permanent. This doubt combined with an increased recognition of the ability of human breeders to direct variation to profitable ends to lead many naturalists c. 1800 to conclude that whilst racial variation in nature might be permanent, the variability of breeds under domestication was by contrast the artificial product of human interventions in the habits, alimentation and reproduction of organisms. As shall be seen, this would influence a normative early interpretation of wild type as what is returned to once artificial intervention is removed. This idea of variation in the wild and under domestication as differing in nature will be further explored in the next chapter.

From Uppsala to Brno – Mendel and the Legacy of the Linnaean Effort to Determine Species

It can be seen that, in spite of Linnaeus' early efforts to systematise and clarify taxonomy, by the late 18th century there was a great deal of debate as to what constituted a species and what was the nature of variation. The limitations of Linnaean transplantation-based strategies of scientific domestication having been made apparent, new means were sought by which to determine species. One of these means would be hybridisation studies. I will now conclude this chapter by showing how Mendel's experimental discoveries were a legacy of Linnaeus' earlier efforts to determine species through garden-based techniques of experimental domestication. Buffon had suggested hybrid sterility as an alternative method of demarcating species and varieties, but had himself lost faith in this technique with the discovery in the 1750s of occasional fertile hybridisations between otherwise well-distinguished species of domesticated songbirds.²⁶³ Linnaeus himself, under the influence of *Peloria*, would from the 1740s onwards contrastingly suggest hybridisation not as a means of distinguishing species but instead of creating new ones. He believed this could account for how tens of thousands of species had originated from a created few in Eden. He doubtless also aspired to discover the

263 Ernst Mayr, 'Joseph Gottlieb Kolreuter's Contributions to Biology', *Osiris*, Vol. 2, 1986, p. 152.

secrets of hybridisation for the purposes of improving human economy. Linnaeus and his students would subsequently search for true-breeding hybrids, with a thesis produced in 1751 listing 100 discovered, though Olby regards most of these as dubious. Nonetheless, several hybrids would eventually be accepted as legitimate species and two, a seed-producing goat's beard hybrid and a cuttings-propagated speedwell, would be produced artificially through hand cross-pollination.²⁶⁴

It was to challenge this idea that hybridisation could be used to produce new species that Joseph Kölreuter would instead develop it as a systematic means of experimentally determining species. Kölreuter believed the formation of new species through hybridisation did not conform to observed nature or with the idea of a well-designed world, thinking that if it were the case “incredible confusion would result in nature”.²⁶⁵ His utilisation of otherwise ordinary garden and farm plants as a means of producing data to defend this worldview can be considered an act of scientific domestication *par excellence*. Kölreuter's first significant discovery was that the seeds of the hybrid goat's beard did not produce plants like their hybrid parent. Instead, this second hybrid generation was a heterogeneous mixture of individuals which overall tended to resemble more their grandparents, the original maternal and paternal parent types from which the hybrid was derived. He thus demonstrated that even those hybrids which overcame the sterility barrier usually could not breed true, instead disintegrating into the species from which they were derived.²⁶⁶ Kölreuter termed this phenomenon 'transformation' or 'restoration', but it would in the 19th century come to be considered a form of reversion (see later chapters).²⁶⁷ Kölreuter would go on to conduct around 140 experimental crossings utilising 13 genera and 54 species over the five years between 1760 and 1765.²⁶⁸ His findings would not only confirm the tendency to reversion in hybrids but also demonstrated the equal contribution of the parent types at a time when the spermism-ovism debate was still extant.²⁶⁹ Kölreuter's hope to use hybridisation to better elucidate the species/variety distinction would, however, not be fully met. He had initially expected that species hybrids would be near or completely sterile but that he would be able to

264 Robert C. Olby, *Origins of Mendelism*, London: Constable and Company Ltd, 1966, pp. 19-26.

265 Ernst Mayr, 'Joseph Gottlieb Kolreuter's Contributions to Biology', *Osiris*, Vol. 2, 1986, p. 142.

266 Robert C. Olby, *Origins of Mendelism*, London: Constable and Company Ltd, 1966, pp. 28-29.

267 Ernst Mayr, 'Joseph Gottlieb Kolreuter's Contributions to Biology', *Osiris*, Vol. 2, 1986, pp. 165-166.

268 *Ibid.*, pp. 137 & 154.

269 *Ibid.*, pp. 158-159.

raise fertility through backcrossing with one of the parent types.²⁷⁰ He based this presumptions on a fluid theory of heredity drawn from alchemy, comparing the production of the hybrid from the parent types to the synthesis of a neutral (hermaphrodite) salt from the mixing of the sulphuric male and mercurial female principles. What he instead found was that hybrid sterility was highly variable and that backcrossing often produced offspring of no greater fertility than the hybrid parent. Kölreuter did, however, successfully complete the ‘transformation’ of several of his hybrids back to one of their parent types. In doing so, he extended the initial crossing experiment into an experimental pedigree, and so established lineage domestication for experimental purposes as a standard practice in hybridisation studies.²⁷¹

After Kölreuter, the experimental scientific production of hybrids and their study became a mainstay of European botany. Thomas Andrew Knight would notably pioneer the deliberate use of hybridisation as a tool for producing new agricultural varieties.²⁷² But the ongoing attempt to find general laws for the determination of species would continue to be frustrated by the great variation in hybrid fertility and sterility. In response to this difficulty, Carl Friedrich Gärtner, the most notable hybridist after Kölreuter and before Mendel, would adopt an instrumentalist approach to species defined purely by constancy in reproduction and thus encompassing both true Linnaean species and constant varieties. Abandoning the search for the general, Gärtner instead sought only to determine the particular biological laws of development – ‘*Bildungsgesetze*’ – active in each species. This is not to say that Gärtner's gardens were not scientifically productive domestic spaces or that his research was without direction. Gärtner carried out hybridisation experiments on a vast scale – over 25 years, he initiated 10,000 experiments utilising 700 species. Whilst he did not believe these experiments could determine general laws, he held that they did reveal the 'elective affinities' between species (i.e. their greater or lesser inclination to hybridise). These interspecies relations were constant and could therefore be mapped onto one another in order to determine general interspecific relations within the plant kingdom. Gärtner can thus be seen as

270 Staffan Muller-Wille and Vitezslav Orel, ‘From Linnaean species to Mendelian factors: Elements of hybridism, 1751-1870’, *Annals of Science*, Vol. 64, No. 2, 2007, pp. 184-186.

271 Ernst Mayr, ‘Joseph Gottlieb Kolreuter's Contributions to Biology’, *Osiris*, Vol. 2, 1986, pp. 143-173.

272 L.H. Bailey, ‘Van Mons and Knight, and the production of varieties’, *The Survival of the Unlike: A Collection of Evolution Essays Suggested by the Study of Domestic Plants*, 5th Ed., New York: The Macmillan Company, 1906, p. 158.

attempting to extend the experimental application of hybridisation in order to empirically determine taxonomic relationships.²⁷³

Whilst Gärtner's own contributions to hybridist thought are largely forgotten, those of one experimentalist he heavily influenced, namely Gregor Mendel, have not. That Mendel was part of a long-standing hybridist tradition was not initially realised by many of those life scientists who rediscovered him circa 1900. Thence has arisen the myth of the 'monk in the garden', the isolated genius ahead of his time, and even Foucault's "veritable monster" operating outside his era's episteme.²⁷⁴ Historians such as Roger Wood and Vítězslav Orel have since corrected this error by showing that Mendel was not isolated but was rather part of a thriving community of Central European experimental breeders centred on Brno, the industrial heartland of the Hapsburg empire.²⁷⁵ Mendel was also familiar with the latest laboratory science of his day, having studied at Vienna under the plant physiologist Franz Unger. The joint influence of Unger and Gärtner can be seen in Mendel's methodology. Like Unger, and unlike Gärtner, Mendel believed his experiments could be used to infer universal laws of plant development and heredity. Mendel's working species concept, however, appears to borrow from Gärtner, as can be seen from the fact that he refers to his pea strains as distinct '*Arten*' ('species') despite their only differing in single characters, as well as justifying this label on the basis that he spent two years selecting and breeding his peas until they "yielded perfectly constant and similar offspring".²⁷⁶

The fact Mendel referred to his strains as 'species' should cause us to be cautious about one common claim about Mendel's works, namely that unlike his predecessors he moved beyond the study of species and the whole organism (*habitus*) as units of hybridisation and instead adopted a traits-based approach which assumed particulate hereditary factors to be the unit of analysis. It is true that Mendel believed that by concentrating on 'detail experiments' involving parent types differing in one or two characteristics, he would be able to more easily

273 Staffan Müller-Wille and Vitezslav Orel, 'From Linnaean species to Mendelian factors: Elements of hybridism, 1751-1870, *Annals of Science*, Vol. 64, No. 2, 2007, pp. 186-191.

274 Staffan Müller-Wille, 'Characters written with invisible ink. Elements of Hybridism 1751-1875', In: *A Cultural History of Heredity II: 18th and 19th Centuries*, Max Planck Preprint 247, 2003, p. 47.

275 Roger J. Wood and Vítězslav Orel, *Genetic Prehistory in Selective Breeding: A Prelude to Mendel*, New York: Oxford University Press, 2001, pp. 1-11.

276 Staffan Müller-Wille and Vítězslav Orel, 'From Linnaean species to Mendelian factors: Elements of hybridism, 1751-1870, *Annals of Science*, Vol. 64, No. 2, 2007, pp. 176-193.

determine patterns of heredity, which could then be scaled up to more complex cases of hybridisation between more differentiated parent types.²⁷⁷ But simply because he believed more similar species would produce more tractable data does not imply that he believed the *habitus* decomposable into traits – it is quite likely he instead thought there to be a basic law for elective affinities that would only be visible in minimally distinct species. It may further be noted that Mendel's concept of segregation, commonly cited as evidence of his particulate interpretation of heredity, was in fact little different from that employed earlier by the French hybridist Naudin, who explicitly defines segregation as occurring between “two different essences”, i.e. two holistic parent types.²⁷⁸ Mendel even follows Naudin in supposing that the meeting of homogeneous essences in reproduction results in blending as opposed to segregation. This is evident in his formulas, which describe homozygous individuals with ‘dominating’ and ‘recessive’ factors as ‘A’ and ‘a’ respectively (compare with the use of ‘AA’ and ‘aa’ in modern genetics, which clearly records the contribution of two like parent factors whilst emphasising non-blending).²⁷⁹

If Mendel was not investigating the nature of an intraspecific particulate heredity, what was he looking for in his experiments? Robert Olby has suggested that the main research question motivating Mendel was determining “*the role of crossing in the production of variability*”. He was, in other words, seeking to answer the question of whether hybridisation could produce new variability and therefore be a potential source of new species. Olby believes that this interpretation of Mendel’s work better explains his turning away from his earlier work with *Pisum* to work with *Hieracium*, a move commonly seen as erroneous but which makes sense if we consider that what Mendel was looking for were ‘constant hybrids’, which *Hieracium* appeared to be very good at producing.²⁸⁰ On this revisionist account, Olby concludes, Mendel was certainly not the kind of researcher he would be interpreted to be by his ‘rediscoverers’ in 1900, that if a Mendelian is understood to be “one who subscribes explicitly to the existence of a finite number of hereditary elements which in the simplest case is two per hereditary trait, only one of which may enter one germ cell, then Mendel was clearly no Mendelian”.²⁸¹

277 Ibid.

278 Robert C. Olby, *Origins of Mendelism*, London: Constable and Company Ltd, 1966, p. 167.

279 Robert C. Olby, ‘Mendel no Mendelian?’, *History of Science*, Vol. 17, 1979, pp. 57-59

280 Ibid., p. 57.

281 Ibid., p. 68.

The purpose of this last section has not been to seek to deflate Mendel's worthy reputation as a highly significant contributor to scientific knowledge but rather to stress the historical context of his work and its relationship with other forms of 18th and 19th century scientific knowledge production which have employed methods of experimental domestication in garden-based settings. I have particularly stressed the importance of such experimental research in the early modern history of taxonomy as an experimental and applied (as opposed to theoretical or paper-based) effort to determine the boundaries between taxa (especially species and varieties), to establish relationships between species and to interrogate the problem of variation. I have also sought to establish in this chapter the theoretical and societal context, in particular the demarcation of natural and human economy, which both motivated the Linnaean effort to reclaim nature's names and influenced the kind of experimental domesticating strategies undertaken for the purposes of scientific knowledge production. Finally, I have also sought to use this chapter to establish the context out of which the idea of the wild type as an ancestor of domestic varieties would develop, specifically by showing how the evolution of this idea was shaped by changes in how relations between society and nature were conceived. I will now turn my attention to the initial appearance of the wild type concept, its place in 19th century natural historical worldviews, and how it was affected by Darwin's reinterpretation of the domestic-wild divide. I will return to Mendel at a later point in this thesis.

Chapter 5 – The Concept of Wild Type amongst Darwin and his Contemporaries

The Conceptual Background of Wild Type and Early Citations

As discussed in the last chapter, the emergence of a distinct concept of ‘wild type’ pivoted on the idea that the human and natural economy constituted distinct orders operating under differing organisational regimes. I suggested Linnaeus as an influential populariser of the idea of natural economy as original, autonomous and well-ordered and human economy as derived, artfully technological and transformative. This interpretation of the society-nature divide promoted the idea that the domestic-wild divide was also one of a distinction between what was original, free-living and acting as a means to its own ends (the wild) and what was artificial, subject to constraint and made to act as a means to human ends (the domestic).

Organisms in nature also had a given place in its economy, whereas those under domestication were forced to serve humankind under many different roles. When combined with a belief that changes in the conditions of existence caused organisms to degenerate so as to adapt to their new circumstances, this led to the common view that organisms under domestication varied more than those in stabler natural conditions. This was corroborated by the visible evidence of there being a great many domestic varieties and only a few wild species that were plausible candidates as ancestral wild types. Buffon would thus be led to conclude that “Men... have greatly altered and modified the domestic kinds”, rendering them “greatly variegated and changed” and establishing “physical and real genera”. But he goes on to state that where those genera “modified by the hand of man” “have but one common origin in Nature, the whole genus ought to constitute but a single species”.²⁸² This statement establishes a distinction between naturally occurring and ancestral wild types and derived, humanly modified domestic varieties, whilst emphasising their shared specificity.

Furthermore, whilst Buffon did believe wild species to degenerate, he emphasised that domestic species are induced to do so at a greater rate and extent through human intervention. He additionally pointed to the dependence on human guardianship of many domestic breeds, noting that “our domestic sheep, in their present condition, could not subsist without the support of man”. He considers this to render it “apparent, that nature never produced them as they exist at present, but that they have degenerated under our care”, and thus moves to

282 Georges-Louis Leclerc Comte de Buffon, *Natural History, General and Particular, Vol VI*, William Smellie (Trans.), London: W. Strahan & T. Cadell, 1785, pp. 220-221.

discredit any claim that domestic stock instead derived from multiple wild ancestors now unknown or extinct.²⁸³

Buffon himself does not use the term 'wild type' – though he does speak of the 'primitive stock', 'principal stock' and 'original stock' (in Smellie's translation at least), which may be ancestral synonyms of the term.²⁸⁴ But even if the terminology is not there, I certainly think it can be said without too much anachronism that the wild type concept is nascent in the work of Buffon and followers such as Blumenbach. It is my guess that casual use of the term 'wild type' may have already existed amongst Anglophone naturalists before 1800, perhaps as part of trying to translate concepts from continental naturalism. To search for early citations of the English term 'wild type', I employed online resources such as Google Books and journal archives on JSTOR, searching 'wild type' and restricting results to before 1850. Whilst far from foolproof, my methods did generate some useful results. The earliest citations of 'wild type' in English language publications that I have found are from between 1820 and 1840, the majority of these being from the last five years of this period. Even in these early citations, the term is used with a degree of sophistication that suggests its use was already well established in scientific discourse prior to these texts appearing. This inference of established usage is further warranted, I believe, by the fact that none of these sources treat 'wild type' as a term in need of explanation – readers' familiarity with the term is assumed. That usage was relatively sophisticated even at this early stage can be shown by analysis of the citations. I believe it can be shown that, by the 1830s, 'wild type' was already being utilised in the three main manners typical in later 19th century usage. These three main uses are: (1) 'wild type' as referring to the posited ancestors of domestic species, and also their untamed descendants, especially those living wild in the region where domestication first occurred; (2) the believed 'wild type' species as used as a point from which to measure the physiological and behavioural divergence of domestic varieties; (3) the 'wild type' as the type to which organisms return in cases of reversion, particularly cases of degeneration brought on by ferity. The latter two senses of 'wild type' are additionally linked by their often involving the presumption that the wild type is a normative as well as genealogical or comparative category.

283 Ibid., pp. 205-206.

284 Ibid., pp. 216 & 381; & Georges-Louis Leclerc Comte de Buffon, *Natural History, General and Particular, Vol VIII*, William Smellie (Trans.), London: W. Strahan & T. Cadell, 1785, p. 34.

The earliest citation of the term ‘wild type’ that I have been able to find is in an 1823 article in *The Philosophical Magazine and Journal*, ‘Memoir on the different Species, Races, and Varieties of the Genus *Brassica* (*Cabbage*), and of the Genera allied to it, which are cultivated in Europe’, by the Genevan botanist Augustin Pyramus de Candolle. Referring to “BRASSICA RAPA OLEIFERA”, “the ‘Wild or oleiferous Turnip’”, de Candolle states that “This third race of Turnips appears to be the wild type of the species, or at least is very near to a wild state”. He lacks direct evidence for this claim, but points to the fact that *oleifera* was “mentioned, and tolerably described, by ancient authors under the name of Wild Turnip” as evidence that it cannot have been recently derived. He moreover states that he himself has reared from seed several individuals of this variety and that they “resembl[e] the figures given by the ancients”.²⁸⁵ Furthermore, this is likely the variety he earlier comments as being “said to be found in a wild state in various parts of Europe”. He has, however, doubts as to whether the turnip actually originates in Europe due to “the facility with which its seeds can be transported from the place where it is cultivated”, which “must leave its native habitat a matter of doubt”, as it is not clear whether wild European turnips are actually the ancestral stock of domesticated varieties or alternatively descended from escaped domestic stock originating outside of Europe.²⁸⁶ This first citation of wild type is a clear example of its use in the first sense described above, namely as referring to the specific wild ancestor of a domestic species and also those of its descendants which remain undomesticated. The citation also illustrates a related line of enquiry of interest to those naturalists seeking to establish the origins of domestic varieties, namely the question of where domestication first occurred. The question of how domestic varieties were derived from the wild type is, however, here little explored.

My second early citation appears ten years later in *The Edinburgh New Philosophical Journal*, in the article ‘On Dwarfs and Giants’. This is an anonymous commentary on the book *Histoire générale et particulière des anomalies de l'organisation chez l'homme et les animaux* by the French zoologist Isidore Geoffroy St Hilaire. It is stated that as to the tendency to dwarfism and gigantism in domestic species St Hilaire believes that they “may be

285 M. Augustin Pyramus de Candolle, ‘Memoir on the different Species, Races, and Varieties of the Genus *Brassica* (*Cabbage*), and of the Genera allied to it, which are cultivated in Europe’, *The Philosophical Magazine and Journal*, Vol. 61, January-June 1823, pp. 186-187.

286 *Ibid.*, p. 184.

divided into two groups, those whose races have all the same height, or nearly so, and those which consist of very large and very small races”. Of the first group, he asserts that “the height of the races or varieties cannot be different from that of the wild type; it may also present a difference of size, less or greater; this difference, however, is always very slight”. He contrasts this with the second group, in which “there are some domestic races existing much larger, and others much smaller than the wild type; but the medium height of the domesticated races, a height which is found exact, or almost so, among many of them, hardly differs, or does not differ at all from the wild type”.²⁸⁷ This is clearly an example of the use of wild type in the second sense mentioned above, namely as a point from which to measure the differentiation from it of domestic varieties. St Hilaire moreover treats the wild type as being the average of the species, its ‘norm’, and believes that variation, if it occurs, always tends to move in equal proportions away from this norm. The wild type for St Hilaire is thus not an arbitrary point of measurement but rather a normative one, varieties, along with anomalies and monstrosities, being products of the deflecting of the natural course of species-specific development.²⁸⁸

St Hilaire's treatment of the wild type as the result of undisturbed natural development and variation as a product of perturbation is clearly another instance of use of a ‘Natural State’ model of variation. As with Linnaeus' idea of reduction, so with wild type there was a general idea that when such perturbations were removed an organism would return to this natural state. As discussed, by the 19th century this natural state was commonly assumed to be the organism in nature and domestication was considered as the artificial imposition of human art upon nature's forms. A return to the natural state thus became conceptualised as a reversion to wild type. The first direct allusion to reversion alongside a citation of ‘wild type’ that I have found is by Thomas Bell in his *History of British Quadrupeds* (1837), where, with reference to the Shetland bull cattle breed, he remarks that “the most interesting circumstance attached to this breed is the obvious tendency which a relaxation of domestic discipline and management produces towards a return to the form of the original wild type;—a fact which has been more particular insisted upon and illustrated in speaking of the relations between the

287 M. Geoffroy St Hilaire, ‘On Dwarfs and Giants’, *The Edinburgh New Philosophical Journal*, Vol. 15, April...October 1833, p. 148.

288 *Ibid.*, p. 146.

Wolf and the different domestic Dogs”.²⁸⁹ It was this normative conception of the wild type, as shall be seen, which would come into conflict with Darwin's claim of an analogy between variation in domestication and the wild.

Natural Disorder: Struggle & Change

As aforementioned, citations of ‘wild type’ start to become quite numerous in the years between 1835 and 1840, to the extent that I think it fair to judge that by the end of this period it had become mainstream terminology amongst British naturalists. It still continued to be used in the original context from which it derived, where a distinction was assumed between variation in nature and in domestication based on believed differences between the natural and human economies. But this rigid demarcation of nature and domesticity was coming under increasing challenge in this period, with significant implications for the nascent wild type concept. In particular, Linnaeus’ idea of nature as an economy whose movements served to maintain a static set of relations between its constituent organisms was attacked on several fronts from the end of the 18th century onwards, with the consequence that “the balance of nature turned from a permanent process into a transient state of affairs, while adaptation turned from a transient state of affairs into a permanent process” (Müller-Wille).²⁹⁰ The first aspect of Linnaeus’ model of nature to be targeted was natural prudence, the idea that there are set limits to rates of reproduction and predation which ensure that species populations are maintained at “just” proportions.²⁹¹ In 1798, Thomas Malthus presented an alternative vision of reproduction in nature. For Malthus there was no internal mechanism that checked species’ propagation. Nature “has scattered the seeds of life abroad with the most profuse and liberal hand”. What restrained species numbers was not prudence but rather necessity, there being a “natural inequality of the two powers of population and of production in the earth”, with the consequence that “The race of plants and the race of animals shrink under this great restrictive law... its effects are waste of seed, sickness, and premature death”.²⁹² This inequality of propagation and production also applied to humans. Though often able to ward

289 Thomas Bell, *A History of British Quadrupeds, including the Cetacea*, London: John van Voorst, 1837, p. 427.

290 Staffan Müller-Wille, 'The Economy of Nature in Classical Natural History', *Studies in the History of Biology*, Vol. 4, No. 4, 2012, p. 45.

291 Carl Linnaeus, 'The Economy of Nature', In: *Miscellaneous Tracts Relating to Natural History, Husbandry and Physick to which is added the Calendar of Flora*, 2nd Edition, Benjamin Stillingfleet (Ed. & Trans.), London: R. and J. Dodsley/S. Baker/T. Paine, 1762, p. 119.

292 Thomas Malthus, *An Essay on the Principle of Population*, London: J. Johnson, 1798, p. 5.

off starvation thanks to their intelligence and technology, where such measures failed or were lacking humans would fall into a “struggle for existence” in which many would die. Malthus thought such contestations were especially common in the pre-civilised past, characterising this as a time of “perpetual struggle for room and food”.²⁹³ Malthus thus raised again the spectre of the state of nature as a Hobbesian war of all against all. Furthermore, his portrayal of population as regulated by necessity as opposed to divinely mandated prudence offered an alternative explanation for the apparent orderliness of nature, one in which divine government need not play a significant part.

New developments in another area of science, geology, also began to undermine the notion of an unchanging economy of nature. 18th century naturalists such as Linnaeus and Buffon commonly assumed the Earth to have undergone significant changes in its history but believed that the order of organic nature had remained relatively static, not least due to organisms’ ability to adapt to new climatic conditions. This belief in the continuity of the organic order received a fatal challenge at the beginning of the 19th century with the discovery and formal scientific identification of undeniably extinct fossil species such as mammoths and mastodons. As a result of these findings, by the 1810s Georges Cuvier was postulating that the history of the Earth had been punctuated by great catastrophes, which had reduced a once vast number of original species to their present number. With the increasing realisation that modern groups such as mammals were either poorly or not at all represented in older rocks, early palaeontologists such as William Buckland began to propose that these catastrophes were due to extreme changes in the environment, leaving few survivors. In place of those exterminated, following each catastrophe God created new and more advanced species to fill their place, this progressive series of creations culminating finally in human beings, the most advanced of God’s creatures.²⁹⁴ What these catastrophist theories amounted to was a rejection of a singular economy of nature operating in perpetual cycles of a fixed equilibrium in favour of a succession of such economies, each destroyed to make way for a superior natural order. Whilst it was generally assumed that these episodes of destruction and succession had stopped now that their end-goal had been reached – human beings – the total continuation of the organic order was rejected and the offices of nature’s economy and their occupants now admitted to not be invulnerable to extinction.

293 Ibid., p. 14.

294 Peter J. Bowler, *Evolution: The History of an Idea*, University of California Press, 2009, p. 114-117.

Catastrophism's rejection by Charles Lyell in his *Principles of Geology* did not rescue the notion of an unchanging organic natural order. Lyell was reviving the uniformitarian ideas first advocated by James Hutton in the 1790s, whereby the planet was viewed as an everlasting and self-sustaining 'world machine' shaped and reshaped by forces of uplift and erosion.²⁹⁵ As part of this uniformitarianism, Lyell was a species fixist, believing that "There are fixed limits beyond which the descendants from common parents can never deviate from a certain type".²⁹⁶ These views would appear largely compatible with the Linnaean model of the economy of nature. But Lyell also acquired ideas from the works of Augustin Pyramus de Candolle, as shown by Trevor Pearce. De Candolle, drawing from Malthus, was one of the first naturalists to suggest that a 'struggle for existence' might occur between non-human living things. In his 1820 *Géographie Botanique*, he affirms that "All the plants of a country... are in a state of war relative to all the others". De Candolle was referring to the now well known phenomenon of the ecological succession of plants, whereby colonisers are gradually replaced by more competitive but typically slower growing species. The success of a species in the competition for a particular patch of ground depended on how well suited it was to the particular environmental conditions that held in an area. De Candolle classified such areas according to soil and climate type, identifying sixteen major classes, and from Linnaeus borrowed the term 'station' to refer to these units of ecological function.²⁹⁷ As with Linnaeus, de Candolle's stations prescribed their occupants to perform particular ecological roles, but they differed in being open to takeover by any invading species that could out-compete the original occupant. Lyell adopted de Candolle's notion of station but placed greater emphasis on the influence of interspecific relations in determining the space available for newcomers to a station. By stressing the role of organisms in determining the possibilities of their environment, Lyell further increased the possibility for dynamic change in the economy of nature. Both inanimate and animate circumstances, he insisted, are in a state of perpetual change, and consequently stations are constantly being created and destroyed. No longer could any species remain rooted in an unchanging office in nature's economy; in the long term, environments degraded and species were forced to move to more amenable

295 Ibid., p. 61.

296 Jean Gayon, *Darwinism's Struggle for Survival: Heredity and the Hypothesis of Natural Selection*, Cobb, Matthew (Trans.), Cambridge University Press, 1998, p. 34.

297 Trevor Pearce, "A great complication of circumstances" – Darwin and the economy of nature', *Journal of the History of Biology*, Vol. 43: 493–528, p. 503.

surroundings or perish.²⁹⁸ Extinction for Lyell was therefore not the result of global catastrophes but rather of the action of everyday geological forces and interspecies competition.

It can thus be seen that at the same time that wild type was becoming mainstream terminology it was being undermined as a concept due to the increasing recognition that the economy of nature was far more dynamic than was posited by Linnaeus. Already in Lyell, the idea had appeared that a species is not restricted to a particular office in nature, being able to invade others or be displaced from its own, and moreover that such stations are temporary spaces which appear and disappear as the conditions of existence and the organic communities they foster change over time. Nonetheless, whilst Lyell showed that the economy of nature was more kinetic than once thought, he at the same time assumed that species were created and unchanging. A shifting environment did not necessitate that species degenerate into new forms, as in Lyell's view the changes in stations were usually local and occurred slowly enough to allow for migration to more tolerable habitations, with species extinction being a rare misfortune. It was true that any migrant species invading a station would need to compete with the organisms already there but most of the time somewhere some migrants would establish themselves and continue their kind. Lyell's belief that species did not significantly change but rather survived by moving to more hospitable regions allowed for the maintenance of a continued natural order of species, though including the odd extinction. This salvaging of the organic order was, however, contingent on a particular interpretation of what the 'war of nature' constituted. For Lyell and also de Candolle, competition was primarily between different species fighting for particular stations in nature. In such a scenario, there was no great need for species to change over time. A species could be pushed out of many stations but as long as it survived in a few marginal environments it was better adapted to than its competitors, it could wait out until conditions better favoured it and then expand its range again.

Charles Darwin's recognition that the 'war of nature' included not only competition between species but also between individuals within a species was therefore key in establishing that change was necessary for species lineages to survive through time. Darwin realised that if

298 Ibid., p. 504-5.

there was competition for stations in nature, it would surely be most fierce between individuals of the same species as “they frequent the same districts, require the same food, and are exposed to the same dangers”.²⁹⁹ Given that species vary in nature and that any variation favourable to survival, however minor, would tend to be preserved in such conditions of struggle, he therefore concluded that species are in a continuous process of gradual adaptation. Reaching this conclusion required Darwin to first reject ‘perfect adaptation’, the view that species are eminently equipped and fitted for their offices in nature. This belief was still evident in Darwin’s unpublished 1844 essay, in which he still maintained the orthodox view that “Most organic beings in a state of nature vary exceedingly little”.³⁰⁰ Dov Ospovat has credited a pessimistic rereading of Malthus as key to Darwin’s change of mind.³⁰¹ How Darwin came to understand the action of Malthus’ population principle on the economy of nature can best be seen in his ‘wedge’ metaphor, this being his comparison of the face of Nature to “a yielding surface, with ten thousand sharp wedges packed close together and driven inward by incessant blows”.³⁰² Organisms, in other words, battle for place in nature, forced into conflict by population pressures that lead to the expulsions or deaths of those less secure individuals and species. There is no inherent stability of place or type and what economy emerges, however orderly appearing, is the product of strife not pre-ordained harmony.

The tendency for favourable variations to be preserved in a state of competition is what Darwin termed ‘natural selection’. In establishing that inherent competition in nature promoted this tendency, Darwin determined that species by necessity could not remain unchanged through time, for the organic relations both between and within species are constantly changing due to the fact of adaptive variations appearing and being propagated. That the rate and extent of these processes of adaptive change is determined by the organic relations holding amongst species is affirmed by Darwin’s assertion that “Natural selection tends only to make each organic being as perfect as, or slightly more perfect than, the other

299 Charles Darwin, *On the Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life*, Penguin Classics, 2009, p. 76.

300 Charles Darwin, ‘Essay of 1844’, *The Foundations of the Origin of Species*, Francis Darwin (Ed.), London: Cambridge University Press, p. 81.

301 Dov Ospovat, ‘Darwin after Malthus’, *Journal of the History of Biology*, Vol. 12, 1979, p. 217.

302 Charles Darwin, *On the Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life*, Penguin Classics, 2009, p. 69.

inhabitants of the same country with which it has to struggle for existence”.³⁰³ By establishing that individuals in a species compete among themselves as well as with other species, indeed do so more fiercely, Darwin denied that species could survive through time unchanged merely by migrating to better conditions when ejected from their station. Organisms not only vary in nature and move to new stations within it, they also change biologically over time as successful new variations are promoted to the detriment of older forms. The capacity for organisms to move and change within the economy of nature could now be argued to be little different from their mobility and plasticity within the human economy. As was observed to be the case with Linnaeus' economy of nature, we can see Darwin as pursuing a strategy of cognitively domesticating wild nature by imposing categories of analysis from discourses of domestication and society, and therefore attaining what I have referred to as domestication by analogy (see my introduction). Where nature was once seen as characterised by constancy and equilibrium, Darwin imported the language of human social competition from Malthusian economics and gradual but continual change through selection from breeders in order to reinterpret nature as unstable, vicious and evolving. By drawing such analogies, Darwin could maintain that there were no grounds for considering domestic and natural varieties as differing in kind. And yet, despite this Darwin himself used the term ‘wild type’, if occasionally, and it survived the Darwinian revolution, indeed increased in popularity. Why might this be? In order to understand wild type’s survival, it is worthwhile establishing how Darwin himself understood it, and in order to do this, it is important to first ascertain how the term was used by other naturalists with whom Darwin corresponded and shared ideas with.

The Use of ‘Wild Type’ by Blyth & Hooker

In pursuing his research for his evolutionary theories, Darwin established a sprawling and international web of communications with naturalists, animal and plant breeders, and other scientists and practitioners working in fields relevant to his enquiries. Of these many correspondents, only one, however, significantly stands out in terms of his use of the term ‘wild type’, this being Edward Blyth, with whom Darwin shared a particularly extensive series of letters between 1855 and 1858. Blyth had already discussed ‘wild type’ in 1840, when he presented ‘An Amended List of the Species of the Genus Ovis’ to the Zoological

303 Ibid., pp. 183-4.

Society of London. Here he had observed that, regarding the Domestic Sheep, *Ovis Aries*, “it is still remarkable that we have certainly not yet discovered the principal wild type, or indeed any species with so long a tail as in many of the domestic breeds, which I cannot doubt existed also in their aboriginal progenitors”, going on to say that the best candidate for such a principal wild type is ‘*O. Gmelinii*’ of central Persia, which corresponds with a prediction he had made in a previous paper that “a wild Sheep more nearly resembling the domestic races than any hitherto discovered would yet occur somewhere in the vicinity of the Caucasus”, this prediction appearing to be based on a belief that sheep were domesticated at the same time and place as goats, whose domestic races he believes are “derived exclusively from the Caucasian *C. Aegagrus*”.³⁰⁴ Blyth had also earlier in 1835 discussed how “The common domestic breeds of the rabbit, ferret, guinea-pig, turkey, goose, and duck... have, in the course of generations, become much larger and heavier (excepting, however, in the case of the turkey) than their *wild prototypes* [emphasis added]”.³⁰⁵ Blyth was thus using ‘wild type’ and its closest synonyms at an early stage in his writings, mainly as an expression of his interest in determining the origins of domestic species and their relative physiological changes compared to their wild ancestors.

Darwin’s regard for Blyth’s zoological expertise considerably predated their establishing an extended correspondence, in 1848 recommending him to Joseph Dalton Hooker who was on an expedition to India where Blyth was based.³⁰⁶ His particular reasons for contacting Blyth in 1855 can be related to certain evidence and opinion he desired regarding the nature of variation under domestication. In particular, Darwin was seeking further proofs to substantiate his view that most domestic variation was the product of the artificial selection of a single or a few original stocks. He was especially concerned to refute the extreme polygenism professed by some animal and plant breeders, a view he characterised as the “doctrine of the origin of our several domestic breeds from several aboriginal stocks”.³⁰⁷ He was also seeking to establish whether or not domestic varieties had a strong tendency towards

304 Edward Blyth, ‘An Amended List of the Species of the Genus *Ovis*’, *Proceedings of the Zoological Society of London*, Volume 8, Issue 1, 1840, p. 74.

305 Edward Blyth, ‘An Attempt to Classify the "Varieties" of Animals, with Observations on the Marked Seasonal and Other Changes Which Naturally Take Place in Various British Species, and Which Do Not Constitute Varieties’, *Magazine of Natural History*, Vol. 8, No. 1, 1835, pp. 45-46.

306 Charles Darwin, Letter to Joseph Dalton Hooker, 10th May 1848. Darwin Correspondence Database, <http://www.darwinproject.ac.uk/entry-1174> accessed on Thu Nov 29 2012.

307 Charles Darwin, *On the Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life*, Penguin Classics, 2009, p. 27.

reversion, for by showing that characters acquired in domesticity were not lost Darwin could prove that modification by selection could be conserved.³⁰⁸ In response to these enquiries by Darwin, Blyth systematically detailed what he knew and believed regarding domestic animals and their origins. Darwin treated Blyth's disclosures on this matter as authoritative, stating later in the *Origin* that "I should value [his opinion on this subject] more than that of almost any one".³⁰⁹

In his correspondence with Darwin, Blyth utilises knowledge of wild types as a basic means of initially classifying domestic organisms into groups, observing that "The varieties of domestic animals may be conveniently distributed into 1, Those of which we know the origin,—2, Those of which the origin (from existing wild types) is probable,—and 3, Those of which the origin is unknown".³¹⁰ When it comes to determining the origins of domesticated organisms, Blyth believes that for certain species all the varieties can be traced to a single wild type, as in the case of the canary. For many other domestic species, however, Blyth asserts there to have been multiple wild types, notably maintaining that regional varieties of dog are the product of the corresponding species of wolf found in the area, stating "Is not the Tibetan Mastiff a development of the T. Wolf, as the Newfoundland Dog is of the Arctic, & the St. Bernard's dog of the European W?".³¹¹ This polygenic belief in multiple wild types is partly facilitated by Blyth's perceiving that in many genii there are "clusters of *species* or *varieties* or *races*" that are "excessively affined", and though they will usually produce sterile offspring in the wild "would more probably blend under favorable circumstances, & more especially if domesticated" (This idea that domestication facilitated hybridisation, and that this explains the greater variability of domestic species, was originally proposed by the German naturalist Peter Simon Pallas in the later 18th century). He believes interspecific blending to have occurred "in the instance of the different races affined to *C. livia* [the pigeon], & the Dogs derived from different wild Canines, & ditto Cats, & Hogs, & Sheep", as

308 Jean Gayon, *Darwinism's Struggle for Survival: Heredity and the Hypothesis of Natural Selection*, Cobb, Matthew (Trans.), Cambridge University Press, 1998, pp. 38-39.

309 Charles Darwin, *On the Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life*, Penguin Classics, 2009, p. 27.

310 Edward Blyth, Letter to Darwin, 30th September or 7th October 1855. Darwin Correspondence Database, <http://www.darwinproject.ac.uk/entry-1761> accessed on Tue Oct 23 2012.

311 Edward Blyth, Letter to Darwin, 21st April 1855. Darwin Correspondence Database, <http://www.darwinproject.ac.uk/entry-1792> accessed on Tue Oct 23 2012.

well as the Horse, which Blyth regards “a blended species, from several wild types”, based especially on the variation evident in ponies.³¹²

As to the question of whether domestic varieties revert to the wild type, Blyth’s views are complex, as can be ascertained from his 1835 paper ‘An Attempt to Classify...’, in which he distinguishes between four kinds of variations, these being simple variations (i.e. ordinary deviations from the mean), acquired variations, breeds and true variations. Blyth believes many of the novel variations found in domestic organisms, especially those of stature, to be mere acquired variations, a consequence of organisms being “supplied regularly with abundance of very nutritious, though often unnatural, food, without the trouble and exertion of having to seek for it”. He thought these oversized and dysfunctional varieties “if turned loose into their natural haunts, would most probably return, in a very few generations, to the form, size, and degree of locomotive ability proper to the species when naturally conditioned”.³¹³ It is on these grounds that Blyth later affirms his belief to Darwin that “we may seek in vain for wild types of *G[allus] giganteus*” (Blyth refers to the asil, a South Asian giant variety of chicken, which he clearly thought a mere acquired variation).³¹⁴ Elsewhere, he informs Darwin that “All the largest Dogs are from cold climates, & all dogs tend to revert to the wild type in hot climates”, as a consequence of being removed from the environment inducing their variation towards large stature (though he considers the Cuban mastiff as a possible exception to this rule of correlation between climate and size).³¹⁵ Blyth’s views on ‘acquired variations’ and their susceptibility to reversion are in many ways not unusual, having roots in the old belief in the capacity of excess nutriment to stimulate variability and in the 18th century idea that many variations in species can be regarded as degenerations induced by exposure to novel climatic conditions. His views on ‘breeds’, on the other hand, are likely derived from William Lawrence, one of his major sources for his 1835 essay. Lawrence observed that when humans impose controls on domestic animal reproduction they

312 Edward Blyth, Letter to Darwin, 7th September 1855. Darwin Correspondence Database, <http://www.darwinproject.ac.uk/entry-1752> accessed on Tue Oct 23 2012.

313 Edward Blyth, ‘An Attempt to Classify the "Varieties" of Animals, with Observations on the Marked Seasonal and Other Changes Which Naturally Take Place in Various British Species, and Which Do Not Constitute Varieties’, *Magazine of Natural History*, Vol. 8, No. 1, 1835, pp. 45-46.

314 Edward Blyth, Letter to Darwin, 23rd January 1856. Darwin Correspondence Database, <http://www.darwinproject.ac.uk/entry-1825> accessed on Wed Oct 24 2012.

315 Edward Blyth, Letter to Darwin, 21st April 1855. Darwin Correspondence Database, <http://www.darwinproject.ac.uk/entry-1792> accessed on Tue Oct 23 2012.

can “effect the most surprising changes in form and qualities”.³¹⁶ Blyth’s ‘breeds’ are the products of such interventions, being “for the most part, artificially brought about by the *direct agency of man*” and therefore “if man did not keep up these breeds by regulating the sexual intercourse, they would all naturally soon revert to the original type”.³¹⁷ Blyth believed the hereditary variations isolated in breeds originated in nature, not domesticity, stressing to Darwin that the wild relatives of domestic varieties are usually quite variable. Blyth gave evidence to Darwin of this variability of wild stocks by sending him skins of the red junglefowl, believed ancestor of the domestic chicken, commenting that his specimens were “illustrative of the variation observable among [junglefowl]”, the length of the tarsus being particularly remarkable in its variability.³¹⁸ Nonetheless, whilst Blyth believed wild stocks to be variable, he also thought that “the original and typical form of... a species is *unquestionably* better adapted to its *natural* habits than any modification of that form; and... the weaker... in a state of nature, is allowed but few opportunities of continuing its race”. Variation from the typical is therefore in nature rarely maintained, but does provide raw material for breeders to derive distinct varieties, breeds which, however, are liable to revert in a state of panmixia. For Blyth, only ‘true varieties’ are relatively invulnerable to some form of reversion to the wild type, as they remain constant in new environments and when crossed, and this is on account of their being “in fact, a kind of deformities, or monstrous births, the peculiarities of which... would very rarely, if ever, be perpetuated in a state of nature; but which, by man's agency, often become the origin of a new race”. Their lack of tendency to reversion he puts down to the fact that “most probably, [the original form] could only be restored, in a direct manner, by the way in which the variety was first produced”, i.e. through a monstrous deviation back towards the type.³¹⁹

Overall, Blyth did provide some theoretical and empirical evidence to support Darwin’s position that many domestic varieties did not have a direct wild counterpart, contrary to the

316 William Lawrence, *Lectures on Comparative Anatomy, Physiology, Zoology, and the Natural history of Man* (8th Ed.), London: John Taylor, 1840, pp. 178-179.

317 Edward Blyth, ‘An Attempt to Classify the "Varieties" of Animals, with Observations on the Marked Seasonal and Other Changes Which Naturally Take Place in Various British Species, and Which Do Not Constitute Varieties’, *Magazine of Natural History*, Vol. 8, No. 1, 1835, pp. 46-47.

318 Edward Blyth, Letter to Darwin, 23rd January 1856. Darwin Correspondence Database, <http://www.darwinproject.ac.uk/entry-1825> accessed on Wed Oct 24 2012.

319 Edward Blyth, ‘An Attempt to Classify the "Varieties" of Animals, with Observations on the Marked Seasonal and Other Changes Which Naturally Take Place in Various British Species, and Which Do Not Constitute Varieties’, *Magazine of Natural History*, Vol. 8, No. 1, 1835, pp. 46-48.

views of some breeders. As to what the source of this novel variability was, Blyth endorsed a combination of hybridisation between closely related species, excessive nourishment and the isolation of otherwise rare hereditary variations through selective breeding as being the main factors responsible. These were all causal influences Darwin acknowledged as possible causes of variation under domesticity. A manner in which Blyth differed was that unlike Darwin he did not consider the products of domestication as having much inherent stability independent of human interventions. Without excess nourishment, domestic giants lost their size within a few generations. Similarly, the distinctive characters of breeds were thought to rapidly disappear if human control over reproduction was relaxed – Darwin, by contrast, came to insist that artificial selection could produce breeds in which the inheritance of distinctive traits was relatively permanent. Even ‘monstrous’ variations, which were strongly inherited, were not thought by Blyth to be able to survive without human help, these being regarded as pathological traits that would be the doom of an organism in the wild. The only kind of variation Blyth may have thought to have a good likelihood of surviving in the medium term in nature would be that variability derived from interspecies hybridisation, as this was both inherited and not inherently debilitating. Nonetheless, Blyth appears to have thought hybrids unlikely to maintain themselves especially long, as he believed there to be a “law... intended by Providence to keep up the typical qualities of a species”, whereby variations from the type always tended to be outcompeted in nature by typical individuals.³²⁰ Struggle existed in nature for Blyth as much as it did for Darwin, but for Blyth it tended to take the form of ‘stabilising selection’. The species type represented an optimal fitting to its place in the economy of nature and the atypical by default were less fit and therefore eliminated in struggle. Hybrids, being intermediary between their parent stocks, would also tend to perish in competition with their wild type relatives as they would be adapted for neither of their parent species’ natural stations. From this it can be seen that in contrast to Darwin, Blyth in his writings before 1859 appears to have still retained a notion of the economy of nature as relatively unchanging, species having set offices which the typical individual was best suited to carrying out, the atypical, ineffective and other deviations being weeded out by competition and predation.

320 Ibid., p. 46.

The idea espoused by Blyth and other contemporary naturalists that domestic varieties had a strong tendency to reversion if human control over them was allowed to slip was challenged shortly before the publication of the *Origin* by Darwin's close associate, Joseph Dalton Hooker. Hooker had himself once been a conventional believer in species fixity but claims in the introduction to his *Flora of Australia* that the presentation of Darwin and Wallace's papers on evolution at the Linnaean Society in 1858 had caused him to revise his views.³²¹ Evidence from his correspondence with Darwin suggests he may have shifted in his views considerably earlier, in an 1844 letter stating that whilst there was clearly an original creation of plants "we can hardly suppose that we have now only the remains of that original stock", an argument he based on the existence of endemic genera on islands of recent geological origin. He therefore suggested that "There may in my opinion have been a series of productions on different spots, & also a gradual change of species".³²² It is therefore clear that Hooker already had misgivings at an early stage with the idea of all species as originally created and unchanging. However, it was only in 1859 that he chose to publically express these views, and his main target was the "prevalent opinion... that there is a tendency in cultivated, and indeed in all varieties, to revert to the type from which they departed". Hooker, admitting himself to have once "quoted this opinion, without questioning its accuracy" when he supported the view of species as permanent, affirmed that he had come to "doubt the existence of this centripetal force in varieties, or at least to believe that in the phrase 'reversion to the wild type', many very different phenomena are included".³²³

Against this 'prevalent view', Hooker observes that "the majority of cultivated vegetables and cerealia... show when neglected no disposition to assume the characters of the wild states of these plants", that moreover "the great and acknowledged difficulty of determining the wild parent species of most of our cultivated fruits, cerealia, etc. ... would not be so were there any disposition in the neglected cultivated races to revert to the wild form". Neglected cultivated plants rather "certainly degenerate, and even die if Nature does not supply the conditions which man... has provided; they become stunted, hard, and woody, and resemble their wild progenitors in so far as all stunted plants resemble wild plants of similar habit; but

321 Joseph Dalton Hooker, *On the Flora of Australia: Its Origin, Affinities and Distribution*, London: John Edward Taylor, 1859, p. ii.

322 Joseph Dalton Hooker, Letter to Charles Darwin, 29th January 1844. Darwin Correspondence Database, <http://www.darwinproject.ac.uk/entry-734> accessed on Sun Dec 2 2012.

323 Joseph Dalton Hooker, *On the Flora of Australia: Its Origin, Affinities and Distribution*, London: John Edward Taylor, 1859, p. viii.

this is not a reversion to the original type, for most of these cultivated races are not merely luxuriant forms of the wild parent". He then attacks the related idea that "by imitating the conditions under which the wild state of a cultivated variety grows, we may induce that variety to revert to its original state", stating that "except in the false sense of reversion above explained [loss of luxuriance], I doubt if this is supported by evidence. Cabbages grown by the seaside are not more like wild Cabbages than those grown elsewhere, and if cultivated states disseminate themselves along the coast, they there retain their cultivated form". He concluded by stating that "Nature operates upon mutable forms by allowing great variation, and displaying little tendency to reversion".³²⁴ Hooker thus rejected the notion that conditions of domestication acted as an interference in the expression of the species type, maintaining that human neglect damaged domestic plants as opposed to allowing them to revert to type. He also rejected the idea that exposure to the wild type's native conditions would cause a domestic strain to adaptively revert to its original form, observing that it retained its cultivated characteristics. This retention of domestic form suggested another factor was in play aside from the environment in determining species typicality and variation in the wild and domesticity, namely heredity. I shall discuss the role of late 19th century theories of heredity and variation on the development of the wild type concept in Chapter 7.

The Use of 'Wild Type' by Darwin

Determining what views Darwin had on 'wild type' is on the surface a difficult task given his use of the term is very occasional. He does not use the term in the *Origin* and does not appear to have utilised it before 1868, when it appears in *Variation of Animals and Plants under Domestication*. Nevertheless, whilst 'wild type' itself does not appear in the *Origin*, a substantial number of synonyms and near synonyms are used in its place, including 'proper type', 'wild prototype', 'aboriginal stock', 'wild stock', and 'aboriginal species'.³²⁵ It might be asked why, given his broad use of synonyms and near synonyms, Darwin does not use 'wild type' in the *Origin*. He was certainly familiar with the term, not least given his lengthy correspondence with Blyth, who used it relatively frequently. He may have simply disliked the term, but then again he utilised it later on several times in *Variation*. A more interesting suggestion is that he was concerned that using 'wild type' could potentially mislead readers

³²⁴ Ibid.

³²⁵ Charles Darwin, *On the Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life*, Penguin Classics, 2009, pp. 100 & 169 [proper type]; p. 27 [wild prototype]; pp. 23, 24 & 27 [aboriginal stock]; p. 27 [wild stock]; pp. 28, 408 & 228 [aboriginal species].

as to his views on nature and domestication. As I have previously maintained, the wild type concept originated out of the idea that domestic and wild organisms were different in kind due to the former being natural productions and the latter artificial. Darwin wanted to establish that the processes underlying variation (as opposed to the practices of selection) were relatively uniform across the domestic-wild divide. In using more neutral terms such as ‘aboriginal species’ and ‘proper type’, which do not indicate whether the derivation of their descendants is natural or artificial, Darwin may have sought to get around any potential presumptions regarding the meaning of wild type that could lead to his work being misinterpreted. Additionally, he may have felt wild type a limited term given it was only applied to the ancestors of domestic organisms, a problematic limitation if we consider that he was considering the descent of all life, not just the descent of domestic varieties.

Darwin appears to have had less qualms using ‘wild type’ in *Variation*, perhaps since he was almost exclusively dealing with domestic varieties and their ancestors in this piece of work so was less afraid of misinterpretation and didn’t feel the need for a neutral language applying both to variation in nature and domestication. Darwin’s actual use of ‘wild type’ in *Variation* takes two primary forms; that of being a means of referring to domestic varieties’ ancestors and that of being an ancestral form which domestic varieties have a tendency to revert to. The two major uses which refer directly to the ancestry of domestic stock are with reference to the progenitors of the domestic dog. Darwin accepted polygenism in the case of the dog, stating that it is “highly probable that the domestic dogs of the world are descended from two well-defined species of wolf (viz. *C. lupus* and *C. latrans*), and from two or three other doubtful species (namely, the European, Indian, and North African wolves); from at least one or two South American canine species; from several races or species of jackal; and perhaps from one or more extinct species”. One of the major sources of evidence for this is that although Darwin believes it possible that introduced domestic dogs might after several generations have acquired traits convergent with those of local wild canids “we can hardly thus account for introduced dogs having given rise to two breeds in the same country, resembling two of its aboriginal species”. One candidate example of dogs with different wild ancestors in neighbouring locales is in Egypt and Nubia, Darwin citing Ehrenberg who “asserts that the domestic dogs of Lower Egypt, and certain mummied dogs, have for their wild type a species of wolf (*C. lupaster*) of the country; whereas the domestic dogs of Nubia and certain other

mummied dogs have the closest relation to a wild species of the same country, viz. *C. sabbar*, which is only a form of the common jackal".³²⁶ Darwin tempers this polygenism by maintaining that "It is notorious how greatly the mental disposition, tastes, habits, consensual movements, loquacity or silence, and tone of voice have varied and been inherited in our domesticated animals. The dog offers the most striking instance of changed mental attributes, and these differences cannot be accounted for by descent from distinct wild types".³²⁷ Darwin thus accepts some degree of polygenism in particular domestic species on the balance of physiological and behavioural evidence but refuses to countenance the 'doctrine of the origin of our several domestic breeds from several aboriginal stocks', previously critiqued in the *Origin*, as this would deny the power of selection to direct variation.

Darwin's most notable consideration in *Variation* of the relationship between wild type and reversion is with reference to pigs. He identifies feral pigs as being the example on which "[t]he common belief that all domesticated animals, when they run wild, revert completely to the character of their parent-stock, is chiefly founded". He does not dismiss this belief but rather seeks to deflate it, affirming that on the basis of the evidence given that "with pigs when feral there is a strong tendency to revert to the wild type; but that this tendency is largely governed by the nature of the climate, amount of exercise, and other causes of change to which they have been subjected". Thus he observes that whilst it is true that feral pigs "reassume their original bristly covering", he points out that they do so "in different degrees, dependent on the climate", those at high altitude being very hirsute, whilst those in hot climates are thinly haired. Similarly with regard to colour, the general tendency to return to those of the wild boar is reported, but deviations from this appear in certain environments, such as the red coats of feral pigs in Jamaica and other tropical climates. Darwin also ascribes the reversion to the general shape and proportions of the wild boar as being what "might have been expected from the amount of exercise which [feral pigs] are compelled to take in search of food".³²⁸ In this passage, Darwin thus does not deny the power of the conditions of existence to affect change in the form of feral organisms. He rather points to there being nothing strange about these changes, being that they are adaptive, and that if there is a

326 Charles Darwin, *The Variation of Animals and Plants under Domestication: Vol. 1*, 2nd ed., London: John Murray, 1888, pp. 25-27.

327 Charles Darwin, *The Variation of Animals and Plants under Domestication: Vol. 2*, 2nd ed., London: John Murray, 1885, p. 404.

328 Charles Darwin, *The Variation of Animals and Plants under Domestication: Vol. 1*, 2nd ed., London: John Murray, 1888, pp. 81-82.

tendency to reversion, it is to be thought of not in terms of a return to type but rather as the reformation of the organism based on its reutilisation of adaptive faculties rendered dormant by domestication. That the degree of reformation differs depending on the nature of the new environment into which domesticated organisms are released is clear from examples such as differences in hair growth according to climate. This is not a product of the organism experiencing some normative force of Nature from which it has previously been shielded by human hands, but rather an expression of the organism's plasticity in responding to changes in circumstance, enabling it to survive in the struggle for life.

Darwin on the Parallels of Nature and Domestication

Darwin's occasional use of 'wild type' should not distract us from the fact that he was very much determined to show that the same laws of variation operated in both the wild and in domestication, that there was, in other words, no significant difference between the products of nature and of human husbandry. This required him to explain away some apparently obvious differences between variation in nature and in domestication, most pointedly the fact that variation in the wild appeared "rare and evanescent", as one of Darwin's sceptics, John Crawford put it, whereas it appeared near ubiquitous in domesticity.³²⁹ Darwin's strategy to combat these doubts was to lift from Lyell's uniformitarianism the two principles of gradualism and actualism. He argued that variation in nature was very much real but occurred at a rate too slow to be easily appreciated in everyday life. It was made further invisible by the fact that its causes are mundane. Minute adaptive variations accumulate over time within a population as a consequence of the selective pressures of competition and the conditions of existence weeding out maladapted individuals. The production of new varieties and subsequently new species occurred on a similar timescale to geological processes like the raising and wearing down of mountain ranges. Both evolutionary and geological processes leave behind more than enough evidence of their occurring for us to ascertain the fact of their having happened. That these processes could not be directly observed by the human eye did not make them any less real. By maintaining that nature worked in the same manner and with the same material as the human breeder, just at a slower rate, Darwin was able to utilise the evidence of variation under domestication as a means of arguing for the possibility that the

³²⁹ John Crawford, 'On the Theory of the Origin of Species by Natural Selection in the Struggle for Life', *Transactions of the Ethnological Society of London*, Vol. 7, 1869, p. 29.

myriad life forms of Earth could have diverged over a vast expanse of time from a common origin.³³⁰

A further intuition which Darwin's claim of a uniformity of laws of variation across the wild-domestic divide came into conflict with was the belief in there being a difference in kind between nature and art, and between the products of nature and art. Most obviously, humans produce art consciously, whereas nature is not conscious in its 'acts', so the argument goes that human products must be intrinsically different from those of nature. Provocatively, Darwin sought to portray the difference between human art and nature's acts as not a matter of our consciousness enhancing our powers but rather as being central to the limitations of our powers when compared to nature. Art, he affirmed, is but an imperfect imitation of the effects of nature, the works of which are "immeasurably superior to man's feeble efforts".³³¹ For Darwin, the difference between art and nature lies in their capacity to perfect. In the case of organisms, nature is vastly more capable of perfectly adapting its creatures according to their needs, whereas human beings cannot shape their domestic varieties perfectly to their requirements. This is because "Man can act only on external and visible characters", whereas nature "cares nothing for appearances, except in so far as they may be useful to any being", and can act "on every internal organ... on the whole machinery of life".³³² Human beings, in other words, cannot manipulate nature for their own ends to the extent that nature can alter itself as humans can and will never comprehend the totality of nature's operations, whereas nature can always utilise its own laws as it does not first require knowledge to do so. Nature's capacity to affect itself will therefore always exceed human capacity to affect nature, and nature will always be stronger than art. Darwin thus rejected the view inherent in the Aristotelian 'Natural State Model', whereby art is seen as operating through interfering in the course of nature, in favour of interpreting art as the imperfect conscious rendition of nature's unconscious operations.

By asserting art and nature to operate through the same kind of processes, the conscious mind being no more inventive in its capacity to generate outcomes than inanimate natural forces,

330 For details on Lyell's gradualism and actualism see: Dov Ospovat, 'Lyell's Theory of Climate', *Journal of the History of Biology*, Vol. 10, No. 2, 1977, pp. 317-339. For Lyell's influence on Darwin, see: Michael Ghiselin, *The Triumph of the Darwinian Method*, USA: Dover Publications, 2003, pp. 13-31.

331 Charles Darwin, *On the Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life*, Penguin Classics, 2009, p. 64.

332 Ibid., pp. 82-83.

Darwin was enabled to claim that not only was his evolutionary power of natural selection analogous to the artificial selection practiced by breeders, but moreover that they were both in fact instances of a general principle of selection. This “great principle” was defined by Darwin as operating under any circumstances in which there exists variability, favourable, neutral and negative. If a favourable variation is heritable and if other variations in the same direction can accumulate, selection is enabled to create new forms of life.³³³ Whether the selector is human or natural can only influence the process by changing fitness conditions. The principle of selection operates in the same manner whether a variation is favoured due to its enabling escape from predation or because it appeals aesthetically to breeders. Selection operates across the boundaries of the natural and human economy, there existing, Darwin believed, a continuum of selection. At one extreme is ‘Methodical Selection’, where human knowledge and intention to modify are highly evident, at the other end is natural selection, with ‘Unconscious Selection’ being intermediate, this being the practice of selecting ‘the best’ of a breed without intention to modify it.³³⁴ A continuum of selection implied that the border between nature and domestication was itself continuous and not rigidly demarcated. Given that this outlook implies that we can never firmly distinguish between the wild and the domestic without some degree of arbitrariness, this further substantiates the claim of this chapter that Darwinian theory undermined the source of wild type’s original meaning, which was predicated on a rigid wild-domestic distinction and assumed a ‘Natural State Model’ of art and nature to hold.

Defenders of the Natural State Model

Darwin’s denial of the ‘Natural State Model’ and of a rigid wild-domestic distinction was itself rejected by many of his contemporary naturalists. Defending its legitimacy thereby became a primary means of attacking Darwinism. Dissenters did not, however, only include critics but also some of Darwin’s close allies and associates. The most notable of these was Alfred Russel Wallace. Whilst commonly his evolutionary ideas are perceived as differing little from Darwin’s, they rested on some conflicting presumptions that here come to light. Wallace believed the distinction between domestic and wild varieties to be real, the latter being ‘permanent’ and not tending to revert to their original type, whereas domestic varieties

333 Charles Darwin, *The Variation of Animals and Plants under Domestication: Vol. 2*, 2nd ed., John Murray, 1885, pp. 176-177.

334 Charles Darwin, *On the Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life*, Penguin Classics, 2009, p. 40.

he thought to necessarily revert if turned feral, or otherwise perished. The reason why reversion occurred in domestic varieties but not in wild ones was because, Wallace claimed, the processes by which they differentiated from their ancestors were different. Varieties formed in the wild through the struggle for existence, with favourable new varieties tending to displace less advantaged older types. For any variety to survive this internecine competition in the long-term, its individuals were thought to “depend upon the full exercise and healthy condition of all their senses and physical powers”. Domestication was believed by Wallace to suspend the struggle for existence, with the result that the capacities of domestic organisms “are only partially exercised, and in some cases are absolutely unused”. These conditions allow that “all variations have an equal chance of continuance”, whereas in the wild all suboptimal variation will lead to extirpation. Domestic organisms thus have an “abnormal, irregular, artificial” nature, many of them being “so far... removed from that just proportion of faculties... by means of which alone an animal left to its own resources can preserve its existence” that they are “depend[ent] altogether on human care”. Once human care is removed, however, domestic varieties “must return to something near the type of the original wild stock, or become altogether extinct”.³³⁵ As a consequence of these beliefs, Wallace did not believe domestic varieties could act as a model for variation in the wild, something unacceptable for Darwin given the importance of domestication as a source of evidence for variability. This disagreement between the two naturalists can simply be understood as a difference in their understanding of how art and nature operate. For Wallace, domestication intervenes in natural processes and suspends their influence, with the consequence that otherwise unfit varieties survive and prosper. For Darwin, human intervention merely changes the parameters of selection, the influence of the principle holding whether what is useful for survival and propagation is better obtaining wild food or better meeting human desire for meat.

Of those naturalists and scientists who defended a version of ‘Natural State Model’ as part of criticising Darwin, as opposed to merely disagreeing, among the most ardent and telling arguments were those made by Fleeming Jenkin in 1867. Jenkin denied that speciation might

335 Alfred Russel Wallace, ‘On the tendency of varieties to depart indefinitely from the original type’, In: *Contributions to the Theory of Natural Selection*, Second Edition, WHS Corrections and Additions, Macmillan and Co., 1871, pp. 26-34 & 38-41; & Jean Gayon, *Darwinism’s Struggle for Survival: Heredity and the Hypothesis of Natural Selection*, Matthew Cobb (Trans.), Cambridge University Press, 1998, pp. 19-59.

occur in nature through the same processes as those that produced domestic varieties. Artificial selection had yet to create a new species and Darwin's claim that all that was required for nature to do such work was more time was for Jenkin counterintuitive, believing it could not be inferred that "if six or sixty years can make a pouter out of a common pigeon, six myriads, may change a pigeon to something like a thrush". Jenkin then introduces a very telling analogy, comparing the idea that speciation could occur through the same processes operating in artificial selection to the inference that "because we observe that a cannon-ball has traversed a mile in a minute, therefore in an hour it will be sixty miles off, and in the course of ages that it will reach the fixed stars". Jenkin takes this analogy as exemplifying how nature works in general, that in the case of organisms it suggests that "the rate of variation in a given direction is not constant, is not erratic; it is a constantly diminishing rate, tending therefore to a limit". This assertion is, he considers, backed by the evidence of breeders' experiences, as it is "established for all cases of man's selection" that "deviation from an average individual can be rapidly effected at first", but that then "the rate of deviation steadily diminishes till it reaches an almost imperceptible amount" and the tendency to revert towards the type becomes overwhelming. He conceptualises this in terms of species as existing within a "sphere of variation", with "the average animal at the centre" and all variation as tending towards this norm as opposed to the surface of the sphere. For Jenkin, the typical is therefore an organism's natural and normative state, and species are fixed in their true form and place in nature even if the possibility of adaptive variation exists. Human art acts against this natural tendency towards type and, whilst it achieves rapid early success in promoting deviation, suffers diminishing returns and cannot ultimately resist nature, for as the edge of the 'sphere' is approached, reversion to type becomes as inevitable as a cannonball's falling to Earth.³³⁶

Jenkin acknowledges that he here presents a very different view of the nature of variation to Darwin, whom he ascribes the belief that "there is no typical or average animal, no sphere of variation, with centre and limits" as inheritance tends to ensure that "the child is more likely to resemble its father than its grandfather, its grandfather than its great-grandfather, etc.", which allows cumulative variation across the generations and renders reversion to distant

336 Anonymous [Fleeming Jenkin], '(Review of) "The origin of species"', *The North British Review*, Vol. 46, 1867, pp. 280-284; & Jean Gayon, *Darwinism's Struggle for Survival: Heredity and the Hypothesis of Natural Selection*, Matthew Cobb (Trans.), Cambridge University Press, 1998, pp. 85-102.

ancestors unlikely. Jenkin presents his as the opposing view of “a race maintained by a continual force in an abnormal condition, and returning to that condition so soon as the force is removed”. He allows that, “*A priori*, perhaps, one view is as probable as the other”, but is adamant that the empirical evidence is on his side.³³⁷ The two competing paradigms identified by Jenkin, one of variation as operating around a fixed norm and within limits, the other of variation as unlimited in the long-term and relative to heredity, would continue to play an important role in evolutionary debates through the rest of the 19th century and beyond. Determining which of these models of variation was correct depended on developing a scientific understanding of the workings of heredity, which would require its eventual domestication and deconstruction. I will leave this issue of the nature of heredity and variation for now but return to it in Chapter 7.

Conclusion

Whilst Darwin did not himself use the term ‘wild type’ with regularity, it may be observed that evolutionary theory, and in particular Darwin’s notion of common descent, did greatly change the status of ‘wild type’. There had previously been an assumption that variation in both domestication and the wild, whilst both caused by organisms encountering new conditions of existence, were fundamentally different in kind. Though variation in the wild occurred passively through natural mechanisms, variation in domestication was actively forced through human intervention and was therefore artificial. Darwin, by drawing analogies between wild and domestic, was able to argue that nature was just as artful as the human breeder and infinitely more patient and perceiving. All the breeder’s artifice did was therefore speed up the course of nature, and therefore the variation they produced did not differ in kind from that found in the wild. By establishing these commonalities, Darwin allowed the familiar language of domestication to be extended to the wild, rendering nature more comprehensible and more amenable to scientific investigation (including, ultimately, scientific domestication).³³⁸ However, by denying there to be a difference in the kind of variation found in the wild and domestication, Darwin also denied the normative power of wild types over domestic varieties. The wild type was instead merely an ancestor from which

³³⁷ Ibid.

³³⁸ Cornell has similarly claimed that Darwin, in establishing his analogy between domestication and the wild, moved from a naturalistic interpretation of variation as driven by forces internal to the organism to a “technological” theory of nature as “picking” variations in much the same artful fashion as breeders. See: John F. Cornell, ‘Analogy and Technology in Darwin’s Vision of Nature’, *Journal of the History of Biology*, Vol. 17, No. 3, 1984, pp. 303-344.

domestic varieties had diverged and could be compared with but to which state they would never truly return. Darwin moreover explained typicality, commonly thought a normative property of species, as a mere effect of heredity's tendency of like to produce like, and reinterpreted variation as being not a perturbation of the type but instead a potential source of new types that would eventually displace their parent stock. These types moreover had not existed for time immemorial but were instead portrayed as emerging out of the struggle for existence, having forced out the less well adapted from the economy of nature. The difference between domestication and the wild was simply that different variations were favoured and different variations died under the human economy. The principles of selection and nature of variation remained however the same.

To conclude, I refer back to the three main manners in which 'wild type' was used in the first half of the 19th century, which I listed at the beginning of this chapter. Darwinian evolutionary theory reduces wild type's direct applicability to the first of these uses only, that of wild type as referring to the posited ancestors of domestic species, and also their untamed descendants, what one might call the 'genealogical' meaning of wild type. The second usage of wild type, as a point from which to measure the physiological and behavioural divergence of domestic varieties (the 'comparative' wild type), is permitted to a certain degree by Darwin. However, the use of wild relations to measure the deviation of domesticated relatives is problematised by the existence of a struggle for life, which ensures that populations exhibit constant if gradual variation. These wild relatives will therefore have also undergone change in the time since their divergence from their domestic cousins, though more slowly, so measurement can never be truly accurate. The wild individual rather stands in as a modern day equivalent of the ancient wild ancestor of domestic lineages but is never fully representative of it. The third use of wild type, as referring to what is returned towards in cases of reversion, particularly cases of degeneration brought on by ferity, which I consider the 'normative' meaning of the term, is almost wholly rejected by Darwin, as can be seen vividly in his disagreements with Jenkin regarding the nature of heredity. It is this version of wild type which was most heavily tied to the Natural State Model, assuming as it did that the type is the natural state of the species and variation as due to interference. As will be shown in the following chapters, Darwin's rejection of all but the first use of wild type as problematic did not stop other naturalists using wild type in its second and third sense.

Moreover, the belief in the comparative value of wild types and in their having normative properties continued to play a prominent role in both the theory and practice of later 19th century naturalists and arguably, as shall be shown, survived in the laboratory work of many early 20th century geneticists. But I will first need to show how it was that it was believed living nature could get into the laboratory, and that will be the subject of my next chapter.

Chapter 6 – Expanding Domestication, Encapsulating the Wild: the New Indoor Nature and the Birth of the Laboratory Lab Sciences, c. 1800-1880

Indoor and Outdoor Science in the Early 19th Century and the Origins of the Lab-Field Divide

In this chapter, I will illustrate some of the changes that occurred in 19th century European perceptions of the boundaries between the wild and domestic as a consequence of the establishment, through technological innovation, of new forms of indoor domestication that created new hybrid spaces for scientific investigation. These new spaces, which attempted to encapsulate fragments of wild nature, would allow an expansion of domesticating practices to allow for the detailed observation, analysis and experimentalisation of new life-forms and organic relationships. The development of these new forms of 'indoor nature' and the scientific studies associated with them would eventually lead to the development of a division in the life sciences between laboratory-based and field-based professions and practices. It was this establishment of 'indoor nature' and the developing belief that only laboratory science could produce exact, reliable scientific knowledge which would be necessary prerequisites for the movement at the end of the 19th century of 'wild types' from nature and into the laboratory.

Parallels between the lab-field divide in the late 19th and 20th century life sciences and the preceding division in post-Linnaean early 19th century natural history between indoor and outdoor science have already been observed by Robert Kohler.³³⁹ Of particular note was the division of labour between expeditionary field naturalism and museum-based systematics and taxonomy. This division in operations was based on a model of periphery, where specimens were collected, prepared and then mobilised, and centre, where incoming specimens from the field were accumulated, further processed and ordered. This kind of division of labour between collection at the periphery and 'centres of calculation' has been argued by Latour to be characteristic of those sciences where 'action at a distance' is required. Müller-Wille has convincingly claimed that 18th and early 19th century natural history was an archetypical example of such a science.³⁴⁰ As to whether this division in labour led to a differentiation in

339 Robert E. Kohler, *Landscapes and Labscapes: Exploring the Lab-Field Border in Biology*, Chicago: The University of Chicago Press, 2002, pp. 1-2.

340 Staffan Müller-Wille, 'Joining Lapland and the Topinambes in Flourishing Holland: Center and Periphery in Linnaean Botany', *Science in Context*, Vol. 16, No. 4, 2003, pp. 461-462; & Bruno Latour, *Science in Action: How to Follow Scientists and Engineers Through Society*, Cambridge, Massachusetts: Harvard

mentalities and attitude to nature, Kohler is somewhat dismissive, stating that “closet naturalists and naturalist voyagers asked essentially the same questions and dealt with the same material specimens, the only difference being that one dealt with it alive and in nature and other, dead and indoors”, and therefore concludes that “Closet science and fieldwork were two ways of doing natural history, not two distinctly different kinds of science”, as he thinks lab life science and field naturalism were to become in the late 19th century.³⁴¹

I will shortly question Kohler’s assumption that this difference of whether naturalists worked with dead or living specimens was inconsequential in the formation of boundaries between indoor and outdoor science. Before I do so, I turn to Dorinda Outram’s account of the ‘new spaces’ developing in natural history c. 1800, which Kohler draws on but disagrees on some of the finer points of. Notably, Kohler maintains that the lab-field border in biology is “probably no older than the mid-nineteenth century”, not existing prior to the ‘laboratory revolution’ of the 1840s to ‘70s when labs displaced museums as the primary centres of scientific research.³⁴² Outram on the other hand appears to see the divide between lab and field as being prefigured in the divide between museum and field, which is where the differentiation between researchers working in the open and within enclosed built spaces began. She in particular points to some 1807 comments by the great museum naturalist Georges Cuvier made in response to the field research of the great expeditionary naturalist Alexander von Humboldt. Cuvier comments that “there is as much difference between the styles and ideas of the field naturalist... and those of the sedentary naturalist, as there is between their talents and qualities”. The field naturalist traverses through a multitude of locales; the sedentary naturalist remains rooted in his study. The field naturalist is immersed in nature and sees organisms in the “full vigour of life”, whereas the sedentary naturalist cannot experience the “great scenery of nature” with the “same vivid intensity”, working only with written reports and more or less well preserved specimens. But on the other hand the field naturalist’s observations are “broken and fleeting”, whilst the sedentary naturalist may “survey all [nature’s] products spread before him... [and] compare them with each other as often as is necessary to reach reliable conclusions”. On this basis Cuvier asserts that “The traveller can only travel one road; it is only really in one’s study... that one can roam freely

University Press, 1987, pp. 215-257.

341 Robert E. Kohler, *Landscapes and Labs: Exploring the Lab-Field Border in Biology*, Chicago: The University of Chicago Press, 2002, p. 2.

342 *Ibid.*, p. 3.

throughout the universe”. The claim is thus, as Outram notes, that “true knowledge of the order of nature comes not from the whole-body experience of crossing the terrain, but from the very fact of the observer’s *distance* from the actuality of nature”. The heroics of the expeditionary naturalist (Humboldt) should not distract from the fact that the undistracted mind of the sedentary naturalist (Cuvier) is better able to grasp the truth of the order of nature. Importantly, field and sedentary naturalists are conceived by Cuvier as possessing different mindsets and experiencing nature in very different manners due to their divergent ways of life.³⁴³ Ultimately, even if, as Kohler maintains, field and museum naturalists originally complemented each other’s work through different approaches to the same questions, their divergent phenomenological experience of nature eventually produced two groups operating under distinct thought-styles whose understanding of the natural world no longer necessarily cohered, especially as fewer and fewer individuals crossed the threshold between indoor and outdoor science.

It should moreover be noted that whether or not the degree of divergence in thought-style between field and research nexus in the Cuvieran model of natural history was significant already at the beginning of the 19th century, there did exist a contemporary research tradition which was commonly critical of museum-centred collection and collation of dead specimens and instead favoured direct engagement with living nature. This was the largely German Romantic naturalism and its associated research programme of *Naturphilosophie*, which was associated with figures such as Johann Goethe and Friedrich Schelling (who popularised the term *Naturphilosophie*). Humboldt, the target of Cuvier’s deflationary account of the expeditionary naturalist, was incidentally widely respected by these Romantics. His idea of nature as a harmoniously interconnected cosmos would heavily influence Schelling, and Humboldt was himself sympathetic to many Romantic ideas, though his research programme of ‘terrestrial physics’ (containing elements of what became geology, biogeography and climatology) remained distinct from that of *Naturphilosophie*.³⁴⁴ But differences of perspective between the German Romantics and French Cuvierans did not stop at a greater respect for the field naturalist. Cuvier believed in the supremacy of his approach in part

343 Dorinda Outram, ‘New Spaces in Natural History’, In: *Cultures of Natural History*, N. Jardine, J.A. Secord and E.C. Spary (Eds.), Cambridge University Press, 1996, pp. 259-262.

344 Robert J. Richards, *The Romantic Conception of Life: Science and Philosophy in the Age of Goethe*, University of Chicago Press, 2002, pp. 129 & 134; & Michael Dettelbach, ‘Humboldtian Science’, In: *Cultures of Natural History*, N. Jardine, J.A. Secord and E.C. Spary (Eds.), Cambridge University Press, 1996, pp. 288-291.

because he drew on the methodology of the greatest of late 18th century French scientists, namely Antoine Lavoisier and his work in analytic chemistry. Lavoisier considered all matter to be ultimately composed of ‘elements’, these being “simple and indivisible atoms”. He did not believe the elements could be known in terms of the atoms themselves but believed that the term ‘element’ could be safely applied to “the last point which analysis is capable of reaching”, that “we must admit, as elements, all the substances into which we are capable, by any means, to reduce bodies by decomposition”.³⁴⁵ Cuvier applied this analytic method, originating from the study of inorganic chemistry, to the study of animal economy. The organism was understood as integrated and co-ordinated but also as explicable in terms of physiological mechanism and function, in particular the function of specific organs and how they functionally related with each other. The most overtly analytic aspect of the Cuvieran approach to animal economy was the understanding of the organism as in effect the mere sum of its parts. One great advantage of this standpoint was that it allowed Cuvier to extrapolate from the species level to higher taxa, being able to draw links between species based on shared similarities of the parts. Based on his assumption that the most vitally important parts would vary least between species, Cuvier was able to group animals into four *embranchements* based on the four kinds of nervous system found in nature (vertebral, radial, articulate and that of molluscs), in doing so not only explicating animal form but also offering a new classificatory system.³⁴⁶

Whatever the successes of Cuvieran science, it was almost all done utilising dead specimens indoors away from wild nature, a distance Cuvier interpreted as allowing him to objectively compare and place organisms within nature, but which Romantics thought fundamentally perverse. For Goethe, true understanding of nature did not arise from material abstraction of specimens from the wilds to the museum but through mental abstraction and idealisation by means of comparison of multiple observed instances, in the process excluding accidental and impure elements to arrive at the ‘pure phenomenon’.³⁴⁷ For his work on the *Urtypus* (‘Archetype’) of the plant, this required lengthy periods of intimate experience with plants in gardens over a number of years and both at home and abroad (his initial research in the late

345 Antoine-Laurent Lavoisier, *Elements of Chemistry, in a New Systematic Order, Containing All the Modern Discoveries, Vol. 1*, Robert Kerr (Trans.), New York: Evert Duykinck/James and Thomas Ronalds, 1806, p. xx.

346 Paul Farber, ‘The Type-Concept in Zoology during the First Half of the Nineteenth Century’, *Journal of the History of Biology*, Vol. 9, No. 1, 1976, pp. 100-102.

347 Lorraine Daston and Peter Galison, *Objectivity*, New York: Zone Books, 2010, pp. 58-59.

1870s was conducted in Italy).³⁴⁸ It is true that Goethe would then work with skeletal material when he turned to the study of the animal archetype, but nonetheless a fundamental difference can be seen between Goethe and Cuvier's two ways of approaching nature. For Cuvier, nature is to be analysed into its parts and then classified on the basis of those parts which least vary across morphological space. For Goethe, by contrast, the whole is studied and differences between individuals are abstracted away so as to arrive at a holistic type which all members partake in. The rejection by Romantic naturalists of the analytic strategies of decomposition and resynthesis was not accidental but rather based on a firm belief that such interventions destroyed the very life they were purported to aid the study of whilst lacking any power to revive it. Thus Goethe comments that "The living thing is dissected into its elements... but one cannot from these put it together again and quicken it".³⁴⁹ There is similarly the protest by the poet Joseph von Eisendorff that before the Romantic approach "nature was dissected atomistically like a dead corpse".³⁵⁰ A corpse was thought to inform little concerning the nature of life as most Romantic naturalists believed life to require a vital force to intervene and organise inert matter, and that this force was absent in the cadaver. For Romantics, therefore, field and museum naturalism were not as neatly complementary as they were for Cuvierans, for they were considered to have very different objects of study, one being a science of the living, the other a science of the dead. The living could not be adequately studied through the dead, nor the whole through the parts, and only life was thought able to bring life to the components of organic matter, it being assumed that its organisational powers could not be artificially replicated in the lab by inorganic means. The study of the dead was not illegitimate but claims for knowledge about the living based on the study of the dead were not to be taken seriously without corroborative observation of live organisms free from constraint. Field and museum studies were ultimately for Romantics not the complementary exploration of a single domain but rather the parallel investigation of two domains, living nature and dead nature.

Due to their strong preference for observation over intervention, the German Romantic naturalists were inclined to study in the field or garden as opposed to indoors if they wished

348 Robert J. Richards, *The Romantic Conception of Life: Science and Philosophy in the Age of Goethe*, University of Chicago Press, 2002, pp. 394-396 & 413-419.

349 Gernot Böhme, 'Knowledge Policy as the Task of Science: On Ethically Relevant Knowledge of Nature', *The Governance of Knowledge*, Nico Stehr (Ed.), Transaction Publishers, 2004, p. 21.

350 Robert J. Richards, *The Romantic Conception of Life: Science and Philosophy in the Age of Goethe*, University of Chicago Press, 2002, p. 18.

to investigate living nature. But this is not to say that they were especially averse to indoors work or that they disavowed work with dead organic material. As mentioned, Goethe's studies of the animal archetype were based in the main on osteological materials and other kinds of dead specimen and not on living animals.³⁵¹ Embryologists such as Lorenz Oken and Karl von Baer would furthermore take Romantic morphology into the lab, where the materials studied were also mainly dead specimens. The important element of the Romantic approach was therefore not a preoccupation with living organisms or a greater proclivity for the outdoors but rather its emphasis on meticulous and repeated observation and favouring of mental idealisation over physical decomposition. It was this epistemic privileging of proximity over distance from nature and interaction-with over intervention-in which marked the major difference between the Romantic and analytic traditions.

Encapsulating Nature: Vivaria & the Art of bringing Nature into the Lab and Home

The conflict between the French analytic tradition and German Romantic naturalism would come to manifest itself in Britain towards the middle of the 19th century. The encounter between these two rival methodologies and perspectives would centre around attempts to facilitate the study of living beings extracted from nature within the walls of the laboratory and home. The major naturalistic tradition in Britain in the early 19th century was natural theology, rooted in works such as John Ray's *Wisdom of God* of 1691 and revived with considerable force by William Paley and others around 1800. The highpoint of 19th century natural theology is identified by Amundson with the eight *Bridgewater Treatises* of 1829-38, which sought to reconcile a creator God with the increasingly apparent evidence for an ancient Earth and for extinction, after which it went into decline. Its assumption of a natural order designed by the Creator to ultimately benefit all its creatures remained, however, influential, as we shall see in the case of the early aquarium popularisers.³⁵²

The analytic tradition first gained a foothold in Britain in the late 1820s, imported by researchers seeking to reform what they saw as a decline in the nation's science. As conducted in British museums and labs, this new style of reasoning, which Elwick refers to as 'analysis:synthesis', emphasising its dual nature, investigated life-forms as composites of

351 Ibid.

352 Ron Amundson, *The Changing Role of the Embryo in Evolutionary Thought: Roots of Evo-Devo*, Cambridge University Press, 2005, pp. 54-55.

parts that were commonly assumed to possess a high degree of autonomy, a position Elwick calls ‘compound individualism’. This supposition had been influenced by investigations into the regenerative capacities of simple animals such as marine worms and the capacity of hydra, polyps and many plants to reproduce through fission of parts. A similarly particulate understanding of development was common among British analytic life science researchers, who, under the influence of the French anatomist Étienne Serres, adhered to a centripetal model of ontogeny as the coming together of formerly separate parts and their integration to form a whole. German Romantic holism arrived in Britain around 1837, when Edinburgh doctor Martin Barry introduced von Baerian embryology. This new holistic style of reasoning, later known as ‘palaetiology’, would dispute Serres' model of ontogeny as centripetal in favour of the view of development as originating out of a single point, a fertilised ovum, and in doing so reject compound individualism and the conflation of sexual and asexual reproduction.³⁵³

Importantly for the story of indoor nature, palaetiology would eventually prevail thanks to a recently introduced piece of scientific technology, the vivarium. A vivarium is a controlled environment for the housing and long-term observation of living specimens, commonly taking the form of a glass enclosure. Vivaria would serve as a novel evidence source which palaetologists could utilise in order to establish their greater access to nature and scientific authority over analysts. Analysts generally based themselves in museums and in adjoining labs and typically dealt either with dead specimens or else sought to dissect living specimens to determine the function of parts, the levels of compound individuality and the extent of regenerative capacities. Their interest in the relations of parts and whole was, in other words, largely ahistorical. Palaetologists, by contrast, emphasised the importance of studying life histories using living animals and plants in controlled conditions tailored to the needs of captive organisms, with observation favoured over intervention.³⁵⁴ The kinds of vivarium utilised and developed during this period for palaetiological research included above all aquaria (particularly marine) and Wardian cases (near airtight glass containers used to house animals and plants in heavily polluted inner cities and to protect exotic plants from exposure to salt spray on long sea voyages). The Wardian case was first developed in the early 1830s

353 James Elwick, ‘Styles of Reasoning in Early to Mid-Victorian Life Research: Analysis:Synthesis and Palaetiology’, *Journal of the History of Biology*, Vol. 40, 2007, pp. 37-54.

354 Ibid.

by its namesake, Nathaniel Ward, and whilst initially a luxury item due to heavy duties on glass became more widely accessible for less affluent scientists with the repeal of these duties in 1845.³⁵⁵ The aquarium is claimed to have been invented in 1841 by Ward as the ‘aquarium’, though there are earlier claims including an 1830 one for Frenchwoman Jeanne Villepreux-Power. Between 1849 and 1850, the chemist Robert Warington would then develop the aquarium as an instrument for scientific domestication in which organisms could be kept alive sometimes even for decades. (Warington, admittedly, was far from the first scientist to use glass vessels for the long-term study of aquatic life – the Scottish baronet John Dalryell, whose work inspired Villepreux-Power, began doing so to study marine invertebrates possibly as early as 1790).³⁵⁶

As Elwick comments “The more that creatures’ entire life cycles could be observed, the more that palaeontology was strengthened”. But it was only in the mid-1850s that palaeontology began to properly overtake the analytic approach. The spread of palaeontology was dependent on the spread of vivaria, which in the late 1840s were still outnumbered by the museums of analysts. Publicity at the 1851 Great Exhibition and the opening of a public marine aquarium at the Zoological Gardens in 1853 all helped facilitate the expansion of vivaria. But it was the popular writings of the amateur field naturalist Philip Henry Gosse which were especially influential, in particular his 1853 *Aquarium*, which helped trigger the growth of a lay fashion for aquaria.³⁵⁷ Gosse’s vivid descriptions of the pleasures of looking after often peculiar marine life-forms interacting in unexpected harmonies, e.g. the symbiosis between hermit crab and anemone, caught the public imagination and inspired popular imitation, despite Gosse’s offering little technical information on tank maintenance. But Gosse did not just focus on spectacle; a fervent Christian and natural theologian (he later wrote the critically reviled *Omphalos*, which attempted to reconcile Biblical chronology with evidence for the ancient age of the Earth by suggesting the world was created 6,000 years ago with the appearance of age), he conceived of aquarium-keeping as a spiritual exercise which “brings

355 David Allen, ‘Tastes and Craves’, In: *Cultures of Natural History*, N. Jardine, J.A. Secord and E.C. Spary (Eds.), Cambridge University Press, 1996, pp. 402-403.

356 Christopher Hamlin, ‘Robert Warington and the Moral Economy of the Aquarium’, *Journal of the History of Biology*, Vol. 19, No. 1, 1986, pp. 131-132; James Elwick, ‘Styles of Reasoning in Early to Mid-Victorian Life Research: Analysis:Synthesis and Palaeontology’, *Journal of the History of Biology*, Vol. 40, 2007, pp. 55-56; & Bernd Brunner, *The Ocean at Home: An Illustrated History of the Aquarium*, New York: Princeton Architectural Press, 2005, pp. 26-30.

357 James Elwick, ‘Styles of Reasoning in Early to Mid-Victorian Life Research: Analysis:Synthesis and Palaeontology’, *Journal of the History of Biology*, Vol. 40, 2007, pp. 56-57.

us, in some sense, into the presence of God”. He moreover emphasised the romantic notion of the aquarium as a miniaturised ocean, mocking the French zoologist Henri Milne-Edwards for utilising a diving bell to study marine life that could far more easily, safely and ‘naturally’ be studied at home through use of aquaria.³⁵⁸ Whilst laudatory regarding vivaria and the study of living organisms indoors, Gosse was scathing of the ‘cabinet naturalism’ of museum analysts, deriding theirs as “a science of dead things; a *necrology*... mainly conversant with dry skins furred or feathered, blackened, shrivelled, and haystuffed... with uncouth forms, disgusting to sight and smell, bleached and shrunken, suspended by threads and immersed in spirit... their colours, changed and modified by death or partial decay”. The aquarium encapsulated nature to allow contemplation of the Creator’s handwork in one’s own home; the ‘distorted things’ analysed in museums offered no such mediation with God and nature.³⁵⁹ Gosse was not trying to do away with indoor science but sought to reform the museum, “to put life into the ‘collecting cases’ that had been designed centuries earlier for lifeless objects”, as Brunner notes.³⁶⁰

Gosse was not the only naturalist to emphasise the virtues of vivaria for studying nature indoors and to criticise the poor quality or absence of prior research on living organisms. Dalyell had earlier commented in his *Rare and Remarkable Animals of Scotland* of 1847 that “In endeavouring to ascertain the history of the animated tribes... [w]e should behold them under the nearest possible circumstances to their mode of life in their natural abode”. He goes on to state that “The real organization and habits of the inferior tribes are never displayed unless in a tranquil, vigorous, and healthy state. When under constraint, placed in an unsuitable medium, or enfeebled by disease, the finest specimens languish: they alter and contract, the relative position of their parts is disturbed, their functions are impaired: the organs most conspicuous or most important during life, often disappear entirely, or they are changed by death, beyond the hope of recognition”. Dalyell, whilst displaying an analytic interest in the relation of parts to whole, was evidently dis-enamoured with typical analytic practices which too often dwelt on using dead or dying organisms and sought to move natural history back to studying living organisms in conditions at least imitative of nature. He was

358 Bernd Brunner, *The Ocean at Home: An Illustrated History of the Aquarium*, New York: Princeton Architectural Press, 2005, pp. 40-49.

359 Graeme Gooday, ‘Nature’ in the Laboratory: Domestication and Discipline with the Microscope in Victorian Life Science’, *The British Journal for the History of Science*, Vol. 24, No. 3, 1991, p. 312.

360 Bernd Brunner, *The Ocean at Home: An Illustrated History of the Aquarium*, New York: Princeton Architectural Press, 2005, pp. 49.

dismissive of many of the results of analytic life science, stating that his readers should “reprehend and distrust the cruel operations and assumed results whereon too many modern anatomists have founded theories”, based as they were on “animals in the agonies of death” (a probable reference to vivisection, which in this period still played a major role in teaching medical students at Edinburgh, Dalrymple’s home city).³⁶¹

Vivaria were thought to offer privileged access to nature not just because they allowed for the extended survival of specimens and their naturalistic behaviour. The aquarist Shirley Hibberd for instance maintained that a well-kept aquarium or Wardian case could simulate nature “not in outward appearances merely, but in *conditions*”. A failed vivarium, i.e. one whose inhabitants died, was argued to be the result not of the contraption's artifice but rather of “error or oversight on the part of the practitioner”, a failing, in other words, to approximate nature.³⁶² Properly attended to, an aquarium could act as the “Ocean on the Table” or “Lake in a Glass”, a piece of nature encapsulated and transported indoors.³⁶³ But this is an unusual understanding of 'nature', for here the natural is something maintained by constant supervision and intervention, with its failure being attributed to failure of supervision. How then, could this be reconciled with the popular view of nature as what lies outside human intervention (as seen in 'natural state models' of naturalness)? The strategy of Hibberd and co. appears to have been to argue that what was problematic about vivaria micro-ecosystems was not their artifice as constructed environments but rather their isolation from the larger environmental milieu. Whereas in nature a comparable micro-ecosystem, e.g. a rock pool, would be renewed by regular tidal contact and interchange with its parent ecosystem, the ocean, this was not possible with those indoor natures isolated within domesticity. With this natural interchange suspended, organisms in these captive environment would die unless their supervisors adequately emulated the effects of these missing natural forces, e.g. through oxygenation, introduction of food stuffs, etc. This compensation through the intervention of the supervisor was therefore justified as necessary to best approximate nature. The theological mindset of many of the early pioneers of aquaria moreover allowed them to conceive of their roles in maintaining indoor nature as analogous to God’s supervision and

361 John Dalrymple, *Rare and Remarkable Animals of Scotland, Vol. I*, London: John Van Voorst, 1847, pp. vi-vii.

362 Christopher Hamlin, ‘Robert Warington and the Moral Economy of the Aquarium’, *Journal of the History of Biology*, Vol. 19, No. 1, 1986, pp. 132-133.

363 Bernd Brunner, *The Ocean at Home: An Illustrated History of the Aquarium*, New York: Princeton Architectural Press, 2005, p. 60.

dispensatory maintenance of outdoor nature. So long, therefore, as their interventions paralleled perceived divine dispensations, these researchers believed themselves to appropriately emulate nature. In sum, indoor nature was widely acknowledged as an artful construct that moreover lacked outdoor nature's capacity for self-regulation. However, it was believed that so long as outdoor conditions were appropriately emulated in miniature, even when this required continuous intervention, then it was acceptable to consider the vivarium as a successful transplanting of nature indoors.

The kinds of interventions necessary to establish and maintain indoor nature were considerable. As a primer, actual experience in the field was a great advantage in that it gave an idea of what one wanted to emulate. Gosse and Dalyell divided time between collecting and observing at the shoreline and further observation and experiment on specimens kept indoors, but as the aquarium became a more standardised piece of scientific equipment, the usual regimes of division of labour took place, with many researchers preferring to use suppliers for their specimens. As Raf De Bont has documented, this tendency would lead to the paradoxical near complete separation of researchers at Anton Dohrn's Naples marine station (founded in 1872) from the very field which that institute was originally designed to offer optimal access to. Instead of collecting specimens themselves at the shore, fishermen were hired to provide the lab scientists with materials, which were brought to a 'sorting room' where the researchers selected their share of the daily catch.³⁶⁴ But this is not to say that when researchers selected their own specimens for investigation that they necessarily extracted a representative sample of nature. John Dalyell, for instance, specifies that "All specimens ought to be healthy, vigorous, and entire", i.e. since extraction from their native environs is stressful, only the fittest individuals should be taken into captivity. But Dalyell goes further to argue that selecting the best individuals and in the right numbers is also necessary for the "truth and accuracy of observation", i.e. those individuals perturbed least by living in number in novel, artificial conditions will provide the best, most naturalistic data. So even if the resulting micro-ecosystem is not representative of the variety of fitness levels found in the sea, this is actually better than taking a random sample, as weaker individuals will produce less naturalistic data under artificial conditions.³⁶⁵

364 Raf de Bont, 'Between the Laboratory and the Deep Blue Sea: Space Issues in the Marine Stations of Naples and Wimereux', *Social Studies of Science*, Vol. 39, 2009, pp. 217-219.

365 John Dalyell, *Rare and Remarkable Animals of Scotland, Vol. I*, London: John Van Voorst, 1847, p. ix.

Once organisms were successfully introduced into a vivarium, there was a need for constant support. There might be an ideal view that these installations were miniature samples of nature in which life was maintained by its being kept in biochemical equilibrium (Warington for instance claimed that his ‘balanced aquarium’ was illustrative of “that beautiful and wonderful provision which we see every where displayed throughout the animal and vegetable kingdoms, whereby their continued existence and stability are so admirably sustained”), but at the same time vivaria could not be maintained as closed systems.³⁶⁶ There was a need, in other words, for the constant introduction of new energy – oxygen, food, etc. – in order to keep things running. Whereas as outdoor nature was happily able to maintain itself, indoor nature was necessarily on life support mediated by its human supervisors. Hibberd went as far as to acknowledge that “an aquarium... is a prison; and as birds in cages require special care to compensate them for confinement, so gold-fishes and sea-anemones, must be looked after, with love of course, for there can be no success without that”.³⁶⁷ Gosse similarly speaks of how “It will still be needful to exercise a watchful supervision of the collection. It must be remembered that both the animals and plants are not in their natural circumstances, and that a certain amount of violence is done to their habits. Death, which spares them not at the bottom of the sea, will visit them in the Aquarium; and hence the vessel should be occasionally looked over, *searched*, as it were, to see if there be any of the specimens dead”.³⁶⁸

In addition to these interventions of introducing nutriment and air and of removing the dead, aquarium keepers furthermore sought to minimise the appearance of conflict. In part this was done through rhetorical measures, in particular by arguing for the necessity of violence in a healthy ecosystem. Gosse was not the only early aquarium enthusiast with a background in natural theology. Christopher Hamlin has shown how Warington’s modelling of the balanced aquarium was guided by the doctrines of contemporary ‘chemico-theologians’ such as James F.W. Johnson and Charles Mansfield, who saw in chemical cycles such as the oxygen-carbon dioxide cycle found in plant and animal respiration evidence for the benevolent hand of God.

366 Christopher Hamlin, ‘Robert Warington and the Moral Economy of the Aquarium’, *Journal of the History of Biology*, Vol. 19, No. 1, 1986, p. 140.

367 *Ibid.*, p. 149.

368 Bernd Brunner, *The Ocean at Home: An Illustrated History of the Aquarium*, New York: Princeton Architectural Press, 2005, p. 48.

Like Linnaeus and Paley, they explained the suffering incurred in ecosystems by the existence of predation as in fact a beneficial means by which life, being good, might be maintained at a maximal level. The chemico-theologians especially emphasised how predation freed up ‘borrowed molecules’ that would otherwise not be optimally recycled.³⁶⁹ But whilst this might have served as a theodicy to account for the general problem of predation in nature, it was less useful as a means of explaining away the violence found in indoor nature, which after all was regulated by man, not God, so therefore was the responsibility of the scientist. Dalyell dealt with this problem by advising that “the observer must guard against those destructive propensities of many of the lower animals”, which appears to have consisted in noting which species tend to attack each other and which are peaceful and being sure to separate and not combine those who wage war on each other.³⁷⁰

The problem that was faced by Warington was that he wanted both to prevent excess conflict and to have long-term equilibrium. Achieving both these key aims was necessary in his view if he was to emulate outdoor nature, designed as it was by God to be optimally balanced and harmonious, indoors. The chemico-theologians Warington took after insisted that predation was a necessary part of the circulation of molecules, and therefore an aquarium without predation did not adequately emulate nature. But the levels of predation in his experimental aquaria tended to be in excess of what was desirable. Warington had settled on the ecological triad of goldfish, snails and tape grass. He believed this ordering would provide balance to the aquarium ecosystem – the tape grass would oxygenate the water, enabling the fish to survive, and the snails would fulfil the dual purpose of consuming dead plant matter (which Warington believed, in line with sanitarian thought of the time, could poison the fish if allowed to rot) and of producing young that would act as one of the fishes’ food sources. The fish and snails would also carbonate the water through respiration, which would promote plant growth. The aquarium would thus be in a “healthy state” of biochemical harmony. But the original species of snail used, *Limnea stagnalis*, had the nasty habit of devouring living as well as dead plant matter, thus endangering the equilibrium of the aquarium. Warington then found that other species of snail were too easily eaten by the fish and therefore were unsuitable. Eventually, two species were found the adult form of which could adequately

369 Christopher Hamlin, ‘Robert Warington and the Moral Economy of the Aquarium’, *Journal of the History of Biology*, Vol. 19, No. 1, 1986, pp. 134-138.

370 John Dalyell, *Rare and Remarkable Animals of Scotland, Vol. I*, London: John Van Voorst, 1847, p. viii.

defend themselves against fish predation. But the fish were so vigorous in their predation of snail young that snail reproduction was “entirely prevented from the fish consuming them the instant they exhibit[ed] signs of locomotion”. Consequently, Warington was forced to periodically introduce new snails into the tank . As Hamlin remarks, “This vision of an unending stream of baby snails serving to sustain the fiction of harmonious equilibrium was a sharp contrast from [Warington’s] 1850 claim that the snails "thrive wonderfully"”. In other experiments involving even more voracious animals, Warington gave up on any attempts to keep them together with their prey and simply kept them separately in special shallow tanks which allowed for the necessary gas exchange to keep them alive. This environment wholly failed to simulate the natural conditions Warington sought to approximate in his balanced aquarium; as Hamlin observes “Here "equilibrium,"... had become virtually a tautology – simply that the fish lived was an indication that it was in equilibrium with its environment”.³⁷¹

As can be seen by the example of Warington, the ‘indoor nature’ created in vivaria not only required continual intervention to maintain but moreover in some cases bore no resemblance to natural states of affair, and yet could still be considered a successful installation on instrumental grounds. In other words, if nature could not be properly emulated, an indoor installation could still be considered successful if it met some lesser project goal. For Warington, for example, whilst he modelled the equilibrium of the balanced aquarium on the perceived balance of nature, where a naturalistic balance could not be attained, a minimal form of ‘equilibrium’, in this case specimen survival, became considered acceptable in its stead. This tendency for life science researchers to circumvent obstacles to the establishment of an ‘indoor nature’ by accepting standards for minimal naturalism that nonetheless fell well short of emulating natural conditions will be further explored later in this chapter.

It should be emphasised that experimenters, not just their troublesome subjects, were just as often to blame for the failure to approximate natural conditions in vivaria. The theological mindset of many of the early aquarium researchers, whilst aiding their self-conception of their interventions as naturalistic, also brought with it moralistic baggage. Often it was felt that it was not enough that aquarium conditions emulate nature, for nature, whilst exhibiting harmony and balance, was after all also a post-Edenic nature ‘red in tooth and claw’. Nature

³⁷¹ Christopher Hamlin, ‘Robert Warington and the Moral Economy of the Aquarium’, *Journal of the History of Biology*, Vol. 19, No. 1, 1986, pp. 139-141 & 144-145.

could and should be improved so as to be brought closer to its pre-Fall standard – as lions had once lain down with lambs, so they should be able to do so again. It was widely believed that domestication could attenuate predatory instincts; the aquarist Kirby, for instance, reported on a showman keeping cats, rats and mice, and hawks and small birds together in cages.³⁷² It can thus be seen that whilst the Baconian belief that domestication could effect the restoration of an originally tame nature was increasingly dismissed by those naturalists convinced of the original character of wildness, it remained for many religiously-inclined experimentalists a powerful idea that guided their domesticating practices and end-goals.

The failure to emulate nature in vivaria was not always due to a belief in a moral duty of restoration. Other researchers simply did not consider approximating natural conditions essential to the validity of their research, so made little effort to do so. Warington had stated that he believed the purpose of the balanced aquarium would be to help answer “questions about physical, chemical, and biological components of aquatic and marine ecosystems”. His interest lay, in other words, in the study of communities of organisms and their physical and chemical requirements for existence. The main group, however, to initially take up aquaria for scientific research were naturalists primarily interested in using them to compile ‘biographies’ of collected species, i.e. not for the purposes of ecological study but rather as a means of distinguishing species from one another and of classifying them based on their life histories. The advantage of the aquarium was not its capacity to simulate nature but rather, as one anonymous aquarist put it, “To bring [the organisms] to us... since we could not go comfortably to them, and to have them up in a witness-box, and make them give an account of themselves”. For this interrogation to be enabled, it was necessary only that organisms have “a portion of their element with them... little comforts... such as stones, sand, mud, and marine-plants”. Another aquarist, James, similarly asserted that all a supervisor needed to provide his prisoners with “to imitate their natural haunts” was “sand, pebbles, and rocks... to afford them shelter”. Again, as with Warington’s changing conceptualisation of equilibrium, we can see the influence of a tendency towards a minimal naturalism whereby markers were established for what counted as ‘natural’, and so long as these were evident in the experimental set-up and end-point, then results attained could be considered as applying both to indoor and outdoor nature. It can moreover be seen that these markers of nature were

372 Ibid., p. 148.

contextually relative to the aims of the experiment – studies of individual behaviour and development for classificatory purposes required only that the set-up be naturalistic enough that specimens could live and grow in reasonable health for enough time to generate interesting data (depending on the species, days, weeks or months), whereas ecosystem studies were more demanding in that ideally multiple species should flourish over several generations and an extended period of time (months, years, even decades). For researchers using aquaria primarily for classification, that their specimens survived at all for some time away from their native environs was proof that their indoor set-up was naturalistic enough for their purposes. They accepted that their charges were most likely to lead brief lives in captivity, one aquarian, J.G. Wood, even going so far as to argue that this was a positive thing in that it meant naturalists would have a steady stream of specimens for dissection and study under the microscope.³⁷³

Based on the attitudes of Wood and other naturalists with similar views on aquarium keeping, it can be seen that despite the advances vivaria offered for studying active organisms, their life histories and ecologies, these pursuits still in many ways remained secondary to the analysis and classification of dead specimens. This was not least in part due to the increased acceptance in this period of microscopy as a means of bringing nature indoors. Goethe had been notably sceptical of the validity of using mechanical means to aid observation, declaiming that “Microscope and telescope are only good for confusing healthy reason”, that “man in himself, insofar as he uses healthy reason, is the greatest and most exact instrument that can exist”.³⁷⁴ Goethe’s critique perhaps should not be dismissed as mere obscurantism; chromatic and spherical aberrations were a ubiquitous part of early 19th century microscope use. Technical advancements in the construction of ‘true’ lenses in the 1820s by J.J. Lister and others are commonly credited as restricting the mechanistic causes of these occlusions. Gooday, however, points to evidence of academic scepticism concerning the naturalness of microscopic observations continuing well into the mid-19th century. It is notable that it was the same romantic naturalists who promoted aquaria, in particular Gosse and the novelist Charles Kingsley, who would in the mid-19th century also advocate the utility of microscopes as means of bringing nature indoors (the aforementioned J.G. Wood was another such co-

³⁷³ Ibid., pp. 133 & 145-147.

³⁷⁴ Pierre Hadot, *The Veil of Isis: An Essay on the History of the Idea of Nature*, Michael Chase (Trans.), The Belknap Press of Harvard University Press, 2006, p. 148.

populariser of microscopy as well as aquaria). Whereas taxonomy was presented as a necrology, microscopy was instead portrayed as a means of accessing previously unseen pastures. Gosse, for example, speaks of the microscope as illuminating a “vast field of marvels” otherwise invisible. Another microscopist, Mary Ward, spoke of viewing through the lens as being equivalent to “visiting a rich but hitherto undiscovered region”. For these naturalists, the microscope slide was a means of bringing nature indoors, a piece of the field which one could explore at one’s table at one’s leisure, and also to some extent a vivarium, a “boundless field of minute organic life” as Cuthbert Collingwood characterised it in 1862.³⁷⁵

As to why the British aquarists so fervently co-promoted microscopy as a further legitimate means, alongside vivaria, of bringing nature indoors, Gooday points to the socio-political advantages of endorsing these research tools for those looking raise the public profile of natural science. Those living in the city had little direct access to nature, so the easiest means of reaching these people and convincing them of the wonder and beauty of living nature was through vivaria. That these people were interested in having access to nature was evidenced by the various 19th century natural history fashions that arose in the growing industrial British cities, such as the fern craze that followed the early development of Wardian cases.³⁷⁶

Allowing nature to reach these citizens was important not only for promoting the cause of science but also of Christianity, the argument from design having since Newton increasingly relied on the complexities of life as opposed to the majesty of the heavens as its primary argument for the necessary existence of a creator (Paley in 1809 notably states that astronomy, as compared with living nature, “is *not* the best medium through which to prove the agency of an intelligent Creator”).³⁷⁷ Microscopes were readily available to the general public thanks to the increased manufacture of cheap instruments from the 1850s onward, e.g. by Field & Son. They were thus, Gooday argues, an ideal vehicle for bringing nature into the homes of the educated public and of convincing them of the power of science to explore natural mystery. The microscope therefore became a tool for the egalitarian spread of science from an often distant nature into the homes of all citizens. This role was celebrated in 1870

375 Graeme Gooday, ‘Nature’ in the Laboratory: Domestication and Discipline with the Microscope in Victorian Life Science’, *The British Journal for the History of Science*, Vol. 24, No. 3, 1991, pp. 320-323 & 327.

376 Ibid.; & David Allen, ‘Tastes and Crazes’, In: *Cultures of Natural History*, N. Jardine, J.A. Secord and E.C. Spary (Eds.), Cambridge University Press, 1996, pp. 400-404.

377 Ron Amundson, *The Changing Role of the Embryo in Evolutionary Thought: Roots of Evo-Devo*, Cambridge University Press, 2005, p. 54.

by the Oxford Professor of Anatomy, George Rolleston, who commented that “The microscope has done very much... in enabling all persons to obtain the necessary minimum of practical acquaintance with the arrangements of the natural world”. Gooday thus concludes that however artificial aspects of microscopy were for the viewing of nature (slides often had to be prepared with chemicals or specimens squashed and immobilised between slides, etc.), the “political expediency” of promoting the microscope as a legitimate locus of indoor nature to audiences who would otherwise have little access to the ‘field’ was motive enough for romantic naturalists such as Gosse, who were otherwise critical of studies of dead or disturbed nature, to overlook such naturalistic failings.³⁷⁸

This is not to say that life scientists utilising microscopes were not self-conscious of the problem of artifice. Even with the restriction of optical aberrations thanks to the advances in lens craft of the 1820s and 1830s, there still remained problems in microscopy of distinguishing natural object from technical artefact. A case in point was the argument over ‘globulism’ that broke out in the 1830s. Globule hypotheses regarding the fundamental composition of living tissue were popular in the early 19th century prior to the rise of cell theory. They originated as an elaboration on the earlier fibre theory of organic constitution, advocates claiming to observe fibres to be composed of more fundamental spherical particles, ‘globules’, which were often said to be arranged like beads on a string. In traditional histories of microscopy, these entities have tended to be presented as the erroneous product of a combination of bad instruments producing optical illusions and bad scientific theorising due to the influence of *naturphilosophie*. Lister and Hodgkin are then attributed to have discredited these fallacious theories through their construction of superior instruments c. 1827 which demonstrated globules not to exist. There are good reasons to be sceptical regarding this received account given, as Pickstone pointed out in 1973, that it is Whiggish, treating globule theory not in terms of its historical context but rather as a failed precursor of cell theory, and as its technical determinism is undermined by the fact that Lister’s lens were neither dramatically superior to pre-existing equipment nor did they prevent globule advocates from continuing to see such entities. Schickore’s more recent analysis of the controversy, whilst sympathising with Pickstone’s revisionism, continues to support a role for

378 Graeme Gooday, ‘Nature’ in the Laboratory: Domestication and Discipline with the Microscope in Victorian Life Science’, *The British Journal for the History of Science*, Vol. 24, No. 3, 1991, pp. 321, 324 & 340.

error in argumentation over the globulist position. But Schickore differs from the received account in pointing to the plethora of kinds of globules observed – varying in size, shape and behaviour – which therefore required a multiplicity of potential sources of artifice to be identified by actors as a means of distinguishing artefact from natural object. Most notably, Ernst Heinrich Weber, Leipzig professor of Anatomy, did in 1830 influentially posit that reports of spherical globules of regular size arranged like beads on a string were most likely optical illusions due to a combination of observations in direct sunlight, which produced interference patterns, and over-magnification. But Weber was himself a globulist, differing from those he criticised in that he believed them to differ in size and not necessarily be spherical or of regular shape. Similarly, Gottfried Treviranus in 1835 would agree with Weber that the ‘strings of beads’ he observed when studying nerve fibres were artefacts. But Treviranus did not put this down to light conditions or magnification – he was after all using an advanced Plössl microscope. Instead, he argued that the true form of nerve fibres was cylindrical and that their globular appearance under the microscope was due to deterioration after death and treatment with water.³⁷⁹

In short, phenomena observed under the microscope commonly could not simply be determined as natural objects or artefacts by sight alone. What was required therefore was the identification of common sources of error and artifice and the establishment of codes of practice which best avoided these problems. Practices which conformed to these codes would then be considered naturalistic. Gooday has discussed how in the case of microscopy in Britain, influential conventions of naturalistic use emerged from the 1850s onwards which would be crystallised into a general set of standard practices in the early 1870s as a result of their being co-opted by T.H. Huxley in his pedagogical program for the training of science teachers at South Kensington. These conventions included preferences for natural light conditions, stable working surfaces, robust but mobile instruments and the development of particular accepted methods of handling and preparing materials for microscopic study, all these being employed with the intention of “minimizing the apparent artificiality and situational idiosyncracies [sic] of the indoor situation; so that whilst socially constructing the ‘natural’ conditions, practitioners... could claim a direct, i.e. unmediated, contact with

379 Jutta Schickore, ‘Error as Historiographical Challenge: The Infamous Globule Hypothesis’, In: *Going Amiss in Experimental Research*, Giora Hon, Jutta Schickore and Friedrich Steinle (Eds.), Boston Studies in Philosophy of Science, Volume 267, Springer Science+Business Media B.V., Boston Studies in Philosophy of Science, Volume 267, Springer Science+Business Media B.V., 2009, pp. 27-42.

Nature”.³⁸⁰ ‘Naturalisation’ was not in this case as much a process of making the effects of scientific instruments less artificial as it was one of convincing student practitioners that their use of such instruments, if conducted according to proper prescriptions, did not significantly disrupt the natural phenomena they were studying. Whilst nature was often invoked as the authority and the source of knowledge in microscopy, in truth what kind of ‘nature’ was investigated by Huxley’s students was delimited by Huxley himself. The plethora of conflicting images that had plagued early 19th century microscopy was thus resolved by implicit collective agreement among senior experts that certain practices did not significantly intervene upon the natural objects being observed and then by these agreed conventions being presented to the next generation of researchers as the given parameters of the natural in microscopic investigations. In this way, Gooday concludes, “canonically 'natural' forms of laboratory research practice arise from the training of practitioners in highly conventionalized 'natural' forms of pedagogical practice”.³⁸¹

To summarise, the development of naturalistic conventions in microscopy can be considered comparable to the ‘minimal naturalism’ I earlier discussed in relation to Warington’s ecological balance research programme and with regards to those naturalists using aquaria as a means primarily of storing live specimens for description, long-term observation and dissection. The development of these conventions should not necessarily be seen as negative. Nature in its full complexity and diversity is impossible to fully miniaturise and bring indoors. To create an indoor nature, aspects of outdoor nature must necessarily be selected for extraction and then tested to determine whether they remain stable for the time period demanded by a particular scientific investigation. But how the extent of stability is measured is also relative to the particular object of research. For instance, if we consider viability as a species of stability, Warington’s snails usually lived short lives, but what he measured was not the lifespan of individuals but rather of the aquarium as a sustainable ecosystem as a whole; by contrast, Wood did think of viability in terms of individuals, but did not see short lifespans as a problem given that his research required a steady supply of dead as well as of the living. Furthermore, because maintaining nature indoors is difficult, researchers tend to gravitate towards experimental practices that can attain naturalistic results with minimal

380 Graeme Gooday, ‘Nature’ in the Laboratory: Domestication and Discipline with the Microscope in Victorian Life Science’, *The British Journal for the History of Science*, Vol. 24, No. 3, 1991, pp. 332-333.
381 *Ibid.*, p. 341.

hassle. What is deemed naturalistic is both a product of individual judgement and social conditioning. The aim is to avoid artifice as any non-analogy between indoor and outdoor nature can be seized on by critics as evidence of the irrelevance and non-transferability to nature of knowledge produced in a particular laboratory. But artifice is a necessary part of the creation of indoor nature due its being brought about through the human interventions of extraction from the outdoors and stabilisation and maintenance indoors. Therefore, researchers tend to establish a set of conditions or components which must be kept maximally naturalistic and free of artifice, whilst the rest are negotiable and do not need to be 'true to nature'. What is necessary and what is negotiable is based on an assessment of which parameters can be manipulated without relevantly affecting the 'natural' behaviour of the phenomena of interest and which are less robust and therefore demand careful treatment. As we shall see, it was advantageous that early 20th century geneticists were able to tie the identity of their wild type experimental subjects to their hereditary constitution, as since genes are highly conserved aspects of an organisms biology they were able to argue around the highly artificial nature of their lab stocks and environment on the basis that so long as a strain carried the wild type gene relevant for a particular experiment, then it could stand in for the natural form of the species. The more robust a phenomena of interest is under lab conditions, the more negotiable secondary phenomena will therefore be interpreted as and the more 'truth to nature' will be sacrificed to increase tractability.

Problems arise for the representativeness for outdoor nature of knowledge acquired from indoor nature when there are errors of judgement with regard to which parameters are in truth necessary or negotiable. But equally, even when parameter judgements are broadly correct, the knowledge produced from observations of and experiments on indoor nature is never fully representative of outdoor nature in the first place because indoor natures are in general constructed and evaluated relative to particular purposes of study. These purposes of study dictate the selection of particular aspects of outdoor nature for isolation, transplantation and stabilisation indoors. Indoor nature is therefore never a truly miniaturised outdoor nature but is rather an abstraction of particular aspects of outdoor nature. Indoor nature can therefore be regarded as constituting what Leonelli has called (with reference to model organisms in contemporary biology) a 'material abstracting' from outdoor nature. 'Material abstracting' is a form of scientific modelling whereby objects extracted from nature and brought under

controlled conditions “are taken to be representative of a [broader] set of phenomena”, e.g. phenomena in a balanced aquarium are treated as comparable with general phenomena in tidal rock pools. It is also characterised by the fact that “Epistemic access to phenomena is granted first and foremost by material [as opposed to intellectual] manipulation”, which requires ‘performative skills’ of being able to interact with the lab environment in such a way as to enable study without producing unwanted artificial or accidental phenomena.³⁸² Whilst Leonelli has focused on 20th and 21st century plant biology, I feel this characterisation equally applies to the ‘indoor nature’ found in various mid-19th century laboratories and homes. Performative skills that can be pointed to, for example, involve particularly the ability of the experimenter to take on functions normally performed by nature, such as aeration, in order to maintain the viability of living indoor nature and in the case of studies of dead nature, the skill to stabilise tissues before decay using correct preservative media to prevent distortions. To these may be added the skill of ensuring that the correct conditions of observation are maintained to prevent interfering phenomena when using intercessional equipment such as the microscope.

Indoor Nature and the 'New Biology'

Through naturalising conventions of practice and the conceiving of the microscope slide as a vivarium, the microscope was able to claim paramount status as an indoor mediator between life scientist and nature, arguably at the expense of true vivaria such as aquaria and Wardian cases, which were less conspicuously utilised as means of bringing nature indoors for serious scientific research for much of the rest of the 19th century. The microscope furthermore leant itself more to analytic research of part-whole relations than it did to the study of life histories – as François Jacob comments “For the eye armed with a microscope, every living organism was finally resolved into a collection of juxtaposed units”.³⁸³ The one major exception to this is effectively the one that proves the rule, namely the growth in studies of microbes and their life histories, microbes being of course those organisms which were too small for the microscopes of the time to dissolve into smaller units. It is therefore not surprising that microbes were targets for rigorous lab-based scientific domestication early on, as they

382 Sabina Leonelli, ‘Performing Abstraction: Two Ways of Modelling *Arabidopsis thaliana*’, *Biology and Philosophy*, Vol. 23, 2008, pp. 523-524.

383 François Jacob, *The Logic of Life: A History of Heredity*, Betty E. Spillmann (Trans.), Princeton University Press, 1973, p. 117.

represented a form of life that, so long as they could be cultured, were easily incorporated into the sterile, uniform and often cramped spaces of modern laboratories.

Generally then, the new lab life sciences – cytology, embryology, plant physiology, microbiology, and evolutionary and functional morphology, to mention a few – can be seen as characterised by an internalist focus which concentrated less on the macroscopic organism and more on internal units within the organism, e.g. cells, embryos, tissues, pathogens and other kinds of hidden structures. As Lynn Nyhart observes, “Natural historians sought to uncover the large-scale pattern of living nature... ‘modern’ biologists in their laboratories sought to penetrate the internal workings of the living organism to discover their fundamental causes”.³⁸⁴ This internalist focus was also influenced by the growth of the idea that the body constituted an enclosed space distinct from the external milieu, an ‘interior milieu’ as the physiologist Claude Bernard dubbed it in the 1850s.³⁸⁵ As a consequence, whereas the field naturalist studied the organism in its outdoor environment, for this new generation of laboratory life scientist the organism commonly *was* the environment, the ‘experimental system’ (to recall Rheinberger’s terminology), within which their ‘epistemic objects’ persisted.³⁸⁶ Both naturalist and lab worker experimented by intervening in an environment and observing the effect on their units of study, but because of the difference in the nature of the environments they investigated, different kinds of interventions were typically required. This allowed lab scientists to justify interventions in organisms that would be considered artificial in the naturalistic tradition, on the grounds that whilst damaging to the integrity of the organism these invasive procedures were not damaging to the integrity of the internal units they studied.³⁸⁷ This is not to say that traditional life history studies such as those conducted by palaeontologists in Britain and ‘scientific zoologists’ in Germany (both for whom vivaria and field studies played an important role in studying organisms) disappeared in the second half of the 19th century – Nyhart has shown that in the German case at least these kind of research programmes remained healthy in this period. Rather, they tended to suffer from

384 Lynn K. Nyhart, ‘Natural History and the ‘New’ Biology’, In: *Cultures of Natural History*, N. Jardine, J.A. Secord and E.C. Spary (Eds.), Cambridge University Press, 1996, p. 426.

385 François Jacob, *The Logic of Life: A History of Heredity*, Betty E. Spillmann (Trans.), Princeton University Press, 1973, pp. 187.

386 Hans-Jörg Rheinberger, *Towards a History of Epistemic Things: Synthesizing Proteins in the Test Tube*, Stanford University Press, 1997, pp. 24-31.

387 François Jacob, *The Logic of Life: A History of Heredity*, Betty E. Spillmann (Trans.), Princeton University Press, 1973, pp. 181-189.

being treated as auxiliary to the dominant paradigm of evolutionary morphology, a perception exacerbated by the fact that the major founders of evolutionary morphology in Britain and Germany, Huxley and Haeckel, both had roots in life history studies but turned away from them in the 1860s as they believed morphology to offer more opportunities to answer evolutionary questions.³⁸⁸

The rise of the laboratory life sciences coincided with a relative decline in importance of museum-based naturalism (though Kohler has shown that systematics and biogeography remained a considerable growth industry for late 19th and early 20th century museums).³⁸⁹ This decline was not purely a matter of outcompetition, for, as Sophie Forgan argues, changes in the social function of museums in this period were also an important influence. Whereas previously museums had been largely research facilities, access to the contents of which was largely restricted to academic researchers and students, in the second half of the 19th century this system of public exclusion began to fall away with the growth of the idea of public education as a means of moral improvement. This had the consequence that museums increasingly catered to “visitors... whose interest had to be caught by carefully arranged and attractive displays”, rather than patrons who could be assumed to have a certain level of understanding of the natural order of collected objects. Forgan acknowledges, however, that there was a significant clash between the epistemologies of museum and lab, namely between museum naturalism's emphasis on ‘passive’ ordering and observation and laboratory biology's emphasis on active intervention and manipulation. In this confrontation, the laboratory possessed an advantage over the museum as it could offer both more exact and more generalisable knowledge due the control practitioners exercised over environment and parameters, which importantly allowed for the replication of experiments and the confirmation of findings across different sites. By contrast, museum naturalism relied on unique specimens kept at single locations which could at best be compared with similar instances but could not be re-created elsewhere.³⁹⁰

388 Lynn K. Nyhart, ‘Natural History and the ‘New’ Biology’, In: *Cultures of Natural History*, N. Jardine, J.A. Secord and E.C. Spary (Eds.), Cambridge University Press, 1996, pp. 427-432.

389 Robert E. Kohler, *All Creatures: Naturalists, Collectors, and Biodiversity, 1850-1950*, Princeton University Press, 2006, pp. vi-xiii.

390 Sophie Forgan, “The Architecture of Display: Museums, Universities, and Objects in Nineteenth Century Britain,” *History of Science*, Vol. 32, 1994, pp. 140-156.

The lab life sciences' control for exactitude was the outcome of the successful standardisation of laboratory equipment and pedagogy. By promoting a maximal similitude of practice and situation across different lab spaces, the laboratory could attain a sense of what Steven Shapin calls 'placelessness' – a genericness of place which helped overcome many of the effects of locality which had previously interfered with the replication of experiments and the confirmation of findings.³⁹¹ 'Placeless' knowledge could, it was argued, be easily extrapolated between labs, in contrast to knowledge produced in the field which was commonly difficult to validly apply outside of its original context of production. Laboratory-produced knowledge thus came to be perceived as possessing a universality lacking in localised and variable outdoor nature, and through what Kohler calls "a kind of cultural body snatching" lab nature came to exemplify broader nature more than any particular instance of outdoor nature could do so. Results attained in controlled indoor conditions were judged reliable and could be extrapolated to new locales by re-creation of the correct parameters, what Schaffer has terms the "multiplication of contexts". Results attained through study in the field or, for that matter, in less strictly controlled domestic environments such as gardens and ordinary households, were dismissed as lacking in rigour as non-lab conditions were seen as uncontrolled, the parameters being set by nature not the scientist, and as particular to the locale of research.³⁹² Exactitude was thus not just an aspiration, it was a necessary condition for credibility. Its espousal thus constituted a form of what Thomas Gieryn calls 'boundary-work', a form of standard-setting designed to prevent amateurs and dilettantes encroaching on the turf of lab science professionals.³⁹³ Even the experimental work of a naturalist as esteemed as Charles Darwin was not immune to attack, as shown by his dispute with Julius Sachs over plant root geotropism. Sachs' issue appears not to have been Darwin's conclusions, which whilst opposed to his own had previously been anticipated by another professional whom Sachs had not gone on to attack. Instead, Sachs almost solely criticised Darwin's 'unprofessional' methods of experiment (his research was based at home and in the garden and he used non-

391 Steven Shapin, *Never Pure: Historical Studies of Science as if It Was Produced by People with Bodies, Situated in Time, Space, Culture, and Society, and Struggling for Credibility and Authority*, John Hopkins University Press, 2010, p. 57.

392 Robert E. Kohler, *Landscapes and Labscapes: Exploring the Lab-Field Border in Biology*, Chicago: The University of Chicago Press, 2002, pp. 3-8; Peter Galison, 'Material Culture, Theoretical Culture and Delocalisation', *Science in the Twentieth Century*, John Krige & Dominique Pestre (Eds.), Netherlands: Harwood Academic Publishers, 1997, p. 677; & Simon Schaffer, 'Making up Discovery', In: *Dimensions of Creativity*, Margaret A. Boden (Ed.), The MIT Press, 1996, pp. 13-52.

393 Thomas F. Gieryn, *Cultural Boundaries of Science: Credibility on the Line*, Chicago & London: The University of Chicago Press, 1999, pp. 15-18.

standard equipment). By establishing limits to the kinds of practitioner, methods and settings that were granted credibility in plant physiology, Soraya de Chadaverian observes that “Sachs redefined the standards of scientific work by turning the laboratory into a privileged place for access to 'nature'”.³⁹⁴ De Chadaverian's observation conforms with Latour's claim that in modern technoscience the phenomena being investigated often require such an extensive technical set-up in order to render them ‘correctly’ visible that the only option for dissenters is “to *build another laboratory*”.³⁹⁵ Thus it was that even though Darwin's hypothesis proved the correct one, it was only accepted among professional plant physiologists when tested in ‘proper’ laboratory conditions.

The demand for exactitude both as a requirement for placeless knowledge and as a condition for credibility would be one influential motivation for the standardisation of living indoor nature, one outcome of which would be the development of lab wild type strains. But this movement towards exactitude and placelessness in laboratory science did not occur in a vacuum. Rather, it can be situated in the broader social context of 19th century industrialisation and the growth of what Canguilhem has called “technological normalization” – the increased standardisation of the constituent parts of ensembles of production through top-down hierarchical organisation conducted by corporations and the state so as to improve efficiency and better sync fabrication and consumption.³⁹⁶ Indoor nature was also to be ‘normalised’, purified of contamination by the original context from which it was extracted and rendered generic and mobile, a process Galison refers to as “*delocalization*”.³⁹⁷ Laboratory science thus moved from emulation of nature indoors in miniature to what Knorr-Cetina appropriately terms ‘enculturation’, this being the process by which nature is ‘brought home’, isolated from the ‘wild’ and subjected to the conditions of the social order of the laboratory so as to “[derive] epistemic effects from the new situation”.³⁹⁸ This also may be compared with what Hacking calls the ‘creation of

394 Soraya de Chadaverian, ‘Laboratory science versus country-house experiments. The controversy between Julius Sachs and Charles Darwin’, *The British Journal for the History of Science*, Vol. 29, 1996, pp. 17-41.

395 Bruno Latour, Bruno Latour, *Science in Action: How to Follow Scientists and Engineers Through Society*, Cambridge, Massachusetts: Harvard University Press, 1987, *Science in Action: How to Follow Scientists and Engineers Through Society*, Cambridge, Massachusetts: Harvard University Press, 1987, p. 79.

396 Georges Canguilhem, *The Normal and the Pathological*, Carolyn R. Fawcett & Robert S. Cohen (Trans.), New York: Zone Books, 1991, pp. 246-247.

397 Peter Galison, ‘Material Culture, Theoretical Culture and Delocalisation’, *Science in the Twentieth Century*, John Krige & Dominique Pestre (Eds.), Netherlands: Harwood Academic Publishers, 1997, p. 676-677.

398 Karin Knorr Cetina, ‘The Couch, the Cathedral, and the Laboratory: On the Relationship between Experiment and Laboratory in Science’, *Science as Practice and Culture*, Andrew Pickering (Ed.), Chicago

phenomena' and his observation, discussing the various 'effects' found in physics (his example is the Hall effect in electromagnetism) that these are in fact human productions not truly found in nature in their undiluted state as "nowhere outside of the laboratory is there such a pure arrangement [without intervening causes]". The enculturation of living nature similarly produces standardised cultures of a kind not found outside of the lab, existing over multiple generations in a single species dominated ecosystem and nourished by bland but plentiful food mediums. Enculturation can overall be regarded as a particularly extreme manifestation of the scientific domestication of nature where the captured natural object is deliberately stripped of as much of its original context as possible and is subjected to new orderings which aim to provoke it into producing phenomena which whilst novel artefacts give simultaneous insight into fundamental aspects of the object's nature. Such artifice is necessary and insightful, Hacking argues, as phenomena of a regular quality upon which generalisations may be based are actually rare in nature (astronomical phenomena are an exception, and therefore the basal status of astronomy in early science is not surprising). To extend the reach of science, it is therefore necessary to abstract from nature and reconstruct it under controlled conditions.³⁹⁹

Given the link proposed between industrialisation and enculturation, it is perhaps no great surprise that the most significant pioneers of pure culturing methods among lab life scientists in this period were those working in the emerging field of microbiology, the development of which paralleled the growth of industries such as breweries in which microbes played a central role in production (breweries, as we shall see, would also play an important role in the later development of plant 'pure line' cultures by Johannsen). Louis Pasteur, for instance, whilst spending the first ten years of his scientific career working in crystallography, then spent two decades (1857-'77) working on problems of fermentation, specifically the 'diseases of beer and wine', and it was this industry research which led him directly towards tackling the question of spontaneous generation and later the diseases of humans and livestock, via studying disease in silkworms, organisms fundamental to another significant industrial enterprise.⁴⁰⁰ Pasteur's research background can moreover be clearly seen to have had a clear

and London: The University of Chicago Press, 1992, p. 118.

399 Ian Hacking, *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science*, Cambridge University Press, 1983, pp. 225-228.

400 Arthur M. Silverstein, 'Pasteur, Pastoriens, and the Dawn of Immunology: The Importance of Specificity', *History and Philosophy of the Life Sciences*, Vol. 22, No. 1, Selected Papers from a Conference Held at the Dibner Institute: 'Pasteur, Germs and the Bacterial Laboratory', 22-23 November 1996, Part I, 2000, pp. 30-

effect on his scientific practice and conduct, as can be seen in the testimony of his disciple Duclaux, who stated him to have “the masterful qualities of a chief of industry who watches everything, lets no detail escape him, wishes to know everything, and who, at the same time, puts himself in personal relation with all his clientele”.⁴⁰¹ Robert Koch’s work also became increasingly enmeshed with the emergent pharmaceutical and chemical industry as the 19th century drew to a close, with research on his tuberculosis antiserum, tuberculin, being eventually outsourced to the company Farbwerke Hoechst in 1892; in this case, however, this was against Koch’s intentions, being a consequence of the failure of tuberculin on the market and the Prussian Ministry of Culture therefore being able to impose this deal as part of their agreeing to support Koch’s institute.⁴⁰²

Enculturation has, as Knorr Cetina notes, significant advantages in that it “enable[s] investigations to be performed in one place, without regard to natural conditions (e.g., weather, seasonal changes, regional differences in visibility, etc.), subject only to the contingencies of local situations (e.g., to the speed and the local resources that scientists can bring to bear on the work)”. These advantages of enculturation have been further stressed by Latour in his study of the Pasteurian ‘war and peace of microbes’. He points first to the weakness of prior efforts at disease control by hygienists, who because they believed that “almost anything... might cause illness” found it “necessary to act upon everything at once, but to act everywhere is to act nowhere”. Pasteurians, by isolating microbial agents and moving disease into the terrain of the laboratory, acted in accord with “the [general] principle of any victory: you must fight the enemy on the terrain that you master”. Whereas the contagions and miasmas of the hygienists were invisible forces operating more or less everywhere, Pasteurians extracted ferments from the environment, brought them under control and, by offering microbes ‘ideal’ conditions in which they could develop blithely, “freed from the competition of other living beings”, made “these invisible agents visible”. So, whereas outdoors the microbes moved freely unseen, infecting and killing humans and

32.

401 Gerald L. Geison, ‘Organization, Products, and Marketing in Pasteur’s Scientific Enterprise’, *History and Philosophy of the Life Sciences*, Vol. 24, No. 1, Selected Papers from a Conference held at the Dibner Institute for the History of Science and Technology, MIT, on ‘Pasteur, Germs and the Bacteriological Laboratory’, 22-23 November 1996, Part II, 2002, p. 37.

402 Christoph Gradmann, ‘Money and Microbes: Robert Koch, Tuberculin and the Foundation of the Institute for Infectious Diseases in Berlin in 1891’, *History and Philosophy of the Life Sciences*, Vol. 22, No. 1, Selected Papers from a Conference Held at the Dibner Institute: ‘Pasteur, Germs and the Bacterial Laboratory’, 22-23 November 1996, Part I, 2000, pp. 75-77.

animals, indoors they were forced to show themselves, becoming pampered prisoners of the Petri dish. Furthermore, once a means was determined of attenuating their virulence, these domesticated microbes could be utilised to inoculate people and livestock against the depredations of the still dangerous wild microbes.⁴⁰³

The artifice and ecological novelty of culturing methods did not escape practitioners. Duclaux, speaking later of the advantages of Koch's solid culturing method, commented on how it "forces each germ to develop on the spot and to form a *colony*".⁴⁰⁴ Given this recognition that the single species colony ecology constitutes a forced non-natural state, it is perhaps ironic that one major use of culturing was as a means of identifying bacterial species. This function was formalised in 1872 when Ferdinand Cohn established the first major system of bacterial classification, which was based on morphological characteristics including those displayed by cultures. A microbe which could not be cultured was therefore a microbe which could not be properly classified. This system had serious shortcomings, as only a fraction of microbe species can be cultured under traditional lab conditions.⁴⁰⁵ Early microbiologists, whilst not aware of the scale of this problem, were far from ignorant of the difficulties of domesticating wild microbes. As Latour observes, "to pasteurize a disease was no easy matter... a link had to be made between a disease and a microbe... after that the microbe had to be isolated, a process that was not always possible... then the microbe had to be cultivated in a favorable medium in such a way as to increase its effects, an operation that was often impossible if the wild microbe refused to allow itself to be domesticated".⁴⁰⁶ But whilst the requirements of Cohn's culture-based taxonomy made it difficult to classify hard-to-culture microbes, it did help bring stability to microbial classification, for it was only as lab cultures feeding on uniform media that many bacteria species retained constancy. Early microbiologists were well aware that outside of the artificially uniform environment of the culture, microbial behaviour and physiology was very changeable. Pasteur, for instance, observed that acetic acid bacterium *Mycoderma aceti*, which in wine breaks down alcohol into acetic acid and carbon dioxide, will once the wine has thus been turned into vinegar

403 Bruno Latour, *The Pasteurization of France*, Alan Sheridan and John Law (Trans.), Harvard University Press, 1988, pp. 20-21, 61-63 & 91.

404 Ibid., p. 82.

405 William C. Summers, 'From Culture as Organism to Organism as Cell: Historical Origins of Bacterial Genetics', *Journal of the History of Biology*, Vol. 24, No. 2, 1991, pp. 171-172.

406 Bruno Latour, *The Pasteurization of France*, Alan Sheridan and John Law (Trans.), Harvard University Press, 1988, p. 105.

switch to turning acetic acid into carbon dioxide and water. Such adaptability had led some observers to suggest that bacterial ‘species’ in fact represented different forms of mutable entities. Most radically, Carl Nägeli argued all microbial life to be a single species in constant flux.⁴⁰⁷ This, of course, would have undermined the very possibility of a bacterial taxonomy, and it is therefore understandable that culture traits, being relatively stable, were utilised as means of demarcating bacterial species despite their being artificial products of laboratory culturing techniques. For microbiologists, it was therefore intuitive that domestication was necessary to understand the natural order of microscopic life – an attitude that we will see early geneticists increasingly apply also to macroscopic life. The success of the domestication and enculturation of animals and plants for the purposes of genetic knowledge production would moreover similarly depend on the ability of geneticists to detect and control tiny structures internal to the organism, in this case genes. The efforts of later 19th century life scientists to domesticate cells, microbes, tissues and other previously invisible or intangible fragments of life which might be found within or extracted from the organism-as-environment can therefore be seen as an important preamble to the geneticist's endeavour to domesticate heredity (including domesticating the wild type). But in order to domesticate heredity, it would first need to be established that heredity could be stabilised, isolated and controlled. For this to happen would require advances in both theory and practice. I will try to summarise some of the more important developments in theory in the following chapter.

407 J. Andrew Mendelsohn, ‘Like All That Lives’: Biology, Medicine and Bacteria in the Age of Pasteur and Koch’, *History and Philosophy of the Life Sciences*, Vol. 24, No. 1, Selected Papers from a Conference held at the Dibner Institute for the History of Science and Technology, MIT, on ‘Pasteur, Germs and the Bacteriological Laboratory’, 22-23 November 1996, Part II, 2002, pp. 13-15.

Chapter 7 – Darwinism, the Late 19th Century Problem of Variation, and the 'Move out of History'

Introduction: The Unresolved Nature of Variation and Darwin's Provisional Hypothesis

I have earlier discussed in this thesis how the wild type concept was restricted and reinterpreted in the context of Darwinian evolutionary theory. Of particular consequence was Darwin's dismissal of normative interpretations of variation based on 'Natural State Models', which he did not believe to accord for what he saw as the widespread evidence of divergence in the course of nature, both in the wild and under domestication. Whereas critics such as Jenkin saw reversion as evidence of such normative variation, Darwin instead reinterpreted the reappearance of ancestral traits in feral organisms as simply an effect of their return to ancestral conditions of existence, which induced the redevelopment of wild traits rendered latent in the process of domestication. In short, Darwin forwarded a unified theory of variation as operating under the same laws in domestication and the wild and assumed a causally-based correlation between changes in the conditions of existence and organismic variation. This left Darwin with two problems, however. Firstly, he required a mechanism to explain how the conditions of existence effected change in organisms. Secondly, his belief that environmental and organismic variation would correlate was an empirically untested presupposition and could therefore be brought into question by contradictory observations. This chapter will explore first how Darwin (largely unsuccessfully) sought to deal with the problem of variation, then explore some of the alternative theories in play towards the end of the 19th century and their role in the division of evolutionism into rival camps, before closing by examining how the rejection of Darwin's theory of correlational variation would presage efforts to domesticate heredity and establish wild type lineages in the laboratory.

It has been suggested by less diligent historians that Darwin did not properly develop an idea of a mechanism to explain heredity and variation until he was shocked into doing so in 1867 by the twin assault of Jenkin's 'swamping' argument and William Thomson's calculation of a young age for the Earth.⁴⁰⁸ This myth survives in many popular accounts of Darwin's work, e.g. 1982's *Darwin for Beginners*: "Confronted by the twin spectres of Kelvin and Jenkin, Darwin began to lose faith in the effectiveness of natural selection. He now felt it necessary

408 For discussion of this myth and its falsity, see: Robert C. Olby, 'Charles Darwin's Manuscript of *Pangenesis*', *The British Journal for the History of Science*, Vol. 1, No. 3, 1963, pp. 251-263; & Peter Vorzimmer, 'Charles Darwin and Blending Inheritance', *Isis*, Vol. 54, No. 3, 1963, pp. 371-390.

to introduce some auxiliary process which would hasten evolutionary change in a purposive direction".⁴⁰⁹ The myth typically goes on to claim that Darwin 'reverted' to a belief in the Lamarckian inheritance of acquired adult characteristics (ignoring the fact that Darwin never ceased believing in such forms of inheritance), on the grounds that this would speed up evolution on a younger Earth, and that the product of this 'reversion' was his 1868 provisional hypothesis of pangenesis. This claim is often backed up by pointing to Darwin's son Francis' assertion that pangenesis was a late addition to his father's evolutionary thought.⁴¹⁰ This last claim, however, is disproved by Charles' declaration in an 1867 letter that the hypothesis was "26 or 27 years old".⁴¹¹ Detailed investigation of Darwin's notebooks by Jonathan Hodge has shed more light on this long incubation of pangenesis. Hodge traces the hypothesis' origins to Darwin's tutelage under Robert Grant at Edinburgh (the term 'gemmule' is Grant's originally) and his reading of early 19th century theories of development and inheritance such as those of his grandfather Erasmus.⁴¹² This derivation from decades-old theories was recognised by some of Darwin's contemporaries, as can be seen from a critical comment by 'Dr. Bastian' that it appeared more "a relic of the old rather than a fitting appanage of the new evolution philosophy".⁴¹³

I will shortly return to the origins of pangenesis when discussing why it was poorly received in 1868. I will first, however, detail the theory itself. The hypothesis suggests the fundamental units of heredity to be molecules called 'gemmules', which are thrown off by cells and are dispersed throughout the body, where they also independently propagate by division. They accumulate in the sexual organs and in the sex cells and are thus passed on to the next generation in reproduction. Gemmules tend to gather with their like, i.e. gemmules produced by cells of the same organismic function and distinctive characteristics, and in ontogeny will act collectively on "partially developed or nascent cells" and cause them to specialise and take on the characteristics of their precursor cells. Where there are inherited

409 Jonathan Miller and Boris van Loon, *Darwin for Beginners*, Cambridge: Icon Books, 1992, p. 138.

410 P. Kyle Stanford, *Exceeding our Grasp: Science, History, and the Problem of Unconceived Alternatives*, Oxford University Press, 2006, p. 76.

411 *Ibid.*, p. 76.

412 M.J.S. Hodge, 'The Darwin of Pangenesis', *Comptes Rendue Biologies*, Vol. 333, 2010, pp. 131-132; & M.J.S. Hodge, 'Darwin Studies at Work: A Re-examination of Three Decisive Years (1835-37)', In: *Nature, Experiment and the Sciences: Essays on Galileo and the History of*, Trevor H. Levere and William R. Shea (Eds.), Boston Studies in Philosophy of Science, Volume 120, Netherlands: Kluwer Academic Publishers, pp. 249-274.

413 Charles Darwin, *The Variation of Animals and Plants under Domestication: Vol. 2*, 2nd ed., London: John Murray, 1885, p. 350.

different varieties of gemmules for one particular trait, the different varieties will compete to influence and develop that trait. In such cases, the most numerous group of gemmules will tend to win out. Variability for Darwin was therefore caused by in part by the “deficiency, superabundance, and transposition” of the particular kinds of gemmule inherited from the parental generation. These variations in number were further affected by the fact that gemmules can also be inherited in a dormant as opposed to active state, and rather than being developed will then be passed on to future generations. This ability for gemmules to be inherited in a dormant state and only activated in later generations was believed by Darwin to explain reversion. Reversion was in this manner explained as a simple effect of the mechanism of heredity and part of the overall capacity for variability as opposed to its being a force antagonistic to divergence. In addition to dealing with the problem of reversion, pangenesis was also intended to answer the charge that there existed limits to variability by positing a means by which novel variations could arise. Darwin here turned to the inheritance of acquired characteristics, positing that when the phenotype is altered through “the direct action of changed conditions on the organisation, and of the increased use or disuse of parts”, “the gemmules from the modified units will be themselves modified”.⁴¹⁴

Darwin always considered pangenesis highly speculative and admitted in an 1867 letter to Asa Gray that he feared it would “be called a mad dream”.⁴¹⁵ That he remained committed to it for so long was because he believed it a uniquely unitary causal mechanism able to explain a whole panoply of otherwise anomalous phenomena of heredity and variation. These included “how it is possible for a character possessed by some remote ancestor suddenly to reappear in the offspring [atavism]; how the effects of increased or decreased use of a limb can be transmitted to the child [inheritance of acquired characteristics]; how the male sexual element can act not solely on the ovules, but occasionally on the mother-form [xenia and telegony]; how a hybrid can be produced by the union of the cellular tissue of two plants independently of the organs of generation [graft hybridisation]; how a limb can be reproduced on the exact line of amputation, with neither too much nor too little added

414 Ibid., pp. 368-390.

415 P. Kyle Stanford, *Exceeding our Grasp: Science, History, and the Problem of Unconceived Alternatives*, Oxford University Press, 2006, p. 65.

[regeneration]; how the same organism may be produced by such widely different processes, as budding and true seminal generation".⁴¹⁶

Pangenesis, was, however, coldly received and for good reason. When Darwin had originally formulated the hypothesis in the early 1840s, he worked under assumptions based on contemporary cell theory, in particular Schwann's theory that cells originated out of 'blastemic' crystallisation from inorganic materials. It was on this basis that Darwin proposed that sex cells might form out of a pre-cellular conglomerating of gemmules. By the 1860s, however, Virchow's doctrine of *omnia cellula e cellula* ('all cells from a cell') had become cytological orthodoxy. Darwin's hypothesis was therefore out of step with recent scientific developments.⁴¹⁷

In an attempt to secure evidential support for his faltering hypothesis, Darwin supported an experiment by his cousin, Francis Galton, which involved giving purebred rabbits blood transfusions from other stock and then breeding them to see if signs of mongrelism would appear, as would be expected if gemmules circulated in the blood. This spectacularly backfired when despite breeding over a number of generations and utilising increasing doses of foreign blood, no mongrelism was detected. This experimental failure embarrassed Darwin, who admitted that "I certainly should have expected that gemmules would have been present in the blood". To save face and theory, he maintained that gemmule circulation in the blood "is no necessary part of the hypothesis", suggesting other vehicles must be responsible for circulation, as would evidently be the case in bloodless organisms like plants and simple animals.⁴¹⁸ Galton's experiments nonetheless discredited pangenesis in many life scientists' minds, at least in the form Darwin proposed.

Galton

Galton would further problematise Darwin's theories of variation when in the early 1870s he discovered a statistical phenomenon that appeared to vindicate Jenkin's earlier claim that there were fixed limitations to ordinary variation (the following details are largely based on

416 Charles Darwin, *The Variation of Animals and Plants under Domestication: Vol. 2*, 2nd ed., London: John Murray, 1885, p. 349.

417 M.J.S. Hodge, 'The Darwin of Pangenesis', *Comptes Rendue Biologies*, Vol. 333, 2010, p. 131.

418 Charles Darwin, *The Variation of Animals and Plants under Domestication: Vol. 2*, 2nd ed., London: John Murray, 1885, p. 350.

Gayon's account of Galton in *Darwinism's Struggle*). Becoming known as 'regression to the mean', it was an effect at the population level, invisible at the individual, whereby although selection could have impressive results directing a lineage in a particular direction, the offspring of any exceptional members within the lineage would tend to be 'meaner' than their parents, that is, they will be closer in character to the original mean of the overall population than their immediate progenitors, as opposed to varying in the direction of selection. This suggested to Galton that all effects of selection were to be considered merely temporary, that whilst the average quality of a stock could be gradually shifted, if selection were relaxed regression would push the population mean back to its original position. For Galton, regression implied that selection was impotent when it came to producing novel variation. It could at best divide a population into sub-populations with distinctive characters, in the manner that humans created breeds out of the extant variation found in domesticated species, but such distinct isolate groups would not remain divergent if reproductive barriers were disabled and normal interbreeding commenced. Galton believed 'mean' characteristics would always predominate in a freely interbreeding population on the basis of a belief in what would later be called the 'Law of Ancestral Heredity'. This 'law' drew upon pre-existing beliefs common particularly amongst animal breeders which interpreted heredity as operating fractionally, with each individual receiving 'blood' from all its ancestral generations, these contributions proportionally diminishing with each generational step back in time. Ancestral heredity connected any selected lineage back to the population from which it derived, and this shared populational inheritance acted as a form of 'racial inertia' that would tend, in its collective weight, to overwhelm over time any recent divergences from the norm produced by selection. Galton's regression to the mean appeared to demonstrate very real limits to the capacity of selection to direct variation, limitations reinforced, it was claimed, by the tendency of ancestral heredity to overwhelm any recent divergence. If true, the extant capacity in any species to vary could not act as the raw material for its evolution.⁴¹⁹

Unlike Jenkin, Galton did believe in evolutionary change but the theories he would develop of how this occurred assumed a completely different set of forces to those of Darwin. Galton was an internalist who thought that the main driver of evolution was spontaneous variation along pre-established developmental pathways. In his 1869 text *Hereditary Genius*, Galton

419 Jean Gayon, *Darwinism's Struggle for Survival: Heredity and the Hypothesis of Natural Selection*, Matthew Cobb (Trans.), Cambridge University Press, 1998, pp. 132-144.

compared the variability of specific types to a multifaceted stone which may rest at equilibrium in many positions (each position corresponding to a type). This polygon is relatively stable when resting, yielding only slightly to minor pressures and returning to its original equilibrium when such pressure is withdrawn. These minor pressures are analogous to the effects of selection on ordinary variability, which is capable of creating subtypes within populations but cannot safeguard them against regressing to the mean once its pressure is withdrawn. The stone can only be made to roll onto a new face and attain a new position of stability from which it will not regress through a “powerful effort”. But even if this effort is attributed to external forces such as selection, these can be considered to have no role in shaping the form of the new type. For Galton the faces of his polygon instead represent pre-existing and fixed ‘islands’ of stability in a sea of possible configurations between the structural components of the organism. For Galton, selection therefore only has a capacity to shape directly the continuous variability found in populations, which is subject to regression and therefore cannot produce permanent new forms. The only constant forms produced in evolution are discontinuous and cannot be altered by selection, as their inherent stability precludes their being moulded.⁴²⁰

Galton's discontinuous variations produced distinct racial populations. Because regression within populations prevented true divergence, the only means by which selection could effect anything evolutionarily permanent was through choosing between these competing races. Galton's idea of evolution through natural selection as a racial contest was shared by Herbert Spencer, the renowned British polymath and public intellectual, and it was this interpretation of selection which informed Spencer's description of evolutionary history as “the survival of the fittest”. This idea of racial contest was informed by the contemporary belief in world history as shaped by a struggle between human races for global supremacy. This was a variation-led model of evolution which treated selection as a merely destructive force, a sieve shaking out the weak and sparing the strong ('sifting' was another of Spencer's terms for his version of selection). Darwin, however, was unconvinced that such inter-populational competition could produce the level of adaptation seen in nature. It was only, he thought, through the differential accumulation of minute and ordinary favourable variations as a consequence of *intra*-populational competition over vast stretches of time that nature could

420 Ibid., pp. 342-351.

achieve the fine-grained fit of purpose to place seen in wild organisms. This accorded selection a creative role as an architect piecing together adaptive structures out of irregular naturally occurring material.⁴²¹

For Galton, unlike Darwin, ancestral heredity would almost always outweigh recent divergence and return lineages to the population type. To effect an equilibrium shift required single large variations, 'sports', in order to overcome this racial inertia. Galton was not the first to question Darwin's reliance on the accumulation of small variations – T.H. Huxley reprimanded Darwin just prior to the *Origin*'s publication for having “loaded yourself with an unnecessary difficulty in adopting *Natura non facit saltum* [‘nature does not proceed in jumps’] so unreservedly”. From 'saltum' derives the term used to describe theories of 'jumping evolution', 'saltationism'.⁴²² Darwin acknowledged the existence of larger variations and asserted that selection could operate on both “excessively slight individual differences... [and] on those greater differences which are called sports” but he did not believe the latter to play a major role in evolution due to their comparative scarcity as material for selection.⁴²³ Darwin became further convinced of the minimal role of sports in evolution as a consequence of Jenkin's 1867 argument that rare variations would generally be diluted down into irrelevance within a few generations due to the tendency of parental inheritances to blend (Darwin commented in the 5th edition of *Origin* that until he read Jenkin “I did not appreciate how rarely single variations, whether slight or strongly marked, could be perpetuated”).⁴²⁴ One of Galton's significant contributions to the case for saltationism was in his pointing to examples such as the inheritance of eye colour in humans and coat colour in dogs where unions between differing parents produced offspring with the trait of one or the other parent as opposed to an intermediary trait. Galton used these to argue that some variations were inherited ‘exclusively’ rather than being blended, and therefore sports could potentially produce new species without the risk of reverting to type.⁴²⁵

421 Ibid., pp. 52-53 & 60-70.

422 Stephen Jay Gould and Niles Eldredge, ‘Punctuated Equilibria: The Tempo and Mode of Evolution Reconsidered’, *Paleobiology*, Vol. 3, No. 2, 1977, p. 115.

423 Charles Darwin, *The Variation of Animals and Plants under Domestication: Vol. 1*, 2nd ed., London: John Murray, 1888, p. 234.

424 Jean Gayon, *Darwinism's Struggle for Survival: Heredity and the Hypothesis of Natural Selection*, Matthew Cobb (Trans.), Cambridge University Press, 1998, p. 96.

425 Ibid., p. 173.

One reason Galton embraced saltationism was due to his rejection of Darwin's theory of pangenesis, which as we have seen assumed the inheritance of acquired adult characteristics. Against this view, Galton held that individuals are “no more than passive transmitters of a nature we have received, and which we have no power to modify”. He went further to state that it would be “an approximately correct view of the origin of our life, if we consider our own embryo to have sprung immediately from those embryos whence our parents were developed, and these from the embryos of their parents, and so on for ever”.⁴²⁶ He would later point to the additional fact that the ova and testes of mammals formed when they developed in the womb, and that therefore if any pangenetic mechanism for the inheritance of acquired characteristics existed, it would likely only affect grandchildren rather than the first generation of offspring and could therefore not directly respond to environmental influences, as Darwin's provisional hypothesis supposed.⁴²⁷

Based on the apparent similarities of their views on acquired characteristics, Galton has been suggested to have anticipated Weismann's hypothesis of the sequestration of the germ-plasm. Bulmer, however, argues that he only does so “in a weak sense”, pointing out that he arrived at his hypotheses from the reverse direction to Weismann, moving from an assumption that acquired adult characteristics are rarely if ever inherited to the conclusion that inheritance can be regarded as taking place from embryo to embryo.⁴²⁸ Moreover, Radick has observed that whilst Galton is often regarded as an “arch hereditarian”, he allowed for a substantial role for environmental factors in the development of hereditary potentials – eugenics, Galton remarked in 1904, deals not only with “all influences that improve the inborn qualities of a race” but also “those that develop them to the utmost advantage”. Galton's environmentalism has been downplayed by many historians of science, Radick suggests, due to the fact that 19th century Galtonism has come to be seen as foreshadowing 20th century Batesonian Mendelism. This interpretation is problematic not least due to the fact that prior to 1900 the most influential British utilisers and developers of Galtonian method and theory were biometricians such as Walter Weldon and Karl Pearson, both among the loudest of Mendelism's early critics. Weldon in particular would argue that Galtonian analysis of

426 Staffan Müller-Wille and Hans Jörg Rheinberger, *A Cultural History of Heredity*, Chicago and London: The University of Chicago Press, 2012, p. 79.

427 Francis Galton, *Natural Inheritance*, New York and London: Macmillan and Co., 1894, p. 14-16.

428 Michael Bulmer, ‘The Development of Francis Galton's Ideas on the Mechanism of Heredity’, *Journal of the History of Biology*, Vol. 32, No. 2, 1999, p. 291.

populational data showed, contrary to the Mendelian claim that trait production could be understood in terms of the uniform action of binary dominant-recessive factor pairs, that instead trait production was highly dependent on the broader genetic and environmental context. In Weldon's eyes, Galton's research supported his belief that dominance "depend[s] not upon the character borne by a germinal determinant, but upon the condition of the determinant itself, and upon its relation to the other germinal determinants, at some moment during fertilisation or subsequent development". This evident diversity of research and theory among Galton's acolytes suggests that his direct influence on anti-environmentalist accounts of variation should not be overestimated.⁴²⁹ The interpretation of Galtonism as a precursor of Mendelism was instead a product of the biometrician-Mendelian debate of the early 1900s, which can subsequently be reinterpreted as a debate within Galtonism (de Marais observes that "Galton was claimed as a father by both Mendelians... and biometricians").⁴³⁰ Bateson's apparent victory in this debate (eased by Weldon's untimely death in 1906) would henceforth shape the interpretation of Galton's science as proto-Mendelian, and this interpretation of Galtonism did, I would argue, aid in the promotion of anti-environmentalist theories of variation.

Whilst arising from different sources, Weismann's theory of germline sequestration and Bateson's anti-environmentalist Galtonism would be important contributors to what Christoph Bonneuil has termed 'the move out of history'. This was a shift in fin de siècle biology towards a dehistoricised view of certain aspects of living things, especially heredity. As already identified by Jean Gayon, heredity shifted in this period from being thought of as a fluid force subject to historical dynamics to being identified as a rigid structure enduring through time. Alongside this movement towards structural thinking, Bonneuil also identifies a growing 'sanctuarisation' of heredity in units deep within the body, a shift "from infinite universe to closed world". These hereditary units additionally underwent 'devitalization', being no longer thought of as comparable to autonomous, self-reproducing and competing microorganisms and more and more treated as stable and immutable bearers of elementary traits.⁴³¹ What applied to heredity also applied to variation, which shifted in this period from

429 Gregory Radick, 'Physics in the Galtonian Sciences of Heredity', *Studies in History and Philosophy of Biological and Biomedical Sciences*, Vol 42, 2011, pp. 129-138.

430 Robert de Marais, 'The Double-Edged Effect of Sir Francis Galton: A Search for the Motives in the Biometrician-Mendelian Debate', *Journal of the History of Biology*, Vol. 7, No. 1, 1974, p. 142.

431 Christoph Bonneuil, 'Producing Identity, Industrializing Purity: Elements for a Cultural History of Genetics', In: *A Cultural History of Heredity IV: Heredity in the Century of the Gene*, Max Planck Preprint

being thought of as a continual product of organism-environment interactions, as in Darwin's correlational theory, to being instead conceived as “a definite, physiological event, the addition or omission of one or more definite elements”, as William Bateson would later describe the early Mendelian interpretation of variation.⁴³² This shift would have major implications for concepts of wild type, which shifted from being understood as a contingent product of temporary stabilities in the natural economy to being reinterpreted as unchanging, constituted by a biochemically stable hereditary constitution and as being open to translocation into the artificial environment and disciplining domesticating regime of the genetics laboratory.

Weismann's Mosaic Theory of Development

Having introduced his concept of sequestration of the germ-plasm, I will now turn my focus to the theories of August Weismann and discuss their impact on Darwinism. As indicated previously, Weismann came to reject the inheritance of acquired adult characteristics and Darwin's correlational theory of variation from the opposite direction to Galton, beginning with a theory of hereditary transmission before drawing conclusions regarding variation. His theory of sequestration was rooted in a mosaic theory of embryonic development.

Purportedly, a fertilised zygote contains all the hereditary units, or ‘determinates’, inherited from its parents. To specialise into brain, bone, blood, etc. cells, these determinates are unequally divided among the daughter cells. All adult somatic cells therefore only have the contingent of determinates relative to their specific function. This raises a problem for reproduction, since if all cells specialise, there will remain no cell with the full complement of determinates necessary for embryonic development.⁴³³ Therefore, Weismann reasoned, the hereditary materials in the fertilised egg must be copied at the beginning of ontogeny, producing an intact, inactivated germ-plasm unutilised in development, which migrates into the reproductive organs.⁴³⁴ Because somatic cells, unlike the germ-line, are not sequestered, any alterations they acquire cannot be passed down to future generations. Consequently, use and disuse of organs in life cannot be a source of novel variation.

343, pp. 86-90.

432 William Bateson, *The Methods and Scope of Genetics*, Cambridge University Press, 1908, p. 48.

433 Ron Amundson, *The Changing Role of the Embryo in Evolutionary Thought: Roots of Evo-Devo*, Cambridge University Press, 2005, pp. 144-146.

434 P. Kyle Stanford, *Exceeding our Grasp: Science, History, and the Problem of Unconceived Alternatives*, Oxford University Press, 2006, p. 110.

In denying the possibility of Lamarckian use inheritance, Weismann was not seeking to undermine Darwin's theories. Rather, Weismann believed that the generation of novel variation was in itself unnecessary for most evolutionary change. He regarded selection to be the 'all-sufficient' evolutionary power and as capable of pushing extant variability in whatever direction was needed, a radical adaptationism diverging from Darwin's own view that natural selection is the "main but not exclusive means of modification".⁴³⁵ He did not think limits to variation significantly impacted the potential creativity of selection, selection being able, like a composer working with the twelve musical notes, to craft great works out of basic components.⁴³⁶ This belief in selection's power extended as far as Weismann claiming that even heredity was subordinate to it. Selection acted as both a principle of conservation and of divergence, maintaining adaptive structures in stable environments and permitting their variation in fluctuating ones. As a consequence, Weismann did not think, as Darwin did, that if selective pressures were relaxed that heredity would ensure the continuation of traits acquired under selection, but rather believed that under a state of relaxed selection degenerative 'regressive evolution' would occur. He for example attributes the loss of the instinct to escape in domestic animals as due to the "cessation of the action of natural selection, and a consequent gradual degeneration of the instinct", as opposed to, say, artificial selection for tameness.⁴³⁷ Elsewhere, he asks "Why have most of our domestic animals lost their original colouring?", to which he answers "Clearly because colour became of little or no importance to them as soon as they were sheltered under the protection of man, while in a wild state it was a great safeguard against detection by their enemies".⁴³⁸ Weismann therefore denied there to be a strict analogy between domestic and wild variation, which were products of the absence and presence of the struggle for existence respectively. If reversion to the wild type did occur, it must be due to re-exposure to the struggle for existence and consequent adaptation, not due to the influence of the conditions of existence.⁴³⁹

435 Charles Darwin, *On the Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life*, Penguin Classics, 2009, p. 15.

436 Sander Gliboff, 'The Pebble and the Planet', Thesis (PhD): Johns Hopkins University, 2001, p. 110.

437 August Weismann, 'Retrogressive Development in Nature', Lilian J. Gould (Trans.), In: *Essays upon Heredity and Kindred Biological Problems, Vol. II*, Edward B. Poulton and Arthur E. Shipley (Eds.), Oxford: Clarendon Press, 1891, pp. 23-24.

438 *Ibid.*, pp. 18-19.

439 Jean Gayon, *Darwinism's Struggle for Survival: Heredity and the Hypothesis of Natural Selection*, Matthew Cobb (Trans.), Cambridge University Press, 1998, pp. 148-153.

Even given Weismann's 'all-sufficient' selection, it was difficult to see how evolution could proceed without at least occasional novel variations. There has been some confusion as to what Weismann's actual views on variation were, facilitated, Rasmus Winther has shown, by the fact that he changed his main theories several times. It has consequently been asserted that Weismann believed in internal sources of variation (Coleman) or else that he thought variability inherent to organisms (Gayon).⁴⁴⁰ Winther has however maintained that Weismann held to an externalist view of variation's causes throughout his career, believing that whilst acquired somatic variations were not inherited, acquired germ-plasm variations were, and that these were only ever produced through being induced by external conditions.⁴⁴¹ Winther identifies four principles stages to Weismann's thinking on variation: 'vague externalism' (1875-1884), 'phylogenetic externalism' (1885-1891), 'hierarchical externalism' (1892-5), and 'externalist selectionism' (1895-1914). Winther asserts that it was only as a phylogenetic externalist that Weismann argued for both the morphological and the variational sequestration of the germ-plasm. The former entails only the physical separation of germ-plasm and soma, whereas the latter also assumes the shielding of the germ-plasm from the direct effects of environmental influences. At this stage, Weismann assumed all variability in multicellular life to be ancestral variability derived from their prokaryote evolutionary progenitors. Whilst Weismann did leave room for changes for external conditions to indirectly cause germ-plasm variations through nutrition, temperature, etc., he did not during this period believe their effects to be significant. Weismann instead believed the great majority of differences between individual multicellular organisms to be due to the effects of amphimixis in sexual reproduction, which had the effect of 'shuffling' the determinants passed down from the parents. He abandoned this position in 1891 in response to a critical paper by Marcus Hartog, which derided the notion that 'shuffling' through amphimixis could explain speciation and the constancy of species differences. Hartog pointed out that either there were species-specific germ-plasms, in which case amphimixis could not explain how new kinds of germ-plasm might arise, or else there was a common ancestral germ-plasm shared by all metazoans, which simply begged the question of why, if the assortment of

440 William Coleman, 'Cell, Nucleus, and Inheritance: An Historical Study', *Proceedings of the American Philosophical Society*, Vol. 109, No. 3, 1965 pp. 153-154; & Jean Gayon, *Darwinism's Struggle for Survival: Heredity and the Hypothesis of Natural Selection*, Matthew Cobb (Trans.), Cambridge University Press, 1998, p. 152.

441 Rasmus G. Winther, 'August Weismann on Germ-Plasm Variation', *Journal of the History of Biology*, Vol. 4, 2001, pp. 519-22 & 526-8.

determinants was what determined species differences, amphimixis did not occasionally produce offspring of a different species to the parent.⁴⁴²

Weismann's post-Hartog 'hierarchical externalism' subsequently dropped the assumption of 'variational sequestration' in favour of the assumption that the metazoan germ-plasm could be directly induced to vary. But Weismann's account of how germ-plasm variations could be induced remained unconvincing in explaining evolutionary change. He for example suggested that excess nourishment could lead to determinants doubling. He believed that in some cases the functionality of these doubled determinants could change but did not specify how. Determinant doubling at best appears to have allowed restricted variation on a theme, not true novelty.⁴⁴³ Similarly, he also suggested that nutritional variations within the body might increase or diminish the effects of particular determinants, but again only quantitative differences as opposed to qualitative variations were thus possible.⁴⁴⁴ Weismann continued to insist that quantitative change was enough, maintaining that "what appears to us a qualitative variation is, in reality, nothing more than a greater or less".⁴⁴⁵ This reluctance to countenance the qualitative production of novelty may seem strange and counter-productive since it forced Weismann to demand more of selection as a creative force. But it can be understood as a product of a time of warring camps within the evolutionist community, which in places promoted a reactionary and polemical radicalism. Weismann for instance proclaimed 'the all-sufficiency of natural selection' as a direct response to his Neo-Lamarckian opponent Herbert Spencer declaring 'the inadequacy of natural selection'. Neither would seek to compromise.⁴⁴⁶ It is this context of polarisation, where qualitative interpretations of variation were increasingly associated with competing variation-led interpretations of evolution, e.g. Batesonian Mendelism and de Vriesian mutationism, which likely explains Weismann's persistent attachment to quantitative variation, even given that it was an unconvincing solution to the problem of the origins of novelty.

442 Ibid., pp. 519-540.

443 Ibid., p. 541.

444 Sander Gliboff, 'The Pebble and the Planet', Thesis (PhD): Johns Hopkins University, 2001, pp. 113-115.

445 August Weismann, *The Evolution Theory, Vol. II*, J. Arthur Thomson & Margaret R. Thomson (Trans.), London: Edward Arnold, 1904, p. 151.

446 Jean Gayon, *Darwinism's Struggle for Survival: Heredity and the Hypothesis of Natural Selection*, Matthew Cobb (Trans.), Cambridge University Press, 1998, pp. 152-153.

Haeckel's Externalism & the Origins of Weismann's Break with Darwinian Orthodoxy

If we accept Winther's revisionist account, Weismann continued throughout his career to believe that external conditions operating on the developing germ-plasm were the most likely significant source of novel variation. Whilst his sequestration theories certainly contributed to the 'sancturisation' of heredity, Weismann was evidently anxious not to be seen as an internalist. It may at this point be asked why Weismann was so opposed to internalist theories of variation given that they appear more compatible with a sanctuarised view of heredity (as we saw, Galton took this path after his rejection of inherited acquired characteristics). To understand why this alternative was not palatable for Weismann, it is necessary to place his views in the context of what for most of the late 19th century was considered 'orthodox' Darwinism. Weismann's Germany was dominated by what might be referred to as the 'Haeckelian consensus'. Ernst Haeckel was Darwin's principal German evangelist and had chiefly won over supporters by downplaying the theory of natural selection's radical contingency and 'English' utilitarianism in favour of claiming continuity with the work of Goethe and Lamarck and arguing selection to complement, not compete, with already popular theorised evolutionary mechanisms, e.g. the inheritance of acquired characteristics and developmental forces such as recapitulation.⁴⁴⁷ The most influential product of Haeckel's evolutionary synthesis was his Darwinian version of recapitulation, the 'biogenetic law', which held that the stages of an embryo in development represent a condensed reiteration of its prior evolutionary history, a law famously summarised as "ontogeny recapitulates phylogeny".⁴⁴⁸

Under Haeckel's influence, Darwinism in Germany was a broad church up until the 1880s. Haeckel, furthermore, was less interested in strict advocacy of natural selection than he was in promoting materialism and biology's reconciliation with physico-chemical law. Haeckel's main concern was therefore to attack what he saw as the principal threats to Darwinian materialism, namely natural theology and vitalist theories of life. Haeckel adhered to a mechanistic monist metaphysics which saw the biological world as an extension of the physical and denied that life processes could be directed by providence, non-physical laws or by the organism itself.⁴⁴⁹ It should be noted that Haeckel's monism was somewhat different

447 Peter J. Bowler, *Evolution: The History of an Idea*, University of California Press, 1989, p. 199.

448 Stephen Jay Gould, *Ontogeny and Phylogeny*, The Belknap Press of Harvard University Press, 1977, p. 1-7.

449 Sander Gliboff, 'The Pebble and the Planet', Thesis (PhD): Johns Hopkins University, 2001, p. 78.

from today's scientific materialism in that it represented a “vitalization of matter as much as a materialization of life”, as Haeckel, whilst hostile to Christianity, had pantheistic sympathies.⁴⁵⁰ Haeckel nonetheless insisted that all biological phenomena were reducible to physico-chemical causes and that any theory which failed to meet this monistic criteria, e.g. by invoking forbidden teleologies or idiosyncratic 'biological laws', was to be dismissed as ‘dualism’. A scientific theory was further only compatible with Haeckel's metaphysics if it concurred with what Gliboff dubs the ‘doctrine of external causes’, following which “the admissible causes of variation could not include either divine providence or any influences of the organism’s own purposes, mind, spirit, or just about any internal initiative”.⁴⁵¹ The Haeckelian demand that the ascribed causes of variation be external presupposed that typicality and divergence were products of organismic responses to the conditions of existence, and therefore tacitly assumed that little could be discovered about evolutionary variation through the study of organisms under unvarying laboratory conditions. Understanding the environment was as important as understanding heredity, and external conditions could therefore not be dismissed from consideration as geneticists would later claim. Wild types, moreover, were the product of wild conditions and could not be domesticated without triggering changes in the organisms.

Weismann, whilst having little time for Haeckel's monism, supported the reductionistic drive to limit the number of causal powers invoked in science. He wrote approvingly that Haeckel’s plastidule theory, by proposing “that heredity depends upon the transference of motion, and variability upon a change of this motion”, helped promote the reductionist goal that “all laws must finally be merged in laws of motion”.⁴⁵² Weismann also endorsed Haeckel’s theory of recapitulation, in earlier research on the origins of caterpillar markings being pleased to state his conclusions as “entirely based on Fritz Muller's and Haeckel's view, that the development of the individual presents the ancestral history *in nuce*, the ontogeny being a condensed recapitulation of the phylogeny”.⁴⁵³ Weismann further shared Haeckel's

450 Robert J. Richards, *The Tragic Sense of Life: Ernst Haeckel and the Struggle over Evolutionary Thought*. Chicago: University of Chicago Press, 2008, pp. 124-128.

451 Sander Gliboff, ‘Monism and Morphology at the Turn of the Twentieth century’, *Pre-circulated paper for the International Colloquium on “The Monist Century, 1845–1945: Science, Secularism and Worldview,”* Queens University, Belfast, 2–3 October 2009, 2009, p. 2.

452 August Weismann, *Studies in the Theory of Descent Vol. II*, Raphael Meldola (Trans.), Sampson Low, Marston, Searle, & Rivington, 1882, pp. 667-669.

453 Stephen Jay Gould, *Ontogeny and Phylogeny*, The Belknap Press of Harvard University Press, 1977, pp. 102-3; & August Weismann, *Studies in the Theory of Descent Vol. I*, Raphael Meldola (Trans.), Sampson

fervent rejection of internalist evolutionary theories, dedicating much of his 1875 book *Studies* to the refutation of attempts, such as Ernst Nägeli's "*automatic perfecting process*", to introduce a "developmental force" into evolution.⁴⁵⁴ Weismann asserts that that the acceptance of "a phyletic vital force... involves a renunciation of the possibility of comprehending the organic world", for "*the admission of this power is directly opposed to the laws of natural science*, which forbid the assumption of *unknown* forces as long as it is not demonstrated that *known* forces are insufficient for the explanation of the phenomena". He also thought such a phyletic vital force incompatible with selection, maintaining that "both together are inconceivable".⁴⁵⁵ Weismann moreover argued that "A change arising from purely internal causes seems to me above all quite untenable, because I cannot imagine how the same material substratum of physical constitution of a species can be transferred to the succeeding generation as two opposing tendencies... All changes, from the least to the greatest, appear to me to depend ultimately on only external influences: they are the response of the organism to external inciting causes".⁴⁵⁶ Consequently, evolutionary variation without external causation could not be entertained, and therefore bringing organisms into uniform controlled conditions, whilst productive of initial variation, would be useless as a means of studying evolutionary variation once a lineage was adapted to lab conditions, for there would be no stimulus to further variation. Any appearance of variation in lab-adapted strains was instead interpreted by Weismann as an effect of panmixia, which had no correlate with the composition of variations by selection under nature's struggle for existence. Haeckel's externalism thus led Weismann to be sceptical of the possible use of domesticated strains as a means of studying general variation.

Haeckel did not respond well to Weismann's theory of germ-line sequestration, which he held to have preformationist implications. Because Haeckel believed preformationism could only work in an adaptive evolutionary context by anticipating, rather than merely reacting to, environmental changes, he considered it to imply the necessity of some form of intelligence

Low, Marston, Searle, & Rivington, 1882, p. 270.
454 August Weismann, *Studies in the Theory of Descent Vol. I*, Raphael Meldola (Trans.), Sampson Low, Marston, Searle, & Rivington, 1882, p. xviii; & Carl von Nägeli, *A Mechanico-Physiological Theory of Organic Evolution*, 2nd Ed., Chicago: The Open Court Publishing Co., 1914, p. 36.
455 August Weismann, *Studies in the Theory of Descent Vol. II*, Raphael Meldola (Trans.), Sampson Low, Marston, Searle, & Rivington, 1882, pp. 637-638 & 705.
456 August Weismann, *Studies in the Theory of Descent Vol. I*, Raphael Meldola (Trans.), Sampson Low, Marston, Searle, & Rivington, 1882, pp. 114-115.

ensuring synchrony between organism and environment (he elsewhere militaristically declares “Either epigenesis and descent or preformation and Creation!”).⁴⁵⁷ Haeckel subsequently cast aspersions on sequestration, maintaining that “The hypothetical ‘continuity of the germ-plasm’ is neither empirically demonstrable nor theoretically acceptable”. He further more pointed to the fact that recent cytological observations had cast doubt on Weismann's original mosaic theory of development through the differential parcelling out of determinants, with observations of “the finer morphological behaviour of the karyoplasm and the cytoplasm in the fertilisation and cleavage of the egg provid[ing] no evidence for it” (the mosaic theory would by 1901 be thoroughly discredited and regulative theories of development preferred in its stead).⁴⁵⁸ Haeckel thereby regarded the idea of the absolute independence of germ-plasm from soma-plasm as “purely speculative” and determinants as “purely hypothetical elements” of which experimentation had done nothing to prove their existence.⁴⁵⁹ Haeckel's determination to refute sequestration demonstrates that whilst Weismann did not view his theories as a threat to externalist doctrine, Haeckel himself perceived otherwise and saw in Weismann a challenge to Darwin's correlational theory of variation.

Weismann drifted further away from Haeckelian orthodoxy with his 1895 adoption of the theory of germinal selection, which Winther cites as signalling Weismann's move to an externalist selectionism.⁴⁶⁰ Germinal selection originated as an attempt to counter Spencer's claim that natural selection could not explain why useful variations regularly arise in evolution. Spencer cited examples such as the extremely rudimentary state of whale hindlimbs, which he believed could only be explained in terms of the Lamarckian effects of disuse as their diminution went far beyond any adaptive requirement.⁴⁶¹ To explain such cases, Weismann postulated that not only can changes in nutrition lead to quantitative

457 Sander Gliboff, ‘The Pebble and the Planet’, Thesis (PhD): Johns Hopkins University, 2001, pp. 103-111; & Sander Gliboff, ‘Monism and Morphology at the Turn of the Twentieth century’, *Pre-circulated paper for the International Colloquium on “The Monist Century, 1845–1945: Science, Secularism and Worldview,”* Queens University, Belfast, 2–3 October 2009, 2009, pp. 138-139.

458 Mario A. Di Gregorio, *From Here to Eternity: Ernst Haeckel and Scientific Faith*, Göttingen: Vandenhoeck & Ruprecht, 2005, pp. 484-485; & Ron Amundson, *The Changing Role of the Embryo in Evolutionary Thought: Roots of Evo-Devo*, Cambridge University Press, 2005, pp. 170-172.

459 Ernst Haeckel, ‘Charles Darwin as an Anthropologist’, *Darwin and Modern Science*, A.C. Seward (Ed.), 1910, p. 140.

460 Rasmus G. Winther, ‘August Weismann on Germ-Plasm Variation’, *Journal of the History of Biology*, Vol. 4, 2001, pp. 542-543.

461 Stephen Jay Gould, *The Structure of Evolutionary Theory*, The Belknap Press of Harvard University Press, 2002, pp. 204-205.

variations in determinants but also that individual determinant-types compete for this nutrition, resulting in the greater attenuation of useless traits. Weismann also believed that germinal selection would increase the general range of variation upon which natural selection could act.⁴⁶² His critics, however, pointed out that this variation by and large still remained quantitative, not qualitative, so did not solve the problem of novelty. Additionally, germinal selection appeared to suggest that internal forces (the competing determinants) could direct evolution along trajectories antithetical to the adaptive interests of the organism as a whole.⁴⁶³ Weismann at first sought to avoid this implication by making the *ad hoc* suggestion of a correlation between selection at the personal and the germinal levels, whereby competition between determinants would only push variation in directions beneficial to the organism as a whole. This idea did not prove credible and in his later rendition of the theory in 1904 Weismann acknowledged that germinal selection on determinants would on occasion direct variation along maladaptive trajectories.⁴⁶⁴ The utility of this alteration was to provide a hypothetical but strictly physicalist causal account for how limited directional and non-adaptive evolution might take place. This helped combat the claims of some anti-Darwinian evolutionists, such as the American paleontologist Alpheus Hyatt, who argued that natural selection could not explain certain patterns in evolutionary history, e.g. the apparent overgrowth of ammonite shells, which he attributed to 'orthogenesis', the non-adaptive internally driven tendency for species to directionally vary over time. Indeed, the American Darwinian Vernon Kellogg approvingly described germinal selection as “a *real* orthogenesis” that he believed could help explain ‘un-Darwinian’ development in ‘causo-mechanical’ terms, i.e. without Hyatt's mysterious internal drives.⁴⁶⁵ But whilst Weismann could now account for non-adaptive variation, he had also embraced a hierarchical theory of selection that ran contrary to the orthodox interpretation of Darwinism as a single-level theory of selection restricted to operating at the level of the organism for the benefit of individuals and their descendants.⁴⁶⁶ The 1904 version of germinal selection therefore eschewed more

462 Rasmus G. Winther, ‘August Weismann on Germ-Plasm Variation’, *Journal of the History of Biology*, Vol. 4, 2001, pp. 542-5.

463 Sander Gliboff, ‘Monism and Morphology at the Turn of the Twentieth century’, *Pre-circulated paper for the International Colloquium on “The Monist Century, 1845–1945: Science, Secularism and Worldview,”* Queens University, Belfast, 2–3 October 2009, 2009, pp. 1-3.

464 Rasmus G. Winther, ‘August Weismann on Germ-Plasm Variation’, *Journal of the History of Biology*, Vol. 4, 2001, p. 544.

465 Vernon L. Kellogg, *Darwinism To-Day*, New York: Henry Holt and Company, 1907, p. 135.

466 Stephen Jay Gould, *The Structure of Evolutionary Theory*, The Belknap Press of Harvard University Press, 2002, p. 63.

optimistic interpretations of natural selection as always tending to work for the ultimate good of organisms and lineages.

Weismann's new ontology of selection left much less room for Haeckel's claim of evolution as mechanistic but generally progressive. Moreover, whilst Weismann continued to profess himself to be an externalist, it is clear that his theories of germinal sequestration and germinal selection played a major role in making appeals to internal forces and downplaying the role of the environment in variation acceptable in Darwinian evolutionary theory. This undermined Haeckel's orthodoxy of the doctrine of external causes and created a rift between Darwinians who continued to accept the inheritance of acquired characteristics and those who believed the germ-line shielded from somatic influences. Whilst Weismann did not deny the influence of external conditions on organismic variation, he constrained their influence and emphasised the role of internal forces at their expense. He therefore, in spite of his externalist inclinations, played a significant role in Bonneuil's 'sanctuarisation' of heredity. Furthermore, by foregrounding the determinant as an independent entity which on occasion acted for its own advantage against that of the organism as a whole, Weismann made a significant contribution to the early 20th century development of genocentrism, with its accompanying rejection of both the primacy of external environmental influences and the importance of genetic interactions in the ontogeny of adaptive traits. Whilst sanctuarisation would enable wild types to cross the domestic-wild divide into the laboratory, this incipient genocentrism would eventually disintegrate wild types into their component genes. In this manner, Weismann's 'Neo-Darwinism', as his pan-selectionist evolutionist paradigm would come to be known, would both contribute to the domestication of wild types and to their ultimate destruction as 'epistemic things'.

Chapter 8 – ‘Revisionary Darwinians’: De Vries & Bateson, their Influences & their Solutions to the Problem of Variation

'Revisionary Darwinism': the Influence of Galton and Weismann

This chapter will examine the origins and development of the research programmes of Hugo de Vries and William Bateson, 'revisionary Darwinians' whose efforts to understand and experimentalise variation would open the way for the domestication of wild types as tools for genetic research. I will begin by examining their theoretical influences, then discuss their individual research histories prior to the 'rediscovery' of Mendel in 1900. I will discuss their post-1900 work in the next chapter. Whilst not downplaying Mendel's contribution to their subsequent thought and practice, I will in this chapter seek to show that both de Vries and Bateson were already convinced before 1900 that the causes of evolutionary variation were internal, that the germ-line was sequestered, and that the most promising avenue for scientific investigation lay in bringing organisms under controlled conditions and subjecting them to experimental intervention (mostly in the form of breeding experiments). This move towards experimentally domesticating heredity and variation would prove a platform for the wild type's crossing the threshold into the laboratory.

I will preface my discussion of de Vries and Bateson by noting that they were part of a larger, heterogeneous group of late 19th century evolutionary theorists and experimentalists investigating variation as a means of establishing the process of evolution. This group, which included many of the founders of genetics, was characterised by a seemingly paradoxical common tendency to dismiss natural selection but profess admiration for Charles Darwin.⁴⁶⁷ Similarly, they were influenced to accept the sanctuarisation of heredity by Weismann's sequestration theory (so had little time for the inheritance of acquired characteristics favoured by many Darwinian 'moderates') but strongly rejected his 'all mighty' selection, tending instead to favour Galtonian saltational variations as the main driver of evolutionary change. This group were therefore not strict anti-Darwinians but they were far from orthodox.⁴⁶⁸ Roll-

467 Bowler notes that this only appears paradoxical to us due to our failure to appreciate that it was common descent not natural selection that most 19th century evolutionists saw as the chief theory and scientific contribution of Darwin's *Origin*. See: Peter J. Bowler, *The Eclipse of Darwinism: Anti-Darwinian Evolution Theories in the Decades around 1900*, The John Hopkins University Press, 1983, pp. 12-13.

468 For an example of members of this group being portrayed as anti-Darwinists, see: Ernst Mayr, *The Growth of Biological Thought: Diversity, Evolution and Inheritance*, The Belknap Press of Harvard University Press, 1982, p. 777 (Bateson and de Vries are described as "essentialists and saltationists" opposed to Darwinism). T.H. Morgan admittedly openly flirted with "anti-Darwinism" (1903) but his hostility seems principally aimed at Weismann's Neo-Darwinism as opposed to Darwinism in general. Notably, he later

Hansen has suggested a possible umbrella term of 'reform Darwinism' to characterise this group (he marks Wilhelm Johannsen out as a 'reform Darwinian' and suggests many of his fellow plant breeders, who were disappointed with the outcomes of systematic mass selection experiments, also qualified under this label).⁴⁶⁹ To avoid confusion with the American anti-Spencerian social Darwinists also termed under this label, I will instead of 'reform Darwinians' use my own alternative coining, 'revisionary Darwinians', a label which, whilst lacking grounding in historical usage, does not have the problem of an established and different prior usage.⁴⁷⁰ I believe this title is also more appropriate given that whilst this grouping was ostensibly loyal to Darwinism, it is certainly true that their attempts to reform evolutionary theory were widely seen as reading Darwin both against the grain of 'moderate' interpretations and in conflict with pan-selectionist 'Neo-Darwinian' interpretations. Alongside de Vries and Bateson, I also include Wilhelm Johannsen and T.H. Morgan, both of whose work I will discuss later, as 'revisionary Darwinians'.

The differences between de Vries and Bateson's revisionary views compared to those orthodox Darwinians who held to the correlational theory of variation are particularly evident when it comes to the matter of 'wild type'. Through their experimental and theoretical work, wild types were transformed from unstable products of the economy of nature to inherently stable types constant in their heredity. This transformation was enabled through both assuming Weismann's sequestration of heredity and developing saltational theories of variation. In his 1894 work *Materials*, Bateson, discussing two forms of cockroach, describes how "as the structure of the typical form varies around its mean condition, so the structure of the variety varies around another mean condition", and from this argues that it may be usual in what he calls 'discontinuous variation' "that in varying the organism passes from a form which is the normal for the type to another form which is a normal [sic] for the variety".⁴⁷¹ De Vries similarly identifies a "'species-forming' variability" which he terms "Mutability", and

states in 1909 that "Darwin's method is our method, the way he pointed out we follow" – See: Thomas Hunt Morgan, *Evolution and Adaptation*, The Macmillan Company, 1903, pp. 154-166, & Thomas Hunt Morgan, 'For Darwin', *The Popular Science Monthly*, J. McKeen Cattell (Ed.), Vol. LCCIV, New York: The Science Press, 1909, p. 380.

469 Nils Roll-Hansen, 'Sources of Wilhelm Johannsen's Genotype Theory', *Journal of the History of Biology*, Vol. 42, 2009, p. 465.

470 For details on American 'reform Darwinism', see: Maureen L. Egan, 'Evolutionary Theory in the Social Philosophy of Charlotte Perkins Gilman', *Hypatia*, Vol. 4, No. 1, The History of Women in Philosophy, 1989, pp. 104-6.

471 William Bateson, *Materials for the Study of Variation treated with Especial Regard for to Discontinuity in the Origin of Species*, Macmillan and Co., 1894, p. 68.

further which is distinguished from the “ordinary or so-called individual variation” which Darwin had identified as the material for the origin of species. This individual variation de Vries held to be incapable of bringing about evolutionary change, regarding it as merely environmentally induced and non-inherited “fluctuation” around the species norm.⁴⁷² It was thus by mutation, a stochastic and non-directional process, that de Vries held wild types and other specific forms to be produced. These forms were moreover considered to be either “elementary species” or “retrograde varieties”, the former being distinct from their parent species due their possessing an additional hereditary character whereas the latter were regarded as derivative but could similarly be kept constant if pedigree bred. De Vries distinguished these “real units” from the “systematic species” of taxonomy, which he regarded “practical units” but “not really existing entities”.⁴⁷³ The affiliation between wild types and other so-called varieties was therefore for de Vries an interspecific, not intraspecific, relationship.

Bateson and de Vries attributed the stability of wild types as unchanging “units of nature” to the stability of the biochemical elements that composed their constitution. The discontinuous character of mutations was seen as further evidence for this chemical basis to the nature of types. De Vries for example observes how “Transitions... are as completely absent between... [elementary species] as they are between the molecules of the chemist”, and compares the production of a new elementary species through mutation to “a chemical substitution”.⁴⁷⁴ Bateson similarly asserts that “It is on the whole not unreasonable to expect that the definiteness of at least some Substantive Variations depends ultimately on the discontinuity of chemical affinities”, citing the example of colour, there being many instances of chromatic discontinuities in nature and it further being well-known that discontinuous pigmentation changes can be effected in organisms by administering chemical reagents.⁴⁷⁵ Bateson and de Vries thus both contributed to the 'devitalization' (Bonneuil) of wild types and

472 Hugo de Vries, *The Mutation Theory: Experiments and Observations on the Origin of Species in the Vegetable Kingdom, Vol. I: The Origin of Species by Mutation*, J.B. Farmer and A.D. Darbishire (Trans.), Chicago: The Open Court Publishing Company, 1909, pp. 4-5.

473 Hugo de Vries, *Species and Varieties: Their Origin by Mutation*, Daniel Trembly MacDougal (ed.), The Open Court Publishing Company, 1905, pp. 12-14.

474 Ibid., p. 460; & Hugo de Vries, *The Mutation Theory: Experiments and Observations on the Origin of Species in the Vegetable Kingdom, Vol. I: The Origin of Species by Mutation*, J.B. Farmer and A.D. Darbishire (Trans.), Chicago: The Open Court Publishing Company, 1909, pp. 3-4.

475 William Bateson, *Materials for the Study of Variation treated with Especial Regard for to Discontinuity in the Origin of Species*, Macmillan and Co., 1894, pp. 71-72.

therefore helped legitimise the idea that not only would the constitution of wild types be unaffected by the transition into an experimental laboratory ecology but also that once domesticated wild types could be depended upon to remain stable, much as the materials of a chemistry lab could be depended on not to deteriorate so long as they were properly purified and isolated. The variation of the progeny of crosses between purified wild types and mutant strains was similarly interpreted as analogous to a reaction generated by the combining of chemicals. Reversion to the wild type was meanwhile interpreted as due to the activation or addition of a hereditary factor previously lost in the transition into domestication.⁴⁷⁶

The scientific domestication of wild types in the lab or experimental garden would not only allow for purification and experimental control over variation but would also allow for study of greater variation than that found in nature itself. Whilst they denied its creative capacity, Bateson and de Vries did very much acknowledge natural selection's destructive ability to exterminate unfit variations. The prevalence of wild types in nature as compared to other varieties was down to their better capacity to survive under such conditions. The laboratory, by 'suspending' the effects of natural selection, allowed deficient and deleterious forms to perpetuate themselves and therefore be studied. De Vries thus comments that whilst in nature most mutants "have as a rule but a very short lease of life", the experimental garden instead preserved what "among wild-growing plants, is lost to observation".⁴⁷⁷ Laboratory conditions were thus valorised over the wild as sites for the study of variation's full range (another example of Kohler's 'cultural body-snatching').

Overall, Bateson and de Vries' new interpretation of wild type was based on an acceptance of Darwin's claim that evolution is based on common descent and not inherently directional, but both scientists rejected the fundamental Darwinian argument that species are historical entities in a state of flux. They recognised that species did tend to show fluctuating variability around a norm but did not regard this as 'real' variation in that they believed it merely to affect the external properties of organisms and not the internal hereditary units which constituted their true species identity. These hereditary units were thought chemical in nature and like other chemical substances tended to be inherently stable and only capable of

476 Hugo de Vries, *Species and Varieties: Their Origin by Mutation*, Daniel Trembly MacDougal (ed.), The Open Court Publishing Company, 1905, pp. 254-5; & Elof Axel Carlson, *The Gene: A Critical History*, W.B. Saunders Company, 1966, pp. 58-62.

477 Hugo de Vries, 'The Origin of Species by Mutation', *Science*, New Series, Vol. 15, No. 384, 1902, p. 723.

discontinuous change. The nature of such changes, it was believed, could only be explicated in terms of internal stochastic dynamics – the external conditions of nature could at most stimulate variation but had no capacity to affect either the form or the direction of the variational change in these internal units as they were almost entirely shielded from its effects. Consequently, wild types and other ‘elementary species’ could be legitimately studied under domestic conditions, as it was not believed that such environs had any effect on their heritable nature. Indeed, it was only in the laboratory that the true ‘units of nature’ could be properly studied in isolation, as in nature closely related elementary species would tend to interbreed and vary into one another. The wild type thus would become a dehistoricised (Bonneuil), de-natured, purified, experimentalised and thoroughly domesticated component of a set of laboratory-based systems of scientific knowledge production. Having explored the impact of Bateson and de Vries' theories on conceptions of wild type, I will now seek to establish how it was they arrived at such radical conclusions.

De Vries

Hugo de Vries was a Dutch botanist who had trained in the study of plant physiology under Julius Sachs, a pedagogy which influenced his career-long tendency to interpret biological phenomena in terms of their biochemistry and the mechanistic function of their parts.⁴⁷⁸ His first major contribution to evolutionary theories of heredity and variation was his 1889 book, *Intracellular Pangenesis*. De Vries specifically credited Darwin and Weismann as his two major influences in arriving at his theory of intracellular pangenesis, stating that it was from the former that he derived the idea of discrete hereditary particles representing individual characters and from the latter that he attained the understanding of acquired somatic characters as playing no significant role in inheritance and evolution.⁴⁷⁹ De Vries' modified theory of pangenesis, whilst having obvious debts to Darwin, is indeed, like Weismann's theory of the germ-plasm, as much an attempt to explain how developmental differentiation occurs as a theory of heredity. Because, unlike Weismann, he worked with plants as opposed to animals, and was therefore familiar with examples of vegetative reproduction (e.g. the ability of a new begonia plant to develop from a leaf or stem), de Vries was unconvinced by the argument for mosaic development and a firm dichotomy between somatoplasm and germ-

478 Stephen Jay Gould, *The Structure of Evolutionary Theory*, The Belknap Press of Harvard University Press, 2002, p. 419-420.

479 Ida H. Stamhuis, Onno G. Meijer and Erik J. A. Zevenhuizen, ‘Hugo de Vries on Heredity, 1889-1903: Statistics, Mendelian Laws, Pangenesis, Mutations’, *Isis*, Vol. 90, No. 2, 1999, p. 243.

plasm. He asserted there to be “nowhere a sharp line of demarcation between the secondary germ-tracks and the somatic tracks of the plant. The latter have developed only quite gradually out of the former. And even though they have in fact often lost the power of reproduction, everything speaks in favor of the fact that they still very frequently possess it potentially”. De Vries bolstered his argument by pointing to the phenomenon of galls, which are formed when a parasite utilises a plant’s own genetic resources to form a protective shell. He observed that since galls “even at their highest differentiation, are built up of only such anatomical elements as are otherwise found in the plant bearing them”, and as they are “not at all restricted [in their components] to the anatomical elements of the organs on which they originate”, that therefore “*all, or at least the greatest number of the cells of the plant-body contain all the hereditary characters of the species in a latent condition*”.⁴⁸⁰ De Vries explained this latency of the pangens in terms of their residing in the cell nucleus. Pangens were only activated when they crossed into the cytoplasm, and it was this comparatively limited motility that he referred to as ‘intracellular pangeneses’.⁴⁸¹ Activation required that a pangen duplicate within the nucleus, with one of the two sibling units migrating into the cytoplasm to perform its functional roles and the other being retained as a developmental resource. This model, unlike Weismann’s, allowed all cells to retain the determinants necessary to produce a new organism, as opposed to exhausting them in their functional usage, thus allowing for vegetative reproduction (which Weismann could only account for with the *ad hoc* supposition that some somatic cells retained latent germ-plasm in their idioplasm).⁴⁸²

Theunissen has argued that de Vries was motivated to change his model of pangen activation and latency from a developmental one in 1889 to an evolutionary one c. 1900 as a consequence of reading Mendel. In the 1889 model, activity and latency were temporary states which were not necessarily inherited, whereas in de Vries’ later *Mutationstheorie* these are permanent, hereditary states of the pangens which are only altered by mutation.⁴⁸³ But

480 Hugo de Vries, *Intracellular Pangenesis*, C. Stuart Gager (Trans.), Chicago: The Open Court Publishing Company, 1910, pp. 115-119.

481 P. Kyle Stanford, *Exceeding our Grasp: Science, History, and the Problem of Unconceived Alternatives*, Oxford University Press, 2006, p. 108.

482 Ida H. Stamhuis, ‘The Reactions on Hugo de Vries’s *Intracellular Pangenesis*; the Discussion with August Weismann’, *Journal of the History of Biology*, Vol. 36, 2003, pp. 146-147.

483 Bert Theunissen, ‘Closing the door on Hugo de Vries’ Mendelism’, *Annals of Science*, Vol. 51, 1994, pp. 236-238.

though de Vries' version of intracellular pangenesis c. 1900 is certainly distinct from its original incarnation, there are important continuities which show that de Vries' views on heredity and variation, and thus on wild type, were already well developed in this earlier period. Firstly, de Vries already in 1889 recognised there to be two distinct forms of variability, 'fluctuating' and 'species-forming', which would form the basis for his later 1894 distinction, also made about the same time by Bateson, between 'continuous' and 'discontinuous' variation.⁴⁸⁴ He based this distinction on Darwin's argument that variability could be attributed to "two distinct groups of causes", the first group being variations in gemmule number and activity, the second being those cases where variation was due to gemmule modification due to the direct action of changed conditions or the use and disuse of parts.⁴⁸⁵ But de Vries differed from Darwin in assuming, following Weismann, that there was no possibility of somatic cells communicating new characters to the germ-line.⁴⁸⁶ This led him to conclude that the formation and nature of novel kinds of pangens as occurred in 'species-forming' variability must be due to stochastic internal forces. In particular, he suggested that whilst "pangens, in their division, produce, as a rule, two new pangens that are like the original one", "exceptionally these two new pangens may be dissimilar", and that as the novel one of these two pangens multiplies it will exercise an influence on the organism's phenotype.⁴⁸⁷ Already at this early stage, de Vries believed this 'species-forming' variability, later his 'mutability', to be the only means (as the name implies) by which new species could arise. 'Fluctuating' variability due to variation in pangen numbers, which produced the individual differences within species, he regarded to be ultimately caused by conditions of the environment such as nutrition. Whilst he accepted them to be "in a way, hereditary" (since elements of the cytoplasm as well as the nucleus were inherited by offspring), he observed that they are both subject to constant change and are on average highly stable over generations – "The average stature of man remains the same in the course of centuries, for the same race (elementary species), but the individual stature changes constantly from one individual to another".⁴⁸⁸

484 Hugo de Vries, *Intracellular Pangenesis*, C. Stuart Gager (Trans.), Chicago: The Open Court Publishing Company, 1910, pp. 73 & 214; & Jean Gayon, *Darwinism's Struggle for Survival: Heredity and the Hypothesis of Natural Selection*, Matthew Cobb (Trans.), Cambridge University Press, 1998, pp. 172.

485 Charles Darwin, *The Variation of Animals and Plants under Domestication: Vol. 2*, 2nd ed., London: John Murray, 1885, p. 390.

486 Hugo de Vries, *Intracellular Pangenesis*, C. Stuart Gager (Trans.), Chicago: The Open Court Publishing Company, 1910, p. 6.

487 *Ibid.*, p. 214.

488 *Ibid.*, pp. 244-245.

De Vries therefore unlike Darwin, but like Galton, did not believe ‘continuous variations’ could ever produce new species. This was furthermore based on an understanding of species as ultimately homogeneous in their inheritance of what he termed “specific characteristics”. De Vries distinguished these from the ‘individual features’ he attributed to changes in pangen numbers induced by the environment, asserting specific characteristics to refer to the “units in the hereditary substance of the nuclear thread”, the “sum total” of which “forms that which distinguishes any given species from all others”. This sum total consisted of those pangens which remained within the nucleus, as opposed to those activated in the cytoplasm, which whilst responsible for pangen expression were unable to influence nucleic pangens. On the basis of this argument, he therefore concluded that “Individual differences are thus not included in the type of the species”, i.e. the phenotypic differences between individuals belie their sharing an identical sum total of pangens in their cell nuclei. De Vries’ belief that all members of an elementary species share the same specific characteristics has been traced by Stamhuis to an erroneous interpretation of Weismann. In his preliminary version of *Intracellular Pangenesis*, de Vries “had written that according to Weismann all the ancestral plasms of one individual were identical”. His friend Jan Willem Moll pointed out that “Certainly Weismann does not assume that the germ plasm is uniform. After all he sketches it as consisting of all kinds of striped and spotted ancestors”. De Vries acknowledged this mistake but continued to maintain an idea of the hereditary substance as homogeneous, resulting in criticism from Weismann himself in 1892.⁴⁸⁹ Whether de Vries’ misreading of Weismann was his inspiration or simply an indication of pre-existing beliefs, what is clear is that he continued to maintain and refine this belief concerning the nature of specific heredity, it being central to his ideas on species and evolution in both this earlier work and in the later *Mutationstheorie*. In particular, de Vries’ idea of the ‘elementary species’ as opposed to the systematic species as the fundamental unit of nature can be seen to have already been at least partially developed by 1889, as seen from the quote above regarding the variation of stature within human races. This implies that de Vries’ idea of the wild type as a fundamentally static entity and ‘elementary species’ in itself was almost certainly well developed long prior to his rediscovery of Mendel.

489 Ida H. Stamhuis, ‘The Reactions on Hugo de Vries’s *Intracellular Pangenesis*; the Discussion with August Weismann’, *Journal of the History of Biology*, Vol. 36, 2003, pp. 141-142.

As to whom the primary influence was in shaping such beliefs about species, it would seem likely that Galton's arguments concerning the impossibility of extant variability overcoming the limits imposed by the populational norm, and his related contention that only internally directed saltational change could create new species, had percolated into de Vries' consciousness through his being immersed in a scientific culture sceptical of the power of natural selection and in which complementary and alternative evolutionary mechanisms were widely entertained. I say percolated as de Vries, according to James Schwartz, only became aware of the work of Adolphe Quetelet (the Belgian statistician whose work on human average variation was the major influence on Galton's methods of analysing heredity) as late as 1887, and only read Galton himself in 1894, having been stimulated to do so when he read a paper of Weldon's the previous year which utilised Galton's statistical methods.⁴⁹⁰ Gould asserts de Vries to have stated himself to have read Quetelet and Galton in the early 1870s, but this is contradicted not only by Schwartz but also by de Vries' statement in 1917 that he had begun his famed *Oenothera* experiments (which started in 1886) prior to reading Quetelet ("I set at work at once, first in the field but soon in the garden. I cultivated over a hundred wild species... Fluctuating variability was everywhere present. Then I chanced to meet with Quetelet's *Anthropométrie*... applied his methods to plants and saw that here the same general laws prevail").⁴⁹¹ This appears to corroborate Schwartz's account. The influence of Quetelet on de Vries' views in *Intracellular Pangenesis* is quite evident in his discussion of fluctuating variation. With regards to Galton, however, it appears de Vries was not directly influenced by him before 1893-4, having in the meantime arrived at many of his parallel conclusions concerning heredity and variation independently. Reading Galton did most likely help de Vries codify his existing perception of variability as bimodal, in particular in his coming to view from 1894 onward variation as either 'continuous' or 'discontinuous'. De Vries also did explicitly reference Galton's polygonal model of variation in his *Mutationstheorie*, a metaphor he considered "very beautiful".⁴⁹² But Galton's main role in influencing de Vries likely lay in confirming what was already suspected and in providing methodological tools to refine the analysis of his findings. Overall, it appears that Quetelet's

490 James Schwartz, *In Pursuit of the Gene: From Darwin to DNA*, Harvard University Press, 2008, pp. 79-80.

491 Stephen Jay Gould, *The Structure of Evolutionary Theory*, The Belknap Press of Harvard University Press, 2002, p. 430; & Hugo de Vries, 'The Origin of the Mutation Theory', *The Monist*, Vol. 27, No. 3, 1917, p. 406.

492 Hugo de Vries, *The Mutation Theory: Experiments and Observations on the Origin of Species in the Vegetable Kingdom, Vol. I: The Origin of Species by Mutation*, J.B. Farmer and A.D. Darbishire (Trans.), Chicago: The Open Court Publishing Company, 1909, pp. 53-55.

methods of analysis were more useful to de Vries, Schwartz observing that they offered a more “simple way to compute the probabilities of the expected outcomes” compared to Galton’s techniques. Quetelet’s probabilistic techniques would prove central to the project that de Vries launched from 1895 onward of determining a “law of pangenes” through the study of hybrids, a research programme which would subsequently lead to de Vries replicating Mendel’s experimental findings.⁴⁹³ But whilst these findings and de Vries’ ‘rediscovery’ of Mendel in 1900 are commonly seen as central to the beginnings of Mendelian genetics, they had little impact on de Vries’ own views on heredity and variation because he saw Mendel’s laws as having little to do with ‘natural heredity’. Mendel’s laws only applied to hybrids, which were uncommon in nature and in which two different sets of pangens were temporarily combined, de Vries believing ‘normal heredity’ to be between like parents with identical hereditary substances. Mendelism was therefore largely a “laboratory phenomenon” which moreover was not informative about the evolutionary processes which were de Vries’ main interest as it did not appear to involve the production of new pangens.⁴⁹⁴

As I have attempted to show above, de Vries’ idiosyncratic views on heredity, variation and evolution were already well developed by 1889 (his interest in mutants, moreover, did not start with *Oenothera Lamarckiana* in 1886, having initiated a project to collect a “herbarium of monstrosities” to study the action of pangens as early as the late 1870s; *Oenothera* was moreover selected from the hundred plus species de Vries claimed to have cultivated as the one which showed the most tendency towards sudden variations).⁴⁹⁵ This is not to say that de Vries’ ideas were fully formed at the time he wrote *Intracellular Pangenesis* but rather that the *Mutationstheorie* of 1901-3 was strongly built upon the back of long-standing prior convictions which predated his experiments on *Oenothera*, his exposure to Quetelet and Galton and also his reading of Weismann. De Vries’ original training in plant physiology may have contributed to his understanding of variation as due to internal processes, but it is unlikely, given his mentor Julius Sachs’ scepticism towards investigating ‘speculative questions’, that his ideas on evolution were much influenced by this work.⁴⁹⁶ If there is a

493 James Schwartz, *In Pursuit of the Gene: From Darwin to DNA*, Harvard University Press, 2008, pp. 80-85.

494 Bert Theunissen, ‘Closing the door on Hugo de Vries’ Mendelism’, *Annals of Science*, Vol. 51, 1994, pp. 247-8.

495 James Schwartz, *In Pursuit of the Gene: From Darwin to DNA*, Harvard University Press, 2008, pp. 74-75, & Hugo de Vries, ‘The Origin of the Mutation Theory’, *The Monist*, Vol. 27, No. 3, 1917, pp 406-407.

496 Philip J. Pauly, *Controlling Life: Jacques Loeb and the Engineering Ideal in Biology*, Oxford University Press, 1987, p. 35.

primary catalyst for de Vries' views to be identified, the best candidate is likely to be his reading of Darwin's *Origin* as a student.⁴⁹⁷ It has long been noted that de Vries had a very unorthodox reading of Darwin's arguments concerning variation and selection.⁴⁹⁸ In particular, de Vries interpreted Darwin's statement that selection works on "chance variations" which "[u]nless such occur natural selection can do nothing" to imply that "Darwin attributed a very great and often preponderating, perhaps even an exclusive, significance to "single variations"', that the 'chance variation' referred to "were not therefore the extreme variants of the ordinary variability" but rather 'sports'.⁴⁹⁹ As we know, Darwin did acknowledge that selection operated both on ordinary variations and on sports, but he regarded the latter as much less important in evolution given their rarity, frequent lack of fit to adaptive needs and tendency to be diluted into irrelevance by blending inheritance. But perhaps de Vries as a student misread Darwin's intention here, with this interpretation of evolution subsequently becoming a fixed idea in his mind that would not be shifted by those who argued him to have misconstrued Darwin's argument. Indeed, it is notable that de Vries would come to ascribe the view that selection primarily acts on and forms species from individual variations to Wallace (he even speaks of this as the "one essential point" on which Wallace and Darwin's selection theory differ). Against Wallace, de Vries asserts that "It is an absolutely unproved assumption that individual variation extends its range by selection", apparently unconscious of the irony that he is here echoing the argument for limits to variation which Fleeming Jenkin had in 1867 directed *against Darwin*.⁵⁰⁰

Because Darwin was ultimately de Vries' primary influence in his understanding of heredity, variation and evolution, I consider it incorrect not to regard him as in some sense still a Darwinian (or, if not a Darwinian, then at least a "most reluctant non-Darwinian", as Gould terms him).⁵⁰¹ That his interpretation of Darwin was not only idiosyncratic but went very much against the grain of both the broader life science community's understanding of the theory and Darwin's own very much justifies his being tagged as a revisionary Darwinist.

497 James Schwartz, *In Pursuit of the Gene: From Darwin to DNA*, Harvard University Press, 2008, p. 69.

498 Stephen Jay Gould, *The Structure of Evolutionary Theory*, The Belknap Press of Harvard University Press, 2002, pp. 415-417.

499 Hugo de Vries, *The Mutation Theory: Experiments and Observations on the Origin of Species in the Vegetable Kingdom, Vol. I: The Origin of Species by Mutation*, J.B. Farmer and A.D. Darbishire (Trans.), Chicago: The Open Court Publishing Company, 1909, pp. 35-36.

500 *Ibid.*, pp. 39-41.

501 Stephen Jay Gould, *The Structure of Evolutionary Theory*, The Belknap Press of Harvard University Press, 2002, p. 415.

Notably, de Vries never appears to have taken natural selection seriously as a factor shaping evolution. As much as his reading Quetelet and Galton can be attributed to the hardening of his belief in the limited nature of fluctuating variation, it is apparent that de Vries' doubts about the possibility of selection creating new species purely by operating on existing variability preceded his coming across these texts. Already in *Intracellular Pangenesis*, we have seen that he attributed individual variations as primarily due to environmental factors such as nutrition. In the *Mutationstheorie*, he becomes even more explicit in these claims, stating that "Nutrition in the widest sense... is at the bottom of all individual variability. Every character varies only in a plus or minus direction. Favorable conditions are responsible for the former, unfavorable ones for the latter". He believes that the variation induced by nutrition can in a sense be inherited, as is made evident in the fact that vigorous parents produce vigorous children, and that since it can in this way be inherited it can be acted on by selection. He on these grounds argues that selection as commonly understood (i.e. as operating on individual variations) in fact "consists in the choice of the most highly nourished". But whilst variation in nutrition can in this way produce changes in characters in a plus or minus direction, de Vries also insists that "*nothing new can arise in this way*". This can be seen as a direct critique of Weismann's notion of variation as limited in this manner but as able to produce new species through its being acted on by selection, which de Vries argues to be impossible, as no true novelty arises by these means. Furthermore, the "fact of regression" ensures that however extreme the variation in selected individuals, there is always a constant "definite backsliding of the mean of the race" which ensures that the limits of ordinary variability are never surpassed.⁵⁰²

It is because of this understanding of ordinary variability as environmentally determined, and fluctuating but limited around a permanent mean that de Vries could only ever conceive as selection as having a permanent effect on the course of evolution in its operating as a Spencerian 'sieve' sorting between distinct species and varieties. But because he considered environmental disparities between nature and garden as essentially unproblematic for the study of heredity and evolution due to their having in general a null effect on the actual pangenesis, de Vries was also broadly supportive of the experimental investigation of heredity

502 Hugo de Vries, *The Mutation Theory: Experiments and Observations on the Origin of Species in the Vegetable Kingdom, Vol. I: The Origin of Species by Mutation*, J.B. Farmer and A.D. Darbishire (Trans.), Chicago: The Open Court Publishing Company, 1909, pp. 88-137.

and evolution in the laboratory. Indeed, he considered his seminal contribution to science to be in showing that “The origin of species is an object of experimental investigation”, placing himself in the footsteps of Lamarck, who showed the origin of species to be a natural phenomenon, and Darwin, who showed it to be an object of inquiry.⁵⁰³ De Vries’ views thus concurred with a growing consensus amongst experimental life scientists that the key to understanding heredity and evolution lay not in observations in the field but in interventions in the lab. De Vries’ conceptualisation of living processes also offered such researchers a theoretical defence against the accusation of field naturalists that results attained under lab conditions were not relevant to organisms in nature. When it came to the study of heredity and mutational variation, de Vries held the only environmental parameters to be of significance were conditions of viability and the presence or absence of factors which might possibly stimulate mutation. De Vries believed the lab would represent an ideal environment for both controlling and utilising such factors, superior, indeed to nature in this manner. By isolating organisms from nature’s struggle for existence and providing them with plentiful nutriment, elementary species other than wild types had a much greater chance of surviving to be studied. And if it could be discovered which external factors might stimulate mutation (de Vries ascribed the amount and direction of mutability to internal causes but believed mutability to be an impermanent quality which must be activated by some external cause), then mutations could be directly induced for study rather than requiring to be collected.⁵⁰⁴ In this manner, de Vries’ arguments were very much key for the notion that wild types could be transferred and experimented on in the laboratory without any significant changes to their nature except for those caused by mutation. He can further be credited as the father of non-Lamarckian experimental evolution, endeavours in which would run alongside those of early genetic analysis and which would be united in the Morgan group’s efforts to both analyse and alter *Drosophila*.

Bateson

William Bateson is primarily known for his post-1900 work, in particular his early adoption of Mendelism and defence of its claims against the arguments of biometricians such as Walter Weldon who continued to profess the evolutionary importance of continuous

503 Hugo de Vries, *Species and Varieties: Their Origin by Mutation*, Daniel Trembly MacDougal (ed.), The Open Court Publishing Company, 1905, p. v.

504 *Ibid.*, pp. 690-1.

variation. His other well-known contribution to the life sciences was terminological, giving the name ‘genetics’ to the study of heredity in 1905 and introducing much now ubiquitous disciplinary nomenclature, including ‘allele’ and the paired terms ‘homozygous’ and ‘heterozygous’.⁵⁰⁵ But focusing on the post-1900 period of Bateson’s work ignores the fact that, like de Vries, Bateson had largely made up his mind about what the nature of heredity, variation and evolution consisted in some time prior to his coming across Mendel (though some of these presumptions would later be challenged in the course of his genetics work). Indeed, the primary reason why Bateson, unlike de Vries, so vigorously adopted and evangelised Mendelism was that his pre-existing beliefs about discontinuity in variation appeared confirmed by it and, moreover, an experimental method for their further investigation was suggested. Bateson, unlike de Vries, had primarily sought to study variation prior to 1900 through fieldwork and fact collection but sincerely wished to experimentalise his research, and it was the provision of a means of doing so, through the analysis of the offspring of hybrids, that Bateson found so valuable in Mendel’s work. De Vries, by contrast, had already developed a sophisticated experimental methodology to research variation, thought hybridisation to have little relevance to the evolutionary processes he was interested in and had attained Mendel-like results independent of his reading the famed 1865 paper, so found this rediscovered work less revelatory.

It was Bateson’s two brief stints working alongside William Keith Brooks in the summers of 1883 and ’84 which first stimulated his interest in studying variation as a means to understanding evolution. Either side of these visits to America, Bateson was training in the study of descriptive morphology at Cambridge. The department there was heavily influenced by the work of Francis Balfour, who had attempted to determine the origins of vertebrates by studying the embryology of organisms considered to be primitive relations such as tunicates and *Amphioxus*. Balfour died in 1882 before Bateson properly began his Cambridge studies but his research programme endured, and Bateson’s first scientific work was on determining the phylogeny of the acorn worm *Balanoglossus*, on the suggestion of his friend and later adversary Weldon. It was as part of studying *Balanoglossus* that Bateson went to work with

505 Elof Axel Carlson, *The Gene: A Critical History*, W.B. Saunders Company, 1966, pp. 9-16.

Brooks, who was in the process of writing and publishing his *Law of Heredity*, ‘a study of the cause of variation, and the origin of living organisms’.⁵⁰⁶

In this book, Brooks proposed an amended version of Darwin's pangenesis whereby gemmules are only produced by cells on those rare occasions when they are stressed by unfavourable conditions. He moreover theorised that the male and female sexual elements had specialised in different directions. The ovum conserved heredity and thus perpetuated the species (Brooks would later claim, against claims his theory was Lamarckian, that he “like Weismann, attributed inheritance to germinal continuity”).⁵⁰⁷ The male sex cell was by contrast the ‘progressive’ element responsible for adaptation and variation, the sperm having “acquired, as its especial and distinctive function, a peculiar power to gather and store up germs”. Sperm, Brooks pointed out, are unlike ova produced throughout a male’s life, and therefore were open to the influences of environmental variation.⁵⁰⁸ Brooks theorised that when an ovum was fertilised by a gemmule-carrying sperm, these gemmules would then be inherited not only by immediate offspring but also would be represented in the ova of female offspring, thus ensuring the continuation of variation in immediate descendants. Cells which began throwing off gemmules would moreover continue to do so “until a favorable variation is seized upon by natural selection”, at which point this would be propagated, selection then acting against gemmule-producing cells given that further variation was no longer adaptive. Brooks saw this hypothesis as a means of showing that the development of new variations could occur in response to environmental change without invoking the Lamarckian claim that external conditions had a direct modifying influence. Brooks was motivated by concerns that Darwin’s original idea of natural selection as operating incrementally across vast swathes of time was incompatible with William Thomson’s estimates for the age of the Earth. He therefore assumed that variation must be better than random to allow for more rapid evolutionary change. He furthermore advocated a saltational theory of variation, in the process referring to both Huxley’s argument that Nature likely occasionally makes ‘jumps’

506 Brian K. Hall, ‘Betrayed by Balanoglossus: William Bateson’s Rejection of Evolutionary Embryology as the Basis for Understanding Evolution’, *Journal of Experimental Zoology Part B: Molecular and Developmental Evolution*, Vol. 304B, 2005, pp. 3-5; & Erik L. Peterson, ‘William Bateson from "Balanoglossus" to "Materials for the Study of Heredity": The Transatlantic Roots of Discontinuity and the (Un)naturalness of Selection’, *Journal of the History of Biology*, Vol. 41, No. 2, 2008, pp. 272-273.

507 William Keith Brooks, ‘An Inherent Error in the Views of Galton and Weismann on Variation’, *Science*, Vol. 1, No. 5, 1895, p. 121.

508 William Keith Brooks, *The Law of Heredity: A Study of the Cause of Variation, and of the Origin of Living Organisms*, Baltimore and New York: John Murphy & Co., 1883, p. 326.

and to Galton's polygon model wherein variation is conceived of as constituting a form of 'facet flipping' between pre-existing stable equilibria.⁵⁰⁹

In the introduction to his book, Brooks had stressed that he had previously refrained from publishing his views as he had hoped to first "submit them to the test of experiment". He envisioned that this would require a programme of scientific domestication involving "the cultivation and hybridization, over many generations, of such animals and plants as will thrive and multiply in confinement". He conceded, however, that he lacked the time and resources for such an endeavour.⁵¹⁰ Brooks subsequently sought to encourage students to take up such experimental work where possible. Brooks' evangelism was highly effective on Bateson, whom he managed to convince to think of variation as a problem to be solved rather than (as his Cambridge teachers and colleagues believed) a mere distraction. Brooks also exposed Bateson to the saltationist arguments of Galton and his polygonal model of organic stability. Ultimately, Brooks' intervention would persuade Bateson to abandon the laboratory in favour of attempting to find evidence in the field for Brooksian-style saltational variation in response to environmental pressures, leading to a permanent break with Cambridge morphology.⁵¹¹ The opportunity to find such evidence arose in 1886 when Bateson undertook a biogeographical expedition to Central Asia where he would spend 18 months researching the fauna of the Aral Sea and neighbouring salt lakes. Bateson believed these bodies of waters to be the remaining residue of an "Asiatic Mediterranean" that had once covered the region, and his original major aim was to catalogue whether any oceanic fauna still survived from this earlier time. This goal was thwarted due to technical difficulties, so in the summer of 1886 he switched to a new project, namely to study whether differentiations between lakes in conditions such as salinity and depth had had the effect of producing parallel differentiations in the organisms found there.⁵¹² This second aim was stimulated by the findings of the Russian biologist Schmankewitsch, who had shown that brine shrimp kept over several generations in gradually desalinated water appear to change into a freshwater

509 Ibid., pp. 296-7.

510 Ibid., p. vii.

511 Marsha L. Richmond, 'The 'Domestication' of Heredity: The Familial Organization of Geneticists at Cambridge University, 1895-1910', *Journal of the History of Biology*, Vol. 39, No. 3, 2006, pp. 576-7.

512 Beatrice Bateson (Ed.), *William Bateson, F.R.S., Naturalist. His Essays and Addresses together with a Short Account of his Life*, Cambridge: Cambridge University Press, 1928, p. 18; & Erik L. Peterson, 'William Bateson from "Balanoglossus" to "Materials for the Study of Heredity": The Transatlantic Roots of Discontinuity and the (Un)naturalness of Selection', *Journal of the History of Biology*, Vol. 41, No. 2, 2008, pp. 277-278.

form, a discovery most likely reported to Bateson by Brooks.⁵¹³ Bateson's research in these saline lakes can be seen as a utilisation of 'natural experiments', this being where researchers attempt to find conditions in the field which parallel the conditions of experimentation in the lab in order to corroborate lab findings.⁵¹⁴

Bateson's aims were ultimately disappointed, finding no definitive proof of a primordial Asian Mediterranean and no clear-cut evidence for a linear relationship "between the variations of animals and the conditions under which they live". Bateson was not put off continuing his study of variation but from this point was sceptical of the idea that the production of variability was in itself an adaptive response to external conditions. He retained from Brooks a belief in saltationism but now believed the origins of variation could only properly be studied through the experimental analysis of internal factors.⁵¹⁵ The results of Bateson's 'natural experiments' also hardened his belief that not only was variation ultimately discontinuous but also that natural selection was a far from all-powerful force. He clearly refers to his experiences in Central Asia in his 1894 *Materials* when he observes that whilst environmental conditions such as salinity are continuous, not only do species "on the whole form a Discontinuous Series" but also "forms which are apparently identical live under conditions which are apparently very different, while species which though closely allied are constantly distinct are found under conditions which are apparently the same". This leads him to conclude that it is most likely that "the relation between environment and structure is not finely adjusted", and consequently the environment cannot be either the directing cause (as held by neo-Lamarckians) or the limiting cause (as held by Darwinians) of specific differences.⁵¹⁶ The Central Asian expedition overall represented a pivotal failure in Bateson's variation research programme, which would now begin moving away from the field and migrate back towards the lab and the garden, and which would now interpret variation, not selection, as ultimately driving evolution.

513 Alan G. Cock and Donald R. Forsdyke, *Treasure Your Exceptions*, Springer Science, 2008, pp. 23-24 & 47.

514 Robert E. Kohler, *Laboratories and Labscapes: Exploring the Lab-Field Border in Biology*, Chicago and London: University of Chicago Press, 2002, pp. 212-218.

515 Staffan Müller-Wille and Marsha L. Richmond, 'Revisiting the Origin of Genetics', In: *Heredity Explored: Between Public Domain and Experimental Science, 1850-1930*, C. Brandt, H.-J. Rheinberger, and S. Müller-Wille (Eds.), Cambridge, MA: MIT Press, 2015.

516 William Bateson, *Materials for the Study of Variation treated with Especial Regard for to Discontinuity in the Origin of Species*, Macmillan and Co., 1894, pp. 5-6.

Having been convinced by his Central Asian expedition that variation must be explained in terms of internal causes but yet to ascertain a particular experimental research methodology for studying such factors, Bateson turned to fact collection and theoretical hypothesising. The primary product of the former would be his great 1894 compendium *Materials for the Study of Variation*; the major fruit of speculation was his vibrational theory of variation, which he first announced in a letter to his sister in September 1891.⁵¹⁷ *Materials* was an ambitious text, “a massive litany of some 886 aberrations covering most phyla”, which sought to collect as many examples as Bateson could find of apparent discontinuous variation in organisms in order to argue for a primary role for discontinuity in evolution.⁵¹⁸ He gathered his information primarily by “ransack[ing] museums, libraries and private collections” through correspondence with a wide range of life scientists and medical professionals, and by contacting farmers and horticultural amateurs.⁵¹⁹ He also relied on field studies performed in the early 1890s in the countryside around Cambridge, which looked at discontinuity in traits such as the secondary sexual characteristics of insects and the floral symmetry of plants (showing that despite his experiences in Central Asia, Bateson continued to see value in field studies as a means of studying variation).⁵²⁰

Bateson believed that in undertaking this project he was following in the footsteps of Darwin, in particular the Darwin of *Variation in Animals and Plants under Domestication*, who had sought to establish laws of variation by collating reports of its various manifestations, regularities and anomalies.⁵²¹ Bateson thus here presents himself as a Darwinian, but as one critical of contemporary trends in evolutionary research, so rightly may be labelled a ‘revisionary Darwinian’ like de Vries (support for my position on Bateson as a Darwinian can be found in Nordmann and Peterson).⁵²² Specifically, he argued that Darwin’s ‘Doctrine of

517 Stuart A. Newman, ‘William Bateson’s Physicalist Ideas’, In: *From Embryology to Evo-Devo: A History of Developmental Evolution*, Manfred D. Laubichler & Jane Maienschein (eds.), The MIT Press, Cambridge: Massachusetts, 2007, pp. 83-84.

518 Erik L. Peterson, ‘William Bateson from "Balanoglossus" to "Materials for the Study of Heredity": The Transatlantic Roots of Discontinuity and the (Un)naturalness of Selection’, *Journal of the History of Biology*, Vol. 41, No. 2, 2008, p. 288.

519 Alan G. Cock and Donald R. Forsdyke, *Treasure Your Exceptions*, Springer Science, 2008, p. 91.

520 Erik L. Peterson, ‘William Bateson from "Balanoglossus" to "Materials for the Study of Heredity": The Transatlantic Roots of Discontinuity and the (Un)naturalness of Selection’, *Journal of the History of Biology*, Vol. 41, No. 2, 2008, p. 284-288.

521 William Bateson, *Materials for the Study of Variation treated with Especial Regard for to Discontinuity in the Origin of Species*, Macmillan and Co., 1894, p. 13.

522 Alfred Nordmann, ‘Darwinians at War: Bateson's Place in Histories of Darwinism’, *Synthese*, Vol. 91, No. 1/2, Philosophy of Biology in Historical and Cultural Contexts, 1992, pp. 53-72; & Erik L. Peterson, ‘William Bateson from "Balanoglossus" to "Materials for the Study of Heredity": The Transatlantic Roots of

Descent' had changed naturalism, which had formerly been devoted to the "indiscriminate accumulation of facts", by providing a needed criterion by which the value of such facts might be judged. This had led to changes in method, but whilst there had been originally "a large field of possibilities", two particular methodologies had become overly relied upon and institutionalised. Bateson identified these as 'the Embryological Method' of morphology and 'the Study of Adaptation'. Both of these had their limitations and had been pushed to the point of no longer being especially productive. Studies of embryology could provide general support for the fact of common descent, but Bateson, based on his own frustrating experiences with *Balanoglossus*, had come to consider the exclusive use of this method to try and ascertain the evolutionary histories of particular lineages as a speculative waste of time. He similarly regarded studies of adaptation as not in truth capable of advancing much beyond the basic conclusion that organisms are 'more or less' adapted to their circumstances. He observed that whilst it was easy to suggest how, for example, dull colouration allowed for edible moths to camouflage themselves and thus survive, there was no means of determining what was "the particular benefit which one dull moth enjoys as the result of his own particular pattern of dullness as compared with the closely similar pattern of the next species". This tied in with what he regarded as an even more serious defect of the adaptationist approach, namely that to determine the utility of a structure required a quantitative method of assessment in order to know "how its presence affects the profit and loss account of the organism", i.e. how its utility offset the cost of its production. In the case of evolutionary history, acquiring such data is "entirely beyond our powers and is likely to remain so indefinitely". Consequently, Bateson concluded that "We have no right to consider the utility of a structure *demonstrated*, in the sense that we may use this demonstration as evidence of the causes which have led to the existence of the structure", unless such data could be somehow attained.⁵²³ Studies of adaptation, in other words, could never provide a definitive causal story for why particular new traits and species arose in evolution. Bateson

Discontinuity and the (Un)naturalness of Selection', *Journal of the History of Biology*, Vol. 41, No. 2, 2008, pp. 291-8.

523 William Bateson, *Materials for the Study of Variation treated with Especial Regard for to Discontinuity in the Origin of Species*, Macmillan and Co., 1894, pp. 7-13; & Brian K. Hall, 'Betrayed by *Balanoglossus*: William Bateson's Rejection of Evolutionary Embryology as the Basis for Understanding Evolution', *Journal of Experimental Zoology Part B: Molecular and Developmental Evolution*, Vol. 304B, 2005, pp. 7-10.

thus anticipates Gould and Lewontin in critiquing adaptationists for their propensity for ‘just-so stories’.⁵²⁴

Having established the shortcomings of the two principal methods utilised by contemporary evolutionists, Bateson argued that reviving Darwin’s historical programme of the ‘Study of Variation’ would thus provide a much required third method for evidencing the ‘Doctrine of Descent’, and moreover perceived it to have greater potential for offering a detailed and credible causal account for evolutionary processes.⁵²⁵ To speak in anachronistic terms, Bateson recognised Darwin as having established a scientific paradigm within the bounds of which all serious natural history research was now conducted; he was not, in promoting the Study of Variation, seeking to establish a new paradigm but rather to help the old one escape the *cul de sac* which he perceived current methodology to have led it to. Bateson’s proposals therefore may have been outside of Darwinian orthodoxy but remained firmly within the “Darwinian landscape”.⁵²⁶

Alongside evidencing the ubiquity of discontinuous variation in organisms, Bateson was also trying to understand how such ‘jumping’ variations might come about. Whilst he had absorbed from Brooks a belief in the importance of variation as a subject of investigation and in the evolutionary importance of saltations, it is not clear whether Bateson was ever particularly convinced of the truth of Brooks’ modified version of pangenesis as a material and mechanistic account of heredity and variation. In *Materials* Bateson heavily criticises Darwin’s original hypothesis for perpetuating what he sees as the erroneous notion that reproduction consists in “the actual body and constitution of the parent... [being] in some way handed on”. Bateson’s critique of pangenesis was partially based on the fact that he saw it as promoting the “assumption that Variation must necessarily be a continuous process; for with the body of the parent to start from, it is hard to conceive the occurrence of

524 Stephen Jay Gould and Richard C. Lewontin, 'The Spandrels of San Marco and the Panglossian Paradigm: A Critique of the Adaptationist Programme', *Proceedings of the Royal Society of London. Series B, Biological Sciences*, Vol. 205, No. 1161, The Evolution of Adaptation by Natural Selection, 1979, pp. 581-598.

525 William Bateson, *Materials for the Study of Variation treated with Especial Regard for to Discontinuity in the Origin of Species*, Macmillan and Co., 1894, pp. 13-14.

526 Alfred Nordmann, 'Darwinians at War: Bateson's Place in Histories of Darwinism', *Synthese*, Vol. 91, No. 1/2, Philosophy of Biology in Historical and Cultural Contexts, 1992, p. 56-57.

discontinuous change”.⁵²⁷ But Bateson’s criticism also extended to other materialist theories of heredity which he deemed ‘static’, a conviction which would eventually lead to his famously rejecting chromosomal accounts of heredity in the 20th century. This was not because Bateson had any sympathies for vitalistic occult forces in biology – very much the opposite, Bateson being firmly convinced that biological phenomena were ultimately grounded in physio-chemical processes. The issue was rather that Bateson was certain that biological phenomena such as heredity, development and variation were fundamentally dynamic in nature and thought particulate theories of inheritance to fail to truly explicate the operation of these phenomena. Bateson instead sought to understand these processes using kinetic force and field models taken from contemporary physics, such as those developed by James Clerk Maxwell and William Thomson. He was not unique in thinking such analogies and links could be drawn between biology and physics, T.H. Huxley in 1889 having suggested organisms to be comparable to vortex whirlpools, and that they, as all other material things, were ultimately made up of ‘aether’ arranged in atoms which themselves were spinning vortices (recall also Quatrefages’ notion of the *tourbillon vital* [‘maelstrom of life’] found in the fertilised ova, which Darwin himself refers to in *Variation*). Importantly for Bateson, kinetic force and field models were both quantifiable and dynamic, and he believed that if biological phenomena could be explained in these terms, it would produce a causal account that was also measurable and predictable.⁵²⁸ Particulate theories of heredity such as pangenesis were in Bateson’s view neither properly quantifiable nor dynamic, for they lacked any account of how hereditary material interacted in order to induce development.⁵²⁹ This attitude of Bateson’s also explains why he was never fully convinced of the value of biometry, for biometric methods only ever measure trait outcomes and can therefore only ever offer phenomenological, correlational accounts of heredity and development.

527 William Bateson, *Materials for the Study of Variation treated with Especial Regard for to Discontinuity in the Origin of Species*, Macmillan and Co., 1894, p. 75.

528 Gregory Radick, ‘Physics in the Galtonian Sciences of Heredity’, *Studies in History and Philosophy of Biological and Biomedical Sciences*, Vol 42, 2011, pp. 134-6; Alan R. Rushton, ‘William Bateson and the chromosome theory of heredity: a reappraisal’, *The British Journal for the History of Science*, Vol. 47 Part 1, No. 172, 2014, pp. 163-165; & Staffan Müller-Wille and Hans Jörg Rheinberger, *A Cultural History of Heredity*, Chicago and London: The University of Chicago Press, 2012, p. 38.

529 Stuart A. Newman, ‘William Bateson’s Physicalist Ideas’, In: *From Embryology to Evo-Devo: A History of Developmental Evolution*, Manfred D. Laubichler & Jane Maienschein (eds.), The MIT Press, Cambridge: Massachusetts, 2007, p. 91.

Bateson's vibratory theory of variation, which he believed offered a dynamic alternative to particulate hypotheses, was based on an analogy between the formation of repetitive morphological patterns, a form of variation he called 'merism', and the nodal Chladni patterns which are produced when granular materials are placed on a plate which is then caused to vibrate by acoustic resonances, the classic example being when sand is spread on the back of a violin which is then played with a bow. Bateson argued that just as discontinuous repetitive patterns in sand could be explained in terms of the vibrations imparted by a bow, so cases of the symmetrical repetition of organic parts could also be explicated in terms of mechanistic forces vibrating through the tissue of a developing organism and in this manner inducing divisions and differentiations, different frequencies producing different conformations of tissue. An exemplary example of merism was segmentation, but Bateson also argued other forms of periodic variation, e.g. zebra stripes, to be likewise produced in such a manner. He further believed that examples of the displacement of parts (e.g. an insect with a leg in place of an antenna), which he termed 'homeotic variations', could also be explained by changes in organic vibrations. Bateson did however acknowledge that his hypothesis was lacking in one important area, namely that unlike the cases of mechanistic configurations he found analogous with organic variations, a favourite example of which was patterns found in beach sand, "We cannot tell what in the [organism] corresponds to the wind or the flow of the current".⁵³⁰ This lack of known causal factors would ensure that Bateson's theory remained inchoate, closed to experimental testing and would fail to find converts. However, he would remain loyal to it at the expense of marginalisation as the 'static' materialist chromosome theory became genetic orthodoxy, and would continue to insist on the vortical nature of life and undulatory nature of variation until his death, even after admitting the truth of the chromosome theory in 1922.⁵³¹

It is important to understand that whilst Bateson has been misunderstood and ridiculed for his vibratory theory of meristic variation, his allied understanding of biological phenomena in chemical terms, which can Whiggishly be interpreted as progressive (see for instance his early adoption of an enzyme [then called 'ferments'] theory of genetic action), arose from the same physicalist mind-set (recall how Haeckel was also led by his reductionist physics to

530 Ibid.; & Alan G. Cock and Donald R. Forsdyke, *Treasure Your Exceptions*, Springer Science, 2008, p. 92.

531 Alan R. Rushton, 'William Bateson and the chromosome theory of heredity: a reappraisal', *The British Journal for the History of Science*, Vol. 47 Part 1, No. 172, 2014, pp. 167-170.

propose a vibrational theory of heredity).⁵³² For Bateson, biochemistry and kinetic field forces together accounted for biological phenomena, but could not do so one without the other. Biochemistry accounted for the constitution of organisms but there was a requirement for physical forces to explain directionality and arrangement in phenomena such as cell division. Bateson did, incidentally, point to variations in biochemistry as another source of discontinuity between varieties and species. Distinguishing them from those variations caused by merism, he referred to these as “Substantive Variations”, and argued that their discontinuity originated due to discontinuities between chemical affinities. He pointed in particular to cases of colour variation in the Icelandic poppy, which has a yellow and an orange variety, and between species in a genus of South African butterflies, where the tips of the fore-wings are generally either orange red or purple. In both cases, colour is the main differentiation between two forms, intermediary forms are rare or non-existent and there is at least a partially known chemical basis for the colour trait. Bateson argued that it was difficult to explain the lack of intermediary forms in terms of the selection of minute variations, since it was both difficult to explain why particular colours, e.g. red and purple, were useful and why any intermediaries would not be. Given this, he “submit[s] that it is easier to suppose that the change from red to purple was from the first complete, and that the choice offered to Selection was between red and purple; and that the tints of the purple and of the red were determined by the chemical properties of the body to which the colour is due”. Whilst Bateson found such cases persuasive, he was not able to provide definitive examples outside of the domain of colour in 1894. He was therefore inclined to believe ‘Substantive Variations’ as to a certain extent less important than meristic ones on the grounds that he did not think colour variations as especially significant in the production of new species. This attitude would to a certain extent shift after 1900 thanks not only to the rediscovery of Mendel but also due to Archibald Garrod’s 1902 discovery of the role of the absence of chemical ferments in congenital metabolic diseases such as alkaptonuria, conditions which Garrod would refer to as ‘chemical sports’.⁵³³ Garrod offered Bateson an instance of chemically-induced discontinuous variation which was non-trivial (unlike colour variations), leading him to consider a greater role in evolution for substantive variation. Nonetheless, it would seem that prior to 1900, Bateson found merism more intriguing and persuasive as a

⁵³² William Bateson, *Mendel's Principles of Heredity*, 2nd Ed., Cambridge University Press, 1909, pp. 268.
⁵³³ Elof Axel Carlson, *The Gene: A Critical History*, W.B. Saunders Company, 1966, pp. 166-167.

major mechanism for evolutionary variation, not least as its effects were more holistic and therefore seemed to offer a plausible means for instant speciation.

Alongside his theorising and fact-collecting, Bateson had been preparing to experimentalise his programme, the desire to do so being announced in *Materials*, where he states that “So long as systematic experiments in breeding are wanting... in this part of biology which is perhaps of greater theoretical and even practical importance than any other, there can be no progress”.⁵³⁴ *Materials* had, as Gould observes, attempted to act as a compendium of discontinuous variation that “might suggest some hints, or at least prove useful *faute de mieux*”, but which ultimately lacked a causal account of how variation was generated.⁵³⁵ Bateson found this fact frustrating and had only held back from experimentation due to its anticipated expense and his lack of funding opportunities following his falling out with Cambridge morphology. Disappointed by the failure of *Materials* to spark better disposed scientists and naturalists into action on behalf of his research programme, Bateson chose to concentrate on producing new data closer to home.⁵³⁶ In 1895, Bateson and his collaborator Edith Saunders began planning a series of experiments to determine whether hybridisation would result in ‘blending heredity’, as was widely thought to occur, or if rather the distinct parental traits would retain discontinuity in the offspring, i.e. if they demonstrated Galton’s ‘exclusive’ heredity, which was thought necessary in order for sports to be perpetuated. These experiments were performed by Saunders on an allotment of the Cambridge Botanical Gardens and focused on crossing three species of plant which exhibited both smooth and hairy leaved varieties. She published results in 1897-9 which appeared to show support for discontinuous over blending heredity but which lacked any discernable hereditary pattern or regularity. Another female collaborator of Bateson’s, Dorothea Pertz, carried out related research on floral variation in *Veronica*, but their jointly published 1898 conclusions were ‘disappointing’ – the ratio of normal to abnormal flowers was the same in filial generations whether the parents were normal or abnormal. Bateson himself concentrated on small-scale experiments with sweet-peas and butterflies among other organisms, using various facilities

534 William Bateson, *Materials for the Study of Variation treated with Especial Regard for to Discontinuity in the Origin of Species*, Macmillan and Co., 1894, p. 76.

535 Stephen Jay Gould, *The Structure of Evolutionary Theory*, The Belknap Press of Harvard University Press, 2002, p. 409.

536 Erik L. Peterson, ‘William Bateson from "Balanoglossus" to "Materials for the Study of Heredity": The Transatlantic Roots of Discontinuity and the (Un)naturalness of Selection’, *Journal of the History of Biology*, Vol. 41, No. 2, 2008, pp. 283-284 & 288.

including the grounds of a local farm, his rooms in the morphology department and later his marital home. None of these experiments received external support until 1897, when Galton convinced the Royal Society's Evolution Committee to give low-level funding to allow Bateson and Saunders to expand their work.⁵³⁷

Given the inability of these early experimental results to confirm much more than the persistence of discontinuous variations, without a hint of an actual causal mechanism, and the sense of marginalisation Bateson no doubt felt as a consequence of his having to make do with whatever facilities and tools he had at hand, it is thus not surprising how enthusiastically he received the work of Mendel. Mendel offered Bateson clarity on methods of statistical analysis (Bateson's poor mathematical abilities being a longstanding weakness), showed him the importance of ensuring varieties bred true by always propagating F2 generations, and pushed him towards understanding character differences in terms of internal factors that were not always phenotypically expressed, whereas previously he had overtly relied on morphological and taxonomical assessments.⁵³⁸ Moreover, Mendel appeared to offer strong corroboration for the discontinuous nature of variation and its exclusive heredity, in particular through his determination of a law of segregation between the inheritances of parents in the offspring. Bateson can here be contrasted with de Vries for whom, as aforementioned, Mendelism wasn't especially exciting or revelatory, the reason being that whilst Bateson had been struggling for a means to analyse his findings on variation in order to determine its causes, de Vries had already developed a sophisticated methodology which he believed elucidated salient facts about evolution. De Vries moreover did not have the same concerns about the effects of blending on the perpetuation of his mutants given that his theory assumed the hereditary material of species to be uniform, with the consequence that outside of hybrid crosses there were no differences to be blended between parents.

The extent of Mendel's impression on Bateson can be seen in his elegiacally stating in 1908 that thanks to Mendelian analysis "For the first time *Variation* and *Reversion* have a concrete, palpable meaning. Hitherto they have stood by in all evolutionary debates, convenient genii,

537 Marsha L. Richmond, 'The 'Domestication' of Heredity: The Familial Organization of Geneticists at Cambridge University, 1895-1910', *Journal of the History of Biology*, Vol. 39, No. 3, 2006 pp. 570-5; & Staffan Müller-Wille and Marsha L. Richmond, 'Revisiting the Origin of Genetics', In: *Heredity Explored: Between Public Domain and Experimental Science, 1850-1930*, C. Brandt, H.-J. Rheinberger, and S. Müller-Wille (Eds.), Cambridge, MA: MIT Press, 2015.

538 Ibid.; & Ibid.

ready to perform as little or as much as might be desired by the conjuror. That vaporous stage of their existence is over; and we see Variation shaping itself as a definite, physiological event, the addition or omission of one or more definite elements; and Reversion as that particular addition or subtraction which brings the total of the elements back to something it had been before in the history of the race".⁵³⁹ Mendel thus helped Bateson feel he had a grasp on the underlying material causes of variation and also showed the tendency to revert to ancestral forms such as wild types to parallel the tendency to diverge, being also a product of the addition or deletion of factors. By demonstrating this symmetry between divergent variation and convergent reversion and the exclusivity of heredity, Bateson and other early geneticists were finally able to overcome Jenkin's claim that rare sports would tend to be diluted out of existence. The proof of segregation, moreover, showed wild types and other varieties to not only be stable when bred to their own kind but also as being recoverable from crosses without risk of contamination from the dissimilar parent, the risk being rather induced by failure to breed F2 generations to test if F1 offspring bred true and to allow selection of homozygous F2 offspring. Furthermore, Mendel's observation of the tendency of certain allelic varieties to dominate over others and thus produce discontinuous phenotypes was understood by Bateson to offer further proof of the chemical nature of the expression of hereditary characters. This not only suggested to him that variations were generally, like chemical elements, inherently stable, but moreover allowed him to see a bigger role for substantive variations in evolution than he had first allotted (in particular through factors and their chemical products being 'present or absent'), to the point that in 1914 he would briefly entertain a degenerationist theory that evolution had almost entirely proceeded through the suppression of the chemical activity of factors inherited from the primordial first organism.⁵⁴⁰ Reading Mendel thus not only shored up a number of Bateson's pre-existing convictions but also further persuaded him of the reality of discontinuity and the stability of discontinuous forms when crossed. Having already been convinced by his field studies of the irrelevance of differences in environmental conditions between nature and the garden or lab when it came to the investigation of heredity and variation, Bateson therefore came to similar conclusions as de Vries regarding the nature of wild types. Wild type could now cross the threshold into

539 William Bateson, *The Methods and Scope of Genetics*, Cambridge University Press, 1908, p. 48.

540 Jean Gayon, *Darwinism's Struggle for Survival: Heredity and the Hypothesis of Natural Selection*, Matthew Cobb (Trans.), Cambridge University Press, 1998, p. 272.

controlled experimental conditions and be domesticated for the purposes of scientific investigation.

Chapter 9 – Bateson and De Vries after Mendelism

Wild Type Crosses the Threshold

I have sought to show in my previous chapters how the way was opened for wild type to enter the lab through the establishment of 'indoor natures', the scientific domestication of sub-organismic elements of the body, and the sancturisation and devitalisation of heredity. I have presented Hugo de Vries and William Bateson as key figures whose theoretical and empirical work on the subjects of variation and evolution helped effect the transition of wild types into controlled experimental conditions. Of the two, de Vries in particular was important in establishing the general practice of investigating heredity and variation through the use of inbred lineages in controlled conditions and for propagating the notion that true-breeding 'elementary species', not the larger Linnaean species accepted by most taxonomists, were the true 'units of nature'. His work in this field was complemented by that of the Danish plant breeder Wilhelm Johannsen. Johannsen had shown in the mid-1890s that the widespread assumption of a general correlation between seed weight and high nitrogen content in commercial strains of barley was erroneous. He did so by breeding from a single ear of grain and genealogically dividing its progeny into pedigree 'lines', a technique first developed in the mid-19th century by the Vilmorin family of plant breeders in France. Johannsen showed that whereas at a populational level seed weight and nitrogen content appeared correlated, at the level of individual pure-bred lineages there was considerable variation between lines, including lines with both high weight and low nitrogen.⁵⁴¹ Variation within lines, was, however, strictly limited, as Johannsen would further show in his 1903 monograph based on follow-up studies on beans. Here he again broke a population down into a number of 'pure lines' and showed that whereas populations exhibited, as Galton had shown, partial regression to the mean, offspring in pure lines exhibited full regression, i.e. each generation exhibited the same range of variability whatever selective program was imposed. From this result, Johannsen concluded that the partiality of regression in populations was due to their being composed of a mixture of types, types which were revealed by dividing the population into pure lines. Full regression showed that within a pure line the same germinal material, what Johannsen would later call the genotype, was shared by all individuals and passed down

541 Staffan Müller-Wille and Marsha L. Richmond, 'Revisiting the Origin of Genetics', In: *Heredity Explored: Between Public Domain and Experimental Science, 1850-1930*, C. Brandt, H.-J. Rheinberger, and S. Müller-Wille (Eds.), Cambridge, MA: MIT Press, 2015.

intact to descendants, with all appearance of variability, e.g. in size of bean, being attributable to environmental influences.⁵⁴²

The work of de Vries and Johannsen thus showed that studying variation at the populational level did not give a full picture of its true nature, indeed could be positively misleading at times. These findings moreover legitimated studies of pedigree cultures under controlled conditions as a less superficial means of investigating variation compared to the biometric studies of natural populations which had been pioneered by researchers such as Walter Weldon. Biometric studies were admitted even by their proponents to be phenomenalist in their penetration of variation (Karl Pearson, a self-declared disciple of the phenomenalist physicist Ernst Mach, openly advocated treating laws of heredity as purely statistical correlations rather than biological hypotheses).⁵⁴³ Laboratory-type studies based on ‘pure lines’, however, promised to reduce varying populations to their constituent invariant lineages and to define these lineages in terms of their hereditary physiological constitution. What de Vries and Johannsen established through their experimental programmes was thus the groundwork for another instance of the “cultural body snatching” (Kohler) that exemplified late 19th and early 20th century laboratory biology.⁵⁴⁴

One of the means by which such a valorisation of lab over nature was made possible was through the establishment of what Star and Griesemer call ‘boundary objects’. These are objects, both concrete and abstract, which “inhabit several intersecting social worlds” and are “both plastic enough to adapt to local needs and... constraints... yet robust enough to maintain a common identity across sites”.⁵⁴⁵ By establishing boundary objects between the lab and nature, lab scientists were able to bring natural objects into the lab, act on them in order to attain knowledge, and then project that knowledge out of the lab and onto nature, based on the assumption that the common identity of their boundary objects in both lab and

542 Jean Gayon, *Darwinism's Struggle for Survival: Heredity and the Hypothesis of Natural Selection*, Matthew Cobb (Trans.), Cambridge University Press, 1998, pp. 260-267.

543 Jean Gayon, ‘The Philosophical History of the Concept of Heredity’, *The Concept of the Gene in Development and Evolution*, Peter J. Beurton, Raphael Falk & Hans-Jörg Rheinberger (eds.), Cambridge University Press, 2000, p. 75.

544 Robert E. Kohler, *Landscapes and Labscapes: Exploring the Lab-Field Border in Biology*, Chicago: The University of Chicago Press, 2002, pp. 3-8.

545 Susan Leigh Star and James R. Griesemer, ‘Institutional Ecology, ‘Translations’ and Boundary Objects: Amateurs and Professionals in Berkeley’s Museum of Vertebrate Zoology, 1907-39’, *Social Studies of Science*, Vol. 19, No. 3, 1989, p. 393.

nature provided an adequate foundation for such a projection. It is my view that the abstract wild type concept and its concrete manifestations, the lab lineages known as ‘wild types’, was one such instance of a boundary object, and one which played a key role in grounding the epistemic claims of classical genetics regarding the nature of evolution, heredity and variation in the wild. In the case of the wild type, the claim of identity was based on the assumed isomorphism between the genetic constitution of natural ‘wild types’ and of lab wild types. I will refer to such claims of identity as claims of *substitutability*. By being an adequate substitute for a truly wild and typical instance of the species, a lab wild type could play an important dual role as both a ‘natural entity’ and a laboratory tool. Both de Vries and Johanssen made important theoretical and empirical contributions that allowed for this claim of identity, namely by showing that natural populations could be broken into true-breeding types that displayed negligible hereditary variability. They also both assumed Weismann’s sequestration of the germ-plasm, which provided assurance that a species could move between nature and domestication without being altered in its fundamental genetic constitution. These developments allowed for the idea that if one of the types in a true-breeding natural population could be identified as the wild type, then this wild type, as defined in terms of its genetic constitution, could paradoxically cross the threshold from wild nature into the severely domesticating regime of the laboratory. Kohler has discussed the crossing of the lab-field threshold by organisms such as *Drosophila* in terms of a movement from one ecosystem to another.⁵⁴⁶ I propose that here we can think of concepts as also crossing this threshold, moving from the conceptual sphere of nature to that of the laboratory. These different conceptual spheres have characteristically been dominated by diverging scientific methods and discourses, nature being associated with observation, authenticity and independence from human domination, the lab with intervention, artifice and subjugation to human power. These distinctions are far from absolute and the boundaries between lab and nature are in many places highly permeable and interacting. Nonetheless, the development of this dichotomy/dialectic has played an important role in the formation of the modern life sciences, as shown by Kohler and others.⁵⁴⁷ The importance of objects and concepts which have helped bridge this divide cannot therefore be easily dismissed. And, as the case of the wild type I believe demonstrates, when objects, e.g. organisms, cross the threshold from one

546 Robert E. Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Life*, The University of Chicago Press, 1994, p. 19.

547 Robert E. Kohler, *Landscapes and Labscapes: Exploring the Lab-Field Border in Biology*, Chicago: The University of Chicago Press, 2002, pp. 3-8.

domain to another, concepts very often follow closely by, especially when they are integral to the interpretation of a scientific object as an epistemic thing. Concepts may therefore also be endangered when the material coherence or primacy of a scientific object is threatened by the forces of experimental analysis and synthesis. In the case of the wild type, this would play out in a debate between holistic and reductionistic tendencies which alternatively sought to preserve the wild type as a unit of nature and scientific analysis, and to disintegrate it into its component traits and genes. I will begin to explore this debate by examining its origins in the differing reception of Mendel by de Vries and Bateson, and their related differing interpretations of wild type.

De Vries on Mendelism, Dominance & Wild Type

As established, de Vries' mutationist experimental programme and theoretical framework were important factors in the development of wild type as a boundary object between nature and the lab, leading towards its eventually becoming a key concept in classical genetics. But it should be emphasised that de Vries himself held to quite a distinct notion of 'wild type' compared to what became established in genetics and that, moreover, wild types did not play a particularly important role in his experimental epistemology or in attaining the empirical aims of his project. This lack of interest in wild types was in a large part based on de Vries' particular views on variation in domestication and nature. De Vries held that because so little was known about the history of domesticated plants it was "impossible to decide whether all... [domestic varieties] are older than culture or have come into existence during historic times, or as some assume, through the agency of man".⁵⁴⁸ Nonetheless, de Vries was prepared to assume "that cultivated species, as a rule, are derived from wild species... The botanic units are compound entities, and the real systematic units in elementary species play the same part as in ordinary wild species".⁵⁴⁹ De Vries believed, in other words, that there was no fundamental difference at a genetic level between variation in domestication and in nature. The appearance of greater variation in domestication was due to a combination of factors. Firstly, species in nature were typically not found in their pure form – "The mixed condition is the rule, purity is the exception". De Vries here saw a parallel between elementary species and chemical elements, which in nature are usually found as compound ores. In both cases,

⁵⁴⁸ Hugo de Vries, *Species and Varieties: Their Origin by Mutation*, Daniel Tremblay MacDougal (ed.), The Open Court Publishing Company, 1905, p. 81.

⁵⁴⁹ *Ibid.*, pp. 90-91.

the purity of elements and races is “a condition implanted in them by man”.⁵⁵⁰ Therefore, when pedigree breeding revealed a host of distinct lineages as existing within a racial population, it was not creating new variation but rather purifying populational variation into its distinct elements. Further ‘variation’ was then created by crossing elementary species which do not usually associate in nature in order to produce new trait complexes. Again, for de Vries this was not ‘real’ variation, which only occurs through the latency and activation of pangens, which produce varieties and reversion to type, and the rare acquisition of novel pangens in progressive mutations. Overall, in de Vries’ assessment much of the additional ‘variation’ claimed to occur in domestication was simply the result of the unstable and evolutionarily insignificant processes of purification and intercrossing of elementary species. The wild type/domestic variety distinction had little use for de Vries as in his view the differences between them were purely products of the recombination of extant genetic material, whereas progressive evolution only occurred through fundamental changes in the germ-plasm. The commonly identified ‘wild types’, moreover, being, he thought, typically admixtures, were usually not real units of nature and were not amenable to experimental investigations that depend on their purification into component elementary species. De Vries did not abandon the idea of wild type altogether but instead employed a minimalist interpretation based on the viability of an elementary species in the struggle for existence – those types which could survive and propagate without human intervention could be thought of as ‘wild types’. This is an unusual definition of ‘wild type’ which is neither normative nor genealogical/historical. It may be perhaps described as the *viability-based* concept of wild type.

Regarding de Vries’ place in Mendelian genetics, once again his views on variation served to distance him to a certain extent from this emerging field. Though one of the claimed Mendel ‘rediscoverers’ of 1900, de Vries quickly came to the conclusion that Mendelian inheritance was irrelevant to long-term evolutionary trends as it only dealt with the recombination of extant hereditary units, not the production of novel pangens. He also observed that Mendel’s law of segregation only applied to what he called ‘bi-sexual’ hybrids and not to ‘uni-sexual’ hybrids. ‘Bi-sexual’ hybrids were the offspring of two parents which possessed the same hereditary make-up but which differed in the *activity* of at least one elementary character, as

550 Ibid., pp. 101-102.

in crosses between parent types and retrogressive varieties. ‘Uni-sexual’ crosses, on the other hand, were between parent types which differed in the *presence* of at least one elementary character, as in crosses between closely related species. For de Vries, uni-sexual crosses were far more interesting than Mendelian hybrids as not only were they a means of determining elementary species, and thus of establishing whether a mutation was progressive or not, but also because he believed that they “lead to the direct production of constant hybrid races”, i.e. potentially represent a means of speciation.⁵⁵¹ That Mendelian hybrids were disposed to segregate, i.e. were inconstant, showed for de Vries that they could not result in the production of new species. Moreover, not only did de Vries think Mendelian inheritance irrelevant to speciation processes but he also did not believe it a useful means of studying ordinary variation. This was because he viewed hybridisation, whether bi-sexual or uni-sexual, as very much the exception to the rule in nature, ‘natural heredity’ being the pairing of like germ-lines within the same elementary species, as opposed to crosses between varieties and parent types or between different elementary species. On these grounds, he dismissed Mendelism as a “laboratory phenomenon”.⁵⁵² As early as October 1901 he implored Bateson by letter to “please don’t stop at Mendel... Mendelism is an exception to the general rule of crossing. It is in no way the rule! It seems to hold good only in derivative cases, such as real variety-characters”.⁵⁵³

Nonetheless, despite his professed misgivings and tendency to deny credit to Mendel for influencing the development of his ideas, de Vries’ 1900 reading of the monk’s 1866 paper did lead to changes in his theories of heredity and variation. One major alteration was de Vries’ adoption of Mendel’s diploid model of bi-parental factor contributions and his Law of Independent Assortment. Whereas in 1889 de Vries had assumed that “All the hereditary characters of the father [and mother] must therefore be transmitted in the nucleus, as potentialities in a latent state” (a view similar to Galton’s idea of ancestral heredity), in his 1900 essay ‘The Law of Segregation of Hybrids’ he asserts that “*The pollen grains and ovules of monohybrids* [crosses between varieties differing in one character] *are not hybrids*

551 Hugo de Vries, *The Mutation Theory: Experiments and Observations on the Origin of Species in the Vegetable Kingdom, Vol. II: The Origin of Species by Mutation*, J.B. Farmer and A.D. Darbishire (Trans.), Chicago: The Open Court Publishing Company, 1910, pp. 576-578 & 584-586.

552 Bert Theunissen, ‘Closing the door on Hugo de Vries’ Mendelism’, *Annals of Science*, Vol. 51, 1994, pp. 247-248.

553 William B. Provine, *Origins of Theoretical Population Genetics*, The University of Chicago Press, 2001, p. 68.

[as would be the case if all parental hereditary characters were transmitted in each sex cell] but belong exclusively to one or the other of the two parental types".⁵⁵⁴ Related to this, he moved from assuming an almost indefinite number of pangens in each nucleus to the view that there were only two for each unit character, one from each parent.⁵⁵⁵ He thus removed himself from his earlier idea that the multiplication of pangens could be responsible for continuous variations towards the idea that all such fluctuations were in fact purely somatic in nature. This further aided the mobility of types between nature and the lab as it further diminished the believed possible influence of conditions of existence on the germ-line.

But the most notable influence Mendel had on de Vries was in shaping his concept of dominance. Mendel had theorised that the uniformity of the first filial generation and the 3:1 character ratio found in the second filial generation of his pea hybrids could be explained in terms of both male and female plant in the parental generation equally contributing to their offspring's inheritance but with one of the factors responsible for trait production being 'dominating', the other 'recessive'. In the first filial generation these distinct factors would be paired in each individual and only the dominating trait would be produced. But in the second filial generation, these factors would be independently assorted and would recombine in such a manner that half of offspring would be paired with like factors, half with differing. In those paired with differing factors, the dominating trait would be expressed, as it would in individuals with a pair of dominating factors, but the recessive trait would be expressed in that quarter of the generation which received a pair of recessive factors. Now for Mendel, it has been argued by Falk, the tendency for one factor to dominate over another was not inherent and absolute but rather a product of interaction between factors.⁵⁵⁶ There was, in other words, no stated 'Law of Dominance' in Mendel's original paper. De Vries, on the other hand, sought to incorporate Mendel's observations of dominating traits into his pre-existing theory of pangen activity. For de Vries, whether a pangen was active or latent was not a product of its interactions with other pangens but rather an intrinsic property that existed independent of such interactions. As a consequence, he also interpreted Mendelian

554 Hugo de Vries, *Intracellular Pangenesis*, C. Stuart Gager (Trans.), Chicago: The Open Court Publishing Company, 1910, p. 180; & Hugo de Vries, 'The Law of Segregation of Hybrids', [1900], In: Curt Stern and Eva R. Sherwood (ed.), *The Origin of Genetics: A Mendel Sourcebook*, San Francisco and London: W.H. Freeman and Company, 1966, p. 112.

555 Raphael Falk, *Genetic Analysis: A History of Genetic Thinking*, Cambridge University Press, 2009, pp. 40-45.

556 *Ibidl*, pp. 52-3.

phenomena in terms not of factor interactions but of differences in factor properties. As to the pattern of these property differences, de Vries noted that “Ordinarily the character higher in the systematic order is the dominating one, or, in cases of known ancestry, it is the older one”. He does note apparent contradictions to this general rule – e.g. the dominance of domestic maize, *Zea mays*, which has a naked seed, over its believed wild type *Zea cryptosperma*, which has a covered seed – but is confident enough in its generality to believe that “In species hybrids... where the relative ages of the parental forms are usually unknown, possibly conclusions may be drawn from crossing experiments [regarding phylogenetic seniority]”.⁵⁵⁷ De Vries’ confidence in the generality of this law of ancestry was likely rooted in its being a long-standing conviction among many breeders that the older a variety the more prepotent its hereditary power. This hypothesis of ancestry was another of the means used to explain the tendency of reversion to wild type, e.g. domestic varieties derive from the wild type, therefore the older wild type is more prepotent in crosses, resulting in reversion. This hypothesis was known to Darwin as ‘Yarrell’s Law’, after the breeder he attributed it to (though belief in it was considerably older).⁵⁵⁸ De Vries’ theory of ancestral dominance thus pushed geneticists to seek to answer an old question – why do wild types tend to dominate over derivative varieties – within the novel theoretical framework of hereditary factors as discrete biochemical units the behaviour of which was understood in terms of intrinsic microstructurally defined properties. This marked the beginnings of a shift in how the relationship between wild type and non-wild type factors was understood and investigated. Whereas before wild/non-wild type trait relations were principally studied in terms of biological function and functional products such as corporeal traits, there was now a move towards studying such relations at the level of fundamental structures such as chromosomes and genes transmitted across generations. De Vries’ theory of dominance thus significantly contributed to the gradual disintegration of the wild type phenotype into the constituent wild type genes responsible for its production.

557 Hugo de Vries, ‘The Law of Segregation of Hybrids’, [1900], In: Curt Stern and Eva R. Sherwood (ed.), *The Origin of Genetics: A Mendel Sourcebook*, San Francisco and London: W.H. Freeman and Company, 1966, p. 111.

558 M.J.S. Hodge and David Kohn, ‘The Immediate Origins of Natural Selection’, In: *The Darwinian Heritage*, David Kohn (Ed.), Princeton University Press, 1985, p. 189.

Bateson on Mendelism, Dominance & Wild Type

That wild type would come to assume a much more prominent role in William Bateson's theoretical framework than it did in de Vries' may seem surprising when one considers the similarities between their views on variation in domestication and the wild. Bateson had argued in 1894 that the frequency of variation "has no necessary relation to the conditions of civilization or domestication". He asserted that "the special fallacy of the belief that great Variation is much rarer in wild than in domesticated animals" had arisen "as the outcome of certain theoretical views [i.e. the adaptationist belief, found in both Darwin and Lamarck, that diversity of environment is correlated with diversity of forms] and has received support from the circumstance that so many of our domesticated animals are variable forms, and that so little heed has been paid to Variation in wild forms". Instead of comparing domestic varieties with their claimed wild types, Bateson proposed instead that "To compare rightly their variability with that of wild animals choice should be made of animals that are also variable though wild", his favoured examples being the vertebrae of sloths, the teeth of great apes and the colour of the dog whelk *Purpura lapillus*. In such cases, "we find a frequency and a range of Variation matched only by the most variable of domesticated animals". This set of arguments shows that at the time of *Materials*, Bateson believed variation to be a uniform and arbitrary process which did not differ across the domestic-wild divide. Whilst it was true that many domestic species were variable and their believed wild types were known or thought to be less so, this did not imply that domestic organisms were more variable than all wild species. As a particular example, Bateson observed that whilst it was true that supernumerary teeth were more common in both domestic dogs and cats than in wild *Canidae* and *Felidae*, dentition was even more variable in wild apes and seals than in these domestic species.⁵⁵⁹

Bateson, unlike de Vries, saw great potential in Mendel's experimental method as a means to determine the nature of variation both generally and evolutionarily. In particular, he saw Mendel's principle of segregation, demonstrated by the recovery of both parent types in the second filial generation, as proof of the discontinuous nature of variation against the claim that variation was blending and therefore continuous.⁵⁶⁰ In contrast, Bateson was sceptical of de Vries' mutation theory – especially the claim of a fundamental difference between

559 William Bateson, *Materials for the Study of Variation treated with Especial Regard for to Discontinuity in the Origin of Species*, Macmillan and Co., 1894, pp. 4-5, 116, 266 & 572.

560 Linley Darden, 'William Bateson and the Promise of Mendelism', *Journal of the History of Biology*, Vol. 10, No. 1, 1977, pp. 93-94.

retrograde and progressive mutations – and of its basis in experiments on *Oenothera*. Bateson noted that crosses between de Vries' evening primrose mutants produced ratios which could not be fitted to any standard Mendelian account, and as a consequence the gametic relations between parent type and mutants and between sister-mutants could not in Bateson's view be determined. "All that can be positively asserted", Bateson maintained, "is that these mutations are forms arising discontinuously"; nothing could be established regarding the actual internal causes, in particular whether the mutations were due to the production of novel pangens.⁵⁶¹ Grounds for doubt were furthered by the fact that de Vries could not provide evidence for his progressive mutations outside of *Oenothera*, whereas, by contrast, Bateson was able to rapidly establish Mendelian inheritance as occurring in multiple species of plant and animal. This led Bateson to conclude that "Outside the [ambiguous] evidence from *Oenothera*... I know no considerable body of facts favourable to that special view of Mutation which de Vries has promulgated".⁵⁶² In rejecting de Vries' theory of speciation as occurring through progressive mutations, Bateson was inclined to take a very different view of Mendel's varieties. Whereas for de Vries these were merely retrograde varieties, relatively uninteresting evolutionarily, Bateson was instead inclined to view these as "nascent species", and their behaviour when crossed as offering insight into processes of evolution as opposed to mere short-term variation.⁵⁶³

Bateson's embrace of Mendelism and rejection of de Vries' experimental findings should not imply that there were not still significant convergences between their two views of variation after 1900. Both agreed that much ordinary variation merely consisted in the recombination of extant variability without the generation of true novelty, with much of the supposed additional variability of domestic forms being attributed to the crossing of distinct elementary species usually geographically separated in nature. Bateson also endorsed de Vries' account of elementary species as the true units of nature, as illustrated by his 1914 statement that "The only definable unit in classification is the homozygous form which breeds true", further maintaining that whilst "the great systematists... have pooled them into arbitrary Linnean species, for the convenience of collectors", "[t]hese "little species", finely cut, true-breeding [and fixed]... are what [the student of the physiology of variation] finds when he examines

561 Alan G. Cock and Donald R. Forsdyke, *Treasure Your Exceptions*, Springer Science, 2008, p. 260.

562 William Bateson, *Problems of Genetics*, Yale University Press, 1913, p. 115.

563 Linley Darden, 'William Bateson and the Promise of Mendelism', *Journal of the History of Biology*, Vol. 10, No. 1, 1977, p. 91.

any so-called variable type".⁵⁶⁴ But where Bateson's ontology of variation differed significantly from de Vries' was in his reluctance to accept the idea that novel factors arising spontaneously might play a central role in evolution, as was assumed in de Vries' theory of progressive mutations. In the course of his experimental investigations, Bateson did come across numerous dominant mutations, such as polydactyly in humans and dominant whiteness in chickens. This led him among other things to reject de Vries' ascription of dominance as related to factor ancestry, as in cases like dominant whiteness in chickens there was no evidence of it being found in the wild type *Gallus bankiva*, leading to the conclusion that it must have arisen after domestication.⁵⁶⁵ But Bateson did not think these cases to be examples of de Vries' progressive mutations. Firstly, unlike the *Oenothera* mutants, which differed from the parent type in multiple aspects, these dominant mutations instead tended to be single trait differences much like those produced by recessive mutations. But another important difference was that whereas de Vries had treated his mutants as representing an independent norm of health from the parent type, judged in their fitness only by the struggle for existence, Bateson on the other hand was much more inclined to treat dominant mutants as diseased forms of the normal or wild type instance of the species. His inclination to do so is already seen in 1894 in his approvingly quoting Virchow's view that "every deviation from the type of the parent animal must have its foundation on a pathological accident".⁵⁶⁶ Thus we may contrast de Vries' viability-based notion of wild type with Bateson's idea of the wild type as not merely viable but moreover as *normal*. This was a normativity based on health and the idea of the wild type as the endpoint of 'normal development' in a species, not, as in earlier natural state-style models of wild type, on ideas of specific essence. There is still an element of teleology, but this is tied to the directed nature of individual ontogeny (which following Haeckel was seen as tied to evolutionary phylogenetic history), not, as in for instance Jenkin's 'sphere of variation' model, to a fixed place in nature.

This associating of variation with pathology and the wild type with health was strengthened by Bateson's corresponding in 1902 with the physician Archibald Garrod. Garrod had studied the congenital human illness alkaptonuria and had noted the predominance of first cousin

564 William Bateson, 'Address of the President of the British Association for the Advancement of Science', *Science*, Vol. 60, No. 1026, 1914, p. 297.

565 William Bateson, *Problems of Genetics*, Yale University Press, 1913, p. 90.

566 William Bateson, *Materials for the Study of Variation treated with Especial Regard for to Discontinuity in the Origin of Species*, Macmillan and Co., 1894, p. 74.

marriages in the parents of sufferers. Bateson suggested to Garrod that this indicated a recessive hereditary condition. Garrod then went on to show that the cause of the disease was the absence of an enzyme that decomposes a specific amino acid, leading him to develop a theory of ‘inborn errors of metabolism’ which classed alkaptonuria as one of a number of hereditary maladies caused by the non-production of a particular enzyme in an important metabolic pathway.⁵⁶⁷ The influence of Garrod’s case of alkaptonuria can be seen in Bateson’s following formulation on hereditary diseases: “If... a disease descends through the affected persons, as a dominant, we may feel every confidence that the condition is caused by the operation of a factor or element added to the usual ingredients of the body. In such cases there is something *present*, probably a definite chemical substance, which has the power of producing the affection... On the contrary, when the disease is recessive we recognize that its appearance is due to the *absence* of some ingredient which is present in the normal body”.⁵⁶⁸ Now what applied to the ‘normal body’ in cases of human diseases also for Bateson applied to the wild type in cases of variation. As to what was present in cases of dominant mutations, Bateson found it difficult to imagine exactly what, commenting that whilst “there is... no special difficulty” in understanding the origin of recessive mutations, which, as we shall see, he attributed to absences, “as soon as it is understood that dominants are caused by an addition we are completely at a loss to account for their origin, for we cannot surmise any source from which they may have been derived”. Tellingly, Bateson follows this statement by making an analogy between the generation of a dominant mutant and a pathogenic outbreak, stating that “Just as when typhoid fever breaks out in his district the medical officer of health knows for certain that the bacillus of typhoid fever has by some means been brought into that district so do we know that when first dominant white fowls arose in the evolution of the domestic breeds, by some means the factor for dominant whiteness got into a bird, or into at least one of its germ-cells”.⁵⁶⁹ This implies that he was more inclined to think of a dominant mutant factor as a heritable invading foreign body than supposing it might be an internally generated novel hereditary unit.

The apogee of Bateson’s scepticism regarding evolution through the generation of novel factors occurred in 1914 at his presidential address to the British Association for the

567 Peter S. Harper, ‘William Bateson, Human Genetics and Medicine’, *Human Genetics*, 2005, Vol. 118, No. 1, pp 143-144.

568 William Bateson, *Mendel’s Principles of Heredity*, 2nd Ed., Cambridge University Press, 1909, p. 232.

569 William Bateson, *Problems of Genetics*, Yale University Press, 1913, pp. 93-94.

Advancement of Science. Here he presented a hypothesis of evolution through degeneration asserting that “we must begin seriously to consider whether the course of evolution can at all reasonably be represented as an unpacking of an original complex which contained within itself the whole range of diversity which living things present”. This position bears some resemblance to Weismann’s earlier idea that all multicellular variation consisted in the rearrangement of hereditary determinants derived from their prokaryote ancestors. Like Weismann, Bateson suggested that the periodic rearrangement of a small number of primordial factors could account for the full course of evolution without need of additional factors, stating that “That which is conferred in variation must rather itself be a change, not of material, but of arrangement, or of motion”. But unlike Weismann, Bateson also proposed that the appearance of some novel traits could be accounted for as due to the fractionation of factors (a suggested possible case was pied coat colouration in animals) and others as due to the loss of inhibiting ‘epistatic’ factors which allowed for the manifestation of suppressed traits. Regarding the latter, Bateson suggested that the apparent greater variation found in domestic varieties, in his example the apple, could be due to the progressive loss of epistatic factors originally present in the wild type crab apple.⁵⁷⁰ The wild type, in other words, possessed potentially all the variation found in domestic varieties but needed to be ‘unpacked’ in order for this to be expressed.

Bateson’s adoption of this degenerationist model of evolution has often been ridiculed by historians of science. Jean Gayon describes the hypothesis as “disconcerting”, and “marked by an explicitly preformist and degenerative bias”.⁵⁷¹ Peter Bowler is more charitable, offering Bateson a get out clause by suggesting that he only “*almost* [emphasis mine] seemed to suggest this as a general theory of evolution”, implying that he could not have been serious.⁵⁷² Cock and Forsdyke are perhaps an exception in suggesting that Bateson, in hypothesising the development of new forms through the “quantitative disintegration” of factors, anticipated “what we now generally ascribe to an amino acid-changing base substitution in a DNA sequence”.⁵⁷³ But not only is this assessment Whiggish, it also commits a fundamental error in its failure to appreciate that Bateson’s theory implied an entropic and

570 William Bateson, ‘Address of the President of the British Association for the Advancement of Science’, *Science*, Vol. 60, No. 1026, 1914, pp. 298-300.

571 Jean Gayon, *Darwinism’s Struggle for Survival: Heredity and the Hypothesis of Natural Selection*, Matthew Cobb (Trans.), Cambridge University Press, 1998, p. 272.

572 Peter J. Bowler, *Evolution: The History of an Idea*, University of California Press, 1989, pp. 275-276.

573 Alan G. Cock and Donald R. Forsdyke, *Treasure Your Exceptions*, Springer Science, 2008, p. 411.

one-way trajectory of evolutionary change, factors being lost and disintegrating but never being gained and reintegrating, with the end result that evolution would eventually run out of material to work with. No such degenerative trajectory is evident in contemporary DNA-based accounts of mutation. But however Bateson's hypothesis is to be judged in a modern light, what is of interest for the purposes of my narrative is how we can fit Bateson's degenerative account of evolution to his treatment of wild types. In particular it should be noted that this 1914 speech was not the first time Bateson had referred to processes of variation in terms of 'unpacking'. 5 years previously in the second edition of his *Mendel's Principles*, Bateson had asserted that "it has become clear that variation, in so far as it consists in the omission of elementary factors, is the consequence of a process of "unpacking"", and the example used to illustrate this was that of "When from a single wild type, man succeeds in producing a multitude of new varieties". This firstly shows that Bateson's 1914 presidential address was not a one-off, as evidenced in particular by his assertion here that "Such variation is not... a progress from a lower degree of complexity to a higher, but the converse... and that the obvious appearance of increased complexity may in reality be the outcome of a process of simplification".⁵⁷⁴ But by looking at the context within which these 1909 comments occur, we can also connect the development of Bateson's degenerationist theory of evolution to an earlier commitment made in order to make sense of a particular aspect of variation and heredity, namely the phenomenon of dominance, which Bateson explained in a very different manner compared to de Vries.

Bateson's Interpretation of Wild Type in the Context of the Presence and Absence Hypothesis

De Vries, as previously discussed, had interpreted the phenomenon of dominance, which for Mendel had been a product of interaction between factors, as being an intrinsic property of pangens, which he observed to be correlated with greater pangen phylogenetic age. De Vries' interpretation can be criticised on a number of accounts, most notably for being something of a *virtus dormativa* attribution ('dominating pangens dominate because they possess a property of dominance'). Nonetheless the idea of dominance as a property of an allele would prove an enduring notion in classical genetics.⁵⁷⁵ This was not without significant resistance from Bateson, who from an early stage was inclined to reject de Vries' account of

⁵⁷⁴ William Bateson, *Mendel's Principles of Heredity*, 2nd Ed., Cambridge University Press, 1909, p. 280.

⁵⁷⁵ Raphael Falk, 'The rise and fall of dominance', *Biology and Philosophy*, Vol. 16, 2001, pp. 288-291.

dominance, becoming from 1906 onwards the most prominent proponent of a rival theory, the ‘presence and absence’ hypothesis. The roots of Bateson’s rejection of dominance as a property go back to his 1902 conflict with the biometrician Weldon. Weldon had attacked an embryonic Mendelism, focusing in particular on the phenomenon of dominance, which he had argued could not be regarded a general law given that even such emblematic cases as green and yellow colour and round and wrinkled shape in peas were irregular and showed many grades in variation. Bateson’s response to this was to point out that in fact Mendel had never made any reference to a ‘Law of Dominance’ in his original paper, only speaking of ‘dominating’ and ‘recessive’ characters. What was clear from Mendel’s findings, Bateson asserted, was that the dominance of, e.g., wrinkled over smooth shape “is a *general* truth for *Pisum sativum*... that it is a universal truth I cannot believe any competent naturalist would imagine, still less assert. Mendel certainly never did”.⁵⁷⁶ Dominance, Bateson implied, was a secondary phenomenon that was not central to Mendel’s theory, being an auxiliary hypothesis to help explain how uniformity in the F1 generation and reversion in the F2 generation could be squared with the independent assortment and segregation of factors. Mendel had never suggested that dominance was universal and the examples Weldon pointed to of irregular dominance merely showed that it was a relative phenomenon, not challenging in the slightest the core Mendelian laws. Bateson, it should be noted, was far from alone in rejecting the interpretation of dominance as a law – Carl Correns, another ‘rediscoverer’ of Mendel, had castigated de Vries in 1900 for assuming dominance to apply to all trait pairs based only on its being shown to apply to many trait pairs. Correns, like Bateson, insisted that dominance was a relative outcome of factor interactions, a vegetative product of development, not, as de Vries would have it, an intrinsic property of factors.⁵⁷⁷

Nonetheless, even if dominance was not in Bateson’s view central to Mendelism, it still required explanation. Importantly, most wild type traits were dominant, implying the phenomenon to have an evolutionary importance. Failing to explain dominance was therefore not optional if Bateson wanted to offer a complete account of evolution. An alternative explanation of dominance had emerged in competition with de Vries’ property-based model in 1903. In this year, both Correns and the mouse geneticist Lucien Cuénot suggested that in cases of complete dominance the dominating trait could be treated as ‘present’ and the

⁵⁷⁶ William Bateson, *Mendel’s Principles of Heredity*, 1st Ed., Cambridge University Press, 1902, pp. 117-119.
⁵⁷⁷ Raphael Falk, ‘The rise and fall of dominance’, *Biology and Philosophy*, Vol. 16, 2001, pp. 291.

recessive trait as 'absent'. This originally was not so much a theory of factors as a theory of factor products. In Cuénot's case, he had been studying the case of crosses between albino coat colour and the dominating black and grey coat colours. He argued that in such cases the total dominance of black and grey in the F1 generation could be treated in terms of the dominance of the character 'pigment' over the character 'absence of pigment'. Cuénot here does not appear to have committed himself to arguing that absence of pigment implied absence of character. It was only later, at the 1906 International Congress of Genetics, that C.C. Hurst would make the argument that the true nature of the 'absence' factor might well, after all, be the very absence of a factor. Hurst furthermore proclaimed this idea of actual factor absence as being more "appeal[ing] to the practical mind" and as less open to objection than the alternative view of 'absence' as due either to a latent factor or a factor of active absence. Bateson adopted the 'presence and absence' model in the same year as Hurst. This is not to say that he did not recognise it to have its problems. Not least of these was that he had already identified dominant characters "which appear to us to be negative", e.g. dominant whiteness in chickens. This led him to propose already the possibility that "such negative characters [are] due to the presence of some inhibiting influence", a harbinger of his later idea of evolution through loss of epistatic factors. But he did not pursue this idea further at this stage.⁵⁷⁸

The extent to which 1906's Bateson truly endorsed Hurst's idea of 'absent' characters as being caused by actual factor absence is unclear. His own response to Hurst's paper was lukewarm, observing that whilst "The facts we have been discussing are very interesting and very important to us", indicating his commitment to some version of the 'presence and absence' hypothesis, he insisted that "until the problem [of 'negative' dominants] is settled we shall be in constant difficulties".⁵⁷⁹ It has often been emphasised by those wanting to argue that Bateson did believe in actual factor absence that he did not properly distinguish between factor and trait, holding both to be aspects of a unitary unit-character, the implication being that Bateson could not understand trait absence except in terms of factor absence. It is certainly true that Bateson did not see transmission and development as clearly demarcated separate processes, as was not unusual among biologists of his time.⁵⁸⁰ But it should be

578 Elof Axel Carlson, *The Gene: A Critical History*, W.B. Saunders Company, 1966, pp. 58-59.

579 C.C. Hurst, 'Mendelian Characters in Plants and Animals', In: *Report of the Third International Conference 1906 on Genetics*, Rev. W. Wilks (Ed.), London: Spottiswoode & Co. Ltd., 1907, p. 129.

580 Elof Axel Carlson, *The Gene: A Critical History*, W.B. Saunders Company, 1966, p. 26.

stressed that Bateson had a heterodox understanding of the causal basis of heredity and variation which gives grounds for belief that this common interpretation of his views on ‘presence and absence’ is mistaken. As noted previously, Bateson rejected particularist accounts of heredity, finding it difficult to reconcile the ‘static’ nature of concrete matter with his conviction that factor action was dynamic and vibratory in nature. Whilst he never developed a fully-fledged theory, he appears to have favoured a colloidal model of the material nature of factors.⁵⁸¹ In this model, a factor could contribute to development in one of two ways, either by affecting organisation or by effecting changes in substance. Factors affected organisation by producing vibrations in the cellular protoplasm, resulting in differential cell divisions and the repetition of parts, what Bateson in 1894’s *Materials* had identified as ‘meristic variations’. Changes in substance were effected by factors through the production of enzymes (then commonly referred to as ‘ferments’), resulting in what Bateson in 1894 had identified as ‘substantive variations’.⁵⁸² That Bateson clearly differentiated between the factor and its capacity to produce effects is seen in his 1909 statement that whilst it may be concluded from Mendelian observations that “in at least a large group of cases the heredity of characters consists in the transmission of the power to produce something with properties resembling those of ferments”, he remarks that it is “scarcely necessary to emphasise the fact that the ferment itself must not be declared to be the factor or thing transmitted, but rather, the power to produce that ferment, or ferment-like body”.⁵⁸³ This may have been a response to the speculations of his Cambridge collaborator Muriel Wheldale, who had suggested that enzymes might represent the actual biochemical units of heredity.⁵⁸⁴

Given this evidenced capacity on Bateson’s behalf to distinguish factor and factor product, the notion that he could not conceive of trait absence without imputing factor absence lacks foundation. This gives increased support for Falk’s alternative interpretation of Bateson’s understanding of ‘presence and absence’ as referring to “functions, not to structures”, i.e. that what was present or absent was a vibration or enzyme, not a factor. Falk would furthermore have us understand that what Bateson was arguing for, contra to the view of dominance and recessiveness as intrinsic properties of paired factors, was the view that instead dominating-

581 Alan R. Rushton, ‘William Bateson and the chromosome theory of heredity: a reappraisal’, *The British Journal for the History of Science*, Vol. 47 Part 1, No. 172, 2014, pp. 168-169.

582 William Bateson, *Problems of Genetics*, Yale University Press, 1913, pp. 86-87.

583 William Bateson, *Mendel’s Principles of Heredity*, 2nd Ed., Cambridge University Press, 1909, p. 268.

584 Robert C. Olby, *The Path to the Double Helix: The Discovery of DNA*, Dover Publications Inc., 1994, pp. 133-135.

recessive relationships should be conceived of as “*quantitative* alternatives of the same unit-character”.⁵⁸⁵ By adopting this position, Bateson would be able to kill several birds with one stone. As aforementioned, it dealt with Weldon’s first major objection to Mendelism, namely that dominance was usually incomplete, by maintaining dominance to be relative not absolute. This also allowed Bateson to tackle Weldon’s argument that Mendelian techniques could not be used to analyse continuous traits by arguing that these could be accounted for in terms of single factors producing differing levels of vibration or enzymes in different individuals. This was therefore in some ways a forebear of the ‘dosage’ accounts of dominance later developed by Haldane and Muller. This was also a parsimonious account, “a simpler system” as Sara Schwartz notes Bateson as expounding, both doing away with the need for purporting extraneous additional factors in cases of continuous variations and unifying the previously divided dominant and recessive alleles into functional aspects of a single factor.⁵⁸⁶

Importantly, adopting a dosage-based account offered Bateson a means of explaining why most wild types were dominant over mutants. The wild type, as previously established, was equated with the idea of the ‘normal body’ in medicine. In the normal body, the factors contributing to the development of a healthy organism all produced an appropriate dose of enzymes or vibrations. Recessive mutations occurred in cases where a factor produced a substandard dose; in cases where the dose was negligible, it could be considered an ‘absence’. It should be noted that the notion of the ‘normal body’ was flexible enough that it was able to incorporate the discovery of “paradoxical” cases where the wild type was a heterozygote, examples of which were found by Bateson and co. in the sweet pea and the currant moth *Abraxas grossidariata*.⁵⁸⁷ In such cases, the ‘presence and absence’ system could simply regard it as normal in a healthy individual of such a species that one of the two copies of the relevant factor would produce a low or negligible dosage of factor product. Dominant mutations, on the other hand, were interpreted in this schema as due to a factor product that was usually not present in the ‘normal body’ interfering with the operation of another factor. For instance, Bateson suggested that dominant whiteness could be caused by

585 Raphael Falk, ‘The rise and fall of dominance’, *Biology and Philosophy*, Vol. 16, 2001, pp. 286 & 293-294.

586 Sara Schwartz, ‘Characters as units and the case of the presence and absence hypothesis’, *Biology and Philosophy*, Vol. 17, 2002, p. 380.

587 William Bateson, ‘Facts Limiting the Theory of Heredity’, *Science*, New Series, Vol. 26, No. 672, 1907, p. 655; & William Bateson, *The Methods and Scope of Genetics*, Cambridge University Press, 1908, pp. 46-47.

“organisms possessing a substance which has the power of suppressing the development of pigment, whether by preventing its excretion or by destroying it when formed”.⁵⁸⁸

Understanding Bateson’s theory of the ‘normal body’ in terms of a dose-based ‘presence and absence’ interpretation of wild type development helps us understand why he was inclined to believe that the factors responsible for dominant mutations might have originated as invading foreign bodies. Whilst no adaptationist, Bateson did believe in the power of natural selection to act as a destructive force weeding out the unfit. This would include recessive mutations in the wild, but as according to Bateson’s ‘presence and absence’ account these were caused by the loss of function of a usually beneficial factor found in the normal body, their persistence through evolutionary time was not unexpected. In contrast, dominant mutations should have been selected out in nature given that the factors responsible for them usually had no beneficial aspect, interfering as they did with the operations of the normal body. One explanation for their occurring in domestication was therefore that they had invaded the organism’s germ-line after they had left nature and had been allowed to persist due to the fact that human breeders were less fastidious than nature in destroying unfit variations, and also in many cases deliberately perpetuated them due to the perceived economic or aesthetic advantages of some mutations. The other alternative was that dominant mutations in nature were perpetuated due to their not being expressed, i.e. being functionally ‘absent’, and so hiding themselves from the sieve of selection. This was an explanation Bateson explored, as we have seen, by supposing inhibiting factors which would prevent dominant mutants from producing vibrations or enzymes. So it can be seen that Bateson’s degenerationist theory of evolution had its roots in his particular conception of wild type in terms of the ‘normal body’ combined with a dosage-based account of factor functional ‘presence and absence’ which made it difficult for him to understand how evolutionarily detrimental dominant mutations might arise except either by invasion from without or by being hidden within from destructive selective forces by inhibiting factors.

Types or Traits?

It should be noted that Bateson’s equation of the wild type with the ‘normal body’ is not unusual in the history of genetics. The 1999 edition of Griffiths et al’s *Modern Genetic Analysis*, a widely used textbook around the turn of the millennium, describes the wild type

588 William Bateson, *Mendel’s Principles of Heredity*, 2nd Ed., Cambridge University Press, 1909, p. 267.

phenotype as requiring for its development that “All essential genes must be capable of producing their functional products”.⁵⁸⁹ The language differs but there is again here a clear equation of the wild type and the normal body. The impact of Bateson’s ‘presence and absence’ modelling of wild type was therefore more long-standing than is widely appreciated. One reason this was the case was that the ‘presence and absence’ system was widely adopted across genetics between 1906 and c. 1912, when it began to be surpassed in fruitfulness by the chromosomal linkage studies pioneered by the Morgan group.⁵⁹⁰ The use of the ‘presence and absence’ system helped bolster the idea that wild types were fundamental entities in genetics which whilst they could be studied in terms of their constituent unit characters could not be fully disintegrated into them on the grounds that the ‘normal body’ was required as a reference in order to determine what constituted variation and whether it was due to loss of factor function (recessive mutations) or the appearance of a novel factor not usually found in healthy individuals (dominant mutations). By contrast, those adopting a more de Vriesian outlook and who attributed dominance to being a property of factors as opposed to a product of interaction were more inclined towards an atomistic view of traits and factors in which wild type and the normal body held a much lesser importance. Atomism at the level of both trait and factor was central to de Vries’ theory of heredity and development, which by his own characterisation consisted in the view that “the total character of a plant is made up of distinct units. These so-called elements of the species, or its elementary characters, are conceived of as tied to bearers of matter, a special form of material bearer corresponding to each individual character”.⁵⁹¹

De Vries’ organism may therefore fairly be regarded a mosaic composed of its elementary characters produced by largely autonomously acting pangens. This idea of the organism was unacceptable for Bateson given his interactionist understanding of biology. Moreover, whereas de Vries’ views implied that organisms could be theoretically decomposed into their constituent traits and pangens without a significant loss of information, Bateson believed that organism was more than the sum of its parts, as for factors to interact they must have a pre-existing milieu within which to interact, much in the same way as forces in physics required a

589 Anthony J.F. Griffiths, William M. Gelbart, Jeffrey H. Miller, & Richard C. Lewontin, *Modern Genetic Analysis*, W.H. Freeman and Company, 1999, p. 70.

590 Elof Axel Carlson, *The Gene: A Critical History*, W.B. Saunders Company, 1966, p. 53-54.

591 Hugo de Vries, ‘The Law of Segregation of Hybrids’, [1900], In: Curt Stern and Eva R. Sherwood (ed.), *The Origin of Genetics: A Mendel Sourcebook*, San Francisco and London: W.H. Freeman and Company, 1966, p. 107.

field within which to interrelate (as shown by Radick and Rushton, Bateson was heavily influenced by the electromagnetic field theories of *fin de siècle* British physics).⁵⁹² This prior milieu, more fundamental than any trait or factor, was referred to by Bateson as the ‘residue’. He had first raised the idea in a 1902 paper with his collaborator Edith Rebecca Saunders, rhetorically asking the question of “Can we rightly conceive of the whole organism as composed of such unit-characters”, as had been argued by de Vries, “or is there some residue – a basis – upon which the unit-characters are imposed?” Bateson and Saunders, favouring the latter, then make a highly intriguing suggestion – that the residue may be responsible for reproductive incompatibility between species, as seen in hybrid sterility, and that therefore “We may be driven to conceive “Species” as a phenomenon belonging to that ‘residue’”. Hybrid sterility, they noted, could not simply be due to factor differences between parents, as Mendelian varieties could still cross without issue.⁵⁹³ Darden is thus correct in assessing Bateson as considering Mendelian varieties to be ‘nascent species’ but only if we interpret nascency in the sense of being a movement in the direction of speciation without this movement having become irreversible.⁵⁹⁴ Overall, this theory of the residue implied that whilst variety-producing factor changes were an important first step in speciation, what was needed for full speciation was a change at the level of the organisational substratum which scaffolded factors and mediated their interactions. Bateson and Saunders argued that the particular effect of a meeting of two distinct residues, as in a species hybridisation, was that the characters could not be properly divided up amongst the gametes, resulting in an individual that was a mosaic of the two species which, unable to produce its own sex cells due to this dual heritage, was consequently sterile.⁵⁹⁵

Bateson’s theory of residue-based speciation presented a direct challenge to de Vries’ pangen-centred theory of mutation in that speciation was conceived not as caused by the activity of hereditary factors but rather as the result of changes in the underlying

592 Gregory Radick, ‘Physics in the Galtonian Sciences of Heredity’, *Studies in History and Philosophy of Biological and Biomedical Sciences*, Vol 42, 2011, pp. 134-6; & Alan R. Rushton, ‘William Bateson and the chromosome theory of heredity: a reappraisal’, *The British Journal for the History of Science*, Vol. 47 Part 1, No. 172, 2014, pp. 163-165.

593 William Bateson and Edith Rebecca Saunders, ‘The facts of heredity in the light of Mendel’s discovery’, *Reports to the Evolution Committee of the Royal Society*, Vol. I., 1902, pp. 125-160.

594 Linley Darden, ‘William Bateson and the Promise of Mendelism’, *Journal of the History of Biology*, Vol. 10, No. 1, 1977, p. 91.

595 William Bateson and Edith Rebecca Saunders, ‘The facts of heredity in the light of Mendel’s discovery’, *Reports to the Evolution Committee of the Royal Society*, Vol. I., 1902, pp. 125-160.

organisational scaffold within which factors were embedded. As to what this residue consisted of, Bateson was not clear, save that it was an “*irresoluble base*”, being what remained unchanging throughout a species’ history whatever changes occurred in its factors.⁵⁹⁶ The principle impact on conceptions of ‘wild type’ had by this thesis was that it defined species not at the level of the unit characters, as in de Vries’ theory of elementary characters and associated pangens, but at the higher level of the organisation of these unit characters. Species, and therefore also wild type, could therefore not be decomposed into traits or factors without its being denatured, and as a consequence the species, and its normal manifestation the wild type, retained an epistemic privilege over traits and factors in Bateson’s worldview. The differences between de Vries and Bateson’s conceptions of species and variation thus led to a conflict within the embryonic field of genetics between those who would privilege traits and factors as fundamental and those who privileged types and organisation.

Bateson’s use of a ‘presence and absence’ based developmental model of trait production was highly successful early on in undermining the de Vriesian atomistic model based on elementary characters and autonomous factors. An instructive case is that of small mammal coat colours, one of the first areas of genetics where ideas of ‘presence and absence’ were utilised and also an early case of a study of the interaction of multiple factors. The first researcher to study the genetics of small mammals is attributed to have been Lucien Cuénot, who in 1902 showed that traits such as coat colour Mendelised (in particular that grey coat colour is dominant over albino), thus demonstrating that Mendelism was not restricted to plants. Bateson was among the researchers following up Cuénot’s experiments and in May 1903 published a paper detailing ‘The Present State of Knowledge of Colour-Hereditry in Mice and Rats’. One notable fact emerging from this publication was that microscopic analysis of mouse hair had shown that the wild type cinnamon or ‘agouti’ trait was in fact compound, being produced by three pigments, black, brown and yellow, which were distributed along the hair in distinct bands. Moreover, it had been shown that “The different colour-types of fancy mice are due to the presence or absence of one or more of these pigments in various amounts”, i.e. that each pigment was produced by an independently assorting factor and that pigment production varied according to factor activity. For example,

⁵⁹⁶ William Bateson, *Mendel’s Principles of Heredity*, 1st Ed., Cambridge University Press, 1902, p. 26.

the ‘Golden Agouti’ variety was formed by the loss of black pigment, the yellow by the further loss of brown, and the albino through the loss of all capability to produce pigment. Other varieties were formed by pigment ‘dilution’, i.e. the lowering in activity of a pigment-producing factor, e.g. ‘Silver-fawn’, a diluted form of the Chocolate variety where the decreased activity of the brown pigment producing factor resulted in hairs having colourless tips.⁵⁹⁷ This early example of a compound trait showed the fallacy of depending on superficial morphological assessments in determining patterns of heredity – what was necessary was to determine what factor products, e.g. pigments, were involved in trait development, and then to analyse variation in terms of the recombinations and activity changes of factors, which were detectable through the pedigree isolation and synthetic hybridisation of factors within lab lineages. The example also showed that there was a distinction between traits as defined in terms of units of selection and as units of factor production – to understand wild types, it was necessary to study the interaction of factors in their production of adaptive traits which would escape the sieve of natural selection; the wild type could not be understood properly by just studying factors in isolation, as de Vries had attempted. The discovery of compound characters thus showed that the wild type was composite but also that the units of heredity could not be treated atomistically at an evolutionary level. Even ignoring Bateson’s residue-based speciation theory, compound characters still showed that evolution operated at the species level not at the level of factors, the wild type and its morphological traits being the product of factor interactions and therefore not fruitfully reducible to individual unit-characters.

An even more impressive application of the ‘presence and absence’ system to explain compound traits was developed by Bateson and Punnett in 1905 to explain the inheritance of different allelomorphs of comb in chickens (Figure 1). The trait associated with the wild type *Gallus bankiva* was in this case the single comb. Two dominant mutations, ‘pea’ and ‘rose’ were identified which when crossed produced a fourth phenotype, ‘walnut’, all animals of the F1 generation being walnut. Crosses between ‘walnut’ birds often produced the classic Mendelian ratio of 9:3:3:1 (9 walnut, 3 pea, 3 rose, 1 single) associated with Mendelian dihybrids (two factors as varying). The reappearance of the wild type was regarded as surprising as it was “not known to have been put in”. It was later determined that some of the

⁵⁹⁷ William Bateson, ‘The Present State of Knowledge of Colour-Heredity in Mice and Rats’, *Proceedings of the Zoological Society of London*, Vol. II, London: Longmans, Green, and Co., 1903, pp. 72-5.

fowls presenting rose and pea combs were heterozygous for the wild type, explaining reversion in the one-in-16 occasions when two wild type-producing factors were paired. Bateson and Punnett had in 1905 supposed that, rather than assuming four distinct alleles, that the unusual behaviour of this set of traits could be explained in terms of the interactions of two factors, rose and pea, and their 'absences', no-rose and no-pea. Heterozygotes between 'present' and 'absent' factors produced the dominant mutant phenotype, whereas the presence of both rose and pea produced the compound walnut character. The wild type, meanwhile, was only produced in the 'absence' of both factors (Figure 2).⁵⁹⁸ This implied to Bateson, as discussed previously, that the products of the pea and rose factors were not found in the 'normal body' of the chicken, implying either that these factors had entered the species after domestication as invading foreign bodies or else that in the wild type *Gallus bankiva* they were usually suppressed by an inhibiting factor since lost in some domestic breeds. Importantly regarding the conflict in genetics between interactionist and atomistic approaches, the case of comb variation in chickens also further demonstrated that even in cases where microscopic analysis did not determine any visible delineations in a trait such as walnut, it could still be the case that the trait was at a genetic level the compound production of multiple factors. This applied to wild type traits as much as it did mutant ones.

In summary, whilst de Vries ultimately rejected the use of wild type as a significant unit of analysis, Bateson embraced it, utilising it as a boundary object bridging not only the domestic-wild and lab-field divides but also between variation and typicality and health and disease, and evolution and degeneration. Bateson was therefore able to use the wild type concept in order to enhance the capacity of genetics to extend its claims beyond the lab to nature and beyond a relatively small array of experimental organisms to life in general. The debate between holist and reductionist interpretations of wild type was not, however, over, and it would not be long before the Batesonian system would again be challenged by a research programme less committed to preserving the wild type as a primary unit of analysis, namely the chromosomal genetics of the Morgan group.

⁵⁹⁸ Lindley Darden, *Theory Change in Science: Strategies from Mendelian Genetics*, Oxford University Press, 1991, pp. 69-70; & William Bateson, *Mendel's Principles of Heredity*, 2nd Ed., Cambridge University Press, 1909, pp. 61-66.

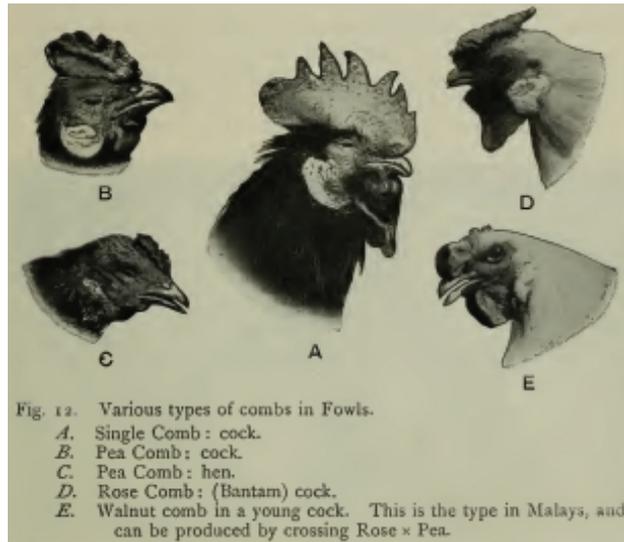


Figure 1 - Source: William Bateson, *Mendel's Principles of Heredity*, 2nd Ed., (1909), p. 61.

1. Rose (domt) R , absence of rose (rec.) r .
 2. Pea (domt) P , absence of pea (rec.) p .

RP RP	RP Rp	RP rP	RP rp
walnut pure	walnut giving rose	walnut giving pea	walnut giving all 4
Rp RP	Rp Rp	Rp rP	Rp rp
walnut giving rose	rose pure	walnut giving all 4	rose giving single
rP RP	rP Rp	rP rP	rP rp
walnut giving pea	walnut giving all 4	pea pure	pea giving single
rp RP	rp Rp	rp rP	rp rp
walnut giving all 4	rose giving single	pea giving single	single pure

Figure 2 - Source: William Bateson, *Mendel's Principles of Heredity*, 2nd Ed., (1909), p. 65.

Chapter 10 – The Chromosome Theory of Mendelian Heredity, the Downfall of Interactionist Models of Wild Type & Its Disintegration Into Genes

For all its evident virtues as a heuristic, Bateson's 'presence and absence' model would be rapidly surpassed as a research programme by the Morgan group's *Drosophila*-based studies of chromosomal linkage from around 1912 onwards. Traditionally, much has been made of the apparent inbuilt limitations of the model, namely that it could only treat alleles as 'present' or 'absent', therefore was unable to incorporate the discovery of multiple allelism, and that it claimed 'absent' alleles to represent actually absent factors, therefore could not account for back mutations.⁵⁹⁹ Whilst it is true that Hurst did appear to endorse actual factor absence c. 1906, I do not believe the evidence is definite that Bateson himself committed to this interpretation. Instead, I suggest it more likely that what Bateson meant when he spoke of 'presence' and 'absence' was the presence or absence of *factor products*, i.e. enzymes or vibrations. As to why Bateson was otherwise interpreted, I point to, following Falk, his adoption of de Vries' terminology of the 'unit-character', with its embedded de Vriesian assumption of a one-to-one relation between factor and trait, as the cause of the widespread perception that by 'absence' Bateson implied the actual absence of a factor.⁶⁰⁰ As I have sought to show, Bateson's interpretation of factor-trait relations was in fact a many-to-one and one-to-many one. Factors could both pluralistically interact to produce compound characters like 'walnut' combs and singularly vary in their generation of products, resulting in a spectrum of 'diluted' phenotypes lying between the fully 'present' and fully 'absent' phenotypes. Bateson's presence-and-absence model was therefore not, as suggested by Darden, "a hypothesis that explains two and only two states of a character", but was in fact able to incorporate much of the data suggesting multiple allelism by instead arguing that such variations were due to a change within a factor in its capacity to produce enzymes or vibrations.⁶⁰¹ Back mutations could similarly be explained as due to a factor re-attaining its capacity to produce.

599 Lindley Darden, *Theory Change in Science: Strategies from Mendelian Genetics*, Oxford University Press, 1991, pp. 69-71; & Elof Axel Carlson, *The Gene: A Critical History*, W.B. Saunders Company, 1966, p. 64.

600 Raphael Falk, 'The rise and fall of dominance', *Biology and Philosophy*, Vol. 16, 2001, p. 286.

601 Lindley Darden, *Theory Change in Science: Strategies from Mendelian Genetics*, Oxford University Press, 1991, p. 70; & Raphael Falk, 'The rise and fall of dominance', *Biology and Philosophy*, Vol. 16, 2001, p. 295.

So if we dismiss these traditional arguments for the surpassing of the ‘presence and absence’ model by the chromosome theory, what other reasons can be given? The main cause, I will suggest, was that whilst the ‘presence and absence’ model was able to incorporate an interactionist and developmental account of the function of factors in the production of traits, one which moreover ontologically prioritised the type over its component factors, it offered no account of overall genomic structure and materiality. This was not least in part because Bateson’s research programme was almost entirely dependent on an inferential analysis of factors based on studying morphological traits. It was thus in many ways a phenomenalist examination of surfaces as a means to infer underlying causes. This is not to say that Bateson and his collaborators did not make substantial efforts to directly investigate these underlying causes, e.g. Muriel Wheldale’s studies of the role of enzymes in colour production in snapdragons, but isolating the chemical reagents involved in trait production was difficult if not impossible much of the time. There was even less experimental access to the causes of meristic variations (assumed to be produced by vibrations), Bateson admitting that these represented a “range of problems of causation from which we are as yet entirely cut off”.⁶⁰² This inaccessibility of the internal causes of traits was one reason why assumptions about the ‘normal body’ of a species, its wild type, were so important in the ‘presence and absence’ system. This can be seen from the example of dominant whiteness, a dominant ‘absent’ trait which Bateson was able to explain by assuming an interaction between the products of the wild type and the mutant factor whereby the latter destroyed the former. The assumption of a wild type norm thus allowed Bateson to explain such apparent anomalies as dominant ‘absences’ as due to the inhibition of wild type factors. But ultimately all these supposed interactions could only be assumed and indirectly investigated through breeding experiments – as to what really was going on under the surface, little could be said.

The issue of the structural arrangement of factors came to a head with the discovery of new forms of associative inheritance. In 1906, Bateson and Punnett, studying sweet peas, found what they called ‘partial coupling’ between long pollen grains and purple flower colour (dominant alleles [represented by Bateson and Punnett as ‘AB’] to the recessive round pollen (‘a’) and white colour [‘b’]). Instances of ‘coupling’ (the consistent co-inheritance of two otherwise distinct characters) had already been found by Correns in the early years of

⁶⁰² William Bateson, *Problems of Genetics*, Yale University Press, 1913, p. 86.

Mendelism. This already suggested some form of structural relation between certain factors. The difference here was that this co-inheritance was incomplete (i.e. there were still instances of the non-coupling traits Ab and aB being inherited together). This had the consequence, unlike complete coupling, of massively distorting the 9:3:3:1 ratio usually expected when crossing individuals differing in two allelic characters. In the case of the sweet pea, for instance, the ratio of long to round pollen was 12:1 in purple flowers as opposed to the expected 3:1. Bateson and his colleagues then found other examples of partial coupling in which different ratios presented themselves. Then in 1908, a second phenomenon was reported, at first called 'spurious allelomorphism', then later 'repulsion', this being where otherwise unrelated factors, instead of being consistently co-inherited, were consistently shown to segregate at the gametic level (this produced a 2:1:1 gametic ratio [AB , aB , Ab] in which a double recessive did not form due to the repulsing nature of A and B). This in turn in 1911 was also shown to be partial with the discovery of a double recessive, 'cretin sterile' (nf), in crosses between 'normal sterile' (Nf) and 'cretin fertile' (nf) ('cretin' was a flower character so named for its open mouth and projecting stigma/'tongue'; 'sterile' referred to sterile anthers). This phenomenon was only visible in large experiments, the initial ratio it was found in (again in peas) being 226:95:97:1. These high ratios were a problem for Bateson. It had originally been noted that these differing 'gametic ratios' could be aligned in a predictive mathematical model based on discontinuous geometric steps. Each step was assumed to represent a cell division in gamete production. But Bateson was already aware that the number of cell divisions usually observed in meiosis (two) was lower than required for most of his coupling ratios to work.⁶⁰³ It was, in other words, a known false model without a working theoretical mechanism. As Bill Wimsatt has emphasised, a model's being false is not in itself a bad thing, as such models often have uses in clarifying data and suggesting future avenues for research.⁶⁰⁴ Nonetheless, so long as the model lacked a mechanism, Bateson and Punnett were unable to offer a proper explanation for their data, and it appears that the high partial repulsion ratios forced their hand. The mechanism proposed to explain these anomalous phenomena was called 'reduplication'. It assumed that during the mitotic multiplication of germ cells prior to gamete formation that a germ cell would first

603 Elof Axel Carlson, *The Gene: A Critical History*, W.B. Saunders Company, 1966, pp. 41-3 & 50-2; & William Bateson and Reginald C. Punnett, 'On Gametic Series involving the Duplication of Certain Terms', *The Journal of Genetics*, Vol. 1, No. 4, pp. 294-301.

604 William Wimsatt, 'False Models as Means to Truer Theories', *Re-engineering Philosophy for Limited Beings: Piecewise Approximations to Reality*, Harvard University Press, 2007, pp. 104-105.

double its contingent of factors then divide twice in such a way that each of the four quadrants would contain one of four possible combinations of alleles, namely AB, ab, Ab, aB. In cases of coupling, quadrants containing paired dominants and recessives would then further divide, whereas in cases of repulsion it would be the cells containing a dominant-recessive combination which would do so. By this means, reduplication would produce the unusual gametic ratios seen in cases of coupling and repulsion.⁶⁰⁵

Reduplication, whilst a lovely example of ‘saving the phenomenon’ through the addition of epicycle-style auxiliary assumptions, was from the very start a highly problematic hypothesis for explaining associative inheritance. Whilst it worked as an abstract model, it could not be correlated with any observations from cytology, a fact pounced upon by the Morgan group’s Alfred Sturtevant, who surmised that the reduplication hypothesis required that geneticists “assume an enormously complex series of cell divisions, many of them differential, proceeding with mathematical regularity and precision, but in a manner for which direct observation furnishes no basis”.⁶⁰⁶ The limitations of a phenomenalist approach as that used by Bateson and his colleagues are thus made clear. Where Sturtevant and his fellow *Drosophilists* differed was in that their chromosomal theory of heredity allowed them to adopt a *correlational* method where phenotypic traits could be correlated with the behaviour of chromosomes. Their theory, in other words, was not simply dependent on analysis of surfaces but could also call on the evidence presented by internal structures. By correlating the evidence of both genetic breeding experiments and of cytology, a form of what Wimsatt refers to as “multiple determination”, the Morgan group were able to offer far more robust hypotheses about genomic structure than Bateson.⁶⁰⁷

It should be noted that theorised correlations between cytological observations of chromosomes and hereditary phenomena were not new in 1910, when Morgan discovered the white-eyed male mutant fly that led him to discover the first instance of ‘sex-limited inheritance’ in *Drosophila*. It had been proposed by Boveri and Sutton in 1902-3 that differences between the chromosomes of male and female sea urchins and grasshoppers

605 Elof Axel Carlson, *The Gene: A Critical History*, W.B. Saunders Company, 1966, pp. 41-43 & 50-2; & William Bateson and Reginald C. Punnett, ‘On Gametic Series involving the Duplication of Certain Terms’, *The Journal of Genetics*, Vol. 1, No. 4, pp. 294-301.

606 Elof Axel Carlson, *The Gene: A Critical History*, W.B. Saunders Company, 1966, p. 55.

607 William Wimsatt, ‘Robustness, Reliability, and Overdetermination’, *Re-engineering Philosophy for Limited Beings: Piecewise Approximations to Reality*, Harvard University Press, 2007, p. 44.

indicated that the determination of sex was based on chromosomal inheritance, and that this therefore implied that the rest of the hereditary factors were also located in chromosomes. They pointed to similarities between chromosome behaviour in meiosis and the Mendelian phenomena of segregation. Even earlier, Weismann had similarly proposed, based on cytological observations of the time, that his hereditary determinants were based on the chromatin of the nucleus. But as Carlson shows, there were initially significant reasons for scepticism among geneticists regarding this correlation. One of these was the fact that the chromosomal theory was purely structural and offered no account of factor action. This was Bateson's main reason for having little time for it, along with his scepticism regarding particulate theories of heredity. But more mundanely, there was also a conflict between cytological findings in the United States and genetic work in Great Britain. Wilson and Stevens' studies of insects had shown that sex was determined by the presence in males of either a single or two differing chromosomes, whereas the British data based on studies of moths and birds suggested it to be females whose pair of sex chromosomes differed. That the case of sex-limited white eyes in *Drosophila* accorded with the prevailing cytological wisdom caused Morgan to take it more seriously. But Morgan also had other good reasons to doubt the Boveri-Sutton hypothesis, namely that if factors were situated together on chromosomes, then why was coupling not much more common than observed. It was therefore fortuitous that later in 1910 Morgan came across a case of partial coupling. Partiality of coupling demonstrated to Morgan that if factors were located on chromosomes then there must be some means of transference between them. Morgan therefore proposed his theory of crossing over, whereby homologous chromosomes conjugating in meiosis were theorised to exchange material when, having twisted around each other, they then split along a single plane. The major difference between this proposed explanatory mechanism and Bateson's reduplication hypothesis was that whilst they attempted to explain the same facts, Morgan based his theory on Frans Alfons Jannssens' cytological observations of chromosomal chiasmata which had been published the previous year.⁶⁰⁸ The superiority of Morgan's over Bateson's model thus lay in the fact the latter lacked the robustness of a correlation between the known facts of genetics and the evidence of cytology.

608 Elof Axel Carlson, *The Gene: A Critical History*, W.B. Saunders Company, 1966, pp. 43-48.

The theory of crossing over allowed Morgan to explain coupling and repulsion as due to the relative closeness or distance between factors on a chromosome. The fact that non-allelic coupling was partial in nature also dealt with the problem of why coupling was not more widely observed, namely that crossing over made cases of coupling at medium-to-long distances difficult to detect without conducting large scale breeding experiments and easily hidden by rounded-up/-down ratios. This theory of linkage would shortly after be adopted by Sturtevant as a founding assumption of the great modelling project of classical genetics, linkage mapping. Linkage mapping would further problematise Bateson's phenomenalist approach to genetics by rendering locus as a new major fact that a competent geneticist should endeavour to explain. Whilst Bateson's 'presence and absence' model was an excellent means of modelling function, it offered no hints as to factor location. But this is not to suggest that linkage-based models of genetic structure did not also have their drawbacks. For whilst 'presence and absence' said nothing about structure, linkage-based models said precious little about function. For most early geneticists, organismic development could not be meaningfully bracketed from factor transmission; after all, the former was in essence a continuation of the latter, and classical geneticists did not directly investigate factors but rather inferred their existence principally through the study of adult somatic traits, which represented as much the endpoint of ontogeny as they did markers for the factors which initiated and mediated it. This, in fact, was very much Morgan's own position prior to 1910. This can be seen in a 1909 statement in which he lambasts the de Vriesian concept of a 'unit-character' with a one-to-one relation between factor and trait, asserting that "we are not justified in speaking of the materials in the germ-cells as the same thing as the adult characters until they develop".⁶⁰⁹ These views are not surprising given that Morgan's background was in embryology, being led towards genetics by the study of the ontogeny of sex determination.⁶¹⁰ Scrutinising Mendelian phenomena with an embryologist's eyes, the younger Morgan was a confirmed sceptic regarding not only the Boveri-Sutton hypothesis but also particulate theories of inheritance, to which his later conversion would be "incomplete and reserved", and the Law of Independent Assortment, which in his view smacked of preformationism. Denouncing Mendelians as turning "facts... into factors at a rapid rate" and "superior jugglery" in their attempts to present Mendelian ratios as absolutes

609 Thomas Hunt Morgan, 'What are "Factors" in Mendelian Explanations?', *American Breeders Association Reports*, Vol. 5, 1909, p. 367.

610 Ron Amundson, *The Changing Role of the Embryo in Evolutionary Thought: Roots of Evo-Devo*, Cambridge University Press, 2005, p. 149.

instead of averages, he instead favoured a more epigenetic approach to inheritance where factor symbols were seen as representing developmental potentialities contained within the sex cells as opposed to marking out discrete particulate factors that unilaterally determined the development of particular adult traits.⁶¹¹ Morgan's commitment to an interactionist and developmentalist account of heredity was thus arguably even stronger than Bateson's. It is therefore no surprise to learn that Morgan enthusiastically adopted the 'presence and absence' system in his early work on genetics and that this influenced his early interpretation of wild type, as I will discuss shortly.

Given the above, it becomes clear that when Morgan chose to bracket off development from transmission after 1910 in his pursuit of the study of chromosomal genetic structure that this was an instrumental decision based on the limitations of the 'presence and absence' system outside of phenomenalist analyses of function. As I will show, this did not mark the end of Morgan's use of the 'presence and absence' system. What I would therefore argue is that at this stage what Morgan was hoping to do was establish a research programme working with both systems as compliments, so that he hoped to be able both to determine structure through the study of chromosomes and function through analysis of factor contributions to trait development. This desire endured throughout the rest of Morgan's lifetime and would later coalesce into his 1926 distinction between transmission and developmental genetics. Whilst Amundson has cited this moment as signalling the cleavage of heredity from development, it must be emphasised that Morgan did not see these two fields as ontologically distinct, rather that they dealt with the same phenomenon but from different perspectives.⁶¹² One reason why Morgan's continued belief in complementarity has been overlooked in favour of viewing him as the great demarcator is that it is often difficult to disentangle his particular contribution from that of collaborators such as Sturtevant, Calvin Bridges and H.J. Muller who, lacking a background in embryology, did tend to see ontogeny as a distinct realm from heredity. A further contributing factor to the flawed idea that Morgan drew a firm line between development and genetics after 1910 has been the tendency to point to his incorporation of aspects of Wilhelm Johannsen's genotype-phenotype distinction into the chromosome theory of heredity. But Amundson argues that this is based on the erroneous interpretation of

611 Nils Roll-Hansen, 'Drosophila Genetics: A Reductionist Research Program', *Journal of the History of Biology*, Vol. 11, No. 1, 1978, p. 182.

612 Ron Amundson, *The Changing Role of the Embryo in Evolutionary Thought: Roots of Evo-Devo*, Cambridge University Press, 2005, p. 151.

Johannsen's distinction as referring to individual organisms when in fact it was a phenomenalist interpretation of patterns of variation and heredity at a *populational* level. The genotype for Johannsen referred to the uniform germinal material shared by all members of a 'pure line', the phenotype to the variation in morphological character expressed within the pure line as a consequence of development under slightly differing circumstances. Johannsen's dichotomy therefore had more in common with the later concept of a 'norm of reaction' resulting from gene-environment interactions than it did with the later use of the genotype-phenotype distinction to represent a definitive demarcation between heredity and development.⁶¹³ Nevertheless, given how the genotype-phenotype dichotomy was later interpreted and also the fact that Morgan did little to encourage any of his students to take up developmental studies, it is perhaps not surprising that this initial distinction of complementary fields would come to serve as a significant obstacle to the development of a unified understanding the role of genes in heredity, ontogeny and evolution.

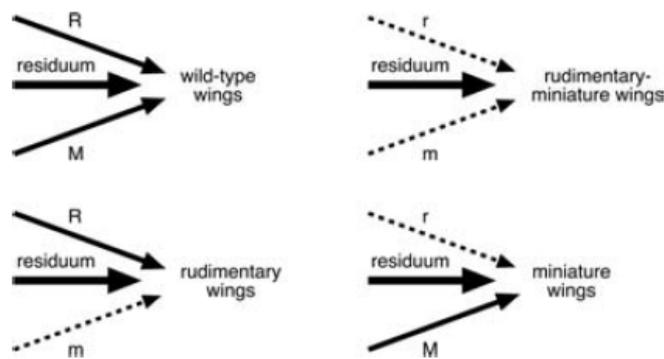


Figure 3 - Source: Raphael Falk, *Genetic Analysis: A History of Genetic Thinking*, Cambridge University Press, 2009, p. 232.

Morgan's choice to end use of a 'presence and absence' style system in his group's genetics research was ultimately due to practical concerns about intractability and a subsequent willingness to adopt instrumental methods to gain results. As has been noted by Amundson and Falk, Morgan was well aware of the developmental complexities involved in the production of traits by genes and had continued to attempt to map these functional aspects of genes onto their structure in the early years of the Mendelian chromosomal heredity research programme. This culminated in his attempt to introduce a new nomenclature in 1912 to account for the interactions of genes in ontogeny. His example was the heredity and development of wings, specifically the wild type trait and the mutant traits rudimentary,

⁶¹³ Ibid., 2005, p. 153.

miniature and rudimentary-miniature (see Figure 3). Using a version of ‘presence and absence’, Morgan gives the gene for miniature the symbol M and that for rudimentary as R. Miniature wings occurred in cases when M was present and R absent and was represented as rM; rudimentary occurred in the reverse case and was represented as Rm. The wild type trait, which Morgan knew to occur only when both R and M were present, was represented as RM. The wild type trait was thus shown to be a compound character requiring the presence of more than one gene to develop. But importantly Morgan did not treat the two genes R and M as sufficient for development. This was made clear by the case of rudimentary-miniature, represented as rm, i.e. as an absence of both genes. Morgan mused that:

“It may seem, on first thought, that no wings at all should appear with M and R absent; but such an interpretation would rest on a false conception, as I take it, of Mendelian factors; for, the absence of R and of M does not mean that all factors for wing are lost – there may be hundreds of factors that enter into the production of wings – but only that when a certain factor, R, is lost from the complex, a miniature wing is produced by the remainder; and when the factor M is lost from the complex of wing-factors, a rudimentary wing is produced by the remainder. When both R and M are absent the remaining factors are still capable of forming as much of the wing as is shown by the rudimentary-miniature wing”.

It is notable that here Morgan has utilised Bateson’s concept of the residue (rechristened the ‘residuum’) but has reconceptualised it as referring to the totality of other genes in their developmental role in the production of the organism, as opposed to Bateson’s original conception of it as referring to an underlying invariant base responsible for the consistent organisation of factors within a species lineage. What is emphasised in this model is that the developmental production of traits is best conceptualised as the result of the contribution of the whole genome as opposed to single genes, or for that matter even small groups of genes. Whilst individual genes are difference-making in that their deletion results in a change in trait production, a gene nonetheless cannot be said to produce the trait as its own contribution to development only matters in the context of the contributions of all other genes. In this model there are, as Falk surmises “*no genes for traits*”.⁶¹⁴ It is the organism and its development

614 Raphael Falk, *Genetic Analysis: A History of Genetic Thinking*, Cambridge University Press, 2009, pp. 231-233.

which is central to this model of heredity and development, not genes. And, as in Bateson's version of the 'presence and absence' system, the wild type continues to hold an ontological privilege over genes in that it is presented as the developmental endpoint attained when all genes active in the 'normal body' are 'present'. The wild type at this stage in the Morgan group's conceptualisation of heredity and development is thus yet to disintegrate into its constituent genes. It is not merely a reference norm based on an ascribed 'typical' laboratory lineage but rather a biological norm grounded in the teleological process of development.

What a contrast is seen a mere two years later in the presentation of genetic contributions to development in *The Mechanism of Mendelian Heredity*! Discussing the case of eye colour, which by this time was known to be affected by at least twenty-five different genes at different loci, it is stated that in the case of a mutation such as pink eye colour that "we may say that a particular factor (p) is the cause of pink". What has changed here? How is it that a single factor is now being credited for the production of a trait when it was acknowledged by Morgan a mere two years previously that it is the genome as a whole which is responsible for trait production, not any individual factor? The answer is quite simple: Morgan's views on heredity and development have not undergone an overnight conversion to genetic atomism. Rather what has happened is that a different idea of causation is being employed here than in 1913's interactionist model of factor contributions to trait development, Morgan et al commenting that "we use cause here in the sense in which science always uses this expression, namely, to mean that a particular system differs from another system only in one special factor". This represents a move from an interactionist developmental account of the gene to a difference-making account which Schwartz calls the 'differential concept of the gene'.⁶¹⁵ This move from a more holistic methodology to an instrumental atomism can also be understood in terms of Griffiths and Sterelny's distinction between 'actual sequence explanations' and 'robust process explanations'; whereas the former style of explanation seeks to explore the nuances of causal history, the latter style is based on "reveal[ing] the *insensitivity* of a particular outcome to some feature of its actual history".⁶¹⁶

615 Ron Amundson, *The Changing Role of the Embryo in Evolutionary Thought: Roots of Evo-Devo*, Cambridge University Press, 2005, p. 149.

616 Kim Sterelny and Paul E. Griffiths, *Sex and Death: An Introduction to Philosophy of Biology*, The University of Chicago Press, 1999, p. 84.

Several factors make clear why Morgan and his colleagues chose to move to this differential interpretation of gene action. Firstly, the ‘presence and absence’ nomenclature was limited in its utility to the interaction of two factors, being, in Morgan’s words, “not sufficiently elastic” to incorporate more – after all, how does one understand ‘absent’ if it can stand for two distinct mutant traits that are recessive to the wild type – the terminology does not allow for two different kinds of absence!⁶¹⁷ Furthermore, the system was impractical as every time a new mutation was discovered it required that the terms be changed. As an example, Falk considers what would occur if a third mutation, ‘vestigial’ (*Vg*) were incorporated into the 1913 ‘presence and absence’ model: “specifying the effect of *Vg*... would imply profound rephrasing of the reference to all three genes. The three single-mutation phenotypes should be now described as miniature-rudimentary for the *M R vg* genotype, rudimentary-vestigial for the *m R Vg* genotype, and miniature-vestigial for the *M r Vg* genotype. The specification of any other gene of the developmental residuum would demand further revision of the description of all the previous genes involved”.⁶¹⁸ The ‘presence and absence’ system could therefore not be sustained given a desire to establish a standardised and consistent terminology for genetics. The perceived inability of the ‘presence and absence’ model to adequately deal with interactions between more than two factors was finally made intolerable for the Morgan group thanks to the relentless and ongoing ‘flood’ of mutants that had begun appearing in their ‘fly room’ from the winter of 1911-2 onwards.⁶¹⁹

As has been emphasised by Falk, the new language of genetics was not neutral but was rather, whatever Morgan’s intentions, a *de facto* genocentric nomenclature. In moving from an actual sequence style of explanation as used in interactionist ‘presence and absence’ models to the style of robust process explanation developed in the differential concept of the gene, a significant background assumption was made in order to produce the insensitive background against which difference-making gene action could occur. Whereas previously the residuum had been openly acknowledged as actively contributing to all trait development, in the differential account of gene action it is treated as a constant backdrop against which gene action may be measured. Whilst the Morgan group all knew and acknowledged that the

617 Thomas Hunt Morgan, ‘Factors and Unit Characters in Mendelian Inheritance’, *The American Naturalist*, Vol. XLVII, No. 553, 1913, p. 12.

618 Raphael Falk, *Genetic Analysis: A History of Genetic Thinking*, Cambridge University Press, 2009, p. 233.

619 Robert E. Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Life*, The University of Chicago Press, 1994, p. 61.

residuum was causally active, by treating it as a contextual constant and the gene as the foregrounded actant they effectively transferred much of the perceived causal power of the residuum to the gene. The 1913 model had kept in view the residuum and emphasised the minute nature of individual factor contributions to development. By pushing the residuum into the background and treating it as a constant, the visibility of individual gene action was magnified but at the cost of granting genes illusory autonomy as ‘atoms’ of heredity and uncontextualised power as difference makers.

One especially notable effect of this instrumental obviation of issues of interaction was that dominance was once more treated as an intrinsic property of dominating genes. Morgan and his collaborators were conscientious of being interpreted as suggesting a one-to-one relation between genes and traits, so insisted that what was being considered a property was the capacity of a gene to produce its primary product, e.g. an enzyme, and not the trait, the many-many relations between gene products involved in ontogeny being not the prerogative of transmission geneticists to study but rather that of developmental geneticists. But nonetheless, they continued to treat the relation between genes and traits *as if* it were one-to-one in that the differential account’s analysis of gene action in effect treated all other contributing factors in development as *ceteris paribus* conditions. Moreover, whereas the ‘presence and absence’ model had offered an attempted explanation of dominance, the Morgan school of structural genetics in effect parcelled off explanations of function for other researchers to investigate. The physiology of dominance was left unexplained; an assumption of dominance as a relatively simplistic and unitary phenomenon took its place in most models of heredity, variation and evolution based on classical genetics research. This, for example, allowed the population geneticist Ronald Fisher to propose in 1928 a selectionist explanation of dominance whereby “Assuming that all heterozygotes were originally intermediate, there would be a tendency for those to survive whose gene complex reacts in the most favorable manner with any new mutation. Thus if the effects of a gene were disadvantageous they would become increasingly recessive, while if beneficial they would shift towards complete dominance, owing to the selection of those heterozygotes in which they were most markedly developed”. This hypothesis was not grounded in any knowledge of physiology but rather in the Morgan group’s discovery of multiple alleles, which Fisher theorised selection could act on so as to ensure that only the allele which produced the fittest phenotype would

predominate at any one locus, so that advantageous genes would become dominant and disadvantageous genes recessive.⁶²⁰ The prevalence of dominant wild type traits was therefore explained as a product of selection in nature. This had the correlative effect of offering theoretical support for the representative quality of lab wild types, as so long as their traits did not look or behave differently from those of wild individuals and were dominant but not inherently disadvantageous, they could be considered as reliable instances of traits predominant in nature. Research done utilising strains with such traits could therefore be extrapolated to nature with less difficulty on the basis of such assumptions.

But note that Fisher's theory did not offer a decent explanation for why dominant mutations that would be inherently disadvantageous in nature, e.g. dominant whiteness, polydactyly, some hereditary diseases, were able to become dominant given that if their heterozygotes had started off as intermediary, why should they become even more disadvantageous? By contrast, Bateson's 'presence and absence' model was able to explain the dominant effect of these mutations in terms of the interaction of factor products. This was perhaps a clue that the causes of dominance are not physiologically simple, or in fact unitary. As Falk observes, dominance "turned out to be one of the more troublesome cases [of concepts from classical genetics] that evaded reduction to molecular terms". Indeed, since the molecular turn the dominance concept has in fact fragmented based on the variety of different allelic phenomena that produce dominance-like relations between phenotypes, with a corresponding variety of new categories arising, e.g. haploinsufficiency, ectopic mRNA expression, constitutive protein activity, toxic protein, dominant negative, pseudodominant. Given these circumstances, it is little surprise to see something of a return to a Batesonian style 'presence and absence' model based on assumptions about the 'normal body', namely Andrew Wilkie's definition of dominance: "The most likely effects of a random gene mutation are that it will either be neutral (normal phenotype) or inactivating. If the latter, the question is whether the inactivation would be clinically manifest in the heterozygote... or only in the homozygote".⁶²¹ The assumption that dominance was a physiologically simple and unitary mechanism, on which Fisher's theory and its corollary support for the representativeness of lab wild types was based, has thus proven ultimately false.

620 Raphael Falk, 'The rise and fall of dominance', *Biology and Philosophy*, Vol. 16, 2001, p. 299-304.

621 Ibid., pp. 313-318; & Andrew O.M. Wilkie, 'The Molecular Basis of Genetic Dominance', *Journal of Medical Genetics*, Vol. 31, 1994, pp. 89-98.

The model of heredity, variation and evolution presented in the Morgan group's 1915 *Mechanism of Mendelian Heredity* was one which acted to magnify the gene at the expense of the organism and foregrounded single gene action at the expense of multi-gene interaction. The wild type was not only pushed into the background but was also fundamentally undermined. Whereas in Morgan's 1913 'presence and absence' model the wild type was shown to be the production of all genes and the product of none individually, the foregrounding of genes over genomes, organism and environment had the effect of attributing the power to produce wild type characteristics to individual genes, the 'wild type alleles'. The portrayal of wild type alleles as producing wild type traits both denied the inherent gene-to-gene and gene-to-environment interactivity that was central to trait development. Troublesome questions regarding the process of ontogeny and the influence of genomic and environmental context were in this way circumvented – as Amundson observes, what mattered in this new system was that “correlations can be traced between the *end products* of ontogeny in successive generations”, whereas the actual process of development itself was black-boxed.⁶²² The wild type was thus no longer grounded, as it had been in the 'presence and absence' system, in its being related to the biological norms apparent in the development of the 'normal body' (though the equation between the two was still widely assumed). It was instead grounded in the 'typical' phenotype, which was assumed to be correlated with a typical genotype. This typical phenotype was furthermore treated as decomposable as opposed to being thought greater than the sum of its parts. For when genes are epistemically privileged as difference-makers, the ontological picture that emerges from such research is of the phenotype as but the sum of individual gene actions.⁶²³ The wild type thus decomposes into the wild type alleles of the typical genotype. This idea of wild type as decomposable into genes I call the *compositional notion of wild type*, and it is the conception of wild type that predominated in the transmission genetics of the Morgan group and those they influenced.

622 Ron Amundson, *The Changing Role of the Embryo in Evolutionary Thought: Roots of Evo-Devo*, Cambridge University Press, 2005, p. 150.

623 Ken Waters has commented that “The fact that classical geneticists routinely constructed experimental situations so that the difference principle would apply undermines the notion that they naively believed that each of the phenotypic characters they studied were preformed in single genes or even gene complexes”. Whilst he is certainly right that Morgan et al were not naïve in their thinking and practice, this nonetheless does not get away from the fact that the change of perspective inherent in the move to genocentric language greatly influenced popular and scientific perceptions of the relationship between genes and organisms to the extent that reductionist interpretations of organisms as composites of gene-produced traits became more commonplace. For Waters' comments, see: C. Kenneth Waters, 'Causes That Make a Difference', *The Journal of Philosophy*, Vol. 104, No. 11, 2007, pp. 558.

Conclusion – The Place of the Wild Type in Model Organism Systems in Classical Genetics

What was a ‘typical phenotype’ in the Morgan group’s model system? In my introduction, I mentioned Little's 1921 ‘Report of the Committee on Genetic form and Nomenclature’, which had concluded that “In most animals and plants it is convenient to settle on a standard type, preferably the wild type, when this is known”.⁶²⁴ Morgan had initially selected *Drosophila* as a 'wild' species and would have developed some understanding of its natural variability. He would therefore have been able to see that over the years the flies he and his students were utilising as wild types gradually became more uniform. Adopting a differential concept of the gene allowed 'wild type' to be assigned on to traits on a token basis as opposed to on a more holistic assessment, and therefore offered a means of getting around the differences between lab wild types and really-wild types. Nonetheless, the label 'wild type' could not be applied arbitrarily, and the idea of a 'real wild type' found in nature still served as something of a guiding model for research, as indicated by the 1921 report.

To explain why a natural standard still remained important in the lab, I borrow a distinction Christopher Di Tiresi makes between ‘reference standards’ and 'natural standards'. Both kinds of standards can be used as tools by acting as constants around which to model, measure and manipulate variation. Reference standards are chosen by convention, whereas natural standards are selected from nature, but this is not, I argue, an exclusive relationship.⁶²⁵ Rather, when the choice of reference standards is overly arbitrary in scientific knowledge production, their utility as epistemic tools can be seriously compromised, for if a laboratory standard has no relevant commonalities or analogies with any target entities or phenomena outside in nature, knowledge produced using it can only circulate between laboratories and cannot be properly extrapolated to the natural world. Such laboratory objects are artefacts and their epistemic meaningfulness is largely restricted to within human-designed artificial worlds. But genetics did not aim merely to study artefacts; it aimed to study patterns of heredity in the lab which could be extrapolated to the wild in order to explain natural phenomena, especially relating to evolution. Consequently, the wild type in classical genetics was required to play a dual role as both reference standard against which variation may be measured *and* natural

624 C.C. Little, ‘Report of the Committee on Genetic form and Nomenclature’, *The American Naturalist*, Vol. 55, No. 637, 1921, p. 176.

625 Christopher Di Tiresi, ‘Taming Variation: Typological Thinking and Scientific Practice in Developmental Biology’, Thesis (PhD): University of Chicago, 2010, pp. 16-17.

standard so as to enable knowledge produced in the lab to be extrapolated to individuals and species living wild under the conditions of the struggle for existence.

Now it is true that wild types were in some ways eminently suited to this role as hybrid standards. As Evelyn Fox Keller observes, model organisms are “not artifactually constructed but selected from nature’s very own workshop”, i.e. they are organisms, not machines, and no matter what alterations are imposed on them by human interventions, these are wholly insignificant compared to the changes wrought on them by nature across evolutionary history. Because of this core naturalness, Keller thinks, model organisms can act much like political representatives for their species and for nature in the lab.⁶²⁶ Wild types, as ostensible stand-ins for the ‘normal’ form of the species in nature, would ideally be exemplary such representatives, and I believe constituted the component of classical genetics model organism systems which was designated principle responsibility for this role of representing the natural in the lab. However, as shall be seen, there were many ways in which the laboratory strains called ‘wild type’ were in actual fact not very wild or typical. I shall now briefly deal with some of the problems this caused.

Even before the Morgan group carried out substantial ‘debugging’ (Kohler), breeding out so called ‘C factors’ and other modifiers that impeded crossing over so as to ensure their linkage maps accurately represented genetic distance, it was the convention in genetics to use heavily inbred ‘pure lines’. Morgan and his colleagues did not hold to the view, like de Vries, that in doing so they were extracting the ‘elementary species’ out of their natural admixture – their interpretation of the nature of evolutionary variation was more fine-grained than that and became even more so with the discovery of multiple allelism, which showed that dominance could hide far more variation in nature than previously suspected. Morgan and his ‘drosophilists’ were therefore perfectly aware that their standard flies were deliberately “constructed from stocks that produced recombination data conforming most closely with Mendelian theory” (Kohler) and that the “bricolage” chromosomes of these ‘wild types’ were derived from several original stocks which had been further inbred to ‘debug’ them. Similarly, the standardised environment of the genetics labs was very different from that of nature, resulting in a distinct ecology. The Morgan group’s *Drosophila* flies, normally active

626 Evelyn Fox Keller, *Making Sense of Life: Explaining Biological Development with Models, Metaphors, and Machines*, Harvard University Press, 2002, pp. 51-52.

only at dawn and dusk, became active around the clock like their caretakers.⁶²⁷ If we recall the earlier discussion in this thesis of definitions of domestication, with an organism being removed from nature's struggle for existence, taken indoors, being supplied with abundant nutriment and having its reproduction controlled being four key factors, then these *Drosophila* 'wild types' must surely have counted among the most domesticated creatures to have yet existed on the planet!

These 'normal' lab conditions furthermore were not always the 'normal' conditions for the development of some of the very mutant traits that standardisation was supposed to expose by making visible their difference. The Morgan group found this out for themselves in the case of mutations such as 'abnormal abdomen', which varied from fully expressed to indistinguishable from wild-type according to whether reared on moist or dry food. In such cases Morgan *et al* assert that "Where [the] environment [required for the mutation's expression] is not the normal one, its discovery is an essential element of the experiment".⁶²⁸ But the fact was that such mutation were usually only discovered by accident, such as, in the case of 'abnormal abdomen', when food was 'improperly' administered and allowed to become wet. This implied that there could be a good deal of hidden variation that was not visible in the 'normal' conditions of the lab. Moreover, cases like this where mutations appeared as wild type phenotypically would be used later in the 1930s by Richard Goldschmidt to argue that the structuralist research programme's basis in assuming inferable correlations between fully developed phenotypes and underlying genotypes was flawed and its findings could only be rescued by once more introducing a physiological perspective that incorporated the role of development and environmental conditions. Goldschmidt dubbed mutants which produced wild type phenotypes under certain environmental conditions 'genocopies', and furthermore also named a parallel category, 'phenocopies', these being phenotypes which were "produced experimentally from the Wild-type form... and cop[y] or [duplicate] the appearance of a mutant (or combination of mutants)".⁶²⁹ The distinction between wild types and mutants, Goldschmidt and other functionalist critics argued, was a laboratory artefact not applicable across all of nature's varying environments.

627 Robert E. Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Life*, The University of Chicago Press, 1994, pp. 65-67, 39-41 & 53.

628 Thomas Hunt Morgan, Alfred H. Sturtevant, Hermann J. Muller, & Calvin B. Bridges, *The Mechanism of Mendelian Inheritance*, Henry Holt and Company, 1922, p. 38.

629 Richard Goldschmidt, *Physiological Genetics*, New York and London: MacGraw-Hill Book Company, Inc., 1938, p. 4.

It was not only the artifice of laboratory conditions which was open to criticism, for there were also those who would question whether the kind of breeding experiments done in genetics had any true correlate in nature, and therefore whether the data they produced could truly be extrapolated to evolutionary phenomena in the wild. “If we are ever able to discover what part hybridization plays in evolution”, the mouse geneticist Maud Slye remarked in 1915, “it is immeasurably more valuable to find out the behaviour of natural species rather than of forms created in the laboratory under more or less artificial conditions, and which are never found outside the laboratory”. Specifically, she insisted it to be “still open to question whether the wild house mouse (*Mus musculus*) inevitably furnishes actual ‘homozygotes’ which will stand every test of the theoretical ‘homozygote’”, i.e. whether actual wild types ever approached the artificial uniformity of character found in ‘wild type’ lab strains. To avert this issue, she insisted on corroborating results attained using “artificial laboratory” strains by repeating crosses using “wild housemice”.⁶³⁰ Slye here was targeting the experimental techniques of fellow mouse geneticist C.C. Little but her critique may be considered representative of a form of anti-reductionist scepticism that equally applied to the work of the Morgan group.

That Morgan was fully aware of the “artificial and unnatural conditions” typical of the laboratory environment and of the contrived character of experimental technique and procedure is clearly demonstrated by his defence of such practices in his immediate follow-up to the *Mechanism of Mendelian Inheritance*, 1916’s *A Critique of the Theory of Evolution*. Responding to criticisms that the results of his experiments do not provide reliable evidence for the actual operation of evolution “in the “open”, nature “at large” or to “wild” types”, Morgan asserts that, if those rejecting the results obtained from the breeding pen, the seed pan, the flower pot and the milk bottle are to be consistent, “this same objection should be extended to the use of the spectroscope in the study of the evolution of the stars, to the use of the test tube and the balance by the chemist, of the galvanometer by the physicist. All these are unnatural instruments used to torture Nature's secrets from her”.⁶³¹ Morgan’s instrumentalism is thus once again displayed here. Wild types, the insinuation is, may be genetically and ecologically quite removed from their truly wild counterparts, ‘normal’ in

630 Maud Slye, ‘A Reply to Dr Little’, *Science*, Vol. 42, No. 1077, 1915, 246-247.

631 Thomas Hunt Morgan, *A Critique of the Theory of Evolution*, Princeton University Press, 1916, pp. 84-85.

only a broad sense, but it is necessary to work with such often artefactual tools if controlled and verifiable data is to be produced. Nature must be treated unnaturally if it is to be studied, and to forego such means would render biology a non-science.

If wild types are instrumental tools, but tools nonetheless which are not wholly artificial due to their lengthy preceding natural history, how is it that they represent nature in the lab? A sceptic might suggest that we have here another case of the conventionalisation of 'natural' practices described by Gooday with reference to 19th century microscopy in Chapter 6.⁶³² But I believe that there is some element of representation here that goes beyond mere convention. In particular, I will argue that the major means by which wild types stand in for nature is through their retention of natural properties and processes. I here again invoke Leonelli's concept of 'material abstracting' (see also Chapter 6), the process by which nature and natural entities entering the lab are stripped of many of their initial found qualities to improve their tractability in the lab but by continuing to retain the remainder of their found qualities may consequently be "taken to be representative of a [broader] set of phenomena". Wild types on this view, by retaining natural properties, may be taken as representative of their species with regard to phenomena relating to retained natural properties, and so long as they are handled in such a manner as to prevent the production of obstructive artefacts (Leonelli's 'performative skills').⁶³³ This importance of underlying homologies between lab reference standard and target systems in nature has similarly been recognised by Schaffner, who remarks that "Such prototypes of necessity need to be representative – to connect analogically to other prototypes – if they are to do their job(s) as surrogates".⁶³⁴

The conditions for what is necessary for a wild type to be a stand-in for nature also determine what kinds of experimental artefacts are acceptable and which not. In other words, in any experimental system there will be aspects of the assemblage which are negotiable (and can therefore be modified to increase tractability, etc.) and others which are non-negotiable, and when a system is intended to produce knowledge that can be extrapolated to particular target systems in nature, these limitations on negotiation will be shaped by the need to maintain

632 Graeme Gooday, 'Nature' in the Laboratory: Domestication and Discipline with the Microscope in Victorian Life Science', *The British Journal for the History of Science*, Vol. 24, No. 3, 1991, p. 341.

633 Sabina Leonelli, 'Performing Abstraction: Two Ways of Modelling *Arabidopsis thaliana*', *Biology and Philosophy*, Vol. 23, 2008, pp. 523-524.

634 Kenneth F. Schaffner, 'Model Organisms and Behavioral Genetics: A Rejoinder', *Philosophy of Science*, Vol. 65, No. 2, 1998, pp. 278-279.

relations of homology or analogy between lab and nature. In the case of lab wild types, I suggest it was the wild type alleles which represented the retained natural properties that allowed for them to be considered representative of genetic phenomena in nature, and that retaining at least some level of assumed isomorphic mapping of the lab wild type genome onto actual wild type genomes was what was considered as fundamentally non-negotiable if the knowledge produced using these wild types was to be considered valid. Major means of determining whether a phenotype was ‘wild type’ included comparison with existing ‘wild type’ lineages, comparison, when possible, with actual wild flies and taxonomic descriptions of the normal characteristics of the species *Drosophila melanogaster*, though the flies used in the Morgan lab were initially known as *Drosophila ampelophila*, the original description of which is credited to Loew in his 1861 *Centuria Secunda (Dipt. Amer. Sept. indigena)*, no. 99, page 101.⁶³⁵ Alongside these phenotypic methods, individuals could also be crossed with other wild types and known mutant lineages to see if they produced expected Mendelian ratios in crosses. So long as it was felt that no non-negotiable properties had been tampered with, the wild type could act as a bridge between the lab and nature, one of Star and Griesemer’s ‘boundary objects’.

That geneticists believed their lab ‘wild types’ to be appropriate representatives of nature can be seen in claims of substitutability between lab wild types and wild types in nature. For example, William E. Castle in 1905 stated that the same range of Mendelian mouse coat colour phenotypes found in the lab can be produced by crossing a purebred albino with “a wild house-mouse of the kind every barn contains”, so long as the latter is “properly selected”.⁶³⁶ This assumption also worked the other way round, as can be seen in Morgan’s 1910 assertion that ‘wild’ flies merely need to be of unrelated stock, not necessarily undomesticated, in order for crosses with mutant strains to produce Mendelian ratios.⁶³⁷ Whilst this can be seen as an instance of Morgan’s instrumentalism, it is also I think reflective of a fundamental belief that the domestic-wild distinction does not matter so long as a lab lineage has a genome that resembles those typically found in nature enough that the same kind of desired results are produced. Claims of substitutability may, I remark, be

635 G.J. Bowles, ‘The Pickled Fruit Fly – *Drosophila ampelophila* Loew.’, *The Canadian Entomologist*, Vol. XIV, No. 6, 1882, p. 102.

636 William E. Castle, *Heredity in Relation to Evolution and Animal Breeding*, D. Appleton and Company, 1911, p. 83.

637 Thomas Hunt Morgan, ‘Sex Limited Inheritance in *Drosophila*’, *Science*, New Series, Vol. 32, No. 812, 1910, p. 121.

considered instances of believed strong metonymy between lab and nature, where the representative quality of wild types is thought to be based on shared properties (i.e. homologies) that allow one individual to be exchanged for another, as opposed to a weak metonymy where the relation between representer and represented is only one of analogy.

This metonymy between lab and nature is not, it has been insisted by sociologists of science of a constructivist persuasion, a natural arrangement but one built up and maintained over time through the gathering of allegiances and the demonstration of epistemic and interventional power. Even where shared properties allow for strong metonymy, there is need for alignment between lab and nature to allow for effective representation. As Bruno Latour observes “If there are identities between actants [e.g. lab wild types and their natural counterparts], this is because they have been constructed at great expense. If there are equivalences, this is because they have been built out of bits and pieces with much toil and sweat, and because they are maintained by force”.⁶³⁸ Steven Shapin has similarly discussed the relation between lab and natural objects as based on the establishment of equivalences where the lab object is in a relation of “standing for” the natural. In his favoured example, the behaviour of mercury-in-glass was incorporated into the set-up of Blaise Pascal’s Puy-de-Dôme experiment of 1648 to stand in for the weight of the atmosphere. In a similar fashion, inbred lab strains were incorporated into genetic experimental set-ups to stand in for the typical form of the species found in nature. But this aligning of lab and nature via metonymic intermediaries such as wild types remained secure, Shapin argued, only so long as it maintained social acceptance as credible – “scientific claims – only provided they achieve credibility – act as a shorthand for the natural world”.⁶³⁹ This need for credibility was one of the reasons why the wild type, as was pointed out by the Committee of 1921, must preferably have some relation to natural standards and cannot just be an arbitrary reference standard. But it also became a serious problem when reasons were brought forward to suppose that lab wild types were not properly representing typical variation in nature.

638 Bruno Latour, *The Pasteurization of France*, Alan Sheridan and John Law (Trans.), Harvard University Press, 1988, p. 162.

639 Steven Shapin, *Never Pure: Historical Studies of Science as if It Was Produced by People with Bodies, Situated in Time, Space, Culture, and Society, and Struggling for Credibility and Authority*, John Hopkins University Press, 2010, pp. 22-23.

The major challenge to the validity of lab wild types as representatives for nature came with the establishment of Theodosius Dobzhansky's evolutionary genetics. Dobzhansky was one of the later members of the Morgan group of fruit fly geneticists. Utilising Theophilus Painter's 1934 discovery that salivary chromosomal banding patterns correlated with existing linkage maps, Dobzhansky began to study chromosomes of wild individuals of *Drosophila pseudoobscura*, a relation of the *melanogaster* species which the Morgan group had first domesticated. Dobzhansky observed that certain features widely present in nature were absent in the lab. In particular he noted the absence in lab wild types of inverted chromosomes. Further investigation turned up that these were the so-called 'C-factors' that Sturtevant and Bridges had in the mid-1910s so vigorously bred out of the lab strains on account of their suppressing crossing over. Dobzhansky, wondering at this difference between lab and field types, theorised that these 'C-factors' might have an evolutionary advantage in preventing beneficial gene clusters from being broken up in crossing over. A feature of natural populations that better enabled their survival in the struggle for existence had been bred out for the human purposes of mapping chromosomes.⁶⁴⁰ Had Sturtevant and Bridges unknowingly negotiated away a non-negotiable with regards to retaining the naturalness of their lab wild type lineages? Did, and should, they care? Dobzhansky thought they should, but this was hardly his most alarming attack on the lab lineage-based method used by the Morgan school, for a second observation far more fundamentally undermined the claimed representativeness of lab wild types as instances of nature. Dobzhansky found much higher variation in chromosome banding patterns than anticipated even given his own less restrictive expectations of natural variation. This made him doubt that a standard strain in a lab could ever be properly representative of typical variation in nature, for his own findings suggested natural variation was not uniformly distributed. He thus observed that "The ability of the gene complexes... [in] chromosomes of wild *Drosophila pseudoobscura* to produce, through recombination, a great variety of new gene complexes disrupts the notion of 'normal' or 'wild type' chromosome, genotype, or phenotype".⁶⁴¹ He subsequently would declare the wild type concept 'refuted' and invalidated, asserting that it was representative of a form of "typological thinking, the roots of which go down to the Platonic philosophy, [and] is

640 Raphael Falk, *Genetic Analysis: A History of Genetic Thinking*, Cambridge University Press, 2009, pp. 112-113 & 161-163.

641 Theodosius Dobzhansky, 'Genetics of natural populations. XIII. Recombination and variability in populations of *Drosophila pseudoobscura*', *Genetics*, Vol. 31, No. 3, 1946, p. 287.

basically anti-evolutionistic”.⁶⁴² But despite his claims that the wild type concept could only exist “because of the reluctance of the human mind to abandon the idea of a finite number of static prototypes underlying the unmanageable... multiformity of the living nature”, he nevertheless admitted that “it is... [in non-polymorphic species] convenient for descriptive purposes to contrast mutant or aberrant individuals or strains with normal or wild type ones”.⁶⁴³

Dobzhansky thus, despite his critique, admits that ‘wild type’ strains can be utilised as a reference standard, and so perform their key role as controls for variance, and moreover that the validity of their use is circumscribed by natural facts, e.g. polymorphisms are a natural fact presenting a problem, implying that some ‘wild types’ can be more natural than others and thus that some can be better used in model organism systems to represent nature. And so, given their long-standing utility and necessity as part of model organism systems in much of genetics, as emphasised by Ankeny’s description (see Introduction) of them as the “first step in the underlying strategy [of classical genetics]”, it is not surprising that wild types have survived into the present day. Whatever their future, they have played a key role as a boundary object bridging the lab and nature. That they are open to critique on grounds of the artefactuality and lack of correlation with actual patterns of variation in nature is an inevitable result of their having to adapt to a role with dual demands in opposing directions. Richard Levins has argued in a 1966 paper that models in population biology must be based on trade-offs between competing demands of generality, realism and precision, qualities contributing to understanding, predicting and modifying nature respectively, models which combine all three qualities being difficult to fruitfully develop.⁶⁴⁴ Whilst here Levins is talking about mathematical models, I will argue that his insights on trade-offs equally apply to the case of wild type and its dual role as control for variance inside the lab and representative of the nature outside. The trade-offs I am interested in here are somewhat different from Levins given that the case here is one of relations of metonymy between wild organisms and laboratory strains, modelling a concrete phenomenon with concrete entities rather than at an

642 A. R. Cordeiro and Theodosius Dobzhansky, ‘Combining Ability of Certain Chromosomes in *Drosophila Willistoni* and Invalidation of the "Wild-Type" Concept’, *The American Naturalist*, Vol. 88, No. 839, 1954, pp. 83.

643 Theodosius Dobzhansky, ‘Genetics of natural populations. XIII. Recombination and variability in populations of *Drosophila pseudoobscura*’, *Genetics*, Vol. 31, No. 3, 1946, p. 287.

644 Richard Levins, ‘The Strategy of Model Building in Population Biology’, *The American Scientist*, Vol. 54, No. 4, 1966, pp. 422-3.

abstract mathematical level. In place of realism, precision and generality, I therefore suggest the related but distinct concreteness, tractability and representativeness (for 'representativeness', I assume a preference for homology based on shared properties over analogy based on similitude and symbolic relations, i.e. for strong over weak metonymy). I have already implied that concreteness has not traditionally been a problem for wild types. Representativeness and tractability, by contrast, have been in consistent tension. To make organisms more tractable to the needs of the genetics lab, they are rendered less representative of their species and of nature (i.e. they become less homologous, decreasing the legitimacy of claims of strong metonymy). But if they are not representative enough of nature, the knowledge derived through their experimental use cannot leave the walls of the laboratory and be applied to nature, which, after all was the original aim of genetic experiments, namely to elucidate the role of heredity and variation in the evolutionary process. Determining what can and what cannot be sacrificed to tractability, what is negotiable and what is non-negotiable, have thus been central to the use of wild types in classical genetics and beyond, for the very credibility of knowledge created in laboratories as applicable to nature rests on this very basis, that there are commonalities between nature and the lab, boundary objects bridging this divide.

In this thesis I have sought to show how the wild type concept first arose out of assumptions of difference between variation in nature and domestication, how it was that it became able to move from nature into the lab, and once in the lab how it has played a central role in the development and success of genetics. I have sought to discuss some of the epistemic, political and cultural influences which contributed to the establishment of a knowledge regime of scientific domestication in early modern Europe, and to illustrate how this allowed the opening up of the epistemic space within which the wild type concept would crystallise. I have also shown how the wild type crossed the threshold into the laboratory, concept following organism across the boundary. Once within the experimental systems of genetics, the wild type became an epistemic object pursued by researchers seeking to determine the true units of nature through experimental purifications and recombinations. I have then shown how reductionist interpretations of wild type eventually overcame more holistic understandings, in the process disintegrating the wild type as epistemic thing into its component genes, what I term the compositional notion of wild type. But the wild type

survived as a technical object, in the form of lab lineages, which continued to play an essential role within the model organism systems of classical genetics as representational stand-ins for nature, enabling knowledge produced in the laboratory to be more easily extrapolated to wild nature beyond. I believe that in writing this thesis I have helped provide wild type with a previously unrecognised history, one which is moreover part of the larger history of research strategies of scientific domestication. It is my hope that I have made a contribution to understanding the long-term cultural context of the strange efforts of early 20th century geneticists to bring exemplars of the wild into conditions of hyper-domestication. I moreover believe my narrative offers a good case-study for how scientific work can be highly epistemically productive despite many of the assumptions grounding its practices being fundamentally flawed or false (as was the case, for instance, with many of the beliefs that early 20th century geneticists utilised to legitimate bringing 'wild types' into the laboratory). I have further shown that the science of the domestic and wild has therefore not been cumulative in its production of knowledge but has instead been dominated by dialectical interactions at both the theoretical and material level between social economies and natural economies, and between human investigators and the nature they seek to understand, control and/or make useful. This interaction has not been a simple one-way domination of wild nature by human domestication strategies, as might be expected given the power over nature we traditionally ascribe scientists in the lab, but has rather been one characterised by negotiation, reflexivity, symbiosis and the co-construction of scientific knowledge. Finally, I believe I have shown that where the line between domestic and wild is drawn in life science research is non-obvious but that such demarcations have non-trivial repercussions in terms of designating what is a 'natural' (i.e. wild) behaviour or constitution, how resistant such wildness is to intervention, and therefore what strategies for knowledge production are acceptably naturalistic and henceforth scientific. The case of 'wild type' has helped us explore how historical changes in the demarcation of this boundary have both been produced and have been productive of new forms of knowledge production, and moreover shown that many disputes within the life sciences have centred on where this boundary should be drawn. Having established its role in the history of the life sciences, it is a logical step that one of my future projects will be to extend analysis to the role of the domestic-wild divide to contemporary scientific research. I hope therefore that this thesis will be the starting point for a more extensive future research programme.

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