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**The Hereditary Hourglass. Genetics
and Epigenetics, 1868-2000**

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PART I

A BROADER CONCEPTION OF HEREDITY

INTRODUCTION
THE HEREDITARY HOURGLASS:
NARROWING AND EXPANDING THE DOMAIN OF HEREDITY¹

Ana Barahona, Edna Suárez, and Hans-Jörg Rheinberger

1. Historical and scientific metaphors

Over the past four decades the study of the phenomena of heredity has caught the attention of historians, sociologists, and philosophers of the life sciences. Undoubtedly, this is reflected in the progress made to unravel the conditions under which the very idea of heredity came to fruition (Churchill 1987; López-Beltrán 2004; Müller-Wille and Rheinberger 2007), as well as in the informed accounts of different practices, tools (be they conceptual or material), and institutions identified with this field of inquiry.² The challenge for the student of science is the same as when confronting other scientific endeavors, namely, the question of how to deal with the continuous research on the particularities of cases and specific problems, while simultaneously seeking to produce a broader picture on the field of heredity, within which isolated events acquire new meanings.³

The questions students of science choose to ask may require both types of answers, the local account and a more extended context. To keep them balanced has been our goal in this project. In order to combine both strategies we adopted the image of the hourglass or sand clock as a metaphor for the shape of the changes that have taken place in the wide-ranging studies of heredity – and some of its implications for the study of evolution –, from the second half of the 19th century up to the present day.

The image of the open extremes and the narrow neck seems appropriate to represent the transition between the rich approaches on heredity recognizing the role of the environment and epigenetic causes during the second half of the 19th century. Then came the narrow understanding

¹ In 2007 the Max Planck Institute for the History of Science (MPIWG) and the Universidad Nacional Autónoma de México (UNAM) started a collaboration under the auspices of the Proalmex program, supported by the DAAD (Deutscher Akademischer Austauschdienst) and Conacyt (Consejo Nacional de Ciencia y Tecnología). Thanks to this project, four German scholars have realized research stays in the Mexico City campus of UNAM, mirrored by the research stays of four Mexican PhD students and four Mexican scholars between 2007 and 2008. As part of this project, we organized the Conference “The Hereditary Pendulum: Narrowing and Expanding the Domain of Heredity,” at the Instituto de Investigaciones Filosóficas, UNAM, on October 1-2, 2008. Our collaboration has privileged interdisciplinary approaches on genetics and epigenetics: cultural studies, philosophy, sociology, and the history of science have contributed to a complex and proliferating view on this field. The present preprint is a result of the fruitful exchanges between participants of this project.

² The number of studies in this field is enormous. Some of the most influential include general narratives like Dunn (1965), Bowler (1989) and Olby (1985). On tools and practices, see Fortun and Mendelsohn (1999), Kohler (1994), Gaudillière and Rheinberger (2004); on institutions, Harwood (1993), Gaudillière and Löwy (2001).

³ This problem has been addressed by a number of scholars, after the predominance of case-studies in the history of science since the mid-1980s. See, for instance, the special section on *Isis* (2005, volume 96) featuring articles by Kohler, Findlen, Kaiser and Shapin; or the efforts by John Pickstone to give an extended account on the “ways of knowing”, using the category of “working-practices” (2001).

of heredity around genes following the rise of the discipline of genetics at the turn of the 20th century. And, as the last century came to a close and the 21st century began, we witnessed a new widening of approaches searching for the causes of heredity, characterized by a broader preoccupation for development, evolution, the environment, and even culture. As we will see, however, the metaphor of the hourglass is – like all metaphors – just as good as the questions and problems it has helped open.

Charles Darwin's *The Variation of Animals and Plants under Domestication*, published in 1868, serves well as a point of departure for this historical image. The context of hereditarian phenomena Darwin was confronted with was rich, if not messy regarding the study of what a century before was contained within the older question of *generation*. And the question of generation as posed in the 18th century did not have the historical connotation that the concept of heredity would acquire during the 19th century.

During this time, as Ohad Parnes (2007) has shown, the same word, *generation*, began to be used to refer to “the act of organismal creation and for a group of individuals sharing nothing but a vaguely defined age” (p. 316). According to Parnes, the conceptualization of populations in terms of generations gave way to a new order of heredity. In this new order, the idea of “transmission” (of knowledge, education, biological traits, and diseases) conveyed temporal and spatial coherence to groups of individuals. Taking together this notion of generation, with the medical contexts in which the metaphor of heredity was *imported from law and became biologically reified* (López Beltrán 2004; 2007), we are faced with the radical cultural changes which resulted in a more historical view of organisms during the 19th century. It is in this context where the acquisition, transmission, and development of traits in organisms took on a new biological natural meaning.

The recognition of different and heterogeneous causes of heredity in the latter part of the 19th century was replaced by a narrower view at the beginning of the 20th century, in parallel with the slow but firm recognition of Mendel's laws, and giving way to what has been called the “century of the gene” (Fox Keller 2000). The emphasis on genes in classical genetics, which culminated in the molecular and informational gene associated with DNA, had a lasting effect on theories of heredity which also influenced the fields of evolution, taxonomy, and medicine. At the end of the 20th century, however, the interest in epigenetics, regulation, and complex bodily and environmental interactions has witnessed a remarkable increase. Nowadays it seems that the late 19th century concerns of anthropologists, developmental biologists, novelists, and psychologists could provide inspiration for the interpretation of some recent developments in the life sciences.

Though we are using a metaphor that specifically focuses on the temporal dimension of science, metaphors have been notorious as cognitive resources in the understanding of hereditary phenomena at large. As already mentioned, the very notion of heredity was metaphorized when imported by medical doctors from the legal to the biological sphere. Staffan Müller-Wille and Hans-Jörg Rheinberger (2007) have argued that this metaphor, with its rich semantics, provided enough room for the creation of a particular *epistemic space* for the study of hereditary phenomena. This epistemic space depends “on a vast configuration of distributed technologies and institutions connected by a system of exchange: botanical gardens, hospitals, chemical and physiological laboratories, genealogical and statistical archives” (p. 25), connections that were made possible, as Müller-Wille and Rheinberger claim, by the rise of capitalism and the bourgeois culture.

As we shall see in the papers that follow, the metaphor of the hourglass cannot accommodate the pace of events in different fields and geographic settings. Nevertheless, it has proved fruitful enough, as the articles included in this volume attest. Perhaps the most important conclusion from this project is that, regardless of all the attention devoted to the studies of heredity in the field of science and history of science in the last decades, there are still many unexplored areas and many questions to ask. It could even be, as some participants of this project remarked, that to some

extent the hourglass image might be a historiographical artifact reflecting this situation. In any case, we are still suffering the effects of decades of historical research centered on a few actors and fields, most of them located in the American and British scenarios. Also, in general, there has been a tendency to reduce the history of biology – not only of heredity – to that of genetics, and this fact affects the image of heredity we have today. Moreover, until recently, there had been a concentration on mainstream approaches (like the history of concepts or institutions), while other approaches have been marginalized (for instance, cultural or philological studies). As these reevaluations continue, they will come to provide a refurbished image of the entire field.

What can we conclude from the previous studies that has not been said by other, broader and more ambitious historical accounts on the field of heredity? To counteract premature generalization, there might be a plurality of hourglasses, one for each field of biological inquiry related to heredity. In any case, we identified fields of inquiry that are not in phase with the expansion of the genetics-epigenetics hourglass: most notably taxonomy, where molecular approaches based on genetic differences have been taking the lead at the beginning of the 21st century. But this is also the case of medicine and of the public perception of the medical applications of genetic approaches, to which we will come back later.

Also, there might be a plurality of hourglasses regarding different geographical and institutional scenarios. This fact is best illustrated by the predominance of developmental genetics in Germany and the slow reaction that historians of biology have had to acknowledge it. There, and well into the 1940s, the study of phenogenetics, physiological genetics, and cytoplasmic inheritance provided a setting not particularly well suited for Mendelism and the chromosome theory, as shown already some time ago by Harwood (1993). The same happened in some parts of the United States and France (Sapp 1987), but research on other scenarios has progressed at a slow rate, despite the interesting lessons we have drawn from those studies, as illustrated by the legal framework put on consanguineous marriages in the late 19th and early 20th century México (González and López-Beltrán, this issue).

Heredity, thus, has been neither a monolithic concept nor the monopoly of a single perspective, even at the height of genetic reductionism. This fact has been well recognized by historians of biology, but it was important to analyze its conceptual implications for the questions posed by scientists in different fields, particularly in anthropology and evolution. On the other hand, for all its diversity, the approach of cultural studies made us realize that the centrality of heredity, in the end, has to do with the very human – intimate, we may say – preoccupations it deals with (see the papers by Barahona, González, González and López, Vackimes, Vedder, Wille, this issue). “It’s all about humans,” seemed to be the conclusion at which we all arrived, once again, when we tried to focus on the centrality given to some questions at very particular moments: the centrality of heredity – of character, of diseases, of virtues, of weaknesses – in late 19th century literature, but also in experimental embryology and in the study of variation; or the meaning of sex and sexual dimorphism at the beginning of the 20th century. Certainly this is a field of historical inquiry in need of much attention.

Last, but not least, we reflected upon the implications of our studies for a more general audience. During our discussions, the public understanding of science emerged as a central issue. We see this “public understanding” of heredity both as a condition that shapes the questions and cognitive resources of scientists and, as a result, of the interaction and social responsibilities of scientists. Metaphors, here again, play a crucial role as representations (and mis-representations as well!) for the broader public. We must not forget that it is this public sphere where cognitive resources are received and transformed into research programs. Ludwik Fleck reflected many decades ago on the exoteric nature of the resources used by scientists in the construction of models and theories.

Moreover, in democratic societies the question of the public understanding of science is paramount. The hourglass seems to be lagging behind in the public sphere. The more scientific research is approaching the complexities of genomes, of the regulatory networks involved, and the many factors at work (geography, history, life cycles) to explain the inheritance of traits and diseases, the more geneticist and deterministic explanations are the most popular ones available for the broader public and, even more importantly, for the marketplace.

2. *A bird's eye view of the papers*

The papers that follow aimed to give answers to the following questions:

1. How appropriate (or, on the contrary, how inaccurate) is the image of the hourglass clock to understand the development of theories of heredity and evolution within this 150-year period? Is it an image suitable for some cultural fields or disciplines but not for others?
2. What is specific about the studies and approaches on “nurture” and “epigenetics” of the 19th century, and what is specific about their early 21st century versions?
3. What are the lasting effects of the 20th century focus on genetics in contemporary developments on heredity and evolution, and on the public understanding and perception of these processes?

Despite the diversity of views and the broad range of issues the papers can be arranged as follows: In the first part we have included historical contributions that deal with perspectives and subjects dealing with heredity in a broad sense (before the 20th century narrowing of the hourglass took place). Coincidentally, the papers include themes and subjects that have been eccentric to the mainstream history of genetics and heredity. Stefan Willer argues that a philological analysis can still be of help for an historical account of the relation between genetics, epigenesis, and epigenetics. “While ‘epigenetics’ ... designates studies that challenge the narrowing of heredity research to the DNA, ‘epigenesis’ refers to developmental theories which are to be traced back to Aristotle, Galen, and William Harvey.” Despite these differences, Willer shows how the early geneticists such as Morgan and even Weissman, made reference to epigenesis either to defend or to attack the material basis of life which was related to the preformist theory.

Meanwhile, Keith Benson sheds light on the diversity of evolutionary views in the late 19th century in the United States, the country which more openly received Darwinism. Through the analysis of William K. Brooks’s research at John Hopkins University he points to the early attempts to incorporate a theory of inheritance and variation which was in line with Darwinism *and* Weissmanism, and opposed to mainstream neo-lamarckist approaches in North America. Implicit in Benson’s paper is the assessment that Brooks’s has been undervalued and unfairly treated by historians of science, given that his notable students include such notorious scientists as T.H. Morgan, and E.B. Wilson, and that he represents early attempts to link Darwinism and Weissmanism.

González Soriano and López-Beltrán show that, after the noun heredity was popularized by French physicians after the 1830’s, psychiatrists, criminologists, and physicians in general, had in mind that heredity was a medical, social, political, and even moral and historical fact, that could be studied through empirical observation. At the end of the 19th century France became an external political and cultural reference, “and French medical thought was central for the ambitious Mexican elites of physicians, fighting to gain positions and effectiveness in a troublingly poor and racially stigmatized society.” In this context the introduction of a medical discourse raising the question of the degenerationist effects of consanguineous unions in Mexico in the late

19th and early 20th centuries, promoted that marriages were used as tools for governmental and social ascent.

Ana Barahona's paper deals with hereditary ideas of medical doctors in the late 19th century in Mexico. She shows that before the introduction of genetics in this country in the 1930's, physicians confronted practical problems and diseases using theoretical, clinical, and practical tools within their reach, including the use of pedigrees for understanding the transmission of pathological heredity. Some conceptions of heredity proposed that physical traits as well as moral traits, can be transmitted, including malformations or defects, from one generation to another. Clinical, therapeutic, and prophylactic tools were designed and introduced in Mexico for the study, treatment and prevention of maladies, with an important influence of French conceptions.

The second group of papers focuses on the narrow conception of heredity. Two of them from a historical point of view, and two of them from a philosophical perspective. In their paper on two different models of gene regulation (the *operon* model of Jacob and Monod, and the model of *batteries* of Britten and Davidson), Vivette García and Edna Suárez make the historiographical point that reconstructing the history of "regulation" from a gene-centered perspective does not allow us to see that evolutionary and developmental concerns played a very different and crucial role in models of gene regulation for higher organisms (eukaryotic cells). The landscape of gene regulation is narrowed, and many theoretical and experimental perspectives are left out of this monophonic history, narrated under the resources of the history of molecular biology. Moreover, García's and Suárez's paper seeks to illustrate the role of fruitful metaphors in the development of research programs (like the cybernetic metaphors involved in the operon model), compared to the limited role of non-productive metaphors, produced within the framework of a theoretical approach, devoid of experimental input, such as the metaphor of "batteries" in Britten's and Davidson's research in the late 1960s and early 1970s.

Also focused on metaphors, Christina Brandt describes how the language of *re-programming* has become a key conceptual tool in the fields of stem cell research and developmental biology. "Re-programming" first appeared in the field of animal cloning, providing a fruitful metaphor within the discourse regime of "information". However, in a striking difference to theoretical uses of metaphors, John Gurdon and his colleagues used "reprogramming" as a way to technically manipulate cells, within the rich experimental culture of the field of cell cloning. From the start, then, reprogramming provided a powerful tool for experimental interventions in the study of regulation and development of higher organisms. Taken together, the papers of Brandt, and García and Suárez, deal with a subject that has been absent or at least under-represented in current reconstructions of the current field of evo-devo: the development of tools and experimental approaches to evolution and development.

Sergio Martínez's paper proposes to think on cultural evolution without restraining models of "culture" to the transmission of ideas and memes, or without extrapolating the mechanisms of biological inheritance to modes of social organization. He sustains that Darwin's idea is more fruitfully applied when we think of the evolution of lineages of artifacts, norms, and representations structured in scientific practices, than when we think of information "mentally encoded". For Martínez, then, the question is how do "cultural items" of the first type get the stability that matters for explaining the cumulative change that is distinctive of cultural processes?

Maria Kronfeldner's paper also deals with the relation between culture and evolution. She first goes through the history of ways in which culture and evolution have been claimed to be analogous (from Darwin and Spencer to William James and Ernst Mach), and then goes to contemporary models of cultural change, which she groups into memetics and dual-inheritance theories. She does this to provide an introduction to the different ways in which the relationship between culture and biology has been understood and to claim that historically there have been many more

variations to the way in which scientific fields have been fruitfully integrated and separated from each other, so as to constitute new research areas. She illustrates her point by focusing on the origins of cultural anthropology, the role played by Alfred L. Kroeber and his defense of so-called “cultural determinism”. Kroeber, Kronfeldner sustains, made the epistemological and pragmatic decision to stay within the cultural dimension alone (as in his aphorism “culture explains culture”) so as to provide a firm boundary from other research fields, including biology, where “hard inheritance” had stabilized with the rise of genetics. With this movement, Kroeber was able to integrate cultural anthropology, by separating cultural inheritance phenomena from biological or genetic heredity.

The last group of papers focuses on a subject of particular concern for students of science: the public perception of genetics. Ulrike Vedder’s essay focuses on the figure of the bachelor in science and literature. She points out that in the 19th century literary discourses, the bachelor epitomized decadence, degeneration, and decay, because of his refusal to start a family and have children. It is this lack of engagement and infertility that precisely brings up the discussion about the end of the human race and calls nature into question. According to Vedder, in many 19th century discourses concerned with genealogy and the family, including medicine and psychology, the bachelor became “the ideal test subject for normalization, insofar as bachelors are considered to stray from the norm.” The bachelor’s infertility constitutes an attack on the power of inheritance and naturalism, as established at the end of the 19th century.

“How does the hourglass metaphor explain the misunderstanding of scientific principles at a time when there is so much information being constantly fed to the public by the media?” Sophia Vackimes tries to answer the question analyzing the hereditary ideas contained in movies that generally reinforce false notions by constructing stories that feed public mistrust and paranoid visions of the world. Vackimes argues that, even though new scientific information was unintelligible to scriptwriters and directors, today DNA has become the focal point in many films, but unfortunately its content in films is increasingly shrouded in what can be described as “DNA mystique”. This is, “films rely on the reduction of biological information in favor of oversimplified content,” leading to simple representations that suffice to explain the findings of modern science. Vackimes concludes that as scientific information becomes more complex and specialized, genetic information in the movies analyzed emphasizes a-historical, unscientific, and culturally contradictory positions, creating a climate of misinformation.

On her part, Matiana González-Silva analyzes the role played by the press in the process of consolidating the genetic approach on human biology and disease in the Spanish context. Analyzing the influential newspaper *El País*, González-Silva tries to show the complexities of the interrelation of scientific journalism with ideologies, disciplinary interests, and broader social and political transformations. She also states that the media needs to be taken into consideration when writing the history of contemporary science. “How did *El País* present human genetics to the Spanish public from its very constitution in 1976 to the years that followed the publication of the human genome sequence?” The positive role played by the newspaper for the popularization of genetics in Spain in the 1990s when the Human Genome Project was launched, led to the acceptance of genetics and its premises, that contributed to consolidate biology in that country. For González-Silva, the media play an important role in the course that science takes in a particular historical context, influencing the laws, funding, and public legitimacy of science.

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‘EPIGENESIS’ IN EPIGENETICS:
SCIENTIFIC KNOWLEDGE, CONCEPTS, AND WORDS

Stefan Willer

Introduction

Epigenetics has been defined many times, and in different ways. Maybe the importance of the epigenetic shift in contemporary biology is best expressed in a negative way, through that which it denies – namely the role of DNA sequences as the sole agents of heredity. In general, then, epigenetics deals with the variety of mechanisms residing outside the DNA and that are in one way or another regulating the expression of genetic information.

The study of epigenetic inheritance as it has been promoted in biology and biomedicine for several years now, includes both cell-to-cell transmission of epigenetic variants during an individual’s lifetime *and* trans-generational inheritance. In the first, narrower sense epigenetic research focuses on the mechanisms that cause the stable change of regulation and expression of genes, and asks how this state can be passed on from cell to cell. From the second, wider perspective epigenetics relates to the study of the effects that were environmentally induced in parents and are then transmitted for one or more generations of descendants. It is this second (stronger) version of epigenetic inheritance that I would like to position and to question in this paper. For, historically considered, it is these trans-generational aspects of epigenetic inheritance that have played an important part in conceptualizing the relation between heredity and development.

But what exactly does it mean to ‘conceptualize’ something in the life sciences? The specific aim of my paper is to delineate in a *philological* manner the ways in which concepts are generated not only out of laboratory work and empirical data, but also out of linguistic – grammatical and semantic – processes. I am interested in the literality of scientific texts, in their wording, in their rhetorical disposition of knowledge. *Dis-position* in the literal sense of the word means change of place. Scientific concepts, as they occur in discourses and texts, are no stable entities, but they are historical in a radical way. Thus I will try to show that the history of the term ‘epigenesis,’ and of the *use* of this term, can indeed contribute to inquiring the current renaissance of epigenetic research in biomedicine.

In fact one could argue that ‘epigenesis’ and ‘epigenetics’ are actually *two* concepts not to be mixed up. While ‘epigenetics’ (as I have said in the beginning) designates studies that challenge the narrowing of heredity research to the DNA, ‘epigenesis’ refers to developmental theories which are to be traced back to Aristotle, Galen, and William Harvey. In 18th century embryology, ‘epigenesis’ was the key-word for conceptualizing the gradual self-organization of new life by means of an essential power or potency, provided by the generative matter of both parents – in contrast to theories of preformation with their concepts of pre-structured, pre-existing germs which were not to develop, but only to unfold (cf. Gasking 1967; Roe 1981; Roger 1993).

Anyway, the resemblance of the two words, ‘epigenesis’ and ‘epigenetics,’ from which derives one and the same adjective ‘epigenetic,’ is far from being accidental. Indeed, the history of ‘epigenesis’ is certainly part of the pre-history of ‘epigenetics.’ In my paper, I would like to give some evidence for the observation that the matter of epigenetics in the rise of modern theories of heredity has been, and not in a marginal way, negotiated in the seemingly outdated terms of ‘epigenesis’ and its counterparts, especially ‘preformation’ and ‘evolution.’ By showing these cross-

references I will try to find out how the use of the words ‘epigenesis’ and ‘epigenetics’ interacts with the history of knowledge about inheritance.

1. *Preformation vs. Epigenesis around 1900*

In his 1910 paper “Chromosomes and Heredity,” American zoologist and later leading geneticist Thomas Hunt Morgan drew a parallel between the content in theories of heredity at the beginning of the 20th century, and the key debate of 18th century embryology of preformation versus epigenesis. Obviously, this comparison touched the relation between heredity and development. In the first sentence of his paper, Morgan already made quite a brisk statement about this relation: “We have come to look upon the problem of heredity as identical with the problem of development.” (Morgan 1910, p. 449) Retrospectively, this assertion comes a little bit as a surprise. For it was one of the main concerns of rising genetics around 1900 to conceptualize heredity *without* thinking of anything that regarded the temporality of an organism, be it growth, nurture, or age. Indeed, Morgan in his paper tried to incorporate development into heredity rather than to reconcile both views. But still, his initial statement somehow seems to be a last attempt to save a range of knowledge about heredity that was about to become neglected.

Morgan, who by 1910 was not convinced by Mendelism and the chromosome theory, obviously understood the identity of heredity and development not as an ontological statement, but as an epistemological problem. He addressed it in the very contention that marked the starting point for his reflections. There are, as Morgan states, two ‘schools’ in modern theories of development and heredity.

The modern literature of development and heredity is permeated through and through by two contending or contrasting views as to how the germ produces the characters of the individual. One school looks upon the egg and sperm as containing *samples* or *particles* of all the characters of the species, race, line, or even of the individual. This view I shall speak of as the *particulate theory of development*. The other school interprets the egg or sperm as a kind of material capable of progressing in definite ways as it passes through a series of stages that we call its development. I shall call this view the *theory of physico-chemical reaction*, or briefly the reaction theory. The resemblance of this comparison to the traditional theories of praeformation and epigenesis is obvious, and I should willingly make the substitution of terms were it not that the terms praeformation and epigenesis have certain historical implications, and, as I wish to emphasize certain things not necessarily implied in the historical usage, I prefer descriptive terms other than these overladen with so many traditions. (Morgan 1910, p. 450)

Nevertheless, Morgan keeps blending the ‘overladen’ historical ‘usage’ into the terminological ‘usage’ of his own paper – not by chance, but explicitly. This is especially true for his view of the particulate theory which is the one that “ascribes everything to the chromosomes” (Morgan 1910, p. 453).

The original conception of praeformation postulated an actual material embryo in the egg; epigenesis denied the existence of that embryo, and justified its denial. Here surely there was a real distinction. But the problem has refined itself in modern times. We no longer look for an actual embryo praeformed but we look for samples of each part, which samples by increasing in size and joining suitably to other parts make the embryo. This is modern praeformation. (Morgan 1910, p. 452)

But still Morgan does not conceal his uneasiness with the theory’s assumption “that unit characters in heredity are praeformed” and with its “strong predilection towards locating their indivisible units in the chromosomes.” (Morgan 1910, p. 454) When Morgan finally rejects this theory – after

having weighed it very carefully – and confirms the other option, his ‘reaction theory,’ he renews the usage of the historical terms:

Our general conclusion is, therefore, that the essential process [...] is a reaction or response in the cells, and is not due to a material segregation [...]. The general point of view that underlies this conclusion is epigenetic, while the contrasting view, that of separation of material, is essentially one of praeformation. (Morgan 1910, p. 479)

It was no original idea of Morgan’s to recapitulate the debate of preformation versus epigenesis when trying to define an up-to-date relation between heredity and development. Around 1900, biologists seemed rather convinced that it makes sense to trace current biological problems back into history. In 1894 already, German cytologist Oscar Hertwig had written a book named *Präformation oder Epigenese* in which he articulated his scepticism about assuming a material hereditarian substance with a fixed location. This kind of conjecture, Hertwig wrote, proceeded “like its predecessor, the theory of preformation of the eighteenth century.” (Hertwig 1894/1900, p. 140) Thus, for Hertwig, the opposition between preformation and epigenesis was definitely a problem of modern biology. In this vein, also William Morton Wheeler in 1899 discussed the current problem of reconciling what he called “pre-determination in the germ” on the one hand and “elaborate morphological structure” on the other (Wheeler 1899, p. 284) in an article featuring the founding father of 18th century epigenesis theories, Caspar Friedrich Wolff.

All of these attempts to object to location theories by linking them to preformationism were likely to remind their readers – although in a subliminal way – that the debate of preformation versus epigenesis also had its metaphysical and ideological share. In its historical context, the contention was not only about more or less pertinent embryological models, but also about the question whether individual growth, development, and character were pre-determined ever since the universe had been created, or whether it happened due to original, self-organizing forces. Thus, making reference to preformationism around 1900 certainly had a polemic point in it.

Nevertheless, this kind of reminiscence was of essential importance when it was about focussing what the modern contention was actually about. This becomes even more evident when we take a look at the publication that had induced the critical statements I have cited so far: namely to August Weismann’s landmark book about the germ plasm from 1892 (*Das Keimplasma: Eine Theorie der Vererbung*). In his introduction, Weismann referred to the old debate, but his argument was directed against *epigenesis*. In order to definitely turn down the idea of an inheritance of acquired characteristics and to reduce heredity to the ‘continuity of the germ plasm,’ Weismann rejected epigenetic development as such:

I was looking for a germ substance able to produce the organism by epigenesis and not by evolution [which represents “preformation” here]. Many attempts in this direction were made, more than once I believed that I had succeeded, but, when further examining the facts, always had to admit that indeed I failed. Thus I finally realized *that there is no such thing as epigenetic development at all*. In the first chapter of this book one will find a formal proof of the reality of evolution. (Weismann 1892, p. xiv, my translation.)

This, of course, was no denial of the fact that an embryo grows and develops, but the refusal of the idea that anything essential could be added to an organism after fertilization (or “amphimixis,” as Weismann called it). According to Weismann, there was full evidence that an organism could not become more *complex* or more *complicated*, once its germ substance had been generated. Instead, Weismann stated, development is all about ‘unfolding’ – which means ‘evolution’ in the 18th century meaning of the word. Before Darwinism, ‘evolution’ was one of the keywords of preformation theories. In that sense, Weismann maintains the “reality of evolution” (“die Wirklichkeit der Evolution”) against the impossibility of epigenesis (Weismann 1892, p. xiv). So,

in Weismann, it is *epigenesis* that plays the mystifying part whereas *preformation* is the rational guiding model – at which it is remarkable that Weismann prudently avoids using the word ‘preformation’ and speaks only of ‘evolution.’

The matter is further complicated by the fact that Weismann’s use of ‘evolution’ is not restricted to its 18th century meaning. Instead, he is quite eager to affiliate his argument to Charles Darwin. Focussing on the generation theory in Darwin’s later work *The Variation of Animals and Plants under Domestication*, Weismann is mainly interested in the concept of ‘pangeneses,’ that is, Darwin’s idea of ‘gemmules’ dispersed through an organism which contain the predispositions of the respective parts of that organism. Weismann claims the pangeneses theory as a first stage of his own theory of the germ plasm and contrasts it with Herbert Spencer’s concept of the so-called ‘physiological units,’ that is, small particles as operators in the hereditary process which Spencer also supposed to be capable of storing information about use and disuse and, consequently, of motivating the inheritance of acquired characteristics. Weismann concludes that the disadvantage of Spencer’s view lies in its merely ‘epigenetic’ fashion:

Spencer’s *units* are carriers of the entire species characteristics by way of their complicated molecular structure [complicirter Molekularbau], whereas Darwin’s *gemmules* are dispositions [Anlagen] of singular cells, which differ from each other. Spencer’s theory is epigenetic, whereas Darwin’s is evolutionary, and this is why Darwin – in my opinion – is superior to Spencer. (Weismann 1892, p. 9, my translation.)

Here, Darwinist evolutionary theory is obviously not connected to long term changes of species and to Darwin’s epoch-making objection to the constancy and continuity of life on earth. It is rather the opposite: Darwinian evolution – in the specific way Weismann cites it here (that is to say: as a theory of generation) – serves as an argument *in favour of* continuity, namely the continuity of the germ plasm. Historically, this may be regarded as an early transfer point from Darwinian to Neo-Darwinian thought.

To sum up so far: In Weismann, the usage of the old debate of epigenesis versus preformation serves to sharpen the presupposed dichotomy of heredity and development, in the sense of the first genetic ‘dogma,’ which is the strict distinction between germ cells (heredity without development) and body cells (development without heredity). Morgan, on the other hand, uses the debate of epigenesis versus preformation in order to *challenge* this dogma by assuming an identity of heredity and development which is *then* to be differentiated.

2. Genealogy and Validity of Scientific Terms

Altogether, the formation of genetics was preoccupied in many ways with epigenetic affairs. Far from having a terminology at its disposal, genetics as a modern discipline had to ‘dis-‘pose’ – to shift, to translate and to transform – the terms that were at hand and originally denoted a much vaster kind of knowledge than the one genetics was developing into. Leaving the matter of epigenesis behind for a moment, we see that this finding is especially true for the word ‘gene’ itself. It was coined by Danish botanist Wilhelm Johannsen in his book on *Elements of Heredity*, originally published in German (first edition, 1909). For Johannsen, ‘gene’ was not the designation for a precise entity. Rather he used it as a maximum of vagueness. ‘Gene’ was coined as the name of a ‘something’ contained in the germ cells and responsible – but only more or less responsible, as Johannsen maintained – for producing the character of the newly engendered organism. For Johannsen, the word ‘gene’ was, as he stated, “free from any kind of hypothesis” about what was really going on between fertilization and the somehow ‘characterized’ organism: “*Das Wort Gen ist also völlig frei von jeder Hypothese.*” (Johannsen 1909/1913, p. 143)

Regarding the literality of scientific notions, it is important to keep in mind that the word 'genetics' had been coined three years earlier than the word 'gene,' by William Bateson. This is to say that 'gene' is not the root of 'genetics.' Still, it is neither the other way round: 'gene,' as a word, has not been derived from 'genetics,' as the denomination of a scientific sub-discipline. When Johannsen suggested the word 'gene,' he did not refer to Bateson's 'genetics' but – just as Weismann had done – to Darwin's construct of 'pangenes.' Darwin, in this already mentioned generation theory developed in his 1868 book *Variation of Animals and Plants under Domestication*, had supposed the 'pangene' to be operative in every act of generation. Only Johannsen judged Darwin's term too complicated, as it was a composite which had the greek *pan* in it (signifying 'all'). To denote the simple idea of a transmitted 'something,' Johannsen said, a simple word was needed, a word without the meaning of 'all' in it, and so he proposed to simply utilize, or, as he said, to valorize ("verwerten," in German) the isolated second part of Darwin's word: "aus Darwin's bekanntem Wort die uns allein interessierende letzte Silbe 'Gen' isoliert zu verwerten." (Johannsen 1909/1913, p. 143)

I do not take it to be irrelevant that Johannsen speaks of valorization when it is about utilizing Darwin's terminology. Thus he not only directs to utilization, but also to value and validity. Speaking of validity, I hope to be able to specify my original intuition of an interrelation between word and concept by drawing a distinction: the distinction between the conceptual validity of a word on the one hand, and its etymology – thus, the genealogy of its formation – on the other. When it is about etymology, we mostly take for granted that complex words derive from simpler ones and that the further we go back in time, the simpler the entities turn out to be, until we finally find the root, the 'radical' of the word. But when we look at the validity of Johannsen's newly invented word 'gene,' we find that the radicalization of the word is artificial and that it is guided by the attempt to single out the root of the *concept*. This coincides with the finding that the noun 'genetics' is far younger than the adjective 'genetic,' which served to signify processes of *coming-into-being* in a very broad sense, long before it derived into the name for a research programme and a scientific discipline.

3. 'Genetic' Thinking around 1800

To give some evidence for the wideness of knowledge the word 'genetic' once covered, I would like to go back to the late 18th century when there was a kind of boom of 'genetic' thinking. This is especially true for German philosophy, for authors like Schelling or Humboldt. The trend was largely due to the success of epigenetic theories in embryology and physiology (especially Caspar Friedrich Wolff and Johann Friedrich Blumenbach). What made those theories specific in contrast to any kind of preformationism, was their double emphasis on the importance of sexual reproduction and on the role of self-organizing development. This was why *generation* in an epigenetic view could become closely linked to *genius* and furnish a leading model for philosophical and poetical productivity. "Dichten ist zeugen," 'writing poetry is engendering,' as German poet Novalis expressed it in the utmost metaphorical conciseness. (Novalis 1978, p. 323; cf. Willer 2007)

As I want to keep following the problem of concept and word and not to exaggerate the historical detour, I will restrict myself to some considerations of just one writer who made exemplary efforts to outline the extent of the genetic principle: Johann Gottfried Herder. In his huge work from the 1780s called *Ideen zur Philosophie der Geschichte der Menschheit (Reflections on the Philosophy of the History of Mankind)*, Herder explicitly referred to Wolff when he tried to characterize the operations of the 'genetic force' which he supposed to be effective throughout nature and history. In a passage that evokes, in an almost hymnal style, the way the chicken grows

inside the egg, Herder connected the gradual and self-organized coming-into-being of the animal to what he called the “inner genius” of man’s existence (Herder 1989, p. 270-273). As I have just suggested, this connection between ‘genius’ and ‘genetic’ is anything but accidental. Herder’s text as a whole is passed through by *gene*-words: ‘genesis,’ ‘genetic’ and ‘genius’ appear again and again, often connected to the German word ‘Geschlecht,’ which, in an Aristotelian fashion, always means ‘genus’ and ‘sex’ at the same time: the whole of the human race *and* the sexuality which is the basis of its existence and procreation.

Regarding the generosity of Herder’s terminology as well as his affinity to epigenetic theories, it is a little surprising that the word ‘epigenesis’ occurs just once in the extensive text – and that it is not affirmed but judged rather sceptically. To speak of ‘epigenesis’ was, according to Herder, not a proper use of words because it evoked the idea that the parts of a growing organism accrued to it from its outside (“[dass] die Glieder von außen zuwüchsen”). This, he stated, was just as improper (“uneigentlich”) as to say, like the preformationists did, that any growth was just about the development of pre-existing germs. Instead, Herder concluded, one should only speak of ‘genesis.’ His definition of ‘genesis’ in the same passage, however, sounds very epigenetic: the working of internal forces which make themselves visible in un-organized matter. (Herder 1989, p. 172)

So, Herder’s objection is not about Wolff’s concept but about the name of this concept. The critique of the improper use is indeed a critique of semantics and of style; especially it deals with the status of metaphorical expressions. It is indeed remarkable that Herder tries to solve this metaphorological problem by deleting the prefix ‘epi-’. The trick is quite similar to Johannsen’s invention of ‘gene’: When it is about something elementary, you need simple words. ‘Epi-’ evoking for Herder something additional, outward, or supplementary, it has to be left out because the essential force is supposed to come from *within*.

Nevertheless, for Herder terminological reduction is not the only way to cope with the problem. The other one is translation. For it is in this passage that Herder transfers the greek word ‘genesis’ into the German word ‘Bildung’ and denotes the way in which force organizes itself in matter with a neologism related to ‘Bildung,’ namely with the verb “zubilden” (Herder 1989, p. 172). ‘Bildung,’ again, is exactly the point where Herder enriches the concept of growth with a concept of learning and education. We have only the latter in mind when we use the German word ‘Bildung’ today in everyday language. Herder’s text is in fact one of the important transfer points in 18th century where this semantic shift from growth to education begins and where consequently both meanings are involved when the word is used. So, ‘Bildung’ for Herder guarantees the translatability from nature into culture and vice versa (cf. Parnes/Vedder/Willer 2008, p. 84-96).

Rhetorically, this is realized in tight parallels where ‘Bildung’ and related words are used for both body and mind, thus marking a kind of identity without denying the differences. For instance, Herder calls the organized human body “das Gebilde,” as far as its ‘genetic disposition’ is concerned – “genetische Disposition” is indeed Herder’s wording – but in the same passage he claims that, in order to become human beings, we are dependent on the “Hülfsmittel der Bildung um uns,” that is, the resources of ‘Bildung’ that do not come from *within*, but are situated *around* us. Herder sometimes speaks of a ‘genesis of mind’ or ‘second genesis’: “geistige Genesis,” “zweite Genesis” (Herder 1989, p. 345). This version of the genetic principle has to do with continuities, transmissions, and changes in the long run, that is, in culture and tradition. It is quite compelling to term this kind of transfer *epigenetic* again, even if Herder does not draw this consequence. In fact, constructing an order of first and second genesis and localizing one kind of ‘Bildung’ *within* us and another one *around* us means to reinstall a logic of the epi-genetic, even though Herder’s intentions were directed to excluding it from his genetic thinking.

4. *'Epigenesis' in Epigenetics*

Returning to the problem of etymologies, of roots, radicals, and derivations, the question presents itself what the prefix *epi-* (greek for 'after,' but also for 'outside') in *epigenetics* actually signifies. This question – “What is ‘Epi’ about Epigenetics?” – has been used as the title of a paper published by science philosopher James Griesemer in 2002. For Griesemer, the question was motivated by a certain discontent with the fact that *anything* that matters beyond DNA should be called epigenetic. In his view, it was Weismann’s doctrine of the separation of germ and soma that was responsible for this unfocussed way of talking. Only when “nearly everything” is excluded from the realm of biological heredity, says Griesemer, then “nearly everything” has to be labeled epigenetic. Griesemer’s counter-proposal is to do away with Weismannism and to accept that heredity and development are intertwined in the very process of reproduction. Thus, Griesemer concludes, genetics is *not* the basis for an understanding of epigenetic processes. Instead, one has to accept that there *is* genetic as well as epigenetic inheritance. Both are related in the same way in which the special is related to the general. (Griesemer 2002, p. 109)

Griesemer’s philosophical conclusion is very convincing that “What counts as epigenetic depends on what counts as genetic.” (Griesemer 2002, p. 97) This is also what French biologist and historian Michel Morange had in mind when he maintained – in a contribution to the same volume in which Griesemer’s paper appeared – that epigenetics “cannot be defined per se, but only in reference and in opposition to genetics.” (Morange 2002, p. 56) The distinction of what *counts as* epigenetic or genetic is historical and functional, and, again, touches the category of validity. So, the labeling of anything as ‘epigenetic’ – be it developmental processes or cell interactions, or be it the study of these concerns – necessarily has a different function *after* the central dogmas of genetics have been formulated, than it had had *before*. As banal as this may sound, I would like to emphasize that in my view, what is ‘epi-’ about epigenetics not only directs to the logical and temporal relation of development and heredity, but also to the historicity of biological concepts.

If I was to turn this into a bold statement, I should say that, biologically speaking, epigenetics examines what happens outside the genes; whereas, historically speaking, epigenetics is what happens after genetics. This may seem to be only a philological subtleness, but I think that it touches an essential factor in the historicity of knowledge. Once the epistemic field is already delimited by the rules of genetics, the use of the word ‘epigenetic’ gains a different status. So, while the cited examples from Morgan and Weismann were attempts to mark out that field, the case is different when in the late 1940s Conrad Waddington coined the term ‘epigenetics’ to designate a research programme that had to mark a difference to the contemporary study of heredity.

Waddington objected to what he called the “extremist” fashion (Waddington 1953, p. 151) of the reigning Neo-Darwinian view that mutational change was entirely at random and that any adaptation could only result from the natural selection of those mutations. But it is not evident from these objections why Waddington should want to make his claim in the name of ‘epigenetics.’ Again, I would like to stress the usage of words rather than to try and define concepts. For it was Waddington himself who, in his paper “Embryology, Epigenetics and Biogenetics” from 1956, emphasized the capacity of his term to embrace several and diachronically diverse meanings.

The fact that the word ‘epigenetics’ is reminiscent of ‘epigenesis’ is to my mind one of the points in its favour. [...] We all realize that, by the time development begins, the zygote contains certain ‘performed’ characters, but that these must interact with one another, in processes of ‘epigenesis,’ before the adult condition is attained. The study of the ‘performed’ characters nowadays belongs to the discipline known as ‘genetics,’ the name ‘epigenetics’ is

suggested as the study of those processes which constitute the epigenesis which is also involved in development. (Waddington 1956, p. 1241)

This reference is likely to draw a very neat distinction between genetics and epigenetics. This certainly has to do with the way in which Waddington conceptualized the difference between genotype and phenotype, the phenotype consisting of 'effects' caused by the genotype. But still, Waddington's denomination not only refers to a distinction, but also to the strict *opposition* between epigenesis and preformation. Using the word 'preformation,' as I have claimed before, was likely to bring back to mind the metaphysical input of the idea that the essentials of life should be prescribed, pre-determined. Using it for the more and more successful science of genetics, as Waddington did in 1956, somehow meant to connect the geneticists, that is: the winners of contemporary scientific economy, to the preformationists of the 18th century, that is: to the losers of the success story of modern science.

Conclusion

To conclude, I have to admit that in my last remarks I have probably been overdoing the interpretation of a very small passage from Waddington's paper which has very few ambitions for a critique of scientific ideology, based only on the symptom of the out-dated words 'epigenesis' and 'preformation.' Still I think that we should not regard such recourses to history in scientific papers as merely ornamental, but should rather take into account their critical potential. When it is about the contention between *concepts*, then the denomination and definition of *terms* is everything but arbitrary, but it plays a crucial role in the interplay of knowledge and rhetoric, especially in a diachronic view when it is about making out symptoms for 'paradigm changes.' In that vein, the history of the two-part word 'epigenesis,' of its use and of its relations to changing counterparts may partake in a rhetoric of knowledge that directs to the unsettled or excluded premises of modern scientific thought.

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WILLIAM KEITH BROOKS (1848-1908) AND THE DEFENSE OF LATE-NINETEENTH CENTURY DARWINIAN EVOLUTION THEORY

Keith R. Benson

Abstract

Accounts of evolution theory in the United States at the end of the nineteenth century often uncritically conflate it with the term “Darwinian.” However, as some recent scholarship reveals, most American evolutionists preferred neo-Lamarckian accounts of organic change over time and/or were dismissive about the role of Darwinian natural selection. William Keith Brooks was one of a few biologists who championed Darwin’s version of evolution theory, especially in his teaching, research, and writing at Johns Hopkins University. In a real sense, he worked to expand the reach of evolution theory including a reconstruction of Darwin’s version of pangenesis, an emphasis on the importance of natural selection in development, and a stress on the extensiveness of variation in the natural world. This overall orientation is important to stress, since Brooks played an influential role in the training of many important North American zoologists, including E.B. Wilson, J. Playfair McMurrich, E.G. Conklin, Thomas Hunt Morgan, and Ross Harrison. Even the British zoologist William Bateson noted Brooks’s influence on his own evolutionary views. Brooks’s legacy, therefore, was one in which he attempted to expand the reach of evolution theory at the end of the nineteenth century.

Introduction: The Hourglass, Evolution, Genetics, and Epigenetics in 19th c. US

The metaphor for this conference to describe the historical relationship between evolution theory, genetics, and epigenetics is the hourglass. It provides an apt heuristic device to depict both the breadth of Charles Darwin’s evolutionary program in 1859 and its gradual narrowing by the beginning of the twentieth century as biologists moved from more speculative claims of historical change to the promise of more causal arguments as promised by the investigation of epigenetic change within an emerging tradition of genetics. That is, in 1859 Darwin offered his novel notion of species divergence via modification in the *Origin* that was characterized not just by his detailed support of the new ideas, but also by its breathtakingly broad and comprehensive historical scope. Darwin’s clear intent was to offer a new view of the natural world that would address natural history’s major problems at mid-century. Despite these goals and largely in response to the suggestions he laid out in his book, naturalists soon encountered many problems with the encompassing Darwinian program. In fact, by the beginning of the twentieth century, many younger biologists turned away from the broad perspective Darwin offered, preferring to center their questions within the exciting new arena of cytology and inheritance; evolutionary claims were narrowed considerably by the emerging preference for studies of epigenetics and genetics.

An examination of William Keith Brooks’s (1848-1908) career at Johns Hopkins University provides an illustration of the hourglass’s narrowing, since he was both an early and enthusiastic supporter of Darwin and, at the same time, the mentor of many of the biologists who adopted the more restricted approaches favored at the turn-of-the-century. As one-half of the original graduate faculty in biology, H. Newall Martin was the other faculty, Brooks played a pivotal role in training a new generation of American biologists, but he has not fared well by historians.¹ That is, he has traditionally been linked to an “older” nineteenth-century tradition in morphology, a tradition

that historians have often disparaged, especially in contrast to the exciting changes that biology experienced with the rediscovery of Mendel's work in 1900 and the discovery of chromosome theory in 1910.² Recently, however, Brooks has attracted more attention from scholars, especially his important studies on the Chesapeake oyster, both through the republication of his 1893 work, *The Oyster*, and through Christine Keiner's dissertation and publications on Brooks's investigations of the oyster in connection with her environmental history of the Chesapeake oyster industry.³ This present study is intended to emphasize Brooks's importance further by highlighting his contributions to the reconstruction of Darwin's evolution theory at the end of the nineteenth century, especially through his attempts to re-establish the centrality of natural selection. At the same time, Brooks's career provides an exemplar of the narrowing of the focus of evolution theory, from its broad interpretation when first announced by Darwin to the narrower perspective associated with studies of epigenetics and genetics circa 1900.

1. Evolution Comes to the United States

The traditional interpretation among historians of biology is that after 1859 biologists quickly converted to Darwinian evolution theory, abandoning the much-derided and problem-riddled natural theology tradition in natural history. Reinforced by favorable reactions of eminent scientists such as Charles Lyell, Thomas Huxley, J.D. Hooker, Asa Gray, and Ernst Haeckel, the growing popularity of evolution theory has led to characterizations of the nineteenth century as "Darwin's Century."⁴ Nevertheless, Darwin's interpretation of evolutionary change encountered immediate problems, a situation recognized by many of his early supporters. For example, and according to Lord Kelvin, there was not enough time for the extensive species change Darwin claimed took place over virtually unlimited amounts of geological time. Additionally, Darwin did not have an explanation for the cause of variation, especially how variation seemingly occurred when it was "needed." Furthermore, within five years of the publication of *On the Origin of Species*, many biologists became extremely skeptical of the creative abilities of Darwin's major mechanism, natural selection. Certainly, it could be used to account for the selection of the best-adapted form, but how could it produce novel forms or, in fact, what was its relationship to the formation of variations? A variety of critics pointed to other issues, especially those who gazed backwards to the natural theology tradition predating Darwin or to versions of design, long a part of the now-discredited natural history. Still, and despite these questions and objections, evolution theory did carry the day after 1859; that is, evolution theory, defined generally as the belief in organic change over time, was accepted and supported by most biologists. However, their support was not always in accord with a strict Darwinian interpretation of evolution theory.

Among the national responses most receptive to Darwin's new work was the United States. As Ron Numbers has persuasively argued in his book, *Darwinism Comes to America*, there was considerable enthusiasm for Darwin among American biologists, even among those who had trained under the Cuvierian naturalist, Louis Agassiz at Harvard's Museum of Comparative Zoology.⁵ But in fact, that actual reception was not an embrace of Darwin's interpretation of evolution theory; that is, Americans tended to accept the new view of organic change over time, but they also included many variations on this general theme, some that wandered widely from Darwin's notions. Thus, the reception might better be described in terms of "evolution comes to America," especially given the inaugural response to Darwin's ideas by the Harvard botanist, Asa Gray. In his review of the *Origin* in the *Atlantic Monthly* in 1860,⁶ Gray accepted evolutionary change but attached it firmly to Christian orthodoxy, claiming natural selection represented God's agency in the natural world. That is, he laid the groundwork for what become known as "theistic evolution," as well as beginning a process of adapting Darwin to the unique social, cultural, and scientific conditions in the New World.

Peter Bowler has extended this position by pointing out the rich variety of non-Darwinian ideas of evolution in two monographs, even emphasizing the gradual emergence of an “American school” that added Lamarckian factors to Darwin’s interpretation.⁷ Indeed, Edward Pfeifer, in an under-appreciated article written several decades ago that demands more attention and scholarly follow-up, points out the existence of a strong and vibrant evolutionary community in the United States, the neo-Lamarckians, who first began publishing their ideas in 1866, then began a journal (the *American Naturalist*), and finally self-identified with Lamarck in the 1880s.⁸ The late Stephen Jay Gould was the latest popularizer of the neo-Lamarckians, including several of the important nineteenth-century American paleontologists in his study *Ontogeny and Phylogeny*,⁹ again emphasizing the French biologist’s popularity among evolutionary thinkers in the United States. Thus, while some historians have conflated the acceptance of evolution theory with the acceptance of Darwinian evolution theory, a more accurate depiction of the situation is that biologists in the United States after 1859 were more typically non-Darwinian. That is, men of science in the new republic wholeheartedly embraced evolution theory but not necessarily in its original Darwinian guise.

Indeed, some of Darwin’s promoters were not as intent on making sure Americans were exposed to pure Darwinian ideas as they were interested in offering new ideas for their countrymen, many of whom had been demoralized by the devastation of the Civil War and were in dire need of fresh perspectives. Thus, E.F. Youmans, a publisher and publicist who realized the financial benefits of providing new insights, actively recruited Herbert Spencer and Ernst Haeckel, both of whom promoted evolution theory, to write “American” editions of their work. Ultimately, each complied and Youmans published hundreds of thousands of copies of the Englishman and the German, all extending Darwinian evolution theory beyond the fauna and flora of the globe to include its application to human society as well. As James Moore has argued, this political aspect of Darwin ultimately led to the phrase “Social Darwinism” (first used in English in 1897 and crossing the Atlantic to the United States a few years later),¹⁰ an ideology that liberally combined Darwin and Lamarck in its teleological goal of assuring westerners, especially Americans, of their ultimate supremacy in world affairs.

Problematically, however, much of what historians have written of the second half of the nineteenth century has too often been seen through the lens of the twentieth century and the ultimately triumphant return to Darwinian evolution theory, stripped bare of French transmutation. On a popular level, this tendency has been heightened by recent debates attempting to resurrect creation theory, almost all of which dramatically target Darwin as the sole agent arguing for species change over time. But this lens actually obscures the rich array of evolutionary ideas that were offered after 1859 and, in so doing, distorts the historical record. In fact, Lester Frank Ward, delivering the presidential address to the Biological Society of Washington in 1891, offered probably the most dominant position concerning evolution theory in the United States at the end of the nineteenth century. Ward noted there were “two great principals of transformism, the functional, as set forth by Lamarck, and the selective, as elaborated by Darwin [...]”¹¹ Borrowing support from both Spencer and Haeckel, throughout his presentation Ward continually emphasized the complementarity of the two positions, arguing that the “great problem” for biology was “heredity which continues to occupy the foreground of all biological discussions.”¹² Ward did not refer to an hourglass, but his interpretation of nineteenth-century developments in evolution theory and heredity nicely encapsulates the metaphor; that is, in 1859 the broad questions were related to the transformation of species, questions to which both Lamarck and Darwin responded. But at the end of the century, the new problems in biology were restricted to issues of heredity. Thus, Darwin’s true legacy for the nineteenth century (and beyond) in the United States was the

influential suggestion of organic change over time guided by selection; the explanation for the change and the mechanisms behind this change remained problematic.

2. Brooks as a Darwinian

During his entire career, Brooks never wavered from his wholesale acceptance and defense of Darwin. Given the popularity of Lamarck, only a few biologists in the United States, a club perhaps strictly limited only to David Starr Jordan and John T. Gulick in addition to their Baltimore colleague, Brooks considered Darwin's theoretical formulation to be completely sufficient and adequately necessary to explain evolutionary change, with natural selection as its major mechanism. Of course, as Brooks noted, Darwin did not understand the principles behind inheritance or variation. But who in 1859 did? Furthermore, as Ward claimed in 1891, who understood these questions by the end of the nineteenth century? It was only after Mendel's work was integrated within the pioneering studies in cytology in the first decade of the twentieth century, that new explanations for inheritance and variation were available, thus leading to the separation of genetics from epigenetics (development). But this understanding belonged to the early twentieth century, not to the forty-year period before. And even with this new understanding, a renaissance and reformation of Darwinian evolution theory did not gradually occur until after the synthetic view of evolution theory was articulated in the early 1940s.

Nevertheless, Brooks maintained a thoroughly orthodox Darwinian view of evolution theory, a view he taught at Johns Hopkins, wrote about in numerous articles, and championed in his neo-Darwinian work on inheritance and variation, *The Law of Heredity* (1883).¹³ Brooks arrived at Johns Hopkins after working under Louis Agassiz at Harvard and then Agassiz's son, Alexander Agassiz, following the elder Agassiz's death in 1873. Like many of his colleagues at the MCZ, Brooks was exposed to ideas of species transmutation or evolution through the ecumenical teaching of Agassiz. But unlike his mentor, Brooks joined the growing cadre of American biologists who adopted the new view of a dynamic natural world, constantly changing in response to a changing environment. His acceptance of the new theory, however, was not based merely on his slavish adherence to the new trend in biology since Brooks, unlike the vast majority of his peers, opted for the strictly Darwinian view of organic change, not the more widely accepted version that appended Darwin to Lamarck's coattails. In so doing, he immediately recognized the need to contend with the many problems of Darwin's theory, a task to which he dedicated his entire career.

As is well known, shortly after the *Origin* was published, Darwin realized he needed to address the question of the cause or causes of variation (actually, this was a problem he mused over prior to the book's publication, as is clear from his notebooks). Why did species appear to vary and how could this appearance be explained in terms of the laws of inheritance? Darwin framed this question, as Larry and Iris Sandler have pointed out, through the nineteenth-century inseparable linkage between inheritance and variation; that is, each represented two sides of the same biological coin.¹⁴ Obviously, Darwin had worried about this problem before his book appeared, but now he faced increasing criticism from his peers and was compelled to respond. In 1868, he published his "Provisional Hypothesis of Pangenesis," in *Variation of Plants and Animals Under Domestication* (the ideas were clearly expressed first in manuscript form in 1865). Darwin suggested that all the cells of organic beings produced gemmules, which collected in the germinal material and then were responsible for the transmission to the next generation of the traits of the cells from which they originated. Darwin also suggested that the gemmules, sometimes active and other times inactive, could explain the linked phenomena of inheritance and variation.¹⁵ While he obviously thought his suggestion had merit, few biologists greeted these hypothetical ideas with any

enthusiasm. Even Darwin's own cousin, Francis Galton, advised him to drop his speculations and, wisely, Darwin seldom referred to pangenesis after 1868.

But the idea of pangenesis captured Brooks's attention. As he mentioned to his new president, Ira Remsen, shortly after arriving in Baltimore, all of the work in biology owed its inspiration to Darwin. He was particularly excited by Darwin's pangenesis, writing a paper for the American Association for the Advancement of Science meeting in August 1876 and published one year later as "A Provisional Hypothesis of Pangenesis."¹⁶ Here, Brooks sought to resurrect Darwin's notions, revising them to avoid the problems of the earlier views. His revision stated that the established characters of species were transmitted through the germinal material when it was properly stimulated. However, new characters were transmitted via the gemmules, which were given off only by cells involved in a specific variation. The gemmules themselves could not form new individuals, but under proper conditions they could reproduce the cell that formed them. Since most cells were well adapted to their surroundings, as a result of the evolutionary process, they did not give off gemmules. However, during periods of selective pressure, the harmonious adjustment between the cell and its environment experienced challenges. If these pressures were sufficient to affect the normal performance of the cell, variable gemmules were produced and transmitted via the germ cells to the next generation.¹⁷

Brooks's hypothesis also included a separate role for the male germinal tissue and the female germinal material, addressing another problem of the time, that of sexual dimorphism. The gemmules produced by the body were stored in the male gland, entered the seminal material, and were transmitted to the egg by impregnation and fertilization. The ovary, on the other hand, lacked the specialized structures for the aggregation and transmission of gemmules. Therefore, while the cells of the female produced gemmules, they were seldom of great importance for the variation which was exhibited in the offspring.¹⁸ The male, then, accounted for adaptations to environmental conditions, thus serving as the creative germinal element. The egg was the conservative material and accounted for the species' apparent adherence to the type.

Brooks's aim with his theory was to answer several of the major objections to Darwin's theory and to provide an hypothesis that would be supported by solid evidence. By stating that only cells undergoing variation in response to selective pressure produced gemmules, Brooks escaped the numerous difficulties of dealing with a vast multitude of replicating gemmules, part of Galton's objections. Second, this view also explained why variations seem to appear when there was a need for the variation. Since gemmules were the cell's response to changing conditions, only when change occurred would they be produced. Third, the new conditions in the environment did not result in variations appearing in that generation. Rather, as was commonly observed by naturalists, variations in response to change always appeared in the subsequent generation. Finally, by theorizing that the different germ cells had markedly different roles in heredity, Brooks appeared to address the question of why species were often sexually dimorphic; thus, the male was the variable member of the species and the female represented the conservative component.

The mature version of Brooks's ideas appeared a few years later in *The Law of Heredity* (1883). Brooks outlined his four-part goal in the book: one, to remodel pangenesis so that only a few gemmules would be produced at any one time; two, to depict the gemmules as not necessarily being present at all times and in all parts of the body; three, to embrace a new class of facts in addition to the known functions of the sexual elements; and four, to discover new and unexpected relations between inheritable phenomena.¹⁹ But an additional goal was to illustrate how his new theory of heredity, a reformulation and reconstruction of Darwin, was intimately tied to the primary cause of evolutionary change, natural selection. Brooks suggested that Darwin's problem stemmed from his insistence that variation was fortuitous. It was difficult for Brooks to understand how such fortuitous changes could account for the observed facts of evolution, especially the

formation of complicated organs, a problem Darwin encountered continuously. Of course, related to this was the vast amount of time Darwin's explanation required for variation to produce perfected structures. Brooks solved this by suggesting that changes in any part of the body will disturb the "harmonious adjustment of related parts" and this disturbance would produce more variations in these structures through the production of additional gemmules until natural selection accomplished its task within "reasonable limits" of time.²⁰ The response of the cells to produce gemmules in a coordinated manner was described by Brooks as an example of "correlated variability."²¹ Thus, natural selection worked through cells that remained conservative during normal conditions and then gave off gemmules during conditions of change. Natural selection also led to the gradual and divergent specialization of the sexual elements and created the physiological division of labor between the male and female material (less derived species tended to reproduce asexually). Thus, there was a reciprocal status between the mechanism of natural selection and the law of heredity.²²

The intimate connection between natural selection and heredity also enabled Brooks to explain why variation was so ubiquitous in the natural world. That is, continual environmental changes prompted continual production of novel gemmules, leading to the potential of variation. The actual production of variations was due to changes within the germinal material, primarily the male germ cells. Thus, and repeating his earlier assumption, it was the reciprocal interaction of the organism and the environment that produced the variations leading to organic change. Such an interpretation, Brooks thought, enabled him to steer a middle course between Darwin's original views and the arguments of the neo-Lamarckians, who insisted only on the direct modifying influences of the environment. As he stated, his "theory furnishes us explanations which lie midway between Darwin's view of the origin of variation and the Lamarckian view, and thus enables us to escape both of these difficulties [...]"²³

Brooks's speculations were almost immediately recognized as addressing many of the problems biologists had with Darwin's version of evolution theory. Even Darwin's major supporter in the United States, Asa Gray, praised Brooks for his work.

An essay which aims to succeed where Darwin failed, to correct some of his judgments, to explain away difficulties in the theory of natural selection which he confessed his inability to meet, and especially which is to account for variation, which, if we remember rightly, Darwin thought unaccountable, is certainly a very ambitious undertaking. But the attempt is made with full knowledge of the actual conditions of the questions involved, and the case is argued with real ability by a naturalist who has already made a mark in investigation and shown aptitude in speculation.²⁴

Brooks's colleague, H.W. Conn also recognized the value of these new views. Writing in his work, *Evolution of Today* in 1889, Conn stated that Brooks's view "is an important theory, because it is the first attempt to explain the origin of simultaneous variations for successive generations in those parts where change is needed."²⁵ Even more impressive was the support from August Weismann, in his *Essay upon Heredity*, when praised Brooks as deserving "great credit, and that his production has been one of those indirect roads along which science has been compelled to travel in order to arrive at the truth."²⁶

3. Brooks at Century's End

Brooks was accurately portrayed, especially after his book on heredity, as one of the leading defenders of Darwin. Not surprisingly, the neo-Lamarckian community, led by Alpheus Hyatt, criticized Brooks's contributions, in large part because he had failed to include information from the fossil record and failed to recognize any paleontological evidence for the "quick evolution of

forms.”²⁷ Brooks, however, was not bothered by this perspective, since he considered the issue of time to be addressed through the close association between natural selection and the production of varied gemmules. Nevertheless, Brooks became increasingly aware there was little if any empirical evidence to support his speculative claims. Indeed, he wrote nothing more about gemmules after the 1883 work, preferring instead to emphasize the role of sexual reproduction in creating sufficient variation upon which natural selection could act. His one known reference to gemmules after 1883 was in a seminar he presented at Johns Hopkins in 1889 when he claimed the role of the particles was to carry a “predisposition” for change to the germ plasm, Weismann’s well-accepted term at the time referring to the hereditary material.²⁸

Brooks then turned to stress his characteristic holistic view of the dynamic relationship between the organism and its environment. Noting the growing attention to the germinal tissue, especially the new microscopical investigations of European embryologists pointing to the potential unique role in inheritance of cellular ultrastructure, usually not well-defined, Brooks reminded his students and readers that

the species is not in the chromatin, nor in the germ cells, nor in the differentiated cells, nor in gemmules, nor in idioplasm, nor in biophore, nor in allelomorphs, nor in living beings at any stage of their existence, because it is in that reciprocal interaction between the living being and the natural world, of which it is a part, which has been called the struggle for existence.²⁹

In part, Brooks was responding to the growing body of evidence from research in cytology linking nuclear material to the material that was transmitted from generation to generation. Exhibiting his comprehensive views of biology, he expressed his profound skepticism that any specific part of the cell or developing organism could be wholly responsible for inheritance and variation.

But Brooks was also responding to those who confused his speculative interpretations of evolution theory as being inspired by Lamarck, a comment leveled at Brooks during the 1890s, perhaps the decade during which Lamarck gained greatest popularity among American biologists. Of course, Brooks’s reliance on environmental challenges to produce the variable gemmules seemed to many to be a Lamarckian explanation. But in an 1895 response in *Science*, Brooks clarified his position, claiming that the appearance of variation was due to the direct influence of the environment, “but its precise character is not.” Departing from those who relied only on inheritance to explain variation, Brooks explained,

[...] that every change which takes place in the organism from the beginning of segmentation to the end of life is called forth by some external stimulus either within the body or without; and yet that the outcome of the whole process of development is what it is because it was all potential in the germ.³⁰

Clearly, Brooks was attempting to hybridize genetics (inheritance) and epigenetics (variation) by combining the heritable “protoplasm” with the variable gemmules. As he claimed, “life is not protoplasm but adjustment.”³¹

A few years later, Brooks became more expansive about his Darwinian stance, noting that he first studied the *Origin of Species* and became “an ardent disciple of Darwin” including the “great law of selection.”³² At the same time, he read about Lamarck in Lyell’s geological work, including the infamous first chapter in which Lyell dismissed Lamarckian transmutation. Claiming there had been no answers to Lyell’s argument, Brooks opined there was no need for him to revisit Lamarck. He clarified his position by indicating that Lyell’s arguments had been advanced and furthered by Weismann. Indeed, as a result, there was little evidence of continuity between somatic tissue and the germinal tissue, therefore it was difficult to understand how these Lamarckian

factors could be adaptive.³³ This position was perfectly in line with his earlier statement that genetics provided the continuity, natural selection produced the variation.

Brooks's final statement on this matter provides unambiguous support that biological explanations combine genetics and epigenetics. As we have seen, Brooks gradually held natural selection completely responsible for variation, since it was natural selection that literally produced the changes. Indeed, Brooks felt it was unfortunate there was confusion between the terms.

[T]he value of natural selection is quite independent of what we may discover, or fail to discover, concerning the true cause of that diversity among individuals which has, by an unfortunate use of words, come to be called variation.³⁴

4. *The Legacy of Brooks*

By the beginning of the twentieth century, Brooks was fighting against the narrowing of biology's hour glass. Preferring an expansive view for evolution theory, Brooks sought to utilize a panoply of arguments to support his assumption that phyletic inheritance alone could not account for the adaptive changes acted upon by natural selection. Brooks had a partitioned view of evolution, understanding genetics to explain inheritance while resorting to adaptation caused by natural selection to produce the resulting ontogenetic events. However, the rise of microscopical investigations centered upon cellular ultrastructure and developmental phenomena narrowed the perspective of many biologists. Given both the excitement and potential promise of this new work, Brooks's students and colleagues began to eschew speculation and hypothetical reasoning, preferring instead to first address early cell events and then, after 1910, to center their work on chromosomes. Not abandoning his framework for evolution theory, Brooks still held to his holistic interpretation that any explanation for inheritance and variation had to include the reciprocity between the organism in its environment.

Thus, Brooks may have backed away from gemmules by the early twentieth century, but he still claimed that species formed epigenetically from the interaction between the living being and the environment. Following the tradition established by Darwin, Brooks accepted the full phylogenetic history of a species as resulting from natural selection and the struggle for existence acting in concert with inheritance. Similarly, ontogenetic development was also a process that could be explained in terms of natural selection and the struggle for existence again coordinating with inheritance. In this manner, both ancestral development and individual development of a species were the combined result of genetics and epigenetics. Since all developmental phenomena were to be explained in these terms, Brooks viewed both heredity and variation as "imperfect views of the facts."³⁵ That is, they were only appearances that resulted from the relationship that existed between the organism and its external environment.³⁶

Brooks's broad and comprehensive version of genetics and epigenetics has faded into obscurity, much like his reputation. At the same time, historians of biology are more familiar with the work of some of his best students, including E.B. Wilson and Thomas Hunt Morgan. But I would like to suggest that it was the influence from Brooks that was largely responsible for Wilson's and Morgan's long-held resistance to explanations for inheritance and variation that depended only upon cellular ultrastructure. Even after narrowing their own perspectives on genetics and epigenetics to the behavior of chromosomes (Wilson remained less enthusiastic about chromosomes than Morgan), they remained somewhat skeptical of this perspective to address fundamental questions of evolution theory. Ultimately, neither Wilson nor Morgan was successful in applying the thin-neck of the hour glass to frame modern evolution theory.

Notes

1. My dissertation, "William Keith Brooks (1848-1908): A Case Study in Morphology and the Development of American Biology" (Oregon State University, 1979), is the only comprehensive treatment of Brooks's career. Indeed, other than a few biographical sketches and some references to his role at Johns Hopkins, his life remains obscure.
2. Despite major challenges to his "revolt from morphology" and his dialectic interpretation of the natural history and descriptive approach as opposed to the physiological and experimental approach, Garland E. Allen's, *Life Science in the Twentieth Century* (New York: John Wiley, 1975) still dominates the scholarly work in the history of biology. Unfortunately, his bias in emphasizing the rise of genetics, especially as it has neglected many other areas in twentieth-century biology, skews our understanding of the character of biology. Indeed, although there was tremendous excitement associated with the new developments in genetics, these did not represent mainstream biology, genetics remained a quite narrow and restricted research area (almost entirely located at Columbia University during the first two decades of the twentieth century), and other more descriptive areas of biology continued to flourish, including cytology, developmental biology, and ecology.
3. Christine Keiner, "W.K. Brooks and the Oyster Question: Science, Politics, and Resource Management in Maryland," *Journal of the History of Biology*, 31 (1998): pp. 383-324 and "Scientists, Oystermen, and Maryland Oyster Conservation Politics, 1880-1969: A Study of Two Cultures," Johns Hopkins University dissertation, 2001.
4. This term actually dates from Loren Eiseley's, *Darwin's Century: Evolution and the Men Who Discovered It* (New York: Doubleday, 1958). But it has also been generalized to describe the entire century; many courses in the history of biology continue to use a title similar to this as an organizing theme for nineteenth-century biology.
5. Ronald L. Numbers, *Darwinism Comes to America* (Cambridge: Harvard University Press, 1998). Numbers also addressed the nature of American responses to Darwin by attempting to depict biologists as either neo-Lamarckian or neo-Darwinian. Although he claims to find fewer followers of Lamarck than I argue here, the essential problem is more closely related to the pragmatic nature of the American response. That is, many of these biologists eschewed labels, preferring to adopt many variations on the general evolutionary theme. Numbers does, however, support the notion that American biologists quickly embraced Darwin.
6. Asa Gray's theistic views of evolution were published in several articles in the *Atlantic Monthly* in 1860. They are also contained in the third part of Asa Gray, *Darwiniana: Essays and Reviews Pertaining to Darwinism* (New York: Appleton, 1876).
7. Peter Bowler refers to the "American school of neo-Lamarckism" in both *The Eclipse of Darwinism* (Baltimore: Johns Hopkins University Press, 1983) and *The Non-Darwinian Revolution* (Baltimore: Johns Hopkins University Press, 1988). Interestingly, this same phrase was used in 1891 by Lester Frank Ward to describe one variant of evolutionists in the United States. In Bowler's work, the neo-Lamarckian position was originally given doctrinal status in *The Eclipse of Darwinism* but was downgraded to "pseudo-Darwinism" in *The Non-Darwinian Revolution*. Perhaps this reflects the wide variety of views encompassing the Lamarckians. At the same time, it should be stressed that the Americans adopted the Lamarckian neologism only after almost 20 years experience with evolutionary ideas. Thus, they were not orthodox followers of Lamarck. Indeed, their interest was to retain an active role of external factors as agents for organic change, in addition to natural selection. In other words, American evolutionists were

heterodox and pragmatic.

8. Edward J. Pfeiffer, "The Genesis of American neo-Lamarckism," *Isis*, 56 (1965): pp. 156-167.
9. Stephen Jay Gould, *Ontogeny and Phylogeny* (Cambridge: Harvard University Press, 1977). The neo-Lamarckians were so dominant in the US that they were able to spearhead the successful fundraising to create a statue of Lamarck, which was placed at the entrance of Paris's Museum d'histoire naturelle in 1909.
10. James Moore, "Socializing Darwinism: Historiography and the Fortunes of a Phrase," in Les Levidow, ed., *Science as Politics* (London: Free Association Books, 1986) and "Deconstructing Darwinism: The Politics of Evolution in the 1860s," *Journal of the History of Biology*, 24 (1991): pp. 353-408. Moore has provided the most thorough documentation of the variations of "Darwinism" at the end of the nineteenth century in these two papers. I would also like to acknowledge his assistance in tracking down the original use of the term "neo-Darwinian," when he suggested it followed Weismann's and Wallace's ardent defense of natural selection at the end of the 1880s. His views correspond accurately to the perspective of Lester Frank Ward, writing on the same subject in 1891.
11. Lester Frank Ward, "Neo-Darwinism and Neo-Lamarckism," *Annual Address of the President of the Biological Society of Washington*, January 24, 1891 (Washington: Press of Gedney & Roberts). My thanks to Paul Farber, who located this important article, the first overt comparison between the neo-Darwinian and the neo-Lamarckian positions I have been able to locate.
12. *Ibid.*, p. 12.
13. As far as I can tell, Brooks never referred to himself as a "neo-Darwinian" in 1883 nor did anyone else use that term at that time. In fact, the first clear reference to that description was in Ward's paper of 1891, although he credited G.J. Romanes in a letter to *Nature*, 30 August 1888 (38: p. 413). It must be emphasized, however, that Romanes used the term "neo-Darwinian" only at the end of a letter that emphasized the two nineteenth-century traditions of transmutation as "Lamarckism" and "Darwinism." Romanes referred to the "school of Wesmann" as neo-Darwinian as compared to Darwinian. Ward also notes that A.S. Packard first used the term "neo-Lamarckian" in 1885. All these references confirm my position that Americans were typically pragmatic, therefore creating real problems to assign them to two, although poorly articulated and defined, positions of Lamarck and Darwin.
14. Iris Sandler and Laurence Sander, "A Conceptual Ambiguity that Contributed to the Neglect of Mendel's Paper," *History and Philosophy of the Life Sciences*, 7 (1985): pp. 3-70.
15. Charles Darwin, *Variation of Plants and Animals Under Domestication*, 2 volumes (London: John Murray, 1868), pp. 369-371.
16. William Keith Brooks, "Provisional Hypothesis of Pangenesis," *American Naturalist*, 11 (1877): pp. 144-147.
17. *Ibid.*, p. 146.
18. *ibid.*, p. 145.
19. William Keith Brooks, *The Law of Heredity. A Study of the Cause of Variation and the Origin of Living Organisms* (Baltimore: J. Murray, 1883), pp. 80-81.
20. *Ibid.*, p. 287.
21. *Ibid.*, p. 291.

22. Ibid., p. 294.
23. Ibid., p. 164.
24. Asa Gray, "Book notices," review of *The Law of Heredity*, by W.K. Brooks, in *Andover Review*, 1 (1884): p. 210.
25. H.W. Conn, *Evolution of Today* (New York and London: G.P. Putnam's Sons, 1889), pp. 218-288.
26. August Weismann, *Essays upon Heredity and Kindred Biological Problems*, edited by Poulton, Schonland and Shipley (Oxford: Clarendon Press, 1889), p. 166.
27. Alpheus Hyall, "Fossil Cephalopoda in the Museum of Comparative Zoology," *Proceedings of the American Association for the Advancement of Science*, 32 (1883): p. 348.
28. "Morphological Seminary – Notes, October 15, 22, 29, 1889" Princeton University, Firestone Library Manuscript Room, E.G. Conklin Papers.
29. W.K. Brooks, "Heredity and Variation: Logical and Biological," *Proceedings of the American Philosophical Society*, 45 (1906): p. 75.
30. W.K. Brooks, "An Inherent Error in the Views of Galton and Weismann on Variation," *Science*, 1 (1895): pp. 124-125.
31. Ibid., p. 126.
32. W.K. Brooks, "Lamarck and Lyell: A Short Way with Lamarckians," *Natural Science*, 1896, p. 89.
33. Ibid., p. 92.
34. W. K. Brooks, "Lyell and Lamarckism: A Rejoinder," *Natural Science*, (1896): p. 115.
35. William Keith Brooks, *Foundations of Zoology* (New York: The Macmillan Company, 1899) p. 220.
36. Ibid, p. 217.

CONSANGUINITY, HEREDITY AND MARRIAGE
THE PATH TO MEDICAL INTERVENTION IN MEXICAN MARRIAGE LAWS

Fabricio González Soriano and Carlos López-Beltrán

Angel del Campo, a Medical school dropout and a brilliant journalist and able novelist under the *nom de plume* of Micrós, published in 1890 his first short story collection called *Ocios y Apuntes*. There we find a brief sketch called “The Boy with the blue spectacles” that had a pitiful “hérédó” child as its subject. We will translate a few paragraphs of it to set the tone for this paper:

- Nanny, why am I not brought out to the street?
- Because you are ill and the air does you no good; but you will see, tomorrow if you take in all your medicines we will go far, far away... all the way to your aunt Pepita’s place.
- Oh yes, far away, and we don’t come back till its dark.
- Yes, till its dark.
- And will you buy me a puppet? I want a puppet.

(...)

I love you more than both mommy and daddy. Cover my feet nanny, don’t go away, hold my hand... And the child fell asleep as the nanny deeply saddened followed the drawings in the rug.

Poor scrofulous child! He was not a child, no; he was a monster. With his enormous head, his paleness, his skinny body; he wore blue spectacles because he had a diseased eyesight, and nothing gave a more intense impression than that unhinged face and those big lenses that seemed like the orbits of a skull. He could barely hold himself on foot with his thin legs and his bulky stomach. He was a freak that produced disgusting pity... His illness had no remedy. It had been inherited from his father and he had been now two years, two long years! tortured by pills and papers, baths and ointments, teaspoons and rubbings...

(...)

Having that child had been a crime. Who was to blame? Who had bequeathed him the stains of vice and of illnesses? Remorse burdened the husband, crowding his dreams with omens, and any memory linked with childhood made his ideas bitter...

(Ángel del Campo, *Ocios y Apuntes* 1958 [1890]).

A complete set of late nineteenth century moral values and social attitudes is displayed by Micrós in these lines. His scientific outlook giving us the high brow judgement of the secular priesthood that hereditarian medicine had been promoting for several decades.

The powerful notion of pathological hérédité was crafted by Napoleonic Physicians as a device to gain access to the inner workings of State Management, and to gain power over the making of Laws. In the 1810’s Emmanuel Fodéré in his epochal *Traité de Médecine Legale* (1813) made one of the first important uses of the notion of hereditary physical constitution aimed at justifying Medical supervision of collective health, and came close to equating the physical hereditary endowment of a Nation with its moral constitution.

As Laure Cartron has shown, the 1820’s, 1830’s and 1840’s were a critical phase in post-Revolutionary France during which the place and role of the family within the tissue of the Nation was being disputed and reorganized around ideological fracture faults, and medical men through the deployment of hereditary notions acquired increasing leverage in the debates. A family group, a lineage and its health (its physical hereditary endowment) was constructed as a central device for

the collective well being of a Nation. The passing of evil dispositions, noxious taints, degenerative ills, down the family hereditary line came to be seen increasingly as a social menace. An important social (and political) call for Physicians became both the policing of familiar hereditary health, and the influencing of government and legal action in order to improve the material *hérédité* of the French nation (Cartron 2006; López-Beltrán 2007).

As is nowadays better known, the noun *hérédité* was popularized by French Physicians after the 1830's, and the powerful conceptual germ it had attached, played a leading role in the dissemination of a pan-European hereditarian stance in medical, biological and agronomical disciplines (Müller-Wille and Rheinberger 2004). Psychiatry, as is also well known, developed dramatic hereditarian notions around different sorts of pathologies (Lucas 1849-50; Piorry 1840; Dowbiggin 1993), and this linked up with the stronghold that hereditarian interpretations developed over common and troubling diseases like Syphilis, Scrofula, Consumption, Alcoholism, etc. to conform the Degenerationist and Racist ideologies expressed in the works of Moreau de Tours, Gobineau, and Morel (López-Beltrán 1992; Pick 1993). Franco-italian criminology (Paré, Lombroso, Ferri) followed suit soon afterwards with its catalogue of hereditarily predestined despicable creatures and their collection of tell-tale physical attributes (Gach 2008).

To the mind of most late 19th century physicians (including psychiatrists, criminologists, etc.) heredity was an unavoidable medical, social, political, even historical fact of phenomenal reach, much in the sense in which to most Biologists nowadays Evolution is a Fact (i.e. one can differ about the details of the actual ways in which the "fact" is produced, but its existence is undeniable). Heredity was everywhere, shaping the bodies and minds of individuals, by seeding in them from their very first moment of physical existence the good or bad elements and dispositions that their ancestors had accumulated through the ages. Once a new body had been arranged very little was left to do for educators, medics and everyone else but try to bring forth, within its fatal limits the best out of each prefigured destiny. This fatalistic stance, with its gloomy mood, was resisted within its own conceptual space with the postulation of balancing forces that "opposed" heredity and opened up the possibility of change and improvement (notoriously Prosper Lucas's notion of *inneité* or variation; see López-Beltrán 2004).

Heredity of all kinds of physical and moral dispositions came to be seen as supported by an overwhelming array of empirical observations. Children that became afflicted by the same diseases or the same depravities as their relatives (many times their parents, but not always). Deformities that lay dormant for one or two generations within a family line and dramatically reappeared in the innocent body of a newborn, to remind the breed of the sins of their elders. Heredity came to be seen, as Oscar Wilde wrote, as "the only God whose real name we know." Its patterns and routes of transmission were minutely followed, described and classified. Direct, indirect, collateral, atavistic, teleogonic, and other similar descriptive adjectives were used to demarcate, for physicians the paths followed by the characters of interest (Lucas 1849-50; Dechambre and Lereboullet 1864-69). Contained within the genealogical flow, moving down from generation to generation, the material components that defined the character and possibilities of individuals, gave shape to breeds that could be differentiated by the qualities of these elements. Families, ethnic groups, races, nations, and any other genealogical vehicles structured for the channelization of hereditary goods (or ills) were themselves shaped by the results, and could be said to possess a collective heritage, a cumulus of goods and ills that gave them their peculiar character. Heredity had effectively replaced Temperament the core concept of ancient humoral hippocratic, as the locus for our understanding of variation between humans. With the same proteic malleability and adaptability to conflicting sets of evidence, heredity formed the core of a new ideology tailor made for enhancing the interests of a small set of recently professionalized bodies of medical practitioners in liberal, meritocratic societies (López-Beltrán 2004; 2007).

We know very well that the hereditarian character of the second half of the 19th century was shared, and fostered, by the theoretical and experimental developments within natural history, cytology, embryology, physiology, agronomy, animal breeding and horticulture. As Michel Foucault described the situation, a complex web of disciplined experimental practices, interested medical programmatic stances, traditional or innovative breeding strategies, conflicting beliefs about the route to the betterment of Human populations (or “stocks”), were interacting at the ever moving borders between lore and science (*savoir et connaissance*) around the increasingly powerful gravitational center of the concept of heredity (Foucault, 1969). It continuously bemuses historians when they find the proliferation of widely divergent views and beliefs around hereditary transmission in different coeval communities, and the apparently stubborn persistence of anachronistic, outdated (*perimée*) approaches in some of them, well beyond their supposed best before date, signaled by the appearance within the authorized (*sanctionnée*) experimental, or physiological tradition. Late 19th and early 20th century medical men are particularly prone to produce this bemusement. They continued to apply what we must reluctantly call neo-lamarckian, and pan-Hereditarian views of transmission well beyond the appearance and dominance of Weissman and Mendel, ignoring – to use a metaphor in use for this story – the swing of the pendulum, or – to use the other metaphor around, the narrowing of the hourglass. It has always turned out to be a very limited, and narrow, historiographic approach to simply accuse of stubbornness and irrationality a whole community of practitioners that carry on with their projects despite there being at their disposal what appears with hindsight as a rational abandonment. It is also not profitable to turn the other way, and just pretend that it is a local irregularity that was easily erased once real science had a say. This historiography reactions tend to be common in contexts like Latin America, where we are continuously faced with historical communities that seemed to be out of date and out of pace with regards to Europe or the United States.

We have not yet fully understood what kind of project, and under what particular set of influences, gave support to the late 19th and early 20th century Medical communities adherence to a hereditarian stance. What were the particular medical, social, political and racial preoccupations that motivated them, and their personal, professional and ideological aims. We have in Micrós’ story a particular instance of a Mexican physician’s gaze. The crystallization in one circumstance of a view of society that includes Christian piety, rational and passionate regret and professional responsibility. The little “hérédo” is emblematic of a failure, not only of an irresponsible, selfish parent, but of a whole society, and particularly of its ruling, knowledgeable classes (medics, lawyers) who should have made things different through timely interventions. Or should they?

In what follows we will describe the developments of medical hereditarian projects that took place in Mexico between the 1880’s and the 1930’s, in the periods before and after the Mexican Revolution (the *Pax Porfiriana* and the early revolutionary decades). Articulated around the locus of civil regulation of marriage, specially of consanguineous marriage, and the fault line between catholic and liberal legal traditions on the one side and medical hygienist proto-eugenicist proposals on the other side, this story aims at revealing the space where the notion of hereditary transmission of bodily and moral features to future generations was actively inserted in the Mexican polis.

A brief historical sketch is needed. As many countries, but more acutely so, Mexico has been since its Independence in 1821 a very heterogeneous collective in search of its true colors, of its definition and structure, and of its adequate laws and rules. Major foundational events were the liberal Constitutions of 1857 (after the 1848 USA invasion) and 1917 (during the Revolution). Mexican families could roughly be separated according to geographic, ethnic and cultural background. Economically and culturally, the diminishing Indígena groups were being assimilated

(following a previous trend) into the lower range of the dominant “mestizo” population, which basically filled the middle ranges, from poor, to well off. The upper layer of the mestizo groups had a wide frontier with the “whiter” Europhile upper class, where a mixture of old criollo families, with some nouveaux riche mestizo and recently migrated Europeans struggled to keep the very unstable country on its feet so they could still dominate it from above. After Spain was defeated Mexican elites turned to the other Europe. France became an external political and cultural reference, for good and for bad. French medical thought was central for the ambitious Mexican elite of physicians, fighting to gain positions and effectiveness in a troublingly poor and racially stigmatized society.

Marriage as a tool for government and as a political pathway for familial social ascent (or accidental descent) has been a crucial element in Mexico. Ever since the instauration of what came to be known as the Castas Society in the New Spain, with its “pigmentocracy” the acquisition of status and wealth was upwardly oriented towards the White, Europhile families (López-Beltrán 2008). After the wars of Independence, similar arrangements were restituted, in which more or less the same boundaries were kept in place. The Castas space was now occupied by the demographically overwhelming mestizos and a mixed Mestizophile and whitening ideology was progressively developed, not without difficulties and tensions, regarding the proper way of civilizing the lagging backward indigenous and poor Mestizo population (Falcón 1996; Basave 1992). Marriage was an ever sensitive issue for families on the one side, and for moralists and social reformers on the other. Our focus in this paper, consanguineous weddings, allows us to show this with clarity. Those unions within the same family were seen at the same time as morally questionable (given the catholic, canonic morals), and politically desirable in many local circumstances outside the main cities (González Ureña 1836). The introduction of a medical discourse raising the question of the degenerationist effects of consanguineous unions, and the concomitant attempt to forbid such practices had thus a mixed reception when it came. What was optimal for a hygienist social reformer (blocking the way to hereditary pathologies that polluted the stock) was suboptimal for a patriarchal racial ideologist (who wanted to keep the family line without dark, indigenous, or even mestizo blood). This organizing societal lines were powerfully shaken and reshuffled during and after the dramatic series of civil wars and social uprisings known as the Mexican Revolution (1910-1929). The emergence of new political actors and groups, claiming their place from the lower and middle classes, the reevaluation of the prehispanic past and its contemporary presence in the shape of remaining indigenous groups, and the tremendous scare that the existence of an unruly and enormous class of underprivileged peasants, ready to go to battle under the right banner or cause, prompted a series of reevaluations that explain the outcome of our marriage story. We will begin by the end, then.

CMP or Prenuptial Medical Certificate

A remarkable triumph for the recent breed of socially oriented physicians emerging from the Revolution was the passing in 1932 of article 98 of the new civil code for the authorization of Civil Marriage. It put in place a mandatory Medical Certificate for being considered apt for married life and reproduction. The main aim was to warrant the partners that neither of them had a venereal disease (i.e. syphilis) nor a contagious disease (i.e. tuberculosis). This event can be said to have been the central point where the discourses and interests of hygienist medical men and lawyers converged after several decades of avoidance or confrontation in policy issues. Although it took several years, after 1932, for the effects of the new code to be felt in actual local marriage practices, the fact that certified physicians had, at least in principle, the power to condone or repress a couple's reproductive intention with hygienist (populational) health related grounds, situated medical authority at a locus it had aimed to occupy since the 1870's. Many things, which we will

detail below, had changed since a group of Mexican physician's had applied all the weaponry of contemporary French hereditarian and degenerationist theorizing in order to establish their right to contribute to the legislation of marriage, in a population surrounded by hereditary menaces like the Mexican seemed to be. Marriage between close relatives was a particular target. Physicians wanted to review legal options open for consanguineous weddings. Conflicting evidence existed about the capacity of consanguinity itself to produce pathological degeneration or not; especially within groups with longstanding endogamous practices. As we shall see, the series of publications that from the 1870's onwards Mexican physicians produced voicing their will to hygienically regulate marriage made little inroads into actual legislation. Let us see how and why.

Hereditary Pathology during Pax Porfiriana

The first civil movement in Mexico towards the legislation and control of consanguineous marriage crystallized in famous liberal civil code of 1871. It made some important changes since the canonical regulation of consanguine marriages: direct line relatives (both blood and affinity based), and collateral relatives up to the second grade (brothers) were forbidden to marry, and the main reason deployed for the prohibition was moral; the stability and welfare of family groups, and the keeping of basic moral decency. Health related issues weighed little, if something in the lawyers' animus. This silence becomes relatively strange, and telling, given that lawyers were by then, together with medical men, deeply immersed in hereditarian degenerationist, pathological and criminological mindsets. This silence signals to our mind a deeply held election of Mexican lawyers which we will only summarily try to explain below.

Mexican physicians that in the late 19th century addressed the subject of consanguineous marriage aimed at problematizing it, moving it beyond the moral and religious and placing it straightforwardly within an objective, scientific discussion of public health. Heredity and its pathological powers was inserted in the discussion. Consanguinity, so to speak, was chosen as a especially weak spot in Mexican reproductive practices, but to our mind it could be seen as a partial, almost heuristical step towards the bigger aim of controlling and regulating, through medical inspection, each and every marriage. Bounty that was denied to the physicians until 1932.

The social devastation that syphilis brought to 19th century Mexican population was crucial for the hereditarian campaign. As elsewhere, incurability and familial transmission were parsimoniously assumed for this disease. Apart from the treatment to mitigate the symptoms, prevention of transmission was the only available winning strategy. Especially the contagion of innocent parts outside the carnal intercourse, children. Although physicians were making very subtle analysis of the etiological routes of downstream genealogical transmission, distinguishing hereditary from congenital contagion, the main strategy remained: avoiding innocent women and children being victimized. A broader issue was of course at stake. As physician F.A.R. de Poincy wrote "it is not only the health of one or a few individuals that is at risk but also the stakes of the whole family and society." De Poincy sided with those that believed the consequence should not be straightforward legal coercion for those infected, but they should be told, and given the grave responsibility of withholding from poisonous coitus (Poincy 1883). This stance had its extreme opponents who judged the carrier of the syphilitic germ to be more than just an unhappy afflicted person who should be made responsible and cautious, but a potential menace to society, especially to the sector of young, virginal, "casadera" girls that were the potential victims of their criminal acts. The latter is inferred of course from the fact that the most probable source of syphilitic infection for young men was their interaction with prostitutes (Quétel 1986).

In the tradition of French legal medicine the theme of consanguineous marriage was taken on board by several Mexican physicians in the late 19th century. Luis Hidalgo y Carpio is a good example, he criticized the extreme views of two French authorities Marc Boudin and Auguste Voisin. The former had sided with British author Arthur Mitchell in attributing to consanguinity an unending list of hereditary pathologies among which deafness, mental retardation and albinism were prominent. Voisin, on the other side of the spectrum, had argued that consanguinity itself was innocent of producing any hereditary ill. Its real effect, reflected in the statistics used by Mitchell and Boudin, was increasing the intensity of the symptoms of an already present disease (Voisin 1865; Mitchell 1864; Boudin 1862). Hidalgo y Carpio, after considering the evidence and situating it in the Mexican scene, concludes, that “consanguineous marriages generally degrade the human stock (race) and produce deafness, idiotism, imbecility, and perhaps other diseases, but its influence is not so fatal that it could not be mitigated or nullified through good hygienic circumstances” (Hidalgo y Carpio 1869). Given the fact however that in Mexico the State was far from able to provide such external help, “it would be most prudent to forbid consanguineous marriage up to the sixth degree in collateral line [up for instance to second cousins], according to civil computation, that is to say to forbid it among uncles and nephews, among first cousins, and among second cousins” (Hidalgo y Carpio 1869).

Only a couple of years before the 1871 civil code, Hidalgo y Carpio makes a strong indictment of the conservative attitude of lawyers that were not willing to (and eventually did not) extend the prohibition. He however did make a counterbalancing argument, perfectly suited for his racially anxious clientele, adhering himself to the then current idea that people should marry into their racial group, and not outside, as the homogeneity of racial elements brings forth virtuous combinations, whereas mixture leads to ill assorted results.

Hidalgo y Carpio was thus not a radical prohibitionist. He felt the need to study carefully the arguments of those colleagues that wanted an overall medical vigilance of marriage based on hereditary threats. They had argued not only consanguineous marriages were a menace, but most unions in which one or both partners are patently or latently afflicted by hereditary taints. Hidalgo y Carpio deployed a very strict analytic argument to show that pathological heredity although a reality was nevertheless very poorly known and most of its regularities were only conjectural. No restrictive legislation that entered into the intimate, private domain of marital choices should be advanced that was based on such fragile evidential basis. It should be up to well informed families, he argued, to make the decision about allowing or stopping a marriage with someone tainted by a hereditary ill.

Several other Porfirian physicians addressed the specific issue of consanguinity in marriage (which shows that it was a peculiar worry for many Mexican families) and in varying degrees they tended to adopt similar stances as Hidalgo y Carpio (Rodríguez 1875; Ruiz 1881; Ruiz 1883; Villarreal 1899). All in all the consensus was that consanguinity ought to be carefully studied and in cases where there is a previous case of hereditary disease in the family a strong recommendation for avoidance of close marriages should be issued. And in cases where the pathological consequences of consanguinity had been corroborated the reinforcement of a previously acquired disease was preferred as the explanation. Moreover a particular tension can be read out of many of the writings and manifestos of Mexican physicians. On the one side the thrust to participate actively in social public affairs, especially by being taken into account by legislators; and on the other side an ideological resistance to invade the private decisions of individuals and families. In Porfirio Parra, an influential positivist physician, member of the “científicos” we find an example of this tension between interventionist and liberal proclivities. In a work at the end of the 19th century (1895) he aims at cutting the wings of his interventionist colleagues by concluding after a long historical and logical disquisition, that consanguinity itself is innocuous, and that it is the ill

or bad ingredients that are potentiated in such close unions. So if people are confident of their good stock they might as well marry their kin. However Parra seems to have a hidden agenda shared by many contemporary physicians. The liberal stance on consanguine marriages opens surreptitiously a space for medical interventionism in all marriages in general, When arguing that consanguinity it is not bad in itself but only when two damaged constitutions get together, the same statement can apply to any marriage, consanguine or not. This is to our perception the theoretical beginnings to the claims for medical supervision of all marriages: potential hereditary pathology in descent implies for everyone a submission to medical observation and marriage regulation informed by physicians.

However weak, interventionist medical encouragement got its first “triumph” when in 1884 the legislators introduced as one (of a few) causes for divorce the evidence of having a hereditary disease (Ministerio de Justicia 1884).

Matrimony under Liberal Law

Marriage in Mexico was successively legislated through two civil codes during the troubled 19th century (Civil Codes of 1871 and 1884). Civil union was pushed forward as the only operational one, and religious marriage sidelined and put under strict surveillance by liberal governments. As we have seen consanguinity was an issue that was addressed successively by legislators. A need was felt to clarify within the socially unstable Mexican scene the bounds of allowed consanguinity. Although rhetorically it was claimed that successive codifications of marriage laws were improvements of previous, politically tainted efforts, after careful scrutiny we consider it a fact that, it was Justo Sierra O’Reilly’s mid 19th century transposition and adaptation of French legislation, in which he modified slightly the canonical tradition, the basis for all marriage legislation (Sierra 1861). Sierra O’Reilly’s work set the tone for posterior legislators’ decisions, which were following closely the moral and the political feelings of the ruling classes rather than any external criteria. The repetitive attempts at influencing the law by the medical men were basically frustrated during that century. On general terms one could guess that legislators are more finely tuned to the moods of the local ruling groups, which somehow needed to sustain the morality and cleanliness of their marriage practices, while at the same time not imposing too severe restrictions over families who practiced some kind or other of endogamy for economic or racial (“pigmentocratic”) reasons. The physicians, on the one hand, were more engrained in wider, hygienist and utopian visions of probable French inspiration, in which the doctors could function as a sort of lay clergy that could participate in the crafting of healthy, racially virtuous, Mexican people. In the late 19th century the separation between these two otherwise close sectors seems to follow the disciplinary bounds. The question was which of the two groups, lawyers or physicians, were to shape the normative frame and influence policy. An important question was at the heart of this issue: what is to be privileged, the State’s control of the reproductive activities of its citizen’s through a moral and legal codification that gives priority to the collective welfare and the sense of future greatness, or the local elite’s right to remain as such, setting the upper bounds and the direction of self managed improvement, decided and acted upon by the families themselves, without obstruction from the State and its apparatuses. It seems clear the latter option was the one taken by the “científicos” during the Porfirian period. All the elements were in place for a more active and hygienic and hereditarian medical intervention on marriage and reproduction. Prudence and probably lack of utopian solidarity with the mestizo majority won the day. Improvement was seen as possible, but limited, and slow. It was only with the upheavals and transformations that La Revolución brought, that the order of values changed, and the same cocktail of elements produced a different result. National eugenics and medically interventionist marriage laws became possible.

Eugenics for a Peasant's Revolution

In the last years of the Porfiriato the State participated in many international scientific and technological meetings (Tenorio 1996). In 1899 and 1902 Mexican physicians were sent as delegates for the international conference on profilaxis of syphilis and venereal diseases. Syphilis continued to be a worldwide scourge and the Mexican scene was not an exception. The old Porfirio Parra, Jesus Zenil and Ricardo Cicero were chosen to travel to the meetings. The circumstances and conceptual settings around those meetings provide a useful window to look at what we may call a rapid change of pace with regards to the evaluation of the medical community of the role of state intervention in reproductive morals. The hate of syphilis (“syphilofobia”) can be said to have been growing in Mexico as much as elsewhere and the need to develop a more active set of interventionist policies acquired increasing urgency. It is not easy to judge how much of that movement is due to external influence, how much it is due to the arrival of new generations of physicians with different set of values. Some practical and theoretical developments were certainly incorporated. But overall there is a striking continuity through the decades in the hereditarian language and its gloomy degenerationist connotations. It is the implications for policy that began to change.

One of the most radical new Mexican “syphilographers”, E. Lavalle adopted a heavily ideologized and charged set of Christian analogies to depict his gloomy vision. Lavalle wrote that among the capital sins there are two that should be under the exclusive domain of physicians (overriding the priest, the lawyer and the judges claims), those are “Gluttony and Lust”. He addresses the latter and defines then the modern sin against hygienic chastity as “any voluntary manifestation of sexuality, material and active, even physiological, each time it does not lend us a warrant of absolute morality which can only be attained under the matrimonial contract (...) Outside of marriage any manifestation of this kind is ‘useless in the actual conditions’ and exposes to contagion of disease that ruin the individual and degenerate the species.” Sexual salvation can only be attained following a combination of four basic ideals: “the chastity of bachelors, fidelity of the wedded, precautions of celibate fornicators, and the docility of all the injured” (Lavalle 1911). With this last precept, Lavalle sets the tone that was to resonate with an increasing number of syphilographers that without any doubt are the precursors of what became in the 1930’s the Mexican eugenics movement (Suárez y López Guazo 2005). The need to localize, record, denounce, and impede the reproduction of those afflicted by syphilis. They, and everybody else should recognize the need of the sacrifice of their individual right to sex and reproduction in order to warrant the superior right of the collectivity to health, and freedom from degeneration and disease in the family lines. Twenty years later an important sector of the medical men and professionals had caught and developed such hints. “It is absurd that in the name of a mythical respect for personal liberty to allow with arms folded the marriage of persons that will only bring forth diseased children, degenerated ones, to whom life only offers the option of suffering, and whom aside from their disgrace will represent a heavy burden, or a danger, for the society that receives them” (Mantilla 1934). By this period the effect of this kind of shift of balance against liberal policies and in favour of interventionist ones was felt when Eugenics became part of the official discourse:

The State cannot abandon at the will of individuals, at their higher or lower level of education, and their often questionable morals, such a matter, it has to directly intervene, many times against the individual’s will, in order to impede the procreation of diseased children, weak and ill formed, and the best way to do it is establishing a federal law that forces all persons wishing to marry, a document that under medical authority establishes that the person is free

from all disease that may be transmitted to the children or the partner (Departamento de Salud Pública 1935).

This was closer to the desire of the medical community of making health a control valve for marriage and in so doing gaining political leverage. How was this road walked? The first official suggestion about the need of a medical intervention through a marriage health certificate was made optional in 1917 in the Family Relationships Law (*Ley de Relaciones Familiares*), written in the context of the new Constitutional Government lead by Venustiano Carranza. This document revealed a renovated vision of the links between marriage, freedom and the State:

(...) it is necessary in order to protect the human species, to disable legally from marriage those (...) who show physical incurable impotence, those affected by syphilis (...) or any other who suffer an incurable and chronic disease that could be contagious or hereditary (...) because all those cases parents transmit to their offspring pathological heredities than make them weak and disabled for intellectual and physical duties (...) all this resulting in a damage against the fatherland (*patria*) (México 1917).

Eventually, however, syphilis, which had been the focus of early interventionists was understood as a curable infectious disease. Degeneration discourse was then revived in support for an effective and obligatory medical verification of marriage. First in 1926 with the *Código Sanitario de los Estados Unidos Mexicanos*. Later in the 1932 Civil Code (*Código Civil*) which was by then on step with the Mexican Eugenic movement; near in time and precepts to the institutionalization of Mexican eugenics movement, which saw several of its targets achieved and institutionalised.

Summarizing, immediately before the downfall of the Porfirian regime the call for a special medically based regulation of marriage was clearly voiced in the work of syphilographers. Those who agreed were still a noisy minority, as the conservative liberal values were still pretty much in place.

The Revolution brought an acceleration of the pace in a direction that has already been signaled. These were progressive movements in the aspiration of creating a national federal medical certificate for marriages. Syphilis and allied venereal diseases were this time the focus, and not anymore consanguineous marriage although at the very bottom of the concerns about cleaning up of marriage appeared systematically a fear against degeneration.

Mexican eugenic movement has received much attention recently (Suárez y López Guazo 2005; Stern 2000). Their noisy presence in the debates of the 1930's have become unavoidable for understanding the shores to which French and locally bred hereditarian medical thought arrived and the Statist Revolutionary shape they acquired. The 1932 Marriage certificate is at the same time a climatic end to the long series of interventions we have summarized here. The story still has many layers to unpack. Beatriz Urías for instance has cleverly revealed to us how all these utopian and revolutionary activity of the Revolutionary medics and lawyers is closely knit with racist projects to transform (modernize) indigenous or backward mestizo populations, and how the *mestizofilia* that apparently became an egalitarian revolutionary ideology only masks a racist notion that the only way forward is the abandonment of the Indian burden. Both racially and culturally (Urías 2007) things have moved on, but much less than we sometimes think. To take a critical look at how our medical geneticists are promoting the so called Mexican Mestizo Genome project and at the kind of racist inferences they are liberally making confirms that we are still there, closer to the Porfirians than is healthy.

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IDEAS OF MEDICAL DOCTORS ON HEREDITY IN MEXICO IN THE LATE 19TH CENTURY

Ana Barahona

Introduction

In the years between 1859 when *The Origin of Species* was published, and 1865 when Mendel's Laws were announced, Mexico was going through military conflicts between conservatives and liberals. The latter had to wait to take control due to the French invasion and the imposition of Maximiliano's Empire (1862-1867) by the alliance between the Mexican conservatives and the Austro-Hungary Empire. At the end of the Empire marked by Maximiliano's execution in 1867, the triumph of the reformist movement was consolidated. The reconstruction thereafter that the government of president Benito Juárez made until 1872 not only included political and economic aspects but also public education, science, and culture.

Thanks to the proclamation of the Organic Law of Public Instruction in 1867 some high level education institutions were reorganized such as the National School of Medicine which had 36 years of existence. The National Preparatory School, the School of Naturalists, the School of Engineering, the National Observatory, the National Academy of Science and Literature, and the Mexican Society of Natural History were created. The Learned Society "Gabino Barreda", the Medical Society of Mexico, and the Philopatric Society and of Beneficence of the Alumni of the School of Medicine were also founded. Along with their forms of diffusion like the *Anales de la Sociedad Metodófila Gabino Barreda* (Annals of the Gabino Barreda Learned Society),¹ the *Gaceta Médica* (the Medical Gazette) and the *Porvenir Filoiátrico* (Philopatric Future), respectively, contributed to the discussions about heredity that took place in Mexico. These forums, new or reorganized, allowed to consolidate, institutionalize and professionalize scientific activity for they included the preparation of new scientific frameworks and the formation of new students.

Although other communities such as the botanists, zoologists, and veterinarians had representation in the academic world, it was the medical community the most dedicated to the study of hereditary phenomena like reproduction, illnesses and malformations. The community of physicians developed the notion of heredity, be it in the sense of appreciating certain traits of diseases that appeared repetitively in some bloodlines, or as traits present at certain age groups, that are considered incurable.

This discussion on heredity comes before the introduction of Mendelism in synchrony with what was going on in other parts of the world. With the introduction of evolutionism in Mexico at the end of the 1870s, the vision that heredity is the passing on of joint moral and physical qualities from parents to offspring, whose laws or tendencies can exist, was consolidated.

During the 19th century, medics looked to ban old beliefs and false myths about man and his diseases. According to Cházaro, "to break with the past, that medicine denied that pathologies were entities that hazardously take over the body. Instead, a rhetoric which made health and disease normal and pathological states, phenomenons in the body, was consolidated. Disease

¹ Gabino Barreda (1820-1881), mexican medic, philosopher, and politician, he continued his studies in Paris from 1847 to 1853, where he met and was a student of Auguste Comte. He introduced positivism in Mexico and was founder of the National Preparatory School.

became an experience, accessible through observation, following the regularities of its manifestations” (Cházaro 2002, p. 17). It was like this that a new way to understand diseases was inaugurated, new methods of study were originated and medical discourse changed radically.

In the first part of this manuscript, the topic of pathological heredity in the work of the Mexican physicist Juan M. Rodríguez will be presented. In the second part, the work of Mexican physician Porfirio Parra on morbid heredity will be analyzed. Finally, I will talk about variation and heredity in the work of José Ramírez. As it will be shown, all these bodies of work were influential and meaningful in the study of heredity in Mexico in the second half of the 19th century.

1. Pathological Heredity: The work of Juan M. Rodríguez

A discourse on heredity appears at the end of the 19th century in Mexico, basically from three different approaches. The first one refers to the laws under which it acts and has to do with normal heredity; the second distinguishes and classifies diseases with hereditary causes and those that respond to different causes; and third, the knowledge of the causes of the alteration of the germ in hereditary diseases. The last two take us to pathological heredity, this means the passing on of diseases where the environment is an important cause of the manifestation or not of the diseases, as well as the damage in germline cells.

The interest in knowing what diseases have a hereditary cause is part of an old medical research program developed basically in Europe, and that was developed with strength in Mexico at the end of the 19th century. It is about finding evidence and presenting proof that helps eliminate the possibility of a different origin, e.g. in the case of teratology, any important event during gestation, or environmental causes.

Among the most recognized medics of the time we find Juan María Rodríguez Arangoiti (1828-1894) obstetrician and teratologist.² At the beginning of the 19th century, the practice of obstetrics was considered as denigrating and in the hands of midwives. In the last years of the Spanish Colony, in the Royal School of Surgery, theoretical and practical knowledge on obstetrics were not taught. This happened after the war of Independence in 1833, when public instruction was reformed and an Establishment for Medical Sciences was created with the subsequent creation of the class of Obstetrics, whose chair was occupied by Rodríguez in 1867 (Rodríguez-Pimentel 2003, p. 526).

In that time, vaginal tact to diagnose the presentation and position of fetuses was not accepted by women and was censored by the general public who considered it immoral. Dr. Rodríguez, together with Dr. Ortega, introduced abdominal auscultation as a substitution method. Later on, Rodríguez modified this version for when the fetus became movable. Once accomplished, a cushion and bandage on the side were used to maintain the presentation of the fetus. He made important contributions to Mexican obstetrics like identifying anomalies of the pelvis caused by rickets, exostosis and echondroma in Mexican women, describing the order of presentations and fetal positions, diverse pelvic and fetal measurements, as well as relating certain labor difficulties

² Teratology was founded in France in the 19th century by Geoffroy Saint Hilaire, and refers to the study of anomalies and monstrosities. Geoffroy posted his theory of embryonic under-development, in which he sustained that the formation of monsters followed precise rules and invariable laws, this means, that monsters had a perfectly normal origin and belonged to a unique plan of creation. Defects occurred when the embryo stopped at some point during its normal development, the stages that reproduce the phases of a normal evolutionary series that ranges from inferior to superior beings, producing a physical lesion after conception due to mechanical causes governed by natural laws. See Geoffroy 1822.

with women's height. He introduced the use of forceps instead of finger insertion in the fetus's mouth, preventing complications such as the baby's jaw luxation (Cházaro 2005).

In addition to obstetrics he became interested in hereditary phenomena and carefully studied polydactyly in three generations of the family of Don Anastasio Alegre, which he considered undoubtedly hereditary, while the erratic behavior of others like ectromelia and extrodactyly, didn't allow to conclusively establish whether they were hereditary or not (Rodríguez 1870; 1871; 1871b). In his 1870 article for example, he mentions that "it's a known fact that parents just as they pass on to their offspring their features, physical constitution, and even their intellectual and moral qualities, they frequently also pass on the diseases and anomalies of organization that are affected in one or more parts of the body. Sometimes the father, sometimes the mother, pass on, at times to males, at times to females, at times both at the same time, the rich heredity of their virtues, their talents, their beauty and their graces, or in others they pass on larvate germ of their disgusting vices, their cruel diseases, their repugnant deformities, that are perpetuated in families like original sin, this means, from one generation to the next" (Rodríguez 1870, p. 217).

Concerning the production of monstrosities, in 1887, Rodríguez sustains that the cravings and desires of the mother have no effect in the formation of the fetus. If this were so, "what would become of the human species? In the term of a few generations you wouldn't see anything but extravagant shapes, disgusting and strange; because, in effect, there are very few women that during their pregnancy stop experiencing desires, scares, worries, deviations, or that don't fix their imagination on some weird and strange object" (Rodríguez 1887, p. 303). As a means to understand the causes of deformities, Rodríguez explained that "in order to someday be able to explain complex phenomenons such as those that refer to anomalies and montrosities that coexist with profound brain lesions, I think it's precise to separate them with careful methods, and that research be conducted beyond the fetus itself, for in many cases you'll maybe find the cause of them in its annexes or the uterus" (Rodríguez 1871, p. 136).

Rodríguez looked among these external causes; accidents, blows, or hard falls, something that could explain the deformity of a child and exonerate the mother of the responsibility of conceiving monsters. "No. Plastic force can only be modified and countered by another force more positive than imagination: by physical violence, like the one that is produced by blows, the fastening of the womb, falls, violent commotions" (Rodríguez 1887, p. 321). For Rodríguez some of the causes of malformation and hereditary diseases are due to material alterations of the elements in reproductive cells before fertilization. "If to what observation teaches you, you add the results of direct experimentation in animals... it is evident that some anomalies, at least, come from the influx that some disturbing causes exert on some organs of the fetus, in their formation or development. Supposing this, I ask, why not admit the influx of these or other disturbing causes on the human germ at the time it is fertilized or before? What is so violent about admitting that the human ovum or the sperm even before they come into contact, suffer one of many modifications that in case they don't inhabitate the formation of a new being, might corrupt and predispose them to develop in a manner more or less different than the ordinary? (...) What does this depend on? The temperature, preassure, electricity, the quantity and quality of the components... But if it weren't like this, how would we explain why a polydactyl, pass on to entire generations the same vicious organization with which they were born? (...) Don't we see people affected by tuberculosis, hysteria, epilepsy, sons, grandsons and great-grandsons of consumptive, hysteric, and epileptic people?" (Rodríguez 1871c, p. 221).

However, many medics still thought that moral and strong impressions could cause failed pregnancies. Rodríguez thought that the influences of these causes would have to be examined carefully for there existed "facts cited in its favor that form an imposing cumulus" (Rodríguez 1872, p. 37). Through heredity, the mother passed on diseases, temperament, strange behaviors,

and was capable of molding the offspring's shape. The notion of heredity, understood not only from a biological perspective, refers to memory: the monster remembered the corporal suffering of its mother, and she did this through the uterus. With teratologic sciences and their emphasis in material causes, the vehicle would have to be a concrete, observable, and localized organ (Gorbach 2000). "... And is it that most of the monstrous products that women give birth to and that resemble animals and other natural bodies, must not only attribute to themselves the vicious concretions formed in the membrane of the uterus, at times by the mucus, at times by the blood, at times by secretions that belong to it, but also, and principally, to the polyps of the womb, moles, the placenta retention, which take an unusual and casual shape. We notice that many other products that women usually produce should be judged in the same manner, specially those that appear under the shape of frogs, toads, mice, snakes, eagles, as well as those that resemble the heads of other birds, of rams, of fish, or with the shape of vegetables" (Rodríguez 1870b, p. 57).

To Rodríguez, the causes of monstrosities were uncertain. "The occasional determining and efficient causes of these and other anomalies of that sort, are (and will be for a very long time) an impenetrable mystery to those who dedicate their time to teratologic studies" (Rodríguez 1888, p. 106). The phenomenon of heredity in these monstrosities fell completely on the mother, although the father was not discarded as possibly responsible. But teratologists and medics only dealt with women and sometimes the fathers were unknown, "all in all, if the present case is due to hereditary influences, it could happen that the cause could go back to the father's bloodline which, unfortunately, is beyond the reach of our research" (Peón Contreras 1872, p. 274).

According to Gorbach, this concern wasn't in the biological mechanisms of heredity, but in individual identity, in the passing on of temperaments, conducts or morphological characteristics (Gorbach 2000). It would seem that the physical and moral traits were tied to biology and that medicine could classify and find the pathways of deviation and control them. For that, measures were included, like: stopping uterine excesses such as masturbation, rest, sitz baths and cold water, opium and diethyl ether, tranquility, and avoid excessive desires at the time of appeasing the effects of female hysteria (Rodríguez 1888).

Heredity can be modified by the environment, education, changes in habits, or the introduction of consorts that come to impose other uses and ways of life. Like Rodríguez, other Mexican physicians give the environment and social surroundings great weight in the passing on of diseases or the predisposition to inherit them.

We see that in the work of Rodríguez and other medics of the time, an aspect can be seen where heredity or what is inherited lives with what is acquired from another generation and accidents during gestation. This means, the shaping of an individual is given by the intervention of heredity in previous generations, such as intrauterine accidents (González 2007). There was no conclusive explanatory framework that could help understand the hereditary phenomena to which we have referred. In the words of Rodríguez, "the explanation of these extraordinary facts is far from the reach of science, like many other wonders of organization, despite what ostentatious and sterile modern philosophy says" (Rodríguez 1887, p. 321).

Towards the end of the 19th century, in the medical community, the theory of mixed heredity which was accepted in Europe prevailed. However, there existed another moment of tension between medics regarding considerations of the monstrous and aberrant, that is what is teratological. Mexican medics "were incrustated in a natural manner in the argument about the monsters of the 18th century in Europe" (Gorbach 2000, p. 43), they declared themselves in favor of epigenism, this means, they stopped viewing the monster as a punishment from God or product of maternal mistakes, and visualized it as a normal product whose origin was similar to that of other individuals, where the normal and the pathological were governed by the same laws.

It can be observed in the work of Rodríguez and other physicians like Ramón López y Muñoz, that there is a conception of heredity where variation and adaptation are treated as synonyms and describe the modifying action of the environment in the conformation of individuals and in particular in the heredity of diseases (López y Muñoz 1875; 1879; 1880).

2. Morbid Heredity: The work of Porfirio Parra

In his work *Tratado de la Herencia Natural* (Treatise on Natural Heredity) published in two volumes in 1847 and 1850, the learned French alienist Prosper Lucas introduced the concept of innateness as the source of variation and the opposite of heredity. In this work he proposed to show how certain patterns of appearances through generations could be explained by the contrary actions of the principles; the variation or innateness; and heredity. Although this work had impressive influence, not only in France, but in other countries, the fact that an organic force conceived only to get rid of aberrations and irregularities existed, turned out to be an idea with little credibility among the majority of naturalists, including Porfirio Parra in Mexico, for it was considered that many other explanations that accounted for the apparition of variations existed, specially congenital ones.

Porfirio Parra y Gutiérrez (1854-1912) distinguished himself as a medic, philosopher, novelist, and poet. He entered the National School of Medicine in 1873 and completed his degree in 1878 with *Ensayo sobre la patogenia de la locura* (Essay about the pathology of madness); he occupied several chairs, these were: Hygiene, Medical emergencies, Descriptive anatomy and External pathology in the School of Medicine. He was a distinguished student of Gabino Barreda in the National Preparatory School, and is considered the teacher of the second positivist generation that his teacher initiated.

Porfirio Parra, in his 1897 article “¿La ineidad es una fuerza antagonista de la herencia, o es una forma de esta última!” (Innateness is an antagonistic force of heredity, or is one of the forms of the latter!) explains that in medical sciences, knowledge related to the causes of diseases and the way to combat them, are pending to be acquired. “Although it is not entirely accurate, like our ancients believed, that knowledge of the causes is the only object of science and forms science as a whole, we can’t fail to recognize that research on the causes forms one of the culminating points of the scientific program. Well, research on the causes being biological phenomena, in which the study of diseases is included, offers enormous difficulties due to the complexity that is peculiar to the organism, presenting in them a bulk of difficulty that teachers of inductive logic have called the plurality of causes and the mix of effects” (Parra 1897, p. 544).

Parra recognized the influence of Louis Pasteur and Claude Bernard in the changes in medical empiricism towards medical sciences, and for him, the disease was a modification of a normal state, ruled by biological laws; pathology was an amplification of physiology and this was a particular case in biology. Therefore, there are laws that apply to all living beings, be that they enjoy good health or that they suffer some disease. Living beings can modify themselves during their lives due to pressure from the environment that surrounds them, and all transitory and lasting change in the environment will tend to produce the corresponding changes in the organisms. “Now, the disease being an organic modification and the environment one of the most effective agents of change, it is rightly inferred that many diseases must recognize by cause, the deadly influx that the environment has exerted over the organism” (Parra 1897, p. 547).

When it comes to hereditary diseases, Parra attributes them to the morbid impulse in one generation that is transmitted to subsequent generations in an uninterrupted manner. “It is that according to the current state of science, the living being is subjected to two great influxes: first, the environmental influx, that works on it from the moment it begins to exist; second, the

hereditary influx, that in virtue of an ancestral impulse, more or less far, exists long before the living being starts to feel its effects. General etiology is clearer due to the influx of this distinction, for all the morbid causes can be divided in two categories: those that come from changes in the environment, and those which are resolved in the hereditary influx” (Parra 1897, p. 548).

Parra argues and strongly criticizes the concept of innateness introduced by Prosper Lucas in 1847, which proposes a third influx or cause not reducible to the environment or to heredity but to morbid impulse, developed in the germ at the moment of its conception (or a short time after). If this impulse were latent during embryonic development, then the disease would manifest at any time in life; but if the morbid impulse was latent for less time than that of gestation, then the individual would be born with a congenital disease, but not hereditary. “...with the purpose of identifying or proving the reality of the force he believed he (Lucas) discovered, he presented it as an antagonist of heredity and capable of counterbalancing or neutralizing its effects, and since the forces are indestructible, they can only be neutralized by other forces that work with sufficient energy in the opposite direction. He believed to have proved that innateness existed, citing facts in which hereditary influx was annihilated and not by means of the environment” (Parra 1897, p. 549).

Cases such as cancer or madness, where hereditary influx seems not to have any weight, but can not be attributed to acquired illnesses either. They could be, according to Lucas, explained by innateness. However, Parra denies the existence of innateness, of which he says, confuses not only medical matters, but also anthropological, moral, and social matters, areas where the real and positive concept of heredity plays a predominant role.

For Parra, heredity can explain the cases presented by Lucas, for heredity is an organic force that presents itself in many forms, such as conservative heredity and accumulative or progressive heredity. “When a living being limits itself to pass on to its descendants the characteristics it received from its progenitors, without passing on any of the modifications it acquired in the course of its life, heredity works as a force that tends to perpetuate an organic type, opposing the modifying influence of the environment. In such cases, this action, as effective as it may be, has no duration but that of a human life, and the modifications begotten by the environment of the organism, endowed with this hereditary form, have a perishable existence like that of the individual that presents them, without acquiring the immortality that heredity passes on, when it fixes them and makes them live on throughout generations” (Parra 1897, p. 551).

But this is not the only form of heredity (the one called conservative, for it tends to preserve a determined organic type preventing the action of the environment that tends to change it), because if it were, Parra says, then living beings would be unchangeable, in species there would only be varieties but not breeds. “But there isn’t only one form of heredity... the contrary exists, that in which the individual passes on not only the qualities it inherited from the parents, but those organic modifications that were verified until the time it became a progenitor” (Parra 1897, p. 551).

This type of heredity is progressive or accumulative, Parra would say, for the changes are always in progress in the organic structure. It indicates that the hereditary influx of the parents is added to the one of the descendants, resulting in an accumulation of hereditary transfer.

In this work Parra also shared the vision of José Ramírez as we will see later. Heredity is a force that can adopt a conservative and progressive tendency at the same time, but with different intensity. “In accumulative (or progressive) heredity a phenomenon worthy of attention is verified. It is very well inferred that there could be antagonism between the organic structure that an individual received through heredity, and the modifications that in the course of a lifetime this structure suffered, and that it will pass on to its subsequent generations; then the interesting fact

of the conflict between hereditary transfers presents itself. Taking into account the various hereditary influxes that an individual can pass on to its children through heredity, and the struggles or conflicts that can emerge between them, we realize the apparent irregularity of concrete facts, better yet we'll realize that if we take into account that hereditary influx could remain in the germ more or less time" (Parra 1897, p. 552). Without a doubt this is the last article that can be found in the 19th century that is about heredity and other related phenomenons.

3. Heredity and Variation: The work of José Ramírez

During the second half of the 19th century the role of conceptions on heredity starts to have fundamental importance in the debates about disease, moral and psychologic heredity, and variation. One of the consequences of the expansion of the empirical base of heredity on other biological phenomenons was the diversity and types of conceptions of heredity were presented, depending on the point of view, as modifying or stabilizing, progressive or conservative, under the general assumption that heredity of the acquired characteristics resulted in a confrontation between heredity and variation. Many naturalists of the time were interested in understanding which of these two tendencies persisted in different characteristics. In the words of López Beltrán, "Some authors started to identify heredity as the main source of stability in taxonomical groups, stating that it was the general corporal structure, the frame or constitution as they are, more than particular traits, what should be considered as affected by heredity. Other authors preferred to emphasize the traditional association of heredity with the passing on of traits relatively secondary, that when settled down and naturalized in a genealogical group originate the variants and breeds (...) These positions created the polarization and tension in which the concept of heredity and its variants were analytically and empirically explored" (López Beltrán 2002, p. 103). When connecting heredity with reproduction and generation, the problem of which characteristics are passed on and why, and which are not, emerged.

The writings of José Ramírez (1852-1904) were important in the medical discussion about heredity of that time. When darwinist discussions in Mexico began, he was named curator of the Museum of Pathological Anatomy of the National School of Medicine in 1877; later on, in 1879 he became a member of the Society of Natural History of which he became secretary and president; he also belonged to other academies like the Society of Geography and Statistics, the Antonio Alzate and the National School of Medicine. It was in the National Medical Institute, founded in 1888, where Ramírez practically realized all his scientific activity.

An important part of his international relations consisted of being a part of the French Society of Hygiene and of the Museum of Natural History of Paris, which were decisive in the development of his ideas on heredity and evolution. He travelled to France in 1888 to the Universal Exhibit in Paris, where more than 800 described and catalogued Mexican species of plants and animals were sent with valuable medical information, and where Ramirez would study some bacterial diseases in the Pasteur Institute. Because of his contributions to botany and zoology, the French government codecorated him with the Legion of Honor.

In 1878 Ramírez explained "that in order to understand the teratological and embryonic origin of variants, races and species, it is precise to remember rapidly the laws of heredity and of adaptation, and understanding these laws, we will describe the phenomena of reproduction in organized beings" (Ramírez 1878).³

³ This article is considered by some authors like Moreno de los Arcos, as one of the texts on biology in which Darwinism was shown as of common use in mexican science at the end of the 19th century. (Moreno de los Arcos 1984).

Ramírez understood the phenomenon of heredity as the passing on of physical and moral characteristics of the parents to their children in sexual reproduction as a purely mechanical fact, the result of the material union of two reproductive organisms. “That heredity, even in man and in sexual reproduction of superior organisms, be it a purely mechanical process, the immediate result of the material union of two reproductive organs, just as in asexual reproduction of inferior organisms, is a fact that nobody can doubt” (Ramírez 1878, p. 239).

For Ramírez it is the ovum the one that “contains the substance that will form the new individual”, while the sperm only “produces the fertilizing substance”. Then, the growth and development of an individual is reduced to a simple multiplication of the “cells” that constitute it, once the gametes mix “due to an unknown reciprocal action, that gives impulse to the development of the new individual” (Ramírez 1878, p. 238).

With Ramírez heredity and evolution appear together, because to him heredity and adaptation are vital activities, whose result is evolution, although heredity and adaptation were used as synonyms. His 1878 article explains that the possibility exists that certain monstrous characteristics be passed on and could conform new species.

Variation appears when there are differences in the internal and external environments and is responsible for evolution. For Ramírez, evolutionary change can happen be it by the constant and prolonged action of abnormal external conditions, or modifications in the life habits or the use and disuse of the organs; by leaps or production of monstrosities; or by hybridization. “The first two have adaptation as a near cause and changes in nutrition as a far cause; the third is caused by the principle of mixed heredity” (Gaona 1998, p. 19). Adaptation in general, and heredity, are considered as expressions of a fundamental physiological property common to all living beings, as a vital inseparable manifestation of the idea of an organism. “(...) The laws of adaptation can be placed in two different series, the series of indirect or of direct or immediate laws. You can also call the laws of the first category, laws of actual adaptation, and of the second, laws of potential adaptation” (Ramírez 1878, p. 241).

Ramírez distinguished two groups of hereditary phenomena, on one hand the heredity of transmitted characteristics, and on the other hand, the heredity of acquired characteristics. “The first heredity is called conservative, and the second, progressive heredity. This distinction is founded on this very important fact: that individuals which belong to any plant or animal species, pass on to their posterity, not only the properties they have inherited from their predecessors, but also the individual properties they have acquired during their life. The latter are passed on in virtue of progressive heredity, the first in virtue of conservative heredity” (Ramírez 1878, p. 239). We can say then that for Ramirez conservative heredity is the inheritance of ancestral characteristics, while progressive heredity happens when the individual inherits the characteristics it acquired during its life.

We see how Ramírez was convinced and accepted heredity of acquired characteristics in a Lamarckian style and believed that these characteristics produce adaptation of the organisms to their environment, and also teratological characteristics. Adaptations and malformations produce change and can give birth to species and breeds in nature.

Conclusions

We can say, with Gonzalez Soriano, that before the introduction of genetics in Mexico, medics confronted practical problems, this means, hereditary diseases and “that due to this they discussed using the theoretical, clinical, and practical tools within their reach. In the Mexican medical community of the 19th century, the way to access the discussion on heredity was through pathological heredity, and this is shown in an important series of discussions that had heredity as

the central topic, but which are illustrative of the role it played in matters oriented by more clinical, pathological, and therapeutical concerns” (González Soriano 2007).

The Mexican medical community of the 19th century accepted hereditary transfer as a fact, and accepted that what defines a disease is the combination between what is passed on and the environment, between the constitution received from the parents in conception and what occurs in the uterus and the exterior. We have seen how conceptions of heredity at the end of the 19th century proposed that physical traits as well as moral traits can be passed on, including of course, diseases, malformations or defects. For that, clinical, therapeutic and prophylactic tools were designed for the study, treatment, and prevention of some diseases and physical traits, with an important influence of ideas from France that explained heredity and variants.

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PART II

THE RESTRICTION OF HEREDITY: CASES IN GENETICS AND EVOLUTION

SWITCHES AND BATTERIES: TWO MODELS OF GENE REGULATION AND A NOTE ON THE HISTORIOGRAPHY OF 20TH CENTURY BIOLOGY

Vivette García and Edna Suárez

Abstract

The first models of gene regulation for both prokaryotic and eukaryotic cells were published in the 1960s, at a time when identification of DNA as the “genetic material” had vindicated classical genetics and materialistic approaches of heredity became the norm in molecular biology (Barnes and Dupré 2008). The operon model developed by F. Jacob and J. Monod (1961), and the model of batteries of genes developed by R.J. Britten and E.H. Davidson (1969), illustrate different features of this “theoretical hourglass”. Both models incorporated cybernetic metaphors, but whereas the operon naturalized the informational properties of genes, the model of batteries assigned a function to the structural properties of eukaryotic genomes in terms of communication, command and control. Also, while one model (the operon) focused on sequences, the other focused on genome organization. We describe in detail these two cases, comparing them against a problem-solving view in the historiography of 20th century biology, in which models of regulation are analyzed as if they were solutions to scientific problems, expressed in the all-pervading language of information and the design of molecular genetics. We argue that treating the history of gene regulation as one of solving problems dismisses the role played by scientist’s efforts to propose models that are specific to their field and have differential value. It also homogenizes the diversity of experimental systems, metaphors, techniques and tools, and provides accounts that over-emphasize theoretical continuities. Our story will show that the landscape of regulation studies in the second half of the 20th century is rich and quite diverse in the types of questions asked and the types of answers given.

1. Introduction

In *The Logic of Life*, François Jacob argues that in science, “an epoch or a culture is characterized not so much by the extension of the knowledge acquired as by the questions posed” (Jacob 1990, p. 13). The questions posed by scientists have been reformulated many times as *scientific problems*, and the answers to these questions have taken the form of *solutions*. In this way, for example, the question “how is gene expression coordinated within cells?” is taken to be the problem of gene regulation, and the *operon* model published by Jacob and Monod in 1961 is considered to be the solution to this problem for prokaryotic cells. The same *problem* was later formulated for eukaryotic cells, for which Britten and Davidson provided a first *solution* in 1969.

This conceptualization of scientific pursuit can be inscribed within the general view that science is fundamentally a problem-solving activity. This view has had implications for the philosophy of science, as we can gather from several models of scientific change proposed in the past century (Kuhn 1962; Laudan 1977; Giere 1988), which measure progress by the type and amount of problems solved (generally by theories). It has also had implications for the methodology of science – or what some authors call an “epistemology of scientific discovery” (Bechtel and Richardson 1993), where the cognitive importance of human reasoning pathways that enable scientific problem-solving are underscored (Newell and Simon 1972; Bechtel and Richardson 1993; Darden 2006). Not surprisingly, this view has also made its way into the historiography of science.

Robert Olby's *The Path to the Double Helix*, which traces the events that led to the solution of the problem of the structure of DNA, as well as some biographies of the field (for instance, Anne Sayre's *Rosalind Franklin and DNA*), not to mention histories that are loyal to the actors' perspectives (like Judson's *The Eighth Day of Creation*) and autobiographies (such as Crick's and Watson's), are emblematic of the exercise of historiographically applying the problem-solving view of science. Some of these narratives are in fact very similar to what Myers (1990) describes as the stories of discovery narrated by the involved actors: "these texts provide a new chronological framework that defines a singular event and gives it meaning as the transformation of one state – an unstable state of ignorance, overconfidence or confusion – to another ordered state in which there is now knowledge" (Myers 1990, p. 103); or as the transformation of an unsolved problem to a problem with a solution.

Autobiographical stories aside, the problem-solving view has also played a role in more critical histories of molecular biology, where historians following the research of specific problems in a certain period of time and region identify the ways in which these were addressed, how certain (local) research traditions and programs provided the tools to solve them, and how all of this contributed to defining molecular biology as a discipline.¹ Research directed towards problems of heredity in France before and after World War II has been analyzed by several historians including Burian, Creager, Gaudillière, Gayon and Zallen, all of whom have arrived at unanticipated conclusions regarding the history of molecular biology, and provided important insights into "extra-Mendelian" contributions to mainstream molecular genetics. This historiographical choice of scrutinizing "major scientific problems" was carefully outlined by Richard Burian in 1993, where he described not only the core set of problems around which the laboratory of Jacques Monod was organized, but also how such problems were articulated, transformed and approached – the process he called "task definition". On our view, two features of molecular biology have made problem-based historical accounts of regulation possible and attractive.

First, the *incognito* status² – as Marcos (2009) would put it – of the genetic code metaphor, whose rapid incorporation into biological discourse prompted the expectation of finding solutions to a few central problems (e.g., how information flows from nucleic acids to proteins or, in its more generic form, from genotype to phenotype).³ Philosophers, historians and sociologists of science have treated this covert use of the metaphor and its resistance to revision extensively (for references, see Suárez 2007).

Second, the undeniable *construction*, by leading scientists, of research programs and agendas encompassing the so-called central problems (such as those that Burian identifies for Monod's research group), which brought together questions that had been treated traditionally by separate research fields, by different teams of workers, and that – in practice – continued to be very diverse in their nature and their treatment. The active exchange of people and information between research teams at Harvard, Berkeley, the National Institutes of Health (NIH) and the Pasteur

¹ There are, of course other ways to address the history of molecular biology. Studies that focus on a particular set of techniques and tools that enable research at the molecular level (Morange 1998; Kay 1997), studies that focus on long-lasting trends on biological research and attend to their institutional and broader social milieu (Abir-Am 1982; Kay 1993; Chadarevian and Kamminga 1998), and studies that focus on objects, material traces and cultures (Rheinberger 1997) are examples of historiographical approaches that are currently used.

² According to Marcos (2009), a metaphor travels time *incognito* insofar as it is "no longer considered as such (except when scrutinized under the bright light of a historical study); this feature of some metaphors is the result of a selective process that rationally justifies our trust in their representative capacity" (Marcos 2009, p. 19).

³ See Keller 2002, 2003 for a discussion on the standardized characterizations of these *problems*.

Institute, conformed what Creager and Gaudillière (1996) have called “a network of cell regulationists” in the 1950s and early 1960s, of which Jacob and Monod were key figures. A meeting that took place at Cold Spring Harbor in 1961 on “cellular regulatory mechanisms” attests not only to the existence of this network of influential scientists, but also to the fact that the problems discussed there included both gene and metabolic (or cellular) regulation, on the grounds that – despite their obvious differences (one dealt with genes, the other with proteins) – they were both addressed from a combination of bacterial genetics and enzymology.

Both gene and metabolic regulation were treated as problems to be solved under the umbrella of cybernetics and information theory despite the practical and material differences concerning the subjects and methods of study. Enzymes and enzyme kinetics, on the one hand, and bacterial genetics, on the other, constituted distinguishable fields of research that nevertheless serviced the new and encompassing informational “discourse regime” (Kay 2000). In this context, terms such as negative feedback and information flow were adapted to both families of problems irrespective of their differences. But there were other ways of thinking about regulatory mechanisms towards the end of the 1960s. At the time when Jacob and Monod were moving away from “a study of metabolic pathways informed by genetic practices” and “towards model-building rooted in the physical chemistry of proteins [i.e., allosteric regulation]” (Creager and Gaudillière 1996, p. 3), other scientists were advancing theoretical models of gene regulation for eukaryotic cells. These efforts did not take place within the approach favored by the network of regulationists that Creager and Gaudillière report on.

For historiographical purposes, then, the study of regulation can be treated as a comprehensive problem within the framework of genetic determinism and according to the informational and cybernetic agendas of molecular biologists during the 1960s.⁴ Creager and Gaudillière (1996) have chosen this approach to unravel the professional relations between different research groups, and although our treatment of the operon model will rely on several problem-based accounts, it is one we will not be developing any further. For comparative purposes, we examine the work of a pair of researchers that did not belong to the aforementioned network of regulationists and thus, in a relevant sense, cannot be classified as being part of the molecular genetics mainstream. Only retrospectively, and conflating all research on the genetic control of cell differentiation, could their work be interpreted as being part of the same problem, and hence, as part of the same story. While we are aware, as Rheinberger has cautioned, that “no historian can, or should, completely abstain from the opiate of hindsight” (Rheinberger 1997, p. 6), we believe that scrutiny of the Britten-Davidson model will shed some light on the diversity of issues (traditions, research programs, tools) surrounding regulation studies during that time. It will also show why a problem-solving view is, in this case, an inadequate historiographical tool. Our strategy will also make evident a discontinuity not only between prokaryotic and eukaryotic models of gene regulation, but also between Britten and Davidson’s first and current models. This result is in agreement with Abir-Am’s critique of “primordial mechanisms” in the history of biology (Abir-Am 1985).

2. Two models of gene regulation

One of the most thorough examinations of the history of the operon has revealed that it articulated three major research traditions: physiology, enzymatic biochemistry and microbiology – basically understood as bacterial genetics – (Gaudillière 1993), and it set off investigation of a new set of

⁴ Indeed, members of the network of regulationists shared the notion of *feedback inhibition*, but they also shared the need to keep their meetings “informal” and “small enough” (letter from Bernard Davis to Jacques Monod 1957, quoted by Creager and Gaudillière 1996, p. 9) to uncover the diversity of tools and goals that could only be revealed by comparing individual agendas.

problems: gene expression and regulation in bacteria. The model proposed by Britten and Davidson incorporated experimental evidence previously obtained by Britten and others in the field of molecular evolution, which itself was the result of the articulation of different types of investigative traditions (Suárez 2001). Britten was a physicist-turned-molecular biologist working at the Carnegie Institution in Washington, and Eric Davidson had obtained his PhD in 1963 under molecular biologist Alfred Mirsky. Although both models attend to the mechanisms of gene regulation, the problems they tackled and ignited differ substantially, and there seems to be no continuity among them. Their perceived degree of success varies considerably as well. Our examination of the ways in which cybernetic and information metaphors made their way into these models suggest that each one contributed different aspects of the geneticist view characteristic of 20th century biology.

Although the first available report of the appearance of the phrase ‘genetic regulation’ in a journal publication concerned the fungus *Neurospora crassa* (Suskind and Kurek 1959), historians of biology have traditionally focused on the *lac* operon as a founding model, adducing that this event marked a turning point in the development of the conceptual apparatus of molecular biology (Saget 1978). This model was a very successful representation for prokaryotic regulation, and the authors obtained the Nobel Prize in 1965 (together with André Lwoff) for their work on the *lac* system. Different historians of biology have given extensive and detailed narratives of this episode; in what follows we will rely on them, but we will do so from a critical stance.

According to the received story of gene regulation, Jacob and Monod were working not only on different subjects and experimental systems (lysogeny and the nature of the prophage in *Pseudomonas pyocyanea*; and bacterial metabolism and enzymatic induction in *Escherichia coli*, respectively), but also on different floors at the Pasteur Institute in Paris. In 1954, shortly after Jacob obtained his PhD, Monod became director of the Institute’s biochemistry department and moved from the attic (where he once shared a laboratory with Lwoff) to his own premises on the ground floor. Genetic analysis of bacteria was performed in the attic, where Jacob and Wollman remained. Monod continued to study biochemistry and enzymatic biosynthesis in *E. coli*. In spite of the distance, Jacob and Monod were still meeting in the hallways, where they arrived at the conclusion that both their sets of empirical evidence could be seen as a manifestation of the *same* phenomenon: “And it was only little by little that – by analyzing each of the systems – we noticed that there were strange similarities between the two systems, and that eventually, it lead to a shared experimental model” (Jacob, oral history, Peoples Archives). The newfound analogy between lysogeny and induction of enzymatic biosynthesis became the central theme of the Harvey Lecture on “Viral Functions” that Jacob dictated in the summer of 1958 (Morange 1998).

The story of the second model is very different. It was brought forth as a theoretical model that recommended a regulatory function for the recently discovered highly repetitive sequences or “satellite DNA” in higher (eukaryotic) cells, and even though it was the most noticeable model available for genetic regulation in eukaryotes during those years, it did not share the success of its prokaryotic counterpart. This does not mean that the model went completely unnoticed. A common impression among those who were practicing biology at that time is that “the model inspired a lot of thought which may indirectly have been involved in framing research programs” (as Jim Griesemer pointed out in personal communication to Vivette García, August 28, 2008). In particular, it inspired research on development and evolution (see for instance Hwu et al. 1986), a ramification that is not taken into consideration in the usual accounts that address the history of gene regulation as a subplot of the history of molecular biology, even though Davidson and Britten were not the only ones to adopt this broader perspective *within* molecular biology (a notable example is Emilé Zuckerkandl). One of the most praising comments made about this model came

from the renown embryologist Conrad Hal Waddington, who had been working on formal models about how gene regulatory products could generate developmental phenomena since the 1930s.

Over the past three decades, Britten and Davidson have explored their theoretical ideas empirically, they have continued to be very active in the subject of gene regulation, and the notion of *gene regulatory networks* – developed by Davidson and collaborators in the 1990s – has become one of the most fruitful conceptual and technological tools in the field of evolutionary developmental biology (see for instance Peterson and Davidson 2000; Davidson, McClay and Hood 2003; Davidson and Erwin 2006; Davidson 2006). This fact is reflected in the recent interest that Davidson’s work has raised among students of science and philosophers of biology (such as Evelyn Keller and Manfred Laubichler).

The theoretical model that Britten and Davidson proposed in 1969, however, has been mostly overlooked by historians of science, with the exception of the historian Michel Morange (1998), who marginally addresses it in his book on the history of molecular biology. It could be argued that the model played a negligible role in the expansion of molecular biology because it failed to immediately bring about an experimental research program. Nevertheless, molecular and developmental biologists have seen it as a first and valuable answer to the question of genetic regulation in metazoan development. For example, two reviewers of Davidson’s books render him an “author with an exceptional historical perspective” (Scott 1986) or an authoritative scholar of the regulation of animal development insofar as he has “studied and written” about it “almost since the beginning of this subject” (Dawid 2006). When treated in the latter sense, this model endures a historical fate analogous to the operon model: contemporary biologists dealing with genetic regulation invoke it as a precursor of current gene regulatory networks.

According to Morange, Britten and Davidson’s model gained importance only after split genes were identified around 1977. The models of gene regulation for eukaryotic cells, he says, then “adopted the spirit, if not the letter, of Roy Britten and Eric Davidson’s model” (Morange 1998, p. 207).⁵ But there is no correlation between split genes and regulation in the works of Britten and Davidson. The apparent asymmetry that Morange describes (that it wasn’t until the identification of split genes that the Britten-Davidson theoretical model was referred to by renown molecular biologists, such as Francis Crick) is built upon a historiographical bias that adheres to a problem-solving view: split genes were identified within the molecular-geneticist approach of “normal” science, they were incorporated into the central dogma of molecular biology – which Crick (1971) reinterpreted in terms of genetic regulation, and thus split genes provided some kind of solution to the problem of gene regulation. Under this interpretation, the Britten-Davidson model can then be subsumed to the history of molecular biology.

But the landscape of eukaryotic gene regulation during the first six years after Britten and Davidson’s 1969 publication continued to be very speculative, and proposals came from different disciplines and areas of expertise, as we can gather from other theoretical models published between 1969 and 1974 (Georgiev 1969; Tsanev and Sandov 1971; Paul 1972; Cook 1973). A deeper inquiry into the trajectory of Britten and Davidson’s model, which emerged during the process of *molecularization* of eukaryotic biology, is required in order to understand and broaden the scope of research being done on gene regulation during this time.

⁵ Scientists were indeed very enthusiastic about this phenomenon, one that accounted for a specific difference between bacteria and higher cells, but laboratories did not take up the study of split genes as a means to understand regulation. Pierre Chambon, one of the scientists who first identified split genes in chicken DNA, continued to study the ovalbumin split gene as a means for understanding gene organization in terms of coding/intervening sequences, rather than as a means for understanding the control of gene expression and regulation (see Breathnach, Mandel and Chambon 1977; Mandel et al. 1978).

3. The operon model revisited

Formulation of the operon model required advancement in at least two problems: lysogeny, studied by Jacob, Lwoff and Wollman, and bacterial growth and biosynthesis, studied by Monod.⁶ But it did not originate as a *solution* to any of these problems. Even before being able to individuate the problem of regulated gene expression it was necessary to establish an analogy between lysogeny and the synthesis of galactosides in bacteria. Although Élie Wollman was Lwoff's assistant and his project dealt with the comparative analysis of lysogeny, he and Monod collaborated on the study of growth inhibition and formation of adaptive enzymes in bacteria infected by phages. Since 1947, enzymatic adaptation was considered to be the best way to study biosynthetic properties of bacteria during phage development (Peyrieras and Morange 2002). Then came Jacob and Wollman's work on bacterial conjugation and the results of the "spaghetti experiment", which showed that "the male chromosome was injected into the female at a constant rate" (Jacob 1979). The renown PaJaMo experiments performed by Jacob and Monod with American biochemist Arthur Pardee (Pardee, Jacob and Monod 1959) pointed in the direction of a regulatory mechanism involved in the synthesis of enzymes. Jacob recalls:

We all noticed the analogies between the results of zygotic induction [originally termed *erotic* induction] with lysogenic bacteria and those of the PaJaMo experiments with the *lac* system... In both cases, a group of normally silent genes could be triggered and become expressed at will; in both cases, this silence was due to a single, distinct gene; CI in phage *lambda*, *i* in the *lac* system; in both cases genetic analysis showed that the wild type allele of this gene was expressed by a cytoplasmic product, a repressor blocking in some way the expression of the other genes. *These analogies appeared so great that the postulate of an identical mechanism seemed to me inescapable* (Jacob 1979, p. 99, our emphasis).

Despite Jacob's own recollections of straightforwardness, the nature and the unity of the mechanism were being disputed. Two apparently contradictory mechanisms, induction and repression, stood in competition. Induction by lactose was necessary for the synthesis of the enzyme in "males", but as soon as its genetic material was transferred to the "female" (via "erotic" or zygotic induction), not only was induction no longer required, but repression of the spontaneous synthesis of β -galactosidase occurred. Two hypotheses could account at once for both antagonistic regulatory mechanisms. The first, termed general induction, understood repression as inhibition of a yet unknown kind of induction. General repression, on the other hand, described induction as the blocking of an unidentified sort of repression. An agitated debate began. Leo Szilard (the

⁶ André Lwoff, who had invited Monod to join the Institute as laboratory director in 1945, and who directed Jacob's doctoral thesis, worked primarily on a phenomenon that was regarded by many as an experimental artifact. Lysogeny, or the infection of bacteria with phages, and the subsequent liberation of viral particles, was rejected by several members of the Phage Group, including Max Delbrück and Alfred Hershey, but rejection of lysogeny was not a mere scientific disagreement. At a Cold Spring Harbor reunion in the summer of 1944, the Phage Group agreed to restrict their investigation of bacteriophages to a set of types that specifically infect *E. coli*. This agreement, the Phage Treaty, sought to standardize practices between laboratories and to facilitate comparison of results (up to that moment, each laboratory possessed a private collection of phage and bacterial hosts). Restriction of the experimental system also disallowed investigation of lysogeny. Phages T1, T2, T3, T4, T5, T6 and T7 are all virulent. Analysis of lysogeny required the use of a temperate phage (*lambda*) and a large bacterial cell that permitted micromanipulation with bacteriological techniques (*Bacillus megatherium*). Bacteria first presented some resistance to infection and the liberation of viral particles could be observed after induction of the prophage with UV light (Brock 1990). Lwoff and Guttman experimentally determined the existence of lysogeny and, as Lwoff would put it, "lysogeny obtained permission to enter the Phage Church" (see also Holmes 2006). Jacob's doctoral thesis versed on the nature of the prophage as a genetic determinant that is incorporated to the hereditary material of the bacterial cell.

Hungarian physicist who had participated in the Manhattan Project) defended the merits of general repression, while Monod retorted by appealing to the virtues of general induction.

The debate was *not* settled theoretically, however. The postulation of a series of concepts such as regulator and structural genes, operons, promoters, and later, messengers, aided in the visualization of the phenomenon. But mostly they were *names* they gave to epistemic things in their experimental systems (to borrow Rheinberger's well-known terminology), rather than being theoretical constructs without empirical basis. Without these names, the ideas of induction and repression made no sense as gene regulatory mechanisms.⁷ Just to give an example, Pardee, Jacob and Monod performed not one but *dozens of experiments* that involved mating of specific bacterial strains, recombination studies, the β -galactoside assay (which itself involved the ad hoc synthesis of a number of chemicals), experiments on the expression and interaction of closely linked genes, and enzyme kinetics analyses, all of which are reported in their 1959 paper on "genetic control and cytoplasmic expression."

One of the metaphors first used by Jacob to describe the mechanism governing enzyme synthesis in the *lac* system was an electronic one. Shortly after the analogy between lysogeny and bacterial metabolism was established "came the idea that repression (or induction) operates not progressively, but rather discontinuously, like a *switch*, by a yes-or-no, an on-or-off mechanism that involves only two states" (Jacob 1988, p. 301). The switch plays a role in Jacob's recollections of the 1950s. He claims to have come up with the idea while watching one of his sons play with an electric train. The child could make the train travel at a different but constant speed just by rapidly turning the switch on or off. Jacob believed that enzyme induction in lysogenic bacteria could work according to the same yes-or-no system.

Monod objected to the switch on two accounts. First, he appealed to enzyme kinetics. Jacob had relied on the fact that differential β -galactosidase synthesis was always linear but, as Monod pointed out, the rate of synthesis varied as a function of the nature and concentration of the inducer (lactose). Monod believed that "this could not be reconciled with an on-or-off system of synthesis" (Jacob 1979, p. 100). Second, Jacob's use of vague physical notions such as "inertia" in his defense of the switch did not aid in convincing Monod. This metaphor *alone* was not rich enough to incorporate the complexities of what they already knew or to generate further knowledge. As Lily Kay has pointed out, during the 1960s, "organization, or hierarchical order of life, was predicated on *specialization* modeled after ideas of division of labor" (Kay 2000, p. 46, our emphasis). Concepts of chemical and biological specificity, which were at the core of Jacob and Monod's understanding of the *lac* system as a cybernetic one, were substituted rather swiftly with metaphors of information (Kay 1997; 2000) In this sense, the elucidation of the operon model contributed to the (restricted) conceptual repertoire of molecular biology – to the narrowing of the theoretical hourglass. But it also enriched the field with its development of different experimental systems, and with the stabilization of genes (molecularly understood) as the stuff that regulates protein synthesis.

4. *The eclipse of the operon and the developmental question*

The development of molecular biology during the mid-1960s and 1970s has been reconstructed as passing through a "classical" or "academic" period (Stent 1968) or as a period of "normal science" (Morange 1998). Another (and in our view more accurate) description of what was happening at

⁷ Monod was keen on finding the most adequate terms to describe biological phenomena. Once he was appointed laboratory director at the Pasteur Institute, he formed the *naming committee*, which offered options and approved the publication of any neologism that was coined in the attic (see Lwoff and Ullman 1979).

this time emphasizes the development of techniques and procedures that would make possible the “extracellular representation of intracellular configurations” (Rheinberger 2009). It was towards this particular interventionist aim that many of the leading scientists that had helped to build the basic concepts, tools, techniques, and procedures of molecular genetics, including the genetic code and the operon model, decided that it was time to address the facts of eukaryotic organisms.

What I mainly wanted was to change material. I wanted to have something – instead of bacteria, I wanted an organism that had eyes, that looked at you and that had a soul. And bacteria don’t really have souls. Hence – and then, there were a lot of discussions. Because the question was – if we want to go on to superior organisms, which one? So two of my friends, Seymour Benzer and Sydney [Brenner], had already taken the plunge. Seymour was working on drosophila... And Sydney had chosen the small worm. So I asked Sydney if I could borrow his small worm, which he did with disgust. He lent it to me, but he wasn’t very happy that I was working with it. But I didn’t really enjoy working with the small worm. Which means that I didn’t work with it for very long. And I thought – drosophila was the perfect system, a tremendous system because of genetics, because of the possibility of, of really – but I thought that importing drosophila to Pasteur, with a sufficiently big enough group so as to be able to do something, wasn’t very reasonable. Whereas the mouse, which is an organism on which bacteria, viruses and everything is tested – it was perfectly reasonable to do a little mouse genetics and to do it with a mouse. Hence the mouse. Which, obviously, didn’t allow me to do as much as with drosophila. But nevertheless, I thought it was much more reasonable to work with mice at Pasteur than with drosophila (Jacob, oral history, Peoples Archives).

Developmental, metabolic and genetic regulation, as well as the application of experimental techniques to new fields (including evolutionary biology and the neurosciences) dominated this phase (see Crick 1982). However, it began to be clear that Monod’s motto, “what is true for *E. coli* is true for the elephant,” did not always work, and that this generalization could not be easily transformed into a research program. Immediately after its publication the operon model entered an obscure phase known as the eclipse, during which it was repudiated by several molecular biologists. But there were also those who devoted time and effort to confirm it. In Harvard, Walter Gilbert and Benno Müller-Hill (1966) isolated the *lac* repressor; and Mark Ptashne (1967) purified a gene product of the *lambda* phage. With the confirmation of the operon also came new challenges and aspirations. Jacob and Monod hoped “to find in superior organisms similar units of regulation, that functioned according to identical principles, although with the complexity required, of course” (Jacob 1998, p. 68). The challenge consisted in showing the relevance of these principles for the development of multicellular organisms from a single cell, which involves a process of differentiation.⁸ The postulation of regulatory genes opened an investigative route: the “operon hypothesis” or a model of regulation of cell differentiation based on repression (Morange 2008). Geneticists Boris Ephrussi and M.C. Weiss approached Jacob and Monod’s model seeking answers to the questions of development. But even these initial advocates soon tired of fidgeting with it. “Despite the intensity of the efforts deployed by Ephrussi, Weiss and others, the variability of the observations did not lead to any major breakthrough” (Morange 2008, p. 23). The embryologist C.H. Waddington objected that molecular biologists trained in microbiology (he was implicitly referring to Jacob and Monod) did not understand the importance of differentiation, of the determination of cellular fates; “This implies that we need a ‘double action’ control mechanism, with one action concerned with determination and the other with activation” or de-repression

⁸ Within the elaborate problem-based historiography that we described in section 1, Burian (1993) has stated that “Monod substituted a technically well-articulated [genetic control of protein synthesis in *E. coli*] for a prior, ill-defined general problem,” namely that of cellular differentiation “in the elephant and all multicellular organisms” (Burian 1993, p. 393).

(Waddington 1969, p. 639). On these grounds, Waddington declared the model of repression a waste of time.

While the operon did suggest the existence of genetic regulatory mechanisms in eukaryotic cells, “[...] for a very long time, the regulation models and the ones of negative things, the people that were working in the regulation of superior organisms in particular were never referring to our stuff” (Jacob, oral history, Peoples Archives). Or they referred to it to show the impossibility of finding operons – more specifically, structural genes – physically linked in the eukaryotic genome (Britten and Davidson 1969, p. 352).⁹ In fact, many of the tools and concepts that had enabled scientists from a previous generation to determine the molecular genetics of bacteria could not be easily adapted or applied to eukaryotic cells – they were unable to solve the problems identified for eukaryotic control of differentiation and development.

5. Britten and Davidson’s eccentric model

Contrary to the rich experimental cultures of the Pastoriens and the group of people involved in metabolic and gene regulation at the time, the model that Roy Britten and Eric Davidson proposed in 1969 did not have a single experiment to support it. On that point they openly said, “We make no attempt to arrive at definitive statements regarding these proposed mechanisms; obviously evidence is not now available to support any model [of gene regulation in higher cells] in detail” (Britten and Davidson 1969, p. 349). Nevertheless, they claimed, first, that lots of experimental data – published by others and referring to different theoretical and experimental contexts – were compatible with their model or even *required* some of the features included in their model. Second, they claimed that their goal was to provide a “relatively concrete commitment [that] will induce discussion and experiment” in the future (ibid., p. 349). The discovery of large fractions of highly repetitive DNA in the genomes of eukaryotic cells by Roy Britten and David Kohne (1968) stood as a structure in search of a function. Here was a phenomenon exclusive of higher cells which, in the eyes of Britten and Davidson, demanded a functional explanation.

At the time of their 1969 publication, Davidson was still at Rockefeller University, where he had obtained his PhD under the direction of Alfred Mirsky. Mirsky is considered one of the pioneers of molecular biology and a well-known expert on the structure of biological macromolecules (proteins). In 1936 he published a celebrated paper with Linus Pauling on how the tertiary structure of proteins affected their function. In the 1950s Mirsky was a well known defendant of the thesis that DNA has a repetitive “boring” structure and, accordingly, he thought that DNA could not be the hereditary material. Davidson inherited from Mirsky the commitment to explaining complex biological functions in terms of structure, something reflected in his long-time focus on regulation at the transcriptional level.

Meanwhile, Roy Britten was already a researcher at the Department of Terrestrial Magnetism of the Carnegie Institution in Washington. Since the late 1950s he had been working on the physical chemistry of nucleic acids with Ellis T. Bolton. The Carnegie laboratory had a close interaction with the laboratory of biochemist Paul Doty at Harvard University.¹⁰ Doty and his student Julius Marmur focused on the physico-chemical properties of DNA and realized that DNA lost its double helical structure when heated in a solution, and that the Watson-Crick helical

⁹ Operons in eukaryotes such as the flatworm *Caenorhabditis elegans* have been more recently identified, however. See Blumenthal T (2004) *Brief Funct Genomic Proteomic*, Nov; 3(3): 199-211 for a review of such findings.

¹⁰ Paul Doty was not just the founder of the Department of Molecular Biology at Harvard, but also of the Belfer Center for Science and International Affairs at Harvard, and a renowned expert in international affairs during the Cold War.

structure was recovered when the solution was slowly cooled. They called this phenomenon *renaturation*, and later it came to be known as *DNA hybridization*.¹¹ DNA renaturation was rapidly adopted by other research teams as a very effective experimental tool. Sol Spiegelman and Alexander Hall first used it to “trap” and purify messenger RNA (Giacomoni 1993), and later on Hall took the technical skill to the Carnegie group at Washington. There, Bolton and Britten applied it to different problems, given what they called “the versatility of the technique” (Suárez 2001). Bolton, in particular, was very interested in evolutionary problems and started using the hybridization of DNA obtained from two different species as a measure of their phylogenetic relationship. The proportion of hybridization between two species of DNA allegedly gave a quantitative measure of the genetic relationship between the species. It was in the context of the molecularization of evolutionary problems that Britten started to analyze the experimental anomaly that led him to the discovery of a fraction of highly repetitive sequences, of more or less 400 bp, which was “universally present” in eukaryotic cells (Britten and Kohne 1968). Since 1964, Britten had wondered if this fraction of highly repetitive sequences had a function, as he stated in the Yearly Reports to the Carnegie Institution during that time (Suárez 2001).

In the need to find a function for these sequences, Britten stood basically alone. A few other scholars, notably molecular evolutionist Emile Zuckerkandl, shifted their focus to eukaryotic regulation and paid some attention to repetitive sequences. Zuckerkandl, however, devoted most of his research to the effects of regulatory mutations in speciation events and the evolution of organisms, a field that has exploded since the discovery of homeotic genes (Zuckerkandl 1997). Most molecular biologists, however, including Leslie Orgel and Francis Crick, thought that these repeated sequences were just “junk” or “selfish DNA” (Orgel and Crick 1980). Britten, by contrast, thought that he had stumbled upon a portion of the eukaryotic genome that performed some kind of function and played an evolutionary role; Davidson agreed with him: “The existence of repeated sequences in higher organisms led us independently to consider models of gene regulation of the type we describe here” (Britten and Davidson 1969, p. 355). The quantity of DNA in repeated sequences, the frequency of repetition, the precision of the repetition, and the distribution pattern of repetitive sequences were central elements in their model, and the only molecular-empirical data that pointed in the direction of a regulatory function.

The same year that these authors published their theory, the Soviet biologist G.P. Georgiev published an article where he sought to establish in eukaryotic cells the structural equivalences of the elements described for the control of gene expression in bacteria. According to Georgiev, the “structural organization of the operon” in eukaryotic cells is based on the existence of “non-informative” regions (putatively, repetitive sequences) of DNA that function as repressors.¹² Although some features of eukaryotic DNA were consistent with this model (for example, the existence of more “non-informative” than “informative” sequences), the match was rather vague and unsupported by specific eukaryotic mechanisms. For Britten and Davidson, the goods were in *activation*, not in repression: “The model [proposed by Georgiev], as described, does not suggest how coordinate control is established when, in a given cell state, many genes need to be *activated*

¹¹ By 1958, hybridization provided evidence that the Watson-Crick model could account for the duplication of the hereditary material and that the helix would not be super-coiled when opened, as some molecular biologists had argued (Max Delbrück championed this critique; see Holmes 2006 for further discussion).

¹² Gregorii Pavlovich Georgiev was one of the first to practice molecular biology in eukaryotes. Throughout the late 1950s and 1960s, he performed studies on the structure of the cell nucleus, on the determination of nuclear RNA, on chromatin structure, and he also collaborated in the development of methodological approaches that became widely used for addressing these issues, such as the electrophoretical method for separation of nucleosomes and subnucleosomal particles, among several others.

together, nor does it indicate how in a different cell state a different but partially overlapping set of genes could be *activated*” (Davidson and Britten 1973, p. 599, our emphasis). The importance that these authors gave to activation marked a distance between them and those who, like Jacob and Monod, continued to focus on repression. Their model also aimed to satisfy the developmental requirement of accounting for differentiation, insofar as “Cell differentiation is based almost certainly on the regulation of gene activity” (Britten and Davidson 1969, p. 349).

According to Peyrieras and Morange (2000), the operon model “formed the conceptual basis that led molecular biologists to move from the study of bacteria to the characterization of the complex processes involved in embryonic development and the control of behavior” (p. 420). Developmental geneticist Walter Gehring (well known for having identified the Homeobox – a highly conserved protein-coding region – in the 1980s) thought this way. Gehring claims to have been “inspired by the famous work of F. Jacob and J. Monod on gene regulation in bacteria” and to have “contemplated the isolation of *Drosophila*-analogs to DNA-binding proteins such as the *E. coli lac* repressor in the 1960s” (interview by Weber 2004, p. 73). But not everyone else working on developmental genetics did. Genetic analyses (using classical genetics methods) on a number of *Drosophila* mutants were being carried out at Caltech by E. Lewis, who developed a model for the negative control of segment identity in developing fly embryos (Lewis 1978). Weber (2004) has pointed out, quite accurately, that the molecularization of developmental biology – especially the study of the genetic control of development in *Drosophila* – cannot be attributed exclusively to the application of molecular genetics (or any of the repression mechanisms through which gene expression could be controlled), but rather to the combination of classical genetics methods (such as chromosomal mapping and the isolation of mutants) with recombinant DNA technology. This combination provided the experimental resources for cloning developmental genes, thus establishing them as “worthy objects of molecular studies” (Weber 2004, p. 72). In this sense, Weber’s story of the “quest for developmental [control] genes” *neither* plays a subsidiary role in the more general history of molecular biology, *nor* does it support a thesis of historical continuity between molecular and developmental genetics.

The distance between Davidson and (operon-inspired) Gehring is exemplified in the following quotation from Gehring’s book, subtitled *The Homeobox Story*: “[I]n an international meeting in which I presented these results [i.e., our finding of putative homeodomains], Eric Davidson sat in the front row and conspicuously shook his head during most of my talk, showing disapproval of my wild hypothesis” (Gehring 1984, p. 53).¹³ Years later, on the other hand, Davidson and Lewis were on the same transcriptional bandwagon. In 1995 Davidson organized, together with Roy Britten and Gary Felsenfeld, a colloquium on the “Biology of developmental transcription control” (held October 26-28 at the National Academy of Science in Irvine, CA) on which Davidson reported: “The colloquium was graced by an after dinner presentation by Ed Lewis, replete with his well-known home movie describing BX-C functions in *Drosophila*” (Davidson 1996, Colloquium paper). Gehring did not attend the colloquium.

As Michel Morange pointedly remarks, Britten and Davidson’s model “lacked the simplicity of their predecessors” (Morange 1998, p. 178). The paper starts with a long list of neologisms and meanings (two out of eight pages), where they introduce the *producer gene*, *receptor gene*, *activator RNA*, *integrator gene*, *sensor gene*, and *battery of genes*. The first five constitute “the minimum number of classes of elements” that can carry out the processes they deem necessary for regulation, and the rest of the paper is organized as to “explore evidence that suggests the existence of the

¹³ Gehring points out as well that, “a few years later, however, he [Davidson] became a convert and was happily cloning homeobox genes from his beloved sea urchins” (Gehring 1984, p. 53). Needless to say that the choice of an experimentally resourceful model organism (*S. purpuratus*) did wonders for Davidson’s work. The last section of our paper will deal with these successes.

elements of the model” (Britten and Davidson 1969, p. 352). In general terms, when an inducer (a hormone, for example) binds to a sensor gene it causes an integrator gene to produce activator RNA that specifically binds to a receptor gene which in turn causes transcription of a producer gene and, thus, the production of a protein. In current terminology, sensor genes control the transcription of producer genes by way of a signaling cascade. The diagrams used are instantiations (rather than generalizations) of ways in which the elements of the model can be integrated in order to perform a regulatory function. This is in accordance with one of the model’s underlying assumptions, which Britten accounted for many years later, namely, that “The control of development is by means of local interactions, rather than global control mechanisms” (Britten 1998, p. 9372). Morange’s reconstruction lacks a detailed analysis of the development of Britten and Davidson’s research, including the identification of this assumption and the different representations they included in the papers they published throughout the 1970s (see Figure 3, below).

One of the most conspicuous features of the 1969 text is the use of the metaphor of *batteries*. The model shows how batteries of genes could be regulated by a single event happening at what they called the *integrator* genes (Figure 1), and also how the transcription of overlapping batteries of genes could be controlled (Figure 2).

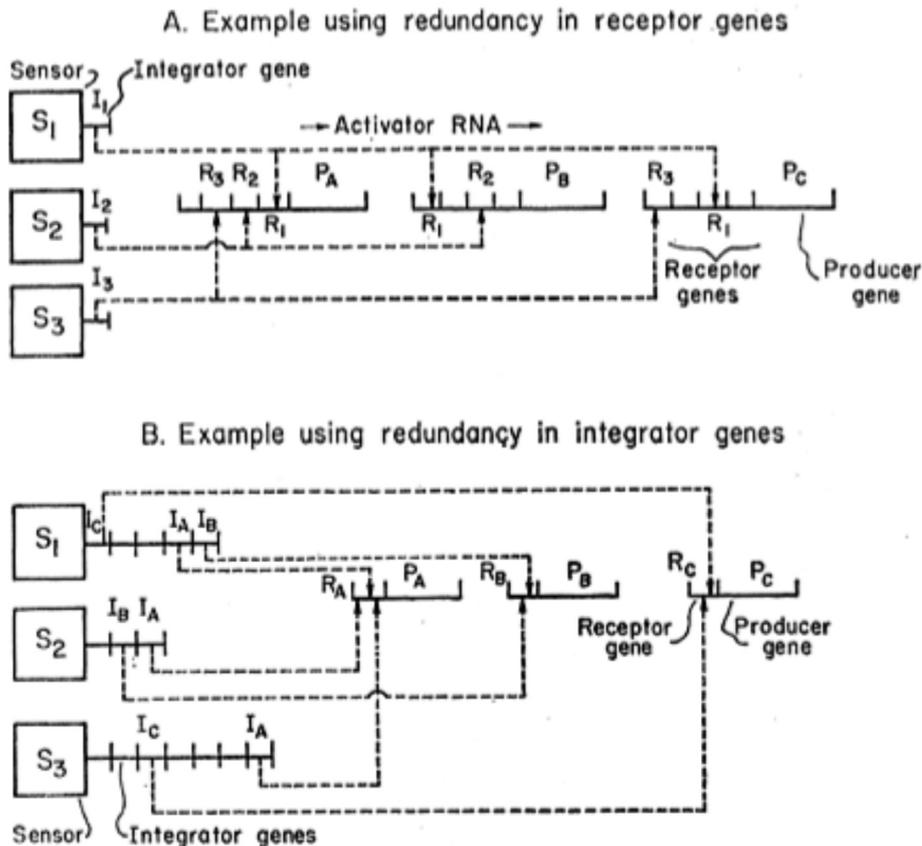


Figure 1: Figure 1 in the original (1969, p. 350). “Types of integrator system within the model. (A) Integrative system depending on redundancy among the regulator genes. (B) Integrative system depending on redundancy among the integrator genes. The diagrams schematize the events that occur after the three sensor genes have initiated transcription of their sensor genes. Activator RNAs diffuse (symbolized by dotted line) from their sites of synthesis – the integrator genes – to receptor genes. The formation of a complex between them leads to active transcription of producer genes A, B, and C.”

Redundancy can take several meanings, according to what we observe in Figure 1. In example A, redundancy in receptor genes indicates that more than one producer gene (P) is activated by a single receptor gene (R): R_3 is redundant in the sense that it is capable of activating both P_A and P_C . In example B, redundancy in integrator genes indicates that there is more than one way of producing activator RNA from an integrator gene: I_A is redundant in the sense that sensor genes S_1, S_2 and S_3 can all lead to its production of an activating molecule (depicted by dotted line) that is complementary to R_A . But also, R_A is redundant in the sense that it is capable of recognizing activator RNA irrespective of whether it was produced by S_1, S_2 or S_3 -induction of I_A . Underlying the possibility of having all these arrangements is the fact that there are large amounts of repeated sequences – what Britten and Davidson call, in general terms, *redundancy* – in the eukaryotic genome.

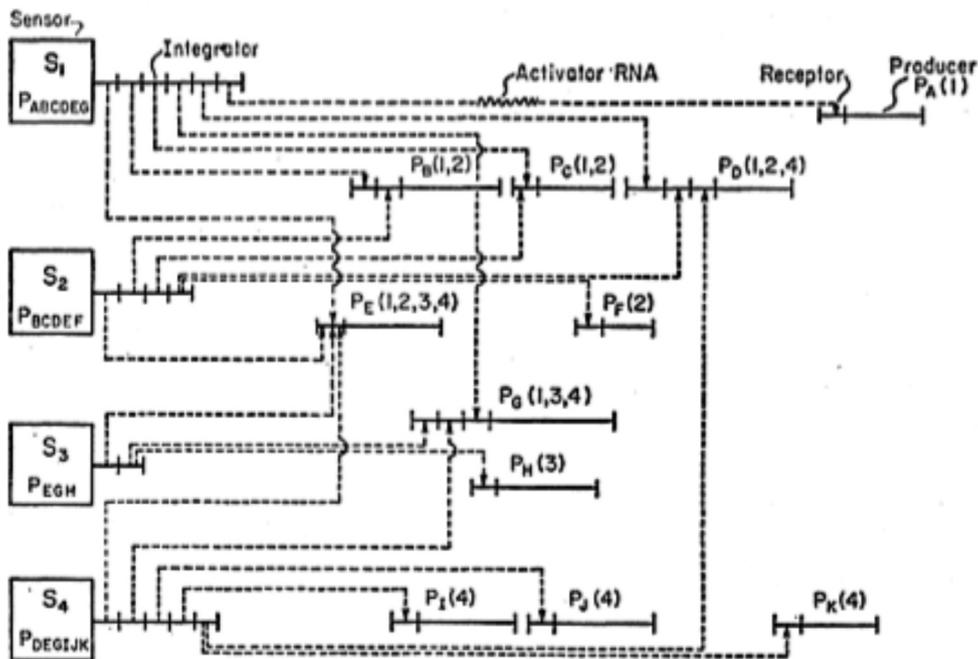


Figure 2: Figure 2 in the original (1969, p. 351). “This diagram is intended to suggest the existence of overlapping batteries of genes and to show how, according to the model, control of their transcription might occur. The dotted lines symbolize the diffusion of activator RNA from its sites of synthesis, the integrator genes, to the receptor genes. The numbers in parentheses show which sensor genes control the transcription of the producer genes. At each sensor the battery of producer genes activated by the sensor is listed. In reality many batteries will be much larger than those shown and some genes will be part of hundreds of batteries.”

The use of batteries as an analogy to understand the serial effects of sensor genes on producer genes (for which, as we have seen, large amounts of repeated sequences is required), stood in contrast with the cybernetic metaphors that the regulationists were using. Jacob later declared that “Britten and Davidson...were making completely eccentric models...who were very successful, but who were completely eccentric, who didn’t have any experimental basis.” (Jacob, oral history, Peoples Archives). Evidence of this eccentricity – of their deviation from the usual pattern of molecular models, and their localization outside mainstream molecular biology – is the interested reception that their model had from at least one person working in the less prestigious field of

evolutionary embryology (Waddington 1969). We will now try to clarify in what sense was the model unconventional, and up to what point it was successful.

A battery is generally defined as a combination of two or more electrochemical cells which store chemical energy that can be converted into electrical energy. The term, however, was coined by Benjamin Franklin for an arrangement of multiple Leyden jars after a battery of cannons. The first use of the term *gene batteries* can be traced to Morgan (1934), who described them as sets of genes that are expressed at different stages during development. Curiously enough, the military roots of the term seem more relevant to understand the intended meaning of Davidson and Britten's original use. In military organizations an artillery battery is a unit of guns, mortars or rockets so grouped as to facilitate battlefield communication, command and control, which is precisely the molecular activity that the authors wanted to convey.

The embryologist C.H. Waddington (who had disposed of the “operon hypothesis”) was one of the few to favor Britten and Davidson's model. Only three months after the model came out, Waddington published a text with a similar title: “Gene regulation in higher cells” (1969). In the first paragraph he writes:

The hypothesis described by Britten and Davidson is the first speculation about the molecular mechanisms that control the epigenesis of higher forms that begins to make sense to an embryologist who has been thinking along these lines for 30 years or more. These authors realize that we have to find a system which can control not single genes but *batteries of genes*. The notion that the gulf between the complexity of the control task and the apparent lack of specificity of such possible controlling agents as histones¹⁴ might be bridged by calling on the informational redundancy suggested by the reiterated DNA sequences is an attractive and rather obvious one – in fact, I have suggested it myself in a less fully worked out form (Waddington 1969, p. 639, our emphasis).

But not all the paragraphs in Waddington's paper were letters of endorsement. He detected an explanatory gap that, to the eyes of an embryologist, could render the model useless. In order to satisfy the requirement of a mechanism of double action – determination of cell fates and activation of genes – that he believed was necessary for an explanation of gene regulation in higher organisms, he proposed a modification to the model. Britten and Davidson had already taken care of the mechanism of activation, so Waddington suggested “inserting another controlling factor between the integrator genes and the receptor genes” (ibid., p. 639) in order to account for determination. This proposal was framed within his own theory of the “epigenetic landscape” (the metaphor he used to describe how ontogeny could be modulated): If an external stimulus (hormonal, for example) managed to alter the state of integrator genes, this change could influence the determination of the ontogenetic route (the path, within the epigenetic landscape) that the cell would follow towards differentiation. In 1971 Britten and Davidson published their “Note on the control of gene expression during development,” in which they offer a first revision of their model and briefly address Waddington's concerns. They claimed that although their model was limited to genetic activity at a “primary level”, that is, at the level where transcription occurs, they were aware that this process is related with many others, like translation, which are equally important.

Meanwhile, the regulationists branched out from general repression to allosteric regulation, focusing on the stereochemistry of proteins (Creager and Gaudillière 1996). Britten and Davidson had come up with a model that drew heavily from a new and complicated matter, namely, the redescription of the materiality of what counted as a gene (see Barnes and Dupré 2008), and were

¹⁴ At that time, the presence of histones – protein cores around which DNA filaments are packed into chromosomes – was thought to be a structural feature that distinguished prokaryotic from eukaryotic cells, and because of their possible intervention in transcription of DNA (by allowing or disallowing its uncoiling), they were good candidates for performing a regulatory function.

eccentric in the most literal sociological sense. They did not belong to the small network of molecular biologists working on regulation; they did not share their linguistic turns and metaphors; they did not share their experimental cultures; they did not participate in the small, reserved meetings at Cold Spring Harbor. At the same time, to the eyes of the regulationists, Davidson “talked a lot, he attended every conference, he flooded conferences with his theory” (Jacob, oral history, Peoples Archives). In *Le champ scientifique*, Pierre Bourdieu describes the effort to disseminate, among colleagues and interested scholars, products with a brand (so to speak) that can be socially correlated with a name as one of the strategies employed by scientists to communicate their results. This strategy, put into use by Davidson, was aimed at achieving “visibility” as a student of gene regulation; what the 1969 model lacked in approval, it gained in “differential value” and “originality” (Bourdieu 1976). But this originality has been constructed a posteriori, as evolutionary developmental biology has gained advocates and Britten and Davidson are looked upon as premature enthusiasts of the field.

Britten and Davidson’s approach to gene regulation did not, however, remain unchanged. Quite the opposite. In subsequent reformulations there appear changes not only in the number of elements of their model (and, occasionally, changes in the names that designate those elements), but also in the characterization of ‘batteries’ in the light of new data. This constant updating of the notion of battery – however subtle – was an explicit understanding that it would, and it should undergo considerable conceptual change. In their 1969 article, the model of regulation has six elements: producer gene, receptor gene, integrator gene, sensor gene, activator RNA and gene battery. A gene battery is defined as “The set of producer genes which is activated when a particular sensor gene activates its set of integrator genes. A particular cell state will usually require the operation of many batteries” (Britten and Davidson 1969, p. 350; also 1971, p. 114).

Two years later, in 1971, there is an additional description of a battery as “a set of producer genes whose products carry out a closely related set of functions” (Britten and Davidson 1971, p. 113). In this same article, the authors give an example that tries to functionally situate this central element of their model, although the language they use continues to be speculative. “An example of a battery would be the producer genes coding for the group of liver enzymes required for purine synthesis, which might well be activated simultaneously” (ibid., p. 113). But the more substantial amendment to their model is the recognition of the consequences (all of them favorable, in their eyes) of new data regarding the organization of the eukaryotic genome, according to which gene sequences belonging to the same battery can occur far away from each other. They even venture to offer a hypothesis of change in regulatory pathways that leads to evolutionary novelty. In 1973, Davidson and Britten offer one more refinement of the term in question: “we define a gene battery as that set of structural genes [before, producer genes] sharing a common receptor sequence and activated together by virtue of this organization [the discontinuous arrangement of structural genes in DNA]” (Davidson and Britten 1973, p. 600). Their use of the terminology of “structural” genes is indicative of their understanding of genes as DNA sequences possessing recognition and binding “sites” for regulatory molecules. Insofar as structural genes are generally (and non-specifically) repressed by way of histones and other chromosomal proteins that disallow uncoiling and transcription of certain DNA sequences, sequence-specific regulatory molecules (either RNA or proteins) are taken to function as activators (“activator message set” and “activator proteins”, respectively, see Figure 3 – although for Britten and Davidson, in 1969, activator RNA could have been a protein). Moreover, they reported the experimental results obtained by different teams that established that only the coding fraction of eukaryotic DNA – and not the repetitive fraction – was being transcribed into messenger RNA. We will return to this fact, since regulation at the transcriptional level has been a hallmark of models for eukaryotic regulation. Retrospectively, Davidson and Britten’s focus on transcriptional processes, and the importance they granted to

tRNA in regulatory processes, characterize not only their subsequent models but also constitute one of the most conspicuous trends in this field. Finally, it should be noticed that it is not until this 1973 article that they credit Morgan for the term ‘battery of genes’. Figure 3 shows the updated version of the model they offer in this publication.

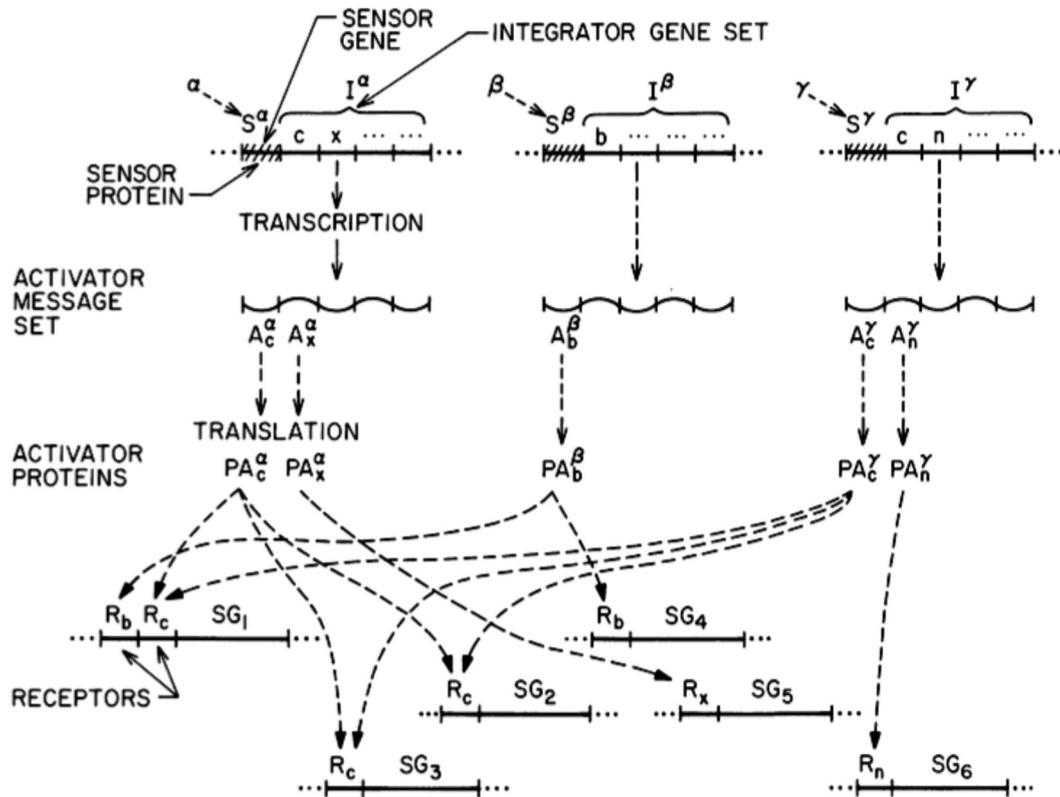


Figure 3: Figure 6 in original (1973, p. 603). “Possible interrelations in the Britten-Davidson regulatory system using protein activator molecules.”

One feature of this diagram is that it is simpler and more general than the previous ones. Here, the emphasis is no longer on the depiction of a relay of commands (especially Figure 1) or of overlapping batteries (Figure 2), but rather on showing the path that the *information* contained in structural genes follows after their activation. As the image caption reads, “many other structural genes could be included in each battery but for the sake of simplicity are not shown” (Davidson and Britten 1973, Fig. 6, p. 603). This contrasts with the caption of the former diagram: “in reality some batteries [of genes] will be much larger than those shown and some genes will be part of some hundreds of batteries” (Britten and Davidson 1969, Fig. 2, p. 351). Despite Britten and Davidson’s claims that their 1969 model could trigger experiments, at the time of its publication there was in fact nothing concrete in it to manipulate.¹⁵ By contrast, this new diagram incorporates traces from hybridization experiments on repetitive sequence transcripts carried out by Davidson and Britten, together with other scientists, which sought to *experimentally* establish the putative regulatory role of different sequences experimentally (Golberg, Galau, Britten and Davidson 1973).

¹⁵ In Keller’s (2000) terminology, Britten and Davidson’s 1969 model was a “model of” but not a “model for”. By the same token, however, the fact that the model invoked “some hundreds of batteries” of genes kept it safely away from “gene-for” theoretical maneuvering, even as it incorporated the language of information.

The range of events that take place after activation of a structural gene is amplified, and new metaphorical descriptors, such as ‘message’, are incorporated into the model. The functions of RNA – transcription and translation – that were only implicitly indicated in the 1969 diagram acquire major importance in the updated version of the model. Unlike the cybernetic metaphors incorporated into the operon, the metaphor of batteries did not easily lead to the creation of new and ubiquitous analogic relations, which meant that its metaphorical origin was not be forgotten. On the contrary. The efforts made by Britten and Davidson to stabilize DNA *organization* (not sequences) as the stuff of regulation were made hand in hand with the indefatigable updating of gene batteries and a constant revision of their evolutionary-developmental approach.

The emphasis on the organization of the genome, over the sequences of individual genes, continued to be a central element in Davidson and Britten’s research on the regulation of development and evolution. In 1986, for instance, their research group published a comparative study of insertions and deletions of repetitive sequences in humans and higher apes. As they report, “little net sequence change has occurred during the evolution of higher apes. Most or all of the members of these families of repeats are interspersed throughout the genome. Therefore, a large number of events of insertion and/or deletion of these DNA sequences has occurred during higher primate evolution” (Hwu et al 1986, p. 3875). To arrive at these conclusions they had used the – by now – familiar hybridization techniques, of which Britten was a pioneer. Nevertheless, their research contrasted with contemporary uses of hybridization for evolutionary studies – and even with Britten’s early uses (see Suárez 2001). While the majority of scientists in the field of molecular *phylogenetics* were still appropriating the techniques in their original use, that is, as a method for quantifying “genetic distance” among species (for instance, Sibley and Ahlquist 1984, 1987, 1990), Britten and Davidson used them to trace the insertions and deletions of big chunks of repetitive DNA at different places in the genomes of higher apes. To put it in different terms: while geneticist approaches to evolution aimed at giving a measure of the proportion of hybridization between genomes (and thus, the distance between species as a percentage of their *sequence* complementarity), Britten and Davidson aimed at locating the role of repetitive fractions in the evolution of regulatory-developmental patterns. Not a single “measure” of genetic distance was given in their paper.¹⁶

The contrast between the operon model and the models stemming from the batteries model – and even more, the contrast between both approaches, one genetic and the other genomic – also points to the relevance of the social organization of science for understanding the different trajectories followed by these models of gene regulation. The fact that Britten and Davidson located their model *outside* mainstream molecular genetics, and within the realm of developmental and evolutionary studies, supports our broader view of the work done on gene regulation in the mid 20th century. Moreover, the theoretical approach of Davidson and Britten was soon to be complemented with a rich experimental culture of their own. One that included the new techniques and technologies of molecular genetics, as well as the older cultures of the maritime stations in biological research (Britten started off as visiting research associate at Caltech’s Kerckhoff Marine Laboratory from 1971 to 1973, where he is now in residence and emeritus). As mentioned before, in 1973 they were already reporting isolated experimental results (mostly from hybridization

¹⁶ This fact and their completely different uses of hybridization kept them away from the debates that were about to explode regarding the uses of these techniques in the study of human and ape evolution. The debate involved leading molecular phylogeneticists and eventually led to the dismissal of DNA hybridization as a source for providing evolutionary distances. The debate was protagonized by ornithologists-turned-molecular anthropologists Charles Sibley and Jon E. Ahlquist, and molecular anthropologists Vincent Sarich and Jonathan Marks. See Marks 2002, Marks et al. 1988, Sarich et al. 1989 and Suárez (forthcoming).

techniques) that pointed to different aspects of their model, results that were interpreted from an evolutionary-developmental perspective and that marked a difference with the prevailing views of molecular geneticists.

6. Current metaphors

The metaphors of *computers* (Yuh and Davidson 1996; Yuh et al. 1998) and *networks* (Arnone and Davidson 1997; Davidson, McClay and Hood 2003; Davidson and Erwin 2006) that frame Davidson's current research in the field of genetic and developmental regulation in eukaryotic cells have proved to be very fruitful for biological research. Their present success, however, cannot be explained by a simple reappraisal of a metaphor that was formulated in the past. Britten and Davidson first used the term *networks* in their 1971 publication "Repetitive and non-repetitive DNA sequences..." where it had a completely different meaning from the one it has today. Networks were structures that resulted from reassociation procedures: "when moderately fragmented DNA (molecular weight 5 to 10 million) is reassociated so that only repetitive DNA can react, large structures are found, which we have termed networks" (Britten and Davidson 1971, p. 123; for this former use see also Britten and Kohne 1968). Each DNA strand used for reassociation contained several repetitive sequences which could interact (reassociate) with the repetitive sequences of other DNA strands. The result of this process was "a rapid branching and growth of network particles" (ibid, p. 123). Moreover, networks could be "efficiently collected by low speed centrifugation or filtration" (ibid, p. 123). Thus, networks were an experimental phenomenon, a *structure* resulting from the repetitive nature of DNA. Today, by contrast, gene regulatory networks refer to *relations* between elements in the genome. Networks are modeled, built, visualized, simulated, annotated; they aid in managing the complexity of regulatory phenomena in metazoans, rather than in providing a solution to a general problem (see Figure 4). A notion of gene battery is retained in this model, as expected, with an updated definition.¹⁷

Today, study of gene regulation in the California purple sea urchin (*S. purpuratus*) in Davidson's "laboratory" at Caltech is built upon an interdisciplinary approach in which a computational analysis of the data generated by the Genetix Arraying Robot with high-throughput is carried out. This robot is located in the Genomics Technology Facility and specializes in the production of libraries (banks of genes) of the sea urchin embryos that the Kerckhoff Marine Laboratory – located in Marina del Rey California – supplies. The software and tools used for visualizing gene regulatory networks (GRNs), such as the BioTapestry application and the NetBuilder environment, which process the data generated by the robot, are developed at Caltech's Center for Computational Regulatory Genomics. A GRN is a collection of a cell's components (usually genes and proteins) which interact with each other (indirectly, through their RNA and protein expression products) and with other molecules in the cell. More precisely, GRNs "consist of the linkages between different *cis*-regulatory systems together with the genes that they govern" (Arnone and Davidson 1997, p. 1857).¹⁸ Figure 4 shows the GRN for endomesoderm specification of *S. purpuratus* and is a good example of the type of model currently being used to understand gene regulation in metazoans.¹⁹

¹⁷ Gene batteries are currently described as "sets of genes that are coordinately expressed because their *cis*-regulatory sequences share homologous target sites for activation" (see Arnone and Davidson 1997, p. 1857).

¹⁸ A *cis*-regulatory system of a gene is composed of the target sites for the transcription factors required for the control of the gene (Arnone and Davidson 1997).

¹⁹ The research summary for the project on endomesoderm specification of *S. purpuratus* reads: "The architecture of the network is emerging from an interdisciplinary approach in which computational analysis is applied to perturbation data obtained by gene expression knockouts and other methods,

Two decades after the publication of this revision, at the height of the informational age in molecular biology, Davidson and his collaborators developed a computational model for gene regulation in the most literal sense: they described the promoter of developmental gene *Endo16* like a genetic computer that operates logically (Yuh, Bolouri and Davidson 1998). Evelyn Keller (2000) has given attention to this work, and has described it as both a “model of” and a “model for” gene regulation in the sense that it is a guide “for doing as much as for thinking” (Keller 2000, p. S77). Her suggestion, however insightful, does little to help us comprehend the complexities of contemporary research that includes – as mentioned – the maintenance and construction of model organisms (themselves a special sort of material *model*) and experimental systems, as well as the use of bioinformatics analyses and computer technologies for representation. The idea of “gene computers” has had important implications for the way some gene regulatory processes are understood and depicted, and it can be argued, as Fernández and Solé (2006) have done, that this property is a prerequisite for modeling networks (especially Boolean networks, of which GRNs are a kind). We now turn briefly to this widespread, metaphor that has made its way into post-genomic studies of gene regulation.²²

In general terms, a network is a graph that represents a collection of elements (nodes) related by connections (lines) that indicate interaction. But the network is not only a representational device: understanding the dynamics of the network’s architecture can shed light on the behavior of the system. What we see in Figure 4 (above) is a structure of interconnected nodes, where genes and proteins constitute the nodes, and their expression products make up the linkages. The notion of network became useful for biological research in the 1990s, after the automatization of molecular biology and the incorporation of informatics into biological research. Just like the success of the information metaphor derived from its general applicability, the power of the network seems to be rooted in its pervasive presence in domains as diverse as economics, computer science and sociology. It is a context-independent notion that can be applied with similar success in different domains (and in different kinds of systems). In the case that concerns us here, it is also well rooted in a rich experimental culture that combines embryology, developmental genetics, computer science, and bioinformatics – like the one prevailing in Davidson’s research facilities at Caltech. Notice that the investigative tradition that frames the modeling of GRNs is very different in theoretical, conceptual, material and – very importantly – social terms, from the context in which Britten and Davidson published their 1969 model. Today, Davidson is a “hub” in the vast network of gene regulationists; he has managed to secure an important amount of funding, material and human resources, and he has taken on diverse responsibilities (like supplying software for the community of developmental biologists working on the California purple sea urchin, and maintaining open source databases that were instrumental for the Sea Urchin Genome Project and continue to be of use). In brief, we could no longer label him “eccentric”.

7. Concluding remarks

Our story of two models of gene regulation points to the central role of experimental systems in the stabilization of epistemic objects that may account for the regulated control of gene expression and to the relevance of the social organization of science for understanding the different trajectories

²² The notion of network is actually quite old. It stems from mathematical graph theory and has been accommodated by research fields as diverse as physics, engineering and sociology. Its application to the modeling of gene regulatory processes, however, is fairly recent. The theoretical modeling of these networks is based on the understanding that the system to be modeled is capable of computation; and their building is based on the comparison/integration of comprehensive datasets, which “requires large numbers of messenger RNA transcripts and protein molecules to be measured simultaneously, in an automated manner with high-throughput and sufficient accuracy” (Winnacker 2003, p. 328).

followed by these models. We also described two events that illustrate the theoretical hourglass of molecular biology. On the one hand, the cybernetic metaphors used by the Pastorianists gave way to the incorporation of the notion of information that substituted that of specificity in molecular biology, which was grounded on biochemical and stereochemical explanations, during the second half of the 20th century (Kay 2000). Lily Kay (2000) has shown that the success of the metaphors of cybernetics and information theory in molecular biology relied on their context-independency, on their lack of material meaning. The concepts borrowed from these fields were successfully applied to the operon model, and naturalized to the extent that their metaphorical origin was soon forgotten. Our story of Britten and Davidson's model, on the other hand, shows that the metaphorical origins of gene batteries have not only *not* been forgotten. The battery has been constantly redefined, over a period of thirty years, so as to incorporate new knowledge and accommodate redescrptions of what counts as the stuff of regulation (be they DNA sequences, repeated fractions of genomes or entire genomes).

The informational language that succeeded in advancing molecular biology at great speed during the second half of the 20th century contributed as well to the construction of experimental systems (powered also by computer-based experimentation in biomedical laboratories) that gave birth to a new, powerful metaphor for molecular (and systems) biology. This is not to say, however, that gene networks are direct descendants of gene batteries. To build on an idea developed by Bourdieu (1976), we find in the continuous rupture of the models of gene regulation the very principle of their historical continuity: They are fruits of a strategy of subversion rather than one of succession, and in this sense, one model of gene regulation (together with its metaphors – however redefined) does not replace another. They are not definitive solutions to problems, but rather, provisional answers to questions framed according to the investigative traditions and experimental systems of their time. The points of comparison that we chose to develop here are neither identified from a problem-solving view nor from a need to establish theoretical continuities (e.g., whether the operon model is the precursor of all regulatory models; if today we can, retrospectively, assign the Britten-Davidson model a role in the disciplinary history of evo-devo). They rather derive from the fragmented condition of the field of regulation, in which the many modulations in discourse and practice point to interesting objects of historical analysis that have not been given sufficient attention and we bring to the fore.

Finally, concerning the metaphor of the “hereditary hourglass” that frames the contributions to this volume, we may conclude that while the operon model stands as an exemplar of genetic “narrowing” in 20th century studies of heredity, alternative models regarding the evolutionary and developmental dimensions of organisms took shape in a relatively shadowy or eccentric place. As we have shown, the model of gene batteries for eukaryotic regulation was framed with different preoccupations in mind. Moreover, informatic transformations affecting late 20th century biological research provided the representational tools of genetic networks, which are now expanding the domains of molecularized biology.

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THE METAPHOR OF “NUCLEAR REPROGRAMMING”: 1970’S CLONING RESEARCH AND BEYOND

Christina Brandt

“Reprogramming” in recent stem cell research and ‘epigenetics’

Today, “reprogramming” is used as a key concept in the field of stem cell research and developmental biology. There has been a significant upturn in the use of this term since the late 1990s, although its first usage, as we will see later, harks back to the late 1960s. But it is only after 1996, the year when “Dolly”, the Scottish cloned sheep was born, that we find an increase in the use of the term “reprogramming” in the field of the life sciences, either in the phrase of “epigenetic reprogramming” or in the notion of “nuclear reprogramming”. As one can see from the ISI Web of Knowledge, there has been a significant rise in the use of this concept especially during the most recent years: You will find 508 entries of articles which had this term in their titles in the last 5 years. This is nearly twice the number of articles published all together during the last three decades.¹ This increased use of the term is surprising for a couple of reasons: On the one hand, the metaphor of “reprogramming” derives from the metaphor of a “genetic program” that entered molecular biology and developmental biology in the 1960s. On the other hand, the metaphor of “reprogramming”, which came into play in biology in the wake of information discourse, seems to have succeeded the metaphor of a “genetic program”, providing a key concept for life sciences. The idea that developmental processes on a genetic level could be explained by comparison to computer programs and cybernetic models, had its heyday during the 1970s. But nowadays the metaphors of “genetic program” or “developmental programs” are amazingly absent from the literature in developmental biology. Instead we find an increasingly amount of literature dealing with issues of “reprogramming”. This replacement of an older metaphor (“program”) by a new one (“reprogramming”) (which is itself a new version of the program metaphor) is closely connected with the newly arising reformulation and redefinition of another (much older) field, namely “epigenetics”.

However, in the following, I will argue that these developments in recent fields of the life sciences are not fully understandable if they are viewed only as reformulation of older problems in a new way. Also, these developments provide more than merely a new experimental style of approaching older problems. Rather, I will argue that the newly arising research field of “reprogramming” is intermingled with a new way of conceptualizing the organism. With respect to the latter, it is not primarily the historical longstanding comparison of organisms and technical entities (such as the computer), which is at stake. Rather, the practices of “reprogramming” are at the center of a new approach in life sciences, which fundamentally affects the understanding of organismic processes in time. By metaphorically suggesting that it is possible to “reset” the internal time of a cell (or even an organism), “reprogramming” is part of a new scientific vision for technically controlling the point in time of the *origin* of an individual organism and, moreover, the idea that it might be possible to reverse time processes by going in some sense “backwards” in

¹ 1970s almost: under 10; during the 1980s double digit number, 1983: 11; 1986: 11; 1987: 9; 1988: 7; 1995: 6; 1997: 13; 1998: 25; 2000: 33; 2001: 43; 2002: 58; 2003: 58; 2004: 86; 2005: 73; 2006: 112; 2007: 129; 2008: 108.

differentiation. This issue of the reversibility of time seems to be at the very center of what cloning research – and the high hopes for its applications in medicine – is about today.

If you have a closer look at some of the very recent articles on exploring the meanings with which the term “reprogramming” is used today, you will find two obvious things: First of all, the metaphor of “reprogramming” seems to be at the core of what is called “epigenetics”. The rise of this metaphor must certainly be understood in the context of new developments in biology which are very much driven by recent stem cell and cloning research (and their practical attitudes) and which are attached to the catchword “epigenetics” (with respect to theoretical outcomes). And secondly, you will soon get the impression that the metaphor of reprogramming is not an element of an elaborated theory of gene activation or part of elaborated explanations of developmental processes. This is a striking difference from the use of the metaphor of a “genetic/developmental program” during the 1960s and 1970s. At least those scientists who consciously introduced and developed the program metaphor in developmental biology made the claim that cybernetic analogies could provide theoretical insights into developmental processes.² In contrast to this theoretical use of the program metaphor during the 1970s, today, “reprogramming” points mostly to a way of technically manipulating cells. “Reprogramming” often refers to a phenomenon, which is technically producible but little understood until now. It reveals a specific kind of practical attitude toward cells, which implies that one has the ability to modify not only cells but also entire organisms. Furthermore, the term has no clear-cut definition instead, it is used with slightly different meanings. Scientists speak about “nuclear reprogramming”, “reprogramming” (without any adjective), “epigenetic reprogramming”, and by doing this, they refer to slightly different things.

For example, the definition of the term “reprogramming”, given by the developmental biologist John Gurdon in 2003, suggests using “reprogramming” as a term that refers to changes in gene activity in a general sense:

Nuclear Reprogramming is a term used to describe changes in gene activity that are induced experimentally by introducing nuclei into a new cytoplasmic environment. When nuclei from partially or fully differentiated cells are transplanted to enucleated eggs of Amphibia or mammals in second meiotic metaphase, blastula or blastocyst embryos can be obtained, and these can form a wide range of tissues and cell types.³

As we will see later, this definition from 2003 is basically the same as Gurdon’s introduction of the notion of “reprogramming” in the late 1960s and early 1970s. Here, reprogramming refers to *changes* in gene activity in a very broad sense, and it is only emphasized that these changes are experimentally induced by a specific method in cell biology, namely the transfer of a somatic cell nucleus into a new cytoplasmic environment. Here, the term is used as a metaphorical description for a process whose detailed mechanisms are still in some sense a black box. However, most scientists today use the term reprogramming with a lot of more connotations. Another example, coming from a special issue on epigenetics published by the journal *Science* in August 2001, shows the central status of the metaphor in epigenetics:

Epigenetic Reprogramming in Mammalian Development:
(...) in mammals there are at least two developmental periods – in germ cells and in preimplantation embryos – in which methylation patterns are reprogrammed genome wide,

² See for example Evelyn Fox Keller: *The Century of the Gene*, chapter 3; Vivette Garcia, Edna Suarez: “Switches and batteries: two models of gene regulation and a note on the historiography of 20th century biology” (this volume).

³ John Gurdon, J.A. Byrne, S. Simonsson: “Nuclear Reprogramming and Stem Cell Creation,” *Proceedings of the National Academy of Sciences* 100 (2003), pp. 11819-11822, quotation: 11819.

generating cells with a broad developmental potential. Epigenetic reprogramming in germ cells is critical for imprinting; reprogramming in early embryos also affects imprinting. Reprogramming is likely to have a crucial role in establishing nuclear totipotency in normal development and in cloned animals (...) ⁴

Here, “to reprogram” becomes a verb for an action that could take place equally well either in the ‘natural environment’ of germ cells or in the laboratory of a scientist. Moreover, “reprogramming” refers to a specific phase in the (germ) cell circle, and it points to a transformation of the genome towards establishing totipotency (the latter is viewed as being related to demethylation, i.e. a structural rearrangement of methylation pattern of the DNA or a chromatin remodeling, for example in the egg). “Reprogramming” here is no longer a metaphor to deal with a black box, but it is a term to describe a specific turning point in the process of cell differentiation.

The following example comes from a review published by Rudolf Jaenisch and Richard Young in the journal *Cell* in February 2008. Jaenisch, a German molecular biologist with a degree in medicine, has been working for decades at the Whitehead Institute in Boston. In the 1970s, he constructed one of the first transgenic mouse models. Nowadays, he is one of the leading scientists in the very rapidly developing field of cloning and stem cell research. In this review, the authors summarize the developments of the last few years, and they provide definitions of the most basic terms in that field, such as “totipotent”, “pluripotent”, “multipotent”, “reprogramming” and “transdifferentiation plasticity”. Here, reprogramming is defined as: “Increase in potency, dedifferentiation. Can be induced by nuclear transfer, cell fusion, genetic manipulation.” ⁵ As we already saw in the previous quotation, “reprogramming” is described as a naturally occurring or technically induced action that increases potency, although the detailed mechanisms are still unknown. According to this, the scientists explain, that “one of the key issues raised by nuclear cloning relates to the mechanism of reprogramming, i.e., how to define the “reprogramming factors” in the egg cytoplasm that convert the epigenome of a somatic cell into that of an embryonic state.” (567). What becomes clear from this article is not only that “reprogramming” has become synonymous with the idea of a reversal of differentiation (it is explicitly defined as “dedifferentiation”), although the “reprogramming factors” are still a kind of miracle. Moreover, “reprogramming” has become a reasearch field of its own. It is not only a term to describe a specific turning point in cell differentiation, but it has become a research field, in which the development of techniques for the artificial induction of “reprogramming” – as a reversal of differentiation of somatic cells – has become the aim of the research. Jaenisch and Young differentiate four “strategies” (567) in this field of “reprogramming somatic cells”: on the one hand, they refer to practices such as cell nuclei transfer or cell fusion which are rooted in embryological transplantation experiments or cell biology that have been developed since the mid-20th century. On the other hand, they described very recently developed practices of what they call “in vitro reprogramming”. This is an attempt to reprogram somatic cells “back to an ES-like state” (571) by using techniques of genetic engineering, namely the introduction of genetic elements into the genome. This was successfully done for the first time by Shinya Yamanaka and K. Takahashi who “reprogrammed” mouse cells by viral mediated induction of the so-called “transcription factors Oct4, Sox2, c-myc, and Kl14”. This was published in 2006, and the resulting stem cells soon became called: iPS (induced pluripotent stem cells), which has already become an established terminology for a slightly established field of practices. ⁶

⁴ Wolf Reik, Wendy Dean, Jörn Walter: “Epigenetic Reprogramming in Mammalian Development,” *Science* 293 (2001), pp 1089-1093, quotation p. 1089.

⁵ Rudolf Jaenisch, Richard Young: “Stem Cells, the Molecular Circuitry of Pluripotency and Nuclear Reprogramming,” *Cell* 132 (2008), pp. 567-582.

⁶ Kazutoshi Takahashi, Shinya Yamanaka: “Induction of pluripotent stem cells from mouse embryonic

The time of the organism in developmental biology and recent stem cell research

In the recent field of cloning and stem cell research a new version of the old research question on the origins of the organism has arisen. Probably for two centuries, the fundamental and basic question in biology has been about the origin of an organism, namely: “How does an organism come to be?”⁷ Evelyn Fox Keller has argued that at the beginning of the 20th century this question was split up into two problems. On the one hand, scientists dealt with the problem of origins at the level of intergenerational transmission, that is the study of traits across generations – as we find it in the emerging field of genetics at that time. On the other hand, the problem of the intragenerational development was studied in embryology. Here, the main question was about how an organism originated or developed out of the fertilized egg over time. During the 20th century, the main questions in embryology (and later on in developmental biology) were the problems of developmental regulation and cell differentiation. In particular, developmental biology deals with processes over time. The “most conspicuous question”, that still remains is, as Keller expresses it: “How can one explain the coordinated and regulated process of cellular differentiation in view of the apparent sameness of the genetic complement of all cells?”⁸

Although cloning techniques (such as cell nuclei transfer) were developed in the context of these research questions during the 20th century, the efforts in fields such as stem cell and cloning research nowadays no longer aim at analytical skills to explain these questions of differentiation, which are actually questions about temporal processes leading to diversity and difference. Instead, they seem to aim at technical skills, which would enable us to reset the differentiated cell, that is, to reverse differentiation in order to create a stage of cellular origin, which is the technical starting point for all those applied bioengineering techniques in stem cell and embryo research toward which all the hope of future possibilities of biomedicine is directed. This seems to go far beyond older attempts to artificially create or manipulate an organism or parts of an organism, which has, of course, a long tradition in 20th-century biology going back to the engineering ideal in the period of J. Loeb. On a deeper level, this shows a changed attitude towards temporal processes. The metaphor of “reprogramming”, understood as “dedifferentiation”, expresses a belief that techniques of bioengineering could enable us to transcend the natural time of a cell/organism not only by extending this artificially (as has already been done in the field of in-vitro cell culture), but especially by turning back cellular time. This seems to be the fundamental new approach at the turn to the 21st century.

In the following, I want to trace the emergence of the metaphor of “reprogramming” in the field of developmental biology of the 1960s and 1970s. I will argue that this metaphor in the first phase was nothing more than a fashionable expression, which came up in the context of the newly arising information discourse of 1960s molecular biology. For example, as early as 1967, Joshua Lederberg discussed the possibilities and “dangers of reprogramming cells”. The molecular biologist differentiated two main approaches: “eugenics, that is programmed evolution, and euphenics, that is the reprogramming of somatic cells and the modification of development.”⁹ The technical implications of the re/program metaphor were unfolded by Lederberg with respect to longstanding eugenic visions. The first use of the metaphor in the field of developmental biology and embryology, however, was without any biopolitical connotation. Here, in the beginning, “reprogramming” referred to nothing more than the assumption that “fundamental changes in gene activity” were caused by the transfer of cell nuclei into a new cytoplasmic environment.

and adult fibroblast cultures by defined factors,” *Cell* 126 (2006), pp. 663-676.

⁷ Evelyn Fox Keller: *Making Sense of Life*, p. vii.

⁸ *Ibid.*, p. 148.

⁹ Joshua Lederberg: “Dangers of Reprogramming Cells,” *Science* 158 (1967), p. 313.

However, during the 1970s (when frog oocytes became an *in vivo* system for analyzing gene expression), the metaphor of “reprogramming” became a common, albeit discussed, phrase in the field of cloning research on *amphibia*. Scientists such as John Gurdon and Marie Di Berardino tried to develop further models out of this metaphor but they also discussed the limits of its semantic range.

Furthermore, there has been a crucial shift in the practical experimentation in the field of cloning research over the course of the last decades: the techniques of cell nuclei transfer were developed in embryology in order to answer fundamental questions about cell differentiation in the developing embryo and – later on – questions on gene expression and the regulation of gene activation. In this context, the “reprogramming metaphor” gained ground. What started as a tool for theoretical questions on cell differentiation became a completely new field for engineering life – a field in which theoretical explanations of development and gene activation are only of subordinate relevance.

*Research on Cell Nuclear Transplantation in the 1960s and 1970s:
From Clones to Reprogramming*

Let me start with a brief overview of cell nuclear transplantation and cloning research in embryology. In the early 1950s, “one of the most fundamental problems, both for genetics and embryology” was the question of “whether the genes in the nuclei of differentiated tissue retain their full range of capacities, or whether some irreversible alteration affects them,” as the British embryologist C.H. Waddington wrote in an article that was published in *Nature* in 1953.¹⁰ In this article, Waddington and his coworker E.M. Pantelouris described their attempts to develop a method for answering this question. Using eggs from the newt *Triturus palmatus* they tried to develop techniques that would permit the transplantation of nuclei from differentiated cells of one kind into “enucleated cells of different developmental potentialities,” as they put it. However, their attempts failed. Although a high proportion of the eggs that were injected with nuclei cleaved, none of them reached the embryonic stage of the gastrula.

Waddington (who is today well known for his introduction of an epigenetic developmental theory¹¹) was not the only scientist who worked on the techniques of nuclear transplantation at the time. At least a handful were engaged in the problem, working with amphibians or other species such as amoebae.¹² The most famous of these scientists belonged to a group that became well known for successful research on cell nuclear transplantation in frogs. This was the group working under the embryologist Robert Briggs at the Institute for Cancer Research in Philadelphia. Together with Thomas King, Briggs developed the basic microsurgical techniques that were necessary for the procedure of cell nuclear transplantation in amphibians. Today, both scientists are often regarded as forerunners in the work of cloning frogs. As we can see from the following, it was indeed due to these two scientists that the notion of the clone entered the field of embryology, although their experiments led to quite a different result than what we would expect today, after Dolly the sheep.

Briggs and King were interested in questions that had occupied embryologists for a long time, namely the interaction of the cytoplasm and nucleus during development, especially the question

¹⁰ C.H. Waddington, E.M. Pantelouris: “Transplantation of Nuclei in Newt’s Eggs,” *Nature* 172 (1953), pp. 1050-1051.

¹¹ See Evelyn Fox Keller: *The Century of the Gene*, Cambridge/Mass., Harvard Univ. Press 2000, pp. 77-80 and pp.117-120.

¹² See for example I.J. Lorch, J.F. Danielli: “Transplantation of Nuclei from Cell to Cell,” *Nature* 166 (1950), pp. 329-330.

of whether or not the cell nucleus became irreversibly differentiated during development. Working with the frog *Rana pipiens* and developing microsurgical techniques, they could show that the nuclei of cells in early embryonic stages that were transferred to enucleated eggs had the potential to induce normal development. But the nuclei of cells from embryos in later stages of development (from the late gastrula) failed to promote normal development when they were transferred into enucleated eggs. The eggs injected with nuclei from later stages cleaved, but the development stopped at a specific point and the resulting embryos had an abnormal shape. In the mid-1950s, Briggs and King therefore concluded that the ability of transplanted nuclei to promote normal development declines as development progresses. They assumed that the nuclei changed during differentiation and that the nuclei's ability to promote normal development became limited.

With this result there arose two new questions, as Briggs and King wrote in 1956:

From the experiments described above, it looks very much as if there is, during development, a progressive restriction of the capacity of endoderm nuclei to promote the coordinated differentiation of the various cell types required for the formation of a normal embryo. The two most pressing questions concerning these nuclear changes are 1) are they specific? and 2) are they stable or reversible?¹³

In order to answer these questions – whether or not these observed changes in the nuclei were specific and stable – Briggs and King introduced a new method into their approach, which they called the “serial transplantation of embryonic nuclei” as a method to produce what they now called “nuclear clones.” Thus, when the term “clone” entered the field of embryology and research on nuclear transplantation, it was the notion of the series that played the important role. Briggs and King transferred nuclei from cells of the late gastrula stage into enucleated eggs. From the blastula stage of these developing eggs, they again transplanted nuclei into enucleated eggs, producing in this way different “nuclear clones.” All members of one clone were descended from one original endoderm nucleus, and each clone group showed the same abnormal morphology compared to embryos that had developed normally. Therefore the scientists came to the conclusion that during embryonic development, the cell nucleus must be changed in a way that is not reversible. “How these changes arise, whether they are specific, and which of the nuclear or perinuclear structures are involved, are problems remaining to be worked out,” Briggs and King wrote in 1956.¹⁴

For Briggs and King, the fascinating problem was not the question of whether an identical copy of an adult organism could be reproduced out of a single cell or nucleus; rather, they were concerned with the problem of whether processes of cell differentiation were reversible or not. Around 1960, this issue was reformulated as the question of whether cell differentiation in the course of embryonic development was accompanied by a loss of genetic information.

In the early 1960s, research on nuclear transplantation got new direction with the work of John Gurdon, who is well known for being the scientist who “cloned” the first animal, a frog, out of an adult cell nucleus. Starting in the late 1950s, John Gurdon, working in the group of the Oxford embryologist Michael Fischberg, used techniques of cell nuclear transplantation similar to those developed by Briggs and King, but Gurdon changed the experimental object. Instead of using the frog *Rana* Gurdon worked with *Xenopus*, which was an established research object in Fischberg's laboratory at that time.¹⁵

¹³ Thomas J. King, Robert Briggs: “Serial Transplantation of Embryonic Nuclei,” *Cold Spring Harbor Symposia on Quantitative Biology* Vol. 21 (Genetic Mechanisms: Structure and Function) 1956, p. 271-290, quotation on p. 276.

¹⁴ *Ibid.*, p. 288.

¹⁵ John Gurdon, Nick Hopwood: “The introduction of *Xenopus laevis* into developmental biology: of

Gurdon’s results differed from Briggs and King’s, suggesting that the process of cell differentiation does not require any stable change to the nucleus of the cell. In 1962, Gurdon announced that 3% of the transfers of nuclei from intestinal epithelial cells of swimming tadpoles resulted in normal adult frogs.¹⁶ From this work he concluded that “these results are therefore consistent with any theory of cell differentiation which does not require that the nucleus of a differentiated cell has lost the genetic information required for the formation of other differentiated somatic cell types.”¹⁷

After spending a year at Caltech and after a short excursion into research on bacteriophage in 1962, Gurdon returned to Great Britain and to frogs. During the 1960s at Oxford, and from 1971 onwards at the MCR Molecular Biology Laboratory in Cambridge, he turned his nuclear transplantation approach into a research system directed at analyzing gene expression.

Gurdon’s work on cell nuclear transplantation in the late 1960s and the 1970s contributed to the redefinition of development as a kind of differential gene activation. Using the procedure originally established to inject nuclei into oocytes, he and his group started to use *Xenopus* oocytes as “living test tubes” for the study of what they called “transcriptional control.”¹⁸ They started to transfer nuclei from very different origins into eggs and oocytes and also began injecting pure macromolecules such as mRNA of different origins, or even purified pieces of DNA. In other words, they started to inject into the oocytes molecular particles that were regarded as the material basis of a genetic “messages.”

It was in this context that the metaphor of “reprogramming” occurred in published articles. In a summary of his work, published in the Proceedings of the Royal Society of London in 1970, Gurdon used the notion of reprogramming for the first time, arguing that his *in vivo* system for attacking the problems of gene expression and gene regulation would have much more advantages than *in vitro* systems in molecular biology:

In conclusion, there are two reasons why the transplantation of nuclei to enucleated eggs seems to present an unusually favourable opportunity for studying the control of gene activity. First, it leads to a *fundamental change or reprogramming* of gene activity on a scale, which it is hard if not impossible to achieve by other procedures at present available. Secondly these changes in gene activity are imposed on *normal* nuclei in *normal* cells. It would seem that nuclear transplantation causes a complete cancellation of any previously established restriction of gene activity, so that successfully transplanted nuclei may then participate freely in all normal stages of development.¹⁹

In his textbook, *Control of Gene Expression in Animal Development*, published a few years later, Gurdon also stressed the usefulness of this *in vivo* system as an additional approach to the well established research models in molecular biology:

Although much valuable information has emerged from the description of translation in normal cells and from the use of cell-free systems for translating added messages, these

empire, pregnancy testing and ribosomal genes,” *Int. J. Dev. Biol.* 44 (200), pp. 43-50.

¹⁶ John Gurdon: “Adult Frogs Derived from the Nuclei of Single Somatic Cells,” *Developmental Biology* 4 (1962), pp. 256-273, especially p. 271.

¹⁷ J. B. Gurdon: “The Developmental Capacity of Nuclei Taken from Intestinal Epithelium Cells of Feeding Tadpoles,” *Journal of Embryology and Experimental Morphology* 10 (1962), pp. 622-640, quotation on p. 637.

¹⁸ J.B. Gurdon, E.M. De Robertis, G. Partington: “Injected nuclei in frog oocytes provide a living cell system for the study of transcriptional control,” *Nature* 260 (1976), pp. 116-120.

¹⁹ J. Gurdon: “Nuclear Transplantation and the Control of Gene Activity in Animal Development,” *Proceedings of the Royal Society*, London 176 (1970), pp. 303-314, quotation pp. 305-306.

methods have certain limitations affecting the kinds of conclusions which they can eventually yield. We now discuss (...) another type of experimental approach which, it is hoped, may combine the advantages of working on living cells with the experimental advantages of working on pure messages from different cell-types.²⁰

In the early 1970s, the results of Gurdon's group included an astonishing phenomenon resulting from the transfer of nuclei between different species. They had transferred nuclei from cultured *Xenopus* kidney (Niere) cells into oocytes of another amphibian species, the newt *Pleurodeles*. As a result, they found proteins that were normally expressed in oocytes, but *not* proteins specific to kidney cells. The proteins were *Xenopus* proteins, and they were distinguishable from the equivalent *Pleurodeles* proteins. Thus, they concluded that the transferred *Xenopus* kidney nuclei, influenced by the new cytoplasm, had switched from one program (the kidney program) to another genetic program that was active in the early stages of development. From this research, the metaphor of "nuclear reprogramming" got new impetus.

In 1979, at a symposium on nuclear transplantation, the metaphor of nuclear reprogramming took center stage. Gurdon and his coworkers defined the phrase now as kind of revision of how genetic information is expressed and regulated throughout embryonic development and as a switch between different genetic programs. Here we find a first attempt to define the metaphor:

'Nuclear reprogramming' is a term used to denote fundamental changes in gene activity. Programs of gene activity differ substantially among types of specialized cells, such as muscle, nerve, and blood, and switches from one program to another never take place under normal conditions. ...Nuclear transplantation is one of very few experimental conditions under which a reprogramming of nuclear activity takes place in a reproducible way. The mechanism responsible for the reprogramming of nuclei transplanted into eggs and oocytes is of interest, since it seems likely to be the same as the mechanism by which genes are activated in normal early development.²¹

Other groups working on nuclear transplantation also strove to elucidate the mechanism of reprogramming. In particular, Marie Di Berardino, a former member of Robert Briggs's group in Philadelphia, worked along lines similar to Gurdon's. Di Berardino was one of the co-organizers of this meeting, which was the first conference that aimed at bringing together all scientists who worked on issues of nuclear transplantation in a variety of different organisms. In the introduction to the published volume produced by this conference, Di Berardino, together with the cell biologist James F. Danielli, discussed the metaphors of genetic program and reprogramming.²² Whereas Gurdon had focused on the reprogramming of the nucleus, they paid greater attention to the interrelations between organisms and their environment by referring to a model of Howard Pattee that distinguished a "program" from an "effector system." Their perspective on the program and reprogramming issue indicates differences in comparison to the contemporary approach in molecular biology. Coming from the field of cytology, they argued explicitly against a reductionist view that was centered on the nucleus or even the DNA. "In recent years," they wrote, "there has been an excessive concentration of research on molecular biology, and a relative neglect of studies of the overall functions of nuclei and cytoplasm" (xiii). The publication of the conference volume was intended to have a "corrective function" by providing "a more balanced view of the operation

²⁰ J. Gurdon: *Control of Gene Expression in Animal Development*, Oxford: Clarendon Press, 1974, p. 52.

²¹ J.B. Gurdon, R.A. Laskey, E.M. De Robertis and G.A. Partington: "Reprogramming of Transplanted Nuclei in Amphibia," in: *Nuclear Transplantation*, ed. by J.F. Danielli, and M.A. DiBerardino, New York: Academic Press, 1979, pp. 161-179.

²² James F. Danielli, Marie A. DiBerardino: "Overview," in: *Nuclear Transplantation*, ed. by J.F. Danielli, and M.A. DiBerardino, New York: Academic Press, 1979, pp. 1-9.

of cell systems” (ibid.). Furthermore, they argued for (what they themselves called) a “holistic view (...) of cells and of cell clones” (p. xiii).

For them, the notion of a “program” was not restricted to the informational content of the DNA. On the contrary, for them, it referred to all genetic components of a cell, what they defined as: “subprograms involving nuclear, organelle, and intracellular symbiont genes” as well as “the control system which act upon them” (3). With the “effector system” they pointed to the local environment of the gene, or, as they put it, “to all nongenic cellular components such as membranes, enzymes, structural proteins, messenger molecules” that may have a regulation function. In their summary, they not only argued that reprogramming might be a misleading metaphor; they also had the idea that the scientific community should start to define their terms more rigorously.

This leads us to the definition of the program of a cell and the meaning of the term reprogramming. It is almost certain, though as Briggs points out in this volume, not proven, that all the cells of organism contain the same nuclear genome, i.e set of nuclear genes, though there are some exceptions (...). If all the cells of an organism contain the same genome, then the different cell lineages must be (metastable) alternative states, i.e. are different expressions or realizations of the program of the organism. Thus, when a cell, or a nucleus, or a cytoplasm switches from one of these states to another, what happens is not, strictly speaking, reprogramming, but a change of expression of the program.

However, the practice has developed of referring to these changes as involving reprogramming (see, e.g. Gurdon et al. this volume), and it is probably simplest to retain this usage for the time being. In due course, as the mechanisms for changing states are elucidated, it will probably be necessary for an international commission to develop a set of rigorously defined terms in this field. (4)

What can be summarized at this point? The metaphor of reprogramming (and genetic program), introduced in the late 1960s, was used in two different ways in the field of cloning research in the 1970s: Whereas Gurdon focused on the possibilities of reprogramming from the perspective of gene activation, DiBerardino and Danielli’s perspective was based on an approach which resembled assumptions in system theories. Gurdon’s research interest aimed at “studying the control of gene activity”²³. With that, his approach was influenced by the ‘information discourse’ in molecular biology. His focus was, so to speak, primarily on the information content in the nucleus, searching then the “cytoplasmic control molecules which exist in all cells throughout development.”²⁴ Although he introduced the metaphor of “reprogramming” he himself seemed to be cautious to use further terms from the field of computer sciences, information theory or cybernetics. (At least, I could not find other metaphors from those fields in his writings). DiBerardino and Danielli criticized not only a reductionistic perspective in molecular biology, they also argued that “reprogramming” was a misleading metaphor. Their scepticism, however, shows that they were well aware about contemporary attempts to develop theoretical models out of the “program” metaphor. At least their talk of “subprograms” might indicate, that they knew the book of James Bonner who took the program metaphor seriously and who tried to develop a (cybernetic) model of “switching networks for developmental processes” – a model, in which he differentiated between a “master programme” and subprogrammes or subroutines.²⁵ However, coming from cytology, their (“holistic”) approach described the cell primarily as a system – and with that, their research focus was on gene-environment interaction.

²³ John Gurdon “Nuclear Transplantation and the Control of Gene Activity in Animal Development,” *Proceedings of the Royal Society*, London 176 (1970), pp. 305/306.

²⁴ John Gurdon: “Egg Cytoplasm and Gene Control in Development,” *Proc. Roy. Soc. of London* 198 (1977), p. 241.

²⁵ J. Bonner: *The Molecular Biology of Development*. Oxford 1965, pp.133-145.

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SCIENCE AS EVOLUTION OF TECHNOLOGIES OF COGNITION

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1. Attempts to develop evolutionary models of social processes have been the testing ground for many proposals as to how understand the relation between the social sciences and biology. One important discussion that goes back to Darwin and his contemporaries concerns the extent to which we can give an evolutionary model of culture.¹ Nowadays we are familiar with a wide variety of such models. There are models that start with a paradigmatic example of how biological models, relying on specific mechanisms of biological inheritance, can explain what is considered a paradigmatically socially structured behavior, and then the solution is extrapolated to other modes of social organization. The sociobiology of E.O. Wilson is a well-known example of this sort of approach. Other approaches identify what is considered the main mechanism for the social transmission of beliefs. Memetics, for example, refers usually to approaches based on the assumption that imitation is the main mechanism of transmission. And Boyd and Richerson have developed a theory based on the explanatory resources of Darwinian “population thinking” (Boyd and Richerson 1985). As in the case of Memetics, Boyd and Richerson assume that an evolutionary model of culture requires the identification of units of cultural replication that are units of information stored in human brains. It follows that an explanation of the way this storage takes place (and changes) is sufficient to explain culture.

Alternatives to such storage accounts of culture usually deny that a significant distinction can be drawn between biological and cultural evolution. Griffiths and Gray point out that since it is not possible to draw a sharp boundary line between channels of biological inheritance and channels of cultural inheritance, we should not try to draw a line between cultural and biological evolution (Griffiths and Gray 1994). Nonetheless, accepting that drawing a sharp boundary between biological and cultural inheritance is not possible does not imply that cultural inheritance is not a distinctive problem with important implications for the social sciences and the philosophy of science. In order to see its distinctive features we have to pay attention not only to the question whether a distinction can be drawn or not drawn between the gene and other causal factors in development, but also, and in particular, we have to address the question of how to model what we can call the “phenomenology of culture”, the stable but changing structure of cultural phenomena. In other words, the issue is how cultural items get the sort of stability that matters for explaining the cumulative sort of change that distinguishes cultural processes. Such question requires studying cases of cultural (stable) traditions that can shed light on the sort of explanation we want. As we shall see, it also requires taking seriously discussions in the cognitive sciences concerning the way in which cognition is grounded in social structures and processes and in particular requires taking seriously views of language that abandon the idea that language is constituted by encodings of mental content.

From the perspective we take in this paper the issue is not about the nature of information, or the way structures of information mentally encoded are transmitted from one agent to another, but it is first of all an issue about lineages of artifacts-norms-representations usually structured in scientific practices that explain the sort of cumulative change we associate with culture. In this paper we will take science as a paradigmatic sort of culture. It is considered paradigmatic because

¹ Talking of culture does not mean to imply a well defined type of social phenomena or processes that are cultural as opposed to merely social. It is rather a matter of emphasis on processes for which the accumulation of modifications is important to understand the sort of process they are.

of its centrality in contemporary life but also because of the fact that the way in which its representational structure is supported by artifacts is relatively easy to grasp (to the extent that we understand the role, in the generation of scientific culture, of things like laboratory practices, observational techniques, mathematical models or diagrams, among other resources), allowing us to draw conclusions about the sort of evolutionary process that support stability and change.

Models of cultural evolution often have presuppositions that conflict with such view. Selectionist models that assume that evolution takes place mainly through ‘blind’ retention are committed to the view that the psychological processes that support culture promote the uncritical acceptance of information acquired from others. And thus, tend to assume (most often implicitly) that such norms are not the result of individual or group learning, or more generally, assume that norms can only play the role of passive constraints in evolution. As Heyes puts it, “to the extent that culture depends on fidelity of social transmission in the face of local environmental fluctuations, the formation of cultural attributes is likely to depend crucially, not on processes of information acquisition (e.g. social learning, imitation and instruction), but on processes that contribute to faithful or ‘blind’ (Campbell 1974; 1983) information retention” (Heyes 1993). But once we take seriously the role of material culture as scientific culture we have to find the way of accommodating the view that material things can be both, and at the same time, part of a process of replication and a process of interaction (Lake 1998; Griesemer 2000).

What I am suggesting is that scientific practice can be used as revealing interesting aspects of the way cultures evolve. This might sound counterintuitive to many ears, and in particular to philosophers of science used to think of science as a rather special kind of culture. I agree that scientific cultures can be special in many ways, but it is hardly the case that there is something that makes scientific culture as a whole special. One way in which science has been understood to be a special sort of culture is related to the idea that science deals with a very special sort of representations. But if as I will argue below, representations cannot be understood as mere passive copies of structure, then such objection does not hold water. Scientific representations, as other culturally significant representations have to be understood as part and parcel of processes in which artifacts represent through its function or use (and the history of such use).²

In sections 2-4 we review different answers that have been given to account for the stability of cultural processes. We shall see that all of them have serious shortcomings. Either because they want to identify one single mechanism that is responsible for the sort of stability that matters, or else because they involve the attributions of intentions in a way that makes culture, by decree, a phenomenon confined to human beings. It might be that there are good reasons for saying that culture is an only human phenomenon, but such assertion has to be understood as an empirical assertion³. In section 5 we will suggest that Goody’s thesis that writing is the technology of the intellect point to a way in which scientific cultures exemplify a kind of stability that matters for explaining cultural processes in general. In section 6, I introduce the concept of representation as scaffolding of further action that will provide the framework in which my proposal (to be developed mainly in section 7 and 8) for understanding science as the technology of cognition can be seen as an extension of Goody’s thesis once the notion of representation as encoding is abandoned in favor of the notion of representation as scaffolding for action or intervention.

Shifting the search for an explanation of the sort of stability that matters, in the case of cultural processes, away from questions about the transmission of mental representations or symbols, lead

² One can add against this chauvinistic view of scientific representations the sort of arguments elaborated by Callender and Cohen 2006.

³ Wimsatt and Griesemer 2007.

unavoidably to take seriously the role of cultural development and models of adaptive design in the explanation.

The concept of generative entrenchment of Wimsatt and its role in evolutionary models of culture will be taken here as a point of departure for my proposal (see in particular Wimsatt 1986; Wimsatt and Griesemer 2007). Wimsatt and Griesemer have shown the crucial importance of a concept of “scaffolding”, closely related to that of generative entrenchment. Our emphasis in material culture will lead to a development of two related but different concepts of “scaffolding”. Scaffolding will be seen to be crucial to understand the way in which lineages of normative environments (articulated in practices) evolve.

2. Memetics (the science of memes as it is called) has been often criticized because memes have too little fidelity to support an evolutionary explanation.⁴ Dawkins has suggested that the objection can be overcome once we distinguish “to copy something” from “to copy instructions.” Dawkins gives the following example (in the preface to *The Meme Machine*, Blackmore 1999). We show a child a Chinese boat and ask her to draw it. The drawing is shown to a second child and asks to draw its version, and so on until we have 20 drawings. Dawkins guesses that the result of the thought experiment is clear, that the last drawing will be so different from the first that no relation could be established between the two. However, ordered in the way they were drawn would certainly allow us to see a path leading from the first to the last. The observation leads to a test for memetic replication. In the case of memetic replication the order in which the copies were made is as informative (or uninformative) as random order.

Dawkins asks us to carry out a second experiment. Instead of asking each child to draw a boat we show one of them how to make a boat following the Origami technique. When the first child has mastered the technique he is asked to show it to a second child, and so on. Dawkins thinks that the result is predictable. Even if it is possible that a child forgets one of the steps of the technique another child might realize what is missing and end up with a boat not better or worse than the first. The paper phenotype is not transmitted and thus the phenotypic defects are not transmitted, only a set of instructions is transmitted, and those **instructions are “self normalizing”**. The idea is that memetics deals with the different ways in which the copying of instructions has an impact on human culture. But how is this self-normalization carried out? In other words, how is this self-normalization to be understood? Dawkins does not say anything about it, and to that extent he is only pointing to an underlying problem, not to a solution. How can the stability in the transmission of instructions be explained?

3. Boyd and Richerson (in Aunger 2000) have developed a different type of evolutionary model of culture, an epidemiological model that is based not so much on the explanatory role of selection of cultural units (as in the case of memetics) but rather on the explanatory role of “population thinking”. They give an answer to the question posed above on the basis of what they consider “three well-established facts”:

1. *There is persistent cultural variation among human groups.* Any explanation of human behavior must account for how this variation arises and how it is maintained.
2. *Culture is information stored in human brains.* Every human culture contains vast amounts of information. Important components of this information are stored in human brains.
3. *Culture is derived.* The psychological mechanisms that allow culture to be transmitted arose in the course of hominid evolution. Culture is not simply a by-product of intelligence and social life.

⁴ Dawkins 1976; Aunger 2000; Blackmore 1999.

On this basis, their explanation of the stability of the replication of instructions is roughly the following. First, it is argued that the ability to acquire novel behaviors by observation is essential for cumulative cultural change. This requires a distinction between observational learning and other mechanisms of social transmission, and in particular requires distinguishing observational learning from mechanisms such as *local enhancement*. Local enhancement occurs when the activity of other animals in the group increases the chance that younger animals will learn a behavior that increases the chances of learning the behavior. A monkey learns through the mother where are the best locations to search for food in this way. But wherever observational learning allows for cumulative cultural change, other mechanisms, including local enhancement, do not. Local enhancement is a mechanism that does not allow for learning taking place on top of what other individual has already learned. Observational learning is thus a set of adaptations that enable humans to learn by observation, and the sort of stability associated with “self-normalizing” pieces of information can be understood directly as a consequence of the role of observational learning in the process of cumulative cultural change. No matter how we end up specifying the underlying mechanisms for observational learning, a precondition for cumulative change is the sort of stability that requires explanation. Again, it seems that the stability in question is presupposed rather than explained. A key question for any explanation of culture is thus, whether the sort of mechanism postulated by Boyd and Richerson, what they call “observational learning”, is indeed as central as they claim it is. They suggest that observational learning can be grounded on empirical findings. But such grounding is only hinted at, and it seems that they rely rather in some questionable epistemic assumptions about the way we learn from experience.⁵

I have no quarrel with the first and third principles proposed by Boyd and Richerson, but I do think that the second one cannot be accepted, and the reason why it cannot be accepted suggests a way of explaining the stability in question. Culture is not merely information stored in human brains, and the extent to which it is something more matters in the explanation of the sort of cumulative cultural change (for which the stability in question is a precondition).

4. Sperber suggests a way of accounting for the stability of cultural items.⁶ Sperber asks us to consider the following variant of the example of Dawkins. A child is asked to look carefully to the drawing in figure 1, and then it is asked to redraw it.

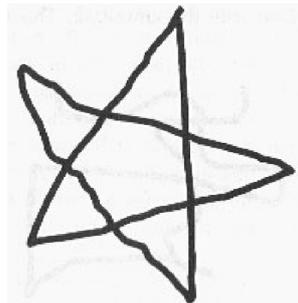


Fig. 1

⁵ Boyd and Richerson assume that all learning is explicit learning, that there cannot be any significant learning that is not explicit. But this is questionable. Polanyi speculated several decades ago that implicit learning is an important sort of learning. Nowadays there are many studies that support this view. See for example Reber 1993. As Reber makes clear, the idea of implicit learning fits nicely a model of learning grounded on Wimsatt’s notion of generative entrenchment. Elsewhere I show how this implicit learning involves the learning of the sort of normative structure that gets reproduced in scientific practices (Martínez 2003). See Netz 1999 for an example of how such implicit learning takes place through the learning of the epistemic use of diagrams in Euclidean Geometry.

⁶ Sperber 2000.

Later one asks a second child to reproduce the drawing of the first child, and so on. Sperber thinks that to the extent that the children will identify what they are drawing, a five peaks star drawn without removing the hand from the paper, the drawings will be stable. According to Sperber this version of the experiment shows clearly something that Dawkins example did not allow us to see, to wit, that it is not the mere fact that there are instructions what makes the replication faithful, but the fact that one recognizes a pattern that one has the capacity to reproduce. In this case it is clear that we are not merely imitating, or observing and then reproducing. It is crucial the recognition of a pattern that is taken as the standard with respect to which the drawing will be judged. Sperber thinks that the most important difference between Dawkins example and his is that in his example it is clear that **one requires not only the ability to describe a given result, but the ability to attribute ends and intentions**. Sperber concludes that *it is this attribution of intentions the cause of the normalizing role played by the instructions*. Instructions are not simply copied from one person to another.

I think Sperber is pointing to an important issue, but it is important to realize that the attribution of intentions requires sharing standards and identifying situations. Unless sufficient standards are shared the attribution of intentions would not play the role it is supposed to play. It is the sharing of situations what provides an explanation of the normalizing role of instructions. We might think that the recognition of structural patterns or natural kinds can play this supporting role. But this cannot be all there is to the answer. Think of Dawkins example. We can recognize an origami ship, even the sort of ship it is constructed, but if we are not familiar with the sort of activity involved in the origami technique we might not be able to understand what is intended. Someone who is familiar with the origami technique, or at least with the folding properties of paper that play a role in the instructions, will be able to learn fast and accurately, and would be the sort of reproducer that could correct a mistake. Similarly, think of the second example of Sperber, the drawing of a five peaks star. If you have never drawn this sort of thing, if you have not played with pencil and paper and have been challenged to do this sort of thing you will have a hard time recognizing what you are supposed to draw. A prerequisite for acquiring the ability to reproduce something (most often) is the recognition that this something **is not merely a type of thing but a type of activity that requires learning**.

Furthermore, as we see later, there are often cases in which stability cannot be explained in terms of the normalizing role of instructions. As we shall see these are not isolated cases, this is often the case when we pay attention to cultural processes whose stability is supported by the normalizing role of artifacts-representations used as symbols.⁷ Roughly, an artifact represents through its symbolized role, through its use. Thus, representation in this sense is not something we can know easily. Learning what a confocal microscope is, involves learning how the microscope is part of a lineage of artifact-representations. It involves learning how it forms part of scientific practices having certain general and specific objectives.⁸ Once it is recognized that what needs explanation is not shared beliefs but shared practices, artifacts-representations have to be in the center of attention of any explanation of the stability that matters in a model of cultural evolution, and a evolutionary model of science in particular.⁹

⁷ See Renfrew 1994.

⁸ Of course, this is not a simple matter. As Halle puts it: "There is no substitute for the difficult work of uncovering the symbolism of particular types of artefacts in particular types of social setting." (Halle 1998, p. 52).

⁹ It might seem that the way I am approaching the question of representation (as part of my effort to characterize the sort of stability that matters in cultural evolution) might be in any case suitable for the characterization of experimental traditions, in which for example we have artifacts like microscopes. However, in the sense that I am using the term, a diagram is an artifact-representation. Feynman's

5. Jack Goody is famous for the thesis that writing is “the technology of the intellect.” The idea is simple and powerful. Writing allows for ideas and norms to be “fixed” (to a text), to have generalizing power, that is, the capacity to be applied to new and diverse situations. Thus norms and standards become abstract representations of different more concrete norms. Literary traditions allow the development of more complex organizations than what is possible without writing, organizations that acquire a certain independence of their own associated often with the custodianship of the books and the preservation of the structure of norms associated with such writings. Goody shows how written formulations of codes or norms encourage its generalization, specialization and tailoring for very specific contexts (trade law, for example) and above all, its transportability to new contexts. Such modifications promote the diversification and selection of the generated alternatives. Written norms can thus accumulate and diversify as part of systems of abstract norms that do not apply to specific activities. Implicit in this account of writing as technology of the intellect there is a thesis about what is culture. Culture is not a mere mental phenomenon or situation, or a capacity to mentality in a genetic sense. Rather, culture is something learned and inherited.

Writing allows learning to diversify into a wide variety of different types of knowledge and allows such knowledge to be passed on through generations, and in that sense writing is associated with a diversification of norms supporting different institutions and practices. It is not important for us now to argue for the specific evolutionary nature of such processes. This could be done in different ways. The point is that such diversification of processes leads to a diversification of norms and practices with continuity in time that is not possible without the written word. The idea of culture implicit in Goody’s thesis is clearly not compatible with the idea of culture as information in the head, but I think it is compatible with the idea of culture as learned practices. This requires generalizing what we take “the written word” to be. Diagrams, and other artifact-representations (at least as they form part of certain sort of practices, laboratory practices, for example) can be seen as part of a generalized sort of writing. Such practices allow the fixing of norms, and its generalization and specialization, as the written word does in the case of laws and other norms.

The idea that culture can be identified with information is no doubt related with a common tendency to make a distinction between culture as abstract or as pertaining to “beliefs” and technique to the material and concrete. The idea of culture implicit in Goody’s characterization of writing as the technology of the intellect, as well as the idea of culture that stands behind our characterization of science as practices grounded on artifact-representations rejects such duality. But this rejection, I claim, requires also abandoning a traditional view of language as a system of representations encoding mental content. Language is more than encodings. The development of an alternative view requires advancing an account of those artifact-representations that I claim support the stability in question.

diagrams are artifact-representations and Euclidean diagrams are artifact-representations. In this connection it might be worth recalling the way in which Netz shows that Deduction gets stabilized as a type of inference (Netz 1999). According to Netz, Diagrams for Euclidean geometers were understood as practices that united the community of Euclidean geometers precisely because such diagrams articulated implicit norms about what was a good inference. For the Greeks, diagrams were not considered appendages of propositions; rather, they were considered to be the core of a proposition. Propositions were individuated by diagrams, and thus such diagrams and the implicit norms they represented (in the sense of artifact-representing I had introduced above) had to be seen as standards for a type of knowledge which was (relatively) autonomous from the propositions it allowed to individuate. That said, I hasten to add that I do not pretend that what I am calling artifact-representations are the only sort of representation there is or matters in cultural evolution. My claim is only that such representations are indispensable to understand the source of the stability that matters in cultural evolution.

A first step is to show how in the cognitive sciences, and in AI in particular, there are well motivated proposals that provide an account of representation that goes in the direction of our proposal.

6. Brooks tells us in 1999 how he came to see the need for a concept of representation that would not require what he called a central processing of symbols. Brooks presents his ideas contrasting two diagrams. The first diagram (figure 2) describes the traditional account.

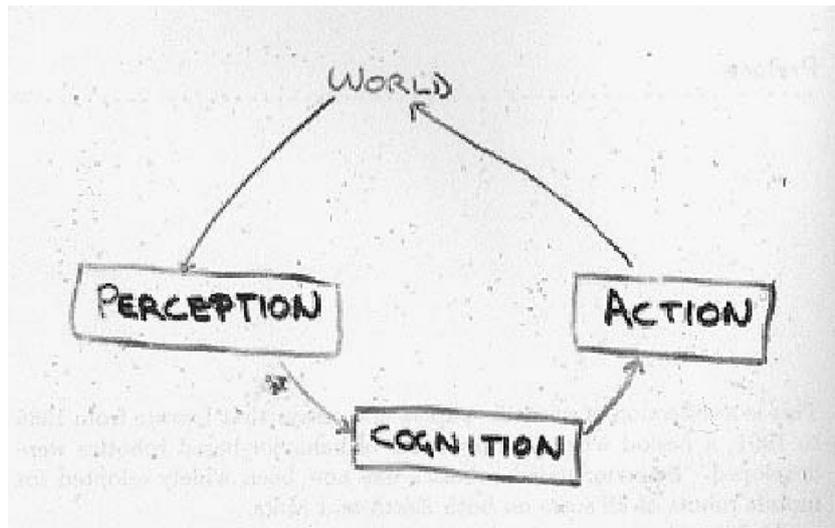


Fig. 2

Cognition is understood as mediating between perceptions and plans of action. Notice that in this view there is a centralized instance devoted to cognitive tasks. In this case an evolutionary model of culture could be developed in terms of the representations of perceptual processes, to the extent that cognition models perception, or in terms of the modeling of action, under the assumption of some ontology of the world.¹⁰ However, such ontology would enter as an unexplained (and ultimately unjustified) assumption, or in any case, as disassociated from the world (as perceived world). In this case an evolutionary model of culture would not be able to model the combination of normative and descriptive elements which constitute culture. In figure 3 Brooks depicts his view.

¹⁰ For example, as Sperber has pointed out in relation to Dawkins account, it is not the mere fact that there are instructions what makes the replication faithful, but *the fact that one recognizes a pattern that one has the capacity to reproduce*. In this case it is clear that we are not merely imitating, or observing (that is, going from the world to perception and then to cognition) and then reproducing (acting). As Sperber points out, it is crucial the recognition of a pattern that is taken as the standard with respect to which the drawing will be judged. But this would require a coordination between perception and action that is not explainable in Dawkins account, nor in Sperber's account, to the extent that such "pattern" involves sharing artifact-representations and the implicit normative structure associated with the relevant representational lineages.

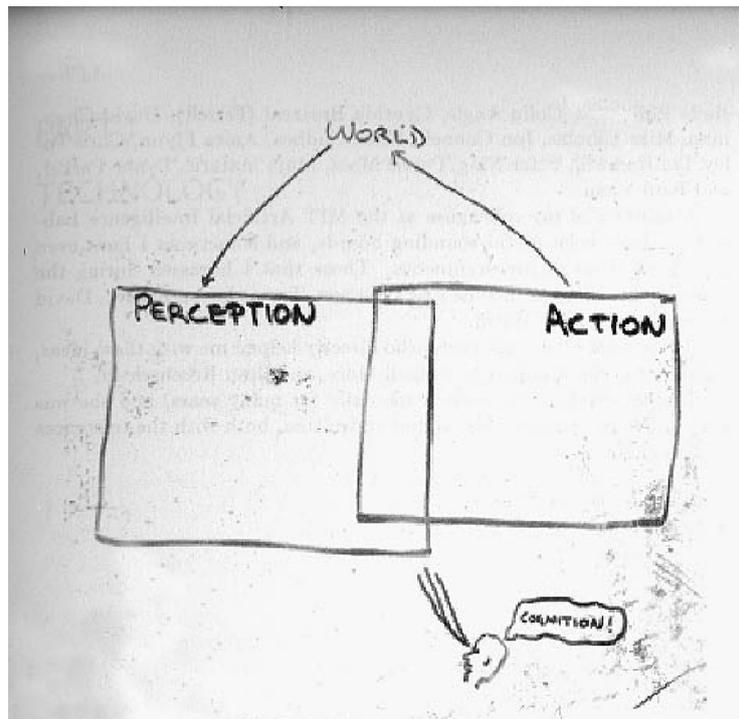


Fig. 3

According to this view there is no centralized cognition, rather, cognition takes place in the overlap of sensory and action systems. Ultimately, says Brooks, cognition is only a phenomenon defined for an observer attributing cognitive capacities to a system that interacts adequately with its environment.

Brooks tells us in the preface to his book 1999 that when he proposed this alternative view he had no idea of how to combine perception and action, or in other words, how to understand the overlap in figure 3. Only later he came to the idea that this could be done through the development of a different cognitive architecture. The key idea is that such cognitive architecture is “bottom-up”, cognition has to be the result of models of constraints that are the product of an evolution of technology which is analogous to the way biological evolution imposes constraints to human cognition. Below we will follow such idea to its consequences for models of cultural evolution that take seriously the concept of artifact-representations.

Bickhard and Terveen develop what can be seen as an alternative to Brooks account (and as far as I can see compatible) answer to understand the overlap in figure 3 (Bickhard and Terveen 1995). They suggest that, since the grounding of symbols is not as important as a characterization of the nature of interactions that ground the representations, the traditional view of representations cannot be made part of an evolutionary account of cognition, since “encodings” can only transform, “encode or recode representations that already exist” (p. 21). But in the interactionist view (the view suggested by Brooks and Bickhardt and Terveen among others), representations are constructed through development and learning, and thus representations have a history (a developmental history of the artifacts through which representations are used) that matters for understanding their role in cognition. Evolution of representation takes place through the accumulation of representational variants which are selected because of their contribution to potential strategies for future interaction (see Bickhard and Terveen 1995; Hendriks-Jansen 1996; Brooks 1999).

7. The point of this excursion into models of cognition is that in order for solving the problem that interests us, in order to account for the stability of normalization procedures which constitute cumulative cultural change, it is crucial to model such normalization procedures as an evolutionary process grounded on artifact-representations. The computational architecture behind traditional models of cognition cannot give the sort of principle explanation that would be required to account for the normative dimension distinctive of representational processes in cultural evolution. In order to be able to give a principled explanation of the origin of norms as this is required to account for culture we have to abandon the traditional account of representation as symbol processing and develop an account of representation grounded on the “overlap” mentioned by Brooks. Artifact-representations would be a way of elaborating such idea in the context of cultural phenomena. For such representations environmental feedback gets represented in use, and thus explains the origin of norms implicit in the characterization of the different situations that matter (i.e. that are significant).¹¹

Roughly, for the purposes of this paper, we will take language to be a systematic characterization of the situations that matter for making sense of the environment for groups of interacting agents as interacting agents in given situations. Language then is a way of abstracting situations from interactions, which can serve as scaffoldings for further abstraction. Such abstraction implicitly or explicitly identifies situations and generates cycles of “repeated assemblies” (see Caporael 2003). A suggestion of how such a view of language can be developed can be found in Bickhard 2009.¹² Now we have the elements required for the formulation of our modified version of Goody’s thesis.

To start with, instead of talking of “intellect” we shall talk of cognition. And the way in which we shall understand technology of cognition is not mere “internal technology”.¹³ Rather it is technology grounded in social relations and activities, distributed in stable environments articulated in practices, the maintenance and diversification of which allows for the diversification of variants of a technology, and its repeated assembly, which leads to its evolution. The claim is that not only writing is “technology of the intellect,” but all activities that are learned as part of practices that promote the stability of norms which in turn promote the spreading of technology (and science in particular). As we are generalizing Goody’s thesis, scientific practices are technologies of the intellect (understood in a broader externalist sense, as a characterization of the “overlap” mentioned by Brooks). In order to make sense of such proposal we have to say how science is to be understood as constituted by practices. And in particular, how practices are constituted that allow us to say that science can be understood as the technology of the intellect, or better, as the technology of cognition. Such account of science is at once an account of science as an evolutionary social process: science as the evolution of learned behavior.

8. Before we turn to an elaboration of such proposal we will have to say something about the crucial concept of scaffolding as a way of incorporating development in an evolutionary model of culture. This has been made above all in models of cultural evolution developed by Wimsatt and

¹¹ Of course, this requires abandoning the idea of language as mere symbol processing of mental representations, and thus requires abandoning the idea that representations can be characterized as mere information. In most of the social sciences such view of language is simply not taken seriously, but as we have seen, it has been important in models of cultural evolution impressed by the idea that culture can be disassociated from technology (and the planning for action).

¹² As Bickhard puts it: “Language is not the only way in which social realities can be interacted with, but language constitutes a(n institutionalized) convention for the productive construction of utterances that have conventional interactions with situation conventions – language is constituted as a conventionalized system for interacting with conventions” (Bickhard 2009, p. 580).

¹³ Cultural change for Goody, at least the sort of change that generates writing, involves a change in “the internal technology (of the intellect) which endows [a person] with the written word” (Goody 1998).

Griesemer. Scaffolding abstract general features of development in such a way that makes understandable how “extraorganismal cultural resources form repeated assemblies that serve as critical scaffolding for the development and inheritance of culture.” (Wimsatt and Griesemer 2007, p. 244). The order in which the configurations of resources turn into stable nodes serving as scaffoldings for further configurations creates “downstream dependencies which entrenches the dependencies in development.” (Wimsatt and Griesemer 2007, p. 244).

In a similar vein, I have suggested that cognitive resources get articulated in what I call “heuristic structures” which serve as scaffoldings for the development of inferential contexts and other cognitive resources.¹⁴ Such scaffolding takes place in the social environment nurtured by relevant institutions and practices. Both notions of scaffolding are quite close. Wimsatt and Griesemer emphasize the repeated assembly of entity-environment relations, and I emphasize the repeated assembly of “heuristic structures”, but ultimately, both notions of scaffolding are closely related with natural ways in which cultural entities become reproductive and form chains of inheritance which are dependent on (organismal and cultural) developmental history. One relevant difference is the following: Wimsatt and Griesemer follow Bickhard in suggesting that scaffolding creates “bracketed trajectories of potential development through artificially created nearby points of stability” (Bickhard 1992, p. 35; quoted in Wimsatt and Griesemer 2007, p. 229). Here the functional role of scaffoldings is closely related to the idea that in given “windows” of time scaffolding lowers “fitness barriers” to developmental performances or achievements. Whereas in the sense I tend to use the term scaffolding is related primarily to the way different resources get distributed in practices as implicit structure required for the display of cognitive abilities in socially meaningful space. They are not provisional in time, but rather implicit or in the background.

I use the notion of scaffolding very much in the sense that cosmologists say that dark matter scaffolds visible matter. Scaffoldings are often implicit resources. But also, scaffolding in my sense includes for example the way in which medieval masters used earlier buildings as “approximate models” to estimate the stability of a new design (see Mark 1990). Such new designs increased its fitness through the use of earlier structures, which in my sense functioned as scaffoldings. This is very much the sense in which I think heuristic structures function as “paradigms” or “approximate models” guiding the evaluation of alternative scientific-technological designs (see Martínez 2003). Such paradigmatic buildings can hardly be thought as “generatively entrenched” in the sense of Wimsatt, but certainly we should think of them as playing a role in the generation of new buildings (and the selection of new variants of designs). One can think of such model-buildings as points of reference in *path dependent developments*.¹⁵ Mark claims for example that different sort of evidence

¹⁴ I have characterized a heuristic structure as a group of heuristic procedures integrated in a normative (hierarchical) structure with functional coherence that gives shape to a practice. A heuristic rule or procedure requires the implicit recognition of a situation or context (which often consists of norms or involves norms or standards) as part of the characterization of the procedure. That the heuristic is not a mere universal rule constrained to a given context can be seen from the fact that a heuristic leads to the right decision or answer (or more generally, answer to norms) in a biased way. Error is not random (a point often emphasized by Wimsatt). A technique is a kind of heuristic structure that leads to the production of standards, phenomena, technology or further techniques. See Martínez 1995 and Martínez 2003.

¹⁵ Margolis (Margolis 1993) argues that the emergence of probability was delayed until the development of a new habit of mind (or as I would prefer to say, heuristic structure) developed that had a use for the new notion. Before the development of such new way of thinking the concept of probability had no use, its use was contained by “barriers” associated with old habits of thinking. Clearly these “barriers” can function as “fitness barriers” in the sense used by Bickhard (and Wimsatt and Griesemer). But such “habits of mind” or “heuristic structures” also function as scaffolding in the sense that they support

support his thesis that the cathedrals of Bourges and Chartres, were constructed with a design that took in consideration lessons that lead to a modification of the buttressing system used in Notre-Dame. Such role of early buildings is analogous to the sense in which early heuristic structures play the roles of referents or “approximate models” for later heuristic structures. In science, the way in which the design of experiments gets modified through the history of science has a similar path dependent structure (see Martínez 1995). The way in which for example J. Margolis talks of “habits of mind” as entrenched responses to ordinary problems that take place without conscious attention is a very good example of scaffoldings in the sense I think is important to emphasize: as reference points for path dependencies.¹⁶

9. Now back to our question. If culture is information store in human brains then the problem of stability is a problem about the reliability of the channels of cultural transmission. In this case “observational learning” or a similar mechanism has to play a central role in the explanation of the stability. To the extent that culture is technology of cognition articulated in artifact-representations, the stability can be explained through path dependence and (generative) entrenchment.¹⁷ Since science is a paradigmatic example of processes constituted by lineages of artifact-representations articulated in practices, science can be seen as evolving technology of cognition. As Brooks suggests for the case of robotics, to the extent that cognitive architecture with explanatory power is “bottom-up”, cognition has to be understood as the result of models of constraints that are the product of evolution of whatever social and cognitive organization we are willing to call culture. In this case, the stability of culture is explained as a by-product of the evolving structure of those scaffoldings that constitute the path dependent processes we identify as culture. Writing is an important example of a cognitive technology that promotes the complexity of cultural organizations thorough its capacity to provide abstract versions of norms that can represent a variety of more concrete norms, and render explicit and stable its content. Scientific practices through the management of artifact-representations constitute technology of cognition that can represent in a stable manner a variety of norms implicit in practices. Such stability promote the diversification and specialization of the sort of concepts, models and explanations that are distinctive of specific scientific practices and that can be seen as paradigmatic examples of cultural evolution.

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artifact-representations that tend to be differentially reproduced.

¹⁶ See Margolis 1993.

¹⁷ The difference between the concepts of generative entrenchment and path dependence are related to the differences I have pointed out between different notions of scaffolding. I elaborate this distinction elsewhere.

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WON'T YOU PLEASE UNITE? CULTURAL EVOLUTION AND KINDS OF SYNTHESIS

Maria E. Kronfeldner

“Nothing in biology makes sense except in the light of evolution,” Dobzhansky (1973) famously said. Today the phrase seems to have mutated to an all-encompassing slogan, spanning all areas of science and society: nothing at all seems to make sense except in the light of evolution. Almost everything that is able to change and does not change in a sudden and abrupt way is said to evolve. Political agendas, partnerships, economies, firms, behavioral patterns, and theories – they evolve. Stars, galaxies and the universe – they evolve too. Richard Dawkins (1983) has tried to convince scientists and the public that we need a ‘universal Darwinism’, while Donald Campbell (1997) and David Hull et al. (2001) defend a ‘general selection theory’. Finally, since the so-called ‘Modern Synthesis’ has gone stale, a new grand synthesis has been announced, or called for, in expanding or (re-)widening the ‘evolutionary synthesis’ of the 1930s to 50s in various directions: towards neutral evolution, post-genomics, epigenetics, eco-evo-devo, and, last, but not least, towards culture.¹ ‘Won’t you please unite,’ in the name of evolution, is the slogan that seems to be everywhere.

For this paper, the most important aspect of these calls for an extension of the Modern Synthesis is that they seem to rely on an implicit epistemic bias: a bias that favors unity rather than difference. It is this bias and the value of specific kinds of syntheses that will be central here. What kind of synthesis the Modern Synthesis actually was, and what or whom it left out, are issues that have since long been a matter of debate.² I won’t say anything on these issues. I will rather address the kinds of synthesis that are involved when we extend the evolutionary synthesis towards culture. By using the history of theories of cultural evolution, I will then develop an outline for an argument against the bias towards synthesis.

After illustrating in section 1 in more detail how culture enters the evolutionary frame and what I mean by an epistemic bias towards synthesis, I shall present in section 2 a short history of theories of cultural evolution, followed by a review of two contemporary models: memetics and contemporary dual inheritance theories. In section 3, I will proceed to an analysis of four kinds of synthesis that usually enter the debate about the relationship between culture and evolution. I will distinguish between (i) *the integration of fields*, (ii) *the heuristic generation of interfields*, (iii) *expansion of validity*, and (iv) *the creation of a common frame of discourse or a ‘big-picture’*. These will encompass the four most important kinds of synthesis involved in theories of cultural evolution. Central for the issue about the epistemic value of synthesis is the relation between (i) and (ii). I shall thus develop in section 4 some critical notes on the value of synthesis from a historical point of view.

The overall aim of the paper is also to introduce a new stance in discussions about cultural evolution. So far, theories of cultural evolution have been addresses from mainly two stances. There are those who take a skeptical stance (e.g. Fracchia and Lewontin 1999): this skeptical or critical stance focuses on conceptual analysis and on finding or denying at a theoretical level the perils of theories of cultural evolution. Yet, there are also those who are fed up with such debates

¹ See the short report in Whitfield (2008) or Blute (2008). For more details on the extended evolutionary synthesis see Pigliucci and Müller (forthcoming).

² See, for instance, Mayr and Provine (1980), Gould (1983), Bechtel (1986), Mayr (1993), Smocovitis (1996), Love (2009).

and simply test the theories in the wilderness of empirical research, i.e. in the different fields touched by theories of cultural evolution. They take the empirical stance. I will take a third, a reflective stance. Its aim is, first, to make the often implicit epistemic criteria explicit – criteria used for evaluating the analogy in the skeptical and the empirical stance. Its aim is, second, to compare how these criteria are connected to specific kinds of synthesis.

1. Three theoretical roles of culture and a bias towards synthesis

In principle, culture can enter an all-encompassing evolutionary perspective in three different ways. First of all, culture can be considered as a factor in the development of individuals, influencing the phenotype and co-determining with other factors the selection pressures of individual organisms. The disciplinary contexts in which this role is important include developmental psychology, other fields of psychology, educational research, and the like.

Second, culture can be taken as a separate system (or process) of heredity and evolution. Cultural change is then treated as an evolutionary process in its own right, i.e. as cultural evolution occurring in addition to biological evolution of organisms and biological species. If culture occupies this theoretical role, then culture is not a factor (part of the explanans of development) but an explanandum, i.e. a phenomenon or subject matter that one wants to explain. The disciplines that have culture as an explanandum in this sense are cultural anthropology, sociology, economics, history, and the like.

Third, culture can appear as a phylogenetic factor in the overall system (or process) of evolution of organisms, which have a body, a mind as well as a culture. As a factor in the phylogenetic evolution of organisms, culture changes not only the phenotype, but also the environment and can lead to effects known as co-evolution, niche construction, or the so-called Baldwin effect etc.³

Here, the second role will be in focus. It is the one that is most interesting if forms of synthesis are at issue, since, historically, the concept of cultural evolution has been involved in two diametrically opposed initiatives: one opposing a specific kind of synthesis at the beginning of the 20th century, and one furthering a specific kind of synthesis today. Both initiatives – the one for separation and the one for unity – led to new important insights, as this paper aims to illustrate.

One of the reasons, however, why unity is often favored is that it is thought to be fruitful in the sense of leading to new insights, theories, or even fields. That this does not exclude that separation can be equally fruitful should be evident, but might well be ignored in discussing synthesizing social sciences and humanities with evolutionary thinking. The close coupling of the proliferation of disciplines in the last 200 years, and the accelerated change in the sciences since then, points already against a bias towards synthesis. Separation has fruitful potential. In this paper, however, I will look at one specific example: theories of cultural change that use concepts from biology to understand culture can be maintained with a clear separationist stance and can be fruitful nonetheless. In other words, the claim is that a separationist stance can also lead to important novel scientific fruits to harvest for scientific change. To use an analogy myself: the evolutionary synthesis showed us that ‘geographic’ isolation is a creative factor in the evolution of species and this paper aims to provide a first step towards a more balanced and contextualised view of the value of synthesis: *isolation and plurality can equally be creative, not only in nature, but also in science.*

To reach this balanced and contextualised point of view the kinds of synthesis involved in the analogical transfer of the concept of evolution to the phenomenon of culture have to be clearly

³ For an analysis of the origin of these three roles and a more detailed account of them see Kronfeldner (2009).

delineated. Let me first point to the kinds of unities that are not at issue. An analogical transfer of evolutionary ideas to culture is neither concerned with, nor excludes hierarchical kinds of a general 'unity of science'. At issue here is not the question of whether culture is part of a compositional hierarchy of entities, with the entities of physics as the most fundamental ones (*ontological unity of science*). Theories of cultural evolution are simply not concerned with this kind of synthesis, even though they are compatible with a compositional hierarchy and the related unity of science. Similarly, at issue is not whether the theory of cultural evolution can be reduced to the theory of biological evolution (*reductive unity of scientific theories*). Most of the time, the question of the unity of the *scientific method* is also not at issue, except for the discussion about quantitative versus qualitative methods in social sciences and humanities.⁴ Thus, we do not have to worry about these traditional, complicated, and in history and philosophy of science extensively treated issues of ontological, theory-reductive, or methodological unity of science. With this in mind we can proceed to discuss other kinds of synthesis. But before we can do so, a clearer picture about theories of cultural evolution has to be outlined.

2. Cultural evolution from Darwin till today

If we extend evolutionary theory to culture as a separate system of heredity and change, we apply the Darwinian 'paradigm' to culture in an analogous or formal manner. Darwinian analogical reasoning was used already back in the days of Darwin. Charles Darwin (1859; 1871) himself spoke of the evolution of languages: they develop and differentiate in a similar fashion as biological species. At the end of first edition of "*On the Origin of Species*" he then wrote: "In the distant future I see open fields for far more important researches. Psychology will be based on a new foundation, that of the necessary acquirement of each mental power and capacity by gradation. Light will be thrown on the origin of man and his history" (Darwin 1859, p. 487). Yet, with 'distant' and 'important' he did not mean fellows like us at the beginning of the 21st century. This becomes evident from the edition of 1877, where he decided to be a bit more specific. The same passage now reads: "In the future I see open fields for far more important researches. Psychology will be based on THE foundation already well laid by Mr. Herbert Spencer, that of the necessary acquirement of each mental power and capacity by gradation. MUCH light will be thrown on the origin of man and his history." (Darwin 1876, p. 427; Emph. added)

Herbert Spencer applied evolutionary thinking to almost everything, including culture, society, and mind.⁵ The latter is often ignored since his social Darwinism has dominated the reception of his philosophy. William James (1880) also wrote in his famous essay on *Great Men and Their Environment*: "A remarkable parallel, which I think has never been noticed, obtains between the facts of social evolution on the one hand, and of zoological evolution as expounded by Mr. Darwin on the other" (James 1880, p. 163). But according to William James, Spencer was still too much of a Lamarckian, which he was. He therefore ends up, James complains, with false pictures about mind and culture. For James, Spencer was not a Darwinist since the decisive part of Darwin's theory was that it allowed portraying humans as free in the following sense. Humans are not just reacting to the world, as in a Lamarckian picture, which he treated as analogical to theories of associationist learning; on the contrary, humans freely create ideas and select them afterwards. After having ideas freely generated in the mind, ideas are tested against the world. Some survive the test, some die. Since it is the accumulation of free acts of individuals, the same holds for

⁴ See for instance Mesoudi (2007) for defending theories of cultural evolution because they allow for quantitative approaches. See Fracchia and Lewontin (1999) for a critique of this as narrow-minded scientism.

⁵ See, for instance, Spencer (1898).

cultural change (i.e. history). In a nutshell, Darwinism applied to mind and culture meant for James freedom, in strong contrast to most people at the end of the 19th century. But it could do so only since he applied evolutionary theory in a strictly analogous manner.

Then Alfred L. Kroeber (1917) came along and used Weismann's Neo-Darwinism, in a similar manner as James used Darwin. But while James focused on the historical importance of the individual and on the independence of the human mind from sense experience and thus from the 'law of association,' Kroeber focused on the independence of cultural change from biological evolution. Yet, he used, as James did, Darwinian theorizing to do so. The point of view he defended was that culture is a phenomenon *sui generis*, and comes 'on top' of biological evolution, since it is, as biological evolution, a system of heredity and change in its own right. And most importantly, he claimed that we could see this parallel only if we take Neo-Darwinism seriously and that meant: to abandon any belief in Lamarckian inheritance of acquired characteristics. I will say more on his case below.

Cziko (1995, p. 134) refers to Alexander Bain as the first one stressing an analogy between biological evolution and scientific discoveries as early as 1868. For Bain the key about scientific discoveries was trial-and-error, which was interpreted as analogous to the process of biological evolution as Darwin described it. Augustus Pitt-Rivers, Thomas H. Huxley, James M. Baldwin, Chancey Wright, Paul Souriau, and Ernst Mach, and certainly many others are also on the list of having drawn an analogy between evolution and the development of human culture and mind.⁶

As indicated above, today evolution is everywhere. There is "evolutionary-" epistemology, game theory, computing, medicine, ethics, aesthetics, economy, psychology, linguistics, pedagogy, evolutionary approaches to creativity, etc., and, last but not least, theories of cultural evolution – the heirs of James' and Kroeber's approaches, focusing on human history or cultural change and using evolutionary theory to understand it. Even if all these different approaches use evolutionary theory, they all try to describe and explain different phenomena, cultural change is only one of them.⁷ Furthermore, even if they want to describe and explain the same phenomenon, they might still pick different elements from contemporary Darwinism or interpret the elements in different manner. One of the differently interpreted elements is the concept of heredity.

Today, there are two schools that dominate the analogical applications of evolutionary theory to cultural change: memetics and dual inheritance theories. The standard reference point of both are two classical papers of Donald T. Campbell (1960; 1965): *Blind Variation and Selective Retention in Creative Thought as in Other Knowledge Process* and *Variation and Selective Retention in Socio-Cultural Evolution*. Before I describe how memetics and dual inheritance theories differ, let me summarize why they are labeled 'Darwinian' and why the label is denied to others. Darwinian models are usually taken to assume specific 'mechanisms' of change, e.g. selection processes, and try to derive macro-patterns from these.⁸ They do not refer to progressive stages. They are variational and populational rather than transformational or essentialist.⁹ They rely on a tripartite model for the mechanism of natural selection: variation – differential reproduction – heredity.¹⁰ Finally, they do allow for neutral change (e.g. drift) and for multi-level selection (e.g. cultural group selection).¹¹ All these points are common characteristics shared by Darwinian approaches

⁶ See Campbell (1960) or Cziko (1995, pp. 134-140).

⁷ For a review of the diversity of evolutionary approaches in the social sciences see O'Malley (2007).

⁸ See Campbell (1965) and Mesoudi (2007).

⁹ See Mayr (1959), Lewontin (1983), Kronfeldner (2007b), and Mesoudi (2007) on this issue.

¹⁰ See Lewontin (1970) and Fracchia and Lewontin (1999).

¹¹ See Mesoudi (2007) for references on drift and Richerson and Boyd (2005) for cultural group selection.

to cultural evolution.¹² Yet, there are also great differences between them: first, with respect to heredity, and second with respect to what the theory is meant to explain.

Memetics relies on the postulate of so-called 'memes,' the alleged basic building blocks of culture, which are considered as having analogous properties and causal roles as genes in biological evolution. Richard Dawkins introduced this idea in his book *The Selfish Gene* (1976). It was mainly Daniel C. Dennett¹³ and David Hull,¹⁴ who backed up memetics with philosophical details. Others followed the idea with varying sophistication and emphasis.¹⁵ For memetics, cultural items are, like genes, replicators and it is the fitness of the meme itself that accounts for the diffusion of cultural items. As evolutionary biology is reducible to the replication of genes, cultural diffusion is reducible to the replication of 'memes' – a process that is guided by the fitness of genes or memes alone. Organisms, in the case of genes, and minds, in the case of memes, are mere hosts that are built by these replicators. They are mere consequences of the replicative power of memes. We can eliminate mind in our account of cultural change – if not ontologically, then as an explanatory important unit. Susan Blackmore is, besides Dennett, most famous for defending this seemingly radical thesis. At the end of her book, *The Meme Machine* (1999) she writes:

This is the power and beauty of memetics: it allows us to see how human lives, language, and creativity all come about through the same kind of replicator power as did design in the biological world. The replicators are different, but the process is the same. We once thought that biological design needed a creator, but we now know that natural selection can do all the designing on its own. Similarly, we once thought that human design required a conscious designer inside us, but we now know that memetic selection can do it on its own. [...] If we take memetics seriously there is no room for anyone or anything to jump into the evolutionary process and stop it, direct it, or do anything to it. There is just the evolutionary process of genes and memes playing itself endlessly out – and no one watching (Blackmore 1999, p. 242).

In a nutshell, according to memeticists, the unit that plays the main *causal role* in cultural change, and hence an important explanatory role, is not the human person, it is memes, which are thought to be 'selfish replicators' like genes. The explanatory goal is the diffusion of cultural units in a population of humans (or even the nature of mind). The time frame for the first explanandum is rather limited, as Gayon (2005) has stressed: it is about 100 years.

In parallel to memetics, Luigi L. Cavalli-Sforza and Marc W. Feldman (1981), Robert Boyd and Peter Richerson (1985, 2005), and William H. Durham (1991) developed the philosophical frame of Campbell into *dual inheritance theories*, quantitative theories of cultural change. The literature on this field, also called gene-culture co-evolution, has exploded in the last couple of years. It finally was widened towards multiple inheritance views, claiming that we actually have at least four different systems of heredity interacting in the evolution of organisms: genetic, epigenetic, behavioral, and cultural heredity (Jablonka and Lamb 2005). It is a tradition that now also includes detailed phylogenetic applications of the Darwinian frame (Gray et al. forthcoming). Cultural evolution is then a part of the overall process of evolution, relying on a specific channel of heredity between organisms. All the approaches here summarized under the label 'multiple inheritance theories' use Darwinism in the sense that they try to describe and explain diffusion processes and

¹² Mesoudi (2007) states that a further common characteristic is that they allow for the inheritance of acquired characteristics. See Kronfeldner (2007b), claiming that it is either wrong, misleading, or tautological to say that cultural evolution relies on the inheritance of acquired characteristics.

¹³ Dennett (1995; 2001; 2002).

¹⁴ Hull (1982; 2000).

¹⁵ E. g. Brodie (1995), Lynch (1996), Balkin (1998), Aunger (2002).

the consequent higher frequency of the cultural items, either in a given population or over phylogenetic, i.e. historical time. Their explanatory goals are mainly two-fold: they either try to uncover cultural changes itself, e.g. the effects of different transmission patterns on the pattern of diffusion; or they try to study how culture coevolves with biology, i.e. how they influence each other. Tracking the phylogeny of cultures and studying the co-evolution of culture and biology includes a much longer time frame than the one for studying cultural change.

Let me refer to three examples to illustrate that these approaches let to some interesting new hypotheses. They try to show for instance in a statistical manner how biologically maladaptive behavior can evolve on the basis of specific cultural transmission settings. Preferences for reduced family size, for instance, are maladaptive in the biological sense, since they reduce the reproductive output. These preferences can nonetheless spread in a population, if the transmission of these preferences is not vertically, between parents and children, but horizontally, between peers and unrelated people. Given horizontal transmission, biologically maladaptive traits can spread. Furthermore, they try to show that different modes of learning (individual learning, prestige bias, conformist bias, success bias, etc., all settings analogous to the biological mechanisms of heredity) lead to different macro-evolutionary patterns. Mesoudi (2007) refers to the following as an example. Bettinger and Eerkens (1999) studied variation in projectile point designs from the prehistoric Great Basin.

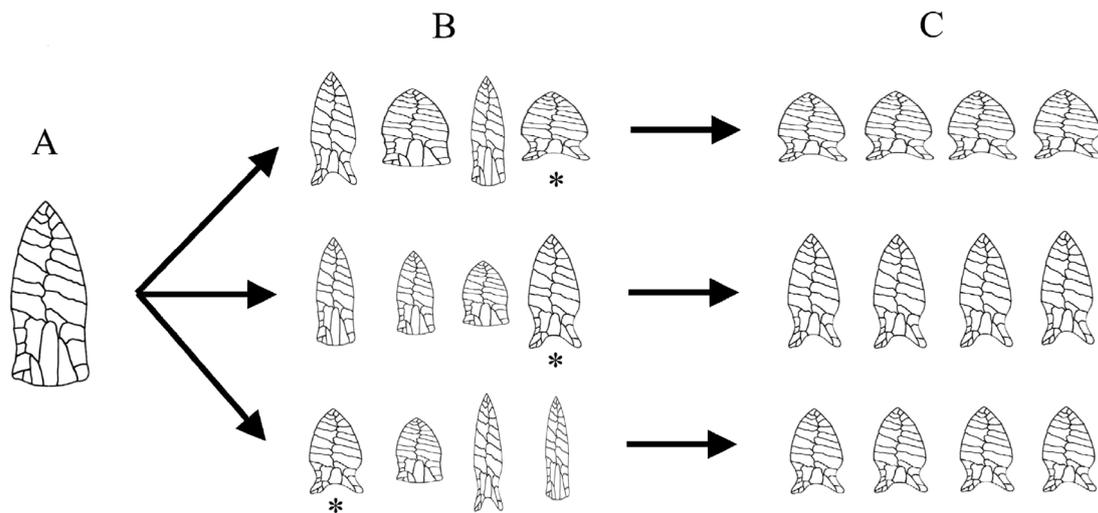


Figure 1 (from Mesoudi 2007, p. 270).

Mesoudi summarizes their account, depicted in Figure 1, as follows: “An ancestral point design (A) spreads to different groups (B) where it diverges due to idiosyncratic individual learning. In C, indirectly biased cultural transmission causes the single point design used by the most successful hunter (marked with a * in B) to spread within each group. According to Bettinger and Eerkens, prehistoric California resembled B, where point attributes correlated poorly with one another, while prehistoric Nevada resembled C, where point attribute inter-correlations were high. This scenario (A→B→C) was simulated experimentally by Mesoudi and O’Brien (in press).”

Finally, dual inheritance theories often argue that culture is a phylogenetic factor in the evolution of organism. Lactose intolerance is the standard example (Richerson and Boyd 2005; Durham 1991). Since some people in some areas relied in the past heavily on dairy farming they now have genes that allows them to digest cow milk even as adults, which fosters dairy farming. In turn, this fosters the selection of genes for milk digestion, etc. This is co-evolution, where we have

nature via culture and culture via nature, not only ontogenetically but also phylogenetically, even though there are no 'genes for' dairy farming. The important consequence that can be derived from such examples of co-evolution is that it revises our dualistic picture about the evolutionary relationship between nature and culture. We learned to believe that humans are distinct because of culture, and that we (as a species) grew out of nature and into culture. We don't. We evolved to our nature via culture and we got our culture via our nature. That is an important message harvested from the 'tree of sciences', (if it is a tree at all), made possible by mutual interaction between the strong but flexible branches of science, i.e. made possible by the disciplinary structure of science allowing for interdisciplinary interaction.

Let me stress some of the differences between memetics and dual inheritance theories. Although dual inheritance approaches rely on the idea that culture is a diffusion process that is analogous to a selection process in nature, they deny that there is a strong analogy between cultural change and biological evolution. According to these approaches, cultural items do not replicate, the origination of novelty is not 'blind' as in biological evolution, and the selection is driven by more-or-less rational decisions of individuals. They also deny that memes have explanatory priority over individuals. In other words, the model is not built on a narrow 'meme selectionism'. They insist that the fate of cultural items is determined by a set of multiple factors, including the more-or-less rational decisions of human persons and the structure of the social system, which are not memes. Nonetheless, they insist on the fruitfulness of using the evolutionary paradigm for describing cultural change.

Thus, the question remains: what do we gain by synthesizing the now often called 'two cultures'¹⁶ of science by using an analogy between biological evolution and cultural change?

3. *Evaluating an analogy and four kinds of synthesis*

Analogies never state similarities in all respects, i.e., a total equivalence of the base and the target of the analogy. An analogy states similarity in dissimilars. We can therefore not condemn an analogy as ill guided, wrong or fruitless simply because there are differences between the base and the target of the analogy, e.g. between cultural heredity and biological heredity. Yet, somehow we have to evaluate the analogy, but how? I suggest using some of the standard epistemic virtues or values discussed in philosophy of science to do so. These values will, finally, also guide us to four kinds of synthesis that can be achieved by using the analogy.

If an analogy is to be a good one, then relevant similarities (e.g. those stressed by the analogy) must exist. That is, the resulting theory must fulfill the standard of empirical adequacy. If the theory claims that culture consists of replicators and this is wrong, which I think it is, then the analogy is empirically inadequate. If it is true that culture is variational, which I think it is, then it is empirically adequate to claim that cultural evolution is a selection process as biological evolution is.¹⁷

The resulting theory should also be internally and externally consistent, as any theory. External consistency is especially important since it asks for *integration* of insights from other disciplines or fields. External consistency leads to integration and provides us thus with the first kind of synthesis that we have to take into account, if we want to understand why people want to use the analogy. Integration is a kind of synthesis often asked for by stakeholders at the crossroad of biology and social sciences. An example will follow below.

¹⁶ With reference to Snow (1969).

¹⁷ See Kronfeldner (2007a) for detailed arguments in that direction.

An analogy should furthermore lead to a theory that is explanatorily adequate. Explanatory adequacy obtains, if an explanation is not tautological and if it is competitive, i.e. if it is offered at a level of ‘depth’ of explanation that is standard in a specific domain occupied with a specific subject matter. Large parts of psychology, for instance, have reached a ‘depth’ of explanation that includes cognitive mechanisms and not merely beliefs and desires. Yet, most of them have not reached the level of neuronal patterns. Yet, the problem for memetics is that it not even reaches the standard of cognitive mechanisms. If meme replication, for instance, simply says that people learn from each other, then replication is not a concept offered at the level of cognitive mechanisms and the claim that culture rests on replication is thus explanatorily fruitless, i.e. trivial.¹⁸

An analogy should also be *heuristically fruitful*, i.e. leading to new descriptions, explanations, or at least new problems. Heuristic fruitfulness is, as external consistency, an important value, especially for this study, since it is, as external consistency, connected to a kind of synthesis between academic disciplines or fields. The three examples of new hypotheses generated by the co-evolutionary program mentioned above indicate that dual inheritance theories can fulfill this standard. In addition, above I presented co-evolutionary explanations, such as the explanation of lactose intolerance, as creating not only new insights but a whole new theory for an area belonging neither to natural sciences nor to social sciences alone. In other words, co-evolutionary theory is an interfield theory.¹⁹ It creates or defines new problems or even fields of problems. This is more than integration, which is crossing boundaries between disciplines in order to get resources for a given problem. Coevolutionary theories thus established what I would like to call a *heuristic synthesis*: the heuristic establishment of new problems or even new interfields.

In classical accounts of epistemic values, discussed in post-Kuhnian approaches to confirmation theory, scope is also on the list of virtues for theories. Yet, an increased scope can refer to different issues. One is expansion, i.e. increasing the validity of a theory by *expanding its range of application*. This is connected to the quest for a reductive unity of science: you reduce a theory if you show that you can derive it from a more general theory, i.e. if this general theory is shown to apply to the to-be-reduced part of the ‘world’. One example should suffice: when you try to reduce mental properties to physical ones, you try to show that physical laws hold for this part of the world in the same way as they hold for stones. Fracchia and Lewontin (1999, p. 54) are thus very likely correct in stating that “the demand for a theory of cultural evolution also arose from among the natural sciences, particularly among evolutionary biologists for whom the ability to explain all properties of all living organism, using a common evolutionary mechanism, is the ultimate test of the validity of their science.” Even though one would have to support this claim with detailed case studies, I think that the motivation biologists have for applying evolutionary theory outside of the realm of biology is very likely often driven by the epistemic value of increasing scope. Richard Dawkins (1982, p. 112), for instance, justified his idea that culture is governed by ‘memes’ along these lines. He did so after he was severely criticized for the idea as not being a fruitful theory of culture. In a nutshell, his reply to the critique was that what he intended was not a theory of culture but rather to illustrate the scope of his concept of replication, which secures the foundation of his gene selectionism.

Others, however, might appreciate theories of cultural evolution for a different reason connected to scope. Philosophers, and certainly many others as well, often watch out for a common frame of discourse or a ‘big picture’. Thus, they want, for instance, a *‘Menschenbild’*, a unified image of man, which none of the specialized sciences can provide anymore from its own sources alone. The current specialization of sciences and the consequent division of labor between them is increasingly judged to be devastating for any such unified understanding of being human. With

¹⁸ See Kronfeldner (2007a) for a detailed critique along these lines.

¹⁹ The term interfield theory stems from a paper from Darden and Maull (1977).

the disciplinary structure of science, human life has been stratified. But for practical or existential reasons, we still strive for a unified picture of ourselves. Thus, a *'Menschenbild'* has to be synthesized out of the bits and pieces offered by the multitude of sciences. Consequently, concepts that allow knitting the bits and pieces together are very likely much welcome, even though they might not do any explanatory or heuristically fruitful work, except the one that it allows the knitting together of the bits and pieces. One of the reasons why David Hull (2000, pp. 43, 46) appreciated memetics is that it allows us to have a common language for constructing a big picture.²⁰ More examples could certainly be named. Yet the intention here is simply that this *can* be the motivation beyond bringing culture and evolution together. It is an important motivation since it provides us with our fourth kind of synthesis: *big-picture-synthesis*.

Four kinds of unity thus emerged from our analysis. They are: (i) *integration*, (ii) *heuristic synthesis*, (iii) *increasing scope*, and (iv) *big-picture-synthesis*. I will not use them to discuss the value of specific version of the analogy between cultural change and human history. Only the following will be important. Given that resistance to the analogy between culture and evolution relies on one or more of the values above, disagreement about the analogy probably also depends on the choice of the value. The analogy might turn out to be justifiably given one value and might fail to do so given another one. One example has to suffice. In their well-known critique of theories of cultural evolution, Fracchia and Lewontin (1999, pp. 67-78) complain, besides other things, that integration or expansion is gained at the cost of explanatory depth, a price they are not prepared to pay. "[B]ecause cultural evolutionary theories are based on a unitary, transhistorical principle, they produce explanations that are too broad to be either falsifiable or explanatory." (ibid., p. 76) Yet, they ignore that others might have reasons for paying that price or that the theory might be heuristically fruitful with respect to specific hypotheses and a good one on that ground. Still others, in turn, might wrongly correlate integration with heuristic fruitfulness and ignore that resisting integration can also be fruitful. They would ignore or wrongly assume a certain relation between the disparate epistemic values.

What I shall do in the remaining is to show that integration and heuristic synthesis – and the respective epistemic values supporting them – are distinct and independent: one can occur without the other. There can be integration that fails to be heuristically fruitful and there can be heuristic synthesis (generation of new ideas, fields, etc.) without integration, i.e. on the basis of separation. Only the latter will be illustrated. There are cases where it is more productive, in the service of scientific change, to batten down the hatches of ones scientific horizon. Sometimes it is fruitful to separate one from other perspectives and to ignore, for specific goals, that, well, everything in reality hangs together and nothing is thus autonomous.

4. *Integration, separation and the fruits from the tree of sciences*

I treat the following views as representative for a widespread bias in current debates about evolution and culture. Outlining the reasons why social scientists have to listen to the 'insights' of evolutionary psychology, Barkow, Cosmides and Tooby write:

Conceptual integration generates this powerful growth in knowledge because it allows investigators to use knowledge developed in other disciplines to solve problems in their own. The causal links between fields create anchor points that allow one to bridge theoretical or methodological gaps that one's own field may not be able to span. This can happen in the behavioral and social sciences, just as it has happened in the natural sciences. Evidence about cultural variation can help cognitive scientists decide between competing models of universal cognitive processes; evidence about the structure of memory and attention can help cultural

²⁰ See also Geertz (1966) against the 'stratificatory' account of man.

anthropologists understand why some myths and ideas spread quickly and easily while others do not [...] At present, crossing such boundaries is often met with xenophobia, packaged in the form of such familiar accusations as ‘intellectual imperialism’ or ‘reductionism.’ But by calling for conceptual integration in the behavioral and social sciences we are neither calling for reductionism nor for the conquest and assimilation of one field by another. Theories of selection pressures are not theories of psychology. And theories of psychology are not theories of culture; they are theories about some of the causal mechanisms that shape cultural forms. [...] conceptual integration simply involves learning to accept with grace the irreplaceable intellectual gifts offered by other fields. To do this, one must accept the tenet of mutual consistency among disciplines, with its allied recognition that there are causal links between them. Compatibility is a misleadingly modest requirement, however for it is an absolute one. Consequently, accepting these gifts is not always easy, because other fields may indeed bring the unwelcome news that favored theories have problems that require reformulation. (Barkow, Cosmides and Tooby 1992, pp. 12-13)

As indicated above, nobody involved in debates about evolution and culture asks for reductionism in the sense that we should give up the disciplinary structure of science. The disciplinary structure of science developed hand in hand with Darwin’s brainchild and stands today as a bulwark in the way of any imperialist, reductionist unification and does so for a reason. But, as said, Barkow, Cosmides and Tooby do not ask for this, they ask for integration, i.e. external consistency.

Describing the way scholars and scientists from different backgrounds discussed the biological foundation of human culture, Peter Weingart reports:

[...] we experienced a Babylonian confusion of disciplinary languages, the thematic unity and social proximity gradually led to the realization that methods could be transferred, terms borrowed, explanations integrated, and intellectual unity achieved, after all. Thus, a consensus emerged. The issue of human culture poses a challenge to the division of the world into the realms of the ‘natural’ and the ‘cultural’, and hence to the disciplinary division of scientific labor. In our view, the appropriate place for the study of human culture is located between biology and the social sciences. (Weingart 1997, viii)

Cosmides and Tooby refer to integration in the sense that cultural anthropologists have to take care that what they claim is consistent with well-established knowledge from evolutionary theory, while considering their version of evolutionary psychology as providing the new ‘irreplaceable intellectual gifts’ everybody has to take into account. I take Weingart to be referring to something else, namely to the interdisciplinary endeavor to join forces in order to explore new fields, e.g. co-evolution, which is more a case of our second kind of synthesis, heuristic synthesis, the creation of something new.

Implicit or explicit in claims such as Barkow et al. seems to be an important assumption: that it is *because of integration* (and probably only in case of integration) that we reach *novelty* (i.e., new ideas, new methods, or new interfield theories representing whole new interdisciplinary fields). In other words, there might be an assumption involved that only integration ‘generates this powerful growth in knowledge’ as Barkow, Cosmides and Tooby put it. A review of a historical example from the history of theories of cultural evolution, representing a standard example in the development of disciplines, shall illustrate that such an assumption is ill guided.

Alfred L. Kroeber (1876-1960), the first ‘Boasian’, had a specific and explicit attitude towards separation and fruitfulness. He wrote the following in 1952, reviewing a productive career in ‘cultural anthropology’:

Any theory that specializes on culture must of course recognize that, in the case of man, society and culture always co-occur, so that the phenomena available necessarily have both a social and a cultural aspect. Since societies comprise individuals and especially since

individuals are heavily shaped by their culture, there is also a third aspect or factor immediately involved in the phenomena, that of psychology or personality – apart from more remote considerations, such as the biological nature of people and the subhuman environment in which they operate. It is of course possible to try to study the cultural, social, and psychological aspects simultaneously and interwoven, as they occur. Such a meshed understanding is obviously the broadest and is therefore desirable in principle. However, it is also much the most difficult to attain, because more variable factors are involved. Also it is plain that the most valid and fruitful synthesis, other things being equal, must be the one which is based on the most acute preceding analysis. Such analysis is going to be more effective if directed at an isolable set of factors than at several interacting ones. Premature and short-circuiting synthesizing is thus avoided by discrimination between the aspects or levels that come associated in phenomena, and by unravelling, out of the snarl with which actuality presents us, the factors of one level at a time and seeing how far they can be traced as such, before retying them into a web of larger understanding with the other strands. The level which I have personally chosen or become addicted to is the cultural one. This is not the only way of proceeding, but it is my way, and it seems the most consistent with an integrative-contextual or 'historical' approach. (Kroeber 1952, p. 7).

Kroeber followed this strategy from the very beginning of his career. He is well-known for his boundary building, defending what has been called a 'cultural determinism', the claim that only culture explains culture, which is demonstrated in the just quoted statement. From the very beginning, cultural determinism was not meant ontological, but epistemological and pragmatic: Kroeber claimed the right to focus, the right to ignore, for a while at least. At the same time, he claimed that others should equally focus since the phenomenon that cultural anthropologists study with their tools are different from the subject matter of biologists. Thus, he claimed authority for a neatly defined part of the phenomena under scrutiny in science, and this part he termed, interchangeably: culture, the superorganic, history, civilization. It is a phenomenon *sui generis*, with its own scientific experts, the cultural anthropologists.

As indicated in the first part of this paper, Kroeber used an analogy between biological and cultural change to establish this autonomy of cultural anthropology. Thus, he secured boundaries by dialectically crossing them. He referred to new developments in biology, mainly the Weismannian theory of heredity. Weismann denied that any inheritance of acquired characteristics is possible and claimed on this basis the all-sufficiency of selection. As Weismann did before him, Kroeber said that only if we replace Lamarckian inheritance with the concept of cultural inheritance, would we be able to see that cultural change is historically not correlated with biological change. One can change without the other and is autonomous in that sense. As long as there is a belief in Lamarckian inheritance, however, we will think of culture as reducible to nature. In the grip of Lamarckism, culture slowly but steadily becomes nature, habit becomes instinct, acquired becomes innate – all via the biological inheritance of acquired characteristics. In the Lamarckian picture, the two kinds of evolutions are correlated: if one changes, the other does too. Historically, belief in the inheritance of acquired characteristics was used to explain the evolution of mental abilities and to claim that cultural differences correlate with racial differences, for instance in Herbert Spencer's philosophy. On the basis of a Weismannian point of view, however, you cannot infer racial differences from cultural differences since the two are independent, decoupled from the very first moment when the first animal managed to learn socially, i.e. from the birth of culture via nature.

Even though people have and still defend scientific racism on all kinds of grounds, I regard Kroeber's claim of the 'autonomy of culture' as a historically important insight that helped to fight the scientific racism of the early 20th century, which was supported by the belief in the inheritance

of acquired characteristics. Thus, Kroeber developed an important and fruitful thesis by using an analogy between cultural and biological heredity.

The analogy was, however, not used for synthesis but for a hard divide: between culture and nature and between cultural anthropology on the one hand and physical anthropology and genetics on the other hand. Note that he did not want to say that physical anthropologists or geneticists don't have a word to say on humans. He only believed that it is fruitful, if each of these has a domain of its own. In the context of his time, I believe, he was right: it certainly was more fruitful at that time that each had a domain of his own.²¹

Since Kroeber used the concept of dual inheritance, he can be considered as a kind of 'precursor' of contemporary dual inheritance theories. Thus, the history of theories of cultural evolution shows that with respect to cultural inheritance there never was a historical hourglass of heredity: heredity was narrowed, but it was not hardened. The multiple inheritance view brought home so vividly now by multiple inheritance theories, was present all the time. Yet heredity was fragmented by the division of scientific labor and by and large stays so until today, even if the fragmented channels of heredity are looked at now from a more integrative and interactionist perspective. We approach a new synthesis, but it is one that presupposes the foregoing separation of the perspectives that shall be united – as separate ones.

In sharp contrast to contemporary dual inheritance theorists, Kroeber used the concept of cultural inheritance to demarcate the domain of cultural anthropology, which was still in the making at that time. He thus defended the place of cultural anthropology, against the social and political hegemony of racist hereditarianism and the scientific force of the new genetics. He crossed the field of anthropology towards biology and used Weismann's theory of heredity in order to establish clear boundaries between the two disciplines at a time when both were expanding their scientific and institutional setting. He was doing so in order to establish a clear specialization, a differentiation, i.e. a clear division of labor, between anthropology and biology, and between physical anthropology and cultural anthropology. When disciplines emerge, it is unlikely that their representatives are open-minded, for 'worldly', i.e. merely pragmatic, reasons: they have to establish themselves first and get a place in the midst of other disciplines. They have to appropriate phenomena. In other words, separationist initiatives have their institutional, social, or political background, as do unificationist ones (Galison and Stump 1996). But despite these social reasons both can lead to fruitful scientific results.

Conclusion and outlook

Depending on context, crossing borders can be used to divide disciplines or to unite them. In both cases, the results may contribute important new insights or even open up whole new continents for research, such as the discipline of cultural anthropology and the field of co-evolution. This is the main point I wanted to make in this paper. A bias towards integration (as an epistemic value) is thus unjustified, and it is so on the following grounds: If integration is valued because it helps us to progress, then separation has to be taken as equally valuable, if it helps us to progress. Whether integration (or separation) is fruitful certainly depends on the circumstances.

The argument for the fruitfulness of integration as well as separation rests on the distinction between four kinds of synthesis: integration, heuristic synthesis, expansion, and big-picture-synthesis. These represent four ways of how two domains can unite via the exchange of methods, concepts, theories, a hypothesis, or evidence.

²¹ See Kronfeldner (2009) for more details and references on his case.

Many issues have been left aside here. The most important ones should at least be named before closing: ambiguity might play an important fruitful role in the trading between disciplines, the epistemic values in the use of analogies might conflict in further ways and it is unclear whether there is a clear hierarchy between them. Finally, many historical details regarding the social and cultural background of the kinds of synthesis and of separation are missing.

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PART III

THE PUBLIC PERCEPTION OF HEREDITY

BETWEEN GENEALOGY, DEGENERATION AND REPRODUCTION: THE FIGURE OF THE BACHELOR IN SCIENCE AND LITERATURE

Ulrike Vedder

1. Families and Bachelors. Introductory Remarks

For nineteenth-century bourgeois society, the state, the people, and selfhood rested essentially upon the family. The family was referred to as the “seed of the state,” as “the precondition for the existence of the [...] society” or even as “the basis of all [...] human and civic happiness.”¹ The family, moreover, constituted the intersection among all those nineteenth-century sciences, either already established or newly emerging, that were busily reconfiguring our knowledge of the human being: education, philosophy, anthropology, medicine, and law. Historically speaking, two new, yet opposite tendencies appeared in the nineteenth century: the family was rendered more natural on the one hand, and became increasingly subject to the law and science on the other. The simultaneous naturalisation, juridification, and scientification of the family resulted in tensions and conflicts without which the constitution of the bourgeois family would have been inconceivable. Thus, the contractual interpretation of marriage and the family, as set out in the civil codes around 1800, runs essentially counter to the bourgeois family as the site of a new anthropology of gender. This develops the ‘nature’ of man and woman into a model of ‘natural love’ between spouses and between parents and children.

The constitution of the bourgeois family was therefore always ridden with potential conflict to that particular point of disintegration that nineteenth-century literature loves to belabour. Roddey Reid, an interdisciplinary cultural studies scholar, even claims that “the so-called modern domestic family has largely been constructed through narratives of absence and figures of pathological deficiency.” (Reid 1993, p. 8) While such ‘deficiencies’ or ‘disorders’ establish normative notions of the family and generativity, they also serve to negotiate alternative images and discourses of the family.

Nineteenth-century debates on the family, genealogy, and heredity considered the bachelor both a failure and a key, thus a controversial figure. In what follows, social, political, scientific, and literary discourses focused on the *male* bachelor, as Michelle Perrot has shown in her discussion of the nineteenth century: “The term ‘bachelor’ always denotes a male. An unmarried woman is a ‘girl’ or ‘an old/extant woman,’ that is, a nobody.”² As an explicit counterfigure to the family, bourgeois contempt hits out at the male bachelor. In his *Dictionnaire des idées reçues*, his “commonplace dictionary”, Gustave Flaubert collected the bourgeois idiom, the bourgeois phrases, and arranged his entries in alphabetical order: a highly ironic undertaking. The *Dictionnaire* contains an entry on the bachelor. It reads thus: “BACHELORS – All bachelors are egotistical and licentious. They should be subject to taxation. What miserable old age they create for themselves!” (“CÉLIBATAIRES – Tous egoïstes et débauchés. On devrait les imposer. Se préparent une triste vieillesse.”) Since bachelorhood is moralised and considered a misfortune arising from individual fault, bachelors should thus be held to account for their lifestyle in monetary terms, that is, through

¹ Cf.: “Keim des Staates” (Savigny 1840, p. 343-44); “Voraussetzung der [...] Gesellschaft” (Riehl 1855, p. 93); “Grundlage [...] alles menschlichen und bürgerlichen Glücks” (Rotteck 1837, p. 386).

² Cf. “le substantif [célibataire] est toujours employé au masculin [...]. Non mariée, la femme est fille ou ‘reste fille’: c’est-à-dire rien.” (Perrot 1987, p. 291-293).

taxation. (I will come back to this in the following part of my paper, in part 2 on “marginalisation”).

In the latter half of the nineteenth century, the bachelor came to epitomise decadence, degeneration, and decay. Moralising and pathologising now converged. The bachelor’s refusal to start a family and procreate negates prevailing bourgeois values and social requirements, thus rendering problematic so-called ‘normalcy’ (I will elaborate on this in part 3 on “normalization”). The bachelor’s ‘infertility’, moreover, evokes the end of the human ‘race’ and calls ‘Nature’ into question. Such notions coincide with the observation that the figure of the bachelor is used to negotiate what Jean Borie has termed the “mythologies of heredity” existing at the time (see part 4 on “Heredity and Degeneration”). The bachelor is thus more than a social type,³ but at once a psychological character and an imaginary figure, an unconventional, egotistical, or neurotic actor and a literary topos. He unites opposed imaginations, making him an extraordinarily productive figure for both the sciences and literature.

2. Marginalisation

The egotistical bachelor, who fails to meet his obligations towards the community (whether this be the family, society, the people, or humanity), is a familiar and well-established topos. There have been recurrent attempts to take hold of the bachelor through property, inheritance, or tax laws. In the German states, in particular in the seventeenth and eighteenth centuries, for instance, the so-called *Hagestolzenrecht* – literally, the law of confirmed bachelorhood – was enacted to ensure that the estates of (non-aristocratic) males who remained unmarried after a certain age would automatically fall to the lord of the manor (that is, the sovereign prince or alderman), or high taxes should be levied on the estates of unmarried males. Both moral and demographic arguments were advanced to account for this practice: Decreasing marriage rates were linked to increasing luxury; widows and orphans should be supported by the taxes levied on bachelorhood. Such arguments reappeared in the latter half of the nineteenth century.

Even where the taxation of bachelors is refuted on the grounds that taxes could not be levied on what did not exist – as Wilhelm Heinrich Riehl, Professor of Cultural History and Statistics, argued in the mid-nineteenth century –, bachelors were considered to live on the edge of society. Such marginalisation is even conceived in literally spatial terms, f.e. by Wilhelm Heinrich Riehl. Published in 1855, Riehl’s study *Die Familie* marks the third volume of his *Natural history of the German people serving as the basis for German social and welfare policy (Naturgeschichte des deutschen Volkes als Grundlage einer deutschen Social-Politik, 1851-1869)*. Aspiring to the political and social revalorisation of the family (which he considers the subsequent requirement for the survival of the states, society, and the personality of the people), Riehl has recourse to the bachelor, specifically his position in the spatial order of the family to criticise the radical shifts from the “German family” (the “old house”) to the French-inspired “modern” family (the “modern house”). Riehl allocates the alcove of the old house to the bachelor – the alcove or oriel is a “corner” or recessed section of the “family room” to which an individual could retire without, however, closing or indeed cutting himself off from the family’s communal living area.⁴ For Riehl, such an “old”

³ He is obviously a social type, too, as a cursory glance at relevant demographic statistics suggests: in the 1890s, for instance, the number of male bachelors in France over the age of 30 rose to approximately 11% of the population.

⁴ Cf.: “Für den Einzelnen ist das moderne Haus wohnlicher, geräumiger geworden, für die Familie enger und ärmer, wie überhaupt die meisten Verbesserungen unserer Lebensweise vorwiegend den Junggesellen und Hagestolzen zu gut kommen. Das architektonische Symbol für die Stellung des Einzelnen zur Familie war im alten Hause der Erker. Im Erker, der eigentlich zum Familienzimmer, zur Wohnhalle

house is a “memorial” to the family, conceived “as an historically growing and continually blooming chain of the generations that the non-distinct rooms of the modern house, incapable of reproduction, and their forever changing tenants and landlords can never become.”⁵ Since the modern house “like most improvements of our way of life principally benefit bachelors” (Riehl 1855, p. 176), bachelorhood once again epitomises the incapacity of reproduction and the principle of negativity, even though Riehl appears to be discussing architectural issues. The cloistered, self-contained bachelor apartment is not only interpreted as a sign of modern sterility, as seen above, but also operates as a popular projection screen for bourgeois fantasies. In the entry entitled “APPARTEMENT de garçon,” Flaubert’s *Dictionnaire des idées reçues* summarises these fantasies thus: “On doit y trouver des choses extraordinaires.” (“One is certain to discover extraordinary things there.”)

Marginalisation, sterility, negativity: Honoré de Balzac’s *Les Célibataires*⁶ – a section of *La Comédie humaine*, his multi-volume collection of interlinked novels –, written in the 1830s and 1840s and dedicated to bachelorhood, explores the incapacity of reproduction and sterility, starting out from (what the preface to the novel *Pierrette* calls) “the author’s profound hatred of all unproductive creatures, bachelors, and spinsters” (“la haine profonde de l’auteur contre tout être improductif, contre les célibataires, les vieilles filles et les vieux garçons”).⁷ On the other hand, Balzac also emphasises the contrary principle, namely the bachelor’s fertility and reproductiveness: the same preface refers to the bachelor as the author’s cherished gold mine from which great treasures can be borne forth.

In Balzac, but also in other literary texts, f.e. by Adalbert Stifter, the great Austrian writer, also in the middle of the 19th century, bachelors have a dual function: On the one hand, bachelors constitute an alternative model to the conjugal bourgeois family, characterised either by productive freedom or infertile loneliness. On the other, bachelors usually remain integrated in family and generational succession, notably in a decisive position as uncles or aunts, or rather as rich uncles and aunts – that is, as lateral relatives presenting an alternative to the paternal family. They appear to offer a way out of the disastrous entrapments of patrilinear genealogy and inheritance running through the novels.

3. Normalisation (via Deviation)

Many literary texts furnish bachelors both with deficiency, loss, and mourning as well as freedom and creativity. Other nineteenth-century discourses, concerned with genealogy, generativity, and the family, including medicine and psychology, vehemently oppose such literary ambivalences. Bachelors serve such discourses as an ideal test subject for normalisation, insofar as bachelors are considered to stray from the norm.

Such deviation is stated in relation to a normalcy for which the bourgeois family must answer. Discourses on bachelors are characterised – at times more, at times less visibly – by constant

gehört, findet der Einzelne wohl seinen Arbeits-, Spiel- und Schmollwinkel, er kann sich dorthin zurückziehen: aber er kann sich nicht abschließen, denn der Erker ist gegen das Zimmer offen. So soll auch der Einzelne zur Familie stehen.” (Riehl 1855, p. 176-77).

⁵ Cf.: “eine historisch wachsende und fortblühende Kette von Geschlechtern, wie es das moderne Haus mit seinen unterschiedslosen, fortbildungsunfähigen Räumen und seinen wechselnden Miethern und Besitzern niemals werden kann.” (Riehl 1855, p. 180-81).

⁶ Balzac’s *Les Célibataires* cycle comprises the novels *Pierrette* (1840), *Le Curé de Tours* (1832), and *La Rabouilleuse* (1842). Together, these form part of the second book *Scènes de la vie de province*, which in turn belongs to the main part of *La Comédie Humaine, Études de mœurs*.

⁷ Balzac 1976, 21.

reference to the family. Paradoxically, the nineteenth century witnessed increasing family classification, although it gave rise to the individual in the modern sense of the term – since the declaration of human rights and its recognition of the individual regardless of origin, status, property, *and family*. So although the bachelor can be considered to epitomise individuality, the emergence of the bourgeois individual goes hand in hand with a fundamental mistrust of the bachelor – whether in the guise of a parasite or family enemy, the laughable or monstrous, the libertine or failure, or the agent responsible for demographic and cultural crises, dwindling birth rates, and decadence. The bachelor becomes the figure threatening the bourgeois family and, even more fundamentally, nineteenth-century bourgeois society, just as he is its product.⁸

Over time, the bachelor came under increasing social, discursive, and scientific scrutiny, only to become operationalised for norm setting, that is, the enforcement of inclusion and exclusion. The debates on degeneration in the latter half of the nineteenth century make this shift particularly apparent. For instance, while the *Dictionnaire encyclopédique des sciences médicales* (1872) contains no entry on “Célibat/Célibataire,” there is an entry on “Mariage,” written by Louis-Adolphe Bertillon, the French physician and statistician.⁹ Bertillon’s entry on “marriage” also comprises a comparison between bachelors and married men. In his summary, Bertillon comments on the data gathered on mortality, crime, morals, physical and mental health as follows:

If demography revealed that at least one third of French territory were inhabited by such a miserable population that the mortality rate is one and a half to three times as high as the rest of the territory; [...] that the annual incidence of madness in this section of the French population is twice as high as the rest, with twice as many suicides, twice as many property violations, twice as many murders [...], one would certainly demand that science, the law, instruction, education, the fiscal system, the sovereign’s favour, and mores committed themselves to diminishing the humiliating and costly ‘surcharge’ on mortality and shamefulness. [...] However, these two populations do not inhabit separate territories, but are intermingled across the entire area; and, ostensibly, they are distinct in one sole respect: one lives under the regime of marriage, the other under that of bachelorhood.¹⁰

Bertillon holds bachelorhood responsible for the statistically proven decline of the French population, thus blaming precisely that section that can be neither sealed off from the rest of the territory nor abandoned to its fate or combatted by social institutions; instead, it is situated firmly

⁸ No matter whether it be family ties resulting from marriage, a bond with God forged through religion, or one with the libido through ‘normal’ sexuality – the figure of the bachelor calls into question all these orders. See Borie 1976.

⁹ Louis-Adolphe Bertillon is also the founder of the still-prevalent notion of population, understood as a system of interacting variables such as natality, mortality, migration, and so forth. He should not to be confused neither with his son Alphonse, the famous anthropometrist and founder of so-called ‘bertillonage’, a biometric system used to identify criminals, nor with his second son Jacques, the statistician and demographer.

¹⁰ Bertillon 1872. See Borie 1976, p. 84-86. (Cf.: “Si la démographie révélait que le tiers au moins du territoire français est occupé par une population tellement misérable que chaque âge est frappé par une mortalité une fois et demie à deux fois plus forte que le reste du territoire; [...] que cette partie de la population française [...] compte annuellement deux fois plus de cas d’aliénation, deux fois plus de suicides, deux fois plus d’attentats contre les propriétés, deux fois plus de meurtres [...], on demanderait à la science, à la loi, à l’instruction, à l’éducation, à l’impôt, à la faveur du souverain, aux mœurs, de s’employer pour diminuer un si humiliant et si onéreux supplément de mortalité et d’ignominie. [...] seulement, au lieu d’occuper un territoire à part, les deux peuples sont mêlés intimement sur toute la surface; et, ostensiblement, une seule chose les distingue: l’un vit sous le régime du mariage, l’autre sous celui du célibat.”).

‘within’ society, the family, the nation, and – last but not least, at the heart of masculinity. Paradoxically, the bachelor’s strong will to individualism is a weakness, a failure, indeed a token of feminisation. Such pathologised bachelorhood, moreover, is contagious. In 1871, Auguste Ambroise Tardieu reiterated the well-known demand for a special tax to be levied on bachelors, advancing new arguments, including that they are the agents of “corruption”: “the bachelor strives to pervert and corrupt those around him; he is the enduring cause of social disorder, unhappiness, and depravity. To the extent that the family consolidates the social edifice, the bachelor acts as its destroyer.”¹¹ These quotations (which could be easily extended) reveal that bachelorhood no longer resulted from social and family traditions, such as from primogeniture (the exclusive right of inheritance belonging to the first born, which made it financially difficult for non-heirs to start families and thus makes them bachelors) or from opting for celibacy on religious grounds. Bachelorhood had instead become a lifestyle that both ruptures social and family traditions and indeed threatens the family and society.

4. Bachelors in the Discourse on Heredity and Degeneration

Bachelorhood, however, is not only contagious; instead, it also ‘saves’ individuals from contagion, that is, from inheriting degenerative phenomena – as claimed by the discourse on degeneration at the time –, insofar as this spells the end of family genealogy. In his study *Ueber nervöse Familien* (1884), the neurologist Paul Julius Möbius demanded that individuals with “serious forms of nervous degeneration” remain bachelors. Möbius’ study blends descriptive, classificatory, and diagnostic observations on the health of individual family members with marital and social hygiene recommendations. While he sets out to “recount the history of some neuropathic families” (Möbius 1884, p. 228), Möbius’ detailed descriptions of four families and their *Stigmata hereditaria* (Möbius 1884, p. 241) all furnished with family trees, and his analysis of their development through as many as five generations, arrives at “practical conclusions”: “Any person who has ever suffered from any kind of serious nervous degeneration should not marry at all. The question whether marital life would agree with such persons disappears from view given the concern that their malady could infect a whole generation.”¹² Here, two entirely different conceptions of transmission converge: inheritance, heredity, and contagion. Such inconsistency in the claims to causality about degeneration also occurs in the conclusions reached about the relationship between individual and family – and, by implication, bachelors and their families: while individuals are classified as a quasi-fateful product of hereditary circumstances, their heirs’ future is seen to depend on them.

Michel Foucault describes this anxiety about the future as the principal anxiety afflicting the nineteenth-century bourgeois family. He observes that whereas the aristocracy had protected its identity through “the antiquity of its ancestry” (Foucault 1998, p. 124), hence through blood relations, the bourgeoisie had recourse to descendance, thus arguing for a sexuality that produces future generations. Since sexuality at the same time threatens progeny, it marks “the source of an entire capital for the species” (Foucault 1998, p. 118). The nineteenth-century bourgeoisie, Foucault notes, employs a discourse of dissolution and degeneration to practise its identity and consolidation

¹¹ Démophile (= Auguste Ambroise Tardieu): *Proposition d’un impôt sur le célibat*, August 1871, quoted in Borie 1976, p. 90: “il [le célibataire] tend toujours à pervertir et à corrompre autour de lui; il est, dans la société, une cause incessante de désordres, de malheurs et de dépravation. Autant la famille consolide l’édifice social, autant le célibat est un agent actif de destruction.”

¹² Cf.: “Jede Person, bei welcher irgend schwerere Formen der nervösen Degeneration aufgetreten sind, sollte überhaupt nicht heirathen. Ob ihr das eheliche Leben zuträglich ist, diese Frage verschwindet neben dem Bedenken, dass ihr Uebel eine ganze Generation anstecken möchte.” (Möbius 1884, p. 242-43).

policies: “many of the themes characteristic of the caste manners of the nobility reappeared in the nineteenth-century bourgeoisie, but in the guise of biological, medical, or eugenic precepts. The concern with genealogy became a preoccupation with heredity.” (Foucault 1998, p. 124)

The late nineteenth-century bachelor considered himself ‘trapped’: embodying infertility and hence the end of the future, he is also imagined to stand at the end of a long generational chain. Seen thus, as strikingly evident in late nineteenth-century literature,¹³ the bachelor not only rejects his family’s future but also its past, by allowing the generational chain to break and a century-old history of lineage to cease. Such rejection challenges the living and the dead. Here, literature brings into play shifted notions of heredity. Oscar Wilde’s *The Picture of Dorian Gray* (1891), for instance, regards Dorian Gray first as a bachelor and as the “last Lord Kelso’s (only) grandson,” hence the last of his line, before considering him “tainted with the monstrous maladies of dead” and inquiring: “Had some strange poisonous germ crept from body to body till it had reached his own?” (Wilde 1988, p. 31, p. 111) The dead inhabit him: against his will, against his self-love, and against his forced individuation; they are present within him – and thus represent the pre-modern conception of the power and presence of the dead.¹⁴ At the same time, the notion of bodily transmitted “germs” alludes to contemporaneous biological concepts of heredity and thus to their discursive power in modernity.

This power – which Jean Borie describes as the nineteenth-century bourgeois “mythologies of heredity” – is directed against the bachelor’s striving for individuality and freedom, in order to identify him as a social *and* biological anomaly: “our progenitors would pursue us irrevocably to remind us of the ridiculous nature of our solitary ambitions.”¹⁵ The family curse that had fatefully linked the generations in early nineteenth-century literature¹⁶ now appears to have entered the discourse on heredity, according to which the solitary bachelor no longer exists. But if bachelorhood – through the amalgamation of social and hereditary discourses on degeneration – becomes an anomaly of human development, the bachelor excludes himself from what is perceived to be the universal law of Nature: “Challenging the norm no longer resides simply in rejecting the modes of existence and mores of a class but in placing oneself in the margins of a law that appears – in fine – to be universal and natural.”¹⁷ The ‘infertility’ of the bachelor thus also constitutes an attack on the power of inheritance, procreation, naturalism, and ‘life’, as established by the end of the nineteenth century.¹⁸

To conclude: The bachelor is thus characterised by his far-reaching imagination. His career in nineteenth-century social, political, scientific, and literary discourses rests particularly upon his ‘failure’ function. It is precisely this ‘detour’ – that is, via the bachelor as a system failure or breakdown, as a castaway, as a counterfigure – that can advance scholarly inquiry into nineteenth-century scientific and cultural debates on the signification of family, genealogy, and heredity.

¹³ See, for instance, Snyder 1999; Prince 2002.

¹⁴ See Oexle 1983; Vedder 2007b.

¹⁵ Borie 1981, p. 181: “la presse incongédiable de nos géniteurs nous [...] suivrait pour [...] nous rappeler le ridicule de nos ambitions solitaires.”

¹⁶ See Vedder 2007a.

¹⁷ Borie 1991, p. 112 : “Défier la norme, cela ne consiste plus simplement à rejeter les façons d’être et les mœurs d’une classe, mais à se placer en marge d’une Loi qui se donne comme universelle et, pour tout dire, *naturelle*.”

¹⁸ For a discussion of literary treatments of the bachelor in the French *décadence*, see “La haine du naturalisme s’accompagne d’une haine de l’hérédité, de la procréation et de tout ce qui a un parfum de vie.” (Bertrand/Biron/Dubois/Paque 1996, p. 42) (Cf.: “The hatred of naturalism coincides with a hatred of heredity, procreation, and anything that bears the scent of life.”).

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MUTANT, HERO OR MONSTER? GENETICS IN CINEMA

Sophia Vackimes

How does the hourglass metaphor explain the misunderstanding of scientific principles at a time when there is so much information being constantly fed to the public by the media? At a time when information is so quickly transmitted by many forms of media, it would probably be desirable that the general public would be well informed about the work of science, its principles and its problems. However, information gathered from various surveys has shown that the general public is not properly informed about the work of science, but rather is informed about science from media such as science fiction.

Studies conducted in English speaking countries such as Australia, England and the United States, have shown that there is a generally negative perception of the work of science (Wellcome 1998) that is constructed from the media. Making use of narratives taken from popular culture (which is a misnomer for commercial products sold in mass to society) in order to give their concerns an expressive framework laymen told the Wellcome study researchers that they believed scientific work is threatening, illicit, and greatly irresponsible in its practices.

It is of course of great concern to many – and especially scientists – that even though we are living in an era of sophisticated scientific advances the public grows further and further away from understanding its premises while increasingly condemning its practices. Regarding cloning, for example, discussions conducted with the focus group in the Wellcome study were “peppered throughout” with “negative references” to films and books “including *The Boys from Brazil*, *Jurassic Park*, *Blade Runner*, *Invasion of the Bodysnatchers*, *Frankenstein*, *Brave New World*, *Stepford Wives*, *Star Trek* and *Alien Resurrection*” (Wellcome 1998, p. 13).

Material coming from literary, popular sources, or movies can be instructive or even critical about the work of science, however, it generally reinforces false notions by constructing stories that feed public mistrust and underscore paranoid visions of the world. While at times films also serve as legitimate warnings against scientific, corporate or governmental abuses, thinking that carefully considers the quandaries and history of science is generally lacking.

Many writers have focused on the work of science as fundamental part of their plots whether represented by Dr. Faust and his alchemical practices or Dr. Moreau deconstructing evolution on his tropical island. Today, DNA has become the focal point in many films but even though there is an overwhelming amount of scientific information cinematic content is increasingly shrouded in what can be described as what Nelkin and Lindee called the “DNA mystique” (Lindee and Nelkin 1995). This is, films rely on the reduction of biological information in favor of oversimplified content while notions that are constructed with increased technological sophistication and little else pass for what the public understands about scientific knowledge, in this way the hourglass image is a mirror effect. On the top is an overwhelming amount of scientific advancement, at the vortex a loss of connection between what is real and what is not, while slowly on the bottom grows, grain of sand at a time, a misunderstanding of what science is all about.

It is interesting to see how the language – visual or narrative – given to genetic material has been represented over time; initially with difficulty, as new scientific information was unintelligible to scriptwriters and directors and subsequently to the general public, and with later purportedly with increased ease as the material become better understood. However, from the appearance of

the first models of the DNA molecule until today a gross reduction of information has occurred in its representation. A simple flash on the screen of what appears to be its intertwined structure suffices for what once was an elaborate effort at offering an explanation of the findings of modern science, and therefore the information presented has become absurdly banal.

Many films imply the use of scientific subjects by using titles such as *The Clones of Bruce Lee* (1977), a virtually unwatchable film in terms of science. The inclusion of the words clone, gene, even IVF (in-vitro-fertilization) in a title or dialog, doesn't truly mean the material engages in science or the ethics of its various technologies, thus failing miserably at scientific representation or at steering the public in one way or the other. Steven Spielberg's *ET; The Extra-Terrestrial* (1982) contains a scene where one of the film's climaxes occurs as it is determined that the creature has DNA, but such opportunistic mention functions merely to further elicit the empathy for the ailing being but is of no further consequence to the story. But at times films might either give a scientific "mini lesson" – aiding and coherence to an anecdote, a storyline, or engage clearly with ethical or moral quandaries regarding the work of science, though many tend to exploit the dark side of science unapologetically.

Perhaps the most thorough treatment of cloning in film is one of the earliest films dealing with the topic; *The Boys From Brazil* (1977), based on the novel by Ira Levine, and directed by Franklin J. Schaffner. This work is directly linked in its timing to the John Gurdon and Verena Uehlinger announcement of the cloning of frogs, and despite its far-fetched plot – a project that entails producing ninety-four clones of Adolf Hitler – the film sternly considers the ethics of creating human life in a laboratory thus setting the discussion in terms of genetics and eugenics. Besides explaining real laboratory difficulties in developing such a project, the scientist on the screen also discusses the cultural environment necessary for the upbringing of the cloned subjects in order to create a circumstance that might result in the upbringing of a cloned individual.

The main character in the story is a Jew, Ezra Lieberman, a Nazi hunter who is trying to figure out a puzzle and while doing so he has found several children that look exactly alike. Notwithstanding, Lieberman makes an all out effort – runs around several countries, conducting interviews with recently widowed women who he finds have strangely similar offspring, trying to figure out why or how it is possible that children from different families can be strikingly alike, or what they would have anything to do with Mengele; one of the clues. It is then that he seeks help from a scientist who explains how it is possible for several human beings share so many common characteristics; Lieberman learns about the possibilities of cloning. The fellow scientist that speaks with him is clear; cloning is no longer something in the realm of science fiction; it is a process being continuously perfected; something that can eventually be done with a well preserved skin specimen from someone who does not necessarily need to be alive. Suddenly, Lieberman understands what he is up against.

The scene is a remarkable example of the state of genetic science of the seventies and the concessions made in fulfilling the suspension of disbelief that is necessary to sustain the plot of a fiction film. In a use of film within a film, scientific demonstrations and explanations are craftily put together in a narrative advanced by a fictional Doctor Bruchner who explains how cloning is done. This scene is effective thanks to the work of Derek Bromhall who is given scientific advisory credit at the beginning of the film, and who was at one point a student of Gurdon's. Bromhall, who was the plaintiff in the famous "boy clone hoax" of the early 1980's¹, crafts an explanation that is

¹ Derek Bromhall filed a \$7 million defamation suit against author David. M. Rorvik and his publisher for having cited him and his work in the book titled *In His Image: The Cloning of a Man*.

scientifically consistent and whose only mistake can nevertheless be explained as a culturally consistent explanation of how character traits are transmitted from one generation to another.²

Dr. Bruckner

This Mengele was sort of a primitive geneticist in his own way, wasn't he? I understand that he experimented on human beings . . .

Herr Lieberman

Twins . . .

Dr. Bruckner

Then he was nothing more than a sadist, really . . .

Herr Lieberman

A sadist with an MD and a Ph.D.

Dr. Bruckner

Well some people would say that's a perfect definition of a scientist . . . What exactly do you mean when you say the boys you saw were more than twins?

Herr Lieberman

Ah, not only did they look alike, but they were also very alike in personality.

Dr. Bruckner

That is unusual; studies show that twins who are separated at birth develop totally different personalities.

Herr Lieberman

But these twins, or should I say triplets, because I believe that my associate saw another, were like the same people but brought up with different languages . . .

Dr. Bruckner

It's impossible of course.

Herr Lieberman

Excuse me doctor, but what is impossible? . . . What is impossible doctor?

Dr. Bruckner

Mononuclear reproduction . . .

Herr Lieberman

Ah, doctor . . .

Dr. Bruckner

Cloning . . . what if I were to tell you that I could take a scrapping of skin from your finger and create another Ezra Lieberman?

² I am indebted to Christina Brandt and to Edna Maria Suarez Diaz for a wonderfully insightful conversation on this scene and cloning in the seventies. The discussion about the mistake consists on considering whether or not the blood cells implanted into an egg that has previously had its own nucleus destroyed is a red or white cell, and whether or not that choice was viable.

Herr Lieberman

I would tell you not to waste your time, nor my finger!

Dr. Bruckner

Anyway, that is cloning. It was first done with plants. A cutting taken from a plant and transplanted grew to be the exact duplicate of the donor plant, now we are doing the same thing with laboratory animals.

Herr Lieberman

You mean you can produce an animal from itself?

Dr. Bruckner

We take the unfertilized egg of an ovulating female and destroy all of its genes and chromosomes we then implant the nucleus from the donor cell which could be taken from a blood sample or even a skin scraping. That cell, with its genetic material intact eventually becomes an embryo and is born as a living creature.

Herr Lieberman

Without parents . . .

Dr. Bruckner

Well, it has no father, because the egg was never fertilized, no mother because its genetic code comes from another being. Can you follow that?

Herr Lieberman

And this creature is an exact duplicate of itself, oh, doctor! How can that be?

Dr. Bruckner

Come along . . . Our experiments began with the simplest of animals shrimps and frogs, animals in which the female eggs are fertilized externally . . . then we moved on to mammals we tried several laboratory animals and found the rabbit most convenient . . . I had to develop instruments which could accomplish the operation, and a whole microinjection system. I will show you how it is done. Here we are removing the eggs from a white rabbit's fallopian tubes. Now you see the egg under a microscope. I've brought the point of a sewing needle to give an idea of size.

Herr Lieberman

They are that small?

Dr. Bruckner

Most mammal eggs are about that size.

Herr Lieberman

Including human eggs?

Dr. Bruckner

Yes. The next step is to destroy the egg nucleus with ultraviolet light, so that none of its genetic make-up remains. Now you see an egg from a white rabbit, ready to be injected with the blood cell from a black rabbit donor. With the injection pipette the blood cells are sucked up and then injected into the egg. After a few hours the eggs in culture divide and are ready to be put back into the female. There they grow into embryos which in a month's time – the normal gestation period – will become baby rabbits.

In this instance a black litter from a white mother, and their black color proves that they have been cloned from the blood cell of a black rabbit.

Herr Lieberman

But isn't it difficult to get the egg back into the female?

Dr. Bruckner

Transferring the eggs isn't a problem, we do it all the time with the laboratory animals. The real tricky part is the microsurgery . . . getting the donor cell into the egg. You are lucky if one in ten survives.

Herr Lieberman

And this can be done with humans?

Dr. Bruckner

If the surgical technique were precise enough.

Herr Lieberman

It's monstrous doctor.

Dr. Bruckner

Why? Wouldn't you want to live in a world full of Mozarts and Picassos? Of course its only a dream, not only would you have to reproduce the genetic code of the donor, but the environmental background as well. Is Mengele trying to reproduce himself?

Herr Lieberman

No, he has brown eyes, and comes from a very wealthy family.

Dr. Bruckner

Let's examine the family background of the donor . . . the father is sixty-five years old, a civil servant, the mother is forty two you said, she dotes on the child . . . spoils, the boy is pale, dark hair, blue eyes, spoilt. Right? Now, Mengele would certainly know every social and environmental detail would have to be reproduced . . . thus if the parents were divorced when the boy was ten this would have to be arranged.

Herr Lieberman

Dr. Bruckner, the one who is cloned, the donor, he has have to be alive, doesn't he?

Dr. Bruckner

Not necessarily, individual cells taken from a donor can be preserved indefinitely, with a sample of Mozart's blood, someone with the skill and equipment could breed a few hundred baby Mozarts . . . My god, if it's really being done, What I'd give to see one of those boys! Herr Liebermann, Herr Liebermann!

Herr Lieberman

Not Mozart doctor, Not Picasso, not a genius who would enrich the world, but a lonely little boy with a domineering father, a customs officer who was fifty-two when he was born, and an affectionate doting mother, who was twenty-nine, the father died at sixty-five, when the boy was nearly fourteen, Adolf Hitler . . .

In the film *Cloned* (Barr 1997), however, made twenty years later such an explanation is completely forfeited. The experiments are explained as “miracles that could become nightmares”, and the only

information on the technology concerns the short phrase “...and you grew this from the cells of a single embryo...” and a very quick image of a pipette piercing a cell’s membrane.

DNA and the Nature/Nurture Debate

Other films have dealt with issues that concern the inheritance of genetic traits, but they do not address DNA in the science fiction sense – with sophisticated images or fancy explanations – but as fantastic, even bizarre – historical events. For example, *The Elephant Man* (Lynch 1980), portrays the so called “discovery” of Joseph Carey Merrick in a freak show in London and how he was taken into care of physician Frederick Treves. The nature/culture controversy is evident throughout the film while ultimately emphasizing the benefits of inculcating Victorian values. At the end the incurable subject drinks tea with nobility, goes to the theatre and is received as a victor, while he adorns himself in lavish style; a being that suffered a metamorphosis much in the style of Eliza Doolittle of Shaw’s *Pygmalion*. Merrick, a severely deformed man, was victim of what is known as Proteus syndrome exhibited a monstrous enlargement of the head, and perhaps also neurofibromatosis type I, deformities of genetic origin that are still contested items – pertaining to their description or naming – in journals such as *Science*. Merrick’s plight is situated more as a good versus evil melodrama in which Treves rescues Merrick from his keeper and other beings who seek to exploit him, and although the medical examination scene in the film is quite interesting the medical information given is not taken up at any later point, for Merrick is deemed incurable, and it is Victorian manners that take an important role in the “rescue” of the “Elephant Man”. By the end of the film the filthy, grotesque, severely deformed being receives royalty in his quarters and drinks tea with members of the British elite. What follows here is the script of the scene where his medical condition is described.

The black and white scene occurs in a university amphitheater in London. Dr. Fredrick Treves speaks from the front of the auditorium and Merrick remains hidden behind a curtain from whence we see his body as a tortured shadow. The physician is presented amongst objects found in many film scenes that surround physicians; glass jars with specimens, blackboards with either mathematical formulas, or medical symbols scribbled across their faces, microscopes, laboratory equipment, etc.

BRIGHT LIGHT

As we pull back and down in a slow spiral we see the light is coming through high windows. We now see several rows of distinguished doctors talking to each other in anticipation. As we continue to spiral down we see Treves before them at a podium. Behind him are two assistants standing beside a curtained stall. Treves raps a pointer stick on the podium to bring the meeting to order. We move behind the stall as the assistants part the curtains and we see the silhouette of the “Elephant Man”. The doctors talk among themselves quietly.

Dr. Treves

*He is English, he is twenty-one years of age
and his name is John Merrick. Gentlemen, in the
course of my profession I have come upon
lamentable deformities of the face
due to injury or disease, as well as
mutilations and contortions of the
body, depending upon like causes; but,*

at no time have I met with such a degraded or perverted version of a human being as this man. I wish to draw your attention to the insidious conditions affecting this patient.

Note, if you will, the extreme enlargement of the skull ... and upper limb, which is totally useless. The alarming curvature of the spine ... Turn him, please ...

Treves (Voice Over)

... the looseness of the skin, and the varying fibrous tumors that cover 90% of the body.

Treves' voice fades as we DISSOLVE TO the Doctors, who at first were rigid and flustered, and now bent forward, concentrating, obviously consumed with interest.

Spiraling down again we see Treves finishing his lecture.

Treves

... And there is every indication that these afflictions have been in existence, and have progressed rapidly, since birth. The Patient also suffers from chronic bronchitis As an interesting side-note, in spite of the afore-mentioned anomalies, the patient's genitals remain entirely intact and unaffected.

Treves nods to the assistants and they go to the "Elephant Man". We see them in shadow untying the loose knot of the loin-cloth.

CLOSE-UP of the shadow of the head of the Elephant Man. It goes up for a breath.

Treves

So then, gentlemen, owing to this series of deformities: The congenital exostosis of the skull; extensive papillomatous growths and large pendulous masses in connection

with the skin; the great enlargement of the right upper limb, involving all the bones; the massive distortion of the head and the extensive areas covered by papillomatous growth, the patient has been called, "The Elephant Man" (Lynch 1980).

As the dates of production for some of these films coincide with the announcement of scientific discoveries and/or consequent social reactions to them, they tend to hype, exaggerate, or mock questions about scientific work, or even scientists themselves to their advantage. We can see Francis Crick (one of the co-discoverers of the structure of DNA in 1953) appear as a character in the comedy *Teknolust* (Hershman-Leeson 2005), while Dolly the ewe cloned by scientists at the *Roslyn Institute* in Scotland in 1995 is referred to quite obviously in the horror movie *Black Sheep* (King 2007) a film where the scientific events are treated as an affront to nature and which triggers degenerate animal behavior in such manner that thousands of sheep become bloody cannibals.

The Good, the Bad, and the Cloned

Science fiction films, as most melodramatic films do, rely on stereotypes that help set up binary oppositions: constructing a fight between good and evil, the known and the unknown, the human and the inhuman, or even creating extraordinary relationships between man and animals even insects or extraterrestrial beings, the relationship animal/human is more common than not, and in fact more racial than not. In fact, not many films deal with cloning black people. Cloning someone who is black, reflects a "backdoor to eugenics" (Duster 2003) ethos that works out as being absolutely convenient especially when the "black" individual in question represents absolute evil, as is the case in the film *Unbreakable* (Night Shyamalan 2000).

The fight good vs. evil is also exemplified with plots where a good scientist turns into an evil doctor, or where "good" parents are approached by an "evil" corporate doctor seeking to sell him new scientific advances in the form of cloning a recently lost child. In these, the dialogs are sometimes constructed to question the ethical dimensions of science, which is not something new, but a rather old issue in Western culture. This storyline can be seen as a reference to celebrated doctors either in literature or as historical characters. They usually draw parallels to Dr. Faustus, Dr. Frankenstein, Doctor Moreau, and of course the non-fictional "angel of death" Doctor Josef Mengele. However, crafted as melodramas the characters in movies lose much of the complexities that make up true human quality. Ironically, it is perhaps Doctor Moreau – whose name means deep black – the character created by H.G. Wells who is rendered not completely evil by film director John Frankenheimer, even though his scientific work might not be to our liking.

For Better or Worse

Gattaca (Niccol 1997) one of the best known films on genetic engineering contains scenes where eugenic choices are made is widely hailed as having a humanistic message which is highly problematic. In a fancy laboratory office the purported hero's parents, receive information about genetic engineering. Seeking to avoid the mistake they made in naturally conceiving him, who turned out to be a faulty human being, they have resorted to be on the safe side with a second one. Initially we witness a scene where a couple is having sex in the back seat of a car. We listen to the main character's interpretation of the events in voice-over:

Vincent

Like most other parents of their day, they were determined that their next child would be brought into the world in what has become the natural way . . .

Cut to scene in a genetics clinic:

Clinician

Your extracted eggs, Marie, have been fertilized with Antonio's sperm. After screening we are left, as you see, with two healthy boys and two very healthy girls. Naturally no critical predispositions to any of the major inheritable diseases . . . All that remains is to select the most compatible candidate. First of all, we may as well decide on gender. Have you given it any thought?

Marie

We would want Vincent to have a brother, you know, to play with . . .

Clinician

Of course . . . Hello Vincent . . . You have specified hazel eyes, brown hair and fair skin . . . I have taken the liberty of eradicating any potentially prejudicial conditions, premature baldness, myopia, alcoholism and addictive susceptibilities; a propensity for violence . . . obesity . . . etc.

Marie

We didn't want . . . diseases yes, but . . .

Anton

Right, were just wondering if it's good to just leave a few things up to chance . . .

This sequence is a good example of people making choices utilizing actuarial thinking, which was designed for and is primarily used to derive risk and benefit in the insurance industry. As a cost assessment tool it attempts to lend answers to problems of potential risk, medical procedures and hospitalization – even end of life care. While beneficial to an enormous industry on economic terms, its related tendency however, is to reduce these problems to biological or medical terms (Nelkin and Tancredi 1994, p. 9) masking all other issues appear like manageable risks.

This film, although many viewers find it to be the most enthralling of the genetic engineering films to date is quite problematic for various reasons. The most important one is that the character that plays the “hero” part, is no hero at all. Even though he cannot achieve his personal goals because he is discriminated against due to physical shortcomings (the reason his parents agreed to genetic engineering for a second child) he manages to fulfill his desires by clever cunning and cheating. Never does he make an attempt to redress his grievances, and whatever he achieves in his life merely serves to satisfy his own needs and not those of anyone else – something a hero always does. Secondly, he passes off organic materials that belong to someone else – a man crippled for life – as his own, cannibalizing the other's body never giving a second thought to what he does, never asking himself if the use of DNA belonging to someone else is ethical. Besides this the film has all sorts of examples of forensic uses of genetic information that merely point to a very sophisticated policing of the members of the society it portrays – which are never questioned. The tacit acceptance of these technologies basically contradicts what most science fiction films seek to do; correct a dystopic system.

Fate and Profit

The dystopian setting where the action takes place is also changing. If many films do indeed show the setting as being a medievaesque laboratory full of formaldehyde jars with body parts, the chic minimalist workspace is increasingly apparent in contemporary films. And this designer setting is where the evil scientist is becoming an exemplar of “corporate evil” as is the case with the character Dr. Richard Wells, in *Godsend*, (Hamm 2004). Here, the scientist is an entrepreneur that works for a clinic engaged in cutting-edge research and which reflects a greedy, inhumane and grossly unethical medical system. In the same vein, In *The Island* (Bay 2005), the CEO of a bio-tech company is a hip personality that deals with quality investments; his factory manufactures – indeed clones – boutique body parts for celebrities, but not much more information is given on the technology. The film is a thriller in which the spare part clones eventually unmask the crimes committed by the corporation and gain their freedom after very, very long car chase scenes in a somewhat futuristic city.

More banal stories are about clones created to substitute for an inferior or unavailable mate, or created to cope with sexual inadequacies, as in the comedies *Multiplicity* (Ramis 1996) or *The 6th Day* (Spoottingswoode 2000). This last one is perhaps worth watching in order to take note of the pet cloning technologies being offered at a sales mall, which are set up in a much more interesting manner than the cloning of the film’s main character and the following two hours of wasted celluloid.

What has increasingly become the stage for commentary about new biological technologies is the gore science fiction film. *Alien: Resurrection* (Jeunet 1997), the fourth of the famous film series, engages its heroes for two hours in the killing of highly violent snake-shaped clones engendered from a slimy, sticky, nauseating outer-space monster; it is however not enough of a commentary on science to warrant consideration, even if the main female character is a clone, she is inseminated by aliens and has to make the decision to kill the monster she gave birth to. It is also noticeable that the stock artifacts of the creepy laboratory – upgraded items apparent in films like *The Elephant Man* or *The Island of Doctor Moreau* – adorn the set.

In this last film, serious consideration of the female role in the genetic/cloning film era is surprisingly absent, and this is quite surprising for as it stands, new technologies are for making or replacing babies, personal fulfillment, the happy family, the return to housework; ideals that are more in tune with the post-World War II return “to the happy family” ideology than with anything remotely modern. It is further striking that the “independent” film *Teknolust* is about a woman scientist who has an unsatisfactory personal life and clones herself to fulfillment – that is, makes three copies of herself who need to drink tea condimented with semen! Then, she finally finds personal fulfillment as she finds a boyfriend, and one of her clones has a child. The film is riddled with many other anti-feminist clichés that are quite surprising to find in a modern science-fiction film – especially one directed by a woman. Besides the outmoded female stereotype: female scientist with awkward looks, bottle bottom glasses, bad looking clothes, an insecure attitude towards male figures, DNA as a cliché item is widely apparent in this film. The colors of the garments worn by each of the three clones are carried over to each of the strands composing the images of DNA appearing on the main character’s computer, her microwave’s door, and the birthday cup-cakes she bakes for her offspring.

Alien: Resurrection does make a passing reflection on the female role when the cloned monster is to be destroyed by the female that gave birth to it. The woman, Ripley, does not exhibit a high degree of moral conflict when she destroys the creature. There is a moment when she seems to hesitate as to her actions but the dilemma is not further developed. A female portrayed as being

non-reflective towards cloning or in-vitro fertilization technologies is a great pity for the issues involved are extraordinarily complex.

Recycling ancient social taboos is the core idea of *Code 46* (Winterbottom 2005), a modernized mise en scène of Oedipus' tragedy, but lamentably, here it is utilized as a lamentable excuse for a tepid love story. The film is quite disappointing in its treatment of the subject – although it does utilize the conventions of cyberpunk – high tech mixed with inner city squalor which usually tends to reflect an art direction and script that is concerned with social critique – as part of the film's aesthetics.

An injunction that will forecast the human conflict in the film appears right at the beginning as a message that scrolls down over a barren landscape. It reads:

Article 1

Any human being who shares the same nuclear gene set as another human being is deemed to be genetically identical. The relations of one are the relations of all. Due to IVF, DI embryo splitting and cloning techniques it is necessary to prevent any accidental or deliberate genetically incestuous reproduction.

therefore:

- i. all prospective parents should be genetically screened before conception if they have 100%, 50% or 25% genetic identity, they are not permitted to conceive
- ii. if the pregnancy is unplanned, the foetus must be screened. any pregnancy resulting from 100%, 50% or 25% genetically related parents must be terminated immediately
- iii. if the parents were ignorant of their genetic relationship then medical intervention is authorized to prevent any further breach of *Code 46*
- iv. if the parents knew they were genetically related prior to conception it is a criminal breach of *Code 46* . . .

The film's main setting is a factory where passes to visit foreign lands are manufactured. As some of these are increasingly being stolen, a fraud investigator is called in to investigate. He, instead of identifying the thief, goes out with her and they have an affair; he eventually finds out that she is one of his mother's clones. As the action develops we find out that the ruling power that dominates everyone's actions is called the sphynx. A neon image of such a creature is the main decoration in the urban landscape. In this film, it is clear that both the strong message presented as a legal ban, plus the innuendoes that allude to Oedypus's plight imply an incestuous relationship. But besides this, it reinforces the idea that such a relationship would necessarily result inheritance of deleterious recessive genes in any offspring.

And what about the woman in the film? Does she suffer any pain for the trespass committed? Not at all. In the end, her memory as well as his are drained of any recollection of a sad love affair – she is banished to the barren lands that composed the backdrop imagery for the incest injunction while the overvoice tells us how much she loves William, her brother/lover. He, in turn, can't remember anything either, and lives happily ever after with his wife and child.

So then, what are we to make of thirty years of films about genetics and cloning? All things considered, postmodern opportunism, misconstrued metaphors, confused scientific imagery, and the intersections between fact and fantasy conform the *imagology* – visual landscape – that distort an understanding of scientific events. It is a pity that as scientific information has become more and more complex cinema has become more and more a medium that emphasizes a-historical, unscientific, and culturally contradictory positions to exploit fears while creating a climate of disinformation.

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HUMAN GENETICS IN THE PRESS THREE LESSONS FROM A CASE STUDY

*Matiana González-Silva**

Announcements of the identification of an increasing number of new genes, alongside promises of new therapies and diagnoses and the launch of the Human Genome Project, turned human genetics into one of the most important disciplines of the last decades of the twentieth century. The rising importance of this branch of research, however, was not only the fruit of scientific breakthroughs, but also came about through the construction of a positive image of this science among the public, political decisions and legitimacy.

This paper explores the role of the press in the process of consolidating the genetic approach to human biology and disease in the Spanish context. Through the careful reading of texts related to the field published in a particular newspaper – *El País* –, its overall intention is to show the complexities of the interrelation of scientific journalism with ideologies, disciplinary interests and broader social and political transformations, and to state that the media needs to be taken into consideration when writing the history of contemporary science.

How did *El País* present human genetics to the Spanish public from its very constitution in 1976 – during the turmoil of the end of the Spanish dictatorship – to the years that followed the publication of the human genome sequence? What role did popularisation play in the evolution of human genetics in Spain? What was said, by whom and for what reasons? What defines *El País'* approach to human genetics in comparison to news that appeared in countries worldwide leading this branch of research? Many questions can be posed with regard to a collection of news items published over three decades during which Spain experienced profound social and political transformations, human genetics turned into one of the pillars of local biology and *El País* became the leading newspaper in the country.

Lessons learned from this enquiry follow a whole corpus of scholarly work on the history of scientific popularisation, science in the public sphere and the shifting relationships between science and its varied audiences. It considers scientific journalism not as a unidirectional process through which knowledge is transmitted from research laboratories to the lay public, but, rather, as the result of complex interactions and as a fundamental element in the construction of techno scientific systems. By the end of the twentieth century, the media had become a major source of images of science and a fundamental influence in public attitudes towards it, therefore shaping the cultural context in which scientific research took place. In consequence, they were also the target of a wide number of actors interested in using them for achieving their particular objectives.¹

* This paper is the result of research that culminated in my PhD dissertation “*Del consejo prematrimonial al Proyecto Genoma Humano. Treinta años de genética humana en El País,*” defended at the *Universitat Autònoma de Barcelona* (UAB) in 2008 and written under the supervision of Jon Arrizabalaga, whose guidance was crucial for my work. I am also grateful to the Mexican National Council on Science and Technology (CONACYT) for funding, and to the Department of History of Science at the Institution Milà and Fontanals-CSIC in Barcelona, for their support.

¹ The bibliography on scientific popularisation is too wide to be reviewed here. Some of the approaches that were most influential to this paper are: Hilgartner (1990), Bensaude-Vincent (1997), Nelkin (1987), Dornan (1999), Shinn and Whitley (1985), and Jurdant (1993). For the public image of human genetics, see: Van Dijck, (1998) and Durant, Bauer and Gaskell (1998), among many others.

Assuming that the press is a major source of representation for the construction of the public image of different objects – science among them –, the study of science in the press allows us to trace the public discussions and controversies that surrounded science at a given historical moment, the actors participating in them, and their respective arguments. It also shows the standpoint of scientific journalists themselves with regard to the role of the public in the progress of science and the role of science in the progress of society. By understanding how the image of human genetics in *El País* changed, how this newspaper's model of scientific popularisation was shaped and what social groups were reflected in its pages, the aim of this paper is to systematise some historical lessons that can be learned from a systematic study of science in the press.

Ideological, social and political influences in science journalism are particularly easy to trace in *El País*, as the newspaper first appeared only six months after the death of Dictator Francisco Franco, who had remained in power for the previous 40 years.² In the late nineteen seventies, Spain moved from a military dictatorship to a democracy that was fully integrated in the European Union, conquering an increasingly peaceful political life. On a scientific level, politicians began to view research as a driving force for the economy.³ Of all the branches of research, biomedicine and genetics were most privileged; particularly areas related to the genetic aetiology of different diseases and to the development and implementation of genetic diagnosis tools. All this took place in the context of an international scientific scene defined by the growing industrialisation and medicalisation of human genetics and the launch of the Human Genome Project, on the one hand,⁴ and the rise of the Public Understanding of Science (PUS) movement aimed at promoting science among the public, on the other.⁵

The considerations that follow are derived from a case study that focuses on more than one thousand pieces of information – including reports, editorials, columns and letters to the editor – that appeared in the newspaper during its first 30 years of publication. Although it takes into consideration the convictions of the team responsible for scientific information at *El País*,⁶ it mainly concentrates on what was actually published, regardless of the publishing process that led to every particular publication.

Texts published in *El País* were considered 'objectified traces' of broader communication processes and the source for the construction of an image of science that was socially shared.⁷ Analysis has been made on two different levels: the evolution of discourses regarding human genetics from the perspective of different social groups, and *El País*' approach to science. In both cases, the national/international axis and the passing of time were taken into consideration, alongside issues such as the geographical origin of the source and the author of the texts, the journalistic genre that was used, the framing of the news and many subtle rhetorical strategies that are harder to appraise but that without doubt constitute the most interesting materials for research. Results can be divided between general trends that were observed, and hypotheses that are likely to explain them.

This was a study of science *in* the press, and not a study of science *through* the press, as questions were not posed on the evolution of human genetics but rather on its presence in a particular newspaper. It is a highly subjective approach that, compared to quantitative methods

² A comprehensive history of *El País* can be found at Seoane and Sueiro (2004).

³ Details on Spanish scientific policies can be found at Sanz Menéndez (1997).

⁴ Van Dijck (1998).

⁵ Bensaude-Vincent (2000).

⁶ Ruiz de Elvira (2004), Pérez Oliva (1998).

⁷ Durant et al. (1998) and Collins (1987), among others, have applied theories that take inspiration from social psychology and the study of science in the public sphere. One of the founding studies of the social construction of the public image of a determined discipline in the press is: Moscovici (1961).

more frequently used for approaching science in the press, contributes no statistical evidence. However, as compensation it endeavours to introduce the generally lacking historical and contextual perspectives into studies on contemporary science communication.

Lessons from a case study

Human genetics as a subject of scientific popularisation became increasingly central in the pages of *El País* as time passed. The image of this science portrayed by the newspaper, however, changed dramatically, both in what different actors said about it, and in the newspaper's approach to it. Beyond the specificities of this case study, some broader conclusions can also be drawn.⁸

(1) *Scientific discourses change according to the evolution of scientific theories and results derived from research, but also to the shifting power of relevant social groups and their corresponding disciplinary and financial interests. The broader political and social context influences the kind of scientific debates that take place in the public sphere.*

The first remarkable transformation of the public image of human genetics in *El País* is the evolution from genetics portrayed as a secondary science to genetics as the leading approach to medicine and the future of biology. During the seventies and early eighties, Spanish scientists used to emphasise social and financial influences on human diseases, mentioning genetics only as a general constitution that could confer a certain predisposition to determined ailments. Human genetics was also portrayed as a highly ideological and potentially conflictive branch of research. It was common for both *El País*' journalists and Spanish scientists to link "biologist" explanations of behaviour and disease with extreme right political stances. Psychosomatic explanations for relevant diseases such as cancer were presented as the most promising ones, according to a social approach to medicine and public health that *El País* was keen to promote on a political level.

The climate of intense debate surrounding the so-called 'transition to democracy', was certainly a determining factor in situating the consequences of genetics in the realm of public policies and in the recurring appeal to ideology for explaining why there were different scientific approaches to the same problems. On the contrary, the consolidation of political and economic stability during the late eighties led to the consequences of genetics being situated in the individual and clinical realms, and to the de-ideologisation of science.

The nineties were in fact the glorious years of the discovery of genes responsible for a wide variety of diseases and behaviours, the announcements of which were accompanied by promises of new diagnoses and therapies. Following the main trends in countries leading research in human genetics, this science was portrayed as the very future of biology and the path towards true understanding of human diseases. Although the influence of such an international context in the coverage that *El País* made during this decade cannot be underestimated, at a local level there are clear examples that show how discourses of particular actors changed dramatically, in a very short time and according to their new interests.

A good example is the launch of genetic diagnosis programmes, particularly that for Alzheimer's disease, which was without doubt the most controversial genetic test ever made available in Spain – and also an area of research and development in which Spanish geneticists were particularly active. In 1992, *El País* reported on the creation of a local "gene bank" for the study of this disease, which would depend on the public *Instituto para Enfermedades del Sistema*

⁸ It is impossible to include in this paper detailed evidence for conclusions summarised in the following lines, which are further developed and largely sustained by citation in: González-Silva (2005, 2007, 2008 and 2009).

Nervioso Central. Its director, Ramón Cacabelos, explained on that occasion that “among the causes” of the disease, “there are genetic, racial, sexual, infectious and psycho-social factors.” According to Cacabelos, only “when the disease appears before the patient is 65 years old, concrete genetic alterations, located in chromosome 21, seem to exist.”⁹

In 1995, *El País* reported that a genetic test for Alzheimer’s disease had become available at an international level. Only three weeks later, Cacabelos reappeared in the newspaper, now as the director of a private institute he had created, dedicated to the study of this disease. His discourse had completely changed as well. He then stated that the main risk for developing Alzheimer’s disease was to have relatives who had suffered from it, and that it could be diagnosed and prevented thirty years before the appearance of first symptoms.¹⁰ Two years later, a programme for the “genetic prevention” of Alzheimer’s disease, was launched with the promise that early diagnosis and preventive treatment could delay the appearance of the dementia by “between 6 and 18 months,” with the consequent saving of money for the national social security system. “Alzheimer’s disease is a purely genetic illness. Even 30 per cent of the cases thought to be sporadic (because they appeared in people with no previous cases in their families), are due to the combination, in one child, of the father’s and the mother’s genes,” Cacabelos said on that occasion.¹¹

Such an extreme genetisation triggered reactions from other members of the Spanish scientific community. In a letter to the editor, María Asunción Morán, lecturer at the Faculty of Medicine at the *Universidad Autónoma de Madrid*, challenged every statement Cacabelos had made: “It is not true that 98 per cent of the cases can be diagnosed,” she said, “there are false positives and false negatives, which could not justify the beginning of treatment when people are 20 years old.”¹² Some weeks later, two members of the Spanish Society of Neurology, Félix Bermejo and Teodoro del Ser, stated that Alzheimer’s disease was “the result of multiple factors, genetic and environmental” and that Cacabelos’ opinions had “no scientific basis.”

In the following years the predictive capacity of genetic tests for Alzheimer’s disease reappeared with a relatively constant pace. Promoters of a programme aimed at determining which patients were suffering from a genetic case of the disease argued that the diagnosis would allow people to better plan their lives, and that “those that adhere to this programme will be the first to participate in the clinical trials for drugs intended to retard or to block the disease.”¹³ Once again, other scientists were swift to disagree, but in the Spanish genetic testing controversy, the causation link between genes and disease was in all cases the core point of debates. The only danger that was mentioned was genetic discrimination by insurance companies or employers, which in fact was not too serious, given the solid Spanish public health system. Almost no mention of other potentially conflictive issues, such as the psychological cost of “knowing” that one would develop a certain disease for which there were no treatments or the conflicts of interests of determined geneticists, can be found.

The fact that the debates focused on the tests’ reliability and avoided any non-technical discussion clearly reinforced the geneticists’ positions, which were also supported by the surrounding genetic enthusiasm fostered by the HGP. It is understandable that promoters tried to frame debates in such a way, but it is surprising that detractors also discussed the subject from

⁹ “Primer banco de genes en España para estudiar el mal de Alzheimer.” *EP* 23-03-1992.

¹⁰ “Propensos al Alzheimer.” *EP* 04-12-1995. “El Alzheimer ya se puede diagnosticar y prevenir con treinta años de antelación.” *EP* 24-12-1995.

¹¹ “4.000 familias españolas con casos de Alzheimer tienen ya su ‘ficha’ genética.” *EP* 02-07-1997.

¹² “Sobre la enfermedad de Alzheimer.” *EP* 09-07-1997. Félix Bermejo y Teodoro del Ser. “Tribuna: ¿Se puede tratar la predisposición a padecer Alzheimer?” *EP* 04-09-1997.

¹³ “Diagnóstico genético del Alzheimer.” *EP* 19-02-2002.

such a perspective, especially when compared to the sociological and political approach *El País* displayed towards genetics during the previous years.

During the genetic testing controversies, *El País* contributed to the attempt to divert discussions away from the more problematic aspects of genetic testing, instead focusing discussions on the pertinence of the genetic explanations of disease. This was certainly a way of supporting them, alongside other strategies such as privileging them in space and visibility, praising the benefits of the testing and explicitly supporting genetic testing programmes in some editorials. As far as detractors were concerned, they were obliged to resort to marginal space in the 'letters to the editor' section to express their point of view, which in turn shows how geneticists had gained influence, obscuring alternative views that nevertheless continued to exist. The use of the reader's space is also relevant and reflects the broader balance of power among scientific disciplines, in such a way that previous geneticists had to resort to it, while other biologists achieved the 'official' spaces of news and interviews.

The incorporation of public policies as a relevant issue in the public agenda – a subject that had been deliberately ignored until then – is another relevant issue during the early twenty-first century that can also be interpreted as a strategy for supporting geneticists. Following the publication of the sequence of the human genome in 2001, Spanish geneticists promoted the creation of a national research institute dedicated to genomics. Following this objective, *El País* started to ask local geneticists how they assessed the state of local genomics, echoing voices that called for greater recognition of the study of the human genome in Spain and more funds. The recurring argument was that there was so much work still to be done after the publication, that it was not too late to incorporate the country into the enterprise, unless the government provided enough support.

In contrast to their journalistic custom, *El País* reported on disputes among different Ministries about controlling genomic research. It also reported on what had happened with the promised funds or which research groups had received new resources. Adding to the geneticists' arguments, journalists also praised the fascinating challenges of genetics in the future, portraying the HGP not as the end, but as the beginning of the biology of a whole century.

In a period in which genomics jumped into the public arena directly related to the allocation of public resources, it is not surprising that some worried voices were also raised. Spanish geneticists involved in the pre-existent research programmes insisted on the importance of distinguishing scientific quality from "mere opportunism". Scientists involved in other areas of biology complained of the privileged treatment genetics had received in the most recent National Plan of Research and Development. Other geneticists, for their part, deplored the fact that any branch of human genetics apart from the medical approach to it, was being ignored, while some neurologists and psychologists complained about what they perceived as an over genetisation of the Spanish research agenda.¹⁴ But from a broader perspective, the idea that was transmitted was that there was wide consensus on the importance of promoting genomics in Spain and that the government should be committed to such an objective.¹⁵

¹⁴ Carlos Avendaño. "¿Ya no es prioritario el cerebro?" *EP*, 06-04-2001.

¹⁵ Some relevant texts that illustrate this issue are: "Destacados científicos españoles exigen al Gobierno un plan urgente." *EP*, 13-02-2001. "Investigadores españoles alertan sobre el elevado coste de la medicina genómica." *EP*, 14-02-2001. "La investigación genómica se coordinará al margen del Ministerio de Ciencia," *El País*, 26-03-2001. "Aznar incumple su promesa de apoyar la investigación del genoma humano." *EP*, 22-10-2001. "Birulés niega que se haya retrasado la fundación de genómica." *EP*, 24-10-2001. "Oportunidad perdida." *EP*, 24-10-2001. "Las ayudas del Gobierno a la genómica disgustan a la comunidad." *EP*, 12-11-2001. "El Gobierno otorga para la genómica seis veces menos financiación de la solicitada." *EP*, 04-02-2002.

The construction of genomics as a political issue and the general promotional attitude towards genetic tests leads us to the next lesson of this study.

(2) Newspapers play an active role in shaping the public image of science and public debates about it. They do this by choosing who they represent, what questions they pose and how they frame news, among other relevant issues and according to a particular project of science popularisation, which is the result of political, social, scientific and ideological factors.

Some hypotheses can be explored in order to explain the support shown by *El País* towards promoters of genetic testing during the nineties. This was observed in the technical framing of controversies, the obscuring of potentially conflicting issues, the privileged space devoted to experts within the field, the “de-ideologisation” of science and, after the end of the HGP, the reports on the lack of funding for genomics and the underlining of the technical difficulties of the new science.

Firstly, it is worth mentioning the growing proximity of reporters to the Spanish biomedical community and their specialisation in science, which made the scientists’ world view easier to share. *El País*’ intention of contributing to the advancement of biology in Spain, alongside the development of human genetics itself, also needs to be considered.

The incorporation of science as a political matter in *El País* can be further read as the result of the position of this newspaper within the Spanish political scene. In 2001, the leader of the Spanish government was José María Aznar. He had turned scientific success into a matter of political legitimacy, although there was a general perception that the Spanish science system was underdeveloped. In the context of an apparently general consensus about the importance of genomics as the very future of biology, *El País* turned the lack of local research on this subject into an excellent new front to attack Aznar’s government, with which it had a very strained relationship. Genomics therefore became a concrete example of the existing gap between the importance given to science in Aznar’s discourses and the real support the government was providing, according to the editorial team of this newspaper.¹⁶

Besides the Spanish political and scientific context, the absence or presence of a local community of scientists working in the field that was reported on is another crucial factor when trying to understand the shaping of *El País*’ model of scientific popularisation, as *El País* dramatically changed its journalistic approach, coinciding with Spaniards entering the international genetic research scene. To give an example: long stories that had enabled problematisation, contextualization and mentioning of different approaches to a particular scientific subject – which had been the rule during the eighties, when genetic explanations were only included as one of many biological approaches –, were substituted by even shorter news that did not explain anything apart from the bare data provided by scientists who had announced their discoveries.¹⁷ Although this can be observed in the general approach to genetics, this trend is much more noticeable in news related to local science than in reports on research in which there were no local interests at stake, namely, the mapping and sequencing of the human genome.

¹⁶ It is interesting to note that once stem cells became a controversial issue in the Spanish science scene, *El País* also turned them into a political subject, with the clear intention of promoting local research. The same happened with human cloning (Alcíbar Cuello, 2004).

¹⁷ This recalls the importance of the journalistic genre in the analysis of specific models of science popularisation, since short informative news left no room to recount the process of scientific creation that was common in the previous years, and contributed to the promotion of a non controversial, unique, neutral and unquestionable image of science.

A hypothesis for explaining differences in *El País*' coverage of different branches of human genetics is that, in the absence of a powerful scientific community, *El País* felt more freedom in defining its own journalistic style than in areas where there was a local community of scientists seeking promotion. This style focused more on the economics, geography and politics of the HGP (sociological aspects of science) than on the promises of scientists and the scientific breakthroughs, typical of popularisation, in the leading countries of the project.

As far as the sequencing and mapping of the human genome project was concerned, chronicles of scientific events went beyond technical announcements and included lobbying, alliances and even gossip among scientists. Journalists talked about different national "scientific cultures" and explained to their readers how sociological aspects influenced issues such as sharing scientific results, patents and establishing scientific priorities. There was also wide discussion about issues such as "genetic determinism", the "philosophy of scientific progress", the "human essence" or the influence of scientific knowledge in the individual's moral responsibility.

However, this approach to a particular branch of human genetics stopped as soon as the local scientists launched their abovementioned advocacy campaign for genomics. *El País* then aligned itself with the leading Spanish scientists and the international trend, adopting a much more promotional journalistic approach to geneticists and forcing the government to support them.

The last lesson of this paper deals with political decisions on funding:

(3) *The image of science portrayed in the press influences the progress of science. However, an increasing amount of scientific news does not necessarily mean more fluid communication channels between science and society or the promotion of public participation in science related affairs.*

The detachment with which *El País* approached the HGP during the nineties should not be mistaken for a lack of support towards the initiative, as in every crucial moment the editorials defended the pertinence and importance of the project. It simply means that the newspaper portrayed this branch of research as a complex activity full of interests and contradictions, in comparison to news on the discovery of genes related to disease and genetic programmes, in which science was portrayed as a neutral activity from which only benefits could be expected.

This generally positive attitude was at the same time the result and the source of the growing power that geneticists acquired in the Spanish scientific scene. It shows that public legitimacy is crucial for consolidating a scientific discipline, and the important role played by the media in such a process. Due to their economic power, their proximity to journalists in *El País* and their public relations campaigns, geneticists were a privileged professional group in the Spanish public sphere. This newspaper contributed to the promotion of these geneticists' interests.

However, some critical opinions can also be found, the interesting feature of which is that they don't seem to have been aimed at modifying the course of science. They did express particular worries and disagreements about some issues, but the advancement of genomics was in fact perceived as inevitable, and no debate on how the Spanish scientific system should be conducted appeared within the pages of *El País*.

The conclusion of such an observation is that public visibility does not necessarily mean broader public participation. This recalls what Dominique Pestre defined as the contemporary "financial market-driven" scientific regime, in which democratic procedures in decision-making occupy a subaltern position with respect to the *de facto* situations created by financial and economic actors.¹⁸ This was the case of *El País*; it did not play the role of a true public arena for

¹⁸ Pestre (2003).

discussing genetics, but rather that of a promoter of local geneticists, also explaining on some occasions the complexities of the international scientific scene.

Conclusions

It is generally accepted that the media has a fundamental influence on contemporary societies, providing social groups with their very public 'existence' through visibility, defining the political agenda and shaping – when not creating – 'public opinion'. News coverage of science, however, has traditionally been much less problematised, although, journalistic texts that are finally published in newspapers – and that constitute the source of a social image of science among the public – are in fact the result of complex interactions between the rhetorical efforts of different social groups and particular newspapers' projects of science popularisation. This lack of problematisation is worrying as it contributes to an uncritical approach to science and makes democratic debates more difficult.

In the particular case presented in this paper, the positive image of genetics, the broad acceptance of its premises in *El País* and the obscuring of dissenting voices during the nineties contributed to making this branch of biology one of the most funded and renowned. This in turns confirms that the media certainly plays an important role in the course that science takes in a particular historical context, influencing the laws, funding and public legitimacy of science. However, this influence does not necessarily occur through a democratic process but rather through the adoption of a particular approach to science – more or less promotional, more or less problematised –, and the fostering of a specific image of what science is, what can be expected from it, how scientists should be treated and how open to third party participation science should be.

Further research related to human genetics in the Spanish public sphere could analyse how public discourses that appeared at *El País* were actually received and appropriated by different audiences in Spain, which would allow a deeper understanding of the processes of science communication. This would include the reception of genetic concepts in other social realms such as education policies, health practices and the legal system. For example, a genetic condition was alleged as an extenuating circumstance in the case of a paedophile judged in Spain as recently as in 1994. This paper is only a first approximation to the role of the press in shaping techno scientific systems, and a call for considering the importance of the media as a relevant source for historical enquiry.

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