

From boundary-work to boundary object: how biology left and re-entered the social sciences

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Abstract: In an archaeological spirit this paper comes back to a founding event in the construction of the twentieth-century episteme, the moment at which the life- and the social sciences parted ways and intense boundary-work was carried out on the biology/society border, with significant benefits for both sides. Galton and Weismann for biology, and Alfred Kroeber for anthropology delimit this founding moment and I argue, expanding on an existing body of historical scholarship, for an implicit convergence of their views.

After this excavation, I look at recent developments in the life sciences, which I have named the ‘social turn’ in biology (Meloni, 2014), and in particular at epigenetics with its promise to destabilize the social/biological border. I claim here that today a different account of ‘the biological’ to that established during the Galton–Kroeber period is emerging. Rather than being used to support a form of boundary-work, biology has become a boundary object that crosses previously erected barriers, allowing different research communities to draw from it.

Keywords: Galton, Kroeber, epigenetics, boundary-work, boundary object

The greater fault is not with the biologists who have explained historical phenomena by organic processes, but with the sociologists who have accepted and welcomed these alien explanations.

(Kroeber, 1916b: 34)

The making of the nature/nurture episteme

What is the ‘social’? What is the ‘biological’? In this article, I suggest that the disciplinary boundaries between these two domains have a historically contingent foundation rather than a logical one. Although there are many possible genealogies of the emergence of the social sciences, related to different national contexts

and intellectual agendas (Foucault, 1969; Ross, 1992; Lallement, 1993; Latour, 1993; Delanty, 1997; Leroux, 1998), here I focus on a rather unexplored lineage of the notion of the 'social': its emergence *via biology*, more specifically, via a certain *movement of self-delimitation of biological heredity* in the late nineteenth century.

This self-delimitation is the making of hard-heredity with Galton (1865), Weismann (1893), and later Johannsen (1911). I take this modernization of the notion of heredity (Meloni, 2016) to be the crucial conceptual event through which boundaries between the social and the biological were made possible and kept firm for all the twentieth century. Rather than ontological necessity, the division between the social and the biological we have known as natural in the last century or so is the contingent effect of a certain closure in a specific epistemic history, that of biological heredity.

Hard heredity, or the 'modern view of heredity' (Johannsen, 1911) is the notion that the hereditary material is *fixed once and for all at conception* and *unaffected by changes* in the environment or phenotype of the parents (Bonduriansky, 2012). Its alternative view, named (later in the twentieth century) soft heredity implies that lifetime experiences of parents and ancestors affect the hereditary material, which is no longer seen as invariable and constant through time. This latter view was mainstream before the rise of hard heredity (even Darwin, courtesy of his pangenesis, stuck to it) and, although associated with Lamarck, largely preceded him.

Breaking with this soft view, the construction of hard heredity was *the key conceptual move* that created the epistemic space within which the Mendelian notion of a stable genetic material became imaginable (Bowler, 1989). The century of the gene (Keller, 2000) depended decisively on the making of hard heredity. At the same time, the mark of hard heredity shaped decisively this understanding of the gene, and accordingly of the biological as 'sequestered' from social influences. Genetic and hard view of heredity became synonymous.

Hard heredity, however, was an important conceptual rupture not only for biological knowledge but also for the broader dynamics of knowledge production. By folding the 'biological' into a germ-plasm utterly separated from environmental inputs, hard heredity paved the way for a radical differentiation of the sociocultural from the organic. In soft heredity, the social is always on the verge of becoming biological: a previous generation's experiences and environments are deemed to be embedded in the biology of a successive one, and habits are turned into biological instincts. Lamarckism or soft heredity is the epistemic condition for a fully biohistorical or biosocial investigation, for an unceasing exchange of the biological and the social. The independence and autonomy of the sociocultural was just an illusion when Lamarckism was the dominant view amongst social scientists, from Spencer to early 1900 (Meloni, 2016). Kroeber, one of the heroes of American anthropology, was probably the author who best understood the many confusions of this quasi-Lamarckian biosocial view and the emancipatory possibilities implied by the rise of hard heredity (Kroeber, 1915, 1916a, 1916b, 1917, 1952; Kronfeldner, 2009; Lock, 2012; see Keller, 2016).

In the first decades of the twentieth century, hard hereditarians, geneticists and cultural anthropologists converged, from different angles, toward a destruction of the ‘vitiating mixture’ of the organic (ie biological) and the social (Kroeber, 1915), symbolized by Lamarckian trends (Stocking, 1968). The epistemic confusion of the social and the biological was no longer tolerable under the new knowledge regime.

Galton’s *English Men of Science: Their Nature and their Nurture* (1874) and Kroeber’s seminal article ‘The Superorganic’ (1917) roughly marked the beginning and final consolidation of an extended historical arc in which a certain *savoir* (Foucault 1969/2002) was produced. Within the conceptual boundaries of this novel epistemic regime about heredity (Müller-Wille and Rheinberger, 2012), new disciplines – the sciences of the sociocultural and the sciences of life, of heritage and heredity – could finally disentangle one from another, and emerge as autonomous enterprises, legitimized in their own terms.

The story of the emergence of the modern (hard) view of heredity has been extensively analysed (for instance, Müller-Wille and Rheinberger, 2012). There are two reasons, however, why it is necessary today to come back to this founding moment.

Firstly, only few studies have *put together* the conjoined movement of the emergence of a novel view of heredity *along with* and *in relation to* the emergence of the social sciences (Stocking, 1968; Kronfeldner, 2009; Lock, 2012) or connected the genealogy of the social sciences with broader epistemic movements in the life sciences (Gissis, 2002; Staum, 2012; Renwick, 2012). There is still little appreciation of how both twentieth-century biology and twentieth-century social sciences *originated from a common view of life*, upon which a symmetric view of ‘the social’ was leant and successfully constructed.

Secondly, possibly for the first time since the closing of that conceptual arc, we are today in the favourable epistemic position to see the *precariousness* of that seminal moment of boundary-work, its contingency and historical dependence on certain assumptions that are being increasingly challenged today. It is by looking at this rapidly shifting terrain that I observe and reinterpret what happened in Kroeber’s time.

When I say ‘favourable epistemic position’, I mean that we are in a moment of epistemic shift in debates on biological heredity and its distinction from sociocultural heritage: over the last decade, and particularly as a consequence of the new awareness of *epigenetic regulation of gene expression*, explicit mentions of a return to views of ‘soft inheritance’ (Richards, 2006; Hanson *et al.*, 2011; Bonduriansky, 2012) or ‘Lamarckian inheritance’ (Jablonka and Lamb, 1995, 2005) have emerged.

Of course, the current new appeal of soft inheritance is not an anachronistic return to the epoch that preceded Galton and Weismann. Lamarckism has been transformed to address the challenges of the century of the gene (Gissis and Jablonka, 2011). Nonetheless the view of biology and heredity that is emerging in our postgenomic time is significantly different from the one shared by Galton, Weismann or Kroeber. The ‘reactive genome’ (Keller, 2014, but previously Scott

Gilbert) that has emerged over the last two decades make it too porous to social inputs, from cell to society, to make the boundary-policing requested by hard hereditarian views effective enough to prevent a continuous smuggling across the *biosocial border*.

This is why, after tracing the historical and conceptual arc between Galton and Kroeber I will then jump (with only a quick stopover on the post-WWII scenario) to a discussion of the recent reconceptualization of the biology/society boundary in our epigenetic times. This jump of eight decades may seem unjustified. Many important things have obviously occurred in the relationship between biology and the socio-cultural domain in this intermediate period between 1920 and 2000. Nonetheless, this period can be seen, in a Kuhnian sense, as a period of *normal science* for the social/biological border (Meloni, 2016). None of the important conceptual movements questioned the significance of the boundary-work performed in the time between Galton and Kroeber. In the 1950s, hard heredity was simply reinforced and made molecular (Crick, 1958). The cold war context, moreover, made extremely unlikely any challenge (in the West) to the hardness of heredity: softness was monopolized by Lysenkoists in the Soviet Union, not a good place to look at for heretic geneticists working on non-Mendelian inheritance (Sapp, 1987). Furthermore, in the 1950s, the fathers of the modern synthesis increasingly argued for the uniqueness of human life, a move that perfectly matched Kroeber's radical disjoining of the social from the 'organic' (Smocovitis, 1999).

New tensions between the life and the social sciences emerged only later when the integrative approach of the modern synthesis was replaced by the 'rhetoric of conquest' pioneered by E. O. Wilson (Ceccarelli, 2003) This was followed by a cycle of reciprocal ambushes by the biological against the social (sociobiology, evolutionary psychology) and the social against the biological (biosociality) but none of these attempts significantly challenged the pertinence of the original separation of a biological world from a sociocultural one. Each of them claimed respectively the ontological or epistemic prevalence of one pole over the other (preparing the terrain for the science wars), but none radically questioned or in the end succeeded in dissolving the boundary between the two.

This is why, to look at the precariousness of that original demarcation, I will go to the present period, in which profound conceptual novelties have affected the status of the biological, and the notion of the gene in particular, on which that view of the biological was built.

The Galton–Kroeber arc: how twentieth-century social sciences took form from the hard hereditarian revolution in biology

Heredity as a blurred concept before Galton and Weismann

In the broader context of the diffusion of evolutionary views and penetration of hereditarian themes in the nineteenth century, the emergence of a specifically dichotomous conceptual space marked by a relationship of 'mutual exclusivity'

(Keller, 2010: 11) between ‘nature’ and ‘nurture’, heredity and environment, the biological and the social, is something that unravels at its own pace and in a very specific intellectual niche. There is no logical necessity for the vast constellation of evolutionary voices in the nineteenth century to think in oppositional terms of ‘nature’ antagonizing ‘nurture’, and in fact the two terms *in their exclusivity* do not appear anywhere before Galton (Keller, 2010).

Neither is there any logical necessity for the hereditarian wave that had influenced medical thought since 1800, to carve out a separate space of ‘nature’ in order to argue for the hereditarian character of certain diseases. In this often conceptually confused landscape, the widespread presence of a mechanism like the inheritance of acquired features, hugely prevalent in Western thought well before Lamarck himself (Zirkle, 1946), made it pointless to engage in any serious boundary-work between ‘nature’ and ‘nurture’, ‘biology’ and the ‘social’. No serious modernist purification of nature and society (Latour, 1993) was possible in a context where ‘ongoing social processes could still affect racial heredity’ (Stocking, 1968: 244) and habits or racial memories (what we would call today socio-cultural experiences) were believed to turn ultimately into biological properties of the organism.

The same discourse applies to the growth of the notion of heredity in medical contexts, especially in France and Britain (Beltran, 2004; Waller, 2001). This growth of medical hereditarianism is certainly the context upon which Galton’s oppositional discourse will resonate decades later. However, underneath this proliferation of a ‘hereditarian’ terminology, the most disparate and contradictory views of heredity could easily coexist (Churchill, 1967). Nineteenth-century medical thought is full of hereditarian etiologies for scrofula (Lomax, 1977), syphilis (Lomax, 1979), gout, consumption (tuberculosis), alcoholism, or insanity, but *not in the sense of attributing them to acquired characters rather than to a fixed nature*, to the ‘biological’ rather than the ‘social’. Before 1900, ‘there was no question in the medical profession but that acquired characteristics could become hereditary, that alcohol, drugs, sub-standard living conditions would debilitate parents and result in their producing weak, degenerate offspring’ (Rosenberg, 1997: 47). Early notions of biological heredity envisioned a blurred, confused mechanism ‘beginning with conception and extending through weaning’ (Rosenberg, 1974: 27).

Hard heredity as an epistemic rupture

This admixture of nature and nurture in the nineteenth-century cultural landscape was mostly broken by the combined influence of a British polymath with statistical passion, Francis Galton (1822–1911) and a German embryologist greatly annoyed by Lamarckian arguments, August Weismann (1834–1914). The literature on Galton and Weismann is immense and it is beyond the goal of this paper to review it (see a summary in Meloni, 2016). What is worth highlighting is that whereas in Galton the hard hereditarian view took form via a mixture of statistical studies and ideological beliefs (Cowan, 1977), in Weismann it was the result of a tortuous investigation that resulted in his view of the continuity of the

germ plasm, something that was profoundly innovative for the age (Churchill, 1967, 2015).

In spite of these differences, there is, however, a significant correspondence between the two authors' narratives (recognized by Weismann himself). Whereas the nineteenth-century thought in terms of mixtures of biological and social influences, both Galton and Weismann were passionate boundary-makers. Both based their new view of heredity on a series of profound dichotomies – *stirp/person* and *nature/nurture* for Galton, *germ plasm/soma* and *immortal/transient* for Weismann – which in a sense rested on a similar alignment.

On the one side, nature and germ plasm, the strong end of this oppositional narrative, were both internalized and reified, severed from external influences and made independent from the notion of individual generation (Keller, 2010; Müller-Wille and Rheinberger, 2012).

On the other side, nurture and soma became the weaker end of this new dialectic: the flank that is open to experiential influences but with no capacity to reach the depth of our hereditary substance.

After Galton and Weismann, heredity is what is perpetuated inside us and beyond us, and this can be now studied and even calculated scientifically, in its percentage of influence, and in its effects upon the 'environment'. Heredity is the space of the inborn and the non-modifiable, and all the rest is nurture: the acquired, the changeable, the 'outside us'.

The consolidation of hard heredity makes the fate of this 'outside us' slightly paradoxical. On the one side it is relegated to a *minor role* in biology, and further marginalized by the rise of a certain non-developmental view of genetics. On the other side, this biological marginalization is actually nurture's fortune: freed from the biological laws of heredity, '*nurture*' is invented as a *non-biological terrain* that can be opened to the incursions of a different scientific gaze, constituting the privileged area of investigation of the sociocultural.

Frequently criticized for its impoverished dichotomous view, what is often forgotten is that *upon this novel conception of heredity, the possibility of a radically different view of the social sciences, finally based on a thorough boundary-work of immunization from the biological, became possible for the first time.*

Making nature, freeing nurture

This novel conceptual landscape known as Neo-Darwinism and iconically represented by Weismann in the 1890s was mostly received in a hostile and even traumatic way in social science quarters (Ward, 1891; Spencer, 1893a and 1893b). This is far from surprising. Looking at anthropological or sociological developments, in their different national contexts, France, Great Britain or the USA for instance, what appears evident is that everywhere before Galton and Weismann gained currency, social science investigations were profoundly entangled with biological themes and suggestions.

In sociology, the international influence of Spencer (Gissis, 2005) is exemplary of this kind of evolutionary *social-cum-biological* thinking. In Spencer, a

Lamarckian framework plays a major theoretical role in providing a mechanism (use-repetition-habitation, see Gissis, 2005) by which habits turn into instincts and cultural inheritance is inextricably mixed with biological heredity. Such Spencerian vision was so widespread that, even in authors who opposed Spencer's *laissez-faire* philosophy the Lamarckian mechanism remained widely shared, as in the first generation of American sociologists (Calhoun, 2007; Breslau, 2007).

French sociology was also profoundly influenced by Lamarckian themes and ideas that 'supplied sociology with a matrix for the formulation of theories on the constitution of modern society' (Gissis, 2002: 73). The same relationship of the early Durkheim to Lamarckian influences is more complex than one might suspect (Gissis, 2002; Marcel and Guillo, 2006), thus showing the profound entanglement of social and biological theories in the early phase of French sociological writings for the different but equally de-biologizing trend in British sociology particularly with L. T. Hobhouse, see Renwick, 2012.

Things for anthropology were not different. George Stocking has shown (1968, 2001) how American anthropology before the rise of culturalism was immersed in a confusion of biology and culture, 'race' and 'civilization' that could not be faced properly *without* and *before* the well-defined concept of heredity supplied by Galton and Weismann. In anthropology, notions like blood, racial temperament and racial memories were the site of an epistemic confusion in which 'what was cultural at any point in time could become physical; what was physical might well have been cultural' (Stocking, 2001: 14).

This continuous back and forth between cultural and biological mechanisms made it impossible for any serious boundary-work to take place. It was this equivocal space of the biological and the cultural that was in the end destroyed by the rise of hard heredity and it was in particular Kroeber who took this revolution seriously. Kroeber understood better than anyone else the impact of the Galton–Weismann paradigm shift, and how beneficial this novel landscape could have been for the process of differentiating and establishing disciplinary boundaries between the socio-cultural sciences and biology (Stocking, 1968; Degler, 1991; Kronfeldner, 2009; Lock, 2012). If a real autonomy of the sociocultural was to be achieved, the Gordian knot of biosocial Lamarckian inheritance was to be cut without hesitation, Kroeber thought.

The road taken and the road not taken: Kroeber and Boas

Although their goal was similar, namely crushing racist doctrines and endorsing equal opportunities for all human groups, the road taken by Kroeber was significantly different from that of Boas. It is important here to insist on the difference between Boas and Kroeber. Boas was less interested than Kroeber in establishing a rigid 'biological vs. sociocultural' boundary. His crucial study on *Changes in the Bodily Form of Descendants of Immigrants* (1910) illustrates his approach, which leaves a channel of communication between the cultural and the physical open to some sort of traffic. In contrast to Boas' biosocial strategy, Kroeber's

autonomy of culture saw plenty of potentialities in the new hard hereditarian paradigm. The folding of the hereditarian substance in the germ plasm, became for Kroeber the best theoretical condition to emancipate the social sciences from biological heredity and reserve their role to the study of 'purely civilizational and non-organic causes' (1917: 182–183; see Kronfeldner, 2009). Rereading 'The Superorganic' (1917/1952) today, what strikes the reader is how Kroeber felt comfortable with Galton (who 'has always evoked my complete respect and has been one of the largest intellectual influences on me' Kroeber, 1952: 22) and, above all, Weismann in whose writings he senses an anticipation of 'modern cultural anthropology's argument that man's mind and culture were independent of biology and depended upon tradition and other social processes' (Cravens, 1978: 38).

It is entirely thanks to Galton and Weismann that Kroeber finds the best possible line of attack on that unacceptable mixture of heredity and civilization that he sees at work in neo-Lamarckian authors, from Spencer, to Le Bon and Lester Ward. These social science scholars who 'will not and can not see the social except through the glass of the organic' (1952: 38) are the true targets of Kroeber's seminal 1917 article. He accuses them of committing the 'fallacy' of thinking of 'the social as organic' (1952: 36), persisting in the 'biological and historical monstrosity' to give a role to 'heredity by acquirement' (1952: 37).

'The Superorganic' is actually the culmination of a series of texts in which Kroeber has already come to terms with the Mendelian revolution as the basis for a new, independent, social science. In 'The Cause of the Belief in the Use of Inheritance' (1916a), for instance, Kroeber criticizes the 'naïve and even primitive method of reasoning by analogy' (1916a: 370) typical of neo-Lamarckian authors. Lamarckianism is seen as the infancy of a biological discipline that instead achieves with Weismann a 'plain maturity'. In this mature phase, 'organic phenomena must be interpreted solely by organic processes' (1916a: 369). In two almost contemporary texts, 'Eighteen Professions' (1915) and 'Inheritance by Magic' (1916b), Kroeber further attacks that 'vitiated mixture of history and biology' (Kroeber, 1915: 285) seen in the work of Lamarckian authors by using the Mendelian 'utter separateness' between the gamete and the zygote (Kroeber, 1916b: 27), to infer that biology cannot in any way explain the achievements of human society (Lock, 2012).

There is in the work of Kroeber a paradoxical but crucial move for the future of the social sciences. He accuses the social scientists of his time of using the lens of (Lamarckian) biology to study in organic terms the irreducible concept of civilization. However, he can condemn a certain biology (Lamarckism) only to the extent that *he himself uses another pair of biological glasses*, namely those offered by Galton and Weismann, to neatly separate the biological from the cultural as non-organic (Kronfeldner, 2009). Social scientists' temptation to imitate biology, in Kroeber's term, is in other words won only by an incorporation of another biological truth, the hard hereditarian one that civilization is not biological, just as biological inheritance is not cultural.

In sum, in order to stop the traffic on the biology/social sciences border, it is the new hard heredity paradigm that Kroeber aims to incorporate 'without

reservation' (1952: 41). Hard heredity allows the social sciences to consolidate that process of boundary-making (to which Kroeber is profoundly committed) in the second decade of the twentieth century, when the Mendelian revolution is decidedly on the rise and neo-Lamarckian suggestions also start to wane outside biology.

The implications of Kroeber's move for the future of the social sciences are huge. Although Kroeber's view of the superorganic was challenged amongst the Boasian ranks themselves, Kroeber clearly anticipates the mainstream way in which the social sciences emancipated themselves from their biological roots. The strategy chosen was to immunize social facts from biological ones ['disregard the organic as such and to deal only with the social' (Kroeber, 1952: 34–35)], claiming that the socio-cultural was a distinctive sphere explicable only in its own terms (the unrecognized analogies with Durkheim's move have been noticed by many, see Degler, 1991; Lock, 2012).

However, to repeat, this immunization strategy could be pursued by only more or less implicitly relying *on a certain prevailing* view of the biological, the gene and heredity. That this biological view is a partial one is emerging only today with profound implications for the tenability of Kroeber's move and the fence-making strategy of the social sciences.

From boundary-work to boundary object: how biology re-entered the social

Kroeber's strategy can be seen as an example of boundary-work (Gieryn, 1999; Kronfeldner, 2009): in this case an ideological effort of demarcation practised *within* scientific disciplines that was aimed at establishing the disciplinary autonomy of the 'social' from the non-social, the latter equated by Kroeber with the organic. We can see at work in Kroeber all the three possible strategies of boundary-work: '*expulsion, expansion, and protection of autonomy*' (Lamont and Molnar, 2002: 179; see also Gieryn 1999: 16 and ff): the expulsion of hollow rival Lamarckian authors; the expansion of the terrain of the superorganic (now filtered from biosocial contaminations); and the protection of its autonomy from any fallacious reduction of the social to the organic, which also meant protecting the autonomy of culture from the claims of physical anthropologists.

The Kroeberian case is only one episode of a major parting of the ways between the life and the social sciences at the beginning of the twentieth century. Of course, this strategy of overt liberation of the sociocultural from biological infiltration has not occurred in the same way and following the same intellectual path in all countries. Kroeber's case may be particularly visible and also idiosyncratic of the strong presence of a genetics and hard hereditarian eugenics movement in America. What is unquestionable is that when a hard-hereditarian view arose in Western countries, the Lamarckian matrix on which much nineteenth-century social science had relied went into a deep crisis. A novel division of labour became pervasive across many disciplines, not only anthropology; and needless to say each discipline found its own peculiar way to depart from the biological (Cravens, 1978; Richards, 1987; Degler, 1991).

As said above, the post-1945 scenario reinforced and stabilized Kroeber's boundary work. Biology was conceived mainly as a site of demarcation between the social from that of the non-social. More importantly, biology was used *within the social sciences themselves* as the symbolic criterion by which to differentiate those social sciences still in their infancy (those still drawing on a biological and evolutionary repertoire) from the mature and therefore no-longer-biological ones. The credibility of the social sciences was at stake in this strategy of debiologization: a bad social science was a social science dressed in biological terms.

For various scientific and political reasons (see a broader reconstruction, in Meloni, 2016), a symmetric movement of a *debiologization of the social and a desocialization of the biological* became the almost only game in town in the post-WWII scenario. The biological and the social emerged as alternative causative explanations of human behaviours: *if not social in origins, it must be biological* and vice versa was the mantra of that period, from the complete destruction of soft heredity around the 1930s to ca. 1990s.

The argument I want to follow is that biology, *after having been the site of this intense boundary work which demarcated the social from the non-social for almost all of the twentieth century*, has turned only in the last two decades into *something that can be described as more akin to a boundary object* (Star and Griesemer, 1989; Bowker and Star, 1999; Star, 2010). Biology is becoming a bridging and flexible theoretical 'object' facilitating the investigations conducted by different research communities; geneticists and sociologists, epigeneticists and anthropologists (see, for instance, Roberts, 2015; Niewöhner, 2015). In Bowker and Star's terms, boundary objects are 'objects that both inhabit several communities of practice and satisfy the informational requirements of each of them. In working practice, they are objects that are able both to travel across borders and maintain some sort of constant identity' (1999: 16). This 'allows different groups to work together without consensus' (Star, 2010: 602).

The case of some recent writings in social science referring to developmental biology and epigenetics illustrates well this shift in conceptualizing biology as a boundary object rather than a classical boundary-work.

What epigenetics brings to this debate

It is beyond the goal of this paper to offer a detailed reconstruction of the rise of epigenetics as a key area of biological research. Following a standard definition, epigenetics can be defined as the 'collective heritable changes in phenotype due to processes that arise independent of primary DNA sequence' (Tollefsbol, 2011: 1). Under this often blurred conceptual umbrella (Meloni and Testa, 2014), we can find today a large number of molecular actors: from DNA methylation, the first epigenetic modification to be discovered already in the 1950s (Rauch and Pfeiffer, 2011), to histone modification, from gene silencing, to X chromosome inactivation (Herce and Murr, 2011).

There are three crucial aspects that demonstrate the appropriateness of reconceptualizing epigenetics as a boundary object that crosses the social/life-sciences divide.

Firstly, the extreme *openness of genetic functioning* via epigenetic mechanisms to the nested levels of *social structures*. In an epigenetic view, social structures are no longer conceived as irrelevant to genetic functioning, or worse a mere reflex of it, but as a causal source of gene regulation and expression that makes every biological process socially patterned (Landecker and Panofsky, 2013).

Secondly, the possibility of *transmission of socio-environmental exposures from one generation to another*, implied by transgenerational epigenetic inheritance (Jablonka and Raz, 2009) is a key mechanism in blurring the demarcation between social and biological processes.

Thirdly, *the potential reversibility* of epigenetic marks such as methylation patterns makes them particularly prone to become amenable to pharmacological and social intervention, in contrast to practically unchangeable genetic variations (Szyf, 2001). Once again, a boundary-crossing mechanism by which social intervention can have direct biological effects is here established.

My contention is that the conjoined use of these three features is already having a significant influence on undermining any residual attempt to erect barriers between the social and the biological in recent social epigenetic writings. I will refer in particular to recent works of biological anthropologists Chris Kuzawa (Northwestern) and Jonathan Wells (London) to illustrate the emergence, via epigenetics and broader developmentalist models, of a new theoretical vocabulary based on 'biosocial mixtures'. This novel operationalization of the biological no longer aims to restrain or reduce the social to it, but to create a hybrid, entangled conceptual channel in which the 'social' is embodied, passed on and reconstituted at each generation thus making the very texture of the 'biological'.

Entangled biosocial terminology after the boundary-work

The work of Kuzawa and Wells has emerged in the last decade in the context of broader evolutionary debates on phenotypic and developmental plasticity, the capacity for an organism to 'tailor phenotype to ecological conditions irrespective of genotype' (Wells, 2010: 3). Epigenetics offers to this view the perfect non-genomic mechanism by which an organism can face rapid ecological changes without needing to change its genetic hardware.

Amongst many contributions to plasticity Kuzawa and Wells stand out not only for the rigour of their theoretical framework but also for proposing an original model of plasticity. They challenge future-oriented approaches in which plasticity is seen as an anticipatory response to possible cues relative to adult ecological conditions (what is known as 'weather forecast': Bateson and Martin, 1999; or the Predictive Adaptive Response model: Gluckman and Hanson, 2004).

In contrast to this, both Kuzawa and Wells have suggested more 'backward-looking' (Kuzawa, 2008) approaches in which what the foetus 'actually sees' is less the present environment and more what Kuzawa calls intergenerational phenotypic inertia, 'an average nutritional environment as sampled over decades and generations' (Kuzawa, 2005: 12–13), a model to which Wells adds the idea (sociobiological in its origin: see Trivers, 1974; Haig, 1993) of a resource conflict between mother and offspring (Wells, 2007b).

At a superficial glance, these (rather technical) debates may seem to have only a marginal impact on my argument for a reconceptualization of biology, via epigenetics, as a boundary object that crosses neatly the increasingly precarious life/social sciences boundary. However, this is not the case. If one looks at the way in which this conceptual background is operationalized in more recent publications by both Kuzawa and Wells, in which the scope of their research is broadened to include more classically ‘sociocultural’ themes like ‘race’ (Kuzawa and Sweet, 2009) and ‘socioeconomic pressures’ (Wells, 2010), it appears evident that a hybrid terminology made of entangled biosocial constructs is definitely replacing twentieth-century dichotomous attempts to police the traffic across the social/biological border.

Embodying race in an epigenetic time

Kuzawa and Sweet’s article ‘Epigenetics and the Embodiment of Race’ (2009) takes its cues from a classic example of the conceptual polarization that has dominated the twentieth century, in this case in debates on health inequality: should we attribute US black–white disparity in cardiovascular disease (CVD) to nature or nurture, genes or the environment, biological or social forces? In such a debate, naturist and nurturist approaches (for lack of better words) have fought their battle from separate corners: ‘explanations’ as Kuzawa and Sweet remark ‘have tended to align with one of two models that emphasize either social or genetic causes’ (2009: 2).

The authors oppose such polarizations, but not by advocating the customary interactionist view (gene x environment) that leaves intact the already preconstituted separation of a social and a biological world. Instead, what is interesting in Kuzawa and Sweet’s article is how the logic of entanglement overcomes interactionism, and the transfer of information from the biological to the social and *vice versa* becomes methodologically impossible to extricate.

The article’s key argument is a developmental/epigenetic origin of black–white health inequalities. Starting from the observation that ‘African American mothers have higher rates of low-birth-weight births than white mothers in the US’, and so by focusing on ‘racial disparity in birth outcomes’ (2009: 6) the authors hypothesize that the persisting discriminatory effects of race as a socially constructed category impact negatively on maternal biology and thus are passed on (always via intrauterine environment) at each successive generation, programming the foetus to a higher CVD vulnerability.

Superficially, the main thesis could be seen as a mere variation on a constructionist or psycho-social reading of health inequality. What is decisive here, however, is the biological embedding of these classically socially constructed factors. In Kuzawa’s model the persisting cycle of health disparities is as much a biological as a social reality, and after the first ‘constructionist’ kick-off, any further demarcation of the two factors becomes literally impossible. A focus on developmental and epigenetic processes means that the biological and the social become, in their supposed original separation, two evanescent entities.

Biology passes into the social structure and the social structure is reproduced and embodied in biology. The social position is a biosocial hybrid organically reconstituted at each generation. The social past is embodied as a biological pattern that affects organic processes driving them toward certain developmental trajectories in the future (Kuzawa and Sweet, 2009: 11).

Maternal capital and metabolic ghetto

Wells' 'Maternal Capital and the Metabolic Ghetto' (2010) has a broader scope than Kuzawa's article. It addresses historical trends in the transgenerational transmission of health inequalities looking in particular at infant growth and stature as a biomarker of socioeconomic exposure. Its hypothesis is that 'the development of metabolic capacity in each offspring generation' is transgenerationally driven by the nutritional experience and environmental exposures of previous generations, and therefore becomes embedded and perpetuated historically.

With respect to my argument for the emergence of a *hybrid biosocial vocabulary*, the key construct on which I want to focus is Wells' notion of 'maternal capital'. Maternal capital is defined by Wells as 'any aspect of maternal phenotype, whether *somatic* or *behavioral*, which enables differential investment in offspring' (2010: 5, my italics). Note that this is a broad definition that *crosses and undermines* any possible organic/cultural or biological/social boundary we might draw. So, an example of *somatic* maternal capital is maternal stature (reflecting 'the experience of recent ancestors as well as developmental experience'), whereas a *behavioural-cognitive* example would be maternal education 'accrued over decades but potentially subject to deterioration if not updated during periods of rapid social change' (2010: 6).

As Wells (2010: 7) clarifies, 'the use of the term capital is deliberately broad, to facilitate integration of different areas' from body size and physique, energy capital (adiposity, extracorporeal larders) to social and cultural capital (relying on Bourdieu): all dimensions, 'biological' or 'social' in twentieth-century categories, that *are always on the brink of being converted one into another*.

In such a paradigm, it is again the maternal channel that is the place where the boundaries between the organic and the sociocultural, nature and nurture, are continuously crossed and remade (see for a critique, Richardson, 2015). The maternal channel becomes in this case what Wells calls a 'metabolic ghetto' (Wells, 2007a), the 'transducing medium' (2010: 14) where social phenomena, political oppression and economic marginalization literally become embedded and transmitted to future generations.

The case of loss of stature in Indian women during British colonialism in favour of British women's growth is a very telling example of the global circulation of maternal capital (Wells, 2010). Power and unequal social structures literally get under the skin, becoming inscribed and reproduced into the maternal body and being transmitted to offspring in terms of their health prospects, which thus in turn become a variable contributing to the magnitude of maternal capital. Importantly, however, neither maternal capital nor offspring health can

be said to be a biological any more than a social effect in this model. They are rather the site of a continuous and bidirectional exchange occurring between what is only later constituted as the 'biological' and the 'social'. The same notion of a metabolic ghetto is employed to cover this hybrid area spanning the 'physical bodily dimension' and the 'impact of economic marginalization on the physiology of reproduction' (Wells, 2010: 11). The implication is that biological plasticity in humans is the outcome of many, often century-long, histories and decisions: political, economic, cultural, military, all things 'sociocultural', of pertinence of social scientists.

Conclusion

In this paper, I have argued for a decline in the use of biology as a form of boundary-work in favour of an increasing reference to the biological as a boundary object that crosses and undermines neatly defined biology/society or biology/culture borders. The two articles only briefly examined above are symptomatic of a new conceptual space created by the rise of a developmentalist and epigenetic view of life processes. The sealed border of nature and nurture established by Galton between what 'a man brings with himself into the world' versus all the influences 'from without that affects him after his birth' (1874: 12) or Kroeber's parallel demarcation of the organic versus the sociocultural, has never looked so inadequate.

What we know today is that the biological cannot be restricted 'to a fraction of what we are only born "with"' (Ingold and Palsson, 2013: 40) exactly as the social is not something supervening later upon an already given biological basis. The same interactionist rhetoric is inadequate to express this entanglement: genes (G) and environment (E) can interact if we believe the two as separate domains, as in a hard heredity view. However, in epigenetics, genetic function is always connected to the incorporation of previous environments, making the picture extremely more complex (Tal *et al.*, 2010).

Biology has ceased to be an ontological and an epistemic marker of difference; it has become instead, for the ascendancy of socio-historically patterned models of genetic functioning (aka epigenetics), an open resource from which many scientific communities can draw. Of course this does not mean that the new biosocial phase will be a golden age for the relationship between the social and the life sciences. Many tensions are foreseeable with the return to a nearly soft-hereditarian paradigm but of different qualities compared to a recent past (Meloni, 2015a, 2015b, 2016). My paper has therefore to be seen less as a celebration of the present moment than an appeal to understand the novelty of the contemporary scenario through different conceptual tools than the ones inherited from the century of the gene. It may soon come a time when a social science *without* biology will be seen as it was in the last century a social science of biological inspiration: some relic of the past inadequate to describe the novelty of the present.

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