THE PRINCIPLE OF PARSIMONY AND HOW AUGUST WEISMANN USED IT

ÍÑIGO ONGAY

ABSTRACT. This paper explores the use of the principle of simplicity in August Weismann's critiques of the inheritance of acquired characters. After considering different accounts of simplicity as a scientific virtue to be taken into account in science evaluation, the paper goes on to scrutinize a particular example of the principle of parsimony at work. The author considers the structure of August Weismann's arguments for the all-sufficiency principle of natural selection (NS) to conclude that a variety of lines of reasoning can be identified in his account and that parsimony plays a different role to each. There has been a long-lasting agreement among historians of biology and philosophers of science alike that the work of Weismann signals that theories involving the heredity of acquired traits are flawed, as such type of inheritance is not possible in evolution.

While much debate has recently arisen in the domain of evolutionary developmental biology challenging the Weismann barrier principle, both the proponents of the "extended synthesis" and the Neodarwinian orthodoxy seem to coincide in that Weismann's arguments favor NS to the virtual exclusion of any other principle regulating the transmission of traits in evolution. Whatever Weismann would have wanted to conclude, I will argue that this understanding of what Weismann's arguments entail is a mistake.

KEY WORDS. Extended synthesis, Lamarckian inheritance, natural selection, parsimony, philosophy of biology.

INTRODUCTION

Ever since its establishment back in the Middle Ages, the principle of parsimony or simplicity has been regarded as an important factor to be taken into consideration in evaluating the merits of contrasting theories. Even with agreement regarding the relevance of the principle, there has been considerable dispute on how simplicity should be properly understood in science and philosophy alike. Some would be fast to identify simplicity with ontological parsimony in the sense of the reduction of the ontologi-

Universidad de Dehusto, Oviedo, España / ongaydefelipe@gmail.com

cal commitments of a given theory (and so the motto goes: *entia non sunt multiplicanda praeter naecessitatem*). Others —*á la* Reichembach— would interpret simplicity as the use of the principle of common cause as opposed to employing explanations involving separate causes, as the preference for models with less adjustable parameters or even as succinctness such as the mathematical formulation of a theory.

The debate, perhaps somewhat unsurprisingly, extends to the question of what the reasons are for parsimony to be granted a decisive weight in theory assessment. It is easy to quote Ockham or Newton among a variety of other equally egregious names of the past, to the effect that simpler theories are to be preferred over comparatively more "complex" ones. It is not so easy to construct a precise characterization of what such simplicity may mean in practical terms or to justify why simpler scientific theories should be given such preference. This is so especially if one agrees that attributing simplicity to the way nature functions is not so straightforward a move as Ockham (or Newton) had envisioned given their specific ontological frameworks.

This is not to say that simplicity, however construed, does not constitute a virtue in scientific reasoning. It is demonstrably true that within a plurality of contexts, "simpler" theories or hypotheses are "better." What is rather unclear though, is what both adjectives may be taken to signify in general (Sober, 1990, 2015). Some philosophers (Rescher, 1990) would point out that simplicity represents an aesthetic virtue, albeit an important one, to which no principled epistemic implications should be attached. Others argue that the principle of parsimony can be defended for theoretical or empirical reasons. In this regard there exist many purported justifications whether of pragmatical, psychological or statistical nature that various people have invoked on different occasions. The point is sometimes made that simpler theories are easier to falsify or to confirm empirically (Popper, 1992; Schulte, 1999). In other cases, philosophers tend to embrace the assumption that more parsimonious theories are more likely true (Jeffreys, 1961; Swinburne 1997), although clarity on why this should be so is generally lacking. All these points of view, however, have proven open to criticism and so it is, all in all, fair to say that there is no present common understanding on why simplicity matters scientifically (provided that it really does).

It is not just that though. As Elliott Sober (2015) has recently shown there is not a simple answer to the question on how comparative degrees of parsimony are to be measured when evaluating different hypotheses. It is well to notice in this respect that in the absence of a general account of what simplicity means precisely, it becomes all the way more difficult to ascertain how to use such principle in the particular cases of theory evaluation. Indeed, perhaps the best thing to say about parsimony is that,

far from representing a single unique canon to be used extensively in all possible cases of scientific debate, there abounds a plurality of principles each giving different (and sometimes incompatible) advice. Under this set of circumstances, it is wise to be prudent (and pluralistic) about what the Ockham's razor really suggests and show some modesty (and much restriction) when thinking why it lends the advice it does.

My goal here is not to encourage modesty. The aim of this paper is to explore the extent to which parsimony, in the multiple ways it may be understood, bears a weight in August Weismann's assessment of two competing hypotheses in evolutionary biology: Natural Selection (NS) on the one hand, and the Inheritance of Acquired Characteristics (IAC) on the other. In a nutshell, what I purport to do is to analyze the structure of Weisman's argument(s) in defense of NS with the aim of showing that the principle of parsimony does have a proper, albeit limited, role to play within such argument. More precisely, the point will be made that as a matter of fact, Weismann presents three arguments on NS, which are relatively independent from one another, and that parsimony bears a very different relationship to each: from *no relationship* in the case of the first argument to *non-zero relationship* in the other two arguments that my paper will identify. Finally, I will conclude by indicating the ways in which the entire issue the paper looks at connects pertinently to a variety of the most intensely debated topics in current evolutionary biology.

It is important in any case to start off by providing some clarification. First, regardless the extent of the debate about what parsimony as an epistemic value consists of, it is noteworthy that, as everybody agrees, arguments based on simplicity constitute cases of *ceteris paribus* arguments. In this respect, no philosopher would ever find it sensible to say that simpler theories are to be preferred over more complex ones *for the sake of their simplicity* alone and irrespective of what the evidence dictates. Parsimony however construed, is employed as an epistemic virtue in assessing different theories precisely when these theories do overlap empirically and, thus, makes no sense at all when such overlapping fails to obtain. Secondly, simplicity works as a contrastive tool: this is to say that parsimony always comes in degrees as long as no theory can be deemed *simple or complex* in absolute terms, although some may be judged simpler in comparison with other relatively more complex alternatives in relation to variable sets of criteria. Finally, August Weismann doesn't explicitly refer to parsimony when arguing against the IAC and in defense of NS. There is good reason for him in not needing to invoke the principle. Weismann is a biologist, not a philosopher of biology and, therefore, he is not in the business of stating formally and in the abstract why one explanation is preferable over another. In this sense, the goal of the paper is to elucidate the logic behind his arguments with the aim to show that some of them—*but*

not all—can be formulated as based on the use of parsimony. This being so, nonetheless, it is worthwhile to note also that parsimony is understood in different ways within each of the arguments.

2. CHARLES DARWIN AND AUGUST WEISMANN ON NATURAL SELECTION

In *On the Origin of Species,* Charles Darwin (1859) famously stated: "I am convinced that Natural Selection has been the main but not the exclusive mean of modification" (p. 69). When Darwin wrote this, it was the IAC model that he had in mind. It is easy to forget, due in part to the later success of NS in evolutionary theory, that the IAC, as a mechanism accounting for evolutionary change exerted a large influence on the work of many a naturalist over the course of the nineteenth century. This applies not only to people like J.B Lamarck or Erasmus Darwin but also, very crucially, to Charles Darwin himself.

Darwin did not merely allow for the theoretical possibility of the IAC having a proper causal role to play within evolution. He actually assumed that the IAC takes *place.* There are two ways in which Darwin contemplates the IAC when construing his own evolutionary theory. First of all, in connection with his doctrine of the gemmules in his 1868 book *The Variation of Animals and Plants under Domestication*. In this work, Darwin presented a theory of heredity entirely based on the idea that certain characteristics, acquired across the lifespan of individual organisms, can later on be transmitted to the next generations of individuals, thus allowing for the observed variations in organic traits to hold. Individual gemmules are in this sense conceived of as conveying the internal mechanism that makes this process possible.

The second way in which Charles Darwin took the IAC into account leads us to consider his conception of the behavior of organisms. In *The Expression of Emotions in Man and Animals* (1872), Darwin was very vocal in exploring the avenue of the transformation of habits into instincts as one of the most salient channels giving rise to the evolutionary transformation of animals. This is largely why this book has often been viewed (Lorenz 1965; Richards, 1987) as a seminal piece regarding to the elucidation of how disciplines as ethology and comparative psychology may play in our understanding of evolution.

There is wide agreement to the effect that A. Weismann helps clarify things in this respect. The story goes that Weismann's contribution to the establishment of evolutionary theory rules out the IAC both empirically and conceptually. As a result of his combined experimental and theoretical attacks on the IAC, NS emerges as the only game in town in a new *panselectionist account of evolution* (Gould, 2002) which, in due time, would prove bound to lead to a *Panglossian Paradigm* in biology (Gould and Lewontin,

1979). While the perils of such one-sided framework to think of evolutionary change have been emphatically abhorred by people like Lewontin or S. J Gould, others (i.e., Dawkins, 1976; Mayr, 1982 to name but a couple) both in the territories of biology and philosophy of biology, not to mention other more contended areas of inquiry such as sociobiology or evolutionary psychology, tend to embrace happily the *adaptationist programme* and its consequences as the only sound way to frame *Darwin's dangerous idea* (Dennet, 1995). Whatever the case, something seems clear enough: while, as we have seen, Darwin demonstrated a rather pluralistic sentiment towards the idea of NS being the *main but not the exclusive mechanism* in evolution, the Modern Synthesis seems in contrasts to rest upon NS as the all-important mean of modification, perhaps with the non–negligible exception of genetic drift. It is easy to see that this historical development in the history of biology may be viewed as representing, as it were, a vindication of the notion of the omnipotence of NS as proposed by Weismann (1893a). A thesis many would object to nowadays on a diversity of grounds with some contemporary biologist and philosophers of science even vindicating the theoretical work of J. B. Lamarck (Gissis and Jablonka, 2011).

3. THE ROLE OF THE PRINCIPLE OF SIMPLICITY IN WEISMANN'S FIRST ARGUMENT AGAINST THE *IAC*: IS THE TRANSMISSION OF ACQUIRED CHARACTERISTICS EMPIRICALLY EMPTY?

My project here is not to choose sides in this contemporary debate, but rather to discuss the role of the principle of simplicity to what Weismann had to say in favor of the all-mightiness of NS as an evolutionary mechanism. I shall start off by considering one argument in which parsimony simply *does not* play a part. Let us not make mistake in here: I will take this first line of reasoning into account for the sake of completeness but it is not my intention to claim that it constitutes an argument based on parsimony. On the contrary, the point will be made that in contrast with the other arguments that do use simplicity, this first line of reasoning doesn't rest on the comparison of degrees of parsimony between different causal accounts of evolution. Consider the following formulation by Weismann in *On Heredity* (1883):

> The hereditary transmission of acquired characteristics remains an intelligible hypothesis, which is only deduced from the facts, which it attempts to explain (pp. 82-83).

It needs to be noticed that given its succinctness, this brief statement may hardly be taken as a well-developed argument. In any event, let us note that concise as he may be, August Weismann is not making the point here

that *the IAC hypothesis is false*. Rather, what this quite sketchy declaration seems to claim is that the transmission of acquired characteristics is *empirically empty*—almost as a circular way of reasoning about certain organic traits. If true, this would entail that the hypothesis in question lacks content. Consider however that this point does not amount to say that the IAC is false. On the contrary, if Weismann's position were right, the hypothesis would fail to be testable and so would be devoid of any truth-value whatsoever.

In a way, this attack on the IAC mechanism parallels the critique of group selection (GS), by G.C. Williams in *Adaptation and Natural Selection* (1966). Contrary to what has been assumed, William's position does not primarily imply that GS is a factually false hypothesis. What Williams argues for is something rather more radical than such empirical denial of the occurrence of GS; he claims that GS is a conceptually flawed notion that really fails to capture the logic of the idea of evolution. Similarly, Weismann here seems to be conceiving the notion of the IAC as a sort of a logical mistake. A circular way of explaining the organic changes in the course of evolution which, due to the conceptual vacuum in which it proceeds, does explain nothing of empirical significance insofar as it is deduced solely from the very *explananda* it targets. Should this attack be sound then the problem with the IAC would seemingly lie in its *ad hocness*.

So interpreted, I find this conclusion extremely unpersuasive, yet instead of examining the reasons for thinking that the argument lacks force, there is something else I would like to reflect upon. For the purposes of this paper, it is worth granting Weismann's contention about emptiness and then examine what connection or lack of connection the argument bears to the principle of parsimony. The answer is a negative one in that the principle of parsimony does not have a role to play within this first attack. In other words, Weismann *does* not say that Lamarckian explanations are unintelligible *because* they are too complicated in comparison to those based solely on NS. And it is for good reasons that this concise line of thought shows no commitment to such an assessment of competing hypotheses on grounds of parsimony. Simply put, if the IAC goes wrong on the ground of its empirical emptiness, then it just cannot be simpler or more complex than any other conceivable hypothesis at hand.

The upshot of the analysis resonates with clarity here. The charges of over-complexity and conceptual futility must be distinguished with scrupulosity when thinking of what this attack on the IAC entails. It is one thing to say that one mechanism is too complex to be accorded scientific preference, and it is another to show such preference for hypotheses to which empirical meaning can be attached. Make no mistake: both rules make perfect epistemic sense when used in separate scenarios pertaining

to scientific evaluation. However, while both provide sensible reasons to prefer one theory to another, it is a mistake to merge them into one.

4. WHAT WOULD WEISMANN'S SECOND LINE OF ARGUMENT SHOW?

There is a second argument against the IAC to be found in Weismann's work. Here is how Weismann presents it in *On Heredity:*

> It is perfectly right to defer an explanation, and to hesitate before we declare a supposed phenomenon to be impossible because we are unable to refer it to any of the known forces. No one can believe that we are acquainted with all the forces of nature. But on the other hand, we must use the greatest caution in dealing with unknown forces, and clear and indubitable facts must be brought forward to prove that the supposed phenomena have a real existence, and that their acceptance is unavoidable.
>
> It has never been proved that acquired characters are transmitted, and it has never been demonstrated that without the aid of such transmission, the evolution of the organic world becomes unintelligible (pp. 80-81).

Notice that this argument is different in character. Firstly, instead of considering the logical flaws involved in thinking of organic changes in terms of the IAC hypothesis, Weismann suggests that the transmission of acquired characters does not hold empirically in a sufficiently compelling manner. It is interesting to see also that this conclusion relevantly connects with what Weismann's own experimental work in biology suggests elsewhere (1888, 1893b). Such work implies that the expected consequences of the IAC fail to be obtain under certain conditions. In contrast with that experimental observation, Weismann is making a further point in here. The point is not that the IAC but not NS is altogether impertinent empirically. After all, there is a sense in which both possibilities do overlap as the IAC, if true, would indeed explain the heritability of adaptive traits at least as completely as NS. Yet the point remains that the phenomena the IAC purports to explain can be completely accounted for *by* NS *and* NS *alone.* In such conditions and considering the experiments *in crucis* conducted by Weismann on the transmission of mutilations among organisms, the conclusion seems to follow that *one then should stay with the most parsimonious (i.e., well understood) hypothesis and avoid invoking a new force factor when the known ones do well.*

It is noteworthy, too, what Weismann's line of thought does *not imply*. Empirical as the present argument is in nature, *it does not mean to entail that the* IAC *hypothesis is unintelligible*. Rather, Weismann assumes now that the IAC is empirically significant even if it might be likely false as proved by his own experiments. Moreover, the argument does not suggest either that the IAC cannot ever hold; on the contrary, Weismann here explicitly

acknowledges that we cannot affirm in a conclusive fashion that the transmission of acquired characteristics on to the next generations of organisms does not ever happen in the course of evolutionary change. It is illustrative to notice that Weismann (1888) does not go overboard in interpreting the implications of his experimental results:

> What do these experiments prove? Do they disprove once and for all the opinion that mutilation can be transmitted? Certainly not, when taken alone. If this conclusion were drawn from these experiments alone and without considering other facts, it might be rightly objected that the number of generations had been far too small. It might be urged that it was probable that the hereditary effects of mutilations would only appear after a greater number of generations had elapsed (p. 443).

Nevertheless, so the argument goes, while for this roughly *Duhem-Quinean* reason one cannot possibly dispense with this eventuality wholesale, the evidence at hand gives no indication so far that this type of transmission really takes place. It is interesting to see that Weismann himself calls for caution on this matter, and rightly so. Perhaps one should always recall the old epistemological motto "absence of evidence is not evidence of absence" (but see Sober, 2008, for discussion). Maybe so. But even with that pinch of methodological modesty in mind, the moral of the story resonates limpidly and aloud here: it is better to be advised to give preference to the known hypothesis when there is no evidence for unknown factors playing a part to bring about the *explananda.*

Now, does this line of reasoning rest on parsimony? At least there is one obvious way to reconstruct it by using simplicity. It is important to note that different from Weismann's account of evolution, Darwin had wanted NS and the IAC to operate in conjunction as two distinct albeit intertwined causal forces driving evolutionary change. Weismann does not explicitly refer to Darwin's position, but his comments show clearly that he has in mind the following: instead of employing such inflation of factors in the *explanans*, it is simpler to keep the account relatively more frugal and refer to NS alone *when ceteris paribus, there is no compelling evidence to the effect that the IAC also obtains*. Simplicity here means restriction in invoking a plurality of causal factors when one suffices to do the explanatory work. If one wants to formulate this conclusion by invoking Ockham's razor(s), it is the *razor of silence and not* the *razor of denial* (Sober 2015) that Weismann endorses now. Rather than denying the existence, let alone the intelligibility of the IAC, what this version of the razor advices is *agnosticism* about something there is no evidence for.

This is a way of reasoning that A. Weismann has also applied elsewhere. One instance of the above is provided by his discussion of the possible effects of a *vital force* over phyletic variations in *Phyletic Parallelism in Meta-*

morphic Species (1882). There is no doubt that the two arguments considered here are different in nature. Precisely as hypotheses about the IAC and the *élan vital* present diverse implications, so, too, do the arguments against them differ accordingly. However, there are two things they have in common: in both cases, Weismann is, on the one hand, parsimoniously arguing for the avoidance of superfluous causal factors while refraining, on the other, from making the significantly stronger contention that such factors can be existentially discarded in principle. It is this symmetry that I want to alert to the reader's attention.

Weismann formulates his issues with the action of such *élan vital* by stating the following:

> (…) To me a vital force appears inadmissible, not only because we cannot understand the phenomena by its aid, but above all because it is superfluous for their explanation. In accordance with general principles the assumption of an unknown force can, however, only be made when it is indispensable to the comprehension of the phenomena (p. 464).

Again, let us not make the mistake of conflating *silence with denial*. Very much as *agnosticism is not to be mistaken for atheism* in that agnostics don't want to say that there is no God, Weismann pauses short of saying that the evidence available suffices to declare the inexistence of any such vital force. What he is interested in showing is something wholly different and way more modest than that. His point here is that *all evidential things being* equal NS is self-sufficient a principle when it comes to explaining the phenomena.

As noted, nothing of this supports the contention that the *élan vital*, old fashioned as it is, cannot possibly act as a yet unknown supplement of the influence of NS. This may sound as explanatorily sterile. Perhaps it is, but that epistemic point hardly proves that the teleology of the vital forces needs to be false. After all, *there might be more things in biology than dreamt of in* NS. Is there a way to turn the use of the razor of silence into the stronger razor of denial? In replying to Edward von Hartmann's remarks on the mechanical character of the principle of NS, Weismann exhibits a reason to *go atheistic* about the vital forces as causes of evolution:

> I am of the opinion that one effect can have but one sufficient cause; if this suffices to produce it, no second cause is required. The hand of a watch necessarily turns once round in a circle in a given time as soon as the spring which sets the mechanism in movement is wound up; in a unwound watch a skillful finger can perhaps give the same movement to the hand, but it is impossible that the latter can receive both from the operator and from the spring *at the same time, the same motion* as that which it would receive through either of these two powers *alone*. In the same manner it appears to me that the variations

> which lead to transformation cannot be at the same time determined by physical and by metaphysical causes, but must depend upon either one or the other (1882, p. 707).

However, this mode of arguing depends on the very disputable contention that two different causal principles cannot operate in conjunction to produce a singular effect. I fear that here's an assumption in urgent need of independent justification before we can safely conclude that the case for *denial* has been made compellingly. It is customary for scientists working in such disciplines as epidemiology, sociology, economics, meteorology or psychology to use multicausal models that appear to run against this form of conservativeness and so, unless more is said in defense of the impossibility of multi-causality it seems hard to see why things should be principally different in evolutionary biology. Actually, it is worth considering that present-days studies in the Extended Synthesis show how abundantly multi-causality is the rule rather than the exception in biology, also.

It may be objected that even though there is nothing wrong with scientist using multicausal models in those contexts in which a plurality of factors are explanatorily required, when a phenomenon is *sufficiently* explained by using a single factor, such *sufficiency* makes it superfluous to contemplate any supplementary assumptions. I agree that this sort of parsimony is entirely reasonable epistemically, but even if that consideration gives a reason for restriction in assessing what is known to exist, it alone leaves unsettled the ontological question about what might exist out there. That is the question we should concern ourselves with if the point is to replace *silence with denial,* yet it is one that we couldn't hope to settle in the absence of an argument for thinking that nature *abhors more when less will serve*.

Weismann doesn't formulate any such argument but that hardly means there is none. In his *Principles of Natural Philosophy,* Newton advances an ontological reason why the mono-causal assumption should obtain: indeed, if *nature is pleased with simplicity and affects not the pomp of superfluous causes,* then it feels just plausible to concede that *we are to admit no more causes than such as are both true and sufficient to explain the appearances*. Regrettably, there is a missing link in here. The *non sequitur* is made apparent if one pauses to consider why to grant that *nature is so pleased*. I suspect that Newton had an answer to this query. Taking his undoubtful theistic background into account, Newton would like to defend the ontological idea that nature is simple because *God wanted it to be so.* Unfortunately, I don't see how this assumption is acceptable and so I believe that in the absence of a circumspect justification of the premise that co-causality is principally impossible, Weismann fails to make this point cogently.

Let us be clear in this connection. I don't mean to say that Weismann is wrong in advocating *silence* about multiple causes insofar as the evidence doesn't suffice to establish them. I don't intend to sanction speculation in science but rather to make the point that there is no cogent argument entailing that *denial* should follow solely from these very reasonable evidentialist reservations. As I have shown above Weismann's epistemological sophistication makes him aware that his arguments both against the IAC and the *élan vital* as causal forces in evolution don't provide grounds for conclusively discarding them. Let us grant something here: he is totally right in that respect.

5. ARE GEMMULES TOO COMPLICATED? AND WHAT DID DARWIN HAVE TO RESPOND TO THAT?

In any event, back to the question of the IAC hypothesis, Weismann is vocal about both his reluctance to go beyond silence and the futility of contemplating such an alternative to NS. This is what he asserts in *The Supposed Transmission of Mutilations:*

> *Although this cannot be strictly proved* (i.e., that the transmission of acquired characters is impossible as a matter of principle), it can nevertheless been shown that the apparatus presupposed by this transmission must be so immensely complex, nay!, so altogether inconceivable, that we are quite justified in doubting the possibility of its existence as long as there are no facts which prove that it must be present (p. 437).

The reason I have italized the first sentence of the paragraph is because it makes clear what Weismann is thinking. At any rate, though, I think that there is a third argument on the IAC to be identified in this paragraph. Similar with the above-analyzed mode of reasoning, the present attack doesn't entail the empirical emptiness of the IAC. Also, much like the second argument, this one rests on the use of the principle of simplicity. However, the interpretation of simplicity is slightly different now as will be shown.

One way in which the paragraph may be interpreted is as if it were assessing the contrastive degrees of complexity of two theoretical accounts (one of which goes unmentioned but is implicitly taken into consideration) with reference to the internal theoretical mechanisms each would imply if true. The target Weismann has in mind by means of such a comparation of degrees of simplicity is not so much the IAC in general, but the specific version of it offered by the Darwinian theory of the gemmules on the one hand, and his own doctrine of the separation between the germline and the soma on the other (Weismann, 1893b). In other words, the argument now turns to consider what mechanism the theory of *pangenesis* would involve for it to obtain in nature, in contrast with the theoretical

requirements of strict separation between the germ and the somatic cells. The conclusion in this connection goes without saying: the doctrine of the gemmules is far too complicated internally when compared to its relatively simpler alternative to command scientific acquiescence, at least in the absence of relevant facts backing complexity over frugality.

Admittedly, Weismann does not say exactly what is complicated about the inner mechanism of the theory of pangenesis. Nonetheless, an obvious answer points out to the fact that while the theory of the gemmules needs to posit germinal plastitudes being produced in the organs of an individual organism during all the stages of its development and then carried to the gammetes by the bloodstream, the germ plasm theory does not make comparable theoretical assumptions. Now, the idea here is that whereas these postulations about the inner mechanism of heredity are required if the theory of the gemmules is to accommodate the IAC, they however are not only too numerous; they are furthermore largely *ad-hoc*. In contrast, Weismann's theory of the separation of the germ-line and the soma would be more parsimonious if simplicity here is taken to indicate a reduction in *the number and the ad-hocness* of the auxiliary hypothesis involved in a theory. By way of comparison, an example from the history of the scientific revolution may illustrate what Weismann has in mind. Many historians and philosophers of science would concur with the familiar observation that the Ptolemaic system of the universe in astronomy was overly complex relative to the Copernican model by virtue of the ample array of *ad hoc* assumptions which the former but not the later needed in order to describe the planetary motions accurately. If I am not mistaken, Weismann wants to present a parallel charge of artificial over-complexity against the gemmules.

One may wonder what a possible line of defense of the doctrine of the gemmules would be like. Fortunately enough for the curious, Darwin himself provides one at the end of *The Variation of Animals and Plants under Domestication* (1868). When confronted with a similar line of objection, Darwin does not respond by obscuring the complexities of his theory, instead he insists on a different—and much stronger—point: if the doctrine of the gemmules is complicated, so, too, are the facts which that theory is meant to cover*: "The Hypothesis of Pangenesis, as applied to the great class of facts just discussed, no doubt is extremely complex, but so assuredly are the facts"* (p. 238). In other words, what Darwin disputes is not so much that his theory makes a great number of theoretical assumptions; he rather acknowledges this and then goes on to deny their *ad hocness* in light of the abundant class of observed facts the theory intends to explain.

I believe that the point is significant, and I don't pretend to say that it proves Darwin's doctrine of the gemmules correct. The point Darwin is making is sound in a more general way: quite independently from the

doctrine of the gemmules, there needs to be room in our understanding of parsimony as a desirable virtue to accommodate the following qualification (echoed by Walter of Chatton's *principle of sufficiency*): *ceteris paribus*, it makes little sense to keep our scientific theories simpler than the facts themselves. Einstein had it exactly right when he (allegedly) said that things should be made as simple as possible but certainly *not simpler.*

Notice also that Darwin and Weismann do not disagree in how to use simplicity in theory evaluation. After all, both accept the implicit idea that simplicity must be qualified relevantly by adding a *ceteris paribus* clause making room for expanded theoretical apparatus to be considered when *simpler explanations are demonstrably unfit.* The discrepancy here seems to be one of emphasis coupled with a different interpretation of what the facts seem to suggest. While Darwin thinks the observed facts warrant a more complex theory in the sense of one with more auxiliary assumptions, Weismann doesn't agree with Darwin's largesse and sees no reason to multiply the theoretical assumptions beyond the use of NS. It is interesting to note in this regard that contemporary research in evolutionary biology shows ways in which Darwin's pluralist attitude seems to be vindicated against Weismann's stinginess.

6. CONCLUSION. WHAT TO MAKE OF WEISMANN'S ARGUMENT(S) IN THE LIGHT OF CURRENT RESEARCH ON EVOLUTION?

To conclude let us return to one of the *leit motive* of this paper. To downplay the prominence of the IAC and conversely highlight NS, Weismann deploys a mode of arguing which, somewhat paradoxically considering the weight it confers on simplicity, is luxuriously pluralistic in character. I have shown that his argument is threefold. It is tempting to think that there are obvious advantages to this way of proceeding. After all, if one of the sides of the argument falls, there will be others standing in good shape. Unfortunately though, there are also pitfalls to this compound of combined arguments, as made evident by the fact that not all its sides are entirely consistent with one another. Showing that the transmission of acquired traits incurs circularity would no doubt be a very good reason to reject its scientific acceptability once and for all. But before starting the cheering, you'd better pause to consider that if the IAC is empty, no assessment whatsoever of its simplicity or complexity would ever obtain. The other two arguments I have identified, however, stand in sharp contrast with this attack on the circular logic of the IAC hypothesis as they both tackle the lack of parsimony of the hypothesis and proceed to contend that the evidence does not provide empirical backing for it. This may be so, but if true would imply also that the hypothesis to be rejected on empirical grounds had an empirical signification to convey at the first place.

Finally, it is also noticeable that this empirical consideration, even if sound, leaves open the question whether the transmission of acquired characters takes place after all.

In a more contemporary note, I will finish with an epilogue pertaining to how Weismann's position connects with some presently debated topics in the field of evolutionary theory. This may look as a rather negative connection, however, in that Weismann's contention that NS is an all mighty principle doesn't seem to stand in good shape nowadays amid increasing developments challenging the panselectionist assumption on which the Synthetic Theory was framed.

Both Lamarckian inheritance and the theory of pangenesis earned a bad reputation in twentieth century biology. There are indeed very good reasons to think that they, alongside any other avatar of the IAC notion, constitute a bad idea scientifically. The experimental work of August Weismann makes crystal clear where they go astray. There is nonetheless a further bone to be picked here. Over the last forty years or so there has been a considerable wealth of research in very many areas of biology signaling that evolution may not be the one-dimensional process the proponents of the Modern Synthesis pictured it to be (Jablonka and Lamb, 2005). The contributions are way too numerous to be listed here with any exhaustivity (see: Avital and Jablonka, 2000; Odling-Smee, et al., 2003; West-Eberhad, 2003; Müller, 2014) as they do encompass both theoretical and experimental work in areas of endeavor so diverse as Evo-Devo, Eco-Devo, epigenetics, phenotypic plasticity, niche construction or the role of genetic accommodation in animal traditions by way of a highly interesting reinterpretation of the Baldwin effect. Varied as they are undoubtedly, they all coincide in pointing to the incompleteness of the Modern Synthesis (Eldredge, 1985) and the need to extend it in directions not contemplated by its architects (Piggliuci and Müller, 2010).

As I have shown elsewhere (Ongay, 2012), much of the fuss comes down to the relevant topic of the causal role of the active behavior of organisms in the evolutionary process. No doubt that J. B. Lamarck put much emphasis on the behavior of individuals as actively driving evolutionary change. Yet, no Neolamarckian worth her salt should see what is going on as a posthumous vindication of her pet theory of inheritance. Before hearing those Neolamarckians claiming "*I told you guys!*" in a self-confident voice, there is something else to consider: these developments don't entail a straightforward refutation of the tenets of the synthesis properly understood (but see Noble, 2017 for a contrasting interpretation), nor do they intimate the genetic transmission of the acquired characters by the use and disuse of the organic features, which, sad as this is considering the multi-faceted biological thinking of Jean Baptiste Lamarck, is just about all that there's to the whole idea of Lamarckian inheritance as it is generally

understood today. At this level, the supporter of biological orthodoxy can rest assured even if he, too, would do better to take notice of something: *empirically, there are conditions in which Weismann barrier seems to be trespassed.*

Make no mistake: just because the barrier might at times appear to be broken, it doesn't prove Weismann wrong conceptually. His position doesn't seem to be in trouble at least so long as he was wise enough to restrict his use of parsimony to the razor of silence. This is a prudent form of agnosticism that not everybody working in the Modern Synthesis has always consistently kept in line with. Though they should have! Here is a good reason not to abandon such modesty: hypotheses that cannot be seen as scientifically acceptable today may make a comeback tomorrow in the face of new evidence of the sort the proponents of the Extended Synthesis are so compellingly marshaling. Having said that, however, there's still a more general lesson to be learned from this, which should lead us to renounce any rhetorical invocation to the all mightiness of NS in evolutionary biology. On occasion (1893) Weismann indulged himself in those excesses even though they go well beyond what his arguments may validly show. The point I want to conclude with is one that, while flying in the face of Newton's passionate exhortations to keep things simple when addressing *Mother Nature,* alarms us to the fact that *things aren't always so simple.* There exists little doubt that *there's grandeur to this view of life* also. Perhaps the best way to capture such ontological *grandeur* would be to twist Newton's call for parsimony in the spirit of the sort of scientific pluralism advocated by John Drupré (1993) or Gustavo Bueno (2013) among several of others. In effect, *there are times at which nature looks rather misleading, as sometimes it seems to have a taste for complexity and the pomp of superfluous causes.*

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