# Author's Response: Is the Homeostat a Passive Machine? Is Life a Passive Phenomenon?

Stefano Franchi

**> Upshot** • The target article suggested that Ashby's device, the homeostat, embodies and illustrates a conception of life as a passive-contingent phenomenon. It advocated renewed experiments with updated and extended versions of his device that would allow us to understand better what passive-contingent life "would be like." In assessing the proposal, we should be particularly careful when dealing with the concept of "passivity," and we should not mistake the proposed theoretical exploration for a substantial metaphysical thesis about life in general.

## Introduction

« 1 » The target article drew a number of varied responses from the commentators. I have been positively surprised by the many different ways in which my main claims have been understood and I am glad to have a chance to articulate their content better.

« 2 » The different challenges the commentators raised can be seen as different views on the truth values of two basic claims that they take to be at the core of the target article. The claims are:

1 | *Ashby's homeostat* (through his reliance on generalized homeostasis) supports a view of life as passive, heteronomous, and contingent.

2 | *Life* is passive, heteronomous, and contingent.

« 3 » Most commentators see the target article as an argument in favor of both (1) and (2). Its conclusion would therefore be:

3 | We should adopt and extend Ross Ashby's generalized homeostasis and build better, updated, and more complex homeostats as models of passive, heteronomous, and contingent biological organisms.

« 4 » The challenges against this argument take (mostly) two forms. Some commentators (e.g., **Inman Harvey**, **Matthew Egbert**, **Takashi Ikegami**) deny that (1) is true. Ashby's homeostat, they contend, is not a passive machine. They also take (2) to be false, for life, they state, is fundamentally active and autonomous. Their conclusion is thus similar to mine, but for the opposite reasons: we should keep working in Ashby's tradition, and update and extend his intuition precisely because his work supports a view of life as a *non-passive* phenomenon.

« 5 » Other commentators (e.g., **Tom Froese** and, to a lesser extent, **Robert Lowe**) accept (1) as true: the homeostat is indeed a passive-contingent machine. However, they reject (2) and take life as essentially active and autonomous, thereby rejecting (3). Their conclusion is that we should abandon or at least seriously temper Ashby's theses when modeling biological life. Thus, the majority of commentators agree that life is active and autonomous and they only differ on whether Ashby's homeostat supports or undermines such claim. **David Vernon** supports both truth and falsity for claim (1), but he ascribes them to a substantial philosophical difference between the two editions of Ashby's major work. The first, 1952 edition would present a non-passive, autonomous homeostat, whereas the second, 1960 edition would indeed switch to a passive device.

« 6 » **Helge Malmgren**'s contribution is the only exception to the consensus. He agrees that the homeostat is a passive machine and even argues that Ashby viewed not only life in general but his own life as well as essentially passive. He then connects Ashby's view of passivity with the views of other non-canonical cognitive scientists (as we would say today): Gustav Theodor Fechner and Sigmund Freud.

« 7 » The main challenge and major source of disagreements between the commentators is then the truth value of (1) – "Is the homeostat a passive, heteronomous, and contingent machine?" – and only to a minor extent about the truth of (2) – "Is life a passive, heteronomous, and contingent phenomenon?" – where an almost unanimous consensus exists. In what follows, I will address these two issues in order, before addressing **Malmgren**'s point.

## Is the homeostat a passive machine?

« 8 » The original claim I put forward in the target article was that the homeostat's relationship to its environment is characterized by three non-equivalent although interrelated dimensions: passivity, heteronomy, and contingency. The statement under discussion would thus have to be decomposed into three separate sub-claims supplemented by an additional discussion of their relationship. I cannot carry out a task of this magnitude here: each of the three concepts under discussion would require an extensive analysis drawing its evidence from current uses in everyday language, from the concepts' history in the philosophical literature, and from their current and past deployments in scientific research. However, the full three-fold treatment, while necessary in a full-blown discussion of the homeostat, is not required in the present context, since the commentators' attention is mainly focused on the first of the three dimensions: passivity. This is not surprising, as the association between passivity and life in ordinary as well as in technical language tends to draw immediate negative connotations. Passivity, in other words, always comes with an associated value-judgment about its bearer. Unsurprisingly, the commentators who accept the homeostat's passivity (**Froese**, **Lowe**, **Vernon$_2$**) immediately cast doubts on its usefulness, while those who defend Ashby's view reject passivity altogether (**Harvey**, **Egbert**, **Ikegami**, **Vernon$_1$**). Who is right, then? Is the homeostat a passive device?

« 9 » The answer would be simple if we had a straightforward and widely-shared understanding of passivity. This is far from being the case. There is a somewhat minimal conception of passivity, which is best expressed by the grammatical passive voice. A subject-verb-direct object (English) construction – "I kicked the rock" – can always be turned into its corresponding passive voice version – "The rock was kicked by me" – because the verb-direct object pair implies the existence of an action whose effects are transferred upon the object. "Minimal" passivity would thus be equivalent to being on the receiving end of someone or something else's action. Call this notion $p_1$. The problem is that $p_1$ falls short of capturing the meaning(s) of our everyday uses of the term "passive." When we identify a person as being "passive," we usually mean more. Typically, we intend to convey a more complex meaning that involves at least the following components: ($p_1$) the person is on the receiving end of some action or event; ($p_2$) the person is capable of being affected by that action, in the sense that some important aspects of her being or behavior will be modified as a consequence; ($p_3$) those changes will be detrimental to the person's well-being and change her situation for the worse. Consider, for instance, the common expression "X is passively accepting her fate." To a greater or lesser extent, all aspects of passivity, from $p_1$ to $p_3$, must be present to make sense of it: some external event is acting upon the person ($p_1$), who is consequently and substantially affected by it ($p_2$), while she would be better off otherwise ($p_3$). Thus passivity implies at least a "capacity to be affected" or a capacity to suffer, often against the sufferer's optimal welfare state. This is why, while we could apply the minimal conception of passivity ($p_1$) when speaking of inanimate objects – as I did with my first example – we seldom speak of rocks and sticks as being "passive." We are straining the English language if we say that "The ball was passively kicked around." We are not when we say that the "The patient was passively following orders." In short, passivity requires more than being on the receiving end of someone else's action. It may even require, as in the previous examples, the capacity to carry out actions that are instigated by an external event (or a series of commands, in this case).

« 10 » There is more. Sometimes, we apply the term to inanimate objects, especially in technical vocabularies. For instance, we often speak of "passive receptors" or of "passive sensors." Here again, we mean something more than being merely on the receiving end: a standard light sensor is a passive receptor because it is capable of being affected by the photons impinging on its surface while not expending any energy. This is consistent with one of the possible origins of the word "passivity," which is usually traced back to the Latin form *passiuitas*, a word possibly linked to the verb *pati*, "to suffer," "to be affected," and possibly a cognate of ancient Greek *paskhein*, which had a similar meaning. In short, passivity is essentially related to the capacity to be affected and be pushed to act – often against the patient's best interests. Let us call this semantic aspect of passivity $p_4$ and provisionally conclude that a truly minimal conception of passivity capable of accounting for the most common uses of the term in ordinary language, and especially when referring to human beings, includes all four mentioned aspects, $p_1$–$p_4$.

« 11 » But passivity is more complex than that. Consider the common expressions "passive resistance" and "passive-aggressive." Even this revised minimal concept of passivity cannot provide a satisfactory account of these occurrences. The semantic content of "passive resistance" is actually much closer to "impassive" than to "passive": a person actively engaged in an act of passive resistance would achieve ultimate success by being truly impassive to the solicitation and teasing exerted on her from the outside: she would be "unaffectable." Call this new aspect of passivity $p_5$. Similarly, the passive-aggressive patient is unwilling to accept those modifications to her being or to her behavior that would normally be produced by external actions (I am referring here to the actual pathological state, whose description we can find, for instance, in the official diagnostic manuals of the American Psychiatric Association 2000). By closing up the normal receptivity to outside influence, she avoids following orders. She is apathetic, in the technical Stoic sense of the word: unable to be affected. Passive-aggressiveness may be a new semantic trait of passivity – call it $p_6$ – or perhaps it is an exaggerated manifestation of the previous component. It may even be an improperly named concept, in spite of some vague relationship to the "ultra-minimal" $p_1$ conception of passivity.

« 12 » Finally, let me add one more etymological piece of evidence to this brief sketch of the complex semantic landscape engulfing a seemingly ordinary concept. The first recorded uses of the Latin term *passiuitas* come from Tertullian, the early 3rd century Christian apologist. His use of the term is seemingly so different from our current linguistic habits that some scholars have been forced to posit the existence of two homonym words, only one of which survived. For Tertullian, *passiuitas* means "sexual promiscuity," "promiscuous intercourse," or, more generally "turbulence," "excessive opening to the outside."[4] Tertullian takes to an extreme the semantic trait $p_2$. He turns the subject's capacity to be affected into an essential and indeed excessive opening to the outside, both literally and, by extension, metaphysically. This is clearly the reading that allows Tertullian to use *passiuitas* in the sense of promiscuity, with explicit reference to sexual commerce. The more "passive" an organism is, the less "proper" it becomes ("at the mercy of everyone" [*pro arbitrio cuiusque*], in Tertullian's words). This is apparent in the sexual domain, but the expression should be intended in the most general sense, as indeed does Tertullian. When passivity comes to mean openness, its meaning shifts toward ex-propriation and loss of property (i.e., loss of what is "proper to" or "appropriate for" the subject both in the metaphysical and in the ethical sense). The passive recipient does not simply receive someone else's action (or worse, for Tertullian): it is so open to such reception that it enters into all kinds of improper, excessive, and ultimately immoral intercourse with the outside. The truly passive subject, in this reading, is essentially "promiscuous" and that means "*always ready* to being acted upon." This form of passivity, for which I will expend my last label, $p_7$, has some interesting characteristics. In the present context, the most important one is the explicit association between the excessive openness to the outside and the "turbulent" activity typical of "lust," to use Tertullian's terms. In his anti-pagan invective, he implies that the truly promiscuous subject is constantly seeking intercourse and therefore constantly engaged in *proximate* actions that make it ready for such. Their *ultimate* (rather, *distal*) goal, however, is to receive someone else, hence their passive character.

« 13 » There are even more aspects I should bring in to characterize passivity

4| In the *Apologeticus*, Tertullian says: "Age iam recogitate, quam liceat erroribus ad incesta miscenda, suppeditante materias *passivitate luxuriae*," which could be translated as: "Now just reflect how many opportunities for errors toward incestuous mixings are made possible by the *passivity of your lust*" (1954: 104); similarly, in a metaphysical treatise, he asserts "Haec inquies non est, – haec *turbulentia et passivitas* non est; sed moderatio et modestia" (literally: "This is not disquiet, this is not *turbulence and passivity*, but rather moderation and modesty," 1954: 431, my emphases throughout).

properly, but I think this brief sketch should suffice to give an idea of the complexity of the concept while providing elements for an informed answer to the original question: "Is the homeostat a passive machine?" Harvey and Egbert argue that it is not, because Ashby's device is constantly engaged in some action in order to keep its relationship to the environment stable (i.e., to achieve homeostasis). Harvey offers a couple of examples: the (possibly mechanical and homeostatically driven) bicyclist who wobbles down the road and constantly changes the bicycle's relationship to the environment in order to keep its angle from the vertical constant; the starving 19th century Irish peasant who emigrates to America to escape the potato famine. Egbert adds a structurally similar third example: the continuous actions and subtle adjustments that are required to keep a pole vertically balanced on one's fingertips. In all cases, they claim, the agent is not passive, because there are actions being carried out (operating the handlebars, emigrating, moving one's hand and arm). These would be effective counterexamples only if we restricted passivity to its first, $p_1$ component. In that case, I would agree that the homeostatically controlled bicycle, the Irish farmer, and the pole-balancing hand are not passive. They are different from rocks being kicked around. As I argued, though, $p_1$ (i.e., "being acted upon") is a severely incomplete characterization of passivity. Though a necessary condition, it is far from being sufficient, to the point that we never use it for rocks and sticks, the only beings to which it could be applied. If we extend passivity to all four conditions $p_1$–$p_4$, therefore including the capacity to be affected and the related capacity to act in response to actions initiated from an external agency, the counterexamples are no longer such. In all three instances, the original source of action that prompted the recipient's adjustments is external to it: the original lack of equilibrium between the bicycle and the road, the potato's disease, the oscillation of the pole around its center of gravity. In all instances the original, external sources of action disturb the equilibrium between external agent and recipient and, due to the recipient's receptivity to that disturbance, force it to undertake restorative actions. The important point is that *the recipient's actions stop as soon as equilibrium is restored.* Such a complete stop is not always possible. It certainly is for the hypothetical Irish farmer envisioned by Harvey. We can reasonably speculate he will stop changing places of residence as soon as he can farm again in his new dwelling. Sometimes restorative actions on the recipient's part cannot stop because the external actions do not stop either. This is the case of the pole on the fingertips example, whose essentially unstable physical equilibrium compels the recipient to undertake a never-ending series of restorative counter-actions which *would* stop if the equilibrium *were* restored.

« 14 » My conclusion is that an adequate reading of passivity forces us to conclude that the behaviors of the cyclist, the farmer, and the hands are indeed passive, for the goal of the (internal) actions is to eliminate the effects of the (external*)* actions. This is a point that Hans Jonas had seen very well. In the *Imperative of Responsibility*, he provided a detailed comment of the cybernetics explanation of a cat's behavior as following a sequence of steps such as:

> "physiological stress (homeostatic 'gradient'); internal secretion and nerve stimulation; readiness for selective triggering of a behavioral pattern; external (sensory) nerve stimulus triggering the appropriate behavior pattern; sensorimotor feedback guiding the course of behavior; subject of action eliminating the initial stress (homeostatic equilibrium), this itself being the 'goal'." (Jonas 1984: 62)

« 15 » Jonas was keen to point out that such a "homeostatic goal" is really no goal at all and concludes that the desired good would just be the "*subjective representation* of the state of indifference or quiet that waits at the end" (ibid., emphasis added). This is why the cybernetic explanation of purposive behavior, according to him, really turns all animals into passive machines whose only real goal is the ultimate end, or death.[5] I think Jonas's characterization of homeostatic equilibrium as an explanatory principle is ultimately correct, although I do not share the conclusions about cybernetics he draws from it. The homeostat is a passive machine because its ultimate goal – but none of its proximate ones – is a lack of action. All the actions it takes are efforts, no matter how futile, to eliminate the externally originated action that prompted it to act in the first place. This is also why the target article recalled William Grey Walter's joking characterization of Ashby's device as a *machina sopora*. I certainly did not mean to say that the homeostat *is* always asleep as Harvey, and perhaps Grey, thought. That would be obviously false, for only a rock would achieve such permanent slumber. Yet, the homeostat's search for equilibrium implies that "sleep" (lack of action, or, at least, minimal action) *would be* the ultimate, although often unattainable goal.

« 16 » Ikegami, I think, makes a somewhat similar point to Harvey's and Egbert's. I admit I am not sure I fully understood the relationship he suggests exists between the behavior of oil droplets in an aqueous pH gradient and the homeostat on the one hand, and DMN networks and ultrastability on the other. I believe his overall argument is meant to extend his previous suggestion (for instance in Ikegami & Suzuki 2008) about the necessity for a living system to change its viability parameters dynamically in order to cope with its necessary, constant, and autonomous movement. In 2008, Ikegami called such a conservation of homeostasis under constantly varying conditions "homeodynamics." The concept introduced the idea that a living homeostatic organism must constantly move around in order "to distinguish likes from dislikes and prey from predators" (Ikegami & Suzuki 2008: 398). It thus needs a baseline level of activity that is concurrent with and related to its equilibrium-seeking homeostatic processes. The "default mode network (DMN)" in the brain translates this homeodynamic architecture to the nervous system/external environment setting. When Ikegami asserts, referring to neuroscientific research as well as to his own work, that a brain is constantly expending

5| It may be worth noting that this is the same conclusion that was reached a few decades earlier by Sigmund Freud when, in *Beyond the Pleasure Principle*, he took the principle of pleasure – which is essentially a homeostatic principle – to its ultimate consequences and posits the existence of a "death drive" – the urge to return to an inanimate state (1964). Of course, Freud did not share Jonas's negative assessment of the theoretical validity of this unpalatable consequence.

energy in order to "maintain memorization as well as reaction to and even prediction of environmental images," he is providing a brain-level equivalent of the autonomous exploratory movement of the organism he had previously described under the rubric of homeodynamics. The overall point is thus that the living organism needs a baseline level of activity that may be an integral and under-appreciated part of a generalized homeostatic system or, perhaps, an additional and to all extents logically prior subsystem that the homeostatic component needs to work with. In either case, the argument would go, the alleged passivity of the homeostat fails to account for this underlying activity level and must be considered a reductive characterization.

« 17 » On the contrary, I believe that the possible existence of such a base level is very consistent with the description of passivity I gave above if we only remember that a recipient's openness to the outside (its "capacity to be affected" or $p_2$), and, even more so, its passive promiscuity – with the inherent search for a "commerce with the environment" (my $p_7$ component) – are important traits of passive behavior. Briefly put, the activity baseline can be considered an operational translation of a subject's capacity to be affected. The distinctive criterion that allows this baseline activity to be reclassified as a form of passivity is, once again, the distinction between the proximate and distal goals of the actions themselves. As Jonas stated (and, subsequently, Francisco Varela confirmed, but previously, albeit in different forms, Aristotle and Immanuel Kant had argued) the supreme goal of an organism's autonomous action is *itself*. It is the imperative to preserve its own bodily integrity against the constant challenges mounted by internal needs and external threats that pushes an organism to posit its own identity as an ever renewable form that needs to be kept constant and indeed posited as the ultimate goal directing every proximate action. On the contrary, the ultimate goal of the baseline activity **Ikegami** describes could not be more different from Jonas's: it is the *other*, or rather the readiness to interact with an external agent and to accept the effects of the actions that may originate in it. The contrast could not be starker, in my opinion.

## On the simulation of concepts

« 18 » On the basis of the previous discussion, I think we can affirm that the homeostat is indeed a passive device, provided we accept a notion of passivity that does not reduce it to a mere lack of action. Such a notion is forced upon us as soon as we examine its use. That being said, I do not mean to imply that we have a perfectly consistent notion of passivity at our disposal, nor that the homeostat's behavior represents a perfect instantiation of this ideal concept. In fact I think the opposite is the case: a close analysis of current linguistic uses (of which the above discussion presents only the barest sketch) and, even more importantly, a close reading of the philosophical works in which passivity is discussed (starting with Aristotle's works and especially *Categories*, *On Generation and Corruption*, and *De Anima*) shows that the concept is anything but a unified and coherent semantic whole.

« 19 » This is precisely why Ashby's work is particularly interesting. As the previous pages will undoubtedly have shown, my background and research is located within philosophy rather than in the sciences. I originally became interested in Ashby's work when trying to articulate a workable conception of passivity that would make sense of the many uses of this concept we find in 20th century European philosophy (starting from Husserl's conception of "passive synthesis" (2001) and continuing pretty much to the present day). Ashby's general homeostasis thesis became a theoretical lens for examining the concept of passivity in general, and the experimental instantiation Ashby devised (the homeostat) a practical tool to investigate its properties. The target article's introduction of the notion of "concept simulation" is meant to provide a general characterization of this procedure. Some commentators disputed the validity of the notion.

« 20 » **Malmgren** points out its linguistic faults and proposes an alternative formulation, equally based on Ashby's work. I will grant him that my term may lack stylistic elegance, and I am open to suggestions on that front. I do not think **Malmgren**'s suggested revision is different from my original proposal, however. He would like to replace "concept simulation" with a three step process (that he attributes to Ashby):

1 | revision of current concepts which are
2 | illustrated through
3 | the simulation of specific physical processes.

« 21 » The sequence is very similar to what I proposed, even though my discussion went in the reverse order. I claimed that Ashby used (3) the homeostat (2) to illustrate (1) a new view of life. Reversing the clauses' order, we obtain that: (1) a new view of life (**Malmgren**'s "revised concepts") (2) was illustrated (3) by a physical apparatus (the homeostat). My contention is that "concept simulation" is an accurate though possibly inelegant designation for clauses (1)–(3). I would like to stress again that (1)–(3) designate a concept simulation because the physical (or electro-mechanical, or, nowadays computational) processes used in step (3) bear no physical similarities whatsoever to the objects whose behavior is being investigated. Ashby never tired of stressing that the homeostat is not a physical analog of the brain or of the organism. Indeed, when Ashby designates a subset of the homeostat's units as the "agent" and the remainder as the "environment," he is not making any assumptions about what the "agent" and the "environment" may stand for. They may stand for the cat approaching the fire, or they may stand for one subsystem of a monkey's brain interacting with the muscular system (to quote two of Ashby's favorite examples in *Design for a Brain*). They may even refer to different collections of neurons interacting with each other, a hypothesis Ashby explicitly entertains when he comes to the discussion of multi-stable systems. The setup is very different in standard simulations used in physical contexts. A Boeing 777 flight-simulator does represent a real plane moving in physical space, and it is used to simulate the behavior of an actual object subject to real physical constraints (gravity, friction, torque, mass, and so on) when a human subject interacts with it. Similarly, a computer simulation of weather patterns simulates the temporal evolution of real spatio-temporal chunks – the fluids in the atmosphere. In contrast, "Agent" and "Environment" designate subsystems that may be individuated differently depending on the chosen perspective and have no built-in analog with the physical world. What do they designate then? And what does the homeostat's behavior simulate? My claim is that they designate a "concept." To be more precise (and even less elegant), I could say that the homeostat simulates the behavior of a system working according to a five-thesis conceptual framework about life, which I called "generalized homeostasis" (borrowing Ashby's own label). "Generalized homeostasis" is not a physical object, even less a psychological one. We certainly cannot pick it up and look under it, as we can do with planes and clouds. In my view, the homeostat is a physical object simulating the behavior of a theoretical one. As I said in the target article, this is not all the homeostat is, because the distinction between concept- and object-simulation individuates the ends of a spectrum of possible experimental options and each specific simulating device used in actual scientific research will present aspects of both. In my opinion, though, the conceptual component has been historically overlooked by Ashby's scholars and followers.

« 22 » **Ikegami** offers a different objection. He observes that "conceptual insights come only after realizing a new artificial system and not the other way around" (§8). This sparse remark could be read in two different ways, each one implying a well-defined hierarchy among disciplines and both eliminating philosophy from the ranks of intellectual undertakings. First, the remark could mean that philosophical results ("conceptual insights") provide a conceptual articulation of what exists (or of what has been built). This seems to me a misguided interpretation that attributes to the philosopher the task of the scientist, whose job is precisely the production of a theoretical understanding of what there is (actually or ideally, in the case of purely formal disciplines). But perhaps **Ikegami** implied that "conceptual insights" refer precisely to the results produced by the theoretical scientist, in which case philosophy exits the picture altogether or is reduced to an epistemological reflection upon science-produced concepts. Even though this neo-Positivism inspired conception of the discipline never ceases to be popular, I think it is mistaken, at least as it applies to the concretely and historically determined existing philosophical practice. Philosophers have historically been engaged in the invention of concepts that allow us to think differently from the existing frameworks. In other words, philosophy – if I may be allowed a dogmatic tone that space limitations render inevitable – fabricates the concepts that will allow us to think of the structure, limitations, and consequences of the intellectual and social situations we happen to live in. I am well aware that I am offering this sweeping statement as a dogmatic pronouncement that is bound to encounter some resistance in my scientific-oriented audience. Yet this is not the place for a systematic treatment of such a large issue.[6] I can perhaps partially remedy this unsatisfactory situation by pointing out that the concept that the target article introduced in §16 was offered precisely as an example of a philosophical concept. The somewhat provocative question it asked – "What is it like to live a passive, fully homeostatic life?" – implied that such a life may not exist at all. That is why, in the language I used above, "passive life" is a "fabricated concept" that is meant to explore a viewpoint on life normally excluded from scientific conversation, as the unanimous rejection of the thesis by all the commentators (**Malmgren** excluded) confirms. In fact, the question entailed an even stronger thesis. Namely, that we will not be able to assess the truth value of a statement such as "life is essentially passive" – nor, by implication, its contradictory one: "life is essentially active" – until we understand what passive life means. With this clarification in mind, let us now move to a discussion of the thesis itself.

## Is life a passive phenomenon? Is it an active one?

« 23 » Recall the argument from which I started this response:

1 | *Ashby's homeostat* supports a view of life as passive, heteronomous, and contingent.
2 | *Life* is passive, heteronomous, and contingent.
3 | We should adopt and extend Ashby's generalized homeostasis and build better, updated, and more complex homeostats as models of passive, heteronomous, and contingent biological organisms.

« 24 » It should now be clear that (1)–(3) is not, in fact, the argument the target article intended to present for discussion and I can only be grateful to the commentators for offering me the chance to restate my point, and hopefully make it clearer. The difference lies in claims (2) and (3), which I would switch and rephrase as follows:

2' Simulations carried out on updated and extended homeostats would allow us a better grasp on what "passive life" would actually be.
3' Once we have a better grasp of "passive life," we could assess whether life actually is passive or active.

« 25 » It follows that I do not think we are presently in a position to determine the truth value of the contradictory propositions "Life is passive" and "Life is active," because we are not in possession of a solid concept of passivity, as I argued in the section "Is the homeostat a passive machine?" of this response.

« 26 » Almost all commentators disagree and offer different evidence in support of life's intrinsic activity. Indeed, **Harvey** takes the truth of the life/activity identity as obviously self-evident, to the point that he thinks my purpose is to "castigate" Ashby by attributing to him a patently false interpretation of life as passive. **Harvey** equates passivity with "stasis or quiescence in the organism itself" and denies that either the homeostat or life in general exhibit any such property. I have already explained above why I think we should reject this view of passivity as inadequate to the phenomenon, and why, therefore, the indubitable existence of proximate actions in living organisms' everyday behavior does not constitute, in my view, a valid objection. I addressed above **Ikegami**'s objection as well, and I will not elaborate further. Similarly to **Ikegami**, **Froese** states (§3) that life is essentially active since "organisms are intrinsically active and their behavior is non-contingent." The evidence in support of this claim is twofold. On the one hand, the difficulties encountered by evolutionary robotic models closely following the passive-contingent Ashbian paradigm (e.g., Iizuka & Di Paolo 2008) may be read as a sign of deeper theoretical issues. I would agree with **Froese** that

6| See Franchi (2005) for a discussion of the relationship between philosophy and science in the specific context of artificial intelligence and cognitive science.

the research he mentions has been only partially successful. However, as I have argued (briefly) in the target article as well as elsewhere (Franchi 2011b), evolutionary robotics models inspired by Ashby's work have stayed away from a full implementation of his generalized homeostatic thesis, let alone his more ambitious multi-stable systems. This does not mean that a fully Ashbian approach would necessarily overcome those difficulties, of course. But I do think it would be fair to consider the problem as an open empirical issue that only further work may help us understand better.

« 27 » On the other hand, Froese claims that this deeper issue has to do with the "traditional fixation on equilibrium dynamics" (§11) and suggests that living organisms are always far-from-equilibrium since an organism falling into equilibrium would lose its ability "to do the work of self-producing its own material identity," which would therefore be tantamount to dying. I am not convinced by this claim, and for two reasons. First, it seems to me that Froese confuses Ashbian equilibrium with its physical counterpart. Keeping essential variables at constant levels (e.g., blood's sugar concentration, to use Ashby's favorite example) will necessary imply a far-from-equilibrium exchange of matter and energy with the environment. I would agree that only non-living matter can be in physical equilibrium with the environment, but I fail to see why keeping internal variables constant would imply physical equilibrium. Second, and more importantly, I think stating that life must be essentially active because a passive organism (in the sense discussed above) would not be able to self-produce its own identity begs the question about life's essential character. Froese's mention of life's self-creation of identity epitomizes Jonas's view, which, as I mentioned in passing, assumes that life is based on self-positing activity, thereby reducing the evidence to the claim that life cannot be passive because otherwise it would not be active. Froese's final remark in §11 that subjects – differently from rocks – can change their behavior even in absence of a change in external conditions brings this point home. It is trivially true that a rock cannot stand up and walk away from the bedrock it is resting upon, while I can do just that (most of the times). But what is under discussion is precisely *why* I can do that, *how* I do it, and under *what* conditions. Saying that subjects can and rocks cannot because the former are active and the latter are passive does not answer any of those questions.

« 28 » I take Lowe's very detailed commentary as a direct answer to the series of questions I just raised and, partially, to the technical shortcomings Froese previously stressed. Lowe maintains that while Ashby's basic approach has shown to be very fruitful, it has turned out to be difficult to scale to multiple parallel tasks. His suggestion is that the key to further progress lies in relenting Ashby's requirement that ultrastability be achieved through random selection of control parameters. The resulting strategy, which he calls the "non-random self-configurable approach," would be able to incorporate a more sophisticated version of homeostasis (i.e., Sterling's 2004 allostasis) and allow more satisfactory modeling of those short-term non-adaptive behaviors that may result in long-term organismic gains. Lowe claims that this approach would account for the undeniable fact that biological agents cannot be passive-contingent in the sense stipulated by Ashby because they often need to operate outside their comfort zone. Lowe's proposal could thus be construed as a first answer to the question my statement (2') above implicitly asked: "What would a passive-contingent life be like?" It would be a life structurally unable to leave short-term comfort zones, would be the reply. While such a form of life may prove adequate for certain classes of organisms (a possibility Lowe does not seem to consider), it would seem to be inadequate as a general requirement.

« 29 » As a philosopher, I cannot avoid being struck by the similarity between the short-term perspective that would seem to follow from Ashby's required random selection of internal parameters and the traditional characterizations of vegetable (or, rather, vegetative) and animal life within the Western tradition (for example in Aristotle's descriptions of plant-like and animal-like forms of life in *On Generation and Corruption*, a description that became enshrined in the philosophical canon). Lowe's proposal about life *in general* would thus be a reformulation of the classic view of *human* life. Aristotle argued that human beings must have a plant-like soul responsive to the organism's internal drives (growth and reproduction); an animal-like soul responsible for the short-term emotion-based interactions with the outside world (fear being the basic emotion that determines an animal's relationship to its environment); and a rational soul that *directs* those lower levels by superimposing long(er)-term planning and ratiocination on life's basic functions. Lowe's hierarchy repeats this classic setup by translating the Aristotelian necessity of *logos* in human life into the necessity of a "mental autonomy [involving] a top-down regulatory influence on [plant-and animal-like] homeostasis" (§10). The analogy with classic philosophical conceptions raises a question that, I believe, remains open in Lowe's description. Namely: the Aristotelian distinction of three different kinds of life principle (or "souls" in the traditional translation) had also the goal of demarcating different regions within the biological universe. Is such a demarcation implicitly entailed by Lowe's non-random self-configurable approach to life? In other words, are all the modules the approach requires necessary for *all* forms of life, or are we confronted with a possibly fundamental biological distinction among different forms? Perhaps only further work in this direction will answer these questions.

« 30 » However, there is a possible alternative route toward a regained ontological uniformity in the biological domain. Lowe's commentary refers to my brief mention of Ashby's more sophisticated version of the homeostat, the Dispersive Adaptive Multistable Systems (DAMS). While it is a fact that Ashby failed to produce a satisfactorily working DAMS, let alone conduct biologically meaningful experiments with it, I do not think its overall architectural principles are necessarily doomed, as several scholars seem to imply (e.g., Pickering 2010, but also Husbands & Holland 2008). As Vernon rightfully stresses, Ashby's core interest was the concept of multistability, which is in fact the topic of the second half of *Design for a Brain* (in both editions). A multistable system is a collection of suitably connected ultrastable sub-systems (the details of this connection vary from the first to the second edition, the latter giving a more prominent role to the environment). In the target article, I pointed out Ashby's explicit claims, on purely theoretical grounds, that the shortcomings of simple generalized homeostatic systems **Lowe** and **Froese** underlined and that Ashby had to a certain extent anticipated (as **Vernon** correctly indicates in §20) would be avoided by a DAMS-like multistable system. This ambitious and very specific claim has not, as far as I know, been empirically tested. The perhaps excessively elliptic remark on which I concluded the target article referred precisely to this possibility: the construction of a (simulated) DAMS-like device would be the next step toward an exploration of passive-contingent life along Ashbian lines and is indeed one of the longer-term goals of the research program the target article introduced.

## On Gustav Theodor Fechner and Sigmund Freud

« 31 » **Malmgren** points out that even if Ashby's general homeostasis thesis – and its related connections to passivity, contingency, and heteronomy – may put him at odds with a substantial portion of the Western intellectual tradition, as I claimed in the target article, his work is very much consistent with the psychological tradition that starts with Gustav Fechner and goes through Sigmund Freud. I completely agree with him. Indeed, I have written on the topic myself, even though, admittedly, I did not dwell at length on the relationship between Ashby and Fechner and focused rather on that with Freud.[7] I do indeed agree with **Malmgren** that the connections between Ashby's theory and psychoanalysis are quite robust, and especially so if we look at Freud's early work (such as the posthumous *Project for a Scientific Psychology* 1964). It is true that, as far as I know, there are few documented instances of an intellectual exchange between the two. Nonetheless, at the personal level, it is similarly true that psychoanalysis attracted strong interest in the late 1940s and 1950s, and especially so in England, due to the influence of Anna Freud's London-based school. In my view, the theoretical connections between Ashby's theory of general homeostasis and Sigmund Freud's conceptualization of an organism's reaction to external stimuli are evident, and indeed were often noted in the 1950s.[8] An important difference, which I stressed in the paper cited above, is that Freud sometimes speaks as if he conceived of an almost ontological distinction between animal homeostasis, which almost always succeeds, and human homeostasis, which almost always fails.[9] This distinction was picked up by the Lacanian school, for which, indeed, human homeostasis is *always* failing, with the consequence that the human being is conceived as a structurally maladapted animal (whose ultimate roots are to be found in humans' neotenic character).[10] Nonetheless, I think that there is much work to be done to re-uncover a possibly cybernetic (or, rather, Ashbian) reinterpretation of psychoanalysis. My 2011a paper was an early step meant to further this line of inquiry. I find Catherine Malabou's recent work (2012), although very critical of psychoanalytic orthodoxy, as very much proceeding in the same direction. In my opinion, her notion of "plasticity" (1996) has important similarities to Ashby's view of homeostasis through ultrastability. But this is not the place to explore this point further.

« 32 » One learning and memory, I cannot but agree with **Malmgren** as well. Perhaps I did not express myself clearly enough. When I stressed (in §35 cited by Malgrem in his §4) that the homeostat lacks "explicit or implicit forms of memory," I meant to say that Ashby's view on learning were in explicit contradiction with theories of explicit learning and memory that see the latter as the storing of representation-like objects or chains of representation-like objects (i.e., plans) as well as theories of implicit learning and memory that follows the orthodox Hebbian dictum ("neurons that fire together wire together"). Ashby was indeed very much interested in showing that *even in the absence of explicit representations or of modified neuronal structures*, an organism could achieve equilibrium in changing as well as in repeating environmental situations (assuming the environment is relatively stable). The whole second half of *Design of a Brain* is meant to bring this point home, in fact, as I believe I stressed in the target article. I thank **Malmgren** for giving me the opportunity to stress this aspect of my interpretation of Ashby. Even though I am personally not primarily interested in learning and memory as a main area of research (we all need to pick our horses, after all), I agree with **Malmgren** that Ashby was considerably invested in the issue. I would also draw the conclusion, which remains somewhat implicit in **Malmgren**'s discussion of learning in §§5–6, that Ashby's theory of "learning" (or, in Ashbian terms: "improved adaptive performances in repeating environmental conditions") is as radical as his overall view of life and strongly deserving of renewed attention.

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7 | This is partly because Fechner's view of stability, while similar to Freud's, as the latter explicitly admits, differs from it in significant respects both at the technical level and in terms of the general framework. See Franchi (2011a) for a discussion.

8 | Among the few studies on the subject, see Walker (1956) and the brief notes in Alexander (1948), who takes for granted the identity between Freud's principle of constancy and Claude Bernard's and Walter Cannon's notions of physiological homeostasis. The behaviorist school, and especially Clark Hull's drive theory (1943), took the analogy even further.

9 | I have briefly dealt with this issue in Franchi (2010). Much work remains to be done, obviously.

10 | Lacan (1988: 51ff.) has an explicit discussion of cybernetics and of Grey Walter's turtles. Lacan's conception of human psychic apparatus as structurally failing was already present in his early pre-psychoanalytic work (2001: 30ff.) and is later intensified until it turns humans into, literally, retarded apes suffering from arrested development.

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