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Artículos

In Medias Res: A Resolution of Some False Dichotomies in Origins of Life Research

In Medias Res: Una resolución de algunas dicotomías falsas en los estudios sobre el origen de la vida

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Abstract

In this paper, we address some of the false dichotomies that pervade contemporary scientific and philosophical research about the origin of life. These dichotomies can be divided into two categories, the methodological and the conceptual. In the first case, we focus on providing an alternative to the problems and paradoxes which arise from trying to eliminate a definition of life from scientific research into life's origins. In the second case, we illustrate how origin of life research is confined by the same conceptual paradigm which continues to plague the mind-body problem. Based on this analysis, we then offer some general criteria that a definition of life should meet.

Keywords: False dichotomies, life, origins of Life, mind-body, theories of life.

Resumen

En este artículo abordamos algunas de las falsas dicotomías que impregnan la investigación científica y filosófica contemporánea sobre el origen de la vida. Estas dicotomías se pueden dividir en dos categorías, la metodológica y la conceptual. En el primer caso, nos enfocamos en proporcionar una alternativa a los problemas y paradojas que surgen al intentar eliminar una definición de vida de la investigación científica en los orígenes de la vida. En el segundo caso, ilustramos cómo el origen de la investigación de la vida está confinado por el mismo paradigma conceptual que continúa plagando el problema mente-cuerpo. Con base en este análisis, ofrecemos algunos criterios generales que una definición de vida debería cumplir.

Palabras clave: Dicotomías falsas, vida, origen de la vida, mente-cuerpo, teorías de la vida

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It is so difficult to find the beginning. Or better: it is difficult to begin at the beginning. And not try to get further back.

Ludwig Wittgenstein

I. Coming to Terms: To Define or Not to Define?

In their paper, "Defining Life", Carol Cleland and Christopher Chyba acknowledge that "The philosophical question of the definition of 'life' has increasing practical importance".[1] Yet, they also argue that defining life is unnecessary, unhelpful, even a mistake, and that "what we really need to focus on is coming up with an adequately general theory of living systems".[2] How can defining life have "increasing practical importance" and at the same time be a mistake? We contend that the source of this confusion is not just one false dichotomy but two, one built upon the other.

The first false dichotomy concerns the place of definitions. According to Cleland and Chyba, we cannot define life until we first articulate "an adequately general theory of living systems." But since we currently lack such a theory, the question arises, 'How do we develop one?' According to Cleland and Chyba the answer is: "There is a scientific program, based on laboratory investigations (for example, investigations into the RNA world) and the empirical search for examples of extraterrestrial life, that are important steps towards formulating such a theory".[3] In other words, empirical research precedes scientific theory, and scientific theory precedes definition. The only problem is, what precedes empirical research? As Cleland and Chyba admit,

> As science makes progress towards understanding the origin of life on Earth, as laboratory experiments approach the synthesis of life (as measured by the criteria of some definitions), and as greater attention is focused on astrobiology and the search for life on Mars and Jupiter's moon Europa, the utility of a general definition grows. In particular, definitions of 'life' are *explicit or implicit* in *any* remote in situ search for extraterrestrial life. The design of life-detection experiments to be performed on Europa or Mars by spacecraft lan-

[1] Cleland, C.; Chyba, C., "Defining 'life' ", in *Origins of life and evolution of the biosphere*, vol. 32, núm. 4, 2002, pp. 387-93.
[2] Cleland, C., "Life's working definition: does it work?", in *Astrobiology Magazine*, 2007.
[3] Cleland & Chyba, "Defining 'life' ".

> ders *depends on assumptions about what life is, and what observations will count as evidence for its detection*.[4]

So, it turns out that the scientific program that Cleland and Chyba are advocating for is ultimately circular, because it is, by their own admission, dependent upon even more basic philosophical conditions; namely a definition of life. But a definition of life was precisely what they wanted to show was dependent upon science! As we explained above, in their view, empirical research precedes scientific theory, and scientific theory precedes definition, but now we see that definition must precede empirical research.

This attempt to treat definitions merely as something that comes after science is what we refer to as the first half of the first dichotomy (or more simply "A"). The second half ("B") can be seen as a sort of reaction against it. Unlike A, B treats a definition of life not as what comes after science but as what comes before, as a sort of precondition. But this position, too, suffers from serious drawbacks. For if a definition of life is merely a precondition, then it is prior to the empirical research it informs and so cannot be falsified by it, since what counts as evidence is determined by that very definition. But then how do we distinguish between genuine science and pseudo-science? Ultimately, there would be no difference, at least not one that we could discern, since all differences in research would be reducible to differences of definition which are absolutely prior to any empirical finding.

If this seems very abstract, let's try looking at the problem another way. Consider Cleland and Chyba's objection against a definition of life. According to their argument, a definition of life cannot be formulated until we first articulate an adequately general theory of living systems. For example, if we try to formulate a definition prior to the articulation of such a theory, then we will be in the same position as those who tried to define water prior to the discovery of molecular theory. So, they contend that *if* life really does exist – which for them, at least, is not a given – we need to first elaborate a scientific theory for life that is at least the equivalent of molecular theory, which obviously has yet to be accomplished. While this argument does *not* prove their point that a definition of life can be either avoided or postponed, it *does* make our argument all the more salient – water is H_2O, and without the influence of empirical research and scientific theory there is no way that we could have known that fact. Thus, a definition of life cannot be merely a precondition. If it were, then the fact that water is H_2O would be no more certain than the claim that water is $C_6H_{12}O_6$.

[4] Cleland & Chyba, "Defining 'life' ".

But if B also fails, then it seems that we face an even bigger problem, to wit, a definition of life appears to be both a necessary precondition for and at the same time an obstacle to genuine scientific research. On the one hand, Cleland and Chyba's failed proposal (A) shows that a definition of life must precede empirical research. On the other hand, we have also seen that when we do place definitions prior to empirical research (B) profound difficulties still arise. So how do we escape this trap? In order to unravel the paradox, we must first recognize that the mistake lies not so much in the answers but rather in the question which prompts them. At first glance, the answers appear to be mutually exclusive: a definition of life must *either* come before *or* after the scientific research. Without an explicit question from which to work from, we can nevertheless infer that the implied question must look something like: 'Does a definition come before or after scientific research?' But a more basic question would be, why structure the question this way in the first place?

This brings us to what we see as the second false dichotomy in origin of life research. In our view, the first follows inexorably from the second. Although they do not address the logic of science explicitly in their paper, it is clear from their statements that Cleland and Chyba regard a definition of life as the product of induction (which we refer to as "C"). Admittedly, this comes through much more strongly in Cleland's other work,[5] but it is still an undeniable influence in her collaborative work as well. It is this more basic assumption about induction which we argue accounts for their stance expressed in A.

To support this characterization, let us first say a few brief words about induction. Due to the attention given to the "problem of induction" in the 20th century alone, a definition of induction will inevitably be contentious. Nevertheless, the way that Cleland and Chyba appear to be thinking of the defining process accords with the Humean view that induction involves a generalization over particular instances. This is clear from their emphasis on the fallibility of a definition of life, their concern for sample size and properties, and ultimately their placement of a definition at the end of scientific investigation. If a definition of life were formulated that merely generalizes over particular instances, the way that many definitions of life do (generally as a list of conditions), then one could hardly fault Cleland and Chyba for disregarding a definition of life given the extreme liability for error involved in formulating such a definition. Thus, their skepticism about the accuracy and enduring viability of any definition derived in this way should therefore be

[5] See Cleland, C., "Life's working definition…"; and Cleland, C., "Life without definitions", *Synthese*, vol. 185, 2012, pp. 125-144.

interpreted as just another way of expressing the problem of induction, thereby confirming their acceptance of induction in this capacity. Because they find no solution to that problem does not undermine this interpretation. In fact, they seem perfectly content to let it remain a problem, because they want to avoid a definition of life altogether.

But this is not the only reason for arriving at this interpretation. Take, for instance, Cleland's emphasis on sample size. In her paper, "Life Without Definitions", she writes that,

> Our experience with life is limited to a single example that we have good scientific reasons for believing could have been at least modestly different. Moreover we have no idea how different life could be from life as we know it on Earth today. Until we encounter forms of life descended from a separate origin we will be in no position to speculate about the possibilities for life considered generally.[6]

For Cleland, the reason that sample size is so important is not because it helps us to look beyond accidental differences at the real essence of a thing, but rather because definitions just *are* the list of accidents a certain sample happens to possess, so a definition for a larger sample will necessarily "explain" more. But if we want to have a better generalization, then we will need to observe even more particulars *because* our generalization is what we infer from particulars as applying to the whole, which is ipso facto an inductive inference.

This emphasis on the priority of particulars also accounts for the order of their proposed approach. If a definition of life is just a generalization over particular instances, then it will necessarily be posterior to those instances. In other words, we will need to first observe and study the particulars before we can make the jump to generalizing about what they as members of a certain kind have in common. This again suggests that the means for reaching a definition of life is an inductive inference, because the conclusion of an inductive inference is a generalization which comes *after* we become acquainted with the individual cases.

The problem with this particular inductive approach (AC), as we have already said, is that it cannot escape circularity, given that the empirical research which is meant to provide the basis for a definition of life is itself dependent upon a definition of life. To escape the circularity problem, B proposed to start with a definition and then move on to scientific research. However, we saw that this position suffered from serious problems of its own. We contend that the reason for these problems is the same reason for the

[6] Cleland, C., "Life without definitions".

problems we saw in A, namely, a misconception of the inference that makes science. C, which is the cause of A, proposed induction as the essential inference. In contrast to C, we also argue that a deductive view of science ("D") is in a similar way the cause of B. If deduction is the inference that makes science, then a basic definition such as the definition of life must come before empirical research, an order which produces all of the problems with B that we have already addressed.

The point in drawing attention to this more basic dichotomy (CD) is to show that so long as we misunderstand the fundamental logic of science, the paradox of the order of definitions from AB will remain insoluble. Let us also add that by referring to these views as dichotomies, we do not mean to suggest that they are in any sense equal in terms of membership – far from it. Nevertheless, it is important to note that a position such as BD does not have to be endorsed by anyone in order to be *assumed* as the only other logical alternative. In fact, of those scientists who are even aware of these issues, probably none of them endorse the BD position, while many, at least in practice, endorse the AC position. Still, these sorts of dichotomies exist, and because they exist, they obscure what is fundamentally at issue.

To resolve the paradox, we contend that the CD dichotomy will have to be abandoned. This is not to suggest that induction and deduction play no role in the logic of science, only that they do not occupy the crucial role as "the inference that makes science." In their classic "The Spandrels of San Marco and the Panglossian Paradigm", Stephen J. Gould and Richard Lewontin[7] used an architectural metaphor to assert that adaptationism is a flawed framework for understanding how species and traits evolved. In a similar way, we contend that classical literature can offer insights into how to think about the defining process.

For instance, consider Homer's famous epic, the *Iliad*. The narrative begins not at the beginning of the story but in the middle, *in medias res* as the literary expression goes, with much of the early story being assumed as part of the crucial background knowledge. A similar process, we suggest, occurs in the process of defining terms. We come to understand the meaning of words by first experiencing how they are used within our own linguistic community.[8] In the case of "life", there are some very clear examples which all competent language users would recognize as correct uses of the term "living": dogs are living; cats are living; trees, turtles, and human beings are living.

[7] Gould, Stephen J. and Richard Lewontin, C., "The Spandrels of San Marco and the Panglossian Paradigm: A Critique of the Adaptationist Programme", in *Proceedings of the Royal Society of London*, núm. 205, 1979, pp. 581-98.

[8] By 'community' we mean the participants in a particular 'language game.'

While this use of the term is sufficient for most of our ordinary, everyday requirements, there is nothing prohibiting a community – for instance, the community of scientists and philosophers – from using the term in a more precise way, to reflect more clearly what life truly is. However, this does not mean that that community of scientists and philosophers can simply dispense with the ordinary meaning any more than the *Iliad* can dispense with the first ten years of the Trojan War. This ordinary meaning or "nominal definition" provides the crucial background, grounded in our experience, which serves as the basis for our own "story", the defining process.

Because a nominal definition is grounded in the data it is not subject to the same arbitrariness that we saw in the axiomatic approach expressed in B. At the same time, a nominal definition is also prior to the scientific research it will ultimately inform, so it avoids A's circularity problem as well. However, it is important to point out that just because it is prior to scientific research does not mean that a nominal definition is naively conceived. "Pre-scientific" is *not*, in this sense, a synonym for the irrational or nonsensical. A nominal definition, according to our use of the term, should be based upon as much knowledge of the subject as possible. What distinguishes a nominal definition then from the kinds of definitions found in A and B is that a nominal definition is *pre-inferential* without being arbitrary. That is, it does not go beyond (by making an inference) what is given in the data, which again is not limited simply to what I happen to see or hear or touch. The data, in this sense, is whatever observations the community is able to accumulate, be it directly or through experiments,[9] which includes the vast amounts of what we now call "scientific data" but which in simpler terms amounts to a history. If we *infer* from this data that life just *is* that list of common properties, then we fall

[9] The objection could be raised that the method we have just described also falls into a circle, in a similar way to Cleland and Chyba. For as we showed earlier, experiments necessarily assume a definition, but here we say that experiments can help inform the definition. Are we just making the same mistake? No. The difference is that unlike Cleland and Chyba we do not treat experiments as the *originative* source of a definition. For us, this is the role of the community insofar as it responsible for labeling a shared phenomenon. But experiments can help contribute to a definition to the extent that they add to the history of that phenomenon, thus further refining how it may be used. To be clear, when we use the term "experiment", in this sense, we mean nothing more than the process of acquiring data. For instance, if I heat water to 100°C, then I will record that the water begins to boil. In this case, I have merely observed water *in a particular situation* and recorded what happens. Or perhaps I cannot directly observe the phenomenon, as in the case of the expansion of the universe (what I actually observe is redshift). In this case, nevertheless, my experiment is still nothing more than the process of observing (albeit indirectly) the expansion of the universe. In neither of these cases, have I inferred a *definition*, that is, I do not conclude the experiment with what the phenomenon in question *is*. In these cases, I am merely adding to a natural history. For Cleland and Chyba, however, experimentation *is* an originative source of a definition since definitions are, in their view, just a generalized statement of the natural history. For them, definition is inferential; for us, a definition, at least at the beginning, is pre-inferential.

back into Cleland and Chyba's circle. But if we merely regard this nominal definition as a place-holder, as an expression not of the whole, but of what is given and in need of explanation, then we avoid the problem of circularity altogether, and then and only then will we also have a solid basis for science, because science is a kind of mediated (i.e. inferential) knowledge.[10]

To illustrate this point, consider Cleland and Chyba's example of water. They are, of course, correct in saying that prior to molecular theory and the discovery that water is, in fact, H_2O, definitions of water could only appeal to a list of properties that is insufficient to make a proper identification. However, they are too quick to dismiss the role nominal definitions play in the discovery. To wit,

> Nevertheless, reference to H_2O does not capture the everyday *meaning* of the term 'water.' The claim that 'water is H_2O' cannot be viewed as defining the familiar English word 'water' since the stuff ordinarily called 'water' in day-to-day language varies widely in chemical and physical composition; it is not just H_2O.[11]

Their argument here is essentially that because the way we ordinarily use the term 'water'[12] is imprecise, and may be applied to more than just H_2O, the terms 'water' and 'H_2O' must mean different things, which would, in turn, challenge our notion that nominal definitions, which begin in our ordinary language, are the crucial progenitors of our more precise and developed definitions. This, by extension, is meant to emphasize their point that 'H_2O' as

[10] In her article, "Life Without Definitions", Cleland distinguishes between two kinds of definition: the "traditional" and the "theoretical." Her use of the term "traditional" is puzzling here, as she identifies John Locke (1632-1704), who is rather late in the history of philosophy, as a representative of the "traditional" account. Such usage overlooks so much variation and major different strains of thought on the topic that to try and group them under one term, 'traditional', is vacuous at best, frightfully erroneous and misleading at worst. Further, although she characterizes the theoretical as "tentative and revisable in light of empirical evidence", it is important to note how fundamentally different this is from our understanding of a nominal definition as a "place-holder." According to our position, a definition is a place-holder insofar as it represents a certain starting point, namely, a shared phenomenon – the "data" – and so is not revisable in the sense that it may be overturned – the way Cleland's theoretical definitions (essentially hypotheses) can be – since a nominal definition is *not* trying to offer an explanation *for* the data; it simply states what the data is. A nominal definition holds a place then in the sense that it represents what must be explained.

[11] Cleland & Chyba, "Defining life….".

[12] Or what they call the "everyday meaning", which is in and of itself ambiguous. 'Water' serves many functions in our ordinary language, not merely that of noun. This is commonly indicated by certain grammatical features such as when I say 'Water!' as a command. But even here the semantics are more nuanced than they might appear. A man at a restaurant with a lump in his throat might very well shout 'Water!', but he will be quite helpless if the waiter dumps a bucket of water on him – you *said* 'Water!'. Yes, but I *meant* a glass of water. Or similarly if a fire chief says 'Water!' he will also be dismayed if his fellow firefighter brings him a glass and not a hose.

both a term and a concept could have only originated *ex nihilo*, as it were, from nothing less than scientific theory. However, what this argument fails to account for (or seems to naively assume the contrary) is the common imprecision of 'H_2O'. As anyone who has been around children (or adults for that matter) can tell you, it is has become entirely commonplace for people to use 'H_2O' in precisely the same ways as 'water' and, more to the point, with precisely the same imprecision. But if this is true, then why prefer 'H_2O' to 'water'? One cannot just write off uses of 'H_2O' that are less than precise as mistakes without extending the same courtesy to 'water.' Nevertheless, Cleland and Chyba insist that 'H_2O' does not add new meaning to 'water'. In their view,

> The claim that water is H_2O therefore may be viewed as introducing ('stipulating') a new meaning for the old, familiar term 'water' within the context of an empirically testable scientific theory. But it is *more accurate* to view it as encapsulating a scientific discovery about the nature of water, rather than as representing a linguistic decision to assign a different meaning to an old term in our language.[13]

The problem with this claim is that, just as we saw with the 'circle', their other statements conflict with their main point. For example, in the sentence immediately preceding the passage above, they write,

> What the molecular account of water as H_2O achieves is a broad, theoretically grounded, scientific understanding of the behavior *of what we ordinarily call 'water'* under a wide range of chemical and physical circumstances. It allows us to explain why and how, for example, stream 'water' differs from ocean 'water'.[14]

As this comparison shows, the conclusion that Cleland and Chyba are intending to demonstrate is undermined by their very attempt to demonstrate it. As they themselves admit, the molecular account of water as H_2O is a scientific understanding of what we ordinarily call 'water.' It is *not*, as they want to suggest, divorced from this common usage and the phenomenon to which it refers. Even still, their account contains an important grain of truth. For as they say, what the molecular account provides is 'an explanation of the how and why.' In other words, what a scientific definition *is* is fundamentally an *explanation* of the nominal definition, giving a *causal account* in the sense of the how and why.

In order to develop this causal account of definition further, we still need to find an alternative to the CD dichotomy. For if we lack the inferential ca-

[13] Cleland & Chyba, "Defining life…".
[14] Cleland & Chyba, "Defining life…"; emphasis added.

pacity required for a causal explanation, then we will be in the same place as where we began. It is on this point, that we must acknowledge our indebtedness to Ernan McMullin and others for directing us to the thought of C.S. Peirce.[15] As many have opined in the years after his death, there is perhaps no greater or more original American philosopher, and his reflections on the logic of science (instigated no doubt by his own 32 years of experience as a professional scientist) are a profound testament to his genius on the subject.[16] In what follows, we will not attempt to make an apology for his views, though they hardly need one given that contemporary reflections on these issues have only bolstered and reinforced the cogency of his position.[17]

II. A Peircean Approach

To illustrate exactly why Peirce is important (and relevant!) to the origin of life, let us now look at how his account of scientific method fares against the problems raised by the CD dichotomy. As we will show, Peirce's position is superior to C and D in at least three ways: 1. It resolves the paradoxes both at the AB and the more basic CD levels; 2. It lays out a principled understanding of *scientific discovery*, impossible if we base our method on either induction or deduction (CD); and 3. It reintegrates deduction and induction into the scientific method but in such a way that their contributions are made intelligible insofar as each inference is responsible for a particular methodological function or "step".

In order to understand step 1, we must first recognize that science, both as a method and as a body of knowledge, is essentially a progressive process driven by its orientation towards discovery.[18] In other words, science, unlike, for instance, mathematics, is not primarily interested in deducing necessary conclusions from certain premises; rather, what defines it and makes

[15] We are especially indebted to the late John Deely.
[16] In this regard, he is far better than Popper who attempts to eliminate induction from science altogether.
[17] For instance, see Bayesian confirmation theory and, in particular, Lipton 2004 chapter 7.
[18] By "progressive" we do not mean to suggest that the process is *always* progressing. There are occasions when one paradigm does not just expand or subsume another into itself but radically breaks away from it, as, for example, the Copernican system did to the Ptolemaic system (though one could argue that even here there is continuity in the observations). Nevertheless, science can still be called 'progressive', insofar as the *method itself*, not simply the knowledge, is oriented towards new discoveries and the attainment (and accumulation) of empirically verifiable truth. In this sense, one could even call radical paradigm shifts 'progressive' insofar as the new paradigm moves beyond (progresses past) the obsolete one. However, this is only possible because science, *as a method*, is not structured to martial against these upheavals but because these upheavals are, in some sense, inherent in science itself.

it unique is its emphatic concern for the contingent, the synthetic, the *new*. Without new data (be it from the lab, the field, or otherwise), science would largely dry up. The problem for both deduction and induction is that neither of these inferences introduce anything new. Deduction merely makes explicit what was already there implicitly, whereas induction takes what we already know about certain cases and simply repeats it when confronted with others. Both of these inferences lack the crucial capacity to *hypothesize*.

Yet if we look to Peirce, even in his earliest work, we find that he has already laid out a solution. In fact, he even calls this solution "hypothesis", though today it is more commonly referred to as "abduction".[19] In his essay, "Deduction, Induction, and Hypothesis",[20] he distinguishes these different modes of inference through his analysis of the categorical syllogism Barbara (AAA). By rearranging the rule (major premise), the case (minor premise), and the result (conclusion), he develops the following logical forms:

Deduction.
Rule: All the beans from this bag are white.
Case: These beans are beans from this bag.
∴ *Result:* These beans are white.

Induction.
Case: These beans are [randomly selected] from this bag.
Result: These beans are white.
∴ *Rule:* All the beans from this bag are white.

Hypothesis (Abduction).
Rule: All the beans from this bag are white.
Result: These beans [oddly] are white.
∴ *Case:* These beans are from this bag. (Peirce 1878).

As is clear from the example above, by reconfiguring the structure of the syllogism, Peirce demonstrates a third mode of inference, which is not reducible to the standard induction/deduction dichotomy. In later work, he would illustrate abduction in a slightly different form:

The surprising fact, *C*, is observed.
But if *A* were true, *C* would be a matter of course.

[19] Peirce also refers to this inference as "retroduction" and "presumption".
[20] Peirce, C. S., "Deduction, induction, and hypothesis", in *Popular Science Monthly*, núm. 13, 1878, pp. 470-482.

Hence, there is reason to suspect that *A* is true. (CP 5.189).

However, in either case, what Peirce's theory of abduction shows is that there is a way out of the CD dichotomy. What we will now try to show is why that is important for science and how it is consistent with the account of explanation and nominal definitions we described above.

In keeping with the spirit of our title, *in medias res*, we will begin by giving an example of how this method plays out in terms of a particular "narrative" process. Take, for instance, the familiar case of water as H_2O. If we remained in the AC or BD way of thinking, we would be stuck in the same labyrinth of difficulties and contradictions elaborated above, but if we begin with a nominal definition we have the perfect springboard from which to make an abductive inference to a possible causal explanation. For example, I might observe (note the agreement between our emphasis on a history which amounts to an accumulation of observations and Peirce's use of observation in his illustration of abduction) that water boils at 100°C, that Gerridae ("water striders") seem to skate along the top of a pond, that water freezes at 0°C and returns to a liquid when its temperature begins to rise, that it is odorless, tasteless, and colorless, that it is an excellent solvent, etc. What explains these properties? More precisely, what could possibly *cause* them? Again, deduction and induction cannot say, since neither one introduces anything new. But abduction *does* introduce something new! As Peirce says, "It is the *only* logical operation which introduces any new idea," (CP 5.172). It is this new idea, the hypothesis, which then becomes the basis for scientific discovery. But abduction cannot do it alone. To have a functional scientific method, other steps are required. The advantage of abduction is not only that it provides a hypothesis; but that, by providing a hypothesis, it gives meaning and purpose to other forms of reasoning within the scientific process, namely deductive and inductive reasoning. To see the importance of this, let us return to the example of water. Now as moderns (or better yet, as postmoderns), living as we do in an age after the discovery of molecular theory, the claim that 'water is H_2O' does not strike us as an especially bold claim. The same could be said of the earth's rotation around the sun. However, if we were living in the 7th century CE these certainly would have been very novel ideas, indeed. The point here is that in order to appreciate the importance of abduction we also have to recognize its limitations, and to recognize its limitations, we need to see clear examples of them. One way of doing that it is through the comparison of rival hypotheses, especially between current models and the long defunct models of, say, the ancient Greeks. Unfortunately, most of these sorts of comparisons tend to (over) emphasize – to the chagrin of many historians of science – the discontinuity between the past and the present, which often leads to unfair caricatures of the older position and thus misunderstandings of what rea-

lly constitutes the groundbreaking discovery. In most cases these caricatures overlook the (at times) tremendous complexity and creativity of earlier (and just to be clear, mistaken) hypotheses. For instance, if we look at Cleland and Chyba's account of pre-molecular definitions of water, it would be easy to dismiss them as naive, even simple-minded. But if we look at Plato's (428-348 BCE) hypothesis in the *Timaeus*, we find that much like the molecular model, his model of the elements appeals to a basic component structure. The structure is, of course, significantly different than that of the molecular model, but they nevertheless share two key insights: 1. That we can explain the whole (the macroscopic or aggregate) by reference to its constituent parts (the microscopic); and 2. The cause of the macroscopic phenomenon is ultimately due to the parts' fundamental structure. The differences are obviously important as well, but suppose we grant that, at least at the phenomenological level, both of these hypotheses equally explain the data. What do we do then? Abduction is the source of both of these hypotheses, to be sure. However, in and of itself, it has no power to adjudicate between them. In part, Peirce's brilliance lies in his recognition that although abduction is the key to initiating science, as a process, science relies on a certain order of operations not reducible to a single inference but consisting of a series of inferences working in conjunction. That is, once a hypothesis has been made (abduction), the next step is to discern what consequences would follow if that hypothesis were the case (deduction). Once we have deduced what implications follow from a certain hypothesis, there is still the matter of verifying whether these implications actually occur (induction); by accepting or rejecting a null hypothesis. If they do not occur, then the hypothesis must be false which means that we must try to imagine another possible explanation (abduction) and deduce its implications which we can then verify or falsify by comparing them against other examples generally (induction). Of course, if they do occur, one can pursue other lines of investigation which entails another abduction and so on and so forth. This cyclical structure of cooperative inferences (what amount to feedback loops) is what produces science's distinctive progressive and yet falsifiable character. What it does not do is exclude the relevance and importance of definitions.

Let us now give a brief summary of the matter so far. We began by showing that certain dichotomies which we labeled AB and CD arise from a confusion about the place of a definition of life in origin of life research and that this confusion is the result of a more fundamental misunderstanding about the basic logic of science. To resolve the question of where a definition of life belongs in origin of life research, we first had to show that the current dichotomy (AB) was untenable, that is, treating a definition of life only as a starting point to, or as an end of, scientific research inevitably leads to a contradictory and therefore unworkable scientific method. We then identified the root cau-

se of the AB dichotomy as another dichotomy (CD), which held that either induction or deduction was the basic scientific inference. This, too, we showed to be untenable both for the reason that it leads to the AB paradox[21] and for other reasons proper to the inferences themselves. Our solution to these problems has been to show that these dichotomies amount to a false choice, and that by formulating the question according to these limitations we preclude the possibility of resolving the problem. More specifically we showed that the confusion over where a definition should go falsely assumes that definitions are all of a single type, thus failing to distinguish between a pre-inferential or *nominal* definition and the inferential or *explanatory* definition sought after and informed by scientific research. By making this distinction we resolve the AB paradox by demonstrating that a definition of life can be *both* a foundation *and* a product of our scientific research. To further substantiate this claim, we then demonstrated that any attempt to drive a wedge between nominal definitions and scientific ones (e.g. as attempted by Cleland and Chyba) results from a misunderstanding of semantic relationships, which, stemming from the same confusion over logical inferences, is ultimately self-defeating. Our next task was to show how Peirce's theory of abduction as part of his more general logic of scientific method provides a way out of the CD dichotomy while also reinforcing if not necessitating the compatibility between nominal definitions and explanatory ones we described above. Lastly, we showed how Peirce's understanding of scientific method provides an intelligible framework in which abduction, deduction, and induction work together towards the end of scientific discovery but in such way which preserves science's distinctive progressive and falsifiable character.

III. Two Branches, One Source: What the Mind-Body Problem Can Tell Us About the Origin of Life

So far, the focus has been on the implicit *methodological* limitations in origin of life research and on providing a scientifically and philosophically cogent alternative (or middle way) which we have characterized according to the literary expression, *in medias res*. In this section, we turn our focus to more substantive conceptual limitations by drawing attention to the parallels between origin of life research and ongoing debates concerning the mind-body problem.

[21] Again, the AB paradox arises from the recognition that a definition of life is ineliminable from origin of life research, but that either place for such a definition (be it at the beginning or at the end) fails.

Our main contention is that although the mind-body problem and the origin of life are, in some sense, distinct issues, they nevertheless share a common conceptual paradigm which lacks the necessary resources to resolve these issues. Instead, what this paradigm amounts to is yet another false dichotomy. As the philosopher and paleontologist, Pierre Teilhard de Chardin, put it,

> On the one hand the materialists insist on talking about objects as though they only consisted of external actions in transient relationships. On the other hand the upholders of a spiritual interpretation are obstinately determined not to go outside a kind of solitary introspection in which things are only looked upon as being shut in upon themselves in their 'immanent' workings. *Both fight on different planes and do not meet; each only sees half the problem*.[22]

The point we are trying to make here is that although the names of these opposing sides may differ from issue to issue, content-wise, it is this same dichotomy, the false choice between materialism and what we today call dualism (which Chardin refers to as the 'spiritual interpretation') which is responsible for the gridlock and apparent insolubility of these problems. In what follows, we propose a new paradigm which avoids these conceptual deficiencies.

To begin, let us first give a brief overview of the mind-body problem and how the current dualist/physicalist paradigm has shaped that discussion. In contemporary discourse, the mind-body problem is commonly put in the form of an inconsistent triad, consisting of the following statements: 1. The mental and physical are distinct; 2. mental states are causally efficacious (i.e. they causally interact with physical and other mental states); and 3. physics is a causally closed system, meaning that causal explanations are, at least in theory, completely describable in the language of physics. The problem is that the acceptance of any two of these statements seems to require the denial of the third. Yet, each of them on their own appears to be true. Thus, we are faced with a rather difficult decision – which statements should we accept and which one must we reject? For physicalists, the answer is to reject the idea that the mental and physical are distinct because distinctness would lead to dualism which contradicts physicalism's fundamental premise that everything is physical. For dualists, on the other hand, the answer is to reject either mental causality, as in the case of epiphenomenalism, or, alternatively, casual closure, as in the case of substance dualism; both of which regard mind as, to some extent, irreducible.

The problem with the dualist/physicalist paradigm as regards the mind-body problem is that much like the AB paradox, this dichotomy *appears* to

[22] De Chardin, T., *The phenomenon of man,* New York: Harper Collins, 1959.

exhaust the realm of possibilities (after all, consciousness is either physical or it is not) and yet neither option, dualism or physicalism, offers a tenable solution – physicalism, no matter how complex or nuanced the formulation, simply cannot overcome the hard problem of consciousness;[23] at the same time, dualism cannot overcome the problem of interaction. Yet the paradigm is constantly perpetuated in endless myriads of scholarly literature which only serve to reinforce rather than diminish just how intractable the problem really is. Occasionally, there are notable dissenters, and they have and should continue to attract attention to this important issue.[24] But, the majority of those working on the mind-body problem today still remain committed physicalists. A significant minority of dualists of one variety or another continue to raise problems for physicalism (and vice versa), but very little has been accomplished save for the further divergence of these two positions in ever more extreme directions, as in the case of panpsychism (a radical form of dualism according to which everything, even quarks and electrons, is conscious) and eliminative materialism (a radical form of physicalism according to which conscious experience like pain, color experience, etc. does not exist). What this leaves us with is, as Roger Vergauwen and others have described, nothing less than Scylla and Charybdis.[25]

What we intend to show now is that this same situation arises in origin of life research. In particular, we contend that the dualist/physicalist paradigm within the mind-body problem is fundamentally identical to the vitalist/mechanist paradigm in origin of life research, and as a result, many of the same mistakes and confusions which plague the former also have consequences for the latter.

To illustrate this point, let us compare these two dichotomies and see where exactly and to what extent they overlap.

Physicalism and Mechanism. The first and most clear similarity is between physicalism and mechanism. As a category of theories in the philosophy of

[23] See David Chalmers "Facing Up to the Problem of Consciousness." The "hard problem" is alternatively referred to as the problem of qualia. "It is undeniable that some organisms are subjects of experience. But the question of how it is that these systems are subjects of experience is perplexing. Why is it that when our cognitive systems engage in visual and auditory information-processing, we have visual or auditory experience: the quality of deep blue, the sensation of middle C? How can we explain why there is something it is like to entertain a mental image, or to experience an emotion? It is widely agreed that experience arises from a physical basis, but we have no good explanation of why and how it so arises. Why should physical processing give rise to a rich inner life at all? It seems objectively unreasonable that it should, and yet it does".

[24] See, for instance, any of the work by Jaegwon Kim, David Chalmers, or Thomas Nagel.

[25] See Vergauwen, R. Will, "Science and Consciousness Ever Meat? Complexity, Symmetry and Qualia, in *Symmetry*, núm. 2, 2010, pp. 1250-1269.

mind, physicalism maintains that mind or consciousness is ultimately and completely a physical phenomenon. Mechanism, on the other hand, tries to show that life, like a machine, is completely reducible to its material parts. Our claim is that these views are really just two *applications* of the same reducto-materialist ontology.[26] To demonstrate this claim, let us begin by assuming that materialism, the claim that everything that exists is material, is true.[27] If one makes this assumption, what follows? One important consequence of materialism is that it necessarily entails reductionism. Development of the notion of supervenience has led some to dispute this connection in philosophy of mind. However, the matter is relatively simple. If a physicalist denies materialism, then that means there is (possibly) something, which is *not* reducible to the physical world, in which case, being physical is not a necessary condition for existence, which is what materialism explicitly denies. So, if materialism is true, then reductionism follows as a matter of fact.[28] This is significant because both physicalism and mechanism are extremely reductive in their outlook on life and consciousness. Again, according to physicalism, consciousness is *reducible* to some physical arrangement or process, and the same is true of the mechanistic view of life; there is simply no need, in either view, to go beyond the material constituents. Now as we have already said, and as we will see again in just a moment, there are strong reasons to believe this simplistic approach is insufficient. If it is, then reductionism must fail. But if reductionism fails, then materialism must also fail, which brings us to our next similarity.[29]

Dualism and Vitalism. If physicalism fails, then, in terms of the contemporary debate, it would likely be assumed that the only alternative is to adopt some form of dualism.[30] Again, what we intend to show is that this assumption is also shared within origin of life research, though within that context it is referred to as 'vitalism.' To defend this claim, let us first say a few words about dualism. Dualism is a category of theories within the philosophy of mind which hold that the mental and the physical are both real but irreduci-

[26] We have chosen to use the term 'materialism' rather than the slightly more popular term 'physicalism' when referring to the metaphysical thesis that everything is physical. The reason for this is to distinguish between the metaphysical claim from the more specific claim that consciousness or mind is physical, which is also called 'physicalism'.

[27] Materialism could also be defined negatively as "the view that if something is *not* material (i.e. physical), then it does not really exist".

[28] Another way to look at is if there is only one kind of thing (monism) and it is physical (materialism), then everything there is must be reducible to that same kind of physical thing (reductionism).

[29] The matter can be expressed using *modus tollens*: If materialism is true, then reductionism is true. Reductionism is false. Therefore, materialism is false. $A \rightarrow B, \sim B, \therefore \sim A$.

[30] The logic behind this conclusion is fairly simple; it is what we call a disjunctive syllogism: $A \vee B, \sim A, \therefore B$ (or alternatively, $A \vee B, \sim B, \therefore A$). In other words, if there are only two alternatives, dualism and physicalism, and physicalism is false, then it means that dualism must be true.

bly distinct, so no matter how complex a physical system (e.g. a brain) may be, it cannot, according to this view, account for conscious states. In a similar way, vitalists have argued that life cannot be reduced to purely physical or chemical components. Instead, they insist that life can only be explained by reference to some separate élan *vital* or vital force (comparable to the Cartesian ego). So, in a broad sense, we could call vitalism a form of dualism, because, like its counterpart in philosophy of mind, it posits the existence of some separate immaterial reality to explain the phenomenon in question –what Gilbert Ryle referred to as the "ghost in the machine".[31] But it is important to note as well that this conclusion stems in part from the assumption that the only way to avoid reductionism is to resort to a dualistic ontology, even if it comes with the consequence that life or mind cannot be accounted for scientifically.

The Hard Problem: Philosophical "Zombies" and Biological "Machines". Admittedly, the majority of those working on these problems today still believe that a reductionist account of life and consciousness are still possible, though they may, as yet, elude us. This hope is largely founded upon the success this approach has yielded in other endeavors, in explaining other various phenomena. However, this confidence has increasingly come to be challenged, especially with regard to consciousness. In what follows, we will lay out one of the more prominent objections to physicalism within the philosophy of mind. After laying out this objection, we then show how a similar objection could be raised against the mechanistic approach to life.

Before we explain the objection, let us first make a few clarifying remarks: 1. When we use the term "hard problem", we are referring to a particular problem within the field of consciousness science and philosophy of mind, also known as the "problem of qualia" or the "problem of phenomenal experience"; we are not merely referring to a problem as hard, or difficult, in the colloquial sense; 2. The hard problem is so called in contrast to what are commonly referred to as the "easy" problems, namely how consciousness relates to some ability, or the performance of some function or behavior. For instance:

- the ability to discriminate, categorize, and react to environmental stimuli;
- the integration of information by a cognitive system;
- the reportability of mental states;
- the ability of a system to access its own internal states;
- the focus of attention;

[31] Ryle, Gilbert, *The Concept of Mind*, Chicago: University of Chicago Press, 1949.

- the deliberate control of behavior;
- the difference between wakefulness and sleep.[32]

While all of these phenomena are associated with consciousness, there is no real issue whether they can be explained scientifically in terms of computational or neural mechanisms. As David Chalmers has said, "If these phenomena were all there was to consciousness, then consciousness would not be much of a problem".[33] Even though it will probably be a long time – Chalmers suggests a century or two – before we have anything close to a complete explanation of these sorts of phenomena, we already have a clear idea of how we *could* explain them. But the real problem, the hard problem, is that consciousness is not simply reducible to these sorts of phenomena. As Thomas Nagel has put it, there is something that these descriptions leave out, namely *what it is like* to be conscious.[34] This subjective aspect of experience is something we are all aware of and, despite some notable attempts, undeniable.[35] As Chalmers notes,

> When we see...we *experience* visual sensations: the felt quality of redness, the experience of dark and light, the quality of depth in a visual field. Other experiences go along with perception in different modalities: the sound of a clarinet, the smell of mothballs. Then there are bodily sensations, from pains to orgasms; mental images that are conjured up internally; the felt quality of emotion, and the experience of a stream of conscious thought. What unites all of these states is that there is something it is like to be in them. All of them are states of experience.[36]

If physicalism were complete it would be able to account for these sorts of phenomena, but unlike the easy problems of consciousness, the hard problem seems not only to evade but to *exclude* scientific explanation. To explain a cognitive function we need only identify the mechanism which performs the function, which is why the methods of cognitive science have been so successful at explaining the easy problems. But the hard problem is not a problem about function, and even when all the relevant functions have been explained, the problem still persists.

[32] Chalmers, D., "Facing up to the problem of consciousness", en *Journal of Consciousness Studies*, vol. 2, núm. 3, 1995, pp. 200-19.
[33] Chalmers, "Facing up to the problem of consciousness".
[34] Nagel, Thomas, "What is it like to be a bat?", in *The Philosophical Review*, vol. LXXXIII, núm. 4, October, 1974, pp. 435-50.
[35] See Dennett, D., *Consciousness explained*, Little, Boston: Brown and Co., 1991; Churchland, P., "Eliminative materialism and the propositional attitudes", in *Journal of Philosophy*, núm. 78, 1981, pp. 67-90.
[36] Chalmers, D., "Facing up to the problem of consciousness".

To illustrate this point, imagine a world completely identical to the actual one with respect to all of the physical facts. According to physicalism, if such a world existed, then it must necessarily contain everything that the actual world contains. However, it seems entirely possible to imagine a world identical to the actual one with regard to all the physical facts, that nevertheless completely lacks conscious experience. This "zombie world", as it is commonly known, would even function the same way as the actual one, from atoms to neural networks to complex human behavior. Yet, it seems clear that this world would be incomplete. But if we can conceive of such a scenario (as apparently we can) then it follows that it must be metaphysically possible. If such a scenario is possible, then it cannot be true that the zombie world necessarily contains everything the actual world contains, even though they are physically identical. Thus, physicalism is false: consciousness is not identical to any physical state.

Whether or not the zombie argument succeeds at disproving physicalism is well beyond the scope of this paper. For our purposes, the primary goal is not to defend it, but merely to show how a similar problem arises for the mechanistic view of life because of the same operative paradigm.

As we have already shown, the mechanistic view of life is essentially the same as the physicalist view of mind. Both are committed to a reductionist project according to which all phenomena can be reduced to their material constituents. In part, what the zombie argument purports to show is that this way of conceiving of the world is incapable of accounting for certain *interior* dimensions, namely, conscious experience. But there is another kind of interiority which the mechanistic view leaves out. As Daniel Nicholson explains,

> Paradoxically, the single most important difference between organisms and machines has its basis in what prima facie appears to be their most obvious similarity...both organisms and machines operate towards the attainment of particular ends; that is, both are *purposive* systems. However, their purposiveness is of a completely different kind. Organisms are *intrinsically* purposive, whereas machines are *extrinsically* purposive.[37]

One way of illustrating this difference is to take, for example, a tree. Looking at a tree, a homesteader might intend the tree for lumber for a house. A frightened squirrel might use the tree for shelter, to, say, escape the homesteader's dog. Still further, the homesteader's wife, being of an artistic bent, might use the tree as the model for a landscape. All of these uses of the tree (construction materials, shelter from predators, and subject of a work of art)

[37] Nicholson, D. J., "Organisms ≠ machines", in *Studies in history and philosophy of biological and biomedical sciences*, vol. 44, núm. 4 Pt B, 2013, pp. 669-78.

are obviously *extrinsic* to the tree itself; that is, it is not part of the nature of a tree to perform those functions. On the other hand, it *is* an intrinsic function of the tree to grow and nourish itself, for its roots to absorb water, its leave to photosynthesize, etc.

The mechanistic view of life does not make this distinction. Indeed, if it is going to be consistent, it must, in fact, *deny* this distinction, since purposiveness is inconsistent with mechanism's materialist ontology. After all, what is *intended* by the organism is not there (it may not even exist!). But if what is intended i.e. the intentional object is not material, then, according to materialism, purposiveness (intentionality) cannot really exist. If this is true, then mechanism faces its own version of the 'hard problem'. And even if we grant mechanism the extrinsic purposiveness its analogy requires, it will not have resolved the problem. As Nicholson explains,

> [T]his argument mistakenly equates purposiveness with behaviour...The purposiveness of a system does not depend on its behavioural response patterns but on the *internal* organizational regime *causally* responsible for them. Servomechanisms lack the self-maintaining organization to enable them to genuinely act on their own behalf.[38]

What we have here is the biological version of the zombie argument, what we call the "living dead" argument. The mechanists, like the physicalists, want to reduce the 'hard problem' to the 'easy problems'; that is, they want to show that some interior characteristic is reducible to behavior or function. But purposiveness is not the behavior; it is the explanation of the behavior.

Eliminativism: Mental and Biological. If materialism, and by extension physicalism, cannot solve these hard problems, then there are only two alternatives: either reject materialism or deny the problems. But in order to deny the problems as a committed materialist, one must also deny the source of the problems, namely, the phenomena themselves. In consciousness studies, this has led some thinkers such as Daniel Dennett and Paul and Patricia Churchland to embrace a position commonly known as eliminative materialism. According to this view, there is no hard problem, but only because they "eliminate" or deny that phenomenal experiences such as pain, color, etc. actually exist. In fact, they deride those who dare to acknowledge such realities as not even making an intelligible claim. Rather, they insist, that all statements about qualia really belong to the realm of "folk psychology", a pejorative term used to describe those statements they consider "unscientific".[39] These include such statements as "I am in pain!"

[38] Nicholson, D. J., "Organisms ≠ machines".
[39] P. M., Churchland 1981, "Eliminative Materialism and the Propositional Attitudes," *Journal of Philosophy*, 78: 67– 90.

or "I see red"; they even compare these statements to discredited ideas such as astrology or magic. But perhaps the most noteworthy point of comparison is their umbrage with the notion of intentionality (or purposiveness) as we discussed above. However, if one rejects intentionality, then as we have shown, the same hard problem arises for life as it does for consciousness. To be consistent, then, the eliminativist must also reject life in the same way as consciousness, in which case, life would not really exist and statements which affirm that it does would have to be labelled "folk biology." While this is certainly an extreme position, it is not unheard of within the origin of life community. Cleland and Chyba admit on multiple occasions that life may not be a 'natural kind', which is just another way of saying life might not really exist. Others have been bolder, explicitly stating that it does not. As Ferris Jabr writes,

> Even today, scientists have no satisfactory or universally accepted definition of life. While pondering this problem, I remembered my brother's devotion to K'Nex roller coasters and my curiosity about the family cat. Why do we think of the former as inanimate and the latter as alive? In the end, aren't they both machines? Granted, a cat is an incredibly complex machine capable of amazing behaviors that a K'Nex set could probably never mimic. But on the most fundamental level, what is the difference between an inanimate machine and a living one? Do people, cats, plants and other creatures belong in one category and K'Nex, computers, stars and rocks in another? My conclusion: No. In fact, I decided, life does not actually exist.[40]

In our view, to deny that life exists is as absurd as it is to deny the existence of consciousness. So rather than argue that said phenomena exist, our aim is to raise awareness of how deep the reason for this rejection goes. Materialism and its variants *cannot* overcome the hard problems, not because of some lack of individual genius, but rather because the concepts themselves *exclude* the phenomena they are trying to explain.

IV. Criteria for the Future

The purpose of addressing these false dichotomies, both methodological and conceptual, is ultimately to provide helpful criteria for future work on origin of life research, specifically for guiding the question of how we ought to define life. In their paper, "The Need for a Universal Definition of Life in Twenty-first-century Biology", Kepa Ruiz-Mirazo and Alvaro Moreno lay

[40] Jabr, F., "Why life does not really exist. Scientific American", 2013. https://blogs.scientificamerican.com/brainwaves/why-life-does-not-really-exist/.

out five requirements that a definition of life should meet. As we will show, by addressing the more fundamental issues within origin of life research, we not only confirm these criteria but significantly strengthen and expand them. In so doing, we demonstrate, once again, not only that a definition of life is helpful,[41] but also inseparable from research into the origin of life.

Moreno and Ruiz-Mirazo begin their discussion by specifying the type of definition required. As they explain,

> Definitions, in general, can be constructed with two main purposes in mind: (1) to demarcate or classify a certain type of phenomenon, and (2) to grasp or express the fundamental nature of that type of phenomenon. The first purpose normally leads to *descriptive* definitions that consist in a set of properties—typically, a checklist—containing all that is required to determine whether a phenomenon belongs to a particular kind, whereas the second, *essentialist* definitions characterize a given phenomenon in terms of its most basic functional mechanisms and organization.[42]

The first kind of definition is similar to those addressed by Cleland and Chyba, while the latter is more similar to the goal of a scientific or explanatory definition as we described it. What our analysis of inferences and the logic of science does is situate these two kinds of definitions in an explanatory continuum, of which these kinds stand out as certain punctuated or discrete points. Having made this distinction, they then lay out their five criteria:

> The definition should:
> (A) be fully coherent with current knowledge in biology, chemistry, and physics;
> (B) avoid redundancies and be self-consistent;
> (C) possess conceptual elegance and deep explanatory power—that is, it must provide a better understanding of the nature of life, guiding our search into its origins and its subsequent maintenance and development;
> (D) be universal, in the sense that it must discriminate the necessary from the contingent features of life, selecting just the former;
> (E) be minimal, but specific enough—that is, it should include just those elements that are common to all forms of life (not being, in principle, restricted to life on Earth), and at the same time, it must put forward a clear operational criterion to tell the living from the inert, clarifying border-line cases, contributing to determine biomarkers, and so on.[43]

[41] "[T]he work of synthesis needed to generate and defend such a definition could actually help form the basis for a general theory of biological systems." Ruiz-Mirazo, K., Moreno, A., "The need for a universal definition of life in twenty-first-century biology", in Terzis, G.; Arp, R. (Eds.), *Information and living systems philosophical and scientific perspectives,* Cambridge: MIT Press, 2011.

[42] Ruiz-Mirazo, K., Moreno, A., "The need...", pp. 3-24.

[43] Ruiz-Mirazo, K., Moreno, A., "The need...".

Our analysis of the scientific method strengthens these criteria in the following ways:

1. *Coherence with current knowledge*: *In medias res*, in our sense of the expression, is an account of how nominal definitions play a role in the progression towards a scientific explanation, and thus how every scientific project is always already begun in the middle of a larger continuum, even as it progresses. This continuum necessarily includes current knowledge in all of the sciences.

2. *Avoid redundancies and contradictions*: Drawing attention to the fundamental role of abduction as the source of novelty within science ensures that a definition will avoid being redundant, while our analysis of the logic of science as a whole ensures that it will avoid being incoherent.

3. *Distinguish the necessary from the contingent; be concise*: In our analysis, we place a high priority on causal explanation as opposed to a merely inductive aggregation of properties. This emphasis on causal explanation helps avoid confusing commonalities in a given sample which are contingent with those causal features that underlie them. At the same time, focusing on causal explanation also helps keep a definition sufficiently concise, because it is not meant to be a replacement for natural history.

Our analysis also expands these criteria by adding the following conceptual recommendations:

1. *Avoid false dichotomies*: *In medias res*, a middle way, is not only a methodological criterion but a conceptual one as well. This is especially important when one considers the false dichotomy between dualism and materialism. Given the serious problems with this paradigm, a definition of life should seek to find a middle ground between these two extremes.

2. *Not reductive*. This middle ground should be non-reductive, that is, it should avoid treating life as something that is reducible to its material constituents. To avoid this, materialism will have to be abandoned.

3. *Not dualistic*. On the other hand, neither can a definition of life be based upon a form of dualism.

4. *Not eliminativist*. At the same time, the answer cannot simply be a denial of the problem. A definition of life must attempt to resolve the hard problem in a way that does not deny that life exists.

5. *Functional accounts*. Just as multiple realizability, the apparent capacity of different organized systems to instantiate a certain property, effectively discredited identity theories of consciousness and opened the

door to functionalism, the variety of life forms on earth and the possibility of life on other planets shows that life, too, is not identical to material systems, but is rather a function or activity of those systems.

V. Summary and Conclusion

The aim of this paper is to draw attention to certain methodological and conceptual dichotomies that are operative in contemporary scientific and philosophical research on the origin of life. Our analysis shows that these dichotomies pose a serious threat to Origins research, due to a want of methodological and conceptual tools to escape from significant challenges, paradoxes, and contradictions which those dichotomies themselves, have ironically created. The methodological limitations consist of two levels, which we refer to as 'AB' and 'CD'. AB represents a dichotomy concerned with the order of a definition of life in relation to scientific research. 'A' maintains that a definition of life can only be established once an adequately general theory of living systems had been formulated. However, we show that 'A' is ultimately circular, because such a theory must already assume a definition of life. 'B', on the other hand, attempts to resolve this problem by reversing the order, placing a definition prior to scientific research. This, too, leads to problems, insofar as B is completely inconsistent with science's progressive and falsifiable nature. The twofold failure of A and B, in turn, leaves us with a rather serious paradox: a definition of life is apparently necessary for origin of life research, but cannot work either as either A or B would require. To solve this problem, we show that the AB dichotomy and thus the paradox is dependent upon a more basic dichotomy concerning the logic of the scientific method, which we call, CD. 'C' maintains that a definition of life is the product of an inductive inference, whereas 'D' holds that it is a deductive axiom. This dichotomy likewise is shown to be false. Neither one can account for the characteristics of science or resolve the difficulties that manifest at the AB level. So, to resolve those problems, we adopt a two-pronged approach. The first prong demonstrates the importance of nominal definitions as a crucial precursor and framework for scientific explanation. This distinction provides a broader conception of definition that enables us to avoid the AB paradox which demands that a definition of life come before or after scientific research. As we show, definitions develop throughout a process, where science begins in the middle. Having given this broader context for a definition, we then provide a logical basis for it, adopted from the work of C.S. Peirce. Beginning with abduction, moving to de-

duction, and lastly induction in a cyclical feedback loop, we show that the pre-scientific and the scientific form a cohesive unity, grounded in a shared phenomenology. With the methodological issues sufficiently resolved, we then turn to the more substantive conceptual problems, which we again find to be the consequence of another false dichotomy, namely, the dualist/materialist paradigm. Through a comparison with the mind-body problem, we illustrate how the mechanist/vitalist paradigm in origin of life research is essentially the same as the dualist/physicalist paradigm within consciousness studies, both being specific adaptations of a broader metaphysical dichotomy between materialism and dualism. If one follows a dualist approach, life and consciousness are beyond empirical science. If a materialist approach is taken, life and consciousness give way to either their own 'hard' problems or, a radical form of eliminativism which denies that either one actually exist. With these options fleshed out, we then enumerated criteria that a definition of life should follow in the future.