Short remarks on organizational closure

Gonçalo Braga

June 6, 2025

Abstract

Some notes on the concept of organizational closure (as in closure of constraints, closure to efficient causation, and similar ones), touching upon some other potentially related things along the way, which I might find important to mention. Hopefully, in not too rant-ish a manner. Majority of examples and discussion is done around minimal organisms or systems which could be argued to exemplify similar properties (e.g. unicellular, proto-cells, autocatalytic biomolecular condensates - think for instance about similar systems to coacervates - but under which some of the catalysts would also serve as scaffold and client molecules). Mostly to be developed over time, in hopes of sorting out some confusion I might have.

1 Biological organization as self-constraint

> An organized being is then not a mere machine, for that has merely a motive power; but it possesses in itself formative power, and such a one, moreover, as it communicates to the materials, which do not possess it (it organizes them). Thus it requires no other purposive principle for its maintenance than the one which it itself produces. In such a product of nature, every part is thought as if it exists only by means of all the others, and so exists for the sake of the others and the whole, i.e., as an instrument (organ). And this reciprocal causation of the parts in the whole distinguishes a machine from an organized being. In the former, the parts only act on one another in turn (so that one part is the instrument of the motion of the other); but in the latter, the parts are reciprocally cause and effect of their form.
>
> Kant (1790)

Organisms are said to be systems which can both construct and maintain themselves. They aren't merely self-organizing, as they can also maintain the capacity to self-organize. And for such to happen, they must have some type of closure at the organizational level. They must maintain this capacity from within their organization (see for instance Rosen (1991); Maturana and Varela (2012); Varela (2025); Moreno and Mossio (2015); Kauffman (2000); Deacon (2021)). It is in this sense, that organisms are said to be self-determined, self-constrained, to be both the means and the end. The constraints in an organism are said to be mutually dependent. They hold a dialectical relation with respect to each other, and as such can't exist independently. Through this view, we get to notions such as closure of constraints, where each constraint in the organism's organization needs to produce at least another one (e.g. Montévil and Mossio (2015); Moreno and Mossio (2015);

Mossio et al. (2013)). This is how organisms achieve some type of causal closure, and how a minimal form of autonomy is achieved (Moreno and Mossio, 2015). It should be noted that this isn't about physical closure, nor is it about a system becoming isolated from its environment. Much to the contrary, any system which is to achieve organizational closure must necessarily be at far-from-equilibrium conditions. It must continually do work in a constrained manner in order for closure to be maintained, and being thermodynamically open is a necessity. Furthermore, one shouldn't associate closure to permanence, neither of constraints nor of relations between these. What needs to be achieved is continuity of this organization (maintenance of organizational closure), regardless of what constraints might be present. In this sense, organizational closure is probably the only property which can be said to be invariant over time in an organism.

Kant (1790) was perhaps one of the first to capture this notion generally, along with the notion of *self-organization* which now might merely stand for systems which come to be more organized, but which can't maintain the constraints that allow such organization (these eventually dissipate away). It's through this organization, which is inherently circular and impredicative, that the concept of intrinsic teleology emerges (e.g. Mossio and Bich (2017); Weber and Varela (2002); Di Paolo (2005); García-Valdecasas and Deacon (2024); García-Valdecasas (2022); Jonas (2001)), as Kant was preoccupied with (Kant, 1790; Weber and Varela, 2002).

2 Circularity of what exactly?

One doesn't need to go beyond unicellularity to appreciate how complex organisms are. Consider the following passage written in Tartar (1961) about the large cilitate (albeit unicellular - on the order of 1 mm) *Stentor coeruleus* :

> When a sample of *coeruleus* is set aside for a week or two without added nutrients the animals starve until individuals are produced which are much smaller than normal daughter cells. Starting with these starvation dwarfs, I cut off substantial portions of the posterior pole and found that pieces as small as 75 μm in diameter or only 1/123rd the volume of large, pre-starvation stentors, could regenerate completely and survive for over 6 days.

This is a single-celled organism which exemplifies remarkable regeneration capabilities (see for instance Slabodnick and Marshall (2014); Tartar (1961); Marshall (2021)).

Asking how it can regenerate in such manner, is not a very different question from asking how it can maintain organizational closure. It is equivalent to asking

how can said systems keep on producing every constraint from within themselves even when perturbed drastically.

I have now mentioned a couple of times that causal closure happens at the constraint level. What does this really mean? At a first glance, one might perhaps state that organisms are composed by networks of processes which produce every component in said network, such that the network can maintain itself. Notwithstanding the generality present here, we can ask: "Every component?". That can't be correct. Being thermodynamically open, it's clear that not every component is produced from within the system. This is where the notion and concept of a constraint is helpful. Constraints can be generally seen as physical objects or structures, which impose some limits on how an underlying process at the detailed dynamical level can change. Regarding a formalist description (and particularly if one takes a dynamical systems theory approach), said constraints appear as externally given, under boundary conditions, control parameters, etc; and these can change but not on the same temporal scale as the objects which are being described under the corresponding dynamical laws at the detailed dynamics level[1]. In the circunstances where the corresponding constraints limit the underlying dynamics so as to leverage them into producing a set of constraints which can keep on doing the same thing (i.e. channeling work into producing further constraints), we say that the system has closure of constraints[2] (e.g. Montévil and Mossio (2015); Moreno and Mossio (2015); Mossio et al. (2013); not exhaustive at all - majority of other references to be found therein).

Afterall, there's an infinitude of dynamical systems in Nature which are self-organizing to a certain degree (Glansdorff et al., 1973), but in these the constraints which allow for such self-organization eventually dissipate away. Organisms, to this extent, can be said to be systems composed by coupled self-organizing processes which compensate for each other's dissipative tendencies. Not only do they maintain this capability for self-organization, but they get better and more complex at it.

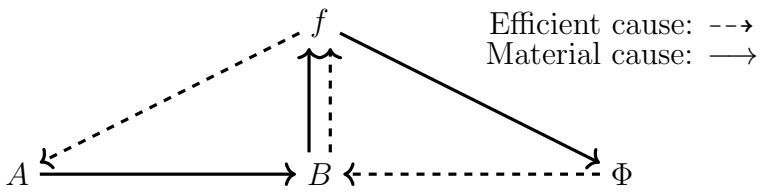
Someone's work which captures this notion of organizational closure in an interesting albeit very abstract manner, is that of Rosen (1991)'s. Rosen (1991) makes heavy use of Aristotelian (be)causes, using efficient and material causes to capture the notion of constraints and that of the constrained, respectively[3]. The central

[1] One can of course also assume some reflexivity conditions, such that the states transversed will also act as transformations on the underlying dynamics (i.e. for instance states will lead to a change of the form of a corresponding set of coupled ODEs, to a change of control parameters, etc). As is found regarding piece-wise differential equations.

[2] Of course there's always the problem of understanding what to consider as a constraint (and as the constrained), through which observables to have such description, all of which are always dependent on the modeller's intentions to capture the behaviour of a certain phenomenon. Additionally, causality is a contentious issue (e.g. Mossio et al. (2013); Craver and Bechtel (2007)).

[3] Rosen (1991) uses these explicitly, whilst formal and final causes are either used implicitly or not at all. Additionally, check Hofmeyr (2017, 2018, 2021) for a general contextualization of

thesis that Rosen (1991) puts forward, is that every material system which is closed to efficient causation is an organism. Represented in the next diagram, is the (M, R)-system which Rosen (1991) chose to represent such causal regime:



where the mappings $f : A \to B$ ("metabolic" mapping, usually interpreted as a set of catalysts f (efficient cause) acting upon a set of substrates A (material cause) transforming these into products B), with $f \in H(A, B)$, $\Phi : B \to H(A, B)$ ("repair" mapping; supposed to reflect the replenishment or regeneration of f), such that $\Phi(b) = f$, and $B : f \to \Phi$ (the "replication" mapping $\beta : H(A, B) \to H(B, H(A, B))$ such that $\beta(f) = \Phi$) being a very restricted mapping as $\Phi(b) = f$ is only to have one solution, are producing each other in a manner such that the system is closed to efficient causation. Interpretations of this (M, R)-system are readily available in the literature, for instance in Letelier et al. (2006); Soto-Andrade et al. (2011); Cárdenas et al. (2010), along with better contextualization of other (M, R)-systems (and other systems closed to efficient causation) in Hofmeyr (2021).

I don't intend to prolong myself too much here, but let me make a few remarks. First, to me it's unclear what these relational abstract diagrams are actually supposed to capture beyond what's trivially visible. Distinctions between different types of entailment are very discernible. For instance, one can make diagrams under which systems which have some type of circular organization are very easy to

these abstract relational models w.r.t. cell biochemistry, with the use of formal cause. Furthermore, Hofmeyr (2021) represents a few more diagrams which are closed to efficient causation. On the issue of formal causation, Hofmeyr (2018, 2021) makes the distinction between intrinsic and extrinsic formal cause, which I presume is based on Oderberg (2021). Oderberg (2021) also seems to subdivide accidental (intrinsic) into contigent and necessary. On top of me not having a proper philosophical background to fully understand what Oderberg (2021) is conveying, majority of examples are every-day ones, to the extent that it's very hard to map these concepts into their suitable form to be used in a molecular context. As it stands, these don't seem to provide enough conceptual value. The same applies to the distinction between closure and openness to formal cause, which seems to only be given a few paragraphs over Hofmeyr (2021). To this same extent, I found Bich et al. (2016) to provide potentially the same distinction between closure and openness to formal cause without use of Aristotelian causes (i.e. with closure and openness to formal cause, seeming to be equivalent respectively to, a system which only has a constitutive regime, and one which on top of a constitutive regime has some regulatory constraints).

distinguish. This is the case when comparing a system where a set of substrates are transformed into products which then serve as substrates again under a different catalyst, for which this type of relation repeats to the extent that the system is closed to material causation (but under which no further catalysts are being produced); and another one which is closed to efficient causation (think for example about an autocatalytic set, where the catalysts (or the efficient causes) produce each other collectively from a given set of substrates; albeit here one should also provide proper co-localization constraints, such that there are some type of constraints on diffusion of the corresponding catalysts and metabolites - e.g. some by-product of the autocatalytic network self-assembling into a membrane or capsid-like structure; some type of phase separation event by supersaturation conditions, as happens in LLPS, leading to some type of constraint on diffusion, etc).

Moreover, these diagrams themselves (and the usual interpretations accompanying them) are supposed to be very general I assume. See for instance Hofmeyr (2021) for a contextualization of these with the respect to cell biochemistry. They aren't supposed to give a similar view to the one we get when describing reaction networks, for instance, or any dynamical approach for that manner. They are apparently supposed to capture very general functional relationships between components, processes, constraints, etc. What this might mean more concretely, is what I'm not sure about. In my view, it only stands as a way to exemplify organizational closure in a very general manner. That's the only conceptual work it seems to have associated to it.

What's the role that organizational closure should have? Merely a principle which should be kept in the back of our minds when modelling some biological system (i.e. any sub-system or set of mechanisms (mechanism as in Bich and Bechtel (2021); Bechtel (2011) - mechanism need not be reductive) which are modelled at any time, should be contextualized with them being a sub-part of an autonomous system, that has its mechanisms organized in such a manner to maintain said autonomy)? It's not by mistake that Rosen (1991) conjectures that organisms (and complex systems by that manner - here regarding Rosennean complexity which is different from more traditional notions (Rosen, 1991)) have no largest model. We have a practically infinite (perhaps the best term here would actually be *indefinite*) amount of ways of decomposing such systems, with corresponding observables which are to be tracked, without ever capturing the full behaviour of such systems. Most of these models can then only be taken as tools, with an appropriate range of applicability, and potentially complementing each other.

And yet, what does one make out of organizational closure? From the organizational account, one gets the notion that this is indeed the defining property of organisms, as in the general case of what an organism is. In fact, it seems to be the only property which is invariant over time in organisms, nevermind the practical difficul-

ties in how to appropriately describe what's to be considered as a constraint (and as corresponding processes being constrained) in any given scenario. The question then is: is organizational closure capturable? If not, why so?
Without taking it into account, it feels as if one is generally studying processes as if they aren't happening in an organism. It is to this extent, that I feel that organizational closure puts "systems" into "systems biology".

3 What should organizational closure provide?

You see in Nature many examples of systems, like hurricanes, candles burning, etc, which have some type of self-organizing capacity and which hold a far-from-equilibrium state for a certain period of time until the constraints which allow it, eventually dissipate away, and the system returns to equilibrium. And then, there are organisms, which can prevent the dissipation of said constraints. In fact, they get better at preventing such dissipation. How (why) should one then not draw a categorical distinction between these two types of systems, for which organization makes the difference?

At an already very small scale, unicellular organisms show remarkable kinetic and spatial control capabilities. Now, it's important to make a few remarks on what control is, and on its possibly different forms. I'll mostly do this in a very shallow and brief manner, according to Bich et al. (2016), for which a separation between constitutive and regulatory constraints is done (and which is based on Moreno and Mossio (2015), and some of the references given therein). Bich et al. (2016) is pretty much devoted to demarcating between *dynamical stability* and *regulation*. These represent two very different ways a system can respond to pertubations (be them internally or externally generated). The former corresponds to the capacity a system has, in a distributed manner[4] through all of its constitutive constraints, to respond to such pertubations, while the latter is respective to a system which can switch and change the core dynamic network and its constitutive constraints (the constitutive regime), through specialized mechanisms which are dynamically decoupled[5] from said underlying constitutive regime. Most of said intent from Bich et al. (2016), comes from the blending, in majority of the literature, of particularly distinctive processes into the same *regulatory* categorization. What's broadly called *homeostasis* - the maintenance of certain variables as invariant (or at least within a certain range) through a certain metabolic regime and through the constitutive constraints available in such metabolic regime (e.g. buffering, feedback and feedforward mechanisms which depend majorly on the concentration of said con-

[4] In stoichiometrically-determined manner.

[5] Such constraints would be largely stoichiometrically independent from the constitutive regime they control, even if they are produced from said regime. See Bich et al. (2016) for more depth.

stitutive constraints, etc) - is called *regulation* and is put in the same bag as for instance *adaptive behaviour*, which achieves this but through different means (e.g. with respect to bacterial metabolism, diauxic shifts imply a change in the metabolic regime through the synthesis of a different set of enzymes to handle different carbon sources). An example of the former (*dynamical stability*) which is given in Bich et al. (2016) is that of feedforward activation or feedback inhibition in the context of flux control in a metabolic pathway. An example of the latter (*regulation*) is that of bacterial chemotaxis. Although it's extremely important to note that Bich et al. (2016) consider it an example of a set of regulatory constraints only in the context of metabolism-independent chemotaxis (check Egbert et al. (2010) for discussion around chemotaxis dependent, independent and based on metabolism; along with some references therein which experimentally verify some of these different regimes in bacteria). I won't touch much more on this paper, and I think its intent can be summed up over the following excerpts (Bich et al., 2016):

> In our view, the problem lies in the tendency to focus on the *effects* - i.e. the systems having adjusted itself in such a way to counter the perturbation - rather than on the *nature* of compensatory responses - i.e. *how* the response is achieved. (emphasis theirs)

with a special emphasis on the problems coming from majorly phenomenologically-driven modelling, which focuses much more on the analysis of how a system responds to pertubations rather than how it responds to said pertubations, with an excerpt from Hofmeyr and Cornish-Bowden (1991) with respect to Metabolic Control Analysis:

> [...] metabolic regulation was usually recognised as the result of observing the performance of the metabolic system, without knowing exactly what the molecular mechanism responsible for this behaviour was [...]

If assumptions behind mass-action kinetics, like those of a dilluted and well mixed environment, held for a cell as more than a reasonable approximation, modelling the behaviour of said systems would be extremely easy. There are of course approaches which aim at modelling systems with non-ideal behaviour, but one can argue that these are majorly phenomenogically driven (say for instance Biochemical Systems Theory (e.g. Voit (2013)) with power-law formalisms, fractal kinetics (e.g. Kopelman (1988)), etc). Not only do cells have an incredible robust and intricate kinetic control, the same description also holds for the spatial control of all of the cell's constraints (in a general manner that is). It is, along these lines, why it's incredibly hard to develop general enough mechanistic explanations, which at the same time capture some part of the system's behaviour, and which actually give us

a useful simplification of the enormous amount of complexity in said systems.
Think for instance about what's necessary for the correct assembly of a large complex, such as the bacterial flaggela motor complex, with the description of said regulatory behaviour for sequential synthesis being present in Alon (2019)(pp 66-68). This description is based purely on the temporal transcription regulation of each gene in the corresponding operons. But what about spatial constraints, regarding diffusion, transport processes, etc? Harmon and Jülicher (2022) for example develop a model to argue about the control over correct assembly of macromolecular complexes, based on the properties of each compartment (and corresponding molecules being produced therein) present in the droplets formed by Liquid-Liquid phase separation (LLPS). These biomolecular condensates formed are membraneless organelles which appear to have very relevant functions in said kinetic and spatial control (see for instance Lyon et al. (2021); Banani et al. (2017); Holehouse and Alberti (2025)). And to think about kinetics in such context, regarding a heterogeneous, often not being able to be described as diluted, milieu; one needs to take into account the formation and control of these condensates (e.g. Kirschbaum (2022)), which will possibly affect the kinetics of other components in or out of such condensates (can concentrate catalysts and substrates in order to increase rates, sequester some molecules in order to prevent inhibition, etc). And although some of the triggers for condensate formation or dissolution seem to be rather specific, such that they can be described by appropriate mechanisms (see Söding et al. (2020) for two proposed ones - Enrichment-Inhibition and Localization-Induction), a good chunk of the interactions modulating such condensates are rather weak ones, which can be disrupted by thermal fluctuations on the order of k_BT. Majority of the molecules in such condensates seem to be multivalent, and such non-specificity seems to be attributed to intrinsically disordered regions.
This same idea of spatial control should also be present in the back of our minds, when approaching metabolism. In the often used abstraction of a metabolic *pathway*, it's also very important to consider how such catalysts, substrates and overall metabolites are being spatially constrained. One often thinks back to the notion of a metabolon as prime example of such, but perhaps such considerations of non-ideal behaviour (as assumed in mass-action kinetics) should be more widespread. See Lauber et al. (2023) for example for modelling work approaching how phase-separation (or similar spatial constraints) might prevent feedback-induced oscillatory behaviour in a metabolic pathway.

And yet, with ever more intricate ways of modelling said sub-systems, it doesn't seem like organizational closure is a capturable aspect. I'm to this extent unsure on how to see this concept being applied beyond what it gives right now: a very general reminder of how said sub-systems should be contextualized with the rest of

the system. It would be a gentle reminder along the lines of: "By the way, remember that the process or set of mechanisms which you are studying is part of an organism, which must maintain its autonomy. It must organize itself so as to produce every constraint from within the system."
In biology, one doesn't get "freebies" as for instance are present in physics with the roles of symmetries and corresponding conservation laws. If anything, the only property which is invariant is organizational closure itself, as mentioned before. And if the notion of invariance and corresponding symmetries is taken seriously here, then organisms are constantly breaking symmetries. To the extent, that organisms might be characterized as having *extended criticality* (Longo et al., 2014).

References

Alon, U. (2019). *An introduction to systems biology: design principles of biological circuits*. Chapman and Hall/CRC.

Banani, S. F., Lee, H. O., Hyman, A. A., and Rosen, M. K. (2017). Biomolecular condensates: organizers of cellular biochemistry. *Nature reviews Molecular cell biology*, 18(5):285–298.

Bechtel, W. (2011). Mechanism and biological explanation. *Philosophy of science*, 78(4):533–557.

Bich, L. and Bechtel, W. (2021). Mechanism, autonomy and biological explanation. *Biology & Philosophy*, 36(6):53.

Bich, L., Mossio, M., Ruiz-Mirazo, K., and Moreno, A. (2016). Biological regulation: controlling the system from within. *Biology & Philosophy*, 31:237–265.

Cárdenas, M. L., Letelier, J.-C., Gutierrez, C., Cornish-Bowden, A., and Soto-Andrade, J. (2010). Closure to efficient causation, computability and artificial life. *Journal of Theoretical Biology*, 263(1):79–92.

Craver, C. F. and Bechtel, W. (2007). Top-down causation without top-down causes. *Biology & philosophy*, 22:547–563.

Deacon, T. W. (2021). How molecules became signs. *Biosemiotics*, 14(3):537–559.

Di Paolo, E. A. (2005). Autopoiesis, adaptivity, teleology, agency. *Phenomenology and the cognitive sciences*, 4(4):429–452.

Egbert, M. D., Barandiaran, X. E., and Di Paolo, E. A. (2010). A minimal model of metabolism-based chemotaxis. *PLoS computational biology*, 6(12):e1001004.

García-Valdecasas, M. (2022). On the naturalisation of teleology: Self-organisation, autopoiesis and teleodynamics. *Adaptive Behavior*, 30(2):103–117.

García-Valdecasas, M. and Deacon, T. W. (2024). Origins of biological teleology: how constraints represent ends. *Synthese*, 204(2):75.

Glansdorff, P., Prigogine, I., and Hill, R. N. (1973). Thermodynamic theory of structure, stability and fluctuations. *American Journal of Physics*, 41(1):147–148.

Harmon, T. S. and Jülicher, F. (2022). Molecular assembly lines in active droplets. *Physical review letters*, 128(10):108102.

Hofmeyr, J.-H. S. (2017). Basic biological anticipation.

Hofmeyr, J.-H. S. (2018). Causation, constructors and codes. *Biosystems*, 164:121–127.

Hofmeyr, J.-H. S. (2021). A biochemically-realisable relational model of the self-manufacturing cell. *Biosystems*, 207:104463.

Hofmeyr, J.-H. S. and Cornish-Bowden, A. (1991). Quantitative assessment of regulation in metabolic systems. *European Journal of Biochemistry*, 200(1):223–236.

Holehouse, A. S. and Alberti, S. (2025). Molecular determinants of condensate composition. *Molecular cell*, 85(2):290–308.

Jonas, H. (2001). *The phenomenon of life: Toward a philosophical biology*. Northwestern University Press.

Kant, I. (1790). *Critique of Judgment*. Barnes & Noble, New York.

Kauffman, S. A. (2000). *Investigations*. Oxford University Press.

Kirschbaum, J. (2022). Chemical reactions as control mechanisms for biomolecular condensates.

Kopelman, R. (1988). Fractal reaction kinetics. *science*, 241(4873):1620–1626.

Lauber, N., Tichacek, O., Narayanankutty, K., De Martino, D., and Ruiz-Mirazo, K. (2023). Collective catalysis under spatial constraints: Phase separation and size-scaling effects on mass action kinetics. *Physical Review E*, 108(4):044410.

Letelier, J.-C., Soto-Andrade, J., Abarzúa, F. G., Cornish-Bowden, A., and Cárdenas, M. L. (2006). Organizational invariance and metabolic closure: analysis in terms of (m, r) systems. *Journal of theoretical biology*, 238(4):949–961.

Longo, G., Montévil, M., Longo, G., and Montévil, M. (2014). From physics to biology by extending criticality and symmetry breakings. *Perspectives on Organisms: Biological Time, Symmetries and Singularities*, pages 161–185.

Lyon, A. S., Peeples, W. B., and Rosen, M. K. (2021). A framework for understanding the functions of biomolecular condensates across scales. *Nature reviews Molecular cell biology*, 22(3):215–235.

Marshall, W. F. (2021). Regeneration in Stentor coeruleus. *Frontiers in Cell and Developmental Biology*, 9:753625.

Maturana, H. R. and Varela, F. J. (2012). *Autopoiesis and cognition: The realization of the living*, volume 42. Springer Science & Business Media.

Montévil, M. and Mossio, M. (2015). Biological organisation as closure of constraints. *Journal of theoretical biology*, 372:179–191.

Moreno, A. and Mossio, M. (2015). Biological autonomy. *A philo*.

Mossio, M. and Bich, L. (2017). What makes biological organisation teleological? *Synthese*, 194(4):1089–1114.

Mossio, M., Bich, L., and Moreno, A. (2013). Emergence, closure and inter-level causation in biological systems. *Erkenntnis*, 78:153–178.

Oderberg, D. S. (2021). Formal causation: accidental and substantial. In *Neo-Aristotelian perspectives on formal causation*, pages 40–61. Routledge.

Rosen, R. (1991). *Life itself: a comprehensive inquiry into the nature, origin, and fabrication of life*. Columbia University Press.

Slabodnick, M. M. and Marshall, W. F. (2014). Stentor coeruleus. *Current Biology*, 24(17):R783–R784.

Söding, J., Zwicker, D., Sohrabi-Jahromi, S., Boehning, M., and Kirschbaum, J. (2020). Mechanisms for active regulation of biomolecular condensates. *Trends in cell biology*, 30(1):4–14.

Soto-Andrade, J., Jaramillo, S., Gutiérrez, C., and Letelier, J.-C. (2011). Ouroboros avatars: A mathematical exploration of self-reference and metabolic closure. In *Advances in artificial life ECAL 2011: Proceedings of the eleventh European conference on the synthesis and simulation of living systems*, pages 763–770. The MIT Press, Cambridge, MA.

Tartar, V. (1961). The biology of stentor. *International series of monographs on pure and applied biology. Division zoology*, 5.

Varela, F. J. (2025). *Principles of biological autonomy*. MIT Press.

Voit, E. O. (2013). Biochemical systems theory: a review. *International Scholarly Research Notices*, 2013(1):897658.

Weber, A. and Varela, F. J. (2002). Life after Kant: Natural purposes and the autopoietic foundations of biological individuality. *Phenomenology and the cognitive sciences*, 1(2):97–125.