

■ Research Paper

The early days of autopoiesis: Heinz and Chile

Francisco J. Varela

CREA Ecole Polytechnique, 1 rue Descartes, 75005 Paris, France

The origin of the notion of autopoiesis is presented from the point of view of the basic intellectual background which gives its specificity, and from the role of the major actors involved in its articulation and its setting in Chile in the late 1960s. Heinz's role in this connection is exemplar, as he was an active, visionary and supportive participant in this evolving conversation.

Keywords autopoiesis; Chile; cognition; epistemology; Heinz von Foerster

My homage to Heinz will be a story. He has been a mentor, friend and inspiration for over thirty years, and there would be many stories to tell. But I think it is most appropriate to dwell on a particularly rich one: his role in the gestation and early days of the notion of autopoiesis. This is the first occasion in which I have publicly spoken about this period,¹ and although I think it is important to go beyond the individual's roles, there is also the background of ideas and of social context that makes science alive.

What does it mean when an idea like autopoiesis, in its strict sense, a theory of cellular organization, gains visibility and prominence beyond professional biology and becomes capable of affecting distant fields of knowledge? My answer is that ultimately we can only understand this phenomenon because the idea contains a *background of important historic sensibilities* with which it aligns and resonates. This background

of tendencies does not appear strictly delineated but rather as a retrospective, because ideas, like history, are possibilities to be cultivated, not a result of some mechanical determinism. At this distance, autopoiesis holds a privileged place, in my opinion, for having clearly and explicitly announced a tendency which today is already a force in many areas of cultural inquiry.

The tendency to which I refer, stated briefly, is the disappearance of intellectual and social space which makes cognition a mentalist representation and the human being a rational agent. It is the disappearance of what Heidegger calls the period of the image of the world and what could also be referred to as Cartesianism. If autopoiesis has been influential it is because it was able to align itself with another project which focuses on the *interpretive capacity of the living being* and conceives of the human as an agent which doesn't discover the world, but rather constitutes it. It is what we could call the ontological turn of modernity which, toward the end of the twentieth century, is taking shape as a new space for social interaction and thought, and which, undoubtedly, is progressively changing the face

¹The reader should be aware that this text has been adapted from a new Preface for the 20th anniversary edition of the Spanish edition of *De Maquinas y Seres Vivos: Una teoría de la organización biológica*, by H. Maturana and F. Varela, Editorial Universitaria, Santiago de Chile, 1994. Thanks to Kirk Anderson for his help in the translation.

of science. In other words, autopoiesis is part of a picture much larger than biology, in which today it holds a privileged position. It is this syntony with an historical tendency, intuited more than known, which is the core of the early ideas on autopoiesis and whose development I hope to trace.

The act of signing one's name to a text, more than claiming it as a personal possession, represents the placing of a milestone on a path. Ideas appear as movements of historical networks in which individuals are formed, rather than vice versa. Thus, Darwin already had Wallace waiting for him, and Victorian England as the substratum; Einstein alone in his Swiss patent office had dialogues with Lorentz against the backdrop of the world of German physics at the end of the century; Crick was already familiar with the ideas of Rose and Pauling when he met Watson, and his attitude was that of Cambridge in the 1950s. *Mutatis mutandi*, the history of autopoiesis also emerges out of prior work and is nourished by a unique substratum. It was all of Chile that played a fundamental role in this story.

Writing this story is, I insist, making a fold in history where men and ideas live because we are points of accumulation among the social networks in which we live, rather than individual wills or characters. One cannot claim to draw together the density of actions and conversations that constitute us in a necessarily unidimensional personal account. I don't pretend that what I say here is an objective narrative. What I offer is, for the first time, my own tentative and open reading of how the notion of autopoiesis emerged, and what has been its significance and development since. I have let everything I say mature over the years, and I believe it to be honest to the degree that I can take responsibility for being one of the direct participants in this creation, while maintaining an awareness that I cannot consider myself a holder of the truth.

To illuminate the background, I must begin with the roots of this story from my personal point of view. Paradoxically, only through recovering how the background appeared in the specificity of my perspective can I communicate to the reader the way in which this invention found its place on a broader horizon.

THE YEARS OF INCUBATION

I belong to a generation of Chilean scientists who had the privilege of being young during one of the most creative periods in the history of the Chilean scientific community, the decade of the 1960s. As a teenager, I had an early vocation for intellectual work and the biological sciences seemed my undoubted destiny. Upon finishing secondary school in 1963, I opted for the Universidad Católica which announced an innovative undergraduate programme in 'Biological Sciences' following the third year of Medicine. As a medical student, I got to know the first researchers who fascinated me, people such as Luis Izquierdo, Juan Vial, Hector Croxato and above all Joaquín Luco, who definitively infected me with a passion for neurobiology. Not far into my first year, I asked Vial if he would take me on as an apprentice in his cellular biology laboratory. He gave me the key to a little door to his laboratory overlooking Calle Marcoleta, where I spent my free time staining myelin on nerve cross-sections.

Juan Vial also gave me good advice, including his recommendation, in 1965, that I move to the newly opened Department of Sciences at the University of Chile to continue my training. It was a crucial step, because I left the world of traditional careers in order to fully enter the universe of exclusive scientific training, until then unknown in Chile. In a few borrowed classrooms on the top floor of the School of Engineering, I found my place to grow: a small group of young people excited about research and pure science, and researcher-professors who taught future scientists with passion.

Apprentice to a Neurobiologist

The last piece of advice Vial gave me was to find work with Humberto Maturana, who had just left the University of Chile's Medical School for the new Department of Sciences. On a beautiful day in April, 1966, I went to see him in his laboratory in the basement of one of the sections of the new school on Calle Independencia. At that time Maturana was already an important researcher, known for his work on the physiology of vision in several classic papers he had

written at Harvard and MIT before returning to Chile.² In Chile he continued to work on the physiology and anatomy of the retina in vertebrates.

To continue my apprenticeship in the trade, Humberto asked me to repeat experiments in electric recording on the optic tectum of the frog, which led me to investigate the problems of vision more deeply than I had ever done with a scientific problem. When I left the laboratory on Independencia to leave for the United States two years later, I had developed the ability to generate my first research ideas. One was a hypothesis about the role of time in the operation of the retina, which led to some experimental predictions which were the origin of my first scientific article.³ Maturana's influence was one of the pillars he gave me during my years of apprenticeship in Chile but it is important that I touch on at least two other influential currents which had and continue to have an enormous impact on my intellectual history. The first was philosophy and certain key readings I discovered during these years of training. The second was discovering the world of cybernetics and theoretical biology; in both areas Heinz's role was to become essential.

Philosophical Reflection

During my high school years my readings in philosophy were as passionate as they were random, mixing Aristotle, Ortega y Gasset, Sartre and Papini. In search of a more systematic training, when I transferred to the Department of Sciences in 1966, I also enrolled in philosophy at the old Instituto Pedagógico, and began to participate regularly in guided readings with Roberto Torreti in the Humanities Centre at the School of Engineering. The Institute's grand ideological controversies didn't interest me as much as what I could discover thanks to the classes of Francisco Solar, which resonated with the German training of Torreti, and which took form in the collections of the Centre's library.

² See in particular the 'classic': H. Maturana, J. Lettvin, W. McCulloch and W. Pitts (1960), anatomy and physiology of vision in the frog, *Journal of General Physiology* 43, 129–175.

³ F. Varela and H. Maturana (1970), Time course of excitation and inhibition in the vertebrate retina, *Experimental Neurology* 26, 53–59.

There I discovered European phenomenology and began a reading, which continues to this day, of Husserl, Heidegger and Merleau-Ponty. For the first time I seemed to find in these authors a preoccupation for the definition of the range of lived experience which I consider fundamental.

The second stunning discovery of these years was the social nature of science. I owe to Felix Schwartzman my early introduction to this world. In his course in the Department of Sciences, I came to know what until then was only known by a minority in Chile, the works of the French school in the history and philosophy of science: Alexandre Koyré (above all), Georges Canguilhem, and Gaston Bachelard. All of these authors express the counterintuitive conviction that scientific ideas are made and change abruptly, and not because of a lucky accumulation of 'purely empirical evidence,' that they are sustained with images and ideas which are neither given nor immutable, and that each age is blind to the foundation of what it considers certain and evident. The general public became aware of all this through Thomas Kuhn's famous book,⁴ which couldn't have existed without the groundwork of the French school whom Kuhn quotes with reverence. Barely 19, Schwartzman's guided readings on the mission of the scientist relieved me forever of my position as naive apprentice and turned me into a critic of what I was receiving as my professional training.

Brain, Machines and Mathematics

During that pioneering era, the Department of Sciences made few concessions when it came to training in mathematics. On my first day of class, without saying a word, the professor began to write: 'Let E be a vector space. The axioms for E are ...' After the initial shock of getting up to speed, I discovered in mathematics a language and a way of thinking that fascinated me. It was at this ripe time that I first encountered Heinz as exponent of mixing mathematics with brain studies. Although I didn't meet him in person until 1968, he became quite immediately a figure of great importance for me. His papers circulated

⁴ T. Kuhn, *The Structure of Scientific Revolutions*, Harvard University Press, 1970.

in the laboratory, with fascinating titles such as 'A Circuitry of Clues for Platonic Ideation'.⁵

In these sources I first realized a long tradition which seeks to express the properties of biological phenomena beyond their material particularities. As we all know now, it is a way of thinking that had only appeared in the 1950s, more specifically with the publication of *Cybernetics*, by Norbert Wiener (1962), and under the influence of another important person at MIT, Warren McCulloch,⁶ whom Humberto had met in 1959–60 when he was working at MIT. Wiener, McCulloch and von Foerster were the pioneers of the *conjunction* of epistemological reflection, experimental research and mathematical modeling. Only many years later was I able to appreciate these early days of cybernetics and the major role Heinz played in them as editor to the Macy Conferences.⁷

Entry Into Experimental Epistemology

Apprenticeship for the trade of neurobiologist wasn't the only thing going on in the laboratory. Humberto had entered a period of frank questioning of certain dominant ideas in neurobiology; discussion, reading and debate were daily events, spurred on by the presence of Gabriela Uribe, a physician of clear epistemological leanings who was working with Maturana at that time. Those were times of search and discussion focusing on what seemed a dissatisfaction, an *anomaly*. A basic dissatisfaction was the notion of information as the key to understanding the brain and cognition; the idea didn't appear to play an explicit role in the biological process. Humberto's intuition was that living beings are, as he said in those days, 'self-referred', and in some way the nervous system was capable of generating its own conditions of reference. It was a question of reformulating an orientation into an 'experimental epistemology', a wonderful term introduced by McCulloch. Gabriela and Humberto had begun a study of

certain chromatic effects similar to those described by E. Land in 1964, which were transformed into the topic around which a first attempt to reformulate visual perception as non-representational was based.

The days of my training in Chile were coming to an end. The Biology Department offered to support me in obtaining a scholarship from Harvard University to do a doctorate. I began to wrap up my student life in Chile aware that I was leaving with a clear focus in experimental epistemology and with three living pillars in my imagination.

Harvard and the Crisis of 1968

I left for Harvard in a Braniff jet on 2 January, 1968, reading a text by Koyré on Plato. I arrived in Cambridge in the midst of a snow storm, with no place to live, far from speaking fluent English, and with the threatening knowledge that if I didn't get straight A's, my scholarship would be taken away. The first few months were hard, but once settled, and getting to know my way around this new kingdom, I leaped head first into courses and seminars of all kinds: anthropology (studies on the natural ethology of primates were beginning), evolution (S. Gould had just arrived at Harvard and was a sharp contrast to the classicist, E. Mayr), mathematics (the theory of dynamic non-linear systems was discovered at this time), and philosophy and linguistics (Chomsky was the dominant figure along with Putnam and Quine). I found in Cambridge libraries until then only imagined, well stocked and open at all hours. I had the impression of having leapt into another galaxy, and I don't remember a single day in which I didn't feel like greedily absorbing everything at hand.

Long afterwards I realized, with great surprise, that compared to most of my doctoral classmates, my interests and vision of science were frankly more heterodox and mature. Beyond that, I realized that talking with professors about epistemological problems, as I was accustomed to doing in Santiago, was not looked upon favourably. The reaction was the same when I attempted to find a way to cultivate my interests in theoretical biology. The MIT of 1968

⁵For a selection of these and other articles see: H. von Foerster, *Observing Systems: Selected Papers*, Interscience, California, 1979.

⁶A selection of his most important work appeared in 1975: W.S. McCulloch, *Embodiments of the Mind*, MIT Press, Cambridge, MA.

⁷For an extraordinary account of the early days of cybernetics and the Macy Conferences see: J.P. Dupuy (1994), *Aux Sources des Sciences Cognitives*, Editions La Découverte, Paris.

had already disappeared, with McCulloch retired and no one to replace him. My only point of reference continued to be von Foerster, whom I visited several times at the Biological Computer Laboratory at the University of Illinois in Urbana, an active and productive centre which he directed in those years. It was easy to see that my intellectual quest would have to be divided in two: the official and the private.

Officially, I was studying under Keith Porter, in whose laboratory I learned to work in cellular biology, and Torsten Wiesel, who not long thereafter received the Nobel prize for his work on 'information processing' in the visual cortex. I focused my interest on comparative aspects of vision and began work on the functional structure of the eyes in insects, which would become the subject of my dissertation. By early 1970 I had already published four articles on the topic, and my dissertation was accepted in April of 1970.

Unofficially, outside the laboratory, I found myself for the first time living in a world infinitely more vast than that of Santiago, with young people from other cultures, in a place where nationalities and races blended. As fate would have it, those were the years of the mythic events that marked my generation. What began in Paris on the night of 10 May, 1968 corresponded to the movement in North America, centred around its opposition to the Vietnam War. The Kent State incident was followed by the first student strikes in which I took part. There were dramatic moments like the night the police forced us out of Harvard Yard. The Cambridge years were for me the discovery of my involvement as a member of society and the possibility of taking responsibility for changes in my social surroundings. It was a rediscovery of myself, far from my Latin American roots which my friends from The Movement exalted in the form of the Cuban revolution. I was not only occupied with science, but also with the dream of a new Latin America belonging to our generation.

Having discovered myself to be a social and political animal accentuated the need to maintain a public silence regarding my true interests. Faithful to the idea of science as an activity that is made and created by jumps and bold innovations, like other members of my generation, I cultivated the intention to return to Chile to create a different science, in which the anomalies

which had already appeared in Chile and were accentuated in the USA could be transformed into scientific practice. Creating my own original science seemed to me to define my obligation to my past and my roots.

I graduated as Doctor of Biology in June of 1970. Against the protests of my professors, I declined a post as a researcher at Harvard and another as Assistant Professor at another American university. I decided to accept a position offered to me by the Department of Sciences, justifiably interested in a return on the investment they had made in my training. I returned to Chile on 2 September, 1970, and Allende's election two days later seemed to be my second and true graduation. At last the work could really begin, with key problems well defined, with the certainty of being as competent and well prepared as anyone on the world scientific scene, and within the context of working in an environment that had a future to build. Having provided the backdrop of the situation in September 1970, I can return now to the specifics of the notion of autopoiesis and its gestation.

THE GESTATION OF THE IDEA

Examining the Problem

The direct antecedent to the gestation of autopoiesis is the text that Maturana wrote in mid-1969, originally entitled 'Neurophysiology of Cognition'. Humberto had continued along his own line of questioning regarding the inadequacy of the ideas of information and representation to understand the biological system. He visited me on several occasions in Cambridge and, as in Santiago, we had long conversations. In the spring semester of 1969, Heinz invited him to come to the Biological Computer Laboratory for a few months, an opportunity which coincided with the international meeting of the Wenner Green Foundation on the subject of 'Cognition: A Multiple View', a visionary title in light of the enormous development of what today are called the cognitive sciences, but until then were not considered a scientific field.

Humberto prepared the text for this meeting, providing for the first time a clear and attractive expression of his matured ideas, in order to

clarify what until then he alluded to as the self-referred nature of living beings, and to definitively identify the notion of representation as the epistemological pivot which had to be changed. From his point of view, it was necessary to centre attention on the internal linking of neuronal processes, and to describe the nervous system as a 'closed' system as the text states. This article marks an important jump, and to this day I still believe that it was the indisputable beginning of a turn in a new direction. I remember having visited Humberto in Illinois and having discussed several difficult parts of the text while he was finishing it. The text appeared shortly thereafter,⁸ and the article opens with a paragraph thanking Heinz and me for the conversations we had on the topic. Not long after that Humberto reworked the text into a more definitive version which came to be called 'Biology of Cognition'.

This text touches summarily on an idea that had been intriguing me for some time, and that as an assistant in the cellular biology course taught by George Wald and James Watson at Harvard had appeared to me as a clear anomaly: one talked about the molecular constitution of the cell, and used terms like self-maintenance, but no one, not even the two reunited Nobel prize winners, knew what was meant by that. What was worse was that when I pushed the discussion in that direction during lunch, the habitual reaction was a typical, 'Francisco, always getting into philosophy'. My notes from that time include several attempts to examine the basic autonomy of the cellular process as the basis of the autonomy of life. Toward the end of 1969, Jean Piaget's *opus magnum* entitled *Biologie et Connaissance*⁹ appeared in the window of Schoenhof's Foreign Books in Cambridge, in which he notes the clear need to reconsider biology on the basis of the autonomy of living systems, but Piaget's language and idiosyncrasies left me unsatisfied.

In his article, Humberto made the connection between the circular nature of neuronal processes and the fact that the organism is also a circular process of metabolic changes, as was

illustrated with reference to a recent article by Commoner in *Science* which discussed the new advances in the biochemistry of metabolism and its evolution. The question under examination then was: if we leave the organization of the nervous system to the side for the moment and focus on the autonomy of life in its cellular form, what can we say? This reflection on the circular nature of metabolism in living beings and its relation to cognitive operations, although barely filling a short page in the definitive version of 'Biology of Cognition', would be a focal point from which the development of the idea of autopoiesis would be drawn.

Those were the final months of 1970, I was back in Chile, and the Biology Department approached me about taking on the introductory course in cellular biology for new students. Maturana and I were now colleagues in the Biology Department, with neighbouring offices in the 'transitional' (but still used) stalls in the new campus of the Department of Sciences on Calle Las Palmeras in Macul. Everything was in place to launch the exploration of the nature of the minimal organization of the living organism, and we didn't waste any time. In my notes the first mature outlines appear at the end of 1970, and toward the end of April 1971 appear more details along with a minimal model which would later be the subject of computer simulation. In May of 1971, the term autopoiesis appears in my notes as the result of the inspiration of our friend José M. Bulnes, who had just published a thesis on the *Quixote* in which he made use of the distinction between *praxis* and *poesis*. A new word was appropriate because we wanted to designate something new. But the word only acquired power in association with the content our text assigned to it; its resonance reaches far beyond the mere charm of a neologism.

Those were months of almost constant work and discussion. Some of the ideas I tested with my students in the cellular biology course, others with colleagues in Chile. It was clear to us that we were embarking on a journey that was consciously revolutionary and anti-orthodox, and that this valour had everything to do with the mood in Chile, where possibilities were unfolding into a collective creativity. The months that led to the development of autopoiesis are inseparable from Chile at that time.

⁸P. Garvin (Ed.), *Cognition: A Multiple View*, Spartan Books, Washington, 1970.

⁹J. Piaget, *Biologie et Connaissance*, Gallimard, Paris, 1969.

During the winter of 1971, we knew that we were dealing with an important concept and we decided to put it in writing. A friend lent us his house on Cachagua beach, where we went twice between June and December. The days at the beach were divided between long walks and a monastic rhythm of writing which Humberto usually began and which I took over later in the day. At the same time I began a first draft (which Humberto revised) of a shorter article which would set forth the principal ideas with the aid of a simulation of a minimal model (which we called the 'Protobe'; more on this below). Around 15 December (again according to my notes of 1971), we had a complete version of a text in English called 'Autopoiesis: The Organization of Living Systems'. The typewritten version came to 76 pages, from which we made several dozen copies using the old blue ink mimeograph method. Although there were several later modifications, this text was to be published much later.

As has occurred often in the history of science, the creative dynamic between Maturana and myself resounded in an ascending spiral, to which a mature interlocutor contributed experience and previous consideration, and a young scientist brought fresh perspectives and ideas. As is clear given the circumstances, the ideas did not emerge in one or two conversations, nor was it a simple question of making explicit what had already been said. What was in the background must be considered a *qualitative* leap. Such transitions are never simple, nor is it possible to retrace exactly how they came about, because there is always a blend of past and present, talents and weaknesses, imagination and inspiration. The mature concept of autopoiesis did have, as we have seen, clear roots, but between an idea and its roots exists a crucial jump. And just as Franklin's work is not the double helix of Watson/Crick, nor that of Lorentz special relativity, the key ingredients of autopoiesis cannot be reduced to the mature expression of the idea, as is easily seen comparing the published texts. This is a limpid example of what I had already learned from my French teachers, that science has discontinuities, that it doesn't function by progressive empirical accumulation, and that it is inseparable from its social and historical context.

One Idea and Two Texts

As is inevitable, understanding unravels over the course of time and in proportion to its effects. So it isn't surprising that the text we finished toward the end of 1971 wasn't accepted immediately. In fact it was sent to at least five publishers and journals, and without exception they considered it unpublishable. I remember in January of 1972, my ex-professor Porter invited me to visit the new Biology Department at the University of Colorado—Boulder, where I gave an enthusiastic talk entitled: 'Cells as Autopoietic Machines'. The reception was cold and distant, as was that of my colleagues at Berkeley whom I visited around that same time.

The difficulties of finding a publisher, added to the political climate in Chile at the end of 1972, made me feel alienated from the international scientific world. At the same time, the enthusiastic reception of certain people whom I respected was of enormous value. The first to have a clear perception of the possibilities of the idea was naturally our friend Heinz in the United States, with whom we had been in constant communication and who came to Chile during those years. Another well-known cyberneticist and system theorist who reacted positively was Stafford Beer, who came to Chile on a regular basis. In fact, Fernando Flores had contracted him on behalf of the government to implement a revolutionary system of communications and regulation of the Chilean economy inspired by the nervous system, which came to be called *Proyecto Cinco*. Beer responded to what was set out in the text with such enthusiasm that we decided to ask him for a preface, which he agreed to write immediately. In January 1972, with a fresh copy of the manuscript, I was invited to Mexico by Ivan Illich, to his CIDOC centre in Cuernavaca. I gave him the manuscript the day I arrived, and I will never forget his reaction the following morning: 'This is a classic text. You have managed to put autonomy at the centre of science'. Through Illich, the text made its way into the hands of the famous psychologist Erich Fromm, who invited me to his home retreat to discuss the new concept, which he immediately incorporated into the book he was writing at the

time.¹⁰ In Chile itself, Fernando Flores and other colleagues from *Proyecto Cinco* were also an attentive public to our way of thinking. With Flores we formed what would come to be a fruitful friendship, and many years later autopoiesis would figure among the important concepts he would use to develop his own ideas. It is hard to express what finding receptivity in people of this quality meant to me at the time.

Meanwhile the text continued to be rejected from a growing list of foreign publishers. So it was natural to address our own university press, and at the end of 1972 we signed a contract that included the translation of the text by Carmen Cienfuegos. *De Máquinas y Seres Vivos: Una teoría de la organización biológica* was printed in April 1973. The original English text did not appear until 1980, when the idea had already acquired a certain popularity, in the prestigious series 'Boston Studies on the Philosophy of Science'. This version contained an introduction signed by Maturana, the text, 'Biology of Cognition', Beer's preface, and the text in question, 'Autopoiesis: the Organization of Living Systems'.¹¹ According to what the editor tells me, this book has been the collection's best-seller.

The brief article written in parallel to the longer text suffered a similar fate. As I mentioned above, in addition to a succinct presentation of the idea of autopoiesis, the intent of the article was to clarify the concept through a minimal case of autopoiesis. Toward the end of 1970 we had come to the conclusion that a simple case of autopoiesis would require two reactions: one of polymerization of membrane elements, the other, the 'metabolic' generation of monomers. The latter had to be a reaction catalysed by a third pre-existing element in the reaction. Once we had designed this reaction scheme, the next obvious step was to test a simulation of this minimal case (which soon came to be called the Protobe in our discussions) using cellular (or tessellated as they were called then) automata, introduced in the 1950s especially by John von Neuman. With the collaboration of Ricardo Uribe of the School of Engineering, the simulation

rapidly provided the results our intuition had led us to expect: the spontaneous emergence in this artificial bi-dimensional world of units which self-distinguished by means of the formation of a 'membrane', and which showed a capacity of self-repair. The paper was sent to several journals including *Science* and *Nature*, with results similar to those of the book: complete rejection. Heinz visited Chile in the winter of 1973, and helped us rewrite the text significantly. He took it back to the United States under his arm and sent it to the editor of the journal *Biosystems*, for which he was a member of the editorial board. The paper received some harsh commentary from reviewers, but not long afterward was accepted and finally published in mid-1974.¹² It is important to mention this article here because it was the first publication on the idea of autopoiesis in English for an international public, which led the international community to take charge of the idea. In addition it anticipated what twenty years later would become the explosive field now called artificial life and cellular automata.

Heinz's visit in July of 1973 took place in the midst of the approaching storm which plunged us all into an atmosphere of permanent crisis, with desperate attempts to stabilize a country that was breaking in two. As a militant supporter of President Allende's government, after 11 September, I found myself threatened. Military intelligence came to the Department with lists of ex-party members, and on two occasions night patrols came looking for me at my house, where I no longer slept. I was dismissed from my post at the university on orders 'from superiors'. With my family I decided to sell everything and leave. The majority of my colleagues in the Department of Sciences also dispersed throughout the world. With the diaspora of the Department's scientists ended a period of science in Chile, an important stage of my personal life, and with it the context which gave birth to the idea of autopoiesis. But naturally the idea would find new avatars, especially outside of Chile.

¹⁰His book *The Anatomy of Aggression*.

¹¹H. Maturana and F. J. Varela, *Autopoiesis and Cognition: The Realization of the Living*, BSPS, Vol. 42, Reidel, Boston, 1980.

¹²F. Varela, H. Maturana and R. Uribe (1974), Autopoiesis: the organization of living systems, its characterization and a model, *Biosystems*, 5, 187-196.

CODA

From my perspective in 1995, autopoiesis does not embody by itself a new vision of life and mind. Beside it appear other equally significant notions such as operational closure, enaction, natural drift and phenomenological methodology.¹³ The empirical references are consequently extended in new programmes of detailed research, be they lymphocyte networks, the motion of insects, or cerebral imaging. It is a question of an edifice of new epistemological concepts and empirical results which have breadth and stand up to rigour. They have been twenty productive years during which the period of the formulation of autopoiesis marks, in retrospect, an important milestone, as should be evident to the reader who has been patient enough to follow me this far.

But if this slow, sustained construction, full of *corsi e ricorsi* as is all intellectual and scientific creation, today has scientific viability, it is because it forms part of an historic sensibility which autopoiesis intuited in 1970–71. As I said at the start, there are no personal creations without a context: that an idea has impact is an historical fact and not a personal adventure or a question of 'being right'. Autopoiesis continues to be a good example of alignment with something which only today appears more clearly configured in various fields of the human cultural endeavour and which I identify with the term ontological turn. That is, a progressive mutation of thought which ends a long dominance of the social space of Cartesianism and

which opens up to the sharp consciousness that humankind and life are the conditions for the possibility of meaning and for the worlds in which we live. That knowing, doing and living are not separate things and that reality and our transitory identity are partners in a constructive dance. This tendency I designate as an ontological turn is not a philosophical mode, but rather a reflection of the life of all things. We are entering a new period of fluidity and flexibility which drags with it the need to reflect on the way in which humans make the worlds they live in, and do not find them already made as a permanent reference.

The occasion of writing this story twenty years later would be sadly wasted if I didn't manage to communicate the importance of expanding the horizon to consider the profoundly social and aesthetic nature from which this idea emerges, beyond science and biology, and beyond the people named as authors. In this sense, the story of autopoiesis has not gone out of date and still can be read backwards in time with some profit. It is definitively a scientific invention and all fields require actors who are sensitive to the anomalies which constantly surround us. These anomalies must be maintained in a state of suspension or cultivation while one can find an alternative expression which reformulates the anomaly as a central problem of life and knowledge. It is also an occasion for me to express my profound gratitude to Heinz, who was right there all along, a full participant in this moving conversation, and beyond that become a great teacher and dear friend.

¹³F. Varela, E. Thompson, E. Rosch (1991), *The Embodied Mind*, Cambridge, MIT Press.

Copyright of Systems Research is the property of John Wiley & Sons, Inc. / Business. The copyright in an individual article may be maintained by the author in certain cases. Content may not be copied or emailed to multiple sites or posted to a listserv without the copyright holder's express written permission. However, users may print, download, or email articles for individual use.