

## **Chapter II**

### **Idea is not enough**

A bottle thrown at sea is not always a good means of being saved. Time is coming to be as realistic as possible. There is a bridge between the theoretical works and the actual life: Investment. No matter how glorious and excellent an invention or a discovery is, in the different fields of science or others, as long as there is no money behind it, it cannot get into the arena of usefulness through a fabrication process to improve the real life of the ordinary people across the world.

The investments usually go toward what can bring back the sure profit. The outcome should be more money if a ground-breaking idea wants to be funded. This paradox has already determined the fate of thousands of great ideas, projects, inventions or discoveries that remained orphaned and isolated without having any chance to be in the service of millions of people in need. The fate of Nicholas

Tesla's works show a classic example of how the capital is decisive on what could be the actual future of some genius gifts.

We read this about him:

“Serbian-American engineer and physicist Nikola Tesla (1856-1943) made dozens of breakthroughs in the production, transmission and application of electric power. He invented the first alternating current (AC) motor and developed AC generation and transmission technology. Though he was famous and respected, he was never able to translate his copious inventions into long-term financial success—unlike his early employer and chief rival, Thomas Edison.”<sup>2</sup>

While the private sector is prominently profit-oriented, the public investment suffers from procedural overwhelming course, limited resources, strict accountability, and many other national or local

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<sup>2</sup> Source: [history.com/topics/inventions/nikola-tesla](https://www.history.com/topics/inventions/nikola-tesla)

urgencies that make it impossible to be generous regarding the innovative endeavors.

It is determined in this unfortunate situation the fate of humanity, with all the perils which are increasing and consequently put the future of all the globe and its inhabitants at risk.

A vicious circle is shaped and acting strongly:

- For saving humanity, you need innovative and creative thoughts.
- Such innovative and creative ideas need money to advance and to be achieved.
- Money is mainly focused on profit-generating proposals, not necessarily on the humanity-saving ones.
- Because the money is spent in the classic detrimental manner for more profit, the collateral harms are accumulated and the fate of the humanity is even further in danger.

This puzzle is old, but has been emboldened during the last two centuries; subsequently, we got where

we are now with a shaky world of global warming, drought, inequality, massive climatic migrations, and potential deadly atomic war, to name just a few ones.

Hundreds, if not thousands of excellent pitches, beneficial for humanity are left without any merited attention, just because they could not rise a good financial perspective for those who would be interested to fund them.

The importance of this reality for us resides in the fact that what we suggest as *Infinitism*, and its operative knowledge of *Infinitylogy*, could have the same ill-fated destiny than many other excellent plans to change fundamentally the way we deal with nature and the universe. There is no reason that the philosophical theory of *Infinitism* might have any impact on those who have wealth, scientific authority, and decisional power, to take it into consideration.

In order to see why many ideas are so neglected and not taken seriously by the individual or institutional

sponsors, we should first make a diagnosis and then learn about causes.

### **Pathology of unsuccessful brilliant ideas**

The causes of this issue can be classified into several categories. We should be reminded that the issue in question is *the lack of attention and interest in many fascinating, creative, innovative, and progressive ideas suggested by great minds during the last decades if not centuries.*

Three categories of the causes or reasons could be recognized:

1. **Lack of managerial and entrepreneurial skills** among the intellectually talented people,
2. **Lack of conformity** and compliance with the prevailing scientific creeds,
3. **Lack of consistent effort** to make the idea known publicly in order to attract the attention.

We add that there are also the technical, administrative or financial reasons proper to the

companies or governments that make them reluctant to take care of such revolutionary ideas. We briefly suggested some clues regarding these reasons above and wish to be focused only on these three deficiencies, coming from the providers of ideas and suggestions themselves.

### **1. Lack of managerial and entrepreneurial skills among the intellectually talented people**

One of the reasons for which we could not pledge a good outcome for our great ideas is the limitations related to our specialization. The great minds are usually focused on one specific turf of knowledge: physics, chemistry, mathematics, astrophysics, or philosophy. Getting a real expertise in these areas requires years and decades of learning, study and experiences. Sometimes, one should put lifetime to get a real proficiency that will lead to producing a non-existing idea or suggestion.

This focus on specific thematic makes that these folks don't know enough about other adjacent fields, and even less about other far areas of sciences or techniques. This is a normal fact in the highly specialized world of the twenty-first century. So, nothing atypical so far.

When we bring up the necessity of transforming ideas into projects, we are talking about a highly technical know-how. This is not the task for everyone. My cousin could be a talented mathematician, but a poor skilled person in the field of management, in general, and project management, in particular.

So, that's why we cannot automatically expect that every brilliant scientific or creative soul to also be an excellent organizer and adroit manager as well. There are for sure always some exceptions, but it's not enough to change the world.

So, what is the way out? What do we do when we have an outstanding idea in our specific field in which we have education and experience, but no organizational

talent to transform it into an entrepreneurial project? Where can one get complementary resources they need to assist their original ideas with corresponding research and studies in order to lead it to a successful outcome? Where can we find funds for hiring other needed specialist staff?

No miraculous answer to this enigma. We are going to do some suggestions and recommendations that are based on our in-course experience. An experience that has just started and requires a long way down to achieve something. But through this ongoing experience, we could give readers some ideas and clues.

Here is a summary of our own situation just as a real example for this first category of deficiencies with regard to the innovative ideas:

The theory of *Infinitism* presents the assertions that are philosophical, general and wide-ranging. These assertions are themselves based on the facts, but not necessarily in a definably systematic way. We need to



go far in the exploration and investigations in order to be able to demonstrate the accuracy of these assertions. This is the burden that *Infinitism* puts on the shoulder of *Infinitylogy*. The latter has a huge task: Checking out and verifying the declarations of *Infinitism* and showing if they are factually accurate, realistic and usable.

For that purpose, *Infinitylogy* should go through the following steps that interlace as well:

- Establishing itself as a credible multidisciplinary field of study,
- Organizing the collective investigations for going through the assertions of *Infinitism*,
- Reporting to the expert communities the results of its experimentations in order to complete them,

Each of these steps has many details and sub-steps:

- Establishing itself as a credible multidisciplinary field of study,

- Definition of *Informatology*
- Defining the framework of this new discipline
- Determining its theoretical principles
- Defining the principles of its methodologies
- Organizing the collective exploration for going through the assertions of *Informatism*
  - Planning the multidisciplinary studies
  - Employing specialized workforce
  - Managing the projects study
  - Completing with extra research, investigations and experimentation
- Reporting the results of its experimentations in order to complete them
  - Publishing internally the results of a research for the departments [of the CDRI<sup>3</sup>]
  - Gathering the internal feedback and preparing the final report

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<sup>3</sup> Center for Research and Development of Informatology (CDRI). See in the next pages the presentation of this center.

- Publishing the reports through credible channels
- Preparing the communal publication of the results

Now, by simply viewing these steps and their first degree of gears, we can see how composite the tasks awaiting our project look like. The question is can one person go through all of these phases and organize them in the best way possible? Maybe, a good managerial talent, with experience in this field, could lead this project professionally. But this manager needs resources for its achievement and completion. Where can we find these resources?

We can see a vicious circle is being shaped and can disappoint the genius thinker -that reads these lines- from pushing his or her ideas to a project and carry it on to its success.

In our case, you can see that, not only the writer had to formulate the idea of *Infinitism* but also had to suggest a new discipline to check it out and

demonstrate its reliability to those who will meet these suggestions.

I started it with a self-confidence related to more than 35 years of academic and professional activities as a prolific and bookworm student, a journalist, a researcher, a professor of university, a director of research, a writer, a translator, a political activist, a political analyst, a manager, and a political organizer. But the mission described above is anyhow much farther from any imagination for being realizable by one person. This is a teamwork mission and cannot be done otherwise, period.

But which team? Where are its members? Could we hope to find some benevolent folks as volunteers to do this job? Not really. We need consistency and discipline, and you cannot have it unless you could hire and wage people to get the job done by them through a sponsored and well-organized good project management process.

*Informatology* needs, in its exploration's teams, many physicists, biologists, mathematicians, philosophers, phenomenologists, computer engineers, AI specialists, software designers, and...

Accordingly, this was to prepare such a process that we established the CRDI (Center for Research and Development of Informatology) for having an organizing framework to follow the steps in an institutional way. The initial phase was not an easy enterprise, since we have had to do several consecutive tasks:

- to carry the basic studies and data gathering on,
- formulating a philosophical theory based on the collected materials,
- writing and developing this theory and conceiving its supportive discipline in some books,
- publishing these books, and then

- promoting them in such a way that they could catch the attention of some people so that they
- invest in our project and then
- use this investment properly to lead the project to a successful accomplishment.

Right now (at the end of 2021) we are just at the stage of promotion of our books so as to raise later the necessary funds for the CRDI. This would not be a laidback undertaking, and the only motivation is to believe in the necessity and even the emergency of what we are doing.

We will continuously report our progress and mention the barriers. This self-reporting is useful since it will also push us to analyze the process and the momentum through which we are moving forward. The important point is that we should never alienate the main goal of this theory:

*Finding the solution to use infinitude in action within matter and generating all*

*materials and energy we need to erect a new civilization in which there is no more pain, war, exploitation, and inequality. A new world where we don't torture or massacre animals to feed ourselves.*

This beautiful utopian image of that fair world will push us to thrive continuously for getting the project done. We should be reminded that any project needs motivation to stay alive and keep going despite numerous barriers and troubles it will meet; we are lucky at this regard since our source of motivation is also endless and *infinite*.

Enough sweet dreams! Let's go over the second category of deficiencies that make many gifted people give up to go forward in the transformation of their idea into project.

## **2. Lack of conformity and compliance with the dominant scientific creeds**

Most of the great ideas question and challenge many established scientific assertions that are shaping

science, academia, industry and economy. The established avowals that are largely accepted by the scientific, academic, and industrial communities outline a kind of worldview.

A worldview draws the boundaries defining the famous “truth”. Even though the scientists don’t like talking and recognizing something titled “truth”, the latter is actually there. Each branch of science actually establishes its own “truths” based on the main scientific general proclamations admitted as ‘truthful’. The scientific truths of the twenty-first century liken, of course relatively, what the ecclesiastical dogmas were at the dark ages: They are not to be challenged with no trouble or without risks. Those who want to be integrated in the scientific community for having a professional position, and a regular income for their life, should think twice before publishing a paper that will upside down the engrained scientific edicts.

*Galileo Galilei* and *Giordano Bruno* are two examples of those who dared to challenge the dogmas during



the middle ages. The former was on the brink of being set on fire for heresy and the latter was effectively so. Today, the academic and scientific institutions are not as cruel as the Inquisition's body of the Church, but they are just as tenacious when confirming and emboldening some of their so-called 'scientific Laws'. While the latter are well theorized and partially experimented, we should not forget that they embody also a set of beliefs that are collectively accepted, and because of this, to them, the obviousness of these rules is such that only the unreasonable minds would seek an alternative to them.

The scientific community is considering some formulas as almost 'absolute', even though they avoid skillfully to employ this term. Their flexibility is however changing since the new ideas became fertile and we have more and more substantiations that start shaking the scientific beliefs.

Some of these principles that could be questionable, even though not easily, are:

- The belief that *there are **Fundamental elements***;
- The belief that *there are the **Elementary particles***;
- The belief that *there is a **Fastest** speed in the universe*;
- The belief that *Time is a **dimension** of matter*;
- The belief that the Universe came to existence because of **Big Bang**;
- The belief that galaxies and clusters that are getting away from each other **are not interrelated**;
- ...

Challenging these beliefs is not an easy task, but it should be possible. Once we get to the necessity of such a challenge, we have then an intellectual duty to dare doing so.

The tricky point in this difficult undertaking is that if you want to defy the obvious scientific assertions, you should not limit yourself to the science itself, but to use also other fields that are capable of clarifying the

issues and provide strong arguments for your purpose.

In the construction of the theory of *Informatism*, we used the immense capacity of philosophy for a limitless and methodical conceptualization while we applied the many principles and experiments of science for demonstrating the philosophical inferences.

Therefore we said, for instance, that

- Matter is **infinitely divisible**.
- Matter is **infinitely composite**.
  - Matter is compositely infinite.
- Consequently, **there is no** ‘elementary particles’ or ‘fundamental elements’.
- **Everything is infinite** or is not.
- We could **change the infinite structure** of matter at any structural level.
- Through the intervention in the infinite levels of the fabric of matter, we can introduce the

changes we want, and get, subsequently, any materials we need and as much as we need.

Now, we should see how *Informatism* and *Informatology*, with these more or less subversive ideas, could avoid having the glum fate of a lot of other similar dissident ideas and proposals that had been, somehow, dead-born. For that, let's also go over the third category of deficiencies for the unsuccessful great ideas.

- **Lack of consistent efforts to make the idea know publicly in order to attract the attention**

What is the value of a great idea if you make it as an article or a book among millions of other articles and books published annually? How and why could the people -and particularly those who can use your ideas or fund them- know that you published a genius hint or suggested an excellent plan to save the planet? How do you want to differentiate your work from many others and attract the attention of those who

can assure the theoretical or practical growth of your wisdom?

When it comes to this point, we see the huge deficit among the genius community. The brilliant brains are habitually so fascinated and sunk in the beauty and magnificence of their intricate thoughts that the only thing they want is to go ahead with their thinking and dig it out more and more, which is a totally legitimate and understandable attitude. But it's not very helpful nor useful in a world that needs urgent action and immediate succor to avoid its projected downfall.

On the other hand, we know that the creative minds are not necessarily very managerial or entrepreneurial persons. They put a paper out and then remain in the hope that the published work will open its way to get the interest of those that should. But in the absolute majority of the cases, such a fairy event doesn't happen.

So what is to be done?

Based on this unfortunate destiny of the absolute majority of the genius ideas, we would like to conclude that nowadays, with this highly busy market of suggestions and proposals, dominated by a strong sense of profit-making, it's not enough to only generate excellent ideas or fabulous proposals; this is just one part of the task any responsible creative mind has to do, just a half. Another part is to find the ways to bring this idea up and set it at the disposition of those who should be working on it for three different purposes:

- At the disposition of the academic and scientific community to assess the theoretical value of the suggestion.
- At the disposition of the entrepreneurial community and venture capital in order to find interest in it, so as to become sponsor and investor for the development and the accomplishment of the idea.
- At the disposition of the general public in order for the chances to multiply by a general viral

course of sharing interest, public debate and increasing awareness in the society for the proposal.

Moving forward in these three directions is itself a huge task that might be not as exciting as the theoretical construction for a creative mind, but it's just as necessary as his or her intellectual efforts.

Is there any blueprint that the genius individuals could follow in order to make known to the public their excellent ideas and to attract scientific and entrepreneurial communities? As long as we know, such a guideline doesn't exist, or at least not per se; but what we are going to do in the following chapter is to present our own map for such a process. We will be suggesting some ways and techniques that could be a pattern for those who want to go ahead in this direction once they produce an original proposal susceptible to help make a better world.

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