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RESEARCH

Theoretical Physics and Theoretical Neuroscience: What Each Can Learn from the Other

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Abstract
This interview with Larry Abbott, a distinguished physicist who made his way from theoretical particle physics to neuroscience, explores similarities and differences between the theoretical scaffoldings of physics and biology and the respective modelling practices. Abbott reflects on the philosophical underpinnings and practical challenges of both fields, emphasising the role of mathematical modelling in understanding complex systems. He discusses the status and the unifying role of the Standard Model in physics, compares it to the theoretical framework at the basis of neuroscience, and addresses the sociological and methodological shifts required when applying physical principles to biological phenomena. The conversation also delves into the impact of recent advances in artificial intelligence on the theory of the brain, probing whether these technological developments enhance our understanding of intelligence and consciousness.

Keywords Computational neuroscience · Theoretical physics · Modelling practices in physics and biology · Artificial intelligence and the human brain · Sociology of science

Introduction

Can the rigorous laws of physics provide a blueprint for deciphering the mysteries of biology? From the methodological standpoint, what does modelling of physical phenomena teach us about modelling biological systems? Vice versa, how can the latter practice shed light on the limits of abstractions typical of physics? These questions set the stage for the conversation between Rocco Gaudenzi and Larry Abbott, a former theoretical particle physicist who has ventured into the realm of neuroscience. The interview touches upon a wide array of topics, from the fundamental nature of modelling in physics and biology to the implications of artificial intelligence (AI) on our understanding of the human brain. Abbott

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articulates the challenges and nuances of applying physical models to biological systems, shedding light on the philosophical and practical differences that distinguish these fields. He offers a perspective on the transition from dealing with the universal principles in physics to grappling with the complex, often less quantifiable aspects of biological systems.

Interview

Rocco Gaudenzi. What is in your experience the main difference you encountered between modelling a physical system and a biological system, e.g. in the realm of neuroscience?

Larry Abbott. Physics is often cited as the prime example of a science based on fundamental principles and laws, while neuroscience is described as lacking such a basis. Although there is undoubtedly some truth to this, on further inspection, the distinction may be more a matter of perception than substance. The standard model in physics, a beautiful theory in which forces arise from symmetries, describes all the known elementary particles and their non-gravitational interactions, which is certainly a strong fundamental basis. However, there are very few physicists who, when working on a problem, start with this theory. Instead, physics operates on a number of different tiers that employ widely diverging descriptions of the world. The apparent unity of physics relies on transitions from relativistic quantum field theory to non-relativistic quantum mechanics, to classical mechanics and classical electromagnetism, and to statistical physics. While these are descriptions of the same world, the transitions are far from trivial, especially those from the quantum to classical levels, with subtleties that give pause to most physicists. Furthermore, in practice there is little transference across these levels, people tend to work on one tier at a time. In a nutshell, all houses rest on foundations, but the question is whether this matters if one never goes to the basement.

At the other supposed extreme, neuroscience is said to lack fundamental principles, yet there is no doubt that all nervous systems work on the basis of interactions between chemical and electric mechanisms that are well understood. What is the difference between describing these interactions as the basis of neuroscience and describing the standard model as the basis of physics? Granted that this is philosophy, what about the practice of modelling in physics and modelling in biology? In both cases, one has to find the appropriate mathematical description that matches the questions being asked to the depth and amount of information known, and to proceed from there. In both cases, the goal is to extend the known into the unknown, to find relationships between things that previously seemed unrelated, and to focus future study on the most interesting aspects of a system. In both cases, it is critical to find the right level of abstraction of the underlying problem that allows progress to be made despite holes in our knowledge and without trivialising the system. In both cases, the mathematics to be used is that of differential equations, various approximations, simulations, and all the usual tricks of analysis. All this is not to deny that there are differences between the two fields, especially with regard to the area of physics I worked in, theoretical particle physics, although even that is time-dependent.

Rocco Gaudenzi. Could we then say that part of the difference between modelling in the fields of physics and biology lies in the fact that the two are at two different stages of development (at least in some specific domains)? Was it a challenge to shift from a physics modelling mindset to modelling biological systems? What did you appreciate and miss in the two activities?

Larry Abbott. Regarding the first question, I will give you an example from my personal experience. In the period before I was a particle physicist, many theorists had given up on a fundamental description of the forces and particles involved in strong interactions and, instead, relied on analytic properties of scattering matrices to compute cross-sections and other quantities of interest. I suspect particle physics at that time was more like theoretical neuroscience now in that the process consisted of making some assumptions and seeing what worked. However, by the time I entered the field, the standard model existed and attention was directed to determining whether that specific model, a relativistic quantum gauge theory called quantum chromodynamics, was correct. Thus, there was a huge amount of attention directed onto a specific "fundamental", all-explaining theory.

In fact, the way I got into neuroscience was through another model that, while by no means a "standard" model, was the subject of great attention among people with physics training, namely the Hopfield model (Hopfield, 1982). Sociologically, this was a great thing. It meant that a group of us were all working on related things, which made it easy to communicate and to follow developments, and allowed us all to build on the ideas of others. Thus, this new experience was similar to my previous experience in particle physics in that both involved working on a well-defined model with a group of like-minded people.

Then, I started really doing neuroscience. This required a switch from a mentality in which you knew the model and had to figure out how to apply it to a particular situation (which, in the case of particle physics, is the hard part because quantum field theory is difficult), to one in which you had to create the model for the occasion (the hard part) and then apply it to the situation–in general, the easy part.

A natural question is whether I miss having, or at least the illusion of having, an elegant and concise "underlying" theory now that I work in neuroscience. In my view, the realisation that forces can be derived from symmetries, and the development of quantum mechanics on which this realisation relies, are the most remarkable things that humans have ever done. At the same time, I have to acknowledge that this profound observation is of little relevance to everyday life. Neuroscience, on the other hand, has no similar cosmic theory, but it does have relevance to everyday experience. I have come to appreciate this and to enjoy the fact that in neuroscience we create models and new descriptions all the time.

Rocco Gaudenzi. Renormalisation group methods isolate the micro-properties which are relevant for macroscopic behaviour from those that do not matter, and is thus premised on some level of scale separation. Is an analogue coarse-graining possible in biological systems or one should be guided by a different principle? Do you envision a theory of brain phenomena that leads from microscopic mechanisms to universal behaviour?

Larry Abbott. The renormalization group approach, a triumph of physics that allows certain properties of a system (e.g. critical exponents) to be computed while requiring minimal knowledge of the system being analysed, provides a good example of differences in philosophy between physics and neuroscience. It is undoubtedly cool to get something for almost nothing. However, what if someone is actually interested in the details of the system being analysed. This person would say that the renormalization group approach has perversely found precisely the quantities whose measurement gives you the least amount of information about the system. I don't know if this exactly characterises the difference between a physicist and a biologist, but it points out that there are two points of view.

The art of modelling is in knowing what to leave out, so all modelling involves some form of coarse-graining. In biological modelling you just have to accept that what you have thrown out in coarse-graining is just as interesting as what you have kept. The goal of many physicists—and I include myself in the group—is to know much without it requiring much of a memory load and, indeed, a physicist can teach the entire first term of introductory physics knowing only Newton's laws and a few force laws and the entire second term knowing only Maxwell's equations. Biology is not like that, but this is for a wonderful reason. Its beauty is understandable, but it is, in some sense, incompressible.

Rocco Gaudenzi. Does that imply that, in general, in modelling biological systems one has to give up more as compared to modelling physical systems?

Larry Abbott. I will answer you by considering that another perceived difference between physics and biology is that there is a widespread feeling that physics gets simpler as one moves down to the "fundamental" particle level, whereas the transition from organisms to their molecules does not appear to make anything simpler. This difference may also be an illusion. It is easier to compute the path of a brick through the air than to compute the magnetic moment of an electron. The dichotomy mentioned in the previous paragraph really comes from whether or not one is willing to ignore many features of a system in pursuit of a few. This is, of course, done in neuroscience and biology as well as in physics, but it is more painful to do it when studying biological systems. The beauty of life is more than skin deep, it extends all the way from the beauty of an animal to the beauty of the molecular mechanisms that keep it alive and, importantly, everything in between. Physics is thought to have "deserts", regions of scale where little of interest happens. Biology doesn't have such deserts.

Rocco Gaudenzi. How have recent advances in AI contribute, or might contribute, to a comprehensive theory of the brain? How do they shed light on thought and the concept of intelligence?

Larry Abbott. In a discussion of different approaches to understanding, certainly one now has to include artificial intelligence as a major new influence. "Give me a lever and a place to stand and I will move the earth" has become "Give me some GPUs and a trillion parameters and I will create intelligence", the difference being that Archimedes did not actually move the earth. The interesting philosophical question here is whether the advances of AI represent a new type of understanding which is far from the model of understanding provided by physics. Richard Feynman said "What I cannot create, I do not understand", but now we must ask "If I can create it, do I understand it?" We now know that a system with a sufficient number of parameters can write text at a human level, and the human brain is one such system. It is not clear that the question of how the parameters got set is within the domain of neuroscience rather than the science of evolution, but the question of how the system works is. For neuroscience, the system is the brain, so the question of how that particular system works will remain no matter how impressive and relevant the advances in artificial intelligence.

Rocco Gaudenzi. Where do you think that physics-inspired computational neuroscience will head to in the future? Are there some physics concepts and tools that have not (yet) been implemented into it?

Larry Abbott. It is wise for the reader to keep in mind that the physics to which I have been comparing theoretical neuroscience is the physics of 40 years ago. This is not just because of my age, but also because this is the version of physics that seems to be in people's minds, even younger people, when they make such comparisons. But physics has changed. Much of what used to be particle physics now involves speculations about theories for which we have no experimental evidence, except in so far as it is negative. This should not serve as a model for neuroscience or, probably, any other type of research. My advice is to stop looking at physics, either the physics of the present or of the past. The tools of physics are the tools of applying mathematics to nature. We have a huge bag of such tools, probably, if you dig around deep enough, a sufficient number to carry us far into the future. What we need is the art of finding the right level of abstraction (all models are abstractions) to experimental data obtained at the right level of detail, with "right" defined, not by philosophical considerations, but by the question being asked. Thus, in the end, it all comes down to asking the right question.

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