Does Stanford's induction apply to engineering sciences?

Lee Tao, Pellone Samuel
Informatique, Génie Mécanique, EPFL

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Supervised by

Esfeld Michael-Andreas, Philosophy of Science
Egg Matthias, Philosophy of Science

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Abstract

Recent works in scientific realism by Stanford (2006) and Chakravartty (2008) have generated interests in the research of a more selective and sophisticated scientific realism. Such debates advance the philosophical development of scientific realism, and arguably shape scientific realism toward a more refined description. Among many scientific theories discussed by Stanford (2006) and Chakravartty (2008), we have found their examples excluding those from engineering sciences, and this attracts our attention. In this paper, we first give an overview of Stanford's induction. Second, we analyze the arguments presented by Stanford (2006) and Chakravartty (2008) in depth to explore possible reasons why engineering sciences are seemly out of the scope of the debates. Third, we re-examine the philosophical debates by Stanford (2006) and Chakravartty (2008) in the context of an interesting case in mechanical engineering, d'Alembert's paradox, presented by Grimberg (2008). We conclude in the final section on the validity of applying Stanford's induction to engineering sciences.

Keyword: Realism, Antirealism, Underdetermination, Pessimistic induction, Stability, Laboratory sciences, Engineering sciences, d'Alembert's paradox

1 Introduction

Contemporary science has reached great practical achievements. Stanford (2006) describes an example of a shower radio. As simple as it might seem, it would not function unless the theories we used to build it, concerning radio waves, electricity, acoustics, and many other technical areas, are accurate descriptions of how things function in nature. It seems fair to say that the fundamental claims of scientific theories must be at least be roughly accurate, under the consideration of the fact that we are sure to make some errors in the details. This line of thinking has been embraced by not only people of good common sense, but also professional philosophers. The accepted view is formally known as scientific realism: the position that the central claims of our best scientific theories about how things function in nature must be at least approximately true.

A strong support of this line of argument was first formulated by science philosopher Hilary Putnam, claiming that "the positive argument for realism is that it is the only philosophy that doesn't make the success of science a miracle." As described by Stanford (2006), the central idea of this "miracle argument" is that *"the only satisfactory explanation for the success of our scientific theories is that they are (at least approximately) true in the most straightforward sense of the term: any other view of the matter leaves it a complete and utter miracle why our best scientific theories are so successful."* Further developments of this line of arguments have been done by many philosophers, Popper; Smart; Boyd; Musgrave; Leplin; Psillos; Kitcher and others. Because of its evident power and appeal, it has been the strongest support of the realists position, as described by Stanford (2006).

Despite the wide influence of the realism arguments, competing considerations have encouraged some philosophers to question the rationale for the realist position. As Stanford (2006) describes, our generation is not the first to develop theories about the nature world. By induction, the history of science consists of a succession of past histories that made radically different claims compared to our own do about the fundamental constitution and workings of nature. Numerous claims that were ultimately shown to be false, despite just the same kinds of accomplishments as in present theories. Contemporary philosophers of science call this argument the Pessimistic Induction (PI): *"the scientific theories of the past have turned out to be false despite exhibiting the same impressive virtues that present theories do, so we should expect our own successful theories to ultimately suffer the same fate. In other words, if the history of science really consists of a succession of increasingly successful theories making radically and fundamentally different claims about the principles of nature, why would we suppose that this process has come to an end with the theories of the present day?"* This method of historical induction is strong, and several inductive arguments against scientific realism have found similar roots as we will see later.

For instance, Larry Laudan has used a similar appeal to the historical record of scientific inquiry. As described by Stanford (2006), he argues that the historical record testifies that innumerable past scientific theories have been remarkably successful, although not true. One wonders if our own theories will meet the same fate. This in turn shows why the realist's inference from the success of a scientific theory to its truth is unwarranted. Notice that defenders of past scientific theories occupied at one time just the same position that we do now: they thought the evident success in prediction, explanation, and intervention afforded us by, Newtonian mechanics for example, rendered it impossible that the theory was false. *"If Newtonian mechanics weren't true, they might have said, then it would have to be a miracle that the theory is so successful and offers such accurate predictions and convincing systematic explanations concerning diverse physical phenomena ranging from the flight of cannonballs to the orbits of the planets. But they were wrong, and Laudan suggests that we would be equally wrong to draw the same conclusion about the successful theories of our own day."* Notice that the example of Newtonian mechanics serves as a motivating example here, and it will also be a testing ground for many arguments in this paper.

In addition to PI, the other central consideration used to challenge the defense of scientific realism is called the Underdetermination of Theories by the Evidence (UTE). *"It is essentially concerned with the possible existence of alternatives to our best scientific theories that share some or all of their empirical implications — that is, quite different accounts of the entities and processes inhabiting some inaccessible domain of nature that nonetheless make the same confirmed predictions about what we should expect to find in the world. No matter how impressive a theory's practical achievements in guiding prediction and intervention are, those achievements do not favor the theory over any alternative that would enjoy just the same degree of empirical successes."* And some philosophers of science have sought to show that every scientific theory must have what they call empirical equivalents:

alternatives sharing all and only the same empirical implications. This line of reasoning, however, is inherently flawed as argued by Stanford (2006).

We hope the several lines of reasoning around scientific realism are now clear. As described by Stanford (2006), *"the concerns about the truth of our best scientific theories prompted by PI and UTE are likely to surprise many thoughtful people. How could it really be like that? Surely our best scientific theories couldn't really be so successful in their practical applications without being at least approximately true in their fundamental claims about nature."* But in fact, the history of science itself provides an abundant treasure trove of examples illustrating just what it is like for scientific theories to enjoy substantial empirical success while being profoundly mistaken in their most fundamental claims about nature.

We revisit the motivating example of Newtonian mechanics and its relation to relativity theory. As described by Stanford (2006), *"gravity is not merely a force exerted by massive objects on one another, but instead reflects the curvature of space-time itself. Gravitational motion is not like two marbles being pulled toward each other by invisible strings, but is instead like two marbles rolling from the lip to the bottom of a shallow bowl, where the bowl represents the deformation of the fabric of space and time itself produced by the masses of the marbles. Nonetheless, for a wide variety of purposes and in a wide variety of contexts, it is extremely useful to think of the world as if Newtonian mechanics were true."* Indeed, Newtonian mechanics is still the physics we use to send rockets to the moon, because it is much simpler to work with than the contemporary alternatives and the empirical predictions. Calculations within the framework of rockets and moons turn out to be quite accurate despite the fact that Newtonian mechanics is profoundly mistaken about the fundamental constitution of nature. *"Although this sometimes invites the counter-claim that Newtonian mechanics itself is "approximately true," this can only mean that its empirical predictions approximate those of its successors across a wide range of contexts. It cannot mean that it is approximately correct as a fundamental description of the physical world. In this respect, Newtonian mechanics is just plain false. And this recognition invites us to ask whether it might not be that all of our own scientific theories are both fundamentally mistaken but at the same time empirically successful in just this same way?"* Given a strong link between Newtonian mechanics and engineering sciences (e.g. mechanical engineering, civil engineering, micro engineering), we are tempted to ask whether engineering sciences are also fundamentally mistaken and nonetheless empirically successful. This line of reasoning will be thoroughly discussed in the following section.

Although fundamentally appealing, PI arguments seem to have reached a stalemate in the philosophy of science. Realists respond to the challenge of the pessimistic induction by pointing out ways in which at least some contemporary theories are indeed distinct from their predecessors, and they therefore reject the validity of the inductive projection from past to present cases. In response, defenders of the pessimistic induction demand to know why just these varieties or degrees of success are special. How should that defuse the proposed induction, noting the existence of earlier varieties and degrees of predictive and explanatory success that (1) were equally thought to be explicable only by the truth of the theories, and that (2) turned out not to be so? As a result, each side simply seems to shift the burden onto the other, which is not altogether convincing.

UTE arguments are in a similar stalemated situation. The arguments offered on both sides of the challenge posed to scientific realism by UTE have not generated anything refreshing. The problem of UTE grows out of the worry that there might be alternatives to even our best scientific theories that would make the same predictions in the cases we have tested, and would therefore be no less well confirmed than our own present successful theories. However, as Stanford (2006) describes, *"it is quite difficult to decide how seriously we ought to take this frankly speculative possibility. In the absence of any evidence, why should we either assume that such alternatives exist, or let the bare possibility that they might exist prevent us from believing the best-confirmed theories we do have?"* It seems sensible for philosophy researchers (Kitcher, Leplin, Achinstein) to insist that any such alternatives actually be produced before we take them seriously about the truth of the contemporary theories.

Facing this stalemated situation, Stanford (2006) devises a new argument to combine the strengths of PI and UTE without falling into the fallacies of both. For UTE arguments, he believes these researchers have concentrated their attention and argumentative efforts on the rather trivial forms of UTE that they can prove to obtain universally, and in the process they have abandoned the effort to show that UTE obtained should actually lead us to question the truth of our best scientific theories. More specifically, *"defenders of UTE have sought to provide us with a procedure (ideally an algorithmic or mechanical procedure) for generating empirical equivalents to any theory at all. They have sought to articulate a procedure for generating alternatives to absolutely any theory that will have precisely the same empirical implications as the original."* It is this strategy of trying to defend the significance of UTE by showing that all theories have empirical equivalents that Stanford (2006) suggests constitutes the inherent flaws for defenders of UTE, for it succeeds only where it gives up any significant challenge to the truth of our best scientific theories. Instead, Stanford (2006) suggests that the historical record offers plain-spoken inductive evidence to the fact that we have repeatedly occupied a position of under-determination across a wide variety of scientific fields. It is because we have repeatedly failed to conceive of all the empirically inequivalent theoretical possibilities that are well confirmed by the evidence available to us. Recalling that PI notes that past successful theories have turned out to be false and suggests that we have no reason to think that present successful theories will not meet the same fate. By contrast, Stanford (2006) proposes the new induction over the history of science–the Problem of Unconceived Alternatives (PUA): *"we have, throughout the history of scientific inquiry and in virtually every scientific field, repeatedly occupied an epistemic position in which we could conceive of only one or a few theories that were well confirmed by the available evidence, while subsequent inquiry would reveal further, radically* *distinct alternatives as well confirmed by the previously available evidence."* One interesting aspect of argument is that Stanford (2006) describes his induction as valid for virtually every scientific field, we would examine this statement closely for engineering sciences in the following sections.

PUA is certainly not perfect. Since the historical record offers at best evidence that we presently occupy a significant under-determination position, rather than a kind of demonstrative proof that advocates have traditionally sought, as admitted by Stanford (2006). Furthermore, unlike constructing empirical equivalents, it does not allow us to say just which actual theories are under-determined by the evidence. On the other hand, Stanford (2006) suggested that *"the search for empirical equivalents has managed to provide convincing evidence of an UTE only where it has transformed the problem into one or another familiar philosophical puzzle. These forms of UTE simply do not threaten to bear out the original concern that the very same evidence leading us to embrace our own scientific theories might turn out to support alternative theories. Abandoning the fascination with them in this connection seems a small price to pay for returning our attention to the kind of UTE that the historical record suggests might pose a substantial challenge to even our most successful scientific theories about nature. Thus, PUA concerns alternatives to our best scientific theories, but not in the same way that the search for UTE does. Furthermore, it draws its force and evidence of its significance from the historical record of scientific inquiry, but not in the same way that PI does."* At its heart is neither the simple historical revelation that even the best scientific theories of any given prior epoch have turned out to be false, nor the concern that we might be able to generate alternative theories. Instead, PUA concerns itself with theories that we should take seriously as competitors to our best accounts of nature if we knew about them, but that are excluded from competition only because we have not conceived them.

PUA has certainly generated new challenges to the realist position, but the realists are not without counteractions. As proposed by Chakravartty (2008), *"the best response to PUA is simply to grant PUA, or to grant that the phenomenon of unconceived alternatives is a fact of scientific life, but to dispute Stanford's argument that this spells the death of realism. Of course scientists do not typically conceive of all promising alternatives to their own theories, and today's scientists are no exceptions. The real question of interest here is whether there is anything like a principled continuity across scientific theories over time, which would allow realists to claim certain aspects of theoretical descriptions as approximately true."* Indeed, this is what realists generally think: our best theories today are our best attempts to describe the world thus far; in the meantime, we have good reason to believe that certain aspects of today's theories will remain true.

There are two main foci in the subsequent sections of this paper. In section 2, we investigate the examples of engineering studies that are immune to Stanford's induction, and their implications on the stability of engineering sciences. We are particularly interested in characterizing the stability of engineering sciences. In section 3, we introduce an interesting case in mechanical engineering, the d'Alembert's paradox, that might challenge the conventional view on the stability of engineering sciences. In section 4, we re-examine the philosophical debates by Chakravartty (2008) and Stanford (2006) in the context of this paradox, and perform relevant philosophical analysis. We conclude in section 5 by combining arguments drawn from cases that support the immunity of engineering sciences to Stanford's induction, and reflections from the d'Alembert's paradox that some cases still require more careful philosophical analysis. We hope these complementary ideas would lead us forward in the understanding of Stanford's induction and engineering sciences.

2 Stanford's induction and engineering sciences

Recent works in scientific realism by Stanford (2006) and Chakravartty (2008) have generated interests in the research of a more selective and sophisticated realism. Such debates advance the philosophical development of scientific realism, and arguably shape scientific realism toward a more refined description. Among many scientific theories discussed by Stanford (2006) and Chakravartty (2008), we have found their examples excluding those from engineering sciences, and this greatly attracts our attention. Might this idiosyncrasy lead people to suspect that examples considered by Stanford form an unrepresentative sample or that there is something about the characteristic of engineering sciences that renders them especially immune to PUA?

Let us now revisit the motivating example of Newtonian mechanics mentioned by Stanford (2006) and consider the science of sending rockets. As mentioned by Stanford (2006), Newtonian mechanics is still the physics we use to send rockets to the moon, because it is much simpler to work with than the contemporary alternatives and the empirical predictions it makes at the scale of rockets and moons turn out to be quite accurate despite the fact that it is profoundly mistaken about the fundamental constitution of nature. Does this mean that rocket science is immune to Stanford's induction because the range of operation is quite accurate to use Newtonian mechanics to make empirical predictions? Also mentioned by Stanford, that PUA might work for virtually every scientific field. If so, what is the PUA for rocket science? This line of reasoning would form the main arguments of the following paragraphs. We would examine several fields of engineering sciences and try to compare their situations with rocket science and Newtonian mechanics, and also we try to see if there is a PUA in each of these engineering disciplines.

To find the reasons why engineering sciences are excluded from Stanford's induction, one simple answer might be engineering sciences rarely touch the philosophical foundations of sciences, and thus they are immune to the concerns of the debates. But this only scratches the surface of a serious question. Engineering sciences systematically apply theories developed by fundamental sciences to a wide variety of fields, and we see how examples of applied sciences enrich and extend fundamental sciences. Shockley, Bardeen, and Brattain invented the junction transistors in the middle of twentieth century, and their invention was initially conceived as achievements of engineers. But this invention initiated active research in material physics and condensed matter physics, and led to better understanding of many useful electronic materials with wide range of applications. If Stanford's induction could apply to the foundations of atomic science, namely, the interpretation of electrons and atoms, could we say the theories adopted by microelectronics researchers are quite accurate in the range of operation presently concerned but are profoundly mistaken about the fundamental constitution of nature? Is there a PUA in microelectronics research community?

For microelectronics research, the situation is relatively similar to that of rocket science and Newtonian mechanics. When it is sufficient not to use advanced quantum mechanics or quantum field theory, semi-classical electron theory is used to make empirical predictions on the behavior of microelectronic materials. In this regard, the theory makes quite accurate prediction in the scale concerned but is fundamentally mistaken about the constitution of nature. The PUA introduced by advanced quantum mechanics and quantum theory are certainly not in the range of interest. Although models and theories made on new microelectronic materials could be flawed, the range of operation and the successful prediction offered by semi-classical electron theory seems to provide some stability. A PUA might exist for microelectronic research community, but the historical induction part of the PUA proposed by Stanford (2006) should be re-examined as the history of microelectronics are unlike the history of physical sciences.

Let us now move to another field of engineering sciences built upon microelectronics. Kilby and Noyce invented integrated circuits in 1960s, and this led to mass production of small, stable, and cheap integrated circuits for digital computers. Henssey and Patterson invented the Reduced Instruction Set Computer (RISC) to make better use of these integrated circuits for high-performance computers, and this led to active research in computer architecture from 1980s until today. Microelectronics forms the foundations of computer architecture, and researchers closely observe the advance of microelectronics. If Stanford's induction could apply to the foundations of microelectronics, it would certainly be worrisome for computer architecture researchers. However, this problem has not been widely recognized by the computer architecture research community. Could we find a PUA in computer architecture research community?

For computer architecture research, the situation is different from that of rocket science and Newtonian mechanics. Computer architecture research community does not rely on basic theory developed by physical sciences. Instead, computer architecture research community relies on quantitative performance measurements and proven design principles not directly related to the constitution of nature. We could not find much similarity between this engineering discipline and the examples discussed by Stanford (2006). A PUA might exist for computer architecture research community, but the historical induction part of the PUA proposed by Stanford (2006) should be re-examined as the history of computer architecture is drastically different from the history of physical sciences.

Let us now move to another field of engineering sciences built upon computer architecture. The advance of computer architecture greatly increased the speed of computation and presented challenges to the security of information as computers could be used as code-breaking devices. Rivest, Shamir, and Adleman proposed the RSA public-key cryptography algorithms in 1978 to produce information protection scheme that is computationally hard to break with today's most advanced computers. The advance of computer architecture plays a critical role for information security, and researchers closely observe the development of the most advanced computers. If Stanford's induction could apply to the foundations of computer architecture, it would certainly be worrying for information security researchers. And indeed this problem has been widely recognized by information security research community as the post-quantum cryptography and is expected to revolutionize the state-of-the-art cryptography. Does quantum computing resemble a PUA in information security?

For information security research, the situation is not the same as that of rocket science and Newtonian mechanics. Information security research relies on mathematical formulae and computational models not directly related to the constitution of nature. However, some assumptions of the models are related to the laws of physics. For instance, it is widely know that some information security researchers perform research based on the assumption that quantum computers would not be realized in short time; other researchers perform research on the crypto-systems assuming quantum computers would become a reality. Could we say quantum computing is a PUA for information security research? We believe not in the same sense given by Stanford. Since the problem with present crypto-system dealing with quantum computing has already been conceived long before the realization of quantum computers. This historical trajectory is somewhat different from the cases given by Stanford's induction. A PUA might exist for information security research community, but the historical induction part of the PUA proposed by Stanford (2006) should be re-examined as the history of information security is distinctive from the history of physical sciences.

The issue that no sufficient data could support Stanford's induction applying to the afore-mentioned engineering sciences could not be satisfactorily explained by the link of these sciences to the foundations of physical sciences. We argue that the distinctive history of these engineering sciences is the key to the immunity of engineering sciences in face of Stanford's induction. For microelectronics, the existence of many unknown materials leads researchers to explore their properties with existing theories. For computer architecture, the existence of manufacturing obstacles such as the purity of substrates, the precision of lithography increases the confidence of researchers that the solutions to these technical challenges would lead to the advance of the field. For information security, the assumption of the upper bound of the growth rate of computing power gives researchers confidence intervals to do meaningful research with existing methods in the foreseeable future. Each of these historical trajectories is itself distinct and requires careful analysis in order to perform meaningful history induction such as PUA.

Motivated by the afore-mentioned examples, we classify engineering sciences in two categories: first, fields that are in similar situation as rocket science and Newtonian mechanics (e.g. microelectronics); second, fields that are in different situation from that of rocket science and Newtonian mechanics (e.g. computer architecture, information security). In both categories, Stanford's induction could not directly apply to the cases of interests. For example, in the case of microelectronics, we understand the PUA of electrons and atoms and its historical induction, but we have not found much similarity in the development of semiconductors, metals, and magnetic materials including their functional properties; in the case of computer architecture, we do not even have a direct link to the theory of physical sciences, but we have quantitative performance measurements and design principles related to the products of physical sciences; in the case of information security, we have computation models based on the assumptions related to the physical sciences, but it is conceived and well understood long before the realization of the evidence (i.e. quantum computers). The history of the first category might be closer to the history of physical sciences, whereas the history of the second category might be far different from that of physical sciences.

Let us now investigate further. If possible, what might the historical induction part of the PUA for these engineering sciences look like? Although arguably justifiable, we might induct on the number of electronic materials engineers failed to analyse with existing theories, we might induct on the number of microprocessors failing to function as expected, we might induct on the number of years when the growth rate of computing speed exceed expectation. In other words, we might make historical induction on the inability of engineers in order to form PUA arguments for these engineering sciences. The reasons why such inductions are hard to make might be: first, they are difficult to organize – the number of electronic materials engineers failed to analyse is time-varying, and not entirely related to the theories; second, they are not very meaningful – the number of microprocessors that fail to function is not well-controlled and is related to manufacturing yield rate; third, they do not exist yet – the growth rate of computer speed roughly doubles every two years for the past twenty years and is known as the Moore's law. Most importantly, the induction would be field-dependent due to the distinctive nature of each of these engineering disciplines. General remarks are not easily drawn. Also note that many engineering sciences are still young compared to the physical sciences, therefore the power of historical induction is diminished.

To coin a phrase, the reason why the stability of engineering sciences are not shaken by Stanford's induction is because the historical induction part of PUA cannot directly apply to engineering sciences with diverse historical trajectories drastically different from that of physical sciences. We believe PUA arguments could be formulated for virtually every scientific field as Stanford claimed. But this would require careful examination of the history of the engineering field of interests. This way, we might be able to teach engineers something about the reasonableness of their methods, and the effectiveness of their techniques. Caution is needed before doing historical induction because the historical trajectories of some engineering sciences are short compared to the physical sciences, and they are also more diversified. It is still an open question whether induction here can draw any powerful arguments such as those for the physical sciences.

We hope the nature of the immunity of engineering sciences to Stanford's induction is now clear. It is because the historical induction part of PUA cannot be applied to the diverse and radically different historical trajectories of many engineering fields. However, while finding many examples of engineering studies that corroborate the immunity of engineering sciences to Stanford's induction is possible, it does not preclude the possibility of the existence of instances of engineering fields that exhibit the historical induction part of PUA proposed by Stanford. It might be hard to find all of such unusual cases, but giving an interesting case that exhibits such concerns might further clarify the essence of engineering sciences. In the following sections, we present an interesting case in mechanical engineering: the d'Alembert's paradox.

3 D'Alembert's paradox

The d'Alembert's paradox, encountered when studying fluid mechanics, is today well known by engineers, and is one of the most particular behavior studying in fluid mechanics. This paradox was first brought to light by Leonhard Euler in 1745 and Jean-Charles Borda in 1766, but it was Jean le Rond d'Alembert who really defined it as a paradox in 1768. The point of this paper is not to describe the historical evolution or study the physical mechanism of the paradox, but rather to give the main idea and insert it in our discussion about Stanford's induction. The d'Alembert's paradox claims that, in the case of a symmetric body surrounding by an ideal fluid and an incompressible flow, the drag, that is the force the fluid exerts on the body, vanishes. It means that the fluid exerts no forces on the body and the latter does not "feel" the presence of the fluid, hence the paradox. Indeed, it is a contradiction between the theoretical prediction (the calculation of the drag) and what we observe in experiment, that is, that all bodies placed in a moving fluid is subject to the actions of the fluid. D'Alembert concluded in "opuscules mathématiques" (1768): *Thus I do not see, I admit, how one can satisfactorily explain by theory the resistance of fluids. On the contrary, it seems to me that the theory, developed in all possible rigor, gives, at least in several cases, a strictly vanishing resistance; a singular paradox which I leave to future Geometers to elucidate*. It was only in the XIX$^{\text{th}}$ century that Barré de Saint-Venant resolved the paradox by considering a real fluid, that is a fluid in which viscosity is taken into account, rather than an ideal fluid.

Let us emphasize the notion of ideal fluid, for we will use it latter. The concept of ideal fluid is to consider a fluid devoid of any viscosity, that is a fluid in which all the shear forces between molecules can be neglected. To illustrate the notion of viscosity, we can take the example of the spin motion of a spoon in water and honey. In water, the motion is much more easy to achieve than in honey and this is due to viscosity. At d'Alembert's time, the notion of viscosity was not understood in terms of the previous definition (concerning a microscopic scale), but rather it was understood in terms of the notion of tenaciousness, that is the capability of a fluid to resist motion.

One might be wondering now, how this paradox could take part in our philosophical analysis about the debate of scientific realism since the theoretical prediction was never believed to be true. The answer is found in the next section where we analyze the philosophical aspect of the paradox and try to understand how we can use it to discuss Stanford's induction.

4 Philosophical analysis of the d'Alembert's paradox

4.1 Stable or unstable ?

As mentioned before, engineering sciences can be divided in two categories, one containing stable theories, like rocket science and newtonian mechanics, and one typified by unstable theories. D'Alembert's paradox is clearly, at a first glance, a separation between two theories, or more precisely, two different ways of considering fluid dynamics problems in general. It is a separation between the theory of inviscid fluids (TIF), which turns out to lack an accurate description of fluid motion in certain cases, and the theory of real fluids (TRF), which allows us to comprehend and describe fluid flow when the former theory cannot. In that sense, d'Alembert's paradox belongs to the unstable part of engineering science, since it spells out the end of an apparently old false theory (TIF). We will see later the justification of use of the term "apparently". Let us clarify these two theories and see how it actually turns out that the first theory is still used nowadays and therefore it is not so clear that the paradox belongs to the unstable part in every case. Indeed, we will see that TIF is not a completely false theory. Moreover, let us analyze the paradox in the philosophical point of view in more details and try to answer the question whether it refutes Stanford's induction or not.

4.2 D'Alembert's paradox and Stanford's pessimism

At d'Alembert's time, people only considered fluid as ideal and it is in this context that the paradox arose. In order to calculate the drag the fluid exerts upon the body, d'Alembert used the previous work done by Daniel Bernoulli in 1738 in which he describes properties of ideal fluids. D'Alembert ended up with a zero drag leaving him perplexed. Hence, this paradox could be construed to cast doubt upon the work of Bernoulli in general, and more specifically upon the concept of ideal fluid until the latter work of Saint-Venant, in which the paradox is resolved by considering a real fluid rather than an ideal one. However, in the period of time between d'Alembert and Saint-Venant, people still believed in and applied the accepted model of that time, namely the ideal fluid model, and this is the reason why we can insert d'Alembert's paradox in our discussion about Stanford's induction.

In the following section, we try to find some arguments that could support Stanford's induction before giving arguments against it in section 4.2.2.

4.2.1 Stanford's induction: pro-arguments

Stanford's induction is based on the problem of unconceived alternatives (section 2). Chakravartty (2008) demonstrates that PUA *"is a version of UTE plus a historical induction, leading to a sceptical conclusion about scientific knowledge"*. To support PUA, one could try to give arguments supporting both UTE and PI components. However, in our case, we focus only on the second component of PUA (PI) rather than the first one (UTE).

Following the PI thesis, there are in the history of science, theories that were believed to be true but then turned out to be false. It is the case for example for the caloric theory of heat, the electromagnetic aether or the optical aether as Laudan (1981) describes. Works done by philosophers deal in most cases about physics examples because it is probably the field of science which is the most shaken by wrong theories. However, we can also find examples in the history of engineering science and in particular is the case of the "sound barrier". Indeed, in the XX$^{\text{th}}$ century when aerodynamics encountered a real expansion, aerodynamicists thought that no air plane, both at that time and even in the future, could overcome the so-called "sound barrier". However, it turned out later that air planes could cross this "barrier" and it is well known by today's engineers that the "sound barrier" is actually a myth. Hence, there are examples that show that engineering science is not immuned to theories that were believed to be true but subsequently turned out to be false.

Concerning d'Alembert's paradox, it constitutes an example supporting PI's argument about transition from older theories toward newer ones. Indeed, theory of ideal fluids is clearly questioned by d'Alembert's paradox and is one of the occupants of the graveyard of false theories that were believed to be true but subsequently — due to the paradox — turned out to be false that Chakravartty (2008) describes. Indeed, if one takes a look retrospectively at d'Alembert's time, fluid mechanics was known mainly through the trustworthy works of Bernoulli describing the behaviour of ideal fluids. But because of the paradox, TIF seemed to encounter a real limit to its validity. Mechanisists at that time, despite the disturbance due to the paradox, still continued to believe in TIF because they did not have anything better to rely on, and because they were unable to conceive of other alternatives

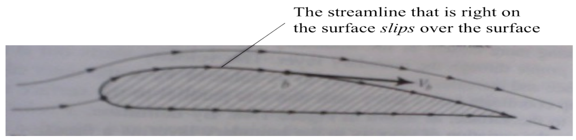

Figure 1: Ideal fluid

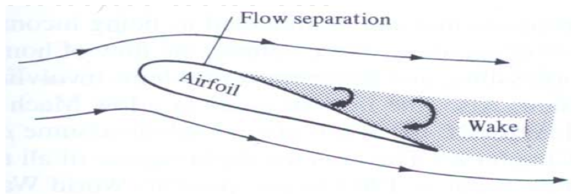

Figure 2: Real fluid

that could have allowed an answer to the paradox. To insist on this inability in developing an alternative, we can utilize the fact that, almost one century latter the discovery of the paradox, scientists were lead to conceive of TRF in the description of a fluid flow, serving St-Venant in resolving the paradox. The "discovery" of this new theory therefore supports the *"conspicuous absence of theories that are consistent with or as equally well-confirmed as those believed, but that are subsequently conceived and adopted"* that Stanford (2006) claims.

Another example, emphasizing the fact that the history of scientific theories is typified, even in engineering science, by a translation from older theories to newer ones, is the case of a flow over a wing, appearing again in aerodynamics. Consider the flow over the body shown in figure 1. In the case of using TIF to describe this flow, one will end up with the streamlines shown in figure 1, where streamlines in direct contact with the body slip over its surface. In the case of TRF description, the presence of frictional shear stress in the flow (viscosity) induce both non-slip of streamlines on the surface and the apparition of a separated region in which the flow recirculates (figure 2). In real life, all engineers know that the flow over a wing behaves as described in the second case — because TIF is an idealization — and if one wants to use TIF, one will miss this separated flow behind the wing and will end up with a wrong result. Thus, in this description of the flow over a wing, engineers have to abandon TIF in favor of TRF, leading to a transition from an old theory (TIF) to a new one (TRF).

From the previous analysis of PI applied to engineering science and following the pessimistic conclusion, current theories will be replaced and regarded as false from some future perspective. Stanford would add some arguments dealing with theorists rather than theories and claim that since scientists at d'Alembert's time were faced with PUA, today's scientists will probably be faced with the same problem. It would means that TRF is probably false, pending some improvements or even a new theory. This argument is somewhat quite disturbing considering the huge amount of theoritical predictions and experiments based on TRF that turn out to be verified in the real word (airplanes can fly because it is well understood thanks to TRF). This leads us to the next point in which we try to defuse Stanford's pessimism.

4.2.2 No place for Stanford's induction

Stanford's arguments about PUA and the new induction (NI) can be refuted using three main points (Egg, forthcoming): two concerning PUA and one concerning the induction. About the unconceived alternatives, they have the property, under certain conditions on continuity of science that we will discuss latter on, to be not radically distinct from theories that were conceived by scientists. Therefore PUA is no longer a threat to realism, because even though there are alternatives, although unconceived, they are similar to the theory at hand. In other words, it means that the conceived theories are similar enough to the theory one already has, and therefore there is no reason anymore to doubt that the theory at hand is at least partially true. For instance, concerning our discussion on engineering science, TRF differs from TIF only through viscosity. Moreover, Stanford (2006) argues that *"realist can only retrospectively identify those parts or aspects of earlier theories that were retained in later theories"*. In other words, one can identify the trustworthy parts of a theory because one has been able to make the distinction, only after knowing the new theory, between the false theory and the new one. However, let us lean on Psillos' (2009) argument and say that the ability to identify the trustworthy parts of a theory does not depend on the fact that we know, in retrospect, which parts of a theory are retained. Indeed, as we will see below in the continuity arguments, fluid mechanics equations are the trustworthy parts of TIF that scientists were able to identify. In particular, we can take the example of Bernoulli's equations that were retained across the change of direction. But it does not depend on knowing that these equations are retained in TRF, but rather because of their empirical success.

The second point for unconceived alternatives, based on Magnus arguments, deals with the plausibility of these alternatives. According to Magnus, the plausibility of unconceived alternatives depends on the historical context in which we consider the scientific theories. Indeed, TRF seems nowadays a plausible theory that engineering science community could not avoid to describe fluids phenomena, but it is not certain that mechanisists at d'Alembert's time would have found TRF plausible. Stanford (2006, sec 1.2) also makes a distinction between mere skeptical fantasies and scientifically serious alternatives that we have to take into account in the identification process. However, this argument can be refuted as follows: it seems today really implausible to us that the knowledge that engineers have about real fluid would be completely wrong and replaced by a brand new concept of fluid. But if someone in the future can actually discover that the way today's engineers describe fluid does not refer to the truth, then we would have to conclude that this brand new scenario was actually a serious alternative that we should have kept in mind. According to

Magnus, *"if we must now take seriously what we might ultimately have to take seriously in the future, then we have to take all skeptical possibilities seriously"*. (Magnus 2010).

The last of the three points mentioned before, is the one dealing with the induction. As we already saw, Stanford claims that since theorists in the past were unable to conceive of other alternatives to false theories, and based on the assumption that present scientists are not radically different from past theorists, then by an induction argument, today's scientists will also fail to conceive of alternatives. One way to counteract this assumption is to say precisely that theories as well as theorists differ from past ones due to improvements in scientific methodology that Devitt (2011) asserts. As an illustrative example, we can refer to as the boundary layer theory that appeared during the expansion of aerodynamics in the XX$^{\text{th}}$ century. The boundary layer is, in a nutshell, a very thin region of a flow in which shear stresses are predominant in comparison to the rest of the flow. Before knowing the boundary layer concept, scientists in the past did not take it into account. But this concept created a real breakthrough in aerodynamic analysis, and today, engineers can not ignore this thin region in the methodology to solve problems.

Let us go further in our analysis. Except for few theories like the example concerning the "sound barrier" given previously, mechanical engineering seems very stable and theories established seem to be true and stay true along time. Even in the ideal / real fluids discussion questioned by d'Alembert's paradox, the stability is ensured since many of engineering applications use ideal fluids theory even now (we now understand the sense of "apparently" false theory in section 4.1). This is justify by the fact that, in certain cases, ideal fluids theory can bring a good description of flows, and engineers do not have to appeal to real fluids theory. In comparison to physics, there are no drastic changes in theories or in directions to describe world's phenomena, such as the electromagnetic aether theory that turned out to be false and replaced by Maxwell's theory latter on. The changes in the electromagnetic aether theory is more drastic than in TIF because, in the case of the former one, it is a false theory, whereas the latter one is partially false, in the sense that engineers are still using the equations of TIF but they no longer believe in the underlying hypothesis. Much more convincing is to say that light is a more complex physical aspect of the world than fluids. Indeed, light is a composition of many unobservable entities, like electrons, protons, quarks, and if one wants to study properties of light, one has no direct access, so that it is much more difficult to understand its properties. Therefore, physicists have to demonstrate a real ability in creating theories to try to understand the real entities and properties of light. Due to this, they are more often exposed as incorrect than those theories involved in mechanical engineering. By contrast, even if fluids are made of molecules and atoms that involve the above-mentioned entities, one do not need, in engineering applications, to describe fluids at a microscopic level; it is sufficient to describe it at a mesoscopic level (scale between microscopic and macroscopic scale), so that it is less challenging than in physics to build a theory, and thus the theory is more immune to wrong stances. One might ask then — besides the fact that the aether concept is false — why engineers are still using TIF whereas physicists are not using the aether any more. As a potential answer, we can assert that the theoretical predictions using TIF has been extensively verified by the experience in laboratories. By contrast, the luminiferous aether has never been typified.

Therefore, the PI component is difficult to admit in our discussion and, as a consequence, makes PUA without strengths.

Let us now emphasize the last point allowing mechanical engineering to survive Stanford's pessimism. As Chakravartty supports, realists can appeal to the principled continuity across theory changes, in order to identify which parts of theories will likely be retained in the future. The identification, according to him, has to be based on causal knowledge, a promising criterion *"that allows one to manipulate it"* (the causal knowledge) *"in highly systematic ways"* (2008). However, he highlights the fact that Stanford, among others, misidentify these trustworthy parts of theories because their identification are based on realism taken at the level of entities, and Chakravartty claims that one has to consider realism rather at the level of properties because it is these properties that will survive through changes in theories. What is new in Chakravartty argument, is that, in the identification process, one does not need to refer to entities, but only to well-confirmed properties. For instance, equations describing fluid flows are retained across the turnover from TIF to TRF, and the reason is because causal properties survive from old to new theory even though we do not speak about the same entities. Indeed, one major property that we can identify in fluid mechanics, which has not seen any resistance through history of mechanics, is the conservation of mass, property that one can use without referring to as fluid particle entity. To borrow one of Chakravartty's example (2008), physicists all agree on the property of negative charge that an electron possesses, but we can not say that all physicists believe in the existence of electrons. In the same way, do TIF and TRF, both in agreement with the fluid particle entity, end up with the same result to describe how fluids flow ? The answer is of course no. But do TIF and TRF endorse the property of compressibility or incompressibility of fluid particle ? Yes they do. One might now ask why it is such a case. In fact, the property of compressibility or not of a fluid is related to the conservation of mass mentioned above. Compressible or not, fluid mass is always conserved, that the fluid being real or considered as ideal.
We could give other examples, like the rotational or the potential properties, but let us investigate further.

Chakravartty claims (2008) that once one owns this causal knowledge argument, one can rescue some other insights from Stanford's pessimism. For instance, structural realists can in fact lean on the argument that through changes of theories across time, there are mathematical structures that can be preserved. Chakravartty defines the term "structure" as being the relationships between properties and he identifies them as mathematical equations. For instance, if we take one of Bernoulli's result concerning the total energy in a fluid, it says that the irrotational flow of a fluid in which the density is constant is homoenergetic, that is, the energy is constant as the flow takes place. This can be translated to a simple equation which makes the link, in particular, between the property of no-swirling (irrotational property) and incompressibility (constant density property) of the fluid particle. This relationship therefore ensures the survival of the properties to Stanford's induction.

Still on the process of analyzing the continuity through changes of theories, let us try to identify which parts of TIF survived Stanford's skepticism. D'Alembert's paradox is indeed a strong argument against TIF and was a real challenge for scientists for nearly a century. However, even when the paradox was fully understood by Saint-Venant, changing the way fluid flows are described, the previous work done by Bernoulli still remained even though the paradox could tend to cast doubt on it (as an example is the one given above concerning the conservation of energy). And a strong proof, is the expansive use of Bernoulli's equations in fluid mechanics even today. More importantly, fluid mechanics is built on three main principles that could not be shaken by Stanford's pessimism. Indeed, TIF predicted a zero drag for the force a fluid exerts on a body, whereas TRF would have predicted a completely different result (which is, by the way, the real result verified experimentally). But through the turnover from TIF to TRF, there are three mathematical equations associated with these three principles that both TIF and TRF endorse. These equations involve properties of fluids, and especially, properties of fluid particles, that can be systematically manipulated in order to ensure the survival of the relationship between these properties. Conversation of fluid mass, fluid momentum and fluid energy are these properties. Therefore, engineers can lean on these properties, allowing them to have a promising criterion which can be used to withstand a prospective future disturbance of theories in mechanical engineering.

Let us summarize what we have argued so far against Stanford's pessimism. The first point that one can use is what Egg (forthcoming) calls the "distinctness of unconceived alternatives": alternatives to a theory, though unconceived, are not radically different from the theory at hand. TIF and TRF differ only because of the consideration of viscosity in the second case. Moreover, we have emphasized the fact that our ability to identify which parts of past theories that were likely to be retained, is not based on retrospect and does not depend on knowing that they were retained in latter theories. Indeed, the fact that Bernoulli's work is retained across the change of theory, is not because one knows, in retrospect, that they are retained in the new theory, but rather because of their empirical sucess. Then we have argued, based on Magnus' arguments (2006), about the plausibility of the unconceived alternatives and said that PUA is actually context-dependent. It does not mean that TRF is today plausible for engineers, that it would have been plausible for scientists at d'Alembert's time. Another argument we have used to counteract Stanford's pessimism is the difference between past and present scientists due to improvements of scientific methodologies along time. Today's engineers do not have the same approach and methodology in resolving a problem than that engineers had at d'Alembert's time. The last very important point we have granted, is the principled continuity across scientific theories over time. One can have a promising criterion if one is able to start the identification process based on causal knowledge. Causal knowledge can be identified by mathematical structures which are in our case the fluid mechanics equations. Furthermore, the identification has to be performed at the level of well-confirmed properties rather than at the level of entities.

5 Conclusions

Recent works in scientific realism by Stanford (2006) and Chakravartty (2008) have generated interests in the research of a more selective and sophisticated realism. Among many scientific theories discussed by Stanford (2006) and Chakravartty (2008), we have found their examples excluding those from engineering sciences, and this greatly attracts our attention. We are particularly interested in finding something about the characteristic of engineering sciences that renders them especially immune to PUA.

Historical induction offers a power tool to challenge scientific realism. From PI to PUA, anti-realists have found powerful and appealing arguments based on historical induction. Thus the validity of historical induction places a central role in applying anti-realism arguments. In this paper, we focus on the validity of applying the historical induction part of PUA to several engineering sciences and find that this historical induction is generally speaking not directly applicable to engineering sciences.

The reason why the stability of engineering sciences are not shaken by Stanford's induction is because the historical induction part of PUA cannot directly apply to engineering sciences with diverse historical trajectories drastically different from that of the physical sciences. We believe PUA arguments could be formulated for virtually every scientific field as Stanford said. But this would require careful examination of the history of the engineering field of interests. This way, we might be able to teach engineers something about the reasonableness of their methods, and the effectiveness of their techniques. Cautions are needed before doing historical induction because the historical trajectories of some engineering sciences are short compared to physical sciences, and they are also more diversified. It is still an open question whether induction here can draw any powerful arguments as those for physical sciences.

PI is for sure difficult to apply because of the reasons given above. However, it seems that PI could find its place in the case of the d'Alembert's paradox. Indeed, the latter spells out the end of an apparently false theory in favour of a new one and increase the size of the graveyard of theories that were believed to be true but then turned out to be false that we find in history of science. Hence, as it is say above, PUA can again be applied in the case of the paradox because scientists were not able to conceive of alternatives.

However, though unconceived, these alternatives are not written in stone and are context-dependent and one has to examine in which historical context scientists were unable to conceive of alternatives. If PUA is plausible, in the sense of the historical context, then one has to take all possibilities, even apparently fanciful, as a serious alternative. What is more challenging for Stanford's pessimism, is that realism in engineering science is regarded at the level of properties rather than entities. And here is the great example of fluid mechanics equations translating relations between properties without referring to entities that possess these properties, and leads to the major point allowing one to argue with Stanford's pessimism, which is the principled continuity. It can actually be applied to engineering science, and engineers have causal knowledge that allows them to manipulate it in systematic ways, namely mathematical structures describing these relations between properties.

Hence, given that many stable examples, for instance microelectronics, computer architecture or information security, though belonging to different categories of sciences, the PUA argument remains applicable to each field, but its historical induction is much more difficult to apply because of the varying diversity and differences of historical backgrounds of engineering science. Furthermore, the seemly unstable example of d'Alembert's paradox could tend to make one believe that Stanford's induction has its place even in engineering science, but as it turned out, thanks to the existence of causal knowledge engineers possess, the stability is preserved. Therefore, we have reasons to believe, undergirded by the argument of the stability and causal knowledge of engineering science, that we can refute Stanford's induction in engineering sciences.

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