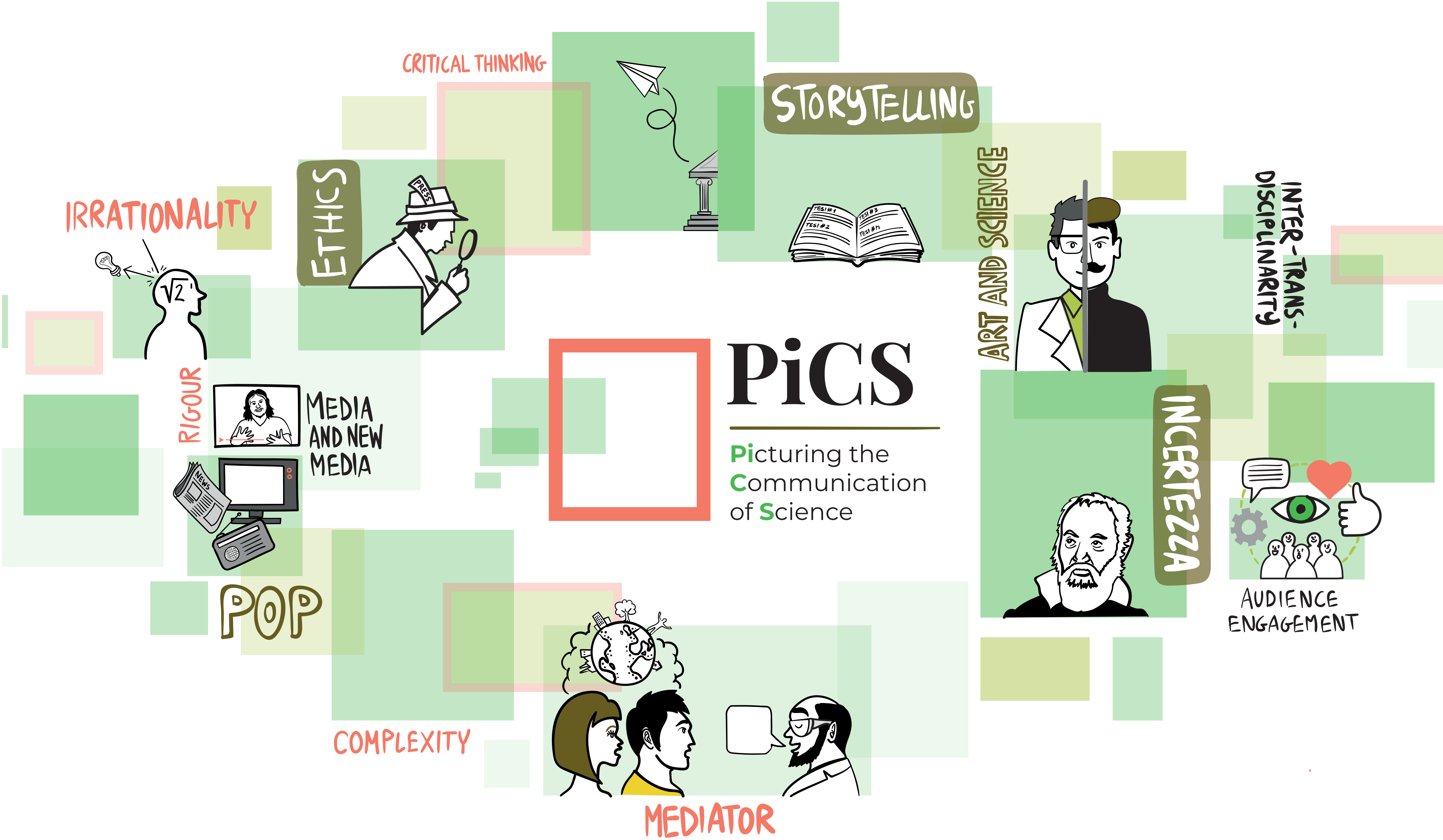

CRITICAL THINKING
STORYTELLING
IRRATIONALITY
ETHICS
PRESS
TESI #1
TESI #2
TESI #3
TESI #4
ART AND SCIENCE
INTER-TRANS-DISCIPLINARITY
PiCS
Picturing the Communication of Science
RIGOUR
MEDIA AND NEW MEDIA
NEWS
INCERTEZZA
AUDIENCE ENGAGEMENT
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COMPLEXITY
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Summary

Knowledge, cognition, and science communication

Vincenzo Crupi

In the research areas that I know best from my own work – philosophy of science and certain areas of cognitive science – science communication is not a prominent topic. In recent times I have been involved in the activities of a panel on science communication promoted by Compagnia di Sanpaolo and coordinated by the association Frame from Turin, and I've come to the conclusion that such inattention is largely mutual: in current (and sometimes intense) discussions on science communication, its nature and methods, specific references to philosophy of science and to cognitive science are fairly rare. This does not have to be a problem, of course. But I happen to think that one could profitably give a closer look at the connection and gain further insight, even starting from rather basic notions and findings.

In selecting some illustrations in support of this suggestion, I took two recent contributions as a point of departure. The first one is by Antonio Gomes da Costa, Director of Science Mediation and Education at Universcience in Paris, to which both Cité des Sciences et de l'Industrie and Palais de la Découverte belong. Da Costa's article, published in February 2019 on *Exsite* (webpage of the European Network of Science Centres and Museums) provides an updated and challenging outlook on a well-known and controversial issue, namely, the role and limits of so-called "deficit model" to understand the relationship between citizens and the sciences (Gomes da Costa 2019). The author of the second short article that I'll consider later, on the other hand, is indeed a philosopher and a most influential one: Timothy Williamson from Oxford University. For some of our current matters of concern, the title of Williamson's piece (published on the New Statesman) go straight to the point:

> "In the post-truth world, we need to remember the philosophy of science: From climate change to vaccination scares, what non-scientists believe about science is literally a matter of life and death".
>
> (Williamson, 2019)

The plane – and natural interpretations

An airplane is flying at constant velocity and constant altitude. The airplane drops down a big metal sphere. Question: *what is the shape of the falling trajectory of the sphere?* At the beginning of the 1980ies, experimental psychologist Michael McCloskey and his collaborator asked this same question to dozens of university students. Thirty-six percent of them drew the trajectory as in the D quadrant in Figure 1, thus adopting a physical theory where the principle of inertia does not apply. Eleven percent of the responses were like in quadrant C: how should they be understood? According to Paolo Bozzi (Bozzi 1990, p. 37), the C interpretation is nothing but D as viewed from a person who is sitting on the airplane. So the "Aristotelian" responses (namely, C and D taken together) would involve 47% of the participants. Response B — chosen by 13% of the participants — has a "pre-modern" feature, too, for here an inertial effect arises, but the downward component of motion is meant to have constant velocity. Finally, 40% of the participants drew a trajectory akin to the picture in quadrant A, approximately parabolic, thus in line with modern physical theory. Among these people, however, one could still sometimes find the idea that the position of the sphere when touching ground would be significantly behind the position reached by the airplane meanwhile (McCloskey 1983, pp. 302-304).

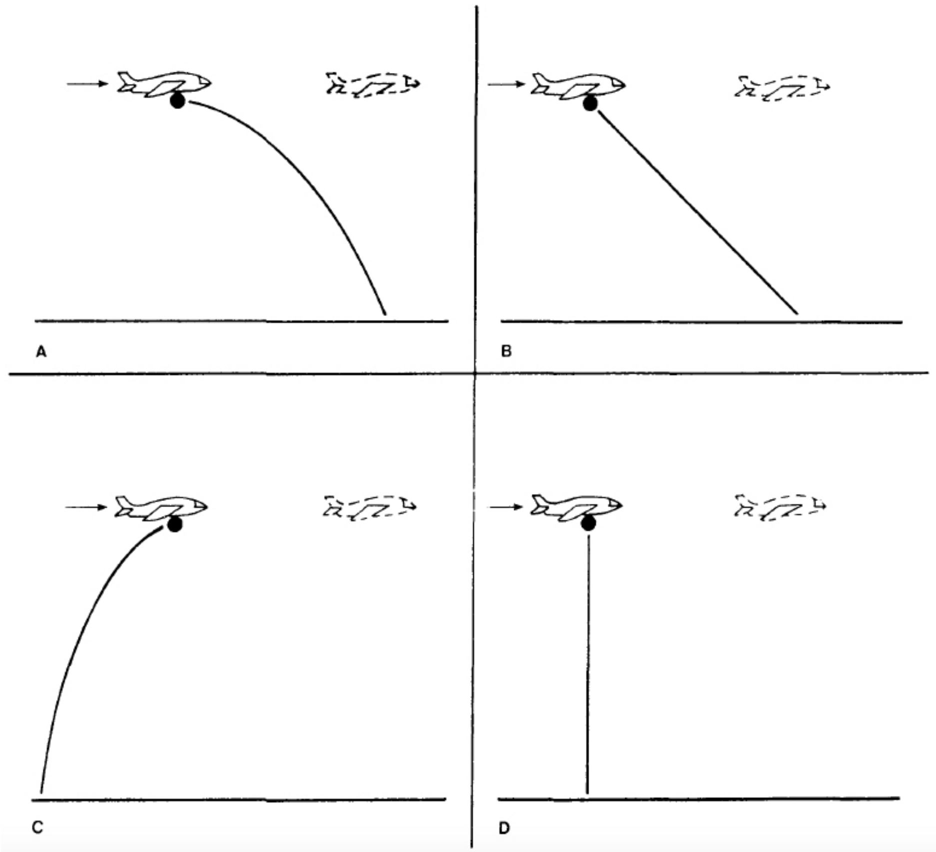


Figure 1.1: Taken from McCloskey (1983), p. 303.

Findings like McCloskey et al.'s (1983) on "naïve physics" illustrate an important phenomenon. Apparently, this has been disregarded not only by advocates of so-called "deficit model" but also in many critical reactions to the deficit model approach — or so I want to argue. Let me explain better.

Following Gomes da Costa (2019), one can see critics of the deficit model pointing out (quite appropriately) that "the 'public' is not an empty vessel needing to be filled" with adequate scientific knowledge. Challenging this idea, opponents of the deficit model have often emphasized that non-scientists might have an amount of legitimate and relevant "lay knowledge" from sources outside a formal scientific approach. In line with metaphorical discourse, this viewpoint seems to suggest a "mixture model": *two* containers would now be involved, both of which *full* of information (scientific and lay knowledge, respectively), although markedly different in format and kind. Effective science communication would then amount to combining these two types of contents without misrepresenting or dismissing any of them, allowing instead for constructive interaction so that a more advanced form of shared knowledge can emerge.

Surely there are cases in which this pattern prevails (say, some health policy matters) and indeed such cases highlight certain shortcomings of the deficit model, suggesting a more symmetric and participatory route to sharing scientific contents. The airplane example above, however, indicates that in many circumstances the limitations of the deficit model arise in quite different respects, and this is the first key point I want to make in this contribution. As concerns problems that science can frame and address effectively (like the trajectory of a falling object), the human mind is not an empty vessel – very true – *not* so much because it harbors alternative, valuable, and compatible contents, but rather because it spontaneously generates intuitive solutions that systematically clash with our best available scientific understanding. Lacking a better label, I will call such intuitively plausible but scientifically unsound solutions “natural interpretations” (the term is borrowed from Feyerabend 1975, but the idea is only partly related).

Natural interpretations are not a form of “non-scientific knowledge” because (to put it crudely) they are usually false. Nonetheless, I would like to emphasize why they deserve careful and specific consideration. Importantly, such consideration is unlikely to arise in either the deficit model or in some kind of “mixture model”, although for different and perhaps opposite reasons.

To begin with, natural interpretations are not alien to the sciences themselves, and especially not to their *history*. In many respects, the most staggering aspects in the evolution of the sciences imply a stepwise process in which natural interpretations are overturned time and again (a pattern tracing back at least to the age of Plato and Aristotle). The example of how Galileo had to confront the “tower argument” put forward by the “Aristotelians” of his time regarding the motion of the Earth (see again Feyerabend 1975 for a classic discussion) is an effective illustration, but by no means the only or even the oldest one. So whenever natural interpretations hinder appropriate reception of certain parts of modern or contemporary science, one can often discern a repetition of fragments of the history of the relevant scientific discipline. It follows that some substantial knowledge of the history of a given science may be an important tool for effective communication of its contents – a tool, one should add, that is rarely offered to the experts themselves in their education.

So in order for science to grow, scientists of the past had to find out (often with difficulty) how certain natural interpretations of phenomena were mistaken. Ideally, communicating science to citizens (non-scientists) today would then imply that they be given the same opportunity. This also means that manners of aloofness or condescension by the experts — which can arise all too easily — should be clearly avoided. The outcome here is the same as when non-scientific lay knowledge is invoked and emphasized, but the starting point and implications are different in important ways. The idea is to understand and to convey explicitly the role of natural interpretations, which leads to a style of science communication focused not only on validated scientific information but also as much as possible on the relevant processes whereby such information is achieved. And here again, I would like to stress that we're dealing with a motivation which is specific and distinct as compared to others (more common and still legitimate, of course) having to do, for instance, with story-telling as a potentially effective tool in the communication of science.

Finally, natural interpretations are, well, natural: their existence is by and large predictable and widespread. To illustrate, I've employed one single classic example concerning so-called naïve physics (see Kubriche, Holyoak, and Hongjing 2017, for a more recent discussion), but I happen to believe that the same phenomenon shows up across a wide range of disciplinary fields (see Carlisle and Shafir 2005 for a different kind of example). Occasionally, natural interpretations partly survive even among experts: think of the true or alleged finalistic residuals in theoretical discussions in evolutionary biology, and how difficult it is to spot them and uncover their mistaken implications. Even more common is for an expert in discipline X to be misled by natural interpretations when s/he addresses a different discipline Y. And, well, for our current purposes, the study of science communication too — as challenging and embryonic as it may be — is itself a scientific discipline!

The hand of cards – and reasoning biases

You are playing cards with your friend David. The game is uncertain and engaging, and it'd be very useful for you to know whether David's hand includes an ace. Consider the following propositions, and assume you know that one and only one of them is true (you do not know which one, though); the other is false.

1. If there is a king in David's hand of cards, then there is an ace.
2. If there is *no* king in David's hand of cards, then there is an ace.

What can you conclude?

A. In David's hand of cards there is an ace.
B. In David's hand of cards there is no ace.
C. In David's hand of cards there might or might not be an ace.

This puzzle was devised by cognitive psychologists Phil Johnson-Laird and Fabien Savary about twenty years ago (Johnson-Laird and Savary 1999). It is a kind of *pure* reasoning task: it has almost nothing to do with what we may know or believe in matters of science, history, or anything else. What did Johnson-Laird and Savary find out? That most people pick up response A. (What did you choose?) That's incorrect, however. Here is why, in brief. Let us suppose that David's hand does in fact include a king and moreover that (1) is the *false* statement: then we can *not* conclude that an ace is the hand, there might well be none. The same happens in case no king is in the hand and (2) is the false statement is.[1] Sounds easy, right?

If you think that the logical problem of the hand of cards is but a puzzle for brainiacs, well, you may be taking it too easy. I've chosen this example because it's relatively quick to explain and less popular than others, but the related research tradition has piled up a massive amount of findings supporting a very clear insight: human rationality is significantly limited and imperfect. True, people cope quite well in many ordinary circumstances through reasoning shortcuts ("heuristics" in the relevant scientific terminology). But when compliance with sharper reasoning principles is required, then intuitive missteps are frequent and systematic, even if — that's important — the needed factual information is available and no exogenous pressure is interfering. (Among many references, one must at least mention Kahneman 2012.)

So the prevailing mistake in the cards example is a "logical illusion" akin to other well-known phenomena. The most widely known is perhaps so-called *confirmation bias* (actually a cluster of related but distinct cognitive mechanisms, see Nickerson 1998). Importantly, *cognitive* errors are labelled cognitive because they're meant to be widespread. Surely, there are important interaction between the contents available to people and the cognitive

1. Once response A is ruled out, C may seem to be the right answer. This conclusion is reasonable for many plausible interpretations of statements (1) and (2). To be precise, however, the matter is a bit more complicated. If we understand (1) and (2) in terms of the usual conditional connective of classical propositional logic, then the only sound response is B, as surprising as this may appear. In fact, by this interpretation of "if…, then…", a conditional statement is true unless the antecedent is true and the consequent is false. As a consequence, the presence of an ace in David's hand would immediately make both (1) and (2) true. But we know that only one of them is true, so there cannot be an ace in the hand! For the rest, it all depends on the king: if one is in the hand, then (1) will be false and (2) true; and if no king is in the hand, then the other way around.

paths that they pursue (accordingly, such interactions, too, are topics of careful investigation in current research). Yet, as far as we know, susceptibility to systematic reasoning biases is not automatically removed by any specific domain knowledge. In fact, the most spectacular achievements of the sciences were not accomplished because cognitive biases magically vanish among scientists, but rather *despite* their persistence. Science represents a paradigm of rationality just because it features a collection of procedures and tricks aimed at reducing the errors arising from misleading intuitions and potentially biased patterns of inference (exemplars of such procedures are, of course, mathematical proof and controlled experimentation).

How is all this related with the deficit model and science communication? To see the connection, let us go back to Gomes da Costa (2019). Gomes da Costa suggests that criticism of the deficit model has been affected by misunderstanding: critics seem to have taken for granted that the model implies filling a gap (the "deficit") of *factual information* that the public allegedly lacks. However — Gomes da Costa notes — already in so-called "Bodmer report" (Royal Society of London, *The Public Understanding of Science*, 1985) one can find much emphasis on how public awareness should be increased as concerns key elements of method for the construction and assessment of scientific contents. Da Costa's (2019) claim is that, if this point is appropriately retained, then the deficit model can remain significant in the analysis and design of interventions. In this way, however, the assumption that "there exists a group that detains the knowledge and skills deemed essential to understand the world and to deal with contemporary challenges, and another group that don't have relevant knowledge to deal with these matters" (Gomes da Costa 2019) remains unscathed, with the only caveat that now the conveyance from one group to the other includes aspects of methods in addition to purely factual information. As seen above, in the "mixture" model, this assumption is challenged directly, claiming for the "public" access to specific forms of knowledge outside the boundaries of institutional scientific work. And in this case, too, I would like to suggest different and independent reasons to revise a sharp distinction between scientists and laypeople. Humans largely share their basic cognitive structures, generally efficient but prone to systematic errors too (Jeng 2006 provides an interesting discussion), and distinctive methodological principles of scientific inquiry turn out to be rather "unnatural" for both experts and non-experts. In fact, precisely there lies much of their crucial importance.

The preface – knowledge and uncertainty

Prof. K is a rigorous and skillful scholar, distinguished in her field. She is a Middle Age historian and collected the outcomes of her recent research work in a book. The text is very rich, including 100 distinct main theses — call them T_1, T_2, ..., T_{100} — of significant scientific interest. In Prof. K's book, each of these 100 claims is supported by arguments whose premises are established (e.g., textual findings from accessible sources). Arguments in favor of each claim are not infallible (like, let's say, a mathematical proof) and yet they're very strong. For these reasons, in the Preface of her book, Prof. K writes "for each one of the theses here submitted, I firmly believe in its truth on the basis of the available evidence". "On the other hand", she goes on, "I also consider it likely that at least one of them be false".

1. *Which one of these opposite statements you find more convincing*?
   - 1a. What Prof. K writes in the Preface does not make sense: it's inconsistent.
   - 1b. What Prof. K writes in the Preface is perfectly consistent and sensible.
2. *Which one of these opposite statements you find more convincing*?
   - 2a. The main claims in Prof. K's book will probably include many pieces of true knowledge.
   - 2b. The main claims in Prof. K's book cannot include many pieces of true knowledge.

Unlike my previous illustrations, the Preface example is not taken from experimental psychology. It's a story adapted from a popular "paradox" (in fact known as the *preface paradox*), much debated in contemporary epistemology (see Clark 2002, pp. 166-168). Moreover, as it turns out, questions such as 1 and 2 above do not to have straightforward answers. In this final part of my contribution, I would like to put forward three remarks: (i) first, that the most "optimistic" pattern of responses, namely 1b and 2a, is plausible; (ii) second, that for certain interesting purposes the Preface story can be taken as a metaphor of *all science*; and (iii) that the analogy with the Preface example illustrates some central philosophical concepts that may be important for science communication.

Before we get to points (i)-(iii), however, I will have to extend the discussion somewhat. It might appear as a (brief) digression, but it'll turn out to be useful in the end, I promise.

Try to ask a physicist if what current physical theories state about, say, electrons is true. She will probably reply something like: "What do you mean by 'true'? The formalism including the term 'electron' yields experimentally testable predictions and indeed very accurate ones. If *this* is what we mean, then yes, it's 'true'. But we cannot say more than this!" If you ask a mathematician a similar question, most frequent answers will be of two different kinds. The first one sounds as follows, more or less: "I believe that mathematical statements are true because they describe abstract and timeless entities and structures. But this belief is unproven and unprovable: an intuitive faith!" The second typical answer (perhaps slightly less popular than the first one, but still very compelling for some) says, on the other hand: "It does not make sense to claim that mathematical statements are true or false, as they do not actually describe anything in the real world. Mathematics is like a very special kind of game: it provides methods to manipulate symbols according to rigorous rules, and that's pretty much all." These are paradigmatic examples, but of course not unique: experts in a certain discipline usually have a *sketch* of the philosophy of *their* science, and that applies to the humanities and social and behavioral sciences too. Following a forceful statement by Daniel Dennett, "there is no such thing as philosophy-free science, there is only science whose philosophical baggage is taken on board without examination" (Dennett 1995, p. 21). As a matter of fact, all examples above reflect philosophical positions that were articulated in a more sophisticated fashion decades (sometimes centuries) ago, and were then extensively discussed, criticized, revised, and improved. We might call such sketchy views "Sunday philosophies", for they provide an expert with quick and plausible answers to queries lying outside the scope of ordinary work in labs and departments – queries sometimes raised precisely by some meddler in unusual contexts, such as a philosopher or a journalist. But since "Sunday philosophies" may sound irreverent, I will employ the more neutral "ancillary philosophies".

Normally, science communication professionals have been educated in some science (again, non-natural sciences are included here), and are thus familiar with at least some elements of ancillary philosophy of science. As pointed out above, ancillary philosophies offer an epistemological and psychological foothold for members of a target disciplinary community. Precisely for this reason, however, *they have not been devised as a sound theoretical basis for* *activities and projects of science communication in general*. Let us take a major example. As I see it, a key philosophical problem for recent debates on science communication is the possible conciliation of *knowledge* and *uncertainty*. Indeed, in many contemporary societies the fallibility of science (to some extent illustrated by its very historical evolution) is a culturally acquired datum, deserving acknowledgment and inviting further clarification. On the other hand, science is still considered as a unique and extraordinary source of knowledge to understand the world and guide individual and societal choices – a fully motivated appreciation, which also amounts to a form of social capital to be preserved. Is there a coherent theoretical view by which these two different ideas (fallibility and knowledge) can be taken together? Trying to derive an answer from ancillary philosophies is bound to fail, I submit: for the reasons outlined above, they do not display the right features of relevance and generality. And this brings us back to Williamson's (2019) article, and to the Preface example.

Williamson emphasizes the need to keep three key elements distinct: *reality*, *data*, and *theories*. Following his illustration (concerning climate change):

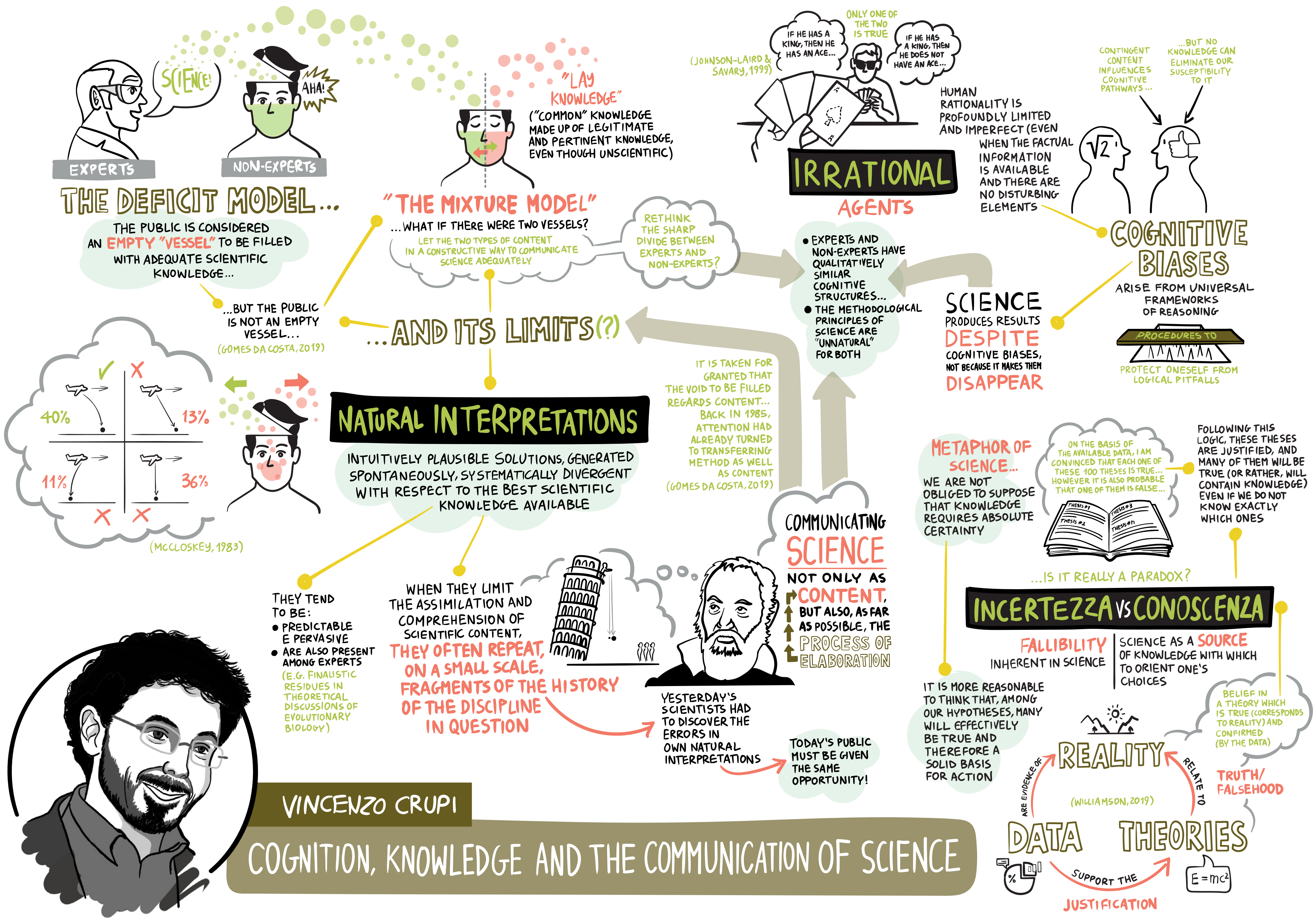
> "Accurate, effective reporting of science must be honest about the nature of the scientific arguments without losing the reader in technicalities. Achieving even an elementary understanding of the science requires distinguishing three dimensions: its subject (such as the past, present, and future climate), evidence about the subject (such as measurements of temperature), and theories about it (such as a hypothetical mechanism for global warming). To confuse any two of these three dimensions leads to alarming mental muddles, in which no theory lacks evidence, or nothing happens unobserved, or a change of theory is a change of climate."
>
> (Williamson, 2019)

Let us make a further step. *Data* provide support (more or less strongly) to *theories* so that they can become accepted: in the traditional epistemological terminology, this is the logical relation of *justification*. The truth or falsity of theories, instead, exclusively depends on the relationship between theory and *reality*: the former may describe (more or less precisely) the latter, or not, and what we believe and why is entirely inconsequential in this respect. It follows that justification and truth are not the same thing. What about *knowledge*? Traditionally, it is often meant to be a belief (in a theory) that is true (matching reality) and also justified (by available data). This is an analysis of knowledge that philosophers have been discussing thoroughly for two thousand years or more, but for our current purposes will work just right.

Sounds too simple? In his article, Williamson is careful to signal how philosophical research (including his own: see Williamson 2000) has developed a number of variants and important refinements of the basic framework above. But remember our goal here, rather modest after all: we mean to figure out if it is possible to outline a general view whereby a conciliation of knowledge and uncertainty is achieved, thus facing some underlying demands of science communication – a purpose, recall, which normally lies outside the reach of ancillary philosophies.

And now, we finally get back to the character in our fictitious story, Prof. K, who has 100 hypotheses, each one strongly supported by available and relevant data. K cannot rule out altogether being wrong, but she considers each of her hypotheses very probable given the data, and thus believes that each one is true. As such hypotheses are many and distinct, however, the probability that at least one among T_1, T_2, ..., T_{100} is false is also significant (simplifying a bit, it is a large sum of many small probabilities), and K is aware of this too. So what she writes in the Preface makes sense, and we have a reconstruction supporting response 1b. So far so good. But can we also say that K's list of major claims in her book includes many pieces of *knowledge*? (That was response 2a above.) After all, one could object, for each of the hypotheses T_1, T_2, ..., T_{100}, we cannot rule out that it's false. Yes, but wait: we still have good reasons (the total available evidence) to think that a good deal of them is true — for we just said that each one is very likely given the data! So K's theses are justified beliefs, and many will be true; hence, many of them neatly fulfil our traditional definition of knowledge. True, we do *not* know *with certainty which ones exactly* amount to actual knowledge, but to say that many pieces of knowledge are among T_1, T_2, ..., T_{100} remains compelling nonetheless

The whole of our science is in a sense just like Prof. K's book. For each of the more advanced and important contents (hypotheses and theories), the possibility of falsity for sure does *exist*, and thus there also exists the (greatly fainter) possibility that all of them are false (Popper, for one, emphasized this point explicitly: "the whole of science might err", Popper 1935/1959, p. 5). But nothing forces us to assume that knowledge demands complete certainty. Following our development of Williamson's (2019) remarks, it is most plausible to think that many of our hypotheses supported by evidence are indeed true, and thus solid ground to understand the world and inform action and policy. To sum up, knowledge and uncertainty are not incompatible. QED.



SCIENCE!
AHA!
EXPERTS
NON-EXPERTS
THE DEFICIT MODEL...
THE PUBLIC IS CONSIDERED AN EMPTY "VESSEL" TO BE FILLED WITH ADEQUATE SCIENTIFIC KNOWLEDGE...
...BUT THE PUBLIC IS NOT AN EMPTY VESSEL...
(GOMES DA COSTA, 2019)
"LAY KNOWLEDGE"
("COMMON" KNOWLEDGE MADE UP OF LEGITIMATE AND PERTINENT KNOWLEDGE, EVEN THOUGH UNSCIENTIFIC)
"THE MIXTURE MODEL"
...WHAT IF THERE WERE TWO VESSELS?
LET THE TWO TYPES OF CONTENT IN A CONSTRUCTIVE WAY TO COMMUNICATE SCIENCE ADEQUATELY
RETHINK THE SHARP DIVIDE BETWEEN EXPERTS AND NON-EXPERTS?
...AND ITS LIMITS(?)
40%
13%
11%
36%
(MCCLOSKEY, 1983)
NATURAL INTERPRETATIONS
INTUITIVELY PLAUSIBLE SOLUTIONS, GENERATED SPONTANEOUSLY, SYSTEMATICALLY DIVERGENT WITH RESPECT TO THE BEST SCIENTIFIC KNOWLEDGE AVAILABLE
THEY TEND TO BE:
● PREDICTABLE E PERVASIVE
● ARE ALSO PRESENT AMONG EXPERTS
(E.G. FINALISTIC RESIDUES IN THEORETICAL DISCUSSIONS OF EVOLUTIONARY BIOLOGY)
WHEN THEY LIMIT THE ASSIMILATION AND COMPREHENSION OF SCIENTIFIC CONTENT,
THEY OFTEN REPEAT, ON A SMALL SCALE, FRAGMENTS OF THE HISTORY OF THE DISCIPLINE IN QUESTION
YESTERDAY'S SCIENTISTS HAD TO DISCOVER THE ERRORS IN OWN NATURAL INTERPRETATIONS
TODAY'S PUBLIC MUST BE GIVEN THE SAME OPPORTUNITY!
IT IS TAKEN FOR GRANTED THAT THE VOID TO BE FILLED REGARDS CONTENT... BACK IN 1985, ATTENTION HAD ALREADY TURNED TO TRANSFERRING METHOD AS WELL AS CONTENT
(GOMES DA COSTA, 2019)
COMMUNICATING SCIENCE
NOT ONLY AS CONTENT, BUT ALSO, AS FAR AS POSSIBLE, THE PROCESS OF ELABORATION
(JOHNSON-LAIRD & SAVARY, 1999)
IF HE HAS A KING, THEN HE HAS AN ACE...
ONLY ONE OF THE TWO IS TRUE
IF HE HAS A KING, THEN HE DOES NOT HAVE AN ACE...
IRRATIONAL AGENTS
● EXPERTS AND NON-EXPERTS HAVE QUALITATIVELY SIMILAR COGNITIVE STRUCTURES...
● THE METHODOLOGICAL PRINCIPLES OF SCIENCE ARE "UNNATURAL" FOR BOTH
HUMAN RATIONALITY IS PROFOUNDLY LIMITED AND IMPERFECT (EVEN WHEN THE FACTUAL INFORMATION IS AVAILABLE AND THERE ARE NO DISTURBING ELEMENTS
CONTINGENT CONTENT INFLUENCES COGNITIVE PATHWAYS...
...BUT NO KNOWLEDGE CAN ELIMINATE OUR SUSCEPTIBILITY TO IT
√2
COGNITIVE BIASES
ARISE FROM UNIVERSAL FRAMEWORKS OF REASONING
PROCEDURES TO
PROTECT ONESELF FROM LOGICAL PITFALLS
SCIENCE PRODUCES RESULTS DESPITE COGNITIVE BIASES, NOT BECAUSE IT MAKES THEM DISAPPEAR
METAPHOR OF SCIENCE...
WE ARE NOT OBLIGED TO SUPPOSE THAT KNOWLEDGE REQUIRES ABSOLUTE CERTAINTY
ON THE BASIS OF THE AVAILABLE DATA, I AM CONVINCED THAT EACH ONE OF THESE 100 THESES IS TRUE... HOWEVER IT IS ALSO PROBABLE THAT ONE OF THEM IS FALSE...
THESIS #1
THESIS #2
THESIS #3
THESIS #n
FOLLOWING THIS LOGIC, THESE THESES ARE JUSTIFIED, AND MANY OF THEM WILL BE TRUE (OR RATHER, WILL CONTAIN KNOWLEDGE) EVEN IF WE DO NOT KNOW EXACTLY WHICH ONES
...IS IT REALLY A PARADOX?
INCERTEZZA VS CONOSCENZA
FALLIBILITY INHERENT IN SCIENCE
SCIENCE AS A SOURCE OF KNOWLEDGE WITH WHICH TO ORIENT ONE'S CHOICES
IT IS MORE REASONABLE TO THINK THAT, AMONG OUR HYPOTHESES, MANY WILL EFFECTIVELY BE TRUE AND THEREFORE A SOLID BASIS FOR ACTION
BELIEF IN A THEORY WHICH IS TRUE (CORRESPONDS TO REALITY) AND CONFIRMED (BY THE DATA)
REALITY
ARE EVIDENCE OF
RELATE TO
(WILLIAMSON, 2019)
TRUTH/ FALSEHOOD
DATA
THEORIES
SUPPORT THE
JUSTIFICATION
%
E=mc²
VINCENZO CRUPI
COGNITION, KNOWLEDGE AND THE COMMUNICATION OF SCIENCE

Bibliography

Bozzi P. (1990). *Fisica ingenua*. Milano: Garzanti, 1990.

Carlisle E. e Shafir E. (2005). *Heuristics and biases in attitudes towards herbal medicines*. In V. Girotto e P. Johnson-Laird (a cura di), *The Shape of Reason. Essays in Honour of Paolo Legrenzi* (pp. 205-224). New York: Psychology Press.

Clark M. (2002). *I paradossi dalla A alla Z*. Milano: Cortina, 2004.

Dennett D. (2013). *Strumenti per pensare*. Milano: Raffaello Cortina, 2014.

Feyerabend P.K. (1975). *Contro il metodo*. Milano: Feltrinelli, 2002.

Gomes da Costa A. (2019). *Knowledge, ignorance, and the ever-lasting deficit model*. Exsite Spokes, February 2019, #49: https://www.ecsite.eu/activities-and-services/news-and-publications/digital-spokes/issue-49#section=section-indepth&href=/feature/depth/knowledgeignorance-and-ever-lasting-deficit-model.

Jeng M. (2006). *A selected history of expectation bias in physics*. American Journal of Physics, 74: 578-583.

Johnson-Laird P.N. e Savary J. (1999). *Illusory inferences: A novel class of erroneous deductions*. Cognition, 71: 191-229.

Kahneman D. (2012). *Pensieri lenti e veloci*. Milano: Mondadori.

Kubricht J.R., Holyoak K.J., e Hongjing L. (2017). *Intuitive physics: Current research and controversies*. Trends in Cognitive Sciences, 21: 749-759.

Marconi D. (2007). *Per la verità*. Torino: Einaudi.

McCloskey M. (1983). *Naive theorie of motion*. In D. Gentner e A.L. Stevens (a cura di), Mental Models (pp. 299-324). New York: Psychology Press.

Nickerson R.S. (1998). *Confirmation bias: A ubiquitous phenomenon in many guises*. Review of General Psychology, 2: 175-220.

Popper K.R. (1935/1959). *Logica della scoperta scientifica*. Torino: Einaudi, 1970.

Williamson T. (2000). *Knowledge and Its Limits*. Oxford (UK): Oxford University Press.

Williamson T. (2019). *In the post-truth world, we need to remember the philosophy of science*. New Statesman, 28 January 2019: www.newstatesman.com/2019/01/post-truth-world-we-need-remember-philosophy-science?fbclid=IwAR3x5bXoOoOWwR36hnoruXdZhTNOYCLtHUsvEg4ZBasPUCB9YO6n5K-tfEY.

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