New Advancements in Neuro-Technology & Progress Complications in Neuro-Psychiatry.

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Abstract *. The purpose of this writing is to consider technology's involvement in relation with neuroepistemology and the philosophy of scientific advancement. It revises a past and current image of the main neurotechnical means used in cognitive neuroscience, specially focusing their strategies of application in the scientific practice. Analysing experimental and instrumental justification and their epistemic dissensions, the proposed landscape offers a tendency towards technoscepticism, grounded in two principles here established as variables, whereof assumption appears to be needed in modern practices for achieving their leading goals. The text studies this ambiance building up an analysis upon the invasiveness of major hyper-structured technological strategies, concluding with a paradox of biotechnical advancement, which future scientific dimensions will certainly overcome.*

Brain Technology, Neuro-Ergonomics, Human-Computer Interaction,
Scientific Practice, Neuroepistemology, Justification

EXPERIMENTAL analysis and basic research have been openly adaptive to the contemporary technological panorama, introducing decade after decade innovative forms of instrumentation, machinery and methods in scientific practice. For clinical affairs (including medical, pharmacological and biochemical ones) it is well known that future will come with great innovations in new areas like cognitive neuroscience and brain technology, presumably responding to the needs of care and welfare. In turn, certain less evident consequences are simultaneously emerging, still to be assessed. The nature of this work attends these interactions between clinical interests (especially medical ones) and technical dispositions, regarding a particular query on limits: how invasive does neurotechnia[1] become for understanding cognition? The purpose of the text is to propose an analytical schema about the scales of complexity in current strategies of hyper-structured technology in neuroscience, revising a landscape of some past and present approaches concerning Central Nervous System (CNS) activities, comparing them with critical perspectives that will create the framework for a future of the field. It will be argued that there are mainly two principles applicable to neurotechnia's advancement (assuming factors like technology's sensitivity to certain accessible information). Both can be seen as potential variables that offer an epistemological reading of how scientific practices could be affected by filters and biases from technological practices. Furthermore, a detailed view in neurophilosophy concerns about the access and validity of the observational testimonies gathered from those practices. Taking this

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[1] 'Neurotechnia' is presented as a global term, commonly used in standard research unifying *Neurotechnics* (techniques applied on neural affairs; as staining or microscopy) with *Neurotechnology* (technologies applied on the nervous system; as magnetic resonances). This text will also use such unified conception for talking altogether about apparatus that prepare and machinery that produces.

perspective into account, the article presents a paradox between these two variables, concerning how neuroscience advances through them.

The writing will cover these aims in three parts: *I,* the first part gives the grounds for the two variables of the aforementioned paradox (the two principles), analysing the concepts of order and complexity as are a core key for explaining scientific practices, managed by neuroscientists via *acceptance* (1) and *accommodation* (2) of technologically originated contents into scientific fields, as well as *justification* (3), rather experimental (based on the experiment's results as a form by which to determine causation in science) or instrumental (based on the utility of a method as justification of its practice). Special room is reserved for *calibration* in experimental justification, arguing in favour of a distinction between *biomarkers* and *technomarkers* when delivering results or producing content, launching questions about their authenticity vs. constructiveness.

II, the second part revises the most used practices in cognitive neuroscience, accounting for exploratory, explicative, productive and preparatory technological strategies, with certain epistemological dissensions upon their structure, and in central cases their history.

III, in the third part, a critical discussion of all these processes is sorted out, analysing 1, how to identify their invasiveness and strategies, 2, the dilemma of instrumentalism with observed affairs —a problem that elicits the classical tension between constructivism and realism—, 3, how to treat such techniques, technologies, instruments, methodologies and so forth, assuming the important division between apparatus that prepare and machines that produce; and finally reviewing 4, the most iconic barriers scientific practice and technological advancement face at current times.

In so doing, the paradox is fully exposed with regards to the variables that appear fundamental in modern neurotechnia, opening these notions to further work and extensions in clinical epistemology and medical anthropology. Finally, concluding remarks will outline a precursory understanding about the intervention of invasiveness in the development of the studied neurotechnias.

I

This first part deals with some core concepts that underpin the notion of scientific practice as a customary process of believing. In this sense, beliefs about specific contents, whereof assumption contributes to the fulfilment of a plurally oriented set of scientific goals, comes close to the belief scientist hold when structuring their theoretical interests or interpreting results. This custom presents certain traits that make it appear as a form of arranging information, that can constitute useful knowledge, which might be accepted and re-elaborated depending on those goals. Thus, ideas of order and how to justify it will be analysed, giving birth to the two variables acquainted beforehand.

. *Technology and ordering knowledge*

Order. Order in technology is a conspicuous affair. Locative conceptualisations, as 'arrangement', 'direction' and other intuitive synonymous meanings could make sense of it for a performative conception (such as "x will do y, y will react towards z…"). Nonetheless, when introducing an epistemic perspective into the field of neurotechnia, the sense of its own order is better understood as an *evaluative actualisation of events*. This is: stating a pretty generally acceptable idea, that technology arranges information delivering useful data, in practical terms one can think that the order technological practices give to information needs to serve right to the scientific interests and goals (if some information is not scientifically useful, it is considered trivial just in the sense it doesn't fit the scientific goals to achieve, the admitted agenda). In this way, any arrangement of information (the affairs of study that will be turned into practicable data) serves for better exploration than raw or brute information. No doubt, that could be accepted by the majority of practicioners, for it is the seed of scientific believing and the rational method in its traditional manner. Established in those terms, this seed of order works as a customary process of belief for epistemic communities, such as medical ones, because it actualises their events through evaluation. Favouring a need for theoretical consensus, ordering as scientific practice itself, and order as its basis, could be both then considered *evaluative actualisations of their events of study*, by which some individual believes that some information, grounded by special instructions and directions, will evaluatively be more appropriate than other which is not ordered that way.

In technologist Raymond Kurzweil's terms, *order* stands for 'information following the prosecution of a goal', adapting John Holland's (1998) view. In this sense, scientific goals, plurally decided, connected with social needs, though also guided by private interests, are oriented towards precise communities that hold the possibilities for actualising those goals and objectives, deciding how to develop things that serve to human and nonhuman requirements, at least in a positive application of technical advancement to the collectivity, if one follows the proposals of scientific practice as social knowledge (cf. Longino 1990) cast out of pluralism and scientific chorality (cf. Cartgright 1999; Kitcher 1993; 2003).

It is a reality that today's sciences are modulated by technological achievements to attain those goals, and because of this fact, it is a question to answer every time what is the nature of those scientific issues covered by technical development, for we should inquire and assess what shall the nature of validity be in these discoveries when mediated by technological observations, manipulations and filters.

Complexity. A second concept, *complexity*, manages other conditions: for example, a goal for live organic creatures can be for instance 'to keep alive', and they intuitively arrange this particular goal onto an order of actions —organic activities can follow or assume some evolutionary developmental instructions, so the more appropriate organic activities get labelled as adapted, whilst those which fall outside the goals perish—. In this sense, the term *order* entitles two meanings, one as instruction (order as command, usually introduced in plural: orders) and another as arrangement (the whole ordering of those commands). Then, the acceptance of the goal 'to keep alive' is oriented by multiple implications in a huge vast scale of time actions that constitute the history of an organism, of species and of natural kingdoms, extracting complexity from this process as an emergent feature of the scale interactions. Complexity can be conceived as the complication of the amount of goals to be achieved through commands, that get arranged in such a way the organism (following the example) achieves them profitably, properly behaving. Holland's (1996) and others's terms (cf. Gell-Mann 1995; Kauffman 1995) afford as well order as a feature of the very act of orienting information, and complexity as grasping the same trait in scale perspectives. This text shall keep such concepts in application to neurotechnia's advancement. Orientation, then, is the interpreted conclusion about how order and complexity perform their role in such advancement: they orient the working affairs of the scientific practice.

. *Orientation: to accept, accommodate and justify*

Orientation is in its decision-making part, the manner science is decided to be practiced. This carries on that previous "ordering information until it is turned into useful data" with a hesitation about how to accept, accommodate and justify that order in behalf of the scale of complexity of the information to be arranged. Since the classical meanings of those terms are conventionally determined, their usage in the text will be shorten to the following instrumental definitions, applied to the interactions between technical interests and scientific ones:

Acceptance can be roughly defined as the believing process by which deciding whether to assume or not to work with certain technological content in scientific practice. This assumption is developed until that content is included into some systematic account by evaluative actualisation, or discarded. *Accommodation* would be briefly defined as another believing process through which some accepted technological content must be integrated, without contradiction, into certain specific theoretical field of science, by a plural epistemic community of integrators. Finally, *justification* can be defined in short as the manner some epistemic communities argue in pro and contra about the implications of those previous processes in interpreting results, theoretical explanation through models, their construction, and so forth… It is the believing process by which the role of technology gets justified in scientific research or doesn't. It can be understood in terms of *experimental justification* (when the technologically originated experiment's results are used for building chains of causation in science) or of *instrumental justification* (when the utility of a method is determined to be what underpins its own practice). For an alternative treatment of justification, cf. Popper (1985, 59-62).

It seems clear in a scientific area conquered by technology for developing itself that, when the amount of scientific goals soars (the very act of complication in which complexity is based), the amount of those processes (acceptance, accommodation and justification) increases as well, orienting the scientific practice with interests from the technological fields too.

. *Two variables of advancement*

Minding these ideas, the present analysis of contemporary technology and its epistemological worries in neurotechnia would do good providing a prime hypothesis: that biotechnological advancement, and specially the studied case of neurotechnia, is observed to follow two main principles, fused as variables of order and complexity as defined above. Both point out how the way scientists find their possible 'agenda of goals' —their working schedules considering how helpful certain technology would be for attaining those interest— translates problems (of the kind of orientated justification and flexibility of epistemic access) to their scientific practice, particularly unfortunate when acquiring, interpreting and discussing results.

Neural studies approach to technology as a condition to observe their own field of research, and as future comes, technology turns the main way of getting acquainted with their affairs of study, and their capacity of justifying and assessing their theories, generally based on experimental procedures as well. These principles elicited, are now presented as *critical variables of technical and scientific advancement that deliver a plausible starting point for evaluating practices,* regarding the scale structures of technology's strategies currently considered. Here are the two variables:

(i) *Concerning scales of complexity*: the more complex the affairs of study get (by increasing the number of scientific goals), the more technological the scientific practice ends up being. It is a version of the *Technological Imperative* announced by Mumford (1964, 5-8).

(ii) *Concerning justificatory strategies*: as the level of technology involved in scientific practice soars, higher is the risk of making up the affairs of study, altering the evidences and increasing the possibility of a negative *invasiveness*, from researchers's and experimenters's issues to specimens, intervening in theoretical justification (e.g., by calibration), and the manner filters and bias of technology get transcribed into scientific results.

For instance: if for attending a cortical and subcortical macroanatomy of the CNS, we can visualise a general taxonomy to the naked eye, when now the scale of complexity in the same practice turns from macro to microanatomy —derived from increasing the scientific goals appropriate and situated to the moment—, to attend certain cortical and subcortical neurotransmitter's dynamics would require more technology to get involved, more complicated, and riskier in terms of possible invasiveness. This works as an actually evident principle of neurotechnia's advancement at almost any research field of the area, popping up new affairs of study from time to time. But the question underlying this scenario is grander, and queries about how technology, along with relevant scientific mechanisms, come both to identify and justify such affairs of study, as their access has been made more and more complex: 'How, without increasing the technical resources, would have been able the scientific practices to set for themselves the goal of seeking the biopaths of neurotransmission?' This problem requires an analysis that minds a classical debate in philosophy of science, between constructivism and realism (and derivatives): 'Do scientists through oriented scientific practices build up their affairs of study to some extent, or rather those previously exist as phenomena, but are solely accessible by means of new technical resources?' The same practices that give form to the whole framework, are responsible for assessing every strategy as well, from the goals and interests their needed technology bears, the processes of accepting them, the way technologies can make up results by synthetic systems of modeling, to even the manner theoretical and clinical practices accommodate new admitted data into the old accepted body of knowledge.

It is worth noting some limitations of this account: 1, those variables do not entirely exhaust the landscape of variables orienting advancement in neurotechnia; some other tensions, political, economical, personal, national, strategical and different resources will influence the validity of the variables and their degree of coherence applied to specific neurotechnias. Also, 2, they are not thought to be always present in every mode of clinical advancement (hence the nature of variables), depending on situated variability and experimental use.

As an example of how these variables work, consider a technique of very common usage in hospitals today, as drainage. It has been satisfactorily proved (in this case, experimentally justified), after almost thirty years of theoretical and clinical comparison, that there exist certain risks of developing brain ventriculitis (infections affecting the internal cavities filled with cerebrospinal fluid) when mechanical treatments like external ventricular drain (EVD) are of necessary application (Korinek et al 2005; Tsang & Leung 2012). It can be argued that EVD is a very useful medical technique that prevents another bunch of risks after surgical intervention (and then instrumentally justified), though as it might be seen, it is also a method of clinical care that needs, specially in neurocritical patients, certain maintenance; it is not just the machine but the trained professionals that facilitate its working properly; and adjustments; it is an endoventricular catheterisation that can course as well with pharmacological perfusion, connecting brain tissue with the outside. Beer et al (2008) expose that cleansing, insertion protocols, fugues of cerebrospinal liquid and manipulations of the EVD's devises are among the most influential risk factors of infection. Rivas-Rodríguez et al (2016, 354) expose in their experiment that what utterly matters (interpreting their results) is not the maintenance of the technique if the protocol is accomplished right, but the time the patient gets exposed to the catheterisation, and the manner subsequent insertions, empathetic care and clinical decisions are undertaken.

Analysing this example, and following the first principle, EVD's implementation is a neurotechnical advancement that happens to occur in certain medical scientific practices in relation to an increasing scale of complexity in the way scientific goals (clinical ones, in this case) are decided when brain drainages and perfusions are prescribed. Now, following the second variable, it must be argued there is a high risk of invasiveness due to the very practice of such technique.

If the argument of Rivas-Rodríguez et al (2016) is accepted, the problem is not about the technique's neatness, but about the very practice of inserting an external body into the brain, exceeding the recommended time for its implementation. As it can be identified, the variables are not a question of how technology perverts "good science", but of how technology must be accepted because it is needed, although with consideration of its potential damnations. This raises up an important issue of discussion: clinical epistemology provokes different arguments in comparison with other forms of science, because specially human biosciences elicit different forms of epistemological treatment that those elicited from, and projected to, classical natural sciences; the ones traditionally admitted as western-thought-based sciences, as modern mathematics, physics or astronomy. A final remark is that instrumental justification, as in the example above, plays an important role determining acceptance and accommodation: just because certain methodological technique is fruitfully used and it responds to the goals a specific community of, say, pathologists and diagnosers, has settled down, it manages to get justified as a scientific practice.

Away of the example now, this is an interesting point: to 'detect something' goes along in medicine with its practicability, its interest on finding a marker to work with, a token that serves for developing anamnesis, diagnosis or treatment. But this contrasts with the need of studying something for exploring its causes, using them with interests on explaining that marker (a need for grounding theories). What serves for curing and caring about a patient, might be not that useful for explaining why that item actually works that way.

This should be a basic distinction in clinical epistemology between experimental justification and instrumental one, that experiments try to justify causation when in turn explanation happens to be instrumentalised by interest on practicability: to apply such information to particular patients. The important thing to highlight is that in this cosmology of ideas, the core contradictory interactions set up some dilemmas, that appear because the previous paradox was generated when meeting neurotechnias's development with highly-complex scientific practices, whatever they serve for.

. Justification, instrumentalism and calibration

It will be stimulating to compare justification in a different albeit close scientific area (physics), since the same problems appear in medical fields, as both instruments's calibration are discussed in a pretty similar way. Arguing how efficient or inefficient can be technologies for justifying affairs, A Franklin takes the example of gravity waves measured by Weber (Franklin 1999; originally from his 1997's version). Here follows a recension:

In the texts there is exposed a very close sense of "artificially mediated means" (technical means) that are used by the experimenters to introduce certainty in their observations, calibrating how sensitive the apparatus (an instrumental medium) will be to the information in focus. In this case, discussion is upon this: «Experimenters calibrated their gravity wave detectors by injecting a pulse of known electrical energy at one end of their antenna and measuring the output of their detector. This served to demonstrate that the apparatus could detect energy pulses and also provided a measure of the sensitivity of the apparatus. One might, however, object that the electrostatic pulses were not an exact analogue of gravity waves. Another experimenter did use a different method of calibration. He used a local, rotating laboratory mass to more closely mimic gravity waves.» (Franklin 1999, 18). —Since the astronomical arguments are of no use to this discussion, I shall omit them, focusing on how experiments justify or do not—.

Exposing a detractor of this measuring method, Franklin writes in his terms Collins's argument about technoscepticism, which he calls the *experimenters's regress*: «There are no other rigorous independent criteria for either a valid result or for a good experimental apparatus, independent of the outcome of the experiment. This leads to the experimenters's regress in which a good detector can only be defined by its obtaining the correct outcome, whereas a correct outcome is one obtained using a good detector. [...] This casts doubt on not only the certainty of experimental evidence, but on its very validity. Thus, experimental evidence cannot provide grounds for scientific knowledge.» (Franklin 1999, 19). In a quite evident form, Collins's worries mind the relatively obvious deficits of experimental justification, considering how if assessing scientific criteria depends on technical characteristics that are driven through this same scientific content, the practice turns the rigmarole of the whiting biting its tail (hence the regress). Then, Franklin studies the experiments and writes this: «Contrary to Collins, I believe that the scientific community made a reasoned judgment and rejected Weber's results and accepted those of his critics. Although no formal rules were applied, i.e. if you make four errors, rather than three, your results lack credibility.» (Franklin 1999, 31).

It is interesting to see how both arguments (even when put by Franklin as contradictory) serve to illuminate the technical regress on instruments sensitivity: we get certainty of instruments's outcome because of calibration, though calibration because of the comparative assessment of different outcomes. One author stresses more the impossibility of justifying through experimentation due to calibrations uncertainty, while the other emphasises the plural-communal nature of contrast. As Franklin says, Collins is conflating the way the experiment works with how to show that it works properly. And even when he states Collins's argument upon calibration fails, he accepts «it is often a legitimate and important factor, and may even be decisive, in determining the validity of an experimental result.» (Franklin 1999, 34). In this sense, there is nothing Franklin says about technical calibration of instrument's sensitivity that could be rendered as a "good" or "bad" justification for observations using that calibrated instrument, because he converges all his efforts on *how* contrast of experimentation assesses validity of calibration, whilst the other author focuses on *why* it occurs.

In a social manner, Franklin is more interested on how justifying observations, and producing scientific knowledge, is guaranteed by the collective implication of re-testing. We can see the same attitude in another texts of his: «Suppose we have an observation that can be made using only one kind of apparatus. Let us also suppose that the apparatus can produce other similar observations that can be corroborated by different techniques. Agreement between these two different techniques gives confidence not only in the observations but also in the ability of the first apparatus to produce valid observations.» (Franklin 1986, 167). Though he proceeds right with community-based experimental neglecting… where should we place then the questions about *why* calibration offers or offers not experimental justification (which is what Collins might have been running after)?

It could be considered they are not just talking from different perspectives, but also looking at distinct processes: maybe there we shall draw a difference between the *sensory capacities of instruments to perceive information* (sensitivity), and *how community-based interactions plurally cast this same instrument's sensitivity out of goals, objectives and socially decided matters to be sensible to*, arguing for a 'sensibility' of instrumental usage, following social knowledge epistemic reasoning.

Comparing this idea with that of Franklin's 1986, one can feel tempted to say that no justification rises up from experimental contrast just because different techniques have corroborated a previous instrument's observation, following Goodman's (1983, 74) 'grue induction' problem. If taking this notion of sensibility into account, every process subsumed in scientific practice will order information measured in the expectation that the so socially decided and directed goals of scientific agenda could be attained. If these goals are profitable or not, is an issue ethics and deontology must care in depth, for it is beyond the scope of the present text. This article is just spotlighting what happens in basic research on neuro-fields when if, for example, brain tumour's cure is decided as a main concern for X country and its general community, then X's socially accepted scientific agenda will enforce encephalic cancer exploration and neglect or surpass research on, for instance, an inoffensive cognitive disease as Bonnet hallucinations: the same occurs deciding which information shall be detected and how in neurotechnia. Further observations of Franklin's lines of thought about probability recognition in experimental acceptance can be seen in Franklin & Howson (1988).

Similar facts on technology's acceptance and accommodation in science are offered in Pickering's analysis of a scientific practice structured by machinery, a problem he poetically calls the *dance of agency*, which «seen asymmetrically from the human end, thus takes the form of a dialectic of resistance and accommodations, where resistance denotes the failure to achieve an intended capture of agency in practice, and accommodation an active human strategy of response to resistance, which can include revisions to goals and intentions as well as to the material form of the machine in question and to the human frame of gestures and social relations that surround it.» (Pickering 1995, 22).

Hereafter the term *sensibility* will be used together with *sensitivity* to focus not on how sensors are directly recognising information, and for so they are calibrated to be sensitive to it, but on how instrumental markers that deploy sensitivity are socially, culturally, economically… calibrated following goals and objectives, that push their sensors of observations more sensible to certain scientific inclinations or cultural whims than to others: "What is there being able to be found by our instruments?" will be a question of sensitivity, while "What is there we want our instruments to be able to find?" is the one for sensibility.

. *Biomarking and calibration*

This same argument is pertinent to biosciences, for technical calibration is determinant in the field. If epistemic acceptance (when scientific circles approve and discuss results) and accommodation (when introducing in theory those new results validated) are both uncertain in the generality of sciences, neurosciences with their technical fluctuations are no exception. Now, the way scientific practice proceeds to calibrate neurotechnias's sensitivity to specific information (optimising sensibility, validating instrumental tasks), happens to be carried on by biological and technical markers:

Biological markers are definable as endogenous modulations, traits that an organism shows as symptomatic expositions to organic attitudes (like risks, joy, shame, pain) and do appear as 1, emotional exhibitions (cry, laughter, redden cheeks, gestural muscle moves…), or 2, welfare-maintenance ones (fever, sweating, odour…) —noticing both are intertwined and serve for the same purposes, albeit maybe culturally differentiated—, or 3, other forms of organic activity. Those modulations of biological origin serve in science as marks, traced for a specific goal, and can be defined as the scientific community of the moment needed, referring them as *biomarkers*. On the other side, technical markers affect upon the previous modulations, and are introduced in technological media for helping detection, in experimentation and guidance, along the scientific practice. Those markers are usually sensors, receptors, instruments or apparatus that exchange certain electromagnetic information with biomarkers of endogenous origin *and can, thus, affect their organic activity* in relation to a suspected hypothesised result, that can be then analysed in terms of instrumental justification, referring them as *technomarkers* (as for example fluorochromy, staining, etc…).

The technological paths to biomarking, that concern calibration and sensibility, deal with different interests: *observation* (exploratory technology), *interception* (descriptive technology) and *manipulation* (control: interventional technology). Handling any of those parameters through theoretical orientation applied through technology, calibration gets defined and redefined one way or another, leading to a hyper-structured technological discrimination[2]. Low-levels of technological structuring, applied into scientific discovery tend to minimise technomarkers's involvement, making easy to define its calibration, lowering markers's sensibility and affecting their sensitivity, giving as a result different forms of certainty compared with high-levels of technological structuring, applied into scientific practice, which tend to increase technomarkers's immersion and influence, showing their advancement can actually follow the second variable of justificatory strategies and invasiveness. An example of this could be the case of 1930's electronic microscopes invention, which reveals an understanding of much more complex information than optic ones, that given this scale principle were demanding high-structured technological involvement.

Rudimentary microscopes, both optic and electronic, deliver partial information, but nothing stops to say the same occurs when hyper-structured technical markers in other areas are involved, for the same form of partiality and uncertainty grasps the orientation those portray. For instance: the German AFM hyper-atomic microscope that attends traces of 77 picometres of an atom's diameter (cf. Hembacher, Giessibl & Mannhart 2004) shows what its admitted and situated calibration-goals let its observations to be sensible to, and it will display different biases because of this way of setting sensitivity. So, an amount of structure doesn't take apart any compromises with constructiveness of exploration: aversely, it appears

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[2] '*Hyper-structured technologies*': this article conceives that structures in technology are thought to be present right when a plurality of goals is introduced in certain technological practice, provoking the scale of complexity of such technology to increase, structuring the results in terms of such different scales. The structure can be adapted to any kind of scale process, for distance, time, effort, ecological consumption, etc… Hyper-structures can be identified in terms of achieving certain amount of goals. This way, it is an analytical term that conceives technology that has hyper-scales of goals to achieve, regardless of the content of the scale (distance, volume, time...). Hyper-structured technologies have then a hyper-amount of goals to achieve when working.

Their results are in terms of that scale, and cannot be compared without certain transformation with other scales. They can also increase calibrations uncertainty, if they follow second variable.

to amplify them. Calibration and experimental justification run certain risk-&-security duality, affecting theory eliminations due to technical participation. An example of a biotechnological need of eliminativism can be seen in abiogenetic projects running after artificial origination of life in one way, or simulation of the chemical originations that took place in actual past times on the other, following the Oparin experiments in the 20s, as Luisi (2006) notes. How this technology faces with which forms of life it neglects or not in theoretical realms is what at least its way of technological eliminativism poses, and it is a sort of a dilemma: technology cannot advance frameworks if theory doesn't make an effort building conditions for their structures, but the more structured technology makes the observation, the less facilities for theoretical validation it gives. It is a control paradox the different practices of neurotechnia suffer from too.

II

Now that the main concepts for a critical analysis have been set out, they can be introduced to the positive fields. This second part proposes a revision of the most used neurotechnias in cognitive neuroscience, accounting their main different application strategies, as well as their epistemological dissensions, extracting schemas on their classification, usage and utility.

. *Contemporary research*

Modern neurotechnology starts with synapsis-modeling and the recognition of electrochemical active biopaths through instrumental observation. One of the early macroanatomical encephalographic famous cases conserved in history tracks the technological observation of brain assisting surgery as described by Luckett (1913). Unfortunately the patient passed away several days after the operation, though the pathology served as a milestone where a radiography was combined with an early method for exploring ventricle gas damage by pneumoencephalography. From those 1913's macro lines of research, an enormous abyss has been overflown, passing through great discoveries like Edgar Douglas Adrian's and Horace Barlow's early works of 1928 on electric displays in brain activity, Wilhelm Reich 1930's determination of bioenergy (Reich 2010a; 2010b), Alan Lloyd Hodgkin and Andrew Fielding Huxley (1939) recording the first action potential, and testing a quantitative description of squid's nerves voltage (1952), to the first micro lines of observation, lighting the primeval artificial recombinatory system of neural nets and biopaths ever created; introduced by neurologist Warren Sturgis McCulloch and logician Walter Harry Pitts in 1943 (resketched in 1952); or the aim of reconstructing a model of neural connections, with Palade & Palay's (1954), Marc Colonnier's (1968), and specially George Grey's (1959) contributions to synapsis-modeling…

New neurotechnia currently splits its field in two, conventionally accounted as structural imaging (SI) and functional imaging (FI) —it is worth to note 'functional' tends to be understood either in terms of biodynamic anatomy (BdA) and of organic activity techniques (OAT)—. Those trends of exploration do enact different forms of scales of complexity (we can say they perform like scientific attitudes, which at times differ in many interests of the scientific goals to achieve), and justification strategies that, as defined before, have a quite interesting dialogue with those scales.

Scales bear certain ordering ('following the prosecution of a goal'), that could go from time scales in which technology delivers results; area, territory, and spatial scales of macro-micro analysis (from whole-body behavioural studies to molecular activity); direct-prime imaging scales or indirect-manipulated reconstruction scales (i.e., hectic sounding vs. 3D printing); static-dynamic scales (i.e., histological nanophotography vs. live tissue videography); invasiveness scales (from talkative and superficial tension reaction to laboratory-orchestrated animal sacrifice); and so on.

Macro studies usually highlight SI while the micro, nano and below ones the BdA and OAI, though any technique is now compatible with both interests. Let us revise SI and FI practices, then.

Anatomical structural studies infer their basic historical grounds tracing biopaths for underpinning neurostructures by different forms of microscopy. Here follows a brief description of their main apparatus.

While optic microscopy uses microthin (~10-60 micrometres) laminated tissue to specify histological features of microstructures, using differential tints that configure staining techniques of preparation (*infra*), from the 30s electronic microscopy works with ultrathin (~40-80 nanometres) items for accessing ultrastructure, shooting electrons directed to it.

Electronic microscopy can now handle different modes of reconstruction: by transmission electron microscopes (TEM) with heavy metal-stained samples that get radiated (normally gold and silver that serve as a technomarker in reaction to a biomarker: for example, neuron's reaction to silver is a biomarker that looks stained and thus serves as a technomarker); or by scanning electron microscopes (SEM), that form 3D reliefs of samples, augmented more than 300.000 magnitudes (samples that are scanned by a flow of electrons that are then received by technical sensory points, technomarkers of the machine). Similar would be serial microscopy or serial section electronic microscopy (SSEM), which enables to calculate volumetric shapes of ultra-structured amplifications with different spatial scales, reconstructing the specimen in 3D. SSEM is of a very close functioning to that of confocal microscopy (CFEM), where different focuses of layers and perspectives end up building a 3D model of the observed affair, usually by implementing lasers. On a different branch, immunohistochemia (IMH) studies a similar range of microscopic observation with other interests and complexities (specially BdA and OAT), experimentally measuring artificial immune responses of an organism visualised through fluorochromy, a technique that is highly applied to anatomy and pathology.

Coming to the degree of invasiveness, all of those SI techniques are of an invasive scale of 0 danger (except IMH which would need animal sacrifice). It has to be taken into account that sliced brains are bodily matter pertaining to a "dead patient" exposed to no suffer, albeit exposed indeed to another form of invasiveness: a very high *percussion*. Technological percussion shall be considered a sort of invasion that changes some fundamental traits, naturally disposed in the matter of study, which is now transformed into artificial or artifactual matter of experimentation, being this a central key to epistemic discussion and dissension. This text considers there are two main features of percussive technology: 1, percussion reconstitutes the specimen, turning it into a scientifically realised affair. And 2, percussive technology operates within calibration as an essential activity in science: when higher complexity scales soar, they produce in turn a growing hyper-structured technology.

Percussion, then, affects the strategies of application of neurotechnia to scientific practices. Such strategies can be conceived as technological orientations, for they modulate the interest of scientific work in research, i.e.: exploratory strategies produce exploratory technology for exploratory research. See in *Table 1* the relation between the main different strategies and percussion.

Table 1 — An overview of the core strategies of neurotechnia in relation to percussion and participation:

Strategies	*Neurotechnias's Fieldwork Participation*	*Percussion Dimension*
Exploratory	Very High	Unknown / Low
Explicative	Low	Very High
Productive	High	Unknown
Preparatory	Necessary	High

As an example of this, and presenting as well SI preparatory techniques, the case of fixation and staining will help to expose the argument. Fixation with formaldehydes and glutaradehydes kills and stabilises the biotic activity of the sample (other liquids and alcohols are used for dehydrating viscera, following inclusion in paraffins and resins). Fixation recreates a nervous condition that natural states of the CNS wouldn't have (as for example, in surgical scenarios).

It also translates original matter of study with subsequent preparatory techniques, as lamination (involving instrumental in fresh-like samples as vibratomes, refrigerated ones as cryostats, and of pressure thickness ones as micro and ultramicrotomes), destructing some tissular equilibrium-ratios that can be specially relevant for other interests (as computational volumetrics in live patients or comparative pathology).

The second preparatory technique, staining, can elicit a special case of percussion. For example, we see that the *silver black reaction,* or Golgi's method (introduced by him in the 1870s) opens an aleatory possibility of neuronal identification that is rare and difficult to assess, for just some neuronal traces of nuclei react to silver, randomly managing to differentiate between histological traits of nuclei aggregations and axonal pathways. It is surprising to note that this, which was one of the most used technique for computational and quantitative studies of neurons, has no behavioural basis: there has been no full answer to why silver casts this chancy relationship with nervous cells, turning it, as Sholl (1956) appreciates, a very subjective approach long ago discussed among neurophysiologists. Faster variations of the Golgi's method were achieved with the addition of osmic acid to the reaction (with the samples in ~3 days perfusion), as Ramón-Moliner (1957) and Davenport (1960) systematically present. Nevertheless this variation still delivers irregularities, being counter-reacting to certain barriers, as selective typologies of cells or distinct animal tissue, to which reaction loses efficacy.

Neurobiologist Korbinian Brodmann used the *black reaction* to differentiate his proposed 52 cortical areas, an experimental solution needed of continuous re-test, theoretical change and a problematic further accommodation (if we mind the historical neglect to Gall's phrenology at the time), precisely because of this and others disadvantages in calibrating a technomarker's sensitivity (reaction to silver) to a biomarker (unknown process of the neurotissular acceptance of silver). For a similar goal, the nucleus content coloration, another famous contribution was that of Franz Nissil, whose works on methylene blue to adapt RNA reactions, and dissociative forms of nuclei's endostructure are currently of normal use. This trajectory draws a great point for medical epistemology: calibration does affect change of proceedings and theory frameworks.

Now, the great advance of this century has been the better understanding of glial bodies, that load with the ~90% of the human encephalic matter, and modern research pays attention to the implications they have for enacting neural processes. From the most vital to the most trivial activities of innervated organisms, they have been observed to be more than brain glue (Allen & Barres 2009; Kettenmann et al 2013). They intervene from metabolisation of the commonest neurotransmitters to fagocitation, from memory and self-location perception (Nishiyama et al 2002; Kim et al 2011: both works regarding the advances of the Japanese discovery of glial protein S100-*beta*'s effects on mnesicoception) to neuropsychiatric diseases where some glial-based failure happens to occur (cf. Shapiro, Bialowas-McGoey & Whitaker-Azmitia 2010 for S100-*beta*'s effects on Down Syndrome and Alzheimer's disease). Those findings were enabled because staining processes have covered this focus of research too: myeline, astrocytes, oligodendrocytes and more glial external biopaths are markable by other different methods as well, like the last adjustments of the *Weigert Pal* technique, or the *Lulox Fast Blue*, through which using peroxidase and hæmalum solutions (the three most used ones are Ehrlich's, Harris's and Mayer's hæmatoxylin) and different alcohols, the directions of terminals can be segregated. The epistemic rigmarole is that all these preparations are instrumentally percussively changing natural affairs *for* observation, orienting the goals and changing the order by which observation of biomarkers works with, affecting the way exploratory technology will be sensible by means of technomarkers. These forms of percussion are a manner of invasion medically assessed with revision studies too.

In macroscopic studies, SI's invasiveness plays an even more important role concerning welfare, safety and suffering, holding a very useful though more invasive branch of neurotechnia.

Alan Cormack and Geofrey Hounsfield conceived and invented in the second half of the 20th century the computerised tomography scan (CT), applying X-ray's electromagnetic emission technology to the medical use. Modern CTs combine different depths of eight 2D black-and-white images to recreate a 3D model of the patient's organs. Percussion of this technology is nowadays unknown, yet it is palpable that risks of specific radiation in other instruments is utterly evident. For example, Addington et al (1959; 1961) and Deichmann et al (1959) experimented with live organisms exposed to microwaves (200 Mc), and proved with subjects that orientation changes along the polarisation wave axis, from perpendicular to horizontal (parallel to them), provoked their animals's death. Nonetheless, HL Konig (1979) holds the theory that electromagnetic fields in other amounts of radiation play an important role in evolution and adaptation. The forces of the electromagnetic spectrum that affect us humans vary from Ultra-Low (ULF are below 300hz) to Very-Low frequencies (VLF start from 300hz) and swift directly to infra red radiations (~300Ghz) and X-rays (~3 x 10 to the 17th hz). Some forms of coalition can be seen in organic activities harmonised to environmental electromagnetic fields. Considering the relation between circadian and sleep brain rhythms aligned to earth basic resonance (10hz), as Schumann and Konig exposed in 1954, there exist the evidences to recognise that planetary distribution of electromagnetic forces have been contributing through biological history to the conditions for human (and other species) cognitive behaviours, so that they shall be had in mind as a possible form of contactual conditioner. The potential for curative or health improving electromagnetic fields's therapy needs to be seriously observed: those magnetic fields empower ambiances of electrodynamism that can interact with the basal ionic equilibrium of synaptic transmission, settled in simultaneous frequency, rendering the same percussive potential capacity for lesions than for healing.

Following these ideas, SI technology involves as well magnetic resonance (MR), whereof main interests are precisely the magnetic alteration of hydrogen atoms's orientation. Using a 3 Tesla electromagnet for standard MRs, the process pumps a signal that momentarily rearranges in a particular orientation each singular randomly oriented proton in each hydrogen atom. It frames a low energy atomic condition that sends back the signal to the equipment's sensors near the head (technomarkers sensitive to this atomic biophysical behaviour, that appears to be the biomarker), capturing and transforming information into a recreation of the observed brain's features. Once again, 'arrangement of information until it turns into useful data' can be appreciated as scientific practice. The observations can be weighted by T1 and by T2 images of protonic density, presenting substances in contrasting colours, where T1 images are usually applied for volumetric exploration, albeit in a very costly and time consuming form, including personal for drawing and assessing comparative results. Intriguingly, a very low percentage of water hydrogen atoms present energy reaction to this method, although it is instrumentally justified as heavily useful.

A similar technology is voxel and regionally quantified water diffusion, that comes from MRs. A precise type of exploration concerning water molecules's movement in the brain is diffusion tensor imaging (DTI), which uses technomarkers sensitive to the kinetics of H_2O flow's directions. Tensors calculating flows of molecular change rapidly detect axonal invariance (since the dynamic effort is obviously parallel to the axon cylinders), making noticeable whereto fibres cohere with others. It recreates this in a current number of 3-40 settled directions, increasing year after year, delivering each one coloured, assisting exploratory and productive tasks (cf. Moseley, Bammer & Illes 2002 cognitive applications). Modern diffusion spectrum imaging (DSI) offers a more clear perception of fibre collisions and entanglements, though lesions and other forms of tissular sickness could modify reception and results. It is a technical method of well extended use for the overall universal exploration of the macrostructure of brain damages, for strokes, cognitive disablements and detection of cranioencephalic traumatisms.

Percussive epistemic considerations can be put forward in close relation to the invasiveness of the other previous methods: it is a recreation of the CNS transformed into a derivative coloured and structural image that rebuilds the natural phenomena, following very precise orders of information, attaining (quite efficiently, meaning teleologically, instrumentally) higher and higher scales of complexity.

It has been argued that the more complex an arrangement of ordered information gets, oriented towards the prosecution of certain goals (to be carried out by scientific practices), the more technology gets involved in for attaining them —which states the principle of complexity (***ı***)—. In connection with this, it is possible that in some cases, given the appropriate ambience, the aforementioned paradox could emerge: the more technology gets involved in the development of scientific practices, the more reconstructive, invasive and percussive it happens to be —which states the variable of justification and invasiveness (***ıı***)—, causing this scenario to be exposed as an epistemic dilemma for justification and veracity to scientific knowledge, theoretically (what is there that science can access to) and systemically (how shall scientists proceed).

. *FI*

Studying the second split, functional imaging, the organic activity exploration reacts in a much similar way to that paradox. Studies on Wada, or sodium amytal test (SAT/ISAP) performed by Canadian neurologist John Atsushi Wada, let physicians to speculate upon language and self-location cognitive capacities of live lucid patients, which is an example of organic activity exploratory techniques (OAT). It is normally seen as a safe technique, though some problems of results's contrasting and interpretational acceptance have been remarked, as usual for individualised test programmes. This querry is shared with another in vivo exploratory technology, electroencephalography (EEG), a famous registration method that developed a way of obtaining information through converter technomarkers, sensitive to the neuronal activity of some biomarkers as pulses, waves, resonance and overall electric noise. EEG measures the bioenergetic variations of action potentials (voltage) in neurons, quantifying their firing rate during experimental controlled conditions via electrodes. The international location system *10-20* arranges those electrodes in an occipital-frontocortical macrostructure. This is present as well in a technical variation, the evoked potentials/related potentials encephalography (EP/ERP), which is used for tracking nuances of the previous data, reoriented to other goals, as the exploration of semantic cognition, specially in language, memory and learning.

A more invasive form of EEG would be subdural electrocortical measurement (SEC) and intracranial electrodynamic technologies (ICE), consisting of a sheet in intimate contact with the brain's faces loaded with electrodes that detect more complex features of neuronal activation procedures. Here we can see the same epistemic problem: it would deliver data with finer resolution, though much advanced percussion, due to the increasing complexity scales of such an information-highlighting process, intervening also invasiveness troubles with results.

In this road of invasive techniques, magnetoencephalography (MEG) constitutes a considerable highly practiced form of registering almost unnoticeable magnetic fields, normally originated on pyramidal endocortical sulci neurons active through III and IV cell layers along the cerebral surface's furrowed lines. It is necessary that cellular aggregations in a magnitude of hundreds of thousands get activated at the same period in order the MEG sensor (neuromagnetometre) to focus areas of electromagnetic fields, being this affair extraordinarily linked to the reality of cooperative activity and hyper-interconnectivity of the CNS's tissue (cf. Damoiseaux & Greicius 2009).

The integrative organic activity of brain architectures has been the counterexample of blind scientific practices: proposed by CS Sherrington beginning the past century, the idea that cooperative needs of several nuclei are rendered by evolutionary paths, rooted into experience-practices (instead of functionally divided tasks), has been one of the biggest contributions to the current understanding of human behavioural-based neuroanatomy. Sherrington (1947) took his idea experimenting with dogs's artificial lesions in sensory channels that, simultaneously, compromised the regular activity of movement-associated regions that were initially untouched. As muscles and senses were driven at once in his experiments, blood vessels and organic activity were proved to work in cooperation too. He first proposed with CS Roy (1890) a core relation between nervous system activity and oxygenation through bloodstream irrigation capacity of purveyance. This proposal turned into the basis for current technologies of blood fluxes in brain scanning, and two modern techniques amplified the advancement: positron emission tomography (PET), that measures bloodstream in brain vascular systems; and functional magnetic resonance (fMR), that interprets brain oxygenation while performing neurological effort. It is analysable in these practices how the management of technical results and theory making are reunited in the following sense: the more technological involvement the field accepts, the more the scientific practice assumes it itself can assess theory change, epistemically resulting in techniques speculation vs. experimental justification debates.

For instance: numerous standardised techniques adopt this discovery of blood oxygen level dependent (BOLD) biomarkers's reactions, introduced in the 60s-70s through MR methodologies, following the thesis that hemodynamic oxygen-glucose ratio requirements of the encephalic region are utterly related with cognitive efforts and operative tasks. Since brain-blood cells's metabolism interacts with magnetic fields, this gave birth to a technical exploration of those affairs, proceeding with the origination of functional magnetic resonance imaging (fMRI). The BOLD method applied to fMRI was first discovered and analysed by neurobioengineer Seiji Ogawa and his group in 1990. Through this technique, subject's cortical (and in a less obvious way subcortical) CNS's areas involved in operational-intentional and pathological activities in experimental conditions can be mapped out (cf. Ogawa et al 1990; Kim & Ogawa 2012; and cf. Ríos-Lago 2008 for neuropsychological applied fMR research). It is interesting to consider how this procedure was used at the same time among different circles for pointing out controversial experimental justifications, from functionalism and modulism (regarding patched and 'imbricated functions of tissue'), confronted with a wider vision in integrativism and conectionism (where organic activities were to be observed as cooperative actions of distinct types of cells, happening thanks to transductances of tissular bioenergy, instead of functional attributions upon it). It seems not the same to base the argument on the existence of activities due to 'inner properties of items', than basing it on 'ecological conditions for the existence of those activities'.

Modern neurophysiological accounts are now drawn in close contact with the last one of these, neglecting for example strong manners of modulism. Does technology develop then systems of experimental reasoning sufficiently critical for changing theoretical foundations? It is well accepted technological interference or intervention is useful for deploying new boundaries in certain already known bodies of theoretical work, but shall now be *assessing those* a new task ascribable to technology too?

There is no reason why to avoid talking about the indirectness of these processes: if measuring the artificial conditions for certain cognitive efforts in experimentation, the practice is then extracting conclusions over a different thing than if analysing direct immediate conditions for any organic activity, including cognitive efforts (in the way a specific experiment defines it in its scientific statements and approaches). This is not to say natural conditions are explorable by means of direct experimental conditions —what shall be a direct condition then?—, but to say that if deep technical variations are a prerequisite to be applied to research, then this at first instance observed information will turn into reconstructed data, clearly and in all its logic, providing re-exposed, re-created knowledge. Take the example of positron emission tomography (PET), one of the most efficient techniques, and full of applications at current times in pathology and clinical diagnosis. Built on a similar thesis —that brain performance requires of specific metabolic activity, in relation to the possibility of developing patterns of ionic conductance of synaptic firing—, PET techniques introduce in the organism substances that will be needed for normal metabolic processes, albeit in radioactive conditions (as carbon 11 or oxygen 15). In brief: the artificially introduced unstable isotopes of those chemicals will start losing positrons, coming into annihilation with the organic ambiance's electrons, emitting two photons (biomarker) that will be recognised by the tomograph (technomarking sensor), delivering a computerised signal, and mapping with differentiated colours the density of reactions in particular areas, allowing a mathematical combinatory system (like stereology) to read where tissular performances don't enable regular metabolic conditions, indicating pathological ones.

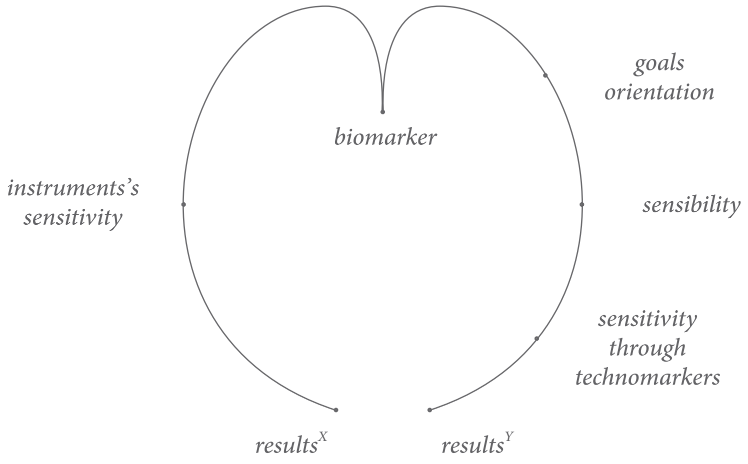
Nevertheless, as Stuss & Knight (2002) expose, lesional methods inform about the need of a region for completing a concrete organic activity, although this emerges as an oriented conclusion derived from tracking a highly selected marked response. Taking into account this technique is specifically sensible to a few biomarkers that are transduced into a singular sensitive technomarker, in experimental conditions, and attuned to artificial exposition… does this offer any epistemic justification on neurocognitive theory making, in order to assume monodirectional biomarking is a fully explanatory base for correct or deficient organic activity? This is important to have in mind when using this kind of results with explicative uses (for example, to explain what causes neurotransmitters's deficient conditions in fybromyalgia's hypersensitivity to pain-events, admitting it is yet an agnogenic disease).

What are we ignoring when orienting our expectations to singularities and defined goals, directing calibration towards barriers of theory? Indirectness in exploration suffices for knowing where to apply surgical methodologies, or for concrete difficult treatments for instance, but the more complex the theoretical need of reasonable causation gets (beyond mere correlation), the more technology reconstructs explorations, and more instrumental instead experimental results are.

Knockout transgenic alterations, biomarking in cardiovascular brain accidents for pathological anatomy exploration, and other perfusion techniques or anaesthetics have been left out of analysis, but the main point was set out: contemporary neurotechnias's performances affect for the best and the worst key inclinations of scientific practice. They lead through different strategies, and generate different results depending on which situated scales of complexity are applied.

See in *Diagram 1* below the epistemic analysis taking into account sceptic perspectives and variables (right hand), compared with naive ones (left hand).

Diagram 1 — Situated usage of technical resources can affect scientific practice orientation towards results. Naive and sceptical perspectives:



III

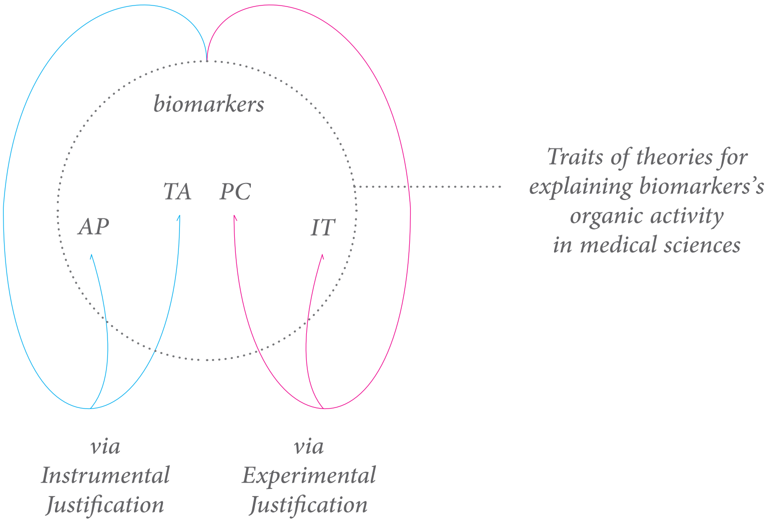
Given the previous panorama, in this last part an epistemological discussion about how its invasiveness potential relates with theoretical working is presented. Cognitive neuroscience is, through its neurotechnia, a particular case of clinical research, because it, in contrast with other forms of the group of "Health Sciences", focuses itself on how to explain the basis of cognition, memory, emotions… grounding the field of study, instead of curing, helping or treating the patient's conditions. This distinction is fundamental, for the scientific practice of its theory making, accommodation of technological contents and the way it ends up justifying affairs as "real", bears a distinctive character. In this sense, here is a relation between the previous SIs and FIs and how they co-inhabit with their application strategies, the usage scientific practice gives to them, and which main traits of theoretical production are of their interests.

. Discussing invasiveness, strategies and justification

As an example with which to begin, light detection could be considered a new "non-invasive" technique that happens to offer very nice precision. Callaway & Yuste (2002) studied two-photons laser microscopy of excitatory reactions in single neurons, showing no invasive features and offering highly developed precise results. For instance, they experimented enacting single unified neurons that got excited by intensity of light, managing to even act over specific parts of the cell. Optic research can be as well very topic, spatially reduced, proceeding with descriptive motivations that interact with a rather "non-invasive practice" (cf. the calcium experiments of Sabatini & Svoboda 2000).

In this terms, high-range precision technology (with descriptive and detective interests) appears to be far more invasive than low-range precision technology (with overall revision motivations). Though, regarding an appliance scale, high-ranged invasion would show more applications and capabilities than low-ranged invasion (at least in clinical fields until now). As exposed in *Diagram 2*, when those concerns are applied to theory making, percussive tensions interacting with the studied affairs (biomarkers that can be affected by technomarkers) emerge in two directions: 1, those theoretical processes bearing traits that include neurotechnias's contents via instrumental justification (left hand), and 2, those others that make so via experimental justification (right hand). Both depart from biomarking for explaining biomarkers using different strategies of tech-

Diagram 2 — Relation between acceptance and accommodation with justification as introducer of invasiveness (danger, percussion or any other alteration). Abbreviations: (AP) Acceptance by Practicality, (TA) Theoretical Accommodation; (PC) Plausible Causation, (IT) Inclusion in Theory. They all affect upon technical content, i.e.: "PA of the usage of staining preparatory techniques as a basis for neurons differential observation".



nological application. The left-handed neurotechnias's contents introduced into scientific practice, are instrumentally justified by epistemic communities for they serve to cure, dealing with treatment, or because those contents work with patients and certain conditions. The right-handed ones occur to be experimentally justified for given the possibility of reproducing certain variables of experimentation with them, they have been satisfactorily proved (the scientific goals attached to their certainty have been achieved). This means that the way percussive contents, or bias from technological admittance into scientific practices, are expressed in certain theory making, will vary depending on which justificatory interest is elicited.

Thus, for instance, if staining is realising certain scientific contents (as neuronal tissue), which would be nearly impossible to see without its intervention, *Acceptance by Practicality* (AP) is a theoretical process that will necessarily be instrumentally justified (although, not by so experimentally justified). That is the key concept in instrumentalism for a technique: given its utility, this serves for grounding its own practice. Following the staining example, in the case of *Theoretical Accommodation* (TA), whenever the *black reaction* is applied as an accepted method, certain communities notice contradictions that can shake results (the case of Brodmann's cortical mapping using black reaction). Then, subsequent theoretical processes will be needed for accommodating anomalies, experimental risks and so forth of having instrumentally accepted a technique bearing unexplained factors. On the other hand, experimentally justified theoretical movements can be also exposed: *Inclusion in Theory* (IT) can be understand as a basic introduction of technologically originated necessary contents (as for example the very existence of dopamine, D_2 cortical receptors, or even distances as nanometres). Now, *Plausible Causation* (PC) would emerge from an experimental correlation from those included contents with beliefs about causal derivation, for example: "certain lesions round caudate nucleus are correlated to motor impediments".

In this sense, as *Table 2* proposes below, those main justificatory interests influence the relation between how neurotechnias's core strategies develop their focus of attention, and the usage of those neurotechnias in the scientific practice, affecting theory making with their results: if for elaborating theoretical content, usually (though not always) in the aim of explaining the organic activity of certain biomarker, theoreticians pick up data with these specific orientation, it is obvious that justificatory interests, goals and objectives of science, and cultural, social, economical, political tensions and so forth have the potential for affecting the results (which will be used in theory for explanation).

Table 2 — Relation between strategies, technological usage in scientific practice, and their justification interests:

Strategies	*Usage*	*Main Justificatory Interests*
Exploratory	Used as observatory witness (microscopy: SEM, CFEM)	AP
Explicative	Used for explaining diagnosis (TC, fMRI) or for theory making	AP, TA; PC, IT
Productive	Used when offering a construction of the observed affair / Used by means of certain construction (brain modeling, surgery)	TA; PC
Preparatory	Used when arranging and changing certain features of the observable affairs for permitting other future technologies to work with it (fixation, staining)	AP, PC

Taking acceptance and its theoretical consequences, altogether they form a problem of 'qualifying' validation, i.e.: technological contents justified into scientific practices must be now validated as creating evidences for certain hypothesis. But who decides what is to be qualified as valid? Larry Laudan sounds this idea in a very versatile manner: «being a consequence of a hypothesis is neither necessary nor sufficient to qualify something as evidence for that hypothesis.» (Laudan 1995, 29). Nonetheless, anyone who observes the scientific development of life sciences must notice instrumental justification serves profusely to scientists in their daily practice. Reviewing *strategy*-centered discussion, some other epistemologists have paid good attention to orientation: for instance, against a monolithic idea of "Science", with a clear method and with clear laws, Philip Kitcher introduces the notion of an alternative procedure in life sciences that seems quite simple and evident, which is to set the theory as a collection of «strategies for answering questions about the history of living things, sometimes locally and in the short-term, sometimes more globally and with attention to a large temporal range.» (Kitcher 2004, 3). That fits with what he calls 'local critique' to scientific methods, and it is what this text has been applying to analysis.

. *Discussing the instrumental and the real*

Facing this epistemic problem acquiring observational evidences, a traditional discussion in philosophy of technology and clinical epistemology appears again. Realism and constructivism controversies —mainly in the wave of Instrumental Realism and Scientific Instrumentalism— can be satisfactorily applied to the actual concern. We see that neuroscientists and neurobioengineers bear no deontic problem assuming their daily working scientific practice is immersed into an artificial reconstruction of the immediate information they want to organise, though they don't relate this belief with the dilemma of justifying by technical evidences what their explorations have been crediting or discrediting.

A quarry can thus be written. Excerpting Woodward's considerations: «Is it perhaps true that the correctness of a theory's claim about manipulability of relationships requires or presupposes that its associated claims about entities must be interpreted realistically?» (Woodward 2003, 112).

This is, are we able to assume the experimental conditions of our explorations bear no explanatory conditions that are precisely invoked by the way we look at things as if they were really there? Strategies then overlap each other. Realities or scientific entities and the manner the systemic practice approaches to them, hold the trouble of relations: isn't it probable that experimental practices could be positively capable to justify, because they use technomarkers that interfere the relation of what such practices want them to be sensitive to, 'actualising', 'realising' or 'making things emerge as real' due to their orientation?

To this the same author responds: scientific disclosures have no need to trace any ontological preoccupation of what they are measuring or experimenting. Such theories claim causal shifts due to interaction —and observation and predictions of those links—, when some entity affects some other entity by adjusting forms of their relation, and that is precisely the way realism contrast 'scientific realisation'… This also means that we can compare different theoretical strategies (both being supposedly right) though underpinned by distinct ontologies (Woodward 2003, 114). Relationships, instead of stuff and matter, carry a new form of enactive ontological prowess that Woodward reads as Instrumental Realism, for «according to the instrumental realist, any scientific theory that makes causal claims (or that offers explanations) is inevitably in the business of making causal claims about what would happen if various counterfactual possibilities were to be realized.» (Woodward 2003, 115).

In contrast, for Scientific Instrumentalism, science itself shall be a descriptive more than explanatory activity (cf. for example van Fraassen 1976; 1980), able to ensure argumental stability just in the case of exploratory processes. But then, what would happen to the technical involvement through which potentially re-elaborated or not ontological perspectives appear to be installed in the very core of the scientific identification of its affairs of study? If explanation, which bears justification, has nothing to do with positive scientific practices following certain conceptions of Scientific Instrumentalism, then theory making shall be at times an activity apart from scientific practice, which seems very counter-intuitive.

At some point, the same concern is traced by Rom Harré when arguing we have two tokens to analyse here: 1, the instrument's relation to a naturally occurring material system —but, is it not that a form or a priori realism gets already implied in this assumption?—, plus 2, the nature of the phenomena created by using it (those scientifically realised, created or augmented modes of presentation of the affairs of study's reactions)… for there is the risk that these second ones «may be phenomena that are brought into being by running the apparatus as a model of some material system. If the apparatus is a model of something in the world, we can ask what is the relation between the phenomena we produce in the apparatus and those that occur naturally.» (Harré 2003, 29-30). In his way, Ian Hacking proposed a close examination of the same problem: if manipulation took place, could we say we have been exploring new phenomena or the same previous to exploring natural ones? (Hacking 1983; 1981).

This is one of the features of the hyper-technical scientific practices that makes the point for this text's worry on assessing those instruments. Instead of 'apparatus', some of them shall be identified as 'machines', which will be a more delicate and appropriate consideration, following the semantics beyond the automatised use of these words. Harré then draws the idea that those technical data processors have qualities being changed before heated up, switched on and manipulated in order to function as 'providers of their data', and it is interesting to note it is their own data, of the technique, the instrument or the machine, what scientists actually manage. With this idea, he builds up his concept of calibration (Harré 2003, 33).

Addressing the root for the term apparatus, from the Latin verb *apparare*, «to prepare, make something ready», we shall notice the instrument for preparing an array of work, or the working matter itself (like Golgi's staining method, formaldehyde, paraffin and even utensils as cryostats and vibratomes), is intentionally different in usage and strategies than those of an apparatus (as a microscope or an fMRI's tube for example). Considering their usage, we shall declare that the first forms prepare affairs *for* justification (for example, through observation), whilst the others introduce variables *of* justification.

Now, this article argues their role is, in different magnitudes, related with their observation's scale of complexity, that is conceivable as that of a *machinery* of knowledge: a microscope is, in its own sense, a machine that produces relevant epistemological knowledge, while an ultramicrotomy brain dissector, intuitively thought as a machine at simple look, happens to be better identified as an apparatus, because instead of exploring it prepares the tissue later to be explored. Attending to this in hyper-structured technological practice, utensils and instruments that serve scientists for preparing shall be regarded as apparatus, whereas those which serve them for producing, as machines. Hence the distinction. This idea comes closer to Harré's schema (2003, 33), by which 'models' are apparatus being part of nature (where the term 'model' is used in an epistemological perspective that assumes scientific knowledges are constructions of the world), and 'instruments' apparatus being detached from nature, for so they belong to artifactuality. Intellectual machinery (as models) intervenes as production in a very obviously different way than objectual or procedural machinery (as microscopes and virtual imaging). This view also grasps Hacking's differentiation between 'theory realism' and 'realism of entities' (Hacking 1983, 37-38). Further research on biomedical theory making through models can be seen in Evelyn Fox Keller's (2003, 213-215) recension.

. *Discussing barriers in CNS modeling*

Now percussion and justification have been outlined, the last discussion can look at how they face special barriers while producing models of the CNS's activities.

A first barrier for modeling in theory making is eliminativism, that usually gets the name of oversimplification or reductionism. For example, injecting MPTP to artificially and momentarily cause lesions in dopaminergic-reacting neurons, serves for the clinical BdA-oriented understanding of Parkinson disease. At this stage, percussive troubles with this technique arise, for it can suffer from a high level of experimental eliminativism, in this case, reductionism to what is properly technomarkable by the chemicals that have been previously instrumentally justified as having an organic relation to a symptom, neglecting the other biomarkers if they exist.

Another barrier is safety uncertainty: an example in bioengineering techniques will be the primeval state of modern nanorobotics's applications to fixing the blood-brain barrier. Nanobots working at the hemato-encephalic disjoining pathologies were proposed by the movement of Robert Freitas Jr and others in the 90s, still active nowadays. As he notices (cf. his *Nanomedicine v.2* 2003, 158), this technology will need to overcome intrusiveness of artifactual organisms affecting the body with side effects, which is a very obvious form of technological invasiveness in clinical practice.

Other boundaries regulate what is the established knowledge, and preconceives the impossibility of new modes of scientific reasoning capable of surpassing their script. Limits of social acceptance also intervene, as the public opinion stating the accepted morals. In this very sense, regional vs. worldwide spread medicine makes the example: serious research programmes in Zen meditation were developed in the 60s in behavioural treatment through respiration, as well as the neuropsychiatric implications of trained Yoga exercising, silent-sited meditation (Zazen) and other forms of ancient medical practices. Though such studies can help CNS modeling and theory making, not just in clinical fields but in pathology and basic research too, their practice and economic investment are usually swept away (Das & Gastaut 1961; Kasamatsu & Hirai 1966). Nevertheless, comparison helps to "naturalise" different biophysical theories, understanding those schedules as constitutive of a "dappled world", following Cartwright's (1999) or Longino's (2000) inclinations in pluralism.

Another kind of limit, that has been explored in philosophy of technology and bioethics, is informed consent and decision making between doctors (or medical personal) and patients, as well as between researchers and participants, both for experimentation in vivo. The analysis of Spruit, van de Poel & Doorn (2016, *Table 4*) exemplifies the relation between the different factors of informed consent and clinical asymmetrical relationships. Relational accounts of biomedical conducting fieldwork can be appreciated in Cassell (1980, 29) too.

A final barrier is the factual one, beyond which science is inapplicable. Its practice just doesn't respond to certainty in predictions, neither it can deal with a serious body of theory, nor even extend it to, or be helped by, experimental justification. Some cases grounding psychiatric theorising can exemplify such scenario.

. *Conclusion*

An epistemic analysis of neurotechnias's current landscape has been presented in this work, studying the way methodologies perform their labour as increasingly involved in scientific practice.

It has been defined that advancement of these technical and technological processes can follow a variable of invasiveness, expressly concerning the implications of hyper-structured technological involvement in growing scales of complexity, falling in a technosceptic perspective: the more complex the interests of the filed get, the more technology is injected into scientific practice. A paradox attending this observation has been finally delivered, in the face of the possible evolution of the area: the more the levels of technology soar for a scientific practice of highly complex information, the more invasive, dangerous, riddled with errors scientific knowledge and practices could potentially become.

In these terms, main conclusions can be displayed: existence of technological percussion, as a sort of invasion that changes fundamental traits of scientific affairs of study, has been analysed and exposed. This led to emphasise that those neurotechnias which are instrumentally and experimentally percussive, can actually be orienting and re-orienting specific scientific goals in the order-&-complexity schema with which to explain biomarkers's organic activity. It has also been remarked that orientation, as a core trait of scientific decision, can be appreciated as a practice of 'arrangement of information until it turns into useful data', a sort of mnemonic sentence used for meaning that the growth of scientific goals affects how accurate or distorted interpretation of results can appear (results produced by instruments which might be necessary to attain such objectives of science). Thus, the whole panorama of practices turns the perspective of realism in science into scepticism.

These concluding factors, open to further evaluation, are presented in the text to be taken into account for a renewed treatment of phenomena in neurophilosophy and philosophy of technology.

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Addington, CH, Osborn, C, Swartz, G, Fischer, FP & Sarkees, YT (1959) «Thermal effects of 200 megacycles (CW) irradiation as related to shape, location and orientation in the field» in Süsskind, C (ed.) (1959) *Proc 3 An Tri-Serv Conf on Biological Effects of Microwave Radiating Equipments*, 10-14, California UP: Berkeley.

Addington, CH, Osborn, C, Swartz, G, Fischer, FP, Neubauer, RA & Sarkees, YT (1961) «Biological effects of microwave energy at 200 Mc» in Peyton, MF (ed.) (1961) *Biological Effects of Microwave Radiation, vol. 1;* 117, Plenum P: NY.

Adrian, E (1928) *The Basis of Sensation: The Action of Sense Organs,* Christophers: London.

Allen, NJ & Barres, BA (2009) «Neuroscience: glia — more than just brain glue» in *Nature 457*: 675-677.

Álvarez-Linera, J (2008) «3TMRI: advances in brain imaging» in *Eur J Radiol 67*: 415-426.

Bandettini, PA (2009) «What's new in neuroimaging methods?» in *Ann NY Acad Sci 156*: 805-821.

Beer R, Lackner P, Pfausler B & Schmutzhard E (2008) «Nosocomial ventriculitis and meningitis in neurocritical care patients» in *J Neurol 255*: 1617-24.

Callaway, E & Yuste, R (2002) «Stimulating neurons with light» in *Current Opinions Neurobiology 12*: 587-592.

Cartwright, N (1999) *The Dappled World*, Cambridge UP: Cambridge.

Cassell, J (1980) «Ethical principles for conducting fieldwork» in *Am Anthropol 82*: 28-41.

Colonnier, M (1968) «Synaptic pattern on different cell types in the different laminae of the cat visual cortex. An electron microscope study» in *Brain Res 9*: 268-287.

Das, N & Gastaut, H (1961) «Variations de l'activité éléctrique du cervaux, du cœur et des muscles squelettiques au cours de la méditation et de l' 'extase' yoguique» in *Electroencephalography Clinical Neurophysiology 13*: 452-456.

Damoiseaux, JS & Greicius, MD (2009) «Greater than the sum of its parts: a review of studies combining structural connectivity and resting-state functional connectivity» in *Brain Struct Funct 213*: 525-533.

Davenport, HA (1960) *Histological and Histochemical Techniques*, Saunders: Philadelphia.

Deichmann, WB, Stephens, FH, Keplinger, M & Lampe, KF (1959) «Acute effects of microwave radiation on experimental animals (24,000 Mc)» in *J Oc Med 1*: 369.

Franklin, A (1986) *The Neglect of Experiment*, Cambridge UP: Cambridge.

— (1999) *Can that be Right? Essays on Experiment, Evidence, and Science*. Boston Studies in the Philosophy of Science. Springer.

Franklin, A & Howson, C (1988) «It probably is a valid experimental result: a Bayesian approach to the epistemology of experiment», in *Studies in the History and Philosophy of Science 19*: 419–427.

Freitas Jr, RA (2003) *Nanomedicine 2*, Landes Biosci: Georgetown.

Gell-Mann, M (1995) *Complexity*, Wiley: NY.

Goodman, N (1983) *Fact, Fiction, and Forecast*, Harvard UP: Cambridge.

Gray, EG (1959) «Axo-somatic and axo-dendritic synapses of the cerebral cortex: an electron microscope study» in *J Anatomy 93*: 420-433.

Hacking, I (1981) «Do we see through a microscope?» in *Pacific Philosophical Quarterly 63*: 305-322.

— (1983) *Representing and Intervening*, Cambridge UP: Cambridge.

Harré, R (2003) «The materiality of instruments in a metaphysics for experiments» in Radder, H (ed.) (2003) *The Philosophy of Scientific Experimentation*, 19-38, Pittsburg UP: Pittsburg.

Hembacher, S, Giessibl, FJ, Mannhart, I (2004) «Force microscopy with light-atom probes» in *Science 305.5682*: 380-383.

Hodgkin, AL & Huxley, AF (1939) «Action potentials recorded from inside a nerve fibre» in *Nature 144*: 710-712.

— (1952) «A quantitative description of membrane current and its applications to conduction and excitation in nerve» in *J Physiology 117*: 500-544.

Holland, J (1996) *How Adaptation Builds Complexity*, Wesley: Reading.

— (1998) *Emergence: From Chaos to Order*, Wesley: Reading.

Kasamatsu, A & Hirai, T (1966) «An electroencephalographic study of the Zen Meditation (Zazen)» in *Folia Psychiatrica et Neurologica Japonica 20*: 315-336.

Kauffman, S (1995) *At Home in the Universe: The Search for the Laws of Self-Orgnaization and Complexity*, Oxford UP: Oxford.

Keller, EF (2003) «Models, simulation, and "computer experiments"» in Radder, H (ed.) (2003) *The Philosophy of Scientific Experimentation*, 198-215, Pittsburg UP: Pittsburg.

Kettenmann, H & Kirchhoff, F & Verkhratsky, A (2013) «Microglia: new roles for the synaptic stripper» in *Neuron 77*: 10-18.

Kim, HS, Seto-Ohshima, A, Nishiyama, H & Itohara, S (2011) «Normal delay eyeblink in conditioningin mice devoid of astrocytic S100B» in *Neurosci Lett 489*: 148-153.

Kim, SG & Ogawa, S (2012) «Biophysical and physiological origins of blood oxygenation level-dependent fMRI signals» in *J Cereb Blood Flow Metab 32*: 1188-1206.

Kitcher, P (1993) *The Advancement of Science*, Oxford UP: Oxford.

— (2003) «What kind of science should be done?» in Lightman, A; Sarewitz, D & Dresser, C (eds.) (2003) *Living with the Genie*, 201-224, Island P: Washington.

— (2004) «Evolutionary theory and the social uses of biology» in *Biol Philos 19*: 1-15.

Korinek, AM, Reina, M, Boch, AL, Rivera, AO, de Bels, D & Puybasset, L (2005) «Prevention of external ventricular drain – related ventriculitis» in *Acta Neurochir 147*: 39-46.

Laudan, L (1995) «Damn the consequences!» in *Proceedings and Addresses of the APA 69*: pg. 27-34.

Longino, H (1990) *Science as Social Knowledge*, Princeton UP: Princeton.

— (2000) «Reply to P Kitcher» in *Philosophy of Science 69*, 573-577.

Luckett, WH (1913) «Air in the ventricles of the brain following a fracture of the skull» in *J Nerv Ment Dis 40*: 326-328.

Luisi, PL (2006) *The Emergence of Life: From Chemical Origins to Synthetic Biology*, Cambridge UP: Cambridge.

McCormick, BH (2002) «Brain Tissue Scanner enables brain microstructure surveys» in *Neurocomputing 44*: 1113-1118.

McCulloch, WS & Pitts, WH (1943) «A logical calculus of the ideas immanent in nervous activity» in *Bulletin Mathematical Biophysics 5*, 115-133.

Mumford, L (1964) «Authoritarian and democratic technics» *in Tech Culture 1*: 1-8.

Nishiyama, H, Knopfel, T, Endo, S & Itohara, S (2002) «Glial protein S100B modulates long-term neuronal synaptic plasticity» in *Proc Natl Acad Sci USA 99*: 4037-42.

Moseley, M, Bammer, R & Illes, J (2002) «Diffusion-tensor imaging of cognitive performance» in *Brain Cognition 50*: 396-413.

Ogawa, S, Lee TM, Kay, AR & Tank, DW (1990) «Brain magnetic resonance imaging with contrast dependent on blood oxygenation» in *Procl Natl Acad Sci USA 87*: 9868-9872.

Palade, GE & Palay, SL (1954) «Electron microscope observations of interneuronal and neuromuscular synapses» in *Anat Rec 118*: 193-202.

Priebe, MH, Robitaille, PML, Abduljalil, AM & Kangarlu, A (2000) «Ultra high resolution imaging of the human head at 8 Tesla: 2Kx2K for Y2K» in *J Computer Assisted Tomography 24*: 2-8.

Ramon-Moliner, E (1957) «A chlorate-formaldehyde modification of the Golgi method» in *Stain Technol 32*: 105-116.

Reich, W (2010a) *Análisis del Carácter*, Paidós Ibérica: Madrid.

— (2010b) *La Función del Orgasmo*, Paidós Ibérica: Madrid.

Ríos-Lago, M (2008) «Resonancia magnética funcional y neuropsicología» in *Radiología 50*: 351-365.

Rivas-Rodríguez, A, Suárez-Mier, B, Rivas-Rodríguez, L & Lana, A (2016) «Riesgo de ventriculitis asociado a cuidados del drenaje ventricular externo en pacientes neurocríticos» in *Rev Neurol 62*: 351-6.

Roy, CS & Sherrington, CS (1890) «On the regulation of blood supply of the brain» in *J Physiology 11*: 85-105.

Sabatini, BL & Svoboda, K (2000) «Analysis of calcium channels in single spines using optical fluctuation analysis» in *Nature 408*: 589-593.

Schwab, ME & Buchli, A (2012) «Plug the real brain drain» in *Nature 483*: 267-268.

Shapiro, LA, Bialowas-McGoey, LA & Whitaker-Azmitia, PM (2010) «Effects of S100B on serotonergic plasticity and neuroinflammation in the hippocampus in Down syndrome and Alzheimer's disease: studies in an S100B overexpressing mouse model» in *Cardiovasc Psychiatry Neurol 2010*: 153657.

Sherrington, CS (1947) *The Integrative Action of the Nervous System*, Yale UP: New Haven. Sherwood, CC, Stimpson, CD & Raghanti, MA et al (2006) «Evolution of increased glia-neuron ratios in frontal cortex, *Proc Natl Acad Sci USA 103*: 13606-13611.

Sholl, DA (1956) *The Organization of the Cerebral Cortex*, Methuen: London.

Shook, JR & Kurtz, P (comps.) (2009) *The Future of Naturalism*, Humanity Books: Amherst NY.

Spruit, SL, van de Poel, I & Doorn, N (2016) «Informed consent in asymmetrical relationships: an investigation into relational factors that influence room for reflection» in *Nanoethics 25 May 2016*. DOI 10.1007/s11569-016-0262-5.

Stuss, DT & Knight, RT (2002) *Principles of Frontal Lobe Function*, Oxford UP: London.

Tsang, A & Leung, G (2012) «External ventricular drain infections« in *Hydrocephalus 7*: 88-98.

van Fraassen, BC (1980) *The Scientific Image*, Clarendon: Oxford.

— (1976) «To save the phenomena» in Boyd, P; Gasper, P & Trout, JD (eds.) (1992) *The Philosophy of Science*, 187-194, MIT P: Cambridge.

Woodward, J (1997) «Explanation, invariance and intervention» in *Philosophy Science 64* (Supp.): 26-41.

— (2003) «Experimentation, causal inference and instrumental realism» in Radder, H (ed.) (2003) *The Philosophy of Scientific Experimentation*, 87-118, Pittsburg UP: Pittsburg.