

The Coupling Argument faced by a Mechanistic-Causal Scientific Psychology: the case for Intracranialism.

1- The Coupling Argument and Psychological Explanation

In their paper 2006 “Why the Mind is Still in the Head” Adams and Aizawa (‘A&A’ henceforth) analyse what they call the ‘Coupling Argument’ in favour of transcranialism. They view the class of arguments such classified as «far and away the primary sort of argument given in support of transcranialism» (pag ?). According to the authors, the basic formulation of the coupling argument takes the form of an inference « from the observation that process X is in some way causally connected (coupled) to a cognitive process Y to the conclusion that X is part of the cognitive process Y. The pattern of reasoning here involves moving from the observation that process X is in some way causally connected (coupled) to a process Y of type Φ to the conclusion that X is part of a process of type Φ » (pag ?). A particular instance of this kind of argument consists in claiming that from observing that the use of pen and paper are *physical aids* in a long calculation one can state that pen and paper are *part* of the cognitive process (the calculation) itself. A&A identify this sort of argument in several notorious transcranialist authors such as Robert Wilson (2004), Alva Nöe (2004), Raymond Gibbs (2001), Tim van Gelder (1995), Andy Clark (2001) and Clark & Chalmers (1998).

This inference, from a ‘coupling claim’ to a ‘constitution’ one is according to A&A, a fallacious one: it rests on an ill-sustained claim of simple and straightforward substitution *salva veritate* between ‘coupled’ and ‘constitutive’ (pag?). Their criticism is not a very sophisticated, they simply put forward a sample of transcranialist arguments and show how the inference from ‘coupled’ to ‘constitutive’ rests on simple verbal twists and ‘smart’ and convenient conceptual slips. Put simply: for A&A transcranialist’s claim for the couple argument inference is based on simple *rhetorical* moves (even if not deliberately) (pag?). It is not surprising, then, that they do not provide a full-blown formal proof of the unsoundness of the inference.

In my opinion A&A fail to acknowledge the real strength of transcranialist’s arguments. In fact, and at least refraining to C&C original use of the argument in their seminal 1998 article, there is more than a simple rhetorical move sustaining the soundness of the inference. As I understand, their general argumentative move is based

on what I freely call the *Explanatory Reformulation of the Coupling Argument* (ERCA). This reformulation is based on an implicit principle (call it The *Constitution Principle* (CP) stating that:

For all Process X and all Cognitive Process Y, *if X is coupled to Y, then, X is constitutive of Y only if a Psychological Predicate Z deployed by the Psychological Explanation E of Y subsumes X*

Since CP is universally closed, it allows the inference from (*Token*) Process Y to Process *Type* Φ . The deployment of CP and its role in ERCA is best understood by calling the very C&C specific argumentative process as stated in their original paper. This directs us to the celebrated discussion of Inga and Otto's examples. Recall Otto's peculiar situation:

Otto suffers from Alzheimer's disease, and like many Alzheimer's patients, he relies on information in the environment to help structure his life. Otto carries a notebook around with him everywhere he goes. When he learns new information, he writes it down. When he needs some old information, he looks it up. For Otto, his notebook plays the role usually played by a biological memory. Today, Otto hears about the exhibition at the Museum of Modern Art, and decides to go see it. He consults the notebook, which says that the museum is on 53rd Street, so he walks to 53rd Street and goes into the museum. (Clark & Chalmers, 1998, 12-13)

According to the Coupling Argument, from here one concludes that Otto's notebook is not only *coupled to* his cognitive activity but plays instead a *constitutive cognitive role* itself. But, and here enters ERCA, C&C conclude this *not directly* but by analyzing how Otto's actions are *psychologically explained*. To better understand the dialectics of their argument we should first address the case of Inga who, contrarily to Otto, has a normal memory:

Inga hears from a friend that there is an exhibition at the Museum of Modern Art, and decides to go see it. She thinks for a moment and recalls that the

museum is on 53rd Street, so she walks to 53rd Street and goes into the museum (Clark & Chalmers, 1998, 12)

A clear and straightforward explanation of Inga's actions would state that «Inga believes that the museum is on 53rd Street, and that she believed this even before she consulted her memory. It was not previously an *occurrent* belief, but then neither are most of our beliefs. The belief was sitting somewhere in memory, waiting to be accessed.» (Clark & Chalmers, 1998, 12). This *dispositional belief* kind of explanation is, according to C&C, also available to Otto's situation:

Clearly, Otto walked to 53rd Street because he wanted to go to the museum and he believed the museum was on 53rd Street. And just as Inga had her belief even before she consulted her memory, it seems reasonable to say that Otto believed the museum was on 53rd Street even before consulting his notebook. For in relevant respects the cases are entirely analogous: the notebook plays for Otto the same role that memory plays for Inga. The information in the notebook functions just like the information constituting an ordinary non-occurrent belief; it just happens that this information lies beyond the skin. (Clark & Chalmers, 1998, 13)

Rephrasing what is here stated: 1- Otto's and Inga's actions share the same *Psychological Explanatory Schema*: “(i)-Inga/Otto *desires* to go to the museum & (ii)-Inga/Otto *believes* that the museum is on 53rd Street”; 2- By applying this schema, one *subsumes* the ‘use of the notebook’ by the predicate *believes* (i.e., the informational content deployed in the notebook counts as a legitimate propositional content to be held as an intentional attitude); 3- Applying the *Constitution Principle*, the use of the notebook can soundly be said to be *constitutive* of Otto's cognitive process. That is, Otto's notebook is as *part* of his cognitive activity as Inga's biological memory is for hers. This, in a nutshell, comprises the Coupling Argument moral derived by the use of ERCA (assuming CP) as applied to this particular case. It should be stressed that this line of reasoning is, in principle, available to all transcranialists in their ‘coupling’ to ‘constitutive’ inference. If so, A&A claim regarding the unsoundness of this inference is much harder to sustain than admitted by the authors.

2- Explanation and Reduction: Folk vs Scientific Psychology

What is the nature of Psychological Explanation *E* of *Y* enounced in CP? As testified above through the examples, C&C answer this in a straightforward way: Belief/desire intentional (folk) explanation. Their choice is supported by claiming that this kind of explanation preserves simplicity and, therefore, consists in a *better* explanation of behavior than a putative scientific (and more complex) alternative:

Of course, *one could* always try to explain my actions in terms of internal processes and a long series of “inputs” and “actions”, but this explanation would be needlessly complex (1998, 5, my emphasis)

To this complaint, A&A rightly (in my view) answer:

We find this sort of considerations hardly telling, given the pragmatics of explanation and the wide range of appeals we are willing to make to intentional ascriptions. Bear in mind, one may be inclined to say that one’s car doesn’t want to start or that one’s plants are thirsty for water. That certainly a lot easier than troubling with *any real complexity having to do with the internal mechanisms of cars and plants*. Perhaps one can get with a “folk psychology” that uses such explanations, but *one should hardly aspire to such an indiscriminating theory for a science*» (2001, 57, my emphasis).

I take this quotation as a crucial one in understanding the status of both folk and scientific explanations and what sets them apart. Scientific theories are *precise*; folk ones are *loose and indeterminate* to a certain extent. The pragmatics of folk explanation can dictate simplicity to a wide range of contextual mundane situations some, for the sake of simplicity, one can soundly attribute intentional states to inanimate objects such as cars and plants. In the present context the problem lies on the fact that we is trying to address Ontological/Metaphysical claims concerning the ‘bounds of Mind’. The trouble here consists in the heavy burden one puts on the

shoulder of a *folk* or *intuitive* notion of 'Mind' when deciding, detailedly, its very own intra or extra cranial ontological boundaries.

The Ontological/Metaphysical claim can be traced back to CP underlying ERCA. There is an implicit metaphysical premise in CP that states: 'Psychological Explanation depicts the 'real' nature of Mind'. It is by relying on this premise that the Coupling Argument has ontological/metaphysical import on deciding if the Mind transcends the brain. The question, therefore (and again) is what kind of psychological explanation (scientific or folk) depicts the nature of 'Mind'. It may be instructive to consider that the *same* premise and the *same* related question sustains a considerable part of the discussions regarding the 'Inter-Theoretic Reduction Reformulation of the Mind-Body Problem' (Bickle, 1998). It is possible, therefore, to try to clarify this issue by turning our attention to the Mind-Body reductionist debate.

Generally, as a topic from Philosophy of Science, an Inter-Theoretic Reduction is viewed as a relation holding between two theories: the theory to be reduced and the reducing one. Independently of the reduction model one adopts (the classical one proposed by Nagel or the New Wave reformulation endorsed by Hooker and Paul Churchland) what becomes clear from the discussed cases of (historic) reduction is that the inter-theoretical relation holds between two *scientific* theories (for instance, the reduction of simple thermodynamics of gases to kinetic theory or of classical Newtonian mechanics to the special theory of relativity). Assuming a widespread general scientific realist stance on what metaphysics is concerned regarding contemporary Philosophy of Science, one reconstructs the mind-body problem as an inter-theoretical reduction where the basic reducing theory corresponds to a neurophysiological one and the theory to be reduced to a scientific psychological one (see Bickle, 1998, 41).

A vast amount of philosophers would, at this point, call the attention to the fact that instead of a scientific psychology what is at issue in the reduction framework is folk psychology. As Patricia Churchland puts it: «Virtually all arguments against reduction, if they are not just confusions about what intertheoretic reduction is, depend on the designation of some aspect of our commonsense framework as correct and irreducible» (Churchland, 1986, 299). Nevertheless, as stated above, taking into account both scientific realism and other successful historical reductions, it becomes clear that folk psychology is at best, a second choice for assuming the role of the

reduced theory within the framework of ‘Inter-Theoretic Reduction Reformulation of the Mind-Body Problem’.

Proponents of folk psychology answer by mobilizing the ‘Only Game in Town’ argument (Fodor (1975)). According to this perspective, a ‘scientific psychology’ is not yet available and, the kind of scientific psychology envisioned is close enough to the sentential ‘propositional attitude’ type of explanation typical of folk psychology such that we can retain the latter (Fodor 1975, Pylyshyn, 1984). Those who propose a scientific psychology as the theory to be reduced need to show that, at very least the fundamentals of such a scientific psychology are already available. That’s exactly what I propose to do in the next section. I will show that, at the level of the very current scientific activity, one can abstract the fundamentals of a framework for a Scientific Psychology capable of positively answer the implicit metaphysical premise in CP, namely that ‘Psychological Explanation depicts the ‘real’ nature of Mind’.

3- A framework for a mechanistic Scientific Psychology

3.1- Mechanistic Explanation and Psychology

What form should a Scientific Psychology adopt? Loosely, any scientific endeavour consists in the deployment of a set of generalized explanations concerning a specific natural (or social) domain. Following this formulation, one can accordingly state that a scientific psychology consists in the deployment of *general explanations* concerning behaviors. The question poses itself. What kind of ‘explanations’ are we seeking and in what sense ‘general’?

The question about the nature of psychological explanations is in itself a well known polemic philosophical quest. Here I will just state what I think is the most promising proposal without providing the argumentative and empirical reasons favouring such choice. The perspective I favour is what is being dubbed ‘New Mechanistic’. Mechanicists adopt a general naturalistic attitude: their claims are not *a priori* formal considerations about the nature and formal structure of science. Rather, they seek a descriptive analysis of the way scientists deploy their explanations (with an emphasis on biological and medical sciences) [Bechtel & Wright, C. (2009); Craver, C. (2007); Darden (2002)]

I will start by briefly present the main theses and basic principles of the ‘New Mechanism’ in Philosophy of Science. An equally brief application of these theses to psychology will precede a more detailed presentation. I start with the very notion of ‘mechanism’.

Generally, a *mechanism* can be considered as a *physical device* consisting in a collection of *entities* and *activities* engaged in the physical causal production of a certain phenomenon given certain organizational principles such as spatial localization of entities or temporal duration of activities (more about this below). Examples of mechanisms studied in natural sciences include cellular metabolism, all sorts of chemical reactions, the blood-pump mechanism or the action potential of neurons (it should be noticed that mechanisms are not restricted to natural-kinds, certain machines are also considered mechanisms the computer being the classical example). Often, mechanisms are structurally/hierarchically organized into different levels, consisting of spatially delimited entities and activities, each performing different functions at increasing lower levels of organization.

A mechanist *explanation*, in turn, is an epistemic activity, shaped by certain methodological heuristics and constraints, that consists in the description of a certain mechanism in which the *explanandum* is a phenomenon f produced by that mechanism M . That is, we want to know *how* f comes about by understanding the inner workings of the mechanism it is believed to produce it. More specifically, the inner workings of a mechanism correspond to its components and the way they are causally connected in order to generate that phenomenon. Generally, scientists adopt a, both structural and functional decompositional strategy where the function or phenomenon f is decomposed into several sub-functions (or sub-phenomena) $\langle f', f'', \dots \rangle$ each corresponding to a certain sub-mechanism $\langle M', M'', \dots \rangle$. Each sub-function is, in turn, capable of being decomposed into further simpler functional-mechanistic units a level ‘below’ (eg: f can be decomposed into $f'1, f'2, \dots, f'n$ each corresponding to a certain mechanical structure $M'1, M'2, \dots, M'n$). This functional decomposition may or may not correspond to the structural hierarchical assembling of mechanisms mentioned above. So, basically, a mechanist explanation is both causal and multilevel.

3.2- Psychological Mechanistic Generalizations as Referents of Natural Kinds

Within the general framework just presented, a *Psychological* mechanistic *explanation* is to be understood as description of a *neural mechanism* M that causally explains a certain *target behavior* B . Following some prominent mechanistic philosophers (Bechtel..., Bickle, 2003), I will show how a mechanistic psychology derives from the field of Behavioral Neuroscience, since it is this discipline that is currently concerned with the neural mechanisms underlying behavior. Nevertheless, we do not simply want neural mechanistic explanations of behaviors but *General* ones in order to satisfy our previous characterization of Psychology as a Science. In what follows I will suggest a unified approach to both the explanatory and generalization claims.

Psychological generalizations PG state general mechanistic/neuronal explanations of behavioural phenomena B . These generalizations are achieved by inductive abstraction from previous neuroscientific empirical results. Usually, these generalisations are achieved from animal experimental findings. For instance, results from protocol experiments of spatial memory in mice serve as a *basis* for constructing the explanatory content of a generalization PG for spatial memory covering all animals that display this mental state, assuming the conservation across species of the relevant structures (in this case, homologues of the CA1 area of the hippocampus in vertebrates). In structural terms, a psychological generalization PG is a triple: $PG = \langle B, f, M \rangle$, where B is a target behaviour (if we take again the example of spatial memory, it could state something like: “the optimisation in spatial navigation of animals in their environment”), f is a mental state explaining B (e.g. spatial memory) and M is a description of a neuronal mechanism explaining f (e.g. the mechanism of LTP in hippocampal place cells). Extensionally, Mcn range over a domain \mathbf{D} of terrestrial multicellular animals.

PG generalizations reveal the structure of *Theoretical Concepts* of Behavior Neuroscience, i.e.: Psychological/Mental Concepts. Psychological Concepts *are* Theoretical Concepts of Behavioral Neuroscience *because* they refer to the putative underlying/neuronal explanation/cause of B , where B is a *Phenomenological Concept* of Behavioral Neuroscience, grouping several *observable* behavioral phenomena. We can summarize this moral by stating that: for *any* Psychological Concept/Theoretic Concept of BN *there is one* corresponding PG generalizations (with the general form ‘ $\langle B, f, M \rangle$ ’). PG generalizations reveal, simultaneously, the *structure* of Theoretical Concepts of BN (i.e.: Psychological/Mental Concepts) and the *meaning* and *reference* of the corresponding theoretical *terms*. Regarding this latter statement, it should be noticed that

PG generalizations are constructed inductively satisfying the normative constraint of *maximising projectability*. The satisfaction of this constraint manifests itself in one important feature concerning the nature of *PG* generalizations: a realist stance towards the referents of psychological terms, i.e., realism of mental states.

Mental states are real in the sense that they correspond to particular neural mechanisms described by *M* conceived as real causal features in the world, according to our best data on the subject (i.e., neuroscience). In practice, neuroscientists conceive mental states as (supposedly) realised by certain neural/componential mechanisms that they try to discover. The following extract from Squire, on the nature of declarative memory and its relation to the hippocampus, can be taken as a typical exemplification of this assumption:

[T]he terms ‘explicit’ memory and ‘declarative’ memory, when one considers the properties that have been associated with each, describe a *biologically real* component of memory that depends on particular structures and connections in the brain.

(1992, p.205, emphasis added).

Neuroscientists constantly assume this theoretical attitude since there are important methodological and pragmatic reasons to act this way. The main reason behind this realistic assumption is precisely the need, mentioned earlier, to develop neuronal explanations with inductive power and projectability. John Bickle is clear about this inductive aspect when discussing the above quotation from Squire and relating it to evolutionary conserved traces:

This approach forges a connection between human neuropsychological data and experimental mammalian research. The ‘particular structures and connections’ namely the hippocampus proper, entorhinal cortex, perirhinal cortex, and perihippocampal gyrus, have homologs across the mammalian class. Since declarative (or explicit) memory is coextensive with hippocampal-requiring memory, the term is applicable to memory research on humans, other primates and rodents.

(2003, p.78).

These considerations, concerning the realist status of mental states and related explanatory relevance, echo recent work from philosophy of science on the topic of *natural kinds*. At the heart of this debate is precisely the way scientific practice organises and classifies reality in terms of *natural kinds* that, by means of their underlying structure, possess inductive relevance. A classical example is the modern chemical classification of substances in terms of their underlying microphysical properties (Boyd, 1991, 1999; Griffiths, 1997, Kroon, 1985).

In order to understand better the role of *PG* generalizations and, in particular, the present discussion concerning realism of mental states, I will address some remarks made by Richard Boyd in his discussion of natural kinds. According to Boyd, the fundamental scientific practical act that establishes a certain term as denoting a natural kind is the process of *accommodation*, within a certain disciplinary matrix, between classificatory/taxonomical practices and ‘real’ causal structures. In the present discussion, within the specific disciplinary matrix which includes neuroscience, general biology, and evolutionary theory as background knowledge, we are able to state that we have find a natural (projectable) kind when we are capable to fit a certain mental state f with the appropriate (evolutionary preserved through species) neural-causal-mechanism M . Rephrasing this idea, Boyd claims that there are two different ways of defining a natural kind: a *programmatically definition* stating the functional role played by that kind within the disciplinary matrix, and an *explanatory definition* referring to the underlying causal properties that *justify* the functional role stated on its programmatic definition (Boyd, 1999, 70). In the context of *PG* generalizations (assuming the general structure $\langle B, f, M \rangle$), the *programmatically definition* of a natural kind K corresponds to the description of the role of f in explaining B whereas the *explanatory definition* consists in M ’s explanation of f . The natural kind *term* referring to the natural kind K is the mental-state term that fills f in a particular *PG* generalization. If we again take the spatial memory CN model as an illustration, spatial memory corresponds to the natural kind where its *programmatically definition* states that spatial memory causes “the optimization in spatial navigation of animals in their environment” and the corresponding *explanatory definition* (that justifies what is stated in the programmatic one) declares that spatial memory is explained by or corresponds to “The mechanism of LTP in Hippocampal Place Cells”.

The important moral to be extracted from the discussion of mental states as natural kinds is that, in order to satisfy the projectability constraint, we have to consider

mental states as real states that *explain* behaviours by virtue of their correlation to specific neuronal-causal mechanism. Two important philosophical consequences follow from this: first: anti-realist conceptions of mental states, in particular operationalist ones (functionalist and behaviourist), are ruled out from *scientific practice* since they are unable to answer the projectability demand. Second: a scientific psychology built upon neural/mechanistic fundamentals and satisfying the *projectability constraint* is prone to ‘cut Mind at its joints’. Regarding this subject matter Paul Griffiths writes the following:

The idea of a natural kind is the subject of long philosophical tradition. The central theme of this tradition is that there are underlying explanations of the correlations of properties that allow us to sort things into distinct kinds. Theory construction “cuts nature at its joints” when it puts together things whose resemblance to one other has such an underlying explanation. When organisms are organized into species or substances divided into chemical elements, they are divided in ways that are *projectable*. (1997, 174, emphasis in the original)

This means that a scientific psychology thus conceived comes close to become the best candidate for a theory depicting the ‘real nature of Mind’ as demanded by the *Constitutive Principle* (CP) underlying ERCA. We should, therefore, reconsider the soundness of the Coupling Argument from this new perspective.

4- *Mechanistic Scientific Psychology and the Coupling Argument*

Recall, then, that according to ERCA, the soundness of the Coupling Argument depends on the adoption of a suitable psychological explanation. In particular, the Coupling Argument is sound if it satisfies CP and unsound otherwise. C&C provide an answer to CP by displaying a folk psychological explanation according to which psychological predicates *subsume* the use of external artifacts thus granting the soundness of the Coupling Argument. Has a Mechanistic Scientific Psychology explanation the same transcranialist outcome? My answer is ‘no’. I will show next that the first and foremost difference between the two approaches (a folk and a scientific ones) concerns the methodological constraint of *behavioral decomposition*. The

different degrees of grain allowed by scientific psychology concerning such decomposition turns such an explanation at odds with the Coupling Argument.

According to C&C, the stream of action taken by Otto can (and should) be decomposed very coarse-grained, in such a way that his and Inga stream of actions can be said to be 'isomorphic' to a certain extent: both actions share the explanatory schema "(i)-Inga/Otto *desires* to go to the museum & (ii)- Inga/Otto *believes* that the museum is on 53rd Street". By applying this schema to Otto's case it is shown that the use of the notebook is subsumed by the psychological predicate 'Believes'. It follows then, as shown in section 1, the vindication of the Coupling Argument soundness.

C&C recognize the possibility of an alternative finer grain decompositional strategy. But they reiterate the 'one step to many' argument according to which such finer grained decomposition fails to preserve explanatory simplicity (C&C 1998, Clark, 2006). This objection was scrutinized in section 2 where we testified the existence of two competing kinds of psychology (folk and scientific) to fulfil the place of 'Psychological Explanation *E*' as stated in CP. There are several reasons favouring a Mechanistic Scientific Psychology to be the chosen candidate to such place. Next I will take into account those reasons.

4.1- Three Reasons for adopting a Mechanistic Scientific Psychology to Fulfil the Place of E in CP

The first tenet addresses the 'only game in town' argument. This was mobilized in favour of Folk Psychology in face of the lack of any tenable and recognized scientific alternative. This challenge is answered straightforwardly by noticing that the mechanistic kind of psychological explanation is in fact part of current scientific practice, namely Behavioral Neuroscience. The more general framework is, it should be assumed, more an abstraction from those practices than an explicit articulated neat set of principles endorsed by those same practitioners. Nevertheless, this 'abstractness' should not distract us from the essential; the mechanistic alternative is well sustained in actual and contemporary scientific practice dealing with the neuronal/mechanical explanations of behaviors. The 'only game in town' argument should, therefore, be dismissed.

The second reason has to do with what was dubbed in section 2 as 'the implicit metaphysical premise in CP', which stated that the candidate to fulfil the place of *E* as the 'Psychological Explanation of *Y*' should depict the "real' nature of Mind'.

As noticed in section 3.2, by satisfying the projectability constraint, the mechanistic psychological explanations referred to *real* mental states conceived as natural kinds. This consequence turns the mechanistic scientific psychology as answering positively (to a considerable extent) to the metaphysical premise in CP.

The third reason is more of a negative towards the adoption of a Folk Psychology candidate. The basic criticism has to do with the looseness of mental states attribution by folk psychological standards. This indeterminacy of mental states attribution was already addressed in section 2 with the A&A testimony on the easiness by which one can attribute mental states to a car or a plant. This problem is an important one in the present context since it deals with the specific grain one should adopt in individuating mental states ascriptions and, by extent, on what grain to adopt in behavioural decomposition. Mark Sprevak (2009), on his discussion about the relation between Extended Mind and Functionalism, extends this indeterminacy to the latter:

All varieties of functionalism contain a parameter that controls how finely or coarsely functional roles should be specified (how much should be abstracted and ignored). If this parameter is set too fine, then one is committed to Martians who differ from us in minor ways not having mental states. If the parameter is set too coarse, then functional role specifications are too easy to satisfy, and systems that are intuitively non-mental wrongly count as mental. (2009, 511)

As a consequence, this indeterminacy even seems to beg the very question concerning the adoption of a clear and precise criterion for deciding the grain of psychological ascriptions. This is clear since what evaluates the criterion for attribution of mental states is, in itself a (intuitive) judgement about good or bad mental attributions! In order to contrast folk psychology and mechanistic scientific psychology on this particular issue take the following illustrative example of how the latter deals with the attribution of mental states in a precise and detailed way.

Should we attribute the mental state 'Fear' to the fruit fly? It is known that researchers tested the conditioning of avoidance behavior in the fruit fly (Tully, 1991). But there is a difference between the target behavior B and the explanation $\langle f, M \rangle$ of that behavior. The mental state denoted by *f* is supposed to *explain* the target behavior and not to be *identified* with it. For instance, taking B as 'The Acquisition of Avoidance Behavior', *f* could state that M is a neural structure such that M causes the acquisition of avoidance behavior, M is domain specific concerning avoidance behavior acquisition (i.e., its physical manipulation would *only* affect avoidance

behavior acquisition – at least in the intended, and therefore, *relevant* experimental protocols) M is input multimodal (there is one structure for all kinds of avoidance acquisition, independently of the sensory modality involved on each particular case), M is a memory/learning structure, etc. These set of constraints states the functional profile of the psychological concept ‘Fear Conditioning’ that explains ‘The Acquisition of Avoidance Behavior’. Assuming the principle according to which ‘mental states cause/explain behaviors’ we can, more or less safely, establish that *f* states the mental/psychological concept that supposedly causally explains B. Assuming the functional profile of ‘Fear Conditioning’, a good candidate for a neural mechanistic structure M satisfying that profile is the Central Amygdala, since a causal connection between this structure and ‘The Acquisition of Avoidance Behavior’ has been experimentally established, it also satisfies the other constraints (eg: it is multimodal and domain specific) and functional and structural homologues have been identified through taxa. Although the Fruit Fly satisfies the general behavioral target B, it cannot be said to possess the mental state Fear since it fails to satisfy *f* and (consequently) M. No neural structure in the Fruit Fly is domain specific concerning avoidance behavior acquisition or input multimodal (let alone some - if even far - amygdaloidal homologous structure). The Fruit Fly fails to satisfy the projectability constraint, **since its neural structures responsible for the avoidance behavior are ‘Fruit Fly specific’.** **Therefore, the Fruit Fly *does not possess* the mental state ‘Fear’ (conditioning).**

The three reasons just stated, not only enable a mechanistic scientific psychology as the most suitable candidate for fulfil the place of ‘Psychological Explanation *E*’ as stated in CP, but also dismisses Folk Psychology as a strong inadequate alternative to assume the same role. With the choice made for scientific psychology to play the part of psychological explanation it remains to show what are the consequences for the soundness of the Coupling Argument *via* ERCA.

4.2- *Scientific Psychology, Causal Explanation and the case for Intracranialism*

So, how does a scientific explanation proceed? As noted before, in current scientific mechanistic explanations a phenomenon *f* to be explained has to be decomposed into *simpler* sub-functions *f’, f’’* and so on ‘downwards’ the mechanism’s functions and components. Rephrasing Otto’s and Inga’s situations within this framework, one would have a first behavioral *explanandum* B (their action of going to

MOMA after hearing about an exhibition) and, as a first step in the explanation a decomposition of B into simple behaviors B', B'', \dots, B_n' . Each simple behavioral 'unit' would consist in a schematic abstract formulation of a behavior B which, in turn, has a corresponding Psychological State f and an explanatory neural mechanism M as stated in psychological generalizations $G1, G2, \dots, Gn$. As an illustration, let's suppose that one of the 'simple' decomposed behaviors B' of the complete Otto's stream of actions corresponds to: 'Otto decides to go to see the exhibition', this particular behavioral *token* (note the contextual markers 'Otto' and 'exhibition') is rephrased into the general/abstractly *type* stated behavior B : 'voluntary-intentional instrumental behavior by agent $x - f$: *Decision Making*' which, in turn, and given, say, the general explanation Gk , corresponds to the neural mechanism/structure M : (simplified) 'prefrontal and parietal cortex -limbic system loop'. This abstract and schematic mechanistic *schema* (type) could, in turn, be '*filled in*' by specific information concerning Otto and his particular decision of going to the exhibition (token). But the onus of the explanation lies on the generalized psychological statements $G1, G2, \dots, Gn$ (the case is not very different from an application of a law of physics to a specific physical system where, for instance, some atmospheric parameters can be added and calculated).

At the end of this scientific-minded decomposition process, we *would not* have isomorphic decompositions between Inga's and Otto's behaviors as suggested by the coarse-grained folk psychological explanation favored by C&C. But the fair question is '*why not*'? How fine-grained is such scientific decomposition in order to grant us the non-isomorphism between Inga's and Otto's behaviors and the *non-subsumption* of the notebook's use by a psychological predicate? Simply as it is, this mechanistic strategy only tells us that we *have* to decompose a behavioral phenomenon but it is mute concerning *how* we should do it. Another way to state the same concern consists in querying for the distinction between complex and simple behavior phenomena.

Although there seems not to exist an effective procedure telling us if, say the action 'Opening a Can' is susceptible of further behavior decomposition or if it counts as a behavioral 'simple', we should not forget the particular context here at issue. We are dealing with a particular question which has to do with the role of external/coupled items in the cognitive process, like in the case of Otto's notebook. Specifically, we are concerned with the soundness or not of the Coupling Argument given ERCA. What we want to know is if CP is vindicated within a scientific psychology context.

Put simply, what is the difference between the folk and scientific approaches that makes the first consider the use of Otto's notebook subsumed by a psychological predicate but not (lets assume) the latter? The straightforward answer is: whereas the psychological approach endorsed by C&C is a *Teleological* one, the mechanistic scientific alternative is *Causal*. This makes all the difference if one considers that Teleological explanations answer *Why* questions whereas Causal explanations answer *How* questions. C&C are explicit concerning the teleological nature of the explanations they promote in order to vindicate the Coupling Argument: «Otto walked to 53rd Street *because* he wanted to go to the museum and he believed the museum was on 53rd Street.» (1998, 13, emphasis added). An intentional teleological explanation is concerned with the ends more than with the means to achieve them. Therefore one can count the end result of a multiplication independently of the mean used to achieve it (a mental calculation, by using pen and paper or by using a pocket calculator). By contrast, a causal explanation turns 'means' into 'ends'; one what to know *how* one uses a notebook, pen and paper or a pocket calculator. For this reason, from a mechanistic-scientific-causal psychology, whenever process X, if it is coupled to a cognitive process Y, X will be a psychological *explanandum* and, therefore, suitable for a further and fine-grained behavioral decomposition.

The obvious moral of the last remarks is that, by adopting the type and grain of scientific psychology, CP is not satisfied (*no* coupled process is subsumed by a psychological predicate). It follows, then that by ERCA, the Coupling Argument is unsound. Lastly, it should be noticed that not only isn't Transcranialism vindicated through the Coupling Argument; also given the neural-mechanistic nature of scientific psychological explanations *and* the fact that every behavioral function *f* committed with the use of external processes should be decomposed into simpler sub-function such not committed, it turns that *all explanations* are restricted to the brain. ERCA, within the context of a scientific psychology, not only denies transcranialism but makes the strong case for intracranialism.

5- Conclusion

According to A&A the Coupling Argument is probably the most pervasive argument favoring a transcranialist conception of Mind. The proper formulation of the argument (as such) depends on its rephrasing within a psychological explanatory context (ERCA). If one insists on adopting a coarse-grained psychological explanatory framework suited to the objective at stake, the Coupling Argument surely follows in a self-fulfilling way and transcranialism is vindicated. But if one seeks for a psychological explanation firmly framed to the actual rigorous scientific practice, with precise methodological and non-self-vindicated principles of application, with a degree of preciseness capable of claiming the uncover of natural kinds, then, the end result tends to be very different from the optimistic transcranialist version. Being causal *par excellence*, a mechanistic-scientific minded psychology will never assume the coarse grained level of explanation needed in order to embrace the use of external items as a constitutive role of the cognitive process itself. On the other hand, the adoption of a neural-mechanistic scientific psychology makes the case for intracranialism, since all the admittedly correct psychological ascriptions are neural-mechanistic ones. From the point of view of our best current science methods, the Mind definitely equals to Brain and nothing else.