

RICERCHE

## The distinction between conscious and unconscious cognition in David R. Shanks's work: A critical assessment

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**Abstract** The notion of unconscious finds support in many experimental studies that use the dissociation method. This method allows us to distinguish between conscious and unconscious mental states when participants cannot explain why they performed as they did in an experiment. The paper will discuss the notion of unconscious by considering David R. Shanks' criticisms of the application of the dissociation method: it will assess three studies Shanks proposes as reexaminations of three other relevant studies in the literature and show how Shanks' work provides an examination of the methodological pitfalls of such studies. The paper will argue that, although Shanks's results are relevant regarding theories about the structure of cognition, his theoretical positions are at best confused and at worst diminish the importance of his research outcomes. It will conclude by showing why Shanks's results that legitimize the role of consciousness in cognition can be problematic for the physicalistic or materialistic framework endorsed by cognitive psychologists.

**KEYWORDS:** Dissociation Method; Dual-process Theories; Cognitive Psychology; Consciousness; Unconscious

**Riassunto** *La distinzione tra cognizione conscia e inconscia nell'opera di David R. Shanks: una valutazione critica* – La nozione di inconscio trova supporto in vari studi sperimentali che utilizzano il metodo della dissociazione. Secondo la letteratura cognitivista questo metodo permette di distinguere tra stati mentali consci e inconsci quando i partecipanti non sono in grado di riportare verbalmente il perché si sono comportati in un certo modo durante l'esecuzione di un compito sperimentale. Il presente articolo discute la nozione di inconscio in relazione alle critiche di David R. Shanks all'applicazione del metodo della dissociazione. Più precisamente, esso valuta tre studi che Shanks propone come repliche e rivalutazioni di altrettante ricerche a favore della cognizione inconscia e mostra l'autore riesca con successo a metterne in luce le carenze metodologiche. Inoltre, sostiene anche che, sebbene i risultati sperimentali di Shanks siano importanti per le teorie sulla struttura della cognizione, le sue posizioni teoriche sono quantomeno confuse e rischiano di ridimensionare la portata delle sue conclusioni empiriche e metodologiche. L'articolo conclude mostrando le ragioni per cui i risultati di Shanks, che legittimano il ruolo della coscienza nella cognizione, siano controversi in quanto adottano una posizione filosofica fisicalistica o materialistica comune alla maggioranza degli psicologi cognitivi.

**PAROLE CHIAVE:** Metodo della dissociazione; Teorie del doppio processo; Psicologia cognitiva; Coscienza; Inconscio

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IT IS ALMOST TRIVIAL TO argue that today experimental psychology strongly relies on the dissociation method. In fact, one of the main tasks of experimental psychology, especially that pertaining to the cognitivist tradition, is to individuate and differentiate the processes of the mind. Thus, the dissociation method is crucial for this task.<sup>1</sup> It is unsurprising that this method is used also in that research subfield that aims to distinguish between conscious and unconscious cognition. It can even be argued that «contemporary studies in unconscious cognition are essentially founded on dissociation, i.e., on how it dissociates with respect to conscious mental processes and representations».<sup>2</sup> In spite of the various types of dissociation paradigms, the application of basic dissociation logic can be simply sketched in this way: participants must perform a cognitive task of some sort and, during and/or after, researchers take and compare two kinds of measures:

- (A) a behavioral index of performance, for example, the score participants obtain in the completion of the task
- (B) an awareness assessment based on the participants' reports about the structure and the comprehension of the task.

The same information is assessed according to two different measures: (A) (a measure of behavioral sensitivity) and (B) (a measure of consciousness sensitivity).<sup>3</sup> On the basis of the outcomes of these two measurements, researchers make these inferences:

- 1) a certain mental state or process is unconscious when participants have good results at (A) and poor results at (B). This means that a dissociation between (A) and (B) is detected
- 2) a certain mental state or process is conscious when participants have good results at both (A) and (B). This means that a dissociation between (A) and (B) is not detected.

Reber provides an application of the dissociation paradigm. In two experiments participants are asked to watch a certain number of strings of letters generated according to the rules of an artificial grammar and to learn to discriminate the grammatical strings (the strings formed according to the rules of the artificial grammar) from the non-grammatical (the strings formed randomly, without following any grammatical rules).<sup>4</sup> At the end of the experiment, participants are informed that the strings of letters were formed according to a rigorous set of rules and asked whether they have any idea what these rules are. Because participants answer "no", they are asked four hint questions that they are not able to answer satisfactorily.<sup>5</sup> Reber concludes that the «[...] peculiar com-

ination of highly efficient behavior with complex stimuli and almost complete lack of verbalizable knowledge about them [...]» empirically proves that learning occurs at an unconscious level. Good results in behavioral performance (the continuous decline in the number of errors throughout the task) and poor results at verbally reporting the rules according to which the strings were formed, indicate a dissociation between conscious and unconscious learning, with the latter being predominant over the former.

Much literature in experimental psychology suggests two strongly intertwined points, one methodological and the other empirical, respectively:

- (a) the dissociation method is valid<sup>6</sup> and reliable<sup>7</sup> because it allows us to deconstruct the mind in its components
- (b) the empirical results suggest that the unconscious mind (and not the conscious one) is the main determinant of behavior.

So far, so good? The debate about the dissociation method (a) and the conscious-unconscious dichotomy (b) is highly controversial. It touches methodological, empirical, and philosophical issues, often in a confused manner. An example of this confusion in the literature can be found in the debate around the dual-process theories of cognition, in which the many dichotomies proposed<sup>8</sup> muddle the comprehension of the phenomenon.<sup>9</sup> An assessment of the work of a single author can promise at least the possibility of providing some clarification of the problems at play by circumscribing the debate. Because of its extent and relevance, the work of David R. Shanks is worth such an assessment. His work can be deemed relevant because it replicates and critically reviews other studies, as well as runs novel experiments and proposes new theoretical perspectives.

The present paper will evaluate the main methodological and empirical issues in Shanks' papers and their theoretical implications. It will do so by considering how Shanks and his colleagues criticize three papers that are distinct in content and methods but similar in their support of the conscious-unconscious dichotomy. Based on such an evaluation, the paper will discuss how the debate around the validity and reliability of the dissociation method is not only controversial but also misconceived and unproductive. The paper will conclude by suggesting that a way to enhance this debate could be to consider the dissociation method in the context of a wider philosophical distinction, that between causes and reasons for action. More precisely, it will argue that, in the dissociation paradigm, the measurement of the unconscious mind refers to the so-called Galilean tradition, whereas the measurement of the conscious

mind to the so-called Aristotelian tradition. «The Galilean tradition in science runs in parallel with the advance of the causal-mechanistic point of view in man's efforts to explain and predict phenomena, the Aristotelian tradition with his efforts to make facts teleologically or finalistically understandable».<sup>10</sup>

## 1 Shanks on the conscious-unconscious dichotomy

Perhaps Shanks' most discussed paper is the one he co-wrote with St. John and published in 1994. Here, the issue of dissociation is crucial because the starting question is «[...] whether there is more than one basic learning mechanism».<sup>11</sup> This question recurs in his work and is formulated in various ways. This paper reviews much literature in the field of implicit learning, and notes that almost all studies of unconscious or implicit learning appeal to a version of the dissociation method. Thus, different indexes of performance are used in order «[...] to find circumstances in which exposure to a set of stimuli leads to detectable learning unaccompanied by any reliable degree of awareness».<sup>12</sup> The main point here is that the presence or absence of awareness is what allows us to dissociate between different learning systems. This is controversial for a series of reasons that can be roughly summarized in the following question: how can we measure consciousness?<sup>13</sup> This is another recurring theme in Shanks' work. In order to illustrate this, the present paper will consider how Shanks deals with it in the critical examination of three papers.

### 1.1 Reber (1967) vs. Tunney and Shanks (2003)

Reber<sup>14</sup> proposes to measure consciousness in the most common way, that is, by asking participants to verbally report whether or not they are conscious of something. At the end of the experiment, participants are asked the four following hint questions:

- (a) Which letter or letters may sentences begin or end with?
- (b) Can sentences begin with a P?, an S?, a T?, a V?, an X?
- (c) Can sentences end with a P?, an S?, a T?, a V?, an X?
- (d) Were there any recurring themes or sequences of letters which seemed to reflect any rules?

Reber reports that:

- no participant was able to respond to (a) correctly
- all participants were able to correctly respond to (b) and (c) only after some suggestions

- only one of the five participants was able to correctly respond to (d).

Tunney and Shanks challenge Reber results. They argue that Reber used a subjective measure of awareness known as verbal introspection that can be easily affected by the response bias. This bias occurs when participants systematically report being unaware when they actually have a certain degree of awareness of the knowledge they are using. In this sense, participants may set their own response criterion at a conservative level and thus report only knowledge held with high confidence. Thus, they may choose to not report their knowledge with low confidence when it is actually conscious and correct.<sup>15</sup> In order to avoid the response bias, in their partial replication study, Tunney and Shanks propose an alternative subjective measure of awareness based on asking participants to verbally report their mental states by means of confidence ratings. In this case, the dissociation between conscious and unconscious cognition is assessed according to the "meta knowledge criterion", that is, when participants are clearly acquiring the knowledge of the information in the task without being aware of doing so.<sup>16</sup> In literature, there are two versions of this criterion. The former is the "guessing criterion", according to which participants show an above-chance behavioral performance while claiming that they are merely guessing. The latter is the "zero-correlation criterion", according to which participants display a mismatch between accuracy in the performance of the task and confidence in their own performance. The guessing criterion assesses how participants perform in the absence of reported awareness, whereas the zero-correlation criterion assesses the relationship between performance and awareness scores.<sup>17</sup>

Tunney and Shanks<sup>18</sup> conclude that their participants showed «[...] higher confidence in correct than in incorrect decisions indicating that the artificial grammar learning was explicit». More precisely, participants' confidence ratings facilitate the prediction of the accuracy in their responses. Discriminating between correct and incorrect responses unconsciously appears untenable because participants do not show any difference in the reaction times in responding correctly or incorrectly.<sup>19</sup> That is, only if participants were able to give the correct responses more quickly than the incorrect ones, would it be plausible to appeal to unconscious determinants in the responses, because one of the features of the unconscious system is that it provides quicker responses than the conscious one. Thus, because this did not occur in Tunney and Shanks' experiments and their participants were fully aware of their confidence ratings, it is plausible to argue that participants' responses depend on their consciousness.

### 1.2 Hayes and Broadbent vs. Green and Shanks

Hayes and Broadbent<sup>20</sup> propose a distinction between two types of learning that differ in terms of selectivity, S-mode learning and U-mode learning. The former occurs through a system known as “abstract working memory”, that is selective, effortful, and available for problem-solving in general. Knowledge derived from S-mode learning can be reported verbally; knowledge derived from U-mode cannot, because it takes place through a system involving unselective and passive aggregation of information about the occurrence of environmental events and features. Hayes and Broadbent claim that the difference between the two modes «[...] is an “architectural one”; each reflects the operation of different processes within the cognitive system».<sup>21</sup> In order to demonstrate this dissociation, Hayes and Broadbent ask participants to perform two different tasks intended to induce two modes of learning respectively.<sup>22</sup> The tasks have the same structure and aims: participants are asked to interact with a virtual person by typing words that describe a behavior. The two take turns: after the participant has entered a word, a new word describing a behavior of the virtual person appears on the computer screen. The relationship between the participant’s behavior and the virtual person depends on its peculiar “personality” (that is, on how the computer interacts). The participant aims to make the virtual person behave “FRIENDLY” (level 8) in a range between “VERY RUDE” (level 1) and “LOVING” (level 12) throughout the task.

The virtual person “ELLIS” is able to elicit the participant’s S-mode, whereas the virtual person “DENHAM” the participant’s U-mode. This is determined by the two different equations governing the virtual people

- ELLIS  $O = I - 2 + (a)$
- DENHAM  $O = I_1 - 2 + (a)$

where  $I$  is the participant’s most recent input,  $O$  is the output of the virtual person that follows  $I$ , and  $(a)$  randomly takes on a value between  $+1$  and  $-1$ . The crucial difference between the two equations is in the immediacy of the virtual person’s response to the participant’s input. More precisely, ELLIS responds to the participant’s immediately preceding input, thus with no lag time, whereas DENHAM responds to the participant’s next-to-last input, thus with a lag of time. After a while, without informing the participants, Hayes and Broadbent introduce an unexpected change so that  $+2$  replaces  $-2$  in both equations.<sup>23</sup> This implies that both ELLIS and DENHAM respond to the participant’s input with two values higher (that means more friendly) than the input. The results show that the no-lag group (ELLIS) performs better than the lag group (DENHAM) and is also able to pro-

vide more valid and reliable verbal reports of the strategies used to accomplish the task. The three experiments demonstrate that:

- ELLIS and DENHAM are able to elicit different modes of learning, respectively the S-mode and U-mode
- there is an “architectural dissociation” in cognition between S-mode learning and U-mode learning.

This is an example of how the conscious system works better than the unconscious one under certain conditions, while the unconscious system works better than the conscious one under other conditions. Green and Shanks<sup>24</sup> offer two replication experiments and a novel experiment in which participants perform with ELLIS and DENHAM without the equation change.<sup>25</sup> In particular, in their novel experiment, Green and Shanks show that, if participants perform 100 trials with ELLIS and DENHAM without the equation change, performance with DENHAM does not reach the same levels of performance with ELLIS.<sup>26</sup> That is, the performances does not appear to be due to the elicitation of the U-mode in place of the S-mode. On the basis of these results and of the missed replication of Hayes and Broadbent’s findings, Green and Shanks conclude that the two tasks are unable to induce two different types of learning: they differ only according to the degree of difficulty.<sup>27</sup>

### 1.3 Persaud vs. Konstantinidis and Shanks

Persaud and colleagues<sup>28</sup> start with their skepticism about the reliability and validity of subjective measures of awareness such as verbal reports and confidence ratings. About the former, they argue that participants can deny being aware if the questions asked are unrelated to the method they think they used to make a choice; about the latter, they point out that participants might underrate their confidence in their conscious knowledge. For these reasons, they introduce a novel objective measure called “post-decision wagering”: participants are asked to make a wager after making a choice. They test participants with the *Iowa Gambling Task* (IGT).<sup>29</sup>

Participants must choose cards from four decks. The top card of the chosen deck is turned over, displaying how much money participants have won or lost. Each deck produces large and small gains and losses but, in the long run, two decks have a positive yield and two a negative yield. In order to succeed, participants must choose more cards from the decks having a positive yield. In the original study using the IGT,<sup>30</sup> participants without prefrontal cortex damage are able to select more cards from the decks having a positive yield before consciously knowing how the task is structured. Because participants are not

informed of the structure of the task but must puzzle it out along the way, the knowledge they acquire is largely implicit.<sup>31</sup>

This claim is matter of controversy because Bechara and colleagues assess the participants' degree of conscious knowledge through two open-ended and vague questions: (1) "Tell me all you know about what is going on in this game" and (2) "Tell me how you feel about this game",<sup>32</sup> which appear unable to provide a valid and reliable evaluation of participants' conscious knowledge.<sup>33</sup> In order to sidestep the uncertainties of verbal reports, Persaud and colleagues<sup>34</sup> propose to their participants a version of the IGT in which they ask them to wager an imaginary £10 or £20 after each time they select a deck. Immediately after a card is chosen, a win or a loss is revealed. Participants are asked to make 100 selections from the four decks, to wager, to see the result of their choices, and then to add to or subtract from an imaginary initial £400. Persaud and colleagues<sup>35</sup> report that positive deck selections begin around trial 40, more or less, and advantageous wagering around trial 70, more or less. The difference between deck selections and advantageous wagering is statistically significant. This displays a dissociation between performance and awareness and thus that learning occurred implicitly in the early stages of the task.

Konstantinidis and Shanks<sup>36</sup> provide both replication studies and variations of the IGT study Persaud et al. propose but fail to replicate their results. Konstantinidis and Shanks<sup>37</sup> show that «[...] wagering did not lag behind the selection of good decks, with both measures (performance and awareness) becoming reliably better than chance very early in the task». The main issue about Persaud and colleagues is whether wagering actually assesses awareness and thus whether it is reliable. This is because most IGT studies in the literature show that the majority of participants are able to provide higher numerical estimates for the good rather than for the bad decks in the early phases of the task.<sup>38</sup>

A possible explanation of this puzzle can be that participants display risk or loss aversion, leading them to make high wagers only later in the task, even when they actually have some degree of conscious knowledge. This hypothesis finds support in the results of Konstantinidis and Shanks Experiment 2,<sup>39</sup> in which participants are presented a version of the IGT with a payoff matrix construed in order to control the effects of risk or loss aversion. Participants are able to select the good decks and to make high wagers early in the task, eliminating any trace of dissociation between performance and awareness.

## 2 On Shanks' criteria for assessing dissociation

The main issue regarding how to detect a dissociation between consciousness and the uncon-

scious is that most researchers agree that no assessment can be "process-pure" in principle.<sup>40</sup> In other words, it is implausible to expect that the performance of a task or test would reflect only a single underlying mental process or state.<sup>41</sup> Every assessment can always be sensitive to both conscious and unconscious processes, at least to a certain extent. In general, this can occur at two levels, that of the tests used for measurement and that of the structure of the task. The papers of Reber and Persaud and colleagues are examples of research pertaining to the first level because both studies use two tests to assess consciousness and the unconscious in a single task. This means that both assume that a single task is able to elicit both conscious and unconscious processes or states that can be distinguished through a subjective and an objective measure in Reber and two objective measures in Persaud and colleagues.

It is worth noting that, at the theoretical level, both studies seem to endorse a skeptical outlook toward introspection as a method of research and thus toward subjective measures in general. More precisely, the papers of Reber and Persaud and colleagues explicitly express this idea,<sup>42</sup> which echoes Watson's position in his behaviorist manifesto.<sup>43</sup> Similarly to Watson, the papers of Reber and Persaud and colleagues express doubt about the subjective character of introspection and subjective measures in general and advocate the use of objective forms of measurement. But the question here is whether objective procedures of measurement are actually more valid and reliable than subjective ones. The answer is far from clear. Lovibond and Shanks focus on the role of consciousness in Pavlovian conditioning, perhaps the simplest form of learning that it is often assumed to work outside consciousness. Although this extends beyond the scope of the present paper, it is useful for dealing with an evident but underrated fact: both subjective and objective measures «[...] are subject to influence and constraint by a range of causal factors above and beyond the primary factor of theoretical interest». <sup>44</sup> For example, according to Lovibond and Shanks, habituation<sup>45</sup> and the influences of external stimuli outside the experimental setting can affect more conditioned responding (an objective measure) than self-reporting (a subjective measure). On the contrary, demand characteristics<sup>46</sup> can affect more self-reporting than conditioned responding. Rather, individual differences and floor and ceiling effects<sup>47</sup> can bias both self-reporting and conditioned responding. It follows that, from a methodological standpoint, it is doubtful that objective measures are more valid and reliable than subjective measures.

Unlike Reber and Persaud and colleagues, Hayes and Broadbent assess the dissociation not only through two different tests but also through

two different tasks, one able to elicit conscious processes and the other able to elicit unconscious ones. Notably, these tests are created in order to assess procedural knowledge (the “know-how” through which participants successfully control the virtual person) and declarative knowledge (the “know that” that participants are able to report about the task).<sup>48</sup> Although in Hayes and Broadbent both the procedural knowledge test and the declarative knowledge test are based on participants’ verbal reports, they are supposed to be so differently structured that they appear to be able to measure two distinct kinds of knowledge. This is an indication that, differently from Reber and Persaud and colleagues, Hayes and Broadbent do not rule out the validity and reliability of subjective/introspective measures and the possibility that consciousness can play a role in determining behavior. A further indication in this sense comes from those results in which, under certain experimental conditions, S-mode learning is more determinant than U-mode learning. However, in spite of the validity and reliability of procedural and declarative tests, the controversial issue in Hayes and Broadbent relies on the claim that ELLIS and DENHAM are able to elicit two different forms of learning.

It is worth noting that Hayes and Broadbent do not share with Reber and Persaud and colleagues skepticism about consciousness. As said above, they point out that in certain conditions consciousness (= S-mode learning) works better than the unconscious (= U-mode learning). The problem here rests on the experimental conditions allowing to elicit one of the two modes. This is a demanding aim because it requires operationalizing two similar tasks that are able to assess two different features of cognition.

The critical examination of Reber, Persaud and colleagues, Hayes and Broadbent, and other studies, allows Shanks and colleagues to reflect on the theoretical underpinnings of the methods researchers use to treat the conscious-unconscious distinction. The *summa* of this reflection can be found in two review articles<sup>49</sup> which propose a series of criteria for assessing the measures of awareness in the experimental setting. Shanks and St. John spell out two criteria:

- *Information Criterion*: before concluding that a participant’s behavior is determined by unconscious influences, it must be established that the information researchers seek through the assessment of consciousness is indeed the information determining the behavioral changes
- *Sensitivity Criterion*: researchers must be able to show that the assessment of consciousness is sensitive to all relevant conscious knowledge.

Newell and Shanks propose some slight modifications: *Information Criterion* becomes the *Relevance Criterion* and the *Sensitivity Criterion* is

specified in more detail by splitting it in three criteria (*Immediacy, Reliability, Sensitivity*):<sup>50</sup>

- *Relevance Criterion*: the assessment should be able to target only that specific information that determined behavioral changes
- *Immediacy Criterion*: the assessment should take place concurrently or at least soon after the behavior in order to avoid forgetting, interferences, and influences to behavior
- *Reliability Criterion*: the assessment should be unaffected by factors that do not influence the behavioral measure such as social desirability and demand characteristics
- *Sensitivity Criterion*: the assessment should be made under the best retrieval conditions.<sup>51</sup>

It is worth noting that, although both papers reach the same conclusions, they differ on a crucial point. In fact, Shanks and St. John<sup>52</sup> conclude by endorsing recognition and prediction tests (two objective measures) and emphasizing that «it is simply a fact of life that tests of verbal recall tend to be less sensitive to small amounts of knowledge than other behavioral measures». Meanwhile, Newell and Shanks<sup>53</sup> conclude «[...] that, when participants are given adequate opportunity to report the knowledge underlying their behavior, there is little, if any, explanatory role played by a phenomenologically inaccessible [...] process». This is a crucial shift from Shanks and St. John’s paper: it means that, because criteria can apply not only to objective but also to subjective measures, consciousness is not only empirically controllable, but it even plays a fundamental role in cognition.

A close examination of the papers of Reber, Persaud and colleagues, and Hayes and Broadbent show that, in different manners, none of these studies fully respects the criteria proposed by Shanks and colleagues. Reber respects the *Relevance* (or *Information*) *Criterion* because the questions proposed target the information responsible for behavioral changes. However, Reber fails to respect the *Sensitivity Criterion*. In fact, questions are not asked concurrently or soon after the behavioral performance and are not clearly formulated to permit retrieval. Questions appear to be excessively difficult for participants to comprehend. Furthermore, Reber does not fully report how his participants responded to questions. He only states that «not one S (subject) answered the first question correctly. By giving successive hints, all Ss were eventually prodded into answering the second and third correctly, and the only concrete response to the fourth was from one S who felt that the sequence VPS (which occurred in nine of his items and which he had seen a total of 48 times) was significant». <sup>54</sup> This suggests that Reber’s participants acquired at least a certain de-

gree of conscious knowledge during the task!

The difficulty with Persaud and colleagues relies on whether post-wagering can work for measuring consciousness. This procedure puts the onset of awareness very late in the task putting to doubt that it meets the *Relevance (or Information) Criterion*. Empirical evidence from most studies using the IGT puts such an onset earlier<sup>55</sup> than Persaud and colleagues' data suggest. Thus, it appears far from clear whether wagering actually measures consciousness or something else.<sup>56</sup> Further, it is not clear whether wagering is neutral regarding such factors as social desirability and demand characteristics. In fact, it is plausible that participants may be averse to risk and thus choose to place low wagers when they are conscious at least to a certain extent.

As discussed above, Hayes and Broadbent move away from Reber and Persaud and colleagues because they use both different tasks and tests to assess dissociation and are not skeptical about the role of consciousness in cognition. Green and Shanks criticisms do not focus on the validity or reliability of the tests themselves, but on the validity and reliability of the tasks proposed. These criticisms focus on the *Information (or Relevance) Criterion*. The strong similarity between DENHAM and ELLIS does not allow us to assess whether or not information or mental processes determining behavior are unconscious.

Shanks' criteria do not consist of a formal procedure for evaluating the various types of measures. They do not provide a checklist of items that a certain test or task must meet for being valid and reliable. Rather, they look more like useful rules of thumb in assessing the rationale at the basis of the studies under examination. In Shanks' work, these criteria are a crucial compass for evaluating the studies and for planning the replication experiments in a field strongly affected by the so-called replication crisis. Perhaps this is what makes Shanks' work difficult to impugn, at least at the methodological and empirical level. Now the question is: What are the main issues Shanks' work touches by applying these criteria? This is something that goes to the heart of contemporary cognitive psychology.

### 3 Verbal reports and the unconscious mind

Before answering the question at the end of the previous paragraph, it is important, at least for the sake of clarity, to briefly return to the three above-discussed papers. Reber and Persaud and colleagues cast doubt on the possibility of obtaining valid and reliable data from consciousness. They make an epistemological and methodological claim against it rather than an ontological claim, because they doubt consciousness can be measured but not that it exists. This is demonstrated by

the underestimation of verbal reporting as a method of inquiry in Reber and the choice of the post-wagering method by Persaud and colleagues.

By contrast, Hayes and Broadbent are not skeptical about consciousness and does not underestimate verbal reporting methods for dealing with it. It clearly points out that, under certain conditions, S-mode learning works much better than U-mode learning. Nonetheless, it interprets the evidence in favor of the U-mode learning as evidence running «[...] counter to a widely held model of mind. This model is that mental life revolves around a common database on which all processes act [...]». This is called the “common-view model”. It postulates that when people must act, they consciously consult an internal representation of the world, «[...] a database of knowledge common to all output processes [...]», and manipulate it in order to choose the best course of action to take.<sup>57</sup> From this common-view model, different types of action (verbal or behavioral) can take emanate. Hayes and Broadbent make neither an ontological nor an epistemological nor methodological claim against consciousness. Rather, what its findings put into question is whether our behavior can be conceived as mainly or fully under conscious control.

In summary, Shanks' assessment of the experimental literature through the lens of the above-discussed criteria leads to the following conclusion: «[...] evidence for the existence of robust unconscious influences on [...] behaviors [...] and many of the key research findings either demonstrate directly that behavior is under conscious control or can be plausibly explained without recourse to unconscious influence».<sup>58</sup> This conclusion has a twofold motivation:

- (1) Dissociation studies do not provide robust evidence for the presence of significant unconscious influences over behavior. Rather, they offer evidence showing that behavior is generally under conscious control. In Newell and Shanks' words, «[...] there is little convincing evidence of unconscious influences on [...] (our behavior) in the areas we review, and that, as a consequence, such influences should not be assigned a primary role in theories of [...] (behavior). This conclusion is consistent with the view that conscious thoughts are by far the primary driver of behavior [...]».<sup>59</sup>
- (2) When the measures used for assessing the degree of awareness are valid and reliable, the chance of finding a dissociation between consciousness and the unconscious is near to zero. This means that a plausible model of the mind cannot postulate many independent and functionally distinct systems but rather a single unitary system.<sup>60</sup> What allows this model to be unitary is the presence of consciousness, a fea-

ture that Shanks assumes to be present even in the most basic mental phenomena, such as Pavlovian conditioning.<sup>61</sup>

Both (1) and (2) run counter to most empirical results of contemporary cognitive psychology. Shanks argues that his results and methodological considerations support the claim that the notion of unconscious is ill-conceived, if not plainly wrong. This is a serious and controversial claim. But let's consider this point in a weaker form: if Shanks' work is valid and reliable at the empirical and methodological levels, this means that, at the very least, assuming that the determinants of behavior are mainly unconscious is not proven beyond all reasonable doubt. This is problematic for contemporary cognitive psychology, which has at its core the notion of the unconscious, although this is not always made explicit.<sup>62</sup> An example of this can be found in Stich's influential proposal:<sup>63</sup> cognitive states can be postulated only in terms of syntactic properties that are inter-related and determine our behavior. Because of their computational nature, cognitive states cannot be accessed by a person's consciousness and be under his conscious control. Furthermore, because computations are postulated to work at the syntactic level, leaving aside that the contents and verbal reports are mainly about the person's contents, verbal reporting is an invalid and unreliable method for dealing with the mental. If we assume that our mind is largely unconscious and thus based on syntactic properties or computations, its contents gathered through verbal reporting are useless: what can count as evidence is only objective or third-person facts such as neural activations or observable behaviors.<sup>64</sup>

The notion of the unconscious in theorizing allows psychologists to create models of mind in terms of the natural sciences. Indeed, this notion of the unconscious fits perfectly in the causal network put forth by the natural sciences, whereas consciousness does not. Consider Kihlstrom provocative statement: «one of the dirty secrets of cognitive psychology is that many who practice it can get along perfectly well without displaying any interest in consciousness at all».<sup>65</sup> As shown above, cognitive psychologists endorse dual-process models, swiftly dividing the mind between a conscious and an unconscious level of functioning. Cognitive psychologists support both design-stance (explanations referring to causes operating at an unconscious level directly inaccessible to the person) and intentional-stance analyses (explanations involving intentions having a role at the conscious level and directly accessible to the person).<sup>66</sup>

There is more than a grain of truth in Kihlstrom statement. Shanks' work clearly shows that cognitive psychologists tend to be suspicious toward both consciousness and its measurement

and prefer design-stance over intentional-stance analyses. The reason for it is simple: contrarily to the (cognitive) unconscious, consciousness and all the notions related to it do not fit perfectly well in the causal network put forth by the natural sciences. Thus, to postulate that «[...] awareness and reportability are intrinsic properties of many mental states»<sup>67</sup> is to allow notions that can be problematic within such a physicalistic or materialistic image of science.

The paper will develop these issues in the concluding paragraph. In the next one, it will discuss which theoretical considerations about consciousness Shanks draws from his empirical results.

#### 4 A confused and unproductive debate

And here comes the pain or, at least, the confusion at the theoretical and philosophical level. Reflecting on the theoretical implications of their results, Newell and Shanks<sup>68</sup> claim to endorse «the folk-psychological view that conscious thoughts cause our decisions and behavior [...]». More precisely, they argue that the so-called common-view model<sup>69</sup> is plausible for all cognition. On this point, in their commentaries on Newell and Shanks' review article, Dijksterhuis and colleagues<sup>70</sup> and Evans<sup>71</sup> vehemently argue against this folk-psychological view. Both comments focus on two strictly connected points:

- (1) In order to be scientific, psychology cannot postulate consciousness in its explanations;
- (2) Folk psychology appeals to pseudo-scientific notions, incorporating supernatural and religious elements.

According to Dijksterhuis and colleagues and Evans, (1) and (2) are connected because postulating consciousness and its causal power is at the basis of all folk theories about the mind. Because most dissociation studies show that the real determinants of our behavior reside in the unconscious mind, we must discard all folk notions as explanatory useless, if not meaningless, pertaining to a domain that appears scientific but actually is not.<sup>72</sup> For example, it is not clear why all the authors involved in such a debate argue that one of the main assumptions of folk psychology is that our mind is mainly conscious, basically all open to introspection and able to determine our actions and behaviors. Although it seems plausible that these assumptions could coherently find place in folk psychology,<sup>73</sup> it is a matter of fact that, because of its low degree of systematization and its openness to social changes, folk psychology is contradictory – within it coexist various contrasting notions. For example, although in a rather vague and rough form, both Freud's idea of the unconscious to explain our parapraxes and slips of



tongue, and the appeal to brain lateralization to explain our skills, are all widespread and common in folk psychology. Thus, the discussion on the value of folk psychology around Newell and Shanks paper appears to be confusing and superfluous. The very issue at stake here is not the connection between folk and scientific psychology (at least not directly) but rather the plausibility of postulating consciousness in psychological explanations and, more in depth, whether consciousness is able to determine behavior.

As shown above, Shanks' work appears to be valid and reliable at the empirical level, more precisely stressing how the measures of consciousness in many dissociation studies are inefficacious and thus unable to provide adequate outcomes. From this point of view, it can be said that there are good reasons to put into question the claim that the unconscious mind is the main (the only?) determinant of our behavior. However, although it is clear that «awareness can be evaluated using careful methods»,<sup>74</sup> and that dissociations studies (at least those reviewed and replicated by Shanks and colleagues) appear to be questionable in this sense, some caution should be used before concluding that unconscious influences on our behavior are very limited: at least, it seems to be too early to argue that the empirical evidence in favor of the unconscious mind is flawed and ill-conceived. In this sense, meta-analyses and replication studies are urgent and necessary in this field. Apart from empirical considerations, it is worth noting that, at the moment, Shanks's empirical work does not appear to be accompanied by a convincing theoretical development.

In addition to their confused and flawed endorsement of folk psychology, in their brief reply to Evans and Dijksterhuis and colleagues, Newell and Shanks also assume that all mental states are at the same time brain states and consciously reportable (or introspectively accessible, to use and old-fashioned terminology),<sup>75</sup> that «[...] awareness and reportability are intrinsic properties of many mental states»,<sup>76</sup> and that consciousness should be viewed «[...] as a property of individuals». <sup>77</sup> Because these claims are incoherent and clumsy at best, they diminish the importance of Shanks's empirical results and methodological considerations. Thus, can such results and considerations really provide some hints about the theoretical development of scientific psychology?

## 5 Concluding remarks: The place of consciousness in cognitive psychology

It is time to take a step back and return to the dissociation paradigm. This paradigm remains at the center of a lively debate regarding its plausibility, in particular about the measures used to assess the extent of the conscious and unconscious influ-

ences on behavior.<sup>78</sup> Within such a debate, Shanks's empirical and methodological work has the merit to show that consciousness is not only measurable and assessable through rigorous methods but also able to determine our behavior, at least as well as our unconscious mind.

As discussed above, this is troublesome for cognitive psychology: dealing with consciousness involves controversial topics such as introspection, subjectivity and intentionality, which do not easily fit within the materialistic or physicalistic framework (more or less explicitly) endorsed by many (if not most) researchers in the field. That is, although cognitive psychologists appeal to intentional-stance analyses in their explanations, they often weave them together with design-stance analyses,<sup>79</sup> with the latter defining the true causes or determinants of a certain behavior and the former appearing to describe a mere epiphenomenon. The distinction between design-stance and intentional-stance analyses appears to reflect (at least partially) the consciousness-unconscious distinction and the kinds of measurements used in dissociation studies. As mentioned above, all these distinctions could be understood in light of a further important and more general distinction, that between the so-called Galilean tradition and the so-called Aristotelean tradition.<sup>80</sup>

Very roughly, these traditions refer to two kinds of sciences, natural and human sciences respectively, and provide us with an explanation of human behavior, but in a very different manner: the Galilean tradition is based on physical or material causes and searches for the general laws or mechanisms of mind and behavior; the Aristotelean tradition is based on intentions and reasons why agents act or intend to act.<sup>81</sup> As we can see, the issue of consciousness appears to be more tractable according to the Aristotelean tradition than according to the Galilean tradition. The issue at stake is that, in dissociation studies, these two traditions coexist and sometimes clash.

It is here suggested that one of the problems with the dissociation paradigm could be that the measures used to assess the conscious and unconscious influences implicitly refer to these two different and incompatible traditions. Shanks's work can be seen as an attempt to give coherence to measures of different sorts and thus to provide an adequate manner to assess the role of consciousness in the determination of behavior. Moreover, at the explanatory level, Shanks's results suggest that the intentional-stance analyses or the explanations following the Aristotelean paradigm can be informative and relevant for cognitive psychology. Of course, this is at odds with a physicalistic and mechanistic picture of the mind and the world for various reasons: to deal with agents' conscious reasons and intentions is to renounce the aim of providing general laws or mechanisms of human

mind and behavior.

That is, if Shanks's results were reliable (and, at the moment, it would be wise to wait and review other meta-analyses and replication studies), they would indicate that not all mental processes can be explained according to this physicalistic and mechanistic picture. So, which lesson should cognitive psychology learn from this?

## Notes

<sup>1</sup> G.R. VANDEN BOS, *APA dictionary of psychology*, p. 324.

<sup>2</sup> L.M. AUGUSTO, *Lost in dissociation*, p. 293. Cf. also P.A. HIGHAM, J.R. VOKEY, J.L. PRITCHARD, *Beyond dissociation logic*; M. SNODGRASS, E. BERNAT, H. SHEVRIN, *Unconscious perception*; L.M. AUGUSTO, *Transitions versus dissociations*.

<sup>3</sup> Cf. B. TIMMERMANS, A. CLEEREMANS, *How can we measure awareness?*, p. 23.

<sup>4</sup> Cf. A.S. REBER, *Implicit learning of artificial grammars*.

<sup>5</sup> *Ibid.*, p. 859.

<sup>6</sup> Cf. R.B. BURNS, C.B. DOBSON, *Experimental psychology*, MTP Press, Lancaster (UK) 1981, pp. 355-365.

<sup>7</sup> *Ibid.*, pp. 336, 332.

<sup>8</sup> Cf. J.S.B.T. EVANS, K.E. STANOVICH, *Dual-process theories of higher cognition*, p. 225, table 1.

<sup>9</sup> L.M. AUGUSTO, *Transitions versus dissociations*, pp. 274. It can even be said that this typical feature of cognitive psychology the foundational dichotomy of dual-process or dual-system theories, «according to which any cognitive phenomenon is either a system-1 (i.e., an unconscious, or type-1, process) or a system-2 phenomenon (i.e., a conscious or type-2 process), but not both [...]» comes directly from the dissociation logic.

<sup>10</sup> G.H. VON WRIGHT, *Explanation and understanding*, pp. 2-3.

<sup>11</sup> D.R. SHANKS, M.F. ST. JOHN, *Characteristics of dissociable human learning systems*, p. 367.

<sup>12</sup> *Ibid.*, p. 369.

<sup>13</sup> B. TIMMERMANS, A. CLEEREMANS, *How can we measure awareness?*, p. 21.

<sup>14</sup> A.S. REBER, *Implicit learning of artificial grammars*, p. 869.

<sup>15</sup> R.J. TUNNEY, D.R. SHANKS, *Subjective measures of awareness and implicit cognition*, p. 1061.

<sup>16</sup> L.M. AUGUSTO, *Lost in dissociation*, p. 295.

<sup>17</sup> Cf. R.J. TUNNEY, D.R. SHANKS, *Subjective measures of awareness and implicit cognition*, pp. 1061-1063.

<sup>18</sup> *Ibid.*, p. 1069.

<sup>19</sup> *Ibid.*, p. 1065.

<sup>20</sup> Cf. N.A. HAYES, D.E. BROADBENT, *Two modes of learning for interactive tasks*.

<sup>21</sup> *Ibid.*, p. 251.

<sup>22</sup> *Ibid.*, p. 253

<sup>23</sup> *Ibid.*, p. 255.

<sup>24</sup> Cf. R.E.A. GREEN, D.R. SHANKS, *On the existence of independent explicit and implicit learning systems*.

<sup>25</sup> *Ibid.*, p. 307.

<sup>26</sup> *Ibid.*, p. 313

<sup>27</sup> *Ibid.*, p. 314.

<sup>28</sup> Cf. N. PERSAUD, P. MCLEOD, A. COWEY, *Post-decision wagering objectively measures awareness*.

<sup>29</sup> *Ibid.*, pp. 259-260.

<sup>30</sup> Cf. A. BECHARA, H. DAMASIO, D. TRANEL, A.R. DAMASIO, *Deciding advantageously before knowing the*

*advantageous strategy*.

<sup>31</sup> D.B. DUNN, T. DALGLEISH, A.D. LAWRENCE, *The somatic marker hypothesis*, p. 244.

<sup>32</sup> A. BECHARA, H. DAMASIO, D. TRANEL, A.R. DAMASIO, *Deciding advantageously before knowing the advantageous strategy*, p. 1293.

<sup>33</sup> G. LO DICO, *Philosophical and empirical approaches to psychology*, pp. 101-102.

<sup>34</sup> N. PERSAUD, P. MCLEOD, A. COWEY, *Post-decision wagering objectively measures awareness*, p. 257.

<sup>35</sup> *Ibid.*, p. 259.

<sup>36</sup> Cf. E. KONSTANTINIDIS, D.R. SHANKS, *Awareness in decision-making*.

<sup>37</sup> *Ibid.*, p. 813.

<sup>38</sup> B.R. NEWELL, D.R. SHANKS, *Unconscious influences on decision making*, p. 12.

<sup>39</sup> E. KONSTANTINIDIS, D.R. SHANKS, *Awareness in decision-making*, pp. 813-815.

<sup>40</sup> B. TIMMERMANS, A. CLEEREMANS, *How can we measure awareness?*, p. 23.

<sup>41</sup> P.A. HIGHAM, J.R. VOKEY, J.L. PRITCHARD, *Beyond dissociation logic*, p. 459.

<sup>42</sup> A.S. REBER, *Implicit learning of artificial grammars*, p. 860: «[...] The introspective verbal reports are notoriously unreliably and usually incomplete». Cf. also N. PERSAUD, P. MCLEOD, A. COWEY, *Post-decision wagering objectively measures awareness*, p. 260: «Subjective measures force the participants to introspect about their awareness: they index not awareness, but awareness of awareness».

<sup>43</sup> J.B. WATSON, *Psychology as the behaviorist views it*, p. 158: «Introspection forms no essential part of its methods (of psychology's methods), nor is the scientific value of its data dependent upon the readiness with which they lend themselves to interpretation in terms of consciousness».

<sup>44</sup> P.F. LOVIBOND, D.R. SHANKS, *The role of awareness in Pavlovian conditioning*, p. 5.

<sup>45</sup> Cf. G.R. VANDENBOS, *APA dictionary of psychology*, p. 479.

<sup>46</sup> Cf. M.T. ORNE, *On the social psychology of the psychological experiment*, p. 779.

<sup>47</sup> Cf. G.R. VANDENBOS, *APA dictionary of psychology*, pp. 166, 427.

<sup>48</sup> Cf. N.A. HAYES, D.E. BROADBENT, *Two modes of learning for interactive tasks*, pp. 254-255, 257.

<sup>49</sup> Cf. D.R. SHANKS, M.F. ST. JOHN, *Characteristics of dissociable human learning systems*; B.R. NEWELL, D.R. SHANKS, *Unconscious influences on decision making: A critical review*.

<sup>50</sup> B.R. NEWELL, D.R. SHANKS, *Unconscious influences on decision making*, p. 5, table 1.

<sup>51</sup> P.A. HIGHAM, J.R. VOKEY, J.L. PRITCHARD, *Beyond dissociation logic*, p. 459. In order to meet this criterion, it is mandatory to demonstrate that a certain test or task is able to assess all conscious knowledge. This can be problematic because «[...] no matter how extensive the test of awareness, it is always possible that a further step could have been taken, which would have revealed the participants' true state of awareness».

<sup>52</sup> D.R. SHANKS, M.F. ST. JOHN, *Characteristics of dissociable human learning systems*, p. 394.

<sup>53</sup> B.R. NEWELL, D.R. SHANKS, *Unconscious influences on decision making*, p. 17.

<sup>54</sup> A.S. REBER, *Implicit learning of artificial grammars*, p. 859.

<sup>55</sup> D.B. DUNN, T. DALGLEISH, A.D. LAWRENCE, *The somatic marker hypothesis*, pp. 245-249.

<sup>56</sup> B.R. NEWELL, D.R. SHANKS, *Unconscious influences on decision making*, p. 12.

<sup>57</sup> D.E. BROADBENT, P. FITZGERALD, M.H.P. BROADBENT, *Implicit and explicit knowledge in the control of complex systems*, p. 33.

<sup>58</sup> B.R. NEWELL, D.R. SHANKS, *Unconscious influences on decision making*, p. 19.

<sup>59</sup> *Ibid.*, p. 2.

<sup>60</sup> D.R. SHANKS, *Attention and awareness in "implicit" sequence learning*, p. 29.

<sup>61</sup> P.F. LOVIBOND, D.R. SHANKS, *The role of awareness in Pavlovian conditioning*, p. 3.

<sup>62</sup> G. LO DICO, *Philosophical and empirical approaches to psychology*, pp. 51-55.

<sup>63</sup> S.P. STICH, *From folk psychology to cognitive science*, pp. 149-183.

<sup>64</sup> E.C. FESER, *Philosophy of mind. A beginner's guide*, p. 169.

<sup>65</sup> J.F. KIHLESTROM, *Conscious versus unconscious cognition*, p. 173.

<sup>66</sup> O.J. FLANAGAN, *The science of the mind*, p. 179. As it will be spelled out in the concluding paragraph, these different explanatory stances reflect the controversial philosophical distinction between causes and reasons for action: the old (yet never solved) philosophical issue of the distinction between causes and reason can play a central role in the debate about the validity and reliability of data coming from the dissociation method.

<sup>67</sup> B.R. NEWELL, D.R. SHANKS, *Unconscious influences on decision making: A critical review*, p. 47.

<sup>68</sup> *Ibid.*, p. 16

<sup>69</sup> Cf. D.E. BROADBENT, P. FITZGERALD, M.H.P. BROADBENT, *Implicit and explicit knowledge in the control of complex systems*; N.A. HAYES, D.E. BROADBENT, *Two modes of learning for interactive tasks*.

<sup>70</sup> A. DIJKSTERHUIS, A. VAN KNIPPENBERG, R.W. HOLLAND, H. VELING, *Newell and Shanks' approach to psychology is a dead end*, p. 25: «Unless one believes that consciousness freely hovers in the air, or is in direct contact with the gods, claiming that psychological processes start in consciousness without further ado does not make sense. [...] Stating it's all starting in consciousness may have been satisfactory to Descartes, but it doesn't work in the twenty-first century».

<sup>71</sup> J.S.B.T. EVANS, *The presumption of consciousness*, pp. 26-27: «[...] it borders in the mystical (or at least Cartesian dualism) to think of consciousness as some kind of "mind stuff" that has powers of causation. [...] I reject the authors' presumption of conscious decision making, which I believe to be shakily founded on folk psychology [...]».

<sup>72</sup> Something similar can be found in the last chapter of an influential book by Stich, in which he defends his syntactic theory of the mind by appealing to the results of dissociation studies that show the prevalence of the unconscious over the conscious mind in determining behavior. Stich is devoted to showing how folk notions cannot find a place in scientific psychology (cf. S.P. STICH, *From folk psychology to cognitive science*). However, unlike Dijksterhuis and colleagues (cf. A. DIJKSTERHUIS, A. VAN KNIPPENBERG, R.W. HOLLAND, H. VELING, *Newell and Shanks' approach to psychology is a dead end*) and Evans (cf. J.S.B.T. EVANS, *The presumption of consciousness*), Stich does not consider folk psychological notions pseudoscientific. Rather, he assumes that, because these notions are deeply rooted in con-

tent, they do not allow for empirical generalizations. Thereby, the inclusion of folk notions in a scientific psychology would bring a degree of vagueness that would hinder systematization. Thus, for Stich the use of these notions in a science of the mind is unwarranted. While these notions are not pseudoscientific *per se*, because of their vague content, they cannot play a part in scientific discourse and thus should be eliminated from it.

<sup>73</sup> Cf. G. LO DICO, *The relationship between introspection and folk psychology*.

<sup>74</sup> B.R. NEWELL, D.R. SHANKS, *The primacy of conscious decision making*, p. 53.

<sup>75</sup> *Ibid.*, p. 46.

<sup>76</sup> *Ibid.*, p. 47.

<sup>77</sup> *Ibid.*, p. 48.

<sup>78</sup> For some recent examples, cf. P.A. HIGHAM, J.R. VOKEY, J.L. PRITCHARD, *Beyond dissociation logic*, pp. 458-459; M. SNODGRASS, E. BERNAT, H. SHEVRIN, *Unconscious perception*; L.M. AUGUSTO, *Lost in dissociation*; L.M. AUGUSTO, *Transitions versus dissociations*, pp. 271-278.

<sup>79</sup> O.J. FLANAGAN, *The science of the mind*, p. 180.

<sup>80</sup> Cf. G.H. VON WRIGHT, *Explanation and understanding*.

<sup>81</sup> Cf. E.C. FESER, *Philosophy of mind*; K. LENNON, *Reasons and causes*, p. 531; G. LO DICO, *Reasons versus causes in Arocha's scientific realism*, pp. 442-443.

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