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Exploring Precision's Role in Psychosis: Insights From Psychedelics for a 'Quadripartite Model'

Aurora Alegiani*

Abstract: The paper examines the connection between the Global Neuronal Workspace Theory of Consciousness (GNW) and Predictive Processing (PP) in understanding psychotic symptoms. The GNW-based account (which I frame as 'tripartite') suggests that an elevated threshold for consciousness, influenced by top-down attention, mediates between neural dysconnectivity and psychosis. However, several interrelated concerns emerge. These include GNW's concentration on inferential processes, its emphasis on information access to consciousness rather than its utilization, and its focus on conscious access while disregarding two-factor models, which, by emphasizing the relevance of both perceptual and cognitive factors in delusions, argue that anomalous perceptual experiences play a significant role in the formation of delusional beliefs. Additionally, the issues of specificity and the persistence of psychotic traits are mentioned. To address these challenges, the paper proposes an incorporation of precision from PP into a four-part model (dysconnectivity, precision/attention, conscious threshold, and psychosis). This enhanced framework is evaluated through recent literature, with a particular focus on research concerning psychedelics and ego dissolution. A sketch of how precision might help address the mentioned issues is outlined. There is still more work to be done to unravel the intricacies of these concerns and to elucidate why impaired precision actually raises the threshold for conscious access.

Keywords: consciousness, precision, psychosis, predictive processing, Global Neuronal Workspace.

1. *Introduction*

This paper delves into the intersection of Global Neuronal Workspace Theory (GNW) and the Predictive Processing (PP) literature (see Section 2) to shed light on the mechanisms underlying psychotic symptoms in schizo-

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phrenia. The relevance of both these theories, as well as of their correlation (cf. Hohwy, 2013, Whyte and Smith, 2021, Alegiani and Marraffa, 2021), and of studies on schizophrenia and psychotic symptomatology has become increasingly pronounced in recent years, as they collectively offer valuable insights into the intricate processes that contribute to the manifestation of symptoms. This growing significance not only advances our comprehension of the condition but also holds promise in paving the way for innovative therapeutic interventions and more effective clinical approaches.

While GNW theory suggests that an elevated threshold for conscious access mediates between neural structural dysconnectivity and psychosis, attributing this elevation to top-down attentional factors, I find noteworthy concerns within this interpretation that call for an expansion of the framework with additional elements and concepts (Section 3). First, there's a concern with the idea that the underlying issue in schizophrenia leading to psychosis is inferential; this is in fact at odds with the dissociation established experimentally between unconscious and conscious processing. Second, there's a concern about the exclusive focus on the access of contents to consciousness within GNW theory; it seems that in psychosis the issues may lie more in how incoming information is utilized in consciousness, rather than in mere access. Third, many established accounts of delusions link them to anomalous perceptual experiences, seemingly inconsistent with GNW's emphasis on conscious access while preserving unconscious processes. These perceptual anomalies are believed to be crucial in both the specificity and persistence of delusions, raising questions about confining the issue to consciousness. Fourth, the claim that limited access to consciousness leads to an overestimation of sensory stimuli that enter consciousness is critiqued, as it fails to acknowledge that underestimation can also occur, as evidenced by studies like Young and Leafhead (1996). To address these challenges, in Section 4 I suggest incorporating the concept of precision from PP into the original tripartite model of dysconnectivity-elevated conscious threshold-psychosis. To support this suggestion, I delve into the role of precision as a critical factor influencing both psychosis and giving rise to experiences resembling *ego dissolution*, mirroring typical psychotic symptoms. The proposal is to extend this framework into a quadripartite model that encompasses dysconnectivity, precision/attention, elevated conscious threshold, and psychosis. This expanded model not only shows potential for resolving the previously mentioned limitations but also enables the convergence of two distinct lines of literature (Section 5). This integration enhances our comprehension of psychosis and facilitates the application of 'parametric variation' (cf. Aimola Davies and Davies, 2009), leading to a more nuanced understanding of psychotic symptomatology.

2. *The Two Theories*

2.1. *Predictive Processing (PP)*

Predictive Processing (PP) stands as a comprehensive theory of brain and cognitive function, asserting that our brains engage in inferential processes throughout our perception and cognition. In the realm of perception, our systems lack direct access to the external environment. Consequently, they must infer the causes behind the sensory stimuli they receive. These causes manifest as hypotheses, subject to evaluation by the brain to predict the cause of the stimulus in question.

Given the absence of a direct, one-to-one correlation between causes and stimuli, the brain navigates life through a continuous process of trial and error, learning from experience. This learning involves the storage of information about the causes of sensory stimuli, allowing the brain to ascribe high probabilities to specific hypotheses when similar stimuli reoccur.

Predictions might turn out to be wrong; or, more precisely, a discrepancy between the selected hypothesis and the actual sensory stimulus might ensue. Such an incongruity results in a prediction error value, that signals that something has failed. According to PP, the main objective of a system is to minimize prediction error signals. To do so, it can either change its models (*perceive*) or intervene in the environment (*act*)[1].

The system achieves this by constructing *hierarchical generative models*, which sketch the intricate relationships between causes and stimuli. These models possess a hierarchical structure, with interwoven relations that guide the system in predicting the causes of encountered stimuli. Such relations comprise a structure where predictions at each level are constrained by input from higher levels, representing broader contextual information, while the prediction errors from lower levels provide feedback about the accuracy of predictions. The system's goal is to construct a predictive architecture that closely tracks the causal-probabilistic relationships between hidden causes and their effects in the environment. This multilevel organization allows the system to capture and model different interactions at various spatial and temporal scales, enhancing its ability to predict the causes of stimuli encountered in the environment.

To illustrate this, consider the scenario of 'hearing' a continuous sound emanating from a neighboring apartment. The sensory stimulus is represented by 'x,' and determining its cause is the task at hand. Given the lack of a strict causal link, one might entertain various hypotheses, ranging from

[1] Later on, these will figure, respectively, as 'perceptual' and 'active' inference.

hallucinations to a live performance by Mozart. Fortunately, through prior learning and model-building, one can select the most probable hypothesis – perhaps identifying the sound as a melody played on a piano by a neighbor. This decision is informed by the stored knowledge that the neighbor is a pianist who regularly plays.

However, if this selected hypothesis turns out to be incorrect (for instance, the piece heard is not in the neighbor's repertoire), a prediction error signal emerges, prompting the need for error minimization. Multiple avenues are available for this purpose: modifying the model (for example, revising the belief that the neighbor always plays her piano), reevaluating the stimulus (e.g., considering characteristics that distinguish it from a live piano), or even contemplating significant changes (such as suspecting that the neighbor has moved or that a deception is in progress). Alternatively, one can take action, such as knocking on the neighbor's door, to verify the source of the sound. These operations are all attempts to minimize prediction errors.

In the context of PP, it is crucial to stress the significance of having a guiding compass for inferential operations. In the event of a prediction error, a system must first assess its reliability, including both its *precision* and its accuracy. Notably, on PP, "precision processing […] map[s] on to attention" (Hohwy, 2012: 6), in the sense that attention is precision optimization[2] (see e.g., Feldman and Friston, 2010). A system must be capable of evaluating the legitimacy of the prediction error, both, so to say, 'vertically' (focusing on the error itself using *first-order precision*) and 'horizontally' (considering the context and employing *second-order precision*). Is the prediction error variable? Can we trust the conditions under which it arises? For instance, if, in the previous scenario, your sibling was in another room playing loud drums, you might suspect that the error signal could be due to noisy environmental factors. Conversely, if you were wearing headphones and listening to something else, your inferential abilities might be compromised. In such cases, the error signal would likely be considered unreliable, and no intervention, neither on the stored models nor in the environment, would be necessary. In other words, no learning process would ensue.

2.2. *The Global Neuronal Workspace Theory of Consciousness (GNW)*

According to the Global Neuronal Workspace (GNW) model, which has evolved over decades of theoretical development, beginning with Baars' cog-

[2] This optimization involves, in the case of perceptual inference for example, giving more significance to the learning signal from the world for units anticipating precision, particularly in perceptual inference. Conversely, in the processing of units anticipating high imprecision, top-down expectations exert greater influence on perception.

nitive theory of a 'global workspace' in 1989, building upon the contributions of Shallice and Posner and culminating in Dehaene, Changeux and collaborators' immense work, consciousness fundamentally involves a stimulus gaining entry into a finite-capacity global neurocognitive workspace. This global workspace serves as a central stage where the stimulus, which would otherwise remain confined within specific, localized brain systems, can be effectively broadcast to a broad spectrum of executive, conceptual, and emotional processes. The underlying architecture of this workspace is established through long-range connections among various cortical regions, with a notable concentration in prefrontal, parieto-temporal, and cingulate associative cortices, as well as their thalamo-cortical loops.

Alongside characterizing a stimulus as conscious, GNW distinguishes between two alternative states in which a stimulus can engage with the workspace: it can exist as either subliminal or preconscious. This categorization is instrumental in comprehending the intricate dynamics involved in achieving conscious access. A stimulus is classified as subliminal when it triggers a very subtle level of activation, yet this activation tends to be weak and rapidly fades away (as exemplified by Dehaene et al. in 2006). On the other hand, a stimulus is considered preconscious when its activation is relatively robust and extends to specific brain regions, such as sensorimotor areas. Nevertheless, despite this heightened activation, such a stimulus fails to gain conscious access because attention is not directed towards it, preventing it from being globally ignited or effectively amplified. This brief analysis underscores the significance of attentional amplification within this framework: for a stimulus to surpass the threshold for conscious access, it necessitates undergoing attentional amplification.

3. *Psychosis and the Threshold for Conscious Access on GNW*

3.1. *The Issue with Top-Down Attention*

Psychosis is often defined by a common theme centered around difficulties in perceiving reality accurately or a breakdown in a person's sense of self. In essence, it comprises two primary features: hallucinations and delusions. Hallucinations involve experiencing perceptual stimuli when there is no actual external or physical trigger for them. Delusions, on the other hand, are persistent false beliefs. These beliefs are not grounded in reality and remain unshaken even when confronted with clear and undeniable evidence that contradicts them.

While I will explicitly reference 'schizophrenia' and psychosis in this paper, it is crucial to recognize the complexity of the topic, including the intricate distinctions pertaining to schizophrenia, even though I will not delve into them in this specific discussion. As Arciniegas (2015) pointed out, for instance, the DSM-5 itself has represented a departure from portraying schizophrenia as the primary psychotic disorder and instead situates it within a spectrum of psychotic disorders. Symptoms within this spectrum encompass hallucinations, delusions, disordered thinking, markedly disorganized or unusual physical behavior (including catatonia), and negative symptoms.

In the following discussion, I will delve into the works of Berkovitch and colleagues (2017; 2018; 2021; 2022). In contrast to an alternative perspective attributing the primary challenges of schizophrenia to bottom-up factors, Berkovitch and colleagues (2017) firmly adhere to the 'top-down interpretation.' According to this viewpoint, they argue that individuals with schizophrenia may indeed show impairments in explicit cognitive processing, explicit recollection, and explicit emotion classification. Additionally, they may experience delays in conscious perception, both in visual masking and inattentional blindness during experimental setups. They also maintain that these individuals exhibit intact unconscious elaboration (ivi: 879). For instance, subliminal priming in number processing remains unaffected, as does the unconscious elaboration in emotional and semantic priming[3].

The focus on top-down processing in schizophrenia is confirmed in experiments such as that presented in Berkovitch and colleagues (2018)[4]. There, the researchers conducted an experiment involving patients with schizophrenia and control subjects. Their goal was to explore how top-down attention affects the perception of visual stimuli. During the experiment, both groups were shown brief visual stimuli (specifically, numbers) followed by masks.

[3] These findings are further confirmed and corroborated by other research, such as Grandgenevre et al. (2015).

[4] It must be acknowledged that the experiments conducted by Berkovitch and colleagues commonly interpret the emergence of the P3 wave in ERP data as associated with conscious perception. This statement must be handled with care, for it is not without controversy. First off, a similar wave, which appears at a later stage, has been detected through MEG (van Aalderen-Smeets et al., 2006). Second, an earlier negative event (variably called N2, N3, or even "visual awareness negativity" [VAN]) peaking at ~260 ms and with a total duration of ~200 ms is also often observed when contrasting conscious to unconscious stimuli (Eklund and Wiens, 2018; Koivisto and Revonsuo, 2010; Pitts et al., 2012, 2014). VAN has been suggested as the earliest electrophysiological correlate of visual awareness (Koivisto and Grassini, 2016), and this claim has been corroborated by magnetoencephalography (MEG) (Andersen et al., 2016). It remains unclear whether P3 is correlated with awareness (Salti et al., 2012), post-perceptual processes (Andersen et al., 2016; Koivisto et al., 2016), or both. In many experiments, the N2 simply precedes the P3b (which figures as another way of referring to P3, cf. Pitts et al., 2012: 346), and their succession may index the spread of global ignition as reflected in intracranial and MEG signals. However, the two waves occasionally dissociate. Most importantly, only the N2 remains under conditions where the stimuli are task-irrelevant yet reported to be consciously perceived (Pitts et al., 2012, 2014).

The participants had two tasks: in one, they were instructed to concentrate on the numbers, while in the other, they needed to focus on the colors surrounding a fixation cross. Throughout these tasks, the researchers closely monitored the brain responses of the participants, specifically through ERPs (event-related potentials). The key findings of the study revealed that when participants concentrated on the numbers, there were observable changes in their ERPs. These changes were indicative of conscious perception. However, in the unattended condition, these changes diminished or disappeared. Of particular significance, in patients with schizophrenia, the research revealed a noteworthy observation: their ability to consciously perceive stimuli appeared to be impaired. This observation implied that conscious perception is a step-by-step process that heavily relies on the allocation of attention. The researchers concluded that *the challenge in schizophrenia is closely tied to difficulties in top-down attention* rather than any issues with early perceptual processing. In simpler terms, the 'basic' information processing functions remain intact in patients, but their capacity to focus attention is noticeably affected.

3.2. *Attention and Connectivity*

Impairments in conscious access, as posited by the Global Neuronal Workspace (GNW) framework, are suggested to be linked to disruptions in long-range connectivity essential for conscious perception. Both classic studies like Sergent et al. (2005) and Del Cul et al. (2007) and more recent ones like van Vugt et al. (2018) underscore the crucial role of this connectivity in conscious perception.

Berkovitch et al. (2018) sought to experimentally test this hypothesis. Using MRI-based tractography, the research aimed to explore the neural underpinnings of an elevated threshold for conscious access. Note, further, that these findings nicely dovetail with the subsequent sections of this paper, since the impaired long-range synchrony of gamma bands is counted as underlying binding (a key phenomenon in integrative processes) in sensory processing.

Previous research noted that individuals with schizophrenia exhibit not only impairments in conscious access but also discontinuities in cerebral structures. These structural changes include alterations in fractional anisotropy, indicating insufficiently myelinated fiber tracts, particularly in the prefrontal cortex and cingulum (Berkovitch et al., 2021: 514).

To investigate these neural structure alterations, the researchers employed diffusion tensor imaging (DTI). This method is based on the idea that the structure of neural matter, including axon structure and myelination status, can constrain water molecules. If water molecules diffuse freely, the structure is isotropic; if they exhibit directionality, the structure is anisotropic. Altered anisotropy in brain tissue may signify anomalies in neural structure, affecting neural connectivity.

Berkovitch and colleagues (2021) evaluated patients with schizophrenia, patients with bipolar disorder (with and without psychotic traits), and control subjects in terms of their consciousness threshold using a visual masking paradigm. Concurrently, they measured neural connectivity using DTI and generalized fractional anisotropy (gFA) on seven cortical bundles, including those implementing the global workspace. The study revealed a negative correlation between mean gFA of specific bundles (IFOF, CLF, and the corpus callosum) and the threshold for conscious access. In simpler terms, higher anisotropy was associated with an increased capacity for conscious perception. Thus, lower thresholds for conscious access were linked to higher anisotropy (Berkovitch, 2018: 81).

Consistent with earlier findings, the visual masking experiment used to gauge the consciousness threshold indicated that individuals with psychotic traits (both those with schizophrenia and bipolar disorder) had a notably higher threshold for conscious access in comparison to control subjects and bipolar patients without psychotic symptoms. Regarding anatomical connectivity, the study revealed a negative correlation between the mean gFA (a measure of anisotropy) of the IFOF (both right and left), CLF (both right and left), and the corpus callosum. In simple terms, this means that a higher degree of anisotropy is associated with an increased capacity for conscious perception. So, the lower the threshold (i.e., the better the ability to consciously perceive), the higher the anisotropy, which aligns with the previous indication about the coherence of water molecule directionality in DTI.

3.3. *The Tripartite Model: Dysconnectivity, Elevated Threshold, and Psychosis. Correlation and Mediation*

The observations presented above led the authors to make further interesting considerations. The experimenters there, recall, noticed both that patients with *psychotic* traits presented an increased *threshold for conscious access*, and that those same patients exhibited *altered anisotropy* (i.e., gFA, cf. Sarrazin et al., 2014). The relationship between these three key elements has been further examined. Such assessment has been conducted by Berkovitch et al. (2021) in a ‘threestep model’ suited for investigating this tripartite relationship. Very importantly, not only did the researchers detect a correlation between high conscious threshold, psychosis, and dysconnectivity in neural tissue, but identified the occurrence of a mediation: dysconnectivity raises the consciousness threshold, which, in turn, fosters psychotic symptoms.

This conclusion is drawn from a comprehensive mediation analysis (ivi: 517). Mediation analysis is a statistical framework that allows researchers to explore the mechanisms underlying observed relationships between variables. In this case, the researchers formulated a hypothesis that dysconnectivity (measured by gFA) would be associated with an elevated threshold for conscious access, and that this elevated threshold, in turn, would be related to the presence of psychotic symptoms. To substantiate this hypothesis, the study employed several statistical methods. First, they ran linear models to assess the relationship between gFA and psychotic symptoms, discovering that the effect was significant for certain brain bundles (CLFs and corpus callosum) but not for others. The study also identified a significant correlation between masking threshold and gFA for all three bundles, establishing a connection between the mediator (masking threshold) and the independent variable (gFA). The mediation analysis showed that the correlation between decreased mean gFA and psychotic symptoms was indeed mediated by the elevated masking threshold for these specific brain bundles. As a result, when masking threshold was included as a covariate in the linear model, the direct effect of gFA on psychotic symptoms became statistically non-significant. This statistical evidence led to the conclusion that, at least within the context of these particular brain regions, the effect of reduced gFA on psychotic symptoms is exclusively explained by its influence on the threshold for conscious access.

3.3.1. *The Limitations of the Model*

At first glance, these observations align well with the idea that an elevated threshold for conscious access could lead to a restriction on the information available to individuals with schizophrenia. This notion lends support to the prevailing interpretation, which identifies *deficient inferential processing* as the central problem in this condition, as proposed by researchers of various perspectives, including those both within and outside the predictive framework, such as Fletcher and Frith (2009), Adams et al. (2013), Powers et al. (2017), and Sterzer et al. (2018). Furthermore, this proposal offers a compelling explanation for how an elevated conscious threshold could lead to psychosis. In general, the inferential interpretation involves critical factors, such as a significant limitation on the information available to the individual, resulting in an imbalance between prior beliefs and actual sensory inputs. It's worth noting that, while this concept shows promise, the precise details require further refinement and development.

One first, more general and architectural[5] question is how the issue can be both *inferential* and *circumscribed* to conscious access simultaneously. Berkovitch and colleagues, for instance, do make reference to the PP framework in their work. Indeed, the latter would postulate that all cognitive processing operates inferentially, from the perceptual right up to the full-blown 'cognitive' levels of elaboration, by virtue of its unificatory posture (for related issues on the matter, see Alegiani and Marraffa, 2021). Thus, if the issue involves inferential processes in general, it should affect all levels of cognitive elaboration, not just conscious access, given that inference plays a fundamental role across all cognition. To put it differently, if the ingredients of our inferential operations are consistent throughout the cognitive system, as PP asserts, then it becomes perplexing how intact unconscious processing can coexist with impaired conscious access.

Second, in the case of delusions, patients often confront counterevidence at a conscious level. The problem seems to lie not in the evidence reaching conscious access but rather in the disruption of inferential[6] processes based on this evidence. For instance, when people close to a delusional individual present counterevidence, the individual remains aware of this information. The challenge lies in the individual's inability to effectively incorporate this counterevidence into their conscious inferential processes. This aspect needs more clarity in the literature. When we discuss an elevated conscious threshold, we refer to information that either doesn't reach consciousness or experiences delayed access. In many instances, though, patients do consciously acknowledge information that could help dispel their delusional beliefs. The issue isn't solely about conscious access, but about how effectively individuals *use* this information. It appears that while useful pieces of counterevidence are consciously perceived by the delusional individual, they struggle to integrate this information effectively with their pre-existing beliefs. Consequently, the issue may not be exclusively about conscious access, as suggested in the referred to studies.

Third, a particularly intricate point that warrants attention is that Berkovitch and colleagues' work insists that the issues in psychosis lie in conscious processing, while many pieces of influential literature in cognitive psychiatry

[5] With the term 'architectural' I intend to consider the general structure through which we examine delusions. In this context, particularly when meaning to conjoin PP and GNW, there's a conflict between viewing cognition as inferential and the possibility of an impairment being restricted to a single cognitive process. Indeed, PP is characterized by a 'fractal-like' approach to cognitive processing: thus, the idea that inferential processing is disrupted in one point of cognition but is intact in others poses a significant challenge within this framework.

[6] This term here must not be taken as referring to predictive processing, but to reasoning through cause and consequence more in general. Basically, it points to the 'simple' process of drawing conclusions from premises we habitually perform in the course of our lives.

frequently associate psychotic traits with perceptual processing as well. For instance, influential explanations of delusions often incorporate a "first factor" in the development of delusional thoughts that revolves around perceptual features. For example, let's consider the case of a friend who strongly believes that her partner is an impostor (Capgras delusion; Capgras and ReboulLachaux, 1923). Many authoritative interpretations, such as Aimola Davies and Davies (2009) and Stone and Young (1997), propose a dual-sided explanation: on the one hand, the belief arises from an anomalous perceptual experience (the "first factor"), while on the other, the individual persists in maintaining the belief despite counterevidence (the "second factor"). This suggests that the issue not only pertains to the belief system, which can be located within the realm of consciousness, but also to the perceptual sphere. The challenge here lies in reconciling this involvement of perception with the centrality of conscious access emphasized in Berkovitch et al.'s (2021) interpretation.

One way to address this challenge is to suggest that the threshold for consciousness, while mediating between global functional connectivity alterations and psychosis, may not be the exclusive locus of the issue. Instead, it could be that the problem is also present in the realm of perception, and the threshold serves as a means through which these impairments manifest as psychosis. While this may seem like a promising solution, it appears to be at odds with Berkovitch and colleagues' findings. They emphasize the dissociation between intact unconscious systems and impaired conscious access.

Alongside presenting the three-step model where dysconnectivity disrupts conscious access, ultimately leading to psychotic symptoms, they also argue that an elevated threshold for conscious access significantly reduces the amount of information entering consciousness, leading to distorted interpretations and a disproportionate emphasis on the limited sensory inputs reaching consciousness. Moreover, preserved unconscious processing may continue to guide behavior implicitly, fostering delusional constructs disconnected from the external world. These references substantiate the direct link between an elevated conscious access threshold and psychotic traits, making it unlikely that the suggested solution can fully explain the phenomenon. Note that this third problem, combined with insights drawn from PP, reinforces the first problem mentioned (the inferential issue). If the issue indeed lies in inferential processing, it should manifest at all levels of cognitive elaboration, not just in conscious access.

Fourth, a challenge arises when considering the reduction of information entering consciousness. Berkovitch and colleagues' referred to account, in fact, points to a consequent overestimation of the reduced sensory stimuli that make it into consciousness. Why is there an overestimation of such contents, rather than an overestimation of established beliefs, akin to cognitive conservatism (cf. Fodor, 1987; Stone and Young, 1997)? In many cases of psychosis, subjects often resist incoming evidence rather than overestimating it. This resistance suggests that sensory contents may be underestimated by the individual, while delusional hypotheses are persistently maintained.

For instance, in the case of a patient with Cotard delusion (see e.g., Young and Leafhead, 1996), who believes she is dead, the individual dismisses counterevidence such as feeling her heart beat or experiencing temperature changes, suggesting that the issue extends beyond conscious access. In some instances, sensory counterevidence that is consciously experienced has no impact on delusional beliefs. Hence, the problem does not seem to be restricted to conscious access but involves the individual's ability to process and integrate this information correctly at a conscious level.

4. *The Parametric Variations: Precision*

In light of the above, the existing explanation for the origins of psychotic traits, which primarily focuses on the threshold for conscious access, appears to be in need of further refinement. To address this, I propose an intriguing hypothesis that considers the concept of *parametric variation* (as hinted at by Aimola Davies and Davies, 2009[7]). In general terms, parametric variation involves the idea of exploring how changing specific parameters or factors can affect a system or condition.

Applied to psychiatry and more specifically to psychosis, the general idea is that while psychosis in its manifestations (hallucinations and delusions) is definitely heterogeneous, these variations and specific traits can be seen as outcomes of adjusting parameters within a unified framework.

I suggest that *alterations in precision* might be the driving force behind this parametric variation. This notion aligns with the idea that individuals with schizophrenia may experience impairments in top-down attention. Notably, recall, precision and attention are equated on PP, thus underscoring the potential value of this hypothesis in exploring the intricate underpinnings of psychotic traits.

In addition to the four issues mentioned earlier, it's essential to recognize that hallucinations and delusions exhibit a range of characteristics that cannot be fully accounted for by merely raising the threshold for conscious access. Two distinct features are particularly evident. For instance, consider the *specificity* and *persistence* of these symptoms. Individuals with schizophrenia

[7] While Aimola Davies and Davies themselves do not delve into parametric variation, they point to its potential in providing a unified explanatory framework within psychiatry. Their perspective takes into account the inherent heterogeneity within the field.

tend to eliminate some of their false beliefs, but there is a subset of these beliefs that they do not eradicate. The specificity is evident in the circumscribed nature of these beliefs, while the persistence is characterized by the fact that, unlike other erroneous beliefs, these particular beliefs remain intact in individuals with schizophrenia. The same principle applies to hallucinations: they revolve around specific experiences, and it is these particular experiences that tend to recur over time.

These considerations lead to the suggestion that the elevation of the threshold for conscious access should be further elaborated to accommodate parametric variation, allowing to account for specific and variable manifestations of psychotic traits. In this vein, then, precision/attention would add a new dimension to the understanding of these phenomena.

Expanding upon the tripartite model, I suggest including precision/attention as a pivotal component that mediates the relationship between dysconnectivity and the elevation of the threshold for conscious access.

4.1. *Proof in Ego-Dissolution*

To substantiate these discoveries, it becomes essential to delve into the PP literature regarding precision and its connection to psychosis. While the interplay between these two factors has frequently been observed (e.g., Adams et al., 2013; Sterzer et al., 2018) it's noteworthy that recent and intriguing findings have emerged. This fresh line of research is primarily founded on the exploration of psychedelics. These substances are often referred to as 'psychotomimetic'[8] because their effects closely mirror those of psychosis. Further, psychedelics have become a further path to explore, not only because their intake in controlled settings allows us to experimentally investigate the features of psychosis, but also because their molecular structure, as well as their interactions with neurotransmitters, allows us to accelerate therapeutic solutions (Letheby, 2021). Studies involving psychedelics specifically explore the concept of ego-dissolution, shedding light on the crucial role that precision plays in the onset and characteristics of psychotic symptoms.

To gain deeper insights into the phenomenon of ego-dissolution and the centrality of precision, an examination of the intact functioning of the 'ego'

[8] It is worth mentioning that, even though psychedelic substances induce the occurrence of 'psychotic' symptoms, it is also true that they are able to replicate other symptoms of schizophrenia as well – this tying the knot between schizophrenia and disintegration even more strongly. For example, ketamine replicates both positive and negative symptoms; its principal mechanism of action (NMDA receptor antagonist) appears to reproduce, from a molecular perspective, traits of schizophrenia's pathophysiology (see Rajpal et al., 2022; McCutcheon et al., 2020). LSD, in turn, is an antagonist of serotonin receptors which are strongly associated with the symptoms of early schizophrenia – such as "ego disorders, affective changes, loosened associations and perceptual alterations" (Vollenweider et al., 1998).

becomes essential. It must be noted that these studies confirm the key role of *integration* in schizophrenia's symptomatology[9].

To do this, I shall first refer to how the 'self' actually works on PP, attempting then to show how the link between precision and psychosis is secured in these studies.

The purpose of this section, as a reminder, is to reinforce the idea that precision plays a pivotal role in the emergence of psychosis, thus providing substance to the construction of a quadripartite model. Precision, in fact, acts as the mechanism that furnishes the intricate details that an explanation centered solely on raising the threshold for conscious access cannot fully explain.

4.1.1. *What it's Like to be a Self on PP*

Since that of "self" is a rather elusive term, it must be made clear how, upon this reading, this concept must be understood. The notion of "self" can be defined as either a dynamic process or as an "entity", whether it's stable or fragile, that springs from a process (think, for instance, of the Jamesian notion of "self", 1890), or in terms of continuity in time (think of Locke, 1690/1694, where the notions of "self" and of personal identity are firmly interwoven[10]). I shall consider the self here on its Jamesian interpretation: the self is the product of a "fundamental sense of unity" we tend to through *integrative* operations. The self as a subject (an "I"), then, exhibits a proces-

[9] It must be noted that the literature is not unanimous on this point. A consistent quantity of the literature, in fact, invokes caution in the juxtaposition between schizophrenia and psychedelics (e.g., Friesen, 2022): even though the latter may help us understand that the impairments involved not only in the case of self-modeling but of psychosis in general are strongly tied to those of binding – which are indeed disrupted in the case of psychedelic drug use –, it is also true that differences between the two – that is, schizophrenia and psychedelics – exist. One example of this line of thought is Rajpal et al. (2022). The authors provide substance to this distinction from two different angles: one neurobiological, the other computational-functional (i.e., in terms of differences regarding Bayesian inference in predictive processing). With these two different approaches in mind, the authors uncovered that, while in general the symptoms of both schizophrenia and psychedelic drug consumption can be explained concerning a "strengthening of sensory information over prior beliefs", the processes that trigger this state of affairs are qualitatively different: computationally, in the case of drugs, it is a reduction of the precision weighting of the priors that takes place; in the case of schizophrenia, the precision of sensory inputs increases. Neurologically, both cases instantiate an increased signal diversity and complexity, but while drugs entail a reduced information flow, schizophrenia shows an increased information flow.

[10] Locke explicitly talks about the "self" when dealing with personal identity. There, he investigates the latter in terms of the concept of "person", which ultimately is a thinking, intelligent being that is able to consider itself one and the same through time and space. Further, "consciousness always accompanies thinking, and it is that which makes every one to be what he calls *self*, and thereby distinguishes himself from all other thinking things, in this alone consists *personal identity*" (Locke, 1694/1999: 319; emphases added).

sual nature; the self as an object of reflection (the "Me") is the product of this "*self-ing*" process (see Mc Adams, 1997 and, e.g., Marraffa and Vistarini, 2019: 3). James's account further articulates self-consciousness in terms of identity: "self-consciousness is a self-describing, an identity forming, which is a unifying, integrative, synthesizing process" (Chiaradonna and Marraffa, 2018: 52).

In this vein, it could be claimed that, by and large, the notion of "self" can be here correlated to that of "identity". In brief, the terms we commonly associate with "identity" – i.e.,what makes us "who we are" through time, what "pulls us together" – are deeply ingrained within the concept of "self" on PP. It appears that, within this framework, the "self" can be understood as a *hierarchical model*, and the elements we attribute our identity to are essentially the higher-level components of this model. Thus, when I explain how a "self" is formed, I am also touching upon the aspects that contribute to shaping our identity – those enduring components that are overarching and form the core of who we think we are.

In the realm of PP, this sense of unity is constructed by the system as it strives to minimize prediction errors. In essence, the self can be seen as a hierarchical generative model that the system constructs to exist. In this regard, one of the most relevant perspectives on the self has been put in place by Hohwy and Michael (2017). On their account, the self is a hierarchical model that encompasses different levels of abstraction and inclusion, just like any other model we employ (see Gładziejewski, 2016).

In this vein, not only is, for instance, the body one of the many causes interacting with the world, but its representation, too, is just one of the many represented causes in the models used to minimize error (Hohwy and Michael, 2017: 367-368).

In making explicit how the self-model is made, the authors note how it is hierarchically structured:

1) At the lower levels are those regularities involving the body, the temporal scale of which is fast.
2) At a higher (intermediate) level are medium-term correlations (in term of days or months).
3) At the highest levels, we find established regularities such as character traits.

A compelling illustration of how different levels interact can be seen in our meteorological monitoring model (ibid). In this model, there are multiple layers of information. At a lower spatiotemporal level, I might come across a weather forecast in the newspaper indicating rain, prompting me to grab an umbrella before heading out. This immediate response to the weather forecast also updates my broader understanding of the seasons. For instance, if I've observed this rainy pattern consistently over the past month, I might conclude that the current season, say Spring, is very rainy. Consequently, I may infer that it's wise to keep an umbrella in my backpack throughout the season. This type of information processing occurs because the system's goal is not just to minimize prediction error in the short term but in the long run (Friston, 2010).

This example is a very simple sketch of how a model is built on PP.

Using this example to illustrate the concept in the context of the self, we can draw a parallel: daily information about the weather stands to the body as the seasons stand to the overarching aspects of the self. To put it in a relatable context, imagine that every day at 6 p.m., you find yourself heading to the fridge with a desire for beer. Over time, this habit not only informs you that you have a preference for beer but also that, at that specific moment, you possess the intentional mental state of desiring beer. Importantly, both the fact that you are a beer lover and the fact that you have a desire for beer on that occasion are included in the generative model of your self, but at a high, deep level. Such acquisitions, in fact, are deeper both spatially and temporally than the fact that a particular moment you are moving your body toward the fridge (which is, in fact, a low-level piece of information).

However, there is a subtle yet crucial distinction between the 'season example' and the 'beer example.' While we can acknowledge the similarities between models of the self and other models, a unique aspect of self-models becomes evident: when we construct a model of ourselves, we are essentially modeling our own model. We perceive ourselves only through the model we have created, similar to how we perceive any other object through our model for it. This self-modeling process is circular in nature. On one hand, it involves perceptual inference, where we refine our self-model by adjusting it based on incoming sensory data. However, this adjustment process is carried out through active inference. In simpler terms, we build and update our self-model through perceptual inference, aligning the model with new data. Yet, this incoming data is, in turn, shaped by our actions. As a result, perceptual and active inference continually interact in a circular manner, shaping and structuring the self-model.

This circularity further goes to show, together with Hohwy and Michael's general account of the self, how we ultimately "pull ourselves together", both within and between levels. Crucially, integration is the premise of keeping a coherent model of our bodies, our medium-term features and traits, and our long-term qualities and objectives.

Further idea of this precarious maintenance that makes the self is conveyed by Letheby and Gerrans (2017). While many elements of their account are in line with Hohwy and Michael's and adhere to the broader principles of PP, several key differences emerge. Both approaches acknowledge that the mind treats the self in a similar manner as it does other objects, constructing and integrating a hierarchy where higher-level models address features at lower levels, ultimately forming the entirety of objects we perceive.

However, a significant divergence emerges: the primary point of disagreement centers on the notion that nothing, not even a self-model (as proposed by Hohwy and Michael), can be considered the self. It seems as though Letheby and Gerrans take the concept of reducing the solidity of the self, which is present in Hohwy and Michael's work, to its utmost consequences.

In doing so, they put binding at center stage. They say (2017: 2): "the mechanisms involved model the self as a heuristic, a way of making information 'sticky', rather than as a way of tracking the fluctuating cognitive fortunes of an actual entity". Then, I dare say, the difference between Hohwy and Michael and Letheby and Gerrans could be thus summarized: while the former conceive the self as '*something*' that is – very provisionally – stuck together, the latter conceive the self as the *glue* that – here, too, precariously – keeps a flux of continuously changing information together. *Both, glaringly, entail integration, and in a very strong and fundamental way*. Regardless of how radically one's stance with regard to the fragmentation of the self is, *integration* remains a pivotal phenomenon.

To get an idea of this latter point, I shall briefly focus on one of these levels, the lowest, "bodily" one, that serves as a basis for the other ones.

4.1.2. *The lowest level: the body*

The self, in line with the foregoing, arises from the body and is shaped by sensory integration. To explore fundamental aspects of the bodily self, experiments on body ownership (Schlicht, 2018) have been utilized. These experiments appear to unveil more fundamental insights than the intricate concept of bodily awareness (cf. Tsakiris, 2010).

A prominent experiment in this context is the "rubber hand illusion," introduced by Botvinick and Cohen (1998). In this experiment, one of the subject's hands is concealed, and a rubber hand is positioned in its place. Both the hidden hand and the rubber hand are simultaneously stimulated. Consequently, the subject observes the simulated touch on the rubber hand while feeling the corresponding sensation on their hidden hand. Due to the synchronous timing of these sensations, the subject concludes that the rubber hand is, in fact, their own (Tsakiris and Haggard, 2005).

This illusion is described in terms of a disruption of a process of "intermodal matching"[11] between what is seen and what is felt. This disruption results in a phenomenon known as proprioceptive drift, where the subject's

[11] This expression, intuitively, refers to the correlation (integration, indeed) between stimuli coming from different modalities.

perception of their body's location shifts. Significantly, the illusion is induced by manipulating the correlations between multisensory signals. When these correlations are altered or disrupted, it becomes evident that *assembling* multisensory information is crucial in shaping one's sense of body ownership.

In sum, not only does the exposure to certain signals of a perceptual nature change the sense of ownership relative to one's body but the self itself could be said to be built by signals from disparate sensory systems, thus corroborating the idea that the latter is not a preexisting substratum to which all those signals are attached.

Once sensory integration has taken place (though note that this is an ongoing process), the components that 'make up my body' are probabilistically treated: what I come to identify as 'my body' is nothing more than that set of data having the highest probability of being 'me'.

What prompts us to categorize specific aspects of our experiences as "body parts" is the observation that they trigger numerous sensory systems, particularly those related to the somatosensory[12] domain. Take the example of a hand: the brain learns to establish a connection between a particular visual stimulus and a corresponding sensory input. This learning process leads us to perceive the hand as the source of these visual and proprioceptive[13] inputs. It's worth noting how this recognition of the hand involves the integration of multiple sensory modalities.

The sense of "this is my hand" that's ingrained in an individual's brain is a result of the association formed when they observe an object making contact with their hand, and they simultaneously experience a proprioceptive sensation. In the context of the rubber hand illusion, the process of updating this internal representation occurs when sensory information is inconsistent. In this case, the update relies on the visual information about the body's expected position, rather than information from proprioception or interoception.

What makes this experiment particularly impactful is that it demonstrates that an individual's mental representations of their own body can indeed change and adapt when multisensory experiences deviate from their expected patterns. This reveals how our understanding of our own bodies can be dynamically altered based on sensory discrepancies.

The rubber hand illusion presents an unusual combination of sensory circumstances that can be best explained by the brain incorporating another object into one's self-perception. The updated model of the self incorporates

[12] This term is employed to refer to sensory systems more in general. It includes the proprioceptive, the exteroceptive, and the interoceptive.

[13] Proprioception equates to the ability to perceive stimuli from the body regarding position, movement, and balance. Even if one is blindfolded, they proprioceptively know if their arm is over their head or along their body.

corrections for prediction errors arising from proprioceptive inputs, aligning their sources with the observed location of the visible body or body part. As a result of this update, the predictions are consistent with the visual inputs, giving greater importance to the location information conveyed by sight over proprioception. In essence, sight "hijacks" proprioception (Gallagher, 2012: 144) and triggers a revision of the self-body model.

Furthermore, research has shown physiological changes in the real hand during the illusory experience, such as a decrease in temperature (Hohwy and Paton, 2010). These results indicate a shift in the perception that the actual limb is a part of oneself, pointing to a reduction in the degree to which the real limb is considered an integral aspect of the self during the illusion. This implies a downsizing in the consideration that the real limb may be part of the self.

This observation is particularly significant because it underscores how the body represents a fragile, probabilistic model that heavily relies on sensory associations. In addition to these insights, it's important to acknowledge that, in line with long-standing theories advocating for a "constructive" and *integrative* approach to the self, PP also supports a robustly "interactionist" perspective regarding the expansion of the self. In other words, PP aligns with the idea that the self is continuously constructed and expanded, akin to the incremental building of a structure, with each new "brick" representing a development in this ongoing process.

These observations hold particular relevance for the concept of precision, as precision plays a crucial role in integration and binding processes (as in, e.g., Spratling, 2008). Therefore, if the core issue in psychotic traits revolves around integration, and precision is a fundamental factor in these integrative processes, this line of reasoning further substantiates the idea that precision is a pivotal factor in the development of psychosis.

4.1.3. *What it's Like to Ego-Dissolve*

The upshot of the binding and integrative resources at play even in the most basic levels of construction of ourselves is self-awareness. The latter ultimately lies in the experience we "report in terms of awareness of being a unified persisting entity: the same person at a time and over time" (Gerrans, 2015: 2). In recent years, phenomena occurring at the level of self-awareness – such as episodes of alteration of this experience – have begun to appear as privileged windows to look through to further explore our integrative processes. In addition to this, since such phenomena are incredibly present in schizophrenia's symptomatology – in psychosis, specifically – it has become more and more evident that the latter is a clear example of the alteration of these integrative capacities. As mentioned earlier, psychedelic substances have become a supplementary tool of exploration: the latter have proved able to replicate the alterations presented in schizophrenia. Namely, the phenomenon of alteration of integrative processes visible both in schizophrenia and in psychedelic intake is termed 'ego-dissolution' (Milliére, 2017). To put it in a metaphorically forceful way: when we 'dis-integrate', what happens is we 'ego-dissolve'.

Self-awareness is a product of the intricate, multi-level integrative processes within the 'self.' Ego dissolution, as an alteration of self-awareness, signifies a simultaneous change in these integrating processes. Schizophrenia encompasses such symptomatology. Notably, psychedelics replicate this phenomenon, both at the lower sensory, hallucinogenic level, and at higher levels, where effects encompass changes in emotion, thinking (including creativity and insight), altered perspectives, and shifts in attention, salience, meaningfulness, and the quality of consciousness itself (paraphrasing Letheby, 2021: 46). Collectively, these lines of reasoning lend further support to the idea that schizophrenia involves an impairment in integration, and conversely, that typical self-awareness results from these binding processes. Further, psychedelics, in light of their more recent reappraisal (see Letheby, 2021; Stoliker et al., 2022; Kałużna, 2022), might be employed to further look into the core of this symptomatology.

Considering this literature, the connection between disintegration and precision becomes explicitly evident through two key points.

First, Letheby and Gerrans (2017) have interestingly noted that the disruption of integration observed in ego dissolution, rather than suppressing conscious experience, actually *intensifies* it: subjects experience objects they would not typically consider and are more prone to have unfiltered experiences. This goes to show that integration, by linking salience and experience together – that is, by making us experience what is salient, and by making us consider salient what is functional to our preservation – ultimately constrains our experience. Ego dissolution, in this sense, would essentially be the experience of cognition stripped of the models of the self, that is, cognition unbounded by integration (cf. Stoliker et al., 2022). Empirical evidence has supported this hypothesis: Sapienza et al. (2023) signaled a global decrease in functional connectivity, highlighting the disintegration of preserved functional circuits and a concurrent increase in overall connectivity in the brain. This finding, in effect, nicely dovetails with the suggestion that an impairment in integration couples with a concomitant intensification of conscious experience.

Stoliker et al. (2022), in particular, tied the notion of 'disintegration' with that of 'desegregation' (Stoliker et al., 2022), where the latter phenomenon has been seen to positively correlate with ego dissolution (Tagliazzucchi et al., 2016). Specifically (Stoliker et al., 2022: 6):

"Desegregation describes the abundant deviation of connectivity from functional pathways – or decreased modularity[14] in brain networks and regions – and is an effect cited in reference to an increased complexity [...] Desegregation in cortical communities may relate the richness of psychedelic phenomenological experience which differs from other altered self-dissolving states of consciousness [...] that may relate to the complexity of a self-generative model."

Second, one very notable point, I think, concerns *serotonin*. The so-called 'serotonin hypothesis' is one of the various theories put together to frame schizophrenia with reference to neurotransmitter alterations (see Stahl, 2018). Considering this particular model, though, a few interesting elements surface. One starting point is that Letheby (2021) has particularly focused on this specific neurotransmitter as one target of psychedelics. His focus is mainly therapeutic[15], but what emerges is that psychedelic substances, by dampening the stringency of our tight self-models, actually alter serotonin. The narrative could be described in the following way: psychedelics have the capacity to modify binding processes, which means they alter the stringency with which our self-models are constructed and can induce phenomena such as ego dissolution. This alteration by psychedelics is closely tied to the impact on serotonin levels. In turn, this connection reveals that serotonin plays a significant role in shaping binding processes. Furthermore, fascinating connections exist. For instance, existing literature suggests that serotonin is the neurotransmitter responsible for encoding precision. This perspective enriches our understanding of how serotonin is involved in regulating and influencing the precision of self-models (e.g., Carhart-Harris and Friston, 2019; Yon and Frith, 2021). This observation can be seen as a confirmation that precision plays a key role in the manifestation of psychotic symptoms in schizophrenia.

A significant connection can be drawn from the previous analysis: serotonin affects both binding and precision. The dampening of precision through psychedelics, resulting in ego dissolution, highlights the close relationship between integration and precision, underscoring the impairment in precision modulation in individuals with schizophrenia. This, remember, further corroborates the suggestion that the shortcomings of the tripartite model should be dealt with by inserting the notion of precision into the picture.

[14] The term 'modularity' here is referred to the fact that functional pathways are not separated from one another, and everything comes to be entangled, albeit in a flexible and mutable way.

[15] Letheby (2021) proposes a positive view of this perspective. The underlying idea is that if the tightness of the links encompassed by our self-models is highly reduced, we can effectively intervene on those models and change them: in short, they are more prone to being revised. To make a simplistic example of a scenario as such, a person affected by depression, who has a very negative representation of herself, through the fraying of her self-model right up at its higher levels, can more easily intervene on it and restructure it, ideally refurbishing it with positive information.

5. *Putting all this to work: further developments*

Although the various problems posed above (cf. Subsection 3.3.1) are far from being solved, I shall here elaborate on them briefly.

Recall, in the context of understanding psychosis, one intriguing challenge is the apparent *selectivity* of the issues related to inference and conscious access. It may seem counterintuitive that if inference processes are impaired, these issues would only manifest at the level of conscious access. This problem requires further exploration and could be approached from two different perspectives for clarification.

One way to address this problem is by considering that the impaired inferential processes are not exclusive to conscious access but rather have a pervasive impact on the entire cognitive system. This perspective suggests that the issues stemming from impaired precision influence all levels of cognitive processing, from the most fundamental sensory perception to higher-order cognitive functions. In this view, conscious access might be the point where these issues become most apparent, given that it represents the culmination of these problems. Conscious processing is where the *consequences* of these pervasive issues are most noticeable.

Another perspective to consider is that the issues related to inference and precision are indeed pervasive throughout the cognitive system, but they become more pronounced when it comes to conscious access. Conscious access, in this context, acts as a bottleneck where the competition between different sources of information and the integration of prior beliefs and incoming evidence is most evident.

Consciousness is where the effects of deficient precision are amplified, leading to the overemphasis of prior beliefs at the expense of incoming evidence. This heightened competition and selection process, determined by top-down attention, increases the consequences of these issues, making them most evident at the level of conscious experience.

In sum, this perspective suggests that the entire cognitive system may be affected by these issues, but it is in conscious access where the impact is most notable.

The second challenge I highlighted when investigating psychosis pertains to the *utilization* of information within the realm of conscious access. The problem extends beyond the mere question of what reaches the conscious mind; it delves into the critical issue of how this information is employed.

One potential solution focuses on the concept of "use" in the context of higher-level information within the hierarchy. This information plays a key role in determining the effect of incoming data as it interacts with conscious access.

In this context, the question becomes not just about the *quantity* of information that emerges into conscious awareness but also the *quality* of its use. The way higher-level beliefs guide the processing of this information is crucial. If these beliefs[16] are rigid and resistant to updating, they can hinder the proper integration of new data. Conversely, flexible and adaptive higher-level beliefs can ensure a more accurate and contextually appropriate interpretation of incoming information[17]. These considerations highlight the need to examine not only the entry point of information into conscious access but also the cognitive "filters" through which this information is processed and evaluated, and, consequently, put to use.

The third worry in this exploration of psychosis revolves around the contribution, for example in the onset of delusions, of *anomalies* in *perceptual* experiences. Within this context, then, it is observed that issues in sensory perception significantly contribute to the development of psychotic experiences (e.g., Stone and Young, 1997; Coltheart, 2007; McKay, 2012). Why, then, focus exclusively on the threshold for conscious access? Precision, as observed all along, could help solve the matter.

Precision, in essence, acts as a 'filtering' mechanism, and when it is impaired, a crucial breakdown occurs. Although precision sure is pervasive on PP, if we are to employ it within the tripartite model presented in this work, then we must suggest that it is in its 'top-down' component that the issue really stands, so that it is in the selection for conscious access based on contextual, high-level information that the impairment is all the more manifest. Nonetheless, PP appears to give us the tools to postulate that an impairment in precision's top-down component can reverberate in perceptual modelling. This is also useful, as we shall see, for the 'specificity' of psychotic traits. It is no wonder that this is one of the toughest nuts to crack in this integration between the tools of PP and the posture of GNW (see Alegiani and Marraffa, 2021, for a few observations on this point). When precision is impaired, the brain struggles to discern between meaningful, contextually relevant sensory data and random, irrelevant sensory noise. The result is an indiscriminate treatment of sensory information, with even erroneous data receiving excessive importance. This indiscriminate processing, influenced by prior beliefs and expectations, leads to the formation of distorted perceptual models. These models serve as the brain's internal representation of the

[16] I am aware of the care that is required in employing a term as such, especially in the context of PP. I shall not delve here into the debate concerning representations and their nature within this framework, as it is incredibly vast and complex.

[17] In predictive processing, a key process involves finding the right balance between the flexibility and rigidity of higher-level priors to avoid under-constraining incoming information, as exemplified in ego-dissolution.

external world. In the context of psychosis, these models become distorted, causing sensory experiences to be inaccurately interpreted, resulting in altered perceptions. Importantly, it is probable to imagine, on PP, that these altered perceptions, originating from issues within the model, including the top-down level of precision evaluation, can simulate perceptual anomalies. This observation is crucial for understanding accounts of psychosis, where anomalous perceptions have oftentimes been presented as 'starting points' for the condition. It might be postulated that these perceptual anomalies are the result rather than the origin of psychosis, as the issue start at higher levels, to then reverberate perceptually and manifest, in fact, at the conscious level of cognition.

The fourth issue presented above concerns the theme of underestimation of incoming evidence, which is emphasised by Berkovitch and colleagues themselves. In all truth, seemingly, it's not a matter of overestimating what gains conscious access but rather a tendency to dismiss incoming information, even when it is relevant. Inizio modulo

One potential solution to this problem focuses on the concept of *rigid* high-level priors within the hierarchical generative models. It is in fact quite intuitive that if these high-level priors become resistant to change, they can play a key role in explaining the mechanism behind the dismissal of incoming information. In sum, the rigid high-level priors orient the interpretation of incoming information. When they are inflexible, they might lead to the outright dismissal of data that contradicts or challenges the web pre-existing beliefs[18]. This can result in the exclusion of relevant information from consciousness.

In line with the foregoing, this process, systematically reiterated, impacts not just conscious access, but the entire cognitive system, thus leading to the persistence of erroneous beliefs and the reinforcement of delusional thinking.

One of the other issues mentioned above focused on the persistence of psychotic traits. Once a delusion is fixed, it is resilient to change. On this point, I find Corlett and colleagues' (2009) focus on memory reconsolidation particularly informing: here, delusions, for example, are seen as forms of memory that resit correction due to impaired precision. Delusions in psychotic states occur in the context of a noisy nervous system striving to construct and maintain a robust set of priors. The heightened noise levels result in more cycles of reactivation and subsequent reconsolidation, leading to the development of a peculiar and 'dysfunctional' set of expectancies about the world. These learned expectations, which are highly resistant to sensory input, form the basis for the persistence of delusional beliefs. In the context of memory reconsolidation, delusions, considered a type of memory, are challenging to correct due to precision impairment. The "transition from a salient episodic experience to a habitual belief about the world" (Corlett et

18 This sure reminds us of "conservatism" (Stone and Young, 1997).

al., 2009: 5) relies on systems responsible for encoding the salience of events, tightly interwoven with learning and memory processes. In fact, interventions directed at these systems can yield memories as enduring as those established through repeated real-world experiences. Thus, discrepancies in prediction error values stemming from issues related to precision can swiftly lead to the shaping of distorted prior beliefs.

The last challenge proposed here consists in understanding the *specificity* of psychotic traits. The latter might lie in the role of perceptual processing in fabricating hierarchical models. This consideration becomes especially relevant in light of the suggestion that the primary point of these challenges is at higher levels of processing, culminating in their manifestation at the conscious level. Once again, we can focus on precision: on PP, the latter assumes a central role in guiding how the brain evaluates the reliability of sensory inputs. When precision is impaired, the brain faces the difficult task of distinguishing between meaningful sensory information and random noise, which encompasses data from perceptual systems. As anomalous prediction errors accumulate, the brain attempts to construct a coherent model of the world that can effectively accommodate these unusual experiences. This accommodation often involves intervening at the level of expectations regarding sensory stimuli, which primarily occurs at higher levels of cognitive elaboration. In this process, erroneous beliefs arise, exhibiting a *duality* that is both specific and situated at higher levels of cognitive processing. These beliefs possess a specificity that pertains to the individuality of psychotic traits, while also being intimately tied to the cognitive elaboration that characterizes higher-order information processing.

One last nut to crack involves a contradiction that the reader might have already noticed: until now, I have referenced to the stiffness of higher-level information in the onset of psychosis. However, the section on ego-dissolution has pointed to a relaxation of priors and expectations[19]. This contradiction might perhaps be tackled by delving into the concept of precision and its flexibility within the model. The case of ego-dissolution might highlight a different facet of precision's influence: precision purports parametric variation to the model, recall. While precision typically reinforces prior beliefs, making them resistant to change, it can also, under certain conditions, permit the relaxation of these priors. This flexibility in precision, when functionally gauged, helps the brain adjust its beliefs to encompass a wider range of experiences and sensory inputs. Precision, in sum, is not solely about rigid beliefs but also encompasses the capacity for belief relaxation. This understanding points out the necessity for model adaptability, as well as the dynamic nature of

[19] The relationship between lower and higher-level components cognition in ego-dissolution is the object of discussion; for two examples, see Carhart-Harris and Friston (2019) and Corlett et al. (2016).

precision in shaping our perception and understanding of the world. It also aligns with the idea that precision can grant parametric variation to the model of psychosis, impacting priors in ways that may involve both relaxation and increased rigidity, depending on the context and conditions.

6. *Conclusion*

The primary aim of this paper was to draw attention to specific concerns within Berkovitch and colleagues' GNW-based account of psychosis (e.g., Berkovitch et al., 2021). I proposed that integrating the concept of precision, derived from Predictive Processing (PP), could potentially address these concerns and enrich the existing tripartite model.

After briefly reviewing the two theories, I elucidated the core tenets of the target GNW-based account to lay the groundwork for the following discussion on precision. While the literature has previously highlighted precision's role in psychosis, I referenced to a quite novel perspective by exploring its connection to ego-dissolution, particularly within the context of psychedelic experiences, as a means to emphasize its relevance to understanding psychotic traits.

In light of this foundation, we put forward some preliminary solutions for further exploration and development, focusing on how precision could serve to mitigate the identified shortcomings. This paper contributes to the ongoing discourse on psychosis, offering a promising avenue for future research and a potential bridge between GNW and PP.

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Focus
“The Non-Neutrality of Medical Knowledge”

Introduction

Gerardo Ienna*, Antonio Piccolomini d'Aragona**

The articles that constitute this special *Focus* have the common aim of exploring – drawing on concrete case studies – how the production of medical knowledge may be, on the one hand, affected by extra-scientific factors and, on the other hand, be used and distorted for social, political, cultural etc. purposes. Following a longstanding tradition, this kind of epistemological issue could be named as the problem of "non-neutrality" of scientific knowledge. This expression is used to refer to a contestation of the image of science understood as disinterested research not subservient to special interests, as free from bias and conditioning, as pure research indifferent to the purposes with which it is produced or to the social responsibility of scientists, and, finally, as pure rational enterprise not susceptible to ideological abuses.

Anticipations of this debate can be traced as early as the Marxist debates on science in the 1930s, which gave rise to the historiographical current known as externalism (an expression used by its detractors). Authors such as Boris Hessen, John D. Bernal, Henryk Grossmann or Edgar Zilsel applied Marxist methodology to studies on science, showing how the development of scientific thought depended on social and economic contexts and thus deconstructing the historiographical model of the history of science as a celebration of great personalities. For the development of an epistemological consciousness of the socio-historical elements affecting the production of scientific knowledge in the medical field, particularly relevant was the work of the Polish microbiologist Ludwik Fleck. In his *Genesis and Development of a Scientific Fact* (1935) he opened a line of research that, however, had a very narrow reception at the time of publication. It was R. K. Merton and Thomas Kuhn who rediscovered this volume by having it translated into

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English in 1979, and thereby making Fleck a classic author in the field of history, philosophy, and sociology of science. Fleck's methodology, precisely because of its ability to enter into the content of science by showing its social determinations, was particularly appreciated in the field of the *Sociology of Scientific Knowledge* (SSK), which saw in this author one of its precursors. As it is easy to imagine, in addition to the authors just mentioned, the institutionalization of the sociology of science, the historical epistemology, and the development of Kuhnian and post-Khunian theories played a key role in creating the preconditions for the emergence of the debate on the neutrality and non-neutrality of science (with special reference recognized to extra-scientific factors in the paradigm shift phase).

Despite these anticipations, the debate on the neutrality and non-neutrality of science definitely took shape during the 1970s and in the wake of the political-cultural climate of the time. While the *Science and Technology Studies* and, especially, the SSK were consolidating on the one hand, the so-called "Radical Science Movements" were also gaining ground. Both of these critical approaches to science developed, albeit in different forms, an analysis of the non-neutrality of science.

On the one hand, the authors afferent to SSK had the ambition to overcome institutional sociology of Mertonian orientation, which they claimed was limited to the analysis of the organization of science and not its cognitive content. Through the adoption of the principles of "causality", "impartiality", "symmetry" and "reflexivity", the Strong Program in SSK believed it could open the "black box" of scientific knowledge and show how its very content could be affected by social determinations. In its developments, it even derived the thesis held by Steven Shapin and Simon Schaffer that "solutions to the problem of knowledge are solutions to the problem of social order".

On the other hand, the Radical Science Movements, adopting a militant perspective, were rather oriented in emphasizing the intricate relationships between science and politics. By the phrase "non-neutrality of science", the Radical Science Movement referred to at least two specific stances: on the one hand, scientific knowledge, like other cultural forms, is influenced by the historical and social conditions in which it is produced. In this sense, science and technology are ideologically influenced by forms of cultural and economic hegemony. On the other hand, science and technology are conceived as conformations of knowledge that cooperate in structuring forms of organization of society, production, power, etc. (social function of science). In this sense, science and technology are tools that can be ideologically used to cooperate in the creation of cultural hegemony. In synthesis, science seems to have a dual positioning in classical Marxist theory: it can be seen as cooperating in the development of the economic and social structure, but it can be also affected by the latter and be seen as an element of the superstructure.

The readers will find in the following pages a variety of ways in which the theme of the non-neutrality of science can be used today in the study of specific historical case-studies covering a time span from the Middle Ages to the present day.

Is there a Science of Healthy Eating?

Donald Gillies*

Abstract: There is a great deal of talk about healthy eating, but also considerable differences of opinion about what constitutes healthy eating, which are illustrated by comparing the Paleo diet with the advice on healthy eating given on the UK's National Health Service website. This raises the question as to whether recommendations about healthy eating are arbitrary, or whether some of them have a genuine scientific basis. The paper argues for the second alternative. It considers the hypothesis H that a diet high in saturated fat causes atherosclerosis and argues that H is well-confirmed empirically by observational and experimental evidence and so should be considered scientific. H is indeed accepted by mainstream science. However, a strange phenomenon has emerged which could be called 'media science'. This is run by journalists rather than medical researchers, but these journalists imitate the procedures of mainstream science. They argue that H is false, and that there is no harm in eating a diet high in saturated fat. The paper gives an analysis of the work of some of these media scientists and concludes that their arguments are largely bogus.

Keywords: saturated fat, confounding, media science.

Disagreements about What Constitutes Healthy Eating

The phrase 'healthy eating' is widely used, and yet there is considerable disagreement about what actually constitutes healthy eating. This can be seen by examining some websites which purport to give a healthy diet. Let us start with thepaleodiet.com[1], which advocates what is called the Paleo diet. This is based on the argument that humans evolved during the hunting-gathering period (the paleolithic), and so we are best adapted to eating the diet which was common at that period. It is this diet, which will keep us healthy. Since

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hunters killed many animals, it follows that meat cannot be bad for humans. Further, it is desirable to eat a large quantity of fruit, nuts and vegetables, since, for the Paleo supporters, these were gathered in the hunter-gatherer period of human evolution. However, the Paleo diet eliminates bread, pasta and rice. The argument for so doing is the following. Such carbohydrates only appeared in the neolithic period with the invention of settled agriculture, which occurred after the hunter-gatherer period, the paleolithic. So, we did not evolve to eat such things as bread, pasta and rice which therefore can only do us harm. The Paleo diet also excludes legumes, such as peas, beans, and chickpeas, presumably for the same reason. On the website (thepaleodiet.com), the Paleo Diet is described as: "Healthy eating, based on science" and the following is a quotation: "Don't Eat: Grains, including oats, pasta, and cereal; Legumes, including beans, soy & peanuts".

Let us turn to another website nhs.uk[2]. This is the website of the UK National Health Service (NHS), and, among a great deal of information about diseases and treatments, it gives some advice about healthy eating. Here are some quotations from this website: "Starchy carbohydrates should make up just over a third of the food you eat. They include potatoes, bread, rice, pasta and cereals". Clearly this completely contradicts the Paleo diet which bans all cereals, as we have seen, and also bans potatoes, though curiously it allows sweet potatoes. What then about the question of meat? The NHS website does not advocate banning meat altogether but advises cutting down the amount of meat consumed. The website states that meat contains saturated fat, and it says: "Try to cut down on your saturated fat intake". It also says: "If you eat a lot of red […] meat, it is recommended that you cut down as there is likely to be a link between red […] meat and bowel cancer". This goes against the Paleo diet, which puts no limit on the amount of meat eaten provided it comes from grass, fed animals and is not processed.

The Paleo diet bans the consumption of beans and other legumes, but the NHS website says: "Pulses, such as beans, peas and lentils, are good alternatives to meat because they are lower in fat and higher in fibre and protein, too". It actually recommends substituting meat with pulses and starchy foods in stews etc, saying: "try using smaller quantities of meat and replacing some of the meat with vegetables, pulses and starchy food in such dishes such as stews, curries and casseroles". This is clearly the opposite of the recommendations of the Paleo diet which bans pulses and starchy foods, while putting no limit on meat consumption. I have given two accounts of what constitutes healthy eating from internet websites and clearly, they differ very considerably. There are several other accounts of what constitutes healthy eating to be found.

[2] Consulted 19 October 2022.

So, what are we to make of all this? It might be concluded that what constitutes healthy eating is just a matter of opinion, and the various opinions are based on speculative theories and anecdotal evidence. However, I do not agree with such a conclusion. My own view is that some theories of healthy eating are scientific in the sense that their claims have been rigorously tested using carefully collected observations and well-designed experiments, and that these claims have been well-confirmed empirically by this evidence. In the next section, I will give an example of such a theory concerning healthy eating.

How to Avoid Atherosclerosis

My example is that of atherosclerosis (from the Greek words: athera = gruel, and sclerosis = hardening), which consists in plaques forming in the arteries. These atherosclerotic plaques have a variety of undesirable effects. They restrict the blood flow in an artery, and, when strongly developed, can lead to thrombosis, that is the formation of a blood clot which may close the artery completely. If this happens in an artery supplying blood to the brain the result is a stroke. If the arteries supplying the legs and feet are affected by atherosclerosis, the result is peripheral vascular disease. A typical symptom when blood flow is restricted without being cut off altogether is intermittent claudication, which consists of pains in the legs when walking which disappear after a short rest. The arteries are no longer able to supply sufficient oxygenated blood to fuel the muscles needed in walking. In more extreme cases, perhaps after a thrombosis, gangrene can set in.

Coronary heart disease is produced by atherosclerosis of the coronary arteries which supply the heart with blood. If these arteries become partially blocked, and blood flow to the heart is restricted, the result is angina pectoris (acute coronary syndrome). This consists of chest pains which arise from exertion but disappear with rest. It is similar to intermittent claudication but affects the heart rather than the legs. A thrombosis in one of the arteries supplying the heart leads to a myocardial infarction (or heart attack). If the blood clot closes off the blood supply from the artery, a portion of the cardiac muscle (myocardium) dies. In 25 to 35 per cent of cases, a first heart attack is fatal.

Atherosclerosis was discovered in the 19th century as a result of autopsies carried out on patients who had died from coronary heart disease and other conditions. These autopsies showed that the disease symptoms in life were associated with atherosclerotic plaques in a section of the arterial system. A further important discovery was made in 1910 when a German chemist (Windaus) showed that atherosclerotic plaques were composed of calcified connective tissue and cholesterol.

So, by the second decade of the twentieth century, atherosclerosis in its various forms had become a well-known disease. However, the causes of atherosclerosis remained obscure. Then in the 1950s, an American nutritionist, Ancel Keys (1904-2004) who was studying coronary heart disease (or CHD), came up with the hypothesis that CHD was caused by eating a diet which was high in saturated fat. It is important here to distinguish saturated fat, which is mainly found in animal fats such as meat, butter, cheese, cream, from monounsaturated fat, found in olive oil, and polyunsaturated fat found in corn oil. Ancel Keys regarded saturated fat as potentially dangerous, but he did not regard monounsaturated fat or polyunsaturated fat as dangerous. In fact, he considered them as on the whole beneficial.

Ancel Keys' work was concerned with CHD, but it largely applies to atherosclerosis in general. If Keys' hypothesis is correct, then atherosclerosis can be avoided by reducing the amount of saturated fat in one's diet. But is the underlying hypothesis H: "A diet high in saturated fat causes atherosclerosis"[3], an arbitrary conjecture or a scientific claim which has been strongly empirically corroborated by the evidence of observations and experiments? In the next section, I will argue that the second alternative is the correct one.

Mainstream Science on Saturated Fat

Ancel Keys organised a large-scale international study to test out his hypothesis H, as applied to coronary heart disease (CHD). This became known as the seven countries study. It was a 'cohort study' or 'prospective study', and so purely observational in character. 16 different cohorts were chosen – at least one in each of the seven countries. The investigations were started between 1958 and 1964, with the plan of examining the participants at the beginning and then every five years[4].

The seven countries were: Finland, Greece, Italy, Japan, the Netherlands, USA, and Yugoslavia. The cohorts chosen consisted of men aged between 40 and 59, since this was the group most at risk of CHD. In the USA, there was one large cohort of 2,571 men who were employees of the American railroad industry. The participants in the 15 remaining cohorts numbered 10,199, giving an average of 680 per cohort. The smallest of these cohorts was 504, and the largest 993.

[3] I will refer to this as 'causal hypothesis H' in what follows.

[4] By 1969, the five-year results were in, and in 1970, the results were published in Ancel Keys (ed.), *Coronary Heart Disease in Seven Countries*, "Circulation", Vol. XLI-XLII, Supplement I, 1970, pp. 1-198.

The diet of the participants was carefully recorded. Samples from the cohorts were taken, and the foods they ate in a week were noted and weighed. This was repeated at different seasons of the year. The foods consumed were analysed to find their constituents in terms of fats, carbohydrates, proteins, etc. This analysis was carried out in different ways, and it was checked that the different estimates agreed[5]. The number of those in each cohort who died of coronary heart disease (CHD) was also recorded.

At the end of the study, for each cohort, the percentage of the diet calories which came from saturated fat, on the x-axis, was plotted against the number per hundred of the cohort who had died of CHD, on the y-axis. The result (see figure 1) shows a close approximation to a linear regression model with a correlation of 0.84[6]. This evidence undoubtedly confirms the causal hypothesis H. But the evidence has weaknesses as well as strengths. Its weakness can be summed up in the well-known maxim that *correlation is not causation*. It is perfectly possible to have a very strong correlation between two variables, which are not causally connected at all. A simple example is provided by Freudenheim[7], who report a series of studies of alcoholics carried out between 1974 and 1981, which showed that the high consumption of alcohol by these individuals was accompanied by high levels of morbidity and mortality from lung cancer. So, we have a strong correlation between heavy

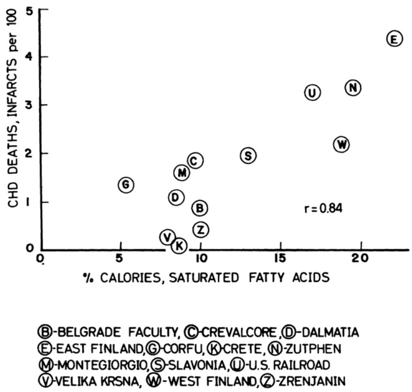

Fig. 1. Source Keys, 1970, Figure XVII.6, p. 174.

[5] Id., *Coronary Heart Disease*, cit., pp. 162-9.

[6] For further details about the seven countries study, see Donald Gillies, *Causality, Probability, and Medicine*, Routledge, London and New York 2019, pp. 108-119.

[7] Jo L. Freudenheim, John Ritz, Stephanie A. Smith-Warner, Demetrius Albanes, Elisa V. Bandera, Piet A. van den Brandt, Graham Colditz, Diane Feskanich, R. Alexandra Goldbohm, Lisa Harnack, Anthony B. Miller, Eric Rimm, Thomas E. Rohan, Thomas A. Sellers, Jarmo Virtamo, Walter C. Willett, and David J. Hunter, *Alcohol consumption and risk of lung cancer: a pooled analysis of cohort studies*, "American Journal of Clinical Nutrition", 82, 2005, pp. 657-667.

drinking and lung cancer. However, few would accept that there is a significant causal link here. Instead, the correlation would be attributed largely to the confounding factor of smoking.

A large percentage of alcoholics are also heavy smokers, and most researchers would judge that it was the smoking of alcoholics rather than their drinking which increased the risk of lung cancer. In such an example the correlation does not show causality, because of the existence of a confounder, which, in this case, is smoking. So, the weakness of the kind of observational statistical evidence which Keys presents in his seven countries study is that it is liable to confounding. However, there are three strategies for dealing with confounding, and, in the case of our causal hypothesis H, they were all applied successfully, thereby showing that Keys' correlation really is causal in character.

The first strategy consists in considering possible confounders and then testing whether they hold or not. As far as the seven countries study is concerned, this can be most conveniently illustrated by confining ourselves to the results from just two countries, namely Japan and the USA There were two cohorts from Japan. One was from the farming village of Tanushimaru and had 509 members. The other was from the fishing village of Ushibuka and had 504 members. There was only one large cohort from the USA consisting of 2,571 railroad men. The various statistics for the 5-year period were as follows. The percentage of calories from saturated fats was 3% for Japan and 17-18% for USA The USA figure was thus between 5.7 and 6 times that of Japan[8]. Among the Japanese cohorts there were 5 deaths from CHD, giving a death rate per 1,000 of 4.9. In the USA cohort, there were 62 deaths from CHD, giving a death rate per 1,000 of 24. The U.S.A death rate from CHD was thus 4.9 times that of Japan.

These figures certainly suggest a causal link between dietary saturated fat and coronary heart disease, but could there be an alternative explanation? The USA railroad men were all white (Caucasian). Could the Japanese have a group of genes which protected them from CHD, and which was lacking among Caucasians? The lower death rates from CHD among the Japanese might be nothing to do with their diet and be the result rather of their genes. To put it another way, there might be a genetic confounder of the hypothetical causal link between dietary saturated fat and CHD.

In fact, it was quite easy in the CHD case to test out the hypothesis of a genetic confounder against data, and this was done by Keys and his colleagues. If there is a genetic confounder, then groups of Japanese who start eating a typical American diet will have an incidence of CHD much lower than that of white Americans, because they will be protected by their anti-CHD genes.

8 A. Keys (ed.), *Coronary Heart Disease*, cit. p. 168.

If on the other hand, there is no genetic confounder, then groups of Japanese who eat a typical American diet will have an incidence of CHD much the same as that of white Americans. All that needs to be done is to compare data on incidence of CHD among Japanese living in Japan and eating the traditional Japanese diet with that among Japanese who have emigrated to the USA and started eating a typical American diet. In 1958, Keys and colleagues carried out an investigation of this sort by comparing Japanese in Japan with Japanese in Hawaii and in California[9]. The overall finding was that death rate from CHD among the Japanese in California was more or less the same as that of white Californians, and much higher than that of Japanese in Japan. The death rate from CHD for Japanese in Hawaii was intermediate between the two values. The observations indicated that as Japanese, who had emigrated, became increasingly Americanised and adopted the American diet, so their death rate from CHD rose to become more or less the same as that of white Americans. There was no indication that the Japanese had any special genetic protection against CHD. These findings were confirmed by a later investigation along similar lines[10]. In fact, the hypothesis that there might be a genetic confounder was strongly disconfirmed.

The second strategy for dealing with confounding is the use of randomization. Suppose that A is strongly correlated to B, and we think A may cause B, but are doubtful whether there may instead be a confounder C which causes B. The first strategy for dealing with this doubt was to think of all the possible confounders C that we can, and then test them out against the data to see if we can refute the hypothesis that C causes B. If we can indeed refute all such hypotheses, this provides good evidence that A causes B. However, it could now be objected that this is all very well, but that there may still be a confounder we have not thought of. Randomization provides a technique for dealing with unknown confounders. Suppose there is a confounder C, which, unknown to us, causes both A and B, and that A does not really have any causal influence on B. In our trial, let us divide the participants randomly by tossing a fair coin, into the control group, who do not receive A, and the intervention group, who do receive A. Then if a particular participant has C, he or she is just as likely to be in the control group as the intervention group. Since C is a cause of B, and A is not a cause of B, then the difference between

9 Ancel Keys, Noboru Kimura, Akira Kusukawa, Brian Bronte-Stewart, Nils Larsen, and Margaret Keys, *Lessons from Serum Cholesterol Studies in Japan, Hawaii and Los Angeles*, "Annals of Internal Medicine", 48, 1958, pp. 83-94.

10 Thomas L. Robertson, Hiroo Kato, George G. Rhoads, Abraham Kagan, Michael Marmot, S. Leonard Syme, Tavia Gordon, Robert M. Worth, Joseph L. Belsky, Donald S. Dock, Michihiro Miyanishi, and Sadahisa Kawamoto, *Epidemiologic Studies of Coronary Heart Disease and Stroke in Japanese Men Living in Japan, Hawaii and California. Incidence of Myocardial Infarction and Death from Coronary Heart Disease*, "The American Journal of Cardiology", 39, 1977, pp. 239-243.

the intervention group and the control group will disappear. If there continues to be a difference between the control group and the intervention group, we can conclude that the intervention variable (A) really is a cause and that confounders have been dealt with. Of course, this argument is not entirely conclusive, since random choice by tossing a fair coin only produces an equal distribution in the limit. In a particular trial, an unbalanced distribution can occur. Despite this problem, randomization still remains a useful technique for the reasons given earlier in this paragraph, and it is widely employed by medical statisticians as a strategy for dealing with confounding.

We can illustrate these points by considering a randomized controlled trial designed to investigate the relationship between the dietary intake of saturated versus unsaturated fat and the development of atherosclerosis. This clinical trial was carried out by Seymour Dayton and his colleagues as follows:

> Male veterans, aged 54-88 years (mean 65.5 years), living in the domiciliary unit of the Los Angeles Veterans Administration Center, were asked to volunteer for this study. A total of 1095 men volunteered, but exclusions and dropouts during the preliminary phases reduced this number so that 846 men were ultimately randomised. Of these, 422 received the conventional control diet, and 424 the experimental diet[11].

The control diet was a conventional American diet containing 40% fat calories, mostly of animal origin. The experimental diet was designed to be as similar to the conventional diet as possible, except that about two thirds of the animal fat calories were replaced by vegetable oils, such as corn, cottonseed, safflower, and soybean. In effect, the total fat content of the diet was about the same, but two thirds of the saturated fat had been replaced by unsaturated fat.

In addition to being randomized, the trial was double blind. The participants did not know whether they were having the experimental diet or the control diet. They were examined at regular intervals, and the values of various variables recorded. However, the physicians carrying out these examinations were also 'blinded' by not being told which diet the participant was on.

Recruitment started in the summer of 1959, and the trial was concluded with a final re-examination of a participant eight years after he had been recruited. No further recordings for the purposes of the trial were made after 15 January 1968. A feature of this trial was that it recorded all atherosclerotic events and not just coronary heart disease. The events recorded were those established by hard criteria, and, in addition to myocardial infarction, and sudden death due to coronary heart disease, they included definite cerebral

[11] Seymour Dayton, Morton Lee Pearce, Hubert Goldman, Alan Harnish, David Plotkin, Martin Shickman, Mark Winfield, Albert Zager, and Wilfrid Dixon, *Controlled Trial for a Diet High in Unsaturated Fat for Prevention of Atherosclerotic Complications*, "The Lancet", 2, 1968, p. 1060.

infarction (i.e. stroke), ruptured aneurysm, and even amputation necessitated by atherosclerosis of arteries in the leg. This choice of outcomes is more logical than focussing exclusively on coronary heart disease, since the other outcomes have the same underlying cause, namely atherosclerosis. The results were as follows. In the control group 70 men died of an atherosclerotic event, as opposed to 48 in the experimental group. The difference was significant at the 5% level[12]. In addition, there were some nonfatal atherosclerotic events established by hard criteria. 96 men in the control group were affected by a fatal or nonfatal atherosclerotic event, as opposed to 66 in the experimental group. The difference was significant at the 1% level.

These results clearly give empirical confirmation to the causal hypothesis H. The seven countries study was observational in character, and so strictly yielded only correlation not causation. The study of the Los Angeles veterans, by contrast, was a randomized, and even double blind, clinical trial, and so had a built-in remedy against confounding. This is the great strength of the Los Angeles veterans trial. In favour of the seven countries study, however, it should be added that Keys and colleagues did consider at least one possible confounder (racial/genetic factors) and showed that it was refuted by further observational evidence.

If we take the evidence of the veterans study together with that of the seven countries study, then the weakness of one piece of evidence is to a large extent cancelled out by the strength of the other. The weakness of the seven countries study was concerned with the problem of confounding, while the randomization used in the veterans study is one of the standard ways of dealing with the problem. The weaknesses of the veterans trial were those characteristic of clinical trials in general, namely sample limitations. The sample was relatively small, and the participants were too elderly to be representative of the target population. However, both these weaknesses were overcome in the seven countries study. The results of the two studies empirically confirmed broadly the same causal hypothesis. So, taken together, they give strong evidence for this hypothesis. This is an instance of the principle that combining evidence of different types all of which support a given hypothesis, greatly strengthens the confirmation of that hypothesis. This could be called the principle of *strength through combining*[13].

[12] Seymour Dayton, Morton Lee Pearce, Sam Hashimoto, Wilfrid Dixon, and Uwamie Tomiyasu, *A Controlled Clinical Trial of a Diet High in Unsaturated Fat in Preventing Complications of Atherosclerosis*, "Circulation", 40(1), 1969, Supplement II, pp. 26-27.

[13] Phyllis Illari, *Mechanistic Evidence: Disambiguating the Russo-Williamson Thesis*, "International Studies in the Philosophy of Science", 25(2), 2011, pp. 139-157. For further details see D. Gillies, *Causality,* cit., pp. 129-132.

The third strategy for dealing with confounding is a consideration of mechanisms. Let us take our usual case in which A and B are strongly correlated, and we are wondering whether this means that A causes B, or whether the correlation is due to confounding. Suppose further that B is some physical illness. Then it may be possible to discover a bodily mechanism M which links A to B. Such mechanisms are usually physiological/biochemical processes which go on in the body. Their existence and nature can be confirmed, or disconfirmed, by laboratory experiments on animals, tissues, cells, and so on. Suppose that the existence of a mechanism M linking A and B is established in this way. Such a result provides some evidence that the correlation is indeed a causal connection, and not the result of confounding. This strategy works very well in the case of causal hypothesis H. In fact, a mechanism has been established by which a diet high in saturated fat leads to atherosclerosis. It can be stated as follows:

1. Diet high in saturated fats *causes.*
2. Raised level of LDL cholesterol in the blood *causes.*
3. Infiltration of some parts of the arterial wall by lipids (namely cholesterol esters) *causes.*
4. Lipids to be absorbed by macrophages, which become foam cells *causes.*
5. Full development of atherosclerotic plaques.

The step from 1 to 2 is very easy to establish and has been confirmed by a variety of observational studies and randomized controlled trials. The steps from 2 to 5 were confirmed by autopsies on humans and by experimental work on animals. The first such experimental work was carried out by a Russian scientist, Anitschkow, who found a method of raising the LDL cholesterol level of rabbits by feeding them a special diet. These rabbits then developed atherosclerosis which is unknown in rabbits eating their usual diet. It could be objected that the experimental production of atherosclerosis in rabbits might be different from what occurs in humans, but Anitschkow replied that the two processes are very similar[14]. The atherosclerotic plaques develop in the experimental rabbits in the same order and in the same places in the arterial system as those in humans which are revealed by autopsies[15]. Taking all this together, we can see that there is a mass of evidence of different kinds which support causal hypothesis H. Applying the principle of strength through combining, we have to conclude that H is very well confirmed empirically and so should be accepted by scientific medicine[16]. Indeed, it has been accepted by

[14] Nikolaj Anitschkow, *Experimental Arteriosclerosis in Animals*, In E. V. Cowdry (ed.), *Arteriosclerosis*, Macmillan, New York 1933, pp. 271-322 (in particular p. 305).

[15] For further details of Anitschkow's work see Gillies, *Causality,* cit., pp. 95-101.

[16] An anonymous referee pointed out, however, that the studies by A. Keys and S. Dayton are

the medical mainstream. We saw this from the NHS website which says: "Try to cut down on your saturated fat intake". This is confirmed by Mike Laker. The idea of this series is to give the standard mainstream views regarding a variety of illnesses. Laker writes: "The main objective of changing your diet is to reduce your saturated fat intake"[17]. Yet despite this consensus of the medical mainstream, not everyone accepts causal hypothesis H, as we will see in the next section.

Media Science on Saturated Fat

The main opponents of the views of the medical mainstream on nutrition are journalists rather than medical researchers. The two most prominent are Gary Taubes, and his follower Nina Teicholz, who published in 2014 a book of 479 pages (*The Big Fat Surprise*) most of which are taken up with criticizing the work of Ancel Keys et al and proposing alternative hypotheses. It is curious that this opposition comes from journalists rather than medical researchers, but Nina Teicholz rather ingeniously uses some of Kuhn's philosophy to explain this. She argues that the ideas of Keys and colleagues on cholesterol constitute the dominant paradigm in this branch of medicine, and so are accepted by nearly all medical researchers who are normal scientists. What is needed however is a "paradigm shift on cholesterol". Moreover Teicholz says: "given the stranglehold that Keys's ideas have held on nutrition researchers for so many decades, it is perhaps inevitable that an alternative hypothesis had to come from an outsider"[18].

So, is there going to be a Kuhnian scientific revolution in nutrition started by two journalists? I will briefly consider this question by examining the arguments presented by Nina Teicholz in her 2014 book. It should be added that arguments of this type are often to be found in newspaper and magazine articles and on the internet. Let me begin by considering a phrase often used by Nina Teicholz and other journalists and bloggers, namely 'low-fat diet'. Thus, for example, Teicholz claims:

of men only, so that strictly we should not consider the results as necessarily applying to women. As we might expect, subsequent studies have shown that causal hypothesis H applies to women as well, but there is in fact a difference between the sexes. Before the menopause, women have lower cholesterol levels than men of the same age eating a similar diet, and correspondingly lower rates of heart disease. Some researchers think that is part of the explanation of why women on average live longer than men.

17 Mike Laker, *Understanding Cholesterol*, Family Doctor Publications in association with the British Medical Association, 2012, p. 85.

18 Nina Teicholz, *The Big Fat Lie*, Scribe, London and Melbourne 2014, pp. 315-316.

A review in 2008 of all studies of the low-fat diet by the United Nation's Food and Agriculture Organization concluded that there is "no probable or convincing evidence" that a high level of fat in the diet causes heart disease …[19]

An unwary reader might suppose that this contradicts Ancel Keys' ideas, but such is not the case. Ancel Keys not only never claimed that a high level of fat in the diet caused heart disease, but he also pointed out that the cohort in his seven countries' study which had the lowest level of heart disease had the highest percentage of dietary calories in the form of fat. This was the cohort from Crete, 40% of whose dietary calories came from fat[20]. The cohort which had the highest level of coronary heart disease was that from East Finland, 39% of whose dietary calories came from fat. What is crucial is not the intake of fat in general, but the intake of *saturated fat*. The Cretans' fat intake came mainly from olive oil, which is monounsaturated fat and does not raise LDL level in the blood. The Cretans' intake of saturated fat was only 8%. By contrast, the fat intake of the East Finland cohort came mainly from butter, cheese, and meat, and their intake of saturated fat was 22% – the highest of any of the cohorts. The East Finland cohort also had the highest blood cholesterol level, and the highest rate of CHD. The important point is that Ancel Keys did not advocate a low-fat diet, but a diet low in saturated fat.

There are also some confusions concerning the Atkins diet which Teicholz discusses from p. 287 on. This diet consists mainly of animal foods such as meat, cheese and eggs, while the intake of bread and other carbohydrates is severely restricted. It turns out that this diet is very effective in achieving weight loss, the reason being that after a few pork chops, the desire for more pork chops declines. So, eating too much on a meat-centred diet is nearly impossible[21]. The effectiveness of the Atkins diet in producing weight loss might seem to contradict Keys' ideas, but really it does not. Keys stresses that it is possible to be slim and fit on a diet which is high in saturated fat and gives the East Finland cohort as an example of this. The problem is that someone can be slim and fit and yet be developing atherosclerotic plaques in their arteries. Thus, the criticism of the Atkins diet, which would be made by those supporting the ideas of Ancel Keys and colleagues is that, although it may be effective in reducing weight, it is likely at the same time to increase LDL level in the blood, and so increase the risk of CHD and other atherosclerotic events. Teicholz discusses an Israeli study conducted in 2008[22]. This compared the effects of three different diets, one of which was assigned to each member of a group of 322 moderately obese middle-aged people, mostly

[19] Ivi, p. 172.
[20] A. Keys (ed.), *Coronary Heart Disease*, p. 168.
[21] N. Teicholz, *The Big Fat Lie*, cit., p. 293.
[22] Ivi, pp. 213-214.

men. One of these diets was the Atkins diet, while another was the Mediterranean diet. Those on the Mediterranean diet lost 10 pounds, while those on the Atkins diet lost 12 pounds. Teicholz comments: "Only LDL-cholesterol looked better for Mediterranean dieters"[23]. In other words, the result is just what Ancel Keys et al. would have predicted. The Atkins dieters lost more weight, but their LDL cholesterol level was higher than that of the Mediterranean dieters. As regards the low-fat and Atkins diets, Teicholz may appear to contradict Ancel Keys' ideas, but does not really do so. In other passages of her book, however, she does indeed put forward claims which contradict those of Keys.

> Eat butter; drink milk whole, and feed it to the whole family. Stock up on creamy cheeses, offal, and sausage, and yes, bacon. None of these foods have been demonstrated to cause obesity, diabetes, or heart disease[24].

Even this passage does not engage very directly with Ancel Keys. Keys writes little about obesity (which was not a problem in his day), or diabetes, and does not put forward any hypotheses about the causes of obesity or diabetes. Moreover, he is careful to point out that there are several different types of heart disease, of which coronary heart disease is only one, albeit the largest category. Still, the passage from Teicholz can be taken as contradicting Keys' claim that a diet high in saturated fat causes coronary heart disease. Why therefore does Teicholz reject the evidence which Keys puts forward for his claim?

Teicholz begins by casting doubt on the accuracy of the data which Keys and his colleagues collected in the seven countries study. They took samples of the diet of each of the cohorts at different seasons and averaged the results. However, in the case of Crete one of the three samples was in Lent, when meat and other animal products were forbidden foods[25]. This sample would therefore have underestimated the amount of saturated fat consumed by the Cretans. Keys mentions this point, but thinks it is not a very serious one since, so he claims, Lent was not very strictly observed in Crete. Of course, obtaining accurate figures on diet is difficult, and problems like that of Lent may introduce some errors. However, given the careful procedures followed by Keys and his fellow researchers, these errors are unlikely to have distorted the general picture to any great degree. Teicholz now goes on to say:

> […] there was also a huge structural limitation to the Seven Countries study; it was an epidemiological investigation and therefore could show only an association, not causation[26].

[23] Ivi, p. 214.
[24] Ivi, p. 335.
[25] Ivi, p. 40.
[26] Ivi, p. 42.

It would be more accurate to speak here of an "observational epidemiological investigation", but with this proviso, what Teicholz says here is quite correct. The correlations found by Keys may not be causal in character because of confounders. This problem has been discussed earlier in the present paper. Teichholz suggests that there may be an alternative explanation, namely sugar. Despite her earlier scepticism about the accuracy of the data produced by the seven countries study, she uses some of this data to argue for this alternative explanation.

> In 1999, when the Seven Countries study's lead Italian researcher, Alessandro Menotti, went back twenty-five years later and looked at data from the study's 12,770 subjects, he noticed an interesting fact: the category of foods that best correlated with coronary mortality was sweets. By "sweets," he meant sugar products and pastries, which had a correlation coefficient with coronary mortality of 0.821…[27]

Here we do have a striking correlation. So, the next step of course is to consider whether this is causal in character, or whether it is confounded by some other variable. Unfortunately, however, Teicholz does not take this step, and nowhere in her book does she consider whether there might be confounders of this correlation of 0.821. I will now undertake this task which she has omitted.

Might this correlation be at least partially confounded by saturated fat? "Sweets" include pastries, and these usually contain a lot of saturated fat. So at least part of the correlation between sweets and coronary mortality might be explained by the intake of saturated fat consequent on eating many types of sweet. It would be better therefore to consider the correlation between sugar intake and coronary mortality. Fortunately, Menotti and colleagues provide also the correlation between sugar and coronary heart disease, which was 0.600[28]. Although lower than 0.821, this is still positive and quite high which could indicate a causal connection, but once again it could be confounded by saturated fat intake because sugar consumption was higher in countries like the USA and Northern Europe where consumption of saturated fat was also higher. Menotti and colleagues argue that confounding of this type may have taken place in another case. There is one curious feature of their results: bread (-0.001) and cereals (-0.305) are both negatively correlated with coronary mortality, but potatoes (0.464) are positively correlated[29]. This difference is surprising since potatoes seem quite similar from a dietary point of view to bread and cereals. Menotti and colleagues claim this about the matter:

[27] *Ibidem.*

[28] Alessandro Menotti, Daan Kromhout, Henry Blackburn, Flaminio Fidanza, Ratko Buzina, and Aulikki Nissinen, *Food intake patterns and 25-year mortality from coronary heart disease: Cross-cultural correlations in the Seven Countries Study*, "European Journal of Epidemiology", 15, 1999, p. 509.

[29] *Ibidem.*

An exception is the consumption of potatoes which is highly positively correlated (univariately) to CHD, likely because their highest consumption is in Finland and The Netherlands where potatoes are accompanied by the highest intake of animal and mainly dairy products[30].

It is thus possible that the positive correlation between sugar intake and coronary mortality is confounded by saturated fat intake. The objection might, however, be raised that the situation could be the other way round, that is to say that the correlations between saturated fat intake and coronary heart disease are confounded by sugar. This situation shows the advantage of applying not just the first, but all three of the strategies for dealing with confounding which were described above. The use of randomization provides evidence against the existence of confounders not explicitly considered, and hence against sugar as a possible confounder. The mechanism linking high LDL levels in the blood to the formation of atherosclerotic plaques does not apply in the case of sugar, since sugar intake does not raise the level of LDL in the blood. So, the second and third strategies provide good evidence that sugar is not a confounder of the correlation between saturated fat and coronary heart disease. Teicholz does not consider sugar the sole cause of CHD. Her hypothesis is that it is sugar in conjunction with refined carbohydrates.

Recent scientific research and the historical record all lead to the conclusion that the consumption of refined carbohydrates leads to a higher risk of obesity, heart disease, and diabetes[31].

Unfortunately, as we have seen, the data in Menotti and colleagues show that consumption of bread and cereals is negatively correlated with coronary heart disease. This hardly lends much support to Teicholz' hypothesis. Teicholz might reply that much of the carbohydrates consumed by participants in the seven countries study was unrefined. This could be true of the Cretans, but certainly not of the Japanese who refined their rice in the period under consideration, and consumed a great deal of it, while having one of the lowest rates of coronary heart disease in the world. Teicholz is rather selective in the data she considers. While citing a correlation from Menotti and colleagues to support her case, she does not mention that the highest correlation on their list is between butter and coronary mortality at 0.887. Despite this, she urges her readers to "eat butter". Teicholz says: "Only carbohydrates have been shown, in clinical experiments, to be the likely principal cause of obesity, heart disease and diabetes"[32].

30 Ivi, p. 511.

31 N. Teicholz, *The Big Fat Lie*, cit., p. 335.

32 *Ibidem*.

This is contradicted by the clinical experiment of Dayton and colleagues which was described above. Here the two diets were identical as far as the intake of sugar and refined carbohydrates were concerned. Yet those who ate the diet with more saturated fat had higher rates of atherosclerosis and coronary heart disease.

We can now sum up as follows. Nina Teicholz, following Gary Taubes, does formulate a causal hypothesis which differs from that of Ancel Keys and colleagues. It is that sugar and refined carbohydrates, and not saturated fat, are the principal cause of coronary heart disease. However, the evidence she presents for this alternative hypothesis is not very convincing. It consists of some positive correlations, but she never considers whether these arise from confounding. In fact, they can all be explained quite plausibly as arising from confounders. Moreover, her selection of correlations to consider is very selective. She gives those, which support her case, but does not mention others, sometimes in the same paper, which go against her case. She claims that her hypothesis is the only one confirmed by clinical experiments, whereas at least one well-known clinical experiment gives strong evidence against it.

Perhaps for these reasons, Nina Teicholz' book has not found many supporters among medical researchers, and there is no sign of a scientific revolution beginning in the field of nutrition. Instead, we have a curious phenomenon, namely the development among journalists of a kind of alternative science based on a paradigm for nutrition different from that accepted by mainstream medical science. An example of this is Alice Hancock's 2019 article *Praise the lard*. Apparently, lard is becoming a fashionable food among celebrity chefs, and Hancock defends lard consumption against its critics: "And until the 1970s, lard was a regular part of the British diet. The average consumption was 55g per week in 1974 […] Today it is less than 5g. The trend is similar in the US"[33]. This would naturally seem a good thing for a follower of mainstream medical opinion on diet, but not for Hancock, who continues:

> Why did it all but vanish from our cupboards? Lard and lardo are victims of a fear of fat that started spreading in the 1950s. According to Nina Teicholz, who spent nine years investigating American dietary science for her book *The Big Fat Surprise*, it can be credited to one man: Ancel Benjamin Keys[34].

Hancock then summarises Teicholz's critique of Keys, concluding: "But the science is now being revised"[35]. The science is certainly being revised but by journalists rather than scientists. A journalist (Hancock) supports

33 Alice Hancock, *Praise the lard*, "Financial Times Magazine", 12/13, 2019, pp. 42-43.

34 *Ibidem*.

35 *Ibidem*.

her defence of lard by citing another journalist (Teicholz), just as a typical research scientist might cite the work of another research scientist. So, the media are producing not just alternative facts, but also alternative science, which could be called 'media science'.

Conclusion

My conclusion is that there is a science of healthy eating. This contains results which are very well confirmed by carefully collected observations and well-designed experiments. It gives recommendations as to what diets are most likely to improve the general level of health, and these recommendations would have a very beneficial effect if they were generally adopted. Unfortunately, there is also a media science which advocates views opposed to those accepted by mainstream science. This media science imitates superficially the practices of mainstream science, but a closer inspection reveals that its arguments are largely bogus. Nonetheless because of its wide circulation, media science almost certainly has a considerable influence and one that is harmful as far of the health of the population is concerned.

Medieval Optics, Scholasticism and Neutrality

Dominique Raynaud*

Abstract: If value judgments are a known source of scientific error, there are cases where the error comes from an *indirect lack of neutrality*, i.e. by simple adherence to a way of thinking, be it based on a thinking routine, an analogy (Bachelard), a presupposition (Holton), a model (Granger) or a paradigm (Kuhn). This article proposes to study the influence of scholasticism on medieval optics, and to show how it hindered the development of optics. By adhering to the idea of the primacy of argumentation over demonstration, Roger Bacon and John Pecham obtained poor results in optics, because, paradoxically enough, they aimed at reconstructing geometric optics without geometry.

Keywords: medieval optics, anatomy, geometry, scholasticism, value judgments.

In a narrow sense, scientific neutrality means that a theory does not make value judgments about its object of investigation: a scientific theory is concerned with 'being' and not with 'should be'[1]. The problem of neutrality is approached differently in philosophy of science, epistemology and history of science. In philosophy of science, the classical approach to the problem of neutrality is to find chains of arguments as to why science should be value-free or value-laden. Broadly speaking, this question divides the proponents of objectivity, scientific materialism and scientific realism[2], for whom scientific statements conform to the structure of reality, and their opponents who claim that reality is unknowable and that truth and objectivity are not

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1 Max Weber, *Die „Objektivität" sozialwissenschaftlicher und sozialpolitischer Erkenntnis*, "Archiv für Sozia wissenschaft und Sozialpolitik", 19, 1904, pp. 22-87. Id., *Der Sinn der „Wertfreiheit" der soziologischen und ökonomischen Wissenschaften*, "Logos", 7, 1917, pp. 40-88.

2 Karl R. Popper, *Objective Knowledge: An Evolutionary Approach*, Oxford 1972, Oxford University Press; Mario Bunge, *Scientific Materialism*, Dordrecht 1981, D. Reidel Publishing Co., French translation: *Matérialisme scientifique*, Paris: Syllepse, 2008; Rom Harré, *Varieties of Realism: A Rationale for the Natural Sciences*, Oxford 1986, B. Blackwell.

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universal notions. For instance, the proponents of historicist theories of rationality assert that a scientific theory has no validity outside a limited space-time[3]. Anti-realists consider that the purpose of science is to produce, not true descriptions of reality, but only models that fit empirical reality[4]. Social constructivists claim that the natural world plays a limited, in any, role in the production of scientific knowledge[5]. The division between the first group (say, the realists) and the second (the anti-realists) is reflected in the issue of values and neutrality. Realists have little choice but to affirm that science is value-free or to emphasize the epistemic values of science, such as objectivity[6]. In comparison, anti-realists can argue that science is value-laden at all stages of its production. This thesis, inaugurated by Richard Ruder[7], has now become the prevalent view of science[8]. It has even led to attempts to demonstrate that there is no gap between facts and values[9]. Other positions exist. For example, it has been argued that objectivity and neutrality do not operate at the same level, so that scientific objectivity can be maintained even in non-neutral contexts[10].

The debate is reflected differently in the history of science. By its very orientation, the history of science is not in a position to say whether science is value-based or not. Indeed, it is quite likely that many historians of science would reject the format of this philosophical question, resorting to abstract notions that are empirically unstable: Which science? Is it sure that all sciences react to values in the same way? Which values? Is it sure that all values produce the same effects? The history of science can nevertheless contribute to the debate on scientific neutrality by documenting historical episodes in which a given science has established a particular relationship with values. In the context of history of science, realism and anti-realism lead to different solutions. For realists, error is always a transient state in the pursuit of truth.

[3] Thomas S. Kuhn, *The Structure of Scientific Revolutions*, Chicago 1962, University Press, tr. it della II ed. *La struttura delle rivoluzioni scientifiche*, Torino 1979, Einaudi.

[4] Bas Van Fraassen, *The Scientific Image*, Oxford 1980, Oxford University Press.

[5] Harry M. Collins, *Stages in the Empirical Program of Relativism*, "Social Studies of Science", 11, 1981, pp. 3-10.

[6] Evandro Agazzi, *Scientific Objectivity and its Contexts*, Cham 2014, Springer International, pp. 419-421, 430-436.

[7] Richard Rudner, *The scientist qua scientist makes value judgments*, "Philosophy of Science", 20, 1953, pp. 1-6.

[8] Robert Proctor, *Value-Free Science? Purity and Power in Modern Knowledge*, Cambridge 1991, Harvard University Press; Harold Kincaid, John Dupré, Alison Wylie, *Value-Free Science? Ideals and Illusions*, Oxford 2007, Oxford University Press.

[9] Hilary Putnam, *The Collapse of the Fact/Value Dichotomy and Other Essays*, Cambridge 2002, Harvard University Press.

[10] Sandra Harding, *After the neutrality ideal: science, politics, and "strong objectivity"*, "Social Research", 59, 1992, pp. 567-587.

For anti-realists, the very notion of truth is irrelevant, and there is no point in invoking any boundary between truth and error. These very common solutions are not the only ones possible in the approach to scientific values and neutrality. An analysis of neutrality is indeed possible within the framework of realism, if one studies the influence exerted by any kind of values on scientific errors.

Insofar as values can induce scientific errors, it is necessary to specify in what sense the term error should be used. The history of errors is not necessarily overhanging and anachronistic. There is a more constructive way of appreciating errors. The historian of science can very well, by placing himself in a given space-time, determine what an author knew, what knowledge was available to him, and whether he could arrive at some result. What bears the name of error is then the *error relative to the situation in which the author was operating*. These errors are then referred to the cognitive, instrumental and material resources available to the author[11]. This way of proceeding has nothing to do with the sterile and anachronistic exercise of detecting errors in the absolute. When an error is detected, its origin and its consequences must be studied.

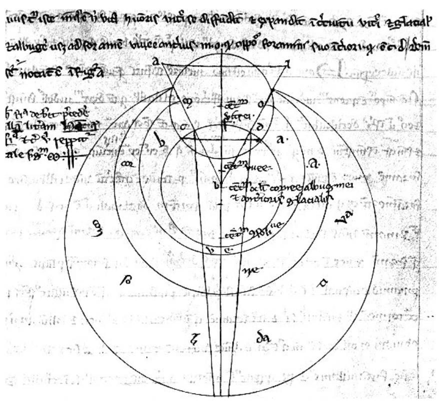
The scientific error can come from direct value judgments. It can also result from an *indirect lack of neutrality*, by adherence to a way of thinking, be it based on a thinking routine, an analogy (Bachelard), a presupposition (Holton), a model (Granger) or a paradigm (Kuhn). This article aims at studying an indirect lack of neutrality in the case of medieval optics, a field combining medicine (ocular anatomy) and physics (geometrical optics). It will document the influence of scholasticism on it, and will show how this method hindered the development of optics. Incidentally, this article will report on how some historians of science have hidden, or minimized, this influence, which also raises the question of the non-neutrality of historians vis-à-vis their subject of study. This article will not attempt to document the well-known problem that the use of the *Arguitur quod… In oppositum… Respondeo dicendum…* can lead to unnecessary raciocinations. For example, Biagio da Parma wrote fifteen pages to state a rule of refraction which was already known in antiquity (*Questiones super perspectiva communi* I, 9)[12]. Besides this problem, the scholastic method seems to have induced the following problems, which will be detailed in the article:

[11] Example: In the *Epistle on the light of the stars*, Ibn al-Haytham claims that the fixed stars and planets have their own light (Ibn Al-Haytham, Walīd 'Arafat and Henry J.J. Winter, The Light of the Stars: A Short Discourse by Ibn Al-Haytham, "The British Journal for the History of Science", 5(3), 1971, p. 284). This thesis is not a contextual error, because the phases of Venus only became observable with the telescope in 1609. Galileo then drew the conclusion that the planets reflected the light of the sun (Galileo Galilei, *Il saggiatore, nel quale con bilancia esquisita e giusta si ponderano le cose contenute nella libra astronomica e filosofica di Lotario Sarsi Sigensano*, Roma 1623, G. Mascardi, p. 217).

[12] Biagio da Parma/Blaise de Parme, *Questiones super perspectiva communi*, (ed. Graziella Federici Vescovini et al.), Paris 2009, J. Vrin, pp. 143-152.

1. The reference to Aristotle incites to multiply the couples of opposites (*Categories* VIII), which can lead to dichotomize what cannot be. This problem is at work in the study of ocular anatomy and pinhole images (Sections 1 and 2).
2. The belief in the power of the scholastic method can induce the rejection of competing methods, including geometric methods that are essential to solve optical problems. This issue arises in the study of pinhole and specular images (Sections 2 and 3).
3. The commentary of texts can lead to an abusive retention of old, false or contrary to experience arguments. This is particularly evident in the passages dealing with the burning spherical mirror (Section 4).

1. *Ocular Anatomy*



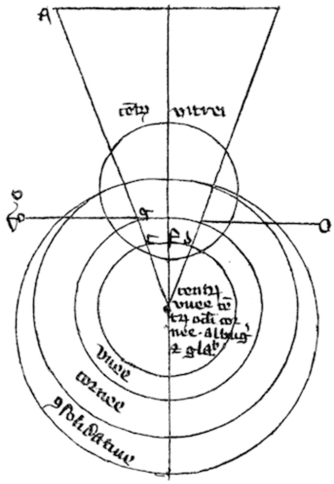


Fig. 1a-b: Bacon's diagram of the eye: London, BL, MS Royal 7 F VIII, fol. 54v (left); Prague, Univerzita Karlova, MS 1552 (right) (from Dominique Raynaud, *Eye Representation and Ocular Terminology from Antiquity to Helmholtz* (Hirschberg History of Ophthalmology, Vol. 16), Amsterdam 2020 Kugler/Wayenborgh, pp. 52-53. Bacon, *Perspectiva* I, 2-3.

Roger Bacon's description of ocular anatomy[13] is drawn from three sources: the *Canon* of Avicenna, al-Majūsī's *Pantegni* translated by Constantine the African, and Alhacen's *Optics*. According to Bacon, the eye has seven

[13] David C. Lindberg, *Roger Bacon and the Origins of Perspectiva in the Middle Ages. A Critical Edition and English Translation, with Introduction and Notes*, Oxford 1996, Oxford University Press, pp. 26-32; Id., *On the applicability of mathematics to nature: Roger Bacon and his predecessors*, "British Journal for the History of Science", 15, 1982, pp. 3-25.

parts: "The eye has therefore three tunics, or tissues, three humours, and a membrane which resembles a spider's web" (*Oculus igitur habet tres tunicas seu panniculos et tres humores et unam telam ad modum tele aranee*). The tunics are detailed in chapter 2. The first is the retina (*rete vel retina*), the second is twofold: the choroid behind, the cornea in front (*secundina… cornea*), the third is also twofold: the sclera behind, the conjunctiva in front (*sclyros… conjunctiva*). The humours are described in Chapter 3. Apart from the arachnoid, which is connected to the humours by convenience, the three humours of the eye are the aqueous humour (*humor albugineus*), the crystalline humour and the vitreous humour. Instead of using the usual terms for the crystalline (*humor crystallinus*) and the vitreous (*humor vitreus*), Bacon introduces a new terminology, which is not used by any of his sources:

> Inside [the arachnoid] there is a body which is called glacialis… But it has two parts. The inner part, located at the end of the [optic] nerve, resembles molten glass, which is why it is called vitreous humor. As for the anterior part, it is similar to ice, hail and crystal […] and, not having received a particular name from the author of the *Perspectiva*, it is called anterior humor glacialis. Others call it the crystalline, glacialis or hail humor, because it resembles these substances [*Et in hac [telam aranee] continetur corpus quod vocatur glaciale… Sed habet duas partes; una est interior apud extremitatem nervi, et est simile vitro liquato, propter quod vocatur humor vitreus; altera pars anterior est similis glaciei et grandini et cristallo… et vocatur anterius glacialis, non habens aliud nomen proprium apud auctorem Perspective. Sed vocatur apud alios humor cristallinus vel glacialis vel grandinosus, quia est similis eis*][14].

The diagram of the excellent manuscript London, British Library, MS Royal 7 F VIII, fol. 54v (Figure 1A) shows the pyramid of rays ABL entering the eye without undergoing refraction on the cornea (while crossing the cornea the ray is strongly deviated, passing from a medium of index $n = 1$ to a medium of index $n = 1.376$). The pyramid of rays is refracted only at the interface of the lens and the vitreous (while the ray is less deviated, passing from a medium of index $n = 1.376$ to a medium of index $n = 1.386$). The Prague manuscript, Univerzita Karlova, MS 1552, presents a more primitive figure, which omits this refraction (Figure 1B). The most noticeable deviation from the previous terminology is the introduction of two terms: *glacialis anterior* to designate the lens and *glacialis interior* to designate the vitreous (*al-ruṭūba al-jalīdiyya = humor glacialis*, *al-ruṭūba al-zujājiyya = humor vitreus*, used by Galen, Ḥunayn ibn Isḥāq, al-Majūsī, Avicenna and Alhacen). The homophony between *interior* and *anterior* and the propensity to reconstruct *interior/exterior* and *anterior/posterior* pairs would be a source of confusion for

[14] D.C. Lindberg, *Roger Bacon* cit., p. 30. See also Adam Bednarski, *Die anatomischen Augenbilder in den Handschriften des Roger Bacon, Johann Peckham und Witelo*, "Sudhoffs Archiv für Geschichte der Medizin", 24, 1931, pp. 60-78; D. Raynaud, *Eye Representation* cit., pp. 51-53.

later commentators, such as Henry of Langenstein, ca. 1375[15]. It is possible that this terminology was influenced by scholasticism. The medieval enumeration of the parts of the eye was already dependent on a search for symmetry between anterior and posterior parts. This is the origin of the couples: *uvea/retina, cornea/secundina, coniunctiva/sclirotica*. The replacement of the lens and the vitreous by the couple *anterior/interior glacialis* results from the same process being applied to the inner part of the eye.

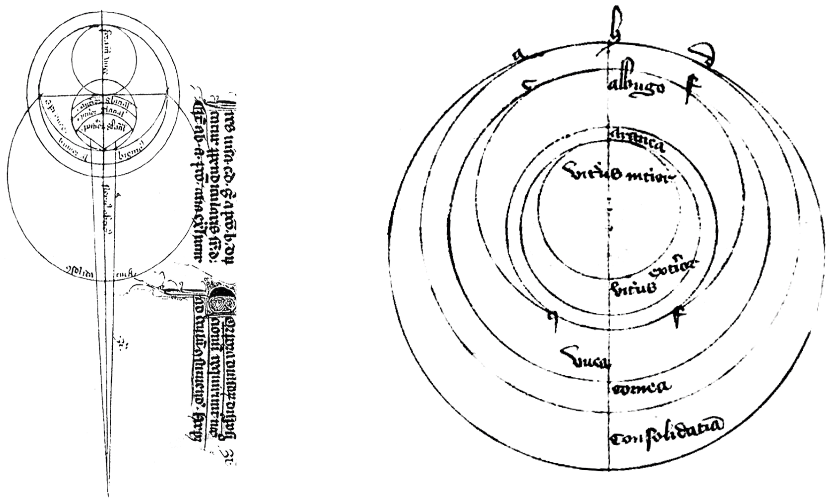


Fig. 2a-b: Pecham's diagram of the eye: Oxford, Bodleian, MS Ashmole 1522, fol. 153v (left); Paris, BnF, MS Latin 7368, fol. 90v (right) (from D. Raynaud, *Eye Representation*, pp. 57-58).

1.1. *Pecham,* Perspectiva communis *I, 31*

John Pecham gives two descriptions of the eye. The first one lists three humors and four tunics, according to an author whose name is not quoted: "And thus, according to the Physicist, the eye has three humors and four tunics" (*Et ita secundum istum Physicum oculus habet tres humores et 4 tunicas)*[16]. It could be Alhacen. The second description lists three humours and seven tunics, according to an unidentified *De elementis*, which is neither Galen's nor Isaac Israeli's[17]. In the first of the two descriptions, Pecham lists

15 D. Raynaud, *Eye Representation and Ocular Terminology from Antiquity to Helmholtz* (Hirschberg History of Ophthalmology, Vol. 16), Amsterdam: Kugler/Wayenborgh, p. 66.

16 D.C. Lindberg, *John Pecham and the Science of Optics: Perspectiva communis*, Madison 1970, University of Wisconsin Press, p. 114.

17 Ivi, p. 249, nn. 91-92.

the parts of the eye starting with the outermost consolidative (*consolidativa*). Then come the cornea (*cornea*) and the uvea (*uvea*). Similarly, the outermost humor is the albuginous humor (*humor albugineus*). The most interior is the humor glacialis (*humor glacialis*) and includes, as in Bacon, the crystalline lens and the vitreous:

> This humor is divided into two, because its anterior part cuts out a larger portion of the sphere, and the globe of the eye is concentric and equidistant from this anterior part of the eye; it also has a posterior part which is called the vitreous [...] these two are surrounded by a kind of delicate web which is called the spider [*Et hic humor dividitur un duos, habet enim anteriorem partem maioris spere portionem et toti oculo concentricam et equidistantem anterior parti visus; habet et posteriorem que vitrea dicitur... et hec due circumdantur tela quadam subtili que aranea appellatur*][18].

The distinction between *glacialis anterior* (lens) and *glacialis interior* (vitreous) does not follow any classical source on the anatomy of the eye: it is inherited from Bacon. One of the best manuscripts, Oxford, Bodleian Library, MS Ashmole 1522, fol. 153v, shows a carefully drawn but inaccurate figure of the eye (Figure 2A). The *glacialis* is divided into three parts: *anterior glacialis* (lens), *interior glacialis* (?) and *posterior glacialis* (vitreous). It is possible that this third part was introduced to respect the anterior/posterior symmetry. In other manuscripts, the diagram of the eye is degenerated, both graphically and terminologically. In the Paris manuscript, Latin 7368, fol. 90v (Figure 2B), it is the vitreous that is divided into two parts: *vitreus interior* (the lens) and *vitreus exterior* (the vitreous). These errors, which are common throughout the manuscript tradition[19], highlight a pitfall of the scholastic method. Bacon and Pecham never practiced dissection. Their knowledge of the eye is purely bookish, which explains why the linguistic division of parts of the eye does not coincide at all with the anatomical structures. This defect also guides the lack of realism in the representation of the eye. The crystalline lens is a convergent meniscus *or* a sphere; the vitreous is a biconvex lens *or* a meniscus; the optic nerve is a canal crossing the tunics of the eye until the glacialis, when it is not simply omitted (Figure 2AB). These errors have pervaded all subsequent literature. Traces of them can be found not only in Lorenzo Ghiberti, *Commentario terzo*, fol. 18v, who uses the expression "glacialis sphere" *spera gratialis!* following Bacon's *Perspectiva* I, 4, 2[20], but also in Leonardo da Vinci, who is reputed to have performed dissections of the eye. Leonardo does however draw a spherical lens in the center of the eye-

[18] D.C. Lindberg, *John Pecham and the Science of Optics* cit., p. 114.

[19] Adam Bednarski, *Die anatomischen Augenbilder in den Handschriften des Roger Bacon, Johann Peckham und Witelo*, "Sudhoffs Archiv für Geschichte der Medizin", 24, 1931, pp. 60-78; D. Raynaud, *Eye Representation* cit, pp. 57-60.

[20] Ed. Bergdolt 1988: 90; D. Raynaud, *Eye Representation* cit, p. 486.

ball, and refers to it under the old name of "glacialis humor" *omore graciale!* (Codex Atlanticus, fol. 599v)[21].

2. *Formation of pinhole images*

2.1. *Bacon,* De speculis comburentibus

This treatise on the burning mirror incorporates a side discussion of the shape of light passing through a polygonal aperture. The question has aroused some interest because the shape of the light spot may differ from that of the light source. Bacon begins by defining the shape of the beam of light by comparing the size of the aperture to that of the source:

> I say that, from the sun to the opening, the light can multiply only in three ways: according to straight parallel lines […] according to lines that intersect in the luminous body […] according to lines that intersect in the direction of the opening [*Dico ergo quod cum lux no possit multiplicari a sole ad foramen super lineas rectas nisi tribus modis, scilicet super rectas et equedistantes… super rectas quod concurrant in luminoso… super lineas concurrentes versus partem foraminis*][22].

This development is nothing but the transposition of the three types of shadows defined by ancient optics to the form that light takes behind the aperture.

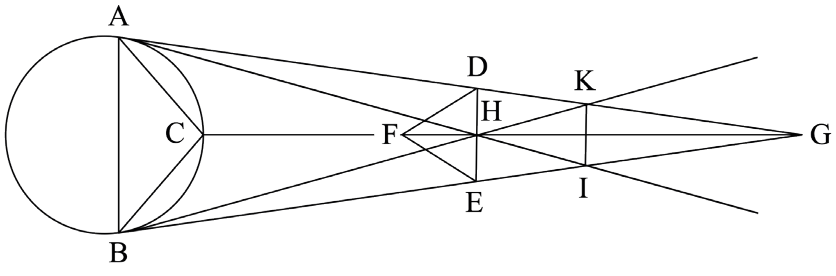


Fig. 3: Light passing through a triangular aperture according to Bacon (redrawn after Lindberg Roger Bacon's *Philosophy of Nature*, cit.).

Let ABC be the solar disk in which is drawn the triangle ABC. Let AD, BE, CF be straight lines by which light propagates to the triangular aperture

[21] D. Raynaud, *Leonardo, Optics, and Ophthalmology*, in F. Fiorani, A. Nova, eds., *Leonardo da Vinci and Optics*, Venezia 2013, Marsilio Editore, pp. 255-276.

[22] David C. Lindberg, *Roger Bacon's Philosophy of Nature. A critical edition of the 'De multiplication specierum' and 'De speculis comburentibus'*, South Bend 1983, St. Augustine's Press, pp. 304-308.

DEF. Straight lines AD, BE, CF intersect at point G behind the opening. In addition, let AH, CH and BH be straight lines passing through H, the center of the aperture (represented, by license, on the side DE). The rays AH and BH will cut BE and AD at points I and K (Figure 3). Bacon concludes:

It is thus evident, according to this figure, that the light of the round pyramid is occulted inside the triangle until IK, and that after IK, it occults the triangle, and that the triangular light gradually narrows from the opening until IK, preserving a triangular shape; it is manifest to the sight that there the maximum narrowing occurs and that then [the light] extends continuously in a round figure. [*Patet igitur secundum dictam figurationem quod lux pyramidis rotunde usque ad IK intra triangulam occultatur, et post sectionem IK triangulam occultat, et quod lux triangula a foramine usque ad IK continue angustatur in figura triangulari; et ibi est maxima angustatione visui manifesta, et continue ultra dilatatur in figura rotunda*][23].

A first failure of Bacon's reasoning is to consider that a qualitative change occurs in IK, IK separating the HK domain where the light is triangular from the KG domain where the light is circular. Bacon employs a dichotomy between the straight and the curved, the rectilinear propagation of light and the rotundity of the observed image; a dichotomy between the HK domain where light is supposed to be triangular and the KG domain where light is supposed to be circular. However, as Alhacen has shown in the *On the Shape of the Eclipse*, the shape of the light spot goes insensitively from angular to circular (ed. Raynaud 2016: 63-66). It is a continuous change, which is, for a given size of the aperture, a function of the focal distance to the screen. Any dichotomous reasoning is therefore an obstacle on the way to the solution. Only a continuous reasoning allows to solve the problem of pinhole images[24].

A second failure is that, although Bacon develops his argument by refer-

[23] Ivi, p. 312.

[24] Comments on Bacon's study of pinhole images are sometimes confusing: "[1] It should be noted, first of all, that Bacon does not associate the problem of pinhole images with the camera obscura… Bacon's work appeared several centuries before such applicability was first suggested. [2] Nor does Bacon relate the theory of pinhole images to observation of eclipses, as did Pseudo-Aristotle and Pseudo-Euclid before him, and John Pecham, William of St. Cloud, and John Kepler after him. [3] Bacon is simply interested in the geometrical modes in which light is propagated" (D.C. Lindberg, *John Pecham and the Science of Optics* cit., p. 220). The sentences are numbered for discussion. To point 1, it must be answered that Bacon's study does not enjoy any primacy: the first mathematical-experimental study of the darkroom was carried out by Alhacen, who gave the solution sought by Bacon (ed. Raynaud 2016). To point 2, it must be said that Roger Bacon explicitly associates the study of light passing through a narrow aperture with solar eclipses. A passage of *De speculis comburentibus* states that "the spherical part of the sun is totally luminous at all times, except during an eclipse, since it is then crescent-shaped [lit. razor-shaped]" (*portio solis sperica totaliter illuminta omni tempore preterquam in eclipsi, quoniam tunc novaculatur*) (ed. Lindberg 1983: 310). To point 3, it should be said that Bacon's exclusive interest in the geometry of the multiplication of light through the aperture is balanced by his poor abilities in geometry (see below).

ring to the letters of a geometric figure, he does not produce any geometric demonstration. The figure and the lettering illustrate a simple observation. The defect seems to result from Bacon's too strong dependence on the regime of sensible evidence (*secundum dictam figurationem… visui manifesta… sic videmus ad sensum…*). Having formulated the problem in unclear terms, Bacon will not find any solution, which he honestly admits: they are "problems which seem to me very difficult and of which I do not know the solutions […] I do not know at all what to say" (*rationes michi multum difficiles et quarum solutiones ignoro… Quid igitur sit hic dicendum penitus ignore)*[25]. This closes the argument.

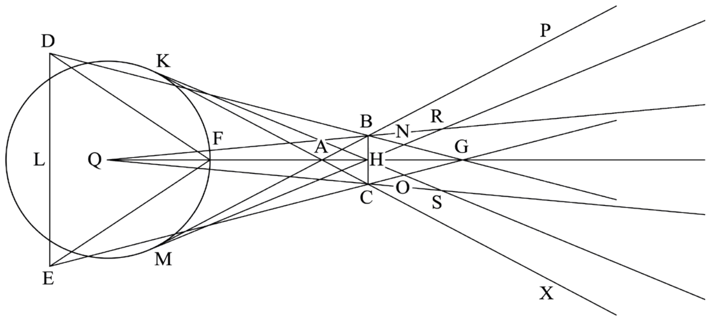


Figure 4: Light passing through a triangular aperture according to Pecham (redrawn after Lindberg, *John Pecham and the Science of Optics*, cit.).

2.2. *Pecham,* Perspectiva communis *I, 5*

Here is how Pecham describes the passage of sunlight through a small triangular aperture:

> The incidences of rays[26] passing through polygonal apertures of moderate size appear rounded on bodies placed in front [of the aperture], and are always wider the further [from the aperture, *NdA*] they are" [*Incidentias radiosas per angularia foramina transeuntes mediocris magnitudinis in obiectis corporalibus rotundari semperque fieri eo maiores quo remotiores*][27].

Let KLM be the solar disk, whose rays pass through the triangular opening ABC (Figure 4). The shape of the light spot that is seen just behind the aperture is not due to the source (which is the KLM circle). The rays reach

25 Ed. Lindberg 1983: 312-316.

26 *Incidentias radiosas*: "incidences of rays". I diverge from the English translation "incident rays". Pecham refers to the part of the ray in contact with the screen, not the whole ray.

27 D.C. Lindberg, *John Pecham and the Science of Optics* cit., pp. 66-82.

the point H, but not the point G, intersection of DB and EC. The rays coming from the center of the sun QB and QC intersect the marginal rays KC and MB at points R and S. Pecham claims that line RS is the limit from which the light spot is no longer triangular but circular, even if "the light acquires circularity gradually" (*Videmus lumen ipsum paulatim rotunditatem acquirere)*[28]. The shape of the light passing through a polygonal aperture would result from two causes: 1° the mixing of the rays passing through the aperture; 2° the spontaneous acquisition of circularity by the light. Pecham thus brings in a final cause: the Platonic idea that the sphere is a perfect form (*sperica figura… perfectissima… nobilissima*), which is taken from *Tractatus*: "As the light is very active, it acquires by itself the circular shape" *Quia igitur lux est maxime activa, acquirit sibi figuram circularem*[29]. Pecham has only two positive pieces of knowledge: 1. The size of the image depends on the focal length; 2. The image is inverted[30]. But his explanation is flawed, because it replaces the geometrical analysis with the finalistic argument that light naturally tends to a circular form (*habet naturalem inclinationem ad figuram circularem*). It is worth noting that Pecham does not vary the shape or size of the opening, which could lead to the solution. He only considers "triangular openings of moderate size" (*per angularia foramina mediocris magnitudinis*).

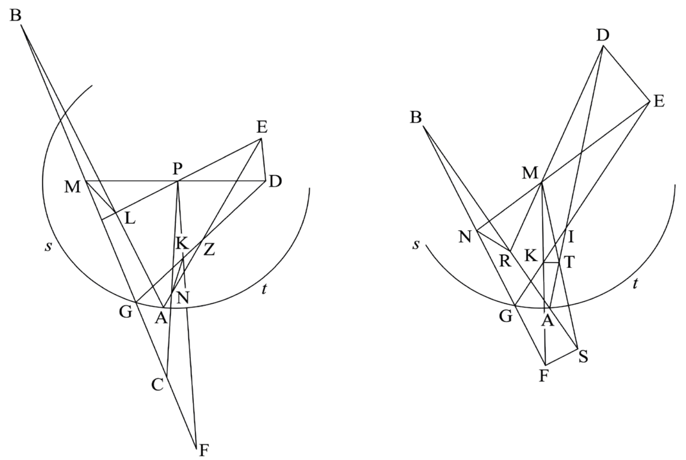


Figure 5a-b: Image location in a concave spherical mirror according to Bacon (left) and Euclid (right)[31].

[28] Ivi, p. 70.

[29] Id., *John Pecham. Tractatus de Perspectiva*, St. Bonaventure 1972, The Franciscan Institute, p. 34.

[30] Id., *John Pecham and the Science of Optics* cit., pp. 66, 70.

[31] Takahashi Ken'ichi, *The Medieval Latin Traditions of Euclid's Catoptrica*, A Critical Edition of *De speculis*, Fukuoka: Kyushu University Press; D.C. Lindberg, *Roger Bacon and the Origins of Perspectiva* cit.

3. *Image Formation in Concave Mirrors*

3.1. *Bacon,* Perspectiva III*, 1.4*

Here is how Bacon determines the location of the image reflected by the concave spherical mirror:

[1] For the visible object, ED, falls to the mirror by the rays intersecting at Z, for although reflection is made from every point, yet only those intersecting each other meet at such a distance in the eye. For the visible object KN, which is within the confluence of the rays, appears otherwise than it is; [2] because, in general, height and depth that are within the confluence of the rays appear inverted; but objects outside of the confluence appear erect, as they are *[1] Visibile enim ED cadit in speculum per radios concurrentes in Z, licet enim ab omni puncto fiat reflexio, tamen soli se intersecantes concurrunt a tanta distantia in oculo. Visibile enim KN, quo est intra radiorum confluentiam, apparet aliter quam est, [2] quia universaliter altitudo et profunditas, que sunt intra radiorum confluentiam, apparent everse; que autem extra apparent erecte, sicut sunt*[32].

Let GA be the concave spherical mirror of center P, two objects ED and KN, the eye being at point B. The two visual rays BAE and BGD intersect at point Z. Bacon claims that if the object ED is beyond (*extra*) the intersection Z, its image is straight (*apparet sicut est*); whereas if the object KN is within (*intra*) the intersection Z, its image is inverted (*KN… apparet aliter quam est*). Compared to the available knowledge, there are many errors:

1. The conclusion is wrong. While in Euclid's figure (Figure 5B) the object KN is seen straight, and the object ED is seen inverted; Bacon change the figure: *et ideo hoc pono figurationem*[33] (Figure 5AB) and draws the opposite conclusion. The geometric analysis reveals the error: the image is inverted (KN) when the rays intersect (EL, DM); it is straight (ED) when the rays do not intersect (KF, NC).
2. The proposition derives from Euclid's *Catoptrics*, which is known in three Latin versions. The comparison shows that Bacon used version II[34]. While the text refers to *Catoptrics* 11, the passage actually combines propositions 12 [passage 1] and 11 [passage 2][35]. As the two propositions study different situations, the combination is wrong.
3. The proposition is void of demonstration. Bacon describes the visual rays

[32] D.C. Lindberg, *Roger Bacon and the Origins of Perspectiva* cit., pp. 276-278.

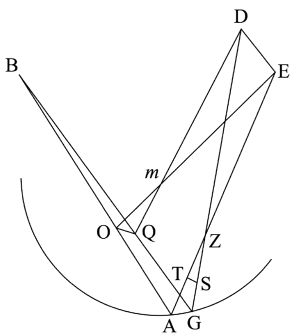
[33] Ivi, p. 278.

[34] A witness of which is the Oxford manuscript, Bodleian Library, MS Auct. F.5.28, fols. 24r-29r. Cfr. T. Ken'ichi, *The Medieval Latin* cit., p. 241, fig. 12.

[35] T. Ken'ichi, *The Medieval Latin* cit., p. 240, 238.

by letters, but he does not invoke either the law of reflection or the rule according to which the image lies at the intersection of the extended visual ray and the cathetus. Bacon *describes* the figure, but does not determine geometrically the position of the image.

This last observation shows that the use of geometric figures has not been properly assessed: "These figures are not purely decorative; they all support a serious and sustained geometric argument"[36]. In the spectrum of possible relations between text and figure, one can distinguish: a *decorative relation*, when the text is decorrelated from the figure; *illustrative*, when the text refers globally to a figure that serves as an example; *descriptive*, when the text describes parts of the figure without justifying them; and *demonstrative*, when the text refers to a figure whose features are justified by optic-geometric principles. With these definitions in mind, it is clear that Bacon's figures respond to descriptive, not demonstrative relationships. We can again read this feature as a preference of the author for scholastic argumentation over geometric demonstration.



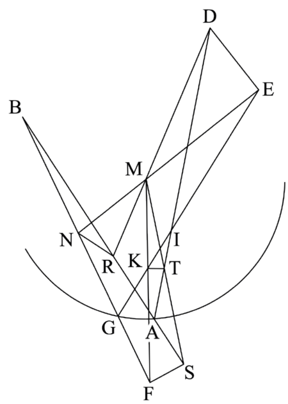


Fig. 6a-b: Image location in a concave spherical mirror according to Pecham (left) and Euclid (right) (redrawn after Lindberg, *John Pecham*, cit. and Takahashi, *The Medieval Latin*, cit.).

3.2. *Pecham,* Perspectiva communis II, 50

"In concave [spherical] mirrors, the object appears sometimes inverted and sometimes upside down" (*In speculis concavis res nunc conversas nunc eversas apparere)*[37] (Figure 6A). This statement is a copy of *Tractatus* 13,

[36] D.C. Lindberg, *Roger Bacon and the Origins of Perspectiva* cit., p. xliv.

[37] D.C. Lindberg, *John Pecham and the Science of Optics* cit., p. 204.

which itself was a verbatim copy of Bacon's *Perspectiva* III, 1.4: *Notandum quod diversimode reflectuntur a speculis concavis… sub humilioribus humiliora*[38], except a "saut du même au même" *BG, qui est… sed BG*. But Bacon's text being a paraphrase of Euclid, *Catoptrics* 11-12 (ed. Heiberg 1895: 304-306; Takahashi 1992: 240), the passage is, strictly speaking, a quotation of a quotation of a quotation. In spite of the rhetorical formula: "It is easily demonstrated from the *De speculis…* " (*Hoc ex libro De speculis faciliter demonstratur…*), the proposition is not demonstrated at all, and shows no facility. The difficulties are multiple:

1. The statement is misleading. Pecham distinguishes *conversas* and *eversas*, which are synonyms. This difficulty was perhaps already present in the Latin version.
2. The body of the text does not reproduce the difference between *conversas* and *eversas*, but joins the Euclidean text in separating objects that "appear inverted" (*apparent eversa*) and those that "appear as they are" (*apparent sicut sunt*). Unfortunately, Pecham is mistaken in his attribution: he claims that the magnitudes placed below the intersection (*intra confluentiam*) are inverted; that those placed beyond the intersection (*extra confluentiam*) are straight; Euclid says the opposite.
3. Based on Bacon, Pecham claims that Euclid is wrong (*credo quod erravit*). But it is Pecham who is wrong, as can be seen by comparing the texts and calculating the position of the images NR and FS of the segments DE and KT (Figure 6B).

The determination of the image involves the law of reflection, the cathetus (and thus the center of curvature of the mirror) and the extended visual ray. First anomaly: Pecham does not include either the cathetus or the center of curvature, which is not even represented on the figure: I add it to the point *m* (Figure 6A). Second anomaly: Pecham does not extend the visual rays BA, BG beyond the mirror surface. Since Pecham uses neither the center of curvature of the mirror, nor the cathetus, nor the extended visual rays, he cannot determine the images of DE and TS. Therefore, he cannot say anything at all about their relative position. This example points to the very limit of the process of trying to solve a question of geometrical optics without geometry. All these problems result from the fact that Pecham rejects the methods of geometrical optics – the very ones used by the authors he quotes – in favor of scholasticism, and that he gets confused by the pairs of opposites. I will elaborate further on Bacon and Pecham's relationship to geometry in the conclusion.

[38] Ivi, pp. 63-64.

4. *Study of the Burning Mirror*

4.1. *Bacon,* De speculis comburentibus

Bacon raises the problem of the burning mirror starting from the last proposition of Euclid's *Catoptrics*: "By means of concave mirrors facing the sun, one can set fire" (*Ex concavis speculis ad solem positis ignis accenditur*)[39]. Bacon surveys the different types of mirrors. He notes that the concentration of rays occurs only in concave mirrors, in different ways depending on whether the mirror is spherical or parabolic. For the spherical mirror, Bacon refers exclusively to the geometrical analysis of Euclid, *Catoptrics* 30[40]. Let EDZ be the sun, ABG the concave spherical mirror whose center of curvature is the point T. From point D on the sun, we draw the axis DB of the mirror passing through point T. Let us draw the ray DG which is reflected at the point K of the axis DB (Figure 7A). The point K is necessarily between B and T. Bacon repeats Euclid's demonstration: GT being the normal of the mirror at point G, the incident ray GD being on one side of the normal GT, the law of reflection imposes that the reflected ray GK is on the other side, on BT. In the original Greek version, the Euclidean proposition had two difficulties:

1. A not very explicit wording suggested that the point K was the focus of all the rays, which is false in the case of the spherical mirror;
2. The second part of the proposition redoubled the error by confusing the focus and the center of curvature of the mirror T.

The Latin version tried to overcome these difficulties by substituting the word *periferia*, which is absent from the Greek text, but did not correct the figure showing the whole of the reflected rays ending at K. Bacon clarifies the meaning of the Latin version:

> Similarly, it can be shown that all rays incident on the surface of the mirror, and equally distant from point B, are reflected to the same point on the line TB […] Similarly, it can be shown that the rays of no other circle compete at the same point [on the axis, *NdA*] [*Similiter potest ostendi quod omnes radii incidentes ad superficiem speculi et equaliter distantes a puncto B ad idem punctum in linea TB reflectuntur… Et eodem modo potest demonstrari quod nullius circulationis radii concurrunt ad eundem punctum ad quem concurrunt radii alterius*][41].

Bacon interprets the word *periferia* not as the meridian arc of the spherical mirror, but as the circumference traced on the mirror by a movement

[39] Id., *Roger Bacon's Philosophy of Nature* cit., p. 272.

[40] Heiberg, *Euclidis Optica, Opticorum recensio Theonis, Catoptrica*, ed. J.L. Heiberg, Leipzig 1893, Teubner, p. 340.

[41] D.C. Lindberg, *Roger Bacon's Philosophy of Nature* cit., pp. 272-274.

of revolution around the axis DB. The points A, G of this circle reflect the incident rays DA, DG to point K; the incident rays falling on another circle of the concave mirror are reflected to another point on the axis.

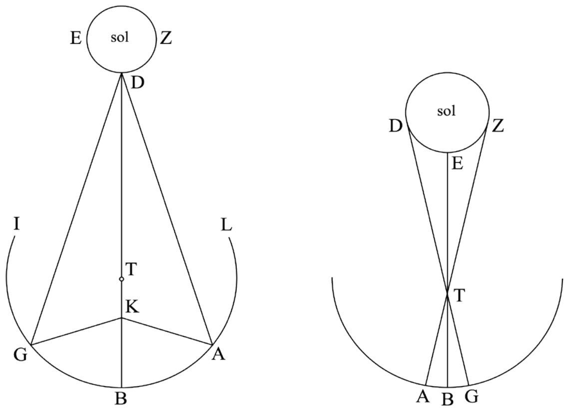


Fig. 7a-b: Focusing of the rays in the spherical burning mirror according to Bacon (redrawn after D.C. Lindberg, *Roger Bacon's Philosophy of Nature*, see footnote).

The second part of the proposition is illustrated by another figure (Figure 7B). Let DEZ be the sun, ABG the concave spherical mirror of center T. From point E let us draw the line ETB passing through T, which will be the axis of revolution of the mirror. From any point D of the sun, a ray DT passes through the center T and falls to the point G of the mirror surface.

And as all these rays fall at equal angles on the surface of the mirror… because they pass through the center, each one is reflected on itself. Therefore, all of them intersect at the center… That's why if oakum is placed at this point, it ignites. [*Et quia omnes isti radii cadunt ad angulos equales super superficiem speculi, quia… transeunt per centrum, ideo quilibet reflectetur in se ipsum. Omnes ergo concurrunt in centrum… quare in eis stupa posita accendetur*][42].

It is curious that after having cleared up the ambiguities of the first part of the proposition, Bacon reproduces the second part without any criticism. This part has two errors:

1. it makes the point T a focus, which does not exist in the case of the spherical mirror;
2. it equates the focus with the center of curvature of the mirror.

[42] D.C. Lindberg, *Roger Bacon's Philosophy of Nature. A Critical Edition, with English Translation, Introduction, and Notes, of De Multiplicatione specierum and De speculis comburentibus*. Oxford 1983, Clarendon Press, p. 274.

The reasons for this conservatism are obscure. They may result from the absence of experimentation as well as from the respect of classical authors. The last pages of *De speculis* are devoted to the study of the parabolic mirror, whose focal property has already been described by Diocles. Bacon's initial statement is promising:

But our effort must now be directed to the surface that reflects all the rays of the sun falling on the whole mirror at a single point, and this is the concave surface of a parabolic body, as demonstrated in the *Liber de speculis comburentibus* [*Sed presens intentio est de superficie que reflectit omnes axes solis incidentes ad eam totam in punctum unum, et hec est superficies concava corporis mukefi, secundum quod demonstratur in Libro de speculis comburentibus*][43].

The reference to Alhacen's *De speculis comburentibus* – which can be identified by the Arabic word *mukefi* "sufficient", designating the parabolic section – suggests that Bacon is about to expose the solution of the burning mirror by following this relevant source.

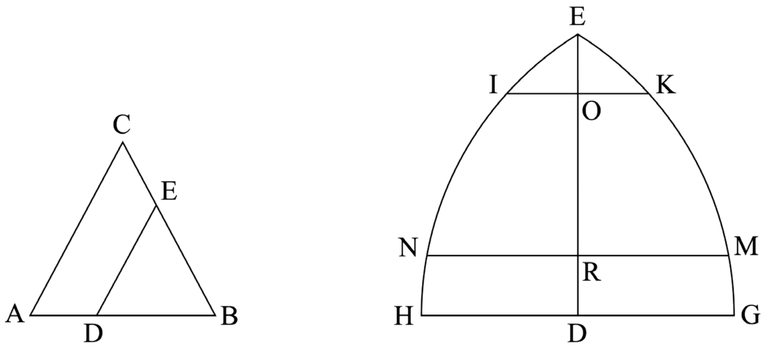


Fig. 8a-b: Side and front views of the parabolic mirror according to Bacon (redrawn after manuscripts)[44].

Let a cone be cut by the axial plane ABC. Suppose that the cone is rectangular at C, hence ∠ACB = ∟. From point D, the center of the base of the cone, draw DE a plane parallel to the generatrix CA perpendicular to the axial plane ABC (Figure 8A). The plane DE cuts the cone according to the desired shape:

[43] D.C. Lindberg, *Roger Bacon's Philosophy of Nature* cit., p. 336.

[44] The diagrams published by Lindberg (D.C. Lindberg, *Roger Bacon's Philosophy of Nature*, cit., p. 338) are not faithful to the handwritten figures. On the latter, the axial triangle ABC is not rectangular at C, and point D is not the center of the base AB as required by the text. The curve HNIEKMG is not a parabola. In accordance with medieval usage, the curve is rendered by two arcs of a circle EG and EH.

The common intersection of this plane surface and the cut half-pyramid is a parabolic section; it is contained by two straight lines and a sinuous line which is neither circular, nor straight, nor composed of segments. Thus, this section has approximately this shape [i.e. the curve HNIEKMG of axis ED, NdA]. [*Communis igitur sectio huius plane superficiei secantis et medietatis pyramidis secte es sectio mukefi; et continetur duabus lineis rectis et una tortuosa, que nec est circularis nec recta nec composita ex rectis. Et est huiusmodi sectio talis figure quasi*][45] (Figure 8B).

The most striking feature of this text is that Bacon does not provide any mathematical characterization of the parabola, neither by the abscissa-ordinate property (Apollonius of Perga), nor by any of the other known properties, such as the focus-direction property (Diocles), the focal property (Anthemius of Tralles), or the subtangent property (Alhacen). While Alhacen's *De speculis comburentibus* is a good mathematical text, all Bacon can say about the parabola is that it is "neither a circle, nor a straight line, nor a broken line". This shortcoming reflects the poor capacity of scholasticism to grapple with the problems of geometrical optics.

Conclusion

This article proposed to study a case of *indirect lack of neutrality* in medieval optics. Bacon and Pecham, adhering to the idea of the primacy of scholastic argumentation over geometrical demonstration, obtained poor results in optics, because, paradoxically enough, they aimed at reconstructing geometric optics without the help of geometry. This judgment is not overhanging, since it is based on the definition of contextual errors given in the introduction: Bacon and Pecham quote classical authors who use geometry to solve optical problems, and yet do not follow their methods. This is an informed rejection. Roger Bacon is sometimes seen as a mathematician who aimed at extending mathematical analysis to all fields, because of the title of Book IV: *On the utility of mathematics for physics*. The judgment that he could be a precursor of the 17th century scientists[46] is abusive.

1. Bacon wrote extensively on the application of mathematics to nature, but instead of analysing the epistemological foundations that could justify the application of mathematics to physics, he *rhetorically* defends the utility of mathematics.

[45] D.C. Lindberg, *Roger Bacon's Philosophy of Nature* cit., p. 338.

[46] The history of these judgments is traced by David Eugene Smith (*The place of Roger Bacon in the history of mathematics*, in A.G. Little (ed.), *Roger Bacon Essays*, Oxford 1914, Clarendon Press), and Nicholas W. Fisher, Sabetai Unguru, *Experimental science and mathematics in Roger Bacon's thought*, "Traditio", 27, 1971, pp. 353-378.

2. Another difficult point is that mathematical proofs, when they exist, are always combined with other registers of proof (mainly empirical and metaphysical). This casts doubt on the very argument of Book IV: if mathematics is the only science that brings certainty, one does not see what benefit there is in combining it with metaphysical arguments. Bacon therefore promotes mathematisation only in form, not in substance. This attitude seems to result from a reluctance to obliterate the ontological difference between mathematics and physics.
3. Historians are divided on Roger Bacon's mathematical contribution; Lindberg and Hackett praise him much.

> Bacon perfectly understood and successfully conveyed the geometrical principles of reflection […] His geometrical analysis of refractive phenomena was similarly successful […] It is clear from these examples that Bacon achieved significant success in fulfilling his project of mathematizing nature[47].

In the same vein, Hackett writes: "Roger Bacon was The Philosopher and Scientist who taught the Latin West to think about light and vision […] Bacon opened the eyes of Western thinkers to the need to do serious mathematical and physical-experimental work on these topics"[48]. His success was in fact very limited. In any case, Bacon's contribution is less demonstrative than the optical sources he had at his disposal and which he cites (mainly Euclid, Alkindi and Alhacen). By refusing to engage in the geometric analysis of problems, Bacon was regularly condemned to failure. In optics, the errors detected concerning the shape of the light behind a narrow aperture, the location of reflected images, or the focus of the spherical mirror, lead to express reservations about his mathematics. The same is true in mathematics, where Bacon's *Geometria speculativa* is in fact a mathematical treatise for beginners[49]. This is why well-informed historians of science have voiced criticisms of Bacon's alleged mathematical skills. For instance, Fisher and Unguru write: "Not only does Bacon commit elementary mathematical errors, but he also demonstrates his inability to understand the fundamental mathematical concepts"[50]. Similar harsh judgments can be found in the literature[51]. These elements finally explain

[47] D.C. Lindberg (ed.), *Roger Bacon and the Origins of Perspectiva* cit., pp. xlv, l.

[48] Jeremiah Hackett (ed.) *Roger Bacon and the Sciences. Commemorative Essays*, Leiden 1997, E.J. Brill, p. 6.

[49] A. George Molland, *Roger Bacon's Geometria Speculativa*, in M. Folkerts and J.P. Hogendijk (eds.), *Vestigia Mathematica. Studies in medieval and early modern mathematics in honour of H.L.L. Busard*, Amsterdam 1993, Rodopi, pp. 265-303.

[50] N.W. Fisher, S. Unguru, *Experimental science* cit., p. 354.

[51] D.E. Smith, *The place of Roger Bacon* cit, p. 182; A.G. Molland, *Roger Bacon's knowledge of mathematics*, in J. Hackett (ed.), *Roger Bacon and the Sciences. Commemorative Essays*, Leiden 1997, E.J. Brill, p. 168; A. Mark Smith, *From Sight to Light*, Chicago 2014, University of Chicago Press, p. 271.

how Bacon's scholastic project leads him to depart from the Greco-Arabic tradition in optics, by approaching the subject from an argumentative rather than a demonstrative angle. It is noteworthy that Bacon does not show the usefulness of mathematics by *mathematizing* natural phenomena, but by producing speculative arguments about the usefulness of mathematics, which is different.

John Pecham is playing in a slightly different configuration. The pedagogical orientation of the *Perspectiva communis* can explain a certain degree of simplification of knowledge – it has been called a *Perspectiva ad asinos*. However, this orientation cannot explain all the errors of the text – and for which Pecham's sources are not responsible. These errors stem from an orientation of thought that favors the scholastic method to the detriment of mathematics. As soon as he can, Pecham abandons geometrical demonstration for speculation. Occasionally, the same occurs in logic: he asserts the existence of causes, but does not establish causal relations, preferring to proceed by superposition of arguments. From this point of view, Pecham's treatise testifies to the difficulty of abandoning the axioms of thought of an intellectual tradition. Pecham discovered rather late Witelo's *Perspectiva*, whose model he wanted to copy. But he did not find a way to articulate these approaches. The project was doomed to failure: one cannot at the same time claim the Euclidean heritage and reject the use of mathematics. In the history of optics, Pecham's *Perspectiva communis* will probably remain the most striking example of a work that met with popularity (judging by the number of manuscripts and editions circulated) but whose intellectual project remains inscrutable, since the author is basically trying to develop a geometrical optics without the help of geometry. Bacon's and Pecham's optics have internal differences, but they share the same ambition: they want to apply to optics the scholastic argumentation based on pairs of opposites rather than mathematical reasoning. Finally, it should be noted that nothing in the historical analysis of these cases of *indirect lack of neutrality* requires abandoning realism. Indeed, all of Bacon and Pecham's shortcomings in optics concern knowledge that was accessible either to observation (neither the structure of the eye, nor the shape of light passing through a narrow aperture depends on social values) or to demonstration (the fact that the reflected image is straight or reversed, or that the rays do not focus at a single point in a spherical concave mirror is not conditioned by social values). These cases simply account for contextual errors that could have been avoided had Bacon and Pecham not departed from the geometric tradition that has nurtured optics since antiquity.

Social and Individual Features of Knowledge: The Non-Neutrality of Science in Personalized Medicine[1]

Giulia Gandolfi*

Abstract: In an age marked by rising conspiracy theories and populism, discussions around the non-neutrality of science often become reductive. This article aims to critically dissect the non-neutrality of science beyond mainstream narratives, offering an epistemic-political analysis rooted in three levels: the historical context of science, its truth validation, and the environment of its practice. The study identifies two dominant elements that underpin science's non-neutrality: its inherent sociality and the individualization of knowledge responsibility. The sociality of science is discussed in light of historic-epistemological insights from Georges Canguilhem and Ludwig Fleck, emphasizing science's value-laden nature. Conversely, the article spotlights the neo-liberal shift post the mid-20th century that places knowledge responsibility squarely on individual citizens, further exacerbating social disparities. As a case study, the piece delves into Personalized Medicine, illustrating how modern knowledge production mechanisms, especially Omic techniques, potentially alienate patients from participating in their own care, reinforcing societal disparities in healthcare accessibility. The overarching intent is to illuminate the intersections between science's non-neutrality and contemporary political-economic structures.

Keywords: non-neutrality, scientific production, political epistemology, personalized medicine, knowledge responsibility.

1. *Introduction*

Considering the non-neutrality of science seems almost obvious and taken for granted today. The increasing amount of conspiracy theories and popu-

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lism have brought the discourse of "controlled science" by the powers that be to the fore. Reflecting on the non-neutrality of science today risks being reduced to these discussions. Asking what it means that science is not neutral should instead be a critical discourse in the Kantian sense of the term, that is, one that analyzes the limits of its efficacy and application. In this sense, I intend to discuss the non-neutrality of science from a point of view different from critical theory, such as that of the Frankfurt School, or from a moral point of view. My goal is to propose an analysis of two elements (the sociality of science and the individualization of the responsibility of knowing) that in the capitalist era make science non-neutral, considering what their political implications are. I would define my interest as epistemic-political, where by political epistemology I mean an approach that develops on three levels:

– The origins (roots) of science: The perspective which acknowledges the historical context underpinnings of every scientific practice.
– Validity: This involves scrutinizing the criteria for truth validation within scientific practices.
– The function of scientific practice: This aspect explores the material and cultural environment in which scientific progress unfolds[2].

In the first section of my work, I will analyze the first characteristic that makes science non-neutral: its sociality. Considering science as a social product implies analyzing on the one hand how scientific formation has social roots, for this reason, I will consider the historical-epistemological analysis done by Georges Canguilhem and Ludwig Fleck, and on the other hand, considering how methods of scientific diffusion are socially determined and determine the type of knowledge production. In this sense, I consider science as a normative activity, that is, as a value-laden activity.

In the second section, I will consider what I have defined as the individualization of scientific knowledge. Starting from the second half of the 20th century, neo-liberal policies have led to investing the citizen with the responsibility to know and to orient themselves within a growing mass of information. This shift highlights and exacerbates existing social inequalities. As responsibility moves away from collective institutions like governments, it inevitably privileges those who already have the resources, knowledge, and capacities to navigate complex systems. Access to knowledge becomes an elitist element capable of producing social differences.

Reflecting on this, I will propose an example of the sociality of science and the individualization of the responsibility of knowledge in the third section. Here I will take the example of Personalized Medicine to discuss how the

[2] Pietro Daniel Omodeo, *Political Epistemology: The Problem of Ideology in Science Studies*, Springer, Dordrecht 2019.

production of knowledge based on Omic techniques distances the patient from the ability to be part of their own care, increasing already existing social differences in the possibility of accessing care. The aim is to show how the non-neutrality of science takes on characteristics connected to models of political and economic management today.

2. *Social normativity of science: essence and aim of scientific knowledge production*

Georges Canguilhem in 1962-1963 gave a lecture in Sorbonne entitled *Le statut social de la science moderne*[3], here, Canguilhem underscores the significance of viewing science as a "social product." Although he does not elucidate the exact meaning of social product in this lecture, we can gain insight by considering the views he expressed on the history of science in *Ideology and Rationality*[4]. We must recognize that scientific concepts possess a history and evolve over time, influenced by cultural, social, and historical contexts. Consequently, we should view the history of science as the examination of their historical development and transformations[5]. Moreover, if we recognize that rationality in science is not static but is constructed by social entities – meaning it is subject to historical and social changes – then we can view the history of science as the study of shifts in rational systems and the transformation of scientific concept applications from a social perspective. Reflecting on Canguilhem's lecture, it can be asserted that the entire history of science is intertwined with society, as all actions, including knowledge production, are social constructs. Science primarily emerges from various technical, or practical, problems which are later popularized and conceptualized. This popularization and conceptualization of science do not occur instantaneously.

The process of knowledge production within the scientific domain often begins with the unique insights of an individual scientist or a group of scientists. Following Canguilhem's perspective, which views scientific knowledge as subsequent to technological activity, we recognize that the drive to discover something new often arises from the need to bridge the gap between technical success and the absence of a fitting theory. A foundational body of knowledge is not necessarily the precursor to a new theory. In fact, it is often the absence of knowledge that compels a scientist to subject their beliefs and suppositions to empirical evidence, particularly of a technological

[3] George Canguilhem, *Le statut social de la science*, GC Centre d'Archives en Philosophie, Histoire et Édition des Sciences, Paris 1962.

[4] Id., *Ideology and Rationality in the History of the Life Sciences*, MIT Press, Chicago 1988.

[5] Ivi, pp. 1-5.

nature. Reflecting on the role of technological instruments, Canguilhem invokes Gaston Bachelard's idea of "phenotechnique". He sees the progression of science as being led by praxis, suggesting that science can only theorize after achieving a technological result. In this context, using an instrument can lead to the formulation of a theory that corresponds either to the operation of the instrument or to the results generated by the instrument itself. An illustrative example of this is the creation of maps featuring longitudes and latitudes in the 18th century. The determination of longitudes necessitated more precise calculations in astronomical science. The process of determining longitude is intricately linked to measuring lunar distances and is facilitated by the use of precise clocks and marine watches. In essence, it is underpinned by technological advancements[6]. In the 18th century, the need for improved longitude calculations converged with the economic interests of European states in colonial domain. This suggests that while new concepts can lead to novel technologies, the diffusion and adoption of specific technological instruments are often influenced by prevailing social and historical practices, which in turn serve broader socio-political goals, such as economic interests in colonial markets. The relationship between technology and theory is not linear. A theory can be contradicted by empirical evidence, and such discrepancies drive the quest for greater accuracy and refinement in theoretical frameworks. When these refined theories gain collective acceptance, they can pave the way for new technological applications.

In the process of theory development, two elements come into play: the individual scientist who engages in research, and the material environment in which they operate. However, turning to Ludwig Fleck introduces a third, pivotal element. According to Fleck, there exists a "thought collective", which he defines as a community of individuals bound together by a shared style of thinking, encompassing common ideas, values, and perceptions of the world. This communal mindset is implying a "thought style," and it profoundly shapes the way members of the collective perceive, evaluate, and generate knowledge[7]. In essence, a "thought collective" refers to a community of individuals who share, exchange, and are mutually influenced by a common set of ideas, all the while operating within a consistent style of thought. This community isn't merely the aggregate of its individual members; the interactions within the group give rise to a collective consciousness that can, often subliminally, influence each participant. Fleck underscores the significance of studying intellectual communities as foundational to epistemology. Analyzing cognitive processes solely from the perspective of individual actions

[6] G. Canguilhem, *Le statut social de la science*, cit., pp. 38-39.

[7] Ludwig Fleck, *Genesis and development of a scientific fact,* The University of Chicago Press, Chicago 1979.

would lead to an incomplete understanding of the broader collective cognitive process. Every piece of knowledge is socially conditioned, challenging the traditional notion that science rests solely on the subject-object dichotomy. Instead, an additional element emerges: the prevailing state of knowledge at any given moment[8]. While it is true that an idea may originate from an individual scientist, two key points stand out:

1. The genesis of an idea is deeply rooted within a social context, specifically within the thought collective. In this sense, the collective serves as a vital precondition for the idea's development.
2. Ideas are dynamic. As they flow from one individual to another within the community, they undergo transformations, and they can even return to their originator in a changed form. This dynamic is central to the evolution of a scientific concept. A concept might be introduced to the community in a form that isn't fully rationalized or objective – essentially, in a non-scientific state. However, after circulating within the community, it can (or can't) attain the rigor and objectivity necessary to fit within the realm of scientific knowledge.

Science's lack of neutrality stems from its inherent social characteristics. It is value-laden and is susceptible to change over time. We can argue that scientific production is both influenced by and shaped by the social realm. A concept emerges within the collective state of knowledge at a particular moment, and it evolves in response to that community's prevailing cognitive norms. In *Nécessité de la diffusion scientifique* Canguilhem states: "As the relationships between science and society have become such that the question of knowledge diffusion, which one could believe was outdated, arises again"[9]. Canguilhem is particularly intrigued by the role of the individual in the circulation of knowledge. The circulation of knowledge itself plays a part in its non-neutrality, as the transmission of scientific knowledge can shift based on the values applied to it. While Fleck emphasizes the idea of a collective cognition, Canguilhem takes a slightly different stance. By "social status of science", he refers both to institutions like academies or universities, but also to public opinion – especially the esteem with which experts are regarded by the general population. Therefore, Canguilhem emphasizes the "environment" of knowledge production, in tandem with the vernacular component, as crucial for groundbreaking discoveries. In his view, it isn't solely the genius of an individual that propels science forward; rather, it's a "social soul" that creates the conditions for scientific progress, as interpreted

[8] Ivi, pp. 20-53.

[9] G. Canguilhem, *Nécessité de la diffusion scientifique*, "Revue de l'enseignement supérieur", 3, 1961, p. 14, my translation.

and understood by the "scientific city". For Canguilhem, if knowledge isn't rooted in a singular subject, then the challenge arises in understanding the dissemination of science. Given that science isn't a solitary endeavor, its application isn't monolithic either. The diffusion of science, which encompasses all societal strata, is, in Canguilhem's view, pivotal to the progression of science itself, owing to its universal mission. This brings us back to the central question of knowledge dissemination.

According to Canguilhem, it falls upon scholars and scientists to contemplate whether the spread of knowledge, when aligned with the objectivity of truth, is an unconditional imperative, irrespective of external factors. They must consider whether communicating research findings to a global scientific community, which transcends political boundaries, is an absolute duty or merely conditional[10]. When viewing knowledge as a product of social processes, we must also acknowledge that science and technology aren't at odds with each other. Instead, they exist in a dialectical and reciprocal relationship, even as they retain their distinct identities. Technology (and technique) sets the stage for scientific exploration, while science provides the societal foundation in which practices, techniques, and technologies evolve. The interplay between science and technique is not static. It is characterized by ongoing shifts, disparities, and even ruptures. Significant imbalances between the two can sometimes lead to detrimental outcomes.

As shown in *La Nécessité de la diffusion scientifique* by Canguilhem, when science is subordinated to technology, it undergoes a transformative process: it relinquishes its inherent autonomy and normative power, merely serving to bolster and rationalize technological advancement. This naturally raises the question: what ensures a harmonious relationship between technology and science? Canguilhem's response, elaborated in the subsequent part of his text, seems rooted in the socio-political realm and the way knowledge production is orchestrated. Managing the production of knowledge is intrinsically a socio-political endeavor, which demands an active engagement with the normative values of both science and technology. That is to say to recognize the values creativity of science. In relation to this, Canguilhem articulates: "Science is popularized by its effects before being vulgarized in its reasons and principles"[11]. The dissemination of scientific knowledge presents two primary challenges. The first pertains to the manner – specifically, the means – through which science is disseminated. The second relates to the intent behind this dissemination, or in other words, the purpose of popularizing a specific body of knowledge. With respect to the means, Canguilhem highlights the use of scientific manuals as an example. Such manuals often

[10] *Ibidem*.
[11] Ivi, p. 9.

present a linear account of scientific events or simply collate scientific data, phenomena, or facts, devoid of the context of their application or genesis. Thomas Kuhn similarly critiques the approach of these manuals in *The structure of scientific revolution*[12].

Similar to Canguilhem's perspective, Kuhn asserts that manuals and textbooks often overlook the social and historical shifts in the production of knowledge. Hence "by disguising such changes, the textbook tendency to make the development of science linear" and continues Kuhn, this "hides a process that lies at the heart of the most significant episodes of scientific development"[13]. This leads to a prevalent inclination to portray the history of science as linear or cumulative, an inclination that can influence even scientists reflecting on their own research. For Canguilhem, the dissemination and diffusion of knowledge isn't simply about relaying a linear and objective understanding. Instead, it should encapsulate the normative, unique, and circumstantial nature inherent to all scientific narratives[14]. Canguilhem believes that this perspective is deeply tied to the purpose of disseminating knowledge. A perceived linearity in science mirrors a linear interpretation of history, suggesting the ongoing evolution of a single, unified process. Such a singular narrative stifles the possibility of significant scientific breakthroughs and socio-political transformations. A purely objective and linear account may accommodate reforms, but it precludes the potential for revolutions. The key point I would like to stress here is that framing science as a linear narrative neglects its intrinsic normative aspect. By "normative", I refer to the capacity of science to define meanings. To grasp this concept of normative science, Canguilhem's perspective is enlightening. He posits that both science and technology are vital activities; they are avenues through which living entities navigate and establish norms in relation to their environment. Canguilhem delves deeper into this interplay in *Machine and Organism*, where he elucidates that science and technology must be considered as two types of activity "each of which borrows from the other sometimes its solutions, sometimes its problems"[15]. Since science is a vital activity[16], it operates on an axiological basis, just as life does. That is to say, that science is fundamentally driven by judgment. In 1945, Canguilhem discussed this in a series of lectures on *Nature et valeur du concept*[17] held at the University of Clermont-Ferrand.

12 Thomas Kuhn, *The Structure of Scientific Revolutions*, University of Chicago Press, Chicago 1996 (1962).

13 Ivi, p. 165.

14 G. Canguilhem, *Nécessité de la diffusion scientifique*, cit., pp. 15-16.

15 G. Canguilhem, *Knowledge of life,* Fordham University Press, New York 2008.

16 *Ibidem*.

17 Id., *Nature et valeur du concept*, GC Centre d'Archives en Philosophie, Histoire et Édition des Sciences, Paris 1945.

In his lecture, Canguilhem delves into the normative function of science, viewing it as a process of producing judgments. He posits that knowledge entails imposing values and preferences onto the environment. Just as with any vital activity, the act of knowing embodies the realization of a choice[18]. Knowledge, as a value laden activity, is creation of truth value. This means that certain values are designated as truth, while others are excluded, being deemed as false. However, like all axiological activities, knowledge doesn't nullify what it excludes. The values not recognized as truth still retain their value. As Canguilhem notes scientific knowledge "is not the destruction or the annulment of a certain order of the perceived qualities" but, "it would be the subordination of such order of qualities to another order"[19].

Building upon Canguilhem's perspective, it is evident that living beings create an order based on judgment, yet only humans possess the abstraction capacity derived from rationalization. This positions science as an extension of a value-laden, experimental activity, rationalizing normative actions. Thus, science is inherently non-neutral, both from within and in its interaction with the external environment. Internally, this non-neutrality stems from the essence of scientific knowledge as a normative, value-driven endeavor, embedded in the creation of norms, values, and judgments. Externally, this non-neutrality emerges from the interplay between this normative activity and its environment, a composite of both material and theoretical dimensions. Therefore, the methods and motives behind the dissemination of scientific knowledge play a crucial role in understanding the external non-neutrality of science. Scientific activity, inherently value-laden, is externally influenced by its modes of dissemination and popularization. The nuances of this relationship are not solely dependent on the individuals producing the knowledge or the society receiving and circulating it. Instead, there's an integral third component: the thought collective. This concept serves as the backdrop against which knowledge is possible, being rooted in both historical and social contexts.

3. *The individualization of knowledge: the concept of risk from 1920s to 2000s*

In the preceding section, I posited that the social determination of science is the primary feature implicating its non-neutrality. Closely linked to this is the second feature that, in my view, augments the non-neutrality of science: the individualization of knowledge responsibility. To illustrate this, I will

18 Id., *The Normal and the Pathological,* Zone Book, New York 1989, p. 122.
19 Id., *Nature et valeur du concept*, cit., p. 84, my translation.

highlight the evolution of responsibility individualization within the health domain. This process of individualization is applicable across all domains of scientific knowledge production, even though each discipline might have traversed varied trajectories to arrive at two common junctures: 1) a pronounced disparity between the expert and the non-expert (or citizen), and 2) the onus on individual citizens to discern which theory, proposition, or expert stance to embrace. Tightly intertwined with this is the shifting control of knowledge production, I will consider for instance healthcare. This control is no longer solely in the hands of the State; rather, it has increasingly been commandeered by private corporations which engage actively in public discourse to influence individual citizens' opinions. Consequently, I will explore the pivotal events that have led to the utilization of medicine in the management of public health. Furthermore, I contend that the interplay between medicine and biopolitics is deeply embedded within capitalist and neoliberal economic frameworks. Indeed, a holistic understanding of how biopolitical mechanisms shaped knowledge production in the 20th century emerges only when we consider both medical knowledge and tangible outcomes – discoveries, medications, treatments, and therapeutic practices – in conjunction with capitalist production and demand policies. This observation, however, does not insinuate that medical practices can only thrive or be effective within a capitalist economic structure. Instead, by understanding the strategies employed within capitalist paradigms, we pave the way for envisioning alternatives. In this perspective I consider the non-neutrality of science. I consider the intertwining of medicine and biopolitics as intricately linked to the entrepreneurial strategies of both states and private enterprises in the post-war era. The production of scientific knowledge in the 20th century was inextricably tied to technological advancements, particularly given that a predominant application of science for over half the century pertained to armament production. Likewise, throughout the entirety of the 20th century, knowledge production was tethered to the economic realm. It's crucial to recognize the pivotal shift that took place during the post-war years, as this transition – from state-centric control to dominion by private entities – signaled a profound transformation in knowledge production. Prior to the outbreak of the First World War, scientific and economic developments were steered by individual states but remained unimpeded by national boundaries. However, with the advent of the war, all states, both capitalist and non-capitalist (e.g., the USSR), came to view scientific research as a crucial tool for assurance, preparation, and defense against potential future conflicts. As noted by David Edgerton, this context, combined with the economic turmoil of the 1930s, compelled nation-states to strive for autarky[20].

[20] David Edgerton, *L'Etat entrepreneur de science,* in Christoph Bonneuil e Dominique Pestre (eds.), *Histoire des Sciences et des Savoirs Volume III: le siècle des technosciences 1914 – 2014*, Le

In essence, during this era, the State largely dominated the realm of knowledge production. The drives of imperialism and governmental incentives in research, particularly in the medical and industrial sectors, were manifestations of this trend. From the onset of the First World War until the Cold War, the State, especially its military arm, emerged as the principal financier of scientific research. This held true even in nations with pronounced neoliberal and capitalist orientations[21]. Such sponsorship arose from each state's desire to devise national strategies to foster the indigenous development of technologies essential for military objectives. However, it wasn't until the 1960s and 1970s that this national stronghold began to diminish, chiefly due to the colossal investments required to innovate and manufacture advanced weaponry and military technologies. These technologies carried a hefty price tag, often beyond the reach of a single State's budget for technological production. As a result, consortiums among nations began to emerge. Take nuclear power as an illustration: only a handful of nations were (and continue to be) capable of crafting efficient nuclear reactors. Rather than developing in-house capabilities, nations unable to produce these advanced systems opted to purchase reactors from the few that could. This typically occurred through agreements structured around international consortia. During this period, a significant transition took place: the dominance of state control began to recede, and the gap left in research and knowledge production was increasingly filled by the private sector.

The neoliberal policies of the 1970s and 1980s hastened the transition away from national ownership in the technological domain. The progression from autarky to consortium, and eventually to globalization, was pivotal as it compelled – and at times incentivized – countries to procure goods from abroad, adhering to standards set by the free market. The State no longer stood as the sole arbiter shaping the nexus between economy, science, and technology. Instead, this role was increasingly assumed by a supra-state entity: the capitalist economy. As Edgerton posits, during this period, the development of information and communication devices became crucial to ensure competitiveness in the marketplace[22].

The domain of information and communication extends beyond just the purview of the state. In the 1980s, research and development (R&D) emerged as the most rapidly expanding sector, even as technological innovations experienced relative stagnation. R&D became the linchpin of all developmental agendas, including those in the medical-pharmacological field, prompting a significant realignment in health policies. The growth

Seuil, Paris 2015, p. 80.

[21] For an analysis of the role of industrialization in discoveries in this period see M. Weatherall, *In Search of a Cure: A History of Pharmaceutical Discovery*, Oxford University Press, Oxford 1990.

[22] D. Edgerton, *L'Etat entrepreneur de science*, cit., p. 81.

of R&D was largely propelled by private entities and institutions aiming to establish a competitive edge in the market. Examining the trajectory of drug development illustrates the evident shift from state-controlled knowledge production to dominion by the private sector. When examining the field of pharmacology, it becomes evident that what was once a monopoly controlled by the State – wherein the state granted licenses to private citizens to practice pharmacy – underwent a significant transformation. The industrialization push during the Second World War transitioned pharmacy into a major industry. As Jean-Paul Gaudillière notes, with the emergence of pharmaceuticals as a significant industrial sector, there was an extensive adoption of screening, which became a cornerstone for medicine thanks to R&D[23].

The primary goal of pharmaceutical companies shifted towards the discovery of novel molecules that could be beneficial in treating a variety of diseases. The pursuit of these molecules was predominantly steered by screening and R&D, which were tasked with understanding and addressing the evolving needs of the population. A pivotal shift contributing to the individualization of responsibility is the patentability of drugs. The privatization of ownership over a chemical formula result in a monopoly over a specific drug by private enterprises, which leverage R&D for targeted communication and development. Historically, prior to the Second World War, medicines were generally not eligible for patenting. However, from the 1960s onwards, coinciding with the emergence of industrial pharmacopoeia, pharmacological innovations became patentable[24]. With this progression, screening and research became intertwined with the intricate steps mandated by administrative law regarding research and innovation. For instance, upon the discovery of a new molecule, it becomes imperative to:

- Seek approval from the relevant committees.
- Highlight potential side effects.
- Specify the therapeutic efficacy for a particular ailment.
- Explore potential applications for related disorders.
- Determine the permissible dosage limits.
- Conduct targeted trials on a subset of the population considered as a representative sample.

The patient's role underwent a transformation in the 1960s. Up until the 1940s and 1950s, screening and statistical research were retroactively applied

[23] Jean-Paul Gaudillière, *Une manière industrielle de savoir*, in *Histoire des Sciences et des Savoirs*, Volume III: *le siècle des technosciences 1914-2014,* pp. 85-105.

[24] For a detailed analysis see J.P. Gaudillière, *How pharmaceuticals became patentable: the production and appropriation of drugs in the twentieth century*, "History and Technology", XXIV, 2, 2008, pp. 99-106.

to the patient. However, with the neoliberal shift of the late 1970s, statistics began to be applied proactively, facilitated by R&D. Screening evolved from merely a tool for generating quantifiable data to an instrument where the representative sample for the screening is predetermined. It is this amalgamation of statistical studies and political imperatives that guides the development of certain drugs over others, prioritizing what are deemed as more pressing needs. Often the needs are determined by economic issues (see next section). During these years, scientific knowledge was molded by such demands. Although the distinction between basic and applied research persists, the emphasis of private enterprises has largely shifted towards applied research, given its ability to cater to specific needs. For example, the primary objective of pharmaceutical marketing is to position a particular "product" as most suitable for the patient's requirements. However, within this framework, drugs are perceived as consumer goods within a marketplace. Knowledge becomes a tool to procure the optimal product tailored to the patient's needs.

We are observing a profound transformation: a pivot from the pursuit of the ideal dosage and drug for a specific condition, to the identification of an ideal target – be it a segment of the population or a subset of patients – to whom a drug could be beneficial. When considering the transition from state-led to private-driven knowledge production, it's essential to underscore that this shift is anchored in a particular theoretical framework. The rush for novel medications is paralleled by a pivotal shift in how the state manages illness and healing: what I have defined as the individualization of responsibility, both morally and economically. I aim to outline the circumstances that transitioned healthcare responsibility from the State to the individual. My objective is to highlight that if the onus to treat and prevent diseases now rests on the individual, it is a direct consequence of placing the responsibility of gaining knowledge (specifically about one's medical condition) on the individual citizen. While the State offers tools to foster knowledge acquisition, it does not equip individuals with a framework to navigate this vast expanse of information.

Between the late nineteenth and early twentieth centuries, an era characterized by intense industrialization and pandemics, the advent of industrial hygiene became prominent. This discipline arose primarily in response to the challenges presented by the high concentrations of workers in production environments and their exposure to toxic materials like lead, silica dust, zinc phosphate, and others. These conditions gave rise to new diseases resulting from pollution and hazardous substances. Governments faced a two-pronged challenge. Firstly, they had to address the practical and theoretical dimensions of workers' exposure to these harmful agents. Secondly, they had to balance this concern with the economic imperative of maintaining a readily available and affordable labor force. Quantitative medicine, with a significant emphasis on toxicology, aimed to identify, quantify, and manage levels of exposure to toxic elements. The goal of safety management was to establish a clear, quantifiable safety threshold within which the population would be deemed safe from harmful effects.

However, by the 1960s, propelled by the rise of environmental movements[25], the broader concept of environmental health began to gain traction among governments. The ever-increasing interconnectedness between the environment and human health compelled medical professionals to re-evaluate traditional methods used to determine toxicological safety limits. This shift in understanding culminated in the 1970s, where the concept of risk assessment became foundational in health management. As Ulrich Beck argues, the industrialization associated with capitalism makes health threats not just hazards but risks[26]. As a result, as Linda Nash states, risk "is part of the common language and culture" while risk assessment, on the other hand, "is a bureaucratic technique that transforms the notion into an operational element of environmental legislation"[27]. The capitalist model of production inherently champions progress and continuous production as essential for its sustenance. Consequently, risk becomes an intrinsic component, a necessary by-product to ensure uninterrupted productivity. Under this framework, health is conceived not merely as a state of well-being but rather as a capacity to balance between productive demands and their subsequent ramifications.

The neoliberal policies that emerged in the early 1980s endeavored to establish a novel social contract, one where mutual acknowledgment of a certain risk threshold was expected of every citizen as a prerequisite to function within society. To operationalize this, there was an impetus to determine scientifically this threshold. Moreover, the period saw an emphasis on devising a systematic methodology to determine the pertinent criteria for its calculation.

While the 1970s and 1980s anchored the normalization of the risk notion, the late 1990s brought about a propulsive element to this paradigm. The ongoing climate crisis, which is inextricably linked with health, prompted neoliberal governments to frame the environment as inherently antagonistic to life. Within this context, risk management was expanded to incorporate the notion that governments, while recognizing the adversarial nature of the environment, should equip citizens with tools to navigate these environmental risks without fundamentally challenging their existence. This shift in responsibility, placing the onus on the individual, not only highlights but can also exacerbate existing social inequalities. As responsibility moves away

[25] See in particular Rachel Carson, *Silent Spring*, Houghton Mifflin Company, Boston 1962.

[26] Ulrich Beck, *Risk Society: Towards a New Modernity*, SAGE Publication, London 1992.

[27] Linda Nash, *Un siècle toxique. Les origines de la "santé environnementale"*, in *Histoire des Sciences et des Savoirs Volume III: le siècle des technosciences 1914-2014,* p. 158.

from collective institutions, it inevitably privileges those who already have the resources, knowledge, and capacities to navigate complex systems of information. Health care, being a vital and often intricate domain, becomes an area where these disparities are starkly evident.

When health care responsibilities are individualized, several factors come into play:

- Information Access: The digital age provides a deluge of information, but not everyone has equal access to reliable and relevant information, nor the literacy to discern and utilize it effectively.
- Economic Resources: Health care, especially in privatized systems, often requires significant financial resources. Those with more economic power can afford better treatments, medications, and preventive care, thus leading to better health outcomes.
- Social Networks: Those with strong social networks often have better access to recommendations, shared experiences, and support structures that can help in navigating health challenges.
- Cultural and Educational Background: Understanding the nuances of health care, advocating for oneself in medical situations, and having the cultural capital to interact effectively with health professionals can make a significant difference in the quality of care one receives.
- Institutional Discrimination: Health care institutions, like other societal structures, can have built-in biases. This can lead to differential treatment based on race, gender, socio-economic status, or other factors.

The result is a system where disparities in health outcomes become more pronounced. Those who are already marginalized, or disadvantaged may find it harder to access quality care, leading to poorer health outcomes, further entrenching cycles of inequality. This approach to health policy effectively creates a hierarchy of health based on socio-economic status and access to resources, which is fundamentally at odds with the principle of health as a universal and collective. This does not align with the narrative advanced by neo-liberal governments. According to them, the poor quality of care can be attributed to an individual's lack of attention; in other words, they believe illness results from the individual's own failings. Access to high-level treatments, such as seeing a specialist, visiting a hospital, or obtaining cutting-edge treatments, hinges largely on two variables: 1) the patient's ability to inform themselves and acquire a deeper understanding of their medical condition, which entails the capacity to engage in discussions with and question their doctor; and 2) the patient's opportunity to partake in screenings, undergo check-ups, and consult a specialist. In the following section, I will apply the two features of science's non-neutrality that I have discussed, providing examples from contemporary medicine.

4. *Personalized medicine: a new paradigm in knowledge production*

In this section, I will examine the example of Personalized Medicine to elucidate how the non-neutrality of science is frequently concealed behind what is deemed objective and rational. The definition of Personalized Medicine provided by the EU is also endorsed by the WHO and is as follows: "A medical model using characterization of individuals' phenotypes and genotypes (eg. molecular profiling, medical imaging, lifestyle data) for tailoring the right therapeutic strategy for the right person at the right time, and/or to determine the predisposition to disease and/or to deliver timely and targeted prevention"[28]. This definition underscores the intertwining of individuality and the economic realm. On one hand, there exists a need to optimize patient care (i.e., the desire to improve patient outcomes and discover new treatments). On the other hand, there is an imperative to optimize the economics and timing of medicine (i.e., the aim to expedite care without squandering resources). As Nikolas Rose contends, with Personalized Medicine, we witness a shift from treatment to prevention through genomic research[29]. Ideally, genetic information would facilitate precision in selecting treatments and doses, thereby reducing mortality rates and adverse reactions, minimizing the use of ineffective drugs, decreasing costs, and enhancing efficacy. Genomics research dates back to the 1980s, but it wasn't until the early 2000s that we witnessed a surge in sequencing projects. Preliminary discussions on the feasibility of a Human Genome Project took place between 1984 and 1986. In 1998, after a five-year plan, The Human Genome Project, outlined another five-year plan, which concluded in 2003. The subsequent year, the Human Genome Project consortium released a draft genomic sequence. The significant turning point came in 2007 when the costs of genome sequencing plummeted, making such procedures increasingly common in chemical, microbiological, and medical laboratories[30].

In other words, only when the cost of research decreased did the potential for expanding that research truly materialize. During the 2000s, the concept of Personalized Medicine began to increasingly intersect with the idea of individual patient genomic and genetic research. A pivotal moment came with an article published on April 16, 1999, in The Wall Street Journal titled *New Era of Personalized Medicine: Targeting Drugs for Each Unique Genetic Profile*, which established the connection between a personalized approach

[28] "Official Journal of the European Union", C 421, December 17, 2015.

[29] Nikolas Rose, *Personalized Medicine: Promises, Problems and Perils of a New Paradigm for Healthcare*, "Procedia - Social and Behavioral Sciences", LXXVII, 2013, pp. 341-352.

[30] See https://www.genome.gov.

to medicine and genetics[31]. In particular, the article describes the formation of the Single Nucleotide Polymorphisms Consortium and its role in promoting genomic research. The consortium's goal was to produce information from genetic mapping that "will help them create drugs specifically designed to target each person's unique genetic profile"[32]. This project is *not* dependent on federal resources, as it is the case of The Human Genome Project. It is in fact Bristol-Mayers, Smith Kline, Roche, Novartis and Glaxo Welcome that invested $45 million in a two-year program to map significant genetic loci for the treatments they were developing. Concurrently, both small and large pharmaceutical companies, some of which were members of the consortium, initiated parallel research endeavors. For instance, Bristol-Myers collaborated with MIT to identify genes associated with an elevated risk of heart disorders, diabetes, and asthma. Simultaneously, Genset SA in France was investigating genetic markers for prostate cancer. Building on what I have previously emphasized regarding R&D, it can be posited that Personalized Medicine refers to a medical approach where drug prescriptions are paired with specific genetic tests or particular genetic variations[33]. In this context, personalized medicine derives its therapeutic potency from pharmacogenetics. This shift implies that medicine had to prioritize pharmacological research in the laboratory over the clinic. Robert Lagereth and Michael Waldholz highlight a specific kind of individuality: the genetic one. While genetics remains central, it is not the sole approach in personalized medicine today. Omics techniques, such as metabolomics, proteomics, epigenomics, and transcriptomics, play an integral role in the current landscape of personalized medicine. The individuality of a patient is fundamentally rooted in their biological traits, especially those discerned through Omics. In this light, the personalization of patient care relies on quantifiable characteristics; in other words, a patient can receive appropriate therapy based on the sequencing and study of their biological traits conducted using Omics techniques. It is evident that prioritizing biological features, analyzable through Omics technology, as the primary determinant of a patient's individuality is a deliberate choice. Clinical aspects, such as a patient's lived experiences, desires, expectations, and social environment, could also play a pivotal role in shaping the appropriate therapeutic intervention. I am not advocating for the exclusion of biological considerations. Instead, I emphasize that there are therapeutic approaches which do not primarily or solely lean on the biologi-

[31] Robert Langreth and Michael Waldhoz, *New era of personalized medicine: targeting drugs for each unique genetic profile*, "The Oncologist", IV, 1999, pp. 426-427.

[32] Ivi, p. 426.

[33] For an analysis see Xavier Guchet, *De la médecine personnalisée à l'exposomique. Environnement et santé à l'ère des big data*, "Multitudes", LXXV, 2, 2019, pp. 72-80.

cal realm for decision-making. Anchoring on biological traits, especially biomarkers, also transforms therapeutic strategies: it entails a nexus between drugs and diagnostics. In essence, a specific response to diagnostic tests dictates the choice of drug for a patient. This approach not only expedites the commercialization of drugs – justified by their targeted applicability based on diagnostic results – but also influences the parameters that define meaningful data for diagnostic tests. In this context, specific genetic and/or molecular markers are crucial to ascertain a drug's efficacy. Consequently, genetic and molecular tests emerge as vital criteria for determining the inclusion or exclusion of a patient in a clinical trial. While Phases II and III of drug development typically necessitate a broad patient base, integrating diagnosis through genetic-molecular methods with therapy allows for a comprehensive genetic mapping of the patient. If an acceptable correlation between the drug and the diagnostic test is established, any individual possessing the markers identified in the diagnostic test becomes a potential candidate for the drug. As posited by Jan Trøst Jørgensen, many contemporary cancer drugs receive approval based on metrics such as response rate and progression-free survival endpoints. Historically, the standard for FDA approval was anchored in demonstrable improvements in overall survival[34]. Consequently, there is an increasing emphasis on identifying a biomarker and subsequently associating it with a specific drug. We can infer that a patient's individuality is closely tied to the uniqueness of their biomarkers. While this perception is not entirely misguided, it becomes intricate when environmental factors are considered. Indeed, as a patient continually interacts with their environment, it prompts a reevaluation of the very definition of a biomarker in modern medicine.

This emphasis on individualizing patients based on their biological traits heralds a paradigm shift in the application of knowledge both within the medical domain and beyond. In the framework of personalized medicine, virtually any data can be linked to biomarkers. What I've characterized as clinical features are brought back to their biological essence, suggesting that every facet of a patient's life can be translated into the biological domain via biomarkers. It's worth noting that in recent times, biomarkers have evolved beyond being exclusively associated with genetic loci. Their definition has expanded, largely because they are influenced by a patient's interaction with their surroundings. This has given rise to the concept of the Exposome. The Exposome is characterized as the entirety of exposures encountered by humans throughout their lives, encompassing all environmental factors, including lifestyle elements, from the prenatal phase onwards. Within the context of personalized medicine, the Exposome is employed as a framework that utilizes

[34] Jan Trøst Jørgensen, *Twenty Years with Personalized Medicine: Past, Present, and Future of Individualized Pharmacotherapy*, "The Oncologist", XXIV, 7, 2019, pp. 432-440.

biomarkers not merely to identify genetic mutations, but also as indicators of exposure, susceptibility (which can be genetic and beyond), and prevailing disease trends in populations[35]. Health is increasingly conceptualized as the prevention and prediction of potential illnesses based on objective markers, namely biomarkers. Such a shift underscores a transformative perspective in knowledge production and research within medicine. Rather than primarily aiming to cure or act *a posteriori*, the emphasis has now predominantly shifted towards disease prevention.

The paradigm of knowledge production within the medical sphere has undergone a profound transformation, particularly with the advent of biomedicine. Biomedicine, a paradigm that melds medicine and biology, stands in stark contrast to the traditional medicine prevalent until the 1980s. Personalized medicine serves as a salient illustration of this shift. Broadly, the proliferation of information has paved the way for innovative research methodologies. By suggesting that individuals now bear the onus for their health, becoming responsible for their knowledge and actively seeking credible information, I aim to highlight that this surge in available information, especially in the medical domain, has revolutionized both the mechanics of knowledge production and our collective approach to it. In their discussion of *Biomedical Platforms*, Keating and Cambrosio emphasize the multi-layered nature of knowledge employed in medicine, one that defies simplistic, partitive definitions[36]. For instance, in the realm of cancer diagnosis, *Personalized Medicine* shifts its lens from merely categorizing cancer based on organ specificity to a nuanced analysis across various markers. According to Peter Keating and Alberto Cambrosio, these levels are co-defined by the pivotal markers and the methodologies deployed to identify them, such as morphological, immunophenotyping, and genetic techniques. A biomedical platform, as they elucidate, can be conceived as a material, spatial, and discursive assembly that acts like a bench upon which conventions of the biological or normal interlace with conventions related to the medical or pathological. Such a platform is not merely an instrument or a device. Instead, it represents a distinct alignment of instruments and individuals, united by shared protocols and activities, anchored by standardized reagents. Consequently, a platform is less about its tangible existence and more about its orchestration – both materially and discursively. As they articulate, a platform serves as a prism,

[35] See Steven Morris Rappaport and Martyn T. Smith, *Epidemiology. Environment and disease risks*, in "*Science*", XXII, 2010, pp. 460-461; and C.P. Wild, *Complementing the Genome with an "Exposome": The Outstanding Challenge of Environmental Exposure Measurement in Molecular Epidemiology*, "Cancer Epidemiology Biomarkers Prevention", XIV, 8, 2005, pp. 1847-1850.

[36] Peter Keating and Alberto Cambrosio, *Biomedical Platforms: Realigning the Normal and the Pathological in Late-Twentieth-Century Medicine*, MIT Press, London 2003.

offering a multifaceted understanding of situations or facts[37]. Platforms in medical knowledge production are far from being neutral. Their foundation lies in specific standpoints, objectives, and methods employed in generating knowledge. Contemporary medicine's knowledge production paradigm can be characterized by its heavy reliance on data accumulation. Take Personalized Medicine as an example: it operates under the guiding principle of "the more data collected, the greater the precision and efficacy." Yet, this notion of "personalization" is somewhat paradoxical. It doesn't imply individualized attention in the truest sense. Rather than each individual receiving a tailor-made treatment, they are assigned to specific categories, derived from data analysis. These categories might range from broad to narrow, but the fundamental mechanism remains consistent: classify the patient within a category that groups them with others, ensuring they receive the treatment optimal for that group.

As I have emphasized, the driving force behind therapeutics in personalized medicine up to 2010-2015 was the principle of formalization. This involved gathering patient data to offer a "universal" and "objective" characterization of their pathology. Such categorization and formalization hinged on the enhancement of data collection methods and the tools associated with it. However, a shift occurred around 2010 with the incorporation of the Exposome concept into personalized medicine. The scope of data collection expanded beyond genetic markers, encompassing a more comprehensive view of individual life experiences. These data are then juxtaposed with inter-individual – or, at a broader scale, population-level – information, often approaching the subject from an epidemiological or preventive perspective. Medical practice began to leverage initiatives initiated by global bodies such as the EU and UN to gather lifestyle data from the populace. A case in point is the Foodome project, championed by the European Food Safety Authority. This project is a component of a broader research initiative aiming to elucidate the lifestyle factors associated with diseases like coronary heart disease and diabetes. Utilizing advanced AI techniques, the project aspires to compile an exhaustive database on food composition, bridging the current knowledge gaps about the biochemical makeup of food[38]. Omics techniques play a pivotal role in contemporary discourses on microbiota, highlighting environmental interactions that defy conventional physical demarcations and classify matter based on microbial distinctions within samples[39]. Microbiomics

[37] Ivi, pp. 44-45.

[38] See https://www.efsa.europa.eu/en/art36grants/article36/gpefsaknow202202-european-foodome-project.

[39] For a detailed exploration on this, refer to Roberta Raffaetà, *Metagenomic Futures: How Microbiome Research is Reconfiguring Health and What it Means to be Human*, Routledge, London 2022.

represents another omics strategy, delving into the study of microorganisms in tandem with their surrounding environment.

In recent years, the term "Sociome" has emerged to describe the examination of social determinants impacting health and disease. Initiatives launched over the past five years primarily aim to codify a spectrum of factors – spanning social, environmental, psychological, and behavioral realms – that modulate disease. These elements find expression through various socio-ecological interactions, communicated through channels like vocal intonations, olfactory signals, or bodily postures.

Phenomics, on the other hand, is dedicated to the exploration of the phenome: a collective representation of traits manifested at cellular, tissue, and organismal levels, differentiated from their genetic underpinnings. In essence, the phenome captures the totality of phenotypic attributes, scrutinized in correlation with their genetic and proteomic counterparts. Contemporary personalized medicine, buoyed by technological advancements, consolidates diverse datasets under the umbrella of "Omic" techniques. This signifies a paradigmatic shift from the erstwhile "gene-disease" monocausal framework to a multicausal model. While genetic data remains pivotal, the integration of diverse Omics data offers greater depth and insight. Modern personalized medicine deals with a significantly larger volume of data compared to its predecessors from the early 2000s. Relying solely on genetic data for diagnosis has become impractical. Rather than pinpointing a direct prescriptive diagnosis based on gene-disease correlations, there is now a focus on assessing the likelihood of an individual developing "unhealthy" conditions. This approach aids in strategically designing therapeutic interventions. Crucially, having a predisposition or an increased risk of disease onset does not equate to an actual pathological state.

Contemporary personalized medicine not only focuses on the "classical" model but also emphasizes probabilistic studies. In making this assertion, it is not my intention to imply that classical models of genetic research have been forsaken; rather, they are actively incorporated and employed within the framework of the new paradigm, as it is the case for those used to detect BRCA1 and BRCA2 mutations. Upon detection, the individual is then enrolled in a screening program. The objective is to mitigate, or if the disease does manifest, limit its progression. Consider a young man born in the Belmont neighborhood of Detroit's suburbs, with the zip code 48227, and of Mexican ethnicity. He has a 25% chance of developing colon cancer due to his genetic lineage. Factors such as his lifestyle – including diet, occupation, and exposure to exhaust and other toxic substances – can elevate this risk by an additional 20%. Given the typical socioeconomic conditions of his environment, there's an 18% probability that his symptoms, if he develops colon cancer, could be more severe than average. Additionally, when one factors in his potential access to medications and specialists, it's possible to estimate a mortality rate within this modern perspective. While this isn't a definitive diagnosis, it does provide a roadmap for potential treatments the young man might require in the future. The onus lies with the individual to become informed about his condition and ensure he accesses the appropriate care when necessary.

According to Rose, we witness a rationalization of the patient[40]. Transitioning from a defined group to an individual only becomes feasible when the individual is made "rationalizable." Groups, when classified within populations, are created through criteria that embody concepts of measurability, objectivity, and commensurability – in essence, rationality. For an individual to fit within a group, they must exhibit the characteristics inherent to that group. To elucidate, for an individual to qualify for a specific group, they must "possess" certain attributes mandated by that group's criteria.

However, since these attributes aren't inherently "natural," establishing commonalities between the individual and the system necessitates abstraction, formalization and choice of a hierarchy. The unique experience of a patient must align with the generalized characteristics of a group. Achieving this requires stripping the individual's experience of much of its unique and contingent nature, enabling it to become standardized and objective. Furthermore, categorizing an individual has political ramifications, such as the access to certain treatments. These outcomes are essentially born out of two processes: rationalization – attributing specific characteristics to an individual – and probability – the likelihood of an individual responding to a treatment based on their attributed characteristics. The development of drugs specifically effective for certain population groups will undoubtedly provide further incentives for pharmaceutical companies to invest in the most lucrative markets, rather than addressing the medical needs of those suffering from the most widespread diseases. According to Rose, we are witnessing a new type of racial segmentation in medicine. Consequently, new drugs – especially biologic ones that are based on genomic sequencing – require a more specific sample. This sample pertains to populations with distinct characteristics that ensure the success of the treatment. These groups can be selected based on specific biases, such as economic ones, because they are 1) increasingly refined, and 2) interlinked with both genetic and social characteristics due to the advancements in Omics technologies. As I emphasized in the previous section, when the individualization of knowledge production is viewed within the medical field, it leads to the perception of patients as individual-consumers[41].

40 N. Rose, *Personalized Medicine*, cit., p. 342.
41 Ivi, p. 348.

5. *Conclusion*

As I have emphasized, there are two primary factors, among others, that determine the non-neutrality of science, which are the sociality of science and the individualization of responsibility in knowledge production. Both are inherently political. The healthcare system serves as an illustrative example of how sociality and individual responsibility intertwine. Insurance systems, as well as mixed public-private healthcare models, increasingly assert that patients should be accountable for their own health. Everyday life attests to the significance of individual capability in generating knowledge and information. For instance, health insurance plans are often customized based on an individual's economic status and lifestyle. The trend towards privatization of care began in the 1970s. Genomic research, frequently backed by private entities like the aforementioned Single Nucleotide Polymorphisms Consortium, particularly in the pharmaceutical sector, aligns with the "open market" perspective of viewing knowledge and information as commodities.

What I aim to highlight is that, on one hand, the production of knowledge is inherently social and collective. This especially pertains to three elements: the subject, the material object of knowledge, and the "taught collective". Instead, starting from the 1970s, there has been a movement towards perceiving knowledge in a contrary manner: private and individual. When viewed historically, concerning how the individualization of a citizen's knowledge responsibility has evolved, two things become evident:

1. The privatization of knowledge production has been a significant catalyst for applied research. This research is steered by the R&D system, targeting market expansion. From the latter half of the 20th century, the progression of knowledge has been largely dictated by market requirements, spurred by neo-liberal policies.
2. These policies have precipitated the individualization of responsibility when it comes to acquiring information. The generation of knowledge isn't solely a collective endeavor anymore; it's also a private one. Indeed, only with the right knowledge can a citizen avail services, like health services. In this context, knowledge serves dual purposes: it's a tool for better service, and simultaneously, the degree of knowledge and, by extension, access to services, is contingent upon an individual's socio-economic standing. An individual in disadvantaged socio-economic conditions often grapples with acquiring knowledge. This, in turn, hinders their ability to access essential services. I contend that the shift towards individual responsibility for knowledge acquisition has positioned knowledge as a prerequisite for a fulfilling social life – one based on access to crucial services like healthcare, employment, and housing. The onus of accessing

knowledge and gathering information now rests squarely on individual citizens, marking a departure from the erstwhile collective approach to knowledge production.

The COVID-19 crisis further underscored this shift. Not only did many face challenges in accessing knowledge and consequently, services, but they also had to navigate a deluge of often contradictory information. To perceive science as a social product is to acknowledge its normative nature – its inherent ability to shape values. Constraining this normative aspect to a purely rational and objectifying paradigm, which I term the "narrative of linear science" fosters a detached view of science and knowledge. This perspective sees human actions merely as responses to immutable laws, rather than as dynamic interactions shaping and being shaped by knowledge.

Conversely, by recognizing the social underpinnings of science – its normativity and consequent non-neutrality – we pave the way for a resurgence of grassroots knowledge production. This bottom-up approach can serve as a bulwark against the pitfalls of conspiracy theories and various manifestations of populism as it preserves both the scientific and the social feature of science.

The Impossible Neutrality of a Disaster in Seveso 1976

Fabio Lusito*

Abstract: The Seveso disaster in 1976 is considered one of the worst environmental and health catastrophes of contemporary times. Because of its dramatic consequences, it opened a new phase of reflections on the use of science and technology in Italy in the second half of the troubled 1970s. This paper aims to explore the problem of the neutrality and non-neutrality of science by investigating the public narrative of the Seveso incident reported by two ideologically opposed newspapers such as the communist *l'Unità* and the center-right bourgeois daily *Corriere della Sera*. Through this comparison, it is possible to highlight the socio-political and ideological influences that affected public opinion on a disaster that was on the one hand environmental and sanitary, and on the other social and economic. In conclusion, the paper casts a transnational glance at the *New York Times* analysis, to highlight the differences in the narratives and to show how deep the Italian political background was in conditioning the reactions, including on the scientific level.

Keywords: history of ecology, neutrality of science, radical science movements, Seveso, science communication.

Introduction

Our reader is a calm, honest, order-loving man who works, produces, and creates income [...] He opens the newspaper to find a serene and balanced word [...]. Our reader opens the newspaper, looks at it, and decides if he wants to read it or throw it away. Without the feeling that we want to break his balls!

1972: At the peak of his career, the Italian actor Gian Maria Volonté was cast to play Giancarlo Bizanti, editor-in-chief of the right-wing newspaper *Il Giornale*. The film, *Sbatti il mostro in prima pagina*, directed by

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Marco Bellocchio[1], tells the relationship between public opinion and mass communication during the controversial 70s in Italy. The film attempted to display the manipulation of the news, the overused "journalistic malpractice"[2]. A political-ideological clash was animating the Country: one of the main purposes of the protesters was to restore the proper value to truth and knowledge, and to draw attention to the non-neutrality of dominant narrative in influencing public opinion. Following this shared belief, also the scientific debate had been affected by original critics. Several cases would provide an analysis about socio-political influences in Italian scientific context[3].

The Seveso disaster should be considered as illustrative to delve into the issue of non-neutrality of science in its public use. Many studies have already examined the event from other points of view[4]. The aim of this study is to investigate the Seveso case through the public narrative of the disaster offered in two important counterposed daily newspapers, both ideologically biased. Furthermore, this paper attempts to show which values and themes have distinguished the different narratives, comparing the communist *l'Unità* and the industrial bourgeoisie newspaper, the liberal *Corriere della Sera*. Was the public given a consistent and objective account of the catastrophe? Or was the reporting not exactly neutral – even medically – due to the effects of socio-political imperatives? This article tries to answer these questions through an analysis tracing the year following the accident. As a last resort, the issue investigates the description of the Seveso catastrophe that emerged in the liberal and democratic *New York Times*, in order to obtain a transnational perspective and identify differences.

1 Marco Bellocchio, *Slap the Monster on Page One*. Jupiter Generale Cinematografica, UTI Produzioni Associate, Labrador Films, 1972.

2 *Segnalazioni cinematografiche*, vol. 75, 1973.

3 The Italian 1960s and 1970s had already offered controversies concerning the use of science and power: Mauro Capocci and Gilberto Corbellini, *La politica della scienza nel secondo dopoguerra*, in *Il Contributo italiano alla storia del Pensiero - Scienze*, Treccani, Roma 2013, pp. 556-562; Daniele Cozzoli and M. Capocci, *Making biomedicine in twentieth-century Italy: Domenico Marotta (1886-1974) and the Italian Higher Institute of Health*, "The British Journal for the History of Science", 44, 4, 2011, pp. 549-574; John Foot, *The Man Who Closed the Asylums: Franco Basaglia and the Revolution in Mental Health Care*, Verso Books, New York 2014; Gerardo Ienna, *Fisici italiani negli anni '70 fra scienza e ideologia*, "Physis", 55, 1-2, 2020, pp. 415-442; Fabio Lusito, *«Diamo l'assalto al cielo!» («Let's assault the sky»): science communication between scientists and citizens and Lombardo Radice's television in Italy in the years of the protests*, "JCOM – Journal of Science Communication", 19, 03, 2020, pp. 1-22.

4 Laura Centemeri, *The Seveso disaster legacy*, in Marco Armiero and Marcus Hall (eds.), *Nature and History in Modern Italy*, Ohio University Press & Swallow Press, Athens 2010, pp. 251-273; Bruna De Marchi, *Seveso: From pollution to regulation*, "International Journal of Environment and Pollution", 7, 4, 1997, p. 526-537; B. De Marchi, Silvio O. Funtowicz, and Jerome R. Ravetz, *Seveso: A paradoxical classic disaster*, in James K. Mitchell (ed.), *The long road to recovery: Community responses to industrial disaster*, United Nations University Press, New York 1996, ch. 4.

Recently, several scholars have studied the historical coverage of science in national newspapers during different ages. The focus has often been on the analysis of popularization practices, the inclusion of scientists in the press and the rise of science displayed in the public sphere[5]. Mostly, these studies concern the area of science communication and its history. Some papers have already reviewed similar topics in the Italian background[6]. The aim of this paper is to assess the socio-political features of the use of science in the public context by examining, from a historical perspective, the consequences of an event that affected both the scientific and the medical as well as the social spheres and, by doing so, to emphasise how challenging it has been to follow an unbiased and objective attitude in communication.

The Seveso emergency is considerable a classic example of risk communication in science[7]. Hence the abundant use of evocative terms such as 'disaster', 'calamity' and 'catastrophe'. Subsequently, reference can be also made to the

[5] Casper Andersen and Hans H. Hjermitslev, *Directing Public Interest: Danish Newspaper Science 1900-1903*, "Centaurus", 51, 2009, pp. 143-167; Geoffrey Belknap, *From a Photograph: Authenticity, Science, and the Periodical Press, 1870-1890*, Bloomsbury, London 2016; Adrian Bingham, *'The monster'? The British popular press and nuclear culture, 1945-early 1960s*, "The British Journal for the History of Science", 45, 4, 2012, pp. 609-624; Suzanne de Cheveigné, Eliséo Veron, *Nobel on the front page: the Nobel physics prizes in French newspapers*, "Public Understanding of Science", 3, 2, 1994, pp. 135-154; Matiana González-Silva, *With or Without Scientists: Reporting on Human Genetics in the Spanish Newspaper El País (1976-2006)*, in Faidra Papanelopoulou, Augustì Nieto-Galan and Enrique Perdiguero (eds.), *Popularizing Science and Technology in the European Periphery, 1800-2000*, Routledge, London & New York 2016, pp. 217-236; A. Nieto-Galan, *From Papers to Newspapers: Miguel Masriera (1901-1981) and the Role of Science Popularization under the Franco Regime*, "Science in Context", 26, 3, 2013, pp. 527-549; Marianne G. Pellechia, *Trends in science coverage: a content analysis of three US newspapers*, "Public Understanding of Science", 6, 1, 1997, pp. 49-68; Jeffrey R. Wigelsworth, *Bipartisan politics and practical knowledge: advertising of public science in two London newspapers, 1695-1720*, "The British Journal for the History of Science", 41, 4, 2008, pp. 517-540; Isabel Zilhão, *The rise and fall of science for all: Science for children voiced by a Portuguese daily newspaper (1924-1933)*, "History of Science", 52, 4, 2014, pp. 454-488.

[6] Massimiliano Bucchi, *La scienza nella stampa quotidiana*, in Francesco Cassata and Claudio Pogliano (eds.), *Scienze e cultura dell'Italia unita, Annali 26*, Einaudi, Torino 2011, pp. 297-320; Id., *Visible Scientists, Media Coverage and National Identity: Nobel Laureates in the Italian Daily Press*, in Bernard Schiele, Michel Claessens and Shunke Shi (eds.), *Science Communication in the World*, Springer, Dordrecht 2012, pp. 259-268; Id., *La scienza e i mass media: la «fusione fredda» nei quotidiani italiani,* "Nuncius", 11, 2, 1996, pp. 581-600; M. Bucchi and Renato G. Mazzolini, *Big science, little news: science coverage in the Italian daily press, 1946-1997*, "Public Understanding of Science", 12, 1, 2003, pp. 7–24; Andrea Candela and Federico Pasquarè Mariotto, *Italian news coverage of radiation in the early decades of the twentieth century: A qualitative and quantitative analysis*, "Public Understanding of Science", 25, 2, 2016, pp. 236-251; F. Lusito, *Divulgare la scienza, rivoluzionare la società: gli interventi giornalistici di Lucio Lombardo Radice*, "Physis", 55, 1-2, 2020, pp. 325-347; Nico Pitrelli, Federico Manzoli, Barbara Montolli, *Science in advertising: uses and consumptions in the Italian press*, "Public Understanding of Science", 1, 2, 2006, pp. 207-220.

[7] Giancarlo Sturloni, *La comunicazione del rischio per la salute e l'ambiente*, Mondadori, Milano 2018.

notion of 'crisis' (in climate, in biodiversity, in the environment), as implied in an Anthropocene horizon nowadays[8]. As has been pointed out lately, "disasters should be understood as stemming from a mixture of environmental, technological, and human factors"[9]. Therefore, the Seveso explosion can be considered entirely appropriate to this definition.

In the aftermath of Covid-19 pandemic, we are particularly aware of what it means to delay assistance and influence the citizens. In this regard, it can be recalled that "science and politics cannot be separated, as the case of pandemic management in history shows", and this also applies to other crises, including environmental one[10]. In this assessment, the human factor played such a significant role that it affected not only safety operations but also the opinion of readers. In fact, the Seveso catastrophe occurred at an intense phase of the public debate on science, and at the climax of the Italian political agon – these aspects often ran in parallel. In this context, non-neutral stances found fertile land.

Silence, please!

Saturday, July 10, 1976, around 12:30 p.m., Italy: in Meda, Lombardy – in the center of the so-called "industrial triangle" – at the ICMESA chemical company, the malfunction of a reactor's control system caused its explosion. A cloud of tetrachlorodibenzo-p-dioxin (TCDD), a highly toxic dioxin, was released. Seveso – not far from the site – was the city most affected by the harmful smoke. A few days later, the first effects on the surrounding flora and fauna became apparent: crops were compromised and some animals died. However, in the media, it seems that nothing happened near Seveso. On July 14, the very first evidence of the toxic release was shown. The next day, the first institutional measures were taken[11]. The situation was surreal: the toxic cloud was being released into the air, but there was absolute silence in the media.

Citizens, who were directly exposed to the risk, were kept unaware of the incident for a week. The first newspapers to report the story were the *Corriere*

[8] Jürgen Renn, *From the History of Science to Geoanthropology*, "Isis", 113, 2, 2022, pp. 377-385; cf. John Robert McNeill and Peter Engelke, *The great acceleration: An environmental history of the Anthropocene since 1945*, Harvard University Press, Harvard 2016.

[9] Julia F. Irwin, Jenny Leigh Smith, *Introduction: On Disaster*, "Isis", 111, 1, 2020, pp. 98-103, pp. 98-99.

[10] Flavio D'Abramo, Giulia Gandolfi, G. Ienna, Pietro Daniel Omodeo and Charles Wolfe, *Political epistemology of pandemic management*, "Mefisto", 5, 1, 2021, pp. 121-146, p. 142.

[11] L. Centemeri, *Ritorno a Seveso. Il danno ambientale, il suo riconoscimento, la sua riparazione*, Bruno Mondadori, Milano 2006, pp. 22-23.

della Sera and the Milan daily *Il Giorno* only on 17 July[12]. At first, catastrophic headlines and poor scientific knowledge worried the population. But the press was quiet by reporting late about the tragic event. *La Stampa*, for instance, in its first article, completely skirt the issue, merely stating "It all started last Saturday" and nothing else[13]. Other newspapers did not report the news: they learned about the ICMESA explosion only through the articles in *Corriere della Sera* and *Il Giorno*, both informed by the Mayor of Seveso.

What caused this long-lasting silence? The communications had all been filtered by the company responsible for the incident, the Swiss *Givaudan*. Even the first checks and tests were on their charge. Referring to a category conceptualized by historians of science Erik Conway and Naomi Oreskes, they were akin to the "Merchants of Doubt": scientists ready to deny the truth and hold "opinions [that] sometimes express ill-informed beliefs, not reliable knowledge"[14]. The company had no interest in spreading the news and alarming the public, nor was it communicated that the issue was dioxin; it was thought, perhaps, that everything would be fine, without the urgency of compromising business.

The Communists' perspective

The unconventional silence soon became an allegation by the communist newspaper *l'Unità*: "ICMESA managers [...] had first kept quiet about the malfunction and then downplayed the level of dangerous air pollution". When referring to the dioxin, the journalist was sure: "The gas is similar to that used in Vietnam by the Americans". Until then, dioxin's effects caused skin burns that injured ten children[15]. The similarity to the situation in Vietnam was functional in equating the leadership of ICMESA and the United States foreign policy: both reprehensible in the communists' view, as the former represented a supporter of the cynical industrial bourgeoisie, the latter as the political-ideological enemy *par excellence*. Suffice it to say that a few years earlier the moon landing had been politicised by invoking comparisons with Vietnam[16]. A similar reference appeared again in the following

12 Editorial, *Un intero quartiere di Seveso inquinato da gas tossici*, "Corriere della Sera" [CdS], 17 July 1976, p. 7; Editorial, *Bimbi rossi e gonfi per una nube di gas*, "Il Giorno", 17 July 1976.

13 M. F., *Intossicati 20 bimbi da una nube chimica a Seveso ed a Meda*, "La Stampa", 18 July 1976, p. 11.

14 Erik Conway and Naomi Oreskes, *Merchants of doubt*, Bloomsbury Press, New York 2010, p. 240.

15 Paolo Zucca, *Fabbrica che avvelena: arrestato il direttore*, "l'Unità", 22 July 1976, p. 6.

16 It was the head of the PCI's Central Press and Propaganda Section Achille Occhetto who proposed a provocative parallel between the moon landing and Vietnam. cf. Achille Occhetto, *La Luna e il Vietnam*, "l'Unità", 26 July 1969, p. 1.

days. This time, the contributor insisted about the possibility that dioxin could kill even years after, providing evidence:

> Scientists from Vietnam and other nations have presented, in numerous scientific forums, the results of research on the frightening experiences in their Country, which were sprayed with heavy amounts of dioxin. […] Dioxin […] can cause serious liver and kidneys damage and appears to cause liver tumors, congenital malformations in infants, and genetic alterations[17].

Authoritative scientists had also recalled some comparisons with Vietnam[18]. Stirring the pot, some journalists exaggerated by calling the cause of the accident a "dioxin bomb"[19]. The opinion of the Health Commission, which did not exclude the possibility of deformed births, was also being juxtaposed to the Vietnamese precedent[20]. Even long after the fact, articles about Vietnam and Seveso were shrewdly paired on the same page[21]. Scientific explanations appeared useful to justify ideological opinions.

Any doubts were dispelled with a further accusation made by the leftist newspaper: *L'Unità* raised the question of the delay in evacuation and assistance. It was said that evacuation from the site had been delayed by seven days despite the urging of the health officer: two workers had warned the authorities of the accident, but the factory managers had only called for generic caution[22]. This situation led the Communist Party to launch an inquiry, which generically focused its attention on health-threatening industries[23]. The newspaper considered ICMESA to be the benchmark of the system: moralist and rhetorical articles were published, filled with eloquent headlines such as "The death in the factory" or "ICMESA continues to produce poisons in our country", disapproving of the very role of the factories, defined as a "trap" for health and a "problem for the society"[24]. It turned into a trial

[17] Paolo Sassi, *La dioxina può uccidere anche a distanza di anni*, "l'Unità", 26 July 1976, p. 5. Cf. Marina Rossanda, *La particolare esperienza del Vietnam*, "l'Unità", 2 September 1976, p. 4.

[18] As the genetist Adriana de Capoa, *La genetica non c'entra*, "l'Unità", 18 August 1976, p. 3.

[19] Orazio Pizzigoni, *Il primo turno di operai è al lavoro a disinnescare la 'bomba alla diossina'*, "l'Unità", 10 August 1976, p. 4.

[20] Bruno Enriotti, *Possibili le nascite deformi*, "l'Unità", 11 August 1976, p. 4.

[21] Massimo Loche, *Vietnam: prime risposte sui micidiali effetti della diossina sull'uomo*, "l'Unità", 22 November 1976, p. 5; Editorial, *Mangiò erba contaminata la mucca morta a Seveso*, "l'Unità", 22 November 1976.

[22] Ennio Elena, *Fu spostata di 7 giorni l'evacuazione sollecitata dall'ufficiale sanitario*, "l'Unità", 26 July 1976, p. 5; Id., *Già 800 milioni i danni della nube*, "l'Unità", 27 July 1976, p. 4; Id., *Silenzi e colpevoli ritardi dietro il dramma di Seveso*, "l'Unità", 28 July 1976, p. 4; Bianca Mazzoni, *Ignorati gli allarmi dei lavoratori*, "l'Unità", 13 August 1976, p. 4.

[23] *Il PCI apre un'inchiesta sulle industrie che attentano alla salute*, "l'Unità", 5 August 1976, p. 4; Michele Urbano, *Il PCI chiede un'inchiesta parlamentare sulle responsabilità del disastro all'ICMESA*, "l'Unità", 1 August 1976, p. 4.

[24] O. Pizzigoni, *La morte in fabbrica*, "l'Unità", 9 August 1976, p. 5; Editorial, *I tentacoli di una*

of the industrial system, awakening controversies over political and public control, the use of toxic substances, and preventive measures for workers[25].

Some scientists and academics tended to support this narrative. According to the anthropologist Ettore Biocca "uncontrolled industrialization in a dependent Country without adequate protective legislation, such as Italy, is turning into the most tremendous pathology humanity has known", and Seveso was a striking example[26]. For the physician Giovanni Berlinguer, a senior member of the Communist Central Committee, Seveso could be a crucial moment to renew the "contribution to the fight against environmental degradation of the Country" caused by wild development[27]. For critics, this meant reconsidering the entire national industrial program[28]. The same emphasis was placed on how pollution affects the cost and welfare of citizens[29].

As contributors or scientists criticized the industrial network, environmental issues emerged, which themes were advancing around the world and had previously been discussed by the Communist Party[30]. The debate was vigorously pursued in the pages of the newspaper by physician, activist and regional communist councillor Laura Conti[31]. Her thoughts, which often start from a technical point, concern the relationship between the industrial use of science and the regard for the "general laws of living nature"[32]. As she stated, "When political decisions have to be based on scientific data, it is absolutely necessary to clarify the relationship between science and politics", to avoid the risk of compromising the correct method[33]. These articles, which were often based on a critical examination of the role of science, turned into explicit political accusations.

multinazionale, "l'Unità", 27 July 1976, p. 4.

25 Guido Manzone, *Processo ai veleni*, "l'Unità", 5 August 1976, p. 3.

26 Ettore Biocca, *La nube di Seveso: dobbiamo tollerare ancora?*, "l'Unità", 7 August 1976, p. 1.

27 E. Elena, *Ricostruire la vita dove si è seminata la distruzione*, "l'Unità", 30 August 1976, p. 4.

28 Giancarlo Angeloni, *Caos nei controlli: ecco come dall'Icmesa 'nasce Seveso'*, "l'Unità", 12 September 1976, p. 5.

29 G. Manzone, *Quanto costa l'inquinamento*, "l'Unità", 20 August 1976, p. 3.

30 Istituto Gramsci, *Uomo natura società. Ecologia e rapporti sociali*, Editori Riuniti, Roma 1972.

31 About the figure of Laura Conti in relation to Seveso, see Stefania Barca, *Lavoro, corpo, ambiente: Laura Conti e le origini dell'ecologia politica in Italia*, "Ricerche Storiche", XLI, 3, 2011, pp. 541-550; Id., *On working-class environmentalism: a historical and transnational overview*, "Interface: a journal for and about social movements", 4, 2, 2012, pp. 61-80; Serenella Iovino, *Dioxin, Power, and Gendered Bodies in Laura Conti's Narratives on Seveso*, in Greta Gaard, Simon Estok and Serpil Oppermann (eds.), *International Perspectives in Feminist Ecocriticism*, Routledge, New York 2013. Conti was also the author of several technical articles, useful to promote immediate and efficient emergency relief policies: Laura Conti, *Le faticose tappe alla ricercar della tremenda diossina*, "l'Unità", 6 August 1976, p. 4; Id., *Come e quando decontaminare*, "l'Unità", 14 August 1976, p. 4; Id., *Defoliazione e scortecciamento per fermare la diossina*, "l'Unità", 26 August 1976, p. 4.

32 Id., *Se il veleno si accumula*, "l'Unità", 27 July 1976, p. 3; cf. Id., *Scetticismo e sconforto: due atteggiamenti sbagliati,* "l'Unità", 23 August 1976, p. 4.

33 Id., *Chi indaga e chi provvede*, "l'Unità", 21 September 1976, p. 3.

All reflections converged in a polemical essay published on the first anniversary of the scandal, with the provocative title "When profit constrains science". Conti stressed the "urgency of making the productive uses of environmental resources compatible [...] with the defence of living conditions". For her, science could not be inhibited and instrumentalized to the logic of profit; it must "explore [...] the problems caused by the instrumentalization to which it has been subjected, and rigorously expose their gravity"[34]. It means to choose for a non-capitalistic science, as Conti has asserted on several occasions by proposing an alternative model based on the idea of a working-class science[35]. As it has been said, "Conti's eco-socialism had to be constructed at the point of production"[36]. Only starting from this background would it have been possible to think of a new approach, the ecological one: all reflections that were later arranged in the book *What is ecology* and recalled in the more evocative *Seen from Seveso*[37].

The principal themes emerged from *l'Unità*'s analysis were the same as those close to the Communist party's awareness: affinities over the Vietnam war, severe criticism of the capitalist system, and the defence of the working-class' health. All in favour of environmental protection[38]. Although biased, the leftist narrative was critical against the dominant (bourgeois) narrative; indeed, the leftist narrative drew attention to the sociological characteristics of the Seveso disaster, but from a scientific perspective. The result was a criticism of the neutrality of science that, whilst not explicitly mentioned, was clearly implied.

The bourgeoise's perspective

The arguments advanced by the bourgeois and pro-industrial press were different, even though Pietro Ottone was in his final stage as editorial director of the *Corriere* and had, earlier, taken the newspaper on a less conservative direction. However, the debut of the *Corriere della Sera*, after the first confusing news, was equally dramatic: with the aim of showing the health consequences, the first photographs of a poisoned citizen were reported with the scaremongering headline "A mysterious gas that kills plants and animals

[34] Id., *Quando il profitto condiziona la scienza*, "l'Unità", 10 July 1977, p. 9.

[35] Id., *Il topo e la scienza*, "l'Unità", 19 October 1977, p. 3.

[36] S. Barca, *Labour and the ecological crisis: The eco-modernist dilemma in western Marxism(s) (1970s-2000s)*, "Geoforum", 98, 2019, pp. 226-235, p. 230.

[37] L. Conti, *Che cos'è l'ecologia*, Mazzotti, Milano 1977; Id., *Visto da Seveso*, Feltrinelli, Milano 1977.

[38] E. Elena, *Il flagello della diossina*, "l'Unità", 10 July 1977, p. 9.

invades a city: fourteen children intoxicated"[39]. It was underlined how the gas had been lethal[40]. From the outset, the potential environmental and health damages were highlighted, renaming the toxic cloud "poison" on several occasions. According to scientists, the toxic substance was considered to be "the most toxic poison in the world", words reported in the insert *Corriere d'informazione*[41], and even the gas was believed to be more dangerous than the others used in the war[42]. However, there was very little scientific and medical knowledge to make such an accurate statement.

The *Corriere* editorial staff didn't need to compare the toxic substance to the gas used in Vietnam, or at least without the obsessiveness usually displayed by left-wing newspapers[43]. But Umberto Eco, in the pages of the *Corriere* itself, claimed that the lack of discussion was startling, admitting: "it is surprising [...] that sixty thousand tons of [...] dioxin were dropped on Vietnam, and in the end, it did not shock anyone"[44]. In their later analyses of the scandal, journalists thought it was more advantageous to blame other western countries: they were accused of espionage operations, of not sharing medical knowledge, of lack of interest in the Italian problem, of suspicious partnerships with ICMESA, and were seen as determined to limit the economy of Brianza[45].

The problems highlighted by the Milanese newspaper were others: not only scientific, ecological or health-related, but also economic[46]. Some companies were so threatened by the consequences of the toxic cloud that journalistic rhetoric went so far as to call the drama a "psychosis": "The craft industry has been severely hit by the toxic cloud [...] The future of our craftsmen is threatened and, with it, one of the productive activities for which we still have reason to be proud"[47]. Sometimes unwise measures were taken to completely shut down factories that were absolutely in order[48].

39 Andrea Bonanni, *Un gas misterioso che uccide piante e animali invade un paese: quattordici bimbi intossicati*, "CdS", 18 July 1976, p. 1.

40 Editorial, *Nube tossica: le immagini del dramma di Seveso*, Ivi, p. 15.

41 Sergio Angeletti, *È il veleno più potente del mondo*, "Corriere d'informazione", 27 July 1976, p. 1.

42 A. Bonanni, *Così pericoloso che non si usa nemmeno in guerra*, "CdS", 22 July 1976, p.1.

43 *L'Unità* was not the only newspaper spreading this comparison; for example, it was usual for *Il Manifesto*, representative of the radical Left universe.

44 Umberto Eco, *Dodici pensieri su Seveso*, "CdS", 17 August 1976, p. 3.

45 Augusto Pozzoli, *Manovre di spionaggio internazionale intorno al filo spinato della zona A*, "CdS", 11 August 1976, p. 7; Anon., *La Nato smentisce ogni rapporto con l'ICMESA*, "CdS", 5 August 1976, p. 5; M.B., *La Svizzera rifiuta le derrate brianzole provenienti da Seveso*, "CdS", 5 August 1976, p. 5; Anon., *L'antidoto? Gli americani ce l'hanno ma è un segreto militare e tacciono,* "CdS", 5 August 1976, p. 5; Anon., *Documento segreto Nato sui danni della diossina*, "CdS", 24 August 1976, p. 9.

46 Francesco Cevasco, *Chi paga per la nube*, "CdS", 12 August 1976, p. 2.

47 Barbara Visentin, *La psicosi della diossina minaccia l'attività di trentamila artigiani*, "CdS", 14 August 1976, p. 7.

48 Giuseppe Di Stefano, *La psicosi del veleno*, *Ibidem*.

In the *Corriere*, the State was put under accusation instead of the industrial system: a paradoxical turnaround from the Left's thesis. For example, the absence of regulations was an argument for blaming Italian politics: "Italy is totally caught off guard in the presence of these dangers. These are of two types: health and economic problems"; thanks to local laws, "ecologically problematic productions are made in Italy" rather than abroad. The conclusion was clear: "Instead of complaining about the misconduct of others, it will be better to take legal remedies, but first of all on the cultural ones, [...] which is the essential precondition"[49]. In this scenario, scientific aspects become secondary in most cases.

After all, science was reassessed in order to defend the goodness of a few technology-driven industries. Liberals asked themselves: Why condemn everyone? Wise scientific policies would be enough: the problem was related to regularize the relationship between science and politics. Scientific uncertainty was caused by political compromises: poor research, expensive instruments and mediocre libraries, even in Milan, "the capital of research and of the pharmaceutical industry"[50]. As the scientists pointed out, the USA provided a proper model to follow[51]. In the *Corriere*, the question of the neutrality of science and its political use was also discussed, but from an opposing point of view and often in the "Science and Technology" pages.

In March, seven months after the accident, the newspaper hosted a panel discussion on Seveso, involving scientists, technicians and politicians. Most recently, the militant left-wing magazine *Sapere* has published a monograph on "alternative" information about the Seveso debate. The meeting was also attended by Luigi Mara, a member of the journal, who was one of the founders of the *Medicina Democratica* movement in 1976[52]. For him, who contested the industrial system, information was all filtered by *La Roche* (the multinational corporation that controlled ICMESA): for this reason, he was convinced that newspapers and other mass media were exclusively reporting misinformation. This meant supporting "owner's science"[53]. Seen as an extremist, Mara was easily dismissed as a demagogue who distrusted scientific methods. After all, he had to deal with moderate politicians and private researchers.

[49] Luciano Caglioti, *Vita difficile in Svizzera per una fabbrica come l'ICMESA*, "CdS"¸ 13 August 1976, p. 2.

[50] Angelo Di Nicolin, *L'incertezza della scienza sulla tragedia di Seveso*, "CdS", 1 September 1976, p. 16.

[51] Vittorio Sgaramella, *Controllo pubblico negli Stati Uniti sulle ricerche rischiose per la comunità*, *Ibidem*.

[52] Cf. L. Centemeri, *Medicina Democratica and the Seveso Disaster: lights and shadows of the Italian movement for environmental health in the 70's*, "HAL", 2009, pp. 1-18.

[53] Editorial, *C'è disinformazione? Accusa e difesa*, "CdS", 17 March 1977, p. 17.

As it turned out, the *Corriere*'s narrative was evidently positioned in such a way as to support industrial prerogatives and bourgeois ideology: pointing at those scientists who were not in line with the dominant view (and reporting such political statements) as "protesters" was functional in politicising and polarising the discussion[54]. However, the actual issue was that scientists and technicians were facing something scientifically unknown: dioxin. Probably, despite the biased evidence, there was a lack of certainty. By defending the capitalist view, the *Corriere* seemed less sensitive to environmental and health issues. The newspaper undoubtedly showed its affinity with people's problems and local obstacles, its concern for health risks (of citizens, not workers), but often preferring the economic and political implications.

Conclusion: The American difference

As expected, the events had an international impact. It is interesting to notice how the events in Seveso were debated in the United States, following the liberal and progressive view of *The New York Times*, representative of the instances of the Democratic Party. Consciously simplifying this complex issue, it could be considered a 'balanced' position, suspended between the demands of the bourgeoisie and the representation of the American soft Left. This cannot be compared to communist ideology or even the industrial perspective of the Italian center-right. It represents an additional point of view with different prerogatives.

The *NYT* did not report what happened in Italy until July 25, even though it was widely known that "an explosion at a chemical factory produced a toxic cloud a fortnight ago"[55]. The information was launched as a flash news. In the following month, all the news was reported with the purpose of informing about updates abroad, with no particular point of view: the situation was described, improvements in damage limitation were emphazised, and developments in health and environmental issues were accounted for[56]. It was also

[54] eg. Anon., *Golfari: 'I ritardi di Seveso sono colpa degli scienziati'*, "CdS", 2 January 1977, p. 11; Editorial, *Le due verità su Seveso a confronto*, "CdS", 16 March 1977, p. 17; Silvio Garattini, *Polemica tra scienziati sulla lotta alla diossina*, "CdS", 9 Aprile 1977, p. 9; Francesco Metrangolo, *Contro la diossina tutto da rifare?*, "CdS", 21 April 1977, p. 1; Anon,, *Il prof. Liberti chiarisce i termini della polemica*, Ivi, p. 13.

[55] Reuters, *179 Italians to Be Moved From Poison Gas Area*, "The New York Times" [NYT], 25 July 1976, p. 15.

[56] Editorial, *20 More Evacuated From Area in Italy Hit by Poison*, "NYT", 29 July 1976, p. 3; Christina Lord, *Italian Leaders Take Up Poison Chemical Problem*, "NYT", 31 July 1976, p. 3; Editorial, *Italian wrangle over poison issue*, "NYT", 4 August 1976, p. 9; Steven V. Roberts, *Poisonous Cloud's Effects Still Baffle Italy's Officer*, "NYT", 13 August 1976, p. 3.

possible to find a reference to the situation in Vietnam in the *NYT*, which indulged in a kind of self-criticism. Reporting the Italian government's request to consult a Vietnamese expert, a reporter stated: "During the Vietnam war, Dr. Ton That Thut of Hanoi's Viet Duc hospital developed a method of treating persons afflicted by chemicals used by United States forces to defoliate jungle hiding places of Communist troops"[57].

The use of the defoliant (the infamous and long-hidden "Agent Orange", composed of dioxin) was clearly admitted[58]. And it happened again a few times later, defining the gas "one of the most toxic substances known"[59]. Perhaps the war, which had ended a year earlier, was now considered frozen in the recent past. In addition to this, much attention – probably due to the juxtaposition of Italy and Catholicism – was devoted to a controversial issue: the right to abortion. For the first time ever, the Italian government was granting the possibility to interrupt a pregnancy because of the risks of the effects of dioxin on newborns. As the newspaper reported, this topic, which used to be a *classic* on the political agenda, came to its peak: "The theological and legal issues surrounding abortion have become a major theme in the wake of last month's spill of highly poisonous chemicals from a factory near the northern city of Milan"[60].

The Italian backwardness of the debate – admittedly complex – was judged reactionary by Americans' progressive and imperialist gaze. For example, the Catholic newspaper *L'ossevatore romano*, published in the Vatican and widely read in Italy, launched its provocations against "insistent laic propaganda"[61], reiterating to its readers that "any kind of abortion is considered illicit because it is the killing of a human being" according to Catholic morality[62]. Even for medical reasons. But the legal-religious topic was not the only one dealt with.

Ecology became gradually central to the narrative proposed by the *NYT*. There was a different sensitivity to environmental issues in the United States; a keen consciousness had arisen since the post-war period and, more recently, was linked to post-1968 criticism[63]. For this reason, the newspaper stressed

57 Editorial, *Italy seeks hell in Hanoi on fumes*, "NYT", 2 August 1976, p. 2.

58 Cf. Peter Sills, *Toxic war: the story of agent orange*, Vanderbilt University Press, Nashville 2014.

59 Editorial, *Seveso's People, Long After Blast, Still Live in Fear*, "NYT", 25 November 1977, p. 48.

60 C. Lord, *Abortion Debate Heats Up in Italy*, "NYT", 9 August 1976, p.7.

61 Editorial, *A Seveso cento giorni dalla nube*, "L'osservatore della Domenica" [OD], 14 November 1976, p. 7.

62 Federico Alessandrini, *Seveso e l'aborto*, "OD", 29 August 1976, p. 3.

63 Cf. Scott H. Dewey, *Working for the environment: organized labor and the origins of environmentalism in the United States, 1948–1970*, "Environmental History", 3, 1, 1998, pp. 45-63.

that "Seveso has been the scene of an ecological disaster that sounds the alarm about humanity's fatally laggard approach to the problems of chemical contamination"[64]. At the same time, concern also increased due to the explosion of another chemical plant in Manfredonia, southern Italy, which occurred a while later and was described as a potential "new ecological disaster"[65]. The Americans' hope was that these accidents would "raise to new levels Italy's awareness of the environment as a problem, and lead to new questions about the course of industrialization"[66].

A year after the catastrophe, Barry Commoner, the American biologist sent to Italy to oversee the situation in Seveso, making a comparison between the United States and the world, criticized the petrochemical industry for the risks it was causing. As he stated, "the petrochemical industry, that elegant new alchemy of our times, dramatizes the paradox of modern technology: its blessings are mixed with plagues". The biologist included Seveso, along with Michigan and Virginia, in a list of the worst recent environmental incidents. In his conclusion, it was necessary to "balance between the hazards and benefits of the petrochemical industry"[67]. With regard to health and the environment, this was a public emergency, with implications for the economy and social welfare. Commoner, by the way, was aware of the Italian situation.

Despite a general progressive approach, the American newspaper showed a neutral narrative, characterized by a detached perspective as well as less involvement in the issue. On the other hand, it is worth noting that when there were no direct implications, it was easy to reduce the narrative to facts. This was probably the only way to be neutral, both scientifically and ideologically. This attitude was impossible to find in Italy, at the centre of political confrontation, where attitudes were Manichean. There were too many areas and hegemonic interests to give the public simple *objectivity*, as it can interpreted in modern terms and as a historical category[68]. It was also complicated to build a common front on ecological and medical issues. In Italy, countering the opposing narrative was more important than coordinating the struggle for compelling political and scientific purposes. The loss of neutrality meant a loss of possibilities.

The neutrality of science was a hard problem for Italian intellectuals, culminating in a radical critique of science itself[69]. Based on this assumption, the Seveso debate was only a 'minor' detail, but in line with the widening range

64 *Seveso disaster*, "NYT", 19 August 1976.

65 *New ecological disaster feared in Italian blast*, "NYT", 1 October 1976, p. 95.

66 Melton S. Davis, *Under the poison cloud*, "NYT", 10 October 1976, p. 220.

67 Barry Commoner, *The promises and perils of petrochemicals*, "NYT", 25 September 1977.

68 Cf. Lorraine Daston and Peter Galison, *Objectivity*, Zone Book, New York 2007.

69 I have discussed this issue in detail in Fabio Lusito, *Un marxista galileiano. Scienza e società in Lucio Lombardo Radice*, Meltemi, Milano 2023, pp. 267-313

of critics. Strongly arguing about the use of science in a capitalist society, the controversy led to fratricidal disputes between militant scientists and promoted – precisely in 1976 – the publication of controversial books such as *The Bee and the Architect* or *Marxism and natural science*, highly symbolic of this heated cultural season[70]. Nevertheless, there was the exploit of new explanations derived from the socio-political echo that came with 1968. Above all, there was a restoration of history in the reading of scientific phenomena, which emphasised external effects. To retain science' objectivity an ideological invention got usual for a multitude of Marxist scientists attracted by the history of science[71].

The drastic criticism developed in Italy has been considered a model of good scientific practice by foreign observers linked to radical science movements[72]. In a few cases, the crisis has offered the opportunity to rethink the very role of science. In this context, moving away from the exclusive horizon of institutional parties and the mainstream media, the duty to pursue ecological and health fights (i.e., the work of *Medicina Democratica* or the born of the environmentalist association *Legambiente*, in the second part of the 70s) has become a prerogative of youth's movements and non-systematic scientists. The same occurred for the meta-reflection on science. In such a short period of time, thanks to its worldwide resonance, Seveso accident has contributed to the awakening of scientific consciences on various fields and invited institutions to update the environmental law[73]. It was time to abandon the old path.

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[70] Giovanni Ciccotti et. al., *L'ape e l'architetto*, Feltrinelli, Milano 1976; Angelo Baracca and Arcangelo Rossi, *Marxismo e scienze naturali*, De Donato, Bari 1976.

[71] Ivi, p. 74.

[72] Hilary Rose and Steven Rose, *The radicalisation of science*, MacMillan, London 1976, p. 31; cf. Jean Marc Lévy-Leblond and Alain Jaubert, *(Auto)critique de la science*, Editions du Seuil, Paris 1975.

[73] Astrid M. Kirchhof and J.R. McNeill, *Nature and the iron curtain: environmental policy and social movements in communist and capitalist countries, 1945–1990*, University of Pittsburgh Press, Pittsburgh 2019; A. Candela, *Storia ambientale dell'energia nucleare: gli anni della contestazione*, Mimesis, Milano 2017; Giorgio Nebbia, *Scritti di storia dell'ambiente e dell'ambientalismo 1970-2013*, Fondazione Micheletti, Brescia 2014; Donatella Della Porta and Mario Diani, *Movimenti senza protesta? L'ambientalismo in Italia*, Il Mulino, Bologna 2004; Roberto Della Seta, *La difesa dell'ambiente in Italia: storia e cultura del movimento ecologista*, Franco Angeli, Milano 2000. Because of Seveso important environmental European directives were released: European Directive 82/501/CEE, known as "Directive Seveso", adopted in Italy only in 1988 as D.P.R. n.175/1988.

A Contribution to the Pasteur-Pouchet Controversy: Pouchet as a Philosopher and Historian of the Natural Sciences

Antonio Piccolomini d'Aragona*

Abstract: Building upon Raynaud's analysis of the controversy between Pasteur and Pouchet over spontaneous generation, I single out the philosophical and historical reasons which led Pouchet to defend the idea that life can appear in the absence of parents. In particular, I retrace the theological arguments used by Pouchet for showing that spontaneous generation copes with the Christian doctrine. Then, I provide an outline of Pouchet's interpretation of the development of natural sciences in the Middle Ages. Finally, I argue that Pouchet's endorsement to spontaneous generation was determined by his looking upon Albert the Great's philosophical and scientific heritage as a combination of *both* theological orthodoxy *and* experimental attitude, a mix that satisfies the metaphysical and epistemological tenets of Pouchet's philosophy of the natural sciences.

Keywords: Pasteur, Pouchet, spontaneous generation, controversy, SSK.

1. *Introduction*

The controversy between Pasteur and Pouchet is a paradigmatic case-study in the *sociology of sciences* – in particular, in the sub-field known as *sociology of scientific controversies*[1]. The debate took place in France in the

[1] Some titles are John Farley and Gerald Geison, *Science, politics, and spontaneous generation in Nineteenth-Century France: the Pasteur-Pouchet debate*, in *Bulletin of the history of medicine*, 48, pp. 161-198, 1974; Erich Mendelsohn, *The political anatomy of controversy in the sciences*, in H. Tristam Engelhardt and Arthur L. Caplan (eds.), *Scientific controversies*, Cambridge University Press, New York, 1987, pp. 93-124; Bruno Latour, *Pasteur et Pouchet: hétérogenèse de l'histoire des sciences*, in Michel Serres (ed.), *Élements d'histoire des sciences*, Bordas, Paris, 1989, pp. 423-445; Harry M. Collins and Trevor J. Pinch, *The Golem. What everyone should know about science*, Cambridge University Press, Cambridge, 1993; and Dominique Raynaud, *Scientific controversies. A socio-historical perspective on the advancement of science*, Transaction Publishers, New Brunswick and London, 2015. The best source book on Pouchet is undoubtedly Marilyne Cantor, *Pouchet savant et*

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years from 1859 to 1864, and involved Pasteur, who stands in need of no presentation, and Pouchet, who was the Director of the Natural History Museum in Rouen, and who had made himself a name with studies and discoveries in botany and, above all, biology – in the latter with e.g. relevant results into the ovulation of mammals[2]. The dispute originated from a prize that the *Académie des Sciences* intended to award whom had settled the experimental validity of *spontaneous generation* (henceforth SG), i.e. the theory that new organisms may appear without parents, and by purely environmental causes. Pouchet had just published *Hétérogénie, ou Traité de la génération spontanée*[3], a 672-page treatise where he defended SG with both metaphysical arguments and alleged experimental evidences – and which he possibly considered as his masterpiece. Pasteur had in fact been stimulated to deal with SG by Pouchet himself, who asked him about a note on lactic fermentation appeared in the *Comptes rendus* of the Academy, where Pasteur seemed to question the conclusions of Pouchet's *Hétérogénie*. After a number of vicissitudes, the controversy eventually resulted in the complete victory of Pasteur, mainly thanks to his 1861 *Mémoire sur les corpuscules organisés qui existent dans l'atmosphère*, a seminal paper in germ theory, which won him the *Alhumbert Prize* competition in 1862[4].

I will not dwell upon the chronological and conceptual development of the debate, which has been told and retold many times, and from many different (not always compatible) standpoints. Nor will I dwell upon its episte-

vulgarisateur. Musée et fécondité, Z'éditions, Nice, 1994. See also Bénédicte Percheron, *Les sciences naturelles à Rouen au XIXe siècle. Muséographie, vulgarisation et réseaux scientifiques*, Éditions Matériologiques, Paris, 2017, pp. 210-218, for the placing of Pouchet's theories on spontaneous generation in the scientific and cultural context of natural sciences in Rouen in the 19th century – Percheron speaks of a Rouen's "school of spontaneous generation".

[2] Pouchet's major works in these fields are Félix Archimède Pouchet, *Zoologie classique ou histoire naturelle du règne animal*, Roret, Paris, 1841; F.A. Pouchet, *Théorie positive de la fécondation des Mammifères*, Paris, Roret, 1842; F. A. Pouchet, *Théorie positive de l'ovulation spontanée et de la fécondation des mammifères et de l'espèce humaine. Ouvrage qui a obtenu le prix de physiologie expérimentale à l'Académie royale des Sciences de Paris au concours de 1845*, Baillière, Paris, 1847. In the books about the ovulation of mammals, Pouchet introduces ten laws, known as *Pouchet's laws*. For these laws, as well as for further details about Pouchet and his complete bibliography, see M. Cantor, *Pouchet savant et vulgarisateur*, cit. For the scientific context, see B. Percheron, *Les sciences naturelles à Rouen*, cit. Concerning Pouchet's work, besides an obvious reference to Cantor's major monograph, Percheron also mentions Landreat's doctoral dissertation in medicine which, however, I could not find anywhere – precise reference as given in Percheron's book: A. Landreat, *Félix-Archimède Pouchet, précurseur de la cytologie vaginale*, University of Rouen, doctoral dissertation in medicine, supervisor R. Laumonier, 1974.

[3] F.A. Pouchet, *Hétérogénie, ou Traité de la génération spontanée*, Baillière, Paris, 1859.

[4] Louis Pasteur, *Mémoire sur les corpuscules organisés qui existent dans l'atmosphère*, ain P. Vallery-Radot (ed.), *Oeuvres de Pasteur. Vol. 2: Fermentations et générations dites spontanées*, Masson, Paris, 1922, pp. 210-320.

mological and sociological evaluation[5]. Rather, I will concentrate on one of these evaluations, i.e. the one put forward by Raynaud[6], and use it as a starting point for investigating the philosophical stances which, based on what one can read both in *Hétérogénie* and in *Histoire des sciences naturelles au Moyen Age*[7], seem to underlie Pouchet's endorsement of SG.

In his reconstruction of the controversy, Raynaud has suggested that Pouchet's faith in SG, although supported by alleged experimental evidence, may have relied upon mainly metaphysical grounds. Also, given that some outcomes of Pouchet's former researches on *infusoria* seem to contradict SG[8], and given that, between these researches and *Hétérogenie*, Pouchet had worked for almost ten years on his *Histoire*, Raynaud infers that Pouchet's "conversion" to SG might be traced back precisely to *Histoire*, and in particular to the *apologia pro* Albert the Great that he puts forward therein.

The main claim of my paper is that both these conclusions of Raynaud are correct and, more precisely, that they are *jointly* correct: Pouchet's endorsed SG since he found, in what he calls the "experimental school" of Albert the Great, an ideal mix of metaphysical and theological orthodoxy, on the one hand, and of scientific concern for empirical matters on the other. This is in line with the broader philosophical remarks that Pouchet raises at the end of the metaphysical chapter of his *Hétérogenie*.

In Section 2, I sum up the points of Raynaud's interpretation which are more relevant to my purposes. In Section 3, I provide an outline of Pouchet's theological arguments in support of SG, as they are found in *Hétérogenie*. In Section 4, I discuss Pouchet's view on the history of the natural sciences in the Middle Ages, in particular the importance he deserves to Albert the Great. In Section 5, I return to *Hétérogenie*, and argue that the perspective under which Pouchet looks upon the tradition dating back to Albert the Great copes with Pouchet's privileged philosophical attitude in biology and akin sciences.

2. *Raynaud on Pouchet*

Raynaud's analysis of the controversy between Pasteur and Pouchet[9] is in many respects critical to others that have been put forward in the literature[10].

[5] See footnote 1.

[6] D. Raynaud, *Scientific controversies*, cit.

[7] F.A. Pouchet, *Histoire des sciences naturelles au Moyen Age: ou, Albert le Grand et son époque considérés comme point de départ de l'école expérimentale*, Baillière, Paris, 1853.

[8] F.A. Pouchet, *Notes sur les organes digestifs et circulatoires des animaux infusoires*, in "Compte Rendus de l'Académie Royale des Sciences", 28, 1848, pp. 516-518.

[9] D. Raynaud, *Scientific controversies*, cit., pp. 51-94.

[10] See footnote 1.

Latour is e.g. said to violate Bloor's *principle of symmetry*[11], while aiming to respect it. *Contra* Latour's claim that: "the winners do not need to be *protected* by the historian, but the losers do, to whom one will grant [...] *a second chance* before the *tribunal of history*"[12]. Raynaud remarks that Bloor's principle

> signified that one must treat beliefs, whether they are true or false, in the same way [...] yet the very notion of "defending the vanquished" contradicts the principle [...] one is no longer defending the "strong" but the "weak"[13].

To what he calls sociological relativism, too often hesitant between historical description and historical judgment, Raynaud prefers Merton's standpoint based on the identification of ethical principles which should normatively guide one's scientific activity, and which may hence play the role of coordinate system for the sociological assessment of scientific controversies[14]. According to Raynaud, many sociological depictions of the Pasteur-Pouchet debate thus suffer from two flaws, both stemming from the tendency to side with the vanquished and, therefore, to counter-balance an allegedly biased *a posteriori* evaluation with an equally unbalanced judgement of opposite sign:

(a) the over-estimation of the importance of certain asymmetries, and the consequent concealment of others, which instead qualify as sociologically significant;

(b) the idea that the duality between defenders and opposers of SG overlaps with other, deeper dualities at play in the philosophical, scientific, or more generally cultural *milieu* of the controversy, e.g. radicals *vs* conservatives, materialists *vs* spiritualists, creationists *vs* evolutionists, and so on.

In what follows, I will not deal with point a) above. I will just limit myself to remarking that Raynaud convincingly maintains that many asymmetries, which are supposed to show that Pasteur occupied a sociologically stronger position than Pouchet, are in fact pseudo-asymmetries. They can be often turned upside down, in favour of Pouchet, or at least return a substantial identity in social strength between the two poles. On the other hand, Raynaud highlights many hidden asymmetries, in the light of which Pouchet may be decidedly said to stand in a better position than Pasteur. This strategy eventually led Raynaud to discover a letter which, originally sent to Pouchet by Geoffroy Saint-Hilaire, was revealed (by stylistic and graphological anal-

11 David Bloor, *Knowledge and social imagery*, London, Routledge, 1976.

12 B. Latour, *Pasteur et Pouchet*, cit., p. 430, translation and italics as found in Raynaud's quotation.

13 D. Raynaud, *Scientific controversies*, cit., p. 81.

14 Robert K. Merton, *The sociology of science. Theoretical and empirical investigations*, Chicago, The University of Chicago Press, 1973.

ysis) to have been counterfeit by the former, in an anti-Mertonian attempt at improving his position in the debate[15].

Point b) is instead more relevant to my purposes. It is a largely shared view that Pouchet was handicapped by a division of the *forum* of the debate into a conservative-spiritualist-creationist pole, on the one hand, and a radical-materialist-evolutionist one on the other. Given the general situation of Napoleon III's France, the former pole was at that time much stronger than the latter, and since opposers and defenders of SG were ascribed to the former and the latter pole respectively, Pouchet had a weaker starting position than Pasteur. This stigma was the sword of Damocles of his inevitable defeat, no matter how many and strong experimental evidences he would have produced in defence of SG.

While the duality between the two poles is unanimously looked at as relevant to the Pasteur-Pouchet controversy, the "spin" of the social effect of the duality undergoes – not so slight – distinctions. Some (like Latour[16]) take the content of the duality itself to have an influence on the scientific dispute, while others (like Geison[17]) maintain that, quite on the contrary, the dispute offered results which could be used the other way around, to animate the social opposition. Therefore, Raynaud remarks,

> some believe that sociological factors conditioned the scientific debate (society → science) over spontaneous generation, others detect the recovery and confirmation of ideological concepts from the scientific results (science → society)[18].

If now, following the "double spin" identified by Raynaud, we try to estimate the burden put on science by the social factors at issue (or vice versa), we end up seeing that it is at least questionable to claim that Pouchet had a worse position than Pasteur *simply because* he defended a theory that was looked upon as radical, materialist or evolutionist. There are in fact two problems. First, by his explicit admission, Pouchet believed in *fixism* and, hence, he was against evolutionism[19]. Also, as we shall see in Section 3, the

[15] On this point, besides D. Raynaud, *Scientific controversies*, cit., see also D. Raynaud, *La correspondence de F.-A. Pouchet avec les membres de l'Académie des Sciences: un réévaluation du débat sur la génération spontanée*, in "European journal of sociology", 40, 1999, pp. 257-276.

[16] B. Latour, *Paster et Pouchet*, cit.

[17] G. Geison, *Pasteur*, in C. C. Gillispie, *Dictionary of scientific biography*, New York, Charles Scribner's Sons, 9, pp. 350-414.

[18] D. Raynaud, *Scientific controversies*, cit., p. 82.

[19] This Pouchet says in his post-*Hétérogénie* book *Nouvelles expériences sur la génération spontanée et la résistance vitale*, Paris, Victor Masson Et Fils,1864. A revealing quote is: "in spite of Mr. Darwin's claims, the fixity of the species is, for natural history as a whole, the most important and the best established fact, as Mr. Flourens put it", F.A. Pouchet, *Nouvelles expériences*, cit., p. 199. On this point see also D. Raynaud, *Scientific controversies*, cit., pp. 83-85. Percheron on the contrary maintains that, at least at the beginning of his career, Pouchet *did not* believe in fixism.

metaphysical part of *Hétérogénie* seems to cast little doubts on the fact that Pouchet was *not* a materialist. Finally, there seem not to be enough elements to establish whether, at the political level, Pouchet was a radical, or rather a conservative[20]. Second, the family of the heterogenists was a very variegated one, whence the dichotomy SG/evolutionism *vs* anti-SG/creationism may turn out to be nothing but a void sociological invention:

> one of the problems [...] in associating evolutionism and spontaneous generations rests […] on the diversity of opinions held by naturalists and microscopists. There were evolutionists heterogenists just as there were creationist heterogenists, evolutionists homogenists just as there were creationist homogenists, and hence there was a range of opinions from which one cannot reasonably draw any conclusion[21].

An example of this diversity may be given *precisely* by Pasteur and Pouchet. As said, Pouchet was not an evolutionist, although he defended SG; at the same time, before speaking of Pasteur as a creationist *qua* opposer of SG, we should bear in mind Raynaud's "double spin" *society → science/science → society*, in the light of which such an ascription may reveal itself to be only

> a "personal correlation" with no connection to the theory of the social determination of scientific knowledge, and at a worst a grave error of assessment which perceives the influence of social factors where in fact there was none[22].

However, Percheron also remarks that Pouchet's mentor, Alexandre-Louis Marquis, was indeed a fixist. See B. Percheron, *Les sciences naturelles à Rouen*, cit., pp. 101-111. I think one could safely claim that Pouchet moved from a *weak* to a *strong* faith in fixism throughout the development of his scientific beliefs. For the religious aspects in the Pasteur-Pouchet controversy, see also Juliette Azoulai, *L'imaginaire religieux dans la controverse sur la génération spontanée (Pasteur-Pouchet)*, in L. Dahan-Gaida, C. Maillard, G. Seginger and L. Talairach-Vielmas (eds.), *Penser le vivant*. Éditions de la maison des sciences de l'homme, Paris, 2021, pp. 175-192.

20 Raynaud himself remarks that Pouchet had relatively strong political connections, sometimes very close to Emperor Napoleon III (e.g., Napoleon's cousin Charles-Lucien Bonaparte), and that he often appealed to them for improving either his academic position, or his strength in other controversies, see D. Raynaud, *Scientific controversies*, cit., p. 64. However, given the development of Napoleon III's political attitudes from 1848 onward, I think that Pouchet's connections to the political establishment cannot serve to argue that he was a *conservative* – this would be as strong as claiming that Napoleon III has always been a conservative. Be that as it may, based on what is said in M. Cantor, *Pouchet savant et vulgarisateur*, cit., Pouchet's could be considered as a progressive rather.

21 D. Raynaud, *Scientific controversies*, cit., p. 86. Cantor also highlights that the interpretation of many basic terms in the debate between Pasteur and Pouchet was not neutral. Such words as *germ*, *heterogeny* or *spontaneity* were liable to different, and often incompatible readings, whose choice affected the scientific and sociological standpoint from which the different poles of the controversy looked at SG, see M. Cantor, *Pouchet savant et vulgarisateur*, cit., pp. 161-165. At the pages 167 and 168 of her book, Cantor also provides a most interesting discussion of the relation between SG and Darwinism within the context of the Pasteur-Pouchet controversy over SG. For the dissemination of such theories as evolutionism in Rouen's cultural and scientific context during Pouchet's times and beyond, see B. Percheron, *Les sciences naturelles à Rouen*, cit., in particular Chapter 2 of Part II.

22 D. Raynaud, *Scientific controversies*, cit., p. 86.

Now, there are two aspects of Pouchet's scientific attitude, both highlighted by Raynaud, and both relative to the "double spin" assessment of the controversy between Pasteur and Pouchet. The first is that "Pouchet supported his thesis [...] with proofs derived from his religious orthodoxy"[23]. The second is that Pouchet's "extra-scientific faith" in the validity of SG, which in fact contradicts some results obtained by Pouchet himself much before the publication of *Hétérogénie*[24], did not come out of the blue, but was "prepared" by Pouchet's work as a historian of the natural sciences in the Middle Ages. Pouchet's *Histoire* would indeed contain, for Raynaud, the key for explaining why and how Pouchet changed his mind about SG, as the book amounts to an *apologia* for Albert the Great, and thus "sheds light on the origins of the quasi-religious manner in which Pouchet would later address the questions of spontaneous generation"[25]. So, Raynaud claims two things. First of all, that Pouchet had mainly metaphysical and theological, rather than experimental reasons, for believing in SG. Secondly, that the source of these reasons is to be found, not only in the metaphysical and theological "proofs" put forward in *Hétérogénie*, but also in Pouchet's interpretation of the historical development of natural sciences in the Middle Ages, especially from Albert the Great onward. One may now ask how these two claims interact, i.e. whether Albert the Great and his legacy provided Pouchet with direct metaphysical and theological arguments, or with some other grounds that convinced the French naturalist of the truth of SG (or both these things). The answer actually stems from a passage of Pouchet's *Histoire* also quoted by Raynaud. Pouchet maintains that Albert the Great embraced

> the universality of both the human and the sacred sciences […] the natural sciences could appear in their fundamental character: their physical utility and their theological utility[26].

But how did this happen? What is the historical significance of Albert the Great, in the framework of (Pouchet's reconstruction of) the development of the natural sciences in the Middle Ages? How could this influence Pouchet's eventual faith in the validity of SG? Of course, we cannot expect Albert the Great to have offered any experimental evidences which could satisfy a naturalist of the 19th century. The answer must hence lie somewhere else. In the next sections, I set up to shed light on this issue, and thus to unfold some aspects which I take to be implicit in Raynaud's analysis, when he says e.g. that Pouchet interests in SG came from the

[23] Ivi, p. 83.
[24] F.A. Pouchet, *Notes sur les organes digestifs et circulatoires des animaux infusoires*, cit.
[25] D. Raynaud, *Scientific controversies*, cit., p. 84.
[26] F.A. Pouchet, *Histoire*, cit., pp. 214, 320, translation by Raynaud.

discovery of the medieval theory of heterogeny, referring to the model of Albertus Magnus [...] articulated within the framework of Christian theology[27].

3. *Pouchet's theological arguments in* Hétérogénie

The second chapter of *Hétérogénie* offers a theological, metaphysical and epistemological support to SG. Its importance within Pouchet's line of thought is underlined by a number of aspects. Besides the structural and quantitative role that, as remarked by Raynaud[28], the chapter plays in the overall architecture of the volume, we may for example quote from the Preface where, while specifying that

> this work can be naturally divided into two sections: the experimental part, which is the fundamental one, and the theoretical part, which is nothing but an ancillary fragment of the first[29],

Pouchet nonetheless admits that he was led to acknowledge the validity of SG, not by the experimental evidences which are to constitute the "fundamental part" of his volume, but by "meditation", i.e.

> as soon as, *upon meditation*, it became evident to me that spontaneous generation was still one of the ways employed by the nature for the reproduction of beings, I applied myself to discover the processes via which one could make these phenomena evident[30].

Also, in the first chapter of *Hétérogénie*, concerning the history of the different positions on SG from antiquity to Pouchet's times[31], the French biologist often presents the theories of his colleagues from the past as *rationally*

[27] D. Raynaud, *Scientific controversies*, cit., p. 84.

[28] *Ibidem*.

[29] F.A. Pouchet, *Hétérogénie*, cit., p. VII. Here and in what follows, the translations of Pouchet's passages are mine.

[30] *Ibidem*.

[31] It may be instructive to remark that, in this historical chapter, the section about the Middle Ages is the *shortest* one. Far from refuting Raynaud's claim that Pouchet's conversion to SG may have been determined by his work as a historian of the natural sciences in the Middle Ages, this seems rather to *confirm* the claim: it is *precisely* because he had already written a whole book on the history of the natural sciences in the Middle Ages, and because of the fact that this book was to his mind a sort of "preparation" to a major discussion of SG, that Pouchet deemed it unnecessary to return to the issue. This is symmetrical to what happens in Pouchet's *Histoire*, where we find very few references to medieval theories on SG (Albert the Great included), which led Raynaud to ask whether Pouchet "was [...] reserving a more thorough examination for a future study", in D. Raynaud, *Scientific controversies*, cit., p. 84. Significantly, Albert the Great is indeed mentioned in *Hétérogénie*, but in the *metaphysical* discussion.

fine, but incapable of being verified for lack of suitable scientific instruments. In the case of Buffon, Pouchet says for example that

> although starting from incorrect observations, the illustrious naturalist was nonetheless following a rational line of thought. It simply sufficed to bring his conceptions in the unknown molecular world[32].

Likewise, but the other way around, the moments when SG could count on more partisans have been for Pouchet those during which new instruments gave theoretical *hypotheses* a firm empirical grounding. E.g., in the section titled *Compound microscope* (i.e. optical microscope), Pouchet writes that

> the animalcules of infinite smallness, which this instrument revealed where their existence was expected the least, naturally led to believe they were born spontaneously. And maybe never could the hypothesis of heterogeny count on a more compact army of partisans than during this second stage of micrography[33].

In a nutshell, Pouchet's attitude towards SG seems therefore to be that a prior theoretical hypothesis, whose acceptability may vary depending on the current state of technological development, is then tested against experimental evidence which, again depending on the available instruments, is *expected* to ground it empirically. As we shall see in Section 5 below, this "teleological positivism" also depends on Pouchet's philosophical view on how biology, and natural sciences in general, should proceed – a standpoint that Pouchet shared with other naturalists of his times[34]. In the first part of the philosophical chapter of *Hétérogénie*, from page 95 to 105, Pouchet puts forward a number of arguments aiming to show that SG is *not* incompatible with the Christian orthodoxy. Many of the arguments presuppose some kind of creationism, so that SG is shown not to cope with evolutionism, and to reinforce the tie between science and religion. Ten arguments, often intertwined with each other, can be identified. Let me sum them up. The first six arguments are *theoretical-theological* in spirit, and they run as follows:

32 F.A. Pouchet, *Hétérogénie*, cit., p. 44.

33 Ivi, p. 39. The first stage that Pouchet refers to is that of the *simple* (i.e. non-optical) *microscope*.

34 The "teleological positivism" that I have referred to should not be confused with the idea that experiments, or more in general experience, *must* adapt to preconceived ideas. In some places, Pouchet criticises this attitude, especially in connection with empirical findings obtained thanks to new scientific instruments, whose biased interpretation is then *required*, rather than expected, to confirm a given theory. Pouchet is a positivist, whence he tributes much importance to an as "neutral" as possible treatment of facts. Still, as we shall see below, Pouchet is also influenced by the (mostly philosophical) tenets of the *Naturphilosophie*, whence his "teleological positivism". For this point, see M. Cantor, *Pouchet savant et vulgarisateur*, cit., pp. 60-61, p. 119, p. 130, and Nils Roll-Hansen, *Experimental method and spontaneous generation: the controversy between Pasteur and Pouchet – 1859-1864*, in "Journal of the history of medicine and allied sciences", 3, 1979, pp. 273-292.

(1) the *fiat lux* is nothing but an instance of SG, "working under divine influence"[35];

(2) if SG exists, then it was wanted by God. We may not be in the position to understand God's decision, but we should even the less oppose to it, if it is the case. Observe that this is a conditional, and partly ethical argument, which seems to involve two presuppositions: first, that phenomena, or at least some of them, exist because God wanted so; second, that SG is compatible with the natural world created or wanted by God;

(3) it would be an irrational, or at least unlikely, limitation of God's powers if He could not animate inert molecules, given that He can on the other hand

> survey the deepnesses of the skies, and weigh the globes spread in the immensity; then, too cramped in his Earth's shore, soar in the infinite spheres and penetrate the uncreated mysteries[36].

This argument is of course in line with argument (2) above.

(4) God can modify the course of natural history. For example, he can provoke the sudden disappearance of certain species, or even violate and reverse the immutable laws of the Universe through miracles and similar events. Why then, given such capabilities, should He not be able to create a mite in the absence of parents?;

(5) it is true that Genesis says that, after the 6th day of the creation, God took a rest. But Genesis *does not* say that God renounced forever using His creative faculty;

(6) even if we insisted on denying that God could actually operate a "daily"[37] spontaneous creation of new beings, we should not deny, on pain of limiting His omnipotence, that He could have laid down some laws of matter "determining the circumstances in which the organisational power can manifest itself, and give birth to new combinations"[38]. Such laws would ultimately make of SG nothing but a variant of "sexual reproduction, fission, budding, etc."[39].

The last four arguments are instead *biblical* in spirit, or more generally, they qualify as *ex auctoritate*, since they appeal to textual evidences to be found in the Scripture, and in the writings of some Church Fathers or of philosophers whose ideas have become part of the Christian dogma:

35 F.A. Pouchet, *Hétérogénie*, cit., p. 95.
36 Ivi., p. 96.
37 Ivi, p. 97.
38 Ivi, p. 98.
39 *Ibidem*.

(7) Pouchet repeats argument (5), as he remarks again that the author of the Genesis does not say anything about what God did after resting on the 7^{th} day, so that nothing impedes that He allowed himself to use again and again His creative faculty. But now Pouchet goes deeper into a sort of "exegetical analysis": since the account of such a magnificent event as the creation "barely takes few lines"[40], we should not expect to find a detailed description of what God did afterwards, since this "naturally derives from the Creator's supreme intelligence and relentless activity"[41]. This argument is partly based on other sources than the Scripture, which Pouchet uses for arguing that, even if the Genesis "does not entitle one to suppose that God could return to His creative activity"[42], it does not exclude either that He "forced himself not to return to His work". Rather,

> from place to place, the Scripture speaks against the rest which the Eternal spirit would be uselessly chained in. Instead, everything seems to indicate that He is never inactive[43];

(8) this latter fact also holds of the creation itself, i.e. "our sacred books speaks against the immobility which we pretend to pin on the creation"[44];

(9) Aristotle was a partisan of SG, so that we should not be surprised if his ideas on this topic come back in the writings of prominent Christian philosophers of the "nice times of the Church"[45], like Saint Augustin or Saint Jerome. As for Saint Augustin, his authority is to Pouchet the "evident proof that the pretences of the heterogenists have never derogated from orthodoxy"[46], and the French naturalist is ready to affirm that

> it is [Saint Augustin's] thesis that we aim at developing here, with the certainty and exactitude that sciences provide us with in the nineteenth century[47];

(10) Aristotle's theses on SG also occur in authors from the Scholasticism or the Society of Jesus. In the former case, Pouchet mentions his medieval champion, i.e. Albert the Great, "Saint Thomas' friend"[48].

While still philosophical in nature, the second part of the second chapter of *Hétérogénie* seems instead to be much less theological, and much more oriented towards some form of metaphysical epistemology of the natural sciences.

40 Ivi, p. 100.
41 Ivi, pp. 100-101.
42 Ivi, p. 100.
43 Ivi, p. 98.
44 Ivi, p. 101.
45 Ivi, p. 103.
46 *Ibidem*.
47 *Ibidem*.
48 Ivi, p. 104.

Before dealing with this, I shall go back to Pouchet's *Histoire*; this will be helpful for a better understanding of Pouchet's point of view in Section 5.

4. *Pouchet on Albert the Great and the natural sciences in the Middle Ages*

Pouchet's *Histoire des sciences naturelles au Moyen Age*, whose sub-title reads *Ou Albert le Grand et son époque considérés comme point de départ de l'école expérimentale*, is a 656-page treatise on the history of natural sciences in the Middle Ages[49]. It is organised in five chapters (plus preface and introduction), one for each of the five "schools" which, in Pouchet's analysis, the medieval history of the natural sciences can be split into. Here is a list of the schools, together with their respective page-numbers:

- Scandinavian school, from page 11 to 36;
- Franco-Gothic school, from page 37 to 110;
- Byzantine school, from page 111 to 137;
- Arab school, from page 138 to 202,
- experimental school, from page 203 to 614[50].

This is not to be taken also as an order of chronological development. In the introduction, Pouchet remarks that none of the schools can be

> circumscribed to a given period or a given country. The schools […] do not provide an identity of time, nor of places. Distinctive features are provided only by the forms which scientific doctrines are informed by[51].

However, this does not mean that all the schools are on a par. In fact, two (interrelated) remarks can be drawn from the list above. First, the experimental school takes more than two thirds of the book. For Pouchet, thus, this is the most relevant school, which "flows" directly into the modern tradition of the

[49] For an overview on the relation between Pouchet's historical work and the writing of histories of natural sciences in the 19th century, see B. Percheron, *Écrire l'histoire naturelle au XIXe siècle. Le style de Felix Archimede Pouchet*, in "Arts et savoirs", 14, 2020. For the relation with the wider context of the natural sciences in Rouen in the 19th century, see instead B. Percheron, *Les sciences naturelles à Rouen*, cit., in particular see Chapter 3 of Part I and Chapter 1 of Part II therein. See also M. Cantor, *Pouchet savant et vulgarisateur*, cit., pp. 74-77.

[50] While it is clear what the Franco-Gothic school, the Byzantine school and the Arab school are, the Scandinavian school requires some clarification. It is given by the "scientific" knowledge, mainly zoological and geographical in nature, stemming from Scandinavian sagas, or from documents connected to early Scandinavian kingdoms.

[51] F.A. Pouchet, *Histoire*, cit. pp. 9-10.

16^{th}-17^{th} century.[52] Second: in spite of the sub-title of the book referring to Albert the Great and his legacy as only the *starting point* of the experimental approach, Pouchet in fact considers Albert the Great and those following his theories as constituting a *full-fledged* form of experimentalism[53]. The latter is understood by Pouchet as the natural evolution of an observational attitude which constitutes in turn a *conditio sine qua non* for natural sciences. And it is precisely during the mid-13^{th} century that such an evolutionary step took place:

> in natural sciences, the observation stage comes necessarily first […]. Aristotle is maybe the deepest observer we can mention. But, after this great man, the rational method became weaker and weaker, and ended up being entirely lost. For a number of centuries, sciences abandoned the sole path along which they could make progresses, and stayed almost sterile into the hands of people whose work was just scholarly research. […] However, to these two periods inaugurated by antiquity, the Middle Ages added a third one: *experimentation*. The power and fruitfulness of the latter, which had been neglected up to that moment, and from which all the brightness of our current knowledge can be derived, was understood by two man of the 13^{th} century: Albert the Great and Roger Bacon[54].

The importance which, in this framework, Pouchet attributes to Albert the Great is seen already from how the part of the book devoted to the German

[52] It is not clear whether Pouchet would consider Renaissance science, or parts of it, as still belonging to the medieval period, or as being, rather, part of modern science. Because of Pouchet's conception of what he calls "experimental school", the commonly adopted criteria for the splitting of the history of science become much "fluid", so different ages overlap, in a somewhat stronger sense than when saying that authors at the crossroads of two periods belong to both these periods. Thus, e.g., Nicholas of Cusa, Regiomontanus and Copernicus are dealt with in the astronomy section of the experimental school, see F.A. Pouchet, *Histoire*, cit., pp. 614-622, and even Leonardo da Vinci is one of the authors mentioned in the section on geology, see Ivi, pp. 508-510.

[53] This is not to say, however, that Pouchet fails to see the differences between medieval experimentalism and modern science. In the introduction he writes for example that he does not aim at "pretending that the medieval age can be compared to ours. The centuries that the Middle Ages laboriously crossed cannot aspire to the fruitfulness or height that modern sciences have achieved", F.A. Poucher, *Histoire*, cit. p. 7. On the other hand, the reason for this seems for Pouchet to be mainly sociological and political: "the status of the society ostensibly explains the causes of this inferiority: the Roman Empire, stressed and languishing, suffered from sterility; the Francs and the people from Germany […] were still too young to give birth to mature productions". Ivi, pp. 7-8.

[54] F.A. Pouchet, *Histoire*, pp. 203-204. Observe that, in outlining the shift from the observational to the experimental stage, no role is attributed to the previous schools which Pouchet dealt with in first four chapters of his book. The approach stemming from Albert the Great and Roger Bacon would thus seem to be directly connected to the Greek and Latin traditions (and, forward, to modern science), not only because of some inherited knowledge – which was already the case for the other schools – but above all because of a shared (and improved) *method*. I think that, based on Pouchet's analysis, a preparatory epistemological role could be attributed to the other medieval schools too, but I cannot deal with this issue here.

philosopher and scientist is structured. While still being "heroic"[55] in spirit, i.e. based on a case-wise survey of single scientists, and while still providing an outline of the historical context which the authors' reference schools stems from, Pouchet overturns the order of the relation between Albert and the sciences he contributed to. For, whereas the other authors are subsumed to macro-sections concerning specific sciences, Albert is, so to speak, a macro-section in himself, which sciences are subsumed to. For example, in spite of the fact that Roger Bacon is said by Pouchet to be on a par with Albert as a founding father of the experimental school, his name is mentioned only in the section for Physics. Albert's section is instead structured as follows:

- general introduction and life;
- works;
- philosophy;
- zoology;
- botany;
- mineralogy;
- physics.

Where it is to be remarked that *each* of these sub-sections also occurs as a macro-section on its own in the chapter on the experimental school (as in the already mentioned case of physics, and as happens for the other schools)[56]. The description of Albert's life, works and theories starts with an *apologia* where it is difficult not to detect Pouchet's enthusiasm in presenting his medieval champion. Here is a piece of textual evidence:

> they called him *Magnus* […] after his vast knowledge in philosophy, theology, mechanics, chemistry, physics, and natural history. […] the Middle Ages cannot offer anything which surpasses him. He deserves the glory of having traced the widest picture of the human knowledge of those times […]. Deep scientist, and immense and immortal figure, he alone would suffice to glorify an entire age! For no one may have ever en-

[55] I am borrowing this way of describing Pouchet's style from M. Cantor, *Pouchet savant et vulgarisateur*, cit., p. 75.

[56] Let me also remark that the section on Albert the Great goes from page 210 (or the like) to page 320, i.e. one fourth of the whole chapter on the experimental school, and one sixth of the whole book. Therefore, this section is a sort of book in the book – perhaps the very initial project, which Pouchet *later on* expanded so to include all the Middle Ages? Be that as it may, this shows that Pouchet worked intensively on Albert the Great, which may confirm the hypothesis formulated by Raynaud, i.e. that the reason why in the *Histoire* there is no mention to Albert's theory of spontaneous generation is that Pouchet planned to expand this as a separate part of his work, later materialised with *Hétérogénie*. In this perspective, Pouchet's *Histoire* may be well conceived of a sort of "preparatory" book for *Hétérogénie*, where the earlier historical (and conceptual) background of the theory of spontaneous generation is shown and laid down.

joyed a greater intelligence than Albert since […] he may have attained the last term of human science[57].

Albert's prominence lies in the fact that he brought experimentalism into medieval natural sciences. This is an achievement he shares with others, like Roger Bacon, but one of the reasons why Albert's experimentalism qualifies as more important is that he was familiar with a *wide* range of sciences[58]. So, experimentalism led him to innovative results in much more fields than, e.g., just that of physics, as was for Bacon. In this sense, Albert offers a synthesis between the encyclopedic tradition which he inherits from the previous centuries (and which Pouchet had dealt with, for the European context, mostly in the chapter about the Franco-Gothic school), and a new scientific method which will later on materialise in Renaissance and modern science. Zoology and botany are, for Pouchet, two sciences where Albert's approach was most fruitful (and which are of special interest for us here)[59]. As for the first, Albert is credited with the discovery of the *animal series* and of the principle of zoological *classification*:

> here we find laid down for the first time the foundations of the animal series! A truly gigantic idea for an age where observation raised such insurmountable problems, and one which had to cross many further centuries before being accepted by the most prominent naturalists. As regards *classification*, our scientist […] starts by declaring the stability of the species that come into the domain of the creation. His method is entirely based on this point. […] But he goes further than that. For the first time he defines what a species is, and shows us the mechanism by which genders are drawn out of species[60].

As for botany, after depicting Albert's experimental achievements in such fields as vegetal anatomy or physiology, especially in connection with the reproductive organs of the plants, as well as in classificatory concerns

57 F.A. Pouchet, *Histoire*, cit., pp. 211-212.

58 For Albert's scientific work in the context of Albert's wider epistemology, see Loris Sturlese, *Il razionalismo filosofico e scientifico di Alberto il Grande*, in *Documenti e studi sulla tradizione filosofica medievale*, 1(2), 1990, pp. 373-426. For a general introduction, see Alain de Libera, *Albert le Grand et la philosophie*, Vrin, Paris, 1990, as well as David Twetten and Steven Baldner, *Introduction to Albert's philosophical work*, in I. Resnick (ed.), *A companion to Albert the Great. Theology, philosophy, and the sciences*, Brill, Leiden, 2012, pp. 163-172.

59 On zoology and botany in Albert the Great, see Gila Wöllmer, *Albert the Great and his botany*, in I. Resnick (ed.), *A companion to Albert the Great*, cit., pp. 221-267, Miguel de Asúa, *War and peace: medicine and natural philosophy in Albert the Great*, in I. Resnick (ed.), *A companion to Albert the Great*, cit., pp. 269-297, Marina Panarelli, *How do plants live and grow? Radical moisture and digestion in Albert the Great's* De vegetalibus, in "Quaestio", 20/2020, 2020, pp. 347-367, and M. Panarelli, *Converting death into life: spontaneous generation from Aristotle's biology to Albert the Great analysis of plants*, in "Quaestio", 22/2022, 2023, pp. 493-508.

60 F.A. Pouchet, *Histoire*, cit., pp. 278-279.

similar to those at play in zoology[61], Pouchet concludes that

> summing up Albert's writings on botany, we recognise he gave this science genuinely good grounds. […] In the 13th century he argued for the very same theses as those we develop today in our amphitheatres. […] In physiology he was, if not the precursor of our age, at least the daring explorer of a number of phenomena which we had to deepen rigorously afterwards[62].

However, it is clear from other substantive parts of Pouchet's presentation of Albert that the experimental achievements of the latter are definitively *not* the sole reason why he is worth being looked upon as the crucial scientist of the Middle Ages. Science is nothing without the right context. Science is nothing without (Christian) faith[63]. Albert's greatness depends on the fact that his having traced the widest picture of the human knowledge of his times, as mentioned in the quotation above, was by him "closed in a circle under the Christian viewpoint, encompassing nature, man, and God"[64]. And even the whole *point* of Albert's research is not science for the sake of itself, but

> the glorification of the Eternal. […] God is revealed to man through his words and works. The creation is the true domain of sciences, so that the latter have become the most powerful lever we can use for proving metaphysical ideas. Albert acknowledged this first, and first used the study of science to support the science of God or theology. This is how he closed the circle of our knowledge[65].

To my mind, the last quotation is crucial if we aim at understanding, not only Pouchet's conception of medieval natural sciences, but his conception of natural sciences *as such*. This is textually highlighted by the fact that Pouchet shifts from speaking of Albert as the one who closed in a circle the knowledge *of his times*, to the much stronger claim that this closure concerns *our* knowledge. So we are told that the domain of the natural sciences is, not

61 Ivi, pp. 302-307.

62 Ivi, p. 308.

63 It may be worth remarking that another author mentioned by Pouchet in connection with more than one science is Avicenna, who is dealt with in the chapter on the Arab school (of course) with reference to medicine, geology, and chemistry. That Avicenna is not for Pouchet as crucial as Albert the Great (possibly because he lacked precisely that Christian side which was so fundamental in the case of Albert) is shown for example by the editorial choice of not having a special section on Avicenna, with specific sub-sections for the sciences which he contributed to (as is for Albert). Rather, Avicenna is *one* of the authors mentioned in the sections of medicine, geology and chemistry in the Arab school, among many other names. See F.A. Pouchet, *Histoire*, cit., pp. 456-458, 477-478, 487-488.

64 Ivi, p. 211.

65 Ivi, pp. 214-215. For the relation between nature and creation in Albert the Great, see Maria Evelina Malgieri, "Universale latissimae universalitatis"*: origine della creazione e natura del fluxus nel* De causis *di Alberto*, in "Quaestio", 20, 2020, pp. 389-413.

the nature, but the creation. Natural sciences work as a lever for proving metaphysical ideas. Albert was the first to understand this, since he used natural sciences as a support for theology. In *this* way, he attained that closure thanks to which he can be considered as the most important scientist of the Middle Ages. And it is perhaps under a closure of this kind that, as suggested by Raynaud in the quotation I have reported at the end of Section 2, Pouchet may conceive of his theory for SG.

However, this is not to say that Pouchet's scientific work is biased by metaphysical ideas, nor that he cannot be considered under all respects as a positivist of his times. The point is not Pouchet's science, but Pouchet's view on what science is about. Of course, if the domain of science is the creation, and if the creation is nothing but the nature looked at from Christian perspective, then science must include an experimental method. But the experimental level is only part of the story. Again because nature is creation, after studying the phenomena experimentally we should go back from the *creata* to the *Creator*. It is of course possible to do science for the sake of itself, namely, without ascending to any transcendental plan. But science is best, maybe properly used as a lever for proving metaphysical ideas. We are thereby back to what in Section 2 I called the "teleological positivism" of Pouchet.

5. *Pouchet's epistemology in* Hétérogénie

Natural sciences are instrumental to proving metaphysical ideas. From this point of view, Pouchet's theological-exegetical arguments in support of SG in the first of part of Chapter 2 of *Hétérogénie*, which I reported in Section 3 above, are *not* misplaced. They could just complement the experimental part, and qualify as even more important than it, as they complete the transcendental ascent when going, so to say, from nature to creation[66]. Thus, the arguments may not simply play the role of providing Pouchet with some "precaution" against potential charges of radicalism, materialism, and evolutionism, but constitute an integral part of what is required to Pouchet's epistemological framework for it to function consistently.

Now, the nature, structure, and sources of this epistemological framework are very clearly stated by Pouchet himself in the second part of Chapter 2 of *Hétérogénie*, which starts by outlining some broad methodological tenets. Pouchet acknowledges the importance of the experimental attitude in the natural sciences:

[66] A very thorough reconstruction of Pouchet's scientific methodology, mainly as applied to his investigations into the ovulation of mammals and into SG, can be found in M. Cantor, *Pouchet savant et vulgarisateur*, cit., in particular pp. 110-112, 130, 151-152, 165-166.

the surest path to follow is, almost always, the experimental approach, which made natural sciences advance very much since Galilei. This approach consists in taking the observation of phenomena as a starting point, and in seeking their causes[67].

The experimental method works locally but, when referred to such delicate issues as SG, which may require one to answer some *ultimate* problems in a given scientific field, say the origins of life, it also proves to be insufficient:

elucidating the important issue of spontaneous generation requires one to appeal to all the resources of human mind, and to put into practice its noblest faculties. […] we can repeat here what Is. Geoffroy Saint-Hilaire said of natural sciences: the observation and the analysis are indispensable, but they are not enough. Reasoning and synthesis also play a role[68].

Therefore, when dealing with SG, we should bear in mind that experimentation is, on the one hand, a *conditio sine qua non* of natural sciences, but also that, on the other, it should not lead us to "the deepest scepticism for everything which does not derive from it"[69]. For this would mean "drying the most fruitful sources of every progress, i.e., the criterium of the intellect, which discusses and judges"[70]. The opposite would amount to disregarding the role played by experimentation, and demanding too much to reasoning, synthesis and intellect. This was the flaw of previous researches on SG, where scientists "pushed their pretences too far, by attributing to heterogeny a fancy power"[71], and this is what obtains in certain tendencies of the *Naturphilosophie* stemming from Schelling:

experimentation, as Schelling understands it, is just the verification of some pre-existing idea. According to his view, material facts should be investigated only for the sake of somehow confirming the prophecies of intellect. However, in our opinion, the German philosophy is here renouncing one of the most fruitful results of experiments. For, if it is true that the latter often torture the matter and its organisation, in order that it testimonies in favour of some pre-conceived ideas, it is also true that the scientist, by acknowledging unusual or unexpected facts that may occur during his operations, derives from that, *a posteriori*, some laws which would have never come up spontaneously[72].

One should thus seek a sort of intermediate way, where "intelligence and experimentation be strictly intertwined"[73]. Here, Pouchet's epistemological champion is Saint-Hilaire who, through an approach which is tied "neither

67 F.A. Pouchet, *Hétérogénie*, cit., p. 105.
68 Ivi, p. 106.
69 *Ibidem*.
70 *Ibidem*.
71 Ivi, p. 107.
72 F.A. Pouchet, *Hétérogénie*, cit., p. 108.
73 *Ibidem*.

to the temerity of the *philosophy of nature*, nor to the shy reservations of the *school of facts*"[74], is able to lead us in a framework where

> the intellect is fertilised by the facts [...] in its most daring deductions, it always relies upon known elements, abstracting theories from them. [...] We introduce in the natural sciences precisely the same elements as those introduced by the leading author of German philosophy but, contrarily to him, we aim to essentially *deductive* theories, rather than to *intuitive* theories[75].

But this also means that, in spite of the criticism to Schelling's approach, Pouchet is not ready to completely renounce a certain understanding of experimentation. That the latter is still looked at from an idealistic or romantic perspective clearly emerges from the first quotation above, where Pouchet illustrates what *rough* experimentalism is expected to be. *Contra* an as much neutral as possible reconstruction of the "schools of facts", Pouchet claims that experimentation is concerned, not with phenomena *as such*, but with a *causal* observation of them. This is in sharp contrast to a by then a classical way of conceiving of experimentation which, in France, had been developed by François Magendie, and which, in Pouchet's times, was upheld by, e.g., Claude Bernard[76].

In line with what Pouchet said in his *Histoire*, natural sciences are understood as a sort of peculiar framework where experimentation is not used for explaining facts in a case-wise manner, but for proving metaphysical ideas, which now boils down to abstracting general theories where facts can be given their right place within the context of a broader and all-encompassing picture[77]. On the other hand, however, the picture cannot obtain by merely putting facts of a given collection side by side. One needs a sort of bird's-eye view, which cannot be provided by experimentation, but stems from faculties of another sort, namely, reasoning, synthesis, and intelligence. The metaphysical flavour here is quite sharp, e.g. it copes well with the typical stance of medieval ontology that the Being is not reducible to a collection of *entia*[78]. A

[74] *Ibidem*, Pouchet's italics.

[75] F.A. Pouchet, *Hétérogénie*, cit., pp. 109-110, Pouchet's italics.

[76] On these points see e.g. Georges Dillemann, *L'éloge de Magendie par Claud Bernard*, in "Histoire de la médecine", 17(4), 1983, pp. 345-349, and Paul Mazliak, *François Magendie. Créateur de la pharmacologie, inspirateur des recherches de Claude Bernard*, in *La revue du praticien*, 63, 2013, pp. 1030-1033. For a more general point of view, one may refer to Laurent Loison, *L'expérimentation dans les sciences de la vie*, in C. Allamel-Raffin, J.-L. Gangloff and Y. Gingras (eds.), *L'expérimentation dans les sciences*, Éditions Matériologiques, Paris, 2022, pp. 51-64.

[77] One could argue that abstraction towards all-encompassing theories is not required, nor can it be, in what Pouchet takes chemistry to be, as shown by Pouchet's later criticism to Pasteur. See for example M. Cantor, *Pouchet savant et vulgarisateur*, cit., pp. 172-175, and D. Raynaud, *Scientific controversies*, cit., pp. 67-73.

[78] A typical reference here is St. Thomas Aquinas, *Concerning being and essence*, Aquinas Press, 2007.

theory of the kind which Pouchet has in mind here cannot reduce to a sum of facts observable in relevant experiments. It will transcend any and all of them by introducing some unity which is "seen" in an intellectual and synthetic way. SG is precisely expected to provide one such picture. Thus, Pouchet has now to apply his epistemological architecture to a justification of both SG as a licit scientific issue, and of the multi-faceted plethora of methods he will use throughout *Hétérogénie* to prove that, in some specific but relevant cases, SG is true[79].

This is what Pouchet does in the remaining pages of the second part of Chapter 2 of his book, starting from a classification of three hypotheses which, in his opinion, have been put forward for explaining the "phenomenon of life"[80]. The first hypothesis, which we may qualify as *materialist*, maintains that life essentially obeys the laws of the matter. It splits into two sub-groups, both stemming, in Pouchet's interpretation, from Descartes: *iatromechanicists* believe that life obeys *mechanical* laws, while *iatrochemists* believe it obeys *chemical* laws[81]. The second hypothesis, called by Pouchet *spiritualism*, claims that life depends on "an immaterial and intelligent principle, which grounds and mysteriously regiments all life acts"[82]. This has a degeneration, called by Pouchet the *mystic school* (e.g. Paracelsus)[83]. The third standpoint is *vitalism*. It "derives fraudulently from the previous two"[84], and argues that life results "neither from some immaterial and intelligent principle, nor from laws governing rough bodies, but from a particular force, which is inherent to organisms"[85].

Pouchet notes that "the essence of the vital principle is as difficult to determine as that of the immaterial principle"[86] used by the spiritualists. In spite of this affirmation, Pouchet seems to have some preference for spiritualism, particularly when the latter is "informed" by the biological flavour of the vitalist approach. Overall, Pouchet can be said to belong to what, in Lakatosian terms, we may call the "vital principle"[87] research programme. The latter constitutes a sort of compromise between materialism and spiritualism, so that it may cope very well with the idea that natural sciences abstract theories (spiritualim-*Naturphilosophie*) from rough data (materialism-experimentation)[88].

[79] For the multi-faceted character of Pouchet's argumentation strategy, see in particular M. Cantor, *Pouchet savant et vulgarisateur*, cit., pp. 174-175.

[80] F.A. Pouchet, *Hétérogénie*, cit., p. 114.

[81] Ivi, 114-115.

[82] Ivi, p. 115.

[83] Ivi, p. 115-116.

[84] Ivi, p. 116.

[85] *Ibidem*.

[86] Ivi, p. 118.

[87] *Ibidem*.

[88] Let me remark that SG is also understood by Cantor as a (Lakatosian) research programme

The alternative to an approach based on the idea of the existence of some "vital principle" or "vital force"[89] would be something in line with what Pouchet called materialism:

> Bichat and Broussais may very well protest that there is nothing in the organism but tissues or excitable and living devices, and that animism is a chimeric entity. Our inner sense revolts against such a pretension, which destroys human dignity. Everything suggests to the philosopher that, if it is true that in the game of the organism a number of acts may have their mover in certain forces inherent to the matter in itself, there are other forces of a higher order deriving from an immaterial power[90].

With this, Pouchet does not want to deny that the materialist explanation may have some effectiveness at some simple levels of the matter. The "vital force" essentially

> assembles particles, and forms organs out of them. But this force does not draw her materials from the chemical elements surrounding the places where it manifests itself. It just groups binary or ternary organic molecules[91].

Based on these general remarks, Pouchet then illustrates the positions of several authors who defended some form of spiritualism or vitalism throughout the centuries, starting from Epicurus (who however is looked upon as a materialist), Democritus and pantheism (with obvious references to Spinoza and Schelling, and less clear references to Kant), and passing through Van Helmont, Stahl (who had been already acknowledged as one of the fathers of animism), Müller, Leibniz (monadology), Plenck, Bremser, Brachet, Euler and Barthez[92], up to the *Naturphilosophie*, to which is due

> the most effective proof of the *eternal antagonism occurring between spirit and matter*, the ideal and the real. And, finally, the institution of this parallelism which is observed within the most sublime conception of the creation: the human species[93]!

Once again in line with the idea that, in investigating such ultimate issues as SG, we had better adopt a standpoint which be intermediate between rough experimentalism and pure idealism, however, Pouchet readily remarks that we should not go "too philosophically", as this would prove detrimental to the cause of SG itself. The methodological principle to be followed is rather the following:

in itself, see M. Cantor, *Pouchet savant et vulgarisateur*, cit., pp. 160-161, 168-171.

89 F.A. Pouchet, *Hétérogénie*, cit., p. 120.

90 Ivi, p. 119.

91 Ivi, p. 120.

92 Ivi, pp. 121-130.

93 Ivi, p. 130.

in each organised being there are two parts: the rough substance which this being is made of, and the vital power which underlies and rules all its elements. And it is this latter which we nonetheless forget about, in spite of the fact that it is this power which, ultimately, constitutes the sole biological essence of beings[94].

This tenet – which Pouchet takes to be respected by the two authors he mentions at the end of Chapter 2 of *Hétérogénie*, i.e. Burdach and Treviranus – finally leads Pouchet to SG properly, thus suggesting what kind of abstract theory he expects to be able to draw from the rough data of his experiments. The reader is thereby informed, *at the very outset of the investigation*, about the (bold) conceptual context she ought to accept *well before* knowing Pouchet's experiments. She is required to see those experiments under a correct light. As I already said, this is very different, not only from the standard understanding of experimentation (in France, in Pouchet's times), but also, *a fortiori*, from the method of Pasteur who, so to say, let the experiments "speak by themselves".

6. *Conclusion*

But also in the case of Pasteur, one might say, we ought to accept some broad conceptual framework before seeing that the experiments speak by themselves. To Pouchet's mind, for example, Pasteur's work may be biased by the unjustified (and wrong) assumption of some form of materialism (or iatrochemism), plus some ingredients of mysticism, since we know that, in private letters, Pouchet calls Pasteur a "modern Paracelsus"[95].

I think that Pouchet's epistemology is anyway weaker than Pasteur's. Even admitting that *both* Pouchet and Pasteur work under bold methodological assumptions, those at play in Pouchet's strategy are based on the idea that those at play in Pasteur's are wrong, whereas this *by contradictio* reasoning is not to be found in Pasteur, whose position may well be, in turn, compatible with some of the principles accepted by Pouchet (although not with all the experimental outcomes of these principles).

Be that as it may, it seems to me that no analysis of the Pasteur-Pouchet controversy can fail to take seriously into account the fact that Pasteur's and Pouchet's fields, chemistry and natural sciences respectively, were undergoing very different historical moments, and interacting differently with the overall scientific context of their times. They were simply responding to different research programmes, in Lakatos' sense. This may permit one to real-

94 Ivi, pp. 132-133.

95 See D. Raynaud, *Scientific controversies*, cit.

ise that one of these programmes (Pouchet's) was, again in Lakatos' terminology, both theoretically and empirically regressive, while the other (Pasteur's) was both empirically and theoretically progressive. Pouchet's methodological and experimental strategy is fine *within* the coordinates of the research programme which Pouchet belonged to, *hence* it is scientific (in Lakatos' sense), *hence* Pouchet may have won the debate. But the fact that, in the end, the winner was Pasteur, may not depend on sociological factors only; Pasteur's experimental programme was much more promising than Pouchet's, and it is a perfectly rational choice of the scientific community of those times to bet on chemistry and seemingly rough data, rather than on natural sciences, metaphysical ideas or abstract theories to be intellectually grounded by multi-sorted rhetorical and experimental heuristics[96].

[96] The differences between the natural sciences and chemistry in Pouchet's and Pasteur's times are discussed also by Raynaud, in D. Raynaud, *Scientific controversies*, cit., pp. 67-73, and, with focus on Pouchet or the natural sciences in general, in M. Cantor, *Pouchet savant et vulgarisateur*, cit., B. Percheron, *Les sciences naturelles à Rouen*, cit.

Recensioni/Reviews

Chris Letheby, *Philosophy of Psychedelics*, Oxford University Press, Oxford 2021, pp. 272.

Ci sono libri che fanno della chiarezza la loro cifra caratteristica, col rischio di allontanare chi è affascinato da ciò che non comprende del tutto. *Philosophy of Psychedelics* del filosofo Chris Letheby, pubblicato da Oxford University Press nel 2021, è uno di questi testi.

Il libro si pone, in modo convincente, come il tentativo di elaborare il contributo che una riflessione filosofica può dare al dibattito accademico sulla comprensione dell'efficacia delle terapie psichedeliche. La proposta di Letheby, giovane filosofo australiano, è quella di una neurofilosofia, ovvero *una filosofia naturale* degli psichedelici che integri gli aspetti empirici della ricerca con considerazioni teoretiche e concettuali. La chiarezza e il rigore sono le qualità principali del testo, in continuità con le altre pubblicazioni di Chris Letheby sul tema psichedelici.

Nel tentativo di comprendere gli effetti dell'esperienza psichedelica, Letheby utilizza un ampio raggio di fonti: report di anonimi utenti, tratti dalla più ampia enciclopedia online sulle droghe, Erowid; l'immancabile testo dello scrittore e influencer psichedelico Michael Pollan, *How to Change Your Mind: What the New Science of Psychedelics Teaches Us About Consciousness, Dying, Addiction, Depression, and Transcendence* (2018); classici della letteratura psichedelica come *The Doors of Perception* (1954) di Aldous Huxley e *The Joyous Cosmology* (1962) di Alan Watts; i testi dello psichiatra Stansilav Grof, e pubblicazioni più recenti come *Sacred knowledge: Psychedelics and Religious Experience* (2015) dello psichiatra William Richards o il ricchissimo testo di Benny Shannon *The Antipodes of Mind* (2002) sull'ayahuasca, la potente miscela psicoattiva amazzonica. Letheby usa la letteratura più recente e rilevante, dalle indagini neuroscientifiche dei "rinascimentali della psichede-

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lia" alle riflessioni filosofiche sulle scienze cognitive. Accanto alla ricchezza di fonti e le qualità di chiarezza e rigore, il testo è utile per due ulteriori ragioni:

1. Svincola il rapporto tra filosofia e uso di sostanze psicoattive dalle trattazioni *mainstream* che si impegnano a evidenziare come eminenti filosofi del passato consumassero ogni tipo di sostanza e che potrebbero rientrare nel genere della patografia *on drugs* (ahimè, abbastanza diffuso in Italia);
2. Difende la tesi per cui l'efficacia terapeutica dell'esperienza psichedelica sia da ricondursi *principalmente* all'alterazione delle rappresentazioni mentali della propria identità narrativa. Pertanto, non sarebbe da ricondurre a processi indipendenti dall'esperienza (vedi neuroplasticità); e neppure a credenze metafisiche, per quanto queste possano essere il corollario di esperienze insolite. La prospettiva naturalistica di Letheby ha il merito indiscusso di descrivere l'esperienza psichedelica *al di qua* di ogni interpretazione spiritualizzante, riconducendola a una dimensione mondana e incarnata. In breve: gli psichedelici sono catalizzatori di esperienze che possono essere correlate a benefici durevoli. Occorre guardare al contenuto dell'esperienza.

Tralasciando il primo punto più generalista, consideriamo il secondo. Chris Letheby sostiene che un gran numero di studi attesta non solo l'efficacia degli psichedelici nella cura di ansia, depressione e dipendenze, ma anche i benefici psicologici duraturi per volontari "in salute". C'è certamente un livello neuroscientifico e farmacologico da tenere in considerazione e Letheby non rinuncia a utilizzare i modelli neuroscientifici per spiegare l'esperienza psichedelica (per es. il modello teoretico del *predictive processing* ancorato al *free-energy principle* del neuroscienziato Karl Friston). Il livello neuroscientifico non è però ritenuto più fondamentale rispetto ai livelli "più alti". Letheby sostiene, anzi, che gli *effetti psicologici* dell'esperienza e la sua elaborazione da parte della persona siano elementi estremamente significativi nel promuovere il potenziale terapeutico e trasformativo a lungo termine. Chi usa o ha usato psichedelici riconosce l'importanza dell'*esperienza* psichedelica per almeno tre motivi:

a) perché è un'esperienza emotiva intensa, che espande il repertorio emozionale permettendo il passaggio dalla negazione all'accettazione delle proprie emozioni (incrementando le *mindfulness related capacities*);
b) perché favorisce l'*insight* psicologico. Il termine, particolarmente felice nell'accostamento alle caratteristiche visionarie degli psichedelici, si riferisce alla capacità di intuire in modo immediato aspetti del proprio comportamento, i propri sentimenti e le proprie emozioni. L'*insight* può portare un ripensamento dei propri valori, obiettivi e credenze, facilitato dall'apprezzamento della contingenza e della mutevolezza dell'immagine narrativa di sé;

c) perché favorisce un senso di accresciuta connessione a sé stessi, al proprio corpo, agli altri, al mondo. Identificandosi con il soggetto che percepisce e non con i contenuti dell'identità narrativa, si incrementa la flessibilità psicologica della persona. Il punto (c) è un elemento che emerge frequentemente dai resoconti degli utilizzatori, insieme all'*insight* psicologico e alla potenza emotiva dell'esperienza.

A un livello più ampio, Letheby riprende la distinzione tra autocoscienza narrativa e autocoscienza minima. La prima si riferisce al senso autobiografico di essere un'entità estesa nel tempo con una storia e una personalità precise e include i tratti di personalità e la storia autobiografica; l'autocoscienza minima si riferisce al puro senso di essere un soggetto che esperisce. Lasciando da parte il contatto con regni ed entità metafisiche, secondo Letheby l'emersione di nuove prospettive riguardo agli aspetti della propria immagine narrativa è uno dei benefici a lungo termine dell'esperienza psichedelica (a suo dire, ciò metterebbe d'accordo tutti gli utilizzatori di psichedelici). Questo ne spiegherebbe il carattere trasformativo: la sensazione di libertà, il disidentificarsi con la propria identità narrativa e l'emergere degli aspetti corporei dell'esperienza cosciente possono portare alla scoperta di modi differenti di concepire sé stessi (d'altronde il termine *psichedelici* allude alla capacità di queste sostanze di rendere manifesti alcuni aspetti della propria mente). Ciò non implica soltanto rivedere eventualmente alcuni dei propri attributi o ruoli principali, ma può favorire un senso di accresciuta coerenza nella propria identità narrativa. Questa prospettiva, tra l'altro, ricorda quanto viene considerato in contesti neo-sciamanici uno degli aspetti fondamentali dell'esperienza psichedelica: il recupero di un frammento psichico o "anima" e l'esperienza di ricostruzione del proprio corpo in seguito al doloroso smembramento.

In altre parole, le conseguenze trasformative degli psichedelici risultano dall'impatto che essi hanno sul *self*, evidenziandone la natura di costrutto virtuale e contingente. L'eclissarsi del modo quotidiano di percepire sé stessi indotto dagli psichedelici ci permette di riconsiderare l'identità narrativa come una costruzione dinamica. Per indicare il processo implicato in questi stati di coscienza, Letheby usa l'espressione "unselfing" derivata dalla riflessione della filosofa Iris Murdoch: un disfare sé stessi per guardare sé e la propria vita usando una lente più ampia. Anzi, le esperienze psichedeliche possono essere vere e proprie esperienze spirituali, se per spirituali intendiamo quelle esperienze in cui

> our sense of connection is enlarged … when we aspire to greater things … [and] when we ask the big questions, and with the idea that connection, aspiration, and reflection on big questions are all ways of 'breaking through the narrow walls of the ego'[1].

[1] Jerome Stone, *Spirituality for naturalists*, "Zygon", 47 (3), 2002, p. 492.

In tal modo anche il concetto di spiritualità, forse un po'ambiguo per un lettore/lettrice italiana, viene considerato nell'ottica di una visione del mondo naturalistica. Il fenomeno di *unselfing* ha a un inevitabile *côté* morale, scrive Letheby. Ci permette di diventare consapevoli dell'acqua in cui, come pesci, nuotiamo. Nella nostra esperienza quotidiana, infatti, diamo per garantita la nostra identità: è parte dello sfondo di domesticità in cui ci muoviamo e viviamo.

Unico neo del libro: manca un lavoro di ricerca critica sulla fenomenologia e sulla microfenomenologia dell'esperienza psichedelica.

Simone Capozzi

Pierre-André Taguieff, *Complottismo*, tr.it. Nicoletta Cavazza, Il Mulino, Bologna 2023, pp. 130.

Apocalissi e mito sono i termini con cui potremmo riassumere l'ultimo breve ma denso lavoro di Pierre-André Taguieff, in originale *Le Théories du complot* (Paris 2021). Paura dell'apocalissi e fascino del mito, per dire meglio. Nella condivisibile ricostruzione operata da Taguieff, il fenomeno del complottismo sarebbe interpretabile come un frutto avvelenato del tradimento della modernità, un tradimento che ha dato origine a una tensione – ad oggi ancora insanabile – tra paura, ansia, incertezza, angoscia, da una parte, e bisogno di compensare e rifuggire tale disagio emotivo ed esistenziale, dall'altra. Al fondo del cospirazionismo vi sarebbe quindi – secondo Taguieff – la paura, vera "passione motrice di coloro che credono ai complotti fittizi", che può "trasformarsi in angoscia di fronte a segni che annunciano una catastrofe" (p. 61). Paura della fine e bisogno di consolazione pongono, secondo il filosofo e sociologo francese, il fenomeno del cospirazionismo a contatto con l'elemento del mito – "che ha la pretesa di spiegare come va il mondo" (p. 56) – conferendo altresì "un orizzonte apocalittico" (p. 61).

La spiegazione cospirazionista, infatti, riesce ad attribuire senso all'evento incomprensibile, e tale attribuzione di senso dona l'agognata consolazione. L'imperativo è quello di "sfuggire ad ogni costo all'ansia, provocata dall'idea che il mondo abbia un andamento caotico" (p. 65). Il modo con il quale le teorie complottistiche forniscono il miracoloso rimedio prêt-à-porter all'angoscia dell'incontrollabile costituisce la caratteristica essenziale di tutte le teorie cospirazioniste, ovvero "la riduzione ad una causa unica". Lo studioso Leon Poliakov, autore di *La causalité diabolique* (1980), la chiama "causalità diabolica", imputabile ad agenti le cui intenzioni sono sempre malvagie e nascoste. Già i primi studiosi di narrazioni complottiste – Karl Popper e Richard Hofstadter, rispettivamente un filosofo e uno storico – evidenziarono, negli anni Cinquanta e Sessanta, tra i tratti principali che le varie teorie cospirazioniste condividono, quello del bias di intenzionalità. Taguieff fa tesoro di questa letteratura e mette opportunatamente in evidenza come uno di questi elementi sia fuor di ogni dubbio "l'ipertrofia dell'intenzionalità", ovvero la tendenza esasperata (e quindi non giustificata) di sovrastimare le cause intenzionali degli eventi. La concezione degli eventi come intenzionali ha l'ammaliante potere di rassicurare: "il male è ovunque, ma tutto è sotto controllo" (p. 19). Ogni evento che provoca stupore, indignazione, angoscia o terrore, può essere interpretato come il prodotto di un complotto organizzato da qualche servizio segreto deviato (p. 15).

Un dato su cui si sofferma più lungamente Taguieff è proprio la reazione che consegue a questa intenzionalità deviata: una reazione di odio verso le figure demonizzate, le quali su un piano storico, paiono sempre le stesse.

> [Le] potenze occulte e malvagie sono costruite come nemici assoluti per i quali i fini (dominare, sfruttare, distruggere) giustificano tutti i mezzi. Queste figure demonizzate sono sempre le stesse: gesuiti, massoni, plutocrati, giudeo-massoni, giudeo-bolscevichi nel mondo di ieri, finanzieri internazionali, imperialisti, capitalisti, neoliberali, nuovi «padroni del mondo», giudeo-crociati o americani-sionisti nel mondo di oggi (p. 62).

Al fine di comprendere l'odio astratto che si concentra su queste figure, Taguieff richiama il concetto di "risentimento", così come Nietzsche prima (nella *Genealogia della morale*) e il fenomenologo Max Scheler poi (nell'opera *Risentimento* del 1912) lo avevano impostato: riscatto dei deboli, degli emarginati, di chi si percepisce come tale. In epoca contemporanea, Richard Hofstadter – di cui si può apprezzare in italiano il suo lavoro pioneristico uscito in USA nel 1964 nella traduzione per Adelphi del 2021 col titolo *Lo stile paranoide nella politica americana* – è stato il primo a sottolineare il ruolo del risentimento, mescolanza di impotenza e odio, nel cospirazionismo. Ansia e collera sembrano, dunque, essere due sbocchi emotivi scaturiti dal tradimento della modernità nei confronti delle promesse di progresso e uguaglianza. E tali condizioni emotive si traducono in un'insoddisfazione che conduce a sua volta a un sospetto ossessivo verso l'altro, soprattutto verso alcune categorie. Ciò ha una ricaduta politica non indifferente. Secondo Taguieff, lo sbocco politico della cultura del sospetto e della sfiducia è il populismo (di destra e di sinistra).

> [L]e risposte cospirazioniste si inseriscono nell'immaginario populista delle società democratiche moderne, un immaginario che deborda notevolmente dal campo delle correnti politiche etichettate come «populiste», siano esse di destra o di sinistra. Il populismo sociale si nutre della cultura della sfiducia che irriga il campo delle credenze complottiste (p. 67).

Un insegnamento che viene dalla prospettiva storica di Taguieff è che le teorie dei complotti sembrano non morire mai, nonostante il passare dei secoli e delle inconfutabili prove contro le loro argomentazioni. Si resta allibiti a leggere la *Carta fondamentale* adottata come Statuto del gruppo terroristico di Hamas – il Movimento della resistenza islamica – resa pubblica il 18 agosto 1988, che sembra in parte essere la "ritraduzione islamica e antisionista del mito del complotto giudaico mondiale, adattato alla guerra totale contro Israele". Nell'articolo 22 si legge:

> Questi nostri nemici [gli ebrei] erano dietro la Rivoluzione francese e la Rivoluzione russa, e molte delle rivoluzioni di cui abbiamo sentito parlare, qua e là nel mondo. È con il denaro che hanno formato organizzazioni segrete nel mondo, per distruggere la società e promuovere gli interessi sionisti. [...] A proposito delle guerre locali e mondiali, ormai tutti sanno che i nostri nemici hanno organizzato la Prima guerra mondiale per distruggere il Califfato islamico. Il nemico ne ha approfittato finanziariamente e ha preso il controllo di molte fonti di ricchezza; ha ottenuto la Dichiarazione Balfour, e ha fondato la Società delle Nazioni come strumento per dominare il mondo (pp. 45-46).

E ancora nell'articolo 32, si menziona il falso storico dei *Protocolli dei Savi di Sion* come testimonianza storica, quindi oggettiva, del progetto sionista. Leggere per credere:

> Oggi si tratta della Palestina, domani di uno o più altri paesi. Perché lo schema sionista non ha limiti, e dopo la Palestina cercherà di espandersi dal Nilo all'Eufrate. Quando avrà digerito la regione di cui si è cibato, guarderà avanti verso un'ulteriore espansione, e così via. Questo è il piano delineato nei *Protocolli dei Savi di Sion*, e il comportamento presente del sionismo costituisce la migliore testimonianza di quanto era stato affermato in quel documento (p. 46).

Si badi bene, i complotti sono sempre esistiti dacché si ha memoria storica, e che siano riusciti o che siano stati smascherati hanno, in qualche modo, cambiato il corso della storia. Taguieff ne elenca alcuni, i più famosi almeno, che vale la pena anche qui menzionare.

> [L]'assassinio di Filippo II di Macedonia (336 a.C.), la congiura di Catilina (63 a.C.), l'assassinio di Cesare da parte di Bruto e i suoi compari (15 marzo 44 a.C.), quello di Caligola (24 gennaio 41), l'arresto e il massacro dei Templari (ottobre 1307), l'assassinio di Etienne Marcel (31 giugno 1358), la congiura dei Pazzi contro i Medici (aprile 1478), la congiura di Amboise (marzo 1560), il complotto mancato di Maria Stuarda contro Elisabetta I (1587), l'assassinio di Enrico III (2 agosto 1589), la cospirazione delle Polveri contro Giacomo I Stuart (ottobre-novembre 1605), l'assassinio di Enrico IV (14 maggio 1610), la cospirazione di Cinq-Mars contro Richelieu (1642), la congiura degli Uguali (1796), il complotto contro Napoleone, l'assassinio di Abramo Lincoln (14 aprile 1865), l'attentato di Sarajevo (28 giugno 1914), l'assassinio di Raspútin (16/30 dicembre 1916), quello di Walther Rathenau da parte

dell'organizzazione Consul (24 giugno 1922), il complotto della Cagoule (1936-37), l'assassinio di Lev Trockij (21 agosto 1940), il complotto del 20 luglio 1944 contro Hitler, il complotto di Stalin contro gli ebrei sovietici, invertito in «complotto dei medici» (1952-53), l'attentato del Petit-Clamart contro il generale de Gaulle (22 agosto 1962), l'assassinio del presidente Anwar al-Sādāt (6 ottobre 1981), quello del Primo ministro libanese Rafīq al-Harīrī (14 febbraio 2005) (pp. 30-31).

È impossibile, quindi, negare che i complotti siano esistiti e con ogni evidenza esisteranno sempre, saranno per definizione segreti e maturati in capo a una minoranza, ma ciò non significa "privilegiare sistematicamente l'ipotesi del complotto, al punto di vedere complotti ovunque e credere che spieghino tutto, o quasi, di come va il mondo" (p. 31), come costringe, invece, la mentalità cospirazionista. Il pensiero cospirazionista è ossessivo, quindi, nel senso deteriore del termine.

Viene da chiedersi allora che tipi di persone siano coloro che credono alle teorie cospirazioniste. Creduloni seriali, risponde Taguieff, da non etichettare come ingenui o ignoranti. Né d'altronde si deve presumere – ci avverte sempre il nostro autore – turbe mentali in ogni complottista (p. 26). Che vi siano complottisti paranoici, poco informati, culturalmente impreparati e cognitivamente fragili non implica che abbiano tutti questo profilo. La chiave riduzionista non consente di dar conto della preoccupante e trasversale diffusione delle teorie complottistiche che riescono a irretire nelle loro trame molte persone, indipendente dal censo e dai titoli accademici. Per via del suo atteggiamento oppositivo, l'anticomplottismo rischia di fare il verso al dogmatismo dei cospirazionisti e, peggio, di non mettere a fuoco alcuni aspetti più subdoli e duttili del fenomeno. Ad esempio, il fatto tutt'altro che raro che un complottista possa non accettare *in toto* tutti i particolari di una narrazione cospiratoria e quindi non credere a ogni singolo passaggio e frammento della teoria cui sente di aderire. L'adesione doxastica – parafrasando Anna Ichino e Juha Raikka, autrici del saggio *Non-Doxastic conspiracy theories* (2020) – è flessibile. Sebbene Taguieff non metta l'accento su questo concetto[1], egli sostiene che per il complottismo oggi si assiste a quello che è stato per il razzismo dal 1945 e per il negazionismo dalla fine degli anni Ottanta: "nessuno si definirebbe razzista, negazionista o complottista" (p. 29). In altre parole, nessuno si arroga o rivendica tale stigma. Il fattore stigmatizzante dell'etichetta *complottismo* non è, però, da sottovalutare neanche in campo scientifico dacché potrebbe influenzare l'approccio allo studio del fenomeno. L'etichettamento negativo dell'espressione "teoria del complotto" dovrebbe essere preso in considerazione quando si compiono analisi scientifiche, le quali per Taguieff dovrebbero partire "da una definizione non dispregiativa delle teorie del complotto" (p. 29).

[1] Sul quale mi permetto di rinviare al volume *Complottisti vulnerabili. Le ragioni profonde del cospirazionismo* di Emiliano Loria, Stefano Iacone, Cristina Meini (Rosenberg&Sellier 2023).

Non saprei quanto sia praticabile una via non dispregiativa del complottismo, ma si può intravedere bene il rischio di una inutile contrapposizione degli scienziati che si rivolga non alla tesi cospirazioniste in sé, quanto alle persone che le sostengono (anche se in parte) e che contribuiscono a diffonderle. C'è da apprezzare, a tal riguardo, gli sforzi di alcuni scienziati nel dialogare – pur se a distanza – con complottisti. Il caso più eclatante è quello di fisici e astronomi americani che si preoccupano di instaurare un dialogo pedagogico con i terrapiattisti, senza implicare un dileggio svalutante delle loro ingenue e talvolta surreali credenze[2].

Torniamo allora alla domanda che ci siamo posti: che tipo di persone sono i cospirazionisti? Uno dei tratti di personalità più marcati è la mancanza di fiducia soprattutto verso le élite dirigenti e le istituzioni politiche, ma la sfiducia può estendersi all'intera società, percepita come pericolosa e ostile. "La sfiducia si combina con il senso di marginalità e impotenza, ma anche con una eccessiva autostima, dato che queste tendenze narcisistiche esprimono il bisogno di sentirsi unici" (p. 27).

Taguieff, purtroppo, non approfondisce gli aspetti più prettamente psicologici, coglie e menziona il bisogno di unicità, una declinazione importante del narcisismo, per poi mettere in rilievo la dimensione sociologica e filosofica in una prospettiva storica che illumina l'origine del complottismo contemporaneo. Un approccio sicuramente più coerente alla sua produzione precedente (in italiano si veda *L'Illusione populista* edito da Bruno Mondadori nel 2006), e senz'altro molto stimolante.

La diffusione del fenomeno cospirazionista rappresenta, secondo il filosofo francese, il sintomo "dell'ansia culturale di massa alimentata dall'informazione" (p. 65). Chiaramente, i social network contribuiscono enormemente alla diffusione delle teorie cospirazioniste, ma – fa notare in maniera del tutto condivisibile Taguieff – le piattaforme soddisfano domande sociali che non hanno esse stesse suscitato, domande che "derivano principalmente da bisogni cognitivi che sono il bisogno di ordine, di comprensione e di senso" (p. 77) Non è quindi tanto lo strumento tecnologico, che è per eccellenza il simbolo della società globalizzata, la causa della diffusione del fenomeno; semmai "la rete" è un fattore di potenziamento, ma non l'elemento scatenante. A ben guardare, è la globalizzazione stessa che è percepita come "incontrollabile e minacciosa" (p. 10) e in grado di generare ansia e incertezza, le quali – a detta di Taguieff e molti con lui – sono il motore del complottismo. Qualche dato in riferimento al caso italiano, riportato dall'autore a confronto con quello francese, può aiutare a comprendere la preoccupante dimensione del problema. Taguieff menziona lo studio di Moreno Mancosu e Salvatore Vassallo (risalente al 2020 e pubblicato nel 2022 sulla *Rivista italiana di*

[2] Il riferimento è alla serie trasmessa per diversi mesi sulla piattaforma Netflix *La Terra è piatta*.

scienza politica), che analizza la valutazione di plausibilità relativa a quattro credenze cospirazioniste:

> 1. Gli sbarchi sulla Luna non sono mai avvenuti e le prove sono state create ad arte dalla NASA e dal governo degli Stati Uniti. 2. Le scie di vapore lasciate dagli aerei sono in realtà agenti chimici spruzzati nell'aria in un programma clandestino diretto dalle istituzioni. 3. I vaccini logorano il sistema immunitario e lo espongono a diverse malattie. 4. Il metodo Stamina per la cura delle malattie neurodegenerative inventato da Davide Vannoni è stato ostacolato dalle case farmaceutiche (p. 84).

I risultati mostrano in maniera assai preoccupante che nel 2020 il 35% dei partecipanti "valutava come plausibile almeno una delle quattro credenze" (p. 85) qui sopra elencate. Di fronte a questo scenario desolante, può esservi un antidoto al complottismo? Difficile ottenere una risposta univoca e Taguieff ha il grande merito di spiegare chiaramente il motivo: se il richiamo al mito gioca un potere attraente così forte, in un contesto di incertezza, ansia e collera, educare le persone allo spirito critico per lottare contro la tentazione complottista può risultare del tutto insufficiente, perché – sostiene Taguieff – il complottismo *intrappola* lo spirito critico.

> [L]o spirito critico risucchiato dalla credenza complottista si ritorce contro sé stesso. La grande astuzia dei complottisti agguerriti è di monopolizzare l'atteggiamento critico al punto da presentarsi come l'incarnazione stessa del pensiero critico, rifiutando ogni dogma e non rispettando alcun tabù. In molti ambiti, lo spirito dell'Illuminismo può ritorcersi contro sé stesso, quando la libertà di mettere in discussione, di dubitare e di approfondire è strumentalizzata in vari modi. E non è sempre facile tracciare un confine netto fra l'esercizio dello spirito critico e i suoi simulacri, soprattutto quando derivano da strategie ciniche particolarmente abili (p. 91).

In conclusione, la diagnosi di Taguieff sta nell'inquadrare l'atteggiamento complottista quale "patologia della diffidenza" (p. 98). Il mondo immaginato dai cospirazionisti assomiglia a quello raffigurato dal filosofo Thomas Hobbes nella sua opera *Il Leviatano*, in cui gli uomini vivono nel regno della paura dell'altro e della "guerra di tutti contro tutti". A colpi di sfiducia e dubbio continuo, il seguace cospirazionista giunge suo malgrado a un paradosso, di cui non si accorge, o non vuole accorgersi. L'*ipersospettosità* fa arenare i suoi passi in un *iperdogmatismo*; dubitare di tutto significa in fondo dubitare di niente. E quel bisogno di coerenza per rifuggire l'ansia – elemento psicologico da cui siamo partiti per comprendere il fenomeno – viene soddisfatto adeguando alcune informazioni ricevute e selezionate dal soggetto-cospirazionista alle credenze che egli/ella possiede già (p. 98).

Per rompere l'accerchiamento in cui si imprigiona l'io con paure e ragionamenti "extralogici", forse il rimedio è un'apertura, fosse anche una piccola fessura nella breccia delle dogmatiche (e ipocrite?) certezze, in grado di mettere il singolo su un piano di comunicazione franca e non pregiudicata in partenza dalla sfiducia. La scommessa di questo libro, nelle parole stesse dell'autore, è "di supporre che il dialogo sia possibile fra tutti i cittadini". Ciò implica ribaltare il piano della sfiducia – almeno verso alcune categorie di fonti di informazioni – in fiducia alla libera discussione nello spazio pubblico. A sua volta, tale fiducia è esplicitamente intesa "come condizione di esercizio della democrazia" (p. 9).

Emiliano Loria

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