Social Ontology, Transient Mental Illness and Justified False Belief[1]

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Abstract: Officially dismissed between 1987 and 1993, hysteria has been the cornerstone for the birth of neurology, psychoanalysis and dynamic psychology along the 19th and 20th centuries. The nosological category is here endorsed as the result of the social construction of scientific facts. Starting from Ian Hacking's reflections on Transient Mental Illnesses, we intend to bring his conclusions towards contextualist epistemology, questioning knowledge as Justified True Belief and further reconsidering the status of DSM categories as scientific kinds. The idea that justificationism may guarantee reliability of knowledge attributions is rejected on a historical base, and knowing is rather considered in the terms of an understanding whose logical and psychological features significantly overlap with the act of believing. Following the work of the later Kuhn, we separate the ideas of scientific revolution and mere taxonomic reformulation. Unlike Hacking, we do not consider DSM rewritings as scientific revolutions. We finally argue that the ontological and methodological premises adopted by DSM and ICD do not yet guarantee on their scientific reliability. Novel revisions are not suitable for better understanding dysfunctional behaviours, as they still fail to account for the phenomenological reality of diagnostic constructs beyond mere social ontology.

Keywords: epistemology, Transient Mental Illness, paradigms, philosophy of science, psychiatry, taxonomy.

Introduction

First defined as a proper nosological category by Jean-Martin Charcot between 1872 and 1878, hysteria represented a medical puzzle for centuries. The set of behaviours collected under the label 'hysteria' have allegedly been describing a neurological disease (Charcot), a brain disease (Briquet), a spirit possession (Middle Ages), a form of suggestion (Babinski), a female disturbance (Jorden), a mental alienation (Pinel), a problem of personal synthesis (Janet), a sexual neurosis (Freud), an outcome of contradicting stimuli (Pavlov), a form of communication (Breton), and more. Born in the 16th century (Pearce 2014), the noun was erased from the *Diagnostic and Statistical Manual of Mental Disorders* (DSM) and from the *International Classification of Diseases* (ICD) between 1987 and 1993, having its symptoms redistributed into other nosological categories,

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currently named somatic symptom, conversion and dissocative disorders, three concepts respectively belonging to the theories of Paul Briquet, Sigmund Freud and Pierre Janet.

The fact of a scientific community eliminating a disease from its standard nomenclature engages with classic scientific realism, a philosophical paradigm assuming the existence of entities independently from a knowing subject. While the reclassification of kinds is not an exception in scientific inquiry, it is usually supported by other complementary shifts: on the ontological level, on the criteria at the base of classification itself, by a change of perspective, a shift in theory or a novel discovery. As we shall see below, this was not the case with DSM revisions. Especially intriguing, in that reclassification effort, is in particular the reintroduction of some once eliminated kinds into the DSM listing, as in the case of multiple personality, suggesting that scientific entities in psychiatry may enter and exit reality somewhat confusedly.

Under what conditions does an entity stop existing? Under what conditions can a dismissed scientific kind be reintroduced in the world after having been eliminated? What kind of ontology brings these entities into existence in the first place?

The issue of DSM rewritings is further related with the very definition of disease in medicine and the distinction between normal and abnormal behaviours (see Faucher and Forest 2021). In nature, diseases can propagate, cause outbreaks and then stop circulating or be successfully eradicated. Illnesses can disappear and come back in different places and times, according to environmental, healthcare, technological, hygienic and even political and military factors. However, when a disease stops occuring in a population or area, it is not accordingly erased from medical dictionaries. The scientific kind continues to exist despite the fact that the disease is not active. If this is true for medicine, psychiatry has been working quite differently in the identification of its objects. Since its first edition in 1952, DSM reached a very critical number of revisions, reformulating kinds quite drastically and frequently. Every time the DSM publishes new sets of nosological categories, a realist may ask: do those kinds correspond to the world as it is, this time? Have those entities really been discovered or have they just been invented? When, on the contrary, American psychiatrists erase entries from their catalogue, one may ask how had doctors been warranted, until then, to believe they were scientifically observing an entity that, according to the new standard, does not exist. And more relevantly for contemporary kinds: how are scientists warranted today to believe they are observing the entities they are trained to diagnose? What kind of knowledge do those kinds and entities represent in science? (on this, see Allsopp et al. 2019; Clark et al. 2017; Hacking 1994, 278; Kuhn 1994, 319). This article aims to approach some of those questions by putting them in dialogue with the definition of knowledge in classical and social epistemology. To do that, we will be following

some works of philosopher of science Ian Hacking who, while remarking that "Yes, anything might happen in psychiatry," (1998, 93) has intensively studied the vanishing category of *hysterical fugue*.

Faced with the disappearance of a disturbance previously widely diagnosed, Hacking searched for an answer to the crucial question: *What made the diagnosis possible?*, thereby theorizing that the reality of such mental illness was the result of an 'ecological niche.' We will transfer this proposal towards contextualist epistemology, to try to consider such niche as a condition for a Justified False Belief to be held by scientists. While asking whether or not contemporary standards can guarantee doctors' current beliefs, we will further wonder: doesn't the social ontology of mental illness constitute an issue for objective knowledge attributions within the medical empirical method?

We will further claim that the selective adoption of social constructionism towards past categories and traditional scientific realism for current kinds represents a philosophical impasse in Hacking's reflections on behavioural dysfunctions, providing a strong argument in favor of the long-held idea that a paradigm shift is still much needed in psychiatry and has not happened yet. According to some (see Wade and Hulligan 2017), the reasons why such shift has not taken place is not to be found within medicine itself, but rather in its surroundings: the social, economical and political forces driving its scientific research and practice.

We will start the discussion from a 1998 book entitled *Mad Travelers. Reflections on the Reality of Transient Mental Illnesses.* There, Hacking approached some notable cases of category rewritings of the DSM. He addressed such instances as follows:

> By a 'transient mental illness' I mean an illness that appears at a time, in a place, and later fades away. It may spread from place to place and reappear from time to time. It may be selective for social class or gender, preferring poor women or rich men. I do not mean that it comes and goes in this or that patient, but that this type of madness exists only at certain times and places. The most famous candidate for a transient mental illness is hysteria, or at any rate its florid French manifestations toward the end of the nineteenth century. (Hacking 1998, 1)

Dealing more precisely with the case of *hysterical fugue*, he further resumed his position about the reality of such a disease in a chapter named *Five Questions, Five Answers*, whose summary will serve as a lead for our discussion, and goes as follows:

> *1. What made fugue possible, as a medical diagnosis?*
>
> An ecological niche, with four principal vectors to be named medical taxonomy, cultural polarity, observability, and release.
>
> *2. What did those old fugueurs suffer from?*

> By 1990s criteria, some suffered from head injuries, some from temporal lobe epilepsy, and some from dissociative fugue.
>
> *3. Were doctors of the day warranted in holding hysterical fugue to be a real mental illness?*
>
> Yes.
>
> *4. Was hysterical fugue a real mental illness?*
>
> No.
>
> *5. Are analogous conclusions to be drawn about transient mental illnesses today?*
>
> Yes. (Hacking 1998, 81)

We will here elaborate on those statements, analyzing the validity context of such scientific claims and questioning some of Hacking's positions. For instance, we will show that several ancient doctors, living in the same epistemic community, at the same level of technological development, with the same diagnostic means and often belonging to the same linguistic community and generation, were not warranted by the context itself in holding hysteria to be real (against point 3). With a parallelism with the well-known Gettier cases, we endorse the idea that justifications, niches or warranties around beliefs do not guarantee, nor represent, knowledge, unless a redefinition of knowledge itself is taken in consideration.

A Philosophical Impasse

Indeed, Hacking's proposal of an 'ecological niche' provides a crucial tool to account for the role of a justification structure in backing a belief and a narrative within a scientific community. It also represents a brilliant case study in favour of Edmund Gettier's critique of justificationism applied to scientific knowledge, leading to reconsider the reality of some scientific kinds or taxonomies that, although protected by the affiliation to the scientific realm, may have very little degree of epistemological reliability. As once observed by James Ladyman, "the fact that a word is used by a group of people to label a kind does not imply that a genuine kind exists" (2013, 50). That seems to be precisely the problem with psychiatric diagnostic constructs. However, Hacking also suggested that the social use of a word can make it real nevertheless. Reality becomes, under this gaze, something more than the sort of 'thing' traditional materialism and scientific realism provided: it becomes a complex system made of people's facts, beliefs, narratives and actions in the world. The idea is all but new and has been famously phrased in the proposition 1.1 of Wittgenstein's *Tractatus*, where he states that "the world is the totality of facts, not of things" (1922, 31). Among them, we find scientific facts as well, including the relation between DSM editorial board rewritings and real world diagnoses on people. That impact is real, as diagnosis provokes social consequences. In the pages of *Working in a New World*, Hacking observed that:

> A disorder with virtually no instances in the 1960s has passed from 'rare' in 1980 to 'surprisingly common' in 1990. For better or worse, such are the effects of forcing a taxonomy onto a body of pratictioners. Patients with the disorder never appear in the clinics of European psychiatrists, who are not under the sway of *DSM* and whose patients see little of it in the popular press, on television, or in the movies. (1994, 303)

The label of 'Transient Mental Illness' has the great virtue of identifying a historical and sociological pattern. The training of psychiatrists under the volatile nosologies of the DSM and ICD produces patients and diagnoses within a specific timeframe. It produces, moreover, several social kinds according to a controversial set of scientific kinds. We had hysterics, schizophrenics and bipolars before anyone could be sure that hysteria, schizophrenia and bipolarism actually exist. However, those social kinds, that Hacking called 'interactive kinds,' (1996; 1999) are real in a sense that goes far beyond scientific materialism, providing a crystal clear illustration of how scientific facts and social constructionism can vividly coincide in concrete circumstances.

> The core of Ian Hacking's reflection on transient mental illnesses is not the constructivism controversy, but rather the concept of 'interactive kinds' [...] Our representation of a person has an effect on that person, as she might act in return on this representation or in relation with it, differently from inanimate objects. This is what Ian Hacking calls 'looping effect.' (Delille and Kirsch 2016, 89)

DSM nosology and the socialization of intern doctors within such cognitive system of kinds train them to read the social world according to those labels, creating a form of reality which was not necessarily there before, one that will modify agents and will be modified by them. Furthermore, separate scientific disciplines, various epistemic communities and the sciences in different countries may differ in their category production system and criteria, as it happens with the DSM and ICD double standards, in open contradiction with scientific universalim. As a result, kinds produced within diverse endeavours are themselves different in their ontological status and may produce different social realities.

Although they all go under the 'scientific' label, kinds produced in physics or biology have very little in common with those produced in psychiatry. Hacking was a particularly bitter critic of the low scientific reliability backing psychiatric beliefs, as he wrote that: "Physics and psychiatry may both have structures of taxonomic kinds, but do not the kinds of physics arise from the science and not from committee vote and lobbying?" (1994, 303). As he further observed, adopting a realist argument, the introduction of the kind 'pulsar' in astrophysics did not change the world as it was. The number of pulsars out there, independent from designation, was the same before and after the discovery of Jocelyn Bell Burnell in 1967. Similarily, it would take some great discovery, advance in theory, well documented evidence in order to cancel or revise the kind 'pulsar' or the existence of 'lithium.' On the contrary, nosological categories of psychiatry do not

seem to require particularly strong evidence or research to be created, eliminated, reintroduced or modified, and they do change social reality.

> The number of pulsars out there was the same, even if the world in which the astrophysicists worked was new. In the case of multiple personality disorder in the US, the world itself has changed. There are a lot more people out there evincing multiple personalities, just because this has become a possible and reinforced way to behave. (Hacking 1994, 304)

The label of 'Transient Mental Illness' also has the virtue to underline the surprising frequency of DSM nomenclature revisions, reflecting the peculiar position of psychiatry within the realm of the sciences:

> The successive editions of the *Diagnostic and Statistical Manual* of the American Psychiatry Association are a case in point. The second edition was published in 1967, the third edition in 1980, and the third edition, revised, in 1987. These are authoritative and authoritarian attempts to produce a standardized psychiatric lexicon, in Kuhn's sense. All mental maladies are clamped to taxonomic trees. The trees were almost completely rebuilt between the second and third editions, and many of the disorders were placed in new relations. Thus 'multiple personality disorder' had completely disappeared from the second edition but has a slot in the third, and was enlarged in the revised third edition, much to the delight of those psychiatrists sardonically called 'multipliers' by their opponents, who in turn were dismayed. (Hacking 1994, 303)

When it comes to consider the unique status of psychiatric constructs within scientific theories, we follow Hacking's lead. Yet, from the perspective of scientific realism, the label 'Transient Mental Illness' may also represent a philosophical impasse. We'll see why in a moment. First, it seems worth to remember that, when Hacking was writing, the very expression 'mental illness' had already been discredited by psychiatrists (see Szasz 1961), with various criticisms that gradually led to the adoption of lighter terms, like 'disorder' and 'dysfunction' to address its objects, an operation which may be considered as a step towards increased reliability or as a mere 'cosmetic' change with no conceptual substance. Thus wrote psychiatrist Thomas Szasz on this matter:

> Psychiatry is conventionally defined as a medical speciality concerned with the diagnosis and treatment of mental diseases. I submit that this definition [...] places psychiatry in the company of alchemy and astrology and commits it to the category of pseudoscience. The reason for this is that there is no such thing as 'mental illness.' (1961, 455)

Adopting a materialist-scientific take, Szasz considered a disease as a:

> pathological alteration of cells, tissues, and organs. If we accept this scientific definition of disease, then it follows that mental illness is a metaphor, and that asserting that view is stating an analytic truth, not subject to empirical falsification. [...] Except for a few objectively identifiable brain diseases, such as Alzheimer's disease, there are neither biological or chemical tests nor biopsy or necropsy findings for verifying or falsifying DSM diagnoses. (2011, 2, 169)

For such reasons, diagnostic constructs of psychiatry would deserve to be openly considered as entities based on a social ontology, just like money, laws, genders, ethnic groups, etc. And there is more when we consider the 'transient' character of such label. By the adjective 'transient' we get to know that a set of categories has existed within a timeframe and has later been erased. However, historians of science know well that all scientific theories are transient, and they are so by definition. So why should one ascribe temporariness especially to some set of dismissed categories of science, in this case psychiatry? In the end, this is how science works (see Chalmers 1976). Even quantum physics, in the very intentions of its creators, provided a temporary set of theoretical constructs that was destined to be overtaken. Sir Roger Penrose recently observed:

> You have to look carefully at Dirac's writings [...] You find the right place and you see Dirac says quantum mechanics is a provisional theory. We need something that explains the collapse of the wave function. We need to go beyond the theory we have now. I happen to be one of the kinds of people [...] who believe quantum mechanics needs to be modified. (Penrose 2020, 51:50)

Nevertheless, in such cases of theoretical shift, both scientific and philosophical vocabulary would rather describe past models as 'dismissed categories,' 'disproven theories,' 'outdated hypothesis;' definitely not 'temporary realities.' The fact of having been temporarily valid does not influence the dismissal. Since scientific observations and findings are inextricably produced within a theory or model, it follows that a change in the latter directly corresponds to a change in the formers (see Feyerabend 1958). Typically, as change happens, a new theory incorporates or replaces the old one, together with its taxonomy and laws (Lakatos 1978; Kuhn 1962). Hereby, this may be the limit of 'Transient Mental Illness' as an analytic tool: if hysteria was not a real mental illness, as stated in point 4 above, why describe it as 'transient mental illness?' A disproven theory does not represent a transient reality. Accordingly, hysteria has never existed, not even temporarily. Once the heliocentric model of the solar system had gained consensus, no one felt the need to describe the former model as a 'Transient Geocentric Astronomy.' Similarly, one does not refer to any 'Transient Four-Elements Chemistry' or anything alike. Consequently, describing a dismissed theory as 'transient' represents little more than a tautology, both in scientific and philosophical terms.

It does something more, too. By proposing the existence of an ecological niche as support for that temporary model, by dismissing the reality of old categories and by accepting the novel ones (points 2 and 4), it somehow suggests that we are now looking at the world from a safe and privileged position, out of history. Arguably, however, some 'ecological niche,' or epistemic context, is at work at any stage of the production of human knowledge about the world. Right now, we as well find ourselves within an ecological niche enabling the implementation of current models, theories and transient knowledge of reality. That knowledge may be proven wrong in the future, thus revealing that we are

currently holding as many 'justified,' 'warranted' false beliefs. Yet, it may still represent knowledge today. This view engages with the incommensurability principle. How can the identification of an ecological niche constitute a warranty for knowledge of ancient doctors? Here comes the impasse: if we call those categories 'transient' instead of 'wrong,' 'biased' or 'dismissed,' we somehow let social constructionism get into the domain of scientific realism, which brings along epistemological relativism. Social ontology and scientific materialism, nominalism and realism, discourse theory and how the world really is, metaphysics and mechanical concepts of physics may merge in a blurred reading, tending to extend and reshape notions of reality, belief and knowledge. We could then proceed until we don't know anymore where we are supposed to draw the line between discoursive and non-discoursive facts, nor if we are allowed to do that at all, following metanarratives and considering scientific knowledge itself as a kind of discourse (Lyotard 1984). This state stands at odds with the scientific ambition to describe the world as it is independently from the observer (pulsars were there before humanity could observe them). Such contradiction became gradually clear to Hacking himself. In fact, some years after the proposal of the distinction between natural (indifferent) kinds and social (interactive) kinds in science, he dropped such idea:

> many commentators of Ian Hacking seem either to disregard or to be unaware of the fact that from 2002, in his lectures at the *College de France*, he questioned the dichotomy between interactive kinds and indifferent kinds. The distinction he draws between natural and social sciences remains unchanged, but he gives up the idea that there exists a homogeneous class of natural kinds. [...] Ian Hacking also declared that he had been too much influenced by psychiatric diagnostic in his historical ontology. (Delille and Kirsch 2016, 89)

Consequently, we find ouselves again facing the old method/object contradiction: DSM entities are proposed within medicine, a traditionally empirical materialist science, but they produce statements about the mind or soul, an abstract metaphysical entity still belonging to a theological tradition. If we accept the social ontology of mental illnesses (after all, had they been brain issues, they would be treated by neurology), those kinds are to be placed right next to genders, social groups, arts, disability, laws, etc., where we are not in the position to advocate for their irreality anymore, as long as their existence comes from a social agreement around their reality. Therefore, the temporariness of their factuality would only rely on the duration of doctors' belief in their existence and on their application onto society, which reacts accordingly.

This analytical impasse is visible in those earlier writings of Hacking, although never embraced openly as an analytical praxis. Let's see this more closely. On the one hand, when he wants to see some category erased, he invokes a new reorganization of kinds, as in the case of Pierre Janet's dissociation (or at least, American psychiatrists' understanding of it):

> In twelve years' time, in 2009, I would like to observe the very last congress at which the dissociative disorders, including dissociative identity disorder, were treated as autonomous illnesses. I would like to see the entire conceptual organization, of multiple personality (dissociative identity disorder), dissociative fugue, and dissociative amnesia, disintegrate, with the symptoms dispersing into a new conceptual organization. Moreover, and this will be offensive to many, I hope that post-traumatic stress disorder, far from absorbing these other disorders, will also disperse and no longer be a usable classification. Finally I hope this will happen as a result of sustained inquiry. The sustained inquiry matters. (Hacking 1998, 99-100)

In such a congress, quite contradictorily, one would be witnessing the vanishing of some 'Transient Mental Illnesses' when they were not 'transient' yet, but still 'real.' With the suggestion that an ecological niche may provide all the sufficient and necessary elements for scientists to be warranted to believe something wrong, or unreal, we find ourselves precisely on the fine line towards social constructionism, though the author openly refused such labeling (see Hacking 1999). Here, a discoursive-like argument is proposed as explanation for past mistakes (doctors were warranted to believe they were observing a non-existing object because an ecological niche was there). On the contrary, when Hacking wanted to see some other category or kind maintained, he went the opposite way. Particularly interesting is the author's belief about schizophrenia:

> Some mental disorders are, in my opinion, real. In the case of schizophrenia, for example, despite the conflicting claims, I hope that within twenty years we shall have a grip on one or two or perhaps three fundamental types of schizophrenia [...] What I hope is that schizophrenia will emerge as one (or several) bodily dysfunctions, neurological, biochemical, or whatever, which we shall be able to help or cure in a theoretically well-understood and a practically well-articulated way. [...] If this hope is fulfilled, then schizophrenia is a real disorder, or perhaps several distinct real disorders are at present called schizophrenia. (Hacking 1998, 98-99)

More than twenty years have passed, and the reality of schizophrenia is still uncertain. Some researchers are still asking whether or not schizophrenia exists (see Repnikov, 2023) others hardly answered it does not exist and invoked the elimination of the category from the DSM (see van Os 2016).

Here, Hacking's argumentation overlooks social ontology and goes straight towards materialism (mental disorders would be real if they were brain issues, neurological, biochemical), a chance which actually does not represent a happy scenario for psychiatry, as such a discovery would most likely move its kinds back to neurology and make of psychiatry a mere pleonasm. This explanatory blurring, standing both for and against social constructionism, reflects an analytical impasse to the extent that it gives a partial elucidation to the genealogy of specific cases while leaving unanswered the most fundamental issues about the divorce between realism and idealism, or otherwise, between materialism and nominalism in psychiatry through the discoursive framework. By accepting the new nomenclature as a replacement for the old dismissed system, one may in fact

fall into a circular loop, simply entering a new 'ecological niche' without questioning the critical factors which led to the collapse of the previous system. For these reasons, we suggest not to adopt the category of 'Transient Mental Illness' to describe the dismissed categories of the history of psychiatry, including hysteria, and prefer the search for and identification of the biases at the very base of the architectural system supporting an unsatisfactory paradigm which is still producing current kinds. As proposed above, we are instead inclined to consider DSM and ICD entities as kinds based on a mere social ontology, under a narrative legitimation which sometimes includes scientific materialism, especially in the form of some hypothetical biological and genetic markers still waiting to be found. A significant step towards a change of paradigm, as we shall see, has rather been taken through the proposal of the transdiagnostic approach (see Dalgeish et al. 2020; Fusar-Poli et al. 2019), which may constitute a real change of paradigm, instead of another revival of outdated Kraepelian premises.

Accordingly, we are not confident that the new taxonomy can successfully describe what had wrongly been identified by an old one, as the epistemological premises and methods of the those systems remained pretty much untouched. We don't see how the ontological issues represented in the DSM can find a solution within the DSM itself until a radically new set of nosological criteria is established to identify and define its objects, instead of encouraging further cosmetic revisions. We think there are good reasons to doubt that the new categories can solve the contradictions expressed by the old ones, and will therefore invoke an extended reading of ecological niches in the frame of classic epistemic contextualism, thus considered as unjustified validation systems for those frequent rewritings. We would rather point towards a structural shift in paradigm as the solution for past and current impasses in the epistemology of psychiatry itself, holding that it is not enough to consider past and current categories as all scientific entities (as long as they are produced by scientists) the nature and properties of which are based on a social ontology.

Justified False Belief and Epistemic Context (Were Doctors of the Day Warranted?)

Otherwise: did ancient doctors know something? Did they have knowledge or just false beliefs?

As a start, we need to provide a brief historical focus around some particular moments in the creation of the cateogry of hysteria, so to further highlight the reasons why we do not consider ancient doctors to have been warranted in believing in its existence. This may trigger a panic epistemic relativism around the definition of knowledge, which we will do our best to prevent.

As seen above, Hacking's reflections on the reality of dismissed kinds of psychiatry inclined towards the consideration of their 'ecological niche' as warranty, or justification (point 3). His position suggests some intriguing parallelism with epistemological justificationism on a historical base, a

correspondence that may be worth an exploration, questioning the very meanings of knowledge, as well as the distinction between knowledge and belief. Did ancient doctors have knowledge, while holding the reality of hysteria? Do contemporary doctors have knowledge, holding the reality of somatic symptom, conversion or dissociation disorder? Or did they simply acquire a form of belief through the training that socialized them within a nosological system, which may or may not represent the world as it really is? Although the question is not new, we hope it will not seem unrelevant.

When we deal with the distinction of knowledge from belief, "practical problems arise especially in two certain fields of discourse: science and religion. Religion relies on what people believe," wrote Juliana Goschler, whereas "unlike religion, science is supposed to rely on what people know. But even most scientists would agree that this 'knowledge' is never complete" (2007, 31). And so are not its warranties. Traditionally, beliefs are said to require no legitimation; we will see that, in some cases, scientific knowledge may behave similarly.

Hacking wrote that doctors of the time were warranted to believe that hysteria existed, *being justified* by observability, release, medical taxonomy, cultural polarity in an 'underlying agreed conceptual structure':

> I asked whether nineteenth-century beliefs were warranted. Unlike true, the praise word warranted is relational. A belief is warranted relative to available evidence and an underlying agreed conceptual structure. In 1997 a belief in the hereditary and neurological character of hysteria would be completely unwarranted. It was warranted in France in 1887. (1998, 91)

We think there are very good reasons to doubt so; and here's why. Indeed, psychiatrists are socialized within a certain model of understanding, produced by other, higher ranked, professionals. In the end of the 19th century, however, different peers had different theories around the same set of observable dysfunctional behaviours. Some were told a cluster of behaviours were to be called hysteria, some others pithiatism, others psychastenia, as we shall see. Therefore, unless we consider social narratives as a fair justification, we have to acknowledge that none of those trainees were warranted enough to believe. Let us briefly bring those arguments into the framework of the classical epistemological problem of knowledge as Justified True Belief. Speculating on Gettier, we may ask ourselves: did those doctors had knowledge? And by the way: is knowing only *knowing something true*? At this point, however, some distinctions may be necessary. Distinctions about the nature of 'the scientific community' as discoursive representation; about the status of the true/false demarcation in relation to a historical scientific model; about the objective character of knowledge and its additional features as an understanding, feeling or thought, as well as a socialization process. We will do that in a moment.

According to objective knowledge, the answer to that question (*did ancient doctors have knowledge?*) should be 'No,' as the objects of their experience did not exist according to the later model. We could claim that, while seeing a mirage in

the desert, doctors believed there was an oasis behind the dunes. However, in the specific case of psychiatry's ontology, methodology of research and diagnostic means, it is still not possible for a psychiatrist to go over the dune and materially confirm or deny a diagnostic hypothesis, as no test can verify nor falsify DSM diagnoses.

Nevertheless: how many psychiatrists would claim they do not know about the reality of DSM categories? How far can we bring the discrepancy between knowing and believing, and how could the new taxonomy represent a better warranty for knowledge attributions? The more one analyses current and former psychiatric nosology, the less clear such demarcation. Not only we endorse Gettier in claiming that justifications do not guarantee knowledge attributions: we also question the very distinction between scientific knowledge and belief conventions within this medical branch, as well as the criteria to separate truth and falsehood of entities based on a social ontology.

Let us try to discuss further on that without invoking absolute relativism. Elsewhere in science, one may state that "scientists *know* that the speed of light is a constant." They do not *believe* that, they *know* that. But in that case, we would also have to acknowledge they know so despite the fact that, according to some, the value of the speed of light seems to have dropped worldwide between 1928 and 1945. This did not prevent metrology from fixing the speed of light by definition in 1972 and from even redefining the meter in terms of speed of light. Something alike happened with the gravitational constant, whose status of constant actually originates from an average of different measures, varying from place to place and from time to time (on those topics, see Sheldrake 2012). Even in the most demanding fields of the sciences, physics, where standards are the highest, trainee scientists have been taught a series of premises about the world within an educational process. Some of those premises will never be empirically tested by the trainee, nor doubted or questioned: they are simply learned and applied in the labour market. This ambiguity between truth and dogma (see Kuhn 1963), and between knowing and believing, gives us the opportunity to elaborate more on Gettier's critics of knowledge as Justified True Belief and to further consider the role of contextualism in the social epistemology of psychiatry, extending some relativistic view to scientific knowledge and mental illness (see Longino 2022; Feldman 1999; 2001).

The classical account of knowledge as JTB (S knows that P if and only if: P is true; S believes that P; S is justified in believing that P) never recovered from the critics of Gettier. All attempts to patch up the traditional analysis in order to evade Gettier have been struggling in vain. Tracking theory, reliability, relevant alternatives, safety condition, causal connection between justification and belief, truth probability, falsehood exclusion... no convincing solution for the problem has been found. It seems that all possible justifications will eventually suffer some sort of fallibility. In the sciences, the issue may be more deeply embedded with the other two terms of the JTB problem: on the one hand, the cognitive status of 'belief'

(meaning here the *action* to believe something and stop doubting, stop searching and accept a fact, a dogma or a premise) and, on the other hand, the distinction between 'true' and 'false,' which is relative to a theory or model and raises the issue of commensurability among paradigms. In the wake of a historical rather than normative philosophical analysis, logics would have to make room for the psychological and social dimensions of human knowledge, better considered as an understanding. Psychological and social relations among individuals are crucial in the production of an understanding, not only on the level of ordinary day by day knowledge, but also in the more refined and demanding fields of scientific knowledge (as seen in Kuhn 1977). When we part with the idea of objective knowledge, where knowledge itself is the correspondence between what the subject believes and how the world really is, other variables become pivotal, such as the relative, subjective and intersubjective dimensions of knowledge. Here, a social epistemology is needed to address the social ontology of mental illness.

On the other hand, the threshold established for something to count as knowledge can move also within the sciences, according to the different standards adopted in various disciplines (see Resnik 2000), and to the challenges skepticism may launch. Quoting David Lewis: "What is true enough in one occasion is not true enough in another." (1979, 352) Additional conditions, further experiments, higher standards can endlessly be required. Such threshold represents the point where an individual stops doubting, suspends skepticism and starts to believe something. But, it has been noticed (Feldman 2001; DeRose 1992), knowledge itself is not a unique nor universal category. Both the everyday context of use of the word 'knows' and its conditions required in science vary according not only to the research context, but also according to who is talking to whom, for what purpose, in which language, where, when, and so on. The variety of meanings of knowledge discourages the idea that knowledge may be one, unique concept with a unilateral definition and essence. For this reason, it may be more fruitful to consider knowledge as an understanding, also within the sciences.

Secondly, in classical epistemology, P has been mainly considered as a sentence, a phrase. Subject S typically knows that the sentence P, which begs the question: do we only know sentences? Moreover, classic accounts did not properly consider the role of a wider community in the production of knowledge. Subject S is mentioned in its own, private relation with the phrase P. However, if we accept that no one develops knowledge alone, in a *vacuum*, that absence represents a relevant shortcoming for a philosophical conception of knowledge. Also, the fact that a willing-to-know subject S is always thought as an individual cognitive agent raises the above mentioned problem of the disagreement among peers (see Longino 2022, 9). Let us show how the case of hysteria is paradigmatic in that respect.

As outlined above, when Charcot produced the nosological category of hysteria for the first time in medicine, the epistemic community around him was not omogeneous: several doctors disagreed with him. While Charcot described

hysteria as a neurological hereditary disease, showing its symptoms through public hypnosis at his lessons at the Salpêtrière, his most brilliant colleague and disciple Babinski claimed that the hysterical manifestations, until then taken as symptoms, were nothing more than a pathological state originated by artificial creations induced by suggestion. Hyppolite Bernheim, Professor of Medicine at Nancy University, shared this opinion and provided demonstration that the phenomena observed during the hypnotic treatment of Charcot would only happen when the patients knew they had to happen. As early as 1904, German physician Armin Steyerthal "predicted in a pamphlet entitled *What Is Hysteria?* that 'within a few years the concept of hysteria will belong to history […] There is no such disease and there never has been'" (in Micale 1994, 501). Surrealist poet and physician André Breton, who studied medicine and received psychiatric training under Babinski, in a manifesto entitled *Le Cinquantenaire de l'hystérie (1878-1928)*, openly claimed that "hysteria is not a pathological phenomenon but may be considered a form of expression" (1928, 20).

Despite the optimistic expectations of Steyerthal, it took a lot more than a few years for doctors to stop believing (or stop knowing) such a disease existed. Even the very act of naming the phenomenon was subject to a serious debate. Doctors of the day had proposed the creation of various scientific objects, with different nosological categories, names, interpretations and descriptions around the same set of behaviours, clinically observed. *Psychasthenia* (Janet), *Pithiathism* (Babinski), *Suggestion* (Bernheim), *Somatization* (Briquet) were all peer candidates for the contest won by hysteria.

Naively, we would possibly be inclined to think that hysteria took over because it was the most precise description, the most reliable theory or the best nosological category available, but history tells a different story. In fact, what made the real difference was not any research or experiment designed to check what category would better correspond to the world and settle disagreements. The critical factor was rather a change in the political clout within the community: originally represented by the influence of Charcot, followed by Babinski after his death, then replaced by Freud's sexual theories abroad, in Vienna.

Nearly every 19th and 20th century doctor had its own theory about the behaviours that ended up to constitute hysteria. It is not possible to claim that doctors in general were warranted in holding hysteria was a real mental illness without eclipsing a good deal of history. From Ivan Pavlov, who believed hysteria to be the result of contradicting stimuli in the cortex (1921) to Breton, who believed hysteria was a form of communication, several doctors had vigorously tried to get their colleagues to change their mind over the myth of hysteria, refusing that nosological category. Babinski even managed to get the Paris Neurological Society to abandon that kind as early as 1901 (see Micale 1993; Babinski 1909). But Freud brought it back to life, projecting its domain into the realm of metaphysics.

> Dominated by Babinski, […] many members confessed openly to what they now regarded as the misdiagnosis of many cases from their earlier medical practice. The meeting on 14 May dealt with the eight topic on the questionnaire: 'Faut-il conserver le mot Hystérie?' [...] Everyone present agreed that hysteria had previously been defined much too elastically. […] Exactly fifteen years after Charcot's death, the most prestigious professional organization neurology dismantled the Salpetrian model of hysteria, symptom by symptom, in two days, just as Charcot had constructed it with such care in two decades. (Micale 1994, 518-519)

We therefore have to acknowledge that, more than epistemically warranted, scientists had been socially shaped, hierarchically trained to believe that entity existed. Such understanding happened through a socialization process, teaching them what to observe and how to categorize it, a procedure that has been deeply examined by Thomas Kuhn in the life of science (1977). Also, an account of the theory of hysteria as a catch-all diagnosis and its character of socially constructed scientific object has long been provided, giving an early overview on the social character of psychiatric ontology (Gilman et al. 1993; Didi-Huberman 1982; Veith 1965). As illustrated by Longino, scientific criteria follow social vectors too and are not purely 'objective':

> the scientific community is a collection of diverse (though maybe not diverse enough) individuals who bring different metaphysical assumptions, different epistemic values, and different social values to their assessment of problems and data. These assumptions and values play a role in determining the evidential relevance of empirical data. They are not (for the most part) subject to empirical assessment. But their plausibility and consequences can be assessed relative to the alternatives. [...] Individual researchers may have unificationist ambitions and certain contexts may require at least a temporary consensus, but these do not translate into universal epistemic criteria. (2022, 12)

We should therefore take into account the various communities, identities, agendas, belief systems around subject S when considering the classical account of knowledge in the philosophy of science. We can then operate a first split in that apparently unique epistemic group of 'doctors,' considering the smaller factions actually involved in the making of hysteria. On the side of the creators of entities, theories and laws, we can no longer claim that there was any justification, or warranty, able to defend the reality of hysteria. Babinski, Bernheim, Pavlov, Briquet, Breton, Freud and Janet, to mention some, all had different opinions. How can one say that Charcot's opinion was more reliable or justified than the others? Surely, Charcot himself had more respect, influence, power and prestige within that community, therefore having a stronger hold on its members. On this, Hacking reported that:

> During the period 1872–1878 Charcot had become the world expert on florid hysteria. Yet a great turf war continued, because gynecologists and obstetricians, masters of the womb, claimed hysteria as their territory. Charcot's central theme was that hysteria was a neurological disorder. It was hereditary – that is, only those so disposed by ancestry could develop it. The best way to wrest hysteria

> from the gynecologists was to declare it to be a disease of both sexes. Male hysteria had always been acknowledged, but usually with a connotation of effeminacy. Charcot found his male hysterics among brawny laborers; there was nothing effeminate about them. (1998, 187)

Another separation has to be considered: that between the epistemic warranties of the nosology creators and those of doctors who were trained under them to embrace an established system of kinds. The good enough reasons for S to believe that P may apply to a portion of doctors and not even be necessary for another: those who receive a category made, a world *prêt-à-penser*. At this lower level, taxonomy becomes little more than a dogma, a belief, the standard transmitted system, no longer discussed and simply applied to the world by means of diagnosis. Only at this level can we claim, with Hacking, that doctors of the day were relationally warranted to believe that P. But we have to admit that their main justification for knowing, or believing, was represented by social forces like authority, hierarchy and creed, and certainly not by their individual experience, rationality, research, critical thinking or logics.

On this respect, particularly suitable seems to be the reflection on *imagined communities* (Anderson 1983). As daily displayed by mass-media's bold declarations attributed to 'the scientific community,' epistemic communities as well are imagined, promoted, discoursive entities. Accordingly, the issue of belief warranties in epistemology deserves a wide reformulation and a further subdivision, describing the various communities a community is made of. In this sense, we may have to revise the issue: when we ask *How were doctors warranted to believe that P?* (being P: "Hysteria is a disease"), the most reasonable answer seems to be: they were told so.

But is anyone willing to admit they scientifically knew so?

Taxonomic Shift and Revolution (Are Current Doctors Warranted?)

If we accept that entities based on a social ontology are just as real as those based on objectivism and materialism, the case of DSM rewritings comes to represent a specific situation where standards for the attribution of (scientific) truth change according to the model in force, as well as with the epistemic community involved. Not only the threshold of justification becomes movable, but also the one between true and false. Accordingly, the epistemological model of reference should become contextualism, where validity criteria, as validation contexts, constantly change. Following Feldman:

> The central contextualist idea is that the truth conditions for attributions of the word 'knows' vary with context [...] Depending on how one analyzes 'knows' the details of this argument will vary. For example, suppose that knowledge requires justification. The word 'justified,' it might be said, is context sensitive. As a result, 'knows' is context sensitive. […] Suppose that knowledge requires reliable belief information. 'Reliable' is itself a context sensitive term. Hence, 'knows' is context sensitive. (2001, 62, 65)

If a belief begins when we suspend doubting and start to assume something, we can consider it, in a sense, as a mode of dogma. This premise, assumption or precondition can be true or false not according to an objective take on reality, knowable by subject S, but only within a theory and model socially promoted:

> whether S knows that q depends, in part, on intersubjectively determined standards. This raises the issue of how precisely those standards are determined. In particular, which social group sets the standards? (Cohen 1987, 14).

Considering the years 1987/1993 as a turning point for hysteria, we should suspect that there was a moment when (universal) standards of knowledge changed, so that a statement that was scientifically true before, was not so afterwards. Without evidence of such a shift, we cannot fully trust current kinds. However, epistemological confusion in psychiatry is so serious that M.D doctors were still adopting the dismissed diagnosis of 'mass hysteria' in 2024 and 2021 scientific papers (see Jeong et al. 2024; Zhao et al. 2021).

As seen, Hacking's reflections consider novel entities as the meter to measure the old kinds abandoned by psychiatry (endorsing commensurability). According to his point 4, old fugueurs were not suffering from hysterical fugue, they were suffering from dissociative fugue. Dismissed categories of DSM would then be considered false when compared to revised listings, an approach that would probably not meet the favor of Thomas Kuhn nor Paul Feyerabend, who considered different paradigms incommensurable.

This take further embraces a realist view, suggesting that only names have changed over a persisting fenomenological reality. And yet, that conclusion is motivated by the nominalist idea that a change in taxonomy may constitute a sufficient condition for the identification of a scientific revolution:

> This is a rare unequivocal example of a scientific revolution, in the sense of Thomas Kuhn's last work. One taxonomy replaces another, to the point that we simply do not know what hysteria was any more. (Hacking 1998, 72)

Such a reading is bringing Kuhnian theory too far. We could not find a passage where Kuhn's *Second Thoughts on Paradigms* could provide the basis for such a conclusion, although his editors wrote elsewhere (1987, 7) that scientific revolutions require changes in the taxonomy provided by scientific language itself. On the contrary, there is a passage in Kuhn's *Afterwords* (1994) where the author himself graciously took distance from Hacking's misreading:

> Though the solution he [Hacking] describes was never quite my own and though my own has developed substantially since the manuscript he cites was written, I take immense pleasure in his paper. [...] His nominalist version of my position [...] does not quite face my problems. The reasons are numerous. (Kuhn 1994, 315)

In addition to the author's remarks, and somehow despite them, we may remember that before a taxonomical shift alone can be described as a scientific revolution, we should consider that it is still controversial whether or not there have ever been, throughout history, scientific revolutions in the strictly Kuhnian

sense. Moreover, the definitive meaning of what exactly is a Kuhnian revolution still represents an object of debate.

However, even when we follow the interpretation outlined by Kuhn, we soon realize that a taxonomical shift is not represented as the essential factor defining paradigm shifts, albeit it may complement a revolution as part of the set of problems, solutions, metaphors, matrixes, symbols, associations, habits of perception a community transmits to its trainees. Kuhn's reconsideration of paradigms does not necessarily imply a redefinition of the revolutionary fact. Indeed, some of the scientific revolutions mentioned in his earlier works did not imply major changes on the taxonomic level. Consider the Copernican Revolution. Although intertwined with broader philosophical and theological beliefs, that scientific shift maintained much of its objects (the planets, for instance) with their previous names, while operating a drastic transformation on the level of their relations, interactions, gravitations, motions and positions. "After Copernicus, astronomers lived in a different world," wrote Kuhn (1962, 117), but many objects of such new world remained pretty much the same, and so did their names. Indeed the Greek 'two sphere universe' model was dropped, but most of the entities maintained their taxonomical designation as before. The Moon gradually lost its status of planet, Uranus obtained it in 1781, but their names remained untouched. Nonetheless, a scientific revolution happened.

While we can consider a taxonomical shift as a part of some revolutions, it seems not reasonable to establish it as a sufficient or necessary criteria to define them. Further elements have to be taken into account. In the specific case of hysteria, the decisive factors that brought to abandoning that entity were widely related to a social, cultural change of narrative, rather than a scientific discovery of any new evidence.

> Under the pressure of public opinion, which considered the word 'hysteria' as stigmatising, the équipe who drafted the DSM-III accommodated hysteria in various compartments, abolishing it from psychiatric nomenclature. [...] In 1993, with the 10th edition of the International Statistical Classification of Diseases and Related Health Problems (CID-10) by the WHO, and in 1994, with the 4th edition of DSM-IV it was established the end of the category of hysteria, together with its redesignation under new diagnostic classifications. (Ávila and Terra 2010, 337, 334)

We then have two options: whether we spouse the sociological idea of science as a human endeavour defined by a social factors, or we state that what caused the elimination of hysteria had nothing to do with 'science' or, at least, with the idea of anything like a value-free, objective, pure scientific research, independent from social narratives. The same applies to homosexuality, diagnosed as a mental illness until 1973, although with the relevant distinction that homosexuality does exist and has merely been depathologized, or rather redefined as 'Sexual Orientation Disturbance.' This brings us straight to Jerome

Wakefield's question about "what makes a mental condition a disorder" (see Faucher and Forest 2021; Brülde 2007).

A few more arguments keep us from following Hacking in claiming that the erasure of hysteria would integrate a scientific revolution. First, if we were to admit the coincidence of taxonomic shift and scientific revolution, we should then claim that having discovered (or decided) that homosexuality was not a mental illness would represent the 1973 scientific revolution in psychiatry. This may excessively diminish both the concept of scientific revolution and the dignity of American psychiatrists. Second, the comparison between DSM rewritings and some classical examples of scientific revolutions discourages the equation 'taxonomic shift' equals 'scientific revolution.' If we take some cases of paradigm shifts mentioned by Kuhn (1962) in the fields of the natural sciences, we find some seven revolutions along 400 years:

1543 – Transition from a Ptolemaic to a Copernican cosmology;

1687 – Transition from Aristotelian to classical mechanics;

1783 – Acceptance of Lavoisier's theory: the chemical revolution;

1859 – Charles Darwin's natural selection;

1905 – Development of quantum mechanics;

1916 – Einstein's special (1905) and then general (1916) theory of relativity;

1965 – Discovery of universal background radiation: the big bang theory.

On the other side, when we then consider DSM revisions alone, excluding ICD, we find 10 reformulations in only 70 years:

1952 – DSM-I;

1965 – DSM-I Special Supplement: on plans for revision to better align with the International Classification of Diseases;

1968 – DSM-II;

1973 – DSM-II 6th printing change: elimination of Homosexuality as a mental disorder and substitution of the new category Sexual Orientation Disturbance;

1980 – DSM-III;

1987 – DSM-III-R – Revised;

1994 – DSM-IV;

2000 – DSM-IV-T – Revised;

2013 – DSM-V;

2022 – DSM-V-TR – Revised.

If we accept the equation, we then have to consider that psychiatry alone passed through 10 scientific revolutions in just 70 years, rewriting its own kinds more often than any other scientific discipline ever. Against Hacking, we claim that

a scientific revolution in psychiatry has not taken place yet, and that the closest step towards it is currently represented by the transdiagnostic model, which is reconsidering not only the names given to clusters of behaviours, but is suggesting "the alternative conceptualizations of the processes implicated in mental health, and provides a platform for novel ways of thinking about onset, maintenance, and clinical treatment and recovery from experiences of disabling mental distress" (Dalgleish et al. 2020, 179). The comprehensive reformulation of such elements might be considered as constituting a proper Kuhnean revolution.

According to scientific discourse, the progressive refinement of knowledge production provides at every step an improved understanding and more accurate description of the world, which is assumed as being made of entities that have always been there and scientists progressively discover, describe and understand. In this process, some objects may have been wrongly identified as belonging to a certain kind, then recognized as unreal in light of a new discovery, model, or fallacy in identification. This implies that dismissed kinds have actually never been there to be observed, so that scientific belief in them was but an illusion. The history of phlogiston and oxygen provides an example of such evolution (Kuhn 1962). According to this discourse, it is the discovery of a better model of understanding, a new comprehension of reality that produces new kinds, providing a better explanation of long observed facts, together with more accurate predictions, being also able to account for past inaccuracy and failures. On this respect, we suspect DSM rewritings represent more the outcome of a long-lasting psychiatry's epistemological confusion than a proof of scientific evolution. Besides the frequency of such changes, it is remarkable that recent DSM and ICD nomenclature models both went back to the premises of Kraepelin's *Compendium der Psychiatrie*, of 1883, often developed against the late ideas of Kraepelin himself (on this, see Heckers et al. 2021): a descriptive model where observation of symptoms becomes the diagnostic criteria itself.

A change in scientific taxonomy might be a complementary condition for scientific revolution, but is not to be regarded as a sufficient one. As many scholars pointed out (Wade and Halligan 2017; Wade 2006; Heckers 2002), a revolution is much needed in psychiatry, but hasn't happened yet. Therefore, unless one accepts to embrace the idea of science itself as socially constructed, which we do not endorse here, it follows that the eliminations of hysteria, homosexuality, multiple personality and more, have rather to be considered merely as minor bias corrections. We further believe that a genuine scientific approach was represented better by Babinski than by Charcot.

Conclusions

DSM and ICD categories of hysterias were socially constructed scientific facts. Their reality, or rather their realization, represents a historical ontological crossroad where scientific knowledge and unjustified false belief happened to coincide.

Social justification around this belief has been challenged since the very beginning of its life, particularly by the presence of several competing theories advanced by peers (Babinski, Berhneim, Breton, Szasz, Lacan and more) showing that very different proposals were based on the same premises and reached radically diverse conclusions by means of shifts in interpretation. This kept us from endorsing the idea that doctors of the day were warranted to believe in its reality. Some of the theories around dysfunctional behaviours proposed a separate taxonomical designation for the observed phenomenon (psychasthenia, pithiatism, suggestion, etc.), some others kept the name hysteria, but advanced quite different content and explanations, fostering the idea that not one, but many hysterias were out there to be possibly known. Other doctors denied the existence of hysteria, adopting equally, when not more rational, arguments, under evidence based, empirical research.

Virtually dismissed by Babinski in 1901, hysteria survived its execution by engaging with the realm of the mind under Janet, James, Breuer and Freud. Both medicine (at first neurology, later psychiatry) and psychology (under psychoanalysis) largely relied on a model of the psyche conceived as a metaphysical entity. In this respect, we can claim that hysteria comes to integrate a specific set of entities in the history of science. We argue that hysteria has to be considered, more than a proper scientific entity, as a historical indicator of new domains of investigation, a necessary myth whose function was to stimulate the development of novel scientific research in the field of the abstract mind, social environment, and their relations with the physical and symbolical body.

As we are unwilling to sociologically support the idea that all entities and kinds created in the history of science are to be considered scientific by default, which would allow for too many false beliefs to gain the status of scientific knowledge, we rather argue in favour of the claim that the evolution of the sciences also requires the adoption of non-scientific concepts in order to advance in knowledge production towards unknown domains, as it happens with the notion of 'life' in biology (see Chomsky and Foucault 2011). In this sense, we also support the pragmatic claim that scientific statements, more than true, have rather to be valid in order to function within the process of knowledge production, and that the validation criteria adopted by the scientific community are subject to contextualist assessments, adapting the standards for knowledge attributions according to the purposes, the fields, the responsibilities and the standards in force (Resnik 2000; Lewis 1979). We thereby propose to consider the life of hysteria under those analytic coordinates. Caution: we are not claiming that hysteria was not scientific according to our current models of knowledge, which would represent an application of a commensurability petition of principle; we are rather drawing attention to the evidence that, to the eyes of many, that category was not acceptable even in the 19th century, when it has been established as a scientific kind.

By taking hysteria as main reference of our research, we have also been showing what kind of ontological and epistemological impasses prevent scientific reductionism alone to account for the definition of so-called dysfunctional behaviours, further giving sense of how scientific practice itself is deeply embedded and unavoidably intertwined with metaphysics, arts, religious beliefs, language and philosophy. We claim that even if, by lucky guess, a biological basis of such dysfunctional behaviors will be discovered one day, the system of classification that produced DSM and ICD categories would not deserve to be considered properly scientific in the sense of the materialist empirical hard sciences, but merely in the realm of the social ontology of its kinds and entities within behavioural and social sciences. Ontologically, scientific facts in psychiatry are thus considered as social facts, for the very fact of being facts instead of things. We have seen that, by adopting a taxonomy of illness based on a social ontology, the epistemology of psychiatry comes to represent a very intriguing case of liminality between hard sciences and social sciences, between science and philosophy, and even between philosophy and arts (Didi-Huberman 1982; Aragon and Breton 1928).

Having to further consider the very confusing puzzle of DSM rewritings overtime, we came to conclude that the methodology adopted by mainstream psychiatry to identify (or create) its diagnostic constructs is still at the mercy of serious ontological and epistemological inconsistencies and contradictions, as widely documented by philosophical, psychological and scientific inquiries (Moncrief 2023; Ghaemi 2013; Szasz 1961). Besides a general method-object contradiction, bringing the abstract mind, human experience and the soul into the materialistic empirical domain of medicine, it has been highlighted how the DSM and ICD taxonomic model that produced several categories which soon had to be dismissed due to their foolishness, has not yet shifted away from its original Kraepelian model, often developed against the late ideas of Kraepelin himself: a descriptive model where observation of symptoms becomes the diagnostic criteria itself.

The fact that the same set of criteria backing the old dismissed diagnostic constructs is still producing the current ones, makes uncertain and untenable the idea that doctors would be more warranted today to believe in the reality of current DSM entities. Some physicians argued that evidence of such ongoing unreliability is represented by the significant overlap between conversion and dissociation, as well as by the arbitrary shift of the term 'dissociation' itself, which has historically identified as both a symptom and a diagnosis, with a very evident semiological contradiction. Further inconsistencies, inherent to current psychiatric taxonomy, have been highlighted by several psychiatrists and represent as many relevant impasses. Among them: the very questionable distinction between the theoretically separate kinds of 'bipolar disorder' and 'schizophrenia' (see Lake 2012; Owen 2010); the fact that the diagnosis represents an artifact of the classification system, not underlying an actual

phenomenological reality (Borgogna et al. 2023); the lack of agreement around the very existence of scientific entities like 'schizophrenia' (Repnikov 2023; Van Os 2016; 2017); the existence of several, overlooked alternative models of classification which would rely on different ontologies, reshaping the entities psychiatry identifies as its object of study (Dalgleish et al. 2020; Kotov et al. 2018; Jaspers 1913); and even the very puzzling, contemporary use of the dismissed diagnosis of 'mass hysteria' in medical journals to describe current clinical reality (Jeong et al. 2024; Zhao et al. 2021).

In opposition to the thesis advanced by Ian Hacking (1994; 1998) the analysis of such abundant, growing arguments led us to conclude that, although DSM revisions have very frequently (perhaps much too frequently) undertaken various 'cosmetic' shifts, attributing new names to sets of human behaviours merely regrouped and reorganized, this does not represent a scientific revolution. As seen above, Thomas Kuhn himself had the chance to take distance from a nominalist reading of his thoughts. As a larger portion of the scientific community is becoming aware of such inadequacies, it has also been calling for a more radical shift from the paradigm in force (see Northoff 2023). Several alternative ontological models have been advanced to replace the current approach overtime, being met with strong resistance regardless of the inappropriateness of the system in place.

With a pattern that represents no novelty in the history of science, an unsatisfactory scientific *status quo* is far too intertwined with social entities, like private healthcare insurances, capitalist agendas of pharmaceutics, careers in academic institutions, State healthcare systems, labour policies, etc., which have been built on recognition and mastery of current taxonomic units and make it even more difficult to drop actual kinds to introduce structural novelties, further illustrating how the production of scientific knowledge is deeply embedded not only with philosophical and theological premises, but also with wider social factors, strongly determining the horizons of its evolution and existence. This brought us to further highlight the social-political component of knowledge production in scientific practice, although without defining science itself as a social construction.

We finally explored how the fact that a certain category does not conform to scientific standards of its time does not prevent the scientific community from believing in it, using it and transmitting it for centuries. What doctors, mental hospitals and other institutions have done on the basis of the 'false belief' of hysteria existed nevertheless, although their reasons, belief systems and methods were standing on the very threshold of scientificity, as do many of current DSM categories today:

> Ludwig Wittgenstein said that in psychology there are experimental methods and conceptual confusion. [...] We have more than that for the mental illnesses. We have the clinical methods of medicine, psychiatry, psychology; we have the innumerable variants of and deviations from psychoanalysis; we have systems

> of self-help, group help, and counselors including priests and gurus; we have the statistical methods of epidemiology and population genetics; we have the experimental methods of biochemistry, neurology, pathology, and molecular biology; we have the theoretical modeling of cognitive science; and we have conceptual confusion. (Hacking 1998, 10)

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