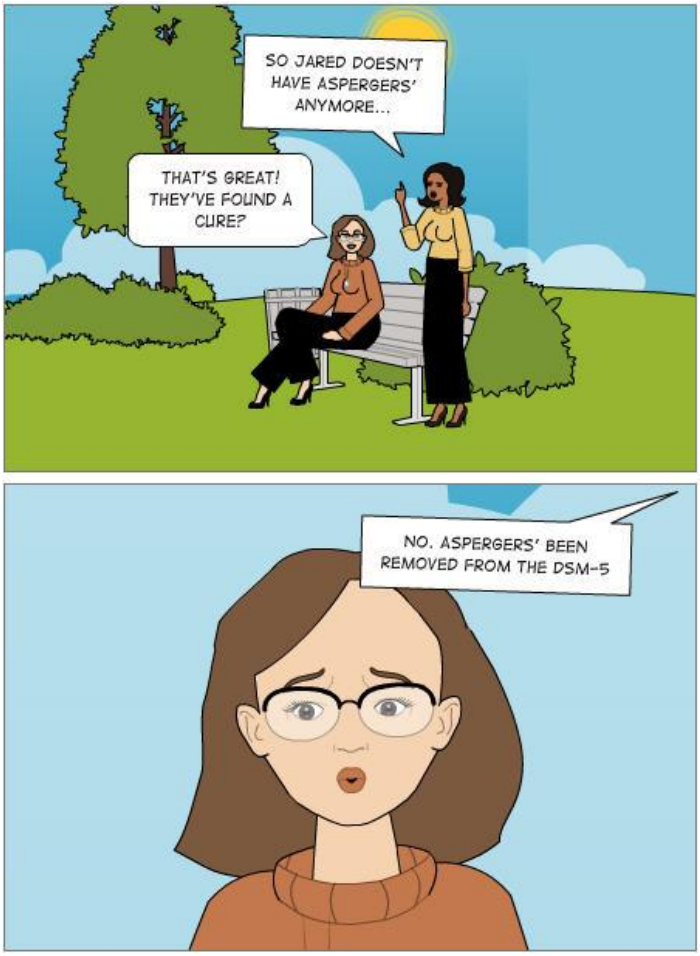
The authority of the DSM and the concepts of Co-production and Boundary Work

How are the members of the DSM-V Mood disorder work-group setting up boundaries in order to eliminate the 'Grief exclusion criterion'?



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The DSM-V

The Diagnostic Statistical Manual of Mental disorders (DSM) - produced by the American Psychiatric Association (APA) - is the most used handbook by clinicians worldwide in order to have a consensus on diagnoses of mental disorders. The aim of creating of the first DSM (DSM I) is according to the APA the standardization of language in psychiatric and psychological settings: "Diagnostic criteria provide a common language for clinical communication and their use has been shown to increase diagnostic agreement between clinicians. (American Psychiatric Association [APA], 2011, 'What is the DSM and what is it used for?'). Despite of being controversial since its publication in 1952, it has remained authoritative in the field of diagnostic psychiatry over the past decades. (Dehue, 2008, p. 23). The DSM I integrated the information derived from systems for collecting census and statistics from psychiatric hospitals. Information from the manual developed by the US army (Medical 203) proved to be useful for establishing the DSM as a utensil for clinical practice as well. The DSM was a variant of the ICD 6, a diagnostic handbook developed by the World Health Organisation (2014, WHO). In the DSM II the APA included biological perspectives and concepts from Emil Kraeplin's classification system. (Decker, 2007). Symptoms were not yet specified in detail for a specific disorder. The DSM has been dramatically revised in 1980, which lead to the DSM I II. The last major revision was in the current fourth version of the DSM: the DSM-IV. The DSM IV consists of a list of mental disorders, the *diagnostic classification*. Accompanying the classification is the associated *set of diagnostic criteria* and finally there is a *descriptive text* included in the disorders. (APA, 2011, 'DSM-IV').

Up to date, there have been four editions of the DSM and two subversions, and the fifth edition, the DSM-V is currently in the making, due for publication in May 2013. The reason for drawing this fifth edition is that the DSM has to keep track of new advancements in science and technology, however, also in order to meet the new and existing needs in society in a better way. Another important reason is to expand the scientific basis for the categories, which seems to be a continuous endeavour for the DSM taskforces. (APA, 2011, 'Overview: The future manual' & 'History'). This new version of the DSM is highly anticipated, however, the proposals for revision of this version of the DSM has stirred up some controversy as well, as happened often with the other editions of the DSM in the past.

One of the proposals has been brought up for revision by the major depression (MDD) sub-work group of the Mood Disorder work-group. Their proposal is "the deletion of the *grief*

exclusion criterion" from the criteria for Major Depression (APA, 2011, 'FAQ'). In the DSM-IV major depression as a result from grief or bereavement was excluded from the criteria, unless persisting longer than 2 months. (APA, 2011, 'DSM-IV'). However, the proposal on the table now, assumes 2 weeks of grief, and if the depressed symptoms, meeting the criteria of element A, B and C in the new DSM-5 are met, the clinician can start diagnosing a Major Depressive Episode (MDE). Former member of the DSM-IV taskforce Allen Frances has criticized the proposal of eliminating the 'grief exclusion criterion', stirring up a controversy which manifested itself in a lively written debate between different clinicians and members of former and current DSM-work groups (Philips, 2010).

Subsequently, I would like to look into detail at the debate and see how the different actors delineate their territory. How does the *Mood disorders work-group* create and recreate the borders of their work, and how are they claiming authority for it? I shall attempt to answer these questions by using the frameworks drawn by Sheila Jasanoff and Thomas Gieryn: respectively the idiom of *co-production* and the concept of *boundary work*. In my opinion, this DSM case is a very interesting subject to look at from the perspective of Science and Technology Studies (STS). STS is particularly interested in the relationship science and society. The DSM makes very clear that society and science/technology, or *techno-science* if you will, is co-produced. 'Co-production' is a term mostly known from the work of Sheila Jasanoff (2004). As the many examples I have borrowed from Jasanoff will show, and as the DSM-case will, it does not make sense any longer to "assume that scientific knowledge comes into being independent of political thought and action, or that social institutions passively rearrange themselves to meet technology's insistent demands" (Jasanoff, 2004, p. 15).

Gieryn's concept of boundary work helps me with carefully breaking down the arguments involved in the debate, and see how the members of the work-group of mood disorders and their counterparts set up the boundaries of their profession, the DSM and science itself.

The Co-Productionist Idiom

According to Jasanoff, although there has been a lot of very important research into the workings of society and science and technology which clarified a great deal, the current discourse is not sufficient in trying to grasp the current developments in the relationship between society and science and technology. (p. 17). *Co-production* is a concept to help with generating a more adequate discourse in order to discuss new scientific and technological

occurrences and their interaction with society. The aim of the concept of co-production is to give a nuanced and as complete possible account of the world, whilst always steering clear of the pitfalls of social and technological /scientific determinism.

> "*Co-production* of scientific knowledge and social and natural order seeks to transcend the tensions (…) between the standard and constructivist images of science and technology. Science is understood as neither a simple reflection of nature nor a mere epiphenomenon of social interaction" (Bal, Bijker & Hendriks, 2009, P. 43).

Types of co-production

Jasanoff distinguishes two strands in the literature based on the concept of co-production: the constitutive line of thought and the interactional. (p.15 & 18). "The former is primarily concerned with the ways in which stability is created and maintained, particularly for emergent phenomena, whether in a particular site where knowledge is made, such as a research laboratory, hospital or legal proceeding, or around a novel techno-scientific object, such as the human genome or a periodic table for chemicals." (p.18 & 19).

This approach enters on the metaphysical level, because it does not take disciplines, fields or realms for granted: it asks for example what a mental disorder as opposed to a physical disorder is. But co-production goes further: it will also ask how a certain state of knowledge came to be at that way, how it is maintained, or why something is discarded. The interactional strand of coproduction has however not its focus on 'what is' but more how it became what it is, and how an already demarcated domain is maintained, even when there is a conflict or ambiguity with the knowledge it pertains.

> "This line of work takes for granted that, in most exercises of world-making, neither science nor society begins with a clean slate but operates always against the backdrop of an extant order, in which people already 'know' in pragmatic terms what counts as nature or science and what as society or culture. Nonetheless, boundary conflicts about where these domains begin and end continually arise and call for resolution (…). Work in the interactional mode probes how human beings organize, and periodically reorganize, their ideas about reality under these circumstances." (p. 19).

Jasanoff, with explaining the second approach, touches upon another concept, that of boundary-work. I will come back to the notion of Boundary-work later, first I want to discuss some of the examples Jasanoff gives of both strands of co-production work.
It is not useful to compare the constitutive line of thought and the interactional one with each other, with the idea that one should be a better fit than the other, contends Jasanoff: both have their merits in different kind of contexts and situations. "(…) some S&TS scholars see co-production as a process that is as foundational as constitution making or state-making in political theory, because it responds to people's deepest metaphysical concerns. (…) Others working in a co-productionist vein are less concerned with metaphysics and more interested in the practical accommodation of new knowledge within existing forms of life" (p. 21).

There are four "recurrent and partially overlapping preoccupations" in STS which can give structure to the co-productionist concept: "The first has to do with the emergence and stabilization of new objects or phenomena (…).The second concerns the framing and resolution of controversy. (…). The third important line of research centers on the intelligibility and portability of the products of science and technology across time, place and institutional contexts. (…) The fourth significant tradition examines the cultural practices of science and technology in contexts that endow them with legitimacy and meaning" (p. 5 & 6).

A nice example of a constitutive co-productionist account, which has to do with the emergence and stabilization, is that of Bruno Latour: *We have never been modern* (1993). He is famous for his 'actor-network theory', in which he makes no difference between human actors/actants and non-human actors/actants when talking about agency: both are capable to act. The actor-network starts from scratch, that is, it is a pure co-productionist account because it supposes that nature and society alike are a part of a cross-boundary system of connections. "(…) It does not presuppose any a priori demarcations of the world before that world is worked upon by human imagination and labor" (p. 22). Although Latour looks into the function and activity of the network and its actants, according to Jasanoff he neglects the questions of "why the organization of technological practices or the credibility of scientific claims varies across cultures; why some actor-networks remain contested and unstable for long periods while others settle quickly; why work at some nodes stabilizes a network more effectively than at others, or what role memories, beliefs, values and ideologies play in sustaining some representations of nature and the social world at the expense of others" (p. 23). It seems he downplays the human component of agency and values in favour of the non-human *actants*, she criticizes.

Also in the emergence and stabilization focus is Andrew Pickering. He tries to fill the gap Latour has left behind. He does that by pointing out that "there are things that machines and devices can do that no thinkable combination of human actors could accomplish without technological enhancement", and the other way around. "The two kinds of actors nevertheless depend on one another at every phase of scientific practice – and Pickering argues, also in cultural practices more broadly." Agency, according to Pickering, is a mix of human and mechanical/material conduct and possibilities and human values, goals and norms. He uses the metaphor of the mangle, in which he throws the latter components. Jasanoff thinks however that the results coming from the 'mangle' are too unpredictable. She is convinced that society has more to say when it comes to managing the technoscience-society relationship. Culture can for example exercise a huge influence on how a technological device is used. (p. 25).Philip Kitcher, for example, leaves more room for human agency; but, he warrants, human beings cannot control or know everything of what nature is able to provide us. There is another example which looks at things slightly different in comparison to Latour and Pickering:

> "If Latour and Pickering focus primarily on the production end of representing the world, [Benedict] Anderson and [James C.] Scott are concerned as much or more with reception, in their case, the uptake of the results of such representations by powerful, and for Scott (1985) also powerless, agents in society" (p. 26).

Anderson looks at the power of representations in connection to the nation state. Scott looks at 'high modernist' planners, who match the way they designed a location geographically and socially, to their 'ideas' and visions. "The purpose of these grand plans was to make citizens and their economic productions more "legible", that is easier to count, survey, order, exploit and control" (p. 26).

"Just as constitutive co-production usefully takes the metaphysics of Latour and the French school of actor-network theory as its point of departure, so the interactionist strand can be grounded, to start with, in the epistemologically oriented Edinburgh school of sociology of scientific knowledge" (p. 28).

Jasanoff explains how, still in the constitutional view of co-productionist work, Steven Shapin and Simon Schaffer have looked at the dispute between Thomas Hobbes and Robert Boyle to understand how two interpretations of scientific conduct were defended and delineated. They are obviously engaged in the *framing and resolution of controversy*.

(p.5).The debate was focused on the credibility of each others experimental methods at first, but in the end they marked a difference in ways of looking at the world, the social order the men were belonging to. Instead of science and politics being separate realms, there is "a necessary parallelism between goings on in these two spheres of human activity" (p. 30). Jasanoff refers to the concept of Boundary Work of Thomas Gieryn (1995) in this respect, underlining the ideas of Shapin and Schaffer "suggesting that experimental science's claims of reliability and truth had to be sustained through elaborate and carefully designed social practices (…)" (p.30). I want to look more into detail in the concept of boundary work in the next chapter.

Boundary work

Thomas Gieryn (1995) coined the concept of boundary work. Boundary work is part of the interactional strand Jasanoff described in her account of co-production. He has looked into Shapin's and Shaffer's description of the dispute of Boyle and Hobbes. His opinion is that the debate is a stereotypical example of a type of boundary work in the field of science:

> "The debate is a classic specimen of a kind of boundary-work involving science, where contending parties carve up the intellectual landscape in discrepant ways, each attaching authority and authenticity to claims and practices of the space in which they also locate themselves, while denying it to those placed outside" (p. 106).

First I want to explain in brief the history and motivation for the concept of boundary-work before I will illustrate the concept of boundary-work with the example of the Boyle-Hobbes conflict further.

It seems as if there are such domains, clearly delineated such as science, technology and society. However, Gieryn contends, they are not as nicely demarcated as we think. It would not be strange to ask what science, technology and society actually are. The first term, science, the concept he is interested in, does not have a uniform meaning. The question 'what is science?' has proved to be difficult to answer. Numerous philosophers, sociologists and other scholars have tried to take up this challenge, among others, the well-known academics Thomas Kuhn, Robert Merton, Karl Popper and more recently Harry Collins and Thomas Gieryn have looked into the subject. This problem of demarcation addresses many questions

concerning science, as Gieryn articulates them:

> "Where does science leave off, and society – or technology- begin? Where is the border between science and non-science? Which claims or practices are scientific? Who is a scientist? What is science?" (p. 393).

The boundary problem has two perspectives. One is called 'essentialist', the other 'constructivist'. *Essentialists* think of science that it has certain internal or external qualities, which distinguishes science from non-science. That is why science is authoritative and is able to make claims about nature. *Constructivists* are convinced that there are no such things as rules, norms, inherent qualities and which are universally applicable, which govern science and delineate it from non-science. They believe instead that the distinguishing of science and non-scientific activities are dependent upon the local circumstances and it is an: "interests-driven pragmatic accomplishment drawing selectively on inconsistent and ambiguous attributes" (p. 393). He gives an overview of the essentialist tradition in history, by going into the studies of Karl Popper, Robert Merton and Thomas Kuhn. They all, in some way, believe in norms and principles governing the scientist's actions: Popper contends that: "Science advances toward truth (though never arriving at certainty) by a combination of bold conjecture and severe criticism" (p. 395). Being very much an essentialist, according to Merton there are four social norms in science: *Communism, Universalism, Disinterestedness* and *Organized skepticism.*

Kuhn is less an essentialist. He focuses on the moral power of *cognitive norms:* scientists are following a certain paradigm. The scientific paradigm, which can be defined as 'way of thinking' structures the way of knowledge-making, because it consists of certain professional background and ideas of the scientists, which they share or come to share, when working under the same paradigm. However, Gieryn explains, after a great endeavour of trying to find how science is demarcated from non-science, unfortunately Kuhn fails to ask what the definition of science is: he does not deem it important, because he is convinced that scientists themselves don't seem to think it is important. Gieryn comments: "What Kuhn chose not to consider is that the degree of consensus in science itself might be a matter of interpretation, negotiation and settlement – by scientists and sometimes other involved parties" (p. 403).Gieryn expresses another critical point by citing Gilbert and Mulkay (1984) that often a given (scientific) field itself often doesn't reach consensus, in contrast to Kuhn

implying that it is actually an aspect of the paradigm-concept. So, how is it possible to solve this boundary problem? Gieryn wonders:

> "If there is nothing inherently, universally, and necessarily distinctive about the methodology, institution, history, or even consequences of science, then why and how is science today routinely assigned a measure of *cognitive authority* rarely enjoyed by other cultural practices offering different accounts of reality? (…) The challenge is to explain the cognitive authority of modern science without attributing to it essential qualities found by sociologists to be anything but essential" (p. 405).

Boundary work is present in all the cases in which boundaries, borders or other separations between areas of knowledge are constructed, established, advocated, reinforced or attacked. What is then science? Science, for Gieryn, is not sharply, solidly demarcated field, insensitive to change. On the contrary, it is a fluid realm, flexible and ever-changing, throughout context, circumstances and time. He looks at different interdisciplinary examples of which he derives his starting point for Boundary work. He explores the sociology of professions, history of cultural classifications and the Feminist studies of science. (pp. 407 - 424). For example, *Andrew Abbott*, with his book 'systems of professions' makes it clear that looking at the conventional categories of 'science' or 'art' does not give any answers, instead, it is important to look at how professionals demarcate their territory, their profession and how they define the tasks and duties within that profession (pp. 409-411). From there, Gieryn switches to the concept of *social worlds*, which can be found in the work of *Everett Hughes*, who also looks more or less at what is done by professionals to distinguish their occupations from other occupations.

The strand *History of cultural classifications* sees science as a 'cultural space' (Gieryn, p. 416). It does not only look at who produces science and how it is produced, but also has attention for who is using it. Questions popping up would be: What does science mean? What are the effect of labelling and categorizing of science in society? How are institutions established and how do they shape society and vice versa? (pp. 416 – 418). The feminist studies of science then, finally, look at the role of women in science. Although there were many women contributing to science in the past, their role has been underrepresented until rather recently. Women were banned from the realm of science, or were banned from the accounts of scientific processes. This is boundary work not on the level of what is scientific

and what is not, but which groups are rendered to contribute to science and which groups are exempted (pp. 422-423).

> "In sum, feminism advances the boundary problem in STS by exposing the gendered configurations of science (and the scientific configurations of women) and by showing the practical utility of such boundary-work for excluding or marginalizing women's place in the scientific enterprise" (pp. 423-424).

Gieryn continues with showing works he finds exemplary for boundary work in STS.

Types of boundary work

According to Gieryn, there are four types of boundary work: *monopolization, expansion, expulsion, and protection*. (p. 106). The examples of monopolization and expansion are the most useful in outlining the DSM-case, thus I will summarize these two examples as clearly as possible. The Boyle and Hobbes conflict is an example of monopolization. Gieryn uses the 'cartographic maps' metaphor to shed light on the debate between Boyle and Hobbes. He explains that they have two different maps of the world, in which they assign a different meaning to objects, have different ideas and backgrounds. According to Gieryn, Boyle attaches 'authenticity' and, most importantly 'authority' to his experimental physiology, banning from the scene the 'non-scientific' fields of metaphysics, politics and religion. For him, nature tells you what science is:

> "Facts gained authenticity through their collectively being seen, via a multiplicity of witnesses extended through three means: opening the house of experiment – the nascent laboratory – for public view, so that like-minded experimentalists could see for themselves; replicating the experiment by building air-pumps throughout Europe; and allowing for *virtual witnessing* of elaborate textual and faithfully detailed graphic representations of the experimental apparatuses and procedures" (p. 425).

Knowledge derived from personal, private experience, such as the experiences religious zealots and alchemists claimed, had no part in philosophy, according to Boyle. This claim is in contrast with Hobbes, who believed that the pursuit of the mind was uttermost important. He believed in Geometry, which was the way to create knowledge.

> "On the philosophy or science side of Hobbes's border was certain knowledge, rational deductions of securing irrevocable, universal, and obligatory assent"(p. 426)

According to Gieryn, Hobbes' knowledge making was happening in the mind. It was not necessary to do experiments, because philosophy is not about looking for facts, but in order to find causal explanations. By logical deduction, you could arrive at an explanation, which no experiment could arrive at. As Gieryn summarizes Hobbes line of thought; "People made knowledge certain by deducing inescapable explanations as they proceeded rationally from agreed-upon definitions" (p.426).
The fact that we do experiments, like Boyle did nowadays, and replication, does not mean it was the logical outcome of the conflict between Boyle and Hobbes.

> "If Boyle's experimental space has achieved legitimacy, it is not because its contours – against those of Hobbes – more closely corresponded to *real* science; nor should we assume that Boyle's cartographic arguments would necessarily work on any other occasion when the boundaries of science are contended (there are no universal determinants of success). Boyle *won* because his space was better able to hold the diverse interests of powers-that-be in Restoration England (…)" (p. 428).

According to Gieryn, the contest is not even over yet. Even although we now depend on experimentation and replication, Boyle's heritage of thought is never far away.

Second is the example of what Gieryn calls expansion. This happens "when insiders seek to push out the frontiers of their cultural authority into spaces already claimed by others" (Gieryn, 1995, p. 429). Gieryn looks, by using the work of Robert Darnton, how the enlightenment philosopher D'alambert attempts to draw the boundaries for philosophy, to which he is convinced reason and empiricism belong and he puts it on a par with 'the science of god'. He not only creates the domain of philosophy, however, he takes the elements from other existing domains, such as religion, and ascribes it to its own philosophical sphere. D'alambert was convinced that "*philosophy*- D'alambert's inside authoritative space- could swallow up whatever counts as genuine knowledge, leaving only poetry and memory outside" (p. 429). By showing that all good came from the earlier philosophers, D'alambert tries to convince others of his claims.

The DSM-case in the light of Co-productionist Idiom and Boundary work

Thus, what can the concepts of Co-production and Boundary-Work mean for the proposal of eliminating the '*bereavement exclusion criterion"*? The co-production concept in itself is not ready to be applied to a case; it is rather something you bear in mind when looking at the links between science and society. As Roland Bal, Wiebe Bijker & Ruud Hendriks put it:

> "Because of this *two sides of the coin* character, a co-productionist approach does not yield great explanatory power, but rather functions as a neat metaphor that helps to describe the intimate relationship between the production of scientific knowledge and natural and social order (…)" (2009, p. 43).

So, while keeping the co-productionist notion in mind, the concept of *boundary work* may offer a practical way to understand the authoritative power of the Diagnostic Statistical Manual of Mental disorders. Some scholars have doubted whether the DSM is scientific, but this is a rather naïve understanding of what science is, as Trudy Dehue points out in her book (2008). As Jasanoff and Gieryn showed, science is not clear defined, factious place. We do not find the basis for science in nature itself. The DSM is an object that is thoroughly 'co-produced'. The continuous revising of the handbook makes clear that it the DSM follows every move society makes. It is important to realize that the 'mental disorders' of the DSM are themselves reflections of the society's developments in thought, aspirations and idea's and technological and scientific developments.

> "Intellectual fields or spaces are not constituted once and for all but continuously reconstituted in discursive practices through which they sometimes achieve institutional stability and obduracy." (Gieryn, 1995, p. 418).

If science is not the solid and untouchable entity as we thought it would be, it is hard to maintain the DSM is unscientific or is, as some said, without any scientific foundation. The DSM categorizes and classifies, and even categorizing and classifying scientifically is the work of human beings. So how then is Boundary work done by the subgroup working on DSM, and how does the rivaling party do their boundary work?

The parties visibly involved in the debate about the elimination of the grief exclusion criterion are: the Mood disorder sub-workgroup of Major Depressive Disorder and task force of the DSM-5, Allen Frances and clinicians writing to the New York Times, responding to Frances' article 'Good Grief'(Frances, 2010).The process of working on how to work on the revision, is described on the APA DSM-V website. In short, this is what happens:

> "Each work group meets regularly, in person and on conference calls. They begin by reviewing *DSM-IV*'s strengths and problems, from which research questions and hypotheses are first developed and then investigated through literature reviews and analyses of existing data. They will also develop research plans, which can be further tested in *DSM-V* field trials involving direct data collection. In order to invite comments from the wider research, clinical, and consumer communities, the APA launched a*DSM-V Prelude* Web site in 2004, where these groups could submit questions, comments, and research findings to be distributed to the relevant work groups" (APA, 2011, 'DSM-V, the future manual').

The reason for the proposal to eliminate the 'grief exclusion criterion' is mainly because -according to the work-group of Mood disorders - it doesn't make sense to have it. In the DSM-III and therefore also the DSM-IV the 'grief exclusion criterion' was based upon the claim that "bereavement related depression differs meaningfully from depression related to other stressful life events". Research conducted by members of the workgroup recently (Kendler, Myers & Zisook, 2008) showed that there are no actual differences: "The similarities of bereavement-related depression and depression related to other stressful life events far outweigh their differences." And therefore, they came to the conclusion that "On their face, these results argue against the continued use of the bereavement exclusion rule in the DSM-V(…)" (2008, p. 1454). However, they do admit that other researchers came to radically different conclusions, which makes it according to them a challenging issue. Still, on visiting earlier literature reviews, it becomes for them plausible to go with the elimination of the *grief exclusion principle*.

As the whole idea of making the new DSM-V is to revise the categories and system of the IV, it is still interesting to see that the work-group now rejects the position of the DSM-IV on the exclusion criterion because it "is not logically defensible". (Kendler, 2010). It is on what they define as science that they do not accept the grief exclusion criterion anymore, they

do not take other sources into account, it seems. This portrays a setting up of boundaries between what the DSM currently is and what it supposed to become: more scientific-based. Another way of boundary work becomes clear with the referral of Kendler (2010) to the DSM-editions from before the DSM-IV: they did not have a grief exclusion criterion. Plus, the World Health Organistion (WHO) he says, did not have this bereavement exclusion from their category of Depression either in their International Classification of Diseases (ICD). The latter is a classical example of boundary work, collecting arguments for the revision by referring to another authority. Gieryn talks about this in his second example of *expulsion* in the case of D'alembert, where it is possible to draw "independent authority for one's own map by linking it to cartographic efforts of earlier generations of boundary-workers (objectification by attributing authorship elsewhere)" (Gieryn, 1995, p. 431).

Allen Frances voiced his concern in the New York Times, because he is concerned that "This [the elimination of the grief exclusion criterion] would be a wholesale medicalization of normal emotion, and it would result in the overdiagnosis and overtreatment of people who would do just fine if left alone to grieve with family and friends, as people always have. It is also a safe bet that the drug companies would quickly and greedily pounce on the opportunity to mount a marketing blitz targeted to the bereaved and a campaign to teach physicians how to treat mourning with a magic pill." By saying that, he implies that allowing grief to be diagnosed as MDD will lead to labels which will lead in their turn in immediate actions from involved physicians. In this context, it is important to understand that the principle aim with writing the diagnostic statistical manual was, in contrast to how it has become used, to standardize the use of language in psychiatry by formulating clear criteria when applying the names of disorders. However, in the practice of health care and research, the criteria for the labels use, came almost automatically to stand for its causes, states Dehue in her book 'De Depressie Epidemie' (2008). She illustrates this with Major Depressive Disorder, which according to here is now the cause of the symptoms where it stands for. The realization that labels derive their foremost meaning from their implications, which can differ throughout time and context, is very important for the position of the DSM, according to Dehue. (p. 16). This is in agreement with what the co-productionist idiom expresses, namely that there is no science without society. There is an interaction. By only referring to the research, which refuted the claim of differences between bereavement related depression and other stressor-related depression (such as divorce, severe physical illness etc.), the work group actually implies that science works separate from society. They assume that *good clinical practice*, which every clinician should perform, will consists of 'watchful waiting' (his means

that a clinician, who has someone presenting with grief symptoms after two weeks, pointing to MDD, can wait (following the rules of clinical conduct) with treatment. However, the physician should keep this person in eyesight, in order to be able to act immediately with treatment whenever the situation would worsen. He emphasizes:

> "Watchful waiting is an important tool for skilled clinicians. As a good internist might adopt a watch and wait attitude toward a diagnosable upper respiratory infection assuming that it is unlikely to progress to a pneumonia, so a good psychiatrist on seeing an individual with major depression after bereavement, would start with a diagnostic evaluation".(Kendler, 2010).

However Frances contests this by implying that most psychiatrists do not 'watchfully' wait and are not capable of distinguishing between grief and MDD or MDE. (Frances, 2010). According to him, the boundaries stretch to far when adopting the revision. In their turn, the sub-work group responds by saying that they then have to exclude depression following every adversity (Kendler 2010). Because they found out in their study (Kendler, Myers & Zisook) that depression after bereavement and depression after a severe stressor, such as a divorce or job loss, shows no difference in symptoms.

The sub-work group has set up the boundaries for the DSM in terms of empirical evidence, whilst Allen Frances looks at what the categories mean in practice: he gives priority to the effect on society. It is in the same way Boyle and Bacon were delineating their views on methodology, in Gieryn's example. As said before, Boyle attached authenticity and authority to his territory, whilst Bacon "knowledge grounded in private and personal experience".(Gieryn, 1995, p. 425). Frances, considering the effect the label may have, chooses not to eliminate the criterion, while, based upon the 'evidence' the work group wants to eliminate the criterion.

Philips' analysis that the debate is pure a result of the different language and terminology the parties use, and are used in the DSM and research of Depression and Bereavement, completely goes past the actual issue at stake. He comes up with a solution in the form of new categories: "We need a category for ordinary grief; one for grief that reaches a level of MDD; and a third, mid range category for depressive reactions to the loss of a loved one that are more than ordinary grief and less than MDD" (Philips, 2010). However, the point Frances makes is that as long as there is grief or bereavement in a DSM category, in practice, this will always lead to a label, which turns grief into a medical thing, being able to be treated.

Another responder, Elena Lister, says that it is a good thing if grief/bereavement is connected to the category of MDD and MDE. According to her, it makes it easier to help people with their grief and assigning them therapy, because when grief is included, the insurance will be more likely to pay. (Lister, 2010). She looks at the functional/practical side of the decision. This is a very different reason for allowing the elimination. Whereas the sub-work group is focusing mainly on outlining the scientific character of the decision, the other parties, whether or not in favour of the elimination, come up with more or less social argument.

Philips does point out something which is just as much boundary work as the debate about the DSM-category of bereavement-related depression, namely, the question: what is exactly the definition of grief? Sidney Zisook and Katherine Shear discuss grief in their attempt to show the importance to the psychiatric field, hence the title of their work "Grief and Bereavement: What psychiatrists need to know" (2009). They try to trace what is 'normal grief' (uncomplicated) and what they call 'complicated' grief. From this study, it becomes clear that nothing tells you what grief is and how to diagnose MDD apart from grief. There is no physical process in the mind or body measurable. It is the outer symptoms which are categorized by the DSM and which then are collected under one denominator. In a society which has concepts as 'evidence based' and 'randomized controlled trials' as their greatest good, it is hard to maintain authority. In my opinion, in order to let the work-group succeed in implementing the revision successfully, they have to give attention to, what they deem, scientific evidence, but also respond to the more or less social concerns of the other parties in the debate.

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