FACULTY OF PSYCHOLOGY
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Considerations Concerning the Dominant Scientific Approach Regarding Depressive Disorders: An Invitation for Critical Reflection

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Introductory Consideration

For many years, the slogan ‘Dare to think’ has been the central motto of our university. This phrase encapsulates the ideal of our institution and is promoted within all of its faculties. Throughout the work on this dissertation, we took the liberty to expand this motto with one adjective ‘Dare to think… *critically*’. In doing so, we wish to make a clear distinction between ‘thinking’, which usually stays within the bounderies of the dominant scientific paradigm; and ‘thinking critically’, which, in our view, embraces a wider outlook – from outside of this dominant paradigm. This dissertation could be seen as a fierce and – from our perspective – necessary critique, of how Western (and increasingly global) society has been responding for many decades to the rising prevalence of depressive disorders throughout the world: mainly in the form of a neo-positivist biomedical and psychological scientific approach. Essentially, we wish to contribute to an expansion of the current lines of ethical, methodological and social inquiry into this soon-to-be global public health priority number one. Critical arguments like the ones we develop do not seem to enter the public domain, even though the extent of scientific progress in dealing with depressive mood remains questionable. In this dissertation, we hope to have made a small contribution to the creation of a more pluralistic scientific debate about the nature of depression – and about its rising prevalence, its treatment, and ultimately, about its prevention

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Chapter I

Depressive Disorders: From Epidemic to Pandemic – Mind The Gap

1. Epidemiological Research Concerning the Prevalence of Depressive Disorders

Around the turn of the last century, several epidemiological studies estimated that depressive disorder was highly prevalent across the world and that this was a matter of utmost concern, given the association of clinically depressed mood with increased risk of suicide, high levels of disability, social isolation and loneliness, and elevated use of health and social care services. Even in Western countries, untouched by war and environmental trauma, surveys find that the prevalence of depressive disorder is disturbingly high. In the USA, for example, 16.6% of citizens are likely to suffer the problem in their lifetime; at an economic expenditure for society of approximately US$ 83.1 billion (Greenberg et al., 2003; Kessler et al., 2005). Lifetime prevalence for MDD and dysthymia in Europe are 12.8% and 4.1%, respectively; point prevalence for MDD and dysthymia in different European countries varies between 1.1% and 3.9 % (ESEMeD 2004a; ESEMeD 2004b). In the UK, psychiatric surveys consistently find that depression and anxiety are the most prevalent psychiatric conditions, afflicting well over 8% of the population (Singleton et al., 2001).

In 2003, it was estimated that depression in adults in the UK had a societal cost of £ 9 billion a year and caused over 109 million working days to be lost annually (Thomas & Morris, 2003). Three years on, the potential societal cost of depression in the UK for 2009/2010 was projected to reach 150 billion pounds, with the inability to work accounting for 30 billion of this total (LSE, 2006). Overall depression accounted for 11% of disability worldwide, and this figure is rising (Hirschfeld et al., 2000, Stewart et al., 2003). Researchers have long argued that MDD

is a chronic disease, but they have been surprised at how quickly it has become the leading international cause of disability in this century. In 2008, DeHue quite rightly introduced the notion of 'The Depression Epidemic'.

If there is good evidence that states of severe and crippling sadness are pervasive, then what can this survey data tell us about the possible causes? One of the more recent global studies of depressive disorder confirms once again its high point prevalence across most of the world (Ferrari et al., 2013). As far as insights into the societal origins of depression are concerned, however, it is difficult to draw many clear-cut conclusions from this study. For example, while people in poor countries can report depressive mood – the point prevalence for places like Colombia (6.31%), Somalia (6.34%) and Congo (6.45%) is not dramatically above the figures for higher income countries like Singapore (5.74%), Switzerland (6.16%) and Luxembourg (6.55%). However, the point prevalence of MDD is unambiguously raised for those unfortunate enough to be caught in the midst of armed conflict or humanitarian emergency; as suggested by the figures from this same survey for the entire North Africa/Middle East region (7.35%) and Afghanistan (22.5%) (Ferrari et al., 2013).

One cannot talk about public health issues on a global scale, without referring to the most authoritative organisation worldwide concerning health matters: the World Health Organisation (WHO). On its website, the WHO states that its goal is ‘to build a better, healthier future for people all over the world’ and ‘to ensure the highest attainable level of health for all people’. Furthermore, the WHO suggests that the study, prevention and management of *mental health* disorders have become increasingly important over the past few decades. The tone of the WHO reports on mental health disorders changed considerably. Earlier reports still leave room for interpretation of the data and are more tentative in their recommendations. In more recent publications, however, WHO explicitly suggests that current prevailing ideas on the prevention

and management of mental health disorders are clearly insufficient (WHO, 2011, infra).

Finally and more importantly, there is the 'gap issue'. The WHO has for many years suggested the importance of 'Minding the Gap' in relation to mental health, even though it was not always very clear to which gap the WHO was principally referring: the treatment gap or the inequality gap. Consequently, we might have been minding the wrong gap for too many years.

2. Mind the Gap – The Secretariat of the Executive Board of the WHO (2011)

In the past couple of decades, scientific knowledge concerning point prevalence and lifetime prevalence for depressive disorders has grown significantly. It has arisen from the successful efforts of researchers to harvest and analyse ever more data, which in turn has fostered the growth of more specific hypotheses about possible causes of the disorder at the level of populations and to more ideas about treatment, prevention and management.

In a report by the Secretariat of the Executive Board of the WHO – published in 2011 – depressive disorders are described as the most prevalent mental health disorders worldwide, and set to become the leading cause of the Global Burden of Disease (GBD) by 2030. Moreover, in one of the first paragraphs, WHO argues that there is far too wide a gap between the need for treatment and it's availability, for most people across the world. Consequently, untreated psychological conditions account for 13% of the total GBD. Between 76% and 85% of people with severe mental disorders obtain no treatment for their condition in low- and middle-income countries; and even in relatively wealthy countries the picture is the same for fully 35% to 50% of those who suffer mental health problems. This same document judges the financing for treatment and prevention of mental disorders to be insufficient. Of those financial resources that *are* directed to mental health, 67% are invested in psychiatric hospital treatment,

despite the historically strong association with 'poor health outcomes and human rights violations' (p 2).

One could argue that this *treatment gap* is indeed incomprehensible for mainly four interrelated reasons, as suggested in this WHO report. First, mental disorders correlate with high mortality rates due to an increased risk of suicide and the comorbidity between mental disorders – specifically depressive disorders – and physical health problems. Second, WHO points out that the social and economic impact of mental disability is both omnipresent and radical (e.g. unemployment, marginalization, poverty, homelessness, incarceration in prisons, …). Third, people suffering from depression are often deprived of social contacts and support, and because of their inability to work they may sometimes lack status within their communities, none of which is likely to help their depressive mood. Fourth, and perhaps strikingly, 'people with mental health conditions often have their human rights violated' (WHO, 2011, p 2) through institutional abuse, which can take the form of unhygienic and inhumane living conditions, and of degrading and coercive practices on the part of clinicians and carers. This complex and seemingly ubiquitous issue – even in relation to the urban poor in industrialised countries – was described in more detail in Whitakers' (2002) much acclaimed *Mad in America*.

Given this particular worrying situation, WHO recommends different strategies to deal with this unfortunate state of affairs. The emphasis, according to the WHO, should be upon improving the provision of good-quality evidence-based mental health interventions. The guiding principles should be cost-effectiveness, affordability and feasibility. It seems that the Executive Board suggests that the associated costs might very well be significantly less important than the projected US$ 16 000 billion global impact of mental disorders in terms of lost economic output over the next 20 years. Indeed, WHO's Mental Health Gap Action Programme – launched in 2008 – mainly aims at '*providing care for all people with mental,*

neurological and substance use disorders, with specific attention to low- and middle-income countries' (WHO, 2011, p 4). Another priority seems to be to deinstitutionalize mental health care and to focus more on community-based mental health services. Finally, in this report it is stated that The Secretariat (of the Executive Board) needs to play a crucial role in expanding the evidence base on mental health interventions (management *and* prevention), with the aim of strengthening mental health care systems around the globe.

Two particular comments need to be made here. First, if closing 'the treatment gap' is the ultimate recommendation, then we want to make sure that the evidence based treatments (EBTs) that we propose, are indeed highly effective. In our third and fourth chapter we will examine this question by way of a critical analysis of a few meta-analytic studies and a critical assessment of the research literature concerning the effectiveness of psychotherapeutic and drug treatments for depressive disorders. As we will discover, this WHO recommendation might need to be re-evaluated. Second, and surprisingly, this official advice is inconsistent with a comprehensive WHO publication from 2009 that principally refers to a quite different gap.

3. Minding the Other Gap

As mentioned, in the 2011 report by the Secretariat of the Executive Board of the WHO, it is advocated that in the future, the main focus should be put on expanding the evidence base on mental health treatments and on efforts to try to close the treatment gap.

However, this recommendation seems inconsistent with an earlier WHO Europe report (2009), which observes that '*the presence or absence of mental health is above all a social indicator and therefore requires social, as well as individual solutions. Opportunities for individuals and communities to retain or achieve social recognition and to stay or become connected,*

contribute significantly to resilience, but social recognition and collective activity are frequent casualties of current economic and cultural trends.' (WHO, 2009, p 38). The recommendations in this WHO 2009 report are very different from those in the later document: the emphasis is upon the need to develop preventative policies and programmes to support improved mental health for the whole population. This publication highlights how the mental health of the population and of the individuals within it, are intimately linked to the extent of social and economic inequality. It suggests that this *inequality gap* is the one that should be closed, if we want to tackle the rising prevalence figures for depressive disorders.

Of course, these two gaps – the one of treatment and the other of social equality – are not mutually exclusive: we can focus on both simultaneously. However, if resources and/or political willingness to close one divide or the other are indeed limited, as the WHO (2011, 2012) suggests, then to which of these disparities should we attend first? The attempt to close the treatment gap aims at results in the short to medium term (possibly a few years), whereas the effort to bridge the inequality gap necessarily entails structural change, over the long term (possibly a few decades). Also, referring to Entman (1989) – amongst many others – the following question seems pivotal: which policymakers are willing to aim at the long term? Indeed, in order to develop a (widely supported) willingness to close the inequality gap, policymakers will have to be provided with solid economic and ethical arguments.

4. The World Mental Health Survey (2012) – A Global Public Health Priority

The World Mental Health Survey (WHO, 2012) further elaborates on the prevalence figures and indicates that depression affects 350 million people worldwide; that on average 1 in 20 people reported to have had a depressive episode in the previous year; and that depression is indeed the leading cause of disability worldwide in terms of total years lost due to disability.

The World Mental Health Survey (WHO, 2012) reconfirmed clearly that depressive disorders were ranked as the third leading cause of the global burden of disease in 2004 and will move into the first place by 2030. Consequently, it is fair to say that, today, depression should be considered and should be treated as an important global public health *priority*.

5. *The 2017 WHO report: 'Depression and Other Common Mental Disorders'*

In 2017, the WHO presented a new publication, entitled '*Depression and Other Common Mental Disorders*', in which it presented the latest available, more detailed and reliable global health estimates for – among other – depressive disorders. In the introduction, the report clearly states its viewpoint: 'Reliable, up-to-date estimates of the proportion of a general population affected by different diseases or health conditions is a critical ingredient of effective health policy, planning and evaluation' (WHO, 2017, p 6). In order to achieve this, WHO has worked with the lead GBD researchers for mental and behavioural disorders, based at the School of Public Health, University of Queensland, Australia. The data reflect syntheses of all available epidemiological literature on depression for the year 2015 – (including regional and country-specific estimates) – and confirm that lowered mood is the main mental health concern worldwide, affecting 4.4% of the global population in 2015, but with significant demographic variations. In most countries, for instance, women report depressive symptoms more often than do men (5.1% versus 3.6%). Although prevalence seems to peak in older adults (55-65 years of age), clinically significant despair can certainly occur in children and adolescents. The highest number of cases (in millions) were reported in the South-East Asia and Western Pacific Regions, respectively. The *highest prevalence* was found in Africa and the Americas. In 2015, a total number of 322 million people across the world were affected, with one third of them deemed to be suffering moderate to severe distress. For comparison, 14.1 million cancer cases

were reported in 2012 around the world (World Cancer Research Fund, 2018). In this, most recent report, the WHO suggests that the risk of being diagnosed with depression is increased by a range of adverse situations and experiences and their sequalae – including poverty, unemployment, social isolation, physical illness, alcohol and drug misuse, and mistreatment at the hands of others – a problem that particularly affects women and people from sexual minorities. In contrast to earlier reports, this more recent publication *does* find higher prevalence rates for depression in low- and middle-income countries (WHO, 2017). In chapter IV, we present a more detailed picture of the research literature which shows that the experience of personal distress is linked with virtually all indices of disempowerment and of low social rank, and with the occurrence of trauma and exploitation.

Losses in health and functioning due to depression are quantified at the population level by multiplying prevalence by the associated average level of disability, which results in estimates of Years Lived with Disability (YLD). As such, well over 50 million YLDs are associated with depressive disorders. Even in high-income countries, where the treatment gap is considered significantly less important (supra), total YLD for 2015 associated with depressive disorders, was estimated at 9 608 000. The highest numbers of total YLDs were reported for the South-East Asia Region and the Western Pacific Region, with 13 967 000 YLDs and 10 525 000 YLDs, respectively. It has to be said that these numbers also reflect the relatively larger populations of these two Regions (including China and India). It was reconfirmed that 'depressive disorders are ranked as the single largest contributor to non-fatal health loss' (7.5% of all YLD). (WHO, 2017, p 13).

Finally, as mentioned before in the first paragraph of this chapter, leading clinicians and theorists agree that acts of significant self-harm and suicide are consistently associated with the diagnosis of depression. Approximately 788 000 people across the world died due to suicide

in the year 2015, and ‘many more’ attempted suicide. Suicide is now considered to be in the top 20 leading causes of death worldwide, and the second leading cause of death among 15-29 year olds globally. No less than 78% of these suicides took place in low- and middle-income countries. However, suicide rates also peak among males in high-income countries with 20 or more per 100 000 population.

6. *Introducing ‘The Depression Pandemic’*

Since before the Second World War, the industrialised societies have increasingly embraced the notion that numbers speak for themselves. We are principally referring here to the intensification of bureaucracy in all sectors of the economy; and more specifically to the rise of the ‘new public management’ and of market driven ideas within the public health sector in which financial cost becomes the yardstick of everything (Dehue, 2008; Verhaeghe 2012; Moloney, 2013). While there surely is a lot that can be argued in favour of ‘a world ruled by accountants’, there are also compelling ethical or methodological arguments against this development (e.g. debatable ethical choices, measurement problems, …). However, we believe it is important to take these rising prevalence figures for depression at face value.

Where Dehue (2008) put forward the notion of ‘The Depression Epidemic’, we argue that today – 10 years after the introduction of this worrying concept – it is probably more accurate to speak of ‘The Depression Pandemic’; an epidemic (non-fatal for most, yet fatal for many) that has spread through large populations, across multiple continents, worldwide. As mentioned, for decades, prevalence figures for depression are rising in all WHO Regions *across* the globe; depressive disorders are the single largest contributor to non-fatal health loss or disability globally and – above all – depression is projected to be the leading contributor to the GBD by 2030. Moreover, the Depression Pandemic seems to spread across the globe at a fast rate: + 18

% worldwide between 2005-2015 (WHO, 2017). All of this seems to indicate that the academic and clinical communities are unable to counter these prevalence figures through a combination of effective prophylactic measures and effective treatments. In what follows in this first chapter, we define the premise on which this dissertation and the reasoning developed in it, is built.

7. The Depression Pandemic Conceals a Conundrum

Both the causes and treatment of depressive mood have been exhaustively researched for well over five decades, and mainly in the language of biomedicine and of clinical psychology, respectively. To read the scientific literature in these fields is to gain a strong impression that the Depression Pandemic is well on the way to being mastered.

For instance, clinical psychologists claim that their theories provide a solid understanding of the causal pathways that lead to depressive mood, and that their evidence-based psychological treatments can reliably treat it. While the field is still riven with debate about which approach might be the most effective for what kind of personal problem – a reluctant consensus has been emerging for a number of years now: very different psychotherapeutic approaches have similar benefits (Stiles, Shapiro & Elliot, 1986; Bergin & Garfield, 1994; Verhaeghe, 2005).

By contrast, psychiatrists have long held that depression is best understood as a brain disease rooted in an imbalance in the cerebral neurotransmitters, especially serotonin (Moncrieff, 2009; Healy, 2012; Goldacre, 2012). Accordingly, the medical treatment of low mood consists mainly of prescribing chemicals (anti-depressants) to restore the neurobiological status quo. Over the last forty years and more, countless studies appear to have confirmed the effectiveness of different pharmaceutical treatments for depression (Kirsch, 2009; Celie et al., 2018).

In sum, most of the academic community within the mental health treatment field appear to agree that they share a comprehensive understanding of the causal pathways that lead to depression *and* that they can recommend an armoury of proven EBTs with which to tackle it. Given the not insignificant availability of these remedies in the developed world, it would seem reasonable to expect that, in those countries with comprehensive health care systems, prevalence figures for depressive disorders would have been following a downward trend perhaps for many years. However, Jorm et al. (2017) recently confirmed that even though statistical modelling suggests that closing the treatment gap should reduce population prevalence, this is *not* the case in the real world. If anything, the tendency runs in the opposite direction, at least for all four of the countries studied by Jorm and colleagues: Australia, Canada, England and the United States (Jorm et al., 2017). For most countries that possess good quality public healthcare systems, the number of people diagnosed with depressive disorder has been climbing for the last twenty years (Horwitz & Wakefield, 2007; Moncrieff, 2009; Horwitz, 2015; Jorm et al., 2017; WHO, 2017).

These patterns do not support the 2011 WHO-recommendation that governments should prioritise the treatment rather than the prevention of depressive disorders. In chapter III, we will define this apparent contradiction between the rising prevalence figures of depression on the one hand, and the claimed mastery of this mental health condition on the other, as 'The Depression Conundrum' (Celie et al., 2017). Of course, there are different plausible explanations for this phenomenon (infra). Throughout this dissertation, we will focus on *one* of these. We will hypothesize that our scientific knowledge concerning depressive disorders might be flawed and, consequently, that we might be mismanaging this major public health issue. As we will discover, our scientific reality might indeed be – first and foremost – a *humanized* reality (Putnam, 1981).

The Depression Pandemic has *not* been kept under control via the combined use of the allegedly effective prophylactic and curative measures that currently exist. It is important, however, to put forward some reflections that help to put these rising prevalence figures into a broader perspective. If – for example – late modern subjectivity is depressogenic by nature, as van den Bergh (2018) and others would argue, then the rising prevalence could be considered largely self-explanatory.

8. Putting the Depression Pandemic into Perspective

We cannot elaborate on all critical perspectives that can be taken with regard to the worldwide rising prevalence figures for depressive mood. We choose to mainly focus on a few more recent and intertwined reflections that might help to contextualize the Depression Pandemic and to put the distressing numbers into a broader perspective in a logical and sensible manner.

8.1. Framing 'Signs of Despair' as an Illness

As we will develop in chapter IV, in the last thirty years, more and more people in Western societies have come to believe that self-realisation and lasting fulfilment are both normal and expectable. At the same time, they have become less tolerant of the experience of unhappiness and despair (Davies, 2015; Petersen, 2011). The pharmaceutical companies help to promote this outlook, through their adverts to consumers and through their sponsorship and patronage of psychiatric publications, conferences and institutions of learning – all of which promote the notion that distressed people can be fixed through chemical treatments (Dehue, 2008; Kirsch, 2009). Perhaps then it is no surprise that, over the years, clinicians and their patients have become more inclined to reach for a formal clinical diagnosis – such as depression, anxiety,

ADHD, or ASD – to name some of the most popular (Horwitz & Wakefield, 2007; May et al., 2004; Rogers & Pilgrim, 2015). But this is only one side of the coin.

There is an equally widespread narrative – found in the popular press, in 'responsible' TV programming, in 'reality' shows, and in an almost uncountable number of websites – which holds that a harsh life can lead to mental health problems and that these can nevertheless be eased or cured, via the application of psychotherapy (Kokanovic, Bendelow & Philip, 2013; Moloney, 2013; Rogers and Pilgrim, 2015). If people in Western countries (and beyond) have been only too willing to frame their troubles in the language of the mental health professionals, then there is a fundamental question to be asked: does the rise in the number of people diagnosed with disorders like 'depression' reflect a genuine increase in sadness and other forms of distress, or is it that too many of us are seeking a psychiatric label for what we might once have seen as an ordinary – albeit disturbing – state of mind? It is likely that both things are happening.

On the one hand, the material world has become a harsher place in which to live. Social inequality, environmental degradation, the intensification (and precariousness) of work, and the shrinking of the public sphere – all of these have beset most of Europe and the United States in the last few decades. Their reign has become stronger in those places hit the hardest by the 2008 financial crash, and by the neoliberal 'austerity' programmes that many national governments have been forced to implement in its wake (Stuckler & Bisu, 2013).

On the other hand, it seems clear that in their official institutions and in the unspoken norms that govern daily life – many industrialised societies are becoming increasingly prescriptive about the range of feelings, emotions and conduct considered *functional* and, therefore, normal and acceptable (Verhaeghe, 2002, 2012; May et al., 2004; Horwitz & Wakefield, 2007; Dehue, 2008, 2014; WHO, 2012; Moloney, 2013; Rogers & Pilgrim, 2015). The growing popularity

of psychiatric diagnosis, with its normalizing gaze (Foucault, 1963), can be seen as the inevitable accompaniment of this way of thinking. We will elaborate on this in the following chapters.

8.2. Social Materialist and Depressive Realist Versions of Psychology as Alternatives to Mainstream Approaches

If the dominant *psychotherapeutic* approaches view sadness and depression as either caused or maintained by the faulty thinking and conduct of the sufferer, then it is worth considering that this is not the only way to see things. There are other schools of thought that, by contrast, view personal demoralisation as an entirely understandable response to the inhumane worlds that so many of us inhabit.

Take for instance, the school of Social-Materialist Psychology (SMP), as formulated by the late British clinical psychologist, David Smail and colleagues (Smail, 1987, 2005; Midlands Psychology Group, 2012). SMP has grown from the confluence of clinical experience within public health services, and of those traditions within the mental health research literature that suggest how social inequality and the systematic abuse of power may underpin most of the distress encountered by therapists working in health settings that are free at the point of delivery – in other words, in services that cater for a less privileged clientele than the one upon which Freud and so many other psychotherapists built their patented schools of treatment. Indeed, SMP is a 'realist' approach, in the sense that it sees humans and their capacity for choice and action as exquisitely (and vulnerably) dependent upon both the embodied history of the individual and of the material and social world that they inhabit. Wellbeing and longevity flow, not from mysterious inner faculties like 'motivation' and 'insight', but from financial security

and social advantage, which themselves are often the gift of the distal environment – of geopolitical movements and decisions that happen far beyond the ambit of the daily lives of most of us (Smail, 2005; Wilkinson & Pickett, 2009). As far as the discipline of psychology is concerned, the approach is also a critical one. Science is viewed as a practice that readily serves political elites and corporate power. The resulting tendency toward distortion and corruption must be seen and resisted, if researchers and theorists are ever to approach the truth.

From the SMP viewpoint, the mental health treatment industries work in a manner entirely contrary to their preferred self-image. They school their willing clients into the belief (or delusion) that, with a little expert help, they can resolve their own personal problems through the judicious application of willpower and of psychological techniques (Smail, 1987; Epstein, 2006; Midlands Psychology Group, 2012). Swayed by a popular culture that encourages us to obsess about what is happening inside of us, few people can see that their troubled thoughts are far more a reflection of the troubled world around them, than of their own failures of motivation and insight. From a clinical standpoint, the lesson is clear. To gain a better understanding of our 'selves' and of our distress, we must learn to cultivate 'outsight' into our surroundings, into how they impose themselves upon us and then work their way under our skin (Smail, 2006; Midlands Psychology Group, 2012).

In its rejection of popular notions of willpower and 'the sovereign self', SMP shares some ground with the perspective known as Depressive Realism (DR) or 'anti-humanism', which is both a philosophical tradition and a school of clinical practice, informed by evolutionary psychology and the work of misanthropically inclined thinkers and historians, including Schopenhauer and Nietzche, and more recently, the British writer, John Gray (2002; 2013; 2015). On the mental health side, the so called 'depressive realism model' of unhappiness grew from research showing that, when compared with people who say that they feel happy and

adjusted – those who suffer low mood seem to possess a more realistic picture of their personal flaws and of their chances of overcoming them (Midlands Psychology Group, 2012; Moloney, 2013). These consistent results have raised doubts about the extent to which 'accurate' thinking is genuinely useful in daily life; even though many of the leading brands of therapy, especially the 'cognitive' ones – were founded upon the idea that the cultivation of 'realism' about ourselves was the path to good mental hygiene. While some practitioners of Cognitive Behavioural Therapy (CBT) – for example – have embraced the creation of positive illusions as the way to happiness, other kinds of talking therapy – such as existentialist and phenomenological approaches have followed the opposite course: in holding that an acceptance of the client's outlook, no matter how despairing it might be, is the only way for them to discover the self-acceptance and dignity required if they are to find a new way forward. There is no cure for the human condition. Existence is often tragic and arbitrary, and to expect it to be otherwise is to live a life of futile evasion (van Deurzen, 1998; Ghaemi, 2007; Feltham, 2016). These therapies are less popular than methods like CBT, for example, which claim to restore ailing minds to a state of 'rational optimism'. But in their fidelity to the experience of the client, the former have a strong affinity with the SMP outlook. We will further integrate these viewpoints in chapters IV and V.

8.3. Late Modern Subjectivity is Depressogenic by Nature

Finally, we introduce a recent dissertation called '*The Stolen Disorder – A Cultural Philosophical Interpretation of the Depression Epidemic'*; a dissertation that connects well on different levels with the earlier reflections we made in this section. In this thesis, van den Bergh (2018) focuses upon 'depression as an *experience*' from a phenomenological-hermeneutic perspective. Van den Bergh (2018) first argues that Western societies have moved beyond

‘modernity’ toward ‘late modernity’; the latter being characterized by an intensification of two key aspects of modernity: ‘dynamization’ and ‘individualization’. He further investigates whether the rising prevalence figures for depression could be considered ‘a sign of the problematic nature of late modern subjectivity’ (p 303). He concludes, among other things, that to be continually self-conscious and competitive is to invite personal insecurity and unhappiness into the midst of our lives. This conclusion fits well with our previous section on DR and SMP, yet van den Bergh took an entirely different road to come to these quite similar conclusions about the societal origins of depressive symptomatology. Essentially, van den Bergh (2018) concludes that depression is a social pathology, more specifically, ‘an elementary problem of *attunement* between individual and world, presubject and ambiance, at a primary, prereflexive bodily-affective level’ (p 308). The focus on embodiment suggests a similarity with David Smail's work (supra) and of phenomenological therapists such as Thomas Fuchs (2013), who has written on depression as an embodied experience – which is all about the feelings that are generated between people and within the body of the sufferer. Van den Bergh suggests that depressive symptoms might tell us more about the *late modern subject position*, then it is telling us about the individual presenting these symptoms.

If despair is routinely framed as an illness; if we mistake the symptoms of distress – our troubled thoughts and feelings – for the ultimate causes, which have far more to do with a damaging world then with anything that we do to our ourselves – then the dominant biomedical and psychological scientific narratives are leading all of us to the wrong questions and the wrong answers. Having briefly explained that there are distinct ways of interpreting the rising prevalence of depression; the question is probably not so much whether or not one agrees with – for example – DR viewpoints or the attunement theory of van den Bergh. From our perspective, the principal question is the following. Why – as a society – do we continue to cleave to the biomedical and individualist-psychological narratives when we try to understand

and treat depressive mood? Decades of medical and psychological inquiry do not seem to have altered the prevalence of this problem; quite the opposite. It is tempting to suggest that these scientific narratives are like mythical tales that, with enough repetition, have started to congeal into dogma, and thus blinding researchers and clinicians to fresh ideas and perspectives.

9. Conclusion

For over three decades, the prevalence figures for depression have been rising in all WHO regions across the globe. Depressive disorders are already the single largest contributor to non-fatal health loss or disability globally and are projected to be the leading contributor to the GBD by 2030. Today, depression is generally understood as an individual pathology and mainly researched through quantitative inquiry. Depression is framed as an illness, as a product of faulty brain chemicals, disordered thinking, or both.

We noted that these rising prevalence figures conceal a scientific conundrum. For decades, most theorists and clinical researchers have held that we possess a comprehensive understanding of the causal pathways that lead to the condition *and* effective EBTs with which to treat and prevent it. The reality of rising prevalence for the condition suggests that these experts are wrong. Consequently, the question arises whether we should not be focusing more upon trying to close the social inequality gap, rather than attempting to treat more and more people with drugs and talking therapy (closing the treatment gap). In the following chapters we will further discuss this predicament, by questioning different aspects of the dominant scientific views.

In the next chapter, we will first discuss the origins and the internal logic of the dominant neo-positivist scientific paradigm within biomedicine and the behavioural sciences. More

specifically, we will focus on how this scientific paradigm positions itself within – what we named – 'the four principal juxtapositions within philosophy and the sciences'. While many would perhaps argue that most has been said concerning these matters, we put forward the following: some important critiques are rarely pronounced; nearly all of these (rarely pronounced) critiques are easily ignored or dismissed and most of these critical reflections are rarely connected with one another. Once pronounced *and* connected, one comes to the conclusion that *interpretative charity* is a key aspect within the behavioural sciences.

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Chapter II

From Myth to Rational Procedure – Todays' Scientific Response to the Rising Prevalence of Depression

1. Investigating and Shaping Reality

In their intriguing and comprehensive manual of the history of Western philosophy, Vermeersch & Braeckman (2008) describe how, across the millennia, human beings – like many other species – have developed all kinds of strategies in order to sustain themselves. One of the most basic of these is the collection of information about the environment in which we live, and with the result that, all over the world, humankind has learned to adapt its conduct to its surroundings. From storytelling, myths, religions, and onward – to contemporary scientific inquiry: – our shared material and social reality has not just been explored and investigated, it has been actively *shaped* through these different ways of speech and thought (Störig, 1959; Kuhn, 1970; Feyerabend, 1975; Putnam, 1981; Verhaeghe, 2002; Vermeersch & Braeckman, 2008; Blomm, 2013; van den Bergh, 2018). For instance, the statement that 'the earth is flat and is located in the centre of the universe' (geocentrism) seems to have been rationally acceptable to most people 3000 years ago and, therefore, it was probably also regarded as a *true* statement in that epoch. In this context, the late Hilary Putnam (1981) argued that truth is considered an *idealization* of rational acceptability (infra). Different approaches shaped reality, knowledge and truth in as many different ways. Not only 'beauty' seems to be in the eye of the beholder.

Depressive mood and sadness are of all times and different cultures and different epochs

responded to these feelings very differently (Verhaeghe, 2002; Horwitz and Wakefield, 2007; Vermeersch & Braeckman, 2008; Dehue, 2008; Moloney, 2013; Rogers and Pilgrim, 2015; van den Bergh, 2018). In what follows, we will briefly consider the two principal interpretative thought frames of the pre-scientific period: myths and major world religions. In doing so, we wish to clarify that the very different mechanisms and strategies of the past millennia do share at least one thing with today's scientific procedure: a strikingly similar *societal functionality*. This functionality could be understood both as serving to hold societies together, lending meaning, stability and purpose to everyday interactions (Lévi-Strauss, 1992); *and* serving the purposes of the dominant social and political groups and institutions (Feyerabend, 1975).

2. Myths and Their Threefold Societal Functionality

Within primitive communities, false or erroneous interpretations of reality, subsequent wrong judgments and all kinds of natural phenomena led to important degrees of anxiety, uncertainty and sadness and, consequently, to the need to construct a coherent and understandable view of the world (Störig, 1959; Vermeersch & Braeckman, 2008). Campbell (1988) and Vermeersch & Braeckman (2008) suggest that uncertainty initiated progressively a need for understanding, cohesion, meaning and interpretative thought frames. These interpretative constructs were often quite stringent and usually entailed strong elements of prohibition and compulsion, as can be deduced – for example – from the code of conduct imposed by taboos, magical rituals, spells and forest stories within the ancient Vedic tradition of the Indian subcontinent, from around 1500 to 500 before the Common Era (Störig, 1959). For traditional hunger-gatherer societies and probably for all cultures before the rise of large states with their centralised religions (and later, with their schools of philosophy and science) – the world was understood and explained principally through shared *myths* (Campbell, 1988; Vermeersch & Braeckman,

2008). The mechanisms and strategies deployed within these myths incarnated a more overarching narrative, a more complex storyline then contemporary folk tales or forest stories. Myths were successful because they were emotionally satisfying (Vermeersch & Braeckman, 2008).

Myths, still according to Vermeersch & Braeckman (2008), originated from human fantasy and served three main purposes for the individual and for their culture. First, these stories brought order to the world, by categorizing all kinds of phenomena. Second, they offered an explanation for the more terrifying phenomena, such as death, suffering and diseases – helping people to feel that the universe might not be chaotic or malign after all, and that it might to some extent be controllable through magic and ritual. Finally, by allowing people to categorise and explain key phenomena, myths confer legitimacy and perhaps inevitability upon the prevailing social order. In these various ways, myths – especially the cosmological ones – had a more 'religious' character than contemporary folk tales and legends. The interpretative thought frames offered through myths promoted group cohesion and helped to reduce the amount of fear and uncertainty in people's lives (Störig, 1959; Campbell, 1988; Vermeersch & Braeckman, 2008).

In the light of all of this, and as far as the idea of depressive mood is concerned, the main thought we want to suggest here has two facets. First – that distress in the shape of anxiety, sadness, insecurity, helplessness and fear have been omnipresent throughout the history of the human species. Second and perhaps less obviously – in so-called 'primitive societies' – such feelings and interpretations were dealt with on a societal level through storytelling and myths, that derived from human fantasy but that were probably just as compelling for the members of a given culture as are the psychiatric and psychological scientific concepts that many of us use today.

3. Major World Religions and Their Dogmatic Universal Claims

From the first millennium before the Common Era, *major religions of revelation* arose within the larger cultural areas of the Middle East, India and China. These religions gradually developed into elaborate and systematized visions of humankind – encompassing its place in the universe, and its ultimate destiny (Störig, 1959; Vermeersch & Braeckman, 2008).

Essentially, all these major world religions strengthened the threefold societal functionality of the myths, mainly by revealing different kinds of so-called 'divine truths'. These eternal verities were revealed through the teaching of a divine messenger or a prophet, and were eventually written down in the form of holy books, including the Vedas (Hinduism), the Tripitaka (Buddhism), the Tanakh (Judaism), the Bible (Christianity) and the Koran (Islam). Their adherents claimed these documents to be of divine origin, but considered purely as myths – the latter represented a huge expansion in the scope and power of culturally sanctioned eschatology, in as much as they were said to be applicable not just to the members of a given tribe but to all of us (Störig, 1959; Campbell, 1988; Vermeersch & Braeckman, 2008). Indeed, this may have been the first time in the history of the human species that universal claims were made for matters of truth, knowledge and codes of conduct.

Moreover, for believers, the presumed divine origin of the writings in the holy books meant that their exalted contents were not open for interpretation, or discussion. These religious writings were dogmatic by nature, and while lending authority and ritual to the religious institutions – and thus helping to bring social stability – they also served the interests of the economic and political elites: often through seductive promises of rewards in the afterlife for those who toiled, but also by the outright coercion and punishment of those who refused to conform. As these major religions became more pervasive – permeating every aspect of each respective culture – they increasingly served to interdict the capacity for independent thought.

It was only in the 18th century CE – during the European Enlightenment period – that these oppressive religious systems begun to gradually lose ground (Störig, 1959; Vermeersch & Braeckman, 2008; Blomm, 2013). Rationality and scientific discovery could no longer be ignored, and thus the sciences started producing new mechanisms, leading to new strategies.

In sum, the interpretative thought frames offered through myths and world religions, resulted in the shaping of human reality in a fivefold *functional* manner. First, reality was ordered through categorizing all kinds of phenomena; second, these phenomena were explained by communicating meaning that was not directly expressed; third, through these processes, the prevailing social order was consolidated and legitimized; fourth, world religions introduced universal claims about reality and, finally, these claims were dogmatic by nature. That is, they were not open for debate. Since the 19th century, the standard historical works have suggested that, in the Western world, a culture of intellectual openness and debate was gradually, but radically introduced by the sciences – continuing into the present day (Störig, 1959; Vermeersch & Braeckman, 2008; Boone, 2011). But was it really?

4. New Interpretative Thought Frames in the Scientific Era

4.1. The Breakthrough of the Sciences

Today, the collection of information is mainly performed through scientific inquiry, more specifically through 'neo-positivist' or 'logical empirical' scientific inquiry, which could be understood as a deepened continuation of 19th century positivism. The direct origin of the neo-positivist paradigm is to be found in a discussion group in Vienna around 1925: the Wiener Kreis (Der Wiener Kreis, 1929; Störig, 1959; Vermeersch & Braeckman, 2008). By this point in time, the sciences had already delivered a vast amount of knowledge about the physical universe, which enabled academics within this circle to create rules for what could count as

scientific evidence and what could not; and to critically and accurately analyse scientific reasoning and scientific methodology, the aim being to amplify and expand human knowledge. They argued that the only valid human knowledge is based upon either logico-mathematical reasoning *or* empirical (observable) evidence (data). Some of the main original pioneers of (neo)positivist thinking were the influential 17th-18th century continental rationalist thinkers like Descartes (1596-1650), Spinoza (1632-1677) and Leibniz (1646-1716); and the 17th-18th century English *empirical* thinkers such as Locke (1632-1704), Berkeley (1684-1754), and finally, Hume (1711-1776) – who advised that any form of knowledge that could not be codified in this way should be committed 'to the flames, for it can contain nothing but sophistry and illusion.' (cited in Vermeersch & Braeckman, 2008, p 298).

In Western Europe, eighteenth century rationalism owed much to the salons where intellectuals gathered to deliberate and debate upon all kinds of philosophical and scientific matters. One of the most influential of these establishments, according Blomm (2013), was the Parisian mansion house of Baron Paul-Henri Thiry d'Holbach; where a group of 'radical thinkers of the Enlightenment' assembled twice a week. Its patrons included Diderot, d'Alembert, Rousseau, Helvétius; and Hume and Adam Smith, amongst many others. Indeed, their ideas resonate in the present time, and many of the 19th century born thinkers whose work continues to shape our outlook and technology in the 21st century – including Gottlieb Frege, Bertand Russel, Ludwig Wittgenstein and Albert Einstein – could be considered as important heirs to this positivist tradition (Vermeersch & Braeckman, 2008).

4.2. The Institutionalization of the Neo-positivist Rational Procedure

Over the past two centuries, mainly due to the great successes of neo-positivist reasoning and methodology within the natural sciences, at an ever increasing pace and in an ever more

sophisticated way, scientific discourse became institutionalized as the global dominant interpretative thought frame for any area of human activity (Störig, 1959; Feyerabend, 1975; Putnam, 1981; Vermeersch & Braeckman, 2008). Consequently, today, scientific discourse, research and analysis are considered paramount within the natural *and* behavioural sciences.

Foucault (1971) points out that any dominant discourse – once it is institutionalized – exerts considerable *external control* over our thinking and conduct, via procedures and rituals that serve to co-opt, constrain or suppress any expression of opposition, or dissent. For Foucault (1971) this means that some things can be said or done and that other things cannot be said or done ('la parole interdite'). Moreover, he argues that it is the dominant discourse that determines to a large extent – through and controlled by its many institutions – what can be considered as true and what considered as false – and this sensibility works its way into our very being. He argues that these external controlling mechanisms foster a largely unconscious or tacit internalisation or incorporation of what can and cannot be said or done, of what is presumed to be true and what is not. In this way, the dominant discourse always results in *internal control*.

'Et l'institution répond: "Tu n'as pas à craindre de commencer; nous sommes tous là pour te montrer que le discours est dans l'ordre des lois; qu'on veille depuis longtemps sur son apparition; qu'une place lui a été faite, qui l'honore mais le désarme; et que, s'il lui arrive d'avoir quelque pouvoir, c'est bien de nous, et de nous seulement, qu'il le tient." (Foucault, 1971, p 9).

In the context of the phenomenon of 'depression' – it is mainly through these institutions, immersed as they are in neo-positivist scientific reasoning that all official attempts to understand and deal with the Depression Pandemic take place. Thus, prevalence is estimated, correlations between certain variables or the effectiveness of treatments are measured,

compounded judgments about the nature of the problem are deduced, EBTs and effective prophylactic measures are proposed. Consequently, over the past decades, the reality around depressive mood and the Depression Pandemic has indeed been shaped in a specific manner. That is, for many lay people and for the vast majority of mental health professionals, it seems unthinkable to reflect upon this pandemic *beyond* medical and psychological scientific discourse. This being the case, we must be sure that this powerful set of accounts and practices amounts to the most accurate and helpful way to classify and treat 'depression'. Indeed, there are two very different ways to consider this issue.

On the one hand, it is predominantly claimed by many scientists (psychiatrists, psychologists, expert-statisticians…) that this institutionalized neo-positivist inquiry brought considerable progress to the fields of psychiatry and clinical psychology. For decades and into the present era, psychiatric and clinical psychological journals have appeared to celebrate the scientific approach of the Depression Pandemic. For example, meta-analytic research on treatment effectiveness is widely regarded as a well-established and highly reliable method for the creation of sound guidelines for clinical practice and for government health policy (Mann, 1994; Hunt, 1997; Rosenthal & DiMatteo, 2001; Cooper, Hedges, & Valentine, 2009; Cordray & Morphy, 2009; Greenhouse & Iyengar, 2009).

On the other hand, in this dissertation, we will argue that we must take a critical stand toward the different institutions that embody neo-positivist reasoning when dealing with depressive disorders. In chapter III, we will question neo-positivism from *within* the boundaries of the paradigm itself. In chapter IV, we will question the dominant scientific institutions from a broader perspective by allowing critical reasoning from *outside* the dominant paradigm. Essentially, we will argue that a crucial problem might be the following: today, reason and method are seen as *one* entity, not as two different human activities (Feyerabend, 1975).

4.3. Against Method

In 1975, Austrian physicist and philosopher of science Paul Feyerabend wrote his acclaimed, yet controversial book *Against Method*. There are good reasons why, a few decades later, Feyerabend is framed as the 'enfant terrible' of the philosophy of science. His unconventional ideas, and – probably more importantly – his provocative style still shock, embarrass and annoy many scientists; perhaps above all because it threatens the dominant paradigm and its institutions. Feyerabend goes further than Foucault (1971) in his critical analysis of scientific discourse, and in doing so, he makes a plea for metaphysical and methodical pluralism.

Despite the overwhelming social authority and influence of the sciences, they are nonetheless built, according to Feyerabend, on poor reasoning. He observes that, ever since the end of the 19th and the beginning of the 20th century, a pervasive belief grew in people's minds that the sciences embodied a *coherent* interpretative thought frame and had brought extremely high degrees of 'regularity and stability' within Western societies. Feyerabend (1975) argues that this is strange, because the sciences mainly produce new *ir*regularities, new questions and new problems due to conflicts between expectations and perceptions. The latter was already demonstrated by Popper (1934). Some of these conflicts are solved, but most are not. According Feyerabend, these unresolved problems or irregularities are mainly 'forgotten', or rather, they are ignored or dismissed in order to maintain the ruling scientific paradigm or the status quo. Consequently, Feyerabend argues – contrary to what is often suggested – that the neo-positivist paradigm offers a far from coherent thought scheme.

The work of Feyerabend and of other critical philosophers of science of the 20th century, as discussed in the next section, will enable us to better understand the actual ideological and scientific positions that are taken today within the fields of psychiatry and clinical psychology.

4.4. The Neo-Positivist Rational Procedure: Three Sobering Criticisms.

Scientific Claims and Interpretative Charity

First, there is the issue of the way in which scientific claims enter the public domain. With the significant rise of scientific discourse ever since the 17th - 18th century – the different scientific disciplines started making, at an ever increasing pace, universal claims about reality. Furthermore, these new scientific interpretative thoughts frames and their generalized inferences could be often considered as dogmatic; producing conclusions, deductions or principles that were (and still are) presented as incontrovertibly true (Kuhn, 1970; Foucault, 1971; Feyerabend, 1975; Putnam, 1981). Today, we have arrived in the age of *scientism*; we seem to have adopted a belief in the scientific method as the pre-eminent source of genuine factual knowledge and as the most authoritative worldview to the exclusion of other viewpoints (Sorell, 1994; Verhaeghe, 2012; Dehue , 2014).

For example, today, Darwin's theory of evolution is broadly accepted within the scientific community and the rejection of evolution is seen as nonsensical and based on absurd reasoning. Nevertheless and strikingly perhaps, Wittgenstein (1967) – not the least among the philosophers of science of the past century – contrasts Darwin's theory *un*favourably with theories in physics: 'In the end you forget entirely every question of verification, you are just sure it must have been like that… Did anyone see this process happening? No. Has anyone seen it happening now? No. The evidence of breeding is just a drop in the bucket'.

Feyerabend (1975) and Putnam (1981) reject the idea that the sciences provide the only true descriptions of reality. Moreover, they explicitly reject the equation of scientific thinking with rational thinking. Within this context, Kuhn (1970) further clarifies that scientific reasoning always functions from *within* the paradigm, meaning that it is the dominant paradigm that

determines what is seen as pertinent and what is not. Many aspects of reality or viewpoints are excluded before reasoning even starts. In order to maintain the dominant scientific discourse, theory, viewpoint, approach; scientists often disregard or reject its' inconsistencies and show large degrees of '*interpretative charity*' (Putnam, 1981). In chapter III, we will give a striking example of this when we review the effectiveness of the psychotherapeutic treatment of depression and argue that – because of charitable interpretation in relation to methodological rigour and outcome – we came to accept the idea of medium to high effectiveness of this treatment; even though it could be argued that the latter might be 'a scientific delusion'.

Scientific claims or views on reality seem to be always fallacious to some extent. From this perspective, nothing is clearly established beyond dispute and scientific findings are always indebted to the prevailing discourse; if we want the 'empirical content of our knowledge' to *grow*, scientific debate and scientific pluralism seem indeed essential (Feyerabend, 1975).

Objectivity is not an Unambiguous Concept

Second, there is the concomitant issue of *objectivity*. Scientific systematic inquiry always attempts to embody two fundamental requirements: an unambiguous formulation of the theory and a strict control of the postulated theory by the facts (Vermeersch & Braeckman, 2008). It is broadly argued that the latter is based on objective observation (inputs); and a subsequent attempt to understand reality through logical classification and comparative reasoning. In other words, the very foundation of scientific inquiry (strict control by the facts) is considered to be 'an objective mental activity', not influenced by personal feelings or opinions (Putnam, 1981). However, it could be argued that the very inputs upon which neo-positivist knowledge is based are conceptually contaminated by a host of 'conceptual choices' prior to reasoning (Kuhn, 1970; Feyerabend, 1975; Putnam, 1981; Verhaeghe, 2002). Putnam (1981) concludes that

contaminated inputs are better than none, but argues that this entire process must lead to a coherent linguistic and symbolic narrative (knowledge) about the object of study. *If* our theoretical formulations and strict control by the facts, result in such a *coherent conceptual scheme*, then we have a rationally acceptable and workable narrative about the object of study (Putnam, 1981; Vermeersch & Braeckman, 2008). If not, then we have a problem.

As indicated, Kuhn (1970), Foucault (1971), Feyerabend (1975), Putnam (1981) or Verhaeghe (2002) argue that the inputs – the basis of our knowledge – are shaped by the dominant social and scientific discourse; by the choices that we make, and by the language and symbols we use to report and describe reality. Our descriptions, even our descriptions of our sensations are 'heavily affected (as are the sensations themselves, for that matter) by a host of conceptual choices' (Putnam, 1981, p 54). Since 'the very inputs upon which our knowledge is based are conceptually contaminated' (Putnam, 1981, p 54); the notion 'objective knowledge' is *not* an unambiguous concept.

Most scientists would rightfully argue that neo-positivist inquiry led to an extremely coherent conceptual scheme within the natural sciences, allowing us to better understand the laws of physics, our universe, our species; leading to remarkable progress in many different fields over the past century. These observations however prompt an obvious but important question, which runs, like a thread, throughout this dissertation: – did neo-positivist inquiry offer the same kind of coherent conceptual scheme within the behavioural sciences; more specifically in the way we deal with depressive mood? We will further argue that this is not the case and that we *do* in fact seem to have a problem. Referring to the central thesis of Feyerabend (1975): we might need alternatives for the viewpoints that are at the centre of the depression discussion.

The Birth of the 'Scientific Specialist'

Vermeersch & Braeckman (2008) have little doubt that the Enlightenment period can be considered as that point in history where our knowledge of the natural world and our ability to systematise and manipulate it through mathematics and technology began to grow rapidly. However, as different scientific disciplines emerged in the 19th century and then onwards into the present time, simultaneously, each individual scientist came to peer at their subject matter through an ever narrower window of expertise (Vermeersch and Braeckman, 2008). The third strand of this critique concerns the origins of the 'scientific specialist' in the Western world.

It is not hard to find examples of this issue in the sphere of mental health treatment. For example, we will later argue that the clinical label 'depression' is of doubtful diagnostic worth (chapter IV). But since the experts in effectiveness research on psychotherapy are not psycho-diagnosticians, then they are often unaware of the extent to which the results of Randomized Controlled Trials (RCTs) are compromised by the fact that the people being treated might not share the same kind of clinical problem, and might not be representative of those people who are regarded as 'depressed', within the wider population.

In what follows, we will attempt to better define what it means today to be a researcher in the fields of psychiatry and clinical psychology. This will allow us to better answer the following question: how do researchers try to grasp the reality of the depressed?

4.5. The Four Underlying Juxtapositions Within the Philosophy of Sciences

In an attempt to try to better understand todays' psychiatric and clinical psychological scientific activity, we choose to, first, identify four principal *juxtapositions* within the philosophy of sciences; and, second, to determine the actual ideological and methodological positions that

are taken today within the field of psychiatry and clinical psychology. This section will allow us to introduce more easily – at the end of this chapter – '*The Principle of Tolerance 2.0*'.

We conceptualize a juxtaposition as follows: placing side by side two contrasting principles or concepts with the aim of comparing these notions with each other. There is no right or wrong side within a juxtaposition, but there is a distinct difference in meaning. Accordingly, the consequences of the position taken within the juxtaposition, or the consequences of these different approaches or viewpoints; are enormous. These principal juxtapositions are all intensely interconnected and essential to our narrative.

4.5.1. Juxtaposition I : Knowledge vs. Truth

This first juxtaposition 'Knowledge vs. Truth' probably deserves most attention. It lies far beyond the scope of this dissertation to summarize all of the philosophical literature that has dealt with this colossal issue. Essentially, we want to make two observations. First, not everything we know should be considered true, and, not everything that we consider true is based upon solid knowledge. Second, knowledge and truth, in a much broader sense, are each related to two entirely different domains of the human experience: scientific research on the one hand, and ethical inquiry on the other. Where knowledge seems accessible to us, truth seems inconceivable by us.

As mentioned, it is often posited that scientific inquiry that leads to replicable empirical findings, results in rationally acceptable knowledge, and in the end, in *true facts* about reality. Therefore, the veracity of these 'scientific facts' is rarely questioned. As scientific knowledge grows, truth changes. For example, as mentioned, the statement 'The earth is flat and is located in the centre of the universe' (geocentrism) was rationally acceptable 3000 years ago and was therefore probably also considered a true statement at that epoch. The statement 'The earth is

an oblate spheroid and revolves around the sun at the centre of the solar system' (heliocentrism) is a rationally acceptable statement today and therefore considered a true statement, today. As indicated in the introduction to this chapter – according to this line of thought, truth is regarded as an idealization of rational acceptability, and will therefore very much depend upon time and place (Putnam, 1981). From this viewpoint, knowledge and truth seem to be nearly the same, or at least they are closely interconnected. Because truth changes as knowledge grows, knowledge and truth evolve almost simultaneously. So where is the juxtaposition or contrast between knowledge and truth?

From our point of view, the juxtaposition 'Knowledge versus Truth' only becomes perceptible in a twofold manner if we first make a sharp distinction between the natural sciences, on the one hand, and the behavioural sciences (or humanities), on the other. The twofold manner can be resumed as follows: one contrast between knowledge and truth can be made from *within* the dominant scientific paradigm, the other contrast refers to scientific pluralism (plurality of standpoints and epistemologies) and allows critical reasoning from *outside* the dominant paradigm. The first contrast is relatively easy to introduce, the second is more complicated. Both contrasts refer to the notion of 'conceptual choices'.

To clarify the first contrast, and referring to the subject matter of this dissertation – we consider the following scientific proposition: '*The diagnostic category of depression is a coherent category'*. Defenders of the checklist-based nosologies, like the DSM – will argue that this proposition represents something we know, founded upon a myriad of empirical findings. For them, this assertion should be considered a true fact. However, as indicated before and as we will substantiate in detail in chapter IV, many critical academics would argue on methodological grounds only – that is, from within the dominant paradigm or approach – that the diagnostic category of depression lacks reliability and validity and, therefore, cannot be

considered a coherent category (Epstein, 2006; Moloney, 2013; Vanheule, 2015; Celie, Moloney & Verhaeghe, 2018). Consequently, the aforementioned piece of knowledge 'The diagnostic category of depression is a coherent category' is not 'true for everyone'. Truth here depends largely on conceptual choices scientists make (e.g. in this case, methodological rigour), and thus, according Putnam (1981) and Verhaeghe (2002), truth here becomes a matter of *ethics*. The argument that truth based on scientific knowledge depends largely on choices that are made prior to reasoning and on the language and the symbols that are used to describe reality, – was carefully developed in Putnam's work, '*Reason, Truth and History'* (1981).

The aforementioned statement based on replicable empirical research, actually results in a scientific proposition that can be true and false at the same time. In this sense, truth is indeed in the eye of the beholder. For this reason alone, it seems that we must distinguish or juxtapose the notions knowledge and truth, because the consequences of the idea that a scientific proposition can be 'true and false at the same time' cannot be overestimated. For example, what do the rising prevalence figures for depression really mean if the diagnostic category is incoherent? Moreover, as mentioned before, if the diagnostic label 'depression' is a moot one, then, logically, those RCTs that seek to examine the effectiveness of a given treatment for depression will be flawed right from the beginning. The accuracy of the diagnosis is crucial for any RCT inquiry into the effectiveness of a given treatment.

The second contrast of 'Knowledge versus Truth' is of a more encompassing nature, beyond the dominant scientific paradigm. Essentially, this contrast will further and more radically locate knowledge in the field of science and truth in the field of ethics. In doing so, we conceptualize these notions as mutually exclusive. In the field of psychiatry and clinical psychology, for example, investigators are often looking for knowledgeable answers to underlying questions that are of an ethical nature, yet these issues are posed in the supposedly

neutral and value free language of empirical research (Verhaeghe, 2002; Dehue, 2008; Moloney, 2013). Of course, these scientific answers can be helpful or indicative, but, as Verhaeghe (2002) observes, the inadequacy of these knowledgeable scientific answers to ethical questions of all sorts, is structurally determined. That is, these answers can never grasp the complexity of the underlying ethical nature of the question. We give two examples.

Underneath the diagnostic question 'Am I depressed?', lie questions of a clear ethical nature: 'What conventions did I not grasp?', 'Am I responsible for my condition?', 'Am I to blame for my condition?' or 'Am I a good father, mother, partner?'. Beyond the question 'Is (individually delivered) psychotherapy effective when treating depressive disorders?' lie important ethical questions: 'Why do we consider depression as an individual pathology?'; or 'Did the psychotherapeutic treatment of depression direct too much attention away from the world that gave rise to this mental condition?'; or 'What is the function of a particular depressive symptom within a patient's subjective internal logic?'. These underlying questions reflect distinct individual and/or societal choices and all these decisions reflect ethical values that are often tacit (Verhaeghe, 2002). Likewise, the answers to these questions should also preferably reflect pluralistic ethical and scientific options and/or positions. This is precisely what Feyerabend (1975) refers to in his often misinterpreted slogan – *'Anything goes'* – which in fact represents his endorsement of methodological and metaphysical pluralism in science, respectively. According Feyerabend it is unwise to *a priori* consider the rational procedure (the dominant scientific paradigm) as the only valid explanatory method; that is, independently of the nature of the situation, the problem that needs to be addressed or the specific events that happened. This seems even more so when dealing with 'the bigger picture issues'; such as underlying ethical questions. The failure of such a 'a priori position' is inherent to the position itself, according Feyerabend (1975).

In summary, when Verhaeghe (2002) talks about 'the typical collision which applies to the total field of the Symbolic', he refers to this collision between (contaminated) knowledge, on the one hand, and (subjective) truth, on the other. When a question is of an ethical nature, scientific knowledge can be helpful, but it can only produce partial answers. According Verhaeghe (2002), scientific knowledge is thought to have mainly four distinct characteristics: knowledge is cumulative, generalizable, transmittable; the ultimate aim of the accumulated knowledge being completeness. In contrast, Verhaeghe (2002) argues that truth is not cumulative nor generalizable and because its incompleteness is structurally determined, truth is transmitted differently than knowledge.

Many questions within the behavioural sciences are considered as 'questions of the order of the ethical'. These questions concern *choices* and the scientific answers that are given can never result in a rationally acceptable and coherent conceptual scheme that applies to all. Based upon his reading of Kant, Putnam (1981) argues that … 'A piece of knowledge (i.e. 'a true statement') is a statement that a rational being would accept on sufficient experience of the kind that it is actually possible for beings with our nature to have. 'Truth' in any other sense is inaccessible to us and inconceivable by us. Truth is ultimate goodness of fit.' (p. 64).

4.5.2. Juxtaposition II : Realism vs. Nominalism

This second juxtaposition to be explored here perhaps needs less explanation, since the contrast between these different viewpoints is easily grasped and understood, as two very distinct conceptions of rationality. Consequently, these viewpoints resulted in radically different scientific projects. The juxtaposition of 'Realism vs. Nominalism' principally refers to the following question: What is the relationship or correspondence between 'words' (or 'symbols') and 'things'? More specifically, what is the relationship or correspondence between 'scientific

denomination' and 'the object of study', between subject and object? How do words (language) and scientific symbols get into a unique correspondence with objects and sets of objects? These questions have been at the core of scientific debate for centuries and gave rise to these two distinct philosophical perspectives (Störig, 1959; Vermeersch & Braeckman, 2008). As we will clarify, pushed to their extremes, both these perspectives become *self-refuting* (Putnam, 1981).

Within the perspective of metaphysical *realism* ('Copy theory of truth') the world consists of 'some fixed totality of *mind-independent* objects'; and 'truth involves some sort of correspondence relation between words or thought-signs and external things or sets of things' (Putnam, 1981, p 49). Realism is strongly related to the idealism of Plato and Hegel, and begins with the belief that ultimately, we can entirely describe reality and all of its mind-independent objects through one true system of scientific concepts (Störig, 1959; Putnam, 1981; Verhaeghe, 2002; Vermeersch & Braeckman, 2008). Realists argue that 'there is exactly one true and complete description of 'the way the world is' (Putnam, 1981, p 49). Moreover, within this line of thought, it is suggested that there is a fixed natural order of things (Verhaeghe, 2002). Putnam (1981) calls this perspective an *externalist* one, 'because it's favourite point of view is a God's Eye point of view' (p 49). Verhaeghe (2002) notes that within the sciences today, this perspective is probably universal – so much so that it is scarcely noticed, and thus constitutes a tacit assumption for most thinkers and researchers.

Without doubt, this guiding realist perspective led to important scientific progress, however, according Putnam (1981) and Verhaeghe (2002) it also led to an oversimplification of reality or a reductionist sense of reality. For example, in the previous chapter we introduced the notion of the Depression Pandemic, based on a multitude of recent epidemiological studies. These studies are founded upon the realist assumption that there is a unique correspondence between the diagnostic label and the depressed individual, between the prevalence numbers as revealed

by the research and the actual number of people who are struggling with mental health problems in the world at large. Putting the Depression Pandemic into perspective (chapter I), we have learned that this supposedly unique correspondence could be regarded indeed as an oversimplification of reality. That is, there are other distinct ways of looking at this issue.

Putnam (1981) argues that realism led to 'the conception according to which a statement is true in case it corresponds to (mind independent) facts' (Preface, IX), and when it coincides with objectively measured empirical evidence; the latter being the essence of the (neo-)positivist paradigm. Precisely *this* conception produced a priori philosophies which leave no room for a rational activity of philosophy. Putnam (1981) considers that this is why extreme realist views on reality are self-refuting.

Within the perspective of metaphysical *nominalism* ('Coherence theory of truth', Pluralism, Pragmatism) it is argued that the question 'What objects does the world consist of?', only makes sense to ask from within a theory of description (Putnam, 1981). The nominalist view or *internalist* perspective on reality holds that there is more than one true description of the world; that there are various points of view reflecting various interests and purposes; and that objects do not exist independently from conceptual schemes (Putnam, 1981). According this view, *every* description of reality is heavily affected by a multitude of conceptual choices and therefor, every description of reality is contaminated. Systems of thoughts, larger societal narratives, elites within societies and the self-serving ideologies that they can sometimes promote: all of these can heavily influence how we think about and describe reality. Indeed, all of these influence our scientific descriptions as well (Kuhn, 1970; Foucault, 1971; Feyerabend, 1975; Putnam, 1981). The latter partially explains why new ideas, as time goes by, always become old ideas. However, if this nominalist perspective is treated as the only way to understand things, then there is a danger that scientific and philosophical reasoning can descend

into an extreme form of relativism or what Feyerabend called ‘scientific anarchism’ (Feyerabend, 1975). To whit: if *all* is relative – then the relative is relative, too. This is why – pushed to its extreme – the nominalist perspective is also self-refuting (Putnam, 1981).

Verhaeghe (2002) concludes that the relationship between ‘subject and object’ is probably best characterised by the notion of ‘inability’ or ‘incompetence’. Even though such conclusion will allow us more easily to express criticism on the dominant scientific approach, this same conclusion should be considered as an indirect plea for humility when we express our own alternative views in chapter V.

4.5.3. Juxtaposition III : Deductive vs. Inductive Sciences

Based upon the methodological approach, Vermeersch & Braeckman (2008) map the sciences into two main domains. On the one hand, there are the formal or deductive sciences: logic and mathematics. The deductive sciences teach us little about the world around us as such, yet they offer forms and symbolic systems which we use when trying to represent reality (Vermeersch & Braeckman, 2008). On the other hand, there are the empirical or inductive sciences, such as the behavioural sciences, the natural sciences and cultural studies; where generalizations are made on the basis of observations made under well-defined or controlled circumstances – which may include the experimental method.

Within the inductive sciences, it is assumed that reliable and valid knowledge about reality is genuinely attainable through the senses, and with or without the use of specialised observational equipment; and so the rational procedure within the inductive sciences begins with empirical data (Vermeersch & Braeckman, 2008). Encouraged by the great successes of inductive reasoning within the natural sciences, most scientists have come to agree that the idea that the representations, inferences and convictions that flow from inductive scientific research

must correspond closely to the world as it actually exists, or with 'reality as such' (Störig, 1959; Putnam, 1981; Verhaeghe, 2002; Vermeersch & Braeckman, 2008).

In fact, this third juxtaposition seems like an unnecessary contrast to make; as mentioned, the behavioural sciences are 'by definition' considered inductive. While the latter is correct, it is often disregarded that *all* statistical models used within the behavioural sciences find their origins in logic and mathematics; in the deductive sciences. We will have more to say about this issue in the context of our discussion of the meta-analytic research concerning the treatment of depressive disorders in chapter III.

Indeed, meta-analysis is a sophisticated method, mathematically speaking, and in this respect it is emblematic of the way that most scientific researchers in the early 21st century will approach their subject matter. Cooper & Hedges (2009) note that theorists come to theorems through logical deduction according postulates, axioms and predefined rules and eventually develop all kinds of 'theoretical blueprints' (e.g. any statistical model). On the other hand, researchers provide the empirical data, which Cooper & Hedges (2009) call 'the bricks'. They point out that 'our data-bricks are not all six-sided and right angled'. While this could be seen as inherent to the experimental method, what follows in Cooper & Hedges' disquisition, sounds worrying:

'They (data-bricks) come in a baffling array of sizes and shapes. Making them fit, securing them with mortar, and seeing whether the resulting construction looks anything like the blueprint is a challenge worthy of the most dedicated, inspired artisan' (Introduction, p 4).

Why should the resulting construction – within research synthesis in the field of clinical psychology for example – look anything like the theoretical blueprint? Why should empirically gathered data resemble or fit a predefined theoretical scheme or plan? Should this argument not be reversed? That is, since 'data-bricks come in a baffling array of sizes and shapes', the

resulting construction – almost self-evidently – will *always* look strange, different or quite unusual (i.e. nothing like the blueprint).

The statistical models that are used within psychological or psychiatric research today, are a result of deductive reasoning which always rests upon a set of *background* assumptions (or axioms). Consequently, the validity of any conclusion drawn from a statistical inference ultimately depends on the validity of the assumptions that were made prior to the deductive rational procedure (Cooper, Hedges & Valentine, 2009). There is not enough space here to analyse these background assumptions in detail. However, referring to our subject matter, we believe it is important to recall that our notions of how to measure depressive disorder and its treatment derive from a long chain of deductive reasoning; formed out of postulates, axioms and predefined rules. As such, the reality of depressive mood is shaped in a particular way. Researchers must ensure that their gathered empirical data fit theoretical blueprints, meaning that the data need to be massaged to adapt them to the algorithms (Cooper & Hedges, 2009). Data massage is often associated with the practice of 'cherry-picking', of selectively excluding or altering data, based on what researchers wish or don't wish it to reflect. While this unfortunately happens (see chapters III and IV), we are also addressing here the issue of data massaging techniques that are required to adapt the data to the blueprint the researchers are working with through stripping unwanted characters and whitespace, converting number and date values into desired formats, organising data into a meaningful structure and so on. The aim of these procedures is to find a story in the data that could raise more hypotheses and, consequently, more interesting statistical analysis. Assuming that the data *do* tell us something valuable about reality – a key question is whether or not this data massaging changes the message that is communicated to the audience? Moreover, as we will discuss in chapter III, some of the key assumptions of the meta-analytic technique for example – assumptions made to be able to fit the data into the theoretical blueprint – are highly debatable or worse: 'esoteric

as to be unfathomable and hence immune for rational consideration' (Berk & Freedman, 2001, p 13). Finally, referring to our subject matter, the depressed seem to be little involved in todays' scientific inductive endeavour. They produce 'data', these data are then 'massaged' in what might be termed a scientific wellness centre, to be able to fit them to the a priori determined models of the scientific fitness centre.

Furthermore, every particular scientific field within the inductive sciences aims to group a set of statements, laws, methodological requirements and theories concerning a more or less coherent problem area. However, as Vermeersch & Braeckman (2008) suggest, the veracity of inductive reasoning can never have the same status as its counterpart. Furthermore, the experimental method comes ideally with a number of stringent methodological requirements if indeed researchers want to be able to make reliable and valid generalized inferences. Within the behavioural sciences, this ideal is always difficult to achieve and the obtained scientific results therefore often need – as mentioned – interpretative charity (Putnam, 1981).

Finally, most expert-statisticians we encountered conclude that the statistical models that are used within psychiatric and psychological research should be considered as approximations of reality that allow us to *communicate* about reality. In other words, they are never definitive, and some humility is required on the part of the researchers and scientists who appeal to them in their work. Moreover, referring to our first chapter, the following question could be raised here: what exactly do we wish to communicate about the Depression Pandemic? For example, do we principally want to communicate 'knowledge about the treatment gap' or 'knowledge about the inequality gap'?

4.5.4. Juxtaposition IV : Positivism vs.. Relativism

Because our juxtapositions are all strongly interconnected, we already interpreted to some

extent the meaning of the signifiers 'positivism' and 'relativism'. These different approaches should be considered as two radically opposing conceptions of reality (Störig, 1959; Putnam, 1981; Vermeersch & Braeckman, 2008). We introduce these two notions as a fourth juxtaposition, mainly to highlight one forgotten or carefully hidden, significant conception: *'The Principle of Tolerance'* (Carnap, 1928).

Relativism has a long history and goes back to the ancient Greeks (Vermeersch & Braeckman, 2008). Essentially, relativism starts with the premise that 'absolute knowledge' or 'absolute moral norms' are utopian ideas and that knowledge, norms and truth – rather than being immutable, will always depend upon time and place (Feyerabend, 1975; Putnam, 1981; Verhaeghe, 2002; Vermeersch & Braeckman, 2008). On this view, all that the sciences can offer are indeed 'constructions' or 'approximations of reality' and the ultimate judgment about the veracity of these – according to this philosophy – ultimately lies within society, *not* within any kind of timeless and objective criterion. Consequently, relativism is strongly and rightfully connected with the notion of nominalism (supra). Some even consider these two notions as interchangeable.

Neo-positivism originates – as indicated – after the first world war (the Wiener Kreiss, supra) and is considered today as the dominant overarching paradigm within the sciences (Vermeersch & Braeckman, 2008). Positivists believe their scientific method is the alpha and omega of rationality, and that they can distinguish meaningful scientific statements from pseudo-statements on the basis of objective criteria (Putnam, 1981). While the initial criterion for making these judgments was one of *verification*, Popper later replaced it, as is well known, with one of *falsification* (Störig, 1959; Putnam, 1981; Vermeersch & Braeckman, 2008). In the post war years, this positivist outlook has been institutionalised within almost every sphere of scientific practice and education, and in how lay people understand the scientific method and

its' ability to distinguish between correct, probably correct, and false statements and between the meaningful and the meaningless. Indeed, many researchers seem to believe that this positivist reasoning will bring us closer to the 'one true and complete description of the way the world is' (Putnam, 1981, p 49). Consequently, in the minds of theorists and researchers, positivism is strongly connected with the notion of realism. But are they right to make this connection?

It turns out that few of the founding fathers of this movement would have shared such a commitment. For instance, in Carnap's *Der Logische Aufbau der Welt* (1928), verification was considered 'ultimately private', based on 'incommunicable' and 'subjective sensations'. Popper also seems to recognize 'the institutionalized nature of the implicit norms to which we appeal in ordinary perceptual judgments' (as cited in Putnam, 1981, p 106). Carnap (1928) argued that scientists should be *tolerant* of divergent rational reconstructions. According to Putnam (1981), Carnap was urged by Neurath – among others – toward the idea of a more public, *inter-subjective* conception of verification. *The Principle of Tolerance*, as Carnap called it, or this non-realist position of the founding fathers of neo-positivism, seems to be entirely forgotten (or studiously ignored) by the reigning interpretation of positivism within the field of clinical psychology and psychiatry. Rather, they seem committed to a form of 'realism' that is quite intolerant of other viewpoints, and that worse still, is blind to its own philosophical ancestry. As Verhaeghe (2002) puts it: today, the neo-positivist scientist proceeds from the objective 'Cartesian' exploration of an 'Aristotelian' object with the aim of discovering the underlying 'Platonic' *invariant*.

Indeed, the Austrian-British analytic philosopher, Wittgenstein (1967) pointed out that positivism requires conventions and norms *prior* to the rational procedure, quite simply because without public rules and assumptions, language and reason would be impossible.

Which is a nominalist position. Upholding a stronger version of relativism, Feyerabend argued that the conventions and norms that guide our rational procedures are often irrational themselves. (Feyerabend, 1975). If these thinkers are even half right, so to speak, then psychology and psychiatry are in trouble as far as their claim to possess replicable and useful knowledge goes.

4.5.5. A Closer Look at Todays' Scientific Positions

We believe that we are now able to better define todays' scientific positions within the fields of psychiatry and clinical psychology in relation to the four principal juxtapositions discussed earlier in this chapter, opening the way to four key critiques toward todays' mental health care.

1. Todays' scientific activity aims to gather knowledge; the notion 'scientific truth' refers to an idealization of rational acceptability.

A principal critique: The inadequacy of knowledgeable scientific answers to ethical questions omnipresent in the fields of psychiatry and clinical psychology, is structurally determined.

2. Todays' scientific activity is strongly connected with the notion of metaphysical realism, albeit implicitly.

A principal critique: This (rarely pronounced) position is in sharp contrast with the nominalist viewpoints of many of the founding fathers of neo-positivism.

3. The fields of psychiatry and clinical psychology are considered to be inductive sciences.

A principal critique: It is often forgotten that all statistical models within these fields originate from deductive reasoning and that empirically gathered data always need to fit the a priori theoretical blueprints.

4. Todays' scientific activity is a descendent of 19th century positivism, hence, the notion of 'neo-positivism' itself. It is argued that the rational procedure and above all the judgments in the world of science are ultimately rooted in objective criteria.

A principal critique: The veracity of scientific statements ultimately lies within society, not within isolated and inviolable acts of measurement or objective criteria. That is to say, observational inputs (data) could be considered 'ultimately private', 'incommunicable' and 'subjective sensations' and therefore inevitably reflect human interests and purposes. They are never 'pure' in some platonic sense. Putnam adds to this fourth critique even more weight through his disquisition about underlying *desiderata*.

4.5.6. Putnam's' Surprising Spin

Throughout the work on this dissertation, we were inspired by the earlier work of Hilary Putnam. We believe that Putnam has important things to say about today's scientific activity. Forty years ago, in his book entitled '*Meaning and the Moral Sciences*' (1978), he described what he saw as three main desiderata for any system of morals: the desire that the basic assumptions underlying the system should have wide appeal; the desire that the moral system should be able to withstand rational criticism, and finally; the desire that the recommended morality should be liveable.

Strikingly, a few years later Putnam (1981) argues that these same desiderata might be applicable to any methodology or system of rational inquiry in any major area of human endeavour. In the context of psychiatry and applied psychology, for example, the second desideratum ('being able to withstand rational criticism') sounds self-evident, but the others are in sharp opposition to the neo-positivist (realist) outlook that is the mainstream one today. Our earlier disquisition concerning Foucault (1971), however, may help to illuminate Putnam's

main argument, to the extent that a Foucauldian reading of the history of the mental health field suggests that todays' rational procedures for assessing the effectiveness of clinical diagnoses and interventions could never have been *institutionalized* without wide societal approval for their basic assumptions and moral consequences; especially when it comes to the efforts to measure and objectify human suffering. We believe that Putnam's desiderata deserve further exploration, and they will be taken up again in subsequent chapters of this dissertation.

For now, we believe it is important to underline that Putnam calls on neo-positivist researchers to humbly accept the idea that their methodology originates from arbitrary choices and/or assumptions and from desiderata, that are made or put forward *prior* to their reasoning. All of these choices and assumptions indeed need approval by a societal majority. Consequently, knowledge is always 'polluted' to some extent and scientific truth (in the sense of 'idealization of rational acceptability') is always indebted to the 'contaminated inputs'. The realist aspirations could perhaps be considered noble or inspiring, but they are *un*realistic nonetheless. The extreme realist or extreme neo-positivist ('God's Eye point of view') perspectives are illusionary propositions. They result too often in dogmatic scientific claims which bear an uncomfortable resemblance to the 'divine truths' of the major world religions.

Of course, we are fully aware of the fact that many academics within the behavioural sciences would agree with this critique. However, this is not the message that is transmitted through most scientific publications, where sweeping statements and generalizations are often made, only to be contradicted by subsequent attempts at experimental replication (this is otherwise known as 'The Proteus Phenomenon' – described by Ioannidis & Trikalinos, 2005). What seems to be forgotten is that, as such, the scientific approximations of reality might gradually lose their credibility.

5. Conclusion: The Principle of Tolerance 2.0

Today, our scientific reality is shaped by neo-positivist scientific inquiry, and by the educational and governmental institutions that enshrine it. Based on the critiques developed in this chapter, we suggest that this dominant neo-positivist paradigm – conceived as a neutral and objective search for the truth – might constitute on quite a few levels a form of myth-making that serves interests and ideas other than 'science'.

Quite evidently, the sciences order reality through categorizing all kinds of phenomena and explain these phenomena by communicating meaning that is not directly expressed. However and less evidently, probably due to the great successes of neo-positivist inquiry within the natural sciences over the past century; it seems that neo-positivist scientific research within the behavioural sciences often result in 'irrefutable and realist claims about reality', leaving scarcely any place for scientific debate. Consequently, we observe little difference between the societal functionality of the interpretative thought frames offered through myths and world religions and the societal functionality of todays' dominant scientific paradigm.

Based upon the arguments developed in this chapter, we suggest that this seems to have happened within the fields of psychiatry and clinical psychology, and that the scientific inferences and theories that they uphold should be considered, at best, as approximations of reality: based on contaminated inputs and always in need of 'interpretative charity'. To frame the 'reality' of depression in a particular scientific way – for example, as a DSM V diagnostic category – most certainly makes communication about mood disorders amongst health professionals easier. However, this act of framing leaves two key questions completely untouched: what exactly is the nature of this form of distress, and is all of this scientific activity around nosology and treatment beneficial for the depressed patient, at the end of the day?

If, as a society, we wish to deal effectively with the rising prevalence of depressive disorders,

then it seems vital to find or regain our willingness to tolerate the existence of different viewpoints and divergent rational reconstructions concerning this major public health issue. Referring to Carnap (1928), we would like to call this attitude '*The Principle of Tolerance 2.0*'. This principle was originally conceptualized as a multidirectional proposition; meaning that scientists who take different positions within the four principal juxtapositions, should be tolerant towards each other's views. This principle will be the starting point for the scientific debate we wish to develop in our penultimate chapter. We suggest that '*The Principle of Tolerance 2.0'* could begin with a more substantiated critical approach toward the dominant scientific approach of the Depression Pandemic.

In the next chapter, we question todays' approach within its own terms (methodological arguments), by way of a critical scrutiny of selected portions of the research literature for psychological EBTs aimed at the cure and prevention of depressive disorders. In chapter IV, we question the dominant scientific paradigm within clinical psychology and psychiatry from a broader perspective by combining critical methodological arguments with critical reasoning from outside the dominant paradigm.

Our guiding research question is the following: Does the dominant scientific approach in dealing with depressive disorders offer fruitful perspectives when trying to counter the rising prevalence figures?

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Chapter III

The Depression Conundrum and The Advantages of Uncertainty

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1. Abstract

According to the WHO (2012), the prevalence of unipolar depressive disorders is rising, even in those places where mental health treatments are widely available. The WHO predicts that these disorders will be the leading contributor to the global burden of disease by 2030. This sobering projection fits poorly with how psychological treatments for depression are presented in the mainstream scientific literature: as highly effective therapies, based upon a sound understanding of the causes of distress. There is a clear discrepancy between the rising prevalence figures on the one hand, and the confident claims of this effectiveness research on the other. This discrepancy prompts a set of complex interlinked questions, which we have called 'The Depression Conundrum'. In search of a partial answer, the aim of our study was to critically analyse five meta-analytic studies investigating the effectiveness of psychological EBTs for depression, all of which had been published in high impact factor journals. Our examination established a number of methodological and statistical shortcomings in every study. Furthermore, we argue that the meta-analytic technique is founded upon problematic assumptions. The implications of our analysis are clear: decades of quantitative research might not allow us to conclude that psychological EBTs for depression are effective. The uncertainty and questions raised by our findings might act as a catalyst to broaden the way in which depression and associated therapies are researched. In addition, it might contribute toward a more vigorous and interdisciplinary debate about how to tackle this soon-to-be global public health priority number one.

Keywords: Depression, EBTs, Meta-Analysis, Effectiveness, Quantitative Research

2. Introduction

Several epidemiological studies have estimated that depressive disorders are increasingly prevalent in the general population around the globe (Ormel et al., 1994; Berardi et al., 1999; Demyttenaere et al., 2004; Kirsch, 2009; Moloney, 2013). As of date, the lifetime prevalence for Major Depressive Disorder (MDD) in the USA has risen to 16,6% with an economic expenditure for society of approximately $ 83.1 billion (Greenberg et al., 2003; Kessler et al., 2005). Lifetime prevalence for MDD and dysthymia in Europe are 12.8% and 4.1% respectively and point prevalence of MDD and dysthymia in different European countries varies between 1.1% and 3.9 % (ESEMeD 2004a; ESEMeD 2004b). Overall depression accounts for 11% of disability worldwide and these disability figures are rising (Hirschfeld et al., 2000; Stewart et al., 2003). Today, it is argued that MDD is a chronic disease, prompting Murray & Lopez's 1997 prediction that MDD will be the second overall cause of disability worldwide by the year 2020. The WHO (2012) suggests that depression is the leading cause of lost productivity due to disability. According to the same study, unipolar depressive disorders were ranked as the third leading cause of the global burden of disease in 2004 and will move into the first place by 2030. Dehue (2008) quite rightly talks about *'The Depression Epidemic*'.

2.1. The Therapeutic Mastery of Depression:'Everybody has won and all must have prizes'

Scores of meta-analyses investigating the effectiveness of psychotherapeutic interventions for depression have been conducted over the past decades (Dobson, 1989; Leichsenring, 2001; Westen & Morrison, 2001; Cuijpers et al., 2007a; 2007b; 2008; 2011a; 2011b; Ekers et al., 2008; Kirsch, 2009; Driesen, 2010; Huntley, 2012; Moloney, 2013). Butler et al. (2006) even

published a systematic review of these meta-analyses. Based on these meta-analytical findings, a seemingly well-established twofold scientific consensus seems to be broadly accepted by most academics.

First, nearly all of this outcome research claims that it is now well established that all researched forms of psychotherapy are effective evidence-based treatments (EBTs) in the treatment of depressive disorders as defined in either the Diagnostic and Statistical Manual of Mental Disorders (DSM), the International Classification of Disease (ICD) or the Research Diagnostic Criteria (RDC). Substantial effects of psychological interventions compared to control conditions have been repeatedly documented over the past decades through a myriad of meta-analytic studies. Moreover, this research suggests that these EBTs arise from or lead to scientifically grounded theories explaining the origins of this condition.

Second, research claims that very different psychotherapeutic approaches have comparable benefits (Rosenzweig, 1936; Stiles, Shapiro & Elliot, 1986; Bergin & Garfield, 1994). 'Everybody has won and all must have prizes' or the well-known 'Dodo Bird Verdict' was broadly introduced in the field of clinical psychology by Luborsky et al. (1975). Countless studies confirmed the Dodo Bird Verdict and so, today, a majority of researchers argue in favour of the *equivalence hypothesis*, the hypothesis that very different forms of psychotherapy are equally effective (Verhaeghe, 2005). Miller et al. (2013) suggest to bury the hatchet between different therapeutic orientations in order to be able to start focussing on the one question that hasn't been adequately resolved yet: 'How does psychotherapy work?'

2.2. The Depression Conundrum

Based upon the previous section, it is reasonable to expect that effective prophylactic measures could be implemented and that in case depression *does* occur, it could be cured. Consequently,

one would anticipate that prevalence figures for depression would decline. As mentioned, this is not the case. We define this apparent contradiction between the rising prevalence figures of depression on the one hand, and the claimed therapeutic mastery of this mental health condition on the other, as 'The Depression Conundrum'.

We suggest four juxtaposed explanations for the 'Depression Conundrum'. First, due to lack of funding, prophylactic measures could not be adequately implemented while the number of possible social causal factors rises (e.g. economic imperatives, labour organization, unemployment, racism, sexism, oppression …) (WHO, 2012; Moloney, 2013). Second, people who suffer from depressive mood do not find easy access to adequate and effective treatments (Huntley et al., 2012). Third, people have great difficulty accepting their depressive condition and seeking treatment for it due to a negative representation of depression, due to a negative societal stigmatization of depression (Verhaeghe, 2002; Dehue, 2008; Moloney, 2013). Fourth, the effectiveness of EBTs might be overestimated. We will further focus on the latter.

The assumption shared by most quantitative researchers is that it is possible to measure symptomatology, treatment and outcome in a reliable and valid way through randomised controlled trials (RCTs) and subsequent meta-analysis. We will question this assumption on a meta-analytical level. We were supported in our research focus because expert-statisticians have convincingly exposed the presence of various fallacies and biases when researchers try to examine the effectiveness of treatments through quantitative meta-analytic research (Hunt, 1997; Hedges & Pigott, 2001; Rosenthal & DiMatteo, 2001; Berk & Freedman, 2001; Borenstein et al., 2009; Matt & Cook, 2009; Cooper, Hedges, & Valentine, 2009).

2.3. The Meta-Analytic Technique: Strengths and Weaknesses

The term *meta-analysis* was coined by Gene Glass (1976) to indicate a more general analysis of different individual analyses (Cooper & Hedges, 2009). Meta-analytic research is widely regarded as a well-established method to sift, classify, simplify and synthesize ostensibly inconsistent results from a corpus of studies presenting the highest levels of scientific evidence (Mann, 1994; Hunt, 1997; Rosenthal & DiMatteo, 2001; Cooper, Hedges, & Valentine, 2009). Accordingly, meta-analytic findings are often used as the basis for policy-making (Mann, 1994; Rosenthal & DiMatteo, 2001; Cordray & Morphy, 2009; Greenhouse & Iyengar, 2009). Rosenthal & DiMatteo (2001) argue that the meta-analytic realm offers a number of advantages if and when conducted by researchers who have the necessary statistical expertise. Meta-analysis is now widely used in biomedicine, the behavioural sciences and the interface of the two (Mann, 1994; Rosenthal & DiMatteo, 2001). 'This popularity has come about partly because these disciplines generate too much information to manage easily and methods are needed to synthesize that information' (Rosenthal & DiMatteo, 2001, p 61). Reflecting upon this vindication, a couple of critical questions arise and need clarification.

First, what exactly is the scientific quality of all this information? In their search concerning the *reporting* within meta-analysis on the quality of the primary research, Orwin & Vevea (2009) did not find very encouraging statements. Light and Pillemer (1984) called the reporting of the quality of primary research studies within research synthesis 'shocking', Orwin and Cordray (1985) used the word 'deficient', while Oliver (1987) used the term 'appalling'. Treatment regimens, therapist characteristics, patient characteristics and methodological features are often inadequately reported within the primary research and/or inaccurately transcribed by coders (Orwin & Vevea, 2009). If both these processes are not adequately reported within the meta-analysis, the scientific quality of the findings might indeed be

questionable. In *The Handbook of Research Synthesis and Meta-Analysis* (Cooper, Hedges, & Valentine, 2009) – an important reference book used today by many research synthesists – it is argued by different expert-statisticians that the past decades of meta-analytic practice have amply demonstrated that primary studies *rarely* present good quality evidence.

Second, is the meta-analytic technique undeniably accurate in its information synthesis and does it generally lead to correct and replicable findings? Inductive research starts from data, quantifies these data and these quantifications must be converging towards underlying objective findings that can be – time and again – replicated, the latter being the very essence of the positivist paradigm (Carnap, 1928; Der Wiener Kreis, 1929; Popper, 1934; Wittgenstein, 1961; Störig, 1959; Putnam, 1981; Vermeersch & Braeckman, 2008). A few critical reflections could be raised here about meta-analytic findings when judged according its own requirements. *The Handbook of Research Synthesis and Meta-Analysis* (2009) warns of the numerous pitfalls, threats and limitations of the meta-analytic technique. Matt & Cook (2009) suggest there is a notable lack of control within meta-analytical studies for the threats to inferences about the existence of an association between treatment and outcome classes, especially *causal* associations. The main threats arise from: low statistical power, unreliability, restriction of range, missing effect sizes in primary studies, unreliability of coding, publication bias, bias in computing effect-sizes, lack of statistical independence and the under justified use of fixed- or random-effects models. All of these issues potentially degrade the integrity of later analyses and should be seen as serious threats to generalized inferences (Matt & Cook, 2009; Borenstein et al., 2009; Cooper, Hedges & Valentine, 2009).

Finally, Orwin & Vevea (2009) focus the attention on subjective judgment calls within meta-analysis. When Orwin and Cordray (1985) reviewed the meta-analytical findings of Smith, Glass and Miller (1980) – still a reference within the field of effectiveness research of

psychotherapy – it was amply demonstrated that judgment problems arose in their research synthesis. Subjective judgment calls need to be made during meta-analysis and these possibly have an impact on the quality of the meta-analysis and, accordingly, on the presented effect-sizes and subsequent general findings. Coding decisions on 'therapist experience' or 'length of therapy', for example, when not clearly or accurately reported in the primary study. Often guessing conventions are created to guess the true value of a certain variable. However, unlike pure observational error, the convention-generated errors may not balance out but consistently under- or overestimate the true value (Orwin & Vevea, 2009). Since coding decisions are often based on reporting deficiencies within the primary research, they potentially combine the possibility of bias *and* error (Orwin & Vevea, 2009). These academics argue that guessing conventions artificially deflate true variance in coded variables, diminishing the sensitivity of the analysis to detect relationships with other variables. All kinds of strategies have been developed to reduce coding error, however, based on our literature review we are inclined to assume that it is extremely difficult to entirely eliminate subjectivity within the coding process. Scientific reality might indeed be, above all, a humanized reality (Kuhn, 1970; Putnam, 1981).

Beyond doubt, the meta-analytic technique brought important scientific progress to many research areas. This technique offers a unique way to synthesize the results from a corpus of studies presenting high levels of scientific evidence. 'Potential advantages of meta-analyses include an increase in power, an improvement in precision, the ability to answer questions not posed by individual studies, and the opportunity to settle controversies arising from conflicting claims. However, they also have the potential to mislead seriously, particularly if specific study designs, within-study biases, variation across studies and reporting biases are not carefully considered.' (Higgins & Green, The Cochrane Collaboration, 2011). In what follows, we conceptualize a non-standardized analysis of five selected meta-analytic studies, primarily based on our reading of *The Handbook of Research Synthesis and Meta-Analysis* (Cooper,

Hedges, & Valentine, 2009).

3. Method

In our study, we hypothesize that decades of quantitative research might not have resulted in unequivocal and replicable knowledge about the effectiveness of psychotherapeutic treatments for depression. If this is the case, this could – at least partially – explain 'The Depression Conundrum'.

3.1. A Search on The Web of Science - Inclusion Criteria

1. We searched for recently published meta-analytical studies on the effectiveness of the psychotherapeutic treatment of depression.

2. These studies had to be published in a journal with impact factor >2. We assume these scientific publications are the ones most consulted by both researchers and clinicians and, accordingly, that these findings represent the highest scientific and social authority.

3. Every study had to deal with a different kind of research question concerning the effectiveness of the psychotherapeutic treatment of adult depression. We sought to obtain an accurate overall impression on the effectiveness of psychotherapy for depression.

We conducted a search on Web of Science (Fig. 1). The following search phrase was introduced: *'effectiveness psychotherapy depression meta-analysis'*. We applied our inclusion criteria in our reading of the abstracts from the 68 studies yielded from the search. Then, from the 16 selected studies (Appendix), we chose the first 5 meta-analytic studies we encountered for further analysis (Table 1). The 11 remaining studies were also examined and we will refer to some of these.

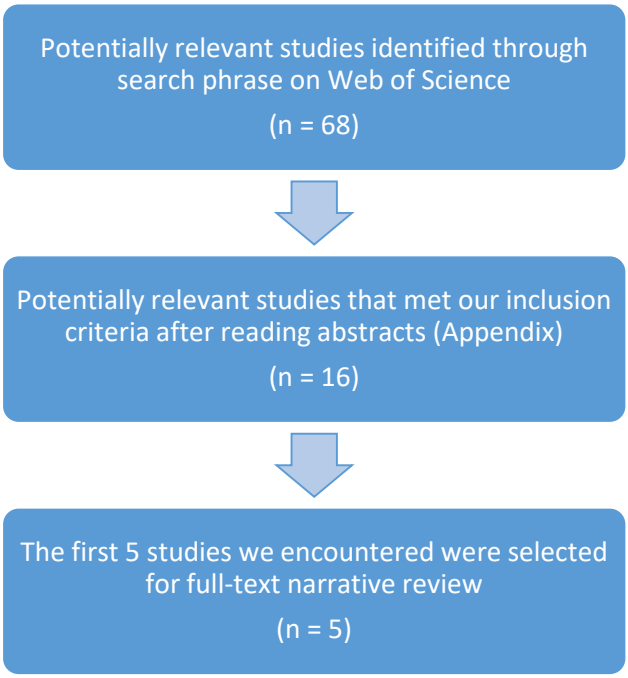

Fig. 1. Process of selection of 5 meta-analytic studies for narrative review

Table 1: *The five meta-analytic studies we analysed in detail.*

Meta-analysis	**Year**	**Journal**	**Impact factor**	**Included studies**	**Main Conclusions as stated in the abstract**
Bortolotti et al.	2008	General Hospital Psychiatry	2.381 High	10	*'Psychological forms of intervention are significantly linked to clinical improvement in depressive symptomatology and may be useful for supplementing usual GP care.'* (p. 293)
Barth et al.	2013	PlosMed	6.13 High	198	*'Overall our results are consistent with the notion that different psychotherapeutic interventions for depression have comparable benefits. However, the robustness of the evidence varies considerably between different psychotherapeutic treatments.'* (p. 1)
Cuijpers et al.	2010	The British Journal of Psychiatry	7.06 High	117	'*The effects of psychotherapy for adult depression seem to be overestimated considerably because of publication bias.'* (p. 173)
Wampold et al.	2011	Clinical Psychological Review	8.146 High	14	*'Extant research on EBT versus TAU reveals that there is insufficient evidence to recommend the transportation of EBTs for anxiety and depression to routine care, particularly when routine care involves psychotherapeutic services.'* (p. 1304)
Huntley et al.	2012	The British Journal of Psychiatry	7.06 High	23	*'Group CBT confers benefit for individuals who are clinically depressed over that of usual care alone. Individually delivered CBT is more effective than group CBT immediately following treatment but after 3 months there is no evidence of difference. The quality of evidence is poor. Evidence about group psychological therapies not based on CBT is particularly limited.'* (p. 184)

3.2. Quality Assessment Parameters

Based on *The Handbook of Research Synthesis and Meta-Analysis* (Cooper, Hedges, & Valentine, 2009), we choose to focus on five quality assessment parameters in order to determine whether or not the main conclusions of our selected studies radiate scientific authority. In what follows, we argue why we choose these parameters.

First, the quality of primary research is known to have a possibly tremendous biasing effect and could therefore potentially threaten the validity of the meta-analysis (Barth et al., 2007; Matt & Cook, 2009; Valentine, 2009). As argued before, primary research studies rarely present good quality evidence (Cooper, Hedges, & Valentine, 2009). High statistical power could be seen as a reliable indicator for good quality. When all primary studies included in the analysis have high statistical power, then the meta-analysis has necessarily high statistical power in the case of fixed-effects analyses or probably high statistical power in the case of random-effects analyses where heterogeneity is low (Hedges & Pigott, 2001). Hedges & Pigott clarify that statistical power of tests in fixed-effects meta-analysis depends on three parameters: sample size, effect-size and the level of statistical significance. In random-effects meta-analysis a fourth parameter is needed in order to be able to estimate statistical power: the between-studies variance component (Maxwell & Delaney, 1990; Diggle, Liang & Zeger, 1994). It should be noted that sample size within meta-analysis has two components: the within-study sample size of the different primary research studies *and* the number of studies included in the meta-analysis. We were particularly cautious concerning meta-analyses of smaller numbers of studies or meta-analyses dealing with research questions where effects were expected to be small. In both cases, meta-analyses do not have necessarily high statistical power (Hedges & Pigott, 2001). Furthermore, the exclusion of individual studies with low statistical power provides some protection against the effects of publication bias (Kraemer et al., 1998). If researchers failed to report the statistical power of the primary research studies, we assumed

they perchance included individual studies with low statistical power. In the present case, we argue that the statistical power of the meta-analysis could possibly be low and we suggest that the findings of these studies should be looked at with prudence, particularly if the number of the included primary studies was low, if effects were expected to be small or in the case of a random-effects meta-analysis where heterogeneity was moderate to high. The robustness of the findings of such studies is questionable. Policy-making based on such findings is problematic.

Second, we focused on whether or not heterogeneity-testing (e.g. chi-square test) was executed and whether or not the inconsistency value I^2 was calculated to explore clinical and methodological heterogeneity between the different studies included in the meta-analysis. Moderate ($I^2 = 50\%$) to high ($I^2 = 75\%$) degrees of heterogeneity indicate a lower scientific weight of the findings (Higgins et al., 2002; 2003; Cooper, Hedges & Valentine, 2009). Heterogeneity should be interpreted as variability not likely due to sampling error, but due to true differences among the studies. We argue that when heterogeneity-testing was not performed or when the degree of heterogeneity was moderate to high, the findings should be looked at with caution. The sturdiness of such findings is genuinely questionable and policy-making based on such findings is precarious (Higgins et al., 2002; 2003).

Third, we investigated whether or not sensitivity analyses were conducted to further test the cogency of the obtained results. Greenhouse & Iyengar (2009) argue that – given the prominent role meta-analysis plays today in policy-making – the need for sensitivity analysis has never been greater. Sensitivity analyses could consist, for example, of excluding studies with noticeable outliers in the statistical analysis because these might distort the overall results. Multiple comparisons – who are not independent of each other – may result in an artificial reduction of heterogeneity and so it is important to conduct additional analyses in which only one comparison per study is included (Cuijpers et al., 2010).

Fourth, we argued in an earlier section that failing to examine and control for a number of other validity threats potentially degrades the integrity of later analyses and should be seen as a serious threat to generalized inferences to broader classes or universes. We investigated whether validity threats were explored.

Finally, based on the reading of Putnam (1981), Elkin (1999), Vermeersch & Braeckman (2008), Cooper, Hedges & Valentine (2009), Moloney (2013) and Miller et al. (2013) we were also particularly interested in whether or not *causal* relationships were established between therapeutic techniques or interventions and the presumed specific effects they have. We realise that this final parameter could easily be disqualified or regarded as highly problematic. Many researchers would argue that meta-analysis is not designed to establish causality. Moreover, *causal claims* are usually not made in meta-analytic studies. However, causal relationships are nearly always *suggested* in the abstracts of meta-analytic research (e.g.: 'Psychological forms of intervention are significantly linked to clinical improvement in depressive symptomatology and may be useful for supplementing usual GP care.' Bortolotti et al., 2008, p. 293). Accordingly, we argue it is essential to specifically put forward the predicament concerning causality. How can we be sure that improvement is due to the treatment itself, rather than other variables such as the contributions made by the client, spontaneous recovery, the individual therapist's personal characteristics, the amount of training, the supervision model used, or any other of a multitude of variables? Putting the question forward 'Were causal relationships established?', is indispensable within effectiveness research, even though we realize the answer to this question is invariably 'No'.

4. Results

We first present a concise appreciation about whether or not our five quality assessment parameters were examined by the five different research teams (Table 2). The sign (*) means that the notion ‘Yes’ (examined) was found flawed or inadequate after further clarification within our more detailed individual review (e.g. lack of data when trying to assess quality of primary research, heterogeneity was tested and reported as ‘high’). Just like the answer ‘No’, the sign (*) thus represents a negative evaluation from our point of view. Table 2 shows a total of 19 negative evaluations out of 25.

Table 2: *Where the five quality assessment parameters examined?*

Study	**Quality Primary Research examined?**	**Heterogeneity testing executed?**	**Sensitivity analysis executed?**	**Other validity threats examined?**	**Causal relations established?**
Bortolotti et al. (2008)	Yes (*)	Yes (*)	Yes (*)	No	No
Barth et al. (2013)	Yes (*)	Yes	Yes	No	No
Cuijpers et al. (2010)	No	Yes	Yes	No	No
Wampold et al. (2011)	Yes (*)	Yes	No	No	No
Huntley et al. (2012)	Yes (*)	Yes (*)	Yes	No	No

Yes = examined. No = not examined. The sign (*) means that the notion 'Yes' was found flawed or inadequate after further clarification within our more detailed individual review (e.g. lack of data when trying to assess quality of primary research, heterogeneity was tested and reported as 'high'). Just like the answer 'No', the sign (*) thus represents a negative evaluation from our point of view. Table 2 shows a total of 19 negative evaluations out of 25.

Review 1: *Bortolotti, B., Menchetti, M., Bellini, F., Montagutti, M.B., Berardi, D. (2008)*

'Psychological forms of intervention are significantly linked to clinical improvement in depressive symptomatology and may be useful for supplementing usual GP care.' (p. 293)

This meta-analytic study is one of the few available examining the effectiveness of the psychotherapeutic treatment of depression compared to usual general practitioner (GP) or compared to antidepressant medication within primary care settings. We will focus on the first

comparison. This publication starts in an encouraging manner. First, the authors excluded studies prior to 1995 because in more recent years several methodological limitations from earlier studies have been overcome (Brown & Schulberg, 1995). Next, the authors assessed the methodological quality of the included 10 trials using the Cochrane Collaboration on Depression Anxiety and Neurosis Quality Rating Scale (Moncrieff et al., 2001). Regrettably however, neither the statistical power of the individual studies nor the statistical power of the meta-analysis were reported, so we assume the authors might have included individual studies with low statistical power. This was particularly problematic given the small number of individual studies involved in this meta-analysis and our previous observations in that regard.

The main analysis showed greater effectiveness of psychological interventions over usual GP care in both short term [standardized mean difference (SMD) = - 0.42, 95% CI)] and long term [SMD = - 0.30, 95% CI]. Both the fixed-effects analysis and the random-effects analysis showed similar results. These overall effect-sizes for both the short-term comparison and the long-term comparison were small effects, according to Cohen's criteria (Cohen, 1988). Because of the thoroughness of GP care provided in the examined trials, the authors suggest that these effects might be actually greater in everyday clinical practice.

Where the heterogeneity test was not significant in the short term, this test showed a high degree of heterogeneity and inconsistency in the long term (I^2 = 70.9%) and the authors point out that they were unable to clarify the reason for this. One sensitivity analysis, excluding three studies based on their inadequate quality rating scores and/or high attrition rates increased somewhat the overall short-term estimate, but there was only a minor difference in the long-term effect-size estimates and the I^2-value remained high for the long-term comparison. Possible publication bias was not examined, nor were any of the other validity threats we mentioned earlier explicitly identified and ruled out. The selected studies had relatively small

sample sizes and high attrition rates. Causal relationships between therapeutic actions and outcome measures were not established within the primary studies. The authors conclude that, mainly due to sample characteristics, their findings could not be generalized to male, child, adolescent or elderly patients, nor could these results be generalized to non-Caucasian patients or depressed patients with substance abuse disorders or with general medical conditions.

In summary, we acknowledge the fact that the many shortcomings of this study were relatively well presented and documented within the discussion section. However, applying our quality assessment criteria, we fail to understand the conclusion as stated in the abstract. We deem that this study offers no solid scientific evidence for this kind of generalized statement. Essentially, this study shows that researchers very often are required to make do with what they have. We would not advise policy-makers to take this study into account while reflecting upon and making decisions about future policy.

Review 2: *Barth, J., Munder, T., Gerger, H., Nüesch, E., Trelle, S., et al. (2013)*

'Overall our results are consistent with the notion that different psychotherapeutic interventions for depression have comparable benefits. However, the robustness of the evidence varies considerably between different psychotherapeutic treatments.' (p. 1)

The conclusions of this study reflect – albeit not perfectly – the above mentioned 'Dodo Bird Verdict' (Luborsky et al., 1975). Within this study, Barth et al. used a fairly new methodology – network meta-analysis – to study the comparative effectiveness of seven psychotherapeutic interventions for depression. The important benefit of this novel technique is that it allows a comparison of *all* conditions in the connected network of all the included studies: direct comparisons within the same trial and indirect comparisons across all trials. However, as the authors point out, it should be mentioned that network meta-analysis makes the questionable

assumption that all included trials originate from the same homogeneous population. A second debatable assumption is made within network meta-analysis, namely, that different treatments each have their own specific rationale and procedure. This allows researchers to group the different treatments and represent them each as one knot in the network. Putting 'brand names' on different forms of psychotherapy is not entirely self-evident. Shedler (2010) argues – from a psychodynamic perspective – that the active ingredients of other therapies include techniques that have long been central to psychodynamic theory and practice. Miller et al. (2013) – taking a different perspective – claim that we do not know how psychotherapy works, that we do not have a clear understanding of its active ingredients, specific rationales and procedures.

A total of 198 primary studies were included in the network, representing 433 conditions, 7 different types of psychotherapy and 15,118 adults with depression. Heterogeneity within the network meta-analysis was low and there was no evidence that direct and indirect estimates were inconsistent. As the authors notice, all of this suggested very good interpretability. Moderate to large effects were found for all seven forms of psychotherapy when compared to the waitlisted subjects. When these interventions were compared to usual care or placebo condition (psychological or pharmaceutical), the researchers found small to moderate effects, except for social skills training. Small or no differences were found when the different interventions were compared with each other and there was no significant difference in effect size when individually delivered therapy was compared with group therapy or internet-based interventions. Indeed, everybody seemed to have won and all seemed to deserve a prize.

However, the authors then put their rather promising initial findings into a more balanced perspective. Treatment effects were smaller when outcome was assessed through self-report measures, blinded observers or within studies where randomization was not adequately concealed. Smaller and less stringently designed studies found larger benefits of psychotherapy

and since 162 studies (out of 198 studies) involved in this network meta-analysis had a small sample size (< 25), stepwise restriction was implemented. Stepwise restriction to only studies with moderate sample size (25 to < 50) and large sample size (> 50) reduced the effect-sizes of all treatments. Stepwise restriction to only studies with large sample size – because of their higher study quality the more accurate analysis within the network – showed moderate effects for only three interventions when compared to waitlist. The reason for this was that stepwise restriction reduced the number of interventions that could be adequately represented in the network. Moreover, most primary studies in this network meta-analysis were conducted in Western countries (58% in the USA) and this could be considered as a serious threat to generalized inferences to broader classes or universes. Of all the studies, 70% investigated cognitive-behavioural interventions while, for example, only 4% of the studies investigated social skills training. Finally, causal relationships between specific therapeutic actions and outcome measures were not established and many validity threats we mentioned before were neither identified nor controlled for in this study.

The authors of this study invite for critical reflection through their exhaustive methodological set-up, through the way the initial findings were put into perspective and through exposing accurately the limitations of their study. We believe the authors would agree with the following additional conclusion: this study primarily shows that a number of distressing issues (supra) surround meta-analytic effectiveness research. This is why we do not agree with the conclusion as stated in the abstract. The findings of this study could be considered as possibly *indicative*, but these findings cannot be presented to the public at large or to policy makers as if they represent a well-grounded scientific consensus.

Review 3: *Cuijpers, P., Smit, F., Bohlmeijer, E., Hollon, S.D., Andersson, G. (2010)*

'The effects of psychotherapy for adult depression seem to be overestimated considerably because of publication bias.' (p. 173)

Publication bias is often seen as a serious menace to the validity of a meta-analytic study (Rosenthal, 1979; Rothstein et al, 2005; Sterne & Egger, 2005; Sutton, 2009). This bias should be interpreted as a mixture of selective publication and selective reporting of outcomes. The goal of this study was to examine indicators of publication bias and to calculate effect sizes that were adjusted for publication bias in order to determine whether or not these adjusted effect sizes differed significantly from the initially presented overall effect sizes. The selection of 117 controlled studies (9537 participants) in which 175 psychotherapeutic treatment conditions were compared with different control conditions, as well as the statistical analyses of the collected data, were carefully carried out. However, the statistical power of the primary research studies was not explicitly reported, many other validity threats (supra) were not identified and controlled for in this study and causal relations between specific therapeutic actions and their presumed outcomes were not explored in this study.

The overall effect size of all 175 comparisons was d = 0.67 (95% CI), which is considered a medium effect (Cohen, 1988). This effect size should be interpreted with caution since heterogeneity was high ($I^2 = 70.27$). Moreover, the funnel plot, first without and then with the imputed studies, showed that smaller studies with lower effect sizes were missing, which indicated the existence of publication bias. Both the Begg & Mazumbar's test and the Egger's test resulted in highly significant indicators of publication bias ($P<0.001$). Adjustment for publication bias according to Duval & Tweedie's trim and fill procedure resulted in a considerable decrease of the overall effect size to d = 0.42 (95% CI). Examining the possible influence of outliers, a new analysis was performed in which all effect sizes of d = 1.5 or larger

were removed. This resulted in an overall mean effect size of d = 0.51 (95% CI). After correcting for publication bias, again, the overall effect size dropped considerably to d = 0.39 (95% CI). Furthermore, since multiple comparisons are not independent from each other, the authors executed analyses in which they included just one comparison per study (comparison with the largest effect size and comparison with the lowest effect size). All indicators of publication bias remained significant (P<0.001). Further sensitivity analyses showed that these indicators remained significant for most subgroups of studies. Because more than half of the comparisons examined CBT, a separate analysis was conducted for this type of treatment. By and large, the results were very similar. Unfortunately the authors failed to mention the degree of heterogeneity for this subgroup. Cuijpers et al. warn that tests for publication bias do not provide direct evidence of such bias and that no statistical imputation method can indeed recover the 'missing truth'. Furthermore, they point out that there are certain weaknesses within the statistical tests used to assess publication bias such as strong dependence on certain assumptions, a tendency for low statistical power of some of these tests or the fact that the algorithm for detecting asymmetry can be influenced by only one or two aberrant studies.

However, given the highly significant indicators of funnel plot asymmetry and based on the 'sobering lesson' to be learned from Rosenthal's' eminent article (1979), we agree with two major conclusions of this research. First, it is very likely that meta-analytic studies overestimate the true effect size of psychotherapy for adult depression due to publication bias. Second, it seems that this effectiveness research is no freer from publication bias than the research on the pharmaceutical treatment of depression as described by Turner et al (2008), Dehue (2008) or Kirsch (2009). As Cuijpers et al. point out, and as is recently suggested by Moloney (2013), psychological treatments have an economic incentive to make the most positive case possible. Psychological treatments that are able to present large effects lead to prestige, power, subsequent lucrative workshop fees and higher session fees.

What does the impact of controlling for just *one* validity threat tell us about the notion 'evidence-based treatments'? Unravelling ‘The Depression Conundrum’ – at least partially – might be easier than initially anticipated. In fact, after reviewing this third meta-analytic study, the following question could be put forward: ‘Might the therapeutic mastery of depression be an illusionary proposition?’

Review 4: *Wampold, B.E., Budge, S.L., Laska, K.M., Del Re, A.C., Baardseth, T.P., Fluckinger, C., Minami, T., Kivlighan II, D.M., Gunn, W. (2011)*

‘Extant research on EBT versus TAU reveals that there is insufficient evidence to recommend the transportation of EBTs for anxiety and depression to routine care, particularly when routine care involves psychotherapeutic services.' (p. 1304)

Within this meta-analytic study the relative efficacy of EBTs when compared to treatment as usual (TAU) was examined through direct comparisons while, at the same time, examining possible confounds such as heterogeneity within the TAU conditions (e.g. TAU with or without psychotherapeutic services) or the specialized training and expertise within EBT conditions. The researchers hypothesized that EBTs would be superior to TAU in the treatment of anxiety and depression in an adult population, but that confounds would moderate this effect. We could argue that some subjective choices made by the research team were quite arbitrary (e.g. the six point rating scale to assess researchers allegiance), however, the methodological set-up of this meta-analytic study deserves recognition. Unfortunately, this study showed primarily what we noted before: researchers are very often required to make do with what they have.

Only 14 studies met the inclusion criteria of this meta-analysis, much of the information regarding the quality of the primary research (e.g. treatment dose, training, supervision, adherence checks), used to assess the overall comparison of EBT versus TAU, was unreported.

Table 1 of this publication clearly shows this distressing fact: in roughly half of the coding sections, the researchers indicated: information not available. When information *was* reported, the authors point out that the design explicitly favoured EBTs. In only three of the included studies, TAU involved psychotherapeutic treatment, but, again, within these studies the EBT condition was favoured (e.g. therapists received additional training and supervision). We found a general lack of data in the first part of the results section and so, the aim of directly comparing EBT versus TAU while examining possible confounds was going to be difficult.

The meta-analysis showed that the overall effect for EBTs versus TAU was d = 0.45 which was significantly greater than zero ($p < .01$) and which represents a significant small to medium effect in favour of EBTs (Cohen, 1988). However, inconsistency was moderate ($I^2 = 58\%$) which provides evidence that the variability among the primary studies was not likely due to sampling error, but due to true differences among the studies. The authors were not able to model how several design confounds would account for this variability. For the between groups test, the mean effect for studies in which TAU was unlikely to be a psychotherapeutic intervention was d = 0.50 in favour of EBTs, which was significantly greater than zero ($k = 9$, $p < .01$). The mean effect size for studies in which TAU clearly comprised psychotherapeutic interventions was d = 0.33 in favour of EBTs, which was not significantly different from zero ($k = 3$, $p = .06$). The difference between these two effect sizes was not significantly different from zero ($df = 1$, $p = .46$).

The authors suggest that strong conclusions and important recommendations were not possible. We fail to understand why they argue in the closing paragraph that 'there does appear to be evidence that implementing EBTs into routine care that does not involve psychotherapy would improve the quality of care'. Few studies were included in this meta-analysis. Those that were included failed to report crucial information and the design, time and again, explicitly favoured

EBTs. Statistical power of the primary studies was not reported, heterogeneity was moderate, final results were not controlled for a number of validity threats (supra). Finally, causal relationships between specific therapeutic actions and outcome were not explored.

Indeed, contrary to what one would expect (cf. the alleged effectiveness of EBTs for depression), this study offers insufficient evidence for the transportation of EBTs to routine care. There were not enough data available and, consequently, meaningful statistical analyses of direct comparisons were impossible. Essentially, this study offers valid arguments for the following conclusion: good quality primary research is hard to find and 'no meta-analysis can ever rise above the quality of the data upon which it depends' (Moloney, 2013, p 92).

Review 5: *Huntley, A.L., Araya, R., Salisbury, C. (2012)*

'Group CBT confers benefit for individuals who are clinically depressed over that of usual care alone. Individually delivered CBT is more effective than group CBT immediately following treatment but after 3 months there is no evidence of difference. The quality of evidence is poor. Evidence about group psychological therapies not based on CBT is particularly limited.' (p. 184).

This publication starts by indicating that waiting lists are long, that resources are limited, that there is a paucity of evidence concerning the effectiveness of group CBT and that group therapies factually treat more patients at the same time and could therefore be more cost-effective.

Twenty-three original RCTs were included in this analysis. Patients were adults of either gender with a primary diagnosis of depression. Group CBT was defined as any form of psychological intervention of three or more participants. Post-treatment outcome was assessed as well as

short-term outcome (> 1 week to 3 months inclusive) and medium to long-term outcome (> 3 months). The secondary outcome measure was cost-effectiveness. The quality of the included studies was assessed using the Cochrane Collaboration's domain-based evaluation tool for assessing risk of bias (Higgins & Green, 2006; 2011). Because of the nature of the studies that met the inclusion criteria, the authors decided to mainly focus on two comparisons: group CBT versus usual care alone (14 studies, 1217 participants) and group CBT versus individually delivered CBT (7 studies, 211 participants).

Many of the 21 studies involved in the two main comparisons had several methodological weaknesses and showed a general lack of sufficient information concerning for example allocation concealment or individuals who had dropped out. A major methodological problem was the small sample size of many studies involved: 'Ten studies (43%) had less than 15 participants in the intervention study arm(s), eight studies (35%) had 16-50 participants per arm and only five studies (22%) had 51 or more participants per arm' (p 186). There was no information provided about the statistical power of the primary research studies nor about the statistical power of the meta-analysis. Based on the aforementioned evaluation tool, Huntley et al. indicated that there was a considerable risk of bias within their study. Causal relationships between specific therapeutic actions and their presumed specific effect(s) were not established and a majority of studies allowed patients within the main comparisons to take concomitant antidepressant medication.

When comparing group CBT to usual care alone, the authors found immediately post-treatment a significant medium treatment effect in favour of group CBT (14 studies, SMD = - 0.55). Further well-advised sensitivity analysis presented no difference to these findings. Only three studies provided data for short-term and medium to long-term follow-up and these studies also showed a medium effect of group CBT over usual care alone (SMD = - 0.47 and SMD = - 0.47

respectively). However, confidence intervals were wide and there was considerable heterogeneity between effect sizes. When comparing group CBT to individually delivered CBT, the authors found immediately post-treatment a small treatment effect in favour of individual CBT (7 studies, SMD = 0.38). No difference in treatment effect was found between the two conditions on short term and medium to long term follow-up. As far as the secondary outcome measure – cost-effectiveness – was concerned, again, the lack of relevant data was notable (e.g. 2 studies). There were indications that group therapies are marginally more expensive than usual care alone and more cost-effective than individually delivered treatment.

The authors themselves consider the quality of their evidence as 'poor'. Given the numerous shortcomings of this study, we argue that the quality of evidence of this study might be considered ‘extremely poor’. As such, we fail to understand why, in the closing paragraph of this study, Huntley et al. indicate that the evidence to support the development of group-based interventions is 'limited but auspicious'. This meta-analysis offers no auspicious evidence in favour of such development, let alone firm scientific evidence. What this study might indicate is the importance of belonging to and sharing with a group of like-minded individuals. The latter resonates with basic logical reasoning and common sense.

5. Discussion

5.1. The Issue of Uncertainty

In our introduction we identified a puzzling situation surrounding depressive mood. On the one hand, we observed that the academic community suggests that we have a considerable understanding of the causes of depression and that we have different psychological EBTs to successfully treat this condition. On the other hand, we noted that worldwide prevalence figures for depression are rising and that the WHO (2012) predicts that unipolar depressive disorders

will be the leading disorder in the global burden of disease by 2030. We called this apparent contradiction 'The Depression Conundrum' and we proposed four juxtaposed explanations. In this paper, we focused on one of these explanations. We examined the methodological quality of the effectiveness research on the subject-matter.

We analysed five recently published meta-analytic studies representing 362 RCTs. We established a number of methodological and statistical shortcomings in every study. Table 2 shows a total of 19 negative evaluations out of 25, meaning that we established in our more detailed review that crucial parameters were either not examined or – when examined – found flawed (e.g. low statistical power, high heterogeneity, lack of data). We argue that unravelling 'The Depression Conundrum' – at least partially – might be easier than initially anticipated because the implications of our analysis are clear: decades of quantitative research might not allow us to conclude that psychological EBTs for depression are effective.

Following Wood & Eagly's (2009) treatise, we deem that our critical reflections place the field of research synthesis in a state of underlying uncertainty. Even though these expert-statisticians argue that research synthesis *rarely* provides definitive answers to the theoretical or empirical questions that inspired the investigation, the question 'How to deal with this issue of uncertainty?' needs to be addressed. Do we need more vigorously executed RCTs and meta-analyses? Do we need a more qualitative research approach? In what follows, we argue there is something to be said for both approaches.

5.2. The Possibility of Gain Through Additional Quantitative Research

Uncertainty suggests a need for additional research and well-supported findings and theories (Wood & Eagly, 2009). The latter should not be treated as a rhetorical device. 'Findings that are homogeneous given adequate power and an appropriate range of conditions suggest an

empirical result that can be accepted with some certainty' (p 457). Following this disquisition, we suggest to focus on a network meta-analysis with the following characteristics. A substantial number of high quality RCT's examining the effectiveness of different psychotherapeutic interventions should be included. Each RCT should show the following characteristics: large sample size, high statistical power, blinded outcome assessment, adequately concealed randomization and the establishment of causal relationships between specific therapeutic actions and outcome. The subsequent meta-analysis should show the following features: high statistical power, low heterogeneity and low inconsistency. Final results should be controlled for all the validity threats and potential biases we discussed earlier. The possible gain of this approach would be that through such design we would be able to actually collect homogeneous findings, meeting the highest levels of scientific evidence. Unfortunately, this approach – however valuable it might be – will take a huge amount of time and funding.

5.3. The Downside Risk of Additional Quantitative Research

Berk & Freedman (2001) argue – based upon their own expertise and upon a multitude of papers by other expert-statisticians – that some of the key assumptions of the meta-analytic technique are highly debatable or worse: 'esoteric as to be unfathomable and hence immune for rational consideration' (p 13).

The key assumption as if subjects are drawn at random from populations is considered gratuitous. The assumptions as if Y_{ij}^{E} (experimentals) and Y_{ij}^{C} (controls) are independent and identically distributed, that these have a common expectation μ_i^E and μ_i^C and that the variances σ_i^2 of both the experimental and the control condition are equal, are regarded by these expert-statisticians as 'phantasmagorical'. Berk & Freedman reason that different studies within a meta-analysis cannot be independent of one another because of an underlying 'social

dependence'. 'Investigators are trained in similar ways, read the same papers, talk to one another, write proposals for funding to the same agencies, and publish the findings after peer review. Earlier studies beget later studies, just as each generation of Ph.D. students trains the next.' (p 12). All these cardinal assumptions within meta-analysis are considered as often 'pleasing', yet 'illusory' (Berk & Freedman, p 12). It could be argued that statistical models are often extended in one way or another in an attempt to evade certain statistical and methodological problems (e.g. random effects models). However, Berk & Freedman clarify that these extensions do not in any way make these models substantially more believable. They conclude that meta-analysis would be an excellent method for research synthesis *if* the assumptions held.

We mentioned earlier that meta-analysis has the potential to mislead seriously because of methodological and statistical flaws (Higgins & Green, The Cochrane Collaboration, 2006, 2011; Cooper, Hedges & Valentine, 2009). If we combine this with the critical reflections made by Berk & Freedman concerning the underlying assumptions, we infer that there is a downside risk to the call for additional quantitative research.

5.4. The Constructivist-Interpretative Stance Within Qualitative Research

A second scientific approach departs from a very different reasoning. Within this line of thought, arguments are developed to adopt the idea that quantitative research might not be able to entirely capture the essence of depressive mood (e.g. symptom assessment) or the effectiveness of different EBTs (Elkin, 1999; Verhaeghe, 2002; Dehue, 2008; WHO, 2012; Moloney, 2013).

Assuming that impoverished or pathogenic environments or contexts trigger depressive mood (WHO, 2009, 2012; Pilgrim & Rogers, 2010; Moloney, 2013), we suggest to make far greater

use of qualitative research, designed to explore and study all kinds of phenomena within their specific contexts while at the same time offering the possibility to map the prevailing socio-economic and political hegemonic discourses. The multiplicity of qualitative inquiry (e.g. narrative research, discourse analysis, interpretative phenomenological research) epitomizes the potential strength of this approach (Potter & Wetherell, 1987; Burman & Parker, 1993; Marecek, 2003; Madill & Gough, 2008). Qualitative research departs from the premise that humans are intentional and meaning-making agents. It thereby asks 'how'-questions instead of 'why'-questions. For example, in regard to the experience of distress and of mental health treatments, qualitative research would aim at exploring in detail the contexts in which depressed people live and the particular functionality of a symptom (Vanheule, 2015). A global research focus on the similarities between these contexts might turn out to be of importance.

Even though not well represented in the mainstream scientific literature, *meta-syntheses* – the qualitative equivalent for meta-analyses – have already delivered some promisng new insights that would be difficult to obtain through quantitative research. For example: computerized therapy for depression could ameliorate considerably through personalization and sensitization of content to individual users, recognizing the need for users to experience a sense of 'self' in the treatment which is at present lacking (Knowles et al., 2014). Also, the finding that traditional masculinity values could serve as barriers but equally as facilitators in the development of coping strategies in depressed men (Krumm et al., 2017).

In an attempt to deal more effectively with the rising prevalence figures for depression, this approach could potentially lead to different kinds of research questions asked concerning the nature and causes of depressive mood, the types of treatment on offer for it, and their effectiveness. The constructivist-interpretative stance within qualitative research allows researchers to expose that there is not 'one' world or one 'objective reality', but different

perspectives on the world (Putnam, 1981; Potter & Wetherell, 1987; Burman & Parker, 1993; Marecek, 2003; Madill & Gough, 2008). It should be noted that a common critique of the epistemology of qualitative research as solely inductive is not correct. In fact, qualitative research is often driven by theory (Marecek, 2003; Vanheule, 2015). Likewise, it should be noted that this qualitative approach will also take a huge amount of time and funding.

5.5. Limitations

We included a relatively small number of meta-analytic studies in this article, yet we did not refrain from making some critical statements. This could be seen as an important limitation. In addition, given the scope of this article, we could not further explore the vast impact of different validity threats to meta-analytic inferences as described in Cooper, Hedges, & Valentine (2009). We realize we did not provide many specific new angles from which the academic community could further explore depressive mood. As might be clear, we are currently reflecting upon meaningful alternatives from different interdisciplinary angles.

6. Conclusion: The Advantages of Uncertainty

Following Wood & Eagly's (2009) reasoning, we conclude that the implications of our critical reflections place the field of clinical psychology in a state of underlying uncertainty. However, this uncertainty has mainly two advantages. First, this uncertainty is an indirect plea for scientific humility within the field of clinical psychology, meaning that the outcome of quantitative research concerning the effectiveness of the psychological treatment of depression could be considered as indicative, but not as representing a well-established scientific consensus. Second, uncertainty stimulates new lines of thought and might therefore be

considered as an indirect plea for a more qualitative research approach to better explore singular contexts, working-mechanisms, the functionality of a symptom, treatment and effectiveness.

The very nature of scientific psychology discourse surrounding depression throughout the past decades may well have led to tunnel vision and to theoretical immobility. We argue in favour of a new and vigorous scientific debate concerning depression. A platform where different kinds of research questions could be raised in an attempt to deal with this soon-to-be public health priority number one. Nothing numbs the human mind as fundamentally as hearing the same familiar words, slogans or scientific statements over and over again (Feyerabend, 1975).

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8. Appendix

List of the 16 studies meeting our inclusion criteria:

1. Bortolotti, B., Menchetti, M., Bellini, F., Montaguti, M.B., & Berardi, D. (2008). Psychological Interventions for Major Depression in Primary Care: a Meta-analytic Review of Randomized Controlled Trials. *General Hospital Psychiatry, 30*(4): 293-302.

2. Barth, J., Munder, T., Gerger, H., Nüesch, E., Trelle, S., et al. (2013). Comparative Efficacy of Seven Psychotherapeutic Interventions for Patients with Depression: A Network Meta-Analysis. *PLoS Med, 10*(5): 1-17. doi:10.1371/journal.pmed.1001454

3. Cuijpers, P., Smit, F., Bohlmeijer, E., Hollon, S.D., & Andersson, G. (2010). Efficacy of Cognitive-Behavioral Therapy and Other Psychological Treatments for Adult Depression: a Meta-analytic Study of Publication Bias. *The British Journal of Psychiatry, 196*(3): 173-178.

4. Wampold, B.E., Budge, S.L., Laska, K.M., Del Re, A.C., Baardseth, T.P., Fluckinger, C., Minami, T., Kivlighan II, D.M., & Gunn, W. (2011) Evidence-based Treatments for Depression and Anxiety versus Treatment-as-usual: a Meta-analysis of Direct Comparisons. *Clinical Psychology Review, 31*(8), 1304-1312.

5. Huntley, A.L., Araya, R., & Salisbury, C. (2012). Group Psychological Therapies for Depression in the Community: Systematic Review and Meta-analysis. *British Journal of Psychiatry, 200*(3), 184-190.

6. Cuijpers, P., van Straten A., & Warmerdam, L. (2007). Behavioral Activation Treatments of Depression: a Meta-analysis. *Clinical Psychological Review, 27*(3): 318-326.

7. Saman, Z., Brealy, S. & Gilbody, S. (2011). The effectiveness of behavioural therapy for the treatment of depression in older adults: a meta-analysis. *International Journal of Geriatric Psychiatry, 26*(12): 1211-1220

8. Ebrahim, S., Montoya, L., Truong, W., Hsu, S., Kamal el Din, M., et al. (2012). Effectiveness of Cognitive Behavioral Therapy for Depression in Patients Receiving Disability Benefits: A Systematic Review and Individual Patient Data Meta-Analysis. *PLoS ONE, 7*(11) https://doi.org/10.1371/journal.pone.0050202

9. Mazzucchelli, T., Kane, R. & Rees C. (2009). Behavioral Activation Treatments for depression in Adults: A Meta-analysis and review. *Clinical Psychology: Science and Practice, 16*(4): 383-411.

10. Cuijpers, P., van Straten, A., & Warmerdam, L. (2007). Problem Solving Therapies for Depression: A Meta-analysis. *European Psychiatry, 22*: 9-15.

11. Leichsenring, F. & Rabung, S. (2008). Effectiveness of Long-term Psychodynamic Psychotherapy. A meta-analysis. *The Journal of the American Medical Association, 300*(13):1551-65.

12. Cuijpers, P., Geraedts, A.S., van Oppen, P., Andersson, G., Markowitz, J.C., et al. (2011). Interpersonal Psychotherapy for Depression: A Meta-analysis. *American Journal of Psychiatry, 168*(6): 581-592.

13. Driessen, E., Cuijpers, P., de Maat, S.G., Abbass, A.A., de Jonghe, F., et al. (2010). The Efficacy of Short-term Psychodynamic Psychotherapy for Depression: a Meta-analysis. *Clinical Psychological Review, 30*(1): 25-36.

14. Westen, D. & Morrison, K. (2001). A Multidimensional Meta-analysis of Treatments for Depression, Panic, and Generalized Anxiety Disorder: an Empirical Examination of the Status of Empirically Supported Therapies. *Journal of Consulting and Clinical Psychology, 69*(6), 875-99.

15. Stalder-Lüthy, F., Messerli-Bürgy, N., Hofer, H., Frischknecht, E., Znoj, H. & Barth, J. (2013). Effect of Psychological Interventions on Depressive Symptoms in Long-Term Rehabilitation After an Acquired Brain Injury: A Systematic Review and Meta-Analysis. *Archives of Physical Medicine and Rehabilitation, 94*(7): 1386-1397

16. Foroushani, P.S., Scheider, J. & Assareh, N. (2011). Meta-review of the effectiveness of computerised CBT in treating depression. *BMC Psychiatry, 11*(131): 1-6

Chapter IV

Breaking The Depression Deadlock – Rethinking Depression Globally

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1. Abstract

According to the WHO (2012), unipolar depressive disorders will be the leading cause of the global burden of disease by 2030, notwithstanding substantial increases in the provision of EBTs for depression. It is seldom remarked that this is odd, since statistical modelling suggests that closing the 'treatment gap' should reduce population prevalence. In an attempt to partially explain this conundrum, we put forward three arguments against the dominant clinical approaches: we argue that the diagnostic construct of depression lacks scientific foundation, and that neither the psychotherapeutic nor pharmacological EBTs are as firmly 'evidence-based' as their proponents claim. We establish that these critical arguments are routinely ignored by most of the leaders in the clinical field. This selective ignorance helps to keep the whole of society in a deadlock when it comes to dealing more effectively with the rising prevalence figures. We suggest that this failure reflects a complex mixture of influences: including the power of the dominant scientific paradigm and the associated notion of 'cognitive dissonance', a set of interlocking professional and economic interests, and a preference for superficially comforting accounts of the origins and nature of personal malaise. Any attempt to break the Depression Deadlock, might have to start with a vigorous interdisciplinary debate about what we understand by 'depression', and about the kinds of research questions that are likely to improve this understanding. Rethinking depression globally, we might need to abandon our faith in treatment, and focus on preventive measures; – which is largely a matter of politics.

Key-words: Depression, EBTs, scientific paradigm, media, cognitive dissonance.

2. Introduction

In 2012 the World Health Organization (WHO) predicted that unipolar depressive disorders would be the leading cause of the global burden of disease by 2030. Disturbing global prevalence figures for depression were published in 2013 and the international public health community pronounced the treatment of depressive mood as an important priority (Ferrari et al., 2013). We were surprised by the WHO prediction and puzzled by these rising prevalence figures, because for decades there has been a consensus within the academic and clinical communities that the multiple causes of depression (and their synergisms) are well understood, and that we have effective evidence-based treatments (EBTs) to tackle the condition. Jorm et al. (2017) recently confirmed that prevalence has not decreased, despite substantial increases in provision of treatment and despite the fact that statistical modelling suggests that closing the 'treatment gap' should reduce population prevalence. For those countries in which these treatments are widely available, it is therefore reasonable to expect that the condition should have been kept under control, via the combined use of the allegedly effective prophylactic and curative measures that currently exist. However, in most of those nations that enjoy good quality public health care systems there has been a steady rise in the number of people diagnosed with this malady since the late twentieth century (Horwitz & Wakefield, 2007; Moncrieff, 2009; Horwitz, 2015; Jorm et al., 2017), which suggests that such expectations are misguided. Thus, rates of prescribing of anti-depressant medication in the UK, doubled between 1998 and 2010; and a similar picture has obtained in the US, where eleven per cent of the population aged over eleven are said to take an antidepressant (Horwitz and Wakefield, 2007; Dorwick & Allen, 2013). Dehue (2008) reports similar findings for the Netherlands.

The meaning of these trends remains open to question. In the Western world and beyond, culturally sanctioned expectations that self-realisation and lasting fulfilment should be the norm

surely contribute to an over diagnosis of major depressive disorder; if only because this pervasive belief in happiness as a right may incline many to regard any sign of despair as an illness, needing to be fixed (Davies, 2015; Petersen, 2011). This notion is encouraged by the advance of drug company advertising aimed directly at consumers (at least in the United States), and by a growing acceptance of a biomedical narrative of human nature and of psychological disturbance; a narrative that often sits – in seeming contradiction – alongside an equally widespread sense that harsh life experiences can engender mental health problems; and that these problems can be eased or cured via talking therapy (Dehue, 2008; Kokanovic, Bendelow & Philip, 2013; Rogers and Pilgrim, 2015). Over the years, clinicians and their distressed patients have become more inclined to reach for a formal clinical diagnosis, where once they would have spoken of ordinary sadness as a legitimate response to troubling events and circumstances (Horwitz and Wakefield, 2007; May et al., 2004; Rogers and Pilgrim, 2015).

If people in Western countries (and beyond) have been only too willing to talk about their troubles in the language of the mental health professionals, then a key question is whether or not the sadness that underlies the currently fashionable psychiatric labels has genuinely increased in prevalence in recent decades. There are reasons to think that it might have done. Since the 1980s, most Western societies have seen a large increase in social and economic inequality as the ideology of 'market forces' and the power of international capital has waxed, and as the power of organised labour has waned (Wilkinson and Pickett, 2009; Verhaeghe, 2012). Almost universally, the dismantling of publicly owned services and the widening gap between the rich, the middling and the poor has been accompanied by worsening physical and mental health in the general population – and especially amongst the most deprived sectors. These trends are visible in a wide range of physical and mental health indicators – from reported rates of 'schizophrenia' to diabetes and obesity, for example. They are also evident in forms of experience and conduct that suggest the integrity or otherwise of the polity: including civic

participation, reported personal loneliness and distrust within neighbourhoods, accident rates at work, and levels of vandalism, violent crime and suicide (Verhaeghe, 2012; Wilkinson and Pickett, 2013; Monbiot, 2016). These toxic trends have accelerated with the 'austerity' policies pursued by many Western governments since the great recession of 2007, which have amounted to an extended attack upon the livelihoods and wellbeing of ordinary people: leading, in some of the hardest hit nations, to tangible reductions in the longevity of the poorest groups (Stuckler & Basu, 2013).

Given the widespread nature of such problems, psychological and drug treatments for unhappiness could scarcely be expected to make a big difference; although there are advocates who argue that the former, in particular, can do so. In the UK, for instance, the economist and government consultant Richard Layard has been instrumental in the creation of a publicly funded programme in England and Wales, aimed at bringing mainly Cognitive Behavioural Therapy (CBT) to any citizen who feels that they are in need of it. Supporters claim that the scheme, now over a decade old, has been a success. Detractors point out that there are no signs that the project has had any impact upon rates of distress within the general population, or that the treatments provided have proven to be anywhere near as effective as claimed (McInness, 2011; Moloney, 2013; Timini, 2014). Indeed, a comparable Swedish project was recently been closed down by the national government that sponsored it, owing to the abject failure of mass CBT on clinical and economic grounds (Moloney, 2013).

In sum, in the field of mental health care, there is a wide gap between the dominant theories about the roots, nature and treatment of depression – and what can actually be shown to be the case. In this paper, we argue that this situation has come about for four main reasons. First, because the diagnostic category of depression itself is problematic, second, because most of the 'official' knowledge concerning the effectiveness of psychotherapeutic remedies is flawed,

third, because a very similar picture obtains for the clinical research conducted into so-called 'anti-depressant' medication, and finally, above all – because all of these issues are either routinely ignored or dismissed by defenders of the status quo in psychiatry and psychology. In the penultimate section of this paper we consider some explanations for why this is so. We conclude that the mental health treatment field and perhaps society as a whole has arrived at a deadlock situation in regard to how this primary health issue is understood and managed, and that more fruitful critical and scientific perspectives are needed if we are to move beyond the present impasse.

3. Four Distressing Arguments

3.1. First Argument: The Diagnostic Category of 'Depression' Is Incoherent

For almost forty years, the Diagnostic and Statistical Manual of Mental Disorders (currently in its fifth edition – or 'DSM V', 2013) has been at the centre of mental health care throughout much of the world – and especially in North America, Europe, and in the English speaking countries generally (Verhaeghe, 2002; Scull, 2016). Supporters of the DSM argue that it is well founded in clinical science; and that its checklist-based approach to categorising mental disorder has brought considerable progress to the field of mental health care and treatment in the form of diagnoses that are both reliable and valid. In what follows, we question all of these claims and especially in regard to the diagnostic label of 'depressive disorder'.

A good starting point is to consider the question of the reliability of this nosological category. Can different clinicians, faced with the same distressed patient, agree upon the same psychiatric diagnosis when following the guidelines laid down in the DSM? The controversial studies by Sandifer, Hordern & Green (1970) or by Rosenhan (1973) showed that psychiatric diagnostic tools (e.g. symptom assessment, self-reports, scales, clinical tests, …) were not without flaws

and that the reliability of psychiatric diagnostics was poor. In the decades since the inception of the DSM III in 1980, with its checklist approach to mental disturbance – it has been argued that the reliability of the protocol for diagnosis has improved considerably, as the symptom lists have become more detailed and refined (Verhaeghe, 2002; Mitchel et al., 2009; Frances, 2013; Moloney, 2013; Vanheule, 2015; Van Os, 2014). Nonetheless, these academics argue that this improved reliability might be an imaginary proposition. In contrast, in relation to depressive disorders, in a review article Wang & Gorenstein (2013) suggest that the Beck Depression Inventory II (BDI-II) – a widely used psychometric instrument based on the DSM diagnostic criteria – shows high reliability and a capacity to discriminate between depressed and non-depressed subjects. However, Vanheule (2015) argues that psychiatric diagnoses of – for example – mood disorders are by no means more reliable than the prototypical approach from the first part of the past century. The reliability appears to have increased – not because of improved agreement amongst clinicians about the nature of the 'symptoms' and about how to identify and record them – but because of a biased interpretation of the data from research studies on diagnosis – which reflects a relaxation of the standards for the statistical evaluation of data (Vanheule, 2015). The tables in Vanheule's book concerning the evolution over the past decades of different norms for evaluating the kappa statistic, speak volumes. These norms became considerably less stringent over the years, creating the illusion of high(er) reliability (Vanheule, 2015). Similarly, Epstein (2006) notes that the BDI was originally standardised against subjective (and therefore scientifically unreliable) clinical judgements, and that the failure to rectify this foundation upon sand must cast doubt upon all descendent versions. However, sceptical voices like these are routinely ignored or dismissed within the mental health treatment literature (Dehue, 2008; Moloney, 2013; Newnes, 2014; Vanheule, 2015; Epstein, 2017).

Furthermore, there is the question of the diagnostic validity of the label 'clinical depression',

wherein the conferment of the category upon the sufferer should provide accurate prognosis and a set of helpful treatment recommendations (Richter et al., 1998; Verhaeghe, 2013). To be valid, a diagnostic category is customarily taken to point to an identifiable pathology within the body – which can be readily identified via symptoms and signs: in this sense, validity presupposes reliability. According to current psychiatric opinion, the core of depression is constituted by a disturbance of mood and feeling, typically linked with negative thoughts (or 'cognitions'), self-judgments, and emotions – such as shame, worry, guilt and anxiety. Bodily or vegetative disturbances may be included as optional extras – ranging from loss of weight to insomnia, and a retardation of movement. However, the tendency is to focus on the cognitive aspects of depression, especially when it comes to psychological therapies (Beck & Alford, 2009). Despite these dominant views however, affective or cognitive symptoms are not seen in every patient complaining of depression, and in many countries, including Greece, Nigeria, China and India, the majority of depressed people seek medical help on the basis of physical complaints (Fancher, 1996; Throop, 2009; Fuchs, 2013).

As far back as 1970, Sandifer, Hordern & Green exposed problems concerning the validity of the diagnosis of depression. Because validity presupposes reliability, the Rosenhan study (1973) also – be it indirectly – challenges the validity of psychiatric diagnoses. Decades later, Pilgrim & Bental (1999), Horwitz & Wakefield (2007) and Fuchs (2013), all of which deal with the topic of validity in their different ways, conclude that the validity problems persist. Verhaeghe (2002) and Moloney (2013) clarify that there are no objective diagnostic tests such as x-rays, brain scans or blood analyses that can confirm the presence of – for example – the diagnostic label 'clinical depression'. In the words of one of the architects of the DSM, the American psychiatrist, Robert Spitzer – mental illnesses such as depression are created by committees and they are 'the only form of disease that can be caught by word of mouth' (quoted in Moloney, 2013, p 34). While this is not entirely true – there are other recognised medical disorders that

have no clear biological referent, including migraine – these diseases differ from psychiatric ones in that the label is usually received with gratitude and, arguably, has fewer moral implications for how the sufferer is viewed by others and by themselves (Rogers and Pilgrim, 2015). Psychiatric diagnoses are above all a social judgment, and Watters (2010) – from an anthropological point of view – suggests that many of the DSM mental illnesses (e.g. depression) could be seen as American exports, which say more about how personal distress is framed and understood within that culture – as a largely individual matter, requiring treatment at that level. Quite different concepts of profound sadness can be found in cultures whose members do not experience themselves so much as separate individuals but as participants within social communities. Disorders of mood or wellbeing are then seen as a tapestry of bodily, interpersonal and 'atmospheric' processes, that arise from the interweaving of person and context (Pilgrim & Bentall, 1999; Throop, 2009; Fuchs, 2013).

These critical observations remind us of the central thesis of Boorstin (1962), in The Image. This academic argued 55 years ago – from a social theoretical perspective – that in the West, it would not be truth, but credibility that in the future would increasingly decide the success or otherwise of technologies and ideas. Claims about the alleged precision and validity of the construct of depression are credibly packaged and are attractive to many policy makers, clinicians and patients – but this does not prevent these claims from being highly questionable when examined with a clear eye: it is the dubious status of the construct of depression that we consider to be *deadlock number one*.

Finally, a subsequent critical consideration as introduction to our second and third argument: if the diagnostic label 'depression' is a moot one, then, logically, every Randomised Controlled Trial (RCT) that seeks to examine the effectiveness of a given treatment for depression will be flawed right from the beginning. This is because researchers cannot know for certain whether

the trial is targeting people who really do share the same kind of clinical problem, or even how well these people represent those who are designated as ‘depressed’ within the wider population.

3.2. Second Argument: The Questionable Effectiveness of Psychotherapeutic EBTs for Depression

Throughout Europe and the United States, psychological treatments for depression and other forms of distress have been in widespread use for well over forty years and have been researched by means of the RCT method for almost half a century. The results have been favourable but also variable from study to study – owing, amongst other things, to disparities in the quality of experimental design, in the selection of participants and of measurements and statistical procedures, and to the many vagaries of time and place that can help to shape the outcome of a given trial. In an attempt to overcome such problems and to achieve some kind of consensus, researchers have turned to the tool of meta-analysis: – wherein a group of published research investigations are selected for their apparent rigour, their results pooled, and then subjected to complex statistical analyses intended to isolate and distil the claimed benefits of the given psychological treatment. In the clinical literature, meta-analyses are seen as authoritative and often as definitive, precisely because they are based upon large numbers of participants and of independent investigations, respectively. In these circumstances, supporters argue, any consistent finding must be telling us something ‘real’ about the given clinical intervention.

The tool of meta-analysis might be in widespread use within the mental health treatment field, but does this mean that it is always employed carefully? To answer this question, we need to think critically about the benchmarks that the analysts are using. Meta-analytic inferences about

the effectiveness of psychotherapy for depression might offer a good basis for health policy-making when they are well supported by significant medium to large effect-sizes, when the quality of the primary research upon which they are based is of a good standard in terms of participant selection and of overall design, execution, and measurement (Cooper et al., 2009; Celie et al., 2017). Moreover and more technically, all potential validity threats to the meta-analytic technique need to be identified and ruled out, heterogeneity within these meta-analytic studies needs to be low and sensitivity analyses should always be conducted to further test the cogency of the obtained results (Celie et al., 2017). Given the prominent role meta-analysis plays today in policy-making, Greenhouse & Iyengar (2009) argue that the need for sensitivity analysis has never been greater. Sensitivity analyses could consist, for example, of excluding studies with noticeable outliers in the statistical analysis because these might distort the overall results. Multiple comparisons – who are not independent of each other – may result in an artificial reduction of heterogeneity and so it is important to conduct additional analyses in which only one comparison per study is included (Cuijpers et al., 2010). Thoughtful statisticians argue that if these and many other conditions are not met, then the meta-analysis will be flawed, and will not allow us to make confident generalisations about the power of the treatment under test (Hunt, 1997; Hedges & Pigott, 2001; Rosenthal & DiMatteo, 2001; Barth et al., 2007; Matt & Cook, 2009; Borenstein et al., 2009; Cooper, Hedges & Valentine, 2009).

Unfortunately, the clinical research literature routinely fails to meet these requirements. As the American academic William Epstein (1995, 2006, 2013, 2017) has long argued, investigations into the effectiveness of talking and behavioural treatments need to be unusually rigorous. Powerful and culturally determined placebo influences and expectations saturate the therapeutic encounter, and can lead both the client and the therapist to exaggerate the benefits of the treatment – especially to themselves. And yet in a number of detailed reviews of some of the leading studies within the research literature, Epstein has noted that there are repeated issues of

client representativeness and attrition, of the adequacy of participant blinding, of the reliability and validity of the outcome measurements, the statistical measures used to interpret them, and of the clinical and institutional allegiance of the researchers and therapists themselves. Over the years, a number of critics have made similar observations, although their voices have been largely ignored within the field (see, for example, Zilbergeld, 1983; Eisner, 2000; Erwin, 2000; Kline, 1988, 1992; Moloney, 2013; Newnes, 2014). In regard to the psychological treatment of depression, a recent examination of five leading meta-analyses yielded a similar picture (Celie et al., 2017). Together, these seemingly compelling analyses represented no less than 362 separate RCTs. Yet the review established serious methodological and statistical shortcomings in each of these five studies. Upon close scrutiny, these meta-analytic studies failed to attain the standards for good quality quantitative research. Based upon these studies, all of which were published in high impact factor journals, one could not decisively claim that psychological EBTs for depression successfully treat this condition. It seemed that the researchers were often required to make do with what they had (poor quality primary research) and – as Moloney (2013) argued – 'no meta-analysis can ever rise above the quality of the data upon which it depends' (p 92).

In sum, it seems that Epstein's (1995) shocking conclusions still hold today. There seem to be no scientifically credible meta-analytic studies available verifying the effectiveness of any form of psychotherapy for depression. Even if we rephrase this last sentence in a milder form ('There seems to be no scientific *consensus* concerning the effectiveness of psychological EBTs for depression'), we conclude that we must consider this issue *deadlock number two*.

3.3. Third Argument: The Doubtful Effectiveness of Pharmaceutical EBTs for Depression.

The medical approach to depression begins with the postulate that the latter is a brain disease

and that the aetiology of different forms of depressive mood is to be found in physiological or heritable factors or a combination of both (Moncrieff, 2009; Healy, 2012; Goldacre, 2012). More specifically, it is argued that certain neurotransmitters in the brain that regulate mood are out of balance. These chemical imbalances are held to be the origin of depressive mood. Accordingly, medical treatment predominantly consists of prescribing chemicals to re-establish the neurobiological balance. In reality this is no more than a hypothesis, but in the clinical literature and in pharmaceutical company advertising it is presented as a well-established 'scientific truth'. However, already in 1998, neuroscientist Elliot Valenstein warned: "What physicians and the public are reading about mental illness is by no means a neutral reflection of all the information that is available" (p 292).

In fact, there are several hypotheses behind the idea of neurobiological imbalance as the causal factor for developing depressive symptomatology. The best known is that depressive mood reflects a lack of the neurotransmitter serotonin within the central nervous system (Kirsch, 2009). Lacasse & Leo (2005) indicate that the impact of the ubiquitous promotion of the serotonin hypothesis should not be underestimated. However, they conclude: 'The incongruence between the scientific literature and the claims made in FDA-regulated SSRI advertisements is remarkable, and possibly unparalleled'. Dehue (2008), Leo & Lacasse (2008), Moncrieff (2009), Kirsch (2009), Healy (2012), Goldacre (2012) and Moloney (2013) – reminding us, among other things, that correlation does not imply cause – clarify that these different hypotheses never transcended the level of presumption and that, therefore, these postulates cannot be considered scientific facts. Indeed, they are 'postulates', 'things that are suggested or assumed as true as the basis for reasoning' (Oxford Dictionary, 2016). Kirsch (2009) talks about 'The Myth of the Chemical Imbalance', Moncrieff (2009) about 'The Myth of the Chemical Cure'. Moloney (2013) argues that defending this idea of neurochemical causation could be compared to the following dubious logic: 'headaches come from a lack of

aspirin' (p 35).

The evidence that challenges the idea of the claimed effectiveness of the pharmaceutical treatment of depression has been growing in recent years (Lacasse & Leo, 2005; Kirsch, 2009; Fournier et al., 2010; Middleton & Moncrieff, 2011; Goldacre, 2012; Healy, 2012; Khan et al., 2012; Moncrieff & Timini, 2014). In his well-known critical analysis, Kirsch (2009) examined a vast number of clinical trials and meta-analyses on the effectiveness of different antidepressants. He concludes that when antidepressants are compared to active placebos, drug-placebo differences in improvement are not statistically significant. Pigott et al. (2010) conclude that the effectiveness of antidepressants is even lower than the modest effect sizes reported in the largest antidepressant effectiveness trial ever conducted (STAR*D). These researchers argue for a reappraisal of the current recommended standard of the medical care of depression. From a more ethical point of view, Dehue (2008) criticizes the growing individual responsibility and accountability placed upon the depressed as a result of this neurobiological approach of depression, along with the commercialization of the pharmaceutical treatment of depression. It seems that today's conventional ideas about the effectiveness of the pharmaceutical treatment of depression are no longer tenable.

One could argue that if antidepressants prove to be statistically as effective as active placebo's, we do not need to bother with the underlying working mechanisms or aetiological hypotheses. We do not agree with such reasoning. Most experts mentioned earlier in this section specifically indicate that it has been broadly presented in the scientific literature over the past decades that these drugs work because of their chemistry, that there is a causal relationship between chemical composition and improvement. Since many recent publications demonstrate that such causal relation has never been documented in a scientifically reliable and valid way and given the numerous *disruptive side effects* these chemicals have, this poses a cardinal problem for the

pharmaceutical treatment of depression. But the problem stretches further than this. There seem to be various problematic methodological issues surrounding the effectiveness research into the pharmaceutical treatment of depression.

RCT studies and subsequent meta-analyses have always been thought of as well-established methodological designs to examine the effects of pharmaceutical treatments. However, 'breaking blind' or figuring out whether one has been given the real drug and not the placebo, for example, has a tremendous effect on the overall quality of this research (Kirsch, 2009). People usually 'break blind' after experiencing all kinds of side effects. When patients believe they have been given the real drug rather than the control placebo, expectations of improvement increase and higher expectations of improvement result in higher *reported* improvement and thus in higher effect-sizes (Vase, Riley & Price, 2002; Kirsch, 2009). These biased effect-sizes result in erroneous inferences about causation and effectiveness of antidepressants.

Critical scrutiny of meta-analytic research in this field reveals more major issues. Meta-analytical research by Turner et al. (2008) reported, after controlling for publication bias, an overall effect of Cohen's d 0.31, which is considered to be fairly small (Cohen, 1988). In addition to the major problem of publication bias (Rosenthal, 1979; Turner et all, 2008; Dehue, 2008; Sutton, 2009; Kirsch, 2009; Cooper & Hedges, 2009), expert-statisticians warn for other validity threats surrounding meta-analytic effectiveness research, such as: academic pressures to find positive results (Ioannidis, 2005; Verhaeghe, 2012); citation bias and the associated allegiance effects (Rothstein & Hopewell, 2009; Greenberg, 2009), language bias (White, 2009; Reed & Baxter, 2009; Rothstein & Hopewell, 2009), duplication bias and availability bias (Rothstein & Hopewell, 2009); and other methodological limitations (Ioannidis, 2005; Orwin & Vevea, 2009). Rosenthal & DiMatteo (2001) and Matt & Cook (2009) point out that even if only one validity threat is not examined, the quality of a meta-analysis might be highly biased.

Unfortunately, according many researchers mentioned in this section, a combination of all these validity threats is at work in most meta-analytic studies. Indeed, there are individual studies, even meta-analytic studies, that report high effect sizes in favour of the pharmaceutical treatment of depression, as Dehue (2008), Moncrieff (2009) or Kirsch (2009) indicate. However, these should be looked at with prudence. It has been amply demonstrated that there are a number of circumstances that inflate these effect sizes artificially. The most important one is leverage or systematic distortion of evidence by the pharmaceutical industry (Dehue, 2008; Kirsch, 2009; Moncrieff, 2009; Goldacre, 2012; Healy, 2012).

In summary: over the past decades, it has become increasingly clear that there is no strong reliable scientific support in favour of the chemical-imbalance theory and the pharmaceutical treatment of depression. Ever since the 1950's, pharmaceutical companies around the globe willingly present correlational relationships as causal relationships (Kirsch, 2009; Moloney, 2013). Much – if not all – of the therapeutic effect of antidepressants seems to be due to the placebo effect (Kirsch, 2009). The specific working mechanisms of antidepressants remain vague and the negative side effects are numerous (Healy, 1997, 2012; Dehue, 2008; Leo & Lacasse, 2008; Moncrieff, 2009; Kirsch, 2009; Goldacre, 2012; Moloney, 2013). Surprisingly, data contradicting the chemical-imbalance theory continue to be ignored and the nineteen-billion-dollar-a-year-and-rising expenditure on a wide variety of antidepressants (Kirsch, 2009) indicates the unwarranted rise of the medical treatment of depression. The scientific evidence justifying these vast medical expenditures seems to be wafer-thin. We observe that few people know about this *deadlock number three* and this brings us to our fourth and principal argument.

3.4. Fourth Argument: All Three Above Mentioned Arguments Are Often Ignored Or Easily Dismissed

We noted before that the three earlier identified problematic issues do not seem to reach policy makers and the general public at large. In this section we look at explanatory mechanisms for this unfortunate state of affairs. Todays' scientific reasoning and knowledge logically derives from the dominant scientific paradigm. As Kuhn (1970) argues, it is this paradigm that decides what is relevant and what is not. We will further conceptualize today's prevailing paradigm as *one* version of reality that allows us to conceive of only one story about depressive mood. Other aspects of this mental health issue remain hidden.

3.4.1. Reassuring Messages and Their Two Main Effects

Let us summarise our earlier arguments in order to extend them. The diagnostic label 'depression' is highly questionable: both the validity and the reliability of the diagnosing protocol are poor and there is no scientific consensus on the aetiology of depressive mood. This disconsolate picture is matched by the paucity of good evidence for the effectiveness of psychotherapeutic and pharmaceutical EBTs for depression. And yet, policy-makers and the public at large receive contrary messages from academic institutions, professional bodies, the pharmaceutical industry, from the employees of public health services and, above all, from the mass media. A spurious certitude about the causes and cures of low mood is the order of the day (Valenstein, 1998; Lacasse & Leo, 2005; Dehue, 2008; Kirsch, 2009; Moloney, 2013; Moncrieff, & Timimi, 2013; Vanheule, 2014; Celie et al., 2017).

Inspired by Vermeersch & Braeckmans' (2009) treatise on the functionality of myth, we reason that these reassuring messages concerning depression mainly have two important consequences for how we view mental distress and expect it to be treated. First, today's psychological and medical discourse *consolidate* the way we look at depression, as a form of medical illness; and

second, both of these scientific discourses also *legitimize* the prevailing hierarchy of treatments and the relationship between patient and professional practitioner. It has become almost unthinkable for lay people and health professionals to evade psychological and/or medical discourse when reflecting upon depression: even where environmental causes or 'triggers' – in the harshness of daily life – might be conceded, a powerful and contradictory discourse remains, that depression is a product of faulty brain chemicals or styles of thinking and behaving – awaiting correction with the help of the appropriate experts (Rogers and Pilgrim, 2015). Too many of us seem to have lost sight of other viewpoints when confronted with this major public health problem. We will further argue that both the media and the notion of cognitive dissonance play a crucial role in the expansion of this scientific position. We will first briefly consider that our reality is multifaceted and that different juxtaposed scientific conceptions of reality are possible and probably even preferable. The latter could offer new vistas when it comes to understanding and helping those struggling with low mood (infra).

3.4.2. Today's Scientific Reality

What does 'scientific reality' mean? Even though the question itself seems to suggest that there is a definite description of *the* scientific reality, it seems wise to remind ourselves that some of the most renowned and most cited philosophers of science of the past century specifically clarify that there is no such thing as 'one objective and righteous scientific reality', there are just different scientific conceptions of reality (Kuhn, 1970; Feyerabend, 1975; Putnam, 1981; Vermeersch & Braeckman, 2009). Putnam (1981) clarifies that these different perspectives derive from the assumptions, underlying desiderata and ethical choices that are adopted before scientific reasoning starts. However, Kuhn (1970) elucidates that the dominant scientific paradigm always exerts considerable productive and coercive power to the extent that other scientific conceptions of reality are often considered as insignificant. Kuhn also emphasizes

that the popularity of the ruling scientific paradigm is transitory. Scientific paradigms preceding and succeeding a *paradigm shift* are so different that the new paradigm cannot be proven or disproven by the rules of the old paradigm and vice versa. Scientific reasoning, according Kuhn, functions from *within* a certain paradigm, meaning that it is the paradigm that determines what is seen as pertinent and what is not, what appears to work and what does not. Feyerabend (1975) extends these ideas further. He argues that todays' knowledge – as a result of the ruling (neo)positivist paradigm – is reduced to method. Such knowledge can be refined, further clarified and prepared for application, but it cannot grow in size and substance. Feyerabend would argue that if, today, meta-analytic research (method) suggests that psychotherapeutic or pharmaceutical treatments for mood disorders are highly effective, we would tend to accept these findings and consider them as things we know (knowledge) about depression and the treatment of this condition. Also – and more importantly – , we would not consider other angles of analysis that deviate from the prevalent method, because the ruling paradigm suggests these are not relevant. Essentially, Feyerabend argues that, today, reason and method are seen as one entity, not as two different human activities. Doing so, Feyerabend offers well-grounded arguments for scientific pluralism as an essential component of *scientific progress* (knowledge that grows in size and substance). The meta-analytic inferences (supra) might indeed represent something worth knowing about depressive mood, but other scientific perspectives or ways of analysis (beyond the prevalent method) might add relevant and potentially crucial knowledge about the condition.

These scholars have put the notion 'scientific reality' in a distinct perspective, not in a distinct definition. It is important to establish that today's ruling scientific paradigm (positivism) or today's scientific reality proceeds from two compelling presuppositions or *realist* notions: the existence of an *objective reality* and the *measurability* of this reality (Putnam, 1985; Verhaeghe, 2002; Vermeersch & Braeckman, 2009). Consequently, it should come as no surprise that

today's media reports about scientific discovery seem to put forward the idea of an existing objective scientific reality.

3.4.3. Media Does Not Inform, It Mainly Deforms the Multifaceted Notion of 'Scientific Reality'

The news and entertainment media saturate our lives via television, internet, radio and a dwindling hard copy press – and so they help to shape the way in which we view our world and its mixture of possibilities and threats (Entman, 1989; Bourdieu, 1998). When it comes to the public's understanding of complex scientific issues, the media often presents its messages as objective. It projects a simplified picture of reality, as something that can be easily glimpsed and understood through the appropriate images and labels and the powerful feelings that they evoke in the consumer (Bourdieu, 1998; Verhofstadt, 2012). Far from being objective in some ultimate sense, however, these meanings are constructed within specific human contexts and are shaped by particular economic and political power relations (Foucault, 1971; Feyerabend, 1975; Entman, 1989; Maly, 2007; Nolan, 2008; Bozic-Vrbancic, 2008; Villegas, 2009; Davies, 2009; Fourie, 2012; Verhofstadt, 2012).

As mentioned, it is more accurate to say that our reality – including 'scientific' reality – is multi-dimensional. Different versions or conceptions of reality are possible and probably even desirable. From this constructivist perspective, the meanings that are generated by our media are immersed in productive power and ideology and tend to serve the interests of elite groups especially – whether economic, political, or scientific (Feyerabend, 1975; Herman & Chomsky, 1994; Bourdieu, 1998; Davies, 2009; Carter, Thomas & Ross, 2011; Boone, 2011). Translated to our subject matter, these media messages could be understood as the result of an interlocking set of interests – of the pharmaceutical industry, of the professions of psychiatry and psychology

(and other mental health industry workers) and of politicians who prefer quick fix answers to complex personal and social problems: ones that would otherwise be very expensive and difficult to even attempt to solve (Moloney, 2013; Epstein, 2017). Likewise, the ubiquity of these messages may in part also reflect the perceived self-interest of lay people, who might prefer simple explanations and quick fixes for their troubles, and who might want to disguise their legitimate disgruntlement with their steadily intensifying workplace demands (in the neoliberal era) as a form of mental illness ('stress', 'burnout' or 'depression'). Collective bargaining power at work has been eroded, and for many, the only way to express protest or take time out from unpleasant work conditions is by adopting the language of illness (Fleming, 2015).

3.4.4. Media Coverage About the Biomedical Approach of Depression

It has been amply demonstrated that what we debated in the previous sections, is precisely what the pharmaceutical industry has been doing while promoting treatment for depression. The clinical and scientific reality of depressive mood is described in a narrow and homogeneous way (e.g. 'Depression is a brain disease, certain neurotransmitters in the brain that regulate mood are out of balance'). Other angles of analysis or interpretation almost seem entirely redundant (Dehue, 2008; Moncrieff, 2009; Kirsch, 2009; Fournier et al., 2010; Middleton & Moncrieff, 2011; Goldacre, 2012; Healy, 2012; Khan et al., 2012; Moncrieff & Timini, 2014; Moloney, 2013).

The well-known *mere-exposure effect* (Zajonc, 1968) does the remainder of the work. Repeated exposure increases perceptual fluency, which, in turn, facilitates positive affect towards the message that has been distributed (Seamon, Brody & Kauff, 1983). More specifically, as a consequence of the very repetitive biomedical media messages about depressive mood,

suggesting the idea of mastery of this issue, a vast majority of people actually see this biomedical approach as genuine and accurate. Consequently, they tend to believe that different pharmaceutical treatments for depression work. These must work, the prevailing scientific paradigm dictates they do. Repetitive, credibly packaged, attractive sounding media reports about an oversimplified scientific reality, facilitate the dissemination of this treatment. However, anti-depressants might quite simply 'work' because stakeholders make dazzling amounts of money selling them and will do anything to maintain these sales (Dehue, 2008; Kirsch, 2009). Is it – beyond doubt – unimaginable to resume the matter this way?

3.4.5. Media Coverage About Psychotherapy

Less known is a critical observation about the psychotherapeutic treatment of depression made by the Cuijpers et al. (2010) research team, an observation that was more thoroughly examined and substantiated by Moloney (2013): psychological treatments that are able to claim large effects – such as CBT, for example – facilitate more research funding, prestige, lucrative workshop fees and higher session fees. Cuijpers et al. (2010) point out that psychotherapeutic effectiveness research is no freer from publication bias than the research on the pharmaceutical treatment of depression as described by Turner et al. (2008), Dehue (2008) or Kirsch (2010). Cuijpers et al. (2010) and Moloney (2013) argue that pharmaceutical treatments are not the only therapeutic area in which truth has fallen prey to economic incentives, which encourage researchers and clinicians to downplay or ignore unfavourable findings and dubious research methods (Moloney, 2013; Newnes, 2014; Epstein, 2017). Repeated positive media coverage (mere-exposure effect, supra) about the effectiveness of psychotherapeutic EBTs could be considered convenient for researchers and therapists, who want to assert the pre-eminence of their own knowledge and techniques. Notwithstanding – as we argued before – that good

evidence for the claimed specific effects of these psychotherapeutic interventions is lacking (Epstein, 1995; Elkin, 1999; Moloney, 2013; Miller et al., 2013; Celie et al., 2017). Indeed, some of the most ardent advocates of talking treatment acknowledge that the question of 'How does psychotherapy work?' remains unanswered (Miller et al., 2013). After more than fifty years of research this is a startling admission.

For all of its humanistic elements, therapy is also a way of earning a living and sometimes a nakedly commercial enterprise (Zilbergeld, 1983; Aschoff, 2015). Therapists are paid to 'pay' attention, they have good reason to sell their attention as highly effective. Following the same logic as in the previous section, we reason that psychotherapeutic treatments also benefit by repeated positive media coverage about its potency. However, psychotherapy might quite simply 'work' because researchers and therapists need to make a living. Is it – beyond doubt – unimaginable to resume the matter this way?

In summary: through all kinds of media we receive powerful reassuring scientific messages about the diagnosing protocol, the aetiology and the treatment of depression. This media coverage invigorates the prevailing scientific paradigm and often describes scientific reality in an oversimplified way. It lacks nuance and leaves little space for alternative viewpoints. The meanings these messages generate consolidate and legitimize persistently the currently accepted views about depression. The paucity of published criticism on these apodictic messages in both popular and scientific media, should *not* surprise. It is a logical result of the coercive and productive power exerted by the ruling scientific paradigm (Kuhn, 1970; Feyerabend, 1975).

3.4.6. The Problem of Cognitive Dissonance

It is very likely that the reading of this manuscript so far makes the reader feel uneasy. Most of the arguments that we have developed are at odds with what we believe we know about depression. This uneasiness could provoke plausible deniability. *Cognitive dissonance* is the psychological term for the unpleasant tensions that arise when facts, opinions or behaviour fundamentally contradict one's belief system, norms or values. More specifically, these cognitions – which could be more broadly interpreted as knowledge, attitudes, emotions, beliefs or behaviour – are perceived as fundamentally incompatible and create stress. Festinger (1957), Harmon-Jones & Mills (1999) and Verhaeghe (2012) note that this cognitive dissonance can potentially have far-reaching consequences. When we are at heart convinced about a set of ideas we are often not capable of accepting contradicting information and so we have the tendency to dismiss this information (Verhaeghe, 2012). Translated to our subject matter: our first, second and third argument create considerable cognitive dissonance within the biomedical sciences, the behavioural sciences and the interface of the two. These arguments seem fundamentally incompatible with what we believe we know about depressive mood. From our perspective, fundamentally incompatible with what the dominant paradigm *allows* us to know through a mainly quantitative research focus. As a consequence, alternative ways of analysis are easily dismissed and potentially valuable reasoning is easily ignored or rejected.

We noted before that the prevailing scientific paradigm – and its inherent interpretative thought frames, theories, methodological requirements, assumptions and postulates – narrows our view on the matters discussed here. As a consequence and congruent with the notion of cognitive dissonance, we tend to disavow that other perusals of reality are possible. However and more importantly, we also tend to disavow the idea that a paradigm presupposes specific desiderata (Putnam, 1981). Kuhn (1970), Foucault (1971), Feyerabend (1975), Putnam (1981), Verhaeghe (2002) and Dehue (2008, 2014) – among others and each from a very different perspective –

expound that systems of thoughts, scientific argumentation and ensuing theories are subjective approaches of reality. They clarify that our scientific reality is first and foremost a *humanized* reality, meaning that humans first make all kinds of ethical and pragmatic choices and that only then, after these choices are made, a paradigm becomes viable and scientific reasoning can start. Consequently, some answers to most urgent questions about depressive mood may very well lay beyond the boundaries of the prevailing scientific paradigm.

3.4.7. Problematic Underlying Presuppositions and Ethical Choices

As suggested, todays' dominant scientific neo-positivist paradigm – a combination of logical positivism and logical empiricism – propagates the idea that meaningful statements can *only* derive from methods of mathematics, logic, and empirical research ('…the scientific method exhausts rationality itself, and testability by that method exhausts meaningfulness…', Putnam, 1985, p 105). That is, no a priori presuppositions or ethical choices are presumed to lay at the basis of neo-positivist scientific inquiry. Indeed, the falsification method is supposed to be independent from a priori desiderata. Putnam (1985) explains that this led to 'the conception according to which a statement is true in case it corresponds to (*mind independent*) facts' (Preface, IX), when it coincides with objectively measured empirical evidence. However, he argues that precisely *this* conception produced – above all – a priori philosophies which leave no room for a rational activity of philosophy. Putnam (1985) considers that this is why extreme positivist views on reality are – just as it is for extreme relativist views – self-refuting.

In sum, neo-positivist inquiry departs from presuppositions and these are essentially and by definition *arbitrary* choices (Kuhn, 1970; Feyerabend, 1975; Putnam, 1981; Verhaeghe, 2002; Vermeersch & Braeckman, 2008; Dehue, 2008). The importance of this ascertainment cannot be overestimated, it will allow us – in the final section of this paper – to introduce more easily

some alternative hypotheses when looking at the rising prevalence of depression. We indicated before that today's research concerning depression and associated treatments begins with two main a priori presuppositions or realist notions: the existence of an objective reality and the measurability of this reality. However, two more ethical choices need to be made before today's quantitative research concerning depressive mood can start.

First, both the ostensibly dissimilar biomedical and psychological approaches assume that depression is saying more about the afflicted individual than it is saying about the circumstances these individuals are dealing with or about their social contexts. As we will later argue, this is a highly questionable point of view (Dehue, 2008; WHO, 2009; Moloney, 2013). Second, both disciplines have an almost entirely *functional* view on mental disturbance. This functionality needs to be understood as follows: a fairly limited range of feelings, emotions and conduct is considered as functional and, therefore, as normal and acceptable in relation to an arbitrarily defined normative conceptualization of how society should be organised and how people should conduct themselves within an ordered community (Verhaeghe, 2002, 2012; Dehue, 2008, 2014; Moloney, 2013). A much wider range of feelings, emotions and conduct is viewed as dysfunctional and therefore considered as anomalous. These dysfunctional feelings and emotions and this dysfunctional behaviour are portrayed as symptoms of an underlying malady that needs to be cured (Moloney, 2013). Happiness, contentment, participation and the obligation to stay well are the standards to which all should aspire.

Table 1: Resuming today's scientific research concerning depressive mood

1. The research findings concerning diagnosis, causality and treatment of depression we discussed earlier, could be seen as a logic consequence of ethical choices and presuppositions that were made prior to reasoning.
2. These choices and assumptions are arbitrary by nature and thus problematic.
3. No major scientific progress was made over the past decades trying to deal effectively with the rising prevalence figures for depression. The way we have been looking at depressive mood over the past decades might have led to a *deadlock situation* concerning this major health priority.

Table 2: Our inevitable conclusion

If the way in which mental health professionals and policy makers today respond to the rising prevalence figures for depression could be qualified as 'arbitrary' rather than based upon good science, and furthermore, if this perspective is leading to extremely poor results when it comes to the treatment and prevention of low mood, then probably the moment has come that critical thinkers start looking at this public health priority differently. We suggest to expand the current lines of social, methodological and ethical inquiry concerning both the roots and the possible amelioration of depressive mood.

4. *Breaking the Depression Deadlock by Expanding Ethical, Methodological and Social Inquiry*

Congruent with our reasoning in the previous section, we will further clarify that any attempt to break the 'Depression Deadlock' must start by disregarding the underlying presuppositions and/or ethical choices of the dominant scientific paradigm. A number of academics – often from very different backgrounds – delivered, in fact, exquisite arguments to do just that. These academics start their reasoning from an entirely different postulate: there is no such thing as one objective and measurable reality (quantitative research, supra), all we have are different *perspectives* on reality. The latter is the epistemological stance from which qualitative research begins.

4.1. From Quantitative Research Focus Towards Qualitative Research Focus.

'Further research is needed to clarify…' is a phrase that can be find in the conclusion section of numerous scientific articles. In the present case, this is a call for more RCT's and more meta-analytic studies, essentially to try to establish relationships between treatment and outcome. We clarified in the earlier sections – mainly based on methodological arguments – that these RCT findings and meta-analytic findings often have little scientific value and that subsequent inferences are often scientifically unreliable. After decades of questionable quantitative research, this 'Further Research Is Needed'-mantra should be altered because this call for more research nearly always refers to more *quantitative* research and nearly always results in a world of marketised individual care (Dehue, 2008, 2014; Moncrieff, 2009; Goldacre, 2012; Healy, 2012; Moloney, 2013; Devisch, 2013).

As we argued before (Celie et al., 2017), qualitative research is designed to explore and study human activity, understanding and experience within their particular contexts, which are often

taken to include the prevailing socio-economic and political narratives. The diversity of qualitative inquiry (e.g. narrative research, discourse analysis, interpretative phenomenological research) epitomizes the potential strength of this approach (Marecek, 2003; Madill & Gough, 2008). Qualitative research begins with the premise that humans are intentional and meaning-making agents. The constructivist-interpretative stance within qualitative research allows researchers to show that there is not 'one' world ('objective reality', 'copy theory of truth', supra), but different outlooks on the world (Ponterotto, 2005). It should be noted that a rather common and dismissive epistemological critique of qualitative research as a solely inductive activity is not correct: this research also often derives from theory (Marecek, 2003; Vanheule, 2015). Beyond doubt, the main advantage of this approach is that researchers can simultaneously examine individuals that suffer from depressive mood – exploring their particular contexts and dispositions through for example 'theory-led thematic analysis' (Hayes, 1997) – *and* the bigger socio-economic and political contexts through for example 'discourse analysis' (Potter & Wetherell, 1987; Burman & Parker, 1993) or 'media-framing analysis' (Shaw & Gilles, 2009). Such multi-dimensional approach could result in a much broader understanding of both individual and societal origins of depressive mood, possibly leading to effective prophylactic measures on the short run and perhaps even leading to some more profound social changes. We argued before that the method of meta-synthesis – the qualitative equivalent of meta-analysis – has already provided promising new insights into how psychotherapy might be made more helpful (Celie et al., 2017).

However, and far more importantly, as argued before, a large body of (mainly) qualitative research already suggests that on the one hand, pathogenic environments foster depressive mood amongst other forms of distress, and that, on the other, the best way to reduce this emotional suffering – is not to treat people – but to create a world in which there is less social and economic inequality, and in which we might take better care of one another (Jahoda, 1982;

Platt, 1984; Smail, 1987, 2006; Lipsdge & Littlewood, 1997; Sayad, 2004; Tinghog, Hemmingson & Lundberg, 2007; Wilkinson & Pickett, 2009; Friedli, 2009; Usher, 2011; Verhaeghe, 2012; Oliver, 2013; Moloney, 2013; Rogers & Pilgrim, 2015). If more qualitative research would further confirm and deepen these ideas, this could lead to insights that have clear implications for *political policy* – which brings us to our next proposition.

4.2. From Individual Treatment Towards Societal Cure

Moloney (2013) dedicates an entire chapter of his book, *The Therapy Industry*, to what he calls '*the hidden injuries of inequality'* (p 94). Very appropriately, this chapter, quoting the words of a British mental health service user, is called 'I'm not ill, I'm *hurt*…', which immediately suggests an entirely different approach toward depressive symptomatology. While Moloney's proposals are not new (e.g.: Szasz, 1974; Wilkinson, 1996; Verhaeghe, 2002, 2012; Melzer, Fryers & Jenkins, 2004; Dehue, 2008; Kirsch, 2009; Pilgrim & Rogers, 2010), he gathers a large body of evidence on the social origins of personal distress that cannot be easily dismissed. The 19th century asylums were filled with the deviant poor; and throughout the following century and more, living in an impoverished or pathogenic environment has consistently been found to go hand in hand with elevated levels of anxiety, low mood, self-harm, psychosis and psychiatric hospital admissions (Marmot & Strafford, 2003; Tomlinson & Walker, 2009; Friedli, 2009; Read, 2010). Indeed, poor mental health correlates not just with lack of money (NSPCC, 2008), but with virtually all indices of disempowerment and of low social rank – including female gender (Usher, 2011), black and minority-ethnic status (Lipsdge & Littlewood, 1997; Sayad, 2004; Tinghog et al., 2007), disability (Oliver, 2013), old age (Rogers & Pilgrim, 2015), sexual minority membership (Rogers and Pilgrim, 2015), unemployment (Jahoda, 1982; Platt, 1984; Whooley et al., 2002) and finally, with being bullied or exploited at work (Walker &

Fencham, 2011). Moreover, ever since neoliberal economic doctrines were put forward as the prime economic model in the 1990's, rates of reported anxiety, low mood, and chronic insecurity and distress have accelerated albeit unevenly across time and place (Verhaeghe, 2012; Moloney, 2013; Rogers and Pilgrim, 2015). All of these arguments from a sociological perspective agree with the Income Inequality Hypothesis (IIH) of Wilkinson & Pickett (2009). They suggest that in those industrialised societies that enjoy an adequate health care and treatment infrastructure, then the health and well-being of the population is influenced not so much by the introduction of the latest medical technologies or psychological insights, as by the overall distribution of wealth, and especially by the economic and social gap between the richest and the poorest. The larger this divide, then the more likely are citizens to be involved in a scramble for status and security and to experience the mutual unease and mistrust that undermine bodily and mental health (Wilkinson and Pickett, 2009). Over the past years, almost 200 studies offered support for the IIH (De Maio, 2014). Moloney (2013) concludes that for some of us mental illness is quite simply 'almost inevitable'; and that the most compelling account of its nature and its sources can be found in what the late British clinical psychologist David Smail described as 'social materialist' psychology. Based upon more than thirty years of observations made from within the UK National Health Service, this approach acknowledges, on the one hand, that talking treatment can do little more than provide comfort and elucidation as to the likely causes of individual malaise. On the other, this form of psychology posits that distress is deeply rooted in the body more than the mind, in the cumulative feelings that result from many years of mistreatment, neglect, or abuse. While words can help us to describe this reality they cannot change it, since for good or ill we acquire our emotional experience ineluctably, in the manner of plants that grow well or struggle, in accord with their situation. If we wish to reduce human suffering then we need to abandon our faith in treatment, and to learn instead to take better care of each other; – which is largely a matter of *politics* (Smail, 1987;

2006). As far as the construct of 'depression' is concerned, this perspective aligns with clinical observations that the majority of patients in the West who are assigned this diagnosis report bodily disturbances – including loss of appetite, fatigue, numbness, sickness or pain – notwithstanding that such reports are often downplayed by clinicians, who focus instead upon the patients' presumptive inner 'cognitive world' – in need of correction via Cognitive Behavioural Therapy (Fuchs, 2013). This 'environmentalist' outlook also makes a good match with the findings of transcultural studies in psychiatry, which suggest that in a majority of cultures – especially those which regard the self as fundamentally interconnected with family and community – persistent low mood is experienced primarily as a disturbance of the body *and* of the sufferer's social bonds (Kleinman and Good, 1985; Gureje, Simon & Ustun, 1997; Throop, 2009). Western researchers and clinicians have often attributed these reports to a lack of 'psychological literacy' on the part of people from supposedly 'backward' cultures. However, the main findings of the cross-cultural researchers do resonate with two important schools within Western psychotherapy, namely the psycho-dynamically oriented attachment theorists (e.g. Fonagy et al., 2002) and the Freudian-Lacanian theorists of 'psychological identity' (Verhaeghe, 2002). Thinkers from both of these schools conceive of the development and maintenance of our sense of who and what we are – of our 'psychological identity' – as always rooted in and defined by 'the Other'. Our identity emerges from a continual process of exchange – of identification and separation – with the people around us, and this process starts at birth. Rather than being seen as two separate entities, the self and 'the Other' are understood as one interconnected aggregate. From these perspectives, and in the light of the epidemiological and clinical literature described above – it seems that the main narratives of personal distress found in none-Western cultures provide a firmer grip upon the nature and origins of chronic sadness (Fuchs, 2013; Throop, 2009; Watters, 2010). Finally, these different perspectives fit comfortably with critical analyses of the construct of depression, which view it as both a social

creation – shaped by the language and ideas used by clinicians and lay people – and as a reflection of how the material and social world can disturb body and mind simultaneously (Pilgrim and Bentall, 1999).

A number of interesting research questions arise from this epistemological position. Did the psychotherapeutic and pharmaceutical treatment of depression direct all attention away from the world that gave rise to these mental conditions? Have these treatments pull up a veil behind which societal problems such as radical social change, loneliness, loss of meaning, a shift to urban life and alienation stay hidden (Stivers, 2004; Moloney, 2013)? Why exactly do we continue to chiefly assume that emotional and mental distress says more about the afflicted individuals than it is saying about the particular contexts against which they might be struggling? What if mental health and illness are indeed produced socially, as the WHO suggested in 2009? Do we have to change our entire treatment focus from individual treatment to *societal cure* – to attempting to change the larger socio-economic systems that surround us? Because of what we noted in the previous section, we infer that qualitative inquiry would probably deliver the more meaningful scientific answers to these issues. There are good reasons to believe that the earlier mentioned notion of 'functionality' will probably be a cardinal theme within this scientific inquiry.

4.3. Towards a Different Kind of Functionality

We argued before that, today, a wide range of interpretations and conduct is viewed as *not functional* in relation to an arbitrarily defined normative conceptualization of how society should be organised and how people should conduct themselves within an ordered community (Verhaeghe, 2002; Dehue, 2008; Moloney, 2013). These interpretations, feelings and this conduct are framed as disorders that need to be cured, mainly because they are not functional

from a neo-liberal meritocratic viewpoint (Verhaeghe, 2012; Moloney, 2013). This could be seen as quite similar to the 19th century moral management practise, as described by Verhaeghe (2002). Moloney (2013) argues that 'the crude method of mental cleansing' (p 155) – referring to a 16th century Flemish painting – was 'so much more direct and honest when compared with what passes for healing of the soul, in the age of psychological science' (p 155), by which he also refers, be it indirectly, to the problem of functionality in relation to normative notions put forward by powerful elites within society. We observed (supra) that there is a perceptible liaison between rising prevalence figures for depression on a global scale and the global neo-liberal socio-economic evolutions and conversions of the past decades where it is implicitly understood that everybody needs to be functional and especially *productive* all the time.

A self-evident consideration that rises from this is the following: do we allow people from time to time to withdraw from these insistent and peremptory requests – that is, to be *not* functional or productive – to be able to cope with a specific situation they are dealing with, without labelling them as 'depressed'? Verhaeghe (2002), Moloney (2013), Frances (2013), van Os (2014), Dehue (2014) and Vanheule (2015) would argue that functionality should be regarded first and foremost – yet not exclusively – in relation to what works for the individual subject. Meaningful scientific answers to questions based on this premise would necessarily result in singular qualitative case constructions and therefor connect much better with the individual needs of the subject consulting with depressive mood. The guiding question within this line of research is the following: 'What is the role and the *function of the symptom* within someone's subjective internal logic?' (Vanheule, 2015). Functionality here refers to an entirely different thing altogether. It seems that todays' medical and even psychological sciences have forgotten about these notions of functionality. Referring to our earlier disquisitions, we infer that the coercive power of the prevailing scientific paradigm, the media and the notions of 'cognitive dissonance' and 'mere-exposure effect' are – at least partially – responsible for this amnesia.

While the ideas and propositions in the final part of this paper are not new, the arguments put forward by all academics mentioned in this section should evoke, in fact, more attention than ever. Indeed, what we hope to have clarified is the following: as time goes by, these arguments are based on ever growing bodies of evidence.

5. Conclusion: Rethinking Depression Globally

In 2012, the WHO predicted that unipolar depressive disorders will be the leading cause of the global burden of disease by 2030. Based on four arguments, we argued that – if we want to deal more effectively with these rising prevalence figures – we might have to rethink the concept of 'depression' and the way we deal with depressive symptomatology. We argued that the diagnostic construct of depression lacks scientific foundation, that psychotherapeutic and pharmacological EBTs are not as firmly 'evidence-based' as their proponents claim and that critical arguments like these are routinely ignored or dismissed by most of the leaders in the clinical field. We further argued that the failure of todays' medical and psychological sciences to respond effectively to these rising prevalence figures necessarily derives from arbitrary presuppositions or choices that are made prior to scientific reasoning and from the way depression is framed in our society by both popular and scientific media.

Rethinking depression globally, we suggest that an international platform under the auspices of the WHO could be created to address this major public health crisis. One of the first goals of such a think tank would be to foster a much broader and *interdisciplinary* scientific dialogue concerning the likely origins of this important public health issue. Indeed, academics from very different fields might have to play a crucial role within this body of experts, since the evidence in favour of – for example – *societal* origins of depressive mood has been growing substantially over the past years. Within this think tank, all existing relevant quantitative and qualitative research concerning depressive mood might be synthesised with the aim, in part, of identifying where more qualitative research might be required. One of the ultimate goals should be to formulate clear *supranational measures* needed to prevent or reduce these societal origins of depressive symptomatology. Indeed, rethinking depression globally, we might need to abandon our faith in treatment, and focus on preventive measures; – which is largely a matter of politics.

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Chapter V

General Discussion: The Twofold Shift – 'Divide and Conquer'

At the beginning of this discussion chapter, we wish to repeat and emphasize that we adhere to the biopsychosocial outlook on mental health introduced by Engel (1977). We have no desire to replace or abandon biomedical and psychological research into depressive disorders. Indeed, our earlier introduction of the 'Principle of Tolerance 2.0' (chapter II) implies that in their thinking and practice in the mental health field, clinicians and researchers should retain the useful aspects of todays' dominant approach; no matter how critical we have been throughout this dissertation; no matter how critical we will remain in this discussion section; and no matter how radically different our approach in this chapter might be conceived. However, we believe that we have made it clear that, over the past decades, too much emphasis has been put on the 'bio' and the 'psycho' of this biopsychosocial model concerning depressive disorders; and with few tangible results in the face of steadily rising prevalence figures. As Dean (2017) argues, too many theorists and researchers have ignored the decisive role of social factors in both the aetiology and course of mental illness. It seems that there is quite a bit of catching up to do.

In chapters III and IV, we discussed some key ideas concerning the expansion of ethical, methodological and social inquiry. In this chapter we will take a more over-arching perspective, based upon our previous arguments – which showed that, in contrast to the reigning biomedical and psychological orthodoxy, there are other legitimate ways to understand the Depression Pandemic. Consequently, we will seek to radically replace some of the established ideas at the

centre of the depression discussion. But before constructing our main twofold argument in this chapter, we will first concretize our own scientific position.

1. Our Own Scientific Position

1.1. Revisiting the Four Juxtapositions

Referring to the four juxtapositions we introduced in chapter II, we wish to clarify that our scientific position throughout this discussion section could be best described as follows.

First, we discussed before that the inadequacy of knowledgeable scientific answers to the omnipresent explicit or implicit ethical questions with which patients confront us, is structurally determined. Consequently, we will further focus on answers with an ethical imprint; answers that reflect individual as well as societal choices. Of course, we will incorporate some knowledgeable (true) scientific facts, but these should be seen as idealizations of rational acceptability; nothing more and nothing less. Second, our interpretation of the reality of the Depression Pandemic stems from a nominalist outlook. We adhere to the idea that there is more than one way to describe the reality of the Depression Pandemic; and that all these descriptions (including ours) are affected by conceptual choices. Third, we will not exclusively focus on the quantitative research approach, where theoretical blueprints that stem from deductive reasoning are used to fit the obtained empirical data in order to shape the reality of the depressed in a specific manner. Fourth, as a consequence of the above, our approach will be a relativist approach. We consider that the veracity of the scientific proposals we will develop in this chapter ultimately lies within society, not within isolated and inviolable acts of measurement or ‘objective’ criteria.

1.2. Revisiting the Societal Role of Scientific Discourse

At the end of our second chapter, we argued that scientific inquiry often produces universal and dogmatic sounding claims about reality to the exclusion of other scientific viewpoints and/or other metaphysical viewpoints. We indicated that, in doing so, todays' scientific activity – as it was within mythical belief systems and major world religions – consolidates and legitimizes the prevailing social order. More specifically, medical and psychological discourse consolidate the way we look at depression, as a medical or psychological illness or disorder; and both these scientific discourses also legitimize the prevailing hierarchy of treatments, the relationship between professional practitioner and patient, the prevailing hierarchy between policy maker and policy consumer (patient). The patient finds her or himself at the very bottom of this social order, as a passive recipient.

Essentially, in what follows, we will reverse this prevailing social order, by putting forward the experience of the disempowered patient as the ultimate compass when it comes to dealing more effectively with the rising prevalence of depression. From this scientific position, subjects presenting depressive symptomatology are – by and large – not 'somatically ill' people, suffering primarily from disordered brain chemicals; or 'psychologically disordered' people because of their faulty styles of thinking. Consequently, these subjects should not be merely treated as patients, in need of professional ministration. Rather, this growing group of people could also be seen as people whose experiences are implicitly (and sometimes explicitly) telling us something important about the way our societies are run: experiences that might, ultimately, contribute to the creation of more humane and compassionate public policies. Fortunately, over the past three decades,

patients (policy consumers) already communicated a lot of information from which critically minded academics have produced a number of well-founded new perspectives from which to view the Depression Pandemic.

2. Restating Our Main Arguments

We will briefly restate our main arguments of the previous chapters in order to be able to, first, answer our guiding research question and, second, introduce and discuss more easily alternative viewpoints and approaches in relation to the rising prevalence of depression.

In chapter I, we mainly argued that the rising prevalence figures for depressive mood are of pandemic proportions and should be considered a global public health priority. We indicated that in regard to the understanding and treatment of depression, there is a clear discrepancy between the steeply rising prevalence figures on the one hand, and the confident scientific claims concerning aetiology, diagnosis and treatment, on the other.

In chapter II, we argued that the dominant neo-positivist scientific approach to deal with these rising prevalence figures rests upon a set of unspoken but questionable epistemological assumptions (e.g. conceptual choices, nominalist viewpoint, a belief in commensurability, …) and that, as an inevitable consequence within the behavioural sciences, the resulting scientific inferences are always in need of interpretative charity. We therefore suggested a need to reinstate 'The Principle of Tolerance': a willingness to entertain varied viewpoints and divergent rational reconstructions concerning this major public health issue. In chapters III and IV, we explored several strands of evidence that suggest that todays' dominant scientific approach to the rising prevalence of depressive disorders is conceptually incoherent.

In chapter III, we proposed a non-standardised way to evaluate meta-analytic effectiveness research concerning the psychotherapeutic treatment of depression. We developed statistical and methodological arguments against the confident claims and inferences that animate this literature. In chapter IV, we added weight to the argument that the science of the field is flawed, by advancing three arguments against the prevailing scientific approach: we showed that the diagnostic construct of depression lacks scientific foundation, and that neither the psychotherapeutic nor pharmacological EBTs for depression are as firmly 'evidence-based' as their proponents claim. We also established that critical arguments like our own are routinely ignored by most of the leaders in the field, and we explained in part why this might be the case.

Essentially, throughout all previous chapters, we argued that the sadness that underlies the diagnostic category of depression is shaped in particular ways by scientific institutionalized discourse: as a psychological disorder, as a brain disease, and above all, as an individual pathology in need of treatment.

3. Answering Our Guiding Research Question

At the end of chapter II, we introduced our guiding research question: 'Does the dominant scientific approach in dealing with depressive disorders offer fruitful perspectives when trying to counter the rising prevalence figures?'. We are now in a position to conclude the following:

There is no evidence that this methodology – which has been maintained for decades – has led to any reduction in the prevalence of depressive disorders or in the symptoms associated with them. If anything, the trends are in the opposite direction. There are few reasons to believe that things

will improve if the science of treatment and prevention continue in their present course. Moreover, we conclude that within this field difficult questions remain unanswered or seem to be avoided. Consequently, we suggest that the leading medical and psychological research institutions have focused too much on the neo-positivist quantitative approach of diagnosis and treatment effectiveness, and are therefore to some degree mismanaging the Depression Pandemic.

If our conclusions sound worrying, we believe that they should not be considered as problematic per se for conventional mental health services, for the regnant academic outlook, for policy makers or the individual patients. As argued before, an admission of uncertainty can open the doorway to the exploration of divergent approaches to fresh thinking about familiar problems.

Referring to Feyerabend (1975), we will begin our discussion with the following premise: if empirical knowledge concerning the rising prevalence of depression (and how to deal with this issue) is to grow, then scientific debate and pluralism are essential. More specifically – and still referring to Feyerabend (1975) – in order to foster this broader outlook, it will be necessary to radically revise some of the received viewpoints and ideas that shape most of the official discourse about ‘depression’.

This proposed replacement of established ideas might constitute a twofold process, comprising, first, what we might call a *diagnostic shift*, as implied in some arguments in the previous chapters; and second, what we might describe as a *policy shift*, in which the conceptual framework includes those who originate the mental health treatment policies as well as upon those who constitute the objects of the latter. We will first clarify the nature of these two shifts, and then become more specific about our alternative propositions.

4. The Twofold Shift

4.1. Introducing The Diagnostic Shift

As mentioned, in 2009, the WHO clearly suggested that mental health is socially produced and is directly and indirectly related at every level to human responses to inequalities. We argued explicitly on numerous occasions throughout the previous chapters and from different scientific angles that depressive disorders (and poor mental health in general) correlate predominantly with virtually all indices of social inequality, of low social rank, of disempowerment. Furthermore, we argued that both psychiatric and clinical psychological scientific discourse seems to have directed attention away from the world that – in all likelihood, contributes to the increasing prevalence of depressive disorders. Moreover, as argued, both of these scientific discourses do not seem to add up to a coherent and workable conceptual scheme as far as the management of this global public health priority is concerned.

Consequently, instead of focussing almost exclusively on diagnosing and treating the depressed (the established idea or approach), we argue that a sustained focus needs to be put on diagnosing the environment. This diagnostic shift is not an entirely new proposition, however, the scientific evidence on which this diagnostic shift could be argued, can no longer be ignored. In what follows, we will revisit some arguments from earlier chapters and substantiate these.

4.2. Diagnosing The Environment: Three Interrelated Cultural Changes

Over the past six years, it struck us how critical inferences coming from entirely different scientific angles – ranging from psychiatry, clinical psychology, developmental psychology, sports psychology, personality psychology, social psychology to different sociological and philosophical

schools – on the societal causation of depressive disorders (and poor mental health in general) seem to converge upon very similar propositions concerning this issue. Of course, we realize that this sense of convergence partly reflects conceptual choices we made while working on our analysis. Nevertheless, the similarity in conclusions is striking. We will first further illustrate this.

We already argued that social inequality, environmental degradation, the intensification (and precariousness) of work, and the shrinking of the public sphere have beset most of Europe and the United States in the last few decades (Smail, 1993, 2005; Melzer et al., 2004; WHO, 2009; Wilkinson & Picket, 2009; Verhaeghe, 2012; Midlands Psychology Group, 2012; Moloney, 2013; Aschoff, 2015; Davies, 2015; Monbiot, 2016; van den Bergh, 2018); especially in those places hit the hardest by the 2008 financial crash, and by all kinds of 'austerity' programmes that many national governments have been forced to implement in its wake (Stuckler & Bisu, 2013). We also argued that many industrialised societies are becoming increasingly prescriptive about the range of feelings, emotions and conduct considered functional or productive and, therefore, normal and acceptable (Verhaeghe, 2002, 2012; May et al., 2004; Horwitz & Wakefield, 2007; Dehue, 2008, 2014; WHO, 2012; Moloney, 2013; Davies, 2015; Rogers & Pilgrim, 2015). Based on the findings of these academics, we established a theoretical and pragmatic link between these societal prescriptions and depressive symptomatology.

We believe it is important at this point in our disquisition to highlight one particular and recently published (high impact factor) article that focuses upon one key operational consequence of todays' prevailing societal ideas and/or prescriptions. Curran & Hill (2017) suggest that because of increasingly demanding social and economic parameters; as a coping mechanism, young people especially are reporting higher levels of perfectionism, characterised by unrealistic standards of

personal and vocational achievement, coupled with a propensity for harsh self-criticism. They argue that this rising perfectionism contributes to the recent epidemics of serious mental illness (Curran & Hill, 2017). Perfectionism is considered here as a multi-dimensional construct in terms of direction: self-oriented perfectionism (directed to the self), socially prescribed perfectionism (perceived to come from others) and other-oriented perfectionism (directed to others). All these dimensions are positively associated with major psychopathology, including clinical depression, chronic anxiety, self-harm, and substance abuse (Curran & Hill, 2017). Socially prescribed perfectionism seems to be the worst influence as far as these higher levels of psychopathology are concerned. Other-oriented perfectionism specifically, is also linked with higher vindictiveness, hostility, the tendency to blame others for personal failure and other setbacks inherent to life; and with lower altruism, compliance with rules and regulations and trust. Moreover, the linear increase of (multi-dimensional) perfectionism remained when controlled for gender and between-country differences.

The internet is a crucible for these trends: across the world, two out of every five minutes spent online are devoted to accessing so called 'social' media platforms – which allow users to create and curate their perfect self-presentation or public image (Global WebIndex, 2016; Curran & Hill, 2017). Since young people today in particular seem to be more demanding of themselves, more demanding of others and perceive that others are more demanding of them (Curran & Hill, 2017) – these 'social' media platforms can have quite perverse 'anti-social' consequences. Rather than connecting people and enabling them to present themselves in a realistic light, widespread usage of these platforms seems to intensify body image concerns and feelings of loneliness and distrust (Grabe et al., 2008; Smink, van Hoeken & Hoek, 2012; Paik & Sanchagrin, 2013; Curran & Hill, 2017). Instead of bringing people together, what these 'social' media platforms predominantly

seem to do is drive people apart and to add their burdens, by promoting the myth that bodily and mental perfection are the norm.

These findings resonate well with the conclusions of Twenge (2014): personality characteristics have considerably changed over time among young people and these reflect not only the immediate social environment, but also the larger overarching social norms at a societal level. Preoccupation with upward social comparison, status possessions, image goods, materialism as a way of perfecting lives; in a broader sense: irrational ideals of the perfectible self, competitiveness and individualism have become the cultural standards to which *all* should aspire while at the same time creating large degrees of uncertainty, inequality, doubt and sadness (Dehue, 2008; Verhaeghe, 2012; Moloney, 2013; Twenge, 2014; Curran& Hill, 2017; van den Bergh, 2018; Wilkinson & Pickett, 2018). As Verhaeghe (2012) and Curran & Hill (2017) explicitly argue – and as argued in other wordings by probably all other critical academics we already mentioned throughout this dissertation – ; ultimately, these changing personality characteristics and subsequent feelings of social alienation and sadness probably mainly reflect three interrelated cultural changes:

(a) the emergence of the neoliberal doctrine and competitive individualism;

(b) the subsequent emergence of meritocracy;

(c) the emergence of anxious and controlling practices.

If these changing personality traits – as a result of these three overarching interrelated cultural changes – are ultimately positively associated with depressive disorders, suicide ideation, ill-being and sadness; then indeed it seems entirely logical and meaningful to consider depressive disorders – first and foremost – as *socially induced* disorders.

As mentioned in our first chapter, van den Bergh (2018) concludes – based on thoughtful reasoning – that depressive symptoms probably tell us more about the late modern subject position, about the societies that we have created over the past three decades, then it is telling us about the individual presenting these symptoms. As the school of SMP (chapter I) has long argued, both the psychiatric and psychological approach of the Depression Pandemic might mistake depressive symptomatology – our troubled thoughts and feelings – for the ultimate causes; thus neglecting to a large extent societal causation.

Referring to chapter II, it now becomes even more tempting to suggest that these scientific narratives concerning the Depression Pandemic are like mythical tales that, with enough repetition, have started to congeal into dogma, and thus blinding researchers and clinicians to fresh ideas and perspectives. Consequently, it seems appropriate to revisit a principal question we asked ourselves in the first chapter: Why – as a society – do we continue to cleave to the biomedical and individualist-psychological narratives when we try to understand and treat depressive mood? We cannot elaborate on the many different perspectives that can be taken to answer this question. Based on all what has been argued throughout this dissertation, we suggest three distinct answers that will better allow us to introduce our alternative proposals:

(a) Defenders of the status quo within the fields of psychiatry and clinical psychology do not seem to fundamentally question their ethical and scientific standpoint in relation to the diagnosis and treatment of depressive disorders. The notions of 'cognitive dissonance', 'the mere exposure effect' and the reassuring messages that come through both scientific and popular media seem to have impeded intellectual openness and scientific debate (chapter IV). Moreover, as argued, today, reason and method are often seen as *one* entity, not as two different human activities (chapter II);

publication pressures are enormous and scientific journals mainly applaud quantitative research that follows mainstream scientific methodology and, consequently, mainstream scientific ideas (chapters III and IV).

(b) These defenders of the status quo within the fields of psychiatry and clinical psychology are in control. It is not in their professional interests to ask too many questions about the bases of what they do; personal and larger economic interests are at stake (chapters III and IV; infra).

(c) Finally, policy-makers are unaware of the magnitude of the problem (pandemic proportions); unaware of the problems with the current approach; and unaware that alternative approaches exist.

This last distinct answer leaves us with the idea of radically moving our focus toward policy makers when dealing with the Depression Pandemic. A policy shift seems inevitable if we wish to deal more effectively with the rising prevalence of depressive disorders; critical thinkers could play a crucial role in this development.

4.3. Introducing The Policy Shift

4.3.1. Established Ideas Place Policies on Rigid Trajectories

We are discussing a fast evolving worldwide pandemic. Policymakers probably play the most defining and pivotal role in how societies deal with pandemics (Baekkeskov, 2016; Piot, Coltart & Atkins, 2017; WHO, 2018). Setting up a comprehensive dialogue with policymakers worldwide seems to be crucial if we want to stop the fast spread of the Depression Pandemic.

Policymaking is always thought to be evidence-based (Baekkeskov, 2016; Piot, Coltart & Atkins, 2017; WHO, 2018). Informed decisions based on scientific evidence are indeed crucial if we want

to deal effectively with the Depression Pandemic and if we want to spend the tax payers' money wisely. However, this scientific evidence can be obtained in different ways, as argued in chapters II and IV. The scientific evidence that is usually considered, largely depends on prevailing societal ideas (e.g. conceptual choices) and the dominant scientific paradigm (chapter II); but also on the resources that are required, the resources that are available and the timeframe in which the policymaker needs to take a decision (WHO, 2018). We indicated that dealing with the Depression Pandemic today is based on highly debatable, but nevertheless 'established' medical and psychological ideas. These established ideas spawn policies that focus, myopically, upon achieving the 'right diagnoses' and upon 'closing the treatment gap'.

In chapter IV, we argued that both popular and scientific media, the 'mere-exposure effect' and the notion of 'cognitive dissonance' play a crucial role in the expansion of todays' scientific positions and associated policy trajectories. Throughout the previous chapters, we questioned the veracity of these scientific positions and their associated research findings. As Baekkeskov (2016) argues, uncertainty nearly always brings about differences in expert judgments and subsequently in policy judgments, but accruing information nearly always pushes to consensus. Baekkeskov (2016) suggests that countervailing new information, even though usually first ignored, opens the possibility for policy shift when this information becomes overwhelming. We clarified that the evidence on which our arguments were based could open a gateway to widened methodological, ethical and societal inquiry; to the formation of new ideas and insights that could result in a significant policy shift.

4.3.2. Replacing Established Ideas at the Centre of the Depression Discussion

As it is, we believe that in the foreseeable future, a policy shift cannot take place concerning the Depression Pandemic. Few policymakers seem to know about the pandemic proportions of the problem discussed here; about the problems we discussed concerning the prevailing scientific standpoint when it comes to the effort to tackle this problem; nor about alternative perspectives. Referring to Berman (2012) and his disquisition on ideational scholarship, we suggest that the way forward for an ideational policy shift (the replacement of established ideas) must include the presentation of clear and easily agreed upon ideas to policy makers. In an attempt to replace the established ideas at the centre of the depression discussion and to provide solid arguments for a policy shift, we will further develop three arguments.

First, we believe we must discuss one more aspect of todays' medical approach toward depressive mood, which we have not addressed yet: genetic studies and neuroimaging. In doing so, we will add more scientific weight to our central thesis that there seems to be little evidence that todays' established ideas have led to any significant advances in dealing with the Depression Pandemic. Second, we will argue that no one should underestimate today's gigantic costs of the Depression Pandemic and that the policy shift we will propose could be – from a purely economic point of view – a very wise decision. Third, we will propose some well-founded ideas that would enable policy makers to more easily introduce effective measures for dealing with the Depression Pandemic.

5. A Clear and Easily Agreed Upon Idea: 'A Collective Medical Failure'

If our critical observations developed in the previous chapters sound extremely worrying and create large degrees of uncertainty, the reader will no doubt have noticed that – so far – we only have treated the biomedical field within clinical psychiatry; we have not yet discussed the more recent promising developments within the medical model: genetic research and neuro-imaging. Both these scientific projects aim at diagnostic specificity of mental disorders (Dean, 2017). Unfortunately, when critically examining these branches concerning depressive disorders, the picture becomes even more worrying.

The Human Genome Project (HGP) and the associated Genome-Wide Association Studies (GWAS) appear to have attained some success in prediction and treatment within areas like oncology. In the mental health treatment field by contrast, this research has found no obvious clinical application (Dean, 2017). More specifically, a mega-analysis of GWAS in major depression – the largest genome-wide analysis of MDD yet conducted – failed to identify solid and replicable findings (Major Depressive Disorder Working Group of the Psychiatric Disorder GWAS Consortium, 2013). The researchers downplayed this failure with the argument that – given the high prevalence of MDD – their sample was too small to detect genetic effects typical for complex traits. While we have great respect for this kind of methodological rigour (e.g. our disquisition on statistical power in chapter III), we suggest that there might be other reasons why no solid and replicable findings were identified. First, as Dean (2017) argues, these GWAS with their focus on diagnostic specificity, seem to entirely neglect the research evidence and clinical evidence for pervasive comorbidity. Second, and still referring to Dean (2017), for the thousands of subjects in the GWAS, the potentially confounding effects of differences in socioeconomic

status and life experience – including trauma – have been largely overlooked: notwithstanding that epigenetic studies have shown that epigenetic change (i.e. systematic differences in gene expression) are a regular and natural consequence of the action of these (and many other) environmental variables upon the human organism. Third, as we argued in chapter III and IV, these GWAS are flawed right from the start, since the diagnostic category is clinically and scientifically dubious. Ironically, Dean (2017) concludes that, on the one hand, GWAS have undercut the very idea of disease specificity through their findings of significant overlap across multiple mental disorders and, on the other hand; that these studies – for a number of reasons – fail to reflect reality.

Neuroimaging or brain imaging (e.g. CT scans, fMRI, Diffusion Tensor Imaging) announced itself as another technique to improve the specificity of mental disorders. In this field, one very recent high impact factor journal publication, caught our attention (Müller et al., 2017). This neuroimaging research synthesis with regard to depression included 59 studies conducted over the past 15 years. This meta-analytic study sought to identify aberrant patterns of brain activation in subjects suffering from unipolar depressive disorder, but found no statistically significant evidence of convergence across the studies involved. Moreover, these findings were *not* attributable to a lack of statistical power. However, what caught our attention even more, was the 'Conclusions and Relevance' section of this study: 'Inconsistencies exist across individual experiments investigating aberrant brain activity in Unipolar Depression and replication problems across previous neuroimaging meta-analyses. For individual experiments, these inconsistencies may relate to use of uncorrected inference procedures, differences in experimental design and contrasts, or heterogeneous clinical populations; meta-analytically, differences may be attributable to varying inclusion and exclusion criteria or rather liberal statistical inference approaches.' (Müller et al., 2017, p 1). Referring to chapter II, this conclusion reflects indeed an explicit *realist* scientific

position; referring to chapter IV, this conclusion almost perfectly reflects the idea of 'cognitive dissonance'. The authors seem to strongly suggest that the absence of statistically significant evidence of convergence across the studies is mainly due to all kinds of methodological problems and seem to be convinced that – if these methodological issues could be resolved – convergence across studies will be found. While all these assumptions might – someday – prove to be accurate, we vigorously recommend – at this point and time – not to take these assumptions too seriously.

Dean (2017) infers that genetic studies and imaging studies have – beyond doubt – enlarged our understanding of the brain structure, the delineation of brain circuits and so forth; but have 'provided little evidence of support for the specificity of disease and treatment, or for clinically useful biomarkers' (Dean, 2017, p 4). Contrary to what is thought by policy makers, so far, the biomedical approach, the genetic project and the neuroimaging endeavour concerning the Depression Pandemic seem to have resulted in 'a collective medical failure' (Dean, 2017, p 9). Nonetheless, new types of anti-depressant drugs are approved by official regulatory agencies on a regular basis, while governments allocate massive budgets to genetic studies and neuroimaging research (Dehue, 2008; Kirsch, 2009; Moncrieff, 2009; Goldacre, 2012; Healy, 2012; Dean, 2017).

6. *Another Clear and Easily Agreed Upon Idea: 'It's the Economy, Stupid!'*

6.1. The Issue of the Two Tables

In chapter I, we indicated that, today, societies – mainly in the developed world – have increasingly embraced the notion that numbers speak for themselves. We were specifically referring to the rise of the 'new public management' and of market driven ideas within the public health sector in which financial cost becomes the yardstick of everything (Dehue, 2008; Verhaeghe 2012; Moloney,

2013). This being the case, we decided that we might as well use this rather unfortunate evolution as one yardstick by which to judge our proposal for a shift in public mental health policy. Indeed, governments and health planners in many nations might be surprised at the real financial costs of the Depression Pandemic and of todays' scientific approach regarding the Depression Pandemic (infra). Or perhaps, they are well aware of these costs, but fail to realize that the current approaches based on drugs and therapies are likely to prove very costly in the long run, since they might never going to be very effective (supra).

The well-known catch phrase... 'It's the economy, stupid!'… was the keynote of Bill Clintons' successful presidential campaign in 1992. This phrase has remained on the lips of the many politicians who seek to win office by convincing their voters that the question of the 'health of the economy' is the fundamental issue. Bregman (2014) argues that the notions 'Gross Domestic Product' (GDP) and 'Economic Growth' have indeed become the twin tracks upon which nearly all electoral campaigns run. As just one indication of these trends, over the last thirty years, we have moved steadily into an era in which public service work – for instance education, administration and health care – is increasingly disparaged by governments, whose fiscal and employment policies favour the world of commerce (Verhaeghe, 2012; Moloney, 2013; Bregman, 2014). The less one contributes to the GDP, then the less important one becomes as a person (Bregman, 2014). It is difficult to overestimate the importance of the notions of 'GDP' and of 'economic growth' or the dominant role that economists play in todays' societies in general and in policy-making circles specifically (Entman, 1989; Bourdieu, 1998; Davies, 2009; Verhaeghe, 2012; Piketty, 2013; Bregman, 2014; van den Bergh, 2018).

Since the latter are extremely sensitive to economic reasoning (Piketty, 2013; Bregman, 2014; Baekkeskov, 2016; WHO, 2018), then it is surely important when addressing this group of policy-

makers to confront them with the exact same phrase… 'It's the economy, stupid!'; even – or especially – in regard to the origins of the Depression Pandemic. To be precise: economic growth and GDP depend on labour and capital (Piketty, 2013; Bregman, 2014), *both* of which can be impaired by disease (Trautmann et al., 2016). As we will show, dealing with the Depression Pandemic via the lens of societal causation and prevention could save hundreds of billions of dollars annually, possibly a few trillions of dollars.

We very much wanted to present two tables at this point and time. Table 1 would have displayed the estimated economic burden of depressive disorders today, with a focus mainly on diagnostic procedures and treatment through EBTs and reactivation. Table 2 would have presented the estimated economic burden of dealing with the depressive disorders, focusing mainly upon prevention through societal measures; our overall aim being to show that the cost of these societal changes and/or preventive measures would, ultimately, be far less than the cost of todays' approaches. While – after studying this issue for many years – we will later argue that this would be the case; having spent weeks trying to compose Tables 1 and 2, we finally decided not to integrate the official figures because these figures are not reliable or accurate: an issue that has probably been best described by Baxter et al. (2013). If we want others to take our methodological critiques seriously (chapter II, III, IV), then we too should listen carefully when experts on prevalence estimation expose the limitations of the global epidemiological data for mental disorders.

6.2. The Problem with Prevalence Studies

In 2013, Baxter et al. reviewed global epidemiological data for mental disorders. Astonishingly,

they infer that of the 77,000 data sources identified, fewer than 1% could be used for deriving national estimates of prevalence. In their compendious and painstaking review, Baxter et al. (2013, p 1) argue that this body of research suffers from two major flaws: '(1) highly variable regional coverage, and (2) important methodological issues that prevented synthesis across studies, including the use of varying case definitions, the selection of samples not allowing generalization, lack of standardized indicators, and incomplete reporting'. Baxter et al. (2013) conclude that for most of the world's population mental disorders will remain invisible to governments and to the relevant health care agencies; and a low priority compared to other major global health agendas.

These conclusions have two far-reaching consequences. On the one hand, the alarming prevalence figures for mental health disorders – and in particular for depressive disorders – are most likely serious underestimations; which means, on the other hand, that – consequently – all numbers concerning the estimated economic burden of depressive disorders are also serious underestimations because these are – at least partially – calculated on the basis of prevalence numbers. Hence, our reluctance to put the numbers concerning annual cost in a table in a scientific publication; the numbers do not reflect reality. Nonetheless, we need to consider some of these numbers and comment on them in order to get a sense of the magnitude of the global estimated economic burden of the Depression Pandemic.

6.3. The 'Older' Estimations: US$ 800 billion Annually and Much More

Given the issue of a serious underestimation of prevalence and – consequently – a serious underestimation of the economic burden involved, it probably does not take a person with a master's degree in economics to see where we are going with this. Referring to Berman (2012),

here is a clear and easily agreed upon idea: the cost of human sadness is gigantic. We selected five different issues (and associated numbers) to deliver proof for this idea.

First, The Executive Board of the WHO (2011) suggested that the global cost of mental disorders in terms of lost economic output over the next 20 years will be US$ 16 000 billion; that is US$ 800 billion annually.

Our comment:

Which part exactly of this vast amount of money could be seen as a consequence of depressive disorders, seems impossible to find out. However, since depressive disorders are considered to be – by far – the most prevalent and invalidating mental health problem worldwide (chapter I), we assume that the lion's share of this lost economic output is caused by depression. Moreover, given our earlier remarks in the previous section (serious underestimation of prevalence of mental disorders); we assume that the real loss in economic output due to human sadness is far more important.

Second, we observed that every year US$ 19 billion are spent on a wide variety of antidepressants, indicating the unwarranted rise of the medical treatment of depression (Kirsch, 2009; chapter IV). This was 9 years ago.

Our comment:

We indicated that the Depression Pandemic seems to have spread across the globe at a fast rate: + 18 % worldwide between 2005-2015 (WHO, 2017). This increase seems to reflect two different tendencies. First, since 2005, more countries across the world have started to witness the prevalence of depression and second; the higher prevalence figures for depressive disorders in

countries that already reported on depressive disorders in 2005. Consequently, we assume that the total expenditure on a variety of antidepressants is far more important today. Given all we discussed so far concerning the mythical belief that these 'anti-depressants' are effective, we assume that this market has grown considerably over the past decade and will continue to do so, but have not found reliable data concerning this unfortunate event. Moreover, there is an even more worrying tendency at hand today: recent studies show that nearly 50% of prescriptions for anti-depressants are written for off-label use and that this regrettable state of affairs is on the rise (Dean, 2017); prompting the idea that the US$ 19 billion a year is most probably a serious underestimation.

Third, the WHO (2011) estimated that median annual mental health expenditures (prevention and treatment) per person range from US$ 0.20 in low-income countries to US$ 44.84 in high-income countries. Globally, annual spending on mental health is less than US$ 2 per person. This relatively low amount per person, still represents over US$ 15 billion a year globally.

Our comment:

Even though prevention and treatment are genuinely less expensive in low-income countries (WHO, 2011), *if* people in low-income countries could receive the same kind of preventive measures and treatment as people in high-income countries, this amount of US$ 15 billion a year globally, would obviously be – again – a serious underestimation. Even more so, since Baxter et al. (2013) suggest that for most of the world's population (e.g. low-income countries) mental disorders are – by and large – still invisible to clinicians, researchers, to the sufferers themselves who might perhaps not regard themselves as depressed (as opposed to sad); and consequently, to policy makers. As discussed, the prevalence numbers are most probably much higher and thus the

cost of treatment and preventive measures would be much higher if all people on this planet were treated equally.

Fourth, in the United States alone, The Human Brain Initiative will cost $ 4.5 billion over the next decade (Markoff & Gorman, 2013); in Europe US$ 1.3 billion is being invested in the Human Brain Project (Abbott & Schiermeir, 2013) and, worldwide, tens of billions US$ are invested annually in genetic research, neuroimaging studies, 'neural circuit detection' and 'computational psychiatry' (Dean, 2017).

Our comment:

We have already argued that these studies have little to show after decades of effort: both for the diagnosis of mental disorder and for the attempt to map clinical symptoms onto specific neural circuits. According to the critical psychiatrist Dean (2017), these projects are alike in their tendency to ignore three crucial questions: (1) What about the 'remarkable plasticity and biological heterogeneity of neural circuits'?; (2) 'How does one develop precision medicines aimed at neural circuits that are subject to constant changes, stemming from mutations, stress and infections' [not to speak of all important social experience]?; (3) 'How can a single drug therapeutically alter a circuit that no doubt will be comprised of tens of millions of neurons and billions of synapses'? (p 8). In a noteworthy publication in *Science*, Xie (2014) points out that the highly selective allocation of huge amounts of money to what could be seen as quite narrow fields of research must raise suspicions about inequalities in the funding and practice of science. As Dean (2017) further substantiates, this scientific inequality is salient in the current approaches to mental health issues in general and toward the Depression Pandemic in particular; while research into socioeconomic adversity and its significant effects on mental wellbeing seems to be entirely underfunded.

Finally, there is the issue of the high mortality rates. People suffering from major depression have an overall increased risk of mortality of 1.4 times greater than that of the general population (WHO, 2017). This is not only because there is important comorbidity between major depression and physical health problems, but, more importantly, because of the high suicide rates among the depressed. It is estimated that, in the year 2015, 788 000 people died by suicide, and that many more attempted it (WHO, 2017). For comparison, it is generally assumed that the 7 year Syrian war resulted in a death toll of 350 000 to 450 000 people; that is, 50 to 65 000 people annually. Without denying the horror of this toll, it represents less than a tenth of the 5.5 million people who died due to suicide in the same time period. Although impossible to find out precisely, it is assumed that the vast majority of these deaths and attempts could be attributed to the distressing and pervasive mood states associated with 'depression', such as sadness, helplessness and hopelessness.

Our comment:

One reflection will suffice here: no one can put a price on a human life; which makes this particular dreadful issue by far the most important cost.

6.4. The 'Recent' Estimations: US$ 8.5 Trillion Annually in 2010, US$ 16.1 Trillion by 2030

As argued, we believe it is impossible to put an exact number on the economic burden of the Depression Pandemic. So far, the five different numbers (and our comments) as well as other costs we mentioned throughout this dissertation, merely underline the extremely high costs involved in dealing with depression today. However, if the above-mentioned numbers sound alarming, it is important to realize the following: they fade away when focussing on more recent estimations.

Based on data from 2010, Trautmann et al. (2016) cleverly distinguish between the global *direct* or visible costs of mental disorders (e.g. medication, physician visits, psychotherapy) and the global *indirect* or invisible economic costs of mental disorders (e.g. income losses due to mortality, disability, early retirement). Using this traditional and easily understood Human Capital Approach, Trautmann et al. (2016) estimate the global economic burden of mental health at US$ 2.5 trillion annually; whereby the indirect costs (US$ 1.7 trillion) are much higher than the direct costs (US$ 0.8 trillion), which contrasts with other key disease groups, such as cardiovascular diseases and cancer, where direct costs are far more important than indirect costs. Generally, mental disorders account for more economic costs than chronic somatic diseases such as cancer or diabetes. Surprisingly, according to these researchers, societies are willing to spend much more on somatic diseases than upon mental disorders.

However, using the Value of Statistical Life (VSL) approach, the numbers become nearly unimaginable. Admittedly, the approach is debatable; all estimation methods for VSL have their weaknesses, but there is a growing academic consensus that – when applied properly – the VSL should be used in cost-benefit analysis to evaluate the efficiency of government policies to reduce risk (Bosworth et al., 2017). The main strength of this approach seems to be that it includes the substantial costs of government health policies that are not immediately obvious. According to Trautman et al. (2016), the VSL approach 'assumes that trade-offs between risks and money can be used to quantify the risk of disability or death associated with mental disorders. This quantification analyzes observed trade-offs or hypothetical preferences, such as data acquired from surveys that ask people how much they would be willing to pay to avoid a particular risk, or how much money they would need to take on that risk'. Consequently, the VSL approach not only accounts for the more obvious direct and indirect economic costs of mental disorders, but also for

costs that people associate with disability and suffering. Using the VSL approach, the global economic burden of mental disorders was estimated at US$ 8.5 trillion in 2010. This estimate is comparable to that for cardiovascular diseases and higher than that for cancer, chronic respiratory diseases, and diabetes. Substantial as it is, this economic burden is expected to almost double by 2030, reaching US$ 16.1 trillion.

In summary, here is indeed another clear and easily agreed upon idea for policy makers. Todays' mental health policies worldwide might not be very effective, but they are certainly expensive: via direct costs in the health care system, and indirectly; via proportionally high productivity losses and an extremely high impact on economic growth; and via trade-offs between risks and money to quantify the risk of disability or death associated with mental disorders. Which part exactly of these unimaginable numbers could be associated with the Depression Pandemic, seems impossible to find out. However, since depressive disorders are considered to be the most prevalent and invalidating mental disorders worldwide (chapter I), again, we logically assume that the lion's share of these costs is caused by depression. Of course, referring to our adherence to the biopsychosocial model of Engel (1977), a policy shift oriented toward the societal causation of depression would not entirely erase these costs, some people might still want or need and greatly benefit from a medical or psychological treatment.

Essentially however, we will further argue two things: a policy shift toward social prevention through societal change, would not only be far more effective, it would also reduce considerably the global economic burden of depressive disorders. Because of a constant preoccupation with cost-benefit models, the latter could draw the attention of policy makers; the first argument could draw the attention of everyone, so we hope.

7. Ideational Policy Logic: New Ideas Could Lead to a Policy Shift

We believe that we have presented well-founded methodological and ethical evidence to suggest that perhaps many ideas that underpin theory, research, treatment and above all – policy – in the management of depressive disorders are no longer tenable or justifiable. Meanwhile, according to the VSL approach, the economic burden of these disorders mounts up to a few trillions of US dollars annually. In what follows, we are not suggesting that all of the existing policy measures should be dispensed with. While it might be helpful to radically replace some of them, others could be modified and integrated with ideas and methods that originate from recent critical research within the mental health field.

Accordingly, we propose a shift in the focus of health and social policy away from treatment and toward prevention: a process best conceptualised as 'societal change'. In what follows, our propositions are based upon a large and diverse body of scientific and clinical evidence. Taken as a whole, this suggests that, contrary to the core assumptions that underpin the governance of neoliberal societies and indeed most psychological therapies – human beings are not entirely free and self-creating agents, capable of overcoming any personal problem with the help of a therapist or medication. Rather, their feelings, thoughts and conduct are indelibly shaped by their social and material world, and it is to this world that we must look, if we are to achieve any lasting relief of their distress (Dehue, 2008; Wilkinson & Pickett, 2009, 2018; Verhaeghe, 2012; Moloney, 2013; Dean, 2017).

To have any hope of reaching this aim, we will need an interdisciplinary approach, one that enables researchers and policy makers to gather around the table in pursuit of a renewed scientific debate

and, we suggest – of the construction and implementation of a national RCT that takes the likely environmental roots of depression seriously. It may be necessary to abandon the styles of argument that prevented progress in this clinical field: ad hominem arguments, authority arguments, everyone-does-it fallacies, straw man fallacies, circular reasoning, and the use of false contradictions, to name but a few.

7.1. Two Guiding Premises

Earlier in this chapter, we have argued that the rising prevalence of depressive disorder and of feelings of sadness and unease more generally are linked with three overarching and interrelated cultural trends that have grown in saliency over the last thirty years: the emergence of the neoliberal (or 'ultraliberal', van den Bergh, 2018) doctrine of competitive individualism; the growth of an officially 'meritocratic' society; and the emergence of anxious and controlling practices. All these cultural changes led to high levels of social and economic inequality (Dehue, 2008; Wilkinson & Picket, 2009; Verhaeghe, 2012; Piketty, 2013; Twenge, 2014; Curran & Hill, 2017; Dean, 2017; van den Bergh, 2018). We argued before that there is a close relation between these levels of inequality and the prevalence of depression and that socioeconomic adversity significantly increases the risk of depressive disorders; in what follows, we will add what we consider to be inescapable evidence for this idea.

We believe – within the context of this dissertation – it would not be meaningful to suggest that these three interrelated cultural changes can be stopped or slowed down altogether. However, we do suggest that it seems meaningful to propose some adjustments to these cultural changes based on two premises; we will further (1) consider depressive disorders – first and foremost – as socially

induced disorders; and (2) we will exclusively propose policy measures of a societal nature that might lead to the creation of more egalitarian societies. Our ambitious aim is to reduce the prevalence of depression and the economic burden that it imposes upon our society.

Surprisingly however, as we will discover, through our proposal, not just the reported rates of depression might improve; many different health issues and social problems might benefit greatly from the implementation of the policies that we will propose.

7.2. Step I: An Assignment for the Academic Community

We inferred on different occasions that over the past decades, the global academic community has too often been in thrall to the dominant scientific paradigm and has thereby helped – albeit indirectly and unintentionally – to foster the rising prevalence of depressive disorder. It has done this by locking its gaze upon a rather narrow range of psychiatric and clinical psychological theories; most of which locate both 'the problem' and its solution within the body and mind of each individual sufferer. In consequence and if progress is to be made, then we suggest that the community of critical academics need to communicate persistently the following three issues to the widest possible audience.

(1.) The conventional framing of depression as an individual illness – whether as a product of faulty brain chemicals or of aberrant styles of thought and behaviour – is not built upon solid science. Rather, it is largely a product of the coercive nature of the dominant scientific paradigm based on a dominant societal discourse. Indeed, there is no evidence that the illness model of depression has lessened the prevalence of this disorder. Across the world, and especially in the 'developed' countries, poor mental health correlates not just with want of money in comparison to

citizens who are better off, but with virtually all indices of disempowerment and of low social rank; WHO (2009) clearly states that mental health and disorder are socially produced. More specifically, they constitute a set of lived human responses – emotionally, bodily, and cognitively – to the experience of living in an unequal society; and these responses can be fatal: every year, across the world, nearly 800 000 people die from suicide.

(2.) Depressive disorders are already the single largest contributor to non-fatal health loss or disability globally and are projected to be the leading contributor to the GBD by 2030. The true economic burden of mental disorders could be best expressed through the VSL approach and this approach indicates that the economic burden of mental illness was US$ 8.5 trillion in 2010 and is expected to rise to US$ 16.1 trillion annually by 2030. A large proportion – impossible to determine how much exactly – could be attributed to depressive disorders.

(3.) All policy measures aiming at the fight against poverty and social and economic inequality, will significantly reduce prevalence figures for depression; statistical modelling suggests that the net returns on these investments could be enormous; spending money on the prevention of mental illness is a sustainable investment (infra).

7.3. Step II: More Social and Economic Equality: The Answer to (Almost) Everything

As mentioned, we wish to consider the phrase 'It's the economy, stupid!' as a bi-directional construct. Policy makers seem to refer to this phrase toward policy consumers to defend their proposals. Likewise, academics and policy consumers should refer to this same phrase when formulating ideas toward policy makers. Given the fact that market driven ideas steer policy making, we believe it is of extreme importance to consider the following.

In his book *Capital in the Twenty-First Century* (2014), Thomas Piketty principally argues that, as the rate of return on capital is greater than the rate of economic growth, we are rapidly evolving toward societies where there is a concentration of wealth or an unequal distribution of wealth, leading to high degrees of economic and social inequality. Piketty's analysis received much appraisal, but also fierce criticism concerning his assumptions, methodology and the normative content of his ideas. However, most of these critiques could be considered ad hominem arguments ('essential thinker of the left', Gissurarson, 2014), straw man fallacies (a simplistic representation of Piketty's ideas; Leef, 2014), allegations of poor quality data – shown since to be spurious (Giles, 2014), and dubious assumptions that public sector privatisation makes for greater 'efficiency' (Leef, 2014). A fairer critique, however, is that Piketty places economic and social inequality as the fulcrum of his analysis, but without clarifying or demonstrating why this should be so (Wolf, 2014; Crook, 2014). In this dissertation we have already explained why – from our perspective – imbalances of wealth and power within a society are key. However, as we approach the end of this work, we are in a position to take stock of all of the evidence gathered and the many arguments reviewed, and in doing so we can put the matter more forcefully: inequality is fundamental when it comes to the mental health both of populations and of individuals.

In their wide ranging and meticulous analysis, the British epidemiologists Richard Wilkinson & Kate Pickett (2009) integrate figures from 20 developed nations and 50 American states, to conclude that – despite their overall affluence – modern societies are best conceptualized as social failures. These researchers show that no less than 10 major health issues and social problems faithfully track the extent of social and economic disparity; and, as indicated before, the rate of mental illness is one of these. The more inequality within a society, then the more health and social problems it is likely to manifest. Reduced life-expectancy and social mobility, elevated rates of

infant mortality, mental illness, obesity, early school-leaving, teenage birth, imprisonment, homicide, and lack of trust: all of these issues and more are significantly associated with income inequality. Wilkinson & Pickett (2009) identified the USA, Portugal, England, Australia and New Zealand (in this order), as the five countries where such disparities are highest. These five countries also scored most on the Index of Health and Social Problems – a device that integrates the ten key social and economic determinants of health and wellbeing described above (Wilkinson & Pickett, 2009). On the other hand, Japan, Finland, Norway, Sweden, Denmark (in this order) emerged as the five countries where income inequality was the lowest; and these countries, correspondingly, scored significantly lower on this index of Health and Social Problems. Wilkinson & Pickett's findings were both astonishing and the source of considerable controversy. However, as mentioned in chapter IV, over the past years, no less than 200 studies offered support for the predictive quality of the Income Inequality Hypothesis (IIH, De Maio, 2014). From our perspective, there are two interconnected observations that can be derived here.

First, the act of trying to tackle income inequality by shifting capital towards those who need it most should not only significantly lower the prevalence figures for depression; it might also significantly reduce the rates of the other nine variables. Second, this is because the degree of national inequality in personal incomes can be used to closely predict the scores reflecting the level of all these health and social problems; the latter being the essence of the IIH.

Indeed, Wilkinson and Pickett (2009) make a *causal argument*, based on the idea that social inequalities do their damage by working their way under our skin, to the extent that humans have an evolved tendency, as social primates, to be sensitive to their own and others position in the social pecking order. More unequal societies encourage us toward competitive striving and envy, and they therefore engage our 'fight and flight' physiology and foster the psychological states that

go with its activation (anxiety, mistrust, low mood). For individual adults with mental health problems, Wilkinson and Pickett (2009) argue for three key groups of causal factors, all of which will reflect the person's social position either directly or indirectly: vulnerabilities acquired in childhood (poor emotional attachments – in other words the effects of neglectful or abusive care givers); low social affiliation (isolation and loneliness); and finally low social status whether defined by wealth and/or badges of prestige. From within the mental health field, Smail (1987, 1993, 2005), Verhaeghe (2012), Moloney (2013), Dean (2017) and van den Bergh (2018) – among many other academics (supra) – came to very similar conclusions about the societal causation of mental health problems in general and depressive mood in particular.

Although the former paragraph summarises the main theme of this chapter, there is another fascinating research finding concerning depressive disorders that could be added here: namely, that even well-off people suffer some ill effects where inequality is high. Flavin et al. (2014, p 1) conclude the following: 'We find robust evidence that citizens find life more satisfying as the degree of government intervention in the economy increases. We find, further, that this result is inelastic to changes in income; that is, high- and low-income citizens appear to find more 'leftist' social policies equally conducive to their subjective well-being'. Inequality is strongly associated to higher prevalence of depressive disorders, not only within the subgroup of low-income citizens, but also within the subgroup of high-income citizens. These results suggest that government policies that promote income equality can have lasting positive effects on the well-being of *all* citizens.

Furthermore, there is another important issue we need to address here. Today, many of the 'old and typical DSM symptoms for depressive disorders' are conceptualized as 'transdiagnostic symptoms' – which, as the phrase suggests – are acknowledged to be common in many supposedly distinct or

categorical psychiatric diagnoses. For instance: low mood, insomnia or hypersomnia, perfectionism and harsh self-criticism, hopelessness, compulsive behaviour, loss of energy and suicidal ideation (van Heycop ten Ham et al., 2014). In the light of Wilkinson & Pickett's analysis, this observation suggests that many of the mental health problems grouped by clinicians under headings other than 'depression' could likewise be ameliorated; by policy measures designed to make a given society more egalitarian.

8. *Bringing Our Discussion Closer to Home*

At the end of this discussion chapter, we wish to bring the debate closer to home. Ultimately, in what follows, we will suggest a quite daring research proposal. Before we do so, we need to briefly outline the Belgian context. On a whole, Belgium is doing reasonably well in Wilkinson & Pickett's (2009) analysis, so it seems.

Belgium scores 6th on the variable 'lowest income inequality' and scores 5th best on the Index of Health and Social Problems (Wilkinson & Pickett, 2009). The current Belgian political authorities – supported by popular media outlets – have referred on different occasions to a recent study of Marx & Van Cant (2018) to underline the strength of today's social and economic equality policies; a study that suggests that social and economic inequality in Belgium has indeed remained stable since the beginning of the 21st century. A first observation that needs to be made here, is the following: it is intellectually unfair that current policy makers take credit for this 'stable inequality', since this study refers to a time period (2000 - 2014) where the current government was not in charge. More importantly however, some of the main arguments of this comprehensive study are of a quite different nature. First, ever since the large increase of inequality between the

late seventies and the late nineties of the 20th century, inequality has not dropped either since 2000. Second, Marx & Van Cant (2018) argue that the containment of inequality in Belgium since 2000 should predominantly be seen as a result of the robust and 'typically Belgian' social dialogue model (between government and social partners) and that, therefore, 'the Belgian experience provides a powerful antidote to views that growing inequalities are inevitable in advanced economies'. Third and probably most importantly, Marx & Van Cant (2018) suggest that, unfortunately, this social consultation model has been undergoing significant erosion since the new national and regional governments came into place in 2014. Consequently, Marx & Van Cant (2018) argue that this 'stable inequality' might soon prove to be a precarious notion.

Moreover, mental illnesses – and depressive disorders in particular – are, like everywhere on the planet, on the rise in Belgium (WHO, 2009; 2011; 2012; 2017) and like anywhere else, the government policies of the past decades do not seem to be able to counter this unfortunate tendency. Furthermore, in 2016, Belgium ranked as number 7 (out of 50 European countries) as far as suicide mortality rates are concerned: 20.7 per 100 000 population (WHO, 2018).

Subsequently, this final reflective section probably needs to start by putting forward the following question concerning depressive mood: if the compelling evidence and critiques of the past forty years we discussed throughout this dissertation, does not seem to be able to shift certain positions concerning causation, treatment and policy measures – then what do we need to do? Clearly, as argued, mental health practitioners, academics and policy makers are not very receptive to ideas that conflict with their interests, with the prevailing myths of our culture (i.e. that we are all autonomous, self-creating individuals) and/or the prevailing scientific approach. Accordingly, we believe it would be extremely difficult – and probably undesirable – to ignore the prevailing scientific paradigm and so, in an attempt to counter the prevalence figures for depression; we will

make a bold suggestion that fits well with today's generally accepted scientific and cultural views. We suggested in this chapter that any idea for a policy shift concerning the matters discussed here needs to have broad approval (Berman, 2012; Baekkeskov, 2016; Piot, Coltart & Atkins, 2017; WHO, 2018). This kind of approval can probably be best obtained through scientific evidence. We assume that what we have discussed so far, might help to convince policy makers, researchers and even the population at large of what follows.

It might be possible to obtain even stronger support or even something close to conclusive proof for the hypothesis that more equal societies are significantly associated with lower prevalence figures for depression and other mental illnesses and could thus achieve large savings in health and social care expenditure (Wilkinson & Pickett, 2009). One way to do this might be through the experimental method: establishing a rigorous longitudinal RCT with high statistical power. An experimental group of low-income citizens would be helped mainly financially throughout a period of (at least) two years, while a control group of low-income citizens would receive 'treatment as usual' (the existing policy measures). Outcome variables would be the 10 variables highlighted in Wilkinson & Pickett's' Index of Health and Social Problems; these could be measured at different intervals. As indicated before, we believe that researchers from different scientific disciplines (psychology, economy, sociology, …) and experts in policy making should be brought together to decide upon the specific methodology and statistical analysis used; and upon the specific content of the financial measures that would be taken within the experimental group.

Science advances by refutation of the null hypothesis and such nationwide longitudinal RCT – further examining the causal relationship between social and economic inequality and (among others) depressive disorders – can do just that and bolster support for the IIH. We suggest that the

public at large should be informed that – in the present case – the IIH predicts that the null hypothesis (no difference between experimental and control group) *would* be refuted and that, consequently, tens (possibly hundreds) of millions of euros of tax payers money could be saved in Belgium. Accordingly, we believe that, the public at large should be allowed to follow this research, allowed to ask questions about it and also allowed to be involved in further decision making based on final results.

We do realize that thinkers like William Epstein (2013, 2017) – a long-time observer and critic of social research of this kind – would probably argue that this is impossible in practice, because researchers cannot take whole sections of a society and isolate them like samples in a petri dish. Also, the basic knowledge that one section of a society is being treated differently or 'preferentially' in relation to one's own group (or geographical region if we are thinking of an experiment) could skew the results. Indeed, the most that any researcher can hope for in this field, is a study that is *suggestive*. We agree with this critique; however, we believe we delivered strong arguments throughout this dissertation to counter this critique in a twofold manner. First, as argued, all policy measures that are in place today, are most certainly based on suggestive studies and, second, as indicated, the causal argument that was made by Wilkinson & Pickett (2009) between inequality and – among others – mental illnesses; has been supported by over 200 meta-analytic studies over the past two decades (De Maio, 2014). Both these arguments should suffice to defend our proposal for a nationwide RCT.

We also realize that it is undeniable that the cost of running and evaluating such an RCT would be formidable and, in the context of today's political field, perhaps insuperable. However, were the project to achieve even partial success, then the potential fiscal and health gains might be huge. Moreover, solid longitudinal research – as opposed to short term research with poor methodology

– in the field of Basic Income (BI) also delivers – time and again – strong evidence that a universal monthly basic income (the equivalent of the financial aid in our RCT proposal) has a number of important effects; *if and when* introduced according certain rules (Bregman, 2014). Furthermore, researchers argue that, next to the already mentioned advantages – such as better physical and mental health, lower crime rates, better education for children and so on – these programmes that basically give a basic monthly income to all, are far less expensive that the current measures (Hanlon, Barrientos & Hulme, 2010; Forget, 2011). If the sentence 'It's the economy, stupid!' (and the reciprocal meaning we gave to this sentence) could be applied to one idea, it is probably the idea of BI (Bregman, 2014). Not only is this measure far less expensive than current measures, it also stimulates the entire economy: more is bought, which – in turn – stimulates employment opportunities and incomes (Hanlon, Barrientos & Hulme, 2010; Forget, 2011; Bregman, 2014).

Finally, if the null hypothesis would be refuted through our RCT, then, as suggested by the findings of Flavin et al. (2014, supra), even citizens within the control group (treatment as usual) and the high-income citizens might report higher degrees of well-being. Furthermore, and probably most importantly, no less than ten different health issues and social problems might possibly be ameliorated at the same time, which would more than justify the enormous cost involved in running such a nationwide RCT. If the outcome on all ten variables would be significantly lower in the experimental group – as IIH predicts – then we might have finally found after years of research, a way to deal more effectively with the Depression Pandemic, with the rising prevalence figures of other mental illnesses and with the 9 other important social problems covered in the critical index mentioned above.

9. Conclusion: 'Divide and Conquer'

In this chapter – as an answer to our guiding research question – we first concluded that there is no evidence that today's scientific approach of the Depression Pandemic has led to any reduction in the prevalence of depressive disorders. We suggested a twofold shift.

First, instead of focussing almost exclusively on diagnosing and treating the depressed (the established approach), we argued that a sustained focus needs to be put on diagnosing the environment (diagnostic shift). We inferred that feelings of social alienation and sadness probably mainly reflect three interrelated cultural changes: the emergence of the neoliberal doctrine and competitive individualism; the subsequent emergence of meritocracy; and the emergence of anxious and controlling practices.

Second, based on the apparent ineffectiveness of current measures to try to counter the Depression Pandemic and the gigantic costs of this scientific endeavour involved; we suggested a policy shift. We proposed some well-founded arguments on which this policy shift – from promoting treatment toward promoting social and economic equality – could be based. We assume that our arguments could enable policy makers to more easily introduce effective measures for dealing with the Depression Pandemic.

In the context of our proposal for a nationwide RCT to further strengthen the hypothesis that more equal societies lead to healthier societies, the familiar phrase 'Divide and Conquer' could acquire a new meaning: – divide capital in a more equitable way and, in doing so, conquer ten major health and social issues. More importantly – from our research focus – we believe that through our RCT proposal, a "way out" is proposed to deal more effectively with the Depression Pandemic, informed by a body of research findings that can no longer be ignored.

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Chapter VI

Concluding Thesis: The Knowledge Vacuum as an Invitation to Intellectual Openness, and Debate

Throughout this dissertation, we have discussed the rapidly evolving Depression Pandemic; more specifically, we have critically discussed the main way our societies have come to deal with this pandemic, i.e. through biomedical and psychological scientific discourse. Many tentative conclusions have been advanced in the previous chapters; it will be recalled that chapters III and IV were stand-alone scientific papers, and that we needed to further integrate these preliminary conclusions in our discussion section (chapter V). Our overarching concluding thesis, which is woven from these preliminary conclusions, could be considered largely incompatible with current approaches toward the Depression Pandemic and suggests that we need to find new ways of dealing with this public health priority.

1. Our concluding thesis

The starting point for this dissertation could be described as follows: since depressive disorders are projected to become the leading cause of the GBD by 2030, how should we – as a society – try to tackle this major public health issue? Two lines of thought have suggested themselves.

First, we believe that we have provided ample evidence to cast doubt upon the conventional approach of the Depression Pandemic. This critique is based on three interrelated observations. The framing of depression as an almost exclusively individual pathology – whether it is seen as a

product of faulty brain chemicals, or of aberrant styles of thought and behaviour – ; neglects the research that shows that social and economic inequalities and socioeconomic adversity have significant effects on mental wellbeing. In addition, we observed that there is no scientific consensus concerning the reliability and validity of the diagnostic category of depression to which the experts refer. Finally, we noticed that there is no solid agreement concerning the effectiveness of todays' EBTs – both pharmaceutical and psychotherapeutic – for depressive disorders. All of this suggests that the current dominant scientific approach will most likely be unable to counter the rising prevalence of depression. The medical and psychological scientific narratives concerning depressive disorders seem to be leading us, in part, to the wrong questions and, subsequently, to the wrong answers. As a consequence, we infer that the current consensus conceals a knowledge vacuum; which, we suggest, might be best interpreted as an invitation to greater intellectual openness and debate.

Second, based on our research, we infer that if we want to address the rising prevalence figures for depressive disorders more effectively, then we probably need to start focussing more on the society that in all likelihood, is partially responsible for them. Indeed, intellectual openness is needed toward the idea that clinicians, researchers and patients themselves might be mistaken when they consider symptoms of distress and negative affect as expressions of a disease or a disorder, instead of considering them to be the effect of a damaging world. We argued that in the last thirty years, our society has become a harsher place in which to live: social and economic inequality is growing, work intensifies just as it becomes more precarious, and the public sphere shrinks. These changes reflect the regnant neoliberal doctrine of competitive individualism; expressed in the rise of a regime of meritocracy in the workplace and the education system, and of a culture of surveillance and control more generally. Self-consciousness and a perceived obligation to be 'free' have become

the normal conditions of a life that is less and less able to guarantee either. We have established a causal link between these overarching social changes and the rising prevalence of personal distress.

Having arrived at the end of our research, we recommend an ideational policy shift in which fresh ideas about the nature and prevention of depression might perhaps begin to flourish. To achieve this, we will need to address policymakers with some key observations: that the rising prevalence figures for clinically low mood and sadness are of pandemic proportions; that todays' scientific approach toward this pandemic is questionable; and that the widely accepted VSL approach indicates how the economic burden of depressive disorders amounts to trillions of dollars annually. Above all, we have to convince policymakers of the need to rethink depressive disorders by learning to ask some uncomfortable questions about the effects of current social and economic inequalities, and about the unspoken and prescriptive norms that govern daily life, e.g. the notion of the cheerful, resilient and essentially self-creating individual as the mirror image of the depressed patient.

Framing depressive disorders as – at least in part – *socially induced* could offer a way to deal more effectively with the Depression Pandemic. We infer that policy measures aiming to reduce social and economic inequality will – in all likelihood – not only significantly reduce the prevalence figures for depression; statistical modelling suggests that they should also yield enormous economic returns for society as a whole.

2. A Twofold Rejection and Some Common Sense in Times of Scientific Inequality

We emphasized on a few occasions that we adhere to the biopsychosocial outlook on mental health introduced by Engel (1977) and that we have no desire to replace or abandon biomedical and psychological research into depressive disorders. However, as probably most clinicians and researchers do, and referring to our diagnostic shift, we reject the idea that the causal pathways that lead to depressive mood are decisively or solely determined by biomedical and/or psychological factors. As a consequence and referring to our policy shift, we reject the expectation that solely the pharmaceutical and/or psychotherapeutic treatments for depression will be able to counter the rising prevalence figures.

We realise that this twofold rejection does not fit well with the reality that, today, scientific inequality (in the funding and practice of science) is salient in the field of mental health research and treatment. For example and as argued, massive budgets are allocated to biomedical research, genetic studies, neuroimaging research and – to a much lesser extent – to research into the effectiveness of psychotherapy; while research into socioeconomic adversity and its significant effects on mental wellbeing seems to be severely underfunded.

Based on the writings of Kuhn (1970), Foucault (1971) and Feyerabend (1975), we inferred that the dominant scientific ideas – once they are institutionalized – always exert considerable external control over the thinking of most clinicians and researchers; as they participate more or less willingly in procedures and rituals that serve to co-opt, constrain or suppress critical thought or dissent. It is through this dominant scientific discourse – mediated by the key institutions of education, research and practice – that all official attempts to understand and deal with the Depression Pandemic take place. We believe we provided ample evidence to argue that, in regard

to the Depression Pandemic, there is too much at stake to allow our thinking to be straight-jacketed in this way; and to infer that it seems both reasonable and meaningful to extent this dominant approach with continued longitudinal research into social determinants for depressive disorders and social preventive measures. Precisely because we seem to live in times of scientific inequality, our critical observations deserve *particular* attention.

Referring to one of Kuhn's' (1970) postulates, we believe that it should not be the dominant scientific paradigm that determines what is seen as pertinent or relevant and what is not. Common sense might help in these matters. The Depression Pandemic represents an important global menace and, as we demonstrated, we do not seem to be able to manage this issue, mainly due to a knowledge vacuum. As a logical consequence, in our thinking and research around this issue, we need to replace some of the existing, yet flawed 'certainties' with fresh and well-advised new ideas (Feyerabend, 1975). We can only hope that the ideas that we have provided throughout our disquisition will not be easily downplayed as impertinent or irrelevant.

3. References

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Considerations Concerning the Dominant Scientific Approach Regarding Depressive Disorders: An Invitation for Critical Reflection.

Summary

The starting point for this thesis is the prediction made by the World Health Organisation (WHO, 2011; 2012) – that depressive disorders will be the leading cause of the Global Burden of Disease by 2030.

In *chapter I*, we reflect upon this prognosis. We present and interpret the rising prevalence figures for depression mainly based on a number of WHO-reports and prevalence studies by some of the worlds' leading epidemiologists. We establish that both the point and lifetime prevalence of this form of disorder could be considered as extremely worrying, and that depression is indeed the most prevalent mental health disorder worldwide. In this chapter, we put forward the notion of the *Depression Pandemic* – an insidious and rising tide of malaise that is afflicting large populations, across multiple continents, worldwide.

In *chapter II* we focus briefly on the origins of the prevailing neo-positivist scientific paradigm within biomedicine, the behavioural sciences, and the interface of the two. We determine the actual positions that are taken within neo-positivist scientific reasoning relative to – what we call – the four principal juxtapositions that, in the Western world, have been at the core of philosophical and scientific debate for centuries: knowledge vs. truth; realism vs. nominalism; deductive vs.

inductive sciences; positivism vs. relativism. In doing so, we explore the internal logic and some of the strengths and the weaknesses of this paradigm. This leads us – referring to Carnap – to argue in favour of 'The Principle of Tolerance 2.0'; a greater willingness to tolerate the existence of different (and often unfamiliar) viewpoints and divergent rational reconstructions concerning the Depression Pandemic. At the end of chapter II, we outline our guiding research question: Does the dominant scientific approach in dealing with depressive disorders offer fruitful perspectives when trying to counter the rising prevalence figures of depressive disorders?

We introduce our main research focus in *chapter III*. This chapter 'The Depression Conundrum and the Advantages of Uncertainty' was published in *Frontiers in Psychology* (2017). In this review paper, we question neo-positivist reasoning (quantitative research) from *within* the bounderies of the dominant scientific paradigm. As a consequence, our reasoning within this chapter is based exclusively on methodological and statistical grounds. Based on the requirements for excellent research synthesis, we analysed in a non-standardized way five meta-analytic studies (362 RCTs) examining the effectiveness of seven forms of psychotherapeutic Evidence Based Treatments (EBTs) for depression. Our examination finds serious methodological and statistical shortcomings in every study. Furthermore, we argue that the meta-analytic technique is founded upon problematic assumptions. The implications of our analysis are outlined: decades of quantitative research might not allow us to conclude that psychological EBTs for depression are effective. This chapter addresses a critical and timely interaction between psychological science and society in its evaluation of applied psychological therapies.

We expanded our initial research focus in *chapter IV*: 'Breaking The Depression Deadlock – Rethinking Depression Globally'. With reference to a comprehensive narrative review of mainly

critical scientific literature concerning depressive disorders which also allows reasoning from *outside* the dominant scientific paradigm; we develop four arguments against todays' clinical approaches. First, we argue that the diagnostic category of depression itself is incoherent. Second, we confirm (referring to chapter III) from various angles that current scientific knowledge concerning the effectiveness of psychotherapeutic remedies might be flawed. Third, we suggest that a very similar picture obtains for the clinical research conducted into so-called 'anti-depressant' medication. Finally, we argue that all of these issues are either routinely ignored or dismissed by defenders of the status quo in psychiatry and psychology. In the penultimate section of this chapter, we consider some explanations for why this is so. This chapter was published in the *Journal of Mental Disorders and Treatment* (2018).

In our discussion section – *chapter V* – we explore other viewpoints to look at the Depression Pandemic, and on the basis of this analysis we suggest a twofold shift. We argue that changing personality traits and subsequent feelings of alienation, impuissance, hopelessness and sadness probably mainly reflect three interrelated cultural changes that in the last thirty years, have particularly marked the spheres of citizenship, employment, education and indeed all relationships that might rest upon foundations of stability and trust. These three interrelated cultural changes are: the emergence of the neoliberal doctrine and competitive individualism; the subsequent emergence of meritocracy; the emergence of anxious and controlling practices. We suggest that, if these changing personality traits – as a result of these overarching cultural changes – are ultimately positively associated with depressive disorders, suicide ideation, ill-being and sadness; then indeed it seems both reasonable and meaningful to consider depressive disorders – first and foremost, yet not exclusively – as socially induced disorders (diagnostic shift). Subsequently, in this chapter we therefore explore the evidence in support of a policy shift: – away from the current focus upon

individual treatment and prevention, and toward the promotion of greater social and economic equality. We argue that policy measures aiming to reduce social and economic inequality will – in all likelihood – not only significantly reduce the prevalence figures for depression; statistical modelling suggests that they should also yield enormous economic returns for society as a whole.

In *chapter VI*, we first present our concluding thesis. We end this dissertation with a final reflection entitled *'A Twofold Rejection and Some Common Sense in Times of Scientific Inequality'*.

Beschouwingen aangaande de dominante wetenschappelijke benadering betreffende depressieve stoornissen: een uitnodiging tot kritische reflectie.

Samenvatting

Het uitgangspunt voor dit proefschrift is de voorspelling van de Wereldgezondheidsorganisatie (WHO, 2011; 2012) dat depressieve stoornissen in 2030 de belangrijkste oorzaak zullen vormen van de 'Global Burden of Disease' (GBD).

In *hoofdstuk I* reflecteren we omtrent deze prognose. We presenteren en interpreteren de stijgende prevalentiecijfers voor depressie, voornamelijk op basis van een aantal WHO-rapporten en prevalentieonderzoeken van enkele toonaangevende epidemiologen. We stellen vast dat zowel de puntprevalentie als de 'life time' prevalentie van depressieve stoornissen als buitengewoon zorgwekkend kunnen worden beschouwd en dat depressieve stoornissen inderdaad de meest voorkomende geestelijke gezondheidsstoornissen zijn. In dit hoofdstuk suggereren we de notie van de Depressie Pandemie; een sluipende en toenemende vorm van malaise en neerslachtigheid die grote bevolkingsgroepen wereldwijd treft.

In *hoofdstuk II* focussen we op de oorsprong van het heersende neo-positivistische wetenschappelijke paradigma binnen de medische wetenschappen, de gedragswetenschappen en hun doorsnede. We bepalen de posities die worden ingenomen binnen de neo-positivistische wetenschappelijke redenering ten opzichte van wat we benoemen als de vier belangrijkste juxtapositities die reeds eeuwenlang de kern vormen van het filosofisch en wetenschappelijk debat binnen het Westerse denken: kennis versus waarheid; realisme versus nominalisme; deductieve

versus inductieve wetenschappen; positivisme versus relativisme. Daarbij verkennen we de interne logica, de sterke en de zwakke punten van dit paradigma. Dit leidt – verwijzend naar Carnap – tot een pleidooi voor het invoeren van 'The Principle of Tolerance 2.0'; concreet: een grotere bereidheid om het bestaan te tolereren van verschillende (en vaak onbekende) gezichtspunten en uiteenlopende rationele reconstructies met betrekking tot de Depressie Pandemie. Aan het einde van dit hoofdstuk schetsen we onze leidende onderzoeksvraag: 'Biedt de dominante wetenschappelijke benadering met betrekking tot depressieve stoornissen vruchtbare perspectieven om de stijgende prevalentiecijfers van depressieve stoornissen tegen te gaan?'.

We introduceren onze belangrijkste onderzoekfocus in *hoofdstuk III*. Het hoofdstuk 'The Depression Conundrum and the Advantages of Uncertainty' werd gepubliceerd in *Frontiers in Psychology* (2017). In dit wetenschappelijke artikel bestuderen we kritisch de neo-positivistische redenering (kwantitatief onderzoek) van *binnenuit* het dominante wetenschappelijke paradigma. Onze redenering in dit hoofdstuk is uitsluitend gebaseerd op methodologische en statistische argumenten. Op basis van de wetenschappelijke vereisten met betrekking tot meta-analytisch onderzoek, analyseerden we op een niet-gestandaardiseerde wijze vijf meta-analytische studies (362 RCT's) die de effectiviteit van zeven psychotherapeutische behandelingen (Evidence Based Treatments, EBT's) voor depressie onderzoeken. Ons onderzoek toont aan dat er in elk van deze meta-analytische studies sprake is van ernstige methodologische en statistische tekortkomingen. Verder stellen we dat de meta-analytische techniek gebaseerd is op problematische assumpties. De implicaties van onze analyse worden geschetst: tientallen jaren van kwantitatief onderzoek laten ons mogelijks niet toe om te concluderen dat psychologische EBT's voor depressie effectief zijn. Dit hoofdstuk staat op een kritische wijze stil bij de interactie tussen psychologische wetenschap en samenleving, door middel van de evaluatie van psychotherapeutische behandelmodellen.

We breiden onze initiële onderzoekfocus uit in *hoofdstuk IV*: 'Breaking the Depression Deadlock – Rethinking Depression Globally'. Binnen deze uitgebreide narratieve review van voornamelijk kritische wetenschappelijke literatuur over depressieve stoornissen, integreren we ook redeneringen van *buiten* het dominante wetenschappelijke paradigma. We ontwikkelen vier argumenten tegen de klinische benaderingen van vandaag. Ten eerste, we suggereren dat de diagnostische categorie van depressie incoherent is. Ten tweede, we bevestigen (verwijzend naar hoofdstuk III) vanuit verschillende invalshoeken dat de huidige wetenschappelijke kennis over de effectiviteit van psychotherapeutische EBT's mogelijk ontoereikend is. Ten derde, we beargumenteren dat een zeer vergelijkbaar beeld wordt verkregen voor het klinisch onderzoek dat is uitgevoerd naar de farmaceutische behandeling, de zogenaamde 'antidepressiva'. Ten slotte, we concluderen dat al deze kwesties routinematig worden genegeerd of verworpen door de verdedigers van het status-quo binnen de psychiatrie en de klinische psychologie. In het voorlaatste deel van dit hoofdstuk bespreken we enkele mogelijke verklaringen waarom dit zo is. Dit hoofdstuk werd gepubliceerd in het *Journal of Mental Disorders and Treatment* (2018).

In ons discussiehoofdstuk – *hoofdstuk V* – verkennen we andere gezichtspunten om naar de Depressie Pandemie te kijken. Op basis van onze analyse stellen we een tweeledige verschuiving voor: een diagnostische verschuiving en een beleidsverschuiving. We beargumenteren dat veranderende persoonlijkheidskenmerken en de daarmee gepaard gaande gevoelens van vervreemding, hulpeloosheid, hopeloosheid en malaise wellicht hoofdzakelijk drie onderling samenhangende en overkoepelende socioculturele veranderingen weerspiegelen. Deze drie onderling gerelateerde socioculturele veranderingen hebben zich de afgelopen dertig jaar doorgezet in onze samenlevingen en zijn nauw verbonden met de noties van burgerschap, werkgelegenheid, onderwijs en met zowat alle maatschappelijke verhoudingen die gebaseerd zijn

op vertrouwen en stabiliteit. We omschrijven deze veranderingen als volgt: de opkomst van de neoliberale doctrine en het competitieve individualisme; de daaropvolgende opkomst van een meritocratisch stelsel; en de opkomst van allerlei angstinducerende controlepraktijken. In dit hoofdstuk suggereren we het volgende: omdat deze veranderende persoonlijkheidskenmerken – als gevolg van overkoepelende socioculturele veranderingen – uiteindelijk positief geassocieerd worden met depressieve symptomatologie, zelfmoordgedachten, ziekte en verdriet; lijkt het inderdaad zowel redelijk als zinvol om depressieve stoornissen – vooreerst, maar niet uitsluitend – te beschouwen als sociaal geïnduceerde stoornissen (diagnostische verschuiving). Vervolgens verkennen we in dit hoofdstuk het bewijsmateriaal ter ondersteuning van een beleidsverschuiving: weg van de huidige focus op individuele behandeling en individuele preventie, en in de richting van de bevordering van grotere sociale en economische gelijkheid. We beargumenteren tenslotte dat beleidsmaatregelen ten behoeve van meer sociale en economische gelijkheid naar alle waarschijnlijkheid de prevalentiecijfers voor depressieve stoornissen significant zullen verlagen; bovendien suggereert statistische modellering dat dergelijke beleidsmaatregelen ook een enorm economisch rendement voor de samenleving zouden moeten opleveren.

In *hoofdstuk VI* presenteren we onze afsluitende these. De verhandeling wordt beëindigd met een laatste reflectie, getiteld ‘A Twofold Rejection and Some Common Sense in Times of Scientific Inequality’.