In Reply: Psychodynamic therapy of depression

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We thank Murray et al. (2022) for their response to our critique (Leichsenring et al., 2021) about the positioning of psychodynamic psychotherapy (PDT) in the 2020 RANZCP clinical practice guidelines for mood disorders (Malhi et al., 2021).

1. The RANZCP guidelines argue with regard to PDT (Malhi et al., 2021: 42) ‘... that not all depressive presentations benefit from this therapeutic approach’, and ‘robust replications are required’. We emphasized that this applies to other forms of psychotherapy in depression as well. In their response, Murray et al. (2022) concede that there is strong evidence for short-term psychodynamic psychotherapy (STPP) for treating major depression. Murray et al. (2022) also stress that at no point the guidelines asserted that cognitive behavioral therapy (CBT) is more efficacious than other psychological treatments for depression. They then continue that their decision to prioritize CBT and interpersonal therapy was ‘pragmatic’ (Murray et al., 2022), ‘... because they have been subjected to more investigation across sites, are more commonly taught in training programs and are familiar to current practitioner networks’. However, the decision to prioritize CBT and interpersonal therapy labeled as ‘pragmatic’ itself is non-scientific. The fact that more studies exist does not imply that a treatment is more efficacious, so this should not be a basis for prioritization. PDT is commonly taught in training programs and current practitioner networks are familiar with that approach, at least in the Western world including in Australia and New Zealand (for reference, see online supplement # 1).

2. In our comment we noted that the RANZCP guidelines incorrectly asserted, ‘there is no evidence to support long-term psychodynamic therapy’ and reported evidence that long-term psychodynamic therapy (LTPP) is effective in complex presentations of depression (Leichsenring et al., 2021). Murray et al. (2022) responded that their assertion referred to the treatment of acute depression for which there is no evidence for LTPP. Our response to this is as follows: Murray et al. (2022) discussed a study we had cited comparing LTPP, psychoanalytic therapy and CBT. In this study, there were no differences in efficacy except for psychoanalytic networks.
therapy being superior to CBT at the 3-year follow-up (online supplement reference #2). Murray et al. (2022) emphasize that LTPP was not superior to CBT. This is true, but this implies that no differences in efficacy were found between LTPP and CBT. As a limitation, however, the statistical power was not high enough to detect small differences. In addition, we cited a study which found no differences in efficacy between LTPP and CBT in patients with chronic depression, with LTPP using more sessions (see online supplement #3), and another study in which LTPP combined with treatment as usual (TAU) was not superior to TAU alone in treatment-resistant depression at the end of treatment but at 24-, 30- and 42-month follow-ups with regard to partial remission (for reference, see online supplement #4). Chronic or treatment-resistant depression can be regarded as the more severe and difficult-to-treat condition compared to acute depression. Thus, if LTPP is efficacious in chronic and treatment-resistant depression, there is no reason to assume that it is not also efficacious in acute depression. As correctly cited by Murray et al. (2022), in another study, LTPP was not superior to STPP in the short term (online supplement reference #5). However, LTPP was superior in the 36-month follow-up with regard to the reduction of depressive symptoms (beck depression inventory, hamilton depression rating scale). Regarding recovery, it is true that in this study LTPP was not superior to STPP, neither in the short-term nor in the long-term outcome.

In addition, the argument put forward by Murray et al. (2022) regarding the evidence for long-term psychodynamic therapy is based on a very limited view concerning the nature of depression. Depression is a notably heterogeneous condition. Many patients with depression present with personality issues such as borderline personality disorder (BPD). In fact, studies have shown that depression is not only a central feature of BPD, but that the nature of depressive experiences is qualitatively different in patients with BPD compared to depressed patients without substantial BPD features, with greater feelings of emptiness, self-harm and risk for suicidality (for a review, see supplement reference #6). A recent Cochrane meta-analysis found that mentalization-based treatment (MBT), a type of LTPP for BPD, was superior to TAU in reducing self-harm, suicidality and depression with moderate to large effects at long-term (>12 months) follow-up (for reference, see online supplement #7). Similarly, a recent meta-analysis found that MBT was associated with large effect sizes (standardized mean difference = 1.03) in reducing suicidality in BPD (see online supplement reference #8). Although we agree that more research on LTPP for depression is required, the absence of a consideration of the effectiveness of different types of LTPP (and other long-term treatments) for patients with more complex presentations is a notable limitation, particularly given that many patients in routine care present with complex depression for which brief psychotherapy is not sufficiently efficacious (see online supplement reference #9). In the context of complex presentations of depression, Malhi et al. (2021) also cited a meta-analysis reporting that PDT and dialectical behavior therapy, but not CBT, are superior to controls in patients with BPD. In spite of this, Malhi et al. (2021: 96) argued that for psychodynamic therapies ‘... there are no RCTs ... to suggest that they may be of some help’. Yet, as noted, meta-analyses provide evidence that PDT is efficacious in complex presentations of depression (Leichsenring et al., 2021).

We stated that neither treatment manuals of STPP for depression nor manuals for the long-term treatment of complex presentations of depression (e.g. with comorbid BPD) promote regression; by contrast, regression is explicitly restricted in these manuals (e.g. Leichsenring and Steinert, 2018). In response, Murray et al. (2022) ask whether regression may be considered a tool in some variants of PDT and state that it has been suggested that transitory regression may occur in the context of working through trauma and that managing distress related to regression is an area of expertise within PDT. Regression may indeed occur during treatment, in PDT as much as in other forms of psychotherapy. This is similar to other psychotherapeutic processes such as transference or resistance which also occur in CBT, interpersonal therapy or in other forms of therapy. We agree with Murray et al. (2022) that managing regression is an area of expertise within PDT. However, this does not imply that regression is promoted in PDT. Regression is only promoted in some variants of classical psychoanalysis for patients who are able to tolerate it. Under these conditions, promoting regression serves to facilitate the development and working through of transference issues.

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The broader benefits of DBS for refractory OCD

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We write having read the informative Viewpoint by Mosley et al. (2021) which, in making a case for the acceptance of deep-brain stimulation (DBS) for refractory obsessive-compulsive disorder (OCD), provides some interesting insights as it reviews the findings of four randomised controlled trials that support the administration of this intervention. The article provides a brief overview of recent data before making a compelling argument as to how the treatment can be offered in a regulated and measured way to those in need, and why both legislative barriers and funding opportunities need to be opened for this initiative.

We write specifically and particularly enthusiastically, however, because OCD is arguably one of the best defined phenomenologies in clinical psychiatry, and as a disorder has one of the clearest mechanistic explanations rooted in the neuroanatomy of the brain. In other words, it is perhaps one of our best models instantiating qualia of the mind in the physicality of neural structures. And as per the figures in Mosely et al.’s article, it exemplifies the synergy of cognitive neuroscience involving neuropsych, neuropathology, modern neuroimaging and psychiatric expertise. Elevating it further still, however, this collaboration yields an enduring therapeutic benefit in one of the most disabling of medical disorders (Fenoy et al., 2022). But this is not all; this is an opportunity to understand, innovate and explore further, particularly in relation to potential predictors of outcome such as duration of illness, comorbidity and symptom subtype (Alonso et al., 2015).

The intriguing observations, noted only briefly, include the dissociability of effects on mood and cognitive flexibility, which depend on stimulation site and other parameters. The fact that stimulation-dependent impulsivity and hypomania can be induced, and that withdrawal or modification of parameters can lead to lowering of mood, signals that there are potential avenues of future research that must be explored.

The article by Mosley and colleagues is particularly relevant as a large number of disorders are at the intersection of psychiatry and neurology with neuropsychiatric manifestations. As more causal pathways are identified which are shared across the spectrum of neurological and psychiatric disorders, this might also stimulate a discussion whether to imbed aspects of neurological training into psychiatry and vice versa. Partial reintegration of the two disciplines, especially in the undergraduate/graduate training and in research, could strengthen both.

In sum, we are in favour of the suggestions made by the group regarding how patients should be assessed, and monitored, and how they should be managed ethically with both a sense of duty and fairness – to provide these troubled patients an opportunity for relief. However, while lending our support to these requests, we also note that this intervention needs to be distinguished from other forms of brain stimulation, where there is no such specificity of action. A clear distinction also needs to be made between DBS and psychosurgery, where the changes involve ablation of tissue and are essentially irreversible.

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The author(s) declared the following potential conflicts of interest with respect

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Supplemental material
Supplemental material for this article is available online.