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The future of psychotherapy research: stop the waste and focus on issues that matter

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It is well-established that psychotherapies can effectively treat depression. In the past four decades, more than 400 randomized controlled trials have been conducted (Cuijpers, 2015), showing that different types of psychotherapy are effective (Barth *et al.* 2013), that the effects do not or only marginally differ from each other, that they are comparable with those of antidepressants, and that combined treatment is more effective than psychotherapy or medication alone (Cuijpers, 2014). However, psychotherapies are probably less effective in chronic depression and dysthymia, when patients have comorbid alcohol problems, and in subthreshold depression, although in these patients therapies can prevent the onset of full-blown major depressive disorders (Cuijpers *et al.* 2014).

The two Editorials in this issue give interesting perspectives on where we are with research on these therapies and what should happen in the future (Hollon, 2015; Solomonov & Barber, 2015). The first thing that stands out is that both agree that although psychotherapies are effective, there is still considerable room for improvement. The Editorials point at the problem that the effects of psychotherapy have been overestimated because of publication bias, but also that the longer term effects of psychotherapies are not well known, except maybe for cognitive-behavioural therapy where longer-term effects have been shown (Karyotaki *et al.* 2014) in several trials. The small effects of psychotherapies and treatments in general in chronic and treatment-resistant depression (Cuijpers *et al.* 2010*d*) and the high relapse rates after successful recovery (Vittengl *et al.* 2007) are also important problems.

I want to underscore that although therapies are effective, there is still much room for improvement. The effect sizes found for treatments for mental disorders do not differ very much from those in general medical disorders (Leucht *et al.* 2012), but still it is estimated that current treatments cannot take away more than one-third of the disease burden of depression, and then only in optimal conditions (Andrews *et al.* 2004). And then there is the problem that more than 40% of the patients do not or only partially respond to treatment and less than one-third of the patients are completely recovered after treatment (Hollon *et al.* 2002). And after successful response the relapse rates are estimated to be 50% after 2 years and up to 80% after 5 years (Vittengl *et al.* 2007). These problems are in addition to the problem that much of the earlier research has considerably overestimated the effects of acute treatments of depression, not only because of publication bias, but also because of the low quality of many trials in the field (Cuijpers *et al.* 2010*c*) and probably researcher allegiance (Munder *et al.* 2013).

Both articles give indications for how future research can contribute to a further reduction of the disease burden. In addition to that, however, I would like to stress that there is also much research that is not needed anymore in the future. In the broader biomedical field it is estimated that about 85% of research is wasted, because of irrelevant questions, inappropriate designs and methods, and biased reporting (Chalmers & Glasziou, 2009). There is no reason to assume that this is very different in randomised trials of psychotherapies for depression. We found earlier that of 115 trials in this field only 11 met all nine generally accepted quality criteria, and that the effects of these 11 studies were considerably smaller than those of lower-quality studies (Cuijpers *et al.* 2010*c*). We also found that adjusting for unpublished trials reduces the effect size of psychotherapies with more than one-third (Cuijpers *et al.* 2010*b*), and reporting in these trials also is often biased (Flint *et al.* 2014).

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In addition to that, a considerable number of studies report on the effects of newly developed psychotherapies for depression (e.g., Bédard *et al.* 2014; Giosan *et al.* 2014) and on comparative trials comparing two types of psychotherapies. It is well-established, however, that all therapies for depression are about equally effective (Barth *et al.* 2013) and that if a new therapy would indeed be more effective than an existing therapy, it would take a new trial with about 800 patients to show its superiority (Cuijpers & van Straten, 2011). If a therapy is found to be superior to an existing therapy in an underpowered trial that would rather raise doubts about the validity of the trial than trust that this new therapy is indeed more effective.

Other trials have focused on examining psychotherapies for depression in specific target populations, like older adults, women with postpartum depression and patients with comorbid general medical disorders. However, meta-analyses consistently show that psychotherapies are effective in all such target groups (Cuijpers *et al.* 2008). By far the majority of trials examining psychotherapies for depression find positive effects, whether these are in specific target groups or in unselected populations of adults. It seems therefore not necessary to spend resources on examining these therapies in all such specific target groups.

In the same way it can be argued that it is no longer needed to examine different treatment formats in randomised controlled, because all research consistently suggests that individual, group, guided self-help and guided Internet-based therapies formats result in comparable outcomes (Cuijpers *et al.* 2010*a*; Andersson *et al.* 2014).

In order to reduce the disease burden of depression much research is needed in many areas. The two articles give several directions for future directions in research on psychological treatments, including research how to reduce drop-out rates for psychotherapies, because they use considerable resources with apparently little benefits (Solomonov & Barber, 2015). But also more research on the long-term effects of therapies, treatment-resistant and chronic depression, the high relapse rates and more knowledge on who benefits from which treatment are important goals for future research, as justly indicated in these two articles in this issue.

In this context it could be added that it is also important to examine how treatments work, because if we understand the processes of how therapies work we may finally start improving the effects of therapies. However, examining working mechanisms of treatments is notoriously complicated (Kazdin, 2007), and requires different types of research, including not only research on mediators in randomised trials, but also experimental studies, theoretical work and animal studies. Although this will require quite some resources, it will be one of the few ways how we can improve the effects of treatments. It has been shown that all pharmacological therapies are about equally effective, that all psychological treatments are equally effective, and that pharmacological and psychological treatments are also equally effective. That means that all treatments we have are about equally effective, while we hardly know anything about who benefits from which treatment. This basically implies that the effects of treatments have not been improved since the 1950s when the tricyclic antidepressants (TCAs) were first tested.

The other approach to a further reduction of the disease burden of depression is to focus on improvement of how to apply treatments better in routine care and how to scale treatment up. Although the effects of treatments may not have improved since their first development, mental health care has made considerable progress in terms of reaching depressed patients and applying therapies to people who can benefit from therapies. That is important because large group of patients who may benefit from treatment still do not use these services enough, like adolescents, older adults, minority and lower socioeconomic groups. And in low- and middle income countries treatments are hardly available at all. It is important therefore to simplify therapies and make them more accessible.

The suggestion by Solomonov and Barber to develop stepwise treatment models offers therefore an interesting perspective, with the simplest and cheapest treatments first, followed by more complicated and intensive therapies. That is in line with more generic models that are aimed at improving existing treatments and making them more efficient, like stepped care (van Straten *et al.* 2015) and collaborative care models (Coventry *et al.* 2014). But there are also other ways to improve access and simplify treatments without reducing their effects, like using trained lay health counsellors for the delivery of treatments in low-resourced countries, as has been done in India (Patel *et al.* 2010), or using Internet-based guided self-help therapies which cost much less time than traditional, individual therapies (Andersson & Cuijpers, 2009).

Psychotherapies and other treatments for adult depression are effective, but there is also much room for improvement. The two Editorials in this issue give a nice overview of where we are and where we should be heading. We need much research, on the long-term effects, on treatments of chronic and treatment-resistant depression, on relapse, and on working mechanisms. But there is also much research

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which we no longer need because it has been examined sufficiently, like new therapies that promise superior effects over existing therapies while they always show equal effects. Resources are scarce and using them for unnecessary research will slow down progress and let patients with depression suffer more than needed.

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Conflict of Interest

None.

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