It is a pleasure to be able to offer this contribution[1] as a small token of my gratitude and respect for Mike. I met Mike during my first semester of attending university as a psychology student, and it was an unforgettable experience. He has from that time on greatly influenced my trajectory (paradoxically, perhaps, in clinical psychology); I have been fascinated by epistemological questions such as how causation is derived with the aid of experiments, and in what influences scientific pursuits, such as the role of metaphors in science. I took every seminar Mike offered at the University of Iceland in those days, and had the good fortune to work with him as a teaching assistant and later as lecturer. It was one of his seminars, termed „Aðferð og meðferð vísinda", which set the stage for the current talk. However, I am most grateful for his friendship and support during those early formative years, and ever since, and hope we will have many more years to think through some of those questions and enjoy each other´s company.

I. Control and Psychotherapy

There is always tension in any applied science between technology, science, and business. The history of psychology in particular is full of dreams of achieving something similar to the physical sciences, physics in particular (see e.g., Leahey, 2000). What has often been forgotten in that comparison is the complexity of our subject matter; nothing in the known universe is quite as complex as humans and their interactions. We need to be realistic while attempting to adopt the most rigorous methods and most sophisticated theoretical frameworks possible. However, while keeping the complexities of the therapeutic relationship in mind, that rigor is often lacking when it comes to evaluating psychotherapies.

It has been known for a long time that experiments are necessary to determine therapeutic efficacy – in the absence of them there is no way to rule out spontaneous recovery (Rachman & Wilson, 1980) and other factors that can account for why people change over time. The field of psychotherapy has used the double-blind randomized placebo control design that was originally developed for testing medications (Baskin, Tierney, Minami, & Wampold, 2003) – although therapists can obviously not be blind to the therapy they are delivering. It is from medication outcome research that our field has adopted the metaphor of the "psychological placebo", which brings to mind the sugar pill. This metaphor has limited our

vision of the potential of non-specific factors in psychotherapy.

Control conditions are often not credible to participants, and psychotherapies tend not to outperform credible controls (e.g. Erwin, 1997). Furthermore, control conditions are often not even “structurally-equivalent” (with regard to factors such as length and number of sessions) to the psychotherapies being evaluated, and structurally equivalent control conditions tend to do as well as the psychosocial interventions that are being tested (Baskin et al., 2003). Thus, unless there is a clear distinction between what constitutes acceptable control conditions in evaluating therapies, many therapies can be made to look more efficacious than they actually are. Wait-list control groups, for example, are appropriate when evaluating the promise of new therapies in the early treatment development, but they are not acceptable in determining if they truly outperform current alternatives. It is stating the obvious that participants are always aware that waiting for treatment is not the same as being in treatment. More subtly, during the past few decades the reliance in outcome research on psychoeducation as a control condition may be misguided, because psychoeducation that seems at first to be credible to participants may lose its credibility in the middle and late phases of treatment. As one example, cognitive-behavioral group therapy (CBGT) for social anxiety disorder has always done better than a control condition involving psychoeducation and support which was deemed to be credible to participants (Heimberg et al., 1990; Heimberg et al., 1998). However, when CBGT was compared to a control condition that was matched to it with regard to structure and non-specific factors, no differences were found in one study (Bjornsson et al., 2011).

Control conditions should be matched as closely as possible with the therapy that is being tested on non-specific factors (like empathy, therapeutic alliance, and group cohesion) (see e.g. Stevens, Hynan, & Allen, 2000; Safer & Hugo, 2006). We should only be testing what the new therapy is supposed to add, based on a theory of the disorder and why this particular therapy should be effective in treating it. Here, a distinction between specific and non-specific factors is necessary.

II. The Specific and Non-Specific Schools in the History of Psychotherapy

Although simplifying greatly, it can be said that there are two major schools in the history of psychotherapy. According to the Specific School, the efficacy of treatment is due to specific treatment factors (like cognitive restructuring in cognitive-behavioral therapy; CBT), that are deduced from a theory of the disorder. However, according to the Non-Specific School, treatment works because of factors that all good therapy should have in common (e.g., empathy and warmth) and not because of any specific treatment factors (see e.g., Wampold, 2001). Historically, the origins of the Non-Specific School can be traced to psychodynamic theorists and therapists gradually becoming more interested in the therapeutic relationship as such, especially after repeated meta-analytic studies found that different therapies have the same outcome (the infamous ´dodo-bird´verdict) (Arkowitz, 1992; Wampold, 2001). The Specific School, on the other hand, can be traced to two overarching theories within psychology; behavior theory and cognitive theory. Specific techniques such as systematic exposure and cognitive restructuring are deduced from behavior theories, cognitive theories and integrated cognitive-behavioral theories of various disorders. Although most behavior and cognitive theorists and therapists would be quick to acknowledge that non-specific factors, such as therapeutic alliance, do play a role, there is nevertheless a tendency within the Specific School to attribute the efficacy of treatment to specific factors and downplay the effects of non-specific factors (see e.g. DeRubeis, Brotman & Gibbons, 2005). The distinction between specific and non-specific factors is central in explaining why a certain therapy may be successful in outcome research (if the control condition is not optimal) even if it is based on a wrong theory; it may capitalize on certain non-specific factors like a compelling rationale, therapists may believe in the therapy that is being tested and so on.

The debate between these two schools has usually been unproductive, with elements of the “all-or-nothing” thinking error in CBT (Ilardi & Craighead, 1994) in that both sides tend to claim all of the gains from treatment with few attempts at reconciliation (Craighead et al., 2005). One way of integrating both approaches is to evaluate how specific and non-specific factors interact over the course of therapy. In order to do this, control conditions must be

developed to match, as closely as possible, the non-specific factors of the psychotherapy being tested (Safer & Hugo, 2006). In a way, this is not a new idea; Beck and colleagues made the famous suggestion in their cognitive therapy manual in 1979 that non-specific factors such as accurate empathy, warmth, and genuineness could be considered necessary but not sufficient for cognitive therapy to be successful (Beck, Rush, Shaw, & Emery, 1979). Little or no research, however, has actually been conducted of investigating how specific factors (like cognitive restructuring) and non-specific factors (like therapeutic alliance) interact with each other by using the methodology suggested above, although such work could have important consequences for how treatment manuals are written and for the training and supervision of therapists.

III. Beyond the "Psychological Placebo": Specifying the Non-Specific

Many authors argue for the incorporation of specific and non-specific factors in both theories and psychotherapies. For example, David and Montgomery (2011) seem to imply that current theories have already incorporated non-specific factors, when they note that such factors "are parts of the theory about the mechanisms of change, together with the specific constructs of a certain psychotherapy." (p. 93). Furthermore, David and Montgomery (2011, p. 95) believe that the incorporation of non-specific factors is reflected in treatment manuals, when they assert that "modern treatment manuals … include a foundation based on common therapeutic factors … ." I think that the opposite is true; non-specific factors are usually not incorporated into theories (especially in CBT), and are more often referred to as good clinical practice (to be respectful, warm, etc.) in treatment manuals. Modern CBT manuals typically only spend a paragraph or a brief subsection on non-specific factors (see e.g., the otherwise excellent manuals by Heimberg & Becker, 2002 and Hope, Heimberg, & Turk, 2010). This is a major shortcoming in the field of psychotherapy, CBT in particular. We cannot assume that non-specific factors are a given, and that therapists will use non-specific factors effectively if instructed to do so in a short paragraph.

There is a related issue that is equally important. David and Montgomery (2011) treat non-specific factors as static entities, for example in the following quotation (p. 96):

The hope is that while placebo (or common factors in the psychotherapy field) has reached its maximum potential, the improvement for a large percentage of patients who do not or do respond well to placebo (or common factors) will come from development of these specific factors (active substance in pharmacotherapy or specific psychological mechanisms in psychotherapy).

Here we can see how the metaphor of the "psychological placebo" (taken from pharmacotherapy outcome research) has put limits on our imagination in developing psychological interventions. I believe, contrary to David and Montgomery, that the future of psychotherapy lies mostly in developing and specifying non-specific factors.

Let's return briefly to the dodo bird verdict – the unproductive debate between the Specific and Non-Specific Schools in the history of psychotherapy. Bruce Wampold, the major living advocate of the dodo bird verdict (see e.g. Wampold, 2001; Wampold, Imel, & Miller, 2009) has been criticized for his assertion that specific factors account for, at most, a minimal portion of the variance (see e.g., Wampold, 2001, and for a recent round of rebuttals, see e.g. Siev, Huppert, & Chambless, 2010; Hofmann & Lohr, 2010; Barlow, 2010). But even if we agree that Wampold went too far it is hard to argue against the weaker, but still compelling, notion that non-specific factors account for the majority of the variance in psychotherapy outcome research. If we can agree on that, then why are we not more interested in these factors? When they work, under what conditions and how? Until now, it seems that the field as a whole has treated them as a given, and there are precious few examples of treatment development that specifies why and how different non-specific techniques (e.g., empathy or group cohesion) could be made to be effective in treating a disorder like major depressive disorder. To take one example, the concepts of validation and invalidation are important constructs in both the theory of how borderline personality disorder develops and is maintained but also in how dialectical behavior therapy (DBT) aims to treat this disorder (Linehan, 1993). Linehan offers specific instructions as to how this non-specific technique (validation) is used in DBT, which is very different from the

commonsensical notion of being warm to clients. In fact, being indiscriminately “warm” can even be invalidating at times to patients who suffer from this disorder. This is an example of how a theory of the disorder and the derived psychotherapy has taken a non-specific factor and made specific uses of it, which has both advanced our understanding of the disorder and helpful to therapists in treating it. On the flipside of the coin; we should avoid certain non-specific techniques. One example is the use of reassurance in the context of body dysmorphic disorder (BDD, see e.g. Phillips, 2009). Providing reassurance to BDD patients that they look “alright” is a common trap that novel therapists sometimes fall into and it can even serve to exacerbate BDD symptoms.

I am not suggesting that psychotherapists and theorists have failed to do any good work clarifying the role of non-specific factors (see e.g., Goldfried, 1991). However, this work has been very limited, and it has been especially neglected in behavior therapy and CBT, partly because of the influence of the Specific School and the metaphor of the psychological placebo. If CBT theorists and therapists, and the field of psychotherapy as a whole, along with funding agencies, were to become truly interested in the potential of different non-specific factors for various disorders, it could open up a completely new way of advancing our field. This work could be focused on exploring how different non-specific factors (e.g., validation and therapeutic alliance) could be made to be effective in distinct contexts, with various treatment modalities and with different disorders. It is very likely that the same non-specific technique could look very different depending on the clinical context and even within subgroups with a given diagnosis (e.g. BDD patients with good insight vs. delusional BDD patients). However, we could also find that the same non-specific factors should be applied similarly for all disorders, or at least large categories of disorders (see e.g. Fairburn, Cooper, & Shafran, 2003; Barlow, Allen, & Choate, 2004). But this work will only take place if we get interested in broadening the current focus of only investigating the current specific techniques derived from CBT theories, and start moving toward specifying non-specific factors, and looking at the interaction between them and specific techniques.

Of course, if and when we reach this new state in the development of our field, these factors would no longer be “non-specific”, since they would be specified by our theories (see Ilardi & Craighead, 1994). That would truly be a sign that our field has resolved the current impasse between the Specific and Non-Specific Schools, and that it has looked beyond the metaphor of the “psychological placebo” toward a more fascinating view of the dynamic

factors in psychotherapy.

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Endnotes

[1] This talk, published here as conference proceedings, is a reprint (with permission from John Wiley & Sons) of large sections of Bjornsson (2011), but goes beyond it. Let me state, however, that Mike´s influence on me should be clear in both versions.