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Letter from the Editor

WHICH OF YOUR FAVORITE HYPOTHESES DID YOU DISPOSE OF TODAY?

25 years ago, on February 27, 1989, Konrad Lorenz died. He was certainly one of the most influential minds that paved the way for the sciences of evolved human behavior. At the same time, he still is the most controversial proponent of ethology.

Lorenz revolutionized the methodology of behavior research, and for his insights in instincts and fixed action patterns, he received the Nobel Prize together with Nikolaas Tinbergen and Karl von Frisch in Physiology or Medicine in 1973.

Tinbergen called him the "Father of Ethology", even though the term was coined in the mid 19th century. The main reason for this might be seen in Lorenz' emphasis on the comparative aspect. The contributions still most influential in today's behavioral sciences are those on instinct behavior, innate releasing mechanisms, supernormal stimuli, and imprinting. His psychohydraulic model of motivation has mostly been replaced by socio-biological and behavior-ecological theories. Theoretically, his work was strongly influenced by the concept of the survival of the species. Late work also contributed to epistemology, and his ideas were further elaborated and popularized by his student Rupert Riedl. A highly controversial person, he remains a central figure for any human ethologist and evolutionary psychologist. The theoretical article by Liane Leedom (2014) in this issue revisits theories dating back to the early days of ethology.

> "Die meisten von uns – dessen müssen wir uns bewußt sein – lieben ihre Hypothesen, und es ist, wie ich einmal sagte, eine zwar schmerzhafte, aber jung und gesund erhaltende Turnübung, täglich, gewissermaßen als Frühsport, seine Lieblingshypothese über Bord zu werfen." (Lorenz, 1973)
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> "We have to acknowledge that most of us love their hypotheses, and, as I once said, it is a painful exercise, but one that keeps us young and healthy like morning gymnastics, to throw your favorite hypothesis overboard every day."

This quote may have been motivated by the need to distance himself from his activities and publications before and during World War II. But even if we leave this aside, the core message of this quote is directed at all of us: The theory we have grown up with, which we cherish and are comfortable with, the theory which explains everything, is only a great theory as long as we put it to test every day. If we start to believe in a theory, we stop being scientists.

A scientist needs to scrutinize empirical data and carefully think what the greater implications of the findings of one study might be. A scientist needs to be honest – not only to the public, but first and foremost to him/herself. We critically

review our work, our methods, and the conclusions we draw – and by doing so, we contribute to scientific progress.

Scientific publications play a central role in this process. They are the main channel through which we share our ideas and findings with our colleagues. They are also the whetting stones for us when we come up with an idea that we would like to turn into a research study. We can find out whether this idea is really original, and if not (which is far more likely), what other scientists have found out. We can evaluate the methodology they employed and weigh the results against methodological limitations. We can find replications and non-replications that further help us to evaluate the robustness of these findings. … or can we? One theme that is being discussed especially in the psychological sciences is the lack of replications. Journal policies at large counteract the publication of replications: Novelty is one of the criteria for acceptance of a submission. Every one of us knows how hard it is to get non-significant findings published. Through the novelty and significance biases of publication organs we run the risk of developing an overperception error: Significant results are published and cited, while studies that fail to replicate said findings are hidden away in some drawer. This might lead to fatal consequences on the large scale of science: We are constantly increasing our Type I error, without correcting for chance. When thinking of the number of statistical tests run worldwide each day just within our field, the number of accidentally significant findings is enormous. If we continue to neglect the communication of replications and non-replications, we might end up in a belief-system of chance effects.

I am not sure what a solution for this problem would be – certainly we cannot try to address this issue by indiscriminately publishing everything. As it stands, the sheer number of publications makes keeping up to date nearly impossible today. However, it might be worth reconsidering whether novelty should remain a criterion of high importance in the decision about acceptance of research reports.

How can we try to counteract the potential skewness of representation of scientific studies? One idea that was put forward during the last meeting of the editorial board of HEB was to introduce a series of special issues dedicated to replications and non-replications, each focussing on one theme. We will start working on those in 2014 and will keep you posted on the process.

In the current issue you will find excellent research reports: Liu and LaFreniere (2014) present results of a research project that won an Owen Aldis Award. In their work, they demonstrate beautifully how methods need to be adapted to changing environmental conditions. Nowak and colleagues (2014) continue research on spousal satisfaction.

The book reviews have always been an important part of HEB, and hopefully provide valuable guidance whether to buy the reviewed books or not.

Elisabeth Oberzaucher
Editor in Chief

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