Reply

# Shooting the messenger won't change the news

Susan Malcolm-Smith [a,*], Mark Solms [b], Oliver Turnbull [c], Colin Tredoux [a]

[a] ACSENT Laboratory, Department of Psychology, University of Cape Town, Rondebosch 7701, South Africa
[b] Departments of Psychology and Neurology, University of Cape Town, South Africa
[c] School of Psychology, University of Wales, Bangor, UK

## ARTICLE INFO

## ABSTRACT

Malcolm-Smith, Solms, Turnbull and Tredoux [Malcolm-Smith, S., Solms, M., Turnbull, O., & Tredoux, C. (2008). Threat in dreams: An adaptation? *Consciousness and Cognition*, 17, 1281–1291.] conducted a rigorous study that sampled two populations differentially exposed to threat in real life, and found that critical predictions from the Threat Simulation Theory of dreams [Revonsuo, A. (2000a). The reinterpretation of dreams: An evolutionary hypothesis of the function of dreaming. *Behavioral and Brain Sciences, 23,* 877-901.; Revonsuo, A. (2000b). Did ancestral humans dream for their lives? *Behavioral and Brain Sciences, 23,* 1063–1082.] were not supported. Specifically, we found no evidence of increased realistic threats to physical survival or enhanced threat avoidance in the dreams of those from the exposed population. Revonsuo and Valli's [Revonsuo, A., & Valli, K. (2008). How to test the threat simulation theory. *Consciousness and Cognition*, 17, 1292-1296.] commentary on our study argues that the methods we used are so flawed as to render the results meaningless. In this response article, we address the criticisms raised in their commentary.

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## 1. Introduction

Threat Simulation Theory (TST) posits that dreaming is an evolved mechanism, selected into human genetic make-up because it provided adaptive advantage to our ancestors. TST argues that dreams provide a cost-free environment in which to rehearse threat perception and avoidance. Adaptive advantage is argued to be bestowed via the same mechanism as that operating in the immune system—responses to particular threats are remembered, and thus future function is improved in the face of similar threats.

The theory predicts that in modern humans, the TS mechanism will be more active in those exposed to survival threats. We tested this prediction by identifying two populations who differ in exposure to survival threats, in the form of violent crime. Despite being four times more likely to have been exposed to a recent life-threatening event, South African participants reported significantly fewer relevant threat dreams than Welsh participants. Successful threat avoidance responses were hardly evident in the dreams of either group. Less than 2% of dream reports featured successful escapes from realistic physical threats.

In their commentary on this research, Revonsuo and Valli (2008) make various criticisms of our method, and suggest that their own method is superior. In the following article, we address the issues they raise, and re-affirm our contention that TST has failed a critical empirical test.

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* Corresponding author. Fax: +021 650 4104.
*E-mail addresses:* Susan.Malcolm-Smith@uct.ac.za (S. Malcolm-Smith).

## 2. How to choose an appropriate data collection method

In their commentary, Revonsuo and Valli characterize the Most Recent Dream (MRD) method of collecting a representative sample of dream reports as biased and unreliable. They claim that the method collects dreams in a 'manner that guarantees significant forgetting, biasing and distortion before the dream is reported' (in press, p. 12). While we agree that obtaining representative samples of dream content is always a vexed issue, we suggest that their characterization of the MRD method is, at best, uninformed.

Given the problems inherent in accessing dream content, how can researchers have any idea of whether their sampling is reliable, and if the reports they collect validly represent the totality of dream experience? There is no definitive answer to this conundrum, but dream researchers have made great efforts to introduce rigorous methods into their data collection. In the 1940s and 1950s, Hall and van de Castle developed a set of methods for empirical investigation of dream content (see Hall, 1951, 1969a; Hall & Van de Castle, 1966). They developed an exhaustive content coding system, and established dream content norms based on thousands of dream reports. These norms have been replicated, validated and updated over the intervening decades (Domhoff, 1996; Domhoff & Kamiya, 1964; Dudley & Fungaroli, 1987; Dudley & Swank, 1990; Hall, 1969b; Hall & Domhoff, 1963, 1964; Hall, Domhoff, Blick, & Weesner, 1982; Schredl, Petra, Bishop, Golitz, & Buschtons, 2003; Tonay, 1990/1991). Domhoff (1996), who continues this research tradition, developed the Most Recent Dream method specifically to enable the collection of large sets of dream data, while ensuring that the datasets gathered provide a representative sample of dream content. The MRD method was validated against the Hall and van de Castle norms (Avila-White, Schneider, & Domhoff, 1999; Domhoff, 2003; Domhoff & Schneider, 1998). Initially, this was achieved by sub-sampling: a set of 500 reports was divided into subsets of 25, 50, 75, 100, etc. The small subsets were found to yield unreliable content estimates. It has been clearly shown that datasets of 100–125 dream reports yield a good approximation of the established content norms. We thus are confident that sample sizes that meet or exceed this criterion (as ours do) provide a representative sample of dream content, as we are able to access it in spontaneously recalled dreams.

We would welcome some evidence as to the reliability or validity of the data collection method now advocated by Revonsuo and Valli, viz. that one should collect 10 or so dreams from each member of a group of participants. Dream series (ie a set of dreams from a single dreamer) are often used in research, but how many dream reports are needed to yield a representative sample of an individual's dream content? The only empirical work we know of that investigates the use of dream series as a data collection method was again conducted by Domhoff and colleagues. Large series of dreams from individuals were sub-sampled, and it was demonstrated that series of less than 20 dream reports from a single subject provide highly unreliable estimates of that individual's dream content (Domhoff, 1996; Domhoff & Schneider, 1998). This work indicated that samples of approximately 75–100 dream reports from a single individual are needed to give a reliable measure of their dream content.

We thus find Revonsuo and Valli's a priori rejection of the MRD method of data collection baffling, given its empirical derivation and careful validation. They provide no methodological evidence in support of their suggested alternative method, which to the best of our knowledge, is in fact invalidated by the available empirical evidence.

## 3. Are our figures reliable?

In support of their contention that the MRD yields unreliable results, Revonsuo and Valli claim that our figures differ across the 2004 and current studies (Malcolm-Smith & Solms, 2004; Malcolm-Smith, Solms, Turnbull, & Tredoux, in press). While it is true that our two SA samples are comparable (although independent—the latest study is not a re-analysis of old data), we would like to point out that the coding definitions differed somewhat across the studies. In the 2004 study, we reported finding *physical threats* in 21% of the dream reports, while in the current study, we found *realistic physical threats* in 8% of SA dream reports. The critical difference is that in the latter study, 'physical threats' were constrained to be *realistic*—hence the dramatic change in frequency. Dreams often feature unrealistic or impossible physical situations, including physical threats. The difference in the reported figures is thus not the result of unreliability in our data collection method. In fact, we would like to draw attention to the remarkable consistency in those figures which *are* directly comparable: realistic life threats were found in 7.04% and 8.48% of reports (2004, p. 225; and in press, Section 3.2.3); and realistic escapes from such threats were found in 2.7% and 1.4% of reports (2004, p. 225; and in press, Section 3.2.4). We thus see a difference of less than 1.5% points across the two studies. In this methodologically challenging field we take this as an encouraging indication that our methods are in fact highly reliable.

## 4. Defining the level at which threat in dreams must be studied

Both in the conclusion and in the introductory section of their commentary Revonsuo and Valli argue very strongly that threat in dreams must be studied at the level of the individual. They take great issue with the term 'gradient of activation', assuming that it refers to the level of activation within the individual dreamer. They state that we are 'incapable of detecting any gradient in activity level' as we do not specify precisely where any individual would fall on such a gradient. This is a misunderstanding of our clearly stated aim—we specify that TST predicts different levels of activation in the different populations we sampled, dependent as this is on differential exposure to threat. Revonsuo and Valli appear to be arguing that

between-groups research design is an invalid technique. This is not an assertion we have previously encountered in the field of empirical dream content research.

Our groups were very clearly differentiated on exposure to ecologically valid threat. Each participant was required to describe any life-threatening event they had experienced, and blind raters ensured that only recent, actual ancestral threats were included in the analysis. SA participants were four times more likely to have had such an experience than their Welsh counterparts. Examples of recent life-threatening events reported by SA participants included: being shot in the stomach during a robbery; waking to find a group of gun-bearing burglars in the house, and subsequently being threatened with death and pistol-whipped; breaking up with an abusive boyfriend who returns, breaks into the home, beats and rapes the participant, and so on. No event which did not directly threaten the life of the participant or that did not occur recently was included. It is clear that had we included threats to significant others and family members, the figures for the SA sample would have increased exponentially. The subjective accounts of how threatened, and by what, the participants felt on a daily basis also clearly show that SA participants feel extremely threatened by violent crime.

It is thus perfectly reasonable to hypothesize that *on average* one should find a difference between groups when those groups are specifically differentiated on relevant criteria—in this instance, recent exposure to actual life-threatening events.

Revonsuo and Valli go on to criticize our dichotomous coding category as invalid, because activation of the threat simulation system is supposed to be a continuous phenomenon. The precise methodological implications of this statement elude us. The argument is misleading—our analysis is not based on a dichotomy, we compare the relative frequency of certain content codes across the two groups. All dream content studies, including those of the authors of TST, are constrained to use categorical data—frequencies and percentages derived from coded content categories. It is not clear exactly how they would like this situation to be remedied—it appears to be an inescapable function of the type of data we work with. Using frequencies and percentages as indicators of the presence of threat scripts in dreams, and thus as an indirect measure of the operation of the hypothesized TS mechanism in dreams, seems to us to be a reasonable approach, and one which has been used in all the studies conducted by TST's authors themselves.

## 5. Clarifying which threats are relevant to a selection-based argument

Revonsuo and Valli argue that our definition of threat in dreams is too narrow. We focus on realistic threats to physical survival, whereas in Revonsuo and Valli's content analyses, psychological and social threats may be coded as being of equivalent severity to physical survival threats. Additionally, they complain that we misunderstand the concept of adaptation: social and psychological threats in dreams have nothing to do with psychosocial well-being in real life. We would like to stress that we do not misunderstand, we simply do not find their arguments tenable. The authors of TST raised a similar point in their commentary on Zadra, Desjardins, and Marcotte (2006) study, and these authors succinctly addressed the issue (see Desjardins & Zadra, 2006). TST's authors consistently dismiss evidence that individuals who do dream often of threat (e.g., those with PTSD) are not well adapted in waking life as being irrelevant to TST. We agree with Desjardins and Zadra in having difficulty accepting the argument that waking social and psychological adjustment has no impact on inclusive fitness. But leaving this aside, our main argument remains that TST's authors consistently fail to adequately explain how psychological and social threats are relevant to TST when they occur in dreams, but are not relevant in waking life.

With regards to defining relevant threat in dreams, it should be pointed out that this argument can be traced back to TST's original formulation in *Behavioral & Brain Sciences* (2000a; 2000b). We would like to emphasize that Revonsuo himself established the definitional criteria we employ. Revonsuo (2000a) initially argued that dream content evidences the activation of ancestral threat scripts, through a predominance of aggression and pursuit themes. In the peer responses to that paper, numerous dream researchers insisted that current dream content does *not* reflect a predominance of threat. To counter this evidence, Revonsuo (2000b) then argued that the context of modern human life does not provide ecologically valid cues to activate the system—ancestral type threat events are needed to activate the TS mechanism, as these are the threats it evolved to handle. He concluded that one should thus not expect to see great numbers of threats in the average western dreamer—the mechanism will only activate in response to ecologically valid threats (Revonsuo, 2000b).

Our contention is that ancestral or ecologically valid threats that are largely absent from the modern context are precisely *severe threats to physical survival.* Severe social and psychological threats abound even in comfortable western cultures. If severe social and psychological threats are not ecologically valid cues capable of activating the system, then how can TST's authors simultaneously argue that their presence in dreams is evidence for activation of the system?

We contend that social and psychological threats must either be seen as part of the set of threats relevant to inclusive fitness, or they must not. If they are relevant to inclusive fitness (we believe they are) then we should expect the threat simulation mechanism to be active in any individuals and social groups exposed to these types of threat. If, however, we must work with the constraint that only severe threats to physical survival activate the TS mechanism (as Revonsuo originally argued) then we must assume that the only threats which this evolved mechanism is able to deal with are realistic threats to physical survival. Thus, only evidence of the presence of these threats in dreams can serve as evidence supporting TST.

As we state in our paper, we do not dispute broadening the definition of threat, to include social and psychological threats. Nevertheless, we maintain that if these threats are relevant to fitness in dreams, they are also relevant to fitness in real life. If these threats are to be included in dream report analysis, they must also be included in the set of cue events that activate the TS system. This would entirely change Revonsuo's original predictions regarding which groups are likely to

have highly active TS mechanisms, as severe social and psychological threats occur broadly in every society. And as seems evident from the established content norms, threat and threat avoidance are not over-represented in dream content.

## 6. Conclusion

In sum, we do not believe that the methodological criticisms raised in Revonsuo and Valli's commentary are substantive. Despite these authors' assertions, we do not perceive marked discrepancies in the data gathered either in their group's research, or in that of the three independent tests that have been conducted by others (viz. Malcolm-Smith & Solms, 2004; Malcolm-Smith et al., in press; Zadra et al., 2006), although there certainly are discrepancies in how that data is interpreted and presented.

As we pointed out in our initial argument, when the incidence of threat most relevant (i.e., physical threat) to the arguments around TST is considered, it appears that upwards of 80% of dream reports do not feature this type of content (Malcolm-Smith & Solms, 2004; Malcolm-Smith et al., in press; Revonsuo & Valli, 2000; Zadra et al., 2006). In more recent studies by TST's proponents, featuring traumatized children (Valli et al., 2005, Valli, Revonsuo, Palkas, & Punamaki, 2006), the main group differences feature social and psychological threats, not physical or life-threatening events. We have already elaborated on the juggling of categories seen in their 2005 paper (Malcolm-Smith et al., in press). In their 2006 paper, the breakdown of types of threat in dream reports goes as follows (see p. 73): the traumatized group did not report more life-threatening events than the controls (25.6% vs 28.3%, respectively), and did not report more physically dangerous events (10% vs 10.7%). The only category in which they reported more threat was socially, psychologically or financially severe threats (30.4% vs 18.5%). The authors, however, chose not to conduct analysis on the subcategories, but only on the totality of reported threat. Their conclusion that the trauma group reported more threat, is thus an artefact of that group scoring higher on a single category, and as we have argued repeatedly, that category is not the one most directly relevant to the predictions or logic of TST. These findings all indicate that individuals living in contexts where physical threat is common do not show greater incidence of this type of threat in dreams. This is very difficult to explain in terms of TST, and strongly suggests that the theory is incorrect.

Furthermore, we stress again that above and beyond the low incidence of relevant threat in dreams, evidence surrounding the lack of successful avoidance responses is critical. It casts grave doubts on the feasibility of the proposed selection mechanism.

TST's authors continue to ignore or gloss over evidence of this nature, whether it occurs in their own data, or in that gathered by independent researchers. For example, in the current commentary Revonsuo and Valli state that the evidence shows that dreamers tend to respond appropriately to threats, and that threats are more often resolved than not, citing their own data and that of Zadra et al. (2006). This is a misrepresentation of the available data. As Desjardins and Zadra (2006) initially pointed out in their response to Valli and Revonsuo's (2006) commentary on their paper (Zadra et al., 2006), TST's authors show a highly unfortunate but consistent tendency to focus on evidence that confirms their hypotheses, while ignoring or misrepresenting data that disconfirms it. In contrast to the picture presented in Revonsuo and Valli's commentary, Zadra et al.'s (2006) data in fact showed that successful escapes are exceedingly rare, and that in most cases (40%) the dream threat was fulfilled. It is astonishing that Revonsuo and Valli present their distorted interpretation again after Desjardins and Zadra drew attention to this precise point in their 2006 response. Another example of evidence that is truly problematic for TST, is that in Valli et al., 2005 study (pp. 201–202), the group of traumatized children faced death or severe losses as a consequence of dream threats significantly more often than did the Finnish control group. Only dreams from the traumatized group featured death of the dreamer. How can TST account for this? In the 2006 paper, no information on consequences of the threat is provided, but in terms of the dreamers' reaction to the threat (p. 74), a greater proportion of controls reacted to the threat (32.9% vs 25.8%), while no response was reported more frequently by the traumatized group (40.5% vs 32.9%). Additionally, from this information it is clear that most often the dreamer did not respond to the threat. This type of data, problematic to say the least for TST, is seldom mentioned when its authors review the available evidence.

The data now available appears to be uniform on this subject—successful avoidance responses tend not to occur in dreams (Bulkeley, Broughton, Sanchez, & Stiller, 2005; Malcolm-Smith & Solms, 2004; Punamaki, 1999; Revonsuo & Valli, 2000; Valli et al., 2005, 2006; Zadra et al., 2006). Successful escape from relevant threat occurs in less than 3% of dreams (Malcolm-Smith & Solms, 2004; Malcolm-Smith et al., in press; Zadra et al., 2006). Where, then, is the mechanism that bestowed adaptive advantage?

One cannot avoid the impression that Revonsuo and colleagues are not really open to the accumulating evidence against their theory, though we hope that our survey of the data will provide them with an opportunity to re-evaluate. It seems worth reminding ourselves that scientific progress requires, and indeed thrives on, the generation of testable hypotheses—of which TST is an entirely respectable example. However, not all hypotheses can be correct, and some interesting ideas have to be jettisoned in the absence of empirical support.

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