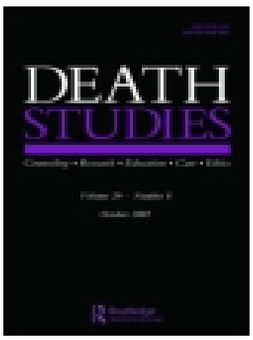

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Epistemological differences in the discussion of the interpersonal theory of suicide: A reply to the response

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ABSTRACT

This paper is a reply to Smith and colleagues' response to our critique of the interpersonal theory of suicide (IPTS). They believe that we mischaracterize and misinterpret aspects of the IPTS. Here, we argue that the problem rather seems to be that we are discussing the IPTS from two different epistemological "planets."

We wrote the paper to initiate what we believe is a much-needed debate about the theory that currently is dominating the suicide research field. We are, therefore, very happy that Smith, Schuler, Fadoir, Marie and Basu took the time to write a response. They admit that reviewing our paper objectively was difficult, since much of Smith's own research has focused on the IPTS (or ITS as referred to by Smith et al., 2019). We appreciate their honesty and are happy that they actually do agree that "we need papers that challenge our assumptions."

The starting point for their response is that they believe we mischaracterize and misinterpret aspects of the IPTS. The problem, however, rather seems to be that we are discussing the IPTS from two different epistemological "planets." Smith et al.'s argumentation seem to be firmly grounded in the biomedical model, with its inherent linear cause-and-effect thinking and where they attempt to *explain* suicide in terms of risk factors (or underlying causes) found by means of quantitative research. The unavoidable consequence is reductionism. This reductionism is indeed reflected in their emphasis that the theory "attempts to *parsimoniously* explain" (italics added) the relationship between contextual factors and suicide through IPTS's three components, which they characterize as "mediators or mechanisms." We, on the other hand, argue that such a complex and contextual phenomenon as suicide, should not be explained, particularly not parsimoniously, and especially not by internal factors only. Instead, we aim towards a more contextual *understanding* of suicide, by focusing on the meaning(s) suicide might have for the individual in their respective contexts. The only way to do that meaningfully is by means of qualitative research, where we can study how individuals may have interpreted themselves, their actions, and their surroundings (Hjelmeland & Knizek, 2016; see von Wright, 1971/2004 for the distinction between explanation and understanding in the philosophy of science). Here, we agree with Bruner (1990, p. xiii) who asked: "Are not plausible interpretations preferable to causal explanations, particularly when the achievement of a causal explanation forces us to artificialize what we are studying to a point almost beyond recognition as representative of human life?"

From our two different epistemological "planets", it is also clear that we understand (the concept of) context differently. Smith et al. disagree that IPTS ignores context and find it odd that we think it does. To explain how context is *integrated* into the IPTS, they mention some special groups that have been studied, for example, veterinarians and maintain that this shows how "the context of one's profession will make more likely exposure to capability enhancing experiences." To us, however, one's profession is merely a *variable* and not at all how we understand the context. In our understanding, each veterinarian lives and works in his/her own specific context (or contexts in the plural, rather); in their professional life, in their private life (family), as well as in their community. It is in interplay with these contexts, an individual might develop suicidality. Suicide must, therefore, be understood in the historical, social, ideological, political,

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economic, cultural, and gender-related contexts in which it occurs.

From our point of departure, context is not a measurable variable, it simply cannot be reduced to one or even several measurable variables. Qualitative research is thus crucial, if we want to understand more of the context contributing to suicidality than what is possible in quantitative research (Hjelmeland & Knizek, 2016). Smith et al., however, "have concerns about the inductive approach of qualitative studies finding generalizable results." Like Joiner (2011), they reduce qualitative research to a mere hypothesis generating a tool that helps "improve our deductive quantitative research" (Smith et al., 2019). It is actually astonishing that some researchers still – in 2019 – view qualitative research as merely a hypothesis-generating tool because they believe their findings cannot be generalized. It is after all 25 years since Steinar Kvale (1994) published his paper "Ten standard objections to qualitative research interviews," where he eloquently rebutted every single one of them (including the one on generalization).

We maintain that suicide must be understood in light of the life history of the individual; in a life-course-perspective (Kjølseth, 2010), and not just through some decontextualized risk factors (human beings are not mechanical machines reacting automatically to risk factors whether we are suicidal or not). Different individuals may have developed their suicidality or a vulnerability for suicide, through many different ways/experiences during their life-course. Hence, with regard to suicide, we, therefore, ask with Kvale (1994, p. 166): "why generalize?" that is by means of *statistical* generalization.

Smith et al. seem to have no reservations with regard to employing results from quantitative risk factor research, organized through the framework of the IPTS, to "arrive at a clinical formulation for an individual's chronic and acute suicide risk." This, in spite of the fact that risk factor-based suicide risk assessments have proven useless to predict suicide at the individual level (Large & Ryan, 2014), and that "suicide risk models are not a suitable basis for clinical decisions" (Large et al., 2018). At the clinical level, we should rather employ *analytical* and/or *theoretical* generalization (Hjelmeland & Knizek, 2011). Then, it is the users of the knowledge (i.e. clinicians) who are responsible for deciding whether or not the findings are applicable for their specific clients. The validity of such generalization thus depends on the relevance of the compared characteristics/situations, which in turn depends on rich and detailed descriptions of the cases (Kvale, 1997). Thus, for clinical work, qualitative research actually is much more relevant than quantitative research.

We argued that the IPTS is not a testable theory. Smith et al. suggest that it could be tested by "longitudinal studies such as the ecological momentary assessment approach (EMA)"; an approach that "assesses the moment-by-moment report of experiences in a manner that could examine the more miniscule over-time variations in suicidal desire." By this proposal, they perpetuate our (and Paniagua, Black, Gallaway, & Coombs, 2010) critique that studies purporting to test the IPTS, are actually not testing it at all. The IPTS is a theory on *suicide*, and is, therefore, impossible to test by means of EMA since this method (specifically said to be prospective) requires participants to frequently report on the variables under study during a specific time period (when alerted by their smartphones). The study Smith et al. refer to on this method, is indeed a smartphone-based EMA study on suicidal *ideation* and risk factors for the same (Kleiman et al., 2017). After discussing a number of other methods that could be used to falsify the IPTS, Smith et al. actually conclude that such studies would neither be ethical nor practically feasible and hence that the IPTS only is "theoretically falsifiable." If IPTS only is theoretically falsifiable, why waste so much time, efforts and money testing it empirically?

From their epistemological "planet", Smith et al. never question the components constituting the IPTS. They seem to take them for granted and "*believe* that the ITS is practically useful and *can be considered a good theory*" (italics added). Researchers just need to work towards "more clear operational definitions of theory constructs" (which is difficult to see how it can be done without disregarding the context) and better methods by which to measure them. Moreover, they argue that "it is *hoped*" that the theory "will lead to identifying risk factors specifically associated with suicide mortality," and that "such an approach *may potentially be superior*" (italics added). They claim that we have misinterpreted the IPTS, but seem to base their own faith in it on beliefs and hopes. Although some of the theory's components certainly can play an important role in some suicides, we have argued that the IPTS as such should be discarded because it makes no sense to understand such a complex and contextual phenomenon as suicide within such a reductionistic framework. Nothing in Smith et al.'s argumentation has convinced us otherwise.

Explaining suicide by internal factors only may also have serious consequences for suicide prevention. If

suicidality is understood only as something internal, suicide preventive efforts will most likely be focused on the individual, which, for instance, is reflected in the popular belief that diagnosing and treating mental disorders will prevent suicide. Numerous studies have demonstrated clearly how crucial for the development and maintenance of suicidality are, in different ways for different individuals, the social, cultural, and political or structural contexts where people live their lives (e.g. Hjelmeland & Knizek, 2016; Staples & Widger, 2012; White, Kral, Marsh, & Morris, 2016). It is, therefore, towards the context suicide preventive efforts should be directed. Indeed, that would be *pre*vention, rather than just *inter*vention (Shahtahmasebi, 2018).

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