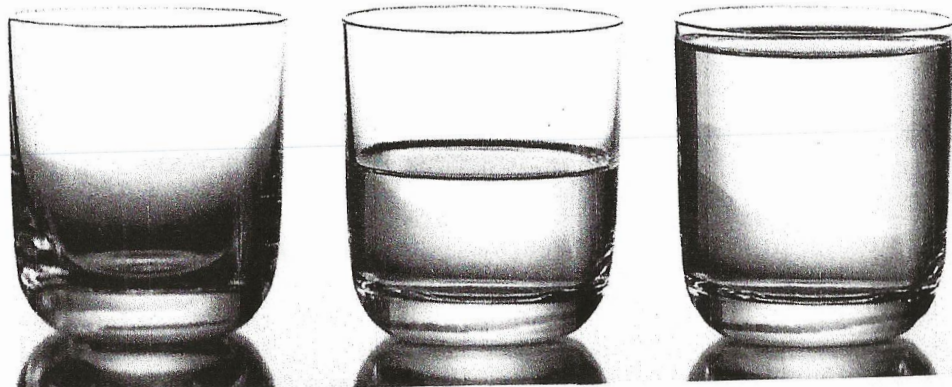


# CRITICAL SUICIDOLOGY

## Transforming Suicide Research and Prevention for the 21st Century

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### A Critical Look at Current Suicide Research

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This chapter critiques contemporary suicide research. The point of departure is a descriptive analysis of the research published/prioritized in the three main international suicide research journals during the years 2011–12: *Archives of Suicide Research (ASR)*, *Crisis*, and *Suicide and Life-Threatening Behavior (SLTB)*. The content of each article published in these journals was scrutinized to determine the focus, method, and findings of each study, as well as where each study was conducted. As described below, a large proportion of the publications came from quantitative, repetitive risk-factor studies that were unable to provide much new or useful knowledge. The problems and limitations of quantitative research in general are discussed and alternative research approaches are suggested throughout. It is, for instance, demonstrated how many of the quantitative studies might have benefited from including a qualitative component and also how “pure” qualitative studies are able to produce new and useful knowledge. Finally, the fact that the vast majority of the publications came from “the West” is problematized. The chapter ends by arguing for a change of direction in suicide research as well as in publication practices and priorities.

#### Current State of Affairs in the Three Main Suicide Research Journals

In a recent review of all the publications in *ASR*, *Crisis*, and *SLTB*, Goldblatt and colleagues (2012) found that in the period 2006–10 these journals



published a preponderance of epidemiological studies – 41 percent, and 45 percent if “pure” risk factor studies were added.<sup>1</sup> They conclude that the research published was narrower than intended by the journals’ parent organizations – the International Academy of Suicide Research, the International Association of Suicide Prevention, and the American Association of Suicidology, respectively – and they emphasize that idiographic and clinical studies as well as case reports are critical to further our understanding of suicidal behaviour. Here, I take a closer look at the *content* of recent publications (2011–12) in each of these journals, starting with the most comprehensive one (in terms of number of articles published annually), *SLTB*.

### Suicide and Life-Threatening Behaviour

Scrutinizing recent publications in *SLTB* is particularly interesting in light of the editor-in-chief’s editorial titled “Scientific Rigor as the Guiding Heuristic for *SLTB*’s editorial stance” (Joiner, 2011). There, Joiner stated that the field needed “hypothesis testing with fair tests using valid and quantifiable metrics” (p. 471) and that in terms of journal space, experimental studies would be prioritized over quasi-experimental and nonexperimental ones, and multistudy papers would be prioritized over single-study ones, as would longitudinal over cross-sectional studies. He emphasized that *SLTB* would also publish, for instance, nonexperimental, single-study, cross-sectional, and qualitative research (notably mentioned last and explicitly reduced to a hypothesis-generating tool), but added that “if push comes to shove – and it often does, and I expect, will continue to do so” (p. 472), such studies would not be prioritized.

This line of argumentation by a journal editor is somewhat puzzling. First, it reveals a rather limited view of what constitutes rigorous research methodology. For instance, *conducted properly*,<sup>2</sup> qualitative research certainly is a rigorous scientific approach (Kvale, 1994). Second, to focus primarily on research methodology in this way is, in my view, to turn things upside down. We need to decide which research questions we currently need answers to *before* we choose the appropriate method to answer them. For instance, we now have thousands of risk factor studies, yet we still understand very little about when, where, how (if at all), and for whom the found risk factors are related to suicide and why it is that the vast majority displaying one or more of them do not kill themselves. To improve our *understanding* of this we need qualitative research (Hjelmeland and Knizek, 2010, 2011a, 2011b).

Hjelmeland and Knizek (2011a) have argued that much of the quantitative (risk factor) research currently produced in the field is uncreative, repetitive, and unable to provide new knowledge of any significance. Joiner (2011, p. 472), however, maintains that those who voice such critiques have viewed quantitative research in a biased way or simply “have not read widely enough,” and that “to be conceptually innovative and at the same time to adhere to rigorous empirical standards requires, if anything, more and certainly not less creativity.” Very well, then ... Let’s take a critical look at *all* the articles published in *SLTB* during the years 2011 and 2012 to see whether they live up to the editor’s standards with regard to creativity, and, more importantly, whether they have contributed any significantly new and useful knowledge.

During 2011 and 2012 (five of the six issues published at the time of writing), *SLTB* published 110 articles. Joiner certainly kept his “promise” of not prioritizing qualitative studies: only two of the articles (mainly quantitative) contained a qualitative part.<sup>3</sup> One of these was rather quantitative in nature in that it consisted mainly of a counting of categories (Huisman, Kerkhof, and Robben, 2011), and in the other (Tousignant et al., 2011), it was unclear how the qualitative data were analyzed/used. More than half (59 percent) of the studies were looking at some risk factors, one way or another, for suicidal ideation, nonfatal suicidal behaviour, and/or suicide. After reading through *all* the papers published in *SLTB* in this period, *creativity* was not what came first to mind. On the contrary. There were many traditional cross-sectional risk factor studies looking at *a few*, most often commonly known, risk factors with or without controlling for a few others, and analyzed in complete isolation from the context of the (individual) participants. (This was contrary to Joiner’s ambition to prioritize longitudinal studies, of which there were only a few.) When reading through all these articles, I repeatedly “learned” that depression and substance abuse are risk factors for suicidal behaviour. That, we have known for a long time. What we don’t know is why, or why most people who are depressed and/or who abuse substances do not kill themselves. Although it has become more common for researchers to at least analyze their data in relation to some theory, or to test some theory (most often Joiner’s interpersonal-psychological theory of suicide; Joiner, 2005; Van Orden et al., 2010), much of the research still is rather uncreative, repetitive, and reductionistic.

Authors should, however, be granted quite some creativity in terms of constantly finding new groups in which to look for the same old (or new)



risk factors. In addition to the usual community samples (youth, and young, middle-aged, or old adults of different ethnicities), patient samples (patients with bipolar disorder, schizophrenia, psychosis, PTSD, anxiety disorder, or psychiatric in-patients or out-patients, general hospital patients, different groups of suicidal patients, for instance, repeaters and nonrepeaters), and military personnel (the Army National Guard, Air Force personnel, deployed soldiers participating in combat, post-deployment soldiers, different groups of veterans), some new and/or relatively special groups were investigated, such as, for example, separated men, depressed women with childhood sexual abuse histories, victims of sexual abuse and/or intimate partner violence, criminal offenders, homeless men, individuals diagnosed with body dysmorphic disorder, traumatic brain-injury patients, severely obese bariatric surgery-seeking individuals, people killing themselves by means of Russian roulette, people living in homes with weapons, and people living at high altitudes.<sup>4</sup> Focus is indeed necessary in research, but why is it necessary to study a few, mostly commonly known risk factors in constantly more peculiar/specific groups when we still know little about the mechanisms linking the risk factors to suicidality in *some* members of any group? Probably the rationale is connected to the common call for replication of a study in less heterogeneous groups. However, if participants' context is taken into consideration, as it should be, any sample will only be "homogeneous" with regard to one or a few variables. Trying to limit heterogeneity by studying constantly more specific/peculiar groups is therefore a rather futile task.

Examples of risk factors found in *SLTB* in 2011–12 were the traditional ones: various kinds of demographic variables (e.g., age, gender, race, level of education, marital status), mental health-related problems (negative affect, depression, PTSD, parental psychiatric problems, psychiatric hospitalization), abuse of and/or dependency on various substances, severe life events, hopelessness, previous suicidal behaviour, family suicide history, history of sexual abuse, and so on, and so on. Although some relatively "new" risk factors were also found – for instance, having delinquent friends, combat exposure, living in a loose- or intermediate-knit family, body dysmorphic-related food intake, or court disposition to shelter care, to name but a few – what was most often found were the same old risk factors that have been demonstrated over and over again for decades. That is, factors that when added up make life so unbearable for (some) people they decide to end it. Such research is not bringing us much closer to *understanding* what makes some people suicidal (Hjelmeland and Knizek, 2011a).

### Archives of Suicide Research

*ASR* is the smallest of the three journals in terms of annual number of papers published but just as "bad" as *SLTB* in terms of publishing traditional risk factor studies. Of the sixty-one articles published in 2011–12, 62 percent were risk factor studies of some sort. These included some new (in relation to *SLTB*) groups under study – namely, forensic psychiatric patients, offspring of bipolar parents, self-burning cases, cancer patients, trauma-exposed substance users in residential treatment, and people with a diagnosis of kleptomania. Three of the published studies (5 percent) had used qualitative methodology (two "pure" qualitative studies and one using mixed methods).

### Crisis

Of the eighty articles (with the editorial in each issue excluded) published in 2011–12, "only" 29 percent were looking at risk factors, one way or the other, for different kinds of suicidal behaviour. In general, more or less the same risk factors as in *SLTB* and *ASR* were studied in more or less the same groups. One new group (relative to *SLTB* and *ASR*) studied here was veterinarians, reflecting the "creativity" of constantly finding new groups in which to study risk factors. Nine of the studies published (11 percent) had used qualitative methodology (three had used both quantitative and qualitative methods). In addition, one study (Mugisha, Knizek, Kinyanda, and Hjelmeland, 2011) described the challenges of conducting qualitative research in a developing country.

### Do Quantitative Risk Factor Studies Contribute Useful/ Meaningful Knowledge?

Authors often state explicitly that they have found the same risk factors as in previous studies. In quantitative research, replication *is* a virtue, but shouldn't there be a limit to how many times it is necessary to *publish* a finding that depression is a risk factor for suicidal behaviour? Sometimes authors report that their findings are at odds with what has been found in other studies. Results sometimes even vary *within* the same study depending on how variables are collapsed in the analysis, thus demonstrating the futility of such an approach. Sometimes as well, surprising findings are reported. However, it should come as no surprise that findings vary from one study to the next, or should we say from one context to the next. When contradictory results are found across studies, one plausible explanation is that the



relationship between a risk factor and an outcome variable is dependent on the presence or absence of other factors (Smedslund, 2009) – in other words, the context is crucial. According to Smedslund (2009, p. 778, p. 779), “statistical methods are unlikely to produce more than local and unstable fragments of knowledge” because personal processes “are influenced by an indefinitely high number of factors, [...] sensitive to outcomes and, hence, always changeable; the regularities that can be found stem from participation in shared meaning systems already implicitly familiar, and they are unique.” So we need qualitative research to develop a deeper *understanding* of what suicide is all about, since in such studies we can take more of the context into consideration than is possible in quantitative (risk factor) studies (e.g., Hjelmeland and Knizek, 2010, 2011a, 2011b). In his argument in *SLTB* for prioritizing multistudy papers over single-study ones, because of “the emphasis on reproducibility,” Joiner (2011, p. 472) completely disregards the significance of context.

As demonstrated above, yet contrary to his own statement, Joiner seems to prioritize publication of cross-sectional studies of a *few* risk factors (as do other journal editors). However, as maintained by Toomela (2007, p. 12), “when mind is studied as a pile of independent fragments, the fundamental problem arises – effects can be attributed to wrong causes.” We can easily replace “mind” in this quote with “suicidal behaviour.” Here is a concrete example of what is problematic with such research: in their cross-sectional study of youth, Messias and colleagues (2011) found that five hours or more of video gaming and Internet use increased the risk for suicidality, whereas one hour or less had a potential protective effect. An interesting question here would be *why*, if at all, excessive video game and Internet use are connected to suicidality. Or is this instead a case of the third variable problem? This sort of a cross-sectional study of two isolated risk factors cannot tell us anything about causality.<sup>5</sup> The authors speculate that excessive video gaming may reflect mental health problems, which suggests that mental health problems are “responsible” for the suicidality. If so, it would have been interesting to find out why these teenagers have mental problems, if indeed they do. This is where qualitative research comes to the fore (Hjelmeland and Knizek, 2010, 2011a, 2011b). A qualitative study that interviewed these teenagers and considered their individual contexts in the analysis could provide some answers to the questions raised in Messias and colleagues’ study. Perhaps young people who play video games for one hour or less every day have parents who care, whereas those who play for five hours or more each day do not (as reflected in the lack of restrictions on how they spend their

time?). In other words, perhaps it is parental care, or rather the lack thereof (and here, excessive video gaming may or may not be a symptom), that is contributing to the suicidality (in addition to other factors, of course). Other factors may also be important. However, a study like the one conducted by Messias and colleagues is unable to reveal these, in contrast to qualitative in-depth interview studies in which we can ask *why* the teenagers spend five hours or more every day playing video games, besides letting them tell their stories about their own suicidal thoughts or suicide attempts. Through rigorous qualitative analysis, we might find out whether various issues are connected to their suicidality, and if so, how.

### Psychological Autopsy Studies

One particular type of risk factor study should be commented on specifically: psychological autopsy studies (PA-studies). PA-studies are the most common approach to searching for risk factors for suicide. Just a few empirical PA-studies were published in the three journals during 2011–12; however, *SLTB* did publish two comprehensive “policy papers” outlining “the next generation of psychological autopsy studies” (Conner et al., 2011, 2012). In Part I of these papers, Conner and colleagues (2011, p. 595) maintain that “the PA remains the only validated approach to explicate the psychological and contextual circumstances near to suicide” and that “they will continue to be the primary method for assessing the causal pathways that lead to suicide for the foreseeable future.” This, in spite of the *numerous* methodological problems inherent in such studies, problems that were carefully outlined by Pouliot and De Leo (2006) under the following rubrics: (1) limitations of the medical model (the framework within which most PA-studies are conducted); (2) problems with the diagnostic instruments used (nonstandardized/ill-defined instruments, or standardized instruments not validated for use by proxies; interviewer bias due to the semistructured nature of the instruments); (3) problems related to the informants (e.g., emotion/attitude-related response bias, no systematic control of type of informants or their relationship with the deceased); (4) problems related to the interviewers (e.g., no control over their psychological and social characteristics, or of their training, if any; interviewer perception bias/error); (5) problems related to the time between suicide and interview; and (6) various problems related to the control groups. Still, in spite of their own arguments, Pouliot and De Leo conclude by praising PA-studies.

PA-studies have mainly focused on mental disorders – indeed, they constitute the main “evidence base” for one of the most established “truths” in



suicide research, namely, that almost all who kill themselves suffer from one or more psychiatric disorders (e.g., Cavanagh, Carson, Sharpe, and Lawrie, 2003). Yet I wonder whether the proponents of PA-studies actually have missed one of the most fundamental issues in research, which is the importance of choosing a method that is appropriate for answering the research question. This leads to a very important question: Is it really possible to assign *valid* psychiatric diagnoses to someone who is dead by interviewing someone else, often many years after the suicide? As I have extensively argued elsewhere (Hjelmeland, Dieserud, Dyregrov, Knizek, and Leenaars, 2012), I say no. When the actual questions asked in such studies are scrutinized, even if standardized instruments have been used, it becomes clear that many of them cannot be answered *reliably* by anyone other than the person being diagnosed. Hence, as a diagnostic tool, PA-studies are methodologically flawed and should be abandoned (Hjelmeland et al., 2012). Conner and colleagues (2011) acknowledge the methodological problems, but they still argue for the continued use of PA-studies as a diagnostic instrument. They make a series of recommendations on how to improve the data on mental disorders, however. In my view, none of these recommendations hold water if some (in actual practice, quite a few) of the questions asked in the diagnostic process cannot be answered *reliably* by proxies (for details, see Hjelmeland et al., 2012). And since validity requires reliability, PA-studies cannot answer the research question (about diagnosis) and should therefore not be used for this purpose (such studies may, however, be useful for issues other than psychiatric diagnostics, especially if the studies are qualitative; see an example of this below). One important consequence of all this is that the "evidence base" for the truism that almost all of those who kill themselves suffered from one or more psychiatric disorders is actually rather weak (Hjelmeland et al., 2012).

### RCT-Studies

At the top of Joiner's (2011) list were experimental studies, and indeed, randomized controlled trials (RCT-studies) are medical research's "gold standard." Researchers resort to such studies in an effort to "control out" all factors they have no control over by means of randomization. Here, a quote from Smedslund (2009, p. 653) seems relevant: "Researchers relying on RCT ... present themselves as representing a scientific approach, and their opponents as less scientific. They take an exclusively empiricist position and do not recognize that the domain of psychological phenomena may be quite inhospitable to empirical generalization, and that natural science methods

may not be very suitable." In fact, "generalizing the findings presupposes stability of the shared meaning systems and the experimental conditions involved, and that if meanings and consequences change, experimental results are also bound to change ... The findings are typically presented as intrinsically stable whereas in reality they reflect phases in ongoing historical processes" (Smedslund, 2012, p. 790). Here too, then, we have reason to ask whether the methodological approach (RCT-studies) is suitable for answering the research question, at least if the study does not include a qualitative part (e.g., Glenton, Lewin, and Scheel, 2011).

### What Kind of Suicide Research Do We Need, Then?

I need to qualify my rather critical view of (epidemiological) risk factor studies. I am not saying that we don't need more epidemiological studies. Of course we need epidemiological studies, in order to follow the trends of suicidal behaviour in different groups and regions as well as in terms of methods used so that necessary measures at the population level can be put in place as necessary. But how many more studies do we need that point out that depression, alcohol abuse, sexual abuse, and so on are important risk factors for suicidal behaviour?

The authors of most risk factor studies speculate about the relationship between the risk factors and suicidal behaviour. But why are such speculations in quantitative cross-sectional studies of two risk factors considered of higher scientific quality than the results of rigorous qualitative analysis? In his 2011 editorial, Joiner writes that "speculative" qualitative hypothesis-generating studies need to be *novel* to be granted journal space. He does not seem to require the same for quantitative studies. With *some* exceptions, the majority of authors do not even suggest that qualitative follow-up studies might have helped answer their questions. This reflects the negative attitudes – indeed, prejudice – often expressed with regard to qualitative studies (e.g., Joiner, 2011). I suggest that if more such risk factor studies are to be published (at least in the "West," where we now have thousands of such studies), they should contain a qualitative (follow-up) part where the quantitative associations found are investigated in depth. We now have an abundance of risk factor studies that can serve as a base for qualitative studies, so in my view, it will in many (most?) cases suffice to do "pure" qualitative studies (e.g., Hjelmeland and Knizek, 2011a, 2011b).

Most of the risk factor studies published in *SLTB* (and elsewhere) are cross-sectional even though Joiner (2011) specifically prefers longitudinal



studies. One question, then, is whether longitudinal studies really are the answer. They do not always provide much new knowledge. For instance, in one longitudinal study, Vander Stoep and colleagues (2011) investigated whether depression and conduct problems at the age of eleven predicted later suicidality and whether the combination of these two variables was a better predictor than each variable separately. That is exactly what they found. This is hardly surprising, given that for decades, suicidality has been considered a multidimensional malaise (Shneidman 1985): people become suicidal when a number of serious problems add up and become too much for the individual to bear. The really *interesting* question is *why* people are depressed or display conduct problems in the first place. The authors make no attempt to answer that question. In other words, that a study is longitudinal does not necessarily mean it provides any new or useful information. This illustrates why Joiner's (2011) emphasis on methodology as the main issue rather than the research question is problematic.

What about all of the other factors that might contribute to making these children suicidal, or not – that is, what about the context? To choose only two out of an indefinite number of relevant variables and follow only these two seems rather meaningless, or at least far too simplistic and reductionistic. And even when more variables are included – as certainly is the case in many studies – some of these variables are controlled for because they are believed to be confounders. Yet they may *not* be confounders – they may in fact be part of the causal pathway – in which case they should not have been adjusted for. Such an approach can actually mask associations between risk factors and suicidal behaviour (e.g., Rhodes et al., 2011).

Everyone agrees that suicidal behaviour is complex (e.g., O'Connor, Platt, and Gordon, 2011). The Norwegian author Merete Morken Andersen once said that a suicide is as complicated as a life. So it is high time for researchers to start considering this complexity. There may have been a time to study risk factors (I have done so myself), but that time has passed and we need other types of research to advance the field of suicidology. Some have argued that we need more complicated quantitative studies (e.g., De Wilde, 2002). In particular, data mining is now being suggested as a fruitful approach (Baca-Garcia et al., 2006). In data mining, software is used to search for patterns and trends in huge databases; this is seen as a useful approach to studying complex phenomena such as suicidal behaviour (Baca-Garcia et al., 2006). That may be so, and admittedly I don't know much about this method, but to me it sounds a bit like the systematization of what my old statistics teacher called "fishing trips in the data."

The field of suicide research has been criticized for being atheoretical (e.g., Knizek and Hjelmeland, 2007). Certainly, many risk factor studies – as well as other studies – set off in rather headless pursuit of potential risk factors without any theoretical rationale or framework. In recent years it has become more common for researchers to analyze their data within a specific theoretical framework and even to test specific theories empirically. For instance, in *SLTB*, more and more studies are testing Joiner's (2005) interpersonal-psychological theory of suicidal behaviour.

There are, however, numerous problems with quantitative research (e.g., Hjelmeland and Knizek, 2011a) whether a theoretical framework is used or not. A few of these are pointed out here. First, the sociocultural context, which is crucial to suicide research (e.g., Hjelmeland, 2010, 2011), cannot be included in any meaningful way in quantitative analyses, given that culture is not a measurable variable (Jenkins, 1994).<sup>6</sup> Second, there is a limit to how many variables can be included in a quantitative analysis before the interactions become impossible to interpret. The more variables to be included, the larger a sample is necessary, and the larger the sample, the more heterogeneous it becomes (in terms of both individual and contextual factors) and the more remote the research results from the individuals those results are meant to benefit. Besides, and importantly, "despite the obvious importance of finding more specific risk factors and their integration into models ... this approach is inherently limited because these models are still linear, whereas the suicidal process is most likely nonlinear by nature ... Adding more variables, even if they are more specific, will not help overcoming linearity" (Schiepek et al., 2011, p. 662). As maintained by Toomela (2007, p. 16), "quantitative theory may simply be inappropriate for understanding many phenomena in the behavioural sciences." Human beings are complex, intentional, meaning-seeking, reflecting, relational beings who are constantly exposed to and influenced by an indefinite number of factors and fortuitous events in constellations unique to the individual. This precludes the possibility of finding general empirical principles (Smedslund, 2009). Hence, models aimed at predicting suicidal behaviour based on risk factors will always have a rather limited sensitivity and/or specificity, since the outcome for an individual will always depend on a number of (constantly changing) variables not included in the model. Smedslund (2009, p. 786) has further argued that since human beings participate in shared meaning systems, "regularities revealed by research merely illustrate what we already know, explicitly or implicitly" (cf. the basis for hypothesis generation). It follows that mainstream quantitative psychological (and hence suicide) research



cannot contribute any genuinely new knowledge (Smedslund 2009) or knowledge relevant for psychological practice (Smedslund 2012). Based on the above, we can only conclude that some "new" thinking in suicide research methodology is long overdue (Hjelmeland and Knizek, 2010, 2011a, 2011b). Unfortunately, this is not reflected to any large extent in the main suicide research journals.

I maintain that what suicide research now needs more than anything is properly conducted qualitative research. With such research, we could take in more of the complexity since we could do in-depth analyses of information from suicidal people in their specific context(s). In the risk factor studies referred to above, the authors often launch a number of speculations as to how the various risk factors found *might* be related to suicidal behaviour. But few of these studies even suggest (even when that suggestion would have been obvious) that a qualitative component to their own study (e.g., a mixed-method design) or a qualitative follow-up study might have helped answer some of their questions. For example, Hill and colleagues (2012, p. 41), in their quantitative study of precipitating events in adolescent suicidal crises, acknowledge that there are several pathways to suicidal behaviour and that we need "an increased *understanding*" (italics mine) of these. With the differentiation between *explanation* and *understanding* in mind (e.g., von Wright, 1971/2004),<sup>7</sup> the most obvious recommendation would have been to conduct qualitative studies. Hill and colleagues (2012, p. 12) even emphasize that "it is important for the advancement of suicide science that *all* of the pathways associated with suicide-related behaviour are considered in order to generate more accurate theoretical understandings on which to base intervention and prevention" (italics mine). Yet their recommendation for future research is more quantitative studies.

Here are some other examples of quantitative studies published in *SLTB* that would have benefited from a qualitative component to further analyze the associations between variables found in the quantitative analyses. Winterrowd, Canetto, and Chavez (2011, p. 60) in their comparative study of Mexican and European American youth state that "contrary to some evidence ... and our expectations, but consistent with the findings of a study of Latino and African American adolescents ... ethnic identity in this study did not protect against suicidality." This is a typical example of a quantitative study examining the same risk factors in different cultural/ethnic/other groups where the researchers sometimes find the same as was found in other groups/studies and sometimes not. However, authors rarely try to gain a deeper *understanding* about *why* this is the case by means of

qualitative research. Instead they speculate about potential explanations based on other quantitative research. Research like this also overlooks the heterogeneity of the ethnic groups under study.

Another example is De Leo, Milner, and Svetlicic (2012), who in their suicide register study found considerable differences in the relationship between suicide and mental illness when Indigenous suicides were compared with non-Indigenous ones and who offered a number of possible explanations for these differences. Here, a qualitative psychological autopsy study with interviews of several informants around each of the suicides could have contributed valuable information regarding reasons for these differences.

A good qualitative PA-study (of elderly people) published elsewhere can serve as an example of how such studies can contribute new and useful knowledge (Kjølseth, 2010). Quantitative PA-studies have found that almost all elderly suicides suffered from one or more psychiatric disorders (e.g., Waern et al., 2002; 97 percent). Above, I pointed out that such PA-studies are unable to attribute *valid* psychiatric diagnoses to people who have died by suicide (Hjelmeland et al., 2012). In her PhD dissertation titled "Control in Life – and in Death," Kjølseth (2010) emphasizes another problem with such studies – namely, that the traditional focus on risk factors (e.g., mental disorders) takes attention away from other, *more relevant* perspectives (e.g., existential perspectives). Many people lived with a number of the risk factors for years before deciding to kill themselves. In other words, the risk factors in themselves were not essential for the suicide. To understand the meaning these factors had for the individual, they must be viewed in relation to that person's lived experience – that is, in relation to who they were, how they lived their lives on the basis of the existing prerequisites, and how their life experience influenced how they dealt with old age (Kjølseth, Ekeberg, and Steihaug, 2009; Kjølseth 2010). The informants in Kjølseth and colleagues' study describe the subjects as having had difficult lives. They had had to deal with loss, illness, and poverty in childhood and adolescence. In later life, too, they faced considerable challenges. They had been action-oriented achievers, conscientious and highly skilled at work, and they had met challenges with proactive strategies. The informants described them as strong-willed, obstinate, and in need of control. They knew what they wanted and did not allow themselves to be influenced by their surroundings, and this could generate conflict in close relationships. With regard to relationships, they were described as keeping an emotional distance, which made it difficult for those around to help them (Kjølseth, Ekeberg, and Steihaug, 2009). All of this made it difficult for them to accept



and adapt to age-related loss of function, since their self-esteem was strongly associated with being productive and in control. Loss of control revealed vulnerability, and they could not tolerate this. Their emotional pattern contributed to their inadequate ability to develop new coping strategies that included an acceptance of their own limitations (Kjølseth, Ekeberg, and Steihaug, 2009). Functional decline meant they no longer had freedom of action and self-determination. They had a realistic view of the future, and they recognized that their losses were irreversible. They had already lost, or knew they would soon lose, *control* over their lives. These losses affected their identity, and they experienced this as losing themselves. They had accepted death and did not want the future as they saw it coming. In other words, they had made an existential choice to end their lives (Kjølseth, Ekeberg, and Steihaug, 2009). Of course, some of them probably were depressed, but they had reason to be and the depression was not the central issue in their suicides.

With regard to my discussion of publication practice here, it should be mentioned that in a "preliminary" quantitative study published in *SLTB* in 2012, O'Riley and Fiske (2012) also found that the need for control was associated with suicidal behaviour in older adults. They added that this result was consistent with previous research findings (they did not, however, refer to Kjølseth's study, which had been published quite some time before they submitted their paper in January 2012). In keeping with Joiner's editorial statements, *SLTB* prioritizes "preliminary" (as explicitly stated in the paper) quantitative risk factor studies that replicate previous findings over good-quality qualitative studies. O'Riley and Fiske (2012, p. 402), among other limitations, even admit to an "unusually low response rate" of 3.5 percent! Another quantitative risk factor study recently published in *SLTB* had a response rate of only 3.6 percent (Friedlander, Nazem, Fiske, Nadorff, and Smith, 2012). There seems to be a gap between ambition and reality with regard to the scientific quality of the papers (as outlined in Joiner's editorial) published in this journal.

### Comments on Published Qualitative Research

It is necessary to comment on some published qualitative studies. I strongly maintain there is an urgent need for more qualitative suicide research. It goes without saying that qualitative studies need to be of good quality in order to be regarded as scientific (Kvale, 1994). However, some of the qualitative studies published in suicide research (and other) journals are of astonishingly poor quality, methodologically speaking. One common problem is

that the specific method of analysis is sometimes not even mentioned, let alone described. The findings thus appear as nothing but the authors' subjective opinions rather than the result of a rigorous analysis of the data. This is unfortunate, for it (understandably) contributes to negative attitudes towards qualitative research being upheld. Probably, this reflects editors' and reviewers' (as well as authors') good intentions and "open-mindedness"; that said, it also betrays inadequate knowledge of qualitative research methodology. Sometimes it seems as if "no method equals qualitative method"; if the data weren't analyzed by means of a quantitative method, it "must" be qualitative. However, unless the actual method of analysis is described, or at least mentioned (it is not enough to say that "qualitative analysis was conducted"!), it is not a proper qualitative study. If, in a quantitative study, the authors had simply stated that "quantitative analysis was conducted" and reported only some numbers without saying whether they were the result of a *t*-test or chi-square test, for example, I doubt that the paper would have been published. The same standard should apply to qualitative studies. Another example of problems in qualitative publications is the definition of "in depth." In one case, for example, twenty-minute interviews were described as in-depth (Li, Phillips, and Cohen, 2012). How "in depth" can you go in twenty minutes?

### New/Useful Knowledge Developed

Given my rather extensive criticism of much of the research published recently in the three main suicide research journals, a relevant question is this: Did these journals contribute *any* new and/or useful knowledge in 2011–12? They did, and I will mention here one example from each journal. In *ASR*, Bryan and colleagues (2012, p. 96), in a theoretical paper, argue extensively for the need to "understand suicide from *within the context of the military culture*" (italics in original). The authors maintain that suicide prevention in the military has built mainly on the traditional mental health service model and that such models will not be effective because they do not reflect any understanding of the particularities of the *military culture*. They provide a number of concrete illustrations. This is an excellent example of how important the sociocultural context is. The authors' line of reasoning could easily be transferred to other cultures.

From *Crisis*, I will mention a qualitative study by Cutcliffe and colleagues (2012). From previous research we know that discharge from a psychiatric hospital heralds a time of increased risk for suicidal behaviour. Cutcliffe and colleagues' paper discusses *how* discharge contributes to increased suicide



risk. They describe this under the principal theme of "trying to survive while living under the proverbial 'Sword of Damocles'" (p. 267). They liken the experience to that of a severely injured soldier sent back into battle after being treated in hospital. Under this main theme are five subthemes: the need for postdischarge support; feelings of being lost, uncertain, and disoriented; feelings of being alone and isolated; expressions that the discharge in no way prevented them from still feeling suicidal; and the many different actions, not all of them healthy, that the participants engaged in to try to comfort themselves after discharge. Cutcliffe and colleagues point to a need to revisit what is currently considered an appropriate time frame for dealing with suicidality in treatment.

It is noteworthy that *SLTB* published a paper that reviewed the research on self-injury (Chandler, Myers, and Platt, 2011). Chandler and colleagues concluded that the self-injury research was based on biased samples that focused on individualistic explanations of self-injury that located the "responsibility" for self-injury clearly in the individual. They emphasized that this represented a narrow field of vision, and they called for rigorous qualitative research that would take into account the sociocultural context. All of this is very much in opposition to Joiner's 2011 editorial but in keeping with my own view. We need more articles like this. More specifically, we need more open discussion on what kind of research the field of suicidology now needs. Open discussions between professionals disagreeing with one another will help move suicide research forward and, we can hope, out of the dead end of repetitious risk factor research in which it currently, to a large extent, seems to find itself.

### Where Is the Published Research Conducted?

In this analysis of publication priorities in the main suicide research journals, I look, finally, at *where* the research was conducted. This has huge consequences for the findings' usefulness in other sociocultural contexts. In *SLTB*, the vast majority (67 percent) of the studies were conducted in the United States. This is perfectly legitimate, given that *SLTB* is the official journal of the American Association of Suicidology. When we add the studies conducted in Canada, we find that 75 percent of the studies published in this journal were conducted in North America (which makes it relevant to ask whether *SLTB* perhaps should be considered a regional rather than an international journal). Only 5 percent of the published articles came from "non-Western" countries (all of them in Asia).

Reflecting the editor-in-chief's inaugural editorial (De Leo, 2008), *Crisis* is the official journal of the International Association of Suicide Prevention. Although most of publications in *Crisis* in 2011–12 came from the so-called West (85 percent), the North American dominance (31 percent) was less pronounced than in *SLTB*. Here, 15 percent ( $n = 12$ ) of the publications were from Asia ( $n = 8$ ) or Africa ( $n = 4$ ). *ASR*, as the official journal of the International Academy for Suicide Research, had the largest proportion of articles from outside "the West" (20 percent – nine from Asia, two from Latin America, and one from Africa). However, this journal at the same time had a rather heavy North American bias (52 percent of the articles). Besides the sixty articles reported here, *ASR* published one cross-cultural study, conducted in the United States and Turkey.

Whether these publication patterns reflect the respective editors' deliberate choices, or where authors prefer to submit their papers, or both, I don't know. Because of, for example, government priorities and the availability of resources and competence, most of the suicide research is conducted in the so-called West. We need more suicide research from other cultural contexts, especially Asia, Africa, and Latin America. I should mention that Barbara Stanley (2007), in her inaugural editorial in *ASR*, specifically emphasized the importance of cross-cultural studies. Yet during the two years studied here, *ASR* published only one "cross-cultural" study (rather, a cross-national comparison).

### Conclusion

It should be clear by now that I am advocating for a change of direction in suicide research. Qualitative research should be accepted and indeed actively encouraged by the editors of suicide research journals, as they in fact are in the new journal *Suicidology Online* (Hjelmeland and Kapusta, 2013)<sup>8</sup> and in many prestigious psychiatric and psychological journals (cf. the National Institute of Mental Health Consortium of Editors on Development and Psychopathology, 1999, and specific author guidelines for qualitative submissions in many journals). We need a change of view with regard not only to what constitutes good, or scientific, research methodology, but also to what type of research is best suited for studying a complex issue such as suicidal *behaviour*, and conversely, what research is *not* suited for this. My view here is in opposition to that of Prinstein (2008, p. 6), who in his "Review of Unique Challenges and Important Directions for Self-Injury Science" seems to want more of the same: "data addressing risk factors specific to



suicidal behaviors are critical and sorely needed." I could not disagree more. When a problem has existed for a while without having been solved, we need to employ new methods (Bjørkum, 2009) and not just continue to do more of the same.

There is already some quite good qualitative suicide research out there, but alas, most of it is not published in "our own" journals. Joiner (2011, p. 471) argues for "valid and quantifiable metrics." But as Einstein supposedly once said: "Not everything that can be counted counts and not everything that counts can be counted." Some things can be counted; other things need to be understood. Suicide is by definition an intentional, purposeful act (Shneidman, 1985), and an intentional act has *meaning* and is always situated in a *cultural* context (Bruner, 1990). "The structure of meaning is not quantitative" (Michell, 2004, p. 316). The same applies to culture (Jenkins, 1994). In the words of Brinkmann (2008, pp. 185–86): "Human living is an interpretative, situated, social, and dynamic affair, and we need qualitative forms of psychology in order to grasp these dimensions of our lives" – dimensions that are highly relevant for suicide. By means of qualitative methodology we can study dynamic, contextual phenomena (and there is no doubt that suicide is such a phenomenon) differently and in more depth than is possible in quantitative research (Hjelmeland and Knizek, 2011b).

When I promote the use of more qualitative suicide research, I am often accused of polarizing the issue. "Of course," some people say, "the best thing to do is mixed-method research." I am not sure I agree. We now have such an excess of quantitative research that it would not hurt to even out this imbalance with more "pure" qualitative research – based, for instance, on the flood of results from quantitative studies that need to be explored further (e.g., Hjelmeland and Knizek, 2011b). In their outline of future directions in the concluding chapter of the comprehensive *International Handbook of Suicide Prevention*, O'Connor, Platt, and Gordon (2011, p. 630) underline the "need to recognize the value of the widest possible range of research methods to generate new knowledge." Such an attitude is, alas, not yet embraced by the editor of the most comprehensive of the suicide research journals (Joiner, 2011). O'Connor, Platt, and Gordon (2011, p. 630) assume that "the use of mixed/multiple methods is likely to become standard practice in the future, allowing us not only to quantify the prevalence of suicidal behavior but also to understand their phenomenology." For that to be meaningful, however, we need to develop and employ qualitative methodology on its own premises and not just as an appendix to quantitative

projects. Only then will it make sense to combine quantitative and qualitative methodology, for instance, in a mixed-methods study (Hjelmeland and Knizek, 2011b). We have several examples of studies claiming to have used mixed methods where the qualitative part appears as an artificial appendix to a mainly quantitative study and the two data sets are not analyzed in an *integrated* way. Mixed-method research is something more than just quantitative plus qualitative research; it is "the third methodological movement" (Tashakkori and Teddlie, 2003, p. 697).

We also need more research with a specific cultural focus conducted both where we already have an abundance of previous risk factor studies and in sociocultural contexts where we have little or no suicide research to date (e.g., Hjelmeland, 2010; Hjelmeland and Knizek, 2011a). But most of all, we need multidisciplinary research. With the current increased focus on epigenetics<sup>9</sup> and hence acknowledgment of the importance of environmental/contextual factors also in biological research, multidisciplinary collaboration in suicide research is now absolutely crucial in order to move the field forward. Research projects could be constructed to allow neurologists, psychiatrists, psychologists, sociologists, anthropologists, and so on to work together so that contributions from their respective fields can be analyzed in an integrated way (e.g., Hjelmeland 2011, 2012). For example, it would be interesting to combine neuro-imaging and/or genetic studies with in-depth qualitative interviews with the same informants.<sup>10</sup> That way, relevant biological *and* contextual (social and cultural) factors could be analyzed in an integrative way.

Last, I emphasize that it has not been my intent to denounce individual researchers where I have used examples of what I think is quite limited research. Researchers can, of course, do whatever research they like, and I'm sure people have lots of different reasons for doing the research they do. My main objective here has been to point out the publication practices – that is, the priorities made by suicide research journal editors with regard to the kind of research they choose to publish and encourage researchers to submit. In my view, journal editors carry more responsibility for the development of suicide research than do individual researchers. If editors stop publishing repetitive and limited (risk factor) studies, researchers will probably stop conducting them. Although there are *some* interesting and important contributions from the three main suicide research journals, the main impression from reading *all* the articles published in these three journals in 2011 and 2012 is that they, perhaps with the exception of *Crisis*, to a large degree publish repetitive risk factor research that does not produce any new or useful knowledge.<sup>11</sup> Valsiner



and colleagues' (2009, pp. vii–viii) point regarding social sciences in general seems relevant here: "One could wonder what in our basic knowledge of some specific phenomena would be lost if we started to play the game of stepwise elimination of published data – how much of the existing 'literature' in peer-reviewed journals of high 'impact factor' could be eliminated ('forgotten') before our current generalized understanding starts to suffer from such purification of science?" From this it may be concluded that we have too much suicide research, and at the same time too little; we have too much of the same old, same old, and too little of the research we need most. We need to encourage and intensify open debates about what kind of research suicidology currently needs more and/or less of in order to progress. This chapter is meant as a contribution to such a debate.

## Notes

- 1 Goldblatt and colleagues' definition of the various categories of studies is somewhat unclear, but here what they describe as epidemiological and risk factor studies are collapsed since most epidemiological studies also contain risk factors.
- 2 It is beyond the scope of this chapter to go into *what* properly conducted qualitative research is, so for that I refer to books on qualitative research methodology.
- 3 This can, of course, partly be due to lack of qualitative submissions, which, in turn, might be due to the editor's publicly flagged negative attitude towards qualitative studies.
- 4 References are far too many to be reported here, but all this is found in issues of *SLTB* published in 2011–12.
- 5 The concept of causality is problematic with relation to (suicidal) behaviour but is beyond the scope of this chapter to discuss.
- 6 Culture cannot be measured on a scale from 1 to 10 where we can say that some have a lot of culture, others have little. Nor is it a dichotomous variable, where we can say that some have culture, others do not. And there is no simple answer to the question of what culture *is*. More than 150 different definitions of this concept exist (Ingstad, 2007).
- 7 Studies focusing on *explanations* normally use quantitative hypothetico-deductive or experimental methodologies based on the biomedical illness model of linear cause and effect from the natural sciences, whereas studies concerned with *understanding* normally use qualitative methods and focus on the meaning(s) suicidal behaviour has for the individual (von Wright, 1971/2004) and where theory of interpretation is essential (Ricoeur 1974).
- 8 This journal now has a new editor, and I don't know in which direction he will take the journal.
- 9 Epigenetics studies how experiences influence genetic expression.
- 10 I have not mentioned (neuro)biological research in this chapter even though there is no doubt that such research is increasing (but little of it is published in the main

suicide research journals). There is no space to go into this research in detail here, but I *have* discussed (neuro)biological suicide research specifically elsewhere (Hjelmeland, 2012).

- 11 It should be mentioned that it is not only in suicide research journals that rather limited risk factors studies are published these days; this is also the case in prestigious medical/mental health journals.

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