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Toward a robust science of suicide: Epistemological, theoretical, and methodological considerations in advancing suicidology

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ABSTRACT

Suicidology is at a crossroads, the crux of which came into plain view recently when Hjelmeland/Knizek's critique of mainstream suicidology was followed by two ardent essays defending suicidology as it currently exists. We stake out a middle-way approach leveraging sociology's unique disciplinary perspective to bridge the two sides to construct a robust transdisciplinary toolbox that helps suicidology advance as science and improve how we study suicide and, therefore, what we know. The essay first examines the underlying structural and cultural reasons for the talking past each other, before turning towards our own understanding of science, methods, and the study of suicide.

Suicidology is at a crossroads; a crossroads that came into plain view when Hjelmeland and Knizek (2019) published an essay critiquing one of the most accepted and prevalent theories in suicidology (Thomas Joiner's [2005] Interpersonal Theory of Suicide [IPTS]) and consequently much of the scholarship and many of the scholars studying suicide. Not long after, two short essays were published by Klonsky (2019) defending the (dominant) scientific approach to suicidology and Smith, Schuler, Fadoir, Marie, and Basu (2019) defending the IPTS more directly. What was profoundly interesting, as "outsiders" (sociologists) with "skin in the game" (suicidologists) is just how much the two "camps" were talking past each other, and just how much this was a barrier to achieving the shared goals of understanding and preventing suicide. The first camp includes a growing number of interdisciplinary scholars who loosely fit under the "critical suicidology" label (Marsh, 2010; White & Morris, 2010), arguing that mainstream suicidology is too narrow in its theoretical and methodological gambit. The other camp, the more prominent, includes a significant proportion of suicidologists who are committed to these frameworks and who are engaged in what Kuhn (1962) referred to as normal science: the hypothetico-deductive approach built around accepted paradigms. Given the relatively harsh tone of Hjelmeland and Knizek's critique and the respondents, who managed to both side-step some of

Hjelmeland and Knizek's major criticisms while also conveying genuine openness, it would seem there is an unbridgeable chasm between these two camps. But we believe that the debate itself is less a barrier to advancing suicidology than it is an opportunity. However, this opportunity requires a middle-ground or third-way approach, which sociology, with its lengthy tradition of bridging social psychology and structural/cultural forces (Moody & Light, 2006), is perfectly positioned to guide.

Indeed, this position seems natural to us, since we need to look no further than the IPTS itself for evidence that it is possible to bridge disciplines within suicidology, as one of the core concepts of the IPTS (and other theories like it) is derived from founding (structural) sociologist Emile Durkheim's (1897 [1951]) *Suicide*. However, at the same time, we completely agree with Hjelmeland and Knizek that the IPTS is quite oddly named and is—from a sociological tradition—clearly an intrapersonal theory of suicide. This confusion over what the IPTS should be named, we argue, is evidence of disciplines talking past each other; an unfortunate but common occurrence in academia. To contribute to the collective effort to expand the methodological and theoretical toolkit of suicidology (and thus, what we know about suicide), we begin by stepping back and examining why we tend to talk past each other in hopes of shedding light on the question "what's going on here" in these

commentaries and in suicidology before turning towards our own understanding of science, methods, and the rigorous study of suicide.

Suicidology as profession

It may seem odd that we begin by thinking about this debate through sociology of professions' lens instead of tackling the substantive issues head on but there is something instructive here. On the one hand, sociology is almost maddening in its tendency towards self-reflexivity to the point of fostering in-fighting, but this very same tendency has also produced scholarship (Abbott, 1993; Ray, 2019) that reveals some of the taken for granted aspects of any social system; including science. On the other hand, considering suicidology through this lens allows us to see how certain questions or ways of measuring/knowing have come to dominate the landscape, not because they are "better" (in the scientific sense), per se, but because of the system that produces and reinforces "suicidological" knowledge production. Most notably, as Hjelmeland and Knizek point out, mainstream suicidology conceptualizes (external) sociocultural reality as mattering to individuals solely in terms of intrapersonal perception, when in fact there are aspects of the social world that are irreducible to individual-level perceptual phenomena. To quote sociologist Amos Hawley (1992, p. 13): "Crime, poverty, pollution, and political disorder are to be understood [as macro-structural phenomena], the tendency to attribute such events to motives or meanness in persons notwithstanding." Thus, science may be the best way of knowing in the abstract, but science, like religion, is a system of knowing built by humans in collective endeavors. It is naturally flawed in so far as it is shaped by the problems all social systems face (divisions of labor, distributions of rewards, and systems of authority) as by the objective pursuit of value neutral knowledge (Merton, 1979).

And so, while Klonsky's (2019) basic argument about the importance of science remains essential to the opportunity we see in moving forward, we also see a need for, as Hjelmeland and Knizek (2019) point out, substantially deeper reflexivity on the part of suicidologists in the production of their science. We must recognize, as many disciplines and professions before us, that suicidology has strong material and ideal interests in policing boundaries, maintaining the status quo, and, in Kuhn's (1962) words, resisting paradigmatic change that challenges the "expert's" power and control over key things like journal

publications, grant funding, and prominent appointments in prestigious departments. Right now the editor and all of the associate editors of *Suicide and Life-Threatening Behavior* (SLTB) are all psychologists or psychiatrists of some sort. In light of the recent calls towards interdisciplinarity of SLTB's parent organization the American Association of Suicidology,¹ is this really the best way to produce a robust suicidological science?

Towards a more robust suicidology

Having acknowledged the scientific landscape surrounding this debate, the question is how do we move forward while taking seriously both sides' position? We believe there are three issues we must engage to accomplish this goal. First, we examine whether the mainstream psychological understanding of "science" is unfairly narrow in light of broader trans-disciplinary discussions in the philosophy of science and methodology. Second, we discuss qualitative methods specifically, and what they can actually do vis-à-vis the common misunderstanding of them found in Klonsky and Smith and colleagues' work. Finally, we turn towards the sociocultural context, and what we see as the most important contribution of Hjelmeland and Knizek's argument, and illuminate, from a sociological perspective, why a sociological or inter-personal and not just a psychological or intra-personal perspective matters.

What is science?

At the core of the debate is the question of "what is science" and how should we go about studying suicide rigorously? Klonsky wisely pushes a common straw man argument (science is about prediction) aside and focuses instead on the more attainable goal of explanation. On this point, we wholeheartedly agree. He loses us a bit when tries to draw parallels between the study of physics and suicide, as the former lends itself to "sets of laws" or axiomatic theorizing, while the latter does not invite this kind of theory (Reynolds, 1971). Laws are predicated on unvarying empirical generalizations made about the relationship between two concepts. We need only ask the question: does suicidality always increase when perceptions of hopelessness and pain increase, or when burdensomeness and thwarted belongingness increase? The obvious answer is no. Suicide, instead, seems better fit for theories that isolate causal processes, which Klonsky and May's (2015) own three-step theory and the IPTS

attempt to do. The role of empirics then is to test theoretical propositions to determine whether these factors are indeed *causal* factors or are they simply key intrapersonal factors with strong correlations to suicidality. Equally problematic is the question as to whether these factors are generalizable across time or space [an issue we will return to shortly].

Klonsky's (2019) defense, ultimately, is that parsimony is better than relativism and nuanced complexity (Healy, 2017). Again, to some extent, we agree here. But, parsimony is typically better suited for axiomatic theory, where sets of deductive axioms, laws, and propositions are neatly embedded within each other (Reynolds, 1971). In causal process theories, the ultimate goal is the elucidation of "the causal mechanisms that link changes in one concept (e.g. burdensomeness or hopelessness) with changes in another (suicide ideation-to-action)" (ibid. 7). Herein lies the weakness in most of these models. Hopelessness does not cause suicide, but rather the mechanism that undergirds a hopeless individual's escalation to suicide is the real theoretical pot of gold.

This brings us to a very important point that will allow us to segue into a conversation about qualitative methods and cultural context more sophisticatedly: finding these causal mechanisms is severely delimited by the tendency to use non-random, clinical populations, and experimental designs. The latter is only useful if one's hypotheses are deduced from an accurate well-supported theory while relying on one or two subpopulations will undoubtedly bias a theory and reduce its comprehensive generalizability. Currently, for instance, IPTS and theories like it are insufficient explanations for a wide range of suicides such as diffusion-based suicide (Abrutyn & Mueller, 2014)—be they mass clusters, point clusters, dyadic diffusion—place-based suicides (Abrutyn, Mueller, & Osborne, 2019; Mueller & Abrutyn, 2016), or social justice/protest suicides (Fei, 2010). The explanations also fail to really explain the so-called "gender paradox" of suicide, or the low rates of suicide among some subgroups like African Americans who have experienced similar structural inequality and a lengthy history of state oppression as other subgroups with extraordinarily high rates (e.g. Native Americans).

Thus, we argue that parsimony is only effective in so far as the model is truly generalizable, and is relative based on the complexity of the phenomenon of interest. Positivist psychology is, often, only studying *one type of suicide* – clinical populations suffering from mental illness – and then extrapolating general theoretical processes from that group. "Suicide" or the

path to it may not be the same in every place or among every subgroup; thus, we should cautiously attempt to understand what "suicide" is in the first place before we begin to try to predict or prevent it. Ironically, this topic is vibrantly examined in anthropology (e.g. Kitanaka, 2012), where myriad ethnographic accounts show how suicide takes on different forms in different places and, notably, times (Barbagli, 2015). This literature casts serious doubt on mainstream theoretical models of suicide. Given this, we shift to the question of method: how can we best address these gaps in knowledge?

Science as a method or methods?

To summarize our position, there is not a method or approach for studying suicide that is *more scientific* than another, because the scientific method is a process of generating rigorous ways of knowing about the natural or social worlds that are valid and reliable. Although within suicidology experimental methods are touted as the gold standard (Klonsky, 2019), they are not the only or even necessarily the best way to generate insights into causal mechanisms (Nagin & Sampson, 2019). For one, the gold-standard status of randomized controlled experiments/trials (RCTs) is no longer certain for a variety of reasons from failure to replicate (Shrout & Rodgers, 2018) to failure to produce insights that actually scale beyond the experiment itself (Deaton & Cartwright, 2018; Sampson, 2010). Indeed, scholars have noted that holding the external context surrounding an RCT constant is nearly impossible, as the knowledge produced by RCTs inherently changes that context, potentially in ways that would impact the RCT's ability to replicate or generalize. This challenges major assumptions that the RCT framework relies upon (see recent discussions in *Proceedings of the National Academy of Sciences of the United States of America* (PNAS) ([Asensio & Delmas, 2015; Dietz, 2015]) and *Social Science & Medicine* [Deaton & Cartwright, 2018]).

Additionally, experiments are not the only way to engage in rigorous quantitative causal inference. As computing power has increased, so has our ability to creatively engage in causal inference using longitudinal survey data (Frank, Maroulis, Duong, & Kelcey, 2013; Hong, 2015); and while these methods are popular in sociological suicidology (see, Abrutyn & Mueller, 2014; Baller & Richardson, 2009), they are rarely considered in psychological positivist suicidology. It is also important to note that qualitative methods are not inherently *less* scientific than quantitative

or experimental if we agree that science is the production of robust replicable insights into how the world works. Instead, qualitative methodologies can be as scientific and even as “objective” as quantitative (and frankly quantitative samples can be as nonscientific or subjective as poorly-executed qualitative research, particularly if scholars utilize nonrandom or unique samples, engage in p-hacking, only publish significant findings, or omit important variables from their inquiry). However, as with quantitative research, the quality of qualitative inquiry is dependent on the research design and analytic procedures.

So what are key features of scientific qualitative research? Though a full methods lesson is beyond the scope of this commentary, it is worth noting 4 key features. First, scientific qualitative research should actively pursue variation in theoretically relevant cases (including counterfactual cases) so that comparisons are analytically possible (Small, 2009). Of course, understanding what cases may matter requires flexibility in the field, a rigorous understanding of what is already known, and a sense of what hypotheses or theoretical mechanisms need to be uncovered. Thus, second, a purely inductive stance is not appropriate (and indeed, is rarely the case [Charmaz, 2006]). Within sociology, a prominent epistemological stance for qualitative work is an *abductive* stance (Timmermans and Tavory, 2012), in which research begins with a deductive approach—that is, it is guided by existing theory—and is geared to look for surprising findings (that do not fit existing theories) that emerge inductively during fieldwork, and thus, encourages the researcher to account for these new insights (for an example, see Mueller & Abrutyn, [2016]). Third, scientific qualitative research must take care to pursue disconfirming evidence as well as confirming and consider explicitly alternative theories or interpretations (Timmermans & Tavory, 2012). Finally, fourth, high-quality qualitative research must ensure that empirical findings are not rooted in biased observations of the field researcher or data analyst. Having multiple ethnographers in the field and multiple individuals coding the data through a rigorous multi-stage coding process are widely accepted ways to protect against these forms of bias (Deterding & Waters, 2018). To be clear, these features are normative, if not universal, within qualitative research. These are the epistemological and methodological standards that are used by qualitative researchers themselves (and the National Science Foundation)² to judge the quality of qualitative projects.

If we accept then, that qualitative work has something to offer science, the exciting question then is what can it offer? Here we would argue, quite a lot, particularly when we do not know much about the phenomenon, like suicide. Qualitative methods can help us understand why two variables are correlated when quantitative insights are lacking (Ivankova, Creswell, & Stick, 2006). They can also contribute to identifying mechanisms, improving our measurement of key theoretical constructs, and generating new theoretical insights, in part by illuminating cases of suicides that do not fit current theories (as Hjelmeland & Knizek, [2019] cogently offer). Thus, a reciprocal, synergistic relationship between all of the methodologies (qualitative, quantitative, and experimental) is possible, and we argue more likely to produce a rigorous, robust, generalizable theory of suicide(s).

Culture beyond perception

Our final task, now, is to engage the other crucial critique Hjelmeland and Knizek (2019) offer: the neglect of cultural context in suicidology. The profession’s literature is once again relevant. Sociologists are often trained, to a fault, to ignore the individual, while psychologists are trained the opposite. Both give lip-service to the other, but because sociological social psychology borrows more heavily from psychology (and not the other way around), sociologists are perhaps more comfortable than most thinking about the link between person and culture, and the thing neither side of the debate is discussing: social structure.

As Klonsky invokes behaviorism in his essay, we are reminded that psychology still individualizes behavior far more than is likely the case in empirical reality. However, a vast literature within social psychology illustrates that social behavior is not driven by stimuli or by randomness; it is planned, even when actors do not have total knowledge of the best goals for themselves or access to the best means to achieve those goals (Burke, 2018). Suicide is no different in that it is often the culmination of some sort of planning. Planning requires the availability of cultural beliefs about suicide that are accessible and applicable to the individual exposed to them. As such, the association between having access to practical capabilities (e.g. having a gun at home) and suicidal behavior requires *normative capacity*, or acquiring and applying beliefs about who, why and how people die by suicide to one’s own situation (Abrutyn, Mueller, & Osborne, 2019; also, Canetto, 2017; Kral, 1994). Local cultures are implicated in the formation of normative capacity.

As such, one major aspect suicidologists need to take more seriously is the fact that emotions, attitudes, and behaviors related to suicide must be learned, and this learning is *collective*. The stories or narratives about suicide that individuals have access to matter greatly to whether suicide is an option for them and for their grief experience and recovery after a suicide loss (Neimeyer, Baldwin, & Gillies, 2006; Neimeyer, Klass, & Dennis 2014).

But, a bigger piece of the puzzle that Hjemelmand and Knizek are advocating for is the fact that some aspects of culture (and structure) are irreducible to the perceptions of individuals. If we are searching for causal processes, then psychology has to take seriously *the objectively real* nature of the social environment. This is not to say perception does not matter, but rather that there is a perception of the environment, and then there is the environment regardless of one's perception. As such, our study must not only shift from individuals in clinical populations to the general public, but also treat corporate units (families, neighborhoods, schools, communities) and categorical units (race, gender, sexuality, nationality, socioeconomic status) as real social facts with real effects on suicide emergent and distinct from the individual members. An extreme analogy is perhaps in order here. The way cultural context is used in mainstream suicidology is very compatible to a rich person telling a poor person or a white person telling a black person that the solution to inequality is a shift in perception rather than shifts in the rigged capitalist system or structural racism, respectively.

Conclusion

As outsiders to suicidology, who have a stake in the scientific study of suicide, we watched with excitement and fascination the debate between the “cultural/critical” wing of suicidology and the positivist-psychological wing. We were heartened by the explicit openness expressed by both Klonky and Smith et al., which we felt made writing this essay worth the time. That is, we hoped to capitalize on this openness and offer further explanation of the valid and important points raised by Hjemelmand and Knizek (without the political complexities of intra-disciplinary struggles of knowledge production) and encourage self-reflection of suicidology as a discipline. We also offer this essay acknowledging our own limitations as non-clinicians in the spirit of furthering conversation and transdisciplinarity. We see a bright future if the gatekeeping can be made more reflexive, and the mainstream

suicidologists can relax their narrow definitions of science so that we can synthesize our efforts and double-down on the goal of prevention and make more prominent the goal of postvention. Though it took sociology about 100 years after Durkheim's great contribution to the scientific study of suicide to begin to get serious and creative about the use of sociological tools for explaining suicide, there is a cadre of diverse scholars who could supercharge suicidology and contribute.

Notes

1. <https://www.suicidology.org/Annual-Conference/52nd-Annual-Conference>
2. <https://www.nsf.gov/pubs/2004/nsf04219/nsf04219.pdf>

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