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What suicide interventions should target

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Recent reviews and national statistics indicate that, so far, our field has made limited progress on fulfilling its central mission of preventing future suicidal thoughts and behaviors (STBs). We posit that a fundamental reason for our lack of progress is the way in which our field tends to think about and select STB intervention targets. Specifically, the vast majority of our intervention targets are derived from untested theoretical assertions, moderate correlates of STBs, or weak risk factors for STBs. None of these forms of evidence permits causal inferences, which is problematic because successful STB interventions must target the causes of STBs. To develop effective interventions, we must employ experimental designs to identify targets that are causal, necessary, and viable.

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How can we keep suicidal thoughts and behaviors (STBs) from happening? This is perhaps the most central and difficult question in suicide research. After several decades of work, we have made little progress in answering this question. Recent reviews indicate that few interventions consistently reduce STBs in comparison to an active control group [1,2,3••,4] and that some of our most promising interventions — cognitive-behavioral approaches — are much less promising when publication bias is considered [5]. Our lack of success is reflected in continued high STB rates, especially in the United States. The United States suicide rate in the year 1900 was approximately 10 per 100 000 people, which was also the rate in the year 2000 [6,7]. Since then, rates have steadily climbed to over 13 suicide deaths per 100 000 people [6]. The rates of suicidal thoughts, plans, and nonfatal attempts have followed a similar pattern [8]. Over the past several decades,

research and prevention efforts have increased dramatically in an effort to address this international public health problem [1,2,3••]. So why have we made so little progress on figuring out how to keep suicidal behaviors from happening?

There are many reasons for this, including the limited scalability, affordability, and accessibility of most traditional interventions [9,10]. But we propose that an even more fundamental issue has been central to obstructing progress in STB interventions: how we think about and select STB intervention targets. Contained within the Discussion Sections of a large proportion of research articles in our literature is some variant on the conclusion that '*these findings suggest that future interventions should target X.*' But do a large proportion of our studies actually allow for this conclusion? Exactly what kind of evidence would we need to make this kind of conclusion? In other words, what should suicide interventions target and what is the best way to identify those targets?

As a field, we have rarely thought through these kinds of questions, and this has led to a haphazard and largely ineffective approach to identifying suicide intervention targets. This is true of traditional interventions and of the most recent and novel STB interventions, including our own [11]. Such novel interventions may show some promise and may improve on traditional scalability limitations, but they still suffer from a core deficit: a target that has an unclear, little, or no causal role in STBs. Even in the best-case scenario of an initially promising intervention, this deficit can prevent progress for decades by making it nearly impossible to effectively refine the target or the way in which the intervention attacks the target. As a result, we expend tremendous resources grasping in the dark for ways to improve an intervention and often see effect sizes decline over time. In other words, it is nearly impossible to optimize *how* to target a given factor in the absence of proper evidence about *which* factors should be targeted. Across the many decades of STB intervention research, we are aware of no intervention that has improved since its inception. This problem is not unique to suicide intervention research. Within the much larger research field aimed at improving cognitive behavioral therapy for depression, meta-analytic evidence indicates that its efficacy has substantially diminished (rather than improved) over the decades [12•].

In the hopes of establishing a more strategic pathway for identifying effective STB intervention targets, we will

briefly outline the limitations of traditional approaches and then describe what suicide interventions should target and how to identify those targets.

Traditional intervention target identification approaches

A common but generally inadvisable approach to identifying intervention targets is to identify STB correlates. Especially when consistent with a popular theory, this rationale can seem convincing. However, even two highly correlated phenomena may have little or nothing to do with one another. For example, between 1999 and 2009, the correlation between United States suicides by suffocation and the United States budget allocation for science and technology was nearly perfect ($r = .99$) [6,13]. Few people would take this evidence to mean that we should reduce spending on science and technology to reduce suicide. Yet, if the same correlation existed between suicide and hopelessness, many may consider this sufficient rationale to target hopelessness within suicide interventions. Regardless of their strength, consistency, and relevance to a given theory, correlates provide little useful information about intervention targets.

Risk factors (i.e. longitudinal predictors) are more compelling than correlates, but they still fall far short of sufficient evidence for designating a given factor as an intervention target. Risk factors provide some evidence for directionality within an association, but it is important not to conflate longitudinal prediction with cause [14]. A given risk factor may simply be correlated with a STB cause; as a result, targeting a risk factor may have no effect on STBs. For example, a history of psychiatric hospitalization is one of the single strongest risk factors for suicide death [15•], but this does not mean that psychiatric hospitalization causes suicide and that ending psychiatric hospitalizations would prevent suicide. Instead, it may be that psychiatric hospitalization is correlated with several potential causes of suicide. As with correlates, risk factors provide little useful information about intervention targets. This is especially true within STB research as recent meta-analyses have shown that no known risk factor, out of the hundreds tested, accurately predicts STBs [15•].

Unlike correlates and risk factors, certain causal factors may represent potentially promising intervention targets. Unfortunately, suicide research has produced little information about causal factors because experimental designs are necessary to make causal inferences. Although the term 'experiment' has been used to refer to a range of designs in our field, causal inferences can only be drawn from designs where the effects of deliberately introducing a manipulation in one group are compared to the effects of not introducing that manipulation in another group (i.e. the counterfactual dependence test of causation) [16]. Randomized controlled trials (RCTs) are experiments, and hundreds of STB RCTs have been conducted; however, none of these have identified intervention targets because: first, they presuppose a target and do not directly test STB causes (i.e. they test the causal effect of an intervention); second, any conclusions from RCTs about STB causes would be based on problematic *ex juvantibus reasoning* [17], which involves reasoning backwards from an effective treatment (e.g. if aspirin relieves a headache, concluding that the headache was caused by insufficient aspirin in the brain); and third, RCTs indicate that existing interventions (and by extension, their targets) are largely ineffective [3••]. Few experiments have been aimed at directly testing potential causes of suicide ideation [18–21] and we are aware of none that have directly tested potential causes of suicidal behavior.

By this metric, most STB theories — which are primarily theories about STB causes — have not been directly tested. Thousands of studies have concluded that they have found support for various STB theories with correlational or longitudinal evidence. But this kind of evidence is very limited because it leaves open so many alternative explanations for construct–suicide associations that it is impossible to justify even the most tenuous of causal inferences. It follows that most of our theories, and the intervention targets deduced from those theories, have little empirical verification or refutation because they have not been directly tested within experimental designs. This helps to explain how so many different STB theories currently co-exist [15•] — without direct tests of causal hypotheses, falsification evidence cannot be used to winnow the theories. Likewise, this helps to explain the limited efficacy of existing interventions: these interventions primarily target moderate correlates and weak risk factors, none of which have been shown to cause STBs.

Complications to identifying intervention targets

We believe that future work should focus heavily on experimental designs to elucidate STB causes; however, three issues should be kept in mind when conducting and interpreting this work. First, experimental designs cannot prove that a particular factor causes STBs [22], they can only help to rule out potential alternative explanations for an association (e.g. that a given factor is a concomitant). More stringent experimental designs (e.g. randomized groups, powerful control manipulations), larger sample sizes, and consistent findings across studies can rule out more alternative explanations and correspondingly can justify stronger causal inferences.

Second, there are obvious ethical limitations to conducting experiments aimed at increasing STBs, but such studies are still possible. Through careful procedures, multiple labs have conducted experiments aimed at testing the potential causes implicit and explicit suicidal thoughts [20,21]. Although experimental studies of

actual suicidal behaviors will always be prohibitive, virtual reality technologies have become advanced, affordable, and flexible enough to permit experiments with virtual suicidal behaviors as outcomes. These virtual behaviors are certainly different from actual suicidal behaviors, but they are realistic enough to provide tentative tests of hypotheses about the causes of suicidal behaviors. Multiple research teams currently have such studies underway.

Third, the field tends to view STB causes as relatively simple, with most theories proposing somewhere between one and ten causal factors. This approach is intuitively appealing, but the causes of many phenomena are extraordinarily complex, involving countless factors, complicated relationships among those factors, and few (if any) singularly necessary or sufficient factors. Most of what we regard as singular causes are actually what Mackie [23] termed *insufficient* but *nonredundant* parts of *unnecessary* but *sufficient* conditions, or INUS conditions. For example, a short circuit is an INUS condition for a house fire. It is insufficient in that it cannot cause a house fire on its own (e.g. oxygen, wood, carpet, lack of a sprinkler system); it is nonredundant to the extent that similar factors are not present; it is part of an unnecessary condition to the extent that other constellations of factors can cause a house fire (e.g. match, arsonist, nearby forest fire, lightning strike); and it is part of a sufficient condition to the extent that the present constellation of factors was able to generate a house fire. Given the complexity of causes, a house fire intervention that targets short circuits would have limited efficacy. In much the same way, factors such as hopelessness may be INUS conditions for STBs, and interventions that target such factors may have limited efficacy.

The greater the complexity of causes, the harder it may be to identify useful intervention targets — so how complex are the causes of suicidal thoughts and behaviors? Evidence suggests that the answer is highly complex. Recent meta-analyses show that hundreds of factors predict suicidal thoughts and behaviors, but all of these predictors are weak [15•]. This indicates that hundreds of factors contribute to suicidal thoughts and behaviors, but all do so inconsistently and likely in conjunction with many other factors. Supporting this possibility, recent machine learning shows that accurate prediction of suicidal behavior may require the complex combination of hundreds of factors [24•]. These two lines of evidence concern prediction; as noted above, the causes (versus predictors) of suicidal thoughts and behaviors are likely to be even more complex. Given this complexity and the fact that experiments can typically only test for one or a small number of potential causal relationships, how can we use experimental methods to identify useful intervention targets? We address this question below.

Three primary qualities of suicide intervention targets

First, multiple well-controlled experimental studies should demonstrate that a given factor likely has a causal relationship with STBs. As described above, correlates and risk factors — even when consistent with a popular theory — are insufficient.

Second, experimental studies should allow for inferences that the factor is a *highly necessary* INUS condition in a large proportion of STBs. That is, the factor should be essential or nearly essential for STBs to occur. Such factors are likely to be insufficient on their own and may even be poor predictors, but they nevertheless play a crucial role. For example, dried leaves and timber are not sufficient to cause a forest fire and are poor predictors of forest fires, but it is difficult (though not impossible) for a forest fire to occur in the context of wet leaves and timber. Although STB causes are highly complex, experiments are capable of identifying these singular, highly necessary INUS conditions. In contrast, INUS conditions that show a low degree of necessity should not be prioritized as intervention targets (e.g. for a forest fire, the presence of campers with propane lamps).

Third, these highly necessary INUS conditions should be viable intervention targets. For example, oxygen is a highly necessary INUS condition for forest fires, but it is not viable to remove all oxygen in the world to prevent forest fires. Similarly, use of one's arms and legs is a highly necessary INUS condition for suicidal behaviors, but it is not viable to prevent everyone from using their arms and legs.

There is one special exception to this 'three primary qualities' rule: factors that are *by definition* viable and highly necessary INUS conditions for suicidal behaviors. Such factors are not biopsychosocial constructs; rather, they are the physical objects/behaviors that constitute the suicidal behavior itself. These factors explicitly exist as the actual instruments and procedures involved in carrying out suicidal behaviors (e.g. guns, access to high places). These factors are typically referred to as 'means' and targeted via restriction or safety strategies. Because of this special definitional property, experiments are not required to identify means-related factors as viable intervention targets (though experiments are necessary to permit causal inferences about the effects of means-related intervention strategies). Successful targeting of means-related factors with laws and policies has been consistently associated with reduced suicidal behaviors [25•,26,27]. We are fortunate that this special class of targets exists because they have provided an avenue for successful prevention of suicidal behavior in the absence of a robust experimental literature. But as evidenced by the rates of STBs, we should not be satisfied with these targets. In our opinion, the identification of other classes

of targets — that is, biopsychosocial targets (versus physical means) that possess the three primary targets described above — should be a top priority of STB research.

Conclusions

A major reason for the continued high rates of suicidal thoughts and behaviors is that our interventions are not targeting the right factors. Most suicide interventions, including our own [11], target factors based on theory, correlational evidence, or longitudinal evidence. By serendipity, interventions based on such evidence may occasionally produce positive effects, but robust, powerful, and progressive interventions will likely continue to elude our field unless we begin prioritizing the identification of targets that are causal, necessary, and viable.

Conflict of interest statement

Nothing declared.

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