

## Contextual analysis and the success of translational research

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### Abstract

The success of translational research can ultimately be judged by the degree to which it reduces the incidence and prevalence of psychological, behavioral, and physical disorders and the major factors influencing them. In our view, we currently place insufficient emphasis on assessing our impact on the social determinants of disorders. As a result, we are failing to affect the incidence and prevalence of critical disorders. Moreover, translational research fails to take into account the full range of interventions that could significantly reduce the incidence and prevalence of our most pressing disorders. These include policy changes, media, and broad cultural change movements. In this paper, we discuss the momentous achievements the tobacco prevention movement made over the last half-century, describe how the lessons gleaned from this success can apply to other prevention efforts, and contrast this success with progress made in battling other major public health concerns. We call for an expansion of the translational research agenda to develop and evaluate broader and more comprehensive strategies to affect well-being in entire populations.

### Keywords

Contextual analysis, Translational research, Behavioral disorders, Cultural change, Policy, Social structure

The ultimate benefit of translational research will be that it improves human well-being. Specifically, translational research will be successful to the extent that it improves our ability to reduce the incidence and prevalence of psychological, behavioral, and physical disorders and the behavioral and environmental factors that contribute to these disorders.

In this paper, we argue that the current thinking about translational research does not sufficiently consider the entire array of influences and interventions that are critical to producing significant reductions in the incidence and prevalence of our most pressing psychological, behavioral, and physical disorders. We believe that current research priorities place too little weight on the identification and experimental evaluation of contextual influences on key public health targets and too little emphasis on assessing

### Implications

**Research:** Researchers will be able to expand their research agenda to develop and evaluate broader, more comprehensive strategies that can affect well-being in the entire populations they encounter.

**Practice:** Practitioners will begin to pay critical attention to assessing our impact on the social determinants of critical disorders, which will have positive effects on the incidence and prevalence of these disorders.

**Policy:** The momentous achievements the tobacco prevention movement made over the last half-century can apply to other prevention efforts, policies, and social movements

our impact on the incidence and prevalence of these targets in entire populations. To explicate this concern, we consider evidence of progress on some major public health concerns and contrast these with progress in reducing cigarette smoking over the past 50 years.

### THE LACK OF PROGRESS ON IMPORTANT PUBLIC HEALTH TARGETS

If the ultimate benefit of translational research is an impact on the prevalence of well-being, we can measure our progress in terms of improvements in key public health outcomes. From this perspective, we see room for improvement. Consider depression and drug abuse, two of the most common and costly psychological/behavioral problems. According to the National Institute on Drug Abuse, the rate of illicit drug use in the USA increased between 2001 and 2012 [1]. With respect to depression, data from the Centers for Disease Control and Prevention indicate that, while 5.4 % of the US population 12 and over were depressed in 2005–2006 [2], this increased to 7.6 % when the same survey was conducted again from 2009 to 2012 [3].

Similar trends can be found across many public health challenges that have a behavioral/

psychological component. For example, in recent years, there have also been significant increases in childhood and adult obesity [4, 5] and adult diabetes [6].

Evidence shows that federally certified disability due to mental illness tripled between 1987 and 2007, although the criteria had not changed [7]. Similarly, Halfon, Houtrow, Larson, and Newacheck [8] analyzed data on childhood disabilities and concluded that childhood disabilities involving emotional, behavioral, and neurological disabilities have been increasing.

This evidence, in combination with the substantial efforts made in recent decades to address these problems, indicates a lack of progress in translating existing knowledge and methods into widespread improvements in many important aspects of human well-being. At least in the areas cited, the facts do not suggest that we are making progress in translating existing knowledge into reductions in the prevalence of important public health problems.

**A CRITIQUE OF CURRENT CONCEPTUALIZATIONS OF TRANSLATIONAL RESEARCH**

A translational research framework is presented in Table 1 and is discussed more fully by Fishbein [et al.] in this special issue. This and related frameworks for translation are founded on the idea that basic research on mechanisms will lead to interventions that will become sufficiently effective that researchers can implement them widely and effectively in the population, thus affecting the prevalence of a problem or disorder in the entire population [9–14]. This framework assumes that basic research will lead to understanding the mechanisms that affect behavioral and physiological disorders and that this understanding will contribute to developing efficacious interventions. The demonstration of the efficacy of such interventions in carefully controlled and often heavily resourced interventions would precede effectiveness trials to test the intervention in circumstances that approximate the way the intervention would be implemented in everyday practice. Finally, we expect effective interventions to be disseminated widely and effectively through research that is now labelled dissemination and implementation research.

We have two concerns with this framework, both of which relate to whether the framework is optimal for ensuring that prevention research leads to changes in the prevalence of problems or disorders in entire populations.

**Too great a reliance on programs and their dissemination**

We would argue that this framework is too much focused on programs and their dissemination and has too little emphasis on interventions that do not have their origins in program development and

Table 1 | Translational research stages

Type	Definition
Type 0 translation (T0)	The fundamental process of translating findings and discoveries from social and biomedical sciences into research with human subjects.
Type 1 translation (T1)	Moving from bench to bedside. Translation of applied theory to methods and program development.
Type 2 translation (T2)	Moving from bedside to practice and involving translation of program development to implementation.
Type 3 translation (T3)	Determining whether efficacy and effectiveness trial outcomes can be replicated under real world settings.
Type 4 translation (T4)	Wide-scale implementation, adoption, and insitutionalization of new guidelines, practices, and policies.
Type 5 translation (T5)	Translation to global communities. Involves fundamental, universal change in attitudes, policies, and social systems.

dissemination, but rather involve policy, media, community organizing, or social movements.

The experience and success of the tobacco control movement illustrates this concern. It is ironic that Greenwald and Cullen [12] presented the original five-phase framework for translational research when Joseph Cullen was at the National Cancer Institute leading efforts to reduce tobacco use. The framework is not consistent with how the tobacco control movement produced what is arguably one of the most important improvements in public health in the twentieth century [15, 16]. Certainly, efficacious smoking cessation and prevention programs were developed [17–19]. However, few of these were ever widely disseminated. Rather, a network of federal research and public health organizations, along with numerous advocacy organizations such as Americans for Nonsmokers' Rights, Action on Smoking and Health, the American Lung Association, and the American Cancer Society, cooperated to advocate for changes in policies and to motivate individual smokers to quit (done mostly with no assistance from any formal program [20]).

This is not to say that research was irrelevant to the progress of the tobacco control movement. But much of the research involved investigating influences on smoking behavior, which then inspired policy and media efforts to affect the incidence of smoking initiation and the prevalence of smoking. For example, research on tobacco marketing made clear that cigarette marketing was a causal influence on adolescent smoking initiation [21]. This became the basis for litigation and legislation placing restrictions on cigarette marketing [22]. Research on the effect of taxation on smoking initiation led to significant increases in taxes on cigarettes [23]. After research demonstrated the value of phone counseling and nicotine replacement therapies, policies began to encourage making these supports more widely available [24]. In addition, ongoing surveillance data on the prevalence of smoking and youth initiation guided these efforts. For example, the rise in smoking initiation in the 1990s prompted efforts to curtail marketing to youth [21].

These same strategies are relevant to affecting the incidence and prevalence of major psychological, behavioral, and health problems. Very similar efforts have been instrumental in reducing the harm associated with excessive use of alcohol [25]. As evidence of the harms associated with alcohol accumulated, policies such as increasing the age at which alcohol could be purchased and increasing the tax on alcohol beverages were implemented and evaluated through natural experiments. The social movement to reduce drunk driving was instrumental in influencing policy adoption and individual behavior.

We are by no means suggesting that the development and dissemination of prevention programs are unimportant. Research on program prevention has produced a broad array of family, school, and community interventions that have proven value in preventing a wide range of problems [26]. However, there are critical risk factors for the development of multiple

problems that are unlikely to be adequately addressed if we depend on only program development and dissemination.

The most important example is poverty. The USA has one of the highest levels of child poverty of any developed nation [27]. Poverty elevates a wide variety of risk factors that affect the development of virtually every one of the psychological, behavioral, and health outcomes we seek to affect [28–30]. There is some evidence that family interventions can produce small improvements in the family's economic well-being [31, 32]. However, given that there is evidence from policy research of policies that can directly reduce family poverty and that increasing family income has preventive benefits [26] and, as we are far from being able to widely and effectively reach the entire populations of families, it would be imprudent to rely solely on program research to address this important problem. Moreover, policies can be established that increase funding and requirements for the implementation of evidence-based programs.

In sum, our progress may be limited if we focus too narrowly on the development and dissemination of evidence-based intervention programs and fail to pursue all of the other levers for affecting public health.

#### Assumptions about the role of physiological research

The kind of comprehensive approach to public health that we are advocating is also impeded by the way in which the role of physiological research is conceptualized. There is no doubt that research on the neuroscience and epigenetic processes involved in development plays a vital role in understanding basic mechanisms that underpin the impact of the environment on the development of psychological, behavioral, and health problems [33]. This understanding can contribute to the identification of subgroups of the population that are not responsive to existing interventions and can, thereby help to identify new and more effective interventions. However, in saying this, it is important to distinguish between mechanist and contextualist conceptions of the problem [34].

From a contextualist perspective, the ultimate goal of our research is to identify malleable variables that can be used to influence the outcome of interest, while from a mechanist perspective the goal is to model the relationships among processes, without necessarily identifying malleable variables that can be exploited to build effective interventions [35]. For example, one might obtain physiological measure of executive function and show that they are reliably related to the development of drug abuse [36]. Such an analysis might pinpoint an important physiological process involved in drug abuse development. However, by itself, it does not identify malleable variables that might improve executive function and thereby prevent drug abuse. This is not to say

that brain function is not malleable, but rather that it can only be influenced through changes in the environment.

In the area of drug abuse, the explosive growth in biological research is undoubtedly contributing to a better understanding of the biological substrata of behavior and could contribute to development of pharmacologic interventions and vaccines that affect drug abuse [37]. However, there is a problem when the National Institute on Drug Abuse (NIDA) states, “Drug addiction is a disease of the human brain” (<http://www.drugabuse.gov/about-nida/directors-page>). The basis for this statement is extensive evidence showing the ways that drug addiction perturbs brain function. Considerable research at NIDA is thus searching for pharmacological interventions to facilitate efforts to stop abusing drugs. All of this is quite useful. But calling drug abuse a disease of the brain obscures the environmental influences that NIDA’s own research shows contribute to drug abuse and the difficulties these influences pose in quitting drug use. This overly narrow biological approach leaves out a critical set of environmental variables that are particularly useful in affecting the incidence and prevalence of drug abuse at a population level.

In sum, basic research on the biological underpinnings of environment-behavior relationships may help to generate increasingly effective interventions. However, research strategies that assume that understanding the biological processes involved in psychological, behavioral, and health problems will lead to effective interventions without attention to the environmental influences that affect those processes, short-change attention to critical environmental influences and are unlikely to advance the goal of affecting populations. We need additional translational research examining the environmental influences on the problems we seek to affect.

#### A CONTEXTUALIST FRAMEWORK FOR TRANSLATIONAL RESEARCH

Translational research might be facilitated by attention to the philosophical assumptions guiding our research. The implicit framework for most biobehavioral research has sometimes been labeled as “mechanistic” because it focuses on identifying the biological and behavioral mechanisms that underlie behavior and disease. However, a recent resurgence in attention to contextualism has emphasized the value of strategies that focus explicitly on identifying variables that both predict *and influence* behavior [26, 34, 35, 38, 39]. The framework does not oppose building models of the relationships among variables, but makes the central goal of such model building the identification of environmental influences on behavior that can be readily manipulated to affect change. This pragmatic approach is very much in keeping with the present concern with influencing the incidence and prevalence of major aspects of human well-being.

#### Target incidence and prevalence of the most prevalent and costly problems

A contextualist approach to translating existing knowledge into population-wide benefit would start with the end in mind. If we seek to reduce the incidence and prevalence of psychological and behavioral problems and physical diseases, we should organize our research efforts to achieve this goal.

Explicitly embracing such a goal would prompt a review of current research priorities to see how well they contribute to this outcome. New programs of research would be designed to contribute to affecting the incidence and prevalence of one or more problems at a population level.

#### Strengthening and expanding the monitoring of incidence and prevalence

Given the heavy emphasis on the ultimate public health goals for such work, a contextual approach naturally requires a strong focus on assessment of key outcomes. Ultimately, to reduce the prevalence of problems we will need data on their rates down to at least the community level [40], as we will use those ongoing data to assess the impact of programs and policies and to guide changes in them as needed. Thus, one area of research would seek to improve the efficiency and accuracy of surveillance methods, including methods of reporting the results to members of the population in ways that influence population behavior and public support for effective programs and policies.

Monitoring the incidence and prevalence is more routine for some problems than for others. For example, the USA currently obtains data from representative samples of the population regarding tobacco, alcohol, and other drug use, but only some states assess the prevalence of anxiety and depression. Furthermore, these data are usually not integrated systematically with intervention efforts that would allow timely identification of more or less effective programs and strategies. Strengthening systems for monitoring well-being should be a high priority for translational research.

#### Reorganizing research priorities in light of the most important risk factors

A pragmatic approach seeking to reduce the incidence and prevalence of public health problems would focus on the most important, malleable risk factors. Population Attributable Risk (PAR) refers to the proportion of cases of a given disease that are due to the risk factor. Specifically, it is the total incidence of cases in a group, minus the incidence for those not exposed to the risk factor, and divided by the total incidence. For example, the PAR of cigarette smoking for lung cancer has been estimated at 85% [41]. PAR is useful in prioritizing targets for public health research and practice. Because of tobacco’s contribution to cancer and heart disease, tobacco control became a very high priority.

However, evidence of the PAR of social factors for death suggests that research on these factors is not receiving enough attention, given their substantial contribution to death. Galea et al. [28] reported, “Approximately 245,000 deaths in the United States in 2000 were attributable to low education, 176,000 to racial segregation, 162,000 to low social support, 133,000 to individual-level poverty, 119,000 to income inequality, and 39,000 to area-level poverty” (p. 1456). They point out that these numbers are comparable to the number of deaths due to myocardial infarction (192,898), cerebrovascular disease (167,661), and lung cancer (155,521). Moreover, the Galea [28] analysis does not consider the contribution that these risk factors make to psychological and behavioral problems. If the nation’s research priorities were based on the goal of ameliorating the risk factors accounting for the greatest harms to well-being, these social factors would receive much more attention.

Elsewhere, we have elaborated the case for focusing on increasing the prevalence of nurturing families and schools because these environments contribute to virtually the entire range of psychological, behavioral, and health problems [26, 42]. Nurturing environments have been characterized as (a) minimizing toxic social and biological conditions, (b) richly reinforcing prosocial behavior, (c) limiting opportunities and influences on problem behavior, and (d) cultivating psychological flexibility, which involves the pragmatic pursuit of valued living. From this perspective, we need coordinated research and practice across multiple agencies to focus on increasing nurturance in families and schools.

This is an area where the benefit of intervention programs is clear and achieving widespread implementation of effective programs should continue to be a high priority. However, there is also a great need for research on public policy that will affect nurturance in families and schools. Such research would not only investigate the impact of policies to increase family economic well-being, it would also investigate how support for such policies can be enhanced.

Shifting our focus from the prevention of individual problems and toward increasing the prevalence of nurturing environments faces an organizational obstacle, namely the current organization of NIH, CDC, SAMHSA, and the Department of Justice. Each organization has subunits working on a specific subset of problems (e.g., depression, crime, obesity) rather than focusing on affecting environments that contribute to all of these problems. From the perspective of the risk factors associated with a single problem, the importance of social environment factors may be obscured by attention to intra-organism risk factors and social risk factors specific to that problem. For example, a recent book on the risk factors for depression [43] enumerates numerous biological factors (e.g., genes, neural structures, neurotransmitters, sleep dysregulation), cognitive factors (e.g., cognitive schemas, optimism, rumination, problem solving), and social factors

(e.g., attachment, life events, parental pathology, marital relations, social support), but mentions poverty only six times in the entire volume.

Yet, given the evidence that poverty and inequality are risk factors for problems as diverse as depression, cigarette smoking, drug abuse, academic failure, risky sexual behavior, antisocial behavior, and cardiovascular disease [26, 44], these risk factors should be receiving far more attention than they are.

*Population attributable burden*—This analysis suggests that we need to characterize risk factors not simply in terms of their contribution to a specific problem, but in terms of their contribution to multiple problems. A risk factor (e.g., a gene) accounting for 20 % of depression cases but not accounting for any other problems deserves less attention than a risk factor that accounts for 20 % of the cases of multiple problems. We should rank order risk factors in terms of what might be termed the *population attributable burden* defined as the degree to which the risk factors account for multiple problems.

#### Intervention research focused on affecting incidence and prevalence

In addition to increasing attention to risk factors that account for multiple problems, embracing the goal of influencing the incidence and prevalence of problems would increase support for research on the *malleable* risk factors that make the greatest contribution to the largest number of problems. It is certainly important to identify non-malleable risk factors, as they may point to subgroups of the population who require different strategies and innovative ways to treat or prevent problems. But our success in affecting incidence and prevalence requires ultimately that we change the environment.

Putting a priority on increasing the prevalence of nurturing families and schools is particularly important, as these environments play a dominant role in generating most psychological, behavioral, and health problems [26]. We currently lack systematic cross-organization efforts to affect their prevalence. Instead, we have piecemeal efforts to affect segments of the population, such as abusive families, a strategy that fails to prevent abuse in the large proportion of families in which undetected abuse is occurring [45].

#### Research strategies for achieving effective implementation of evidence-based interventions

Even if the bulk of research continues to focus on getting evidence-based programs widely and effectively implemented (rather than broader environmental interventions at a public health level), a contextualist framework, which emphasizes identifying malleable variables that affect target phenomena could accelerate progress with implementation. Contextualism starts with the unique act or event in context and seeks malleable variables that affect that event [34]. This is

particularly relevant when we turn to Types 3 and 4 translational researches [10], where basic knowledge about the influences on individual and organizational adoption, implementation, and maintenance of interventions remains limited. Before we can test implementation strategies in randomized trials, we need to isolate the malleable influences on implementation in experimental studies of individual cases.

The success of randomized trials in validating many treatment and prevention interventions across a broad range of cases seems to have obscured the fact that our efficacious interventions were largely the product of research focused on contextual influences on individual behavior. For example, most of what we have learned about the importance of consequences for human behavior came from research that examined the effects of manipulating consequences for individuals [46]. After establishing the efficacy of environmental manipulations in affecting the behavior of individuals (parents, children, and students), programs were created whose impact on population samples could be tested through randomized trials.

It seems likely that problems such as how we influence organizations to adopt and maintain evidence-based interventions may require research that starts with an individual organization, identifies plausible influences on that organization's practices, and manipulates contextual influences to test their effect on organizational practices. Generalizable principles may well be discovered and randomized trials may validate the generalizability of those principles, but we must first identify effective variables in individual cases. Interrupted time series designs are the most appropriate design for this problem [47].

A specific example of this issue concerns efforts to integrate behavioral health care into primary care [48]. In working with coordinated care organizations in Oregon that are seeking to integrate behavioral health and primary, we have observed efforts to affect integration across multiple clinics—both those that are integrating primary care into behavior change clinics (e.g., mental health facilities) and those integrating behavioral health into primary care. These efforts are attempting to get integration across the entire range of disorders. We think that the achieving effective integration requires more of a single-case approach in which individual clinics test the impact of specific strategies for particular problems. For example, a clinic could focus on the glycemic control of patients with type II diabetes and test well-defined ways of providing behavioral change support to patients, while monitoring whether the proportion of patients with appropriate glycemic control increases. Such a strategy could be implemented in one clinic at a time, in a multiple baseline design [47], which would provide a methodologically sound assessment of the impact of the strategy.

One of the reviewers of the initial version of the paper suggested that we describe how the framework we are advocating would be applied to a “new problem...that the authors already believe well-

illustrated the broader approach.” The problem of poverty provides an excellent example because it is implicated in such a diverse set of problems and dealing with it requires a strategy, as suggested above, that exceeds the implementation of programs. To effectively address this problem, the following steps seem essential:

1. Articulate the scientific consensus regarding the effects of poverty on the well-being of families and communicate these facts in scientific and nonscientific media.
2. Articulate the evidence regarding the programs, policies, and practices that have proven benefit in increasing family economic security or in affecting the development of family members in directions that reduce the risk of inter-generational poverty.
3. Develop, implement, evaluate, and refine strategies for achieving the public policies needed to (a) make family economic security a fundamental goal of public policymaking and (b) implement the policies and programs that can be shown to reduce the family economic insecurity in order to steadily increase the proportion of families that are economically secure.

## CONCLUSION

Our success in translating existing knowledge into significant reductions in the prevalence of psychological, behavioral, and physical disorders will be facilitated by increasing attention to the contextual influences on these disorders and the evaluation of all of the strategies that could affect prevalence.

The promise of translational research will be realized if we evaluate public health progress in terms of changes in the incidence and prevalence of the most common and costly psychological, behavioral, and physical disorders.

In addition, we need to identify the social and other environmental influences that account for the largest burden of disorder across multiple disorders. In particular, social stressors (e.g., abuse, neglect, and conflict) contribute to such a wide array of psychological behavioral and health disorders that it is appropriate to develop population-wide efforts to reduce this type of stress.

Finally, we need to concentrate on developing and evaluating diverse strategies for reducing the prevalence of stressful family and school environments, as these venues have the greatest impact on the development of disorders. In addition to widely implementing evidence-based family, preschool, and school programs, the strategies should include policies to increase the incomes of poorer families, policies to reduce the use of punishment in schools and in the criminal justice system, and policies to increase supports for the transition from adolescence to young adulthood in the child welfare and juvenile justice systems.

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