How Psychosocial Research Can Help the National Institute of Mental Health Achieve Its Grand Challenge to Reduce the Burden of Mental Illnesses and Psychological Disorders

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The National Institute of Mental Health (NIMH) plays an enormous role in establishing the agenda for mental health research across the country (its 2016 appropriation was nearly $1.5 billion; NIMH, 2016a). As the primary funder of research that will lead to development of new assessments and interventions to identify and combat mental illness, the priorities set by NIMH have a major impact on the mental health of our nation and training of the next generation of clinical scientists. Joshua Gordon has recently begun his term as the new Director of NIMH and has been meeting with different organizations to understand how they can contribute to the grand challenge of reducing the burden of mental illness. As a group of clinical psychological scientists (most representing the Coalition for the Advancement and Application of Psychological Science), he asked what we saw as key gaps in our understanding of the burden of mental illnesses and psychological disorders that psychosocial research could help fill. In response, we first present data illustrating how funding trends have shifted toward biomedical research over the past 18 years and then consider the objectives NIMH has defined in its recent strategic plan (U.S. Department of Health and Human Services, National Institutes of Health, & National Institute of Mental Health, 2015). We then note ways that advances in psychosocial research can help achieve these objectives. Critically, this involves integrating psychosocial and biomedical approaches to efficiently relieve the suffering of millions of Americans who struggle with mental illnesses and psychological disorders.

Keywords: psychosocial, biomedical, National Institute of Mental Health, funding, grants

The National Institute of Mental Health (NIMH) is the largest funder of mental health research in the world, and its budget represents approximately 5% of the total budget of the National Institutes of Health (NIH; Insel, 2015). The

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institute maintains a diverse portfolio of funded projects, ranging from basic psychopathology investigations to multisite randomized controlled treatment trials. This history of supporting the full gamut of psychopathology research has led to truly groundbreaking accomplishments, including, but by no means limited to, efficacious interventions for bulimia (e.g., Agras, Walsh, Fairburn, Wilson, & Kraemer, 2000); dialectical behavior therapy for borderline personality disorder (e.g., Linehan et al., 2006); prevention efforts for substance abuse (in youth at risk for suicide; King, Gipson, Horwitz, & Opperman, 2015), violence (see Webster-Stratton & Taylor, 2001), and suicide (e.g., Brown et al., 2005); and identification of cognitive deficits tied to schizophrenia (see M. F. Green et al., 2004)—among numerous other accomplishments that have reduced mental health-linked morbidity and mortality. There can be no doubt that the availability of funding from NIMH has greatly facilitated the development of efficacious and cost-effective psychosocial assessments and interventions—ones that have significantly advanced the ways in which psychiatric conditions are identified, prevented, and alleviated.

However, many researchers have perceived, but could not easily verify, a shift in funding prioritization toward biomedical investigations. Strong claims from concerned researchers in popular news outlets have raised warnings that the heavy emphasis on basic biomedical and neuroscience research is "strangling [NIMH's] clinical research budget" (Markowitz, 2016, para. 3) and even claims that NIMH "has lost sight of its most fundamental mission: finding ways to ease the burden of mental illness for those affected by it today" (Lewis-Fernández, 2016, para 2).

These perceived shifts have created considerable concern among many groups in the psychological science community, with particular worries about stalling improvements in—and dissemination efforts for—existing efficacious nonbiomedical treatments, hindering the development of new, innovative prevention and intervention efforts, and harming junior scientists whose career paths are stymied by the lack of available funding to support their best ideas. Fundamentally, there is a concern that the strong prioritization of biomedical research has not allowed NIMH to achieve its basic mission, as evidenced by the lack of progress at reducing the burden of mental illness in a substantive way (see review of burden in Kazdin & Blase, 2011).

These worries were compounded by a review of the recent strategic plan submitted by the NIMH for public comment and ultimately adopted. For many, the plan appears to be informed disproportionately by an emphasis on biomedical processes and neuroscience over psychosocial conceptualizations of mental illnesses and psychological disorders (NIMH, 2015). For instance, the text explaining the first strategic objective, "Define the Mechanisms of Complex Behaviors," is almost exclusively focused on biological mechanisms, with the substrategies listing "molecules, cells, and neural circuits associated with complex behaviors," "genomic and non-genomic factors associated with mental illnesses," and the "connectomes for mental illnesses" (NIMH, 2015, Strategic Objective 1, para 6) as the key areas of investigation. Not surprisingly, this has alarmed researchers who argue that a more balanced scope of inquiry is essential to both address the mental health needs of today and discover the new interventions of tomorrow: "Of major concern is that this shift in the NIMH funding for research to uncover biological causes of psychological disorders may eclipse the funding for psychotherapy research" (Goldfried, 2016, p. 80; see also seminal writing by Kendler, 2012, on the dangers of privileging one level of analysis in trying to understand the etiology of complex psychiatric problems).

Partly as a result of these concerns, more than 50 different national organizations cosigned a letter to the NIMH search committee as it was seeking Thomas Insel's replacement, asking the committee to include among its "criteria the need for a Director who recognizes the central role of psychosocial, cultural, and behavioral research in addressing the mental health needs of our nation" (B. Teachman, personal communication, August 13, 2018, para. 1). Moreover, a group of current and former members of the NIMH National Advisory Mental Health Council (arguably the group with the best "insider" view to the decision making of the NIMH) felt sufficiently concerned to state in an editorial that "in recent years, the NIMH funding allocation has prioritized searching for neurobiological mechanisms of mental illness," and argued that "a disproportionate investment in neuroscience is as imprudent as investing only in growth stocks and neglecting less risky investments that yield im-

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mediate albeit potentially more modest benefits" (Lewis-Fernández et al., 2016, pp. 507–508). Although we are not aware of any reports that have directly examined changes in the balance of the portfolio in terms of psychosocial versus biomedical research over time, there are good reasons to suspect that the portfolio has been very imbalanced in recent years. Lewis-Fernández et al. (2016) reported that based on information "presented at open access Council meetings, yearly total investment in non-HIV/AIDS services and interventions research since 2012 comprises ~15% of the NIMH budget, with basic and translational neuroscience research accounting for the remaining 85%" (p. 508).

However, the absence of clear data to inform whether the concerns about shifts in funding priorities are warranted has made it difficult for clinical scientists to effectively respond to the changes in the field. Thus, in this article, we address two broad aims. First, we aim to quantify the shifts in funding priorities. The NIH maintains a publicly accessible database of funded projects, both completed and ongoing, dating back to 1997. This database was relied upon to evaluate the types of projects funded (i.e., psychosocial or biomedical) stratified by year. The second aim is to highlight the contributions to each of the four broad domains of the NIMH strategic plan that can be made by psychosocial researchers. We support the important broad objectives laid out by NIMH in their strategic plan and see no reason those objectives need to focus on either psychosocial or biomedical approaches in an unbalanced way, and consider both approaches compatible and their integration essential for NIMH to reach its goals. As the data will show, however, recent funding trends indicate that a major imbalance has occurred. We wish to make the case that a better balance is

in NIMH's and the field's interest to most efficiently and effectively reduce the burden of mental illnesses and psychological disorders. The second half of the article thus offers a partial roadmap of ways that psychosocial research can play a central role in addressing the priorities articulated by the NIMH. The examples we provide are in no way comprehensive but highlight a sample of the many benefits that could follow from a more balanced portfolio.

Funding Priorities in the National Institute of Mental Health, 1997–2015

The NIH RePORTer database contains investigator-generated abstracts along with data on the funding mechanism, the year the project was funded, the dollar amount of funding by year (combined direct and indirect costs), the number of years of funding, and the principal investigator, to name a few of the variables. For the purposes of this investigation, the coders were kept blind to all the listed variables and only had access to the investigator-generated abstract that described the research. A random sample of 15% of all funded projects, stratified by year, was culled from the database. Only grants that were R (research project grants), F (i.e., predoctoral training grants), and K (mentored scientist training grants) were examined. P (large multicenter projects) and Z (NIMH internal awards) grants were excluded, in part because these were generally small in number. This resulted in a total of 2,028 abstracts from 1997 through 2015. Abstracts were downloaded into the data-coding program DistillerSR (Evidence Partners, 2015).

All abstracts were coded on a 5-point scale ranging from $1 = $ *entirely focused on biomedical topics* to $5 = $ *entirely focused on psychosocial topics*. Four doctoral-level graduate student raters coded approximately 10% of abstracts to assess interrater reliability. The coders were clinical doctoral students working with the second author, who provided regular supervision throughout the project. High interrater reliability was obtained when scores of 1 or 2 were collapsed, scores of 4 or 5 were collapsed, and ratings of 3 were excluded ($n = 4$ abstracts with this rating, $\kappa = 0.87$; reliability on the original scale, treating it as a 5-point continuous measure, was $\kappa = 0.43$).

Figure 1 illustrates the change in the proportion of proposals that were primarily or exclusively biomedical in focus across all grant mechanisms. There was a significant positive slope for this funding trend across all funding mechanisms, $r(1237) = 0.84$, $p < .001$. This rate of change in the funding of primarily biomedical research projects was similar for investigator-initiated (R01) grants alone, $r(476) = 0.87$, $p < .001$. A test of the differences in regression slopes indicated that there was, however, no difference in the increase in award size for R01 grants, $F(1,475) = 3.97$, $p = ns$, suggesting that the proportion of biomedical grants awarded increased, but they did not re-

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ceive disproportionately larger awards than psychosocial grants. This is notable given that biomedical research is often more costly because of expensive procedures and larger research teams.

These data allow for a quantitative evaluation of a greater shift toward funding biomedically oriented projects, but the data coded from the NIH RePORTer database have some important limitations. First, the investigator-submitted abstract was the only source for determining the primary thrust of the research. As these abstracts are often less rigorously reviewed than other components of the proposal, it is possible that the descriptions underrepresent either psychosocial or biomedical components of the project. Second, it is also important to bear in mind that although we did evaluate a random sampling of proposals, stratified by year, this still represents a fraction of total funded research. Nonetheless, we expect that the data presented here represent a reliable approximation of the funding trends evident in NIMH over an 18-year period. Third, these abstracts provide data on *funded* grants but leave open the important question of what proportion of *submitted* grant applications focused on psychosocial or biomedical research questions or approaches. Learning about the application pool will be critical (these data are not publicly available), but a biased pool can occur for many reasons, including researchers "giving up" and not submitting grants (something we have heard anecdotally on many occasions) or researchers perceiving that the calls for proposals were mainly seeking biomedically oriented submissions. Along these lines, frequent references to "biomarkers" and "experimental medicine" in NIMH materials and requests for proposals have given many researchers the impression that psychosocial research is less valued. Re-

lated concerns about it becoming increasingly difficult to receive approval to submit large grants (i.e., those over $500,000 per year), along with the severe limitations to investigator-initiated clinical trials, have all added to the impression that it is harder to have nonbiomedical research under review. It is thus essential for the field to learn more about changes in rates of *submitted* psychosocial versus biomedical grants. In an ideal world, there would be a version of RePORTer that includes the abstracts of nonfunded as well as funded work, so that the funding patterns could be even more transparent. (We recognize this idea raises challenges around protecting confidentiality and intellectual property but nonetheless believe discussing this possibility and ways to help address privacy concerns would be worthwhile).

Data Show the Perceived Shifts in Funding Priorities Are Real: Where to From Here?

In light of recent policies from NIMH, notably the Research Domain Criteria (RDoC), which explicitly define psychiatric illness as "brain diseases" (Insel et al., 2010), one might expect this trend of increasing funding for biomedically oriented projects to continue, and this is a commonly repeated fear in the psychological science community. That said, we greatly appreciate that the new NIMH Director, Joshua Gordon, has been open to conversations about the value of a more balanced portfolio as well as prior discussions with Bruce Cuthbert, then Acting Director, who engaged in a "Q&A" on psychosocial research at NIMH, culminating in NIMH's posting on "Psychosocial Research at NIMH: A Primer" (NIMH, 2016b). Moreover, we are encouraged by Gordon's recent statement that

> a crucial toolset with the potential for near-term impact comprises psychosocial interventions. Indeed, for some conditions (e.g., eating disorders, borderline personality disorder, conduct disorders among youth), behavioral, cognitive, interper-
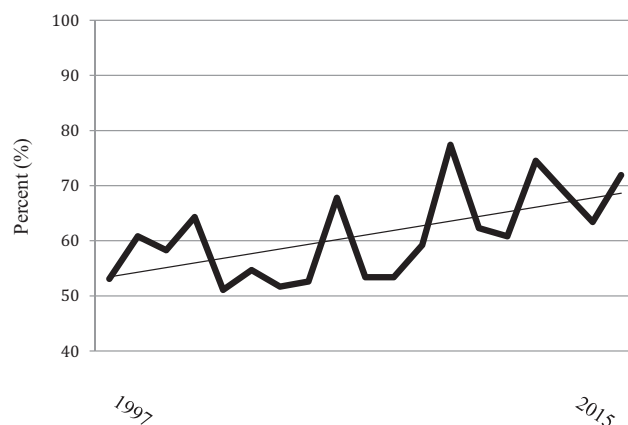

Figure 1. Proportion of funded research projects that were rated primarily or entirely biomedical in focus.

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sonal, and other psychosocial treatments have proven to be the best supported, or in some cases, the only evidence-based approaches. (Gordon, 2017, para. 3)

Remarkably, the prior director, Insel, appears to be rethinking his prioritization of biomedical research, stating in a recent interview,

I spent 13 years at NIMH really pushing on the neuroscience and genetics of mental disorders, and when I look back on that I realize that while I think I succeeded at getting lots of really cool papers published by cool scientists at fairly large costs—I think $20 billion—I don't think we moved the needle in reducing suicide, reducing hospitalizations, improving recovery for the tens of millions of people who have mental illness . . . I hold myself accountable for that. (Insel, quoted in Rogers, 2017, para. 5)

This recognition by former and current NIMH directors is important and promising, as are the new funding mechanisms released by NIMH that specifically target nonbiomedical approaches, that is, "Development of Psychosocial Therapeutic and Preventive Interventions for Mental Disorders" (Department of Health and Human Services, 2016) and "Confirmatory Efficacy Trials of Non-Pharmacological Interventions for Mental Disorders" (Department of Health and Human Services, 2017). Thus, we shift to looking forward in response to Gordon's question to us about the ways that new psychosocial research can help reduce the burden of mental illnesses and psychological disorders, and in the spirit of collaboration. We highlight just a few of the many ways that psychosocial research can complement biomedical research to help NIMH achieve its goals by considering promising, novel lines of psychosocial research that fall within each of NIMH's identi-

fied strategic objectives and substrategies (using the specific wording from the NIMH, 2015, strategic plan). In this way, we are not challenging the basic strategic objectives laid out by NIMH; rather, we are approaching this collaboratively to note how we believe NIMH can more effectively achieve its stated objectives.

Our intent is not to speak solely to Gordon, though we certainly do think it is important to share the concerns we are hearing and the data we are examining with NIMH, especially given the relatively unique way we have categorized the data, which allows for a very direct discussion about the emphasis on the "bio" part in biomarkers. Nonetheless, Gordon is not the only intended audience for this article. Rather, we use the format of a "brief" to Gordon as a means to convey a larger message to psychologists (in the clinical field and across other areas) about how psychosocial research can align with NIMH priorities. One reason for this effort is to encourage more fundable applications. This is a critical issue given recent evidence from NIMH (see "NIMH's Portfolio Balance: Quality Science Comes First"; NIMH, 2018) suggesting that the primary divisions that fund translational and services research have had substantial drops over the past decade in the number of applications they receive. Although applications to different divisions do not align neatly with our categorization of funding for mainly psychosocial versus mainly biomedical grants, it nonetheless provides important clues about the need to encourage more psychosocial research applications that will align with NIMH priorities.

Of course, NIMH's mission can, and does, shift over time. In fact, NIMH's mission was once focused on meeting current service provision needs. As outlined in the 10-volume series "Action for Mental Health," which was transmitted to Congress in 1960, the report set a goal of achieving "a national program that would approach adequacy in meeting the individual needs of the mentally ill people of America" (see "Important Events in NIMH History" in National Institutes of Health Almanac, 2017), which is arguably different from NIMH's present objectives. Given our goals, we consider how NIMH's *current* stated objectives can, in theory, provide numerous opportunities for psychosocial research contributions from all areas of psychology. To be clear, we do not object to NIMH's current stated mission and see no reason why transforming "the understanding and treatment of mental illnesses through basic and clinical research, paving the way for prevention, recovery, and cure" (NIMH, n.d., para 3) is discordant with a portfolio that carefully balances contributions from both psychosocial and biomedical research. The concern is with the way the mission has been operationalized, that is, with an increasing focus on biomedical research such that the necessary integrative contributions from psychosocial research are being neglected. This is problematic for many reasons, not the least of which is that biological determinants are culturally and environmentally
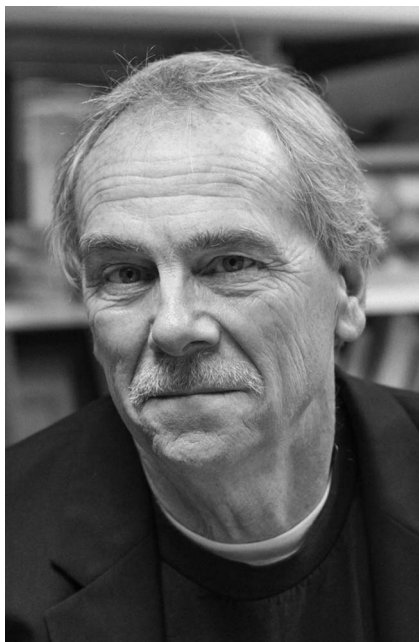
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embedded, so that we are likely to fail in applying the biological findings without this nuanced understanding. Thus, we recognize the importance of biomedical research and do not challenge NIMH's mission but do question its implementation and the reliance on a relatively narrow set of approaches that miss swathes of methodology. Transformative research is needed in both the biomedical and psychosocial realms, and their integration; as we note below, there are many exciting beginnings to this much-needed work that can help highlight new directions for the future.

NIMH Strategic Objective 1: Define the Mechanisms of Complex Behaviors

Strategy 1.1: Describe the Molecules, Cells, and Neural Circuits Associated With Complex Behaviors

A key word here is *behavior*. The brain is an organ that has evolved to interact with the environment, and it is both modified by the input that it receives and acts upon that input in the form of behaviors intended to deal with and (if possible) alter its surroundings. We have made great strides in defining human behavior and developing ways of measuring it. However, there is still much to be learned, and behavioral science is critical to further understanding the key building blocks of behavior, including understanding the crucial components of behavior that should be linked to particular circuits and cells (evolved adaptations designed to enhance the replication of the genes from which they arose) and developing methods to measure these behaviors reliably and validly, particularly in real-life settings that best capture

the experiences and behaviors that drive the need for mental health services and functional impairment.

There is the need to develop assays that can measure the behavior and experience of humans in their everyday life so as to better understand the ecological validity of laboratory-based measures and treatments. Psychological scientists have been involved in the development of tools such as ecological momentary assessment and other mobile behavioral assessments, which allow us to tap into the lived experience of individuals on a day-to-day basis. For instance, studies that prompt people via their smartphone to report on their current affect and social interactions at various times throughout the day and pair that data with passively sensed indicators, such as accelerometer and global positioning system (GPS) data, can shed light on how social anxiety and depression are associated with different temporal links between travel and movement patterns, social interactions, and changes in affect (e.g., Chow et al., 2017). This can allow for more direct tests of prominent theories, such as behavioral activation, that are the basis of current treatment advances. Such tools have been used both in the context of treatment provision (e.g., Areàn, Hoa Ly, & Andersson, 2016) and in the context of understanding the mechanisms of illness development and maintenance using ecological momentary assessment to understand emotional function in psychosis (e.g., Gard & Kring, 2009) and emotional stability in borderline personality and mood disorders (e.g., Scheiderer, Wang, Tomko, Wood, & Trull, 2016). As we continue and expand in this domain, this type of research will provide crucial information about which behaviors need to be assessed, how to assess them, and evidence when they have changed in everyday life.

Another key area in which behavioral science is important is in better understanding the translation between behavior measured in animal models and behavior measured in humans in order to identify and validate homology across species. Much of the work on molecules and cells, and even circuits, happens at the level of animal models that allow a level of precision and invasiveness not always possible in humans. However, that work will come to naught if the behaviors to which it is linked in animals do not have clear, useful, and predictive homologies in humans. We unfortunately have many examples of such animal models not translating well, and this may be improved by behavioral scientists' working more closely with neuroscientists to identify and understand key facets of behavior that translate across species.

Such issues have been particularly prominent in the domains of cognition, including memory and attention. There has been much work on the basic science of memory as well as preclinical testing of potential therapeutics in animals using paradigms such as novel object recognition. However, it has become increasingly clear that results in animals using such paradigms do not have predictive utility for identifying

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whether such therapeutics would be effective in enhancing memory in humans (Young, Amitai, & Geyer, 2012). There are numerous examples in which a new potential pharmacological agent that enhances some aspect of cognition in rodent models did not prove to be efficacious in human trials, as nicely illustrated in a recent meta-analysis of studies of alpha-7 nicotinic agonist effects on cognition in rodent versus human studies (Lewis, van Schalkwyk, & Bloch, 2017). As such, psychological scientists focused on understanding cognitive and affective psychological science are needed to work with animal scientists to identify paradigms that do have good homology across species (as exemplified with the Sustained Attention Task; Lustig & Sarter, 2016).

Strategy 1.2: Identify the Genomic and Nongenomic Factors Associated With Mental Illnesses

Behavioral science is crucial to identifying the nongenomic factors that influence the development of mental illnesses and psychological disorders, either on their own or in interaction with genetic factors. For example, there has been years of research on the influence of "stress" as a risk factor for mental illness and outstanding basic science research using informative animal models (e.g., Meaney, 2016; Sapolsky, 2015). Although we have a strong global idea of what stress is and how it relates to mental illnesses and psychological disorders, we still know relatively little about exactly what types of stress are most impactful for which people under which circumstances and at what age. For example, Gregory Miller and Edith Chen have been

doing work that illustrates how early life stress and adversity can become "embedded" and have sustained effects through initiating a proinflammatory phenotype that can help perpetuate negative physical and mental health outcomes (G. E. Miller & Chen, 2013). As another example, there is work showing that exposure to early life adversity moderates both hormonal and gene expression responses to acute stress exposure (Schwaiger et al., 2016).

Further, we know little about the nongenomic psychosocial elements that serve as important resilience factors that may reduce the negative impact of various stressors or mitigate the risk posed by certain genetic factors. These resilience factors may include personality characteristics, familial relations, local context and environment, extended family interactions, peer relations, and so forth. Once identified and their mechanisms understood, they could be harnessed in the service of promoting resilience in the face of adversity. An example in this domain relates to the translation of animal work on parenting and stress-hypoactive periods to humans. The animal literature provides consistent evidence that certain maternal behaviors in rodents are linked to reduced stress responsivity in pups at key developmental periods, effects that can have long-lasting effects through the animal's lifetime (e.g., Szyf, Weaver, Champagne, Diorio, & Meaney, 2005). Psychosocial research is translating these effects to humans, identifying which maternal or paternal behaviors are those that confer protection from stress reactivity in children, what the developmental course of these effects are in humans, and what implications disruptions in these behaviors have for long-term mental health outcomes. For example, maternal support in early childhood is associated with greater hippocampal volume across school age and subsequent better emotion regulation (e.g., Luby, Belden, Harms, Tillman, & Barch, 2016). Further, maternal presence can buffer against fear conditioning in children, even among those exposed to early trauma (van Rooij et al., 2017).

Finally, recent efforts to identify specific genes or polymorphisms that may be moderators or mediators of treatment outcome (termed *therapygenetics*) have emerged as a potent translational effort that connects psychosocial interventions to developments in gene technology (Eley et al., 2012). As genetic assessment becomes increasingly economically viable, the goal of personalized medicine may be further realized through, for example, matching psychosocial interventions to clients based on genetic factors (for example, catechol-o-methyl transferase gene [COMTVal158Met] as a specific liability in treatment outcome using exposure for panic disorder; Lonsdorf et al., 2010). As an illustration of this line of research, see the recent work from David Steffens's laboratory on the role of the COMTVal158Met in depression in older adults (Zannas, McQuoid, Steffens, Chrousos, & Taylor, 2012).

Strategy 1.3: Map the Connectomes for Mental Illnesses

The major efforts in the field to map human brain connectomes and to understand both their basic characteristics and their relevance to mental illnesses and psychological disorders have already benefitted greatly from the contribution of both fundamental and translational behavioral science. For example, the field now has access to data from the Human Connectome Project (Van Essen et al., 2013), a large-scale project mapping individual variation in human functional and structural brain connectivity in 1,200 individuals (including monozygotic and dizygotic twins and their siblings) using novel state-of-the-art imaging methods. This project included numerous measures of behavior across a spectrum of phenotypes relevant to understanding mental health that could be linked to measures of structural and functional connectivity (Barch et al., 2013). Published studies from this data set have focused on indicators of fluid intelligence (Finn et al., 2015; Hearne, Mattingley, & Cocchi, 2016), sleep (Curtis, Williams, Jones, & Anderson, 2016), depression and anxiety (Ely et al., 2016; Petrican, Saverino, Shayna Rosenbaum, & Grady, 2015), and subclinical psychosis (Sheffield, Kandala, Burgess, Harms, & Barch, 2016), to name just a few. In this domain, as in others, we will benefit by integrating work from the research of cognitive, affective, and social scientists delineating key aspects of human behavior in relation to brain connectivity with the work of clinical scientists mapping the ways in which these behaviors are related to risk for mental illness (see Barch, 2017; Poldrack & Yarkoni, 2016). Ongoing efforts to link behavior to patterns of functional and structural brain connectivity will greatly benefit from further development of precise, reliable, and valid measures of behavior that can be used in studies on a scale much larger than previous work, such as large-scale genomic studies that need better phenotyping tools to move beyond categorical diagnostic labels.

NIMH Strategic Objective 2: Chart Mental Illness Trajectories to Determine When, Where, and How to Intervene

Strategy 2.1: Characterize the Developmental Trajectories of Brain Maturation and Dimensions of Behavior to Understand the Roots of Mental Illnesses Across Diverse Populations

The study of different developmental pathways that can lead to the onset of mental illnesses and psychological disorders across diverse populations requires a greater understanding of cultural and environmental inputs that shape these pathways. More work is needed to determine how different types of environmental experiences may confer risk for psychopathology through different biological and psychological mechanisms. For instance, prior work has suggested that adverse social experiences, such as maltreatment or discrimination, confer risk for psychopathology in behavioral genetic studies (Jaffee et al., 2005), representative samples (J. G. Green et al., 2010; McLaughlin et al., 2010), longitudinal studies (Kim & Cicchetti, 2010), and studies introducing exposure adversity via experimental manipulations (Humphreys et al., 2015; Zeanah et al., 2009). In rodent models, the risks associated with adversity are presumed to be linked to physiological stress response systems (e.g., Eiland & McEwen, 2012). However, in humans, most studies observe complex patterns of responses to stress that do not mimic observations in rodent models (McLaughlin, Sheridan, Alves, & Mendes, 2014; McLaughlin et al., 2015), suggesting that the social environment interacts with neural plasticity to change brain maturation processes. Analogously, social experiences can serve as a critical protective factor, such as evidence that more responsive parenting is associated with longer telomeres among children who have experienced early life stress, indicating that parenting practices can buffer the normative telomere shortening (that is a biomarker of early adversity; Asok, Bernard, Roth, Rosen, & Dozier, 2013).

Sheridan and McLaughlin have built on decades of behavioral work to demonstrate that different environmental exposures (exposure to violence vs. lack of exposure to enriching cognitive experiences) impact different neural systems over time. For example, after controlling for threat exposure, deprivation exposure has been linked with brain regions implicated in complex cognitive ability (Lambert, King, Monahan, & McLaughlin, 2017; Sheridan, Peverill, Finn, & McLaughlin, 2017), and this link longitudinally predicts risk for externalizing psychopathology (A. B. Miller et al., 2018). In contrast, controlling for deprivation, threat exposure is linked with differences in neural pathways related to fear learning and stress reactivity, which mediate associations between threat and psychopathology (Busso, McLaughlin, & Sheridan, 2017; McLaughlin et al., 2016). This research offers just one example of the need to examine brain maturation within a social context.

It is similarly critical to understand psychosocial triggers of gene expression and related processes. For example, social rejection triggers changes in gene expression, prompting upregulation in proinflammatory cytokines, proteinases, and receptors for inflammatory mediators (e.g., IL-1β, IL-8, and HLADR, to name a few) and systematic downregulation of genes involved in antiviral immunity (e.g., STAT1, OAS1, & IFI27; see G. E. Miller, Chen, & Parker, 2011; G. E. Miller, Cohen, & Ritchey, 2002; Slavich & Cole, 2013; Slavich & Irwin, 2014). Importantly, these effects in the immune system are not restricted to peripheral systems. Through the process of glucocorticoid resistance, proinflammatory cytokines affect se-

rotonin synthesis within the brain (Slavich & Irwin, 2014), suggesting potential implications for brain development.

Developmental psychologists have articulated numerous types of social experiences that may be relevant to this line of research. For example, in the domain of youths' peer relationships, Murray-Close's work offers a useful synthesis of literature on children's peer status, peer victimization, and antisocial behaviors, each associated with unique physiological concomitants (Murray-Close, 2013a, 2013b). This literature also has articulated how the deleterious effects of negative social experiences may be shaped by gender and culture socialization (Rose & Rudolph, 2006).

Overall, extant research suggests that the study of brain maturation and developmental trajectories must account for the environment in which youth develop, with expertise needed in the conceptualization and measurement of sociocultural impacts and expertise in theories of psychosocial predictors of development.

Strategy 2.2: Identify Clinically Useful Biomarkers and Behavioral Indicators That Predict Change Across the Trajectory Of Illness

Remarkably little is known regarding the early biological or behavioral indicators of severe mental illnesses. In particular, more work is needed to identify symptom clusters present early in illness, or within subsets of people, that may be associated with more maladaptive illness trajectories. Naturally, the success of this objective depends on a synthesis of work on behavioral experiences that may trigger biological vulnerabilities, behavioral markers of physiological risk factors, and a developmental perspective on heterotypic continuity/discontinuity of early risk and vulnerability markers.

To date, the psychological literature has offered substantial contributions toward identifying biological and behavioral markers that predict illness trajectories. For instance, substantial recent work has demonstrated that onset of psychosis or trajectories of illness severity can be predicted by diminished functional capacity measured via behavioral tasks (McLaughlin et al., 2016) and neurocognitive abilities (Seidman et al., 2016). Research on adolescent suicide reveals support for multilevel models suggesting that trajectories of self-injurious thoughts and behaviors are predicted longitudinally by interactions between social stress and atypical parasympathetic stress responses (e.g., Giletta et al., 2015; A. B. Miller et al., 2017). Persistent antisocial behavior can be differentiated from transient delinquency based on volumetric brain measurement and callous/unemotional behavioral traits (e.g., Pardini, Raine, Erickson, & Loeber, 2014). Research reveals that attention-deficit hyperactivity disorder trajectories also can be predicted through a variety of biopsychosocial constructs, including more problematic family functioning and more stressful life

events (Biederman, Petty, Clarke, Lomedico, & Faraone, 2011). As these examples illustrate, the identification of early markers for mental illnesses and psychological disorders will benefit from continued work examining both biological and behavioral indicators, as the manner in which development of psychosocial constructs (e.g., cognitive, emotional, moral, social development) reciprocally transacts with biological development continuously over time, yielding complex multilevel interactions that are most relevant for understanding illness trajectories (see dynamic systems theory; Magnusson & Stattin, 2006).

NIMH Strategic Objective 3: Strive for Prevention and Cures

Strategy 3.1: Develop New Treatments Based on Discoveries in Genomics, Neuroscience, and Behavioral Science

As noted in the strategic plan, translational research is essential in applying the knowledge gained from so-called basic research to develop new interventions. A critical step in the process is identifying the most effective targets for treatment. NIMH has heavily invested in RDoC as a way to identify more precise diagnostics given the clear benefits that may emerge by looking across different levels of analysis and moving away from rigid, reified disorder categories.

For RDoC to deliver on its promise, a greater understanding of psychosocial targets and levels of analysis will be essential so that the work does not result in snapshots of constructs that do not translate into meaningful intervention targets (because the context in which acting on that target is embedded has not been considered). Currently, the RDoC materials include separate text noting the importance of development and the environment, but these ideas are not actually integrated in the matrix. It is critical to learn how RDoC constructs vary at different ages and critical periods; this will help to determine when the rate and type of observed changes reflect normative developmental variation or the start of psychopathology, allowing for earlier, more targeted support during critical periods. Analogously, investigating how cultural and environmental factors impact the RDoC constructs will enable treatments to be far more effective because adaptations to the treatment context will occur at the early diagnostic stage, instead of as an afterthought, in reaction to problems that arise when translating findings and interventions to a new context. Relatedly, incorporating an evolutionary perspective that recognizes evolved adaptations (akin to the way a fever is an adaptive bodily defense) can help to understand why changes occur across levels from genes to phenotypes, and so forth. Further, extrapolations from evolutionary theory can enhance understanding of treatment mechanisms; for instance, An-

drews, Bharwani, Lee, Fox, and Thomson (2015) suggest that antidepressant medications work through different mechanisms than is currently believed (they perturb basic homeostatic processes) and that the likelihood of relapse following treatment termination can be predicted by the extent of this homeostatic perturbation.

In addition to expanding what is included in the RDoC matrix, new approaches are needed to understand connections across problem areas. Models that move away from old assumptions about disorder categories that exist as latent entities, and instead consider the causal relations among symptoms (such as network models; e.g., Borsboom & Cramer, 2013), can allow us to map how co-occurring problems actually develop and change over time. Network models are likely to advance our ability to identify specific prevention and intervention targets that serve as central or hub symptoms that operate as tipping points for a cascade of other problems. Similarly, animal models can both elucidate the interplay between different brain regions in responding to stress and also provide clues as to the transdiagnostic impact of stressors, prompting the development of novel interventions (see Maier & Seligman, 2016).

Strategy 3.2: Develop Ways to Tailor Existing and New Interventions to Optimize Outcomes

There has long been recognition of the need to personalize treatments and match a given person to a specific intervention, but the field has had considerable difficulty doing this effectively. Meeting this need will require a broad perspective on possible ways to match people to treatments (to add to the current emphasis on trying to find a biological marker that will signify that a given intervention is likely to be effective). Examining both biological and psychosocial prerandomization variables in a way that integrates multiple sources of information into predictive models holds great potential to more successfully decide for whom to intervene. For example, recent work by Robert DeRubeis and colleagues (e.g., DeRubeis et al., 2014) on development and validation of a Personalized Advantage Index to predict optimal response to depression treatments based on individual differences (e.g., marital and employment status, life events, comorbidity, and prior medication trials) points to the promise of this approach for advancing our current understanding of matching. It also promises to make our tests of mediation more powerful given only those patients who respond to a specific intervention will have done so in response to a particular mechanism; moderation implies differential mediation (Kazdin, 2007).

Beyond deciding for whom to intervene and with what intervention approach, there are many open questions about when to intervene and when to change course. The growing field of "just-in-time adaptive interventions" holds great promise for tailoring interventions to maximize outcomes

(see Nahum-Shani et al., 2016). For instance, paradigms that integrate active (e.g., responses to prompts using ecological momentary assessment) and passive (e.g., GPS coordinates, accelerometer, physiological data) sensors to detect changes in the individual, such as a shift in mood or onset of a stressful event, can help identify specific moments when a microintervention could be helpful. Similarly, interventions that change in intensity, content, or delivery model based on user behavior or early signs of nonresponding can allow for more efficient allocation of resources and matching of intervention components to an individual's dynamically changing needs. For example, Bonnie Spring's work in technology-delivered behavioral health interventions using SMART (Sequential Multiple Assignment Randomized Trial) designs that add additional care components, such as coaching and text messages, based on poor early responding may greatly advance older versions of stepped care models (see discussion in Pellegrini, Pfammatter, Conroy, & Spring, 2015). Basic research in cognitive psychology is also needed to guide adaptations of treatments to improve patients' learning during therapy and enhance memory for therapy material (e.g., see Harvey et al., 2016).

Strategy 3.3: Test Interventions for Effectiveness in Community Practice Settings

The rise of dissemination and implementation research has broadened the ways we think about conducting effectiveness research. Beyond the standard calls for (a) testing alternate delivery models, (b) assessing a wider range of outcomes in the "real world," (c) evaluating approaches that can reach more diverse and geographically isolated populations, (d) more studies of prevention efforts with high-risk groups (e.g., those with genetic vulnerabilities), (e) bringing care directly to people rather than waiting for them to come to a clinic, (f) including longer term follow-up periods in trials, and (g) testing the impact of more flexible and inclusive trials, there are also great opportunities for (h) testing prevention and intervention efforts in the community that are delivered by nonmental health professionals. There remains much we do not understand about the effectiveness of interventions delivered by peers or by nonmental health professional counselors, though initial work on the delivery of interpersonal counseling (which derives from well-validated interpersonal psychotherapy) to reduce and prevent worsening of depressive symptoms suggests there are exciting opportunities to expand our reach for offering research-supported services at scale in diverse settings (see Weissman et al., 2014, for an example in primary care).

At the same time as expanding our conceptions about how and where and by whom to deliver prevention and intervention efforts, there is a great deal to understand about what mental health consumers and their families actually want and the reasons they do and do not seek treatment. We know

that many people do not seek mental health care (and, according to lore, the modal number of therapy sessions attended is just one), but there is almost no rigorous experimental work about what would make care more appealing (though see an intriguing early test of direct-to-consumer marketing for psychosocial treatment by Gallo, Comer, Barlow, Clarke, & Antony, 2015). Similarly, although we know that there is correlational evidence for the importance of a strong alliance between providers and patients, these data have been somewhat limited in their clinical utility in part because of the paucity of experimental work in this area.

NIMH Strategic Objective 4: Strengthen the Public Health Impact of NIMH-Supported Research

Strategy 4.1: Improve the Efficiency and Effectiveness of Existing Mental Health Services Through Research

There are already numerous efficacious treatment protocols for a broad range of psychiatric problems. However, these protocols have often been developed in research settings, and the degree that these protocols can be translated to service delivery settings remains to be demonstrated. Indeed, it has been suggested that significant barriers exist to translating protocols to nonresearch settings (McHugh & Barlow, 2010). Developing protocols that are expressly designed for dissemination as part of their development can help make this translation more effective. For instance, the Unified Protocol (Farchione et al., 2012), which targets conditions characterized by neuroticism, was designed with ease of dissemination as an explicit target. Recent work has expanded the Unified Protocol for application in other disorders, such as borderline personality disorder (Sauer-Zavala, Bentley, & Wilner, 2016) and child and adolescent anxiety problems (Allen, Tsao, Seidman, Ehrenreich-May, & Zeltzer, 2012). Further focus on interventions that target dissemination potential during the early treatment development phase can help address this translation problem.

Research has shown that practitioners can deliver evidence-based treatment, and even carefully controlled protocol-driven interventions, but after engagement in the research is completed, there is "therapist drift" back to previously established patterns of care that may stray from the available evidence (McHugh & Barlow, 2010). This is an important area that requires strengthening, which may be accomplished through infrastructure support for education aimed at training clinicians in evidence-based care. There is promising early work in this area, such as innovations in the development of "interagency treatment teams" (Aarons et al., 2014) to improve correspondence among providers and increase reliance on evidence-based care. Additionally, re-

search to test how different models of training lead to differential long-term use of evidence-based treatments is needed. These areas warrant far more research support to identify best practices for dissemination of evidence-based treatments into direct service delivery settings.

Strategy 4.2: Establish Research-Practice Partnerships to Improve Dissemination, Implementation, and Continuous Improvement of Evidence-Based Mental Health Services

Goldfried et al. (2014) have described the "two way bridge" between clinicians and researchers, which explicitly recognizes that there is a disconnect between these two groups, with researchers often in the position of effectively "lecturing" clinicians about how to implement mental health services. Efforts to facilitate the flow of information from clinicians to researchers, who can, in turn, integrate these critical service delivery experiences into effective therapeutic strategies, is essential for improving the acceptability of evidence-based practices. Along these lines, there have been approaches to connect clinicians and researchers, with positive results. For example, a network approach to facilitate correspondence between clinicians and researchers was established in France to improve treatment for schizophrenia (Schürhoff et al., 2015). In the United Kingdom and United States, practice-based research networks are serving an important role by having clinicians gather data "on the ground" about the outcomes in their practice so that the data collected have immediate clinical utility (see Parry, Castonguay, Borkovec, & Wolf, 2010). Efforts such as these are essential for fostering a positive feedback system to hasten the effective adoption and implementation of evidence-based services. This line of research could draw upon other disciplines within psychology, notably, social and experimental cognitive psychologists, who could address challenges of in-group bias (i.e., Molenberghs, 2013) when attempting to facilitate bidirectional collaboration between researchers and practitioners. Given the numerous ways in which these biases can interfere with the flow of information between groups (van Rooy, van Overwalle, Vanhoomissen, Labiouse, & French, 2003), experts on group processes would be ideal to promote better communication between researchers and practitioners.

Strategy 4.3: Develop Innovative Service Delivery Models to Improve Dramatically the Outcomes of Mental Health Services Received in Diverse Communities and Populations

The NIMH has sponsored research into treatment development that has revolutionized mental health. However, there remains a large gap between the research that shows what treatments are effective and the degree that these are

widely available to the public (McHugh & Barlow, 2010). Outreach to underrepresented groups is an especially important mission for mental health practitioners given the morbidity associated with untreated psychiatric disorders. Testing new service delivery models will be needed to meet this goal. For example, Chorpita et al. (2013) showed that evidence-based care could be delivered in a range of settings, including community clinics and schools, in the treatment of youth anxiety, depression, and disruptive behavior problems. Similarly, Patel and colleagues have trained nonprofessional lay counselors to deliver a culturally adapted version of behavioral activation to successfully treat depression in patients drawn from primary care settings in rural India (Patel et al., 2017). As there are substantial variations among service delivery settings for different age groups and diverse communities, populations, and conditions, much more work is needed in this area.

In addition to determining the best models for delivery of care in traditional settings, considerable work is underway that leverages new and emerging technologies for delivering mental health interventions. As noted earlier, one example involves employing ecological momentary assessment as a platform for delivering ongoing measurement of change as well as promoting change with prompts for interventions, such as in smoking cessation (Ruscio, Muench, Brede, & Waters, 2016). Other technological advances are available that could be modified and tested for their utility in a wide range of assessment and intervention methods (Luiselli & Fischer, 2016). Examples of how additional disciplines within psychology could contribute significantly to this area include enlisting school psychologists to get interventions to youth (i.e., Noell, Volz, Henderson, & Williams, 2017) and health psychologists to promote health behaviors and medical care compliance (van Os et al., 2017).

Strategy 4.4: Develop New Capacity for Research That Evaluates the Public Health Impact of Mental Health Services Innovations

Community psychology has contributed greatly to examining the public health impact of treatment research. For instance, benchmarking research that identifies the degree that evidence-based treatments are implemented with fidelity has been emphasized in European countries (e.g., the United Kingdom's promotion and evaluation of cognitive–behavioral therapy for multiple psychiatric disorders), and further research in the United States will be helpful to determine best practices that can serve as models to improve more poorly performing sites and services. In the United Kingdom, simply posting aggregate outcomes by clinical service on a publicly available website has led underperforming service providers to find innovative ways to better serve the public in their catchment areas and match the outcomes achieved by better performing providers (Clark,

2018). This strategy would benefit from involvement of marketing experts to get information on effective therapies to the groups most likely to utilize the information. This has been done, for example, in disseminating effective treatment for obsessive–compulsive disorder through a direct-to-consumer marketing campaign (Szymanski, 2012).

Industrial-organizational psychologists would be ideal partners to identify consumer groups and ways to better disseminate information to the public through organization stakeholders. For example, an initiative in Canada addressed the needs of individuals with severe mental illness by developing vocational programs employing clinicians, community psychologists, and industrial-organizational psychologists. Together, these professional groups identified the optimum methods of coordinating and integrating best practices in treatment with vocational training (Kirsh, Krupa, Cockburn, & Gewurtz, 2006). Partnerships between diverse professionals within psychology resulted in improved worker retention and provision of a work environment for a population that might otherwise struggle to secure long-term employment.

Conclusion and Call to Action

Periods of transition can be optimal times to rethink priorities and strategies. We believe that the change in leadership at the NIMH represents just such an opportunity to recalibrate the NIMH portfolio and better balance and integrate the full range of research approaches that are necessary to make significant inroads into alleviating the burden of mental illness and psychological disorders. As our analysis of funding rates illustrates, there has been a steady drift toward prioritizing biomedically oriented research at the expense of more psychosocially oriented research over the past 20 years. This has, in some ways, prioritized potential long-term gains at the expense of more immediate gains, and hampered our ability to link biological processes to the human environments, social factors, and behaviors that shape and define mental illnesses and psychological disorders. We have reviewed a few of the many ways that psychosocial research is central to achieving the NIMH strategic objectives, providing numerous concrete examples of such work and its relevance to successful achievement of these objectives.

As such, we argue that the time has come to rebalance these funding priorities so as to wisely invest in both biological research, which may eventually identify genetic and neurobiological factors that contribute to mental illnesses and psychological disorders, and psychosocial research, which can aid the dissemination of current psychosocial interventions that have known effectiveness, develop new efficacious psychosocial interventions, and identify psychosocial factors that we also know shape mental illnesses and psychological disorders. In other words, we have tools in the current toolbox that work, but we are not deploying

them optimally to help reduce the burden of mental illnesses and psychological disorders. In addition, we need to harness the expertise of both seasoned investigators and newcomers to the field to generate new prevention and intervention approaches, as well as identify future psychosocial targets for intervention, using all of the evolving technologies that are becoming available. The new NIMH funding mechanisms released this year that specifically target nonbiomedical approaches are a promising step in this regard, as is Gordon and the NIMH leadership's openness to discussing these issues (e.g., at a virtual town hall led by the Coalition for the Advancement and Application of Psychological Science in November 2017), and we look forward to continued conversation. The stakes are high.

It is also incumbent on clinical researchers to be up front about the very real limits of current psychosocial prevention and intervention effects. We do have some good tools in the toolbox, but our current approaches do not achieve sufficiently reliable, robust, or widespread effects, and some of our most efficacious approaches (e.g., exposure therapy) are not appealing or acceptable to many patients (see Garcia-Palacios, Botella, Hoffman, & Fabregat, 2007). Moreover, attrition is high for many interventions and relapse remains a serious problem; there is much we need to learn about the mechanisms underlying both treatment success and failure, and too little is understood about the effectiveness of interventions across diverse samples. A large dose of humility is thus in order about any one field's ability to address the grand challenges laid out in the NIMH strategic objectives. Psychosocial research is not a panacea, especially on its own, but a portfolio that has better balance and integration across fields may hold promise.

That balance and integration can hopefully come, in part, through psychosocial researchers applying for more grants, as we have emphasized, but there are other important ways for psychosocial researchers to contribute to (and help change) the system. This includes agreeing to serve on NIMH study sections or the National Advisory Mental Health Council. Although these service roles are time consuming, it is essential to have psychology (including psychosocial researchers) well represented during these discussions when funding decisions are made. In addition, we want to make sure that NIMH program officers and other staff are aware of the work we do, so inviting them to be discussants at conferences can help to make the value of our work clear. We also encourage researchers to get politically engaged, whether that be talking to elected representatives about funding needs (e.g., through American Psychological Association's science leadership programs) or even running for office themselves (e.g., 314 ACTION actively encourages scientists to run for public office).

There are many ways for psychological scientists to have impact, and it cannot happen in a vacuum. A more balanced approach to funding a diversity of research questions and approaches is needed to meet the grand challenge of NIMH to reduce the burden of mental illnesses and psychological disorders. Moreover, ensuring that truly innovative, investigator-initiated clinical trials (that may not yet have a known mechanism[1]) can find support will be important to lead to the discoveries that are so desperately needed. Now is the time to make this change so that we are in a position 10 years from now to look back and see clear progress in our prevention, assessment, and treatment of mental illnesses and psychological disorders.

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[1] Although we applaud NIMH's efforts to increase the field's understanding of the mechanisms underlying treatment effects, we are also concerned about the unidirectional push to always first identify a mechanism and then detect an effect. There are numerous historical examples of an important intervention being discovered first (sometimes by the chance observation of desirable outcomes), and the specification of mechanisms occurred later. Given that it is often easier to detect an effect than to explain it, we may miss the opportunity to discover new efficacious interventions by requiring researchers to always work from mechanism to outcome.

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