Editorial

Where Do We Go Next? Behavioral and Social Change for Child Survival

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The articles in this special issue of the *Journal of Health Communication* offer a comprehensive and nuanced survey of the extensive literature on behavior change linked to the improvement of global child health. The contributions are packed with empirical data and findings drawn from an extensive and scattered literature. This is not a small feat considering that behavior change and child health include large, unwieldy datasets located at the crossing of various technical areas and disciplines.

Child survival brings together myriad specialties and public health programs as well as disciplinary approaches and concepts from across the social sciences. It comprises numerous health issues and diseases—from infectious diseases to nutrition, from health policy to quality of services. It includes dozens of behaviors—from hand-washing to antenatal care, immunization to the use of bed nets—performed by mothers, relatives, and health providers. Child health behaviors are affected by numerous factors at the individual, community, social, and structural levels: knowledge, attitudes, perceptions, cognition, social norms, access to health services, power inequalities, gender roles, and policies and procedures. Programs to address behaviors related to child health have been implemented in communities and countries with significant particularities and differences across the world. Interventions have selected a wide range of strategies and tactics—from peer education to media campaigns to mHealth—to tackle behavioral obstacles and contribute to improving the health of children, mothers, and caregivers.

Identifying patterns amid this jumbled, chaotic body of evidence is not easy. It requires vast scientific and technical expertise, collaboration across disciplines and institutions, and familiarity with the heteroglossia of the literature on behavior and social change. There are no ready-made, straightforward conclusions waiting to be collected and shared. Instead, we find disconnected findings, different experiences, and parallel arguments produced by scholars and practitioners with widely different disciplinary and theoretical backgrounds. The articles admirably organize the enormous body of evidence in coherent findings and arguments. They patiently dissect the data, connect the empirical and analytical dots, discuss arguments, and advance propositions. Collectively, they provide an unprecedented, panoramic,

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and detailed view of the field that helps us make sense of the wealth of programmatic experiences.

The analysis and conclusions should be recommended reading for researchers, program planners, and policymakers who are interested in up-to-date assessments about what we know across child health behaviors, research and programmatic gaps, and future directions for inquiry and practice. This is a truly ambitious state of the art that successfully delivers a sweeping view of evidence, arguments, successes, and challenges.

It is difficult to offer a summary that would do justice to the enormous territory covered by the articles. Key themes, however, appear in the eight contributions and build on existing arguments in global health, development, and social change. The evidence and conclusions help us refine our knowledge about the linkages between behavioral and social change in child survival with important lessons for the burgeoning field of global health communication (Obregón & Waisbord, 2012).

The purpose of this article is twofold: (a) to offer a critical summary of arguments and gaps and (b) to make recommendations for further research and programmatic priorities.

What Do We Know?

The articles confirm that child survival programs need to be based on realistic, evidence-based expectations to promote behavior change. We have solid knowledge about what interventions might lead to positive transformations under specific circumstances and in particular settings. Among other findings, better knowledge about services (from immunization to counseling), positive attitudes about specific interventions (from certain nutritional foods to bednets), self-efficacy about particular actions (e.g., breastfeeding, child feeding), positive community norms about certain practices (e.g., regular visits to antenatal care centers, HIV testing for mothers and babies), and low barriers of access to service and health technologies, are more likely to lead to positive behaviors and health indicators. These are significant findings produced by researchers and programs for the past decades around the world, particularly since UNICEF originally called for the child survival revolution in the early 1980s.

These insights yield two important lessons. First, certain interventions might be conducive to change if they are the right way to address specific behavioral obstacles. Rather than solutions driving programmatic priorities, programs need to define strategies and tactics that are based on nuanced considerations of problems as well as past successes and failures. If negative attitudes about health services deter families from seeking care in established health systems, then, it seems sensible for communities to address existing concerns and improve perceptions. If high fees and transportation costs discourage families from getting diagnosed or using specific health technologies, then, solutions need to tackle those obstacles. Put it differently, only if we understand the relative weight of specific behavioral determinants that make healthy practices difficult or impossible can we then identify suitable courses of action based on past experiences and innovative ideas. Such problem-based thinking is needed to rectify the tendency to focus on strategies and tools. Flawless strategies and exciting tools are as good as they are adequate to address effectively the problems at stake.

Second, the articles demonstrate that no behavioral challenge in child survival should be approached with a single tactic. A combination of policy, technological, communicational, and educational tactics aimed at transforming behavioral obstacles at different levels increases the prospects of effective programming.

It is worth keeping this in mind in light of the perennial inclination to find single magic bullets to achieve impact. Programs should not search for panaceas or be driven by game-changing buzzwords given that the problems they address are embedded in complex social factors. Nor should they be expected to deliver unreasonable

results based solely on conventional wisdom and personal convictions about the transformative potential of certain tools. Enthusiasm about specific ideas should be tempered by evidence and embedded in a nuanced understanding of challenges.

Consider mHealth, which has received a tremendous amount of interest in recent years, much as old technologies (from radio to grassroots video to computer centers) attracted significant attention and funding decades ago. Mobile platforms are helpful to address specific obstacles that affect child survival (e.g., health care practices of caregivers, quality of health systems), as the chapter by Higgs and colleagues (2014) demonstrates, but they may not be effective to change community distrust or mistreatment of mothers and families that undermine effective care. Peer education may help to address misperceptions about nutrition practices and encourage breastfeeding among mothers and influencers. However, if other reasons explain why newborns are undernourished are different (e.g., availability of nutritious foods, control over decisions about expenditures and foods in households, stigmatized behaviors), a different, complementary set of interventions may be needed.

What Do We Need to Know?

Certainly, there are important data gaps in the behavioral literature on child survival. Two gaps deserve particular attention: evidence about the behavioral and social impact of interventions on community, social, and policy issues, and the scale and sustainability of results.

It is undeniable that we have more evidence about effective interventions to address behavioral obstacles at the individual level rather than at the community, social, and system/policy levels. We know better what to do when individual knowledge, attitudes, cognition, and self-efficacy are significant obstacles for improving child health. For decades, a range of communication interventions has successfully affected “ideational” factors that influence individual behaviors (Bertrand et al., 2012). Communication and behavioral science is, undoubtedly, more sophisticated and better equipped than in the past to change individual knowledge, perceptions and other psychological factors that influence behavioral preferences (Elder et al., 2014). Programs might be able to change knowledge and attitudes and affect child survival behaviors, as long as those are the main social determinants of specific practices, and there are no other significant obstacles such as access to health services or negative social norms. A significant body of evidence has shown the impact of conditional cash transfers on primary health care and child survival (Guanais, 2013; Lim et al., 2010).

However, we lack a similarly strong body of evidence about effective interventions to address challenges in communities, societies, and health systems. Many articles in this issue reach this conclusion and recommend further attention. No doubt, progress has been made in recent years, as shown by the articles on stigma (Nayar et al., 2014), community engagement (Farnsworth et al., 2014), health system strengthening (Vélez et al., 2014), and gender empowerment (Kraft et al., 2014). Yet, much remains to be done, particularly linking health policies and programs with behavioral outcomes.

On this issue, it is necessary to adopt a social change perspective that foregrounds the idea that social organizations (communities, organizations, governments) need to be collectively mobilized to address social problems. Solutions do not come only from disseminating information or changing individual attitudes or behaviors using sporadic, short-term campaigns. Rather, what is needed is to continue refining appropriate interventions to address social factors underlying the decisions of families and communities: social norms, trust and solidarity, power inequalities in households, neighborhoods, and societies, and the quality of health services particularly in poor and low-income communities.

Social change is not simply the accumulation of changes at the individual level. It demands significant transformations of social structures (norms, gender roles, trust) and institutions (health systems, funding, policies) underpinning and influencing individual decisions. Such changes make certain health practices more socially acceptable, equip mothers and families with social resources and networks, bring communities together around common problems, and prompt various stakeholders to support actions to change behaviors.

A broad social change perspective on child survival could help address a long-standing question in the communication and behavioral literature: the information-behavior gap. This gap reflects the limitations of the old conviction that knowledge leads to behavioral changes. It has been widely assumed that improved knowledge about disease transmission or available technologies and services would lead to positive behavioral changes. In some cases, this applies when there are no other significant barriers. Yet, this assumption is mistaken given that positive changes in knowledge (as well as on attitudes and cognition) do not always materialize in new healthy behaviors. The problem is espousing a narrow perspective about drivers of change that ignores or downplays social determinants that weigh more heavily than individual knowledge on individual and collective decisions. A social change perspective, then, offers a different entry point into child survival. It assesses the significance of a range of social forces that affect specific health-related behaviors, and opens up a range of possible interventions.

Also, it is necessary to address research gaps related to scale and sustainability. Both remain important challenges not only for behavioral and social research but for global health and development as well. As questions about aid effectiveness rightfully gained visibility during the past decade, these issues have moved to the center. We have evidence about behavioral programs that deliver results in particular settings and limited periods of time (during and immediately after interventions).

Answers about impact at scale and beyond the timeline of specific programs, however, are more difficult to come by. How do we scale-up effective behavioral and social results beyond the impact of “boutique” interventions? How do we know that what works in one community might be implemented elsewhere? What evidence is needed to generalize results and program impact? Similar questions are relevant to the issue of sustainability: How do we know, encourage, and ensure that changes promoted by specific interventions are sustainable through time? What is the likelihood that promising interventions are sustainable? Here I am thinking, for example, about the sustainability of subsidies and cash transfers aimed at encouraging visits to health posts after funds dry up or programs shift priorities or the long-term impact of mHealth messaging and reminders to health care givers and families once programs end.

Addressing these gaps demands fresh thinking among all parties involved. What we know and do not know is not accidental: It is the result of prevalent theoretical frameworks and questions in academic behavioral research as well as research priorities among program funders and managers.

On the one hand, researchers need to overcome empirical dispersion grounded in the wide technical and behavioral agenda of child survival and the interdisciplinary nature of the field. Studies need to identify common theoretical and research questions about social determinants of health practices. What actions effectively modify social obstacles to healthy behaviors across case studies? How do findings inform sophisticated theoretical arguments about causal pathways?

On the other hand, program funders and managers need to support flexible, comprehensive, and different approaches to impact data using various methods. As long as studies are tasked only with documenting short-term impact, we are unlikely to have data to determine whether changes are sustainable. As long as programs do not support large-scale interventions and assessments, we may not

know if results can be replicated and expanded. As long as interest in documenting successes and failures remains scant, we will not have a rich dataset to determine how behavioral and social change works. This is why it is important to bring in innovative ideas to impact studies on the basis of what we need to know to address persistent questions and move the field forward.

What Arguments Do We Need?

Despite gaps, the articles convincingly demonstrate that we have a substantive body of evidence which answers some, certainly not all, important questions about the impact of behavioral and social interventions on child survival. If findings are available, why program funders and managers continue to ask behavioral researchers and practitioners, not without a tinge of skepticism, “show me the data”? Why do questions about effectiveness still dog behavior and social change programs? Can we convince the unconvinced? What data do we need?

Let me suggest the following explanation: Even if the aforementioned research gaps are meticulously addressed, it is not obvious that new findings might satisfactorily respond to questions about the effect of behavioral interventions. The challenge is not the data *per se*. All kinds of data on effective interventions have been produced and will be produced in the future. Funders and managers, however, expect parsimonious explanations showing clear causal pathways linking interventions, behaviors, and health conditions. They do not just ask for data showing “what causes what” in specific cases. They expect coherent, elegant propositions that generalize and predict that certain stimuli/interventions will consistently lead to certain outcomes (and be cost-effective). The expectation is that behavioral studies need to deliver arguments that meet basic conditions of scientific research, namely validity, replicability, generalizability, and predictability.

It is not obvious that behavioral and social researchers can effectively meet this challenge or settle question once and forever. The stimulus-response model of research, conventionally accepted in epidemiological and medical research, uneasily fits the ontology of the subject of inquiry in the social sciences. This is, obviously, an issue that has been extensively debated in the epistemology of the sciences and produced writings that fill entire bookshelves. No need to summarize arguments and differences here. Suffice to say that it is difficult, if not impossible, for behavioral and social studies to produce the kind of theoretical arguments that meet standard criteria that defines “scientific” evidence in medical and public health research. As some articles show, health behavior can effectively use randomized control trials, widely considered the “gold standard” in medical scientific research, to produce evidence. Yet, other quantitative and qualitative methodologies are equally needed to yield a complex, nuanced body of evidence demonstrating impact. Continuous engagement with funders and program managers is necessary to harmonize expectations about evidence data, enrich the methodological toolkit, and identify potential financial and technical support.

Neither replicating and generalizing findings nor generating evidence-based predictions are the main challenges. We do have propositions that come close to meeting these requirements. A substantive body of evidence shows that media interventions and interpersonal communication programs, if planned and executed correctly, change levels of knowledge about specific health behaviors, disease transmission and prevention, and so on. Certain policy interventions such as cash incentives and transportation vouchers effectively encourage families to use health services. Community programs modify stigma and perceptions about health and disease. Messages delivered via mobile platforms effectively remind health professionals about case management practices.

The key challenge is different: The social complexity of health issues at the core of child survival programs. The articles patently demonstrate the range of behaviors and sociocultural settings and challenges, as well as the multilayered social determinants affecting specific health practices. Child survival comprises many behaviors such as breastfeeding, immunization, newborn care, institutional childbirth, and many others. Decisions about myriad behaviors are nestled in multiple factors: gender roles, self and collective efficacy, social norms, quality and access to health services, power structures, public trust of care providers, stigma, and so on. The weight of these determinants is not similar across communities around the world. No single variable neatly and inevitably affects specific behaviors across all instances.

Therefore, even if the literature produces persuasive and plentiful data about how specific interventions modify particular ideational factors or other determinants of health behaviors, it might not deliver the kind of concise arguments stating and predicting that particular programs will necessarily change myriad social determinants that influence child survival behaviors.

Social behaviors and determinants are too unwieldy, unpredictable, and contingent to meet consistently specific expectations about scientific arguments. What might work for one set of behaviors and social determinants might not be appropriate for others. What might work in one community might not be directly applicable elsewhere given social, cultural, and policy differences. What might work for one-off, episodic behaviors might not work for regular behaviors. No wonder, then, questions about effectiveness—the impact of interventions across populations and contexts (Balster, Levy, & Stammer, 2014)—remain critical.

Given discrepancies between expectations, it is important to continue conversations across scientific, professional, and institutional cultures to find common language and define common expectations. Researchers need to prioritize lines of inquiry that foreground causal pathways and add to theories of behavioral and social change. It is regrettable that empirical studies do not consistently engage with existing theories of social change. Descriptive studies about specific behavioral programs are mostly of interest to technical specialists concerned with, for example, improving rates of breastfeeding, increasing the percentage of institutional childbirths, and raising levels of children's immunization. They might not, however, attract attention from researchers and program managers interested and confronting similar behavioral and social problems.

Therefore, empirical findings about child survival programs need to be embedded in common questions and cross-cutting arguments about social processes and behavioral determinants. If stigma deters mothers from breastfeeding or seeking health care, how can stigma be effectively addressed? If negative social norms discourage families from getting children immunized or feeding health foods, how is norm change possible? Are policy incentives effective to promote sustainable behavior change? Do quality health services overcome distrust and resistance among families? Does community empowerment consistently change child survival behaviors? What are effective ways to address gender inequalities in ways that they have positive ripple effects across health behaviors?

Organizing research in clusters of common theoretical questions exploring the links among interventions, social determinants, and health behaviors and indicators might get us closer to responding to questions such as “what intervention leads to what behavioral change and health outcome.”

In closing, future research and programs need to cultivate a broad understanding of behavioral and social change, be driven by problem-based assessments and past experiences, support for innovative impact studies that address scale and sustainability, and promote collective efforts to refine coherent and elegant theories

of change. The articles in this issue provide plenty of insights and evidence to guide future behavioral and social research applied to child survival programming.

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