

The need for scaling up research on effective strategies for delivering child-survival interventions

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Considerations as to the feasibility with which child survival interventions can be delivered at high levels of population coverage, have been described as a central criterion of interventions intended to reduce child mortality.¹ Accordingly, over two-thirds of child deaths have been demonstrated to be preventable through the delivery of effective and low-cost health interventions, the integrated delivery of which, have also been suggested to be among the most effective strategies for improving child survival.² Despite this, and as the countdown to the Millennium Development Goals 2015 has intensified, commensurate admonitions to identify and augment the scale at which highly effective delivery channels for delivering child survival interventions, have remained far less vigorous. This has also been the case despite the fact that at present, the prospects that many countries have of falling short of achieving the fourth millennium development goal, of reducing child mortality by two-thirds - between 1990 and 2015, has become ever more likely.³

It was within this context that the 2008 Lancet Series on the Rebirth and Revision of Alma Ata, which staunchly advocated for a renewal of interest in the importance and potential of primary health care for improving maternal, newborn, and child health (MNCH), delineated four major factors contributing to the failure of the delivery of effective MNCH interventions at scale.⁴ This list identified the lack of a universally agreed upon minimum set of interventions that should be delivered to all women, newborns and children who require them; inadequate attention to demand creation for these interventions at the community level; shortages of well-trained staff and community health workers; and the failure to allocate the necessary resources to ensure the functionality of first level health facilities (including the forging of stronger links between facility-based staff and community-based workers) - as the main reasons for this drawback.⁴

Yet still, despite this acute awareness of the factors impeding the improved delivery of interventions, investments in maternal, newborn, and child health interventions have lost out over the past few decades and have been pitifully low despite the magnitude of the health burden, the availability of cost-effective

interventions, and the potential gains for such investments.⁵ Such gains include considerable reductions in neonatal and child morbidity and mortality, medium and long-term cost savings by national governments and health systems, and the averting of developmental complications, which may result from the neglect of unameliorated pediatric illnesses. Similarly, within the context of investments in neonatal survival interventions research, Martines *et al.* asserted that research into intervention delivery strategies was a priority for achieving universal coverage of effective health interventions.⁶

These findings, therefore make it unjustified that investments in child health research which focus on new interventions, have far exceeded those on delivery, in spite of the evidence that emphasizes the large potential contribution of the latter to mortality burden reduction.⁷ Part of the explanation that has been offered for this seemingly counter-intuitive phenomenon, has been ascribed to the fact that the dominant model of research priority setting has been driven by criteria, such as, interests of different advocacy groups and donors, media exposure, the individual biases of the members of policy-making panels, the attractiveness of research results, the novelty of proposed research, and the potential for publication in high-impact journals.⁷ A tendency for research to assess the effects of the biological aspect of an intervention, with little consideration for the requirements of the health care system (in which it is being delivered) for effective delivery, has also been an observable limitation constraining the levels of coverage of neonatal interventions.⁸

Furthermore, when compounded by other factors, such as stringent word counts required by publishing companies, attempting to incorporate data such as those involving the best delivery channel mechanisms with which an intervention can be delivered, can become an even more formidable task.

Moreover, research into alternative strategies for strengthening the delivery of interventions, have received little attention, despite the fact that a proportion of the failure to achieve adequate and equitable population coverage with good public health programmes, can be attributed to weaknesses in health delivery systems.⁹ This lack is largely attributable to the fact that effectiveness studies have often failed to separate the health impact of a specific intervention (such as a vaccine), from the delivery strategy used to reach the target population-for example, making a vaccine available in health facilities, with outreach posts, delivering it through community health workers, or organizing national immunization days.⁹ Also, many efficacy trials and effectiveness assessments use methods of service delivery that are ill-suited to scaling up within

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available resource constraints, due to concerns of being able to measure the impact in situations that maximize the chance of a positive effect.⁹

Nevertheless, this dearth in research has however, not been entirely grim. For example, the proliferation of guidelines offering assistance to researchers and policy makers when assessing the quality of clinical trials, are instruments, which can potentially partly ameliorate this situation. This is because such guidelines can be used to encourage authors to report on the delivery channels used to deliver interventions, when publishing their methods and or results. The 2010 CONSORT (Consolidated Standards of Reporting Trials) Statement on guidelines for reporting randomized trials, provide a clear example of situations in which such suggestions can be made.¹⁰ The fifth item on the checklist, which suggests information worthy of reporting for randomized trials, recommends that authors offer information on the interventions performed for each group in a trial, with sufficient details to allow replication, including how the interventions were actually administered.¹⁰ Such reporting could clearly provide an opportunity to mention the medium employed for delivering an intervention.

In addition, although this is an important area of research where a significant gap currently exists, and which will require significant resource investments if it is to be filled, the challenges inherent in conducting such research are also not negligible. Thus, strengthening delivery channel research will involve commitment, scientific rigor, meticulousness, and substantial planning, if they are to yield informative and useful results. As such, current and future research endeavoring to investigate the delivery channels of inter-

ventions will necessarily and invariably have to take several critical factors into consideration. For instance, factors such as the social context in which the intervention was delivered will have to be assessed, as this factor is inextricably linked to the impact of an intervention. Similarly, and more specifically, factors such as the cultural and economic conditions of the setting, should also be identified, examined and taken into consideration, as they may also influence the effectiveness and impact of an intervention's delivery.

Future endorsements or recommendations offered for the use of specific delivery channel mechanisms, should also, as much as possible, be pre-ceded by randomized controlled trials comparing their relative levels of efficacy and effectiveness. Delivery channel information should also provide details such as on the intensity with which the delivery channel was employed, the frequency with which it was applied, as well as the means through which they were applied (e.g. through mass media, community health workers, immunization days etc.).

Furthermore, dynamic and pragmatic delivery mechanisms will be needed in order to deliver interventions at high levels of population coverage, and at the level of the community in many developing countries, the latter being the location in which where they are needed most. This is especially so in light of realities, such as that many women in developing countries continue to deliver children at home and will likely continue to do so in the near foreseeable future. Other concomitant realities, such as the fact that first-line child providers - especially in the case of newborns - are traditionally family members or traditional birth attendants, and the fact that mothers may often be confined to the home after giving

birth, also emphasizes the importance that should be given to reaching these families through a combination of approaches.⁶ Additionally, chronic shortages in human resources within the health care workforces of many developing countries with high child morbidity and mortality burdens and where health systems and services are chronically and severely under-funded, may also be afforded greater benefits by such research, as it may provide them with more insights on how to optimize their use of limited available human and material resources. As seen, an unjustifiable gap persists between available research, and that needed to facilitate a greater supply of knowledge on the effectiveness of key child-survival delivery channel mechanisms. Cost-effective, affordable and highly scalable interventions also exist, but their utility can only be optimized, if the key delivery channel mechanisms with which they can be delivered are known and harnessed. But undoubtedly, more investments are needed, if this research is to be encouraged and carried out.

That said, such undertakings would undoubtedly present varied and un-negligible challenges during the process. However, the cost of these investments will be considerably modest compared to the cost in lost infant, child, as well as maternal lives that may result if such measures are not taken.

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