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# Pragmatic politics and epistemological diversity: the contested and authoritative uses of historical evidence in the Safe Motherhood Initiative

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In recent years, the demand for cost-effective evidence of health impact has grown exponentially, often to the exclusion of other disciplines and of epidemiology's longstanding interest in the multivariate determinants of health. Drawing on an ethnography of the Safe Motherhood Initiative, this paper focuses on experts who, in producing historical case-study evidence, exceptionally inhabit a stigmatising epistemological position while still successfully commanding the respect of policy makers. To theorise the sources and effects of this epistemic diversity, we draw on the anthropology and sociology of science, and specifically on Holmes and Marcus' analyses of 'para-ethnographic' modes of reasoning.

'If you look at the UK [United Kingdom] over the last 100 years, we have developed, reduced mortality, etc ... by putting all the pieces together ... but ... I don't think we are doing this in developing countries. I'm always a bit surprised and concerned when agencies think, well, if we just focus on this [technical component], we'll get it right. But we've never done that in any developed country. We've always had all of it, together.' (Epidemiologist and policy adviser)

'Decision makers just don't get as excited about "process". We keep repeating the same thing over and over.... We have evidence of [the importance of skilled attendants and institutional deliveries] in historical evidence, in trends, but that sort of evidence is not enough.' (Public health researcher and senior adviser to the United Nations)

## Introduction: denouncing 'evidence-base advocacy'

Over the past two decades, the demand for experimentally derived cost-effective evidence of the health impact of proposed clinical and social interventions in global public health has grown exponentially, often to the exclusion of other epistemological traditions within epidemiology and allied social science disciplines (Lambert et al,

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2006). Some epidemiologists and public health specialists have publically bemoaned this shift towards impact research, arguing that it has undermined one of epidemiology's core defining features and strengths, namely an interest in multivariate understandings of the interconnected biological, social and economic determinants of health (Davey Smith et al, 2001; Victora et al, 2004; McPake, 2006). By and large, it is those experts who support a broad health systems and socioeconomic developmental approach to health improvement who are at the forefront of these views (eg, Travis et al, 2004).

According to some scholars, the demand for impact evidence can be partially attributed to the infiltration of neoliberal market-principles in global public health over the past 20–30 years (Strathern, 2000; Mykhalovskiy and Weir, 2004). Also important is the growth in global accountability mechanisms, such as the Millennium Development Goals (MDG) initiative and monitoring framework (McCoy, 2004; Travis et al, 2004). For major donors operating increasingly at a global level, impact evidence facilitates the calculation of the health returns on investments and provides a key mechanism for holding donor recipients (including governments) to account (Laurell and Arellano, 1996). As a result of this changing global health field, the past two decades have witnessed the rise of self-identified global health 'advocacy coalitions' comprised of networks of experts devoted to keeping a particular health issue on the agenda through the use of various tools that 'generate political priority' (eg, Shiffman, 2003).

Evidence production counts among the most central of these tools. As we have noted elsewhere (Béhague and Storeng, 2008), subfields that can base their policy recommendations on high-quality experimental evidence, now deemed the gold standard method for evaluating virtually all types of health programmes, are seen to be more readily prioritised and given funding by global agencies, national governmental organisations and non-governmental organisations (NGOs). Yet even those experts who are openly critical of the excessive demand for impact evidence often feel powerless to counter the institutional structures that account for these developments. Some even find themselves (un)willingly producing impact research even when they do not deem it programmatically or epistemologically necessary (Béhague and Storeng, 2008). Those who have grown particularly frustrated with this process now use the term 'evidence-based advocacy' not as a neutral synonym of 'evidence-based policy' but rather in a cynical way to refer to the political and competitive uses of evidence in advocating for coalition-sustaining funds. For these experts, evidence-based advocacy stands in detrimental contradistinction to what evidence-based policy making should be comprised of, namely the use of evidence to better understand mechanisms of change and to engage with programmatic problem solving in the settings in which interventions are being implemented (Storeng and Béhague, 2012).

This paper draws on ethnographic research to explore how these debates are playing themselves out in unusual and provocative ways in the Safe Motherhood Initiative (SMI) subfield. The SMI is unique in a number of ways, not least because it has often seen itself as marginally situated vis-à-vis the broader field of global health. Indeed, as we began our fieldwork, we were surprised to learn that when maternal health was selected to be one of the eight MDGs, this represented a mixed blessing; while the MDG suggested both a long-awaited global recognition of the importance of

maternal health, many feared that this added attention would come with additional pressures to produce evidence of impact within a timeframe that would invariably only consider ‘quick-fix’ interventions. In this and other ways, key SMI actors are critically engaged with debates regarding the need for epistemic diversity and the limitations of evidence-based advocacy.

What is more significant is the fact that SMI actors are also finding ways to push for the conceptualisation of health improvement as a social, political and governance issue through the production not of experimental impact evidence, but rather, of observational forms of evidence; most notably, historical case studies of *naturally* occurring mortality declines. As we will demonstrate, historical case studies are being used within the maternal health field to challenge dominant epistemic values. Yet this type of epistemic diversity has not been wholly marginalised; rather, it has emerged as an unlikely (if also indirect) source of empiric clout. By using historical insight and case-study methods, SMI experts are inhabiting a potentially stigmatising evidence-production position that risks undermining their already fragile authority as policy-savvy scientists, while, paradoxically, still successfully securing funding and commanding growing respect.

## Methods

Our ethnography was conducted on a continuous part-time basis from 2004 to 2009. We relied on open-ended in-depth interviews, participant observation and a review of published and grey literature, including informal documents provided to us by our informants. A total of 72 experts were interviewed (see Table 1), identified opportunistically through professional networking, publications and conference proceedings; 29 of these were from academic institutions, primarily in the UK, the United States (US), Belgium and Norway, with a minority (6) from collaborating research institutions in Africa and Asia. Although they self-identified as maternal health experts, many informants had experience of working in multiple domains of international public health.

**Table 1: Distribution of informants according to role at the time of the interview**

Role	Number
United Nations agency officials	12
Bilateral agency officials	11
International academic researchers	23
NGOs or foundation representatives	17
National-level policy makers, programme managers and researchers	9
Total	72

Interviews followed an open-ended guide to explore definitions of evidence, professional experiences with the production and use of evidence for policy making,

historical shifts in policy, debates around integration and the strengthening of health systems, and the safe motherhood field's relationship with donors. Participant observation was conducted within academic settings, as well as at 20 research meetings, academic conferences and policy meetings. Of these, eight were not specific to maternal health but focused on general public health or child, neonatal or reproductive health. For details, see Béhague and Storeng (2007, 2008) and Storeng (2010).

## Roots of exceptionality

The SMI was launched in 1987 in an attempt to redress what experts identified as the marginalisation of the 'M' in maternal and child health programmes, or the 'neglected tragedy of maternal mortality' (Rosenfield and Maine, 1985: 83). Since then, a recognisable network of researchers, advocates and policy experts has emerged. At approximately 200 to 300 people globally, the SMI is small relative to other prominent global health coalitions such as, for example, the Global Fund to Fight HIV/AIDS, Tuberculosis and Malaria. Its self-reflective history, notably the topic of several editorials, is mired in what Storeng (2010) has called a 'narrative of failure', which refers to both lack of progress in maternal mortality decline and the SMI coalition's lack of ability to be effective advocates for funds and political will at the global level. One could further speculate that it is *because of* the continually reflexive and public uses of this narrative of failure that the SMI has in fact over time managed to position itself strategically and effectively, devising and advocating for solutions to maternal ill-health and mortality that have held sway globally and in low- and middle-income countries (eg, Abouzahr, 2003).

Such institutional authority has been achieved despite the fact that producing evidence of efficacy of interventions to reduce maternal mortality (MM) is exceedingly difficult, primarily because MM is complex to measure and of low enough prevalence (even in high prevalence settings) to make conducting experimental evaluations a veritable logistical challenge (Starrs, 2006). Added to this constraint is the sociodemographic composition of the first generation of SMI experts comprised of a mixture of: first, Francophone public health experts, many of whom assert that evidence-based medicine in Anglophone contexts has been taken to an inappropriate extreme; and second, female population scientists of a generation for whom the population sciences represented not only informative fields of scientific activity but also politicised tools for exposing gender and economic inequities. A few of our informants working in more established and male-dominated global health coalitions depicted the first phases of the SMI as having been comprised of "just a bunch of feminists" swayed by ideology more than by science.

Contrary to this depiction, and possibly in response to it, SMI researchers have invested a great deal of effort in tackling the so-called 'measurement trap', a term used in a key 1992 article that highlighted the way in which lack of data is intricately linked to lack of prioritisation (Graham and Campbell, 1992). In fact, a few key leaders have devoted the better part of their careers to devising innovative measurement techniques for estimating the MM ratio (MMR) in data-poor developing country contexts, not so much due to absolute conviction in measurement, but rather because of a

pragmatic recognition of the political centrality of easy-to-standardise measurement techniques. As we will show, however, these very same actors have not become mere measurement 'technocrats', losing sight of the specific socio-institutional contexts in which statistical indicators should be interpreted. Rather, they have mixed canonical statistical methods with in-depth historical case studies, appealing to both normative and (increasingly) marginal epistemologies and, through this, implicitly contesting the rise of a cost-effectiveness framework in global health and the broader neoliberal values of which this framework is a part.

## On the borders of normative styles of reasoning

To theorise the sources and effects of this epistemological diversity, we draw on a growing body of philosophically inspired literature on the history, sociology and anthropology of scientific epistemic cultures, genres or styles of reasoning (Fleck, 1979 [1935]; Hacking, 1992; Foucault, 1994 [1970]; Cetina, 1999; Rose, 2007; Morgan, 2012). Of all such styles, the rise of statistics and notions of risk, and of how statistics engender powerful truth-values integral to modern forms of governance, has been the topic of considerable attention (Daston, 1992; Porter, 1992; Rose, 1996).

As authors have shown, it is precisely the assumption of neutrality and objectivity that gives science an autonomous and self-vindicating style of reasoning (Hacking, 1992; Pickering, 1992). This style of reasoning typically responds to unexpected observations not by engaging in fundamental reinterpretations, altering key research questions or even scrutinising epistemological limitations, but by identifying such observations to be either 'outliers' or artefacts of poor measurement technologies. Anthropologists and sociologists have now provided exceptionally insightful analyses of the centrality of this medico-scientific ethos in the global dissemination of template-based approaches to clinical and public health practice (Geissler, 2001; Timmermans and Berg, 2003; Adams, 2005; Lambert, 2006).

In studies of the global health arena, less attention has been given to how epistemic genres divide and multiply (Cetina, 1999). Our initial ethnographic research led us to wonder how it is that alternative epistemologies emerge not simply through highly polarised debates on the marginalised fringes of normative epistemologies, but through the everyday practices of influential scientists who are willing to seriously consider 'aberrant' empirical observations that confront both their assumptions of the world and the normative epistemologies they use to understand it (cf Harding, 2008). As we will demonstrate empirically below, some SMI researchers do not adopt a self-vindicating style of reasoning but, rather, reconsider the epistemological assumptions imbedded in their methodological approaches as they actively shape a new and more *interactive* relationship with the unruly world they observe.

Holmes and Marcus (2008: 237) have described these to be 'para-ethnographic' modes of reasoning that emerge from the 'de facto and self-conscious critical faculty that operates in any expert domain as a way of dealing with contradiction, exception, facts that are fugitive, and that suggest a social realm not in alignment with the representations generated by the application of the reigning statistical mode of analysis'. Although 'para-ethnographic' ways of knowing are normally relegated

to the unscientific and less powerful realms of ‘anecdote and intuition’, Holmes and Marcus describe the ‘sustained puzzles’ that compel (and empower) experts to destabilise statistical forms of reasoning that tend towards simplification and the marginalisation of contingency (or ‘context’) (Holmes and Marcus, 2008: 238). It is precisely this process of destabilisation via the use of historical and case-study evidence that interests us in this paper.

## Early historical insight: the comprehensive agenda

The SMI was launched nine years after the 1978 Alma-Ata Declaration, in which primary healthcare was endorsed as a fundamental human right and as a cornerstone of development as a whole (Rosenfield and Maine, 1985). Alma-Ata focused on creating conditions that would ensure maximum community and individual self-reliance and participation in developing a strong healthcare system, which included the building up of a well-integrated multi-tiered health system and partnering with sectors outside of health, including education and civil society (WHO, 1978).

The post-Alma-Ata era of the 1980s was typified by a recognisable split in public health ideologies, with a then-growing contingent of stakeholders arguing that comprehensive approaches were too expensive and lengthy to implement in poor countries and that interim approaches based on ‘selective’ primary healthcare – such as the GOBI (Growth monitoring, Oral rehydration, Breastfeeding and Immunisation) initiative in child health – should be actively endorsed (Rifkin and Walt, 1986). As the director-general of the World Health Organization, a supporter of comprehensive approaches, stated in 1987 in relation to MM, ‘[t]he roots of much MM lie in discrimination against women, in terms of legal status and access to education, financial resources and health care, including family planning’ (Mahler, 1987: 668).

Selective approaches, in contrast, translated primarily into the training and promotion of traditional birth attendants (TBAs) and the implementation of antenatal care risk screening programmes – the two main community-based primary care strategies that had received most attention and investment in the 1970s. Throughout the 1970s and 1980s, selective primary healthcare became increasingly popular among donors and governments who, concerned with limited budgets, preferred the identification of targeted interventions that would (theoretically) impact on mortality quickly (Cueto, 2004).

From these early days, however, a good portion of maternal health experts either rejected selective approaches or felt uncomfortable endorsing them, even when these were explicitly justified as ‘interim’ strategies that should not stand in contradistinction to longer-term and more sustainable solutions. As one of the founding members of the SMI stated in support of the comprehensive agenda, “I think the maternal health field has suffered for always saying we need cheaper short-term solutions. I think we’ve always gone for the ineffective things, like antenatal screening and TBA training.” To be sure, by the mid to late 1980s, epidemiological trend data from those low-income countries where MM could be monitored showed virtually no decline in the MMR, despite the relatively significant investments that had been made into these two strategies (Goodburn and Campbell, 2001).

The question of how to interpret this trend data and whether even to consider them valid was highly contentious. Strong epistemological views against accepting trend data as anything more than speculative or at times within the margin of ‘statistical error’ were beginning to take hold, yet those who wanted to learn from history considered trend data to be more than mere outliers or ‘fugitive facts’ (Holmes and Marcus, 2008). To provide these observed ‘facts’ with a stronger empirical and interpretive base, some then turned to the works of medical historians who had analysed mortality declines in late 19th and early 20th-century European countries. In doing so, SMI experts were also partially attempting to address the debilitating ‘measurement gap’ by learning from countries that had instated accurate statistical surveillance systems already in the mid-1850s.

Among the most well known of the medical historians read and cited by these innovative SMI experts was Irvine Loudon, a family doctor from Oxfordshire who retired in his fifties to take an academic position as medical historian. Loudon was the first to note that unlike the all-cause mortality decline that had occurred in most Western European countries with improved nutrition and socioeconomic conditions throughout the 19th century, MM appeared to be ‘relatively insensitive’ to broader social development, for it had remained high – as high as in many of today’s developing countries – well into the 20th century (Loudon, 1986, 1991). Case-study analyses of the two to three decades in which MM did eventually decline showed that it was not vast amounts of technological investment and socioeconomic development that was required. Rather, Loudon attributed MM decline to political prioritisation of the issue, more intensely skilled and accountable midwives, and concerted coordinated health system action to improve the therapeutic management of childbirth, including both the curtailing of unnecessary medical interventions and the more effective control of sepsis (Loudon, 1986, 1991).

Significantly, these early studies also showed that late 19th-century Sweden appeared to have managed to decline its MMR earlier than most other countries, despite the fact that it was among the poorest and most rural of European nations. Using a combination of qualitative and quantitative social historical methods, a group of Swedish public health obstetricians began investigating why this might be (Högberg et al, 1986). The answers to this puzzle clearly supported a comprehensive primary healthcare approach: in addition to key clinical innovations (eg, control of sepsis), Sweden’s successful MM decline was attributed to a *synergistic* combination of factors, including the creation of a trained and fully accountable rural midwifery system, increased awareness of the problem through advances in localised statistical surveillance run by local monitoring committees, ensuing political will and systematic governance of the problem of inequitable access to care (Högberg et al, 1986).

The ‘popularisation’ of these historical studies into mainstream public health in the 1990s was central to the creation of political momentum for the SMI as a whole, despite the fact that the comprehensive approach they appeared to endorse was at odds with the selective ideologies that were gaining ground at the time. In part, this contradiction can be explained if we recognise that what appealed to the wider global health audience was not a comprehensive agenda per se, but the key policy implication of this body of literature: as one key stakeholder explained, “these studies showed that

public health action *can* make a difference”. That is, through targeted health system action, MM *can* decline even while a country is still poor and largely rural, struggling with public health problems of the kind affecting many developing countries today. Not coincidentally, it was precisely during this time that MM in developing countries was reconceptualised as not just a ‘neglected tragedy’ but a ‘preventable tragedy’ that would require the urgent response of donors and the industrialised West (Rosenfield and Maine, 1985).

Even so, SMI stakeholders’ relationship with historical insight did come to have a substantive impact on ways of thinking, among both those who supported and those who were sceptical of comprehensive approaches. As one of the founding members of the SMI, notably an epidemiologist, explained:

‘To me, the historical evidence [has been key]. The fact that in the development of the West, MM was unchanged for a hundred years, while everything else went down ... it was a big scandal actually, at the time, the medical journals around 1930, 1933 ... and then ... it disappeared as a public health problem within 15 years, having not changed for 100 years.... This says to me, we’re dealing with a very strange kind of critter here.... And we can’t use our same assumptions. To me, that kind of stuff is definitive.’

Just how historical case-study material, comprised of archival, epidemiological trend data, and qualitative social history, came to be taken as ‘definitive’ can at least partially be attributed to the way safe motherhood researchers themselves read and cited the works of medical historians. Equally important was the fact that Loudon, being an outsider to the field of safe motherhood as well as a medical doctor, occupied an authoritative and distanced position relative to those at the heart of the SMI. In his main monograph, published in 1992, his innovative methodological approach was made explicit, as was the fact that he situated his work, epistemologically, within a ‘moderate’ empirically grounded but critical position:

Although some notable histories of maternal care have been confined to a socio-historical or feminist approach with scarcely a statistic, let alone a statistical evaluation in sight, I believe that without rigorous statistical analysis, the history of maternal care can easily become impressionistic, unreliable and in the end unsatisfying. If there is a danger that a purely demographic approach may deflect attention from features of central importance which are inherently unmeasurable ... there is also the danger that without statistical analysis large conclusions are often based on the shaky foundation of thin evidence and small unrepresentative samples. (Loudon, 1992a: 5)

The extent of the influence that this early body of literature had in the global arena is manifest in a World Health Organization document published in 1994 entitled *Mother–baby package: Implementing safe motherhood in countries* (WHO, 1994). Making direct reference to Loudon’s historical research, the authors of this report explained in simple language that although historic Sweden was still largely a rural country with

a scattered population, ‘training and rural assignment of qualified midwives led to a considerable reduction of MM’ during the second half of the 19th century (WHO, 1994: 7). In crucial ways, the authors retained Loudon’s broader messages regarding the importance of effective governance, equity and empowered midwifery in their delineations of a range of immediate-impact and long-term initiatives needed to reduce maternal mortality.

## The public health lens: identifying ‘modifiability’

In the mid- to late 1990s, this broad-based perspective was ardently challenged. A powerful contingent of the SMI began promoting an alternative interpretation of Loudon’s research to buttress a more selective policy position, one that endorsed access to emergency obstetric care – or EmOC, as it was coined – as the single most important intervention for MM reduction. Maine and Rosenfeld (1999: 481), probably the most influential of researchers who promoted this view, argued in a highly cited 1999 editorial that of the many different subcomponents officially endorsed in the comprehensive safe motherhood agenda of the time, ‘only one – essential obstetric care – includes actions that can substantially reduce maternal deaths’.

At the heart of this reinterpretation of the historical record was both the view that public health’s primary remit should be to focus on identifying ‘modifiable determinants’ and a core concern with advocacy and the political life of the SMI itself (Pearce, 1996). According to Maine and Rosenfeld (1999), the main reason the safe motherhood field had failed either to make a dent in the MMR of most developing countries or to become a well-established global advocacy coalition was because, unlike the GOBI strategy adopted in child health, it ‘lacked a clear strategic focus’ and endorsed approaches that made policy makers feel as though reducing MM ‘would require dauntingly vast efforts’ (1999: 481). Although proponents of this position agreed that EmOC should ideally be implemented in concert with a series of strategies, the resource-poor ‘reality in countries with high MMR’ is such that leaders need to know ‘whether to give priority [either] to more skilled birth attendants (SBAs) [working in home environments and primary/secondary care clinics] or to EmOC [housed in fully equipped health centres]’ (Paxton et al, 2005: 183).

Critics of this policy position were not hard to find, particularly among those experts who had originally popularised Loudon’s works. Several of our informants told us that Maine and Rosenfeld’s position represented a selective interpretation of the historical evidence on MM decline, one that assimilated only the ‘technical’ (clinical and administrative) conclusions about treatment, while ignoring the very important messages about political will, social momentum and community accountability – that is, the broader statistically ‘unmeasurable’ variables that Loudon had identified. The ironies of this interpretive turn were highlighted repeatedly by some of our informants; the same historical analyses that had pointed to the importance of social and political factors were being used to justify the targeted focus on EmOC – a focus that, as another informant argued, “was at risk of becoming the new targeted panacea”.

Despite the growing pervasiveness of these critical voices, there was also a growing sense that it would be damaging to the field’s reputation to demonstrate a lack of

programmatic consensus by *not* endorsing a more selective EmOC approach. Here was a fresh new and simple policy proposal that was attracting considerable interest from the donors. To criticise the focus on EmOC for being too ‘selective’ and ignoring ‘complexity’ or ‘context’ was tantamount to repudiating donor support and undermining the field’s international credibility. This sense, according to some informants, is what eventually completed the policy shift from the mid- to late 1990s from an integrated comprehensive approach to a more exclusively curative approach focused on either EmOC only (in a more targeted version of this shift) or EmOC together with the provision of SBAs. Several of our informants who felt troubled by this shift even felt that they had played a ‘complicit’ if somewhat inadvertent role in endorsing these more ‘technocratic’ interpretations of the historical record just by virtue of their failure to keep repeating Loudon’s broader messages.

From the late 1990s onwards, the maternal health field would continue to grapple with the same core tension – one of attempting to maintain a holistic policy position while contending with the rise of ‘selective’ ways of reasoning promulgated by the growing dominance of cost-effectiveness agendas. Two types of research responses to this tension would emerge in the years that ensued. The first and most aligned with cost-effectiveness rationales pushed forward with the call for evaluations of intervention packages that would theoretically provide the greatest impact on mortality with the least economic investment. A second research response, continuing in the tradition of earlier historical studies, was pushed forth by those who were concerned with the way effectiveness-evidence models feed into the fragmentation of comprehensive approaches and neglect the broader mechanisms of change that early historical analyses had highlighted. In the sections that follow, we will look first at the growth in cost-effectiveness practices and then consider how and why case-study research – which we take to be representative of para-ethnographic modes of reasoning – grew out of dissatisfaction with the experimental ‘taming of complexity’ (cf Hacking, 1990).

## **Cost-effectiveness and the search for political clout**

Throughout the early to mid 2000s, public denouncements of the lack of evidence for the policy shift towards facility-based interventions (be this based on EmOC or SBAs or, at best, a combination of both) came to a head. Some of this critique was external to the safe motherhood community and came from child and reproductive health experts, who worried that the focus on professionalising birthing care in secondary and/or tertiary-level facilities would take attention and funds away from the community-based cadre of health workers so integral to delivering child and reproductive health interventions. One prominent child health expert argued, for example, that this policy shift had been inappropriately based on no more than “observational epidemiology, quantitative history” and “dubious analyses of mortality trends”. Such critical denouncements were common; several of our informants explained that the potential for the biased use of historical research is reinforced by the ‘weak’ nature of such evidence, the lack of generalisability, the inability to prove causality and thus, the propensity to be used as a tool for reinforcing non-scientific

and ideologically driven policy preferences. “History contributed [to the EmOC/SBA agenda],” one informant described cynically, “and experts love to use this so-called ... evidence, but [history] is not evidence actually, but rather a robust interpretation of a given set of facts.” The relative epistemological weakness of historical case-study evidence was thus recast as an anathema to evolving evidence-based values, thereby becoming the Achilles’ heel of safe motherhood advocacy efforts.

A small minority of epidemiologists and demographers in safe motherhood were heavily influenced by these critiques and therefore endorsed the view that the epistemological certainty provided by experimental evaluations should be a prerequisite for policy development as it relates to any and all types of interventions. This epistemological position was seen as essential for rectifying the subfield’s fragile position in global health. In a 2003 landmark article entitled ‘Where is the E in maternal health’ – a title that was meant to recall Rosenfield and Maine’s highly successful 1985 publication (Rosenfield and Maine, 1985) – Miller and other prominent maternal health experts argued that the field would urgently need to overturn the predominance of ‘inadequate tools to assess intervention effectiveness’, including historical analyses that, as they claimed, ‘do not meet rigorous standards of causality’ (Miller et al, 2003: 13–14). Some of our informants went so far as to argue that ‘process evaluations’, which many public health experts are calling for as a means of understanding *how* interventions work and if they can be exported to other contexts, are not necessary. “As long as a given intervention is proven to work through a trial,” said one such informant, “it can be faithfully recommended.”

Importantly, such strict epistemological conviction was infrequent. Most of our informants, population scientists and policy experts alike, did not question historical studies’ truth-value, particularly when reflecting on the broader mechanisms of long-term sustainable change that these studies highlighted. Others explicitly argued in favour of adopting a pluralistic epistemological approach using cost-effectiveness data together with insights from case histories in order to put – as a senior researcher and policy adviser described – the “pieces of the jigsaw puzzle” together and come up with a holistic and rational policy position. Although *all* our informants were involved in producing or using trial data, a handful of them even argued, quite vociferously in meetings and workshops, that functioning health systems, commitment to equity and strengthened governance are such obvious requirements for sustainable health change that they should not require more than basic observational evidence to be officially endorsed.

## Defending epistemic flexibility

One could speculate that it is because of the growth of cost-effectiveness ways of reasoning that an alternative agenda has been subtly gaining ground since the early 2000s. This agenda is genealogically linked to the early days of Loudon’s research. But it is also more than this; as we will show below, it is also the result of specific personal-empirical experiences that SMI researchers have had in the field, experiences where they are confronted with ‘fugitive’ facts and interpret these through what we argue is a para-ethnographic lens.

The most notable of these ‘para-ethnographic’ experts are Belgian: two public health medical doctors, De Brouwere and Van Lerberghe, working in a country much less permeated by the evidence-based movement and at the Prince Leopold Institute of Tropical Medicine in Antwerp, an institution known for its commitment to public health implementation over and above research. In a series of articles published strategically in Anglophone journals, these authors interpreted Loudon’s research for a policy audience and furthered his method by including a broader number of countries to explain differences in the rates of MM decline in the industrialised countries of the early 20th century.

One such publication (De Brouwere et al, 1998) features an impressive graph of MM at the beginning of the 20th century in which the United States, New Zealand and Scotland stand out as having MMRs that are three to four times the rate found in Sweden, Denmark and the Netherlands. “It was really striking to see these [contrasting] curves,” described one informant, “because the countries were [broadly] equivalent, by standard measures of socioeconomic development.”

In analysing the reasons that might account for such dissimilar MMR trajectories, these publications lent support to the factors originally identified by Loudon. Like Loudon, they suggested that MM had declined more slowly in the former countries in part because of professional conflicts between obstetricians and midwives, which contributed to the marginalisation of midwives and thus to reduced access to skilled attendance as a whole (De Brouwere et al, 1998; Van Lerberghe and De Brouwere, 2001). ‘The history of these relative successes and failures,’ Van Leberghe and De Brouwere (2001: 11) stated, ‘is to a large extent a history of different approaches to the professionalisation of delivery care, even before technology-assisted hospital delivery became the norm.’ Interested in investigating the diverse mechanisms that might account for these differences, they also found that obstetricians’ poor-quality use of medical technology was actually contributing to maternal deaths. The authors noted: ‘Those countries that managed to get doctors to co-operate with a midwifery-based policy fared relatively well. Where doctors won the battle for professional dominance – and for their share of the market – women died’ (Van Lerberghe and De Brouwere, 2001: 18).

Having a greater and more diverse empirical base with which to work than Loudon had had, these researchers outlined an ‘evidence-informed’ model of effective delivery care that postulated a series of technical *and* political ingredients – ‘and the importance of their inter-relationships’, as several informants highlighted – that are essential for any country to achieve large-scale MM reductions. Their publications were thus a direct challenge to the politically expedient EmOC policy that had been drawn from Loudon’s work throughout the 1990s, as described above. Rather than supporting the view that the history of MM had fundamentally been about treatment, this body of literature argued that the introduction of medical technologies for birth and SBA *cannot* ensure sustained MM decline *without* concomitant *equitable* socioeconomic, professional and political developments relating to health system functioning. As one such researcher explained, using epidemiological language of causality to make claims that would in fact be near-impossible to substantiate through epidemiological methods alone,

‘I think Loudon got it wrong, I think he got the “necessary” but not “sufficient” bit. I think he was right that the medical technologies were necessary – that they came into place and made a big difference – so in that sense he was right. But, what I think he didn’t look at was the health systems and political context in which that happened. Whereas I think Vincent’s [de Brouwere] work does that...’

Along similar lines, another informant pointed out that Loudon had in fact showed that MM does not respond “*spontaneously*” to socioeconomic development, but this should not be taken to mean that it does not require concerted systemic effort and investment. Prompted by this informant’s views, we returned to Loudon’s original works and found that he had been careful to state that ‘mortality was relatively insensitive to social and economic determinants *except* in so far as these determine the type and quality of birth attendants’ (Loudon, 1992b: 1560, emphasis). MM decline, he emphasised, depended on an effective system of governance and the convergence of ‘a large number of factors, therapeutic, educational, and administrative’ (1992b: 1560). Importantly, what the informant claiming that Loudon “got it wrong” may be highlighting is a subtle distinction in Loudon’s writings: while his monograph certainly emphasised the synergy of sociopolitical and therapeutic factors, his articles written for a global health audience were markedly more focused on the (more simplified) call for better access to trained personnel and medical technology (Loudon, 2000).

## The context critique

The reworking of early historical evidence in light of a renewed interest in multivariate influences, and in ‘interim (selective) and long-term (comprehensive) strategies’, effectively set the stage for the production of a series of additional case studies of natural declines in MM occurring, this time from the mid-20th century in low- to middle-income countries. These were, once again, conducted not by historians but by population scientists (epidemiologists and demographers) compelled by the insights that epistemic diversity was awarding the field. In a series of publications, population-level MM reductions in Costa Rica, Cuba, Malaysia and Sri Lanka from the 1950s onwards were studied, together with more recent if less substantial improvements in Bolivia, rural China, Egypt, Honduras, Indonesia, Jamaica and Zimbabwe (Koblinsky et al, 1999; Koblinsky and Cambell, 2003; Pathmanathan et al, 2003; Liljestrand and Pathmanathan, 2004).

By considering countries with socio-epidemiological profiles that are allegedly more similar to contemporary developing country contexts, these authors’ explicit aim was to adopt a context-specific framework and, through this, to address an emerging critique that questions the applicability of universalising ‘lessons learned’ from the industrialised West to countries across the globe. Thus, rather than discern universally applicable lessons or even intervention packages, these authors used observational case-study material to analyse MM declines in relation to the variability in service delivery ‘models’ that can be found in most developing countries today; these range from home delivery by a non-professional (such as a traditional birth attendant or a

relative) to a SBA in secondary-level facilities to near-total population coverage of deliveries in a referral facility (hospital) with comprehensive essential obstetric care by a professional SBA (such as an obstetrician or a midwife) (Koblinsky et al, 1999).

Results showed that although some countries had experienced MM reductions with the first model (home delivery by a non-professional), improvements appeared to stagnate once MMRs reached 100 or so per 100,000 live births, still well above the ratio found in most developed countries. However, in the presence of strong referral mechanisms, countries that had introduced a more skilled cadre of birth attendant in low-level facilities had witnessed significant mortality reductions. The studies also controversially showed that an all-hospital-birth model, although arguably the most technologically advanced, does not necessarily lead to significant mortality decline and may even contribute to high MM levels, especially where there is poor quality of care and high levels of mortality from unsafe abortion (Koblinsky and Cambell, 2003).

The idea that there could be different potential models for successful mortality decline was an important message that was instigated, as those involved in producing these studies told us, by 'informal' (ie, inductive/para-ethnographic) observations that they and their colleagues had begun making during their travels in developing countries. These observations showed that, since the global policy shift away from TBA training that had occurred in the late 1980s and 1990s, many countries began striving for either an all-hospital model or for a high proportion of skilled birth attendance to be met not by midwives, but by physicians. One key informant noted that although "there have been notable increases in skilled attendance around the world, every single bit of that increase is due to the use of a physician ... [and not midwives].... And talk about ignoring evidence [from case-study material], but nobody is going there in that direction". In highlighting the 'age-old' but somewhat neglected issue of professional conflicts between midwifery and obstetrics, this informant was also flagging the issue of quality of care regardless of the cadre of provider that is at play, since in many settings, hospitals and secondary-level facilities have not been adequately equipped to deal with a rapid increase in numbers of patients. These observations and ensuing case studies thus led several researchers to reject what they argued is an artificial and unproductive dichotomy between community-based and facility-based approaches (or indeed even more reductionistically between SBA and EmOC). As one of the researchers of the above-cited studies explained,

'[They] did a deep dive, an ecological study, in Malaysia and Sri Lanka, and other countries, much of the text is on the World Bank website. The point of departure was the work that had been done in Sweden, etc. It is now being criticised, because people are saying, oh, that is 19th century. So [they] set out to say, ok, let's examine a few success stories from 20th century. What did they do, did they go for, obstetric emergencies or did they go for skilled attendants? And in these countries, to them, this was a no-go. Of course they did both.'

The key to success thus appeared to be neither SBA nor EmOC, nor even the rapid adoption of facility-based births, but rather an incremental and pragmatic approach

to ensuring equitable access to good-quality, skilled attendance and coordination between different levels of care, a process that should, in turn, be underpinned by strong political support, elimination of financial barriers, and accountability of local officials and providers for their performance (Koblinsky et al, 1999). Several informants even referred to these new case studies as additional evidence of the inaccuracy of the overly-technocratic interpretation of Loudon's original work discussed above.

Despite a clear commitment to moving away from uni-causal ways of thinking and towards complex understandings of multi-causality, these publications nevertheless put forth a pragmatic focus. By defining different models for the organisation of delivery care for countries with different epidemiological profiles and health system capabilities, countries were encouraged to reject universal blueprints and identify solutions that would be better adapted to each country's health system and context (Koblinsky and Cambell, 2003). Indeed, several of our more policy-oriented informants highlighted the 'operational' value of these studies time and again. The case-study method, explained one key expert, is "strongly grounded with the stakeholder at the country level and it looks toward country-level and regional-level success as a guiding principle in its learning". Another bilateral donor agency representative explained: "These countries reduced their MMR by half every seven to 10 years, for like 50 years. And it wasn't rocket science, it wasn't magic. It was just kind of putting one foot in front of the other."

Recent attention to the success story provided by Bangladesh, which has reduced its MMR over the past 30 years *despite* low coverage of SBAs and high levels of home birthing, is perhaps the most significant recent example of the openness with which SMI experts are entering into a new and quite flexible epistemological relationship with empirical diversity. In-depth analyses of the Bangladeshi data suggest that MM decline can be attributed to a range of factors, including a fall in abortion-related deaths, better access to EmOC and community-based delivery care systems in case of emergencies, as well as key policies that expand women's access to education and more affordable health services (Chowdhury et al, 2009). Those involved in these studies do not at all believe that the Bangladesh case study invalidates the general importance now attributed to SBAs or facility-based birth. Rather, they point to the complexity of MM reduction and to the premise that no single strategy presents an elixir to the problem of MM. "MM reduction," one informant inspired by these publications explained, "is much more complex than [just training and] putting in skilled birth attendants." At the close of the first decade of the 21st century, SMI experts thus found themselves repeating a message they had been focusing on for more than two decades. It is perhaps out of a need for wider support for the idea that health change is not simply about training health workers to scale-up interventions that some SMI researchers have begun forging relationships with the burgeoning and slightly more radical 'health systems' subfield. Among these is Barbara McPake, a respected researcher who draws from complexity theory to develop a powerful critique of the overuse of 'black-box' cost-effectiveness evaluations that fail to produce knowledge about the processes through which interventions are hypothesised to work (McPake, 2006).

## Conclusion: the ethics of epistemological power

In reflecting on the issues raised in this paper, and specifically on the difficulties of keeping epistemic diversity on the agenda, some of our informants made recourse to key ethical arguments that we (to make our own position clear) empathise with and support. Key among these is the way political and advocacy uses of cost-effectiveness evidence induce the conceptual and pragmatic marginalisation of other forms of evidence, including not just case-study approaches, but also basic monitoring data, which are on the whole either of poor quality or under-utilised (Gabrysch et al, 2011). In fact, as historians such as Loudon highlighted, one key ingredient in national-level success stories like that of Sweden relates to just how crucial the careful documentation and monitoring of MM through localised statistics and case-study analyses was for generating political will and identifying mechanisms of change that could feed into localised pragmatic action over the long term (for similar debates in the field of health systems and broader development, see Mills, 2012, and White, 2009, respectively).

So powerful have experimental designs become that some of our informants from developing countries felt that investing in randomised controlled trials over other forms of evidence production would make their countries 'advanced', both because nationals are given training in the most sophisticated of methods and because it is assumed that basing policy decisions on trial data ensures that resources are not wasted (Béhague et al, 2009). Countering knowledge of this dynamic, the more critically minded of our informants pointed out that industrialised countries reduced MM well before the popularisation of experimental epistemologies, with some going further to argue that the health system fragmentation induced by the cost-effectiveness ethos has been positively detrimental. "I am convinced that our over-emphasis on evidence and numbers has basically slowed safe motherhood down," a demographer active in both academia and an NGO stated, and "if we only focus on small things that go on within facilities or small things that affect women's behaviour without looking at the political and social environment in which policy decisions are made, we can't really hope to get very far." For researchers such as these, the combined weight of observational studies, historical research and clinical common sense should be considered sufficiently conclusive to make further experimental study into the effectiveness of targeted interventions unjustifiable and indeed unethical.

A less explicit but no less important ethical dilemma raised by our informants relates to the extent to which the tenets of objective and neutral science have dampened open and transparent debate about the politics of health, particularly when evidence is used in the service of a neoliberal cost-savings agenda. In fundamental ways, historical case-study analyses have pointed to the importance of the political processes that account for improved health including, most significantly, the role of governments in ensuring health equity. Yet this is a topic that few of our informants were willing to discuss directly, for complex reasons that we hope to explore in subsequent publications.

This paper gives us occasion to end by putting forward a note of caution with regards to the development and use of critical insight. Anthropologists and those supporting a 'critical global health' epistemology have often voiced frustration at the way new global health constellations tend to create universalising meta-narratives that

oversimplify local realities. Although highlighting simplification is certainly empirically accurate, we must also tend to the para-ethnographic forces within global health that, no matter how minimal, push towards diversification and complexification, not only in the periphery, but also within the powerful centres of evidence production and policy development. As Loudon (2000: 241s) himself cautioned in the introduction to one of his more applied public health articles:

Whether it is the history of medicine, politics, war, or anything else, it is dangerous to assume that the determinants of events in the past will operate in the same way in the present. If the butterfly of chaos theory flaps its wings in different places at different times, the results are never the same twice’.

Yet Loudon, as we have noted, also used his complex historical work to produce clear ‘take-home’ messages for the contemporary world of global policy making. Like Loudon, many of our informants appeared to simultaneously (or perhaps iteratively) create oversimplified accounts of health realities at the same time that they resisted and modified such globalising narratives, in part by advancing methodologically diverse agendas. In fact, it may be that processes of simplification serve as a heuristic tool against which ‘para-ethnographic’ complexity can be epistemologically supported, not for its own sake but as a method of carefully dissecting and identifying effective programmatic action.

### Note

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