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REVIEW

Impact of user fees on maternal health service utilization and related health outcomes: a systematic review

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Objective To assess the evidence of the impact of user fees on maternal health service utilization and related health outcomes in low- and middle-income countries, as well as their impact on inequalities in these outcomes.

Methods Studies were identified by modifying a search strategy from a related systematic review. Primary studies of any design were included if they reported the effect of fee changes on maternal health service utilization, related health outcomes and inequalities in these outcomes. For each study, data were systematically extracted and a quality assessment conducted. Due to the heterogeneity of study methods, results were examined narratively.

Findings Twenty studies were included. Designs and analytic approaches comprised: two interrupted time series, eight repeated cross-sectional, nine before-and-after without comparison groups and one before-and-after in three groups. Overall, the quality of studies was poor. Few studies addressed potential sources of bias, such as secular trends over time, and even basic tests of statistical significance were often not reported. Consistency in the direction of effects provided some evidence of an increase in facility delivery in particular after fees were removed, as well as possible increases in the number of managed delivery complications. There was little evidence of the effect on health outcomes or inequality in accessing care and, where available, the direction of effect varied.

Conclusion Despite the global momentum to abolish user fees for maternal and child health services, robust evidence quantifying impact remains scant. Improved methods for evaluating and reporting on these interventions are recommended, including better descriptions of the interventions and context, looking at a range of outcome measures, and adopting robust analytical methods that allow for adjustment of underlying and seasonal trends, reporting immediate as well as longer-term (e.g. at 6 months and 1 year) effects and using comparison groups where possible.

Keyword Maternal health, user fees, evaluation methods, literature review

KEY MESSAGES

- Most studies to evaluate the impact of user fees on utilization of maternal health services employ poor methods and therefore cannot produce reliable estimates of effect.
- Nevertheless, consistency in the direction of effects provided some evidence that removal of fees increases facility delivery in particular, and may also increase the number of managed delivery complications. Few studies look at impact of user fees on related health outcomes or on equity in access to care.
- User fee changes are often large-scale programmes which are best evaluated using interrupted time series or controlled before-and-after study designs both of which facilitate estimating effect sizes net of temporal trends.
- Impacts on facility delivery must be evaluated jointly with indicators of quality of care, access to emergency obstetric care and equity for a comprehensive understanding of user fee effects.

Introduction

Millennium Development Goals 4 and 5—to reduce child mortality and improve maternal health—remain important global health challenges (United Nations 2011), and ensuring skilled birth attendance is a crucial intervention for achieving these goals (Martines *et al.* 2005; Campbell *et al.* 2006). Numerous factors influence skilled birth attendance including the cost of normal and emergency care (Ensor and Ronoh 2005; Borghi *et al.* 2006; Gabrysch and Campbell 2009). The cost of delivery care is particularly problematic for three reasons. First, delivery care costs can represent a substantial proportion of a household's income and can lead to households being driven into poverty. Second, as birth outcomes are uncertain, the cost of delivery is uncertain, making it difficult to plan ahead and save for these costs. And third, delivery services are often essential as the consequences of lack of treatment can be fatal for the mother and/or her baby. A study of health service utilization in ~50 developing countries found that the rich–poor gap for skilled care at delivery was larger than for other health interventions, such as immunization or treatment of fever (Gwatkin *et al.* 2004). Significant rich–poor gaps also exist for antenatal care (ANC) (Gwatkin *et al.* 2004) which is a concern as ANC is part of the continuum of maternity care and a key part of the strategy to reduce neonatal and maternal deaths.

User fees for health services were introduced or substantially increased in many low-income countries in the 1980s and early 1990s as part of structural adjustment policies promoted by the World Bank (Akin *et al.* 1987) and in African countries in particular following the 1987 joint World Health Organization/United Nations Children's Fund Bamako Initiative (Hardon 1990). The Bamako Initiative aimed to address severe problems in the financing of primary health care including maternity care. User charges for essential drugs were introduced to generate funds to improve the quality of health services and equity in access to these services. However, after charges were introduced, there was extensive debate about their utility and effects. Some argued that they provided needed additional revenue for the health sector, improved quality of care and promoted efficient utilization of services (Litvack and Bodart 1993; Collins *et al.* 1996; Setel *et al.* 2007; Por and Bigdeli 2009). Others argued that fees should be abolished as they raised relatively little revenue, discouraged needed healthcare utilization and posed a regressive and unaffordable cost burden on poor households (Yoder 1989; Russell and Gilson 1997; Save the Childen UK 2008; Yates 2009). In 1993, the World Bank was citing strong efficiency arguments for a publicly financed essential clinical package which included antenatal and delivery care (World Bank 1993). And recently, numerous countries in sub-Saharan Africa have officially abolished or reduced user fees for childbirth services with the aim of increasing skilled birth attendance and reducing the catastrophic costs of emergency delivery care (Figure 1).

Despite the problems user fees can pose for maternal health and the momentum to abolish them, reviews of user fee studies have not examined the full body of published evidence of their impact on outcomes relevant to maternal and neonatal health (MNH) (Nanda 2002; Lagarde and Palmer 2011; Ridde and Morestin 2011). The objective of our review is to examine this evidence in low- and middle-income countries with respect to maternal health service utilization, health outcomes and inequalities in these measures, and to highlight appropriate methodologies and outstanding questions that can contribute to future research.

Methods

Inclusion criteria

Peer-reviewed primary studies of any design were eligible for inclusion. No language restrictions were applied. We also did not place any restrictions on the level of the intervention, i.e. changes could have been at the facility, regional or national level. Interventions could have introduced, increased, decreased or abolished user fees for any maternal health service. User fees were defined as formal out-of-pocket payments at the point-of-service. Studies of other maternal health financing interventions, such as voucher schemes, subsidized insurance or financial incentives, were excluded. Outcomes were any quantitative measure of the effect on utilization of ANC, facility delivery, maternal or perinatal complications or deaths, and inequalities in any of these outcomes. In this review, inequality refers to the absolute difference in the proportion of health service utilization or health outcomes between different socio-demographic groups. No study examined quality of care impacts quantitatively, and so we excluded this outcome. Studies of trends in health services that were not subject to fee changes were excluded. Studies simulating impact using models were also excluded.

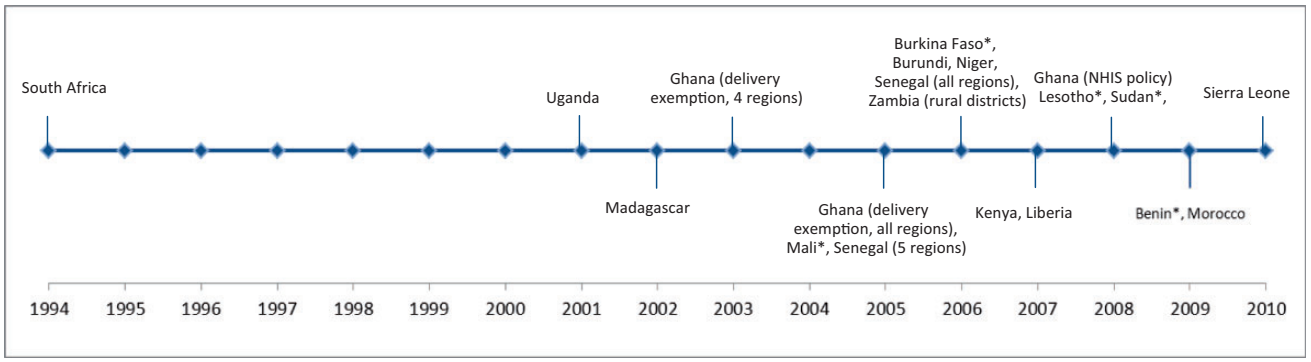

Figure 1 National policies abolishing or reducing user fees for maternity care services. *Countries with fee exemptions that do not cover all fees or all delivery services (e.g. exemptions only cover caesarean sections). *Sources:* Burundi (Ajia 2006), Ghana (Penfold *et al.* 2007; Daily Graphic 2008), Kenya (Chuma *et al.* 2009), Madagascar (Madagascar Ministry of Economy Finance and Budget 2002), Senegal (Witter *et al.* 2008), Uganda (Nabyonga *et al.* 2005), Zambia (Masiye *et al.* 2008), South Africa (Wilkinson *et al.* 2001), Burkina Faso (Look 2010), Niger (Yates 2009), Mali (FEMhealth), Morocco (FEMhealth), Benin (FEMhealth), Liberia (IRIN Humanitarian News and Analysis 2007) and Sierra Leone (IRIN Humanitarian News and Analysis 2010).

Search strategy

Our search strategy modified one that was used in a Cochrane systematic review by Lagarde and Palmer (2011) on the impact of user fees on health service utilization in low- and middle-income countries. We used their search terms but loosened the study design requirements (Lagarde and Palmer only included quasi-experimental and experimental studies) and added a restriction of pregnancy- and childbirth-relevant papers using the search terms used by the Cochrane Pregnancy and Childbirth Group to identify studies of pregnancy (The Editorial Team 2011). We conducted our search on December 12, 2011. We searched databases Medline, Embase and EconLit. We examined all 34 studies identified in Lagarde *et al.*'s review (17 they included and 17 they excluded) and selected those with MNH-relevant outcomes. Then, we reviewed all the references of (1) two reviews of maternal health financing (Ensor and Ronoh 2005; Borghi *et al.* 2006), (2) a scoping review of the abolition of user fees in healthcare services in Africa (Ridde and Morestin 2011) and (3) all included studies. We also used either the forward citation feature in Web of Science or the related citation feature in PubMed to identify and review studies which cited any of the included studies. Finally, we communicated with experts in the field. If two papers reported relevant identical information from the same study, the most recent publication was included. Two reviewers independently assessed retrieved articles, and reached agreement on which papers to include and on which data to extract.

Data extraction and analysis

Data were extracted to describe the nature of fee policy interventions, study methods and study results. User fee amounts were contextualized by reporting the per capita gross domestic product (GDP) in the year of the fee change. Both values were standardized to year 2000 US dollars (US$). We assessed the overall strength of evidence with respect to effect estimates using GRADE criteria, particularly, the study design, study quality and consistency in results (GRADE Working Group 2004).

With regard to results, we extracted information on the impact of fee changes on ANC visits, facility deliveries, maternal or perinatal complications or deaths, and inequalities in any of these outcomes. We analysed study results narratively according to the nature of the intervention, methodological strength of the study and reported outcome. Heterogeneity of interventions, methods and outcomes precluded a meta-analysis.

Results

Description of studies

Twenty studies met our inclusion criteria. Four (Mbugua *et al.* 1995; Benjamin *et al.* 2001; Wilkinson *et al.* 2001; Bratt *et al.* 2002) were from Lagarde and Palmer's (2011) included studies and two (Akashi *et al.* 2004; Jacobs and Price 2004) were from their excluded studies. We identified no studies from the updates of the database searches, nine (De Bethune *et al.* 1989; Osuga and Nordberg 1993; Owa *et al.* 1995; Schneider and Gilson 1999; Daponte *et al.* 2000; Deininger and Mpuga 2005; Asante *et al.* 2007; Bosu *et al.* 2007; Penfold *et al.* 2007) from reviewing references of relevant articles, one (Witter *et al.* 2010) from forward and related citation searches and four (Ekwempu *et al.* 1990; Ridde *et al.* 2011; Steinhardt *et al.* 2011; Witter *et al.* 2011) from communications with experts in the field. Figure 2 outlines the study selection process.

Tables 1 and 2 describe the nature of interventions from included studies. Studies about fee introduction or increase were published between 1989 and 2004, assessing policies implemented between 1984 and 2002. Studies about fee removal or reduction were published more recently (between 1999 and 2011), reflecting the momentum towards abolishing fees. These studies evaluated policies implemented between 1994 and 2009. Two studies were conducted in Cambodia (Akashi *et al.* 2004; Jacobs and Price 2004), one each in Papua New Guinea (Benjamin *et al.* 2001), Ecuador (Bratt *et al.* 2002), Afghanistan (Steinhardt *et al.* 2011) and Nepal (Witter *et al.* 2011) and all others (14 studies) in sub-Saharan African countries.

The amount of the fee change was reported in 12 of the 20 studies and was most often omitted in studies evaluating fee removal (Table 2). Official fee changes ranged from small amounts (<0.1% of GDP per ANC visit in South Africa and Ecuador) (Wilkinson *et al.* 2001; Bratt *et al.* 2002) to substantial amounts (8 and 10% of GDP for childbirth services in Nepal

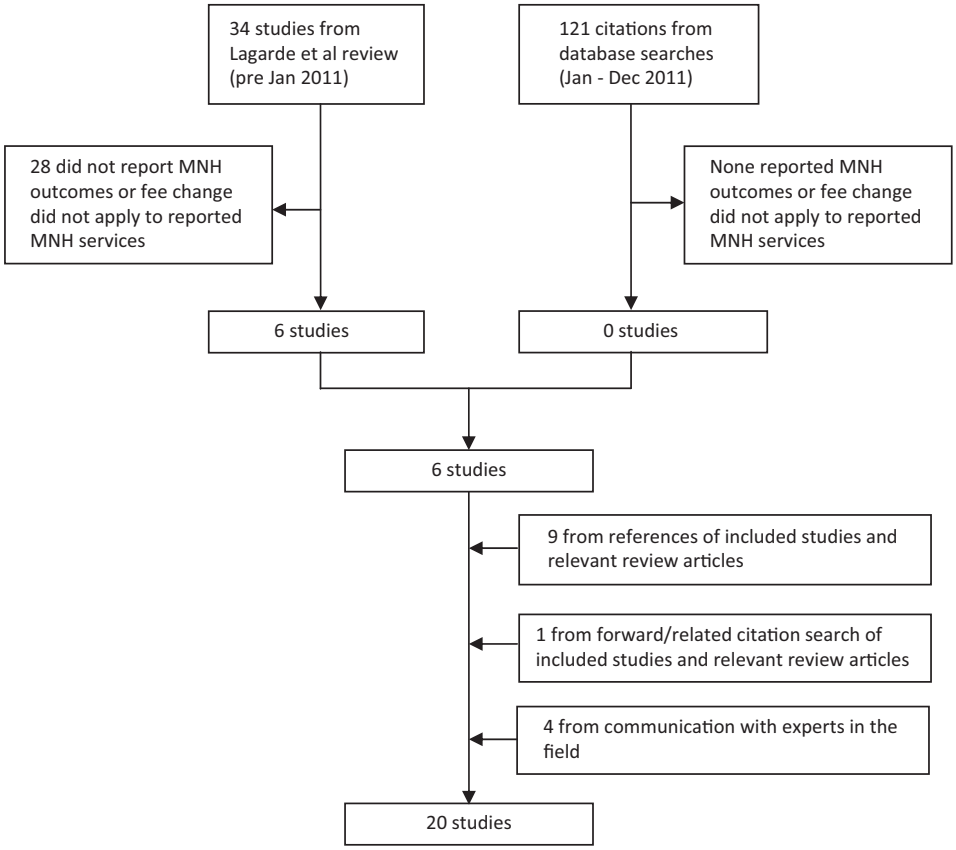

Figure 2 Study selection process.

Table 1 Nature of fee policy interventions, studies of fee introductions or increases

Study (country)	Date of change	Policy scale	Direction of change (affected maternity service)	Fee amount (constant 2000 US$)	Per cent change in fees	GDP per capita[a] (constant 2000 US$)
Akashi *et al.* 2004[b] (Cambodia)	April 1997	Facility	Introduced (childbirth)	$23.69	↑∞	$239
Benjamin *et al.* 2001 (Papua New Guinea)	July 1995	District	Increased (childbirth)	$3.06 → 12.26	↑301%	$756
	February 1996		Introduced (ANC)	$0.55	↑∞	$793
Bratt *et al.* 2002 (Ecuador)	November 1996	Facility	Increased (ANC)	$13.75 → 16.08[c]	↑17%	$1342
				$11.52 → 15.77	↑37%	
				$12.16 → 18.79	↑55%	
De Bethune *et al.* 1989 (Zaire)	1986	Rural health zone	Increased (any care episode)	*30Z* → *50Z*[d]	↑67%	$252
Ekwempu *et al.* 1990 (Nigeria)	1985	Facility	Introduced (unspecified)	Post-policy fees unspecified	↑∞	$314
	1988		Increased (unspecified)		unknown	$325
Jacobs and Price 2004 (Cambodia)	November 2001	Facility	Introduced (childbirth)	$2.58–5.16	↑∞	$304
	April 2002		Increased (childbirth)	$2.50 → 5.00, $5.00 → 7.49	↑50–100%	$319
Mbugua *et al.* 1995 (Kenya)	December 1989	National	Introduced (inpatient fee)	$4.76–11.90 per day	↑∞	$446
Osuga and Nordberg 1993 (Kenya)	December 1989	National	Increased (childbirth)	$5.02 → 10.04	↑103%	$446
Owa *et al.* 1995 (Nigeria)	1984	Facility	Introduced (ANC, childbirth)	Post-policy fees unspecified	↑∞	$294

∞, change from no formal fees to formal fees.

[a]GDP in the year of the policy change.

[b]Intervention included improvements in quality of care.

[c]Bratt *et al.* compared effects in three groups, each with a different fee increase.

[d]Period of hyperinflation, no reliable exchange rate to US$.

Table 2 Nature of fee policy interventions, studies of fee abolition or reductions

Study (country)	Date of change	Policy scale	Direction of change (affected maternity service)	Fee amount (constant 2000 US$)	Per cent change in fees	GDP per capita[a] (constant 2000 US$)
Asante *et al.* 2007 (Ghana)	April 2005	National	Reduced (childbirth)	$7.61 → 3.41 (caesarean)	↓55%	$290
				$0.76 → 0.49 (normal)	↓36%	
Bosu *et al.* 2007 (Ghana)	September 2003 (Central Region)	National	Removed (childbirth)	Pre-policy fees unspecified	↓100%	$272
	April 2005 (Volta Region)					$290
Daponte *et al.* 2000 (South Africa)	June 1994[b]	Facility	Removed (ANC, other unspecified maternal care)	Pre-policy fees unspecified	↓100%	$2934
Deininger and Mpuga 2005 (Uganda)	March 2001	National	Removed (all services)	Pre-policy fees unspecified	↓100%	$258
Penfold *et al.* 2007 (Ghana)	September 2003 (Central Region)	National	Removed (childbirth)	Pre-policy fees unspecified	↓100%	$272
	April 2005 (Volta Region)					$290
Ridde *et al.* 2011 (Burkina Faso)	January–April 2007[c]	National	Reduced (childbirth)	$1.59 → 0.32 (caesarean)	↓80%	$260
Schneider and Gilson 1999 (South Africa)	June 1994[b]	National	Removed (ANC, other unspecified maternal care)	Pre-policy fees unspecified	↓100%	$2934
Steinhardt *et al.* 2011 (Afghanistan)	April 2008	National	Removed (all PHC)	Pre-policy fees unspecified	↓100%	$290[d]
Wilkinson *et al.* 2001 (South Africa)	June 1994	National	Removed (ANC)	$0.95 → 0 (per visit)	↓100%	$2934
Witter *et al.* 2010 (Senegal)	January 2005	National	Removed (childbirth)	$1.60 → 0 (normal)	↓100%	$522
				$17.16–23.94 → 0 (caesarean)		
Witter *et al.* 2011 (Nepal)	January 2009	National	Removed (childbirth)	$1.89–5.78 → 0 (normal)	↓100%	$261
				$8.24–20.02 → 0 (complication/caesarean)		

PHC, primary health care.

[a]GDP in the year of the policy change.

[b]Date of change was taken from Wilkinson *et al.* (2001).

[c]Policy launch varied from January to April across the eight districts where facility delivery was studied.

[d]In 2011 US$, GDP per capita for 2008 US$ not available.

and Cambodia, respectively) (Akashi *et al.* 2004; Witter *et al.* 2011). Studies on fee introduction or increase tended to evaluate facility- or district-level policies, whereas studies on fee removal or reduction tended to evaluate national-level policies.

Methodological issues

All studies used non-randomized designs (Table 3): two interrupted time series (longitudinal data analysed to estimate intervention effect net of any underlying temporal trend), eight repeated cross-sectional (longitudinal data without analysis of any underlying temporal trend), nine before-and-after without comparison groups and one before-and-after in three groups with different user fee increases. This latter study was envisaged as a cluster randomized trial, but was analysed as a before-and-after study in three groups due to concerns over the integrity of the experimental design (Bratt *et al.* 2002).

Given the study designs, there is considerable scope for bias in all the studies included in this review. For example, in the nine before-and-after studies, without knowing the underlying time trend, it is impossible to attribute any change over time to the user fee policy change. Only three studies make a serious attempt to produce causal estimates—the before-and-after study in three groups and the two interrupted time series studies. Bratt *et al.* (2002) increased the credibility of their findings by examining a dose-response effect with increasing fees in three groups. Wilkinson *et al.* (2001) and Steinhardt *et al.* (2011) used interrupted time series methods to estimate effects net of underlying temporal trends. Notably, many other studies had multiple months of pre- and post-intervention data and sufficient events a month to carry out a trend analysis but chose to collapse these data into simple before-and-after comparisons (Table 3, columns E–H).

As indicated in Table 3, almost all studies were hospital- or facility-based. Yet, how representative facility clients were of the target group of pregnant and delivering women was not presented in any study. Although Penfold *et al.* (2007) and Asante *et al.* (2007) analysed household survey data rather than facility data, the representativeness of their samples is also uncertain as the percentage of selected individuals who agreed to participate in these surveys (response rate) was not presented.

Poor approaches to analysis generated further concerns. Few studies controlled for or discussed potential sources of bias, such as underlying temporal trends. More than half (12) of the studies did not report the statistical significance of effect estimates, and three of these showed changes in outcomes graphically without any quantification. By reporting on only one outcome (e.g. facility delivery) rather than a range of outcomes (e.g. facility delivery, deliveries with complications and caesarean section), most studies further reduced the scope for understanding the impact of the policy change. And, with the exception of Wilkinson *et al.* (2001) and Steinhardt *et al.* (2011) there was no discussion of the quality of the facility data used to assess impact, although it is well known that the quality of routine data in low-income settings is often suspect (Abouzahr and Boerma 2005).

Finally, studies did not adequately discuss contextual or implementation factors that could influence effects (e.g. informal fees or quality of care). Three exceptions to this were Asante *et al.* (2007), Akashi *et al.* (2004) and Steinhardt *et al.* (2011). Although in principle Ghana's 2003 and 2005 delivery fee exemption policy abolished all childbirth-related user fees, Asante *et al.* (2007) reported that in practice fees were only reduced (by 55 and 36% for caesarean and normal deliveries, respectively). Akashi *et al.* (2004) indicated that the formal fees introduced at a hospital in Cambodia were similar to previous unofficial fees, and therefore fee introduction in this setting was perceived positively as it made costs predictable and therefore did not deter utilization of services. Authors also specified that fee changes were accompanied by quality of care improvements, which also positively influenced utilization. Steinhardt *et al.* (2011) speculated that lack of increases in preventive and promotive care, such as ANC, were due to these services being largely free (92.6%) even before the fee ban they studied.

Overall, using GRADE criteria, the above factors contributed to a low or very low strength of evidence for estimating the impact of fee interventions on all outcomes (Table 4).

Impact on outcomes (Table 5)

Effects on antenatal care

Nine studies reported on the impact of user fees on ANC visits. As expected, visits tended to decrease with the introduction of fees and increase following the removal of fees. Exceptions to this pattern were increased attendance at a hospital in Cambodia following fee introduction (Akashi *et al.* 2004), and following fee abolitions, no impact on the use of a mobile clinic serving 14 rural communities in South Africa (Wilkinson *et al.* 2001) and an unsustained increase in health facilities in Afghanistan (Steinhardt *et al.* 2011). Authors of the Cambodian study ascribed the increase to improved quality and the predictability of formal fees replacing informal fees of similar magnitude. The South African and Afghan studies estimated effects net of underlying trends. Authors of the South African study speculated that the low fee per visit (0.03% of GDP; Table 2) did not represent significant enough savings to counteract an ongoing trend of declining ANC use, and as stated previously, in Afghanistan, ANC services had been largely free even before the fee ban.

Effects on facility deliveries

Seventeen studies examined the impact of user fees on facility delivery. As with ANC visits, facility delivery tended to increase with the introduction of fees and decrease following the removal of fees. Exceptions were the Cambodian hospital study where increasing facility delivery was observed following fee introduction and quality of care improvements (Akashi *et al.* 2004), and three other studies in Cambodia (Jacobs and Price 2004), South Africa (Schneider and Gilson 1999) and Burkina Faso (Ridde *et al.* 2011) which showed temporal and geographic fluctuations in facility delivery but no overall impact following fee changes. The only study that estimated effects net of temporal trends reported an increase of 7.3 (not statistically significant) monthly facility deliveries in district hospitals in Afghanistan immediately following fee removal, although authors acknowledged that user fees had previously only been applied at a minority of facilities for drug costs for deliveries

Table 3 Study methods

A Study	B Design	C Setting	D Source of outcome data	E Mean events per month[a]	F Study period months (data points)	G	H
					Before	After	Total[b]
Introduced or increased fees							
Akashi *et al.* 2004	Repeated cross-sectional	1 hospital	Facility data	900+ (ANC), 300+ (deliveries)	20 (1)	12, 12 (2)	56 (3)
Benjamin *et al.* 2001	Repeated cross-sectional	1 hospital, 4 clinics	Facility data	200+ (ANC)	25 (25)	23 (23)	48 (48)
Bratt *et al.* 2002	Before and after in three groups	15 clinics	Facility data	58+ (ANC)	3 (1)	3 (1)	6 (2)
De Bethune *et al.* 1989	Before and after	1 rural health zone	Facility data	243+ (ANC)	12 (1)	12 (1)	24 (2)
Ekwempu *et al.* 1990	Repeated cross-sectional	1 hospital	Facility data	249+ (deliveries)	12 (1)	12, 12 (2)	72 (3)
Jacobs and Price 2004	Repeated cross-sectional	1 hospital	Facility data	28+ (deliveries)	3 (1)	3 (2)	3 (3)
Mbugua *et al.* 1995	Before and after	1 hospital, 1 health centre	Facility data	92+ (deliveries)	9 (1)	9 (1)	21 (2)
Osuga and Nordberg 1993	Before and after	1 hospital, 1 health centre	Facility data	252+ (deliveries)	6 (1)	6 (1)	18 (2)
Owa *et al.* 1995	Repeated cross-sectional	1 hospital	Facility data	80+ (ANC), 44+(deliveries)	36 (3)	84 (7)	120 (10)
Removed or decreased fees							
Asante *et al.* 2007	Before and after	6 Central, 6 Volta districts	Household survey	n.s	n.s. (1)	n.s. (1)	n.s. (2)
Bosu *et al.* 2007	Before and after	9 Central, 12 Volta hospitals	Facility data, clinical notes	786+ (Central, deliveries)	Central: 12 (1)	12 (1)	24 (2)
				505+ (Volta, deliveries)	Volta: 11 (1)	11 (1)	23 (2)
Daponte *et al.* 2000	Repeated cross-sectional	1 hospital	Facility data, clinical notes	373+ (deliveries)	12 (1)	36 (3)	55 (4)
Deininger and Mpuga 2005	Repeated cross-sectional	National	Facility data	n.s.	n.s.	n.s.	n.s.
Penfold *et al.* 2007	Before and after	6 Central, 6 Volta districts	Household survey	35+ (Central, deliveries)	Central: 18 (1)	18 (1)	36 (2)
				110+ (Volta, deliveries)	Volta: 6 (1)	6 (1)	12 (2)
Ridde *et al.* 2011	Repeated cross-sectional	211 health facilities	Facility data	≈20–30 (deliveries)	36 (36)	29 (29)	69 (69)
Schneider and Gilson 1999	Before and after	13 health facilities	Facility data	n.s.	12 (1)	12 (1)	24 (2)
Steinhardt *et al.* 2011	Interrupted time series	701 facilities	Facility data	150+ (ANC), 40+ (deliveries)	36 (36)	14 (14)	51 (51)
Wilkinson *et al.* 2001	Interrupted time series	1 mobile clinic	Facility data	50+ (ANC)	29 (9)	46 (15)	75 (24)
Witter *et al.* 2010	Before and after	6 districts	Facility data	n.s.	12 (1)	12 (1)	24 (2)
Witter *et al.* 2011	Before and after	11 hospitals, 6 health centres, 5 health posts	Facility data	n.s.	10 (1)	10 (1)	20 (2)

n.s., not stated.

[a]If a study reports both ANC first visits and total ANC, the lower value of ANC first visits is used.

[b]Total is not always sum of before and after due to excluded periods, e.g. Akashi *et al.* excluded 3 months before and 9 months after fee change from analysis.

Table 4 Quality of evidence

Outcome	No. of studies	Quality of studies[a]	Strength of evidence
ANC, first visits	4	- 1 moderate, 3 weak - 1 study adjusted for temporal trend - 2 reported statistical significance - 2 population-based, 2 facility-based	Very low
ANC, total visits	7	- 3 moderate, 4 weak - 2 studies adjusted for temporal trend - 1 showed a dose-response gradient - 6 reported statistical significance - 1 population-based, 6 facility-based	Low
Facility delivery	17	- 1 moderate, 16 weak - 1 study adjusted for temporal trend - 6 reported statistical significance - 4 population-based, 13 facility-based	Low
Complications	6	- 6 weak - No study adjusted for temporal trend - 1 reported statistical significance - 2 population-based, 4 facility-based	Very low
Maternal deaths	3	- 3 weak - No study adjusted for temporal trend - 1 reported statistical significance - 3 facility-based	Very low
Perinatal deaths	2	- 2 weak - No adjustment for temporal trend - No reporting of statistical significance - 2 facility-based	Very low
Inequality	1	- 1 weak - No adjustment for temporal trend - No reporting of statistical significance - Population-based	Very low

[a]Factors affecting confidence in the estimate of the impact (direction and magnitude) of fee change.

(Steinhardt *et al.* 2011). This initial increase was followed by a slightly declining trend.

Following fee introductions, two studies in urban hospitals in Nigeria reported increases in the proportion of delivery admissions with complications (Ekwempu *et al.* 1990; Owa *et al.* 1995). Ekwempu *et al.* (1990) highlighted that this pattern was observed despite no change in the number of obstetricians and an increase in midwives. Authors of both studies suggested their findings reflected reduced and delayed health-seeking behaviour resulting from households being unable to cope with the financial burden imposed by fees. In Ghana (Asante *et al.* 2007; Bosu *et al.* 2007; Witter *et al.* 2010), Senegal (Witter *et al.* 2010) and Nepal (Witter *et al.* 2011) increases in pregnant women with complications being attended (with hypertensive disease, haemorrhage or undergoing caesarean delivery) were observed following fee removal or reductions. In these studies, the percentage increase in attended deliveries with complications was always higher than the percentage increase in facility deliveries overall (Asante *et al.* 2007; Bosu *et al.* 2007; Witter *et al.* 2010). For example, in Senegal, facility deliveries increased by 10%, whereas caesarean sections increased by 33% (Witter *et al.* 2010). None of these studies used methods that estimated the magnitude of the effect that was attributable to fee changes.

Effects on maternal and perinatal mortality

The two Nigerian hospital studies also reported increases in maternal and perinatal deaths following fee introduction (Ekwempu *et al.* 1990; Owa *et al.* 1995). Conversely, following delivery fee exemptions, Bosu *et al.* (2007) reported reductions in institutional maternal mortality ratios (MMRs) and delivery-related mortality ratios in Ghana's Central and Volta regions.[1] However, Daponte *et al.* (2000) found that the institutional MMR at a tertiary hospital in South Africa increased following fee removal. Authors speculated that quality of care deteriorated as an increased patient load was not accompanied by corresponding increases in staff and other facility resources. As was the case with studies on facility delivery, the magnitude of the effect attributable to fee changes was not estimated in any of these studies, e.g. Bosu *et al.* (2007)

Table 5 Direction of reported effects

Design	Study	ANC, first visit	ANC, total visits	Facility delivery	Deliveries with complications[a]	Maternal deaths	Perinatal deaths	Inequality
Introduced or increased fees								
1 Quasi-experimental	Bratt *et al.* 2002		−					
8 Non-experimental	Akashi *et al.* 2004[b]		+	+				
	Benjamin *et al.* 2001	?		?				
	De Bethune *et al.* 1989	−						
	Ekwempu *et al.* 1990			−	+	+		
	Jacobs and Price 2004			±				
	Mbugua *et al.* 1995			−				
	Osuga and Nordberg 1993			−				
	Owa *et al.* 1995		−	−	+		+	
Removed or decreased fees								
2 Quasi-experimental	Steinhardt *et al.* 2011		?	?				
	Wilkinson *et al.* 2001	±	±					
9 Non-experimental	Asante *et al.* 2007			+	+			
	Bosu *et al.* 2007			+	+	−		
	Daponte *et al.* 2000			+		+		
	Deininger and Mpuga 2005	+	+	+				
	Penfold *et al.* 2007			+				?
	Ridde *et al.* 2011			±				
	Schneider and Gilson 1999		+	±				
	Witter *et al.* 2010			+	+			
	Witter *et al.* 2011			+	+			

−, decreased; ±, no effect; +, increased; ? unclear trend.

[a]Studies introducing fees reported increased complications on admission; studies removing fees reported increased number of managed complications, e.g. increases in caesarean sections.

[b]Intervention included improvements in quality of care.

acknowledged that there had already been a downward trend in MMR before delivery fees were abolished.

Effects on inequality

Only one study sought to assess the impact of fee changes among different socioeconomic groups. Following free delivery care in Ghana, the proportion of facility deliveries increased in all wealth quintiles (Penfold *et al.* 2007). However, this did not consistently result in reductions in inequality. The difference in the proportion of facility deliveries between women in the richest and poorest wealth quintiles decreased by 11% in the Volta Region, while remaining unchanged in the Central Region. And, the difference in the proportion of facility deliveries between the most and least educated women increased by 19% in Volta Region, while it decreased by 21% in Central Region. It was unclear whether these inequality trends resulted from free delivery care.

Discussion

To our knowledge, this is the first systematic review of peer-reviewed studies assessing the impact of user fee changes on maternal health services and related outcomes. Our results indicate that despite the momentum in policies removing user fees, the magnitude of their effect on utilization of MNH services remains uncertain. In addition, evidence of effects on related health outcomes and inequalities was infrequent and variable. The low strength of existing evidence results from weak study designs combined with poor analysis. Few studies addressed potential sources of bias, such as secular trends over time, and even basic tests of statistical significance were often not reported. There was also a limited choice of outcome indicators used and a lack of discussion of contextual factors to help interpret policy effects.

Our findings are similar to those from two recent systematic reviews on the impact of introducing or abolishing user fees in general in middle- and low-income countries (Lagarde and Palmer 2011; Ridde and Morestin 2011). Both reviews concluded that removing user fees generally increased service utilization, and highlighted the low quality of the evidence, particularly with regard to the magnitude and sustainability of effects over the long term, and the uncertain impact on health outcomes and socioeconomic inequalities. They also observed that fee removal had the potential to negatively impact service quality, and that quality improvements may increase utilization even where fees are introduced. These observations were made by Daponte *et al.* (2000) and Akashi *et al.* (2004), respectively,

although neither study explicitly examined quality of care. Ridde and Morestin (2011) also concluded that there is considerable lack of knowledge of contexts and implementation procedures, exemplified in our review by many studies not even reporting user fee amounts.

Our findings build on these two reviews by researching the evidence specific to maternity care services, as this is a major focus of the global health movement to abolish user fees (Figure 1). Twelve of the 20 studies we reviewed were not included by Lagarde and Palmer (2011) or Ridde and Morestin (2011). The history of the user fee debate suggests that the proliferation of weak studies promotes selective and opportunistic use of evidence on this issue. To advance the debate on user fees in a constructive manner, more rigorous studies with both internal and external validity are needed.

Improving methods

Design higher quality non-experimental studies

As indicated by the designs of the included studies (Table 3), non-experimental methods are typical in this area of research. Randomized controlled trials do not lend themselves to the evaluation of large-scale interventions, such as user fee policy changes, although they may be possible in some instances (Ansah *et al.* 2009). They are often not politically desirable and are not feasible when interventions are evaluated retrospectively, as was the case in all the studies identified in this review. As indicated in Table 2, fee removal policies in particular are often national, making it difficult to identify an appropriate comparison group. Even when implementation dates are staggered across regions, the reasons for the different dates may be associated with the outcome(s) of interest making an unadjusted comparison between implementing and non-implementing regions invalid (e.g. in Ghana, poorer regions implemented the free delivery care policy before richer regions and wealth is known to be associated with facility delivery).

Unfortunately, non-experimental designs that simply compare cross-sectional measures in the same population before and after an intervention (the approach adopted by 17 of 20 studies in this review) offer little potential to produce unbiased estimates of effect. This is because they cannot take into account the many factors other than the intervention which could account for a change in the outcome. As illustrated in Lagarde (2012), a difference between two points in time could simply be due to non-stationarity (e.g. an underlying increasing trend) or expected periodic fluctuations (e.g. seasonal variation). To assess whether changes can be attributed to a particular intervention, more robust study designs are needed, and numerous resources elaborate these designs (Rossi and Freeman 1993; Angrist and Pischke 2008). We lack the space to discuss in detail each and every approach. Instead, we wish to focus on two of the methods used in some of the studies above—the interrupted time series design used by Steinhardt *et al.* (2011) and the controlled before-and-after design employed by Bratt *et al.* (2002).

Interrupted time series analysis looks for a change in the level, or slope of the temporal trend in the outcome at the time of the intervention's introduction. This approach is particularly applicable when evaluating full coverage programmes in which no comparison group is likely to exist (Rossi and Freeman 1993). The design reduces the risk of bias that plagues simple cross-sectional comparisons by adjusting regression models for time-varying factors such as an underlying trend. Interrupted time series analysis can also adjust for any changes in the population pre- and post-intervention that may confound effects and can be used to examine immediate and longer-term effects. If the timing or intensity of the intervention varies across settings, a controlled before-and-after design can also improve impact estimates. The controlled before-and-after approach computes a double difference, one over time (before-after) and one across settings (between areas with different levels of the intervention). Subtracting the pre-policy difference in outcomes from the post-policy difference attempts to adjust for pre-existing differences in outcomes, thereby producing a more plausible impact estimate. These two methods can be combined and extended to include many time periods and groups, as well as by conducting a dose-response analysis. The importance of employing such designs whenever possible is highlighted by a study in Bangladesh which showed that the introduction of a maternity programme coincided with declining trends in maternal mortality in programme and non-programme areas (Ronsmans *et al.* 1997). Without the non-programme area comparison, the observed decline could have been incorrectly attributed to the maternity programme.

Although interrupted time series and controlled before and after methods can provide more accurate effect estimates, they also have important technical considerations. Interrupted time series studies require a sufficient number of pre- and post-intervention observations to model trends accurately. However, there is no consensus on what is sufficiently long as this depends on how much the series fluctuates. Interrupted time series methods also work best if the intervention is implemented at one point in time in all study settings and has an immediate and marked effect. When implementation and/or effects are delayed, temporary or variable, estimating impact becomes more difficult. Time series are also subject to autocorrelation between proximate temporal measures which may require statistical adjustment. For controlled before-and-after designs, non-intervention areas may not always be comparable with intervention areas due to differential socioeconomic or environmental factors, or even parallel interventions (Victora *et al.* 2011), and data to quantify differences are not always available. The controlled before-and-after design also assumes that what differs between non-intervention and intervention settings is invariant in time, which may also not be the case. Although these technical considerations pose challenges, employing these more robust study designs is nonetheless essential to improve the validity of estimating user fee policy effects. We have adopted this approach since conducting this review and were able to show policy-associated increases in uptake of facility delivery that went beyond the pre-existing increasing trend, and which controlled for other factors, such as seasonality of facility delivery (Dzakpasu *et al.* 2012). We were also able to demonstrate greater improvements among the poorest and anticipate being able to assess impacts among women with labour and birth complications or at increased risk of such complications.

Assess complex nature of real-world environments

The complex nature of real-world environments and health systems means that fee interventions can impact and interact with a wide range of factors, in which case it is not wholly appropriate to focus on one primary outcome. To better understand such interventions, researchers need to study a range of outcomes, assess longer term effects and gather qualitative data about contextual and implementation factors. Research methods exist that can encompass both quantitative and qualitative findings (Robert *et al.* 2012).

Key contextual issues are the relative magnitude of the fee change, informal fees and quality of care. As mentioned earlier, Akashi *et al.* (2004) indicated that the formal fees introduced at a hospital in Cambodia were accompanied by quality of care improvements and that the combination of predictable costs and improved quality promoted service utilization. In contrast, Daponte *et al.* (2000) suggested that following fee removal, the MMR at a tertiary hospital in South Africa increased due to deteriorating quality of care. Knowing that out-of-pocket fees did not actually increase or that quality of care had deteriorated was critical to understanding the effect of user fee policies in these two settings. Studies should always discuss the actual value of the fee change relative to pre-policy fees and/or a measure of household income such a per capita GDP to provide a sense of the relative magnitude of the fee change. And studies should also consider whether any quality of care issues may be responsible for observed changes.

With regard to outcomes, no single indicator can provide a comprehensive picture of policy impacts on access to care or health outcomes. For example, following fee removal Daponte *et al.* (2000) observed an increase in facility delivery which would have been interpreted as a positive result if the study had not also looked at MMRs and observed an associated increase in the latter. Similarly, an increase in the proportion of deliveries with complications treated in health facilities is difficult to interpret without additional information. It may reflect an increase in the number of managed complications which is a positive effect, no change in the number of managed complications (numerator) but a reduction in the number of facility births (denominator) which may be positive or negative, or an increase in the number of women developing complications due to reduced or delayed health-seeking behaviour which is a negative effect (Ekwempu *et al.* 1990; Owa *et al.* 1995). Indicators that provide information on the number and proportion of women using services and an indication of health impacts must be assessed jointly to better understand intervention effects.

Assessing both short and longer term impacts is also important. Effects may be immediate but not be sustained if increased utilization reduces the quality of care and this in turn reduces utilization, or effects may be delayed if fee changes are implemented gradually or knowledge of them diffuses slowly. Estimating effects at standardized follow-up periods, e.g. immediate, 6 months and 1 year, would not only provide a more comprehensive picture of impact but also facilitate comparison of effects across studies.

Discuss sample representativeness

Another potential source of concern in many studies was sample representativeness. As indicated in Table 3, almost all studies were hospital or facility-based. Yet, the representativeness of facility clients of the target group of pregnant and delivering women was not discussed in any study. Without this information, it is not possible to know whether facility-level changes reflect population-level changes in health-seeking behaviour. For example, we do not know whether an increase in deliveries at one facility reflects an overall increase in facility delivery rather than a shift in clients from other health facilities. Population-level impacts can only be assessed if all facilities in a particular catchment area are studied, or if facility-based statistics are converted to population-based statistics based on the total expected deliveries in that catchment area. Expected deliveries can be derived by multiplying census population estimates by birth or fertility rates, but this method can be problematic if the catchment area is difficult to define or if birth and fertility rates are for more aggregated levels or out-of-date. However, some attempt to establish the representativeness of facility clients is essential for better understanding of policy effects.

Examine quality of routine health information

With the exception of Penfold *et al.* (2007) and Asante *et al.*'s (2007) use of household survey data, health facility data were used to assess effects on health service utilization and outcomes. This highlights the unique importance of data from routine health information systems, particularly, for conducting retrospective studies and the consequent need for their accuracy. By explicitly commenting on the high quality of record keeping in the mobile clinic in which their study was based, Wilkinson *et al.* (2001) assured readers that their findings were unlikely to be a result of spurious data. Steinhardt *et al.* (2011) excluded facilities missing more than 3 months of data, but did not comment on the quality of data from included facilities, and other studies did not comment on data accuracy at all despite the well-established fact that routine health information in low-income countries is often of poor quality (Graham and Campbell 1992; Allotey and Reidpath 2000; Abouzahr and Boerma 2005). Poor data quality and lack of data use can be a self-reinforcing vicious cycle, so the use of routine facility data should be encouraged. However, this should be accompanied by an assessment of the accuracy and content of such data not only to reduce the risk of spurious findings but also to highlight data quality issues in need of improvement. As highlighted by Victora *et al.* (2011), routine health systems data will be increasingly important for evaluating large-scale programmes which cannot be assessed with traditional study designs.

Unanswered impact questions

Magnitude of effects and cost-effectiveness

Despite the poor quality of available evidence, reported results with regard to utilization suggested effects in the expected direction: a tendency towards decreased utilization of ANC and facility delivery after the introduction of fees and increased utilization after the removal of fees. However, this effect should not be taken for granted, as the two studies that took temporal trends into account showed. Following fee removal, Wilkinson *et al.* (2001) observed no impact on ANC visits and Steinhardt *et al.* (2011) observed an unsustained increase in ANC visits and in facility delivery at district hospitals. Estimating the magnitude of effects (immediate and longer term) attributable to fee

changes is central to any assessment of intervention effects and is also needed for any assessment of the cost-effectiveness of fee changes.

Impact on access to emergency obstetric care

Ensuring all women give birth in the presence of a skilled birth attendant with access to emergency obstetric care is advocated for improving maternal and newborn survival (Martines *et al.* 2005; Campbell *et al.* 2006). Based on this paradigm, it is important to measure two groups of indicators: those indicating the level of utilization of health services by all women giving birth and those indicating the level of utilization by women developing complications during pregnancy. Facility delivery is a reasonable proxy for the former in settings where home births are not attended by midwives or doctors and was measured in 17 included studies. However, only six studies reported on changes in complications managed in health facilities (Table 5). Indicators of access to emergency care are more important outcome measures than facility delivery, as this is vital to prevent maternal and neonatal deaths.

Impact on quality of care

There was little quantified evidence of the impact of user fees on the quality of available maternity services, such as drugs and supplies, staffing levels, staffing attitudes and waiting times. Quality of maternity care is particularly critical for deliveries which may require surgical skills and blood transfusions, among other competencies. Although, as discussed earlier, the quality of available services can modify the effect of user fees on health service utilization, user fees can also influence quality of care. Their influence can be direct and positive if revenue from fees is used to improve service provision (e.g. availability of drugs). But as reported by Daponte *et al.* (2000) , removal of fees can also indirectly and negatively impact service provision if increased demand exceeds service capacity. It is important to know whether and how quality of care has been impacted by fee changes.

Impact on maternal and neonatal health outcomes

Attributing changes in maternal or perinatal morbidity or mortality to user fee changes is even more difficult than attribution of changes in utilization, as these outcomes are not only influenced by numerous determinants including health service quality but are also further down the causal chain. Removing user fees can lead to unintended effects, such as worsening health outcomes if existing health services cannot cope with the increased demand created by fee removal (Daponte *et al.* 2000). This is particularly true for delivery services, as not only the absence of services but also poor quality services can cause adverse outcomes including death. Increased or decreased use of health services cannot be assumed to be associated with increased or decreased health outcomes. This has to be explicitly assessed.

In addition to quality of care, other important considerations are the case mix of the population (or subpopulation) increasing or decreasing service utilization and the efficacy of the health service in relation to the outcome of interest. For example, if an increase in utilization occurred mainly among lower risk women fewer health improvements would be expected at the population level than if utilization increased among higher risk women. With regard to efficacy, it is reasonable to assume that a highly efficacious service, such as maternal tetanus immunization, would prevent maternal and neonatal tetanus deaths. However, for other services, such as facility delivery, efficacy is equivocal. Although there is sound evidence that facility-based care can improve birth outcomes, e.g. through active management of the third stage of labour to prevent haemorrhage (Campbell *et al.* 2006), settings with higher rates of facility delivery do not necessarily have the lowest MMRs (Koblinsky *et al.* 1999). Inferring health impacts from use of maternal health services, therefore, requires careful contextual analysis to build a convincing case (Campbell 1999; World Health Organization *et al.* 2009).

Impact on equity in access to care and health outcomes

Only one study reported effects of user fees on inequalities in the utilization of maternity services, and its findings were variable (Penfold *et al.* 2007). The dearth of evidence on this topic was surprising given the important concern that user fees pose a regressive and unaffordable cost burden on poor households, and the equity and poverty reduction goals expressed by fee removal or reduction policies. The scarcity of studies may be partly explained by the fact that routine health facility data on which most analyses were based often lack the sociodemographic variables needed for equity analyses. But this possible explanation was not stated in any of the papers, missing the opportunity to highlight an area of data quality in need of attention. All things being equal, higher fees would deter utilization, whereas lower fees would encourage utilization. Hence if the poor are disproportionally excluded from services because of user fees, introducing or eliminating these should have a larger effect on the poor. However, the poor are also disproportionally affected by other barriers to care, such as distance from services, lack of transportation, costs of transport and possibly cultural or self-efficacy obstacles. Not examining actual impacts on inequality risks a lack of awareness of unintended effects such as increasing inequality and fails to quantify the degree to which user fees as opposed to other factors act as a barrier for the poor.

Review's strengths and limitations

This review systematically looks at the impact of user fees on maternal health services and related health outcomes. A strength is that it examines the evidence specific to maternity care, which is a major focus of the global health movement to abolish user fees for health services. It also builds on previous literature reviews on user fees and health service utilization in low- and middle-income countries by broadening inclusion criteria to studies of any design (albeit for a narrow range of services). As we only included articles published in peer-reviewed journals, we may have omitted some relevant studies but it is unlikely that this would have changed our findings as they were similar to those of related reviews. Including studies of any design lowered the strength of the evidence. However, this evidence interpreted with necessary caveats is preferable to an absence of evidence and also provided an opportunity to highlight methodological issues in need of improvement.

Conclusion

Consistency in the direction of effects provides some evidence that user fees have an effect on utilization of maternity health services, particularly on facility delivery. In environments where the capacity exists to provide good quality care, increasing facility delivery through the removal of user fees is therefore an important and effective public health strategy. However, user fee impacts on health outcomes and socioeconomic inequalities in service utilization and health outcomes are less certain. A more comprehensive understanding of user fee effects requires improved evaluation studies, specifically, better descriptions of the intervention including reporting the amount of official and unofficial user fees charges, use of stronger observational study designs, such as interrupted time series that facilitate estimating net effects, and consideration of more outcomes to make a convincing argument of the nature of effects. These outcomes should include measures of quality of care, as this is an important modifier of policy effects, and measures of access to emergency care, as this is critical for improved MNH. Finally, measures should be taken to monitor the impact of user fee changes on inequalities in utilization of maternal health services and health outcomes.

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None declared.

Endnote

[1] Institutional maternal deaths refer to health facility deaths meeting the International Classification of Diseases 10th Revision definition of a maternal death: death of a woman while pregnant or within 42 days of termination of pregnancy, irrespective of the duration and site of the pregnancy, from any cause related to or aggravated by the pregnancy or its management but not from accidental or incidental causes. Bosu *et al.* defined delivery-related deaths as a subgroup of institutional maternal deaths that occurred 'during labour and selected deaths in the post-partum period'. Causes of 'selected deaths' were not specified. The post-partum period for delivery-related deaths was limited to 10 days. All studies mentioned in this section (Bosu *et al*., Ekwempu *et al*., Owa *et al.* and Daponte *et al*.) ascertained deaths from health facility records.

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