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[Intervention Review]

The impact of contracting out on health outcomes and use of health services in low and middle-income countries

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ABSTRACT

Background

Recent literature on the lack of efficiency and acceptability of publicly provided health services has led to an interest in the use of partnerships with the private sector to deliver public services.

Objectives

To assess the effectiveness of contracting out healthcare services in improving access to care in low and middle-income countries and, where possible, health outcomes.

Search methods

We searched a wide range of international databases, including the Cochrane Central Register of Controlled Trials (CENTRAL), MEDLINE and EMBASE, in addition to development studies and economic databases. We also searched the websites and online resources of numerous international agencies, organisations and universities to find relevant grey literature. The original searches were conducted between November 2005 and April 2006. An updated search in MEDLINE was carried out in May 2009.

Selection criteria

Contracting out health services is defined as the provision of healthcare services on behalf of the government by non-state providers. Studies had to include an objective measure of at least one of the following outcomes: health care utilisation, health expenditure, health outcomes or equity outcomes. Studies also needed to use one of the following study designs: randomised controlled trial, non-randomised controlled trial, interrupted time series analysis or controlled before and after study.

Data collection and analysis

We made an attempt to present results from the different studies in a systematic way, however due to the diversity of sources, contexts and methods used, we undertook a narrative synthesis.

Main results

Three studies met our inclusion criteria (one after re-analysis of data). These studies suggest that contracting out services to non-state providers can increase access and utilisation of health services. One study found a reduction in out-of-pocket expenditures and improvement in some health outcomes. However, methodological weaknesses and particularities of the reported programme settings limit the strength and generalisability of their conclusions.

The impact of contracting out on health outcomes and use of health services in low and middle-income countries (Review)

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Authors' conclusions

Three studies suggest that contracting out may be an appropriate response to scale up service delivery in particular settings, such as post-conflict or fragile states. Evidence was not presented on whether this approach was more effective than making a similar investment in the public sector, as there was not an exact control available in any of the settings. In addition, the introduction of non-state providers into some settings and not others also brings many potentially confounding variables, such as the presence of additional management expertise or expatriate doctors, which may improve drug supply or increase utilisation.

PLAIN LANGUAGE SUMMARY

The impact of contracting out on health outcomes and use of health services in low and middle-income countries

We present results from three studies on the effectiveness of contracting out services to non-state providers. All studies had methodological limitations. The existing evidence suggests that contracting out services may increase access and utilization of health services in underserved areas for poorer population groups but the evidence base is weak. In addition it is not clear what particular action(s) implemented by the NGOs may lead to this effect.

SUMMARY OF FINDINGS FOR THE MAIN COMPARISON *[Explanation]*

Outcomes	Relative effect	Quality of evidence	Comments
Health services utilisation	<ul style="list-style-type: none"> - In one study (cluster-RCT), there were differences in 2 of 8 outcomes measured (an absolute increase of 21% and 19% in use of public facilities and uptake of vitamin A) - In the CBA study there was an increase of 20.8% in the number deliveries attended by health personnel. However, there was no effect on the duration of hospital stay or bed occupancy - The third study showed an immediate increase of more than 130% (+ 144% on daily visits, + 135% on monthly visits), but the effect faded with time (3 studies) 	Very low	2 observational studies (CBA and ITS) with high risk of bias, and one cluster-RCT with high risk of bias
Healthcare expenditure	Reduction in household health expenditures, although the size of the effect was difficult to estimate (decrease in the range of US\$15 to \$56 in annualised individual curative care spending)	Low	1 RCT with a high risk of bias
Health outcomes	The probability of individuals reporting that they had been sick in the past month was reduced. There was also a decrease in the incidence of diarrhoea in infants.	Low	1 RCT with a high risk of bias

CBA = controlled before and after study

ITS = interrupted time series

RCT = randomised controlled trial

BACKGROUND

Since the late 1980s contracts and contracting have become central themes in the transformation of public sector management taking place in many countries (Palmer 2000). This arrangement consists of contracting a non-state provider to deliver a range of clinical or preventive services to a specified population. A contract document usually specifies the type, quantity and period of time

during which the services will be provided on behalf of the government (Palmer 2000). A typical example for developing countries consists of hiring a non-governmental organisation (NGO) to provide primary health care for a specific geographic area, such as a district.

The theoretical roots of this mechanism stem from a particular current that developed in the 1990s (the 'new public manage-

ment'), which emphasised the inefficiency of traditional bureaucratic structures and supported the idea that the introduction of forms of market mechanisms would improve public service delivery (Hood 1991).

In developing countries, both the private-for-profit and private-not-for-profit sectors are often important and well-resourced providers of healthcare services. The motivation for contracting with the private sector is both to utilise these resources in the service of the broadest population, and to improve the efficiency of publicly funded services (Palmer 2000). The application of contracting out, it has been argued, allows a greater focus on measurable results, increases managerial autonomy, draws on private sector expertise and increases the effectiveness and efficiency of services through competition. Contracting out has also been encouraged by a range of external factors: the need to scale-up vertical programmes quickly, concerns over the quality of services dispensed in the public sector and lack of personnel in the public sectors.

Opponents of these reforms have questioned the extent to which the advantages of competition could, in fact, occur in low-income contexts given the relative lack of providers. Similarly, they have highlighted the difficulty and costs in specifying and monitoring contracts, thereby challenging the potential efficiency gains. Finally, the possibility of fragmenting health systems further and undermining the stewardship role of the state has been noted (Palmer 2006).

Contracting is considered a financing strategy in the sense that it is a way of spending public sector funds to deliver services. Payment can be in a block sum or per activity (fee for service), or per head of the population covered by the contract (capitation). The purchaser (government or donor) usually monitors either by asking for activity reports from the provider or by conducting their own surveys to establish the level of health care use and health outcome improvement in the population.

Experiences to date fall into two separate categories: small-scale contracts, usually limited to one or several health facilities, and contracting out experiences that have occurred on a large scale in a particular context. The first category usually entails experiences limited to specific services or to a facility. A number of experiences are noted by Rosen 2000 in the delivery of reproductive health in Latin America. Other experiences reported as case studies have been carried out in some African countries (South Africa, Zimbabwe) (Mills 1997) and Asian countries (India, Papua New Guinea and Thailand). Many of these early contracting experiences were for non-clinical services such as catering and laundry (Bennett 1997).

Currently the idea of contracting out service delivery is a hot topic in many fragile or post-conflict settings. An early example of this took place in Guatemala in 1996 after the war (La Forgia 2004). At the time, the majority of the population (and all of the rural

population) had no access to medical services, and nor did the Ministry have the capacity to expand coverage sufficiently. In more recent years, contracting out services to non-state providers such as NGOs has also been proposed as an efficient way of promoting access to good quality services for poorer groups, in comparison with the difficulties experienced by governments in providing similar services (Bhushan 2002). The use of contracting continues to rise, with relatively large-scale contracting recently occurring in Afghanistan, Pakistan, Bangladesh, India, Rwanda, Southern Sudan and the Democratic Republic of the Congo (DRC) (Loevinsohn 2004; Palmer 2006).

Despite the growing interest in such strategies, the evidence for their effectiveness is still scarce. Several reviews of the literature have been already carried out on contracting out experiences in developing countries. The first one (Mills 1998), which reported experiences of contracting out non-clinical services in Southern Africa, found mixed evidence of the potential benefits of contracting out to private providers. A second review (England 2004) focused on the capacity of contracting out strategies to benefit the poorest, and underlined the lack of robust evidence in that respect. Based on 10 experiences with at least before and after outcomes and their comparisons with the contract objectives, a third review (Loevinsohn 2005) concluded that contracting out could be very effective and should be expanded, with more rigorous evaluations. Finally, very recently, a more inclusive review of the literature (Liu 2007) suggests that if contracting out seems to have improved access to health, effects on other dimensions such as quality of services, efficiency and equity remain unknown.

Definitions of contracting out vary across these reviews, thereby precluding a real comparison of results. None of these reviews systematically appraised the quality of studies included. The current review uses more exclusive inclusion criteria for assessing the effectiveness of interventions and restricts evidence to experimental and quasi-experimental designs.

OBJECTIVES

This review aims to assess the effectiveness of contracting out healthcare services on improving the health outcomes of populations and access to healthcare services in low and middle-income countries. Changes in population access to health services will be evaluated through changes in the use of health services and changes in healthcare expenditure.

METHODS

Criteria for considering studies for this review

Types of studies

Three types of studies have been included in the review:

1. Randomised controlled trials (RCTs) or cluster-randomised controlled trials.

2. Controlled before and after (CBA) studies. For this review, the control group for both RCTs and CBAs had to be areas or health facilities where the provision of health services was undertaken by the public using a traditional type of management, i.e. without the possibility for healthcare managers at the lower levels to define the remuneration levels of their staff.

3. Interrupted time series (ITS) studies. We included ITS studies if their authors used appropriate statistical methods or data (this includes time series analyses - Auto-Regressive Integrated Moving Average (ARIMA) models) or, when they failed to do so, we attempted to re-analyse the data ourselves, provided that:

- i) the point in time when the intervention/change occurred was clearly defined;
- ii) there were at least enough data points before and after the intervention to account for possible seasonal effects (e.g. in the case of monthly data, 12 data points before and after the intervention date). This latter criterion differs from the one recommended by Cochrane EPOC Group, which only requires three points before and after the intervention. This choice was motivated by the nature of the longitudinal data employed in the literature of interest. Indeed, studies usually use routinely collected utilisation data, which are particularly sensitive to seasonal variations. Therefore it is better to have long data series to control for seasonal biases in the analysis.

Types of participants

We excluded studies that did not take place in a low or middle-income country, as defined by the World Bank (World Bank 2006). Units of study were the populations that would potentially access health services. Participants included users and non-users of health services, as well as health facilities, where outcome data could have been collected.

We considered for inclusion studies that focused on contracting out with either private-for-profit and not-for-profit providers. We did not limit the scope of our study to a particular level of healthcare delivery and included all types of clinical services in the review.

Types of interventions

This review focused on contracting out of healthcare services. This intervention had to be defined by the following characteristics:

- A formal contractual relationship between the government and a non-state provider had to have been defined.
- The object of the contract had to be the provision of health services for a specific geographic area and period of time, on

behalf of the government. We excluded contracting out of non-clinical services such as laundry and food.

- The entity delivering the health services on behalf of the government had to belong to the private sector (either for-profit or not-for-profit). Therefore, we excluded 'contracting in', whereby autonomy of management (and resources) is given to an *internal entity* to reach some agreed targets from this review.

Types of outcome measures

Primary outcomes

Primary outcomes were changes in healthcare utilisation or healthcare expenditure, which were both considered as proxies for dimensions of change in access to health services.

- Access to care can be measured by changes in utilisation patterns of health facilities or services (immunisation coverage, number of visits, rates of hospitalisation, etc.), equivalent information collected directly from the population through surveys, or both. Information related to distance travelled or travel time was out of the scope of our study unless it was related to healthcare expenditure (see below).
- Health care expenditure was considered when it reflected direct (and indirect) costs borne by the patient, his/her family, or both.

Secondary outcomes

Secondary outcomes included changes in equity of utilisation or patient outcomes.

- Changes in equity of access: increased access for disadvantaged groups or a reduction in gaps in coverage. This criterion would require a baseline analysis and categorisation of the population of interest by socio-economic status. Any methodology for such classification was acceptable provided it was adequately described and explained.
- Changes in health outcomes, measured by measurements of nutritional status, morbidity and mortality rates (broken down by age group, sex, etc.) were also of interest.

It should be noted that measures of quality of services were sometimes reported but not considered as primary outcomes, considering the difficulty of reporting them in an objective and coherent manner.

All measures of utilisation or patient outcomes had to be objective and we did not include measurements based on attitudes, beliefs or perceptions.

Search methods for identification of studies

Electronic searches

The search to identify studies for this review was initially carried out as a part of a much wider review on health financing mechanisms [Lagarde 2006](#) dealing with the effects of several financing strategies ([Lagarde 2006](#)). The broad review has been split into several sub-reviews, including the present one. The search methodology therefore includes terms that encompass a broader scope than the one defined for this review.

We originally searched the following electronic databases without language or date restrictions:

- the Cochrane EPOC Group Register (and the database of studies awaiting assessment), 20/01/2006;
- the Cochrane Central Register of Controlled Trials (CENTRAL) (*The Cochrane Library* 2006, issue 1), 20/01/2006;
- PubMed, 11/11/2005;
- EMBASE (Athens), 19/04/2006;
- Popline, 08/12/2005;
- African Healthline (bibliographic databases on African health issues), 28/04/2006;
- IBSS (International Bibliography in Social Sciences, Athens interface), 19/04/2006;
- the Database of Abstracts of Reviews of Effectiveness, 20/01/2006;
- BLDS, 03/11/2005;
- ID21, 24/11/2005;
- ELDIS, 25/11/2005;
- the Antwerp Institute of Tropical Medicine database, 26/01/2006;
- Jstor, 26/01/2005;
- Inter-Science (Wiley), 16/12/2005;
- ScienceDirect, 16/12/2005;
- IDEAS (Repec), 20/01/2005;
- LILACS, 19/04/2006;
- CAB-Direct (Global Health), 17/04/2006;
- Healthcare Management Information Consortium (HMIC), 17/04/2006;
- World Health Organization Library Information System (WHOLIS), 18/04/2006;
- MEDCARIB, 19/04/2006;
- ADOLEC, 19/04/2006;
- FRANCIS, 16/12/2005;
- BDSR, 16/12/2005; and
- USAID database, 04/11/2005.

We carried out an updated search of MEDLINE in May 2009. No new studies were identified as potentially relevant for the review. The detailed search strategy used is set out in [Appendix 1](#).

The PubMed search strategy was mainly developed using reviews cited in the background section of the protocol ([Lagarde 2006](#)) and their references.

The original search strategy was developed without the usual EPOC methodology filter. However, the updated search strategy has now introduced such a methodology filter to limit study de-

signs to randomised trials, controlled trials, time series analyses and controlled before and after studies.

The detailed search strategy used for PubMed can be found in [Appendix 1](#). We translated this search strategy for the other databases using the appropriate controlled vocabulary, as applicable. Search strategies for electronic databases used selected index terms and free text terms. In addition, we used a number of free text terms to browse other databases or lists of studies: 'health financing', 'contracting', 'pay for performance', 'outsourcing', 'supply-side incentive', 'performance payment', 'output-based payment' and 'P4P'.

Searching other resources

We also carried out an extensive search of grey literature resources between the months of November 2005 and February 2006.

We searched the websites and online resources of UNICEF, the World Bank, Partnerships for Health Reforms, Abt Associates, Management Sciences for Health (MSH), Oxford Policy Management, Save the Children, Oxfam and a number of other networks or organisation websites, such as The Private Sector Partnerships-*One*, the Indian Council for Research on International Economic Relations, Equinet - The Network for Equity in Health in Southern Africa and the Organization for Social Science Research in Eastern and Southern Africa (OSSREA).

We searched the websites and online resources (working papers) of numerous university research centres, including the Institute of Social Studies, The Hague, the University of Southampton, the International Centre for Diarrhoeal Disease Research, the Centre for Health and Population Research, Dhaka, the Boston University Institute for Economic Development, Harvard Initiative for Global Health, Cornell Food and Nutrition Policy Program, the Institute of Development Studies (University of Sussex), the London School of Hygiene and Tropical Medicine (HEFP website), the Institute of Policy Analysis and Research (IPAR) in Kenya, the Development Policy Research Unit of the University of Cape Town and the Netherlands Institute for Southern Africa.

We screened the reference lists of all of the relevant references retrieved. We contacted authors of relevant papers or known experts in the fields of interest to identify additional studies, including unpublished and ongoing studies.

Data collection and analysis

Selection of studies

Two authors (ML and NP) independently selected the studies to be included in the review. We resolved any disagreements by discussion.

Data extraction and management

We used a standardised data extraction form to record the following information from included studies:

- type of study (individual or cluster-randomised trial, controlled before and after study, interrupted time series);
- duration of study;
- study setting (country, key features of the healthcare system, external support, other health financing options in place, other ongoing economic/political/social reforms);
- characteristics of the participants (catchment area size, characteristics of the population, existing health service provision etc.);
- characteristics of the intervention (nature of the contractor, scope and characteristics of the contract); and
- main outcome measures and results.

In one case we included additional outcomes (overall cost of the programme) because this provided additional insight.

Assessment of risk of bias in included studies

We slightly adapted the standard criteria recommended by the Cochrane EPOC Group (EPOC 2002) to match the particularities of the studies found in the field of interest. For example, criteria about following up patients or doctors were not relevant as most of the studies used population survey data. Follow-up surveys, when carried out, would therefore not be done with the same population but with a new random sample. In addition, we added some specific criteria to account for some of the limitations of studies found (e.g. no statistical analysis performed or failure to account for clustering effects). Appendix 2 presents the detailed list of all quality criteria used, and explains the amendments introduced to the original EPOC criteria for each type of design.

The criteria for RCTs and cluster-RCTs were:

1. concealment of allocation;
2. protection against exclusion bias;
3. appropriate sampling strategy;
4. appropriate analysis;
5. reliable primary outcomes measures;
6. protection against detection bias;
7. baseline measurement of outcomes; and
8. protection against contamination.

The criteria for CBA studies were:

1. baseline measurement of outcomes;
2. baseline characteristics of studies using second site as control;
3. protection against exclusion or selection bias;
4. protection against contamination;
5. reliable primary outcomes measures; and
6. appropriate analysis of data.

The criteria for ITS studies were:

1. protection against changes;
2. appropriate analysis of the data (or re-analysis possible);

3. protection against selection bias;
4. reliability of outcome data;
5. number of points specified;
6. intervention effect specified; and
7. protection against detection bias.

Two authors assessed the quality of the selected studies independently. We resolved discrepancies in quality ratings by discussion. After assessment of all quality criteria, we classified the studies into three categories according to their risk of bias:

- low risk of bias = all criteria scored as 'done';
- moderate risk of bias = one or two criteria scored as 'not clear' or 'not done';
- high risk of bias = more than two criteria scored as 'not clear' or 'not done'.

Our assessment of the risk of bias in the included studies is presented in Table 1.

Data synthesis

One of the included studies provided longitudinal data but had failed to analyse it in a relevant manner. The authors of the study computed means before and after the intervention, without accounting for trends. Therefore the data, as reported in the paper, were re-analysed. We then examined these data series with the following segmented regression model to control for secular trends and potential serial correlation of data, and detect any significant changes after the policy change:

$$Y_t = b_0 + b_1 * \text{Time} + \beta_2 * \text{intervention} + b_3 * \text{Postslope} + \epsilon_t$$

Where Y_t is the outcome variable at time t . Time is a continuous variable indicating time from the start of the study up to the end, to capture any structural trend. Intervention is coded 0 for pre-intervention timepoints and 1 for post-intervention timepoints. Postslope is coded 0 up to the last point before the intervention phase and coded sequentially from 1 thereafter. When auto-correlation was detected by a Durbin-Watson test, it was corrected with a Prais-Winsten regression. Using the regression results obtained, we then calculated the predicted outcome measure for the date after the intervention and at regular intervals afterwards. We carried out no prediction beyond the scope of the original data series used.

For controlled before and after studies, we presented the outcome measures before and after, in both intervention and control areas whenever they were available in the original studies. Based on those, we then systematically calculated the 'net' or 'relative' effect of the intervention (NE), by accounting for differences in outcomes between control and intervention sites (at follow up and baseline):

$$NE = (\text{INT}_{\text{followup}} - \text{INT}_{\text{baseline}}) / \text{INT}_{\text{baseline}} - (\text{CONT}_{\text{followup}} - \text{CONT}_{\text{baseline}}) / \text{CONT}_{\text{baseline}}$$

The first part of the formula calculates the percent change in the outcome measure in intervention sites, while the second part computes the same for control sites.

For one of the included studies (Bloom 2006), outcome measures are reported for all provinces (both control and intervention) and the treatment effects reported in Table 2, Table 3, Table 4, Table 5 and Table 6 are the regression coefficient of the interaction between the treatment variable and the follow-up survey. This is virtually equivalent to calculating the relative effect of the intervention, while also accounting for the impact of other potential confounding factors, included as regressors in the analysis (measures of wealth and socio-economic variables).

Our confidence in the available estimates of effects was graded using an approach similar to the one recommended by the GRADE Working Group (GRADE 2004). The GRADE quality scores are High, Moderate, Low and Very low. When grading the quality of evidence, we initially graded ITS studies as 'Low' quality. This was led by our conviction that those studies had generally used unreliable sources of data, and no or not comparable control sites. We felt that in the context of health systems intervention, this precluded us from obtaining any reliable measure of effects.

RESULTS

Description of studies

See: [Characteristics of included studies](#); [Characteristics of excluded studies](#).

Study designs

The three included studies were:

1. a controlled before and after study from Bolivia (Lavadenz 2001);
2. an interrupted time series study from Pakistan (Ali 2005); and
3. a cluster-randomised controlled trial from Cambodia (Bloom 2006).

Characteristics of settings and patients

In two cases contracting out was motivated by weaknesses or absence of public system to provide health care. In Cambodia problems of governance in the public system were outlined and in Pakistan small coverage by the public sector was a problem.

In Cambodia and Pakistan the programmes took place in rural areas, while in Bolivia the study had an urban setting (neighbourhood of the capital city, La Paz).

Characteristics of interventions

All three contracts were with private not-for-profit providers. A description of the contracts is presented in Table 7.

In Pakistan (Ali 2005) a pilot project was initiated in the district of Rahimyar Khan (3.7 million inhabitants, mostly rural) between the provincial government and the Punjab Rural Support Program (PRSP), a non-governmental organisation (NGO). PRSP took over management of 104 Basic Health Units (BHU). They organised them into clusters of three, each cluster being headed by a doctor who had formerly been in charge of only one BHU. The doctors' salaries were increased due to these increased duties, and they also ceased to do private practice.

In Cambodia (Bloom 2006; Bhushan 2002; Schwartz 2004) the 'Contracting for Health Services Pilot Project' (1998 to 2002), funded by the Asian Bank for Development and the World Bank (Loevinsohn 2000), contracted NGOs to provide district health-care services in some selected districts. The pilot project spanning from 1998 to 2002 aimed to compare the results of both contracting for services (contracting out) and contracting external management to run public services (contracting in), with services run (as usual) by public sector district health teams. In the contracted out districts the NGOs were free to make any reforms that they wished to service delivery. These included the banning of private practice, increasing of health worker salaries and introduction of user fees in at least one district.

In Bolivia (Lavadenz 2001) a network of eight health centres and one hospital were contracted out in two stages. First the hospital in the district was delegated to the NGO, then the rest of the facilities in the district were put under the NGO's management to ensure continuity of care and limit the bypass of primary care facilities. Precise activities under the contract are not detailed in the paper.

Characteristics of outcomes

Utilisation outcomes reported were based on facility data for two studies (number of visits in Pakistan, bed occupancy rate and deliveries in Bolivia). The remaining study from Cambodia (Bloom 2006) provided results from household surveys on health outcomes, health service utilisation and household health expenditures (see Table 8).

Risk of bias in included studies

Each of the studies presented methodological weaknesses in their analysis, design or both.

The study from Cambodia (Bloom 2006) was designed as a cluster-randomised controlled trial. This was limited by several flaws that are likely to have biased estimates of effects:

1. out of the original 12 randomised districts that were selected, only eight were finally included as not enough bids were received from potential service providers (Bloom 2006);

2. the number of clusters chosen was too limited to enable the randomisation process to be successful: there were only two clusters (districts) randomly selected to be contracted out and four control districts;

3. it seems that financial resources available for contracted districts were 85% greater than those of control districts (see additional Table 6, reproduced from Bloom 2006);

4. the analysis of Bhushan 2002 and Schwartz 2004 does not take clustering effects into account, although that of Bloom 2006 does.

The study from Bolivia (Lavadenz 2001) was a controlled before and after (CBA) study, which suffers from a non-equivalence of control site and intervention areas. While the intervention arm consisted of contracted-out facilities, the convenient control site chosen is a maternity unit where no change in management has occurred. Little information is provided on this maternity unit, so it is difficult to know to what extent this can constitute an adequate control site. In fact, it is dubious that a network of facilities can be compared to a particular ward of a single hospital. Besides, a few differences of outcomes at baseline and errors of computation we found in the paper do not encourage trust in the overall quality of the study.

Information provided on the study in Pakistan is limited. Although mention of this experience is provided by Loevinsohn 2004 and Loevinsohn 2005, the original consultancy report provided little information on potential confounding factors (Ali 2005). Therefore, estimates of effects calculated by re-analysis of the data provided in this report are likely to suffer from biases due to concurrent events or changes occurring in the health system over the period of time considered (almost three years).

Effects of interventions

See: [Summary of findings for the main comparison](#)

Cambodia

The following results are found by Bloom 2006 from Cambodia. The changes are mostly reported as percentage change in the outcome measures (often proportions), accounting for baseline differences in outcomes between the control and intervention sites. We have included this study as it tried to take clustering effects into account and correct for the problems in randomisation (see Table 2, Table 3, Table 4, Table 5 and Table 6 for a summary of results relevant to this review):

Health utilisation outcomes

Contracting out had a significant positive impact on two of the eight targeted outcomes: it induced an increase of nearly 29 percentage points in the use of public facilities and of 42 percentage points in the uptake of vitamin A (while in the same time the

comparison group increased by respectively 8 and 23 percentage points). Bloom 2006 finds that despite significant and important coefficients, none of the other outcomes have been significantly affected by the contracting out approach. Unlike the conclusions of Schwartz 2004, Bloom 2006 conclude that contracting out has not had a significant impact on immunisation rates, and the increase may be explained by the general secular increase of service provision in Cambodia at the time.

Health outcomes

Contracting out significantly reduced the probability of individuals reporting that they had been sick in the past month. Results also suggest a decrease in the incidence of diarrhoea in infants (though the effect is less significant). Due to the limited size of the sample, no effect could be detected on newborn mortality.

Health expenditure

Contracting out reduced household health expenditures, which is consistent with a lessening of private sector use. Due to a skewed distribution and some suspicious high reported expenses, it is difficult to assess the exact effect. Bloom et al. suggest that contracting out led to a reduction of health expenditures of between US\$56 and \$15 (Bloom 2006).

Bolivia

The results for this study (Lavadenz 2001) and changes detected were calculated by the review authors. We report changes in percentages. Based on the limited available outcome measures from the facilities' information system, contracting out seems to have led to an increase in health services utilisation (see additional Table 9).

- There was an increase in deliveries attended by health personnel in the health intervention district after the contracting out of the hospital (+ 24% compared to + 14.5% for the comparison district). The relative effect of contracting out the hospital would therefore be an increase of 9.4%. Similarly, contracting out the rest of the primary care facilities in the district led to a further 8.8% 'net' increase in the deliveries (measured at the district level). Overall, the two phases of outsourcing led to a 20.8% increase in deliveries in facilities.

- Contracting out the hospital diminished both average duration of stay and bed occupancy rate by 16.2% and 22.3% respectively. The second phase of contracting out the primary facilities reversed this trend and bed occupancy and average duration of stay increased by 23% and 8.3% respectively.

However, these results are not very satisfactory as there was a concurrent extension of the insurance scheme which probably contributed to the boosting of demand for health services. A lack of

a statistical check on the robustness of the results, as well as limited outcome measures, represent critical limitations of the study validity.

Pakistan

A re-analysis of available data from Pakistan (Ali 2005) shows a steep increase in health services utilisation data (average number of daily and monthly consultations in the basic health units) just after the beginning of the contracting out strategy. The re-analysis shows that the average number of visits per day and the number of monthly visits increased respectively by 135% and 144% immediately after the intervention started. However, the effect of the intervention was not sustained, as both outcomes were still higher than pre-intervention but steadily declining in the 18 months following the start of the intervention (see Table 10).

DISCUSSION

Drawing conclusions from this review is challenging due to the complexity of an 'intervention' such as contracting out. Unlike more simple, clinical interventions there is an array of factors that can influence a strategy like contracting. First, the very label 'contracting out' can mean different things in different settings and in fact even in the studies included in this review it involves different elements. What is actually implemented is the result of the role and decisions of key actors and interactions at quite decentralised levels, therefore implementation issues need to be looked at very carefully to understand what causal pathways can explain the effects observed. We tried to address this limitation by providing detailed contextual elements, but this was limited by the material provided in the original studies.

Contracting out is often presented as a 'pay-for-performance' type of intervention where well-designed incentives are supposed to bring about better performance on the supply side. However, there are in fact a number of different components in a strategy to contract out services to non-public providers that may be instrumental in the observed effect. These include the possible role of a new management style (motivating their human resources in a more efficient way, for example), the possible 'reputation' effect that private providers might benefit from (in particular in comparison with public providers), the potential role of the incentives and objectives included in the contract, or the implementation of thorough monitoring systems and sanctions (which are usually absent in the delivery of health services within the public sector). Some authors highlight the importance of incentives for health workers in explaining the success of contracting (Bloom 2006), yet it is unclear whether a non-governmental organisation (NGO) being in charge of the management and control plays a role or whether individual incentives implemented by government services could

have similar effects. Unfortunately, the included studies provided very little description of the actual measures implemented by the contractor in terms of management, organisation, salaries etc. to gain the differences in effect that were observed. Other factors, such as the reputation effect of the arrival of an international NGO, may be just as important in bringing about the observed effect as the contracting intervention per se. The quality of the studies reviewed is not strong enough to safeguard against this risk of bias.

Several elements might potentially alter the effects of contracting out strategies.

Firstly, government capacity to manage the contract may be challenging the success of contracting out strategies. The broader the services contracted, the harder it will be to define a contract precisely. Other studies have focused on the role of the contract design as a determinant of performance (Palmer 2003; Strong 2005). They emphasise that when contract specification is incomplete, monitoring is likely to be difficult. Weak capacity within the government might therefore compromise the successful implementation of contracting out strategies.

The feasibility of adequately monitoring service delivery in remote areas is also a key implementation issue. Under such circumstances the motivation of the contractor may be critical. The more remote the point of service delivery, or the more complex the service to be delivered, the more likely it appears that contracts will be governed by informal means.

Finally, the introduction of non-state providers immediately brings many potentially confounding variables such as the presence of additional management expertise or culture style, or expatriate doctors, which could themselves be the reasons for improved quality of services and hence increased utilisation.

The complexity of the contracting out intervention therefore makes it virtually impossible to link its possible effectiveness to one single incentive mechanism.

AUTHORS' CONCLUSIONS

Implications for practice

Although the channels through which contracting out increases service delivery are unclear, it seems to be an effective option in settings where the government is unable to reach populations adequately. However, due to the limited evidence available on contracting out in low and middle-income countries, policy-makers should closely evaluate the effects and describe the components of their strategies.

A number of other experiences, including some reviewed here, underline the usefulness of contracting out to private providers where the public sector is absent or too weak. This is the case for under-served areas or post-conflict settings (Marek 1999). In such

settings, it might be quicker to re-deploy public funds to private providers than to build up a public health system.

Governments should pay particular attention to the elements included in the contract they draw up with private providers, in particular the targets on which their performance will be assessed. For example, if the contract focuses on a defined set of outcomes, there is a risk that contractees might divert their effort from unmeasured to measured outcomes. Although the evidence from Cambodia suggests that this might not necessarily happen, contracts should be carefully designed with such issues in mind.

Implications for research

There is still a need for some good quality research in this area. The poor quality of the studies included in this review suggests that so far there have been few attempts to try to evaluate the effects of contracting out health services rigorously. There is a need for new research using experimental or quasi-experimental studies to study these issues. Yet the complexity of contracting out strategies, as well as the variety of ways to actually implement them, calls for additional complementary research. Process evaluation of the implementation of such strategies should also be carried out to understand better the systematic differences between contracting

out services to private providers and using the equivalent funding for the public provision of services.

Other questions that would need to be addressed by such studies would be the cost-effectiveness of contracting out, compared to using funds to strengthen the public sector, and the effects of contracting out on the quality of services provided. Other studies not included in this review have compared the cost and quality of contracted services to those provided by the public sector, but only at one point in time. It is therefore not possible to determine whether there are other systematic differences between the services (Broomberg 1997; Mills 2004).

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* *Indicates the major publication for the study*

CHARACTERISTICS OF STUDIES

Characteristics of included studies [ordered by study ID]

Ali 2005

Methods	ITS	
Participants	Country: Pakistan 104 Basic Health Units contracted out (the whole district of Rahimyar Khan)	
Interventions	Contracting out of primary health services (both curative and preventive)	
Outcomes	Health utilisation (number of visits per day/month)	
Notes	<p>Contextual factors</p> <p>Problems of staff motivation (absenteeism, private practice) initiated the programme</p> <p>The contracting process was not a competitive one</p> <p>In the first year of the pilot experience (until July 2004), procurement of drugs was still managed by the government. This changed during the second year, when the contractee took over</p>	
<i>Risk of bias</i>		
Item	Authors' judgement	Description
Allocation concealment?	Unclear	-

Bloom 2006

Methods	CCT	
Participants	Country: Cambodia 2 districts contracted out 4 districts run by government The entire project covered a total population of about 1.26 million people (11% of the population of Cambodia)	
Interventions	Contracting out of primary health services	
Outcomes	Health utilisation (immunisation coverage) and household health expenditure	
Notes	<p>Contextual factors</p> <p>Before the beginning of the programme, corruption and informal payment, and private practice of publicly funded health workers were major issues</p> <p>The contacting process was competitive, but only international NGOs won the bids</p> <p>Construction of facilities: in Cambodia, during the contracting project, the number of functioning health centres in the whole country grew from 60 to 900</p>	
<i>Risk of bias</i>		

Bloom 2006 (Continued)

Item	Authors' judgement	Description
Allocation concealment?	Unclear	-

Lavadenz 2001

Methods	CBA
Participants	Country: Bolivia Contracting out of 1 district (1 hospital and 8 health centres) El Alto, a poor municipality next to La Paz
Interventions	Contracting out of maternal health services
Outcomes	Health utilisation (bed occupancy rate, number of deliveries)
Notes	Contextual factors 3 periods are described in the study: <ul style="list-style-type: none"> • Jan 1990 to June 1999: baseline • Aug 1999 to Jan 2000: implementation of an insurance scheme (Seguro Basico de Salud) and contracting of the hospital • Feb 2000 to June 2000: contracting of the whole district The introduction of the insurance scheme is therefore an important factor

Risk of bias

Item	Authors' judgement	Description
Allocation concealment?	Unclear	-

CBA = controlled before and after study

CCT = controlled clinical trial

ITS = interrupted time series

NGO = non-governmental organisation

Characteristics of excluded studies [ordered by study ID]

Study	Reason for exclusion
Bhushan 2002	Bloom 2006 re-analysed the same experiment with more appropriate statistical analysis (accounting for clustering and trying to control for other biases)
Jack 2003	Case study

(Continued)

La Forgia 2004	Case study
Loevinsohn 2001	Focus of the paper was out of the scope of the review; was on the design of the scheme, not the effect
Marek 1999	Description of 2 large-scale projects where preventive and nutrition interventions were contracted out. In both cases, the authors present longitudinal data collected after the beginning of the intervention
McPake 1995	Case study
Mills 1997	Case study
Nieves 2000	Case studies
Palmer 2005	Case studies
Schwartz 2004	Bloom 2006 re-analysed the same experiment with more appropriate statistical analysis (accounting for clustering and trying to control for other biases)
Slack 2005	Compilation of case studies

DATA AND ANALYSES

This review has no analyses.

ADDITIONAL TABLES

Table 1. Assessment of risk of bias in included studies

Controlled before and after (CBA) studies										
Study ID	Base-line measurement	Baseline characteristics (equivalent control site)	Protection against exclusion or selection bias	Protection against contamination	Reliability of outcome measures	Appropriate analysis	Over-all risk of bias	Notes		
Lavadenz 2001	DONE	NOT DONE	NOT DONE	NOT CLEAR	NOT CLEAR	NOT DONE	High risk of bias	No statistical analysis to test the robustness of results; control and intervention sites very different; intervention and control districts are very close, so contamination may have occurred; only facility-based outcomes		
Randomised controlled trial										
Study ID	Concealment of allocation	Protection against exclusion bias	Sampling	Appropriate analysis (clustering)	Quality/reliability of the data	Protection against detection bias	Base-line measurement	Protection against contamination	Over-all risk of bias	Notes
Bloom 2006	NOT DONE	DONE	NOT DONE	DONE	DONE	NOT CLEAR	DONE	NOT CLEAR	High risk of bias	Despite major flaws in the study design (problems in the allocation of the contracts, limited number of clusters) this

Table 1. Assessment of risk of bias in included studies (Continued)

Study ID	Protection against changes	Appropriate analysis	No selection bias in the sample framing	Quality of outcome data	Number of points specified	Intervention effect specified	Detection bias	Overall risk of bias	Notes
Interrupted time series									
Ali 2005	NOT DONE	DONE (re-analysis)	NOT DONE	NOT CLEAR	DONE	DONE	DONE	High risk of bias	No information provided on concurrent events, nor on the number and type of facilities from which the figures come

Table 2. Treatment effects on health service outcomes specified as targets in the contracts

	% Fully immunised children aged 12 to 23 months	% Children who received high-dose vitamin A in the past 12 months	% Women who delivered in the past year and had at least 2 antenatal care visits	% Women who delivered in the prior year with a trained professional	% Women who delivered in the prior year in a facility (public or private)	% Women (with a live child aged 6 to 23 months old) who currently use a contraception method	% Women who gave birth in the prior 24 months and know about modern contraception methods	% Population who choose a public sector facility when needing a curative care consultation
Baseline measure in all provinces (%)	34	43	9	24	3	13	22	4
Follow-up measure in all provinces	81	61	35	34	1	23	8	13

Table 2. Treatment effects on health service outcomes specified as targets in the contracts (Continued)

(%)								
Treat- ment effect of contract- ing out (change in percent- age points)	15	4.17***	26.3	-12.3	7.4	-3.8	7.3	28.9***

Notes: this table reports the results from Bloom 2006 regressions with province X year effects. Standard errors are corrected for clustering at the district level. Stars indicate significance under clustering: * at 10%; ** at 5%; *** at 1%.

Table 3. Treatment effects on health service outcomes not specified as targets in the contracts

	Outcomes not specified as targets	
	Probability of receiving a treatment for diarrhoea when needed)	Average number of additional antenatal checks received by women (beyond 2)
Baseline measure <i>in all provinces</i>	0.89	0.65
Follow-up measure <i>in all provinces</i>	1	3
Treatment effect of contracting out (relative change in outcome measure, accounting for differences in baseline outcomes)	14.4*	0.578

Notes: this table reports the results from Bloom 2006 regressions with province X year effects. Standard errors are corrected for clustering at the district level. Stars indicate significance under clustering: * at 10%; ** at 5%; *** at 1%.

Table 4. Treatment effects (regression results) on final health outcomes

	Probability of having reported ill during past month	Probability of having diarrhoea in the past month	Child < 1 alive
Baseline measure in all provinces	0.202	0.35	0.97
Follow-up measure in all provinces	0.185	0.26	0.97
Treatment effect of contracting out (relative change in outcome measure - account-	-0.145	-0.252	-0.043

Table 4. Treatment effects (regression results) on final health outcomes (Continued)

ing for differences in baseline outcomes)			
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Notes: this table reports the results from Bloom 2006 regressions with province X year effects. Standard errors are corrected for clustering at the district level. Stars indicate significance under clustering: * at 10%; ** at 5%; *** at 1%.

Table 5. Treatment effects on self-reported care-seeking behaviour

	Proportion of individuals who consulted a health provider when ill (by type of healthcare provider)				(Conditional on consulting a provider), proportion of individuals who visited a type of healthcare provider			
	None	Unqualified provider	Qualified private provider	Qualified public provider	Unqualified provider	Qualified private provider	Qualified public provider	Average effect size (qualified provider)
Baseline measure in all provinces (%)	82.4	8.5	9.5	0.9	48	44	6	-
Follow-up measure in all provinces (%)	82.6	8.1	8	3.1	44	41	16	-
Treatment effect of contracting out (change in percentage points)	11.8	-9.8	-9.0*	5.4***	-20.5	-10.1	27.9***	70.1***

Notes: this table reports the results from Bloom 2006 regressions with province X year effects. Standard errors are corrected for clustering at the district level. Stars indicate significance under clustering: * at 10%; ** at 5%; *** at 1%.

Table 6. Treatment effects on health care spending (annualised individual curative care spending)

	All individuals	All individuals excluding 0.5% tails [#]	All individuals excluding those who spent > US\$100 last month [#]
Baseline average healthcare spending in all provinces	\$18.76	\$10.42	\$15.17

Table 6. Treatment effects on health care spending (annualised individual curative care spending) (Continued)

Follow-up average healthcare spending in all provinces	\$12.12	\$7.51	\$9.84
Treatment effect of contracting out (relative change in outcome measure - accounting for differences in baseline outcomes)	-55.856***	-22.122**	-15.608*

Notes: this table reports the results from Bloom 2006 regressions with province X year effects. Standard errors are corrected for clustering at the district level. Stars indicate significance under clustering: * at 10%; ** at 5%; *** at 1%.

Bloom et al. argue that due to the non-normality of the distribution of self-reported healthcare expenditures as well as extreme cases within this portion of the population, average expenditures would be skewed by the upper tail and therefore exclude it.

Table 7. Description of contracts

Study ID	Country	Contractor	Contractee	Nature of the contract	Measures taken by the NGO
Ali 2005	Pakistan	The district government of Rahimyar Khan, the provincial health department	National NGO called PRSP (Punjab Rural Support Program)	Transfer of the control, management and use of buildings, furniture and equipment of 104 Basic Health Units (BHUs) in the district of Rahimyar Khan and transfer of all relevant budgetary provision (salaries including for unfilled posts, medicine, maintenance, building repairs, equipment, etc.) to be able to provide all relevant curative and preventive primary health services The contract was signed for 5 years	Division of all BHUs into 35 clusters of 3, where 1 doctor is appointed as a team leader Facilitation of Medical Officers' mobility through an interest-free car loan Increase of doctors' salaries (nearly threefold) and prohibition on private practice
Bloom 2006	Cambodia	The Ministry of Health of Cambodia	International NGOs - different for each district	Transfer of the control, management and use of all means in district hospitals, sub-district health centres and health posts to provide all promotive, preven-	Increase of salaries (significant increase, but unclear how much) Supervision by expatriate staff (between 0.5 and 3 per district) Ban on private practice

Table 7. Description of contracts (Continued)

				<p>tive and curative services (known as the Minimum Package of activities) in the districts</p> <p>Performance was measured and poor performance could result in sanction and non-renewal of the contract. 8 performance targets were identified, essentially for child and maternal health</p>	(for 1 of the 2 NGOs) Firing non-co-operative personnel (between 1 and 3)
Lavadenz 2001	Bolivia	Federal Ministry of Health and Social Provision, Departmental Government and El Alto Municipality	NGO	Contracting out of the management of the hospital in the first instance (Aug 1999 to Jan 2000) and then of the whole district	Not mentioned

NGO = non-governmental organisation

Table 8. Outcome measures

Study ID/ Intervention	Health outcomes	Health services utilisation outcomes	Other
Bloom 2006	Self-reported illness in the past month Incidence of diarrhoea among young children	Uptake of vitamin A for children, antenatal care, deliveries in a health facility, number of assisted deliveries, use of contraception, reported use of public facilities when sick	Household health expenditures
Ali 2005	None	Number of visits at the facility (monthly)	-
Lavadenz 2001	None	Number of deliveries, bed occupancy rate, average stay duration	-

Table 9. Results based on data from Lavandez 2001

	Deliveries			Bed occupancy rate			Average stay duration		
	Interven- tion district	Compara- son district	Relative effect	Interven- tion district	Compara- son district	Relative effect	Interven- tion district	Compara- son district	Relative effect
Effect of contracting out the hospital + insurance scheme (phase 2 - phase1): % change	24.0%	14.5%	9.4%	-5.4%	16.9%	-22.3%	-12.5%	3.7%	-16.2%
Effect of contracting out the rest of the network (phase 3 - phase 2): % change	13.8%	5.0%	8.8%	24.3%	1.3%	23.0%	4.8%	-3.6%	8.3%
Effect of insurance + contracting out of hospital and health centres (phase 3 - phase 1): % change	41.1%	20.2%	20.8%	17.6%	18.5%	-0.9%	-8.3%	0.0%	-8.3%

Table 10. Results of the re-analysis of data from Ali 2005

Outcome	Immediate impact (% change compared to month before the intervention)	Impact after 6 months (% change compared to month before the intervention)	Impact after 12 months (% change compared to month before the intervention)	Impact after 18 months (% change compared to month before the intervention)

Table 10. Results of the re-analysis of data from Ali 2005 (Continued)

Number of daily out-patient visits	135%	109%	84%	59%
Number of monthly out-patient visits	144%	120%	97%	73%

APPENDICES

Appendix I. Search strategy used for PubMed

The search in PubMed was also restricted to all the developing countries listed on the World Bank website, by selecting all relevant geographical categories as exploded terms.

Some pilot searches led us to use quite general (exploded) MeSH terms, as it was noticed that several relevant articles were indexed with generic MeSH terms, or not particularly appropriate ones. For example, a study on Ghana would not be referenced under 'Ghana' but under 'Africa'. Besides, since including 'Africa[MeSH]' would also include all MeSH terms of lower levels, it was decided to include mainly higher level MeSH terms for delimiting the geographic scope of the study (see #1 below). A few countries were excluded (see #6).

A similar approach was taken for specifying the topic filters of the search. Generic MeSH terms were used (see #2), and more selective terms that are currently used in the literature were added as free text references (see #3). However, because this would potentially return a large number of irrelevant studies, it was decided to limit this by excluding some irrelevant studies (see #4).

These different filters were then rearranged together (see #7, #8 and #9).

1	Search "Developing countries"[MeSH] OR "Africa"[MeSH] OR "Central America"[MeSH] OR "South America"[MeSH] OR "Latin America"[MeSH] OR "Mexico"[MeSH] OR "Asia"[MeSH] OR "Commonwealth of Independent States"[MeSH] OR "Pacific Islands"[MeSH] OR "Indian Ocean Islands"[MeSH] OR "Europe, Eastern"[MeSH]
2	Search ("Economics"[MeSH] OR "Economics"[SH] OR "socioeconomic factors"[MeSH]) AND ("Delivery of health care"[MeSH] OR "health services research"[MeSH] OR "health planning"[MeSH] OR "health services "[MeSH] OR "utilization"[SH])
3	Search "Fees and charges"[MeSH] OR user fee[TIAB] OR user fees[TIAB] OR social insurance[TIAB] OR health insurance[TIAB] OR community-based insurance[TIAB] OR prepayment plan[TIAB] OR prepayment plans[TIAB] OR prepayment scheme[TIAB] OR prepayment schemes[TIAB] OR conditional cash transfers[TIAB] OR cost recovery[TIAB] OR prepayment[TIAB] OR contracting out [TIAB] OR output-based contract[TIAB] OR pay for performance [TIAB]
4	Search "Personnel Downsizing"[MeSH] OR "workplace"[MeSH] OR "health planning guidelines"[MeSH] OR "patient freedom of choice laws "[MeSH] OR "preferred provider organizations"[MeSH] OR "provider-sponsored organizations"[MeSH] OR "emergency Medical Service Communication Systems"[MeSH] OR "Genetic Services"[MeSH] OR "Medical Errors"[MeSH] OR Chemicals and Drugs Category[MAJR] OR "Drug industry"[MAJR] OR "epidemiology"[MAJR] OR "Patents"[MAJR] OR "War"[MAJR] OR Anatomy Category[MAJR] OR "Child Abuse"[MeSH] OR ("Technology and Food and Beverages Category"[MAJR] NOT "food supply"[MeSH])

(Continued)

5	Search Practice Guideline[ptyp] OR Letter[ptyp] OR Editorial[ptyp] “Clinical Trial”[ptyp] OR “Clinical Trial, Phase I”[ptyp] OR “Clinical Trial, Phase II”[ptyp] OR “Clinical Trial, Phase III”[ptyp] OR “Clinical Trial, Phase IV”[ptyp]
6	Search “Japan”[MeSH] OR “Korea”[MeSH] OR “Taiwan”[MeSH] OR “New Zealand”[MeSH] OR “Singapore”[MeSH] OR “Israel”[MeSH]
7	Search #1 AND #2 NOT #4 NOT #5 NOT #6
8	Search #1 AND #3 NOT #4 NOT #5 NOT #6
9	Search #8 OR #7

Ovid MEDLINE(R) 1950 to April Week 4 2009

Searched 5 May 2009

1. “Fees and Charges”/
2. Fees, Dental/
3. Fees, Medical/
4. Fees, Pharmaceutical/
5. Prescription Fees/
6. Hospital Charges/
7. Capitation Fee/
8. Fee-for-Service Plans/
9. “Cost Sharing”/
10. Contract Services/
11. Outsourced Services/
12. Prepaid Health Plans/
13. Prospective Payment System/
14. Insurance, Health/
15. ((medical or dental or pharmac\$ or dispensing or drug or drugs or medicament? or medicine? or prescript\$ or consultation? or treatment? or registration? or hospital? or care) adj3 (fee? or charge?)).tw.
16. ((user? or patient? or outpatient? or inpatient?) adj3 (fee? or charge? or pay\$)).tw.
17. fee for service?.tw.
18. capitation.tw.
19. ((pay\$ or cash or money or monetary or economic or financial) adj3 incentive?).tw.
20. (pay\$ adj3 performance).tw.
21. p4p.tw.
22. ((result? or performance) adj based).tw.
23. ((result? or performance or output or out put) adj2 (financ\$ or pay\$ or incentive? or initiative? or bonus\$)).tw.
24. ((cash or pay\$) adj3 (condition\$ or contingent or requirement?)).tw.
25. ((cash or pay\$ or monetary or money) adj3 transfer\$).tw.
26. cost sharing.tw.
27. cost recover\$.tw.
28. price change?.tw.
29. (contract or contracts or contracting).tw.
30. (outsourc\$ or out sourc\$).tw.
31. (risk sharing or shared risk?).tw.
32. (prospective adj (pay\$ or reimbursement?)).tw.
33. (prepay\$ or pre pay\$ or prepaid or pre paid).tw.
34. ((health or medical) adj insurance?).tw.

35. ((social or community) adj3 (insurance? or financ\$)).tw.
36. demand side.tw.
37. supply side.tw.
38. (financ\$ adj (strategy or strategies)).tw.
39. or/1-38
40. Developing Countries/
41. Medically Underserved Area/
42. exp Africa/ or exp "Africa South of the Sahara"/ or exp Asia/ or exp South America/ or exp Latin America/ or exp Central America/
43. (Africa or Asia or South America or Latin America or Central America).tw.
44. (American Samoa or Argentina or Belize or Botswana or Brazil or Bulgaria or Chile or Comoros or Costa Rica or Croatia or Dominica or Equatorial Guinea or Gabon or Grenada or Hungary or Kazakhstan or Latvia or Lebanon or Libya or Lithuania or Malaysia or Mauritius or Mexico or Micronesia or Montenegro or Oman or Palau or Panama or Poland or Romania or Russia or Seychelles or Slovakia or South Africa or "Saint Kitts and Nevis" or Saint Lucia or "Saint Vincent and the Grenadines" or Turkey or Uruguay or Venezuela or Yugoslavia).sh,tw. or Guinea.tw. or Libia.tw. or libyan.tw. or Mayotte.tw. or Northern Mariana Islands.tw. or Russian Federation.tw. or Samoa.tw. or Serbia.tw. or Slovak Republic.tw. or "St Kitts and Nevis".tw. or St Lucia.tw. or "St Vincent and the Grenadines".tw.
45. (Albania or Algeria or Angola or Armenia or Azerbaijan or Belarus or Bhutan or Bolivia or "Bosnia and Herzegovina" or Cameroon or China or Colombia or Congo or Cuba or Djibouti or Dominican Republic or Ecuador or Egypt or El Salvador or Fiji or "Georgia (Republic)" or Guam or Guatemala or Guyana or Honduras or Indian Ocean Islands or Indonesia or Iran or Iraq or Jamaica or Jordan or Lesotho or "Macedonia (Republic)" or Marshall Islands or Micronesia or Middle East or Moldova or Morocco or Namibia or Nicaragua or Paraguay or Peru or Philippines or Samoa or Sri Lanka or Suriname or Swaziland or Syria or Thailand or Tonga or Tunisia or Turkmenistan or Ukraine or Vanuatu).sh,tw. or Bosnia.tw. or Cape Verde.tw. or Gaza.tw. or Georgia.tw. or Kiribati.tw. or Macedonia.tw. or Maldives.tw. or Marshall Islands.tw. or Palestine.tw. or Syrian Arab Republic.tw. or West Bank.tw.
46. (Afghanistan or Bangladesh or Benin or Burkina Faso or Burundi or Cambodia or Central African Republic or Chad or Comoros or "Democratic Republic of the Congo" or Cote d'Ivoire or Eritrea or Ethiopia or Gambia or Ghana or Guinea or Guinea-Bissau or Haiti or India or Kenya or Korea or Kyrgyzstan or Laos or Liberia or Madagascar or Malawi or Mali or Mauritania or Melanesia or Mongolia or Mozambique or Myanmar or Nepal or Niger or Nigeria or Pakistan or Papua New Guinea or Rwanda or Senegal or Sierra Leone or Somalia or Sudan or Tajikistan or Tanzania or East Timor or Togo or Uganda or Uzbekistan or Vietnam or Yemen or Zambia or Zimbabwe).sh,tw. or Burma.tw. or Congo.tw. or Kyrgyz.tw. or Lao.tw. or North Korea.tw. or Salomon Islands.tw. or Sao Tome.tw. or Timor.tw. or Viet Nam.tw.
47. ((developing or less\$ developed or third world or under developed or middle income or low income or underserved or under served or deprived or poor\$) adj (count\$ or nation? or state? or population?)).tw.
48. (Imic or Imics).tw.
49. or/40-48
50. randomised controlled trial.pt.
51. random\$.tw.
52. intervention\$.tw.
53. control\$.tw.
54. evaluat\$.tw.
55. effect?.tw.
56. or/50-55
57. Animals/
58. Humans/
59. 57 not (57 and 58)
60. 56 not 59
61. 39 and 49 and 60

Appendix 2. Quality criteria used for appraising quality of included studies

This appendix presents the detail of all of the criteria used in the appraisal of included studies.

Controlled before and after (CBA) studies

In the following list, criteria one, two and four are directly taken from the list of standard criteria of the Cochrane EPOC Group. Criteria three and five are adapted from the original criteria to make them more relevant to the specificities of the studies included in this review. We rephrased standards to judge the risk of exclusion or selection bias to be more adapted to the types of population-based studies that might be included in the review. We also adapted the criterion on quality and reliability of data to reflect better the risks of bias relating to the type of outcomes that were the primary focus of the review.

Criteria six was added following preliminary findings which showed that statistical significance of studies was not systematically computed or available in the studies found.

Finally, we omitted a standard criterion of the *Cochrane Handbook for Systematic Reviews of Interventions* (Handbook 2008) on the blinded assessment of primary outcomes. We judged that this was not relevant for the types of outcomes this review focused on.

1. **Baseline outcome characteristics:** DONE if outcomes were measured prior to the intervention, and no substantial differences were present across study groups (e.g. where multiple pre-intervention measures describe similar trends in intervention and control groups); NOT CLEAR if baseline measures are not reported, or if it is unclear whether baseline measures are substantially different across study groups; NOT DONE if there are differences at baseline in main outcome measures which are likely to undermine the post-intervention differences (e.g. are differences between the groups before the intervention similar to those found post-intervention?)

2. **Equivalent control sites:** DONE if characteristics of study and control sites are reported and similar (in terms of 1) population, 2) facilities and 3) external influence characteristics); NOT CLEAR if it is not clear in the paper, e.g. characteristics are mentioned in the text but no data are presented; NOT DONE if there is no report of characteristics either in the text or a table OR if baseline characteristics are reported and there are differences between study and control providers.

3. **Protection against exclusion or selection bias:** DONE if outcome measures obtained from the whole population or a representative sample of the population (and the control group) were studied; NOT CLEAR if not specified in the paper; NOT DONE if outcome measures were not obtained from a representative sample.

4. **Protection against contamination:** DONE if allocation was by community, institution or practice and it is unlikely that the control group received the intervention; NOT CLEAR if communication (i.e. individuals present in one control group cannot move and benefit from the interventions in experimental areas) between treatment and control group was likely to occur; NOT DONE if it is likely that the control group received the intervention (e.g. cross-over studies or if patients rather than providers were randomised).

5. **Quality/reliability of outcome measures:** DONE if the outcome is obtained from some automated system (e.g. length of hospital stay) or comes from another objective source; NOT CLEAR if reliability is not reported for outcome measures that are obtained by chart extraction or collected by an individual (will be treated as NOT DONE if information cannot be obtained from the authors); and NOT DONE if the primary data are reportedly of a poor quality.

6. **Appropriate analysis:** DONE if statistical significance of differences in outcomes was tested and/or statistical analysis was appropriate; NOT CLEAR if statistical significance of results is not specified in the paper or if the analysis chosen was not appropriate; NOT DONE if statistical significance of results was not tested.

Randomised controlled trials

All the following criteria are taken from the standard EPOC criteria (EPOC 2002), except for criteria three and four. Indeed, we judged it important to add specific criteria for cluster-randomised trials for two reasons. Firstly, because the interventions of interest are more likely to be implemented at community level, they would require such study designs. Secondly, issues regarding sampling and analysis have been identified as particular concerns that might lead to biases when analysing cluster-randomised trials (Ukoumunne 1999). We also omitted one criterion on exclusion bias concerning the follow up of professionals. It was judged not relevant for the focus of our review (where studies are all focusing on populations).

1. **Concealment of allocation:** DONE if the unit of allocation was by institution, team or professional and any random process is described explicitly, e.g. the use of random number tables or coin flips; OR the unit of allocation was by patient or episode of care and there was some form of centralised randomisation scheme, an on-site computer system or sealed opaque envelopes were used; NOT CLEAR if the unit of allocation is not described explicitly OR the unit of allocation was by patient or episode of care and the authors report using a 'list' or 'table', 'envelopes' or 'sealed envelopes' for allocation; NOT DONE if the authors report using alternation such as reference to case record numbers, dates of birth, day of the week or any other such approach (as in CCTs) OR the unit of allocation

was by patient or episode of care and the authors report using any allocation process that is entirely transparent before assignment such as an open list of random numbers or assignments OR allocation was altered (by investigators, professionals or patients).

2. **Protection against exclusion bias:** DONE if outcome measures obtained for 80% to 100% of subjects randomised (or a biased sample) or for patients who entered the trial (we did not assume 100% follow up unless stated explicitly); NOT CLEAR if not specified in the paper; NOT DONE if outcome measures obtained for less than 80% of subjects randomised (or a biased, non-representative sample).

3. **Sampling (for cluster-randomised trials):** DONE if sampling took cluster effects/bias into account or if the sample is large enough to provide robust results; NOT CLEAR if not specified in the paper; NOT DONE if the sampling is too small to provide robust results.

4. **Appropriate analysis (for cluster-randomised trials):** DONE if the analysis accounted for cluster effects/bias; NOT CLEAR if not specified in the paper; NOT DONE if the analysis did not account for cluster effects/bias.

5. **Quality/reliability of the data:** DONE if the outcome is obtained from some automated system (e.g. length of hospital stay) or comes from another objective source; NOT CLEAR if reliability is not reported for outcome measures that are obtained by chart extraction or collected by an individual (will be treated as NOT DONE if information cannot be obtained from the authors); and NOT DONE if the primary data are reportedly of a poor quality.

6. **Protection against detection bias:** DONE if the authors state explicitly that the primary outcome variables were assessed blindly OR the outcome variables are objective, e.g. length of hospital stay, drug levels as assessed by a standardised test; NOT CLEAR if not specified in the paper; NOT DONE if the outcome(s) were not assessed blindly.

7. **Baseline measurement:** DONE if performance or patient outcomes were measured prior to the intervention, and no substantial differences were present across study groups (e.g. where multiple pre-intervention measures describe similar trends in intervention and control groups); NOT CLEAR if baseline measures are not reported, or if it is unclear whether baseline measures are substantially different across study groups; NOT DONE if there are differences at baseline in main outcome measures likely to undermine the post-intervention differences (e.g. are differences between the groups before the intervention similar to those found post-intervention?).

8. **Protection against contamination:** DONE if allocation was by community, institution or practice and it is unlikely that the control received the intervention; NOT CLEAR if professionals were allocated within a clinic or practice and it is possible that communication between experimental and group professionals could have occurred; NOT DONE if it is likely that the control group received the intervention (e.g. cross-over trials or if patients rather than professionals were randomised).

Interrupted time series (ITS) analyses

We decided to modify the criteria proposed by EPOC slightly, and have provided some explanation on why we decided to do this. Basically, we argue that health service utilisation data (which are the longitudinal data used for the ITS included here) are subject to seasonal variation. In order to account for this potential bias, we decided to include studies that provided data where seasonal variation could be minimally accounted for (hence the requirement, for example, for 12 months before and after the intervention in the case of monthly data)

1. **Protection against changes:** DONE if the intervention occurred independently of other changes over time; NOT CLEAR if not specified (NOT DONE if information cannot be obtained from the authors); NOT DONE if reported that intervention was not independent of other changes in time.

2. **Appropriate analysis:** DONE if ARIMA (Auto-Regressive Integrated Moving Average) models were used OR time series regression models were used to analyse the data and serial correlation was adjusted/tested for OR if reanalysis performed; NOT CLEAR if not specified; NOT DONE if it is clear that neither of the conditions above not met.

3. **No selection bias in the sample framing:** DONE if outcome measures are obtained from the whole population or a representative sample of the population studied; NOT CLEAR if not specified (will be treated as NOT DONE if information cannot be obtained from the authors); NOT DONE if data set is not drawn from a representative sample.

4. **Quality/reliability of outcome data:** DONE if the outcome is obtained from some automated system (e.g. length of hospital stay) or comes from another objective source; NOT CLEAR if reliability is not reported for outcome measures that are obtained by chart extraction or collected by an individual (will be treated as NOT DONE if information cannot be obtained from the authors); and NOT DONE if the primary data are reportedly of a poor quality.

5. **Number of points specified:** DONE if monthly data for at least 12 months (or more) pre- and post-intervention were used (or an equivalent number allowing the analysis of seasonal variations); NOT CLEAR if fewer data points are given with a convincing argument that no seasonal variations occurred; NOT DONE if few data points are provided and seasonal variations are likely to have occurred.

6. **Intervention effect specified:** DONE if point of analysis was the point of intervention OR a rational explanation for the timing of intervention effect was given by the author(s).

7. **Detection bias:** DONE if it is reported that intervention itself was unlikely to affect data collection (for example, sources and methods of data collection were the same before and after the intervention).

HISTORY

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CONTRIBUTIONS OF AUTHORS

ML and NP prepared the protocol, ML conducted the searches, ML and NP applied the inclusion criteria, assessed the quality and extracted the data for the included studies. ML prepared the report and NP commented and edited it.

DECLARATIONS OF INTEREST

None known.

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INDEX TERMS

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