Unconditional cash transfers for reducing poverty and vulnerabilities: effect on use of health services and health outcomes in low- and middle-income countries (Protocol)

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TABLE OF CONTENTS

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A B S T R A C T

This is the protocol for a review and there is no abstract. The objectives are as follows:

The primary objective of this review is to assess the effect of UCTs for reducing poverty and vulnerabilities on the use of health services and health outcomes in children and adults in LMICs.

Secondary objectives of the review are:

- to assess the effect of UCTs on relevant social determinants of health and health care expenditure in LMICs; and

- to assess the relative effectiveness of UCTs compared with CCTs with regards to improving use of health services and health outcomes in LMICs.

B A C K G R O U N D

Description of the condition

This review will focus on the effect of unconditional cash transfers (UCTs), a type of social protection intervention, on the use of health services and health outcomes in low- and middle-income countries (LMICs). More specifically, we will review those UCTs that principally aim to reduce either or both of income poverty and vulnerabilities. Income poverty and vulnerabilities in LMICs present central concerns for national governments, international organisations, nongovernmental organisations and civil society (Alvaredo 2013)

Income poverty

Income poverty (daily income of USD 2.00 or less) affects more than 30% of the population in a typical LMIC (Alvarado 2013), with an estimated 1.2 billion people living in extreme poverty (daily income of USD 1.25 or less) in 2010 (Olinto 2013). While overall extreme income poverty has considerably reduced over the last two decades, partially driven by rapid advances in China, it remains at problematic levels in several LMICs (Alvarado 2013). Income poverty is an important social determinant of health (CSDH 2008; McDonough 2005). It is linked to ill-health and causes (or exacerbates) both environmental and other social determinants of health, such as access to clean drinking water and sanitation, as well as education, employment and housing (CSDH 2008; McDonough 2005).

Vulnerabilities

Vulnerabilities commonly tackled by UCTs are being an orphan, old, disabled or affected by HIV (Arnold 2011; Garcia 2012). Over 0.1 billion children in LMICs have lost one or both of their parents to HIV, conflict or other causes (Stover 2007). Many of these orphans live in poverty or suffer from other vulnerabilities, including having to work to secure sufficient income or living with HIV (Stover 2007). The number of older people in LMICs has steadily increased, driven by lower fertility rates and increased life expectancy. Old age is associated with multiple vulnerabilities (including poverty and disability), especially in LMICs without universal old age pensions. Disability and living with HIV (or in a family affected by HIV) is also associated with multiple vulnerabilities, including unemployment and poverty. These diverse and interconnected vulnerabilities are central social determinants of health in LMICs (CSDH 2008).

Description of the intervention

Social protection

Social protection is defined as "protecting individuals and households during periods when they cannot engage in gainful employment or obtain enough income to secure their livelihoods - due to unemployment, sickness, chronic ill health or disability, old age or care responsibilities" (p16, UNRISD 2010). In what has been termed the "quiet revolution", social protection policies have increasingly gained prominence on development agendas around the world (Barrientos 2008). These policies comprise three types of interventions, namely labour market, social insurance and social assistance interventions (Arnold 2011). Social assistance interventions are "noncontributory transfer programs targeted in some manner to the poor and those vulnerable to poverty and shocks"

to ensure an adequate standard of living (p4, Grosh 2008). Types of social assistance interventions include cash transfers, in-kind transfers, fee waivers, subsidies and public works programmes.
The World Health Organization (WHO) Commission on Social Determinants of Health (CSDH 2008) and other experts (Marmot 2010; Marmot 2012; WHO 2011) have recommended some policies promoting social protection over the life course to policy makers as effective interventions for addressing the social determinants of health (e.g., income poverty and vulnerabilities) to improve individual and population health and health equity in LMICs. The Commission advised that: "Governments, where necessary with help from donors and civil society organizations, and where appropriate in collaboration with employers, build universal social protection systems and increase their generosity towards a level that is sufficient for healthy living" (p87, CSDH 2008). Development banks such as the World Bank have also expressed the opinion that "social protection programs can have a direct positive impact on poor families as they help build human capital and productivity as a result of better health, more schooling, and greater skills" (World Bank 2013).

Cash transfers for reducing poverty or vulnerabilities

Cash transfers are cash payments provided by formal institutions (governmental, international or nongovernmental organisations) to selected recipients generally for meeting their minimum consumption needs (Garcia 2012). They first gained popularity during the 1990s as interventions used by several South American countries to counter the negative effects of the 1980s debt crises (Arnold 2011; Garcia 2012). However, they have proliferated in many LMICs around the world, especially rapidly since the early 2000s (Arnold 2011; Garcia 2012). Today, cash transfers are common in middle-income countries and the WHO Americas (especially South American) and South-East Asian regions, but have recently been introduced in low-income countries and the WHO African, European, Eastern Mediterranean and Western Pacific regions (Garcia 2012). The primary funders and administrators of cash transfers are national governments, international organisations (often development banks) and donors, as well as nongovernmental organisations (especially common in Africa) (Garcia 2012). Between 2007 and 2010, development assistance spending on cash transfers more than sextupled (from USD 23 million to USD 150 million), mostly driven by increases in dedicated donor funding (Global Humanitarian Assistance 2012). An estimated total of between 0.8 and 1.0 billion persons in LMICs received a cash transfer in 2011 (Arnold 2011).
The basic economic rationale for cash transfers is that additional income from these interventions prevents recipients from adverse personal or systemic income shocks and protects their standard of living by enabling them to maintain their spending on essential goods (e.g., food and medicines) and services (e.g., health services)

during financially lean times, without needing to sell their assets or become indebted (Arnold 2011). Cash transfers are also expected to promote wealth creation by enabling recipients to build human capital (including better health), accruing savings to purchase productive assets and obtaining access to loans with better conditions (Arnold 2011). Furthermore, by providing additional income to poor or otherwise vulnerable persons, cash transfers are also expected to change opinions, attitudes and relationships between citizens, and between citizens and their government (Arnold 2011). For example, a cash transfer may increase the social status and inclusion of the recipient group by providing the recipients with additional income and may influence citizens' electoral support for the government, depending on such factors as the transfer's social acceptability and perceived fairness (Garcia 2012). Moreover, it is argued that cash transfers reduce poverty and vulnerabilities more effectively and cost-effectively than other public sector investments (Fiszbein 2009). For example, compared to in-kind transfers, cash transfers maximise utility by giving recipients greater flexibility to satisfy their specific needs by not predetermining a commodity (Fiszbein 2009), do not accrue the high costs of storing and transporting goods (Lagarde 2009) and are less prone to leakage through corruption (Lagarde 2009).

Cash transfer interventions have diverse objectives, designs and methods of implementation. However, two broad types of cash transfers can be differentiated. The first type, which this review focuses on, are cash transfers provided regularly over extended periods of time to reduce income poverty and vulnerabilities sustainably (Arnold 2011; Garcia 2012). The majority of regular, extended cash transfers primarily aim to reduce income poverty by addressing transitory poverty over the short term and, in turn, chronic and intergenerational poverty over the long term (Arnold 2011; Garcia 2012). Some cash transfers primarily (or as a second objective beside poverty reduction) aim to reduce vulnerabilities in target populations (Arnold 2011; Garcia 2012). The second general type of cash transfer, which is outside the scope of this review, are one-off, short-term payments provided after natural or humanitarian disasters for immediate financial relief, or to incentivise desirable actions such as repatriation of refugees or reintegration of former soldiers after an armed conflict (Arnold 2011; Garcia 2012; Global Humanitarian Assistance 2012).

Unconditional cash transfers for reducing poverty or vulnerabilities

Cash transfers for reducing poverty or vulnerabilities can be differentiated by their degree of conditionality into UCTs and conditional cash transfers (CCTs). UCTs have no conditions beyond a broadly defined eligibility category that defines a segment of the population, such as poor people or orphans, as eligible (i.e., who one is) (Garcia 2012). In contrast, CCTs are provided conditional on engaging in prescribed actions (sometimes called co-responsibilities), such as using certain health services or attending school

(i.e., what one does) (Garcia 2012). Most UCTs define eligibility criteria, but UCTs have no conditions or co-responsibilities attached to their receipt (Garcia 2012). CCTs can be further differentiated according to the consequences of noncompliance. If no penalties are imposed for noncompliance, then their conditions are described as 'soft'; if noncompliance does lead to penalties, then their conditions are 'hard' (Garcia 2012). 'Fuzzy' cash transfers that do not neatly fit into the classic classification of UCTs versus CCTs also exist (Baird 2013). For example, some transfers may be designed to be unconditional, but administered by organisations (e.g., the ministry of health) that create *de facto* conditions (e.g., enrolment for the cash transfer is linked to enrolment into a vaccination programme). This review focuses on genuine UCTs, that is cash transfers for reducing poverty or vulnerabilities, whose design does not attach any (soft or hard) conditions.

The underlying theory for the use of UCTs understands the poor as rational actors and assumes that providing them with additional income will result in them engaging in desired behaviours, through which they will eventually graduate from poverty and overcome their vulnerabilities (Arnold 2011). This theory expects UCTs to generate similar, beneficial behaviour change to CCTs, because recipients are motivated and able to engage in the behaviours that CCTs are conditioned on. UCTs could also generate greater behaviour change, because they are more socially acceptable and less stigmatising for their recipients than CCTs. In contrast, the alternative theory underpinning the application of CCTs argues that "poor households lack full information on the long-term benefits of preventive health care and education" and that conditions are required to ensure the cash transfer generates the desired behaviours among its recipients (p49, Arnold 2011). This theory expects CCTs to generate greater behaviour change than UCTs, because CCTs incentivise desired behaviours not only through income effects, but also through (imposed) substitution effects (Fiszbein 2009; Garcia 2012). It is sometimes also argued that conditioning cash transfers may increase their political feasibility (Garcia 2012).

Some experts have made the case for using cash transfers as policy tools specifically for addressing key social determinants of health (poverty and vulnerabilities) to improve the health of socio-economically disadvantaged populations and, in turn, health and health equity in the population in LMICs (Forde 2012). However, it is unknown whether UCTs for reducing poverty and vulnerabilities improve the use of health services and, ultimately, health outcomes and, if so, by how much.

Furthermore, it is also unknown whether UCTs more effectively and cost-effectively improve the use of health services and health outcomes in LMICs than do CCTs (Arnold 2011; Baird 2012; Fiszbein 2009; Robertson 2013; Schubert 2006). However, it has been hypothesised that UCTs, under certain conditions, are more effective (Schubert 2006). For example, conditioning a cash transfer on the use of health services is not expected to provide any health benefits, if health services are inaccessible or of insufficient

quality. In addition, if use of health services increases due to a conditional cash transfer (CCT) without adjustment on the supply side, overall quality of care may suffer. Moreover, attaching conditions to a cash transfer could increase the social stigma attached to the transfer, which could reduce its positive health effects. On the other hand, implementing UCTs may be less politically feasible, especially in middle-income countries, because UCTs are commonly seen merely as a cash give away to the poor and vulnerable. For example, in the Philippines, policy makers decided to condition a cash transfer after deliberately considering the transfer's political feasibility (personal communication, Jed Friedman, 25 April 2014). It has also been hypothesised that UCTs are more cost-effective (Schubert 2006). The reasons are that conditioning a cash transfer results in additional direct, indirect and opportunity costs to the recipients from having to comply with the conditions, as well as additional costs to the administrator for monitoring recipients' compliance with the conditions. Costs to recipients are often higher in persons with a lower socioeconomic position, with a potential perverse effect on health equity. On the other hand, there could be savings from not paying persons eligible for

a CCT who do not comply with the required conditions, and if these savings more than compensate for the CCT's additional administrative costs, then this would make the CCT cheaper than an equivalent UCT programme (Baird 2011). Therefore, if UCTs are found to be just as effective as CCTs (or marginally less effective, but effective nevertheless), they may be the preferable option in LMICs (as long as their implementation is politically feasible). The reasons are that CCTs have the additional requirement for adequate supply of services for conditions to be met, potentially carry higher costs for both the recipients and the administrator and they require adequate compliance monitoring systems.

How the intervention might work

A conceptual model of the causal relationship between an unconditional cash transfer (UCT) and a health outcome is presented in Figure 1. UCTs may impact health outcomes either through income (any causal pathway operating through A in Figure 1) or directly (pathway I).

Figure 1. Conceptual framework of the causal relationship between an unconditional cash transfer for reducing poverty and vulnerabilities and the use of health services and health outcomes
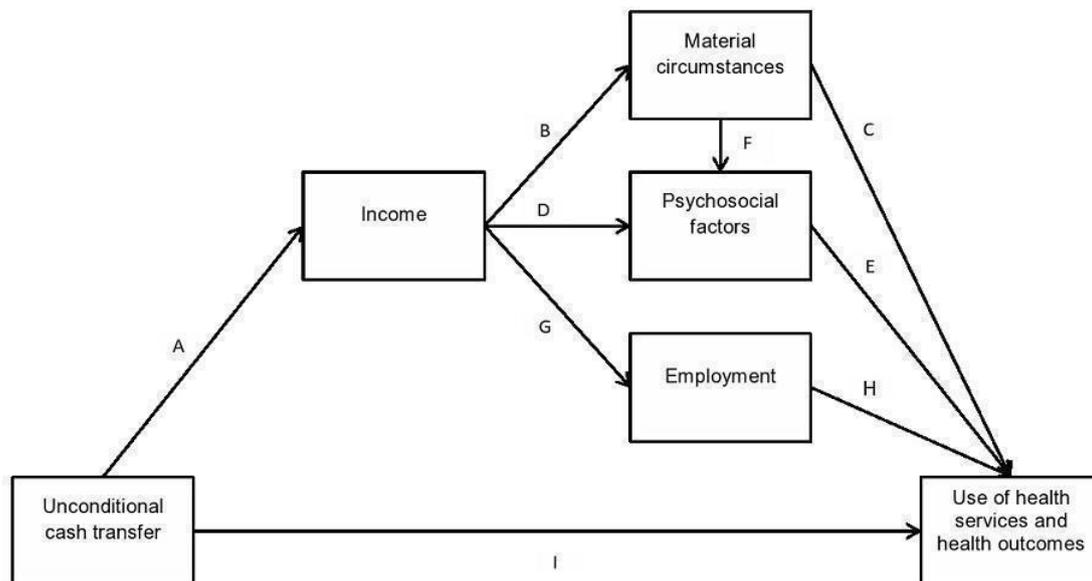
Income pathway

The primary causal pathway through which UCTs impact health is through income. There is conclusive evidence demonstrating that

cash transfer programmes reduce the depth or severity of income poverty in children and adults in LMICs (Arnold 2011; Barrientos 2006). This reduction of the risk of income poverty in the recipient

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4

household in and of itself may influence (likely improve) health outcomes. More specifically, income from publicly funded cash transfers may impact health at the individual level through four types of causal effects: (1) Direct consumption effects (pathway A-B-C in Figure 1); (2) direct status effects (pathway A-D-E); (3) combined consumption and status effects (pathway A-B-F-E); and (4) employment effects (pathway A-G-H) (Borjas 2013; Lundberg 2010). In direct consumption effects, income influences material conditions, which determine health through physical mechanisms (Lundberg 2010). For example, if recipients of an UCT used the additional income from the transfer to purchase goods and services that benefit their health, such as health services or nutritious food, then the UCT would be expected to improve health outcomes in the recipients. However, if recipients used the income from UCTs to purchase goods and services that damage their health, such as tobacco or alcohol, then the UCT would be expected to negatively affect health outcomes in its recipients. Another consumption effect would be differential investment behaviour on the part of the household and greater diversification of economic activities into higher risk, but higher expected return activities, both of which may influence health outcomes.

In the second type of effect, called direct status effects, the additional income from an UCT impacts the health of recipients through psychosocial mechanisms associated with the recipients changing their relative income position (Lundberg 2010). For example, the additional income from an UCT could increase a recipient's income position (relative to relevant individuals or comparison groups), enhancing their social status, reducing psychosocial stress and, ultimately, improving physical and mental health outcomes. In the third type of effect, called combined consumption and status effects, income impacts health through both physical and psychological mechanisms, namely material conditions and, in turn, social inclusion (Lundberg 2010). For example, if recipients used the additional income from an UCT to purchase goods and services that enhanced their inclusion in a social group (e.g., club membership), then this may positively impact their health. The level to which this social group promotes health is expected to mediate the level to which the additional income from the UCT increases health. So, social inclusion in groups promoting healthy behaviours (e.g., exercising and eating nutritious food) is expected to have more positive health effects than social inclusion in groups promoting unhealthy behaviours (e.g., tobacco and alcohol use). In the fourth type of causal effect, called employment effects, income impacts health through changing employment (Borjas 2013). For example, assuming that leisure time is a normal good, additional income from an UCT would be expected to reduce the number of hours the recipient works, which, in turn, may impact health outcomes. Alternatively, a recipient of an UCT could keep her hours worked constant, but switch to an occupation with a lower wage, which could also impact health outcomes. The level to which the UCT would be expected to increase health would depend on the level to which a reduction in employment changed

health, which likely depends on such factors as the status and condition of the employment (Benach 2010a; Benach 2010b). For example, an UCT would be expected to increase health more in recipients who reduced their working hours in employment with negative working conditions that decreased health (e.g., through exposing the recipient to hazardous substances) than in employment with positive working conditions that improved health (e.g., through increasing the recipients' sense of self efficacy and self worth).

Furthermore, the theory of a minimum income for a healthy living hypothesises that income over a certain threshold is a prerequisite for good health (Morris 2000; Morris 2007). While minimum income thresholds have been calculated for selected populations in some high-income countries, they have not yet been established for LMICs (Gorman 2007). An UCT would be expected to have a more positive health effect in recipients whose income it lifts above the minimum threshold than in recipients whose income it does not lift above the threshold.

Direct pathway

UCTs may also directly affect health through welfare security (pathway I in Figure 1) (Pega 2012; Sjöberg 2010). Welfare security is a sense of psychological security from knowing that specific (or combinations of) cash transfers ensure income supplementation in times of financial hardship (Pega 2012; Sjöberg 2010). A recent study demonstrated that high-income countries with cash transfers for the unemployed had higher levels of employment-related welfare security and subjective well-being than high-income countries without such transfers (Sjöberg 2010).

Why it is important to do this review

This review differs from previous reviews in that it investigates the impact specifically of UCTs for reducing poverty and vulnerabilities on the use of health services and health outcomes in LMICs. It will also synthesise existing evidence on the relative effectiveness of UCTs compared to CCTs in improving the use of health services and health outcomes in LMICs. Note that a second systematic review on the effect of UCTs for humanitarian assistance, the second broad type of UCTs, on the use of health services and health outcomes in LMICs will be conducted in tandem with this review. This systematic review evidence is particularly important, considering the relatively low costs and administrative ease of implementing UCTs.

Previous reviews have been conducted on the effect of CCTs on use of health services and health outcomes in LMICs (Gaarder 2010; Lagarde 2009), and of in-work tax credits (CCTs provisional on up-take or retention of paid employment) on health status in adults (Pega 2013). Several reviews have also combined various

financial credit interventions in assessing their impact on health. Lucas et al reviewed the impact of UCTs and CCTs on health outcomes in children in high-income countries (Lucas 2008). Boccia et al reviewed the effect of UCTs, CCTs and micro-finance interventions on risk factors for tuberculosis (Boccia 2012). Bassani et al reviewed the effect of UCTs, CCTs, voucher programmes and removal of user fees on the use of health services and health outcomes in children (Bassani 2013). Finally, three reviews of the effects of UCTs and CCTs on the incidence of HIV in LMICs have been conducted (Adato 2009; Heise 2013; Pettifor 2012). UCTs, CCTs and other financial interventions may differ in their effect on health in LMICs (Arnold 2011; Baird 2012; Fiszbein 2009; Robertson 2013; Schubert 2006), therefore the evidence should be reviewed separately for each of these types of interventions.

National governments, international organisations, nongovernmental organisations and civil society require systematic review evidence on the effectiveness of different types of cash transfers in improving the use of health services and health outcomes in LMICs to be able to prioritise, plan, cost and implement the most suitable and effective cash transfer type or types. This review will provide such systematic review evidence for UCTs. It will also provide such evidence on the relative effectiveness of UCTs versus CCTs.

OBJECTIVES

The primary objective of this review is to assess the effect of UCTs for reducing poverty and vulnerabilities on the use of health services and health outcomes in children and adults in LMICs.

Secondary objectives of the review are:

- to assess the effect of UCTs on relevant social determinants of health and health care expenditure in LMICs; and

- to assess the relative effectiveness of UCTs compared with CCTs with regards to improving use of health services and health outcomes in LMICs.

METHODS

Criteria for considering studies for this review

Types of studies

This review will include randomised and quasi-randomised (for example, alternate allocation or allocation by date of birth) controlled trials. We will also include controlled before-and-after studies, interrupted times series studies and cohort studies. Only con-

trolled before-and-after studies that meet the minimum methodological criteria defined in the Cochrane Effective Practice and Organisation of Care Group guidelines will be included (Cochrane EPOC 2012a): two or more sites in each intervention arm; intervention and control group are collected contemporaneously; and intervention and control sites are comparable (for example, we will exclude studies that compare two urban with two rural sites). We will include interrupted time series studies if they fulfil the Cochrane Public Health Group's recommended minimum methodological criteria (Cochrane PHG 2011): at least three time points before and after the intervention and a clearly defined intervention point. To be included in this review, cohort studies will at minimum: have three or more repeated measurements and have controlled (or attempted to control) for confounding (for example, through standardisation, stratification or matching) or reverse causation (for example, through instrumental variable analysis).

To assess the effectiveness of UCTs (primary review objective), we will include studies with two types of comparators. First, we will include studies comparing a group receiving an UCT with a group not receiving the UCT. Second, we will include studies comparing a group receiving an UCT with a group receiving a considerably smaller income amount from the UCT. For the latter type of studies, we will consult the review advisory group to establish whether the income amount from the UCT received by the comparison group is 'considerably smaller' than that received by the intervention group. To assess the relative effectiveness of UCTs versus CCTs (secondary review objective), we will also include studies comparing a group receiving an UCT with a group receiving a CCT (in a comparable context and setting).

Types of participants

This review will include both children (0 to 14 years) and adults (≥ 15 years) residing in a LMIC (as defined by the World Bank (World Bank 2014)).

Types of interventions

This review will include UCTs for reducing poverty or vulnerabilities, defined as:
- an in-hand cash payment (possibly paid directly into a bank account or provided in the form of a value card);
- unconditional (i.e., the cash transfer may have certain eligibility criteria, but does not have any (soft or hard) conditions attached to its receipt);
- noncontributory (i.e., the cash transfer is not a payment from a social insurance system that recipients have previously contributed to);
- provided by a formal institution (national governmental, international or nongovernmental organisation) or as part of a scientific study;
- provided with the goal of reducing income poverty or vulnerability (e.g., old age, disability or HIV infection);

- with the individual or household as the receiver unit (i.e., the cash transfers is not received by communities); and
- provided regularly (i.e., twice or more over a one-year period) and over extended periods of time (i.e., eligible families in theory continue receiving the cash transfer over time until they become ineligible).

We will include UCTs paid exclusively to women and those paid to all genders. We will include 'fuzzy' UCTs (Baird 2013), as long as they were explicitly designed to be unconditional. If 'fuzzy' UCTs are included, we will describe the contexts that produced potential *de facto* conditions (e.g., administrative linking of the cash transfer) or perceived conditions (e.g., messaging around the cash transfer) in the review. We will exclude UCTs for assistance in humanitarian disasters (Arnold 2011; Garcia 2012; Global Humanitarian Assistance 2012), because they address different causal pathways and therefore may have a different effect on use of health services and health outcomes. If a study is excluded due to the intervention being a CCT or a 'fuzzy' UCT that is a CCT by design, then we will note this as a reason for study exclusion in the review. We will include UCTs that are standalone interventions, but we will exclude UCTs provided in combination with or alongside other interventions.

Types of outcome measures

We chose outcomes to ensure comparability with the Lagarde 2009 review of the impact of CCTs on the use of health services and health outcomes in LMICs. If a study reports measures for several included outcomes, then we will include one measure for each of the reported outcomes in the review. If a study reports multiple measures for the same outcome, then we will prioritise the measure that is most consistent with the measure reported in the other included studies. We will include studies reporting outcomes for any time period.

Primary outcomes

The primary outcomes of the review will be:
- use of health services; and
- health outcomes.

Regarding the use of health services, the review will include objective and subjective measures of the final consumption of health services. These measures can be either administrative records or survey data of change in the use of health facilities or services, such as number of visits to preventive services, immunisation rates and hospitalisation rates. We will consider neither the distance travelled, nor the travel time required to access the facilities or services. Included health outcomes will be mortality, morbidity, nutritional outcomes and anthropometric measures, whether measured subjectively as rated by a clinician, patient or carer (e.g., self rated health or diagnosis of mental or physical condition) or measured objectively (e.g., obesity or infection with a disease). We will also include potential harms (e.g., alcohol use, tobacco use and consumption of unhealthy foods).

Secondary outcomes

The secondary outcomes of the review will be:
- relevant social determinants of health (e.g., income, education, employment and social cohesion); and
- health care expenditure.

With regards to health care expenditure, we will only include measures of direct and indirect costs borne by the health care recipient.

Search methods for identification of studies

Electronic searches

Academic databases

Appendix 1 presents the search strategy for Ovid MEDLINE(R) 1946 to Present with Daily Update, developed based on the previous Lagarde 2009 and Pega 2013 systematic reviews of the effect of cash transfer interventions on health. We will use this strategy to search the following databases for relevant records:
- Cochrane Public Health Group Specialised Register;
- Cochrane Central Register of Controlled Trial (CENTRAL) (*The Cochrane Library*, current issue);
- Ovid MEDLINE(R) 1946 to Present with Daily Update (1946 to present);
- EMBASE (1947 to present);
- Academic Search Premier (1990 to present);
- Business Source Complete (1990 to present);
- CINAHL (1937 to present);
- EconLit (1969 to present);
- 3IE database (1990 to present);
- PsycINFO (1920 to present);
- PubMed (1920 to present);
- Scopus (1995 to present);
- Social Sciences Citation Index (1955 to present);
- Sociological Abstracts (1952 to present);
- The Campbell Library: The Campbell Collaboration (*The Campbell Library*, current issue);
- TRoPHI (1920 to present); and
- WHOLIS (1948 to present).

We will adapt the subject heading terminology and syntax of search terms according to the requirements of the individual databases. We will search records written in any language. When we are nearing completion of the review, we will search the PubMed database

Unconditional cash transfers for reducing poverty and vulnerabilities: effect on use of health services and health outcomes in low- and middle-income countries (Protocol)

7

for the most recent publications (e.g., e-publications ahead of print) over the last six months.

Grey literature databases

We will search the following grey literature databases:
- ProQuest Dissertations & Theses Database;
- System for Information on Grey Literature in Europe - Open-Grey (www.opengrey.eu/);
- The Directory of Open Access Repositories - OpenDOAR (www.opendoar.org/);
- EconPapers (www.econpapers.repec.org);
- Social Science Research Network - SSRN eLibrary (www.ssrn.com/); and
- National Bureau of Economic Research (www.nber.org/).

Internet search engines

We will screen the first 30 hits on the Internet search engines Google Scholar and Scirus, using terms similar to those used for searches of the bibliographic databases.

Targeted Internet searching of key organisational websites

We will search the websites of key international organisations and donors, especially the:
- African Development Bank (www.afdb.org);
- Asian Development Bank (www.adb.org);
- European Bank for Reconstruction and Development (www.ebrd.com);
- Inter-American Development Bank (www.iadb.org);
- World Bank (www.worldbank.org); and
- United Kingdom Department for International Development (www.gov.uk/government/organisations/department-for-international-development).

We will not conduct a targeted search of the website of the World Health Organization, because we will search WHOLIS, which comprehensively indexes publications from the organisation.

Searching other resources

Academic journals and reference lists of key articles

We will handsearch the issues published over the last 12 months of the three journals with the highest number of included studies and reference lists of all relevant records for additional relevant studies.

Advisory group

An advisory group of research experts and policy makers in the area of cash transfers and health has been convened to guide the development of this protocol. This advisory group will also be convened to guide the review. During the selection stage, should we find studies that compare an intervention group with a comparison group receiving a smaller amount of income from an UCT, then we will request the advisory groups' advice by email regarding whether the difference in income amount between the intervention and comparison groups can be considered 'considerable'. During the data synthesis stage, we will provide the advisory group with a list of all relevant studies identified by the search and request that group members alert us by email to any other studies (whether published or unpublished, whether completed or ongoing) they are or become aware of while the review is being conducted.

Data collection and analysis

Selection of studies

The database search for relevant literature will be assisted by a research librarian and will return the titles and abstracts of each record. One author will initially screen the title and abstract of each identified record for relevance, eliminating obviously irrelevant records (FP, SW, SYL, RP). We will screen the full text of each record without an abstract to establish its relevance. We will identify and exclude duplicate records.

At least two authors will then independently screen the abstract of each potentially relevant record in depth for eligibility (FP, SW, SYL, RP). We will retrieve records selected for full-text screening. If a retrieved record is written in a language other than those spoken by the authors (Dutch, English, French, German, Italian and Spanish), then it will be translated into English.

Two authors (FP, SW, SYL, RP) will then independently establish whether a record undergoing full-text screening meets the inclusion criteria for the review. A third author (SKL, RS) will resolve disagreements about the inclusion of a record. We will document the reasons for excluding key records that have undergone full-text screening in the table of 'Characteristics of excluded studies'.

Data extraction and management

Two review authors (FP, SW, SYL, RP) will independently extract data for each study included in the review. We will use a modified version of the data extraction form recommended by the Cochrane Public Health Group (Cochrane PHG 2011). Before the authors commence data extraction, they will pilot the data extraction form to ensure that data are extracted in a standardised way. If discrepancies arise between the data extraction forms of the two authors, a third author (SKL, RS) will resolve these.

At a minimum, we will extract data for the following categories: study eligibility (including study characteristics such as study design, participants, intervention, duration of intervention and outcomes measures); study details (including study aims and methods); intervention groups (including group names); outcomes and results (including for subgroups).

Where information is available from the record on the context, implementation, cost and sustainability of the UCT or comparator (i.e., either lower magnitude UCT or CCT), we will also extract this. However, where this information is not available from the record, but reference is made to other records for this information, we will extract the information from the other records. The types of contextual information we will extract will include design features of the UCT such as its population coverage (e.g., as measured by the percentage of the total population who receive the UCT) and its generosity (e.g., as assessed by the percentage contribution of an average income from the UCT to the national average total income). We will report this information on the context, implementation, cost and sustainability of the UCT in the table of 'Characteristics of included studies'.

We will also extract data on key socio-demographic characteristics of participants at baseline and at the endpoint within the PROGRESS framework (Cochrane PHG 2011), for the purpose of assessing the interventions' equity impact. The extracted socio-demographic characteristics will include education, ethnicity, gender, gender identity, occupation, place of residency, sexual orientation, socio-economic status, social status and religious affiliation. Additionally, we will incorporate the Cochrane-Campbell Methods Group Equity Checklist in our data extraction form (Campbell & Cochrane Equity Methods Group 2011). We will also record whether the intervention comprised dedicated strategies to support disadvantaged populations.

We will extract data on potential measured confounders and the methods for confounder control. We will extract information on the comparator (i.e., definitions of the control group). We will enter, store and manage the extracted data in Review Manager (RevMan) 5.2 (RevMan 2012).

Assessment of risk of bias in included studies

Two authors (FP, SW, SYL, RP) will independently assess the risk of bias in the included studies. Where differences arise, all authors will jointly discuss these until they reach agreement (FP, SW, SYL, RP, SKL, RS).

We will apply The Cochrane Collaboration's 'Risk of bias' tool to assess the risk of bias in included randomised and quasi-randomised controlled trials (Higgins 2011). We will use the Cochrane Effective Practice and Organisation of Care 'Risk of bias' criteria to assess the risk of bias in controlled before-and-after and interrupted time-series studies (Cochrane EPOC 2012b). For studies that have a separate control group (i.e., controlled before-and-after studies), we will add to these criteria an item assessing

the risk of bias from confounding.

No credible, standardised tool for assessing the risk of bias in cohort studies currently exists (Sanderson 2007). However, as we have done previously (Pega 2013), we will follow the best practice recommendation to assess the specific features of cohort studies and the extent to which these may introduce bias (Centre for Reviews and Dissemination 2009; Appendix 3 in Joyce 2010). At minimum, we will assess the risk of bias in the following features: sampling strategy; response rates; sample representativeness; attrition; participant allocation; exposure assessment; outcome assessment; reporting and control of key confounders and control of reverse causation.

We will assess and report risk of bias at the outcome level, first for each outcome for each study (i.e., risk of bias of an individual study) and then for each outcome across all studies (i.e., risk of bias in the whole body of evidence).

Measures of treatment effect

In studies with separate control groups (i.e., randomised and quasi-randomised controlled trials and controlled before-and-after studies) and cohort studies, for dichotomous and ordinal health outcomes, treatment effects are likely to be measured as risk ratios, odds ratios or risk differences between treatment and control groups. As recommended by the Cochrane Public Health Group (Cochrane PHG 2011), we will prioritise and report risk ratios. If risk ratios are not provided, but data that enable calculation of the risk ratio are presented, then we will calculate risk ratios. If data that enable calculation of a risk ratio are not provided in a study's records, we will contact the principal authors of the study records by email (using the email addresses provided in the respective record) or phone (using the phone directories of the organisations the authors are affiliated with). We will request a risk ratio measure or the information to calculate the risk ratio from the authors. If we cannot establish the risk ratio, we will report the measure of treatment effect that is provided in the study records. For continuous outcome variables, treatment effects are likely reported as mean differences between treatment and control groups (e.g., the outcome measures from before the implementation of an UCT minus the outcome measures after the implementation of the UCT). We will report these mean differences for these outcomes. In studies without separate control groups (i.e., interrupted time series), for dichotomous, ordinal and continuous outcomes, treatment effects are likely to be measured as change in level and slope before and after the UCT implementation within individuals. We will report these measures in the review.

If adjusted (for confounding) and unadjusted treatment effect measures are presented for an outcome, we will report the adjusted treatment effect measures. If a study presents only unadjusted treatment effect measures, as long as between-group differences in covariates at baseline and potential confounding variables are reported, we will adjust the treatment effect measures for these

Unconditional cash transfers for reducing poverty and vulnerabilities: effect on use of health services and health outcomes in low- and middle-income countries (Protocol)

9

variables. If the information required for adjusting for these variables is not presented in a study's records, we will contact the principal study authors by email or phone with the request to provide adjusted treatment effect measures.

If studies present intention-to-treat effect estimates, then we will prioritise these (e.g., over average causal treatment effect estimates).

We will provide 95% confidence intervals for treatment effect measures, if feasible. If a study uses different scales when reporting the same outcome, then the review will report the standardised mean difference.

Unit of analysis issues

We will screen all studies for unit of analysis issues from randomisation (or non-random allocation) of participant clusters, treatment with multiple interventions and multiple observations for the same outcome at different time points.

If a study randomises (or allocates) participant clusters (e.g., cluster-randomised trials) without controlling for clustering effects in the analysis, we will request individual-level data from the principal study author by email or phone and, where feasible, reanalyse the data to control for the clustering. If a study uses multiple observations for the same outcome at different time points, we will also request individual-level data from the principal study author and reanalyse these data, using all observations over the whole follow-up period for each participant.

If studies with multiple intervention groups compare multiple possible intervention group pairings (e.g., 'Group A versus Group B', 'Group A versus Group C' and 'Group B versus Group C'), then the same intervention group (e.g., 'Group A') will not appear twice in meta-analyses (e.g., if 'Group A versus Group B' is included, then 'Group A versus Group C' is excluded).

We will exclude studies with unit of analysis issues for which we are unable to retrieve individual-level data for reanalysis from the data synthesis.

Dealing with missing data

Selective or incomplete measuring or reporting could result in relevant information on a study (e.g., methods, outcomes and/or results) being missing. For example, data on participants could be missing because these participants were (randomly or systematically) lost to follow-up or not reported. Another example is that information required for subgroup analyses, such as participant age, gender or level of income, may not be reported (or measured). We will request any missing relevant information from principal study authors by email or phone.

Moreover, data could be 'not missing at random' (Little 1987), whether due to publication bias, selective reporting bias, systematic loss to follow-up or systematic exclusion of individuals from studies. We will identify data 'not missing at random' and request these from principal study authors via email or phone.

If we cannot retrieve missing data, we will report the available data, but consider the potential impact of the missing data on the review findings in the 'Discussion' section.

Assessment of heterogeneity

Studies that will be included in this review may be methodologically and clinically heterogenous. Methodological heterogeneity can occur due to studies using different study designs (e.g., randomised controlled trial versus cohort study), experiencing different risks of bias and employing different statistical methods. Clinical heterogeneity occurs when the review includes studies where the types of interventions, settings and included participants differ between studies. If studies included in this review are found to be methodologically and clinically heterogenous, then we expect that they will also be statistically heterogenous.

We will not meta-analyse studies that differ considerably in their study designs, participants, interventions (including intervention design (e.g., exposure frequency and duration), context and implementation) and outcomes.

If two or more studies report the same outcome and are sufficiently homogenous in their design, participants and intervention to potentially be meta-analysed, we will assess the statistical heterogeneity of these studies in order to establish the feasibility (or not) of meta-analysis. To quantify the level of statistical heterogeneity in these studies, we will calculate the I^2 statistic using RevMan 5.2 (RevMan 2012).

Assessment of reporting biases

Publication bias could occur if this review does not comprehensively identify all studies that are eligible for inclusion in the review. For example, studies with unwelcome or null findings may not have progressed to publication in the academic literature and may therefore may not be indexed in the databases that we will search. We will employ a comprehensive search strategy to avoid missing eligible studies. In addition to academic databases, we will search databases of grey literature, dissertations, theses and conference proceedings, as well as the websites of key institutions, the Cochrane Central Register of Controlled Trials (CENTRAL) and the Cochrane Public Health Group Specialised Register. Additionally, we will ask the review advisory board members and other key experts to identify unpublished studies. We will include any eligible unpublished studies that we identify in the review.

The review will also include articles written in any language to minimise the risk of language bias.

If 10 or more eligible studies reporting the same outcome are identified, we will produce a funnel plot and test for funnel plot asymmetry to assess the presence of publication bias for the outcome. We will establish the most suitable test for funnel plot asymmetry, using the dedicated recommendations given by The Cochrane Collaboration (Higgins 2011).

Unconditional cash transfers for reducing poverty and vulnerabilities: effect on use of health services and health outcomes in low- and middle-income countries (Protocol)

10

Data synthesis

If a meta-analysis of studies reporting the same outcome is feasible, we will conduct the meta-analysis using the RevMan 5.2 software (RevMan 2012). We will apply a random-effects model to address heterogeneity. We will not adjust effect size estimates in any way. If studies are too heterogenous to permit meta-analysis, we will synthesise the studies' results narratively. In this narrative synthesis, we will report results separately for each outcome. To avoid introducing bias, we will not emphasise any one study in the review. We will assess the quality of the evidence in two stages: at first for each outcome and then for the entire body of evidence. We will apply the GRADE considerations, assessing quality based on factors such as study limitations, consistency of effect estimates, imprecision, indirectness and publication bias (Cochrane PHG 2011).

We will present results for the key measure of each of the primary outcomes of the review (use of health services, health outcomes) in a 'Summary of findings' table. If a study presents alternative outcome measures, we will list these in the notes to the 'Summary of findings' table. We will present secondary outcomes measures in a second 'Summary of findings' table. These tables will present, for each primary or secondary outcome, the number of included studies and participants, the treatment effect estimate (or estimates) and the assessment of the overall quality of the body of evidence for the outcome.

Subgroup analysis and investigation of heterogeneity

Provided that suitable data are available, we will conduct subgroup analyses by conditionality to determine the relative effectiveness of UCTs (including 'fuzzy' UCTs) versus CCTs. We will also conduct subgroup analyses by any UCT versus no UCT/CCT and generous UCT versus less generous UCT to determine whether the use of different comparators results in different results. Furthermore, we will also conduct subgroup analyses on the meta-analyses or narrative syntheses of the primary outcomes by age (children and adults), gender (female and male), level of income (e.g., total personal or household annual income after tax) and WHO region (Africa, Americas, Eastern Mediterranean, Europe, South-East Asia and Western Pacific). These subgroup analyses will identify any differential impacts of UCTs by these key dimensions. For example, the subgroup analyses by WHO region will enable identification of potential differences in the direction and size of the effect of treatment with an UCT between different geographical regions (e.g., African countries versus South-East Asian countries). Therefore, the subgroup analyses will provide important equity perspectives. If the subgroup analyses include a sufficiently large number of studies to conduct meaningful statistical testing, we will perform t-tests and Chi^2 tests to determine the statistical significance (or not) of between-subgroup differences in treatment effects.

Sensitivity analysis

We will conduct sensitivity analyses of meta-analyses in order to evaluate whether the sizes of the combined effect estimates are robust across studies that differ in their methodological quality. For this purpose, we will re-run the meta-analysis with only high-quality studies assessed as carrying a low risk of bias.

If we will combine parallel and cross-over randomised trials in a meta-analysis, we will perform a sensitivity analysis that will exclusively include the parallel trials. There is some evidence that even small amounts of income from cash transfers (at least CCTs) may have large effects on social outcomes (e.g., Baird 2011; Filmer 2011), therefore we will conduct a sensitivity analysis that will include only studies with no UCT as the comparator.

Unconditional cash transfers for reducing poverty and vulnerabilities: effect on use of health services and health outcomes in low- and middle-income countries (Protocol)   11

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* *Indicates the major publication for the study*

APPENDICES

Appendix 1. Appendix 1: Search strategy for Ovid MEDLINE(R) 1946 to Present with Daily Update

Intervention terms

1. maternal welfare/
2. public policy/
3. social welfare/
4. exp social security/
5. (social adj (assistance or polic$ or welfare or insurance$ or protection)).ti,ab.
6. public assistance.ti,ab.
7. family policy.mp
8. ((financial or cash or pay$ or monetary or money) adj3 (transfer$ or measure$ or incentive$ or allowance$ or exclu$ or reform$ or gain$ or credit$1 or benefit$1)).ti,ab
9. or/1-8

Study terms

10. randomized controlled trial/
11. random$.ti,ab.
12. random allocation/
13. placebos/
14. placebo$.ti,ab.
15. single-blind method/
16. double-blind method/
17. ((single or double or triple or treble) adj blind$).ti,ab.
18. control groups/

19. exp clinical trial/
20. comparative Study/
21. intervention studies/
22. exp cohort studies/
23. evaluation studies/
24. program evaluation/
25. (time adj series).ti,ab.
26. quasi-experiment$.ti,ab.
27. (pre test or pretest or pre-intervention or post test or posttest or post-intervention).ti,ab.
28. controlled before.ab,ti.
29. independent panel.ti,ab.
30. panel stud$.ti,ab.
31. intervention$ stud$.ti,ab.
32. "before and after".ti,ab.
33. repeat$ measure$.ti,ab.
34. evaluat$ stud$.ti,ab.
35. compari$ stud$.ti,ab.
36. (trial or follow-up assessment$ or follow up assessment$ or groups).ti,ab
37. ((intervention or interventional or process or program) adj8 (evaluat$ or effect$ or outcome$)).ab,ti.
38. (program or programme or secondary analys$).ti,ab
39. ((evaluat$ or intervention$ or treatment$) and (control$ or study or program$ or comparison or comparative)).ab,ti.
40. or/10-39

Country terms

41. Developing Countries/
42. Medically Underserved Area/
43. exp Africa/ or exp "Africa South of the Sahara"/ or exp Asia/ or exp South America/ or exp Latin America/ or exp Central America/
44. (Africa or Asia or South America or Latin America or Central America).tw.
45. (American Samoa$ or Argentin$ or Beliz$ or Botswana$ or Brazil$ or Bulgaria$ or Chile$ or Comoro$ or Costa Rica$ or Croatia$ or Dominica$ or Equatorial Guinea$ or Gabon$ or Grenada$ or Hungar$ or Kazakh$ or Latvia$ or Leban$ or Libya$ or Lithuania$ or Malaysia$ or Mauriti$ or Mexic$ or Micronesia$ or Montenegr$ or Oman$ or Palau$ or Panama$ or Poland or Polish 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 Turk$ or Urugua$ or Venezuel$ or Yugoslavia$).sh,tw. or Guinea$.tw. or Libia$.tw. or Mayotte.tw. or Northern Mariana Island$.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.
46. (Albania$ or Algeria$ or Angol$ or Armenia$ or Azerbaijan$ or Belarus$ or Bhutan$ or Bolivia$ or "Bosnia and Herzegovina" or Bosnian$ or Cameroon$ or China or Chinese or Colombia$ or Congo$ or Cuba$ or Djibouti$ or Dominican Republic$ or Ecuador$ or Egypt$ or El Salvador$ or Fiji$ or "Georgia (Republic)" or Goergian$ or Guam$ or Guatemal$ or Guyana$ or Hondur$ or Indian Ocean Island$ or Indonesia$ or Iran$ or Iraq$ or Jamaica$ or Jordan$ or Lesotho or "Macedonia (Republic)" or Marshall Island$ or Micronesia$ or Middle East$ or Moldova$ or Morocc$ or Namibia$ or Nicaragua$ or Paraguay$ or Peru$ or Philippin$ or Samoa$ or Sri Lanka$ or Suriname$ or Swaziland$ or Syria$ or Thai$ or Tonga$ or Tunisia$ or Turkmen$ or Ukrain$ or Vanuatu).sh,tw. or Bosnia$.tw. or Cape Verd$.tw. or Gaza.tw. or Georgia$.tw. or Kiribati$.tw. or Macedonia$.tw. or Maldives.tw. or Marshall Island$.tw. or Palestin$.tw. or Syrian Arab Republic$.tw. or West Bank.tw.
47. (Afghan$ 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 Kyrgyz$ or Laos or Laot$ or Liberia$ or Madagascar or Malagasy or Malawi$ or Mali$ or Mauritania$ or Melanesia$ or Mongolia$ or Mozambi$ 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 Uzbek$ or Vietnam$ or Yemen$ or Zambia$ or Zimbabw$).sh,tw. or Burm$.tw. or Congo$.tw. or Lao.tw. or North Korea$.tw. or Salomon Island$.tw. or Sao Tome.tw. or Timor$.tw. or Viet Nam.tw.
48. ((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.

49. (lmic or lmics).tw.
50. or/41-49
51. 10 and 40 and 50

CONTRIBUTIONS OF AUTHORS

Pega led all aspects of the writing and development of this protocol, including having the idea for and conceptualising the review topic, developing the search strategy and writing the first draft. Walter, Sze, Pabayo, Lhachimi and Saith contributed to the protocol writing and development.

DECLARATIONS OF INTEREST

Pega: None known. Walter: None known. Liu: None known. Pabayo: None known. Lhachimi: None known. Saith: None known. Oxford Policy Management has been involved in the implementation and evaluation of a number of cash transfer schemes in low- and middle-income countries.

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Unconditional cash transfers for reducing poverty and vulnerabilities: effect on use of health services and health outcomes in low- and middle-income countries (Protocol)

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16