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Agricultural input subsidies for improving productivity, farm income, consumer welfare and wider growth in low- and middle-income countries

A systematic review

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Agricultural input subsidies for improving productivity, farm income, consumer welfare and wider growth in low- and middle-income countries: a systematic review

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Conflict of interest statement

Chirwa and Dorward are engaged in evaluations of the Malawi Farm Input Subsidy Programme and have published on this and more widely on input subsidy impacts. Any work of theirs was independently assessed by other members of the team. There are no other conflicts of interest to declare of which we are aware.

Executive summary

Background

In recent decades, agricultural productivity in low- and lower-middle-income countries, particularly in Africa, has fallen increasingly behind that of upper middle-income countries. Adequate use of agricultural inputs such as improved seeds and inorganic fertilisers has been identified as one way of enhancing agricultural productivity. However, these inputs can be financially unaffordable or unattractive to many poor farmers in developing countries.

Agricultural input subsidies aim to make inputs available to users at below market costs as a way of incentivising adoption, increasing agricultural productivity and profitability, increasing food availability and access and ultimately reducing poverty and stimulating economic growth. They were common in poor rural economies in the 1960s and 70s. Their use declined in the 1980s and 90s, but recent years have witnessed a resurgence of interest and investment, mainly in Africa. There remains considerable debate regarding the effectiveness and efficiency of their use and the conditions under which they may or may not work.

Objectives

This systematic review explores the effects of agricultural input subsidies on agricultural productivity, farm incomes, consumer welfare and wider growth in low- and lower-middle-income countries. This research question is divided into the following primary and secondary research questions:

1. What are the effects of agricultural input subsidies on agricultural productivity and beneficiary incomes and welfare?
2. What are the effects of agricultural input subsidies on consumer welfare and wider economic growth?

Search methods

We carried out a systematic search for includable studies in a wide range of sources and using a variety of search methods. We searched academic and online databases, carried out forwards and backwards citation tracking of included studies, and consulted experts. There were no restrictions on publication year, type or language, though searches were undertaken in English. The main searches were completed in November 2013. However, we incorporated additional papers after this date where they became available before our analysis was completed.

Selection criteria

To be included, studies had to examine the effects of agricultural input subsidies, including products, machinery, seeds or fertilisers, on farmers, farm households, wage labourers or food consumers in low- or lower- middle-income countries. Eligible comparisons included no active agricultural input subsidy intervention, wait-list, alternate input subsidy intervention, or other interventions providing access to inputs. We included experimental or quasi-experimental studies to address our primary research question regarding primary outcomes of adoption, productivity and farm income. We included econometric modelling studies to address our secondary research question on consumer welfare and wider economic growth outcomes. Studies were assessed by a single reviewer at both title and abstract level and full-text level. A second reviewer then checked screening decisions taken at full-text level.

Data collection and analysis

We extracted a range of data including bibliographic details, outcomes, time period covered, study design and outcomes data. For our primary research questions we synthesised evidence from experimental and quasi-experimental studies using meta-analysis, meta-regression analysis and a qualitative synthesis of relevant implementation and contextual factors. For our secondary research questions we synthesised evidence from modelling studies narratively and displayed effects in scatter plots where possible.

Main results

We identified 15 experimental and quasi-experimental studies that assess the effectiveness of agricultural input subsidies on adoption, yield and farm incomes. We also identified 16 studies that use computable models that simulate the effect of agricultural input subsidies on measures of consumer welfare and wider growth.

Overall, the evidence base is limited with a disproportionate focus on subsidy programmes in sub-Saharan Africa and in particular on the case of Malawi. Most studies also have a focus on fertilisers and/or seeds rather than other types of inputs.

We undertook meta-analysis of experimental and quasi-experimental studies to examine the effect of agricultural input subsidies on adoption, productivity, household income and poverty. The findings for primary outcomes are as follows:

- *Adoption*: Meta-analysis of seven experimental and quasi-experimental studies indicates an increase in adoption by 0.23 standard deviations (SD) (95% confidence interval (CI) [0.08, 0.38]) for farmers receiving agricultural input subsidies versus those not receiving agricultural input subsidies.
- *Productivity*: Across five studies, which were able to account adequate for confounding, there is an increase in yields of 0.11 SD (95% CI [0.05, 0.18]) for agricultural input subsidy recipients, compared to non-recipients.
- *Farm income*: Recipient farmer income, measured by household expenditure and income and crop income and revenue from four studies, increases by 0.17 standard deviations (SD) (95% CI [0.10, 0.25]), over that of non-recipients.
- *Poverty*: Only two studies report the effects of agricultural input subsidies on poverty, making it difficult to draw any clear conclusion.

Meta-regression found no association (positive or negative) between subsidy size and agricultural outcomes. However, narrative synthesis of data relating to programme implementation, input subsidy delivery mechanisms, farmer take-up and usage of inputs, leakage of vouchers or inputs, and other associated factors indicates several points at which the theory of change for input subsidies breaks down. Subsidy vouchers do not always reach farmers in the quantities intended. Furthermore, where they do reach farmers they are not always used, and as a result providing subsidised inputs may not necessarily increase the amount of inputs used by farmers in absolute terms.

We also synthesised data from simulation modelling studies of consumer welfare- and economic growth-related outcomes including staple food prices and consumption, labour demand and agricultural wages, poverty incidence and gross domestic product (GDP). Results suggest that the relationships between the size of the change in subsidy and the outcomes of interest to be in line with our theory of change.

However, analysis of modelling studies also indicates that factors such as how subsidies are funded, world input prices and beneficiary targeting can all play important roles in determining the effectiveness of input subsidies and their relative value compared to alternative policy options for agricultural development and poverty alleviation.

Conclusions

Overall, this review finds generally positive results for both primary and secondary outcomes across our theory of change. Included studies provide evidence linking fertiliser and seed subsidies to increased use of the subsidised inputs, higher agricultural yields and increased income among farm households, while the limited evidence relating to effects on poverty make it difficult to draw any clear conclusion. Models simulating subsidy effects show the introduction or increase in subsidies generally results in positive effects for consumers and wider economic growth.

However, the review also indicates the importance of programme implementation and wider contextual factors. A narrative synthesis of data from experimental and quasi-experimental studies finds implementation problems, with inputs not always made available or used as planned. Modelling studies indicate that the positive effects of subsidies are sensitive to changes in contextual factors endogenous and exogenous to the subsidy itself.

There are also a number of implications for research. The review finds a relatively small evidence base of both experimental and quasi-experimental studies, and econometric modelling studies. The evidence base focuses on a limited number of countries and evidence from a wider set of contexts where subsidies are used would be welcome.

Mixed-methods, theory-based impact evaluations can explore different levels of subsidies and unpack outcomes and assumptions along the causal chain, for different sub-groups of beneficiaries. Simulation models studies should make more use of rigorous evidence from experimental and quasi-experimental studies in determining coefficients used for household behaviour and the micro-economic effects of subsidies. Furthermore, including multiple simulations in modelling studies to offer a range of different possible scenarios may be of more use to policy makers rather than simple ‘with or without subsidy’ comparisons. Researchers should ensure that they more clearly report methodological approaches, relevant statistical information and the type and size of input subsidy implemented or modelled.

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1. Background

1.1 The problem, condition or issue

In recent decades, agricultural productivity in low- and lower-middle-income countries, particularly in Africa, has fallen increasingly behind that of upper middle-income countries. The agricultural sector in most African countries continues to rely on farming systems where smallholder farmers are reliant on family resources for investment (NEPAD, 2013). While the agricultural sector in Africa has seen increasing growth in output, and continues to be the main driver of economic growth in many countries in the region, productivity remains low compared to other developing regions.

Agricultural growth that has occurred in the region has mainly been due to extensification, increasing use of more marginal lands and the mobilisation of more labour. This has resulted in agricultural labour and per hectare productivity remaining low despite productivity growth.

As NEPAD (2013) notes, cereal yields in Africa are less than half of those obtained in Asia. Substantial agricultural intensification has not occurred in the region. According to the World Bank (2009), for instance, cereal yields per hectare moved from a little over 1 ton per hectare in 1960 to 4.5 tons per hectare in 2005 in South Asian countries, compared to about 0.9 tons per hectare in 1960 to a little over 1 ton per hectare in 2005 in sub-Saharan Africa. Between 1961 and 2009, cereal yields in sub-Saharan Africa grew by 0.95 percent, compared to 2.40 percent in East Asia, 1.95 percent in Latin America and Caribbean and 1.95 percent in South Asia (Chirwa & Dorward, 2013).

A broad range of factors are thought to contribute to the low levels of agricultural productivity in sub-Saharan Africa. Wiggins and Leturque (2010) provide a helpful summary of the main explanations posited for the region's poor agricultural performance, taking into account the considerable inter-regional variation. Among the issues identified by the authors are limited production potential due to liquidity and labour constraints, unfavourable geographical and environmental conditions and environmental degradation, which they link to a lack of technical innovation. They also point to government and market failures (the former involving policy that deters investors, resulting in too little investment, the latter failing to deliver credit and input services and overcome poverty traps) and unfavourable global market forces arising from OECD subsidies for their own agricultural producers, unfair international trade rules and limited demand for farm output.

There is a broad consensus in the literature that a key explanatory factor for sub-Saharan Africa's low agricultural productivity, in comparison to other regions of the world, is the region's low rates of fertiliser use. For instance, between 2002 and 2009, nitrogen application averaged 5.9 kg per hectare in sub-Saharan Africa compared to 106.0 kg per hectare in Asia and 36.6 kg in South America (Chirwa & Dorward, 2013). NEPAD (2013) identifies some of the factors driving these low rates of use; among them credit constraints for farmers, increasing costs of key inputs and a lack of technical knowledge regarding input use on the part of farmers.

According to the FAO, 12.5 percent of the world's population are undernourished (FAO, 2013). There is an urgent need to improve food security. Increased agricultural productivity has been identified as an important means for improving food insecurity and for stimulating economic growth in agriculture-based economies. Adequate use of improved agricultural inputs (such as improved seeds and inorganic fertilisers) can help increase productivity in low productivity areas of the developing world (Buringh & Dudal, 1987; Gordon, 2000; Hazell et al., 2007; Ajah & Nmadu, 2012). However, there is a strong concern that the inputs and technologies needed to achieve increased productivity are financially unaffordable or unattractive to many poor farmers in developing countries (e.g. Wiggins & Brooks, 2010).

1.2 The intervention

Agricultural input subsidy interventions aim to make particular inputs, most commonly fertilisers and seeds, available to potential users at below market costs as a way of incentivising adoption, increasing agricultural productivity and profitability and ultimately reducing poverty and stimulating economic growth among farm households.

Examples include tax exemptions, free provision of agricultural inputs, price subsidies where inputs are made available at lower prices to consumers or, as is common in many contemporary contexts, the provision of vouchers to farm households that they are free to redeem in local markets. Agricultural inputs that can be subsidised include seeds, fertilisers, pesticides, herbicides, animal feed, drugs, machinery and fuel. Subsidies are most often only targeted at a few inputs and are in many cases limited to fertilisers or seeds.

Subsidies usually cover only a small number of these inputs, for instance seed and fertiliser packs in Malawi or fertiliser subsidies in Indonesia, and may only target producers of particular staple or cash crops. They can take a variety of forms, from free provision of the actuals goods (fertilisers, seeds, power, etc.), to vouchers redeemable through commercial markets. The size of subsidies also varies widely across contexts. This may be due to several factors including attempts to limit market distortions or user dependency, or may simply be due to resource constraints on the part of government. Subsidy schemes are often targeted at those least able to purchase inputs at market prices, or seek to otherwise target particular users depending on the intended objectives of the subsidy (Dorward & Chirwa, 2014).

The underlying assumption of subsidy schemes is that by reducing the costs of the use of fertiliser and other inputs, their use will increase, thereby leading to production increases, particularly if the subsidised inputs are used by households facing input market failure (Druilhe & Barreiro-Hurlé, 2012).

Agricultural input subsidies were common in poor rural economies in the 1960s and 70s, but conventional wisdom, especially among international lending institutions such as the World Bank and IMF, deemed them ineffective by the 1980s and 90s and their use declined (Dorward, 2009). However, in recent years, there has been a resurgence of interest and investment, mainly in Africa, in so-called 'smart subsidies'. These subsidies seek to maximise the multiple benefits of subsidies to different stakeholders while minimising their distortionary effects on *inter alia* efficient commercial market operation and development (Morris et al., 2007). The key features of smart subsidies include: promotion of fertilisers as part of a wider agricultural strategy; leveraging the private sector through the use of redeemable vouchers that can promote competition among input suppliers, giving farmers market choices; planning some form of exit strategy into the scheme from its inception; and, a focus on ensuring sustainability and promoting pro-poor economic growth (Morris et al., 2007).

There remains, however, considerable debate among policy makers and analysts regarding the effectiveness and efficiency of investments in agricultural input subsidies and the conditions under which they may or may not work (Wiggins & Brooks, 2010; Kilic et al., 2013; Pauw & Thurlow, 2014).

1.3 How the intervention might work and theory of change

The theory of change for intervening in input markets through input subsidies is that subsidies will lead to incrementally increased use of subsidised inputs, which will in turn lead to increased agricultural productivity and production. This will result in increases in incomes for farm households as well as wider effects on consumer welfare through lower food prices, increasing demand for labour, higher wages and incomes, reductions in poverty and increases in overall economic growth (Figure 1).

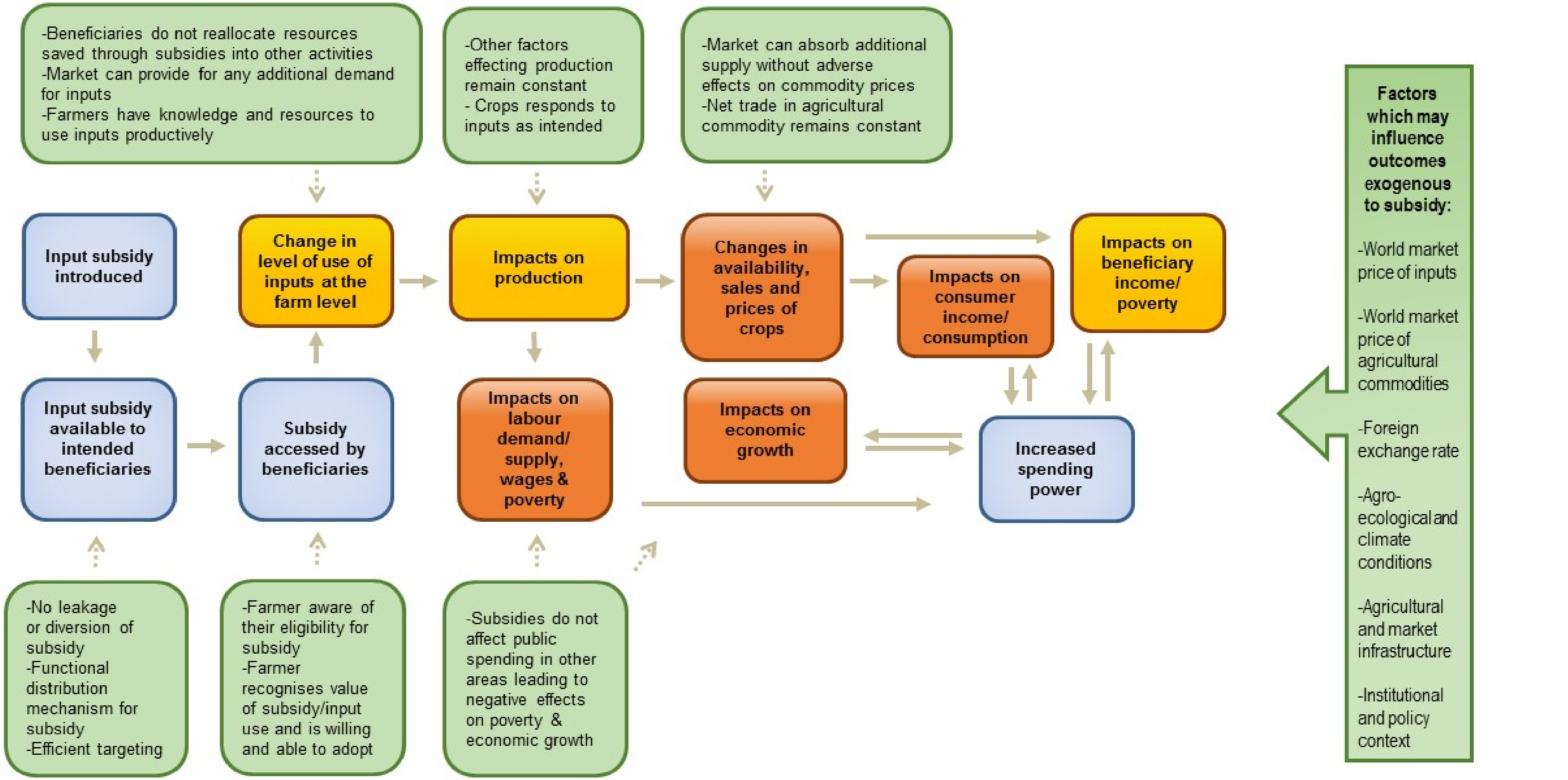
The effect of a subsidy programme on these outcomes is itself affected by changes in a number of intermediate outcomes and by the validity of underlying assumptions. Firstly, where a subsidy programme is introduced, there must be a functioning distribution mechanism in place to make the subsidy available to farmers at the local level. Potential corruption in the supply chain also needs to be addressed to prevent leakage or subsidy diversion.

Where subsidies are made available at the local level, farmers must be aware of their eligibility to access them and recognise the value of the subsidy/ input to actually make use of them. Markets also need to be able to provide for any additional demand for inputs that subsidies may stimulate (Dorward & Chirwa, 2013).

For adoption of the subsidy at the farm level, usually measured through increases in the use of the subsidised input, the subsidised inputs must actually be utilised by farmers rather than being sold or otherwise diverted. However, where subsidised inputs are sold, they may still lead to changes in farm incomes through increased cash incomes from their sale despite not actually impacting productivity or yield at the farm level (Kaiyatsa, 2015).

Where farmers do access subsidies, displacement of commercial sales of inputs may occur. This would mean farmers access the subsidised inputs but do not increase the amount of inputs they use overall. Where this occurs, farmers may make savings through having access to subsidised inputs but without any increase in farm productivity or production (yield) (Kato & Greeley, 2016).

Figure 1: Theory of change



Primary outcomes | Secondary outcomes

Assumption | Other element in the theory of change

Source: authors, based on reviews of theoretical literature (e.g. Dorward, 2009)

The impacts of input subsidies can extend beyond the farm household. Where subsidies result in incremental use of inputs and increased production, this can lead to changes in labour demand. In the short term, as production increases, it follows that so too does demand for agricultural labour, especially during labour-intensive periods such as planting and harvesting. Furthermore, where subsidies make production a more viable option for resource-poor households, they may focus on their own production rather than supply labour to better-off households. These changes can tighten the labour market and lead to increases in real wages, thereby increasing incomes and welfare among agricultural labourers (Dorward & Chirwa, 2010).

Increased production can also lead to changes in crop prices in the market. This is most likely to occur where the supply of a crop increases but demand does not rise in tandem, leading to a decrease in crop prices. This may offset the income gains for farm households from increased productivity and production (Dorward & Chirwa, 2013). However, among consumers, decreases in prices can lead to increased consumption of both food and non-food items due to savings in food expenditure.

In many low- and lower-middle- income countries (L&LMICs), farm households are both producers and consumers of staple crops, making ascertaining the net effects of changes in crop prices on farm households a complex process.

The changes in agricultural productivity and production, crop prices and labour demands and wages can have wider impacts on private-sector development and human and financial capital accumulation in a region or country, in turn effecting national economic growth as measured through GDP (Ricker-Gilbert et al., 2013). GDP is thus the final outcome measure of interest in our theory of change.

The broad range of factors potentially affecting or affected by subsidy programmes means many important factors central to the consideration of input subsidy programmes are outside the scope of this review. This includes issues relating to: environmental factors such as soil health and climatic conditions; infrastructure such as roads, irrigation systems, etc.; the institutional and policy context in which programmes are implemented; the technology characteristics of the inputs subsidised, and the degree of national and international market integration, among others.

1.4 Why it is important to do this systematic review

Agricultural input subsidies have been a key part of agricultural policy in many L&LMICs since the 1960s and are thoughts to have played a key but time-limited role in economic development (Timmer, 2004; Fan et al., 2008). However, despite widespread agreement regarding their positive impact on agricultural productivity in some contexts, notably in the Green Revolution of the 1960s and 70s, the general consensus among lending bodies and international donor agencies in the 1980s and 90s was that subsidies were largely ineffective and inefficient policy instruments. This was especially the case in Africa, where they were seen to have contributed to government overspending and a number of fiscal and macroeconomic problems (Dorward & Chirwa, 2014). Empirical studies at the time showed a range of negative impacts associated with their use. These included: cost control issues, diversion (inputs being stolen or used by others than the intended recipients), overuse of inputs and capital, unequal benefit to the wealthy, and distortionary effects inhibiting private investment in agricultural services (e.g. Ellis, 1992; Morris et al., 2007; Timmer et al., 2009).

Recent years have seen this viewpoint challenged by a reassessment of the potential role of subsidies in agricultural and wider economic development (e.g. Fan et al., 2004; Djurfeldt et al., 2005; Dorward, 2009). This renewed interest in subsidies has, at least in part been driven by the emergence of a number of innovative subsidy models and delivery systems working in collaboration with, rather than opposition to, the private sector (Dorward & Chirwa, 2014).

Calls from African governments and NGOs for the use of subsidies to address agricultural stagnation in Africa have grown stronger in recent years. This has resulted in a shift from a sceptical to a more supportive stance from donors such as the World Bank and the UK Department for International Development (Chinsinga, 2007).

This reappraisal of the evidence on input subsidies and the changing consensus on their potential effectiveness makes this an important and timely topic for systematic review.

The literature on implementing subsidies and their impacts in different contexts has previously been reviewed with mixed findings on many outcomes (e.g. Acharya & Jogi, 2007; Fan et al., 2008; Wiggins & Brooks, 2010; Chirwa & Dorward, 2013; Jayne & Rashid, 2013; Ricker-Gilbert et al., 2013; Gautam, 2015). However, to the best of our knowledge, no review of agricultural input subsidies using systematic searching, data collection, critical appraisal and statistical synthesis using meta-analysis, has yet been published.

Furthermore, previous literature reviews have not been sufficiently theoretically rigorous in addressing the diversity of existing programmes, outcomes and impacts discussed above. Therefore, this publication not only provides the first systematic review of this topic but also addresses a major gap existing in general literature reviews by taking a more holistic approach in investigating direct and indirect effects across the theory of change.

2. Objectives

The objective of our review is to answer the question: “what is the effectiveness of agricultural input subsidies in improving productivity, farm incomes, consumer welfare and wider growth in low- and lower-middle-income countries?”

This question was broken down into two main research questions (see also Figure 1):

1. What are the effects of agricultural input subsidies on agricultural productivity and beneficiary incomes and welfare (**research question 1a**), and what might explain variation in these effects (**research question 1b**)?
2. What are the effects of agricultural input subsidies on consumer welfare and wider economic growth (**research question 2**)?

3. Methods

The methodology for this systematic review is based on a published protocol (Dorward et al., 2014). The following section sets out the criteria for including studies in the review, the search strategy, the approach to assessing the risk of bias in included studies and methods of synthesis.

3.1 Criteria for considering studies for this systematic review

To be included, studies had to examine the effects of agricultural input subsidies in a lower- or lower middle-income country. The inclusion criteria follow the conventional population, intervention, comparator, outcome, and study design (PICOS) structure, with two research questions drawing on different bodies of research. Research question 1 (RQ1) relates to beneficiary outcomes, while research question 2 (RQ2) relates to consumer welfare and wider economic growth. RQ1 can be addressed through experimental and quasi-experimental studies, but RQ2 is far less amenable to such designs. We therefore included simulation modelling studies to address that question.

In this systematic review, we use the term 'study' to refer to a unique evaluation of a development programme.

The different elements of the systematic review question pose different challenges for the evaluation of subsidy impacts. This is illustrated in the theory of change (Figure 1); some impacts affect subsidy beneficiaries directly (for example, changes in productivity and incomes), while others affect beneficiaries and non-beneficiaries indirectly (net farm incomes, wages rates, consumer welfare and wider growth). While direct impacts are amenable to experimental and quasi-experimental study, indirect impacts are more difficult to assess in such a manner, as both the subsidies and their market impacts would need to be confined to a particular area that is comparable to an area without subsidies. In view of these differences in the applicability of experimental or quasi-experimental methods, we included different study methodologies to address each research question. We only included studies that involve some counterfactual comparison of results with and without subsidy treatments.

Research question 1: admissible study designs included randomised control trials and studies that use some formal methods for removing likely biases from non-random assignment of subsidy receipt. Such methods include regression studies using difference-in-differences (or fixed-effects models), instrumental variables regression, regression discontinuity, and propensity score matching methods, as appropriate for analysing panel or cross-sectional household data with randomised or quasi-randomised beneficiary selection or beneficiary selection by programme planners and participants.

Research question 2: admissible study designs included all experimental and quasi-experimental designs admissible for primary outcomes. We also included models that allow comparison of with and without subsidy situations (for example, partial equilibrium model [PEM], CGE, and other statistical models that link direct subsidy impacts into wider labour and produce markets) where the effect of the input subsidy change alone is discernible (i.e. not where it is combined with other policy changes).

3.1.1 Types of participants

Eligible populations were people for whom data had been collected at any level (e.g. country, region, community, household or individual) living in a low- or lower-middle-income country at the time the intervention was carried out. 'Low- and lower-middle-income countries' as defined in March 2012 by the World Bank were divided according to 2008 GNI per capita, calculated using the World Bank Atlas method[1]. We chose to focus on this set of countries as they have been the subject of most debate regarding the potential impacts of input subsidies, and are those for which stimulating agriculture is likely to be most critical. The populations of interest within these countries both direct beneficiaries of the intervention (farmers and farm households) and those who may be indirectly affected (wage labourers and food consumers).

3.1.2 Types of interventions

Types of interventions included

The interventions included in the review were restricted to direct agricultural producer subsidies for inputs. 'Agriculture' was defined as animal or crop production (i.e. excluding forestry and fisheries). 'Agricultural input subsidies' were defined as grants (or loans, if repaid at below the market price) given to a farmer as a means of reducing the market price of a specific input used in agricultural production or providing it free of charge. Credit and loans not tied to agricultural inputs and unsubsidised inputs are not included in this systematic review as they have distinct economic effects and are covered elsewhere in a much broader literature. We included any of the following types of agricultural input subsidies:

- Tax exemption
- General price subsidy
- Administration mechanism
- Free supply
- Targeted
- Rationed
- Coupon/voucher.

Eligible subsidised inputs included:

- Seed
- Fertiliser
- Pesticide
- Herbicide
- Feed
- Drugs
- Machinery
- Fuel

Types of interventions excluded

We excluded early-stage agricultural research station field trials and humanitarian relief programmes, as the adoption of these trial inputs and such emergency interventions are unrepresentative of impacts of input subsidies in normal agricultural practice.

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[1] http://data.worldbank.org/news/new-country-classifications

3.1.3 Types of outcomes

The outcomes considered in this systematic review are as listed in Table 1. They are classified as primary or secondary outcomes, as described below.

Primary or final outcomes

Primary outcomes include direct static effects such as adoption or use of subsidised inputs, agricultural production and productivity and indirect effects such as net farm income and poverty among farm households. 'Adoption' is measured in terms of farmers' usage of subsidised inputs. 'Agricultural productivity' is measured in broad terms by production per resource unit such as yields per unit of land, net revenue (profits per unit of land) and production per unit of labour. 'Agricultural production' includes total production per farm. 'Net farm income' is measured by the value of production at market prices, net of cost of purchased inputs; it may or may not also be considered net of imputed costs (e.g. of own land or family labour). 'Farm household poverty' is net change in poverty rates between beneficiary and non-beneficiary households.

Secondary or intermediate outcomes

Secondary outcomes of interest include indirect dynamic outcomes relating to consumer welfare and wider growth, which result from changes in agricultural production and productivity and net farm income. 'Consumer welfare' is measured by changes in real incomes and incidence of poverty in the wider population that are commonly used as proxy measures of welfare in benefit-cost analysis (Sadoulet & de Janvry, 1995; Alston et al., 2000). Other measures of consumer welfare included in the review are retail prices and consumption of both food and non-food items, labour market effects such as demand and wages and welfare calculated as compensating variation.

'Wider growth' refers to growth in the agricultural and wider economy as measured by agricultural or overall GDP growth. These effects would only be expected where there are also direct production effects.

Table 1: Summary of included outcomes

Primary:
Adoption Usage of subsidised inputs per unit of land
Productivity Yields per unit land Production per unit labour Total production per farm
Impacts on farm incomes and poverty among farm households Value of production at market prices, net of cost of purchased inputs, farm household poverty
Secondary:
Impacts on consumer welfare Food prices Consumption Expenditure Labour market effects (labour demand & wages) Real income Poverty
Impacts on wider growth GDP growth

Types of settings
Eligible comparisons include no active agricultural input subsidy intervention, wait-list, alternate input subsidy intervention, or other interventions providing access to inputs.

3.2 Search methods for identification of studies

We searched for articles that met our inclusion criteria across various databases and publications, listed below. There were no restrictions on publication year, type or language. Searches were undertaken in English. Specific search strings were devised to collect the appropriate papers from each type of database and records were collected in EndNote citation management software (Clarivate Analytics, LDN, UK). Potentially relevant papers were also identified by screening the key journals listed, and these too were incorporated into EndNote.

We devised a search string to capture relevant papers with the help of a search specialist (see Appendix 1 for the full search string). The search string was used to search a range of databases, selected for their known strength in covering the agricultural economics literature. It also drew on appropriate CAB Thesaurus terms for CAB Abstracts, plus relevant non-thesaurus identifier terms for free-text searching. An example of the search used for the CAB Direct database is also provided. The majority of the searches were conducted between 2 September and 11 November 2013.

Searches were not delimited by year of publication to ensure that all potentially eligible publications were included in the systematic review. This included, in addition to the peer-reviewed journal and book material, non-peer-reviewed material, conference papers, organization reports, working papers and other similar publications.

3.2.1 Electronic searches

We searched the following databases:

- 3ie Systematic Review Database and Impact Evaluation Repository
- Ageconsearch (http://ageconsearch.umn.edu/)
- Agricola
- AGRIS
- British Library for Development Studies
- CAB Direct
- Dissertations Express (http://disexpress.umi.com/dxweb)
- Ebsco: Econlit and Africa Wide
- ELDIS
- IDEAS (Economic and Finance Research) , including the RePec database http://ideas.repec.org/
- IFPRI library
- JOLIS
- Networked Digital Library of Theses and Dissertations (NDLTD) (www.theses.org)
- Social Sciences Citation Index (ISI Web of Knowledge)
- USAID library
- USDA's Economic Research Service site

Other information sources including grey literature:

- Google (Advanced Search)
- Google Scholar
- OECD/DAC Evaluation database
- Open-Grey

3.2.2 Hand and bibliographic search

We also hand-searched the following journals:

- Agricultural Economics
- American Economic Review
- American Economic Journal – Applied Economics
- American Journal of Agricultural Economics
- Economic Development and Cultural Change
- European Review of Agricultural Economics
- Journal of Agricultural Economics
- Journal of Development Economics
- World Development

Finally, bibliographic back-referencing was conducted from existing reviews on the topic (Chirwa & Dorward, 2013; Jayne & Rashid, 2013; Ricker-Gilbert et al., 2013). Citation searches in Web of Science and Google Scholar for included papers were conducted, and the names of key identified authors were searched to ensure recent papers had not been missed. We also contacted key authors to request relevant papers.

3.2.3 Reference management and screening procedures

All studies retrieved from our search were inputted to Endnote and then duplicate records were removed. Studies were assessed for inclusion at two stages, firstly at title and abstract, and then at full-text. A single reviewer assessed studies for eligibility for inclusion at title and abstract. At full-text, studies were again assessed by one reviewer, with coding decisions then checked by a second reviewer, with level of agreements at >85%. Disagreement regarding inclusion/exclusion of papers was resolved by consensus, or following assessment by a third reviewer.

3.3 Data collection and analysis

We extracted a range of data including bibliographic details, outcomes, period covered, study design and outcomes data. Data were extracted for each study by a single team member, with this process independently repeated for a random sample of 10 percent of studies by another team member, in order to assess and reinforce consistency of coding.

Where studies did not provide information on the size of subsidy, wherever possible we identified programme names and periods of implementation. We then used secondary sources of information in order to clarify the level of subsidy provided by a given programme over the period covered by the study.

3.3.1 Data extraction and management

We extracted a range of data including bibliographic details, outcomes, period covered, study design and outcomes data. Data were extracted for each study by a single team member, with this process independently repeated for a random sample of 10 percent of studies by another team member, in order to assess and reinforce consistency of coding. Where studies did not provide information on the size of subsidy, wherever possible we identified programme names and periods of implementation. We then used secondary sources of information in order to clarify the level of subsidy provided by a given programme over the period covered by the study.

3.3.2 Assessment of risk of bias in included studies

We carried out two distinct risk of bias analyses, one for experimental and quasi-experimental studies and one for modelling studies. The risk of bias tools are provided in Appendices 4 and 5.

Risk of bias domains were made up of a set of screening questions to determine whether a particular bias was controllable in a given study, guidance for the reviewer to rely on while scoring the risk of bias for the outcome, and the justification for making a judgment for every domain and outcome reported. Risk of bias scores were not used as weights in the analysis. However, for experimental and quasi-experimental evidence we did explore sensitivity using risk of bias categories for each outcome. In the narrative synthesis of modelling studies, studies with a high risk of bias are clearly demarcated. Where included studies were conducted by members of the review team (see Dorward &

Chirwa, 2009; Chirwa, 2010), the risk of bias analysis for these studies was conducted independently by other authors.

Risk of bias: experimental and quasi-experimental studies

The following categories of bias were used to assess experimental and quasi-experimental studies based on tools from Waddington et al. (2012) and Stewart et al. (2014; itself drawing on Sterne et al., 2013): (1) participant selection bias, (2) confounding bias, (3) ineffective randomisation bias, (4) unintended interventions, (5) missing data, (6) reporting bias, and (7) result selection bias. Studies were then rated for an overall risk of bias indicating critical, high, moderate or low risk of bias, as appropriate. The risk of bias tool is reported in Appendix 4.

Critical Appraisal: Modelling Criteria

Modelling studies were critically appraised using a tool developed by the review authors. The tool contains 10 criteria grouped into three categories:

1. Source and quality of data used in the modelling, taking into account: whether the data are empirical from a reliable source and consistent/comparable across time; whether the source of elasticities in the model are reported; whether reasons for choice of data are reported or justified.
2. Specification of the model, taking into account: whether the type of model has been used before; whether the model is dynamic or static; whether the assumptions underlying the model are reported and plausible; whether there are attempts to calibrate or otherwise test the validity of the model; whether the sensitivity of the model to changes in some variables is apparent, for instance through changing model variables across different scenarios (sensitivity analysis).
3. Comprehensiveness of reporting/plausibility of results, taking into account: whether results are described in detail and are plausible compared to real-world effects; whether any limitations/contradictions in results are discussed.

Studies were then given an overall rating indicating high or low threat to validity, as appropriate. Where a study failed on a predefined number of criteria in any of the three categories outlined above, they were appraised as having a high threat to validity. The critical appraisal tool is reported in Appendix 5.

3.3.3 Unit of analysis issues

We used the appropriate unit of analysis for clustered studies when calculating standard errors of the effect. For clustered studies, if the authors did not state that they had done so, we adjusted the standard error upwards using the standard formula in the Cochrane Handbook (Higgins & Green, 2011).

3.3.4 Dealing with missing data

To calculate standardised mean differences, data on the standard deviation of the outcome variable are needed. Where this was not reported, we applied information available about the sample size to other information reported in the paper, such as the value of the t-test for the difference in means across intervention and comparison groups (see Lipsey & Wilson, 2001). We also used the risk/response ratio to measure changes in poverty aggregates (e.g. headcount, poverty gap, squared-poverty gap). Where data were not reported for confidence intervals in simulation studies (e.g. due to lack of sensitivity analysis), we reported effect sizes only.

3.3.5 Assessment of heterogeneity

The chi-square (χ^2) test was used to investigate heterogeneity. A low p value or a large chi-squared statistic relative to its degree of freedom provides evidence of heterogeneity of intervention effects. I-squared and tau-squared were used to quantify, respectively, the percentage of the variability in effect estimates that is due to heterogeneity rather than sampling error, and the absolute value of the heterogeneity measured in standard deviations of the outcome.

3.3.6 Data synthesis

As we included different types of evidence for each of our research questions, we also adopted different methods of synthesis. For our primary research questions on the effects of agricultural input subsidies on beneficiary level outcomes, we synthesised evidence from experimental and quasi-experimental studies using meta-analysis, meta-regression analysis and a qualitative synthesis of relevant contextual factors.

For our secondary research questions regarding effects on consumer welfare and wider economic growth, we synthesised evidence from modelling studies narratively and displayed effects from included studies in scatter plots for ease of comparison where possible. Ideally, we would have been able to conduct a meta-analysis for all studies; however, it was not possible to calculate effect sizes for the modelling studies, as they do not report sample sizes or measures of uncertainty such as standard deviation or standard error.

3.3.7 Synthesis of experimental and quasi-experimental studies (Primary Outcome)

We undertook meta-analysis to examine the effect of input subsidies on our primary outcome of interest. All pooled effect sizes were calculated under random effects models, as they relate to different populations in different locations, at different times. Where it was not possible to calculate effect sizes or acceptable to synthesise results into a meta-analysis due to missing data such as standard deviations, we report results narratively.

We present effect sizes and 95 percent confidence intervals (95% CIs) using forest plots. Where constructs were considered sufficiently similar, we estimated pooled effect sizes across studies using inverse-variance weighted random effects meta-analysis using Stata software (Stata Corp, TX, USA). We undertook sub-group analysis by crop type and examined sensitivity of findings to risk of bias assessment.

We undertook meta-regression analysis to examine whether there was a correlation between the size of subsidy and primary outcomes of interest: adoption, yield and income. We also systematically extracted and narratively synthesised data from included experimental and quasi-experimental studies to examine programme implementation, input subsidy delivery mechanisms, farmer take-up and usage of inputs, leakage of vouchers or inputs, and other associated factors.

3.3.8 Calculating and estimating effect sizes

We extracted data to compute standardised mean difference effect sizes for continuous outcomes, and odds ratios for dichotomous outcomes, using methods outlined in Lipsey and Wilson (2001). We calculated effect sizes, standard errors, and confidence intervals based on the information provided in included studies. To ensure a meaningful comparison across outcome measures, we used Hedge's' g (sample size corrected) standardised mean difference (SMD). This statistic measured the effect size of the interventions in units of standard deviations. This standardisation allowed for the comparison of outcomes. We also calculated response ratios (RR) where the data allowed it.

We ensured that effect sizes were calculated consistently, so that the direction of change reflects a uniform increase or decrease in the outcome variable across studies (e.g. where studies estimate effects of introducing or removing subsidies). Information on effect sizes extracted from each study is in Appendix 3.

3.3.9 Criteria for determining independent findings

We only included a single effect size per study for any given outcome (Becker et al., 2007). This ensures that each meta-analysis only pools findings that are statistically independent. Where studies reported outcomes at different times of follow-up, the data point at the longest period of follow-up was used for effect size calculations.

We used the following decision criteria to determine independent findings:

(1) Where multiple specifications are presented for a single study, we chose the method with the lowest risk of bias (usually the least parsimonious in terms of covariates for quasi-experiments).
(2) Where we had multiple independent estimates for sub-populations, we calculated a 'summary effect size' using inverse-variance weighted random effects meta-analysis, as used in Baird et al. (2013).
(3) Where we had multiple dependent estimates we calculated a 'synthetic effect size', using the approach given in Borenstein et al. (2009; Chapter 24).

3.3.10 Synthesis of modelling studies (Secondary Outcomes)

Data were extracted from modelling studies on all secondary outcomes of interest. Information on coefficients extracted from each modelling study is in Appendix 3.

Computable general and partial equilibrium and other included econometric models used to simulate subsidy effects do not provide measures of variance and, as such, are not amenable to statistical meta-analysis. We thus performed a narrative synthesis of effects grouping studies by outcomes of interest. Where studies provide information on the percentage point change in the percentage of the market price of the input covered by the subsidy, it was plotted against the percent change in the outcome variable on a scatter plot.

Where modelling studies contain additional simulations of effects under differing scenarios relevant to our research questions we also report these findings narratively. To better explain our results, we also extracted additional information (type of inputs being subsidised, primary staple crops produced in country) to try to capture some of the heterogeneity of the interventions simulated in different models.

No regression lines were fitted to the scatter plots. This was due to the issue of study dependency. More than a single estimate of effect is included from a single study for several outcomes of interest. Additionally, several of the included studies model the effects of a single programme, the Malawi Farm Input Subsidy Programme (FISP). As such, the scatter plots are included to provide ease of comparison across studies rather than to quantitatively synthesise model effects.

Furthermore, it was not possible to weight observations in the analysis, as would be done in meta-regression analysis, because the effects are not estimated using systematic sensitivity analysis to estimate standard errors. Where studies (n = 2) did not report enough information to allow inclusion in the scatter plots, we report the findings narratively.

Where studies simulate effects for a single outcome under different scenarios, for example, short-run and long-run effects, or funding through direct or indirect taxation, we included all data points in the scatter plots. Where studies specify a range of models with differing levels of responsiveness to changes simulated in the model, we plotted either the general equilibrium model or the model closest to the 'real-world' scenario. Where studies reported effects for different decades, we averaged effects across decades. This was only necessary for one study (Fan, 2007).

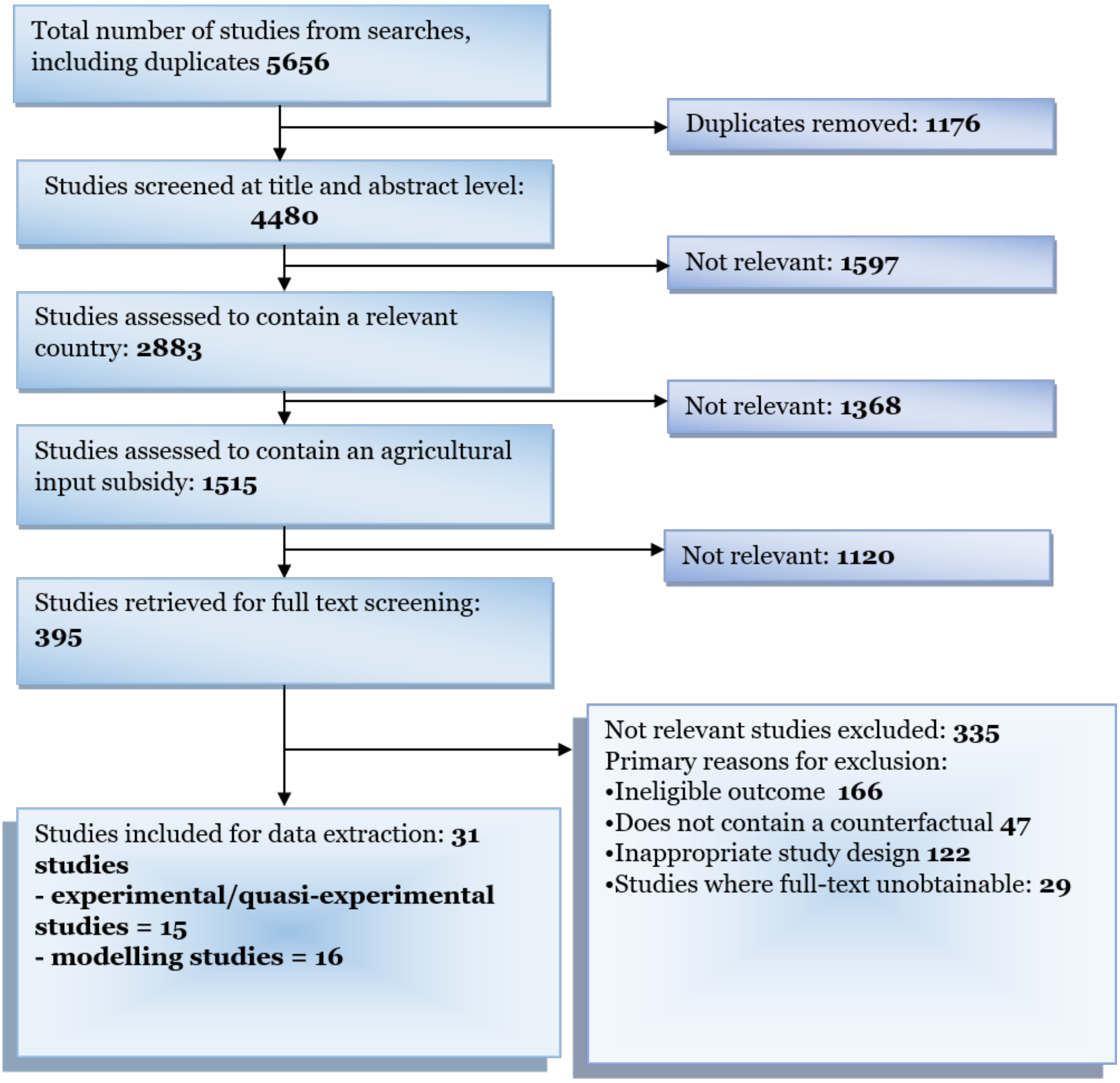
4. Results

4.1 Description of included studies

The search initially identified 5,656 studies. From these initial search results, 1,176 duplicates were removed, leaving 4,480 records. Appendix 2 provides the initial hits for each database searched. After screening at title and abstract according to L&LMIC country criteria, a further 1,597 records were removed, leaving 2,883 records. Screening for a relevant agricultural input subsidy removed a further 1,368 records. The remaining 1,515 records were screened at title and abstract for studies that linked input subsidies to changes in relevant outcomes, allowing the discarding of 1,120 records.

The remaining 395 records were screened at full text. After screening, 31 papers comprising 15 experimental/quasi experimental studies and 16 modelling studies were included. The search results are summarised in Figure 2.

Figure 2: PRISMA flowchart of search strategy



We included 15 experimental and quasi-experimental studies and 16 simulation modelling studies. The majority of the included studies (n = 27) relate to sub-Saharan Africa. Fifteen of these are from Malawi, with many of these evaluating the Malawian Farm Input Subsidy Programme (FISP). This programme changed over time, providing different rates of subsidy. Thus, our included studies all represent unique datasets. Studies from other countries in sub-Saharan Africa come from Zambia (n =3), Ethiopia (n = 2), Tanzania (n = 2), Ghana (n = 1), Nigeria (n=1), Ghana (n = 1), Madagascar (n = 1), Mali (n = 1), Mozambique (n = 1). Studies from outside sub-Saharan Africa are from Indonesia (n = 2) and India (n = 2).

Studies examined a variety of different fertiliser and seed subsidy/voucher interventions. Some studies focus on more than a single country. Despite the inclusive search strategy employed, no studies of subsidies of inputs such as drugs, fuel, machinery and animal feed were found that met our inclusion criteria. One modelling study by Fan et al. (2007) does look at effects of irrigation and electricity as well as fertiliser subsidies. Our included experimental and quasi-experimental studies only examined primary outcomes, while modelling studies were only included if they modelled effects on our secondary outcomes relating to consumer welfare and wider growth effects.

Of the fifteen experimental and quasi-experimental studies that met our inclusion criteria, all but one reported on evaluations of input subsidies programmes in sub-Saharan Africa (Mali = 1; Malawi = 7; Mozambique = 1; Nigeria = 1; Tanzania = 1; Zambia = 3). The remaining study reports on an evaluation of a programme in India. Evaluated programmes provided subsidised or free seeds and fertiliser to farmers, often in the form of a voucher. Ten interventions subsidised both seeds and fertiliser as a package, three seeds only and two fertiliser only. Some programmes provided a limited amount of inputs free of charge, but most provided inputs at reduced cost (ranging from as little as 22 percent to as high as 92 percent of cost). These subsidies were typically available only for a specified amount of inputs, though not all studies clearly reported the total amount of inputs that could be bought at subsidised rates. Studies adopted a range of study types to evaluate programmes. There were two randomised controlled trials, one field experiment, and others included instrumental variables, matching methods and other quantitative analyses with intervention and comparison groups using methods to control for selection bias and confounding.

Of those studies that were included in the meta-analysis, seven reported some measure of input use and adoption, seven examined some measure of yield or agricultural production, four some measure of household income, and two a measure of poverty.

Of the sixteen included simulation modelling studies, nine are computable general equilibrium models, two are partial equilibrium models and five are other econometric models[2]. The majority of studies (n = 13) focus on sub-Saharan Africa. Nine focus on Malawi, two of which also model outcomes in an additional sub-Saharan African country (Zambia and Ghana).

4.1.1 Excluded studies

Of the 335 records excluded at full-text, the primary reasons for exclusion were ineligible outcomes (166 records), the lack of an includable counterfactual comparison (47), or inappropriate study design (122). Twenty-nine records were identified which may have been relevant to the review but where the full-text was unobtainable. Studies were often excludable for more than one reason, but once a study met one exclusion criteria, further reasons were not sought.

4.1.2 Risk of bias in included studies

Full risk of bias results for each included studies are provided in Appendix 6.

We identified 15 experimental or quasi-experimental studies assessing the effects of agricultural input subsidies on primary outcomes. Figure 3 presents a summary of findings from the risk of bias appraisal of included studies.

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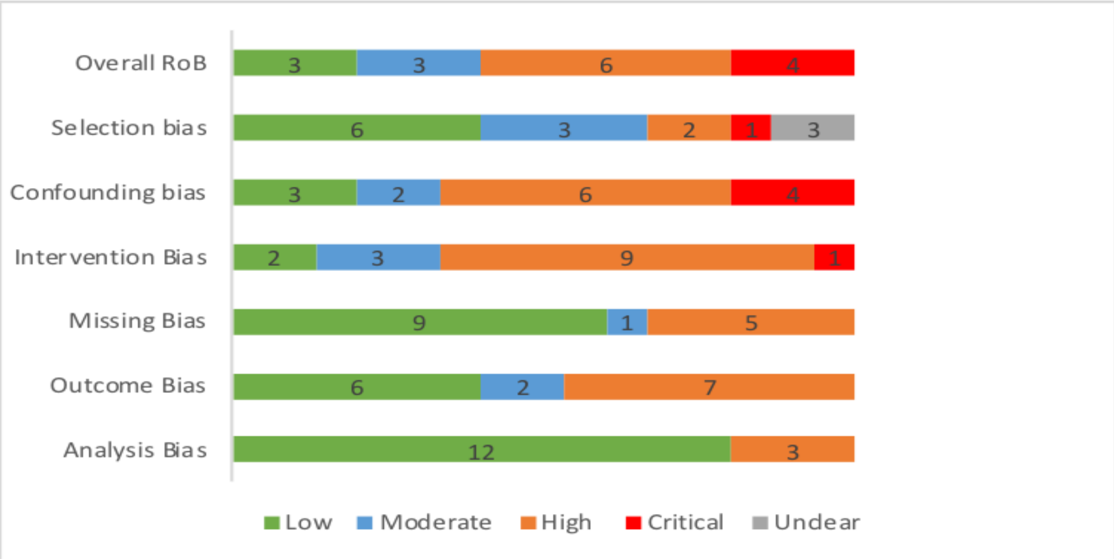
[2] Multimarket model; multiple-output and multiple input framework; supply demand model; output supply function economic model estimation; Arellano-Bond model using regression.

Overall, there is a high risk of bias within the sample of included studies (see Figure 3). Only three studies were found to have a low risk of bias (Bardhan & Mookherjee, 2011, Carter et al., 2013, Holden, 2013), with the majority of studies found to have a high risk of bias (n=6).

Selection bias due to baseline confounding, bias due to departures from interventions, and outcome reporting bias were the main reasons for the high overall risk of bias within this body of evidence. Selection bias resulted mainly from incomplete reporting. Only a minority of studies provided detailed information about how and what types of participants were chosen for interventions. The main cause of baseline confounding emerged from a failure of research teams to establish comparable experimental groups at baseline. For example, Denning et al. (2009) evaluated the effects of a subsidy programme in the context of a Millennium Villages Project (MVP) and purposively identified a control area, evaluating endline values without attempting to assess whether the conditions in the control village and the MVP were comparable.

Four studies were rated as having a critical risk of bias due to baseline confounding (Ajayi et al., 2009; Denning, 2009; Kamanga, 2010; Parameswaran, 2012). As a result, these are excluded from the meta-analysis and meta-regression.

Figure 3: Risk of bias summary for experimental and quasi-experimental studies

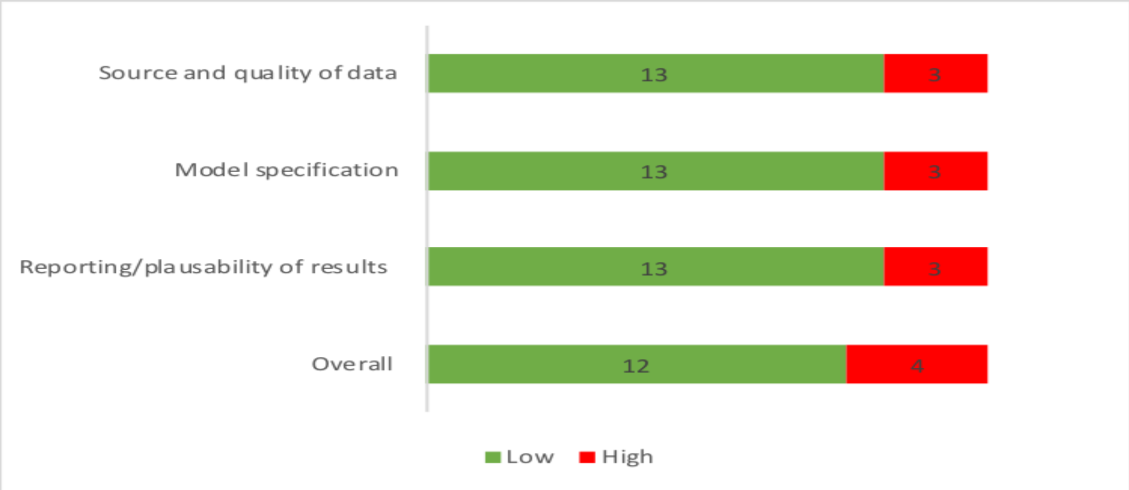


The high prevalence of bias owing to departures from intended interventions is partly the result of the inherent properties of agricultural subsidy programmes. Many subsidy programmes struggled to ensure the scheduled provision of subsidy instruments, such as vouchers, which were often distributed late or not available when farmers required the entitled inputs. Political interference in the distribution of subsidy further led to unintended changes to programmes.

The extrapolation of agricultural income data from harvest and sales values under perfect market conditions based on unrealistic assumptions also led to high reporting risk of bias for outcomes. Selection bias, missing data, as well as bias in selection of results reported did not present major sources of bias.

We included 16 modelling studies that simulate the effects of agricultural input subsidies on consumer welfare and/or wider economic growth. Twelve of the 16 studies were assessed as having a low risk of bias. Four studies (Tower, 1987; Rosegrant & Kasryno, 1991; Govindan & Babu, 2001; Mapila, 2013) were assessed as having a high risk of bias due to failing to meet the minimum criteria under one of our categories of bias. Figure 4 presents a summary of findings from the risk of bias appraisal of included modelling studies. Four studies failed to meet minimum criteria overall.

Figure 4: Risk of bias summary for modelling studies



4.2 Synthesis of results

We first present results of meta-analysis of outcomes reported by included studies (research question 1a). Then we present results of meta-regression analysis to explore heterogeneity statistically. We also provide a narrative synthesis of contextual and implementation factors to explore the effects of agricultural input subsidies on adoption, productivity and net farm income (research question 1b). Finally, we draw on a narrative synthesis of effects and scatter plots to explore the effects of input subsidies on consumer welfare and wider growth (research question 2).

4.2.1 Meta-analysis of experimental and quasi-experimental studies (RQ1)

Information on each of the included experiments/quasi-experiments is provided in Table 2 (and more detailed information in Appendix 7), including information on the study design, intervention and outcomes measured.

The results of the meta-analysis are structured along the causal chain, starting with adoption, measured as usage of subsidised inputs, then examining effects on agricultural productivity in the form of yields, before examining the effect on farmer income and poverty status. All pooled effects were calculated using random effects models, as the evidence relates to different populations in different locations, at different times. We also conduct sub-group analysis by crop type and sensitivity analyses[3].

[3] One study provided separate outcomes data for two different crop types (World Bank, 2014). We created a synthetic effect to include in the main analysis.

Table 2: Table of characteristics: experimental/quasi-experimental studies (primary outcomes)

Study	Study Type	Setting	Intervention	Outcome measure	Risk of bias
Ajayi et al. 2009	Feasibility plot experiment	Zambia	50% fertiliser price subsidy. Comparison: open market priced fertiliser (no subsidy)	Labour input (days per annum); Profitability	Critical*
Awotide et al. 2013	Randomised controlled trial	Nigeria	Voucher subsidy for 40% of costs for rice seed. Comparison: no subsidy	Yield (kg/ha); Crop income; Poverty headcount (%)	High
Bardhan & Mookherjee 2011	Time series panel data	India	Price subsidy. Subsidised agricultural inputs in the form of mini-kits containing seeds for rice, oilseeds and potatoes, fertilisers and pesticides. The authors state kits were provided "at throw away prices". The size of the subsidy is not provided. Comparison: no subsidy	Farm productivity value/ha	Low
Carter et al. 2013	Randomised controlled trial	Mozambique	Voucher subsidy for 73% of costs for improved maize seed and fertiliser package for cultivation of a half hectare of maize. Comparison: no subsidy	Fertiliser use (kg/ha); Seeds use (kg/ha); Yields (kg/ha)	Low
Chibwana et al. 2012	MNL and Instrumental Variables regression based, panel data	Malawi	Voucher subsidy for costs for 2 kg of hybrid maize seed or 4 kg of open pollinated maize and a 92% subsidy for 50 kg of maize fertiliser. Some households also received vouchers for 50kg of tobacco fertiliser. Comparison: no subsidy	Yield Kg/ha	High
Chirwa 2010	Propensity score matching; OLS regression	Malawi	The programme provided 10-15 kg of fertilisers and ample hybrid maize seed free of charge suitable for planting 0.1 hectares of land. Comparison: no subsidy	HH annual expenditure (MK)	Moderate
Denning 2009	controlled before versus after	Malawi	Voucher subsidy for 63% of fertiliser costs and free maize seed under Millennium Villages Project. Comparison: national subsidies programme	Yield (t/ha)	Critical*
Holden 2013	Prospective; mixed	Malawi	Voucher subsidy for 64%, 73% and 91% (in 2006,	Yield (kg/ha)	Low

Study	Study Type	Setting	Intervention	Outcome measure	Risk of bias
	effects. Matching		2007 and 2009 respectively) for costs of fertiliser and free maize seed. Comparison: no subsidy		
Kamanga 2010	prospective; controlled before versus after	Malawi	63% price subsidy for fertiliser and free maize seed. Comparison: no subsidy	Yield (t/acre)	Critical*
Karamba 2013	OLS regression model and Instrumental variables	Malawi	Voucher subsidy for 91% of costs for fertiliser and maize seed. Comparison: no subsidy	Output per hectare	High
Mason & Smale 2011	Panel data regression	Zambia	60% price subsidy for maize seeds. Comparison: no subsidy	HH Income (Total in ZMK); Poverty levels; Subsidised seed use (kg); Yields (harvest in kg)	High
Mather & Kelly 2012	OLS regression; correlated random effects	Mali	22% price subsidy for urea and 43% price subsidy for basal fertilisers for rice producers. Comparison: no subsidy	Yield average partial effect	High
Parameswaran 2012	Retrospective; linear regression analysis and time-series	Malawi	Subsidised fertiliser and maize seed. The size of the subsidy is not provided. Comparison: no subsidy	Yield (t/sq km)	Critical*
Smale & Birol 2013	3-stage regression tobit & instrumental variables	Zambia	Voucher subsidy covers 50-75% of the cost of improved maize seed Comparison: no subsidy	Input use partial effect	Moderate
World Bank 2014	Prospective, DID	Tanzania	50% price subsidy for fertiliser and maize seed packs. Comparison: no subsidy	Yield; Revenue TSh/ac	High

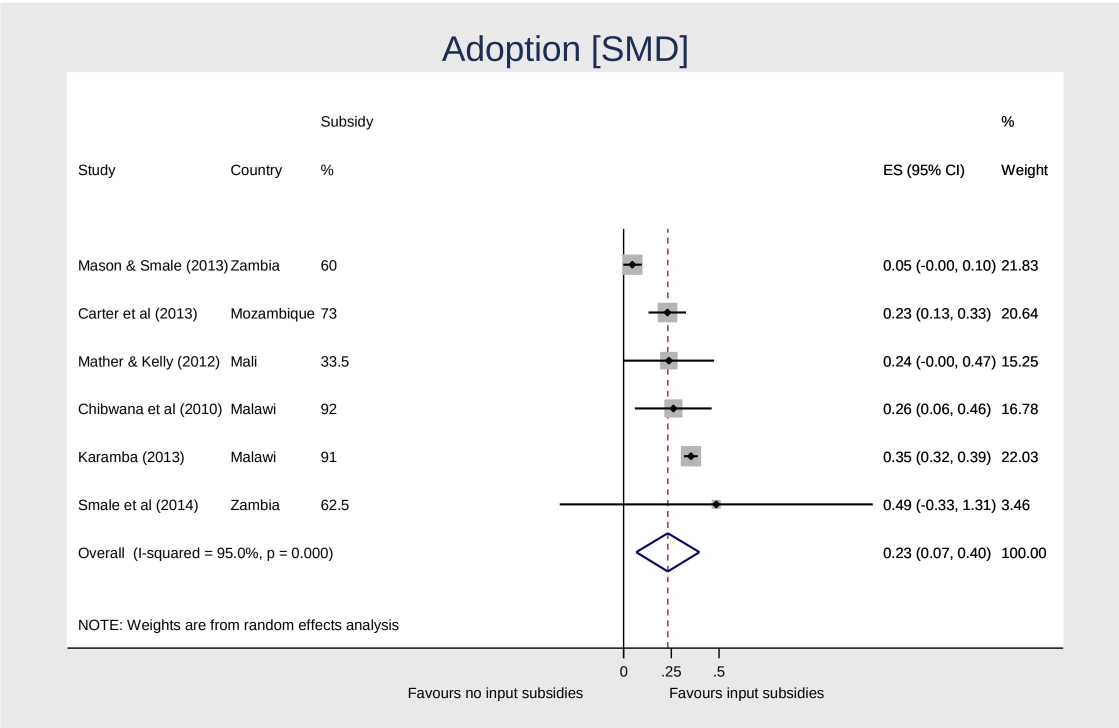
Notes: DID = Difference-in-differences; MNL = Multinomial logit; OLS = Ordinary Least Square

4.2.2 Input use and adoption

Six included studies report on the effects of agricultural input subsidies on adoption, measured as farmers' usage of subsidised fertiliser or seeds, primarily in kg/ha. Effect sizes for adoption are expressed as standardised mean difference (SMD), indicating the change in adoption among farmers receiving input subsidies versus that in the non-intervention comparison group. This is represented as the number of standard deviation changes in the outcome.

Figure 5 shows the overall average effect of agricultural input subsidies on adoption 0.23, 95% CI [0.07, 0.408] (χ^2=108.87 (df=6), p=0.000; I^2=95.0%; Tau^2=0. 0323). While all studies indicate a positive impact on outcomes, tests of homogeneity suggest a high degree of between-study variability, suggesting that different contextual factors affect effect sizes.

Figure 5: Adoption of subsidised inputs



The sub-group analysis to assess adoption by type of input is presented in Figure 6. It shows that adoption of fertilisers (SMD=0.35, 95% CI [0.31, 0.38]) or fertilisers and seeds (SMD=0.32, 95% CI [0.23, 0.41]) is larger than that for seeds only (SMD=0.07, 05% CI [0.00, 0.15]), suggesting the complementarity of fertilisers and improved seeds. The results from the sub-group analysis should, however, be interpreted cautiously, as there are few studies in each of the sub-groups.

Figure 6: Adoption of subsidised inputs by input type

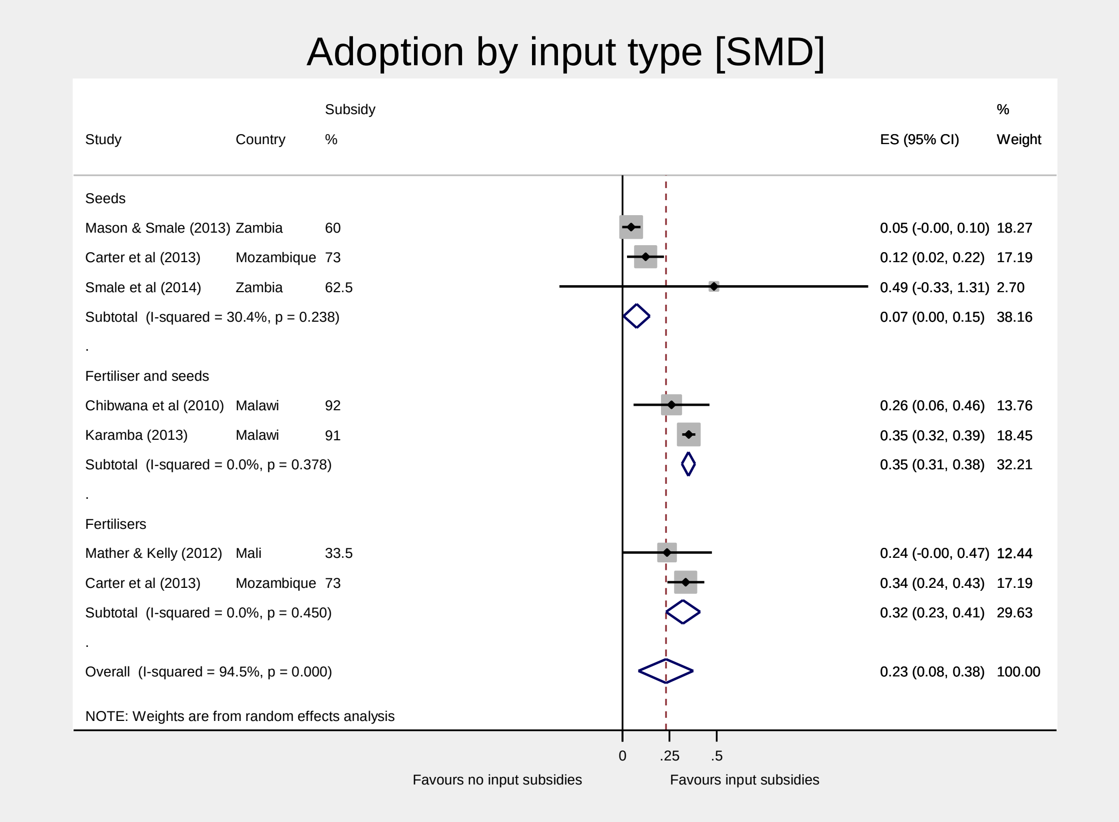


Table 3 summarises all results of the meta-analyses for adoption. It includes a sub-group analysis by crop type. We also examined whether the findings are sensitive to the risk of bias status of the included studies. The small number of studies in each risk of bias category means that caution should be taken when interpreting the findings. The analysis indicates that studies assessed as being of lower risk of bias show larger effects on average than those with moderate or high risk of bias (SMD=0.35, 95% CI=0.08, 0.38; χ^2=19.59 (*df*=4), *p*=0.001; I^2=79.6%; Tau^2=0.0097).

Table 3: Adoption of subsidised inputs

Adoption	SMD	95% confidence interval		χ^2 (p)	No. obs.	I^2	Tau^2
Overall	0.23	0.07	0.40	108.87 (0.000)	6	95.0%	0. 0323
Subgroup analysis by input type							
Seeds input	0.07	0.00	0.15	2.87 (0.238)	3	30.4%	0.0013
Fertiliser input	0.32	0.23	0.41	0.57 (0.450)	2	0.0%	0.0000
Fertiliser and seeds	0.35	0.31	0.38	0.78 (0.378)	2	0.0%	0.0000
Subgroup analysis by crop type							
Rice	0.24	-0.00	0.47	0.00	1	.	.
Maize	0.19	0.05	0.34	29.88 (0.000)	5	86.6%	0.0193
Subgroup analysis by risk of bias status							
High risk of bias	0.05	-0.00	0.10	0.00	1	.	.
Moderate risk of bias	0.49	-0.33	1.31	0.00	1	.	.
Low risk of bias	0.35	0.08	0.38	19.59 (0.001)	5	79.6%	0.0097
Subgroup analysis by study design							
RCT	0.23	0.13	0.33	0.00	1	.	.
Non-randomised study	0.23	0.03	0.44	99.55 (0.000)	5	96.0%	0.0402

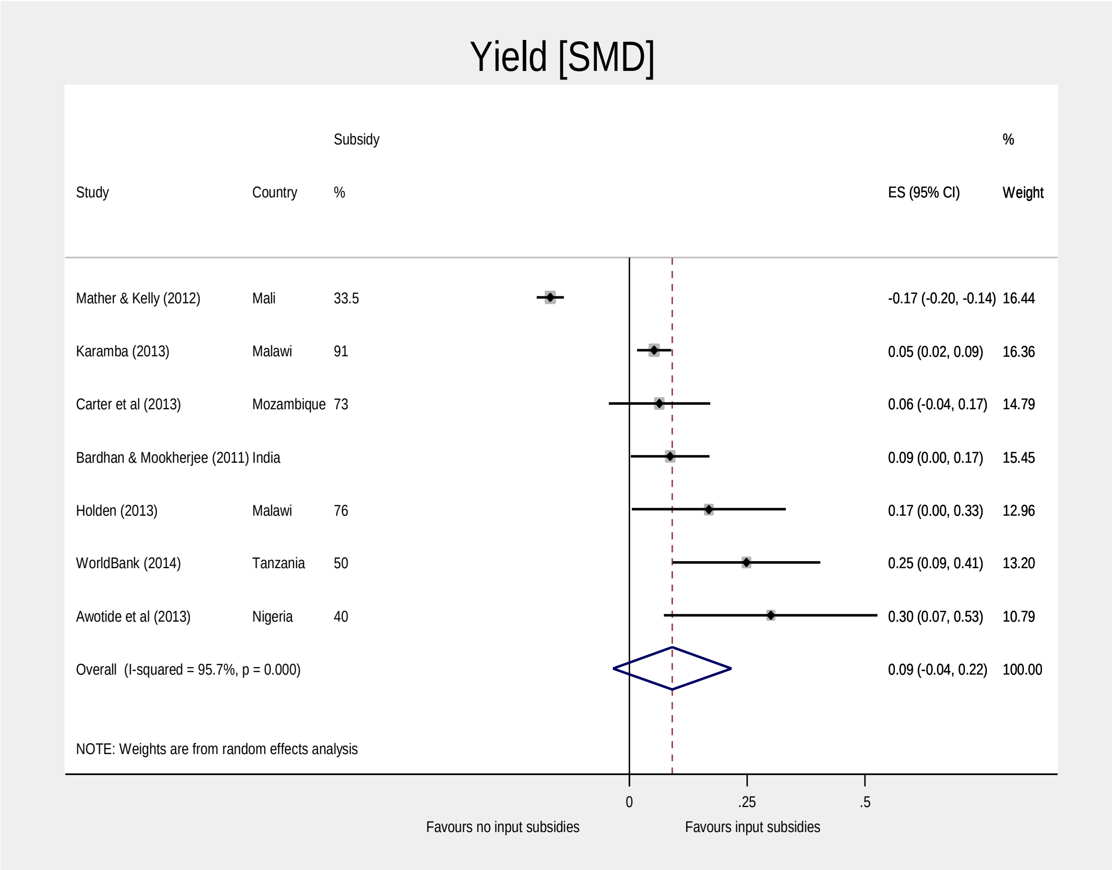
Notes: SMD = standardised mean difference; No. obs. = number of observations

4.2.3 Agricultural productivity

Seven of our included studies examine yield per hectare as a measure of the effects of agricultural input subsidies on agricultural productivity (Bardhan & Mookherjee, 2011, Mather & Kelly, 2012; Awotide et al., 2013; Carter, 2013a; Holden, 2013; Karamba, 2013, World Bank, 2014). Table 4 provides the meta-analysis for the effects of agricultural input subsidies on yields. The evidence indicates that input subsidies interventions lead to sizeable increases in yields for recipient farmers. Effect sizes for yield are expressed as standardised mean difference (SMD), indicating the change in yield among farmers receiving input subsidies versus that in the non-intervention comparison group. The overall average effect of agricultural input subsidies on yield is 0.09, 95% CI [-0.04, 0.22]. Tests of heterogeneity again suggest a high degree of between-study variability (χ^2=138.78 (*df*=6), *p*=0.000; I^2=95.7%; Tau^2=0.0250).

All but one study indicate a positive impact on yield. An outlier study by Mather and Kelly (2012) was the only one to show a non-positive effect on yields, finding that farmers receiving the input subsidies actually had a significantly poorer yield than those who did not (SMD=-0.17, 95% CI [-0.20, -0.14]). However, Mather and Kelly (2012) indicate that ‘water control’ problems such as flooding during the rainy season had a large negative impact on rice yields when compared with the same area pre-intervention[4]. They conclude that input subsidies may not be effective if they are not accompanied by improvements in water control and management practises.

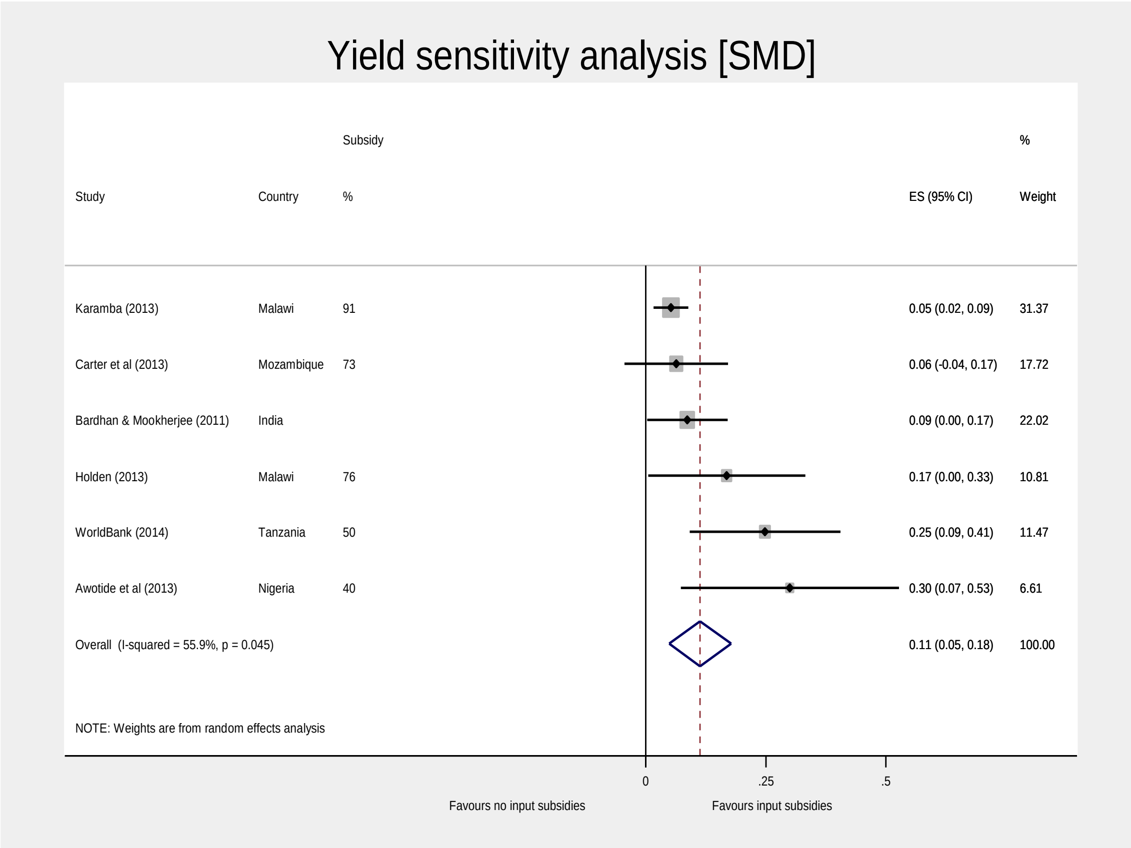
Figure 7: Effect of input subsidies on yield



Exclusion of the outlier study (Mather & Kelly, 2012) on these grounds shows that overall, the effect of subsidies on yields is statistically significant (SMD=0.11, 95% CI [0.05, 0.18]), and also reduces between-study variation substantially (χ^2=11.34 (*df*=5), *p*=0.045; I^2=55.9%; Tau^2=0.0031). The results of this analysis are shown in Figure 8.

[4] Mather and Kelly (2012) is a cohort study that compares outcomes for the same farmers in 2006 and in 2008. The absence of any contemporaneous comparator makes it difficult to fully account for how far ‘water control’ problems were responsible for observed outcomes. This type of problem is one that can affect all studies with this type of design.

Figure 8: Effect of input subsidies on yield - sensitivity analysis



The sub-group analysis in Figure 9 examines the effects of input subsidies by crop type. The results suggest that more effective outcomes might be obtained if subsidies focused on a specified crop such as rice (0.25, 95% CI [0.09, 0.41]) or maize (0.18, 95% CI [0.02, 0.33]) rather than a mix of crops (0.06, 95% CI [0.02, 0.09]). The results from the sub-group analysis should be interpreted cautiously as there are few studies in each of the sub-groups. An overview of all the meta-analysis results related to yield is provided in Table 4.

An examination of the findings by risk of bias status of the included studies indicated that the studies assessed as being of low risk of bias show on average lower effects than those with high risk of bias (SMD=0.06, 95% CI=0.03, 0.09; χ^2=2.23 (*df*= 3), *p*=0.526; I^2=0.0%; Tau^2=0.0000).

Figure 9: Effect of input subsidies on yield by crop type

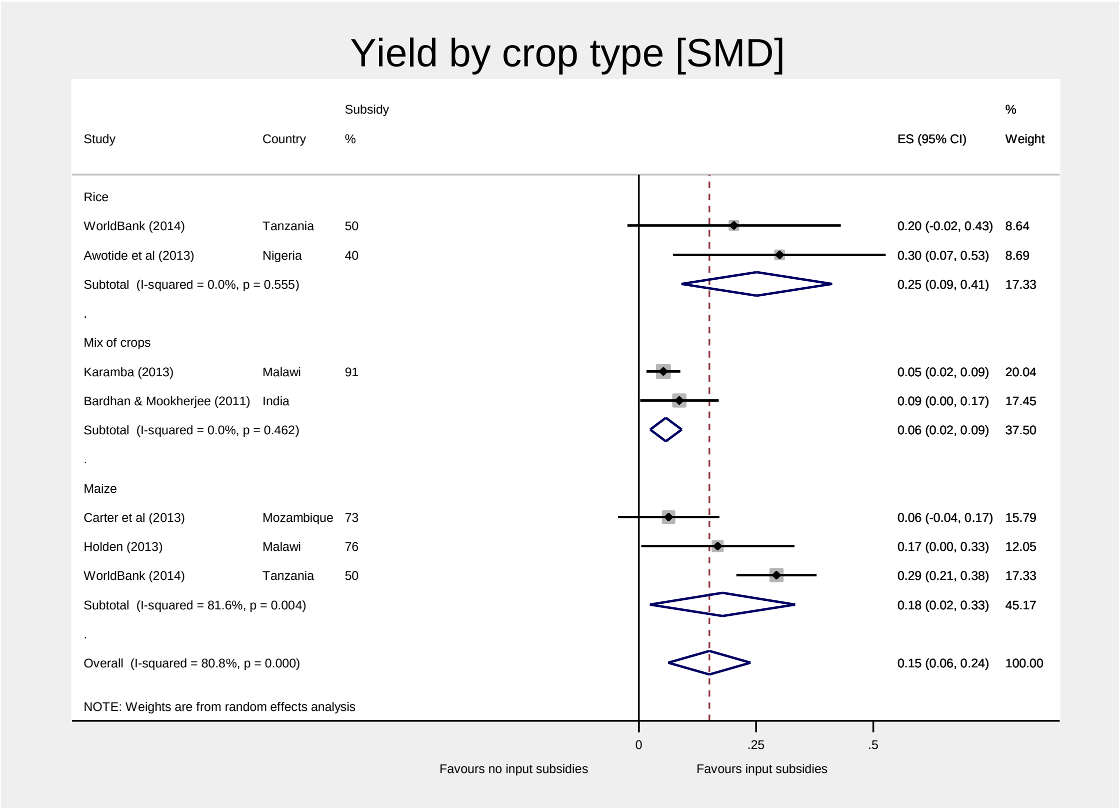


Table 4: Effect of input subsidies on yield

Yield	SMD	95% confidence interval		χ^2 (p)	No. obs.	I^2	Tau^2
Overall	0.09	-0.04	0.24	138.78 (0.000)	7	95.7%	0.0250
Overall sensitivity analysis	0.11	0.05	0.18	11.34 (0.045)	6	55.9%	0.0031
Subgroup analysis by input type*							
Seeds input	0.3	0.07	0.53	0.00	1	.	.
Fertiliser input	0.05	0.02	0.09	0.00	1	.	.
Fertiliser and seeds	0.12	0.05	0.19	(0.213)	4	33.3%	0.0019
Subgroup analysis by crop type*							
Rice	0.25	0.09	0.41	0.35 (0.555)	2	0.0%	0.0000
Maize	0.18	0.02	0.33	10.88 (0.004)	3	81.6%	0.0150
Mix of crops **	0.06	0.02	0.09	0. 54 (0.462)	2	0.0%	0.0000
Subgroup analysis by risk of bias status							
High risk of bias	0.27	0.14	0.39	0.13 (0.714)	2	0.0%	0.0000

Yield	SMD	95% confidence interval		χ^2 (p)	No. obs.	I^2	*Tau*^{2}
Low risk of bias	0.06	0.03	0.09	2.23 (0.526)	4	0.0%	0.0000
Subgroup analysis by study design							
RCT	0.16	-0.07	0.39	3.40 (0.065)	2	70.6%	0.0197
Non-randomised study	0.11	0.03	0.18	7.43 (0.059)	4	59.6%	0.0031

SMD = standardised mean difference
**Excludes Mather & Kelly (2012)*
***Mix of crops = where subsidised farmers farmed a mix of crops, typically some combination of maize, rice, legumes and tobacco.*
Results including Mather & Kelly, 2012 in the sub-group analyses as follows:
Rice (crop type): SMD = -0.10, 95%CI(-0.24, 0.44)
Fertiliser (input type): SMD = -0.058, 95%CI(-0.274, 0.158)
Low risk of bias studies (RoB status): SMD = 0.033, 95% CI(-0.104, 0.170).

4.2.4 Farm and farm household income poverty rates

The evidence also indicates that input subsidy interventions improve outcomes for income (comprising measures of revenue, profit and income). Effect sizes for these outcomes are expressed as standardised mean difference (SMD), indicating the change in outcomes among farmers receiving input subsidies versus that in the non-intervention comparison group. We combine measures for crop and household income, annual household expenditure and crop revenue in the meta-analysis. Figure 10 shows the overall average effect of agricultural input subsidies on revenue, profit and income is 0.17, 95% CI [0.10, 0.25] (χ^2=74.53 (*df*= 3), *p*=0.045; I^2=96.0%; *Tau*^{2}=0.0043). Again, although all studies indicate a positive impact on outcomes, tests of homogeneity indicate a high degree of between-study variability. This is at least in part likely to be due to the different types of measures that the studies use to capture revenue, profit and income; for example, as shown by the expected smaller effect sizes for income (which measures revenue minus costs) (Awotide et al., 2013; Mason & Smale, 2013) and expenditure (Chirwa, 2010) versus revenue (World Bank, 2014)[5]. Table 5 summarises all results of the meta-analyses for income.

[5] Chirwa (2010): household annual expenditure; Mason (2013): household income; Awotide et al. (2013): productivity, rice revenue; World Bank (2014): productivity, maize and rice revenue.

Figure 10: Effect of input subsidies on farm income

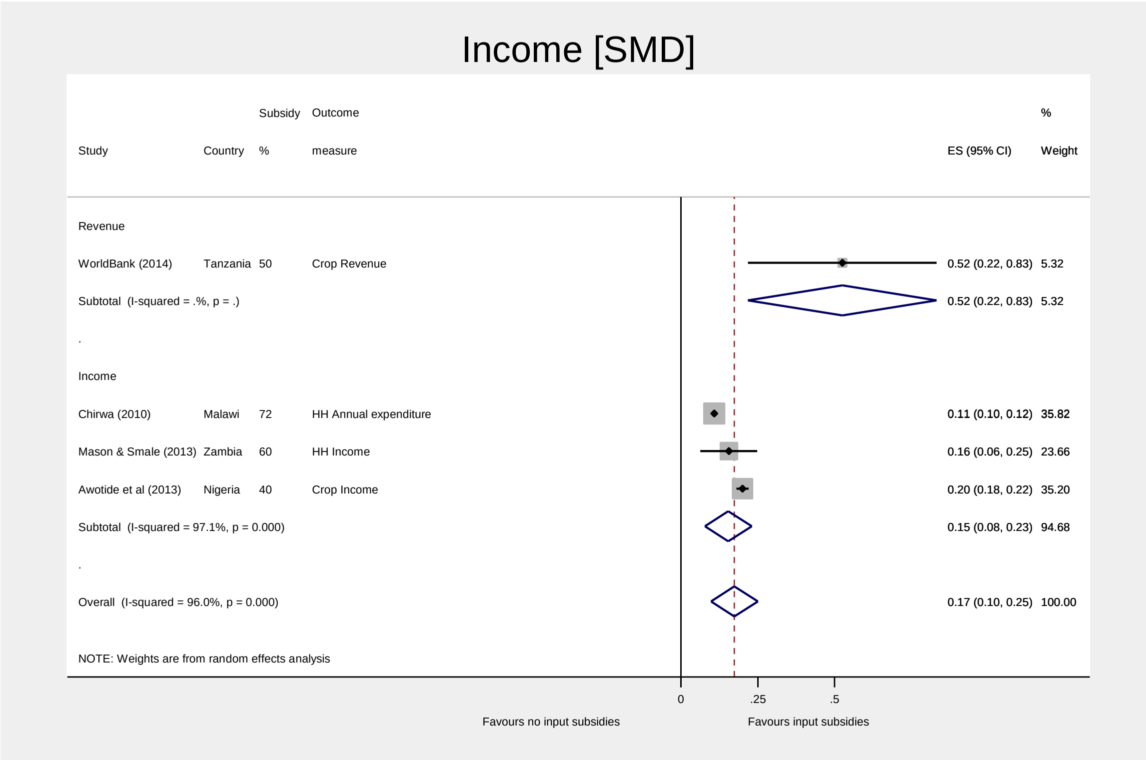


Table 5: Effect of input subsidies on farm income

Income	SMD	95% confidence interval		χ^2 (p)	No. obs.	I^2	Tau^2
Overall	0.17	0.10	0.25	74.53 (0.000)	4	96.0%	0.0043
Subgroup analysis by income type							
Income	0.15	0.08	0.23	68.08 (0.000)	3	97.1%	0.0039
Revenue	0.52	0.22	0.83	0.00	1	.	.
Subgroup analysis by input type							
Seeds input	0.20	0.18	0.22	0.86 (0.354)	2	.	.
Fertiliser and seeds	0.29	-0.12	0.69	7.06 (0.008)	2	85.8%	0.0745
Subgroup analysis by crop type							
Rice	0.20	0.18	0.22	0.14 (0.714)	2	.	0.0000
Maize	0.30	0.09	0.52	34.84 (0.000)	3	94.3%	0.0316
Subgroup analysis by risk of bias status							
High risk of bias	0.21	0.12	0.30	5.19 (0.075)	3	61.4%	0.0034
Moderate risk of bias	0.17	0.10	0.25	0.00	1	.	.

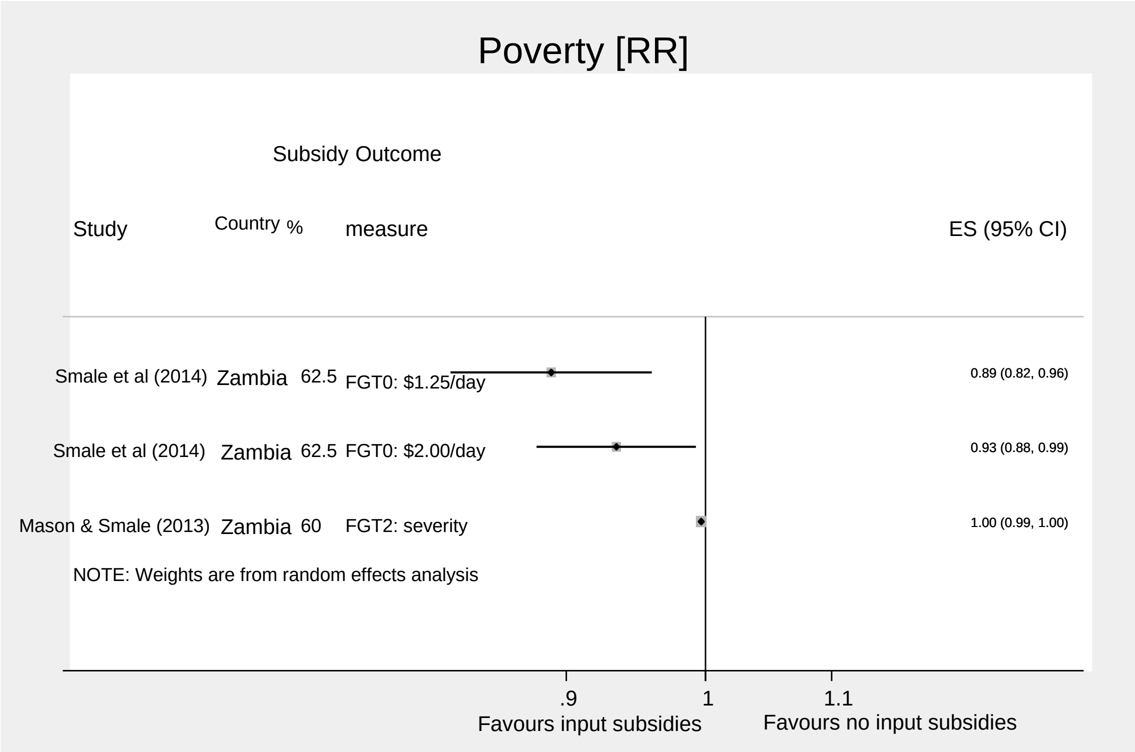
Income	SMD	95% confidence interval		χ^2 (p)	No. obs.	I^2	Tau^2
Subgroup analysis by study design							
RCT	0.20	0.18	0.22	0.00	1	.	.
Non-randomised study	0.17	0.06	0.28	8.04 (0.018)	3	75.1%	0.0062

Note: SMD = standardised mean difference.

Two studies provided three different measures of poverty reduction as shown in a forest plot (Figure 11)[6]. Table 6 shows results by risk of bias. Effect sizes for poverty reduction are calculated as risk ratios (RR). A reduction in poverty is measured as values of RR between 0 and 1. Increases in poverty are measured as values of RR greater than 1. All RR effect sizes can be interpreted as the percentage change for the treatment group over that for the comparison group. A study by Smale and Birol (2013) conducted in Zambia found an 11 percent decrease in the numbers of farmers living beneath the $1.25 poverty line and a smaller 7 percent decrease in those living beneath the $2.00 poverty line, whereas Mason and Smale (2011) found no significant effect on the severity of farm household poverty (the degree of inequality below the poverty line). Both studies providing outcomes data on poverty reported on interventions providing seed inputs for maize crops. In Figure 11, we do not provide an overall effect size, as the outcome constructs being measured are so different (see footnote 6).

[6] Smale & Birol (2013): Foster-Greer-Thorbecke (FGT) headcount ratio above poverty line of $1.25/day (FGTα=0). Smale & Birol (2013): FGT headcount ratio above poverty line of $/2.00/day (FGTα=0). Mason (2013): FGT poverty severity index (FGTα=2)

Figure 11: Effect of input subsidies on poverty among beneficiaries



Note: RR = risk ratio.

4.2.5 Meta-regression results

Theoretically, a larger subsidy can be expected to have a larger impact on outcomes of interest if it leads to greater absolute use of fertiliser and/or seeds. However, there are other factors that may mitigate or limit their impact (see *assumptions* in Figure 1).

To examine whether the size of subsidy had an impact on outcomes, we extracted information on subsidy size expressed as a percentage reduction in price wherever possible for our included studies (see Table 6)[7]. We then undertook meta-regression analysis to examine whether there was a correlation between size of subsidy and outcomes of interest: adoption, yield and income. Given the small sample of studies for each outcome of interest, we undertook a ‘naive’ analysis to assess the relationship between subsidy size and outcomes without controlling for covariates. The results should be interpreted with further caution because this analysis was based on very small sample sizes and we were unable to control for other potentially key variables.

Figures 12, 13 and 14 show the correlation between subsidy size and outcomes of interest, using meta-regression plots (sometimes called ‘bubble plots’), with each data point weighted by the inverse of study variance (relative weight of each study indicated by size of bubble). Table 7 summarises the results of the analysis.

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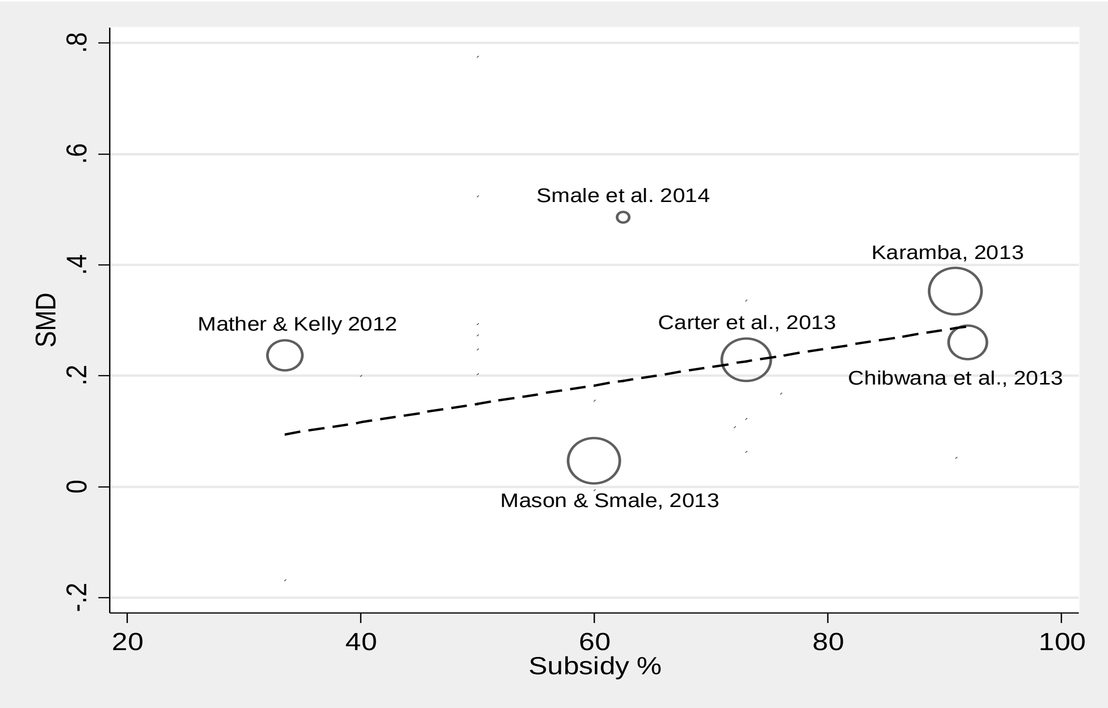
[7] We were able to do this for only ten included studies. Where papers associated with included studies did not provide this information, we undertook internet searches in order to confirm the size of subsidy for our included programmes. Where programmes provided a range of subsidy sizes (typically where programmes ran over multiple years or provided different subsidy rates for different inputs), we included the mid-point of this range in the meta-regression analysis.

The meta-regression indicates a small, positive relationship between subsidy size and adoption. Though not statistically significant, this relationship is in the expected direction, with larger subsidy sizes associated with higher use of subsidised inputs.

The meta-regressions also show small, negative relationships between subsidy size and yield as well as between subsidy size and income[8]. However, again these relationships are not statistically significant. Consequently, the meta-regression analysis provides no evidence of an association (positive or negative) between subsidy size and agricultural outcomes. We explore what other factors may help determine outcomes in a narrative synthesis below.

Figure 12: Meta-regression plot of adoption on subsidy size



[8] We excluded Mather and Kelly (2012) from this analysis due to the severe impact of poor irrigation infrastructure maintenance on the outcomes in this study. We excluded World Bank (2014) from the model 3 analysis on income as it provided data only in the form of revenue per acre, while the others all provided data in the form of income.

Figure 13: Meta-regression plot of yields on subsidy size

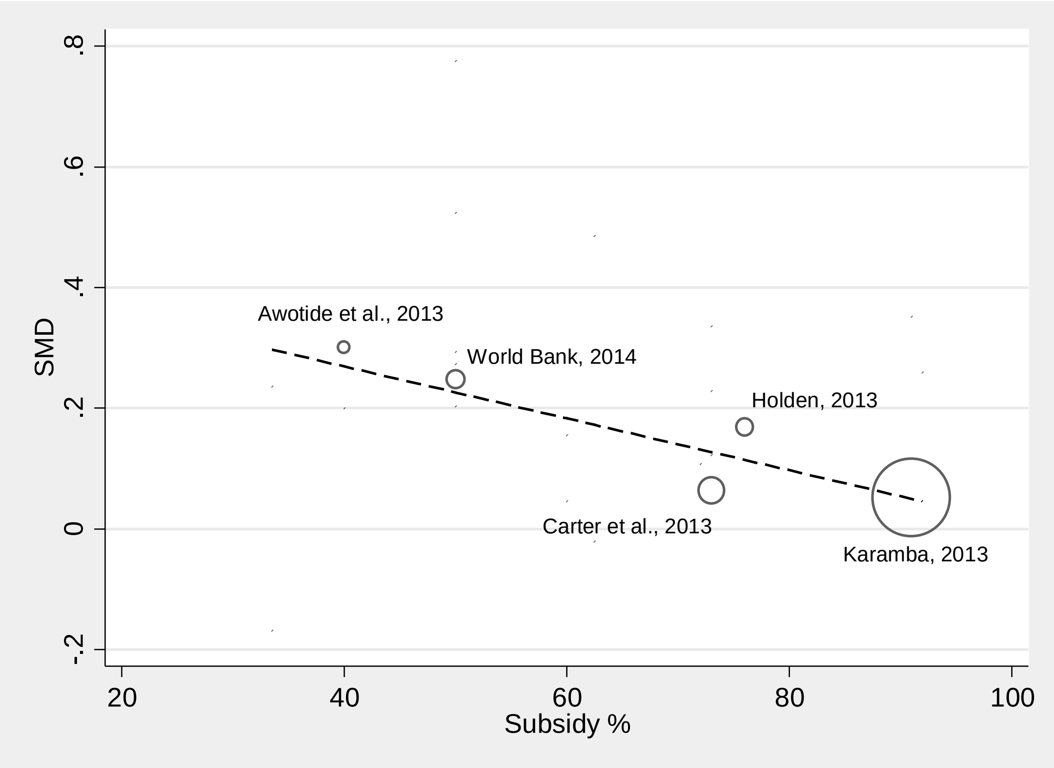


Figure 14: Meta-regression plot of income on subsidy size

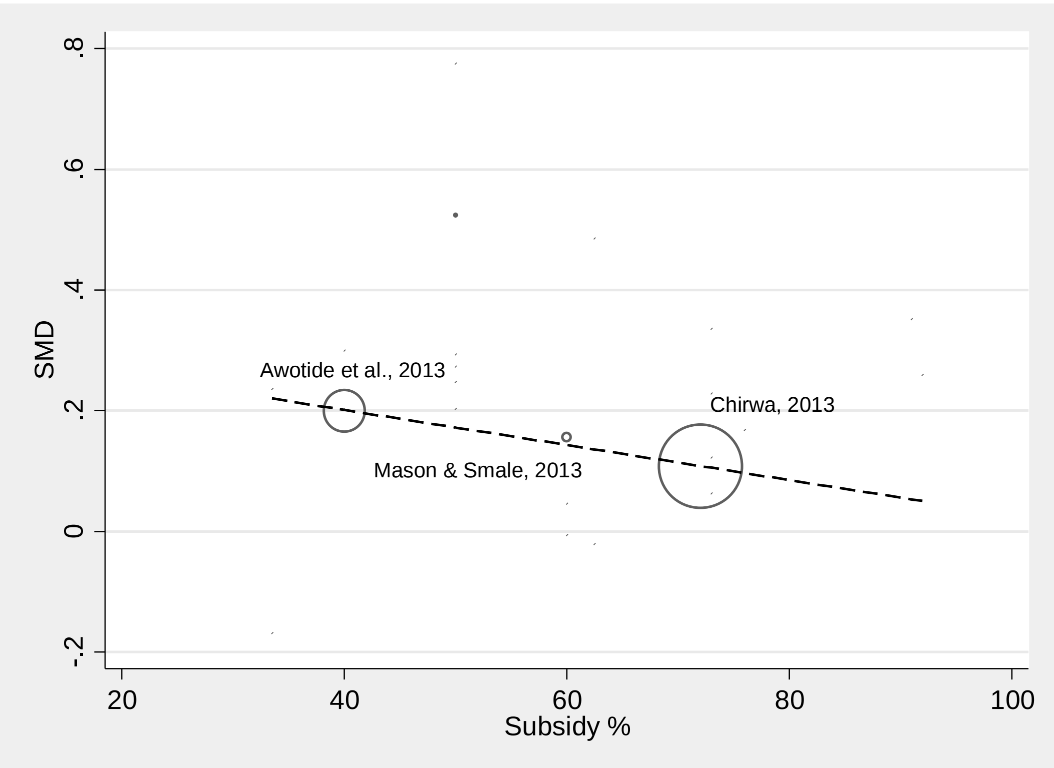


Table 6: Meta-regression analysis of agricultural outcomes on subsidy size

	Coefficient	***p>t***	***95%CIs***	
Model 1: adoption				
Subsidy	0.0033315	0.309	-0.0046205	0.0112836
Constant	-0.0171711	0.940	-0.6115783	0.5772361
Number of observations	6			
Tau^2	0.01333			
I^2 residual	78.92%			
Adjusted R^2	14.66%			
Model 2: yield*				
Subsidy	-0.0043009	0.055	-0.008785	0.0001832
Constant	0.441218	0.037	0.0522489	0.8301870
Number of observations	5			
Tau^2	0			
I^2 residual	0.00%			
Adjusted R^2	100.00%			
Model 3: income**				
Subsidy	-0.0028773	0.096	-0.0084151	0.0026605
Constant	0.3153725	0.056	-0.0380959	0.6688408
Number of observations	3			
Tau^2	0.000035			
I^2 residual	0.00%			
Adjusted R^2	98.62%			

Subsidy represents the percentage reduction in price of the subsidised agricultural input
**Excludes Mather & Kelly (2012)*
***Excludes World Bank (2014)*
Notes: We excluded Mather & Kelly (2012) from this analysis due to the severe impact of poor irrigation infrastructure maintenance on the outcomes in this study. We excluded World Bank (2014) from the model 3 analysis on income as it provided data only in the form of revenue per acre, while the others all provided data in the form of income.

4.3.4 Narrative synthesis of implementation and contextual factors (RQ1)

We systematically extracted and narratively synthesised data from included experimental studies to examine programme implementation, input subsidy delivery mechanisms, farmer take-up and usage of inputs, leakage of vouchers or inputs, and other associated factors. The extracted information is provided in full in Appendix 8. Here we summarise that information to explore the early stages and assumptions in our theory of change (Figure 1).

A survey by Carter et al. (2013) in Mozambique found that only 50 percent of farmers with the right to receive a voucher for input subsidies actually collected one. Around half of the farmers that did not collect vouchers cited a lack of money as being the critical factor, with a further 17 percent saying that they were absent at distribution time and 15 percent citing late voucher distribution as the key factor. Studies by Smale and Birol (2013) in Zambia and the World Bank (2014) in Tanzania also noted the high cost of inputs, even after subsidies had been applied. Beneficiary farmers participating in the National Agricultural Input Voucher Scheme (NAIVS) in Tanzania also received vouchers late, sometimes well after the beginning of the growing season (World Bank, 2014).

Receiving vouchers late may reduce their usefulness to farmers and therefore limit farmers' desire to buy subsidised inputs or to apply them on target crops in the current growing season. Four studies reported that farmers did not actually end up with the number of inputs to which their vouchers would have entitled them. In the case of Karamba's (2013) evaluation of the Malawi Farm Input Subsidy Programme (FISP), shortages at input supply points may have been a factor. In the case of Holden's (2013) evaluation of the Targeted Fertiliser Subsidy Programme in Malawi, corruption was a likely factor, something we explore later on in this section. A study by Kamanga (2010) in Malawi reported that village committees shared vouchers so farmers only received half of the inputs to which they were entitled, while a study by the World Bank (2014) reported that farmers themselves shared some of their inputs with their neighbours.

Four studies reported that farmers admitted to selling or exchanging some vouchers or inputs. This was always reported to be on a small scale, though authors often mentioned that the figures were probably underrepresented. Carter et al. (2013) reported that 4% of the farmers surveyed admitted to having sold fertiliser. Karamba (2013) and Awotide et al. (2014) also discovered that selling fertiliser was a problem; however, they did not report the results in figures. According to Holden and Lunduka (2012), 1% of farmers receiving subsidised inputs in Malawi admitted selling coupons, but this is likely to be an underestimate as around 25% of surveyed farmers said they were offered coupons on the secondary market. Chibwana et al. (2010) reported that there was some elite capture of coupons in villages, which may have followed from village chiefs or village committees being given a higher number of vouchers.

There were also reports of more systematic corruption related to the distribution on vouchers for input subsidies. A World Bank evaluation (2014) of the National Agricultural Input Voucher Scheme (NAIVS) in Tanzania reported that some vouchers were fraudulently redeemed. There were multiple rumours and some confirmed cases of district officials working with agro dealers to redeem vouchers for their own benefit. Holden and Lunduka (2012) mention that there was possible corruption in the tendering process to supply fertilisers for the Farm Input Subsidy Programme (FISP) in Malawi – contracts were offered to some suppliers that had prices up to 20% higher than their competitors. These authors also provided various anecdotal examples of corruption affecting FISP. They claimed that FISP was used to help secure the re-election of the president of Malawi in the 2009 election and gave examples of instances where a top political party member was caught with vouchers, a thief was jailed for selling vouchers but later released, and instances of illegal printing and circulation of fake coupons. According to Holden and Lunduka (2012), some farmers were asked to pay extra money to receive inputs, yet this money paid by farmers after the subsidy had been applied may not have ever been transferred to the Ministry of Agriculture. Clearly, corruption can have a fundamental impact on the delivery of inputs subsidies programmes. Holden and Lunduka (2012) concluded that transparency and accountability need to be foremost in the design of such programmes if this type of corruption is to be minimised.

Even when farmers received vouchers, they did not always use them as intended. Carter et al. (2013) report that take-up was very low, with only 28 percent of the treatment group using the package for maize production. Reasons given included using the vouchers on another crop (67%), not having used inputs at time of survey (25%) and selling inputs (4%). Delivery of inputs after the start of the growing season probably contributed to farmers' decisions not to use inputs or to use them on other crops altogether. Chirwa (2010) reported that farmers receiving inputs under the Starter Pack (TIP) programme in Malawi applied their fertiliser over a greater area than it was suitable for and were not advised how to apply it correctly. Kamaga (2010) echoed this, finding that farmers applied around 20 kg per acre rather than the recommended 100 kg per acre.

There is also some evidence that input subsidies 'crowded out' commercial inputs to some extent, with farmers reallocating at least some of the resources they would otherwise have spent on fertiliser or seeds (Carter et al., 2013; Holden, 2013; Mason & Smale, 2013). Holden (2013) estimated that one-third of fertilisers used in the Malawi FISP contributed to crowding out of commercial demand. The findings of the meta-regression analyses provide no evidence of an association (positive or negative) between subsidy size and agricultural outcomes[9].

It seems clear from the narrative synthesis that programme implementation and take-up vary from programme to programme, with important consequences for programme outcomes.

Finally, drought was also reported to have had a powerful impact on programmes in three cases (Holden & Lunduka, 2012; Mather & Kelly, 2012; Carter et al., 2013). Authors of these studies encourage future input subsidies programmes to include complementary components if farmers are expected to cope with severe-weather-related effects on their crops.

In conclusion, for subsidised inputs to produce intended primary outcomes, farmers need to receive subsidies and use them in sufficient quantities for them to be effective. However, we find evidence that there are several points at which the theorised impact pathway for input subsidies breaks down. There is evidence that subsidy vouchers do not always reach farmers in the quantities intended, even if they do reach farmers they are not always used, and as a result providing subsidised inputs may not necessarily increase the amount of inputs used by farmers in absolute terms.

4.2.6 Synthesis of modelling studies (RQ2)

We included 16 simulation models reporting on our secondary outcomes of interest relating to the effects of agricultural input subsidies on consumer welfare and wider economic growth. Studies modelling the effect of input subsidies against a counterfactual scenario either without or with an alternative subsidy were eligible for inclusion in the review. However, all included studies have a without subsidy baseline scenario. Study characteristics of included modelling studies are provided in Table 8. While experimental and quasi-experimental evidence examining our secondary outcome of interest was eligible for inclusion in the review, we did not find any studies that met these criteria.

Nine studies examined fertiliser subsidies, six studies examined fertiliser and seeds and one study looked at the provision of fertiliser, irrigation and electricity. Included studies

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[9] We excluded Mather and Kelly (2012) from this analysis due to the severe impact of poor irrigation infrastructure maintenance on the outcomes in this study. We excluded World Bank (2014) from the model 3 analysis on income as it provided data only in the form of revenue per acre, while the others all provided data in the form of income.

simulated effects on consumption of primary crops (n = 6), household expenditure (n = 1), agricultural commodity prices (n = 8), labour demand (n = 5), wages (n = 4), real incomes (n = 5), poverty incidence (n = 5) and GDP (n = 7). Included studies model increases, decreases, introduction and complete removal of subsidies. Programmes in included studies provided inputs at reduced costs through price reductions, either directly, through value-added tax (VAT) reduction on targeted inputs, or through the provision of vouchers. The percentage of the price covered by subsidies ranges from 8 to 96 percent of input prices.

All included studies used more than a single source of data for constructing their models. Several included studies used a social accounting matrix to calibrate models. Studies tend to use nationwide household surveys to model subsidy effects at the farm level. None of our included studies stipulates whether coefficients for micro-economic and household behaviour were derived from observational or experimental/quasi-experimental evidence. A range of other sources of data were used in included models such as government economic, agricultural, demographic and climate data as well as, in some cases, data from non-experimental primary studies.

In all, eight of the nine studies focusing on Malawi explicitly model either the entirety or part of the Farm Input Subsidy Programme (FISP). The programme provides free seeds and a price subsidy for fertiliser for up to a limited weight of fertiliser. The percentage of the price of fertilisers covered by the subsidy has varied in size over time and varies across models in included studies. One study (Tower, 1987, high threat to validity), models a fertiliser subsidy in Malawi but does not explicitly state it is the FISP. Two studies examine input subsidies in Ethiopia, one in Madagascar and one in Tanzania. The three included studies that focused on countries outside of sub-Saharan Africa were examining programmes in Indonesia (2) India (1).

Fourteen studies provided enough information to produce scatter plots showing the percentage point change in the price of the input covered by the subsidy against the absolute percent change in secondary outcomes of interest. The plots are provided to give a visual representation of how outcomes vary depending on the percentage point change in the price of inputs being subsidised. However, the plots do not show important mediating factors such as the percentage of farmers targeted or reached by the subsidy. Where studies simulate effects under more than a single scenario, both simulations are contained in the plots, potentially further biasing findings. Nonetheless, the plots provide a useful visual aid in understanding the range of effects reported in simulations from included studies. Where studies (n = 2) did not report enough information to include in the scatter plots, we reported their findings narratively only.

The large number of parameters that can potentially mediate the effects of subsidies and the complex nature of interaction between these parameters make precise estimates of effect in models difficult to achieve. The results from the modelling studies presented here should thus be interpreted as being general indicators of subsidies' effects on outcomes of interest. They should not be interpreted as providing precise estimates of effects.

Table 7: Characteristics of simulation modelling studies

Study	Study Type	Setting	Intervention	Outcome measure	Threat to validity
Arndt (2014)	CGE	Malawi	Fertiliser & seeds. Covers two-thirds of fertiliser price up to 100kg of fertiliser and provides a starter pack of free seeds. Effects are modelled under two scenarios, with subsides funded through direct taxation and through indirect taxation.	Production, Retail price, Labour, Poverty, GDP, Wage, Welfare	Low
Buffie & Atolia (2009)	CGE	Malawi	Fertiliser & seeds. Covers 65 percent of fertiliser price up to 100kg of fertiliser and provides a starter pack of free seeds Effects are modelled under two scenarios, with subsides funded through reduced infrastructure and through reduced infrastructure spending.	Retail price, Poverty, Real Income, GDP	Low
Caria (2011)	CGE & micro-model simulation	Ethiopia	Fertiliser. Covers 50 percent of fertiliser price	Production, Consumption, Wage, GDP	Low
Dorward & Chrirwa (2013)	PEM	Malawi	Fertiliser & seeds. Covers two-thirds of fertiliser price up to 100kg of fertiliser and provides a starter pack of free seeds	Real Income	Low
Douillet et al. (2012)	CGE	Malawi	Fertiliser & seeds. Covers 72 percent of fertiliser price up to 100kg of fertiliser and provides a starter pack of free seeds	Production, Retail Price, Poverty, Wage, GDP	Low

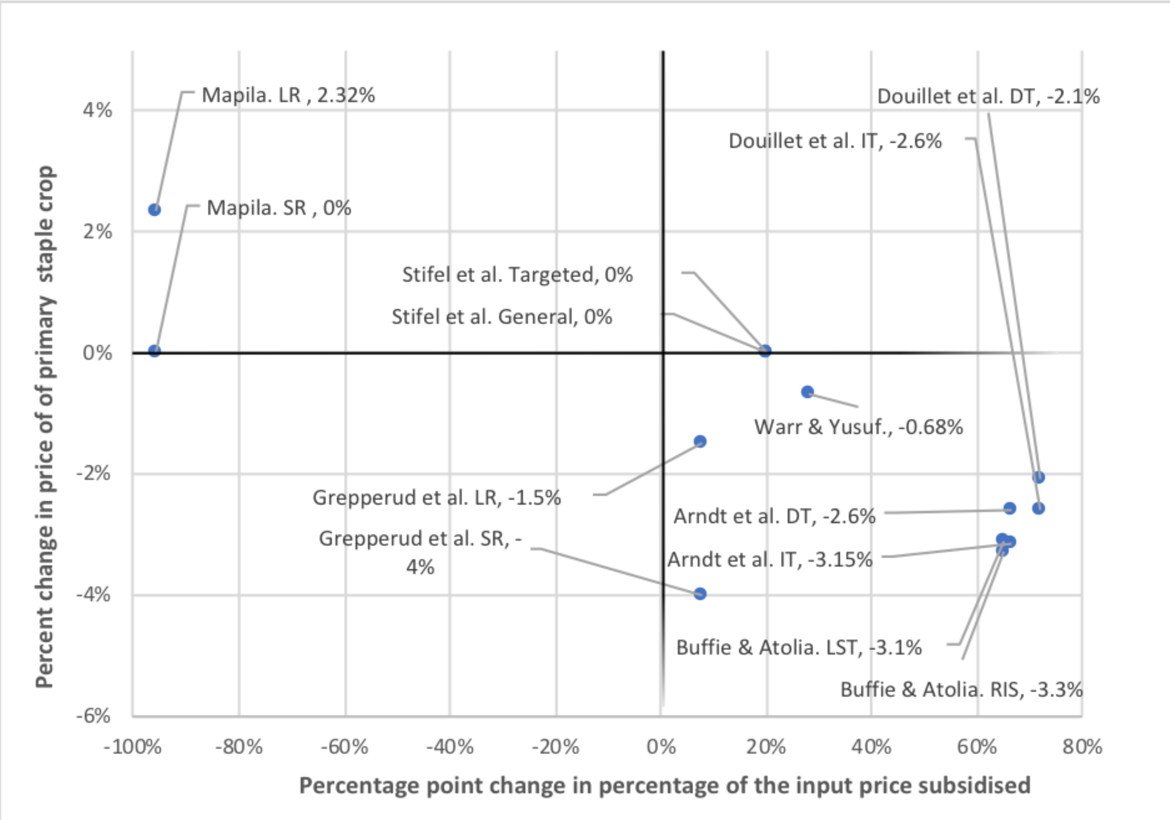
Study	Study Type	Setting	Intervention	Outcome measure	Threat to validity
Fan (2007)	Multi-equation investment model (time series data)	India	Fertiliser & seeds. Fertiliser, electricity and irrigation subsidies. No subsidy size provided.	Poverty	Low
Filipski (2011)	CGE	Ghana & Malawi	Fertiliser & seeds. Ghana: Fertiliser. Covers 29 percent of fertiliser price. Malawi: Covers two-thirds of fertiliser price up to 100kg of fertiliser and provides a starter pack of free seeds.	Welfare, income	Low
Govindan & Babu (2010)	Multiple-output and multiple input framework	Malawi	Fertiliser. Reduction of subsidy covering 25 percent of fertiliser price.	Labour demand	High
Grepperud (1999)	CGE	Tanzania	Fertiliser. Reduction in import tax on fertiliser leading to an implicit subsidy of 15 percent of fertiliser price Effects are modelled in the short run (10 years) and long run (20 years).	Production, Consumption, Retail Price, Real Income, GDP	Low
Holden & Lofgren (2005)	CGE	Ethiopia	Fertiliser. Models subsidy increases and decreases of 10 percent in subsidy covering 20 percent of fertiliser price	Labour	Low

Study	Study Type	Setting	Intervention	Outcome measure	Threat to validity
Mapila (2013)	PEM (recursive multi-equation)	Malawi	Fertiliser. Covers 96 percent of the fertiliser price up to 100kg of fertiliser and provides a starter pack of free seeds	Production, Consumption, Retail price, Labour, Poverty, GDP	High
Ricker Gilbert (2013)	Economic model estimations; (Arellano-Bond)	Malawi	Fertiliser & seeds. Models a doubling in weight and per capita quantity of fertiliser provided through subsidy programmes in Malawi and Zambia	Retail Price	Low
Rosegrant & Kasryno (1991)	Supply Demand Model	Indonesia	Fertiliser. Covers 62 percent of fertiliser price for rice farmers. Effects are modelled in the short run and long run (over five years).	Production	High
Stifel et al. (2004)	Multimarket model	Madagascar	Fertiliser covers 20 percent of fertiliser price	Production, Consumption, Retail price, Poverty, Real Income,	Low
Tower (1987)	CGE	Malawi	Fertiliser. Covers 10 percent of fertiliser price	Production, Consumption, Labour, Wage,	High
Warr & Yusuf (2014)	GEM	Indonesia	Fertiliser. Covers 43 percent of fertiliser price	Production, Consumption, Retail price, Labour, Poverty, GDP	Low

Note: CGE: Computable generalised equilibrium model. PEM: Partial Equilibrium Model. GEM: General Equilibrium Model

Eight studies model the effects of fertiliser subsidies on retail prices of staple crops at the national level (Figure 15). One study, Stifel et al. (2004), also provides measures of the effect on prices of other food and non-food items. Seven of these eight studies provide information on the percentage point change in the percentage of the input price covered by the subsidy and the percent change in the prices of staple foods for consumers.

Figure 15: Effect of input subsidies on price of staple crop



**LR: long run, SR: short run. IT: indirect tax, DT: direct tax. RIS: reduced infrastructure spending. LST: lump sum taxes.*

Six studies show an increase in the percentage of fertiliser price covered by subsidies results in a decrease in crop price, while one study shows no effect (Stifel et al., 2004). One study finds a reduction in subsidy results in no effect on price in the short run and an increase in the long term when dynamic effects are incorporated into the model (Mapila, 2013, high threat to validity).

Four studies examine the impacts of the Malawi input subsidy on consumer maize prices in the country. Arndt et al. (2014) estimates that when the subsidy covers two-thirds of fertiliser costs for farmers, it results in a 3.15 percent decrease in maize prices when financed through indirect taxes and a 2.6 percent decrease when financed through direct taxes.

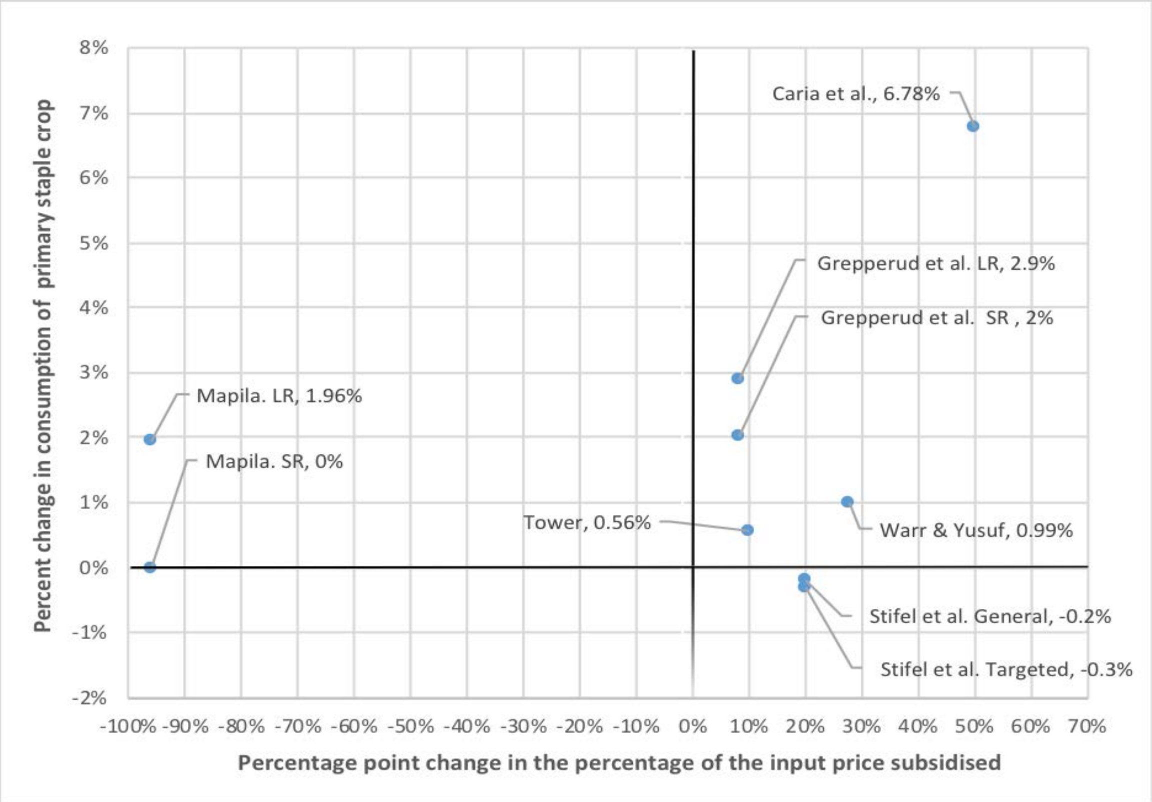
Douillet et al. (2012) also simulate the effects of the subsidy covering two-thirds of fertiliser price and somewhat smaller effects with 2.6 percent decrease when funded through indirect taxes and a 2.1 percent decrease when financed through direct taxes. Buffie & Atolia (2009) simulate the effects of the subsidy where it covers 65 percent of fertiliser price. They find that the subsidy results in a 3.1 percent reduction in the price of domestic foodstuffs where the programme is funded through lump sum taxes and a 3.3 percent reduction when funded through reduced infrastructure spending.

Mapila (2013, high threat to validity) shows that a complete removal of subsidy leads to an increase in crop price. The model simulates the effect of complete removal of subsidy in Malawi where the fertiliser subsidy covers 96 percent of fertiliser price. The author finds no short-term impact on prices but a long-run effect of a 2.3 percent price increase when dynamic effects are incorporated into the model.

Warr & Yusuf (2014) find a 27.8 percentage point increase in the percentage of the price of fertiliser subsidised in Indonesia results in a 0.68 percent decrease in rice prices. Stifel et al. (2004) find a 20 percent fertiliser price subsidy in Madagascar to have no effect on rice prices irrespective of whether it is targeted at poorer households or made available to the general population.

Rickert-Gilbert (2013) examine whether or not, and to what extent, an increase in the quantity of subsidised fertiliser allocated to districts in Malawi and/or Zambia affects retail maize prices. They estimate that if Malawi were to double the quantity of fertiliser delivered through its subsidy programme it would reduce the price of maize on average by between 1.2 and 1.6 percent, while in Zambia a doubling of the quantity of fertiliser delivered through its subsidy programme would reduce prices by between 2 and 2.8 percent.

Figure 16: Effect of input subsidies on consumption of staple crop



**LR: Long run, SR: short run.*

Six studies simulate the effects of subsidies on consumption of one or more staple crops primarily targeted by the subsidy at the national level. One study also reports changes in consumption of other food and non-food items (Stifle et al., 2004) while another study reports effects on expenditure on staple foods (Rosegrant & Kasryno, 1991, high threat to validity).

For each of these six studies, we extracted data on both the percentage point change in the percentage of the input price covered by the subsidy and the percent change in consumption of the primary staple crop (Figure 16).

Four studies show an increase in subsidy leads to an increase in consumption of the primary staple food targeted by the subsidy. One study, Stifel et al. (2004), shows an increase in the percentage of input price covered by subsidy results in a negative effect on rice consumption, the primary crop targeted by the subsidy, but an increase in consumption of other staple and non-staple foods (Stifel et al., 2004). The study by Mapila (2013, high threat to validity) finds complete removal of subsidy results in no change in maize consumption in the short run. However, it finds an increase of 1.96% in the long run when dynamic effects are included in the model.

Caria et al. (2013) simulate a subsidy covering 50 percent of fertiliser costs for farmers in Ethiopia. They find the subsidy results in a combined 6.78 percent increase in the consumption of the three main staple produced (teff, wheat and maize).

Grepperud et al. (1999) simulate a reduction in import taxes on fertilisers in Tanzania, which results in an implicit subsidy, resulting in an eight percentage-point reduction in fertiliser price for farmers. They find a short-run (10-year) effect of a two percent increase in maize consumption and a long-run (20-year) effect of a 2.9 percent increase.

Two studies simulate the effects of subsidies on consumption of maize in Malawi. Mapila (2013, high threat to validity) simulates the removal of the subsidy at 2012 levels when the programme provided a starter pack of free seeds and a price subsidy covering 96 percent of the market price for up to 100 kg of fertilisers for farmers. The author finds the removal of the subsidy to result in no change in maize consumption in the short run and a 1.96 percent increase in the long run. This is attributed the lack of effect in the short run to the price inelasticity of maize consumption in Malawi but provides no explanation of the long-run effects. In contrast, Tower (1987, high threat to validity) simulates the effects of a subsidy covering 10 percent of fertiliser price in Malawi and finds it results in a 0.56 percent increase in maize consumption.

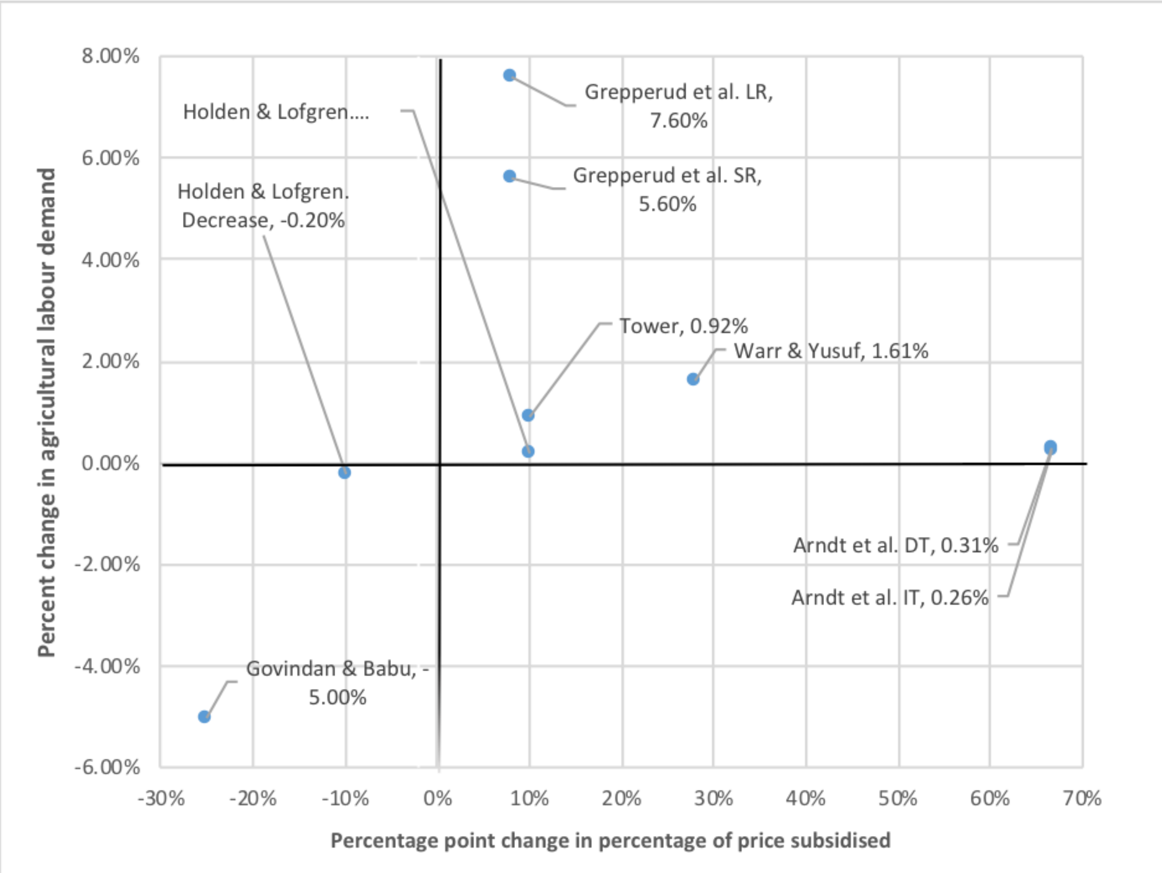
Stifel et al. (2004) simulate the effects of a subsidy covering 20 percent of fertiliser prices on consumption of a number of both food and non-food items in Madagascar. They find a 0.2 percent reduction in rice consumption when the subsidy is made available to the general population and a 0.3 percent reduction when the subsidy is targeted at the poor. They attribute this negative effect to increasing consumption of other staple and non-staple foods, which they find to increase by an average of 0.6 percent (no targeting) and 0.48 percent (poor targeted) under a with-subsidy scenario. They also find the subsidy results in a 1.5 percent reduction in the consumption of non-food items when targeted and 0.9 percent reduction when targeted at the poor.

Warr & Yusuf (2014) simulate a 27.8 percentage point increase in the percentage of the price of fertiliser covered by subsidy (from a baseline subsidy of 15.9 percentage of price covered) in Indonesia. This results in 43.7 percent of the price of fertiliser being covered by subsidy. They find the increase in subsidy to result in a one percent increase in rice consumption.

Rosegrant and Kasryno (1991, high threat to validity) simulate the effect of the removal of a subsidy covering 66.2 percent of fertiliser price in Indonesia. They find a 7.9 percent increase in consumer expenditure on rice in the short run (five years) and 6.7 percent increase in the long run.

Six studies report effects of subsidies on employment in the agricultural sector or demand for unskilled or agricultural labour in the economy. For each of these six studies we extracted data on both the percentage point change in the percentage of the input price covered by the subsidy and the percent change in agricultural labour demand (Figure 17).

Figure 17: Effect of input subsidies on agricultural labour demand



*LR: long run, SR: short run. IT: indirect tax, DT: direct tax.

All six studies that examine the effects of subsides on labour demand or employment in the agricultural sector find a positive effect. Three studies simulate the effect of input subsidies on demand for labour and agricultural wages in Malawi while a further study examines the subsidies impacts on agricultural wages alone. Four studies report subsidy effects on agricultural wages and are reported below narratively.

Arndt et al. (2014) simulate the effect of the subsidy providing free seeds and two-thirds of the cost of fertilisers under two scenarios. They find a 2.6 percent increase in the share of employment in agriculture and a 4.2 percent increase in the average agricultural wage when the subsidy is funded through indirect taxes. They find a 3.1 percent increase in employment in agriculture and a seven percent increase in the average agricultural wage when the subsidy is funded through direct taxes.

Govinindan and Babu (2010, high threat to validity) simulate a 25 percentage point reduction in the percentage of fertiliser price subsidised. They find a five percent decrease in labour demand in the economy as a whole. Tower (1987, high threat to validity) simulates a subsidy covering ten percent of fertiliser price and finds a 0.9 percent increase in employment in the smallholder sector and 0.57 percent increase in agricultural wages.

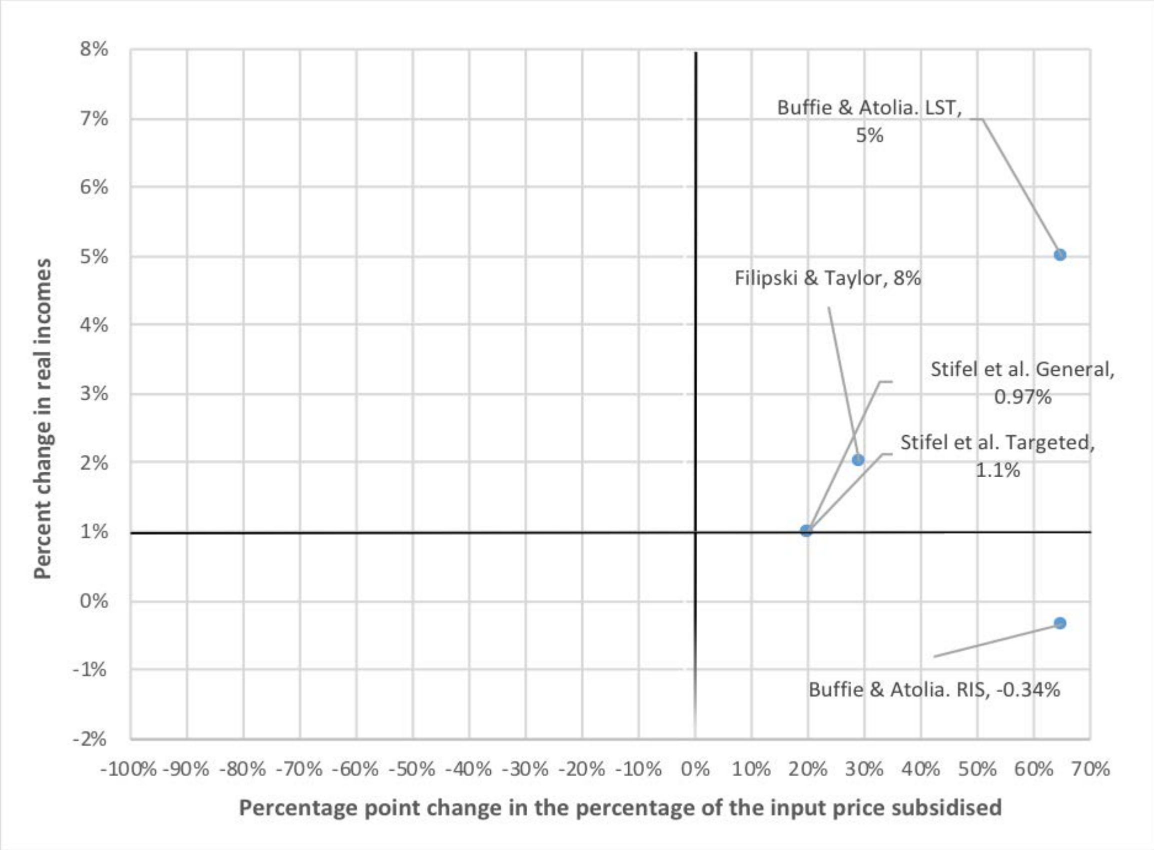
Holden and Lofgren (2005) simulate both an increase and a decrease in the percentage of fertiliser price being subsidised in Ethiopia. Under the first scenario, they model an increase in the percentage of the price being subsidised from 20 percent to 30 percent and find a 0.2 percent increase in labour demand in the rural economy. Under the second scenario, they simulate a decrease in subsidy from 20 percent of fertiliser price covered to ten percent and find a 0.2 percent decrease in labour demand. Caria et al. (2011) simulate the introduction of a subsidy covering 50 percent of the price of fertilisers in Ethiopia and find it results in a 0.24 percent increase in agricultural wages.

Grepperud et al. (1999) simulate an implicit eight-percentage point increase in the percentage of fertiliser price covered by subsidy in Tanzania. They find a 5.6 percent increase short-run effect (ten years) and 7.6 percent increase long-run (20-year) effect in labour demand.

Warr & Yusuf (2014) simulate an increase in the percentage of the fertiliser price being subsidised in Indonesia from 15.9 percent to 43.7 percent. They find this results in a 1.6 percent increase in the demand for unskilled labour.

Four studies simulate the effects of subsidies on real incomes among the general population at the national level and are plotted in Figure 18. For each of these four studies we extracted data on both the percentage point change in the percentage of the input price covered by the subsidy and the percent change in real incomes (Figure 18).

Figure 18: Effect of input subsidies on real incomes



Three studies examine the effects of inputs subsidies in Malawi on real incomes. Dorward and Chirwa (2013) simulate effects for the years 2005 to 2011, during which time the programme covered two-thirds of the cost of fertilisers as well providing starter seed packs and find an 11 percent increase in real incomes.

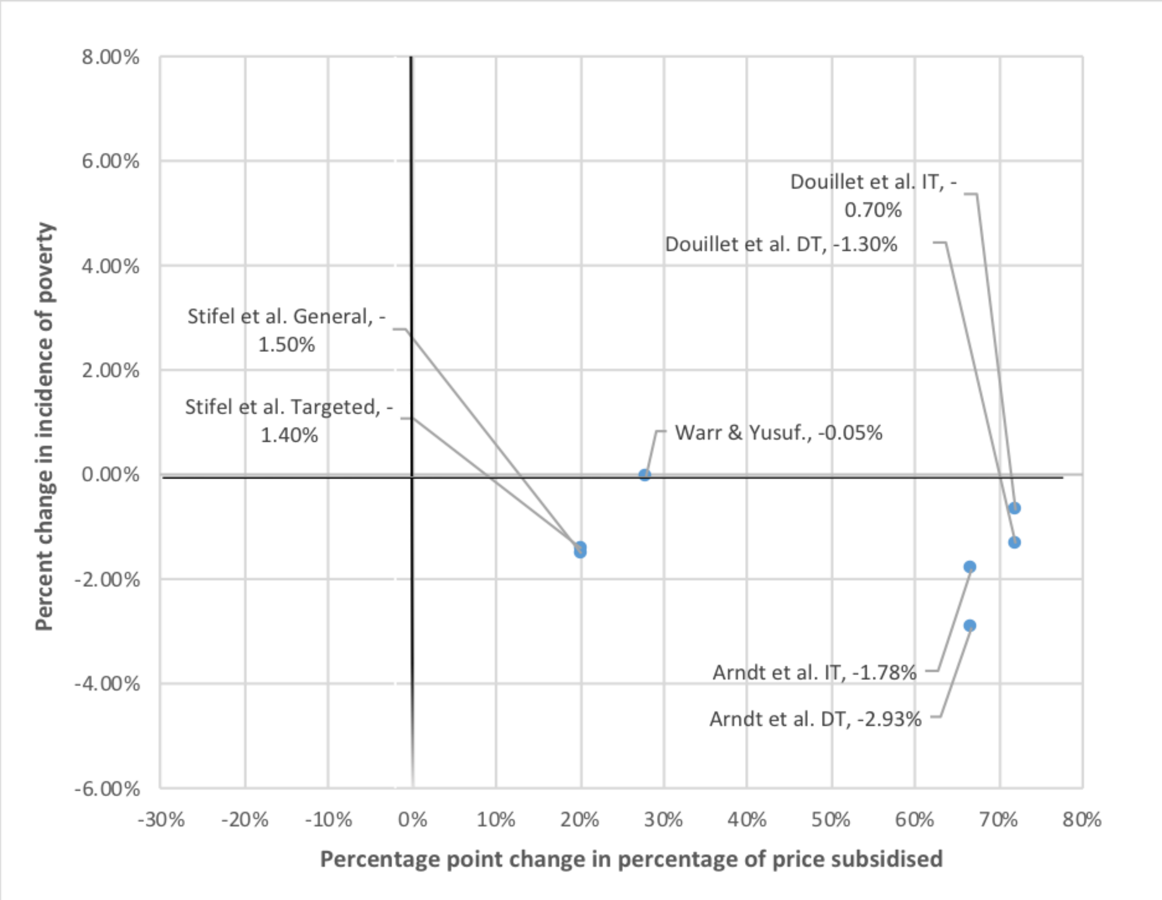
Buffie and Atolia (2009) model the programme at a 65 percent price subsidy and find a smaller but still sizable impact of a 4.9 percent increase in real incomes when the programme is funded through lump sum taxes but a 3.4 percent decrease when funded through a reduction in infrastructure spending.

The simulation by Stifel et al. (2004) of the effects of a 20 percent fertiliser price subsidy given to rice farmers in Madagascar shows a 0.97 percent increase in incomes when the subsidy is targeted at the poor and a 1.1 percent increase when the subsidy is not targeted.

Filipski and Taylor (2011) find that subsidies covering a free seed pack and two-thirds of fertiliser costs up to 100 kg of fertiliser result in a 0.8 percent increase in nominal incomes among non-beneficiaries.

Five studies report effects of subsidies on incidence of poverty, all of which show a positive effect on poverty reduction of between 0.05 percent and 2.93 percent. Four of these studies provide data on the percentage point change in the percentage of the input price covered by the subsidy and percent change in poverty incidence and are plotted in Figure 19.

Figure 19: Effect of input subsidies on poverty incidence



* IT: indirect tax, DT: direct tax.

Arndt et al. (2014) find the input subsidy programme in Malawi covering seeds and two-thirds of the price of fertiliser results in a 1.78 percent reduction in incidence of poverty when funded through indirect taxes and a 2.93 percent reduction when funded through direct taxes.

Douillet et al. (2015) model the subsidy covering seeds and 72 percent of fertiliser price and also find positive, albeit smaller, effects with a 0.7 percent reduction in incidence of poverty when funded through indirect taxes and a 1.3 percent reduction when funded through direct taxes.

Stifel et al. (2004) find the subsidy programme in Madagascar covering 20% of the price of fertiliser leads to a 1.5 percent reduction in poverty when targeted at the general population and a 1.4 percent reduction when targeted at the poor.

Warr & Yusuf's (2014) simulation of a 27.8 percentage point increase in percentage of fertiliser price covered by subsidy in Indonesia results in a 0.047 percent reduction in poverty incidence.

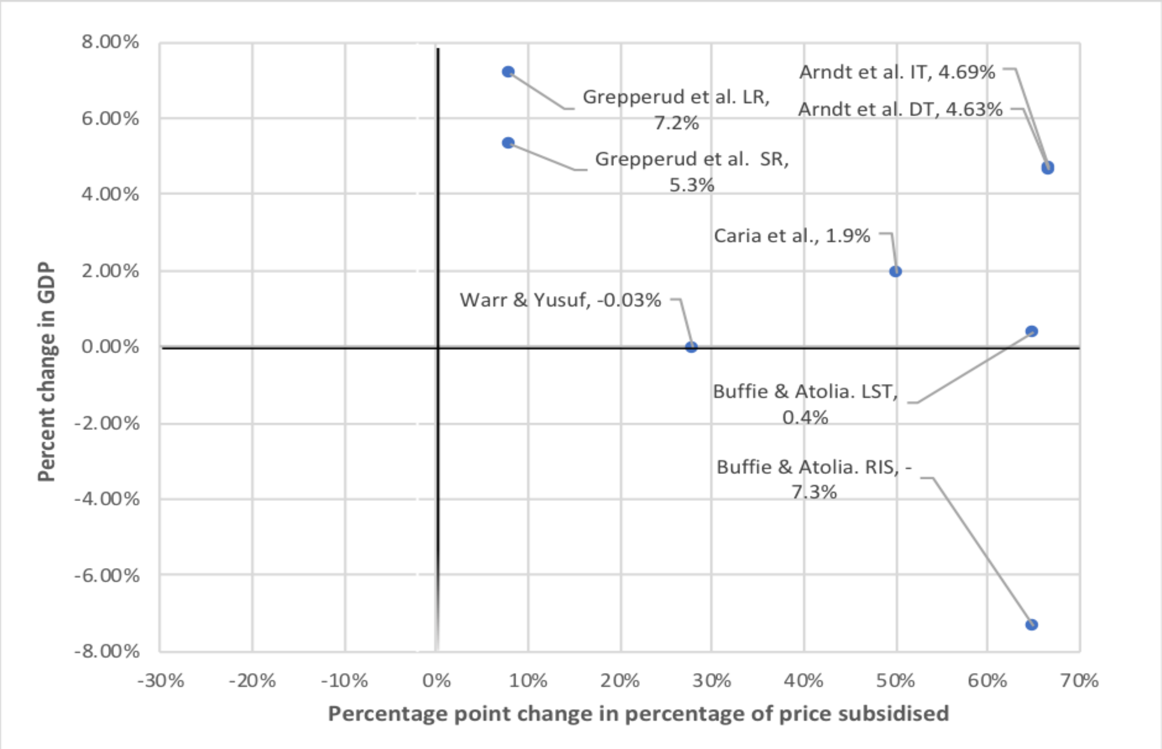
Fan (2007) estimates effects of irrigation, fertiliser and power subsidies on rural poverty reduction in the 1960s through to the 1990s. He finds average returns in terms of the decrease in the number of poor people per million population per every million rupees spent across the four decades to be 110, 105 and 60 for irrigation,[10] fertiliser and power subsidies, respectively. He finds the cost-benefit ratio in terms of poverty reduction for fertiliser subsidies to have decreased from 166 poor per million population per million rupees spent in the 1960s to only 24 in the 1990s. For electricity subsidies the returns fell from a reduction of 166 poor per million population per million rupees spent in the 1960s to 24 in the 1990s. For irrigation returns fell from 182 poor per million to 113 in the 1980s (with no data available for the 1990s).

Two studies (Filipski & Taylor, 2011; Arndt, 2014) provide measures of the effects of subsidies on welfare in the population where welfare is calculated as compensating variation. Arndt (2014) estimates subsidies in Malawi to result in a 2.79 percent increase in welfare. Filipski and Taylor (2011) find subsidies in Ghana result in no welfare increase while subsidies in Malawi result in a 0.8 percent welfare increase.

Five studies report effects of subsidies on incidence of GDP. All five studies provide data on the percentage point change in the percentage of the input price covered by the subsidy and percent change in GDP and are plotted in Figure 20.

One study finds the effect of the subsidy on GDP to be 0.4 percent when financed through indirect taxes but -7.3 percent when it is funded through reduced infrastructure spending.

Figure 20: Effect of input subsidies on GDP



*LR: long run, SR: short run. IT: indirect tax, DT: direct tax. LST: lump sum taxes, RIS: reduced infrastructure spending.

[10] Irrigation data for the 1990s is missing from his analysis.

Warr & Yusuf's (2004) simulation of a 27.8 percentage point increase in price subsidy for fertilisers in Indonesia finds the subsidy results in a .033 percent reduction in GDP. Grepperud et al. (1999) simulates the effect of an implicit input subsidy through reduced import taxes on fertiliser in Tanzania. They find a 5.3 percent increase in GDP in the short run (5.7 percent increase in agricultural GDP and 7.9 percent increase in non-agricultural GDP) and a 7.2 percent increase in the long run (five percent increase in agricultural GDP and 6.7 percent increase in non-agricultural GDP).

Fan (2007) estimates effects of irrigation, fertiliser and power subsidies on returns to agricultural GDP from the 1960s to the 1990s. The author measures returns as a ratio of rupees spent to rupees returned through agricultural GDP, finding average effects in the across the four decades to be 2.11, 1.71, 1.09 and 2.11 for fertiliser and power subsidies and irrigation (with irrigation data for the 1990s missing) respectively. He finds the cost-benefit ratio for fertiliser and power subsidies in the 1990s to be less than half what they were in the 1960s.

4.2.7 How changes in underlying assumptions affect secondary outcoe sof interest (RQ2)

Modes of funding

How input subsidies are funded can affect both the subsidies effectiveness and opportunity costs. Introducing or increasing the size of input subsidies may affect other areas of public expenditure, resulting in counterproductive effects on intended outcomes. For instance, where subsidies are funded through reduced spending in other areas of agricultural support, such as extension services or rural infrastructure, this may lead to a net negative effect.

Three included studies simulate effects of subsidies under different funding mechanisms, all of which focus on Malawi. Two studies (Douillet et al., 2012; Arndt et al., 2014) look at subsidy effects when financed through direct taxation and indirect taxation, while one study (Buffie & Atolia, 2009) examines effects when subsidies are funded through increases in lump sum taxes or through a reduction in infrastructure spending.

Arndt et al. (2014) find funding subsidies through direct taxation to be more effective across a range of outcomes modelled with greater reduction in price of staple foods, increased demand for labour and agricultural wages and poverty reduction but slightly lower GDP growth than under indirect taxes.

Douillet et al. (2012) find a slightly larger reduction in the price of staple foods and a slightly higher GDP under indirect taxation but a more substantial increase in agricultural wages and a greater reduction in poverty under direct taxation. Buffie & Atolia (2009) simulate the effects of subsidies funded through lump sum taxes or through reductions in infrastructure spending. They find broadly positive effects under funding through lump sum taxes but detrimental effects when subsidies are financed through reduced infrastructure spending it results in reduced agricultural wages, real incomes and GDP.

World Input Prices

The world price of inputs and agricultural commodities can influence the effectiveness and cost-benefit ratio of subsidy programmes. Where world prices of agricultural export crops change, so too do benefits to beneficiaries, consumers and the wider economy.

Fluctuation in world input prices can strongly influence the overall costs and associated macroeconomic impacts. As world prices rise, the cost of subsidising a given percentage of fertiliser prices for beneficiaries becomes more expensive. As such, how changes in world prices are simulated in models has an important bearing on the estimated effects.

Arndt et al. (2014), Buffie and Atolia (2009) and Douillet (2012) all provide simulations of Malawi's FISP under different world fertiliser price scenarios.

Arndt et al. (2014) find subsidy effects on household welfare (measured as equivalent variation in consumption) and poverty decline as world fertiliser prices increase. They find increases in world fertiliser prices of between zero and 50 percent result in welfare effects between 2.79 and two percent and poverty reduction effects between -1.78 and .9 percent while programme costs increase by 64 percent and programme cost-benefit ratios decrease from 1.62 to 1.22.

Buffie and Atolia (2009) model subsidy effects at three different ratios of world fertiliser price to subsidised prices (ratios of world to subsidised price of two, three and five). They find large differences in effect under differing world price scenarios with reductions in maize prices varying from -2.2 to -5 percent and increases in real incomes varying from between 2.9 percent and nine percent when the programme is funded through lump sum taxes.

Douillet et al. (2012), simulates fertiliser and fuel price shocks under a 'with and without' subsidy scenario. They find GDP at factor cost to be between 3.6 to 3.8 percent higher in a with subsidy scenario than in a without subsidy scenario in both fertiliser and fuel price shock scenario. The remaining studies do not report results under differing world price scenarios. Several included studies do, however, mention dissipating returns to subsidies where world input prices increase but do not provide quantitative measures of differing effect under different world prices (for instance Stifel et al., 2004; Chirwa & Dorward, 2009; Warr & Yusuf, 2014).

Target Beneficiaries

How subsidy programmes are targeted is an important factor affecting both the scale of programmes and their potential impacts on consumers. Only one included study, Stifel et al. (2014), simulates subsidy effects under different targeting modalities. They model the fertiliser input programme in Madagascar targeted at both the poor and the general population. They find a marginally larger impact on poverty reduction under general targeting with a 1.5 percent reduction compared to a 1.4 percent reduction under a general targeting scenario. However, they find targeting poorer farmers results in a larger effect on real incomes with poor and general targeting scenarios resulting in 1.13 percent and a .98 percent increases, respectively.

Another important issue in estimating the effects of a subsidy is the degree of efficiency in targeting beneficiaries. One of our included studies, Buffie and Atolia (2009), simulates subsidy effects in Malawi under a scenario of perfect targeting where 100% of subsidies reach intended beneficiaries and under an alternative inefficient targeting scenario where 35 percent of subsidy goes to farmers other than intended beneficiaries. Perhaps unsurprisingly, they find perfect marginal targeting to lead to better outcomes regardless of funding mechanism or world fertiliser price.

5. Discussion

5.1 Summary of main results

This systematic review synthesised evidence on the effectiveness of agricultural input subsidies in improving productivity and farm incomes as well as consumer welfare and wider growth in low- and lower-middle-income countries. In order to explore outcomes across the whole theory of change, we considered a wide range of evaluation designs and methodologies for inclusion. We searched academic and online databases, carried out citation tracking of included studies and contacted experts to ensure our search was as comprehensive as possible.

We identified 15 experimental and quasi-experimental studies assessing the effectiveness of agricultural input subsidies on adoption, productivity and farm incomes. We also identified a further 16 studies that use econometric models that simulate the effect of agricultural input subsidies on measures of consumer welfare and wider growth.

The majority of included studies, 27 of 31, are from sub-Saharan Africa with almost half of all studies from Malawi alone. Of the remaining studies, two are from each of Indonesia and India. Overall, there is a limited evidence base, a lack of evidence pertaining to input subsidies programmes outside of sub-Saharan Africa and a focus of the evidence on the particular case of input subsidies in Malawi.

We calculated effect sizes from experimental and quasi-experimental studies for adoption, productivity, household income and poverty. Meta-analysis of seven studies indicates an overall positive and statistically significant average effect for agricultural input subsidies on adoption (SMD=0.23, 95% CI [0.08, 0.38]). For productivity, across seven studies, we find a positive average effect (SMD=0.09, 95% CI [-0.04, 0.22]). However, this finding is not statistically significant. Of the studies included in this meta-analysis, only a single study found a non-positive effect on yield, which the authors (Mather and Kelly, 2012) attribute to significant flooding in treatment areas. If this outlier is removed from the analysis, the average effect on yields is positive and statistically significant (SMD=0.11, 95% CI [0.05, 0.18]). Meta-analysis also found a positive and statistically significant average effect on various measures of on revenue, profit and income across four studies is (SMD=0.17, 95% CI [0.10, 0.25]). Only two studies report effects on poverty. Only two studies report the effects of agricultural input subsidies on poverty, making it difficult to draw any clear conclusion. A study by Smale and Birol (2013) conducted in Zambia found an 11% decrease in the number of farmers living beneath the $1.25 poverty line and a smaller 7% decrease in those living beneath the $2.00 poverty line. However, Mason and Smale (2011) found no significant effect on the severity of farm household poverty (the degree of inequality below the poverty line).

Meta-regression provides no evidence of an association (positive or negative) between subsidy size and agricultural outcomes.

We systematically extracted and narratively synthesised data from included experimental and quasi-experimental studies to examine programme implementation, input subsidy delivery mechanisms, farmer take-up and usage of inputs, leakage of vouchers or inputs, and other associated factors. We find evidence that there are several points at which the theory of change for input subsidies breaks down. Subsidy vouchers do not always reach farmers in the quantities intended, and even if they do reach farmers, they are not always used as intended. As a result, providing subsidised inputs may not necessarily increase the amount of inputs used by farmers in absolute terms.

We extracted data from included econometric modelling studies on consumer welfare and economic growth related outcomes including food prices, consumption, labour demand and agricultural wages, poverty incidence and GDP. Where possible we then displayed effect sizes in scatter plots to provide an overall picture of the relationship between the percentage point changes in the percentage of the input price subsidised and the outcome of interest.

Model simulations in included studies tend to follow the theory of change presented in this review, with an introduction or increase of subsidies leading to positive effects on secondary outcome measures and a reduction leading to negative effects. However, only small amounts of the variation in outcome measures in the scatter plots are explained by the percentage point change in the percentage of the price covered by the subsidy.

It should also be borne in mind that the functional relationships specified between sectors, agents, goods and prices in models are in most cases approximations. How these relationships are specified can influence the output of models to a very great degree. For instance, a number of modelling studies simulate broadly similar changes in fertiliser subsidies in Malawi over a similar time-period with, in some cases, quite different results (see reporting of studies in section 4.9 by Buffie & Atolia, 2009; Douillet et al., 2012; Dorward & Chirwa, 2013, Mapila, 2013; Arndt et al., 2014). As such, findings from modelling studies should be interpreted with caution. Nonetheless, it is encouraging that included studies quite consistently show positive effects, albeit of a relatively small magnitude, in consumer welfare and economic growth.

Where modelling studies include simulations under different scenarios, they offer some valuable insights into how the effects of subsidies can change depending on factors both endogenous and exogenous to the design and implementation of subsidies. Factors such as how subsidies are funded, prices of inputs on world markets and the format and efficiency of beneficiary targeting can all play important roles in determining the effectiveness of input subsidies and their relative value compared to alternative policy options for agricultural development and poverty alleviation.

5.2 Overall completeness and applicability of evidence

We found 15 experimental and quasi-experimental studies looking at primary outcomes and 16 econometric modelling studies looking at secondary outcomes. Overall, this represents a fairly limited evidence base on agricultural input subsidies. We found as few as two or three studies to provide results for some outcomes (for example, poverty or real income). Even where the evidence base was comparatively largest, we still found no more than seven studies that examine a common outcome. This is especially important, when one considers the mixed quality of the evidence base and the relatively high number of high and critical risk of bias experimental and quasi-experimental studies. Geographically, included studies are highly concentrated in sub-Saharan Africa. The studies also focus on seeds and fertilisers over other types of studies and typically look at inputs and outcomes for maize, or to a lesser extent, rice and vegetables.

We had hoped to be able to undertake moderator analyses to enable us to investigate how a variety of factors affected outcomes (for example, subsidy design and implementation, market, livelihoods and economy characteristics, infrastructure, climate and weather conditions). However, reporting of such factors was extremely limited and precluded these types of analysis. Finally, as outlined earlier in the discussion, the evidence base is very limited in terms of geographical coverage.

5.3 Quality of the evidence

The quality of the experimental and quasi-experimental evidence was varied and the proportion of high and critical risk of bias ratings was relatively high (nine of fifteen studies). We found few randomised controlled trials, with studies employing a range of methods for analysis. Selection bias due to baseline confounding, bias due to departures from interventions, and outcome reporting bias were the main reasons for the high overall risk of bias within this body of evidence. Given that the studies included in this review are of mixed quality, their results should be interpreted with caution.

5.4 Limitations and potential biases in the review process

This review is based on a published protocol (Dorward et al., 2013) and employed a comprehensive search and clear criteria for inclusion. Our main search was completed in November 2013 and no search update was undertaken since. Appendix 2 contains a full list of search dates.

We list some of the studies that came to our notice after completion of searches below.

Takeshima and Nkonya (2014) was published after the search date but would have been excluded on the basis that there was no measure of direct or indirect effect on relevant outcome: the focus is on crowding out. Gine et al. (2014) and Ricker-Gilbert and Jayne (2016) were not available at the time when our search was completed. Mason et al. (2015) would have been excluded on the basis that there was no measure of direct or indirect effect on relevant outcome.

We also recognise that the application of our exclusion criteria excluded some prominent papers that consider aspects of input subsidies analysis. Amongst these, Holden and Lunduka (2012), Pan and Christiaensen (2012), Jayne and Rashid (2013), Mason and Jayne (2013), Liverpool-Tasie (2014) and Minten et al. (2013) were excluded on the basis that they did not report effects on any includable outcomes. Likewise, Xu et al. (2009) and Ricker-Gilbert et al. (2011) did not have a suitable counterfactual design. Duflo et al. (2008), Beaman et al. (2013) and Darko et al. (2014) examine the effects of fertiliser use rather than subsidy provision, while Marenya and Barrett (2009), Xu (2009), Matsumoto and Yamano (2011) and Sheahan et al. (2013) also do not assess the impacts of subsidies.

Studies were assessed by a single reviewer at both title and abstract and full-text. A second reviewer then checked screening decisions taken at full-text. This may mean that some potentially includable studies were mistakenly excluded from the review.

5.5 Deviations from the protocol

The review largely followed the steps set out in the study protocol. Moderator analysis of studies included for research question 1 was limited by the lack of reporting of moderator variables, although some moderators were explored for crop type, bias assessment and study design. We also conducted a meta-regression analysis and narrative synthesis of qualitative data to explore how and why outcomes were observed. We did not undertake any investigation of publication bias due to the limited numbers of studies included in the analysis (less than 10 studies for any single outcome).

5.6 Agreements and disagreements with other studies or reviews

To our knowledge, no systematic review of agricultural input subsidies has been published until now. However, various literature reviews have examined the implementation and impacts of subsidies in a variety of contexts.

Some past literature reviews have argued that subsidies have played a positive, albeit time limited, role in stimulating agricultural productivity and growth in some contexts, notably in India during the green revolution of the 1960s and 70s. However, the consensus is that even where subsidies have had positive effects, they have tended to diminish considerably over time (Timmer, 2004; Fan et al., 2008).

Past reviews have identified a number of problems such as issues with cost control, diversion of subsidised inputs to those outside the programme, crowding out of commercial sales and overuse of inputs (e.g. Ellis, 1992; Morris et al., 2007; Timmer et al., 2009). Findings from our narrative synthesis of implementation and contextual factors in included studies showed evidence that these issues were identified as problems in a number of studies. However, we find that these problems do not, on the whole, negate the potential positives of input subsidies. We find subsidies have had positive effects on adoption, productivity and farm incomes. Nonetheless, we echo the findings of past literature reviews on the topic (Wiggins and Brookes, 2012; Ricker-Gilbert, 2013) that programme design is critical.

Econometric studies included in the review show evidence for positive effects on consumer welfare and wider economic growth. However, studies that examine effects under different scenarios showing positive effects are likely to be sensitive to changes in modes of funding, efficient targeting of beneficiaries and world input prices. In this sense, the review also echoes the conclusions of past literature reviews that governments need to carefully consider the benefits and distributional effects of input subsidy programmes relative to other uses of scarce public resources (e.g. Ellis, 1992; Morris et al., 2007; Timmer et al., 2009).

6. Author's conclusions

6.1 Implications for practice and policy

Agricultural input subsidies have generated much debate regarding their effectiveness in improving outcomes for farmers and consumers and for stimulating wider growth. Overall, this review finds positive results for both primary and secondary outcomes across our theory of change. Included experimental and quasi-experimental studies provide evidence linking fertiliser and seeds subsidies to increased use of the subsidised inputs, higher agricultural yields and increased income and among farm households, while the limited evidence relating to effects on poverty make it difficult to draw any clear conclusion.

Econometric models simulating the effects of subsidies on secondary outcomes of interest show that the introduction or increase of subsidies results in lower prices and increases in consumption of staple crops, increased demand for agricultural labour, higher agricultural wages and real incomes, reduced incidence of poverty among consumer households and increases in GDP.

Evidence also indicates that subsidy effects on consumer welfare and economic growth are generally greater when subsidies are funded through direct taxation rather than indirect taxation or reduced infrastructure spending. One study found effects on poverty reduction to be greater where subsidies target the poor rather than the general population. Positive effects of subsidies were found to decline as world input prices increase and the opportunity costs of subsidy programmes increases. These findings show how important contextual factors both endogenous and exogenous to the subsidy itself can be in determining effectiveness.

We systematically extracted and narratively synthesised data from included experimental studies to examine programme implementation, finding that programmes are often implemented poorly, with inputs not always made available or used as planned. A simple meta-regression analysis indicated no relationship (positive or negative) between subsidy size and agricultural outcomes. This finding is likely down to the greater importance of implementation and contextual factors in determining outcomes. Increasing subsidy sizes may not be effective if programmes are affected by problems such as the selling or exchanging of vouchers or inputs and, in some cases, more systematic corruption such as fraud in the redeeming of vouchers and in the tendering process to suppliers.

Overall, the evidence indicates that agricultural input subsidies can have a positive effect on a number of the outcomes across the theory of change included in this review. However, programme implementation is often poor, with problems with both delivery and take-up and this is likely to impact on effectiveness.

Furthermore, the effectiveness and cost effectiveness of input subsidies when compared to alternative policy options cannot be assumed. Government capacity for implementation, appropriate mechanisms for funding and potential sensitivity to changes in the wider economic environment are key and should be taken into account when considering policy options.

6.2 Implications for Research

This review finds a relatively small evidence base of both experimental and quasi-experimental studies, and econometric modelling studies. A relatively large proportion of experimental and quasi-experimental studies had high or critical risk of bias, with baseline confounding, fidelity of implementation and outcome reporting particularly problematic. This is a challenging area to evaluate and we found few studies that randomise the provision of subsidised inputs to recipient households. Typically, quasi-experimental studies are limited to difference-in-difference approaches in estimating effects. However, where subsidies are targeted at certain population groups, for example those living below the poverty line and smallholder farmers, study designs such as regression discontinuity design might be feasible, and a greater use of statistical matching could improve rigour in the estimated effects.

Experimental and quasi-experimental studies tend to examine outcomes only in the short-term and there is a lack of studies that can tell us something about longer-term impacts or those after programmes have been phased out. Furthermore, studies do not explore the effects of schemes for female-headed households or particular ethnicities or castes, for example. Studies also tended to concentrate on just a few outcomes in the causal chain. The use of theory-based impact evaluations that can explore outcomes and assumptions along the causal chain and unpack impacts for different sub-groups and over longer time periods would help answer questions about why schemes are effective, or not.

Underlying assumptions in econometric models are likely to influence estimates of effects in simulations. This means the quality and completeness of data used in models is important in determining their usefulness. Where rigorous experimental and quasi-experimental evidence is available, modelling studies should make more use of such evidence in calculating coefficients, for example in modelling household behaviour and the micro-economic effects of subsidies. None of our included modelling studies explicitly stated they used such evidence.

Furthermore, no included modelling studies provide simulations of effects under different climate or ecological scenarios. Including a range of simulations in modelling studies, offering a range of different possible scenarios may be of more use to policy makers rather than simple 'with or without subsidy' simulations.

Studies included in this review are highly concentrated in sub-Saharan Africa and in Malawi in particular. Research of all types from a wider geographical spread of countries is needed to ascertain the effectiveness of subsidies in different contexts.

Our final point relates to the standards of reporting in included studies. Generally, methodological details are reported poorly, making it difficult to judge inclusion, assess risk of bias, and calculate effect sizes. It was particularly difficult to calculate standardised effect sizes from some experimental and quasi-experimental studies due to the limited nature of reporting. Clear reporting of outcomes data, standard deviations and sample sizes for treatment and control groups at end line in experimental and quasi-experimental studies would help support their inclusion in systematic reviews. Clearer reporting of the type and size of input subsidy implemented or modelled would also greatly help any further synthesis in this area.

Appendix A: Electronic searches

(LDC* OR LIC OR LICs OR LMIC* OR "developing countr*" OR "low income countr*" OR "third world countr*" OR "Latin America" OR Afghanistan OR Bangladesh OR Benin OR "Burkina Faso" OR "Burkina-Faso" OR Burundi OR Cambodia OR "Central African Republic" OR Chad OR Comoros OR Congo OR Eritrea OR Ethiopia OR Gambia OR Guinea OR "Guinea-Bissau" OR "Guinea Bissau" OR Haiti OR Kenya OR "North Korea" OR "Democratic Republic Korea" OR "Democratic People's Republic Korea" OR Kyrgyzstan OR "Kyrgyz Republic" OR Liberia OR Madagascar OR Malawi OR Mali OR Mozambique OR Myanmar OR Nepal OR Niger OR Rwanda OR "Sierra Leone" OR Somalia OR "South Sudan" OR Tajikistan OR Tanzania OR Togo OR Uganda OR Zimbabwe OR Rhodesia OR Armenia OR Bhutan OR Bolivia OR Cameroon OR "Cape Verde" OR Congo OR "Ivory Coast" OR "Cote d'Ivoire" OR Djibouti OR Egypt OR "El Salvador" OR Georgia OR Ghana OR Guatemala OR Guyana OR Mauritania OR Honduras OR Indonesia OR India OR Kiribati OR Kosovo OR Lao OR Laos OR Lesotho OR Micronesia OR Moldova OR Mongolia OR Morocco OR Nicaragua OR Nigeria OR Pakistan OR "Papua New Guinea" OR Paraguay OR Philippines OR Samoa OR "Sao Tome" OR Senegal OR "Solomon Islands" OR "Sri Lanka" OR Sudan OR Swaziland OR Syria OR "Syrian Arab Republic" OR "East Timor" OR "Timor Leste" OR "Timor-Leste" OR Ukraine OR Uzbekistan OR Vanuatu OR Vietnam OR Gaza OR "West Bank" OR Yemen OR Zambia) AND ("agricultur*" OR "farm*")

AND (subsidy OR subsidies OR subsidis* OR subsidiz* OR voucher* OR "co-payment*" OR copayment* OR reimburs* OR "tariff removal" OR "tax exempt*" OR "tax relief" OR "social franchise*" OR "price ceiling*" OR "price control*" OR "social marketing" OR "tariff exemption*" OR "demand side finance" OR "price support*")

AND (input* OR fertilis* OR fertiliz* OR seed* OR pesticide* OR insecticid* OR herbicid* OR fungicid* OR pump* OR crop* OR livestock OR feed OR drugs OR vaccin* OR immuniz* OR immunis* OR machine* OR fuel OR irrigat*))

Search Example: CAB Direct

The search string was modified as needed. An example of how the string was modified to search databases, in this case for CAB direct database, is given below:

((LDC* OR LIC OR LICs OR LMIC* OR "developing countr*" OR "low income countr*" OR "third world countr*" OR "Latin America" OR Afghanistan OR Bangladesh OR Benin OR "Burkina Faso" OR "Burkina-Faso" OR Burundi OR Cambodia OR "Central African Republic" OR Chad OR Comoros OR Congo) AND ("agricultur*" OR "farm*") AND (subsidy OR subsidies OR subsidis* OR subsidiz* OR voucher* OR "co-payment*" OR copayment* OR reimburs* OR "tariff removal" OR "tax exempt*" OR "tax relief" OR "social franchise*" OR "price ceiling*" OR "price control*" OR "social marketing" OR "tariff exemption*" OR "demand side finance" OR "price support*") AND (cc=EE140 OR cc=EE145 OR input* OR fertilis* OR fertiliz* OR seed* OR pesticide* OR insecticid* OR herbicid* OR fungicid* OR pump* OR crop* OR livestock OR feed OR drugs OR vaccin*

OR immuniz* or immunis* OR machine* OR fuel OR irrigat*))
OR
(("Ivory Coast" OR "Cote d'Ivoire" OR Djibouti OR Egypt OR "El Salvador" OR Georgia OR Ghana OR Guatemala OR Guyana OR Mauritania OR Honduras OR Indonesia OR India OR Kiribati OR Kosovo OR Lao OR Laos OR Lesotho OR Micronesia OR Moldova OR Mongolia OR Morocco OR Nicaragua OR Nigeria OR Pakistan OR "Papua New Guinea") AND ("agricultur*" OR "farm*") AND (subsidy OR subsidies OR subsidis* OR subsidiz* OR voucher* OR "co-payment*" OR copayment* OR reimburs* OR "tariff removal" OR "tax exempt*" OR "tax relief" OR "social franchise*" OR "price ceiling*" OR "price control*" OR "social marketing" OR "tariff exemption*" OR "demand side finance" OR "price support*") AND (cc=EE140 OR cc=EE145 OR input* OR fertilis* OR fertiliz* OR seed* OR pesticide* OR insecticid* OR herbicid* OR fungicid* OR pump* OR crop* OR livestock OR feed OR drugs OR vaccin* OR immuniz* or immunis* OR machine* OR fuel OR irrigat*))
OR
((Paraguay OR Philippines OR Samoa OR "Sao Tome" OR Senegal OR "Solomon Islands" OR "Sri Lanka" OR Sudan OR Swaziland OR Syria OR "Syrian Arab Republic" OR "East Timor" OR "Timor Leste" OR "Timor-Leste" OR Ukraine OR Uzbekistan OR Vanuatu OR Vietnam OR Gaza OR "West Bank" OR Yemen OR Zambia) AND (subsidy OR subsidies OR subsidis* OR subsidiz* OR voucher* OR "co-payment*" OR copayment* OR reimburs* OR "tariff removal" OR "tax exempt*" OR "tax relief" OR "social franchise*" OR "price ceiling*" OR "price control*" OR "social marketing" OR "tariff exemption*" OR "demand side finance" OR "price support*") AND (cc=EE140 OR cc=EE145 OR input* OR fertilis* OR fertiliz* OR seed* OR pesticide* OR insecticid* OR herbicid* OR fungicid* OR pump* OR crop* OR livestock OR feed OR drugs OR vaccin* OR immuniz* OR immunis* OR machine* OR fuel OR irrigat*))

Appendix B: Search results

Database searched	Last date searched	Number of studies retrieved
3ie Systematic Review Database	26/07/2013	25
Ageconsearch	18/07/2013	85
Agricola	16/01/2014	11
AGRIS	02/10/2013	135
British Library for Development Studies	19/09/2013	77
CAB Direct	11/11/2013	1434
Dissertations Express	19/09/2013	14
Ebsco: Econlit and Africa Wide	14/20/2013	119
ELDIS (Agriculture and Food Section)	01/10/2013	33
IDEAS	30/09/2013	88
ISI Web of Knowledge	25/07/2013	3600
JOLIS	17/09/2013	85
NDLTD	01/10/2011	20
USDA Economic Research Service	16/09/2013	8
Grey Literature		
Google	16/10/2013	50
Google Scholar	16/10/2013	50
Hand Searches of Oxford Bodleian Social Science Library		
Agricultural Economics	29/10/2013	5
American Economic Review	30-31/10/2013	8
American Journal of Agricultural Economics (Journal of Farm Economics)	30/10/2013 30/10/2013	
Economic Development and Cultural Change	30/10/2013	
European Review of Agricultural Economics	30/10/2013	1
Journal of Agricultural Economics	30/10/2013	6
Journal of Development Economics	30/10/2013	
World Development	30/10/2013	7

Appendix C: Effect size data extraction

Table C1: Overview of effect size calculations from experimental and quasi-experimental studies

Study	Setting	Intervention	Outcome measure	Results (SMD/ITT/RC (SE); RR (SE))
Mason & Smale 2013	Zambia	Maize seed (40% of cost)	Subsidised seed use (kg)	RC: 0.416 (p: 0.063); SD of DV: 30.9, p: 0.063
Carter et al. 2013	Mozambique	Fertiliser and maize seed vouchers at 73% subsidy	Seeds use (kg/ha)	ITT: 3.1 (1.27)
Carter et al. 2013	Mozambique	Fertiliser and maize seed vouchers at 73% subsidy	Fertiliser use (kg/ha)	ITT: 14.75 (2.20); 38.61 (5.85)
Chibwana 2010	Malawi	Fertilisers at 8%, maize	Fertiliser use (kg/ha)	RC: 128; SD of DV: 161 t=2.536; SMD: 0.8678 (0.0115) [0.6574; 1.0781]; 1.70 (1.23)
Mather & Kelly 2012	Mali	Fertilisers for rice (22% urea and 43% basal fertilisers)	Fertiliser use (kg/ha)	SMD: 0.2365 (0.0148) [-0.002; 0.475]; 1.11 (0.00)
Karamba 2013	Malawi	Fertiliser and seed coupons	Fertiliser use kg/ha (IV)	RC: 1.59 (3.50)
Karamba 2013	Malawi	Fertiliser and seed coupons	Fertiliser use kg/ha (OLS)	RC: 23.87 (1.22); t=19.57; 1.81 (1.03)
World Bank 2014	Tanzania	Fertiliser and maize seeds (50%)	Yield (kg/acre)	RC: 0.35 (0.052); t=6.7; 1.06 (1.01)
World Bank 2014	Tanzania	Fertiliser and rice seeds (50%)	Yield (kg/acre)	RC: 0.208 (0.119); t=1.75; 1.03 (1.02)
Holden 2013	Malawi	Fertilisers and maize seed subsidy	Yield (kg/ha)	SMD 0.1686 (0.007) [0.0046; 0.3325]; 1.22 (1.10)
Mather & Kelly 2012	Mali	Fertilisers for rice (22% urea and 43% basal fertilisers)	Yield (kg/ha)	SMD (-) 0.1683 (0.0148) [-0.4064; 0.0698]; 0.93 (0.00)
Karamba 2013	Malawi	Fertiliser and seed coupons	Yield/ha (IV)	RC: 0.831 (0.294)
Karamba 2013	Malawi	Fertiliser and seed coupons	Yield/ha (OLS)	RC: 0.196 (0.025)
Mason & Smale 2013	Zambia	Maize seed (40% of cost)	Yields (harvest in kg)	RC: 0.0043751 (p: 0.000); SD of DV:

Study	Setting	Intervention	Outcome measure	Results (SMD/ITT/RC (SE); RR (SE))
				3167; p: 0.000
Carter et al. 2103	Mozambique	Fertiliser and maize seed vouchers at 73% subsidy	Yields (kg/ha)	ITT: 431.98 (372.9)
Mather & Kelly 2012	Mali	Fertilisers for rice (22% urea and 43% basal fertilisers)	Rice sales (kg)	RC: 0.264 (0.042); SD of DV: 359.1; t=6.31; p=0.000
Bardhan & Mookherjee 2011	India	Rice, potatoes and oilseeds (seeds, fertilisers and pesticides)	Farm productivity (IV) Log value/ha	RC: 0.448 (0.221)
Bardhan & Mookherjee 2011	India	Rice, potatoes and oilseeds (seeds, fertilisers and pesticides)	Farm productivity (OLS) Log value/ha	RC: 0.474 (0.087)
Mason & Smale 2011	Zambia	Maize seed (40% of cost)	HH Income (Total in ZMK)	RC: 0.0025839 (p: 0.001); SD of DV: 14666 p: 0.001
Awotide et al. 2013	Nigeria	Seed vouchers for rice	Income (N/ha)	SMD: 0.20 (0.01) [-0.03; 0.43]; 1.20 (0.00)
Chirwa 2010	Malawi	Starter pack fertiliser subsidy	HH annual expenditure (MK)	SMD (-) 0.139 (0.001) [-0.184; -0.093]; 0.992 (0.998)
Chirwa 2010	Malawi	AISP fertiliser subsidy	HH annual expenditure (MK)	SMD 0.108 (0.005) [-0.034; 0.249]; 1.008 (1.005)
Mason & Smale 2011 Smale et al. 2014	Zambia Zambia	Maize seed (40% of cost) Maize seed	Poverty levels (Foster-Greer-Thorbecke index) Subsidised seed use (kg)	RC: -0.0016872 (p: 0.000); SD of DV: 0.305 ; p:≈ 0.000; SMD : (-) 0.0034 (0.0006) [-0.0454; 0.0521] SE: 0.08 (0.00)

Notes: SMD=standardized mean difference, RR=relative risk, RC=regression coefficient, ITT=intention to treat, SD of DV=standard deviation of dependent variable, IV=instrumental variable, OLS=ordinary least squares.

Table C2: Overview of coefficients from simulation models

Arndt (2014) CGE

Model	Crop prices	Crop	Labour	Labour (measure)	Wage	Wage measure	Poverty Head-count	GDP	Agri GDP	Non-Agri GDP	Wel-fare
Indirect tax. Jointly funded	-0.0315	Real maize price index	0.0026	Farm employmen t share	0.042	Average farm wage	-0.0178	0.0469	0.1537	-0.0057	0.0279
direct tax. Jointly funded	-0.026	Real maize price index	0.0031	Farm employmen t share	0.0707	Average farm wage	-0.0293	0.0463	0.1539	-0.0065	0.0267

Buffie & Atolia (2009) CGE. Model: imperfect targeting (i.e. real world) situation, for inclusion in the review. The model with fertilizer prices (i.e. Phs-3) and infrastructure at mid-point (R=2).

Model	Staple crop prices	Crop	Agri wages	Real incomes	GDP
Lump sum taxes	-0.031	Maize	0.023	0.049	0.004
Reduced infrastructure spending	-0.033	Maize	-0.062	-0.034	-0.073

Caria et al. 2011 CGE & Micro-model simulation. FERT model

Model	Consumption	Consumption (Measure)	Wage	Wage measure
FERT	0.0678	Teff, wheat, Maize	0.0024	Agri wage

Dorward & Chirwa (2013)

Model	Real incomes

SHI Equilibrium trajectory model.	0.11								
Douillet et al. (2012)									
Model	Crop prices	Crop	Wage	Wage measure	Poverty (headcount)	GDP	Agri GDP	Non-Agri GDP	Addition-al info
Indirect tax. Jointly funded	-0.026	Maize	0.034	Farm wage	-0.007	0.011	13.9	-0.9/-0.6	Industry/ser vices
direct tax. Jointly funded	-0.021	Maize	0.06	Farm wage	-0.013	0.01	13.9	-0.7/-0.8	Industry/ser vices
Fan 2007	Multi-equation investment model (time series data)								
Model	Agri GDP								
Irrigation (1960's-1980's) irrigation	2.113								
Fertilizer (1960's-1990's) fertilzier	1.7125								
Power (1960's-1990's) power	1.0925								
Filipski & Taylor (2011)						CGE			
Model	Real Income	Welfare	Welfare as 'Compens-ating variation'						
Ghana		0							
Malawi	0.008	0.008							
Govindan & Babu	Multiple-output and multiple input framework								

Model	Labour demand							
Only model	-0.05							
Grepperud et al. (1999)	CGE: Import tax reduction							
Model	Consumption	Consumption (Measure)	Retail Price	Labour	Labour (measure)	GDP	Agri GDP	Non-Agri GDP
Short run 2000.	0.02	Maize	-0.04	0.056	Agricultural Labour	0.053	0.057	0.079
Long run 2010.	0.029	Maize	-0.015	0.076	Agricultural Labour	0.072	0.05	0.067
Holden & Lofgren (2005)					CGE			
Model	Labour	Labour (measure)						
Decrease	-0.002	village import of labour						
Increase	0.002	village import of labour						
Mapila (2013)	PEM (Recursive multi-equation): Complete removal of fertilizer subsidy program							
Model	Consumption	Consumption (Measure)	Retail Price	Retail Price Measure				
Short run	0	Maize	0	Maize				
Long run	0.0196	Maize	0.0232	Maize				
Ricker Gilbert et al. (2013)	Output supply function; Economic model estimations; regression/correlation calculations (Arellano-Bond)							
Model	Crop prices	Crop						

Malawi	-.012 to -.016	Maize						
Zambia	-.02 to -.028	Maize						
Malawi Doubling (by weight)	-.025 to - 0.018	Maize						
Zambia Doubling (by weight)	-.020 to -.028	Maize						
Malawi Doubling (per capita)	-25	Maize						
Zambia Doubling (per capita)	-0.018	Maize						
Rosegrant & Kasryno (1991)					Supply Demand Model			
Model	Expenditure	Expenditure (Measure)						
Short run 1995	0.079067803	Rice						
Long run 2000	0.067217424	Rice						
Stifel & Randrianarisoa (2004)								
Model	Consumption	Crop	Other crop	Non food	Retail Price	Retail Price (measure)	Poverty (head count)	Real Income
Targeted.	-0.002	Rice	0.062	-0.015	0	Rice	-0.015	0.00975
General	-0.003	Rice	0.048	-0.009	0	Rice	-0.014	0.01125
Tower (1987)	Consumption	Consumption	Labour	Wage	Wage measure			
Model	0.0056	Maize	0.0092	0.0057	Wage in the smallholder			

					sector	
Warr & Yusuf (2014)			GEM. Sima .25 model. The middle model of three was chosen			
		Retail Price	GDP	Poverty (headcount)	Wage	Wage measure
Sim .25	0.00987	-0.0068	-0.00066	−0.00047	0.0057	Wage in the smallholder sector

Appendix D: Risk of bias assessment of experimental and quasi-experimental studies

For experimental/quasi-experimental research, the potential risk of bias of the included studies was assessed using a tool adapted from Waddington et al. (2012) and Stewart et al. (2014), which is itself an adapted version of an early version of the Cochrane assessment tool for non-randomised studies (ROBINS-I) (Sterne et al., 2013). The applied tool assessed seven domains of potential bias as outlined below.

1. **Selection bias** referred to the methods and techniques used to identify and recruit eligible research participants. To be rated as low risk of bias, studies were required to provide a clear description of how and why the research sample was chosen, controlling, for example, for the social and economic background of participants might influence the performance of the programme. Additional points controlled for sufficient sample size to yield statistical significance and whether control and treatment groups were derived from comparable populations using similar recruitment processes.
2. **Bias due to confounding** primarily aimed to assess whether the experimental/quasi-experimental groups were comparable, resulting in a situation in which differences between groups at endline could be solely attributed to the applied intervention. This required a rigorous analysis of potential confounders that might have introduced observable and unobservable differences between experimental/quasi-experimental groups that – rather than intervention – account for differences in group performances at endline. The risk of bias tool assessed whether a detailed description of both experimental/quasi-experimental groups at baseline was provided that investigated potential systematic differences between groups. When study designs were known to be unable to fully control for unobservable differences between groups (e.g. experimental/quasi-experimental designs), authors were expected to have conducted an appropriate analysis that controlled for all potential critical confounding variables.
3. **Bias due to ineffective randomisation** was only assessed in randomised-controlled trials (RCTs). This is a rigorous research design which, if applied correctly, rules out the prevalence of selection bias and baseline confounding. RCTs were deemed not to be subject to risk of bias domains 1 and 2 as long as the randomisation process was judged to have been implemented adequately. The tool controlled for these by assessing, for example, random allocation techniques and descriptive statistics at baseline.
4. **Intervention bias** assessed whether internal or external factors changed the scheduled application of the intervention. In the contexts of agricultural subsidies, for example, the prevalence of a drought in the research area could strongly affect the study results. Often co-interventions such as market access or provision of technological inputs could further influence study findings. The risk of bias tool investigated whether influencing factors prevailed and how the research team responded to them. Study results that were strongly influenced by treatment switches and implementation failures were excluded from the synthesis stage of the systematic review. Lastly, this domain of bias also assessed the possibility of control groups gaining access to the applied programme.

5. **Bias due to missing data** assessed whether the study suffered from some form of attrition. The study might have been unable to track participants from baseline to endline, or might have been unable to collect particular data sets due to unforeseen events such as natural or political crises. In either case, the tool examined whether endline intervention groups were free of critical differences in missing data. If data were missing, authors were expected to describe in detail the reason for attrition and, where possible, account for the missing data in the analysis using statistical methods.
6. **Outcome reporting bias** examined the conceptual validity of outcome measures and whether they provided an adequate reflection of the outcomes the study had set out to measure. In the context of agricultural subsidies, a well-known shortcoming of outcome measures is extrapolating agricultural income from harvest data and the calculated market value of harvest. Often farmers are unable to gain fair market access that would allow them to sell their harvest at prevailing market prices, leaving their agricultural income well below the assumed value under perfect market conditions. The tool also investigated whether outcomes measures and their application were consistent across experimental/quasi-experimental groups. There are some important deviations when adapting the risk of bias tool from a health care to a development research context concerning the practice of blinding assessors and the commence of follow-up data.
7. **Bias in selection of results** reported the transparent presentation of research findings. The tool assessed whether reported results might have been one among many findings and reported as they best fit the authors' hypotheses. This assessed the applied methods of analysis and whether reported outcomes were consistent with the proposed outcomes at the protocol stage or during the description of the study design.

Risk of bias ratings were assigned for each of the seven domains, varying from low, moderate, high, to critical ratings. Where sufficient detail to make a judgment was not possible, the risk was deemed as unclear. In addition, an 'overall' judgement was calculated for each study to determine whether the study should be included in the synthesis. After assessing each domain, the overall risk of bias per outcome was determined using a numeric threshold. Once two out of the six risk of bias domains were judged at a given risk of bias level, the study was allocated that as overall judgment. For instance, if a study received four low-risk ratings and two high-risk ratings, the overall judgement was recorded as high risk of bias. This threshold was applied for the allocation of moderate- and high-risk ratings only. In the case of critical risk of bias judgments, a single critical rating in any of the domains led to the immediate overall outcome judgment to be regarded as critical.

Table D1: Stewart et al. (2014) Risk of Bias Tool

Study type	Methodological appraisal criteria				Response		
					Yes	No	Comment / risk of bias judgment
Quantitative (Non-randomised; randomised-controlled)	**I. Selection bias:** (Are participants recruited in a way that minimises selection bias?) <u>Appraisal indicators:</u> Consider whether						
Common non-random design include:	*i. There is a clear description of how and why sample was chosen*						
(A) Non-randomised CT	*ii. There is adequate sample size to allow for representative and/or statistically significant conclusions*						
(B) Cohort studies	*iii. Participants recruited in the control group were sampled from the same population as that of the treatment*						
(C) Case-control *(D) Cross-sectional analytical studies*	*iv. Group allocation process attempted to control for potential risk of bias*						
Most common ways of controlling for bias due to baseline confounding:	**Low risk of bias**	**Risk of bias**	**High risk of bias**	**Critical risk of bias**	*Worth to continue:*		
• Matching attempts to emulate randomisation *• Propensity score matching and*	**II. Bias due to baseline confounding:** (Is confounding potentially controllable in the context of this study?) <u>Appraisal indicators:</u>						

methods • *Stratification where sub-groups have been compared* • *Regression analysis where covariates are adjusted for* *Randomised designs: Randomised Control Trial (RCT)*	Consider whether			
	i. The treatment and control group are comparable at baseline			
	ii. Matching was applied, and in case, featured sufficient criteria			

Appendix E: Critical appraisal of modelling studies

The critical appraisal tool for studies using economic models differed from the tool used for experimental/quasi-experimental research, and was developed by review authors. Four critical appraisal domains were assessed as explained below:

1. **Quality of raw data used for modelling:** this assessed whether the empirical data used in the model specifications and calculation was reliable. This referred to the source of the data as well as the data collection process. It also examined whether the quality of data was likely to be consistent in the case of time-series data or regional and national comparisons. Where authors claimed to provide a nationally representative sample, the relevance of this claim was investigated. We also determined whether elasticities used in the model were stipulated and calculated based on reliable sources.
2. **Relevance and plausibility of the economic model:** this reviewed the quality of the applied model itself. The tool assessed the number and design of variables used in the model specifications; if available, the plausibility of the assumptions underlying the model; as well as whether authors attempted to empirically validate the model or parts of it (e.g. calibration, goodness of fit etc. depending on the model type).
4. **Reporting of results:** this refers to the quality of reporting. Criteria reviewed included whether authors might have 'cherry-picked' favourable results; whether modelling results compare against observed 'real-world' effects; and whether the limitations and contradictions of the results are discussed.

The allocation of threat to validity judgements for modelling studies also differed from the allocation process for experimental/quasi-experimental research. For modelling studies, there was no relative risk of bias rating (i.e. from low to critical). Rather, we used a system of assessing 'high threat to validity' in which each critical appraisal domain was judged as either high or low threat. Where any criterion was judged to be at high threat to validity, the study was judged to be high threat to validity overall. As noted in Dorward et al. (2014) the diversity of economic models negates a relative scale of risk of bias judgments across different types of models. Nonetheless, we felt where models failed to report or take into account in their model some key factors, the results of the model need to be interpreted with caution.

Calculation of overall validity

The threat to validity for studies' findings were rated across the four critical appraisal domains. Included studies were allocated an overall threat to validity depending on their score in the four individual domains. Once a study received a judgments of high threat to validity in any domain, it was automatically allocated high threat to validity overall.

Table E1: Validity assessment tool for modelling studies

Validity domain	Number	Decision rule	Yes/No	Notes
1. Source and quality of data				
	i	Are the raw data derived from reliable sources (Social accounting matrix, government accounts, nationwide household surveys, experimental studies etc.)?		
	ii	Are the nature and source of elasticities are stipulated?		
	iii	If multiple data sources (e.g. time series; regional/national comparisons) were used, were the data collection across these sources consistent/comparable?		
	iv	If there are national models, is the data source is likely to present nationally representative information?		
	v	Are the reasons for choosing data used justified?		
Overall criteria 1:		Less than 4 responses 'yes' = high threat. All 4 responses 'yes' = low threat.		
2. Model specification				
	i	Has the model type been used before?		
	ii	Is the model dynamic (rather than static)?		
	iii	If the assumptions underlying the model have been stated, are they plausible?		
	iv	Have there been attempts to test the calibration or otherwise test the validity of the model?		
	v	Does the model contain more than a single scenario (ie the sensitivity of the model to changes in some variables is apparent)?		
Overall criteria 2:		Less than 3 responses 'yes' = high threat. 3 or more responses 'yes'= low threat.		
3. Reporting/plausibility of results				
	i	Are the model's results described in detail?		
	ii	Are the model's results plausible in comparison to 'real word' effects?		
	iii	Are the limitations (and, if relevant, any contradictions) of the model's results discussed?		
Overall criteria 3:		Less than 3 responses 'yes' = high risk. 3 responses 'yes' = low risk.		
Overall bias assessment:		If any criterion (1, 2 or 3) is high= high threat to validity. If all criteria (1, 2 and 3) are low = low threat to validity.		

Appendix F: Results of critical appraisal

Table F1: Experimental and quasi-experimental studies risk of bias summary

Study	Design	Overall bias assessment	Selection bias	Bias due to baseline confounding	Bias due to ineffective randomisation	Bias due to departures from intended interventions	Bias due to missing data	Outcome reporting bias	Bias in selection of results reported
Ajayi et al. (2009)	Prospective; feasibility plot experiment	Critical	Unclear	Critical	n/a	Moderate	Low	Moderate	Low
Awotide et al. 2013	RCT	High	Moderate	High	Critical	Moderate	Low	High	High
Bardhan & Mookherjee 2011	Retrospective; time series panel data	Low	Low	Low	n/a	Low	Low	Low	Low
Carter et al. 2013	RCT	Low	Low	Low	Low	Low	Low	Low	Low
Chibwana 2010	MNL and IV regression based on panel data + simple plot level yield response function	High	High	High	n/a	High	Unclear	Unclear	Low
Chirwa 2010	Retrospective; PSM; OLS regression	Moderate	Moderate	High	n/a	Moderate	Low	Moderate	Low
Denning et al. 2009	Controlled before versus after	Critical	Unclear	Critical	n/a	High	Unclear	Unclear	High
Holden 2013	Prospective; DID on field level	Low	Low	Low	n/a	Low	Moderat e	Low	Low
Kamanga 2010	Prospective; DID	Critical	Low	Critical	n/a	High	Low	Low	High
Karamba 2013	OLS regression model	High	Moderate	High	n/a	High	High	Low	Low

Study	Design	Overall bias assessment	Selection bias	Bias due to baseline confounding	Bias due to ineffective randomisation	Bias due to departures from intended interventions	Bias due to missing data	Outcome reporting bias	Bias in selection of results reported
	based on HH survey and IV								
Mason & Smale 2013	Retrospective; panel data regression	High	Unclear	High	n/a	High	Unclear	High	Low
Mather & Kelly 2012	Household survey data: OLS regression; CREs	High	High	High	n/a	High	Low	High	Low
Parameswaran 2012	Retrospective; different panel data to model DID	Critical	Critical	Critical	n/a	Critical	Unclear	Unclear	Low
Smale et al. 2014	3-stage regression (tobit & IV) to predict demand based on cross-sectional survey data	Moderate	Moderate	Moderate	n/a	High	Low	Low	Low
World Bank 2014	Prospective; DID	High	High	High	n/a	High	Low	Unclear	Low

Table F2: Critical appraisal of modelling studies

Study name	Overall threat to validity	Source and quality of data	Model specification	Plausibility of results
Arndt (2014)	Low threat to validity	Low threat to validity	Low threat to validity	Low threat to validity
Buffie & Atolia (2009)	Low threat to validity	Low threat to validity	Low threat to validity	Low threat to validity
Caria (2011)	Low threat to validity	Low threat to validity	Low threat to validity	Low threat to validity
Dorward & Chirwa (2013)	Low threat to validity	Low threat to validity	Low threat to validity	Low threat to validity
Douillet (2012)	Low threat to validity	Low threat to validity	Low threat to validity	Low threat to validity
Fan (2007)	Low threat to validity	Low threat to validity	Low threat to validity	Low threat to validity
Fillispi & Taylor (2011)	Low threat to validity	Low threat to validity	Low threat to validity	Low threat to validity
Govindan & Babu (2010)	High threat to validity	High threat to validity	High threat to validity	High threat to validity
Grepp-erud (1999)	Low threat to validity	Low threat to validity	Low threat to validity	Low threat to validity
Holden & Lofgren (2005)	Low threat to validity	Low threat to validity	Low threat to validity	Low threat to validity
Mapila (2013)	High threat to validity	Low threat to validity	High threat to validity	High threat to validity
Rickert Gilbert (2013)	Low threat to validity	Low threat to validity	Low threat to validity	Low threat to validity
Rosegrant & Kasryno (1991)	High threat to validity	High threat to validity	Low threat to validity	High threat to validity
Stifel et al. (2004)	Low threat to validity	Low threat to validity	Low threat to validity	Low threat to validity
Tower (1987)	High threat to validity	High threat to validity	High threat to validity	Low threat to validity
Warr & Yusuf (2014)	Low threat to validity	Low threat to validity	Low threat to validity	Low threat to validity

Appendix G: Detailed study characteristics for included experimental and quasi-experimental studies

STUDY*	INTERVENTION	IMPLEMENTATION/CONTEXT	OUTCOME	FINDINGS
Awotide et al. 2013 Matched-based RCT study Risk of bias: High	Country: Nigeria (Osun, Niger and Kano) Crop: Rice Subsidy: Seed voucher	Effect modifiers: Gender of head of household; access to irrigation; secondary occupation Targeting and scheme: The ERI adopted the seed voucher system to grant some randomly selected rice farmers' access to certified improved rice seed at a subsidised rate for two production seasons (2008/09 and 2009/10). The voucher was designed to be used in just one day. All the treated farmers were supposed to come to a meeting point (in most cases, the village square) on an agreed date and time for the collection of the seed voucher and immediately proceed to the agro-dealer to collect the desired seed varieties. The agro-dealers later redeemed their money from the designated banks. The design of the voucher system was to eliminate or at best discourage the creation of a secondary market for the voucher.	Rice income (N/ha)	SMD (SE): 0.20 (0.01) [-0.03; 0.43]; RR (SE): 1.20 (0.00)
Bardhan & Mookherjee 2011 Retrospective time series panel date regression-based study Risk of bias:	Country: India (West Bengal) Crop: Rice, potato and oilseeds Subsidy: Minikits containing seeds, fertilisers and	Effect modifiers: Subsidised credit was provided by state-owned banks under the Integrated Rural Development Program (IRDP) from 1978 onward; two land reform programmes (including the tenancy registration programme Operation Barga); man-days of employment generated by the GP infrastructure programmes; and minor government irrigation programmes, also contributed to productivity gains.	Farm productivity (OLS) Log value/ha (1919 observations)	Regression coefficient (SE): 0.474 (0.087) Regression coefficient (SE): 0.448 (0.221)

STUDY*	INTERVENTION	IMPLEMENTATION/CONTEXT	OUTCOME	FINDINGS
Low	pesticides Programme: Part of a series of the Indian government's Farm Service Delivery Programmes Population: n=550 farms	Targeting and Scheme: Local governments ran under various schemes sponsored by the central and state government. The resources percolated down from the central government to GPs through the state government, its district-wide allocations, and then through the upper tiers of the panchayats at the block and district levels. Upper tiers of the panchayats selected their allocation across different GPs. The responsibility of the latter was either to allocate them across households and farms within their jurisdiction or to recommend beneficiaries to local implementing agencies, such as government banks and agriculture offices. The agricultural minikits were sold very cheaply to beneficiaries selected by the local government by the agriculture office in the relevant block (the tier of local government intermediate between the village and district). Within villages the programme was targeted fairly well by GPs, though the inter-village allocations exhibited biases against villages with a high proportion of landless and low-caste groups.	Farm productivity (IV) Log value/ha (2193 observations)	
Carter et al. 2013 Regression-based RCT studies Risk of bias: Low	Country: Mozambique (Manica province) Crop: Maize Subsidy: Fertiliser	Effect modifiers: Leakage of vouchers to the control group; low voucher pick-up and use rates by lottery winners; experience of use of modern inputs; liquidity; late distribution of vouchers and a late drought significantly reduced the benefits of the programme Targeting and Scheme: Lists of eligible farmers were	Fertiliser use (kg/ha)	ITT (SE): 14.75 (2.20); regression coefficient (SE): 38.61 (5.85); t-stat/p-value of regression coefficient: t=6.6

STUDY*	INTERVENTION	IMPLEMENTATION/CONTEXT	OUTCOME	FINDINGS
	and seed vouchers at 73% of the seed and fertiliser package cost Programme: Agro-input subsidy programme over the years 2009–10 and 2010–11. Population: 75 villages (1593 households of which 795 received vouchers and 798 were the control)	created jointly by agricultural extension, local leaders, and agro-input retailers, under the supervision of the International Fertilizer Development Center. Individuals were deemed eligible for a voucher coupon if they met the standard programme criteria: farming between 0.5 hectare and 5 hectares of maize; being a "progressive farmer," defined as a producer interested in modernisation of their production methods and commercial farming; having access to agricultural extension and access to input and output markets; being able and willing to pay for the remaining 27% of the package cost. Only one person per household was allowed to register. The farmers were informed that a lottery would occur and only half of those on the list would win a voucher.		
Chibwana et al. 2010 MNL and Instrumental Variables regression based, panel data Risk of bias: High	Country: Malawi (Kasungu and Machinga Districts, Central and Southern Malawi respectively) Crop: Maize Subsidy: Fertilisers and seeds	Effect modifiers: Uneven roll out of FISP and widespread leakage; cash constraints; politics affecting selection of beneficiaries. Targeting and Scheme: The FISP uses a series of coupon-vouchers that enable households to purchase fertiliser, hybrid seed and/or pesticides at greatly reduced prices. Beneficiaries in 2007–08 and 2008–09 required the household: (1) owned land being cultivated during the relevant season; (2) were bona fide residents of the village; (3) only had one eligible	Fertiliser use (kg/ha)	Regression coefficient: 128; SD of DV: 161 DV is fertiliser use (associated with voucher receipt); t-stat/p-value of regression coefficient: t=2.536; SMD (SE): 0.8678 (0.0115) [0.6574; 1.0781]; RR (SE): 1.70 (1.23)

STUDY*	INTERVENTION	IMPLEMENTATION/CONTEXT	OUTCOME	FINDINGS
	vouchers (in 2008–09, each voucher entitled a household to 50 kg of maize fertiliser at 8% of market price, and 2 kg of hybrid maize seed (or 4 kg of open pollinated maize) for free). Programme: Farm Input Subsidy Programme (FISP) (in 2009) Population: n=380 observations	member; and (4) would be prioritised if they included vulnerable groups, especially headed by children and women. The Ministry of Agriculture distributed the coupons to districts, and traditional authorities (TAs) then allocated them to villages. Village heads, in collaboration with Village Development Committees (VDCs), identified beneficiary households within their jurisdictions. Results show subsidies need to be better targeted and support revising distribution of coupons to households; promoting hybrid seed over fertiliser for maize might improve the programme.		
Chirwa 2010 Matched-based study Risk of bias: Moderate	Country: Malawi Crop: Maize Subsidy: Fertiliser coupons Programme: Starter Pack (TIP)	Effect modifiers: Coupon distribution; access to key basic services e.g. roads, markets, education, extension, credit; fertiliser application efficiency (rates, timing); corruption Discussion: The impact of the input subsidy programmes in Malawi becomes stronger as policy makers improve on the quantities of inputs subsidised. The benefits can further be maximised if	Household annual expenditure (MK) TIP Household annual expenditure	SMD (SE): (-) 0.139 (0.001) [-0.184; -0.093]; RR (SE): 0.992 (0.998) SMD (SE): 0.108 (0.005) [-0.034; 0.249]; RR (SE): 1.008 (1.005)

STUDY*	INTERVENTION	IMPLEMENTATION/CONTEXT	OUTCOME	FINDINGS
	programme 2003/04 and Agricultural Input Subsidy Programme (AISP) 2006/07 Population: n=7890 for TIP and n=1147 for AISP	such programmes are complemented with projects aimed at improving access to basic services in the targeted areas such as roads and markets.	(MK) AISP	
Holden 2013 Prospective; Mixed effects. Matching Risk of bias: Low	Country: Malawi Crop: Maize Subsidy: Fertilisers and seeds coupons Programme: Malawian targeted input subsidy programme (FISP) (2006-2009) Population: n=450 households	Effect modifiers: Factors affecting access to coupons (less likely to reach female-headed households) and adoption of improved maize; efficiency of fertiliser use; timeliness of fertiliser distribution; rainfall levels (data correspond to only good rainfall years) Discussion: Implications are that the subsidy programme does not crowd out other crops but rather facilitates maize intensification while leaving more area for other crops. The programme is therefore complementary with crop diversification (contrary to other studies). Furthermore, maize that received fertilisers was more likely to be intercropped than maize not receiving fertilisers (contrary to claims that fertiliser subsidies lead to mono-cropping of maize). Targeting: Unobservable household characteristics possibly affecting access include their social	Yield (kg/ha)	SMD (SE): 0.1686 (0.007) [0.0046; 0.3325]; RR (SE): 1.22 (1.10)

STUDY*	INTERVENTION	IMPLEMENTATION/CONTEXT	OUTCOME	FINDINGS
		networks, position, influence, kinship ties, information available and decisions made by those responsible for the targeting. Actual targeting criteria may be different from the official targeting criteria which relate to poverty, vulnerability, etc.		
Karamba 2013 OLS regression model and Instrumental Variables Risk of bias: High	Country: Malawi Crop: Maize and legumes Subsidy: Fertiliser and seed coupons. Programme: Farm Input Subsidy Programme (FISP) (2009/10 growing season) Population: n=12,465 rural agricultural households that cultivated land in the 2009/10 rainy season	Effect modifiers: Farmers' time preference, risk, access to complementary inputs, and household tastes, structure and behaviour Targeting and Scheme: The Farm Input Subsidy Programme (FISP) is intended to assist smallholder farm households achieve food self-sufficiency and increase incomes via increased crop production and food security at the household and national level. Programme beneficiaries should be (i) fulltime 'resource-poor' smallholder farmers, (ii) residents in the village, and (iii) own land that will be cultivated in the agricultural season they enter the programme. Household heads that are elderly, HIV-positive, female, children, orphans, physically challenged; or household heads that take care of elderly or physically challenged household members are specifically targeting. Only one farmer per household should benefit. The programme targets over 50% of smallholder farm households in Malawi. Eligible households receive coupons redeemable for fertiliser and improved seed at 30% below market price. Targeting and Scheme: The coupons for maize	Fertiliser use kg/ha (OLS) n=12354 observations Fertiliser use kg/ha (IV) n=12354 observations Fertiliser use kg/ha (IV) n=12354 observations Output/ha (OLS) n=11652	Regression coefficient (SE): 23.87 (1.22); t-stat/p-value of regression coefficient: t=19.57; RR (SE): 1.81 (1.03) Regression coefficient (SE): 23.87 (1.22); t-stat/p-value of regression coefficient: t=19.57; RR (SE): 1.81 (1.03) Regression coefficient (SE): 1.59 (3.50) Regression coefficient (SE): 1.59 (3.50)

STUDY*	INTERVENTION	IMPLEMENTATION/CONTEXT	OUTCOME	FINDINGS
		fertilisers allowed recipients to buy a 50 kg bag of basal dressing for maize called NPK and a 50 kg bag of top dressing for maize called Urea for 500 Malawi Kwacha (MK) per bag. The maize seed coupon which was valued at 1500 MK subsidised either a 5 kg bag of hybrid maize seed or a 10 kg bag of open pollinated variety (OPV) maize seed. The last coupon, a flexi-voucher, could be redeemed for a free 1 kg bag of legume seed (groundnuts, pigeon peas, cowpeas, and beans). Beneficiaries could also purchase a 200 g bottle of maize storage pesticides at a subsidized price of 100 MK although no coupon was provided for this input.	observations Output/ha (IV) n=11652 observations Fertiliser use kg/ha (OLS) n=12354 observations	Regression coefficient (SE): 0.196 (0.025) Regression coefficient (SE): 0.831 (0.294)
Mason & Smale 2013 Retrospective panel data regression-based study Risk of bias: High	Country: Zambia Crop: Maize Subsidy: Seed at 40% of the market cost Programme: Originally known as the Fertilizer Support Program (FSP), the subsidy programme was renamed the	Effect modifiers: Households whose head is related to the village headman or chief appear to have preferential access to subsidised seed. This suggests an uneven playing field, which may attenuate the positive effects of the programme on farm household well-being. Targeting and Scheme: FISP operates by selecting private suppliers through a tender process. Local transporters distribute inputs to designated collection points. Registered farmer organizations issue inputs to approved farmers, who pay a portion of the costs via the organizations. Beneficiary farmers must meet specified criteria, including good credit history, capacity to grow 1–5 ha of maize, and to pay the	Input use (subsidised seed in kg) (6462 observations) Yields (maize harvest in kg) (6389 observations)	Regression coefficient (SE): 0.416 (p-value: 0.063); SD of DV: 30.9 DV is seed planted (associated with subsidised seeds provided); t-stat/p-value of regression coefficient: approx. p: 0.063 Regression coefficient (SE): 0.0043751 (p-value: 0.000); SD of DV: 3167 DV is maize

STUDY*	INTERVENTION	IMPLEMENTATION/CONTEXT	OUTCOME	FINDINGS
	Farmer Input Support Programme (FISP) in 2009 Population: n=3,200 smallholder maize growers in Zambia during the 2002–2003 and 2006–2007 agricultural seasons	farmer share of input costs.	HH Income ('Total' in ZMK) (6456 observations) Poverty levels (Foster-Greer-Thorbecke index) (6462 observations)	harvest (associated with seeds planted); t-stat/p-value of regression coefficient: approx. p: 0.000 Regression coefficient (SE): 0.0025839 (p-value: 0.001); SD of DV: 14666 DV is HH income (associated with seeds planted); t-stat/p-value of regression coefficient: approx. p: 0.001 Regression coefficient (SE): -0.0016872 (p-value: 0.000); SD of DV: 0.305 DV is poverty levels (associated with seeds planted); t-stat/p-value of regression coefficient: approx. p: 0.000; SMD (SE): (-) 0.0034 (0.0006) [-0.0454; 0.0521]
Mather & Kelly 2012	Country: Mali	Effect modifiers: For rice yields: climatic conditions,	Input use	SMD (SE): 0.2365

STUDY*	INTERVENTION	IMPLEMENTATION/CONTEXT	OUTCOME	FINDINGS
OLS regression; Correlated Random Effects Risk of bias: High	(Segou region, Office du Niger) Crop: Rice Subsidy: Price (22% for urea and 43% for basal fertilisers) Programme: Initiative Riz (IR or Rice Initiative) Population: n=136 households for production data and 392 households for sales data.	water management, fertiliser supply, poor quality seeds, late planting; for fertiliser demand: household socioeconomic and demographic status, market access, agro-ecological and unobserved time-constant effects; for whole economy: where the private sector is relatively active and average wealth is higher, subsidies have substantially crowded out the private sector, in some cases may lower overall fertiliser use. In poorer areas with inactive private sector, subsidies help to generate demand and crowd in private sector retailers. Because Zambia's fertiliser subsidy programme claims 35–40% of the overall public budget to agriculture, there is also a public investment crowding out dimension. Opportunity cost of subsidy programmes may crowd out other investments that might improve rural living standards. Targeting and scheme: The IR made subsidised fertiliser available to rice producers nationwide with a particular focus on farmers in the Office du Niger, where roughly 50% of Mali's rice is produced. The goal of the programme was to increase domestic rice production by 50% over the 2007/08 level, thereby increasing marketable surpluses and putting downward pressure on cereal prices.	(fertilisers) kg/ha, n=136 HH Yield (kg/ha), n=136 HH Rice sales (kg) n=392 households	(0.0148) [-0.002; 0.475]; RR (SE): 1.11 (0.00) SMD (SE): (-) 0.1683 (0.0148) [-0.4064; 0.0698]; RR (SE): 0.93 (0.00) [negative value explained by drought in endline year] regression coefficient (SE): 0.264 (0.042); SD of DV: 359.1 DV is HH rice sales (associated with rice production); t-stat/p-value of regression coefficient: t=6.31; p=0.000
Smale et al. 2014 3-stage	Country: Zambia Crop: Hybrid maize	Effect modifiers: District of residence, literacy level, land per capita, asset values, cell and radio ownership, rainfall affect access to subsidy and use of subsidised seed	Seed use kg/ha (OLS) n=12354 observations	SMD (SE): 0.08 (0.00) Farmers who did not benefit from the

STUDY*	INTERVENTION	IMPLEMENTATION/CONTEXT	OUTCOME	FINDINGS
regression Tobit & Instrumental Variables Risk of Bias: Moderate	Subsidy: Hybrid maize seed coupons. Programme: Farm Input Subsidy Programme (FISP) (2009/10 growing season) Population: n=1126 households that cultivated land in the 2009/10 rainy season	Targeting and Scheme: The analysis focuses on demand for seed, characterising smallholders with a high predicted demand for hybrid seed who were not reached by the subsidy programme. Cross-sectional data is used from the 2010 agricultural season and an instrumented control function approach to test the hypothesis that the subsidy on hybrid maize seed in Zambia is selectively biased. Consistent with other literature, the subsidy is a recursive determinant of seed demand, but in 2010, its recipients had more land, more assets, and lower poverty rates. Findings illustrate the social costs of the programme as currently designed and highlight the need to build alternative supply channels if poorer maize growers are to grow hybrid seed.		subsidy planted an average of 10.1 kg, less than half the amount planted by beneficiaries (23.2 kg).
World Bank 2014 Prospective DID Risk of bias: High	Country: Tanzania Crop: Maize and rice Subsidy: Fertiliser and seed (one acre pack) vouchers at 50% of the market cost Programme: National Agricultural Input	Effect modifiers: Region (aridity); voucher allocation; commercial fertiliser displacement; delayed delivery of vouchers and inputs; delayed payment of seed and fertiliser suppliers; misuse of vouchers Targeting and Scheme: The National Agricultural Input Voucher Scheme (NAIVS) is a market smart input subsidy programme designed in response to the sharp rise in global grain and fertiliser prices in 2007 and 2008. It aims to raise maize and rice production, and thus preserve Tanzania’s household and national food security. From 2008 to 2013, c. US$300 million	Yield (kg/acre)	Maize: regression coefficient (SE): 0.35 (0.052);); t-stat/p-value of regression coefficient: t=6.7; RR (SE): 1.06 (1.01) Rice: regression coefficient (SE): 0.208 (0.119);); t-stat/p-value of regression coefficient: t=1.75; RR (SE): 1.03 (1.02)

STUDY*	INTERVENTION	IMPLEMENTATION/CONTEXT	OUTCOME	FINDINGS
	Voucher Scheme (NAIVS) Population: n=200 villages	spent on giving >2.5 million smallholder farmers a 50% subsidy on a 1-acre package of maize or rice seed, and chemical fertiliser. Each targeted farmer was offered vouchers for seed, basal and top dress fertiliser redeemable, with a 50% cash top-up payment, at a local retail outlet. After 3 years of subsidy, farmers were expected to buy their own inputs. Using commercial agro-dealers encouraged the development and expansion of sustainable wholesale to retail input supply channels.		

Appendix H: Information reported on implementation fidelity and take-up

Study	Voucher delivery/collection	Leakage	Take-up/compliance	Other
Ajayi et al. 2009	No information	No information	No information	No information
Awotide et al. 2013	No information	Some participants used the seed for other purposes such as exchange, resale (p. 106).	No information	No information
Bardhan & Mukherjee 2011	No information		No information	No information
Carter et al. 2013	Late distribution of vouchers meant they were used late in growing season with ramifications for take-up, usage and impact (p. 1). Late distribution attributed to complexity/demands of supplying the inputs (p. 21). Only 50% of those entitled to receive a voucher actually received the voucher. Farmer credit was a big constraint (p. 9). Survey found that of farmers who had the right to receive a voucher but did not, 46% mentioned the reason was lack of money, 17% absent at distribution time, and 16% late distribution (p. 11).	A survey found that 4% of farmers admitted to having sold fertiliser. The authors thought this figure was probably under-reported (p. 11).	In some cases, subsidised inputs crowded out fertiliser that farmers would otherwise have bought (p. 10). Very low take-up: only 28% of the treatment group used the package for maize production (p. 21). Of those that received a voucher, only 57% redeemed it and used it for maize production. Reasons for not redeeming: 54 % money, 36% non-availability or late arrival of vouchers, or distance to collect money to be able to complete the transaction. Reasons given for not using it for maize:	Drought affected the impact of the inputs package (p. 1).

Study	Voucher delivery/collection	Leakage	Take-up/compliance	Other
			used on other crop 67%, 25% not yet used, 4% sold (p. 9).	
Chibwana et al. 2010	Village elites were more likely to receive an above average number of coupons (p. 17, Chibwana BASIS). Leakage of vouchers was a big problem (p. 4, Chibwana BASIS).	No information	No information	No information
Chirwa 2010	No information	No information	TIP farmers spread the small amount of fertiliser provided over a greater area than it was suitable for. Farmers were also not advised properly on how to apply fertiliser (p. 21).	No information
Denning et al. 2009	No information	No information	No information	No information
Holden 2013	On average, farmers received less than the standard two bags of fertiliser. Only 11% of female-headed and 29% of male-headed households received the full package. There was significant leakage at higher levels as well as some sharing between farmers (pp. 9–10, Holden & Lunduka, 2012 FDS).	Authors cite a World Bank report regarding possible corruption in tendering process to supply fertilisers: e.g. contracts were offered to some companies with prices as much as 20% higher than competitors (p. 18, Holden & Lunduka, 2012 FDS) Authors cite anecdotal examples of corruption: 1) a	Authors estimate that one third of fertilisers used in the subsidy programme contributed to crowding out of commercial demand (pp. 4–5, 13, Holden & Lunduka, 2012 FDS)	Authors note vulnerability to droughts of such programmes (p. 22, Holden & Lunduka, 2012 FDS).

Study	Voucher delivery/collection	Leakage	Take-up/compliance	Other
		top political party member caught with vouchers, 2) a thief caught selling vouchers was jailed but then released (Holden & Lunduka 2010), illegal printing of coupons and circulation of fake coupons, no proper record keeping. Subsidy programme was instrumental for re-election of president at 2009 election. Additionally, voucher recipients were often asked to pay an extra 200MK per fertiliser bag and no audit undertaken on the 800MK per bag to be transferred to the Ministry of Agriculture – indications that the money disappeared (pp. 18 p–19, Holden & Lunduka, 2012 FDS) A survey found 1% admitted selling coupons, which was probably an underestimation as around 25% said they were offered coupons on the secondary market (pp. 12–13, Holden & Lunduka, 2012		

Study	Voucher delivery/collection	Leakage	Take-up/compliance	Other
		FDS).		
Kamanga 2010	The programme targeted particular farmers but the villages shared inputs. The subsidy provided ×2 bags of fertiliser per household, but the village committee distributed ×1 per household (p. 52).	No information	Farmers applied 20 kg per acre rather than recommended 100 kg per acre (p. 53).	No information
Karamba 2013	Not all beneficiaries received the complete package of coupons. A complete package consisted of 4 coupons. However, 27% of beneficiary households received 1 coupon, 37% 2 coupons, 30% 3 coupons and the rest (6%) at least 4 coupons. A possible explanation is that local authorities may have diluted the distribution of coupons to reduce social divisiveness, opting to allocate fewer coupons to more households so that more households could benefit (pp. 11, 15–16).	Some redeemed coupons were either exchanged for another input (4%) or shared (11%) with a fellow farmer for nothing in return or both. Only 5% of coupons were not redeemed for various reasons including theft, selling coupons, giving them away, and shortages at the input suppliers (p. 16). Despite heavy subsidies, the value of each fertiliser coupon was greater than 10% of annual household income for about 40% of the population (p. 11).	No information	No information
Mason & Smale 2013	No information	No information	Some evidence of crowding out of commercial seed (p.	No information

Study	Voucher delivery/collection	Leakage	Take-up/compliance	Other
			668).	
Mather and Kelly 2012	No information	No information	No information	Extreme weather affected yields.
Parameswaran 2012	No information	No selling of vouchers, but author suggests survey bias might have limited reporting possible selling of vouchers.	Only one district with lower than 80% usage of inputs.	No information
Smale et al 2014	No information	No information	Using the package requires farmers to have access to a fair amount of financial resources (p. 4).	No information
World Bank 2014	Many farmers received their vouchers late, sometimes well after the beginning of the planting season (p. 8). Farmers tended to redeem fewer than the 3 vouchers received. This partly reflects the willingness of beneficiaries to share vouchers with neighbours. Lower redemption rates also reflect the late delivery of vouchers and inputs (p. 32).	The authors estimated that less than 1% of the vouchers were fraudulently redeemed. However, there were many rumours and reports of district officials working with local agro-dealers to redeem vouchers for their own benefit. Some of these cases were prosecuted by the police and anti-corruption agency (p. 9).	In some years, rising fertiliser prices in particular required that farmers pay 55–60% of the input cost.	No information

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Names and dates in italics refer to the designated study titles, which may be based on a single paper or represent the lead paper in a study that comprises several papers.

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