

Contract Farming: A Review

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Abstract

Although many urban areas around the world have grown steadily in recent years, the structural transformation, wherein an economy goes from relying primarily on agriculture and natural resources to relying primarily on manufacturing, has eluded many developing countries. In those countries, contract farming, whereby processors contract out the production of some agricultural commodity to growers, is often seen as a means of spurring the development of an agribusiness sector, and thus launch the structural transformation. As a result, contract farming has been extensively researched by economists and other social scientists over the last 30 years. We review the findings of the economics literature on contract farming and discuss its implications for development policy and research. In so doing, we highlight the methodological weaknesses that limit much of the literature on contract farming in answering questions of relevance for policy. Despite valiant research effort, many of the core features of contract farming imply substantial challenges for researchers aiming to study the question “Does contract farming improve welfare?” We conclude with a discussion of where we see the literature on contract farming evolving over the next few decades.

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1. Introduction

A key factor distinguishing the poor—those who live on less than \$3.10 on average per day—from the extreme poor—those who live on less than \$1.90 on average per day—is that the latter are much more likely to derive their livelihood from agriculture (Castaneda et al. 2018).¹ An easily observable and widely accepted reason for this reality is a persistent lack of economic specialization throughout the developing world, particularly in rural areas and the agricultural sector. Despite the rapid growth of urban areas, the structural transformation—the transition from an economy based on agriculture and natural resources to an economy based on manufacturing—has so far eluded many of the world’s poorest countries. Improving the livelihoods of the extreme poor by increasing the productivity of agriculture is therefore an often explicitly stated goal of international development policy.

One popular policy proposal among donors and multilateral agencies is for recipient governments to facilitate the expansion of contract farming,² wherein growers and buyers come to an agreement about the production of a specific agricultural commodity. Such schemes are not uncontroversial. On the one hand, many understand contract farming as an efficient and beneficial means of reducing transaction costs (Grosh 1994), thereby leading to improvements in terms of efficiency, if not of welfare. A host of empirical studies purport to find positive income effects for growers who participate in contract farming (Ashraf et al. 2009; Bellemare 2012; Minten et al. 2009; Miyata et al. 2009; Narayanan 2014; Rao and Qaim 2011; Schipmann and Qaim 2010). Others observe benefits in the form of farm profitability (Briones 2015; Huddlestone and Tonts 2007; Mishra et al. 2016), household asset holdings (Michelson

¹ See Ferreira et al. (2016) for a discussion of the poverty line conventions we adopt here.

² See, for instance, Eaton and Shepherd (2001) in a document prepared for the Food and Agriculture (FAO) of the United Nations titled *Contract Farming: Partnerships for Growth*. More recently, Sarkar (2014) discusses leaked policy documents from a leading consulting firm which suggested that the government encourage farmers to enter into contract farming in West Bengal. For their part, Kaur et al. (2016) suggest that institutions such as contract farming can counter the spate of farmer suicides in India, and Shukla et al. (2016) discuss the role of public-private partnerships in agricultural development policy.

2013), household food security (Bellemare and Novak 2017), and subjective well-being (Dedehouanou et al. 2013).

On the other hand, many are skeptical about the impacts of contract farming. Singh (2002a; 2002b) raises concerns about environmental degradation; Porter and Phillips-Howard (1997) argue that even if there are monetary gains from participating in contract farming, this need not be welfare-enhancing since participating farmers are forced to work longer hours and perhaps use their own children for cheap labor; Little and Watts (1994) worry that contract farming may increase income and wealth inequality. Supporting inequality concerns, Isager et al. (2018) observe that contract farming leads to the concentration of wealth in Tanzania, and Michelson (2013) finds that farmers with more advantageous endowments of land and water are likely to participate in contract farming in Nicaragua. Finally, Ragasa et al. (2018) find that the average increases in yield associated with participation in contract farming are not large enough to compensate for the associated higher input requirements in Ghana. Each of these findings suggest limitations to contract farming as an effective strategy to alleviate poverty in rural areas.

Recent work has attempted to summarize and synthesize these findings. Wang et al. (2014) reviewed the literature on the effect of contract farming on farm productivity and household income. They find that 92% of studies estimate a positive effect of contract farming participation on productivity, and 75% estimate a positive effect on income. More recently, in a systematic review of the literature on the income effect of contract farming, Ton et al. (2018) found evidence of both publication and survivorship bias. This allows for potentially spurious general conclusions about the effectiveness of contract farming.³

Our goal is to shed a more nuanced light on the literature on contract farming in developing countries. Several factors limit researchers' ability to draw any broad conclusions about the effectiveness of contract farming as a policy tool or institutional arrangement. First and foremost, causal

³ For other reviews, see also Bijman (2008), Croppenstedt et al. (2013), Minot (1986), Oya (2012), Senanayake (2005), and Singh (2000).

identification is difficult in empirical studies on contract farming (Barret et al. 2012) and so the internal validity of this literature is relatively low. Many early empirical studies rely on cross-sectional data and simply compare mean outcomes between households who participate in contract farming and those who do not (Goldsmith 1985; Singh 2002a; Singh 2002b). A particularly challenging limitation of these studies is selection bias, or the fact that farmers choose whether to participate in contract farming on the basis of factors that are both unobserved by researchers and highly likely to be confounders. Aiming to improve on these early studies, many researchers employ econometric techniques such as a selection-correction methods or instrumental variables estimation (see, for example, Bolwig et al. 2009; Briones 2015; Miyata et al. 2009; Schipmann and Qaim 2010; Simmons et al. 2005; and Warning and Key 2002). The legitimacy of the estimates within these studies relies on the validity of the variables excluded from the equation of interest in both selection-correction and instrumental variables studies. As we discuss below, the identifying assumptions in many of these studies do not hold up very well to closer scrutiny.

Second, the effects of contract farming are highly heterogeneous and context-dependent, and so external validity is also relatively low. Simmons et al. (2005), for instance, study contracts for maize, poultry, and rice in Indonesia, and find that higher returns for participating households than for nonparticipants in both maize and poultry contracts, but not in rice contracts. Therefore, even within a common context, effects are inconsistent across commodities. This heterogeneity aligns with the idea that contract farming arrangements arise out of the need to reduce transaction costs (Grosch 1994). Since different settings experience different types of transaction costs, the effects of contract farming will likewise differ. Ultimately, this limits external validity, i.e., the ability to extrapolate research findings across time and space.

The remainder of this article is organized as follows. In the next section we offer some background information on the institution of contract farming and define the central question of this literature, viz.

“Does contract farming make people better off?” In section 3 we review the literature. Section 4 discusses our perspective on the directions the contract farming literature should take over the next few decades. We conclude in section 5 with a summary and some concluding remarks.

2. Defining the Question

Our core question in this review is “Does contract farming improve welfare?” Before we can begin answering this question, it is worth spending time defining what the constituent parts of this question—“contract farming,” “improve,” and “welfare”—actually mean.

2.1 What is contract farming?

In its most generic terms, contract farming is an agreement between a grower and a processor regarding the production of an agricultural commodity. In practice, the specific terms and structure of these contracts can vary quite dramatically. Due to limitations of space, we cannot go into the details of how contract farming agreements can vary in their specific terms; the interested reader can start with Bellemare (2012), who offers a discussion of the different features found in the contracts in his data.

In the literature, the terms “contract farming” and “outgrower scheme” are often used interchangeably. Some studies, however, draw a distinction between the two: “contract farming” refers to private schemes and “outgrower scheme” refers to those arrangements involving public enterprises, parastatals, government agencies, or NGOs (Glover 1990; Glover and Kusterer 1990). In this review, we focus solely on contract farming, i.e., on arrangements between private actors.⁴ Moreover, we focus solely on contract farming in developing countries. Lastly, both due to space limitations and because the authors are both applied economists, we focus on the economics literature. Specifically, we retain for

⁴ To keep the size of this review manageable, we focus throughout this review on contracts between private actors, and so we exclude cases where the processor—the principal, in the principal-agent relationship that contract farming constitute—is a public-sector entity or parastatal.

this review all empirical articles in the EconLit database which contained the terms “contract farming,” “outgrower,” or “grower-processor” in their abstract. This yielded a total of 114 articles, most of which we discuss in this review. Those articles we do not discuss were simply too far from our focus to be included in this review. In addition, there were two articles which we could not unearth copies of, and so while they are in our list of references, we will not otherwise discuss them (Gayathiri and Valliammai 2011, Kaur et al. 2017). That said, we will also discuss some articles in our review that were not included in the 114 articles we obtained from our search of EconLit, often including them because they look at an interesting research question or because they do something particularly well. Finally, we do not discuss articles that focus on the determinants of participation in contract farming, as the conclusions of such studies tend to be more likely to be subject to Simpson’s paradox than studies aimed at testing the impact of contract farming on welfare.⁵

2.2 Who is involved?

Typically, when the literature discusses participants in contract farming the people being referred to are smallholder farmers. This presents a methodological challenge. As discussed by Barrett et al. (2012), there are several stages in the contracting agreement where either the buyer or the grower—or both—make decisions. In particular, buyers contract with the growers whom they think are the most likely to maximize their profits, which may exclude smallholder farmers (and thus the extreme poor, since landholdings are often one of the main sources of wealth) due to a perception among buyers that larger and better-endowed farmers are more profitable contracting partners. If this exclusion of smallholders is widespread then concerns about the effects of contract farming on inequality become all the more valid.

⁵ On Simpson’s paradox, see Pearl (2009). The reader interested in the determinants of participation in contract farming can read Abebe et al. (2013), Cembalo et al. (2014), Divya and Mahendran (2014), Guo et al. (2007), Karaan (2002), Ochieng et al. (2017), Roldan et al. (2013), Rout et al. (2013), Singh (2000), and Wang et al. (2011).

Additionally, some studies are interested in farmers, or non-farmers, who don't participate in contract farming. Studies by Minten et al. (2007) and Schipmann and Qaim (2010) look for the presence of spillover effects on nonparticipating crops and farmers. Porter and Phillips-Howard (1997) express serious concern for the welfare of laborers who may be made worse-off due to contract farming. These concerns extend beyond the welfare effects of contract farming within the agricultural labor market and toward issues relating to labor exploitation, worker conditions, and other dimensions of welfare. Despite the findings of these studies, a relatively open question relates to the general equilibrium effects of contract farming, whereby the institution may have indirect effects those who neither participate as growers or buyers through price and relative income effects, not to mention through other possible positive or negative externalities. Finally, little to no attention is spent considering the welfare of the buyer within contract farming agreements. This is likely the case because of little general concern about buyers, be they multinational corporations or farmer cooperatives, being exploited by growers.

2.3 What is welfare?

Welfare, particularly at the level of the household, is a complex concept. First off, household welfare is multidimensional if only because a household is composed of several individuals, each with his or her own level of welfare, and combining each individual's welfare into a single household measure of welfare is often computationally unsatisfactory, if not impossible (Gorman, 1953). Second, though economists use the concept of utility when performing welfare analysis, utility can neither be directly observed nor quantitatively measured. As such, the vast majority of studies adopt a welfarist approach, approximating welfare with income. Others use alternative, non-welfarist measures such as subjective well-being (Dedehouanou et al. 2013), household food security (Bellemare and Novak 2017), household assets (Michelson 2013), or farm profits and productivity (Briones 2015; Huddleston and Tonts 2007; Mishra et al. 2016).

Finally, the observation of participation in contract farming suggests, at minimum, that households expect their welfare to improve due to engaging in contract farming. We do not *a priori* know, however, how or in what dimension any household expects its welfare to improve. Additionally, real world experience tends to be idiosyncratic. Therefore, barring any coercive contracting, the sustained participation in contract forming of the same households over time should suggest that contract farming does indeed improve household welfare.⁶ Otherwise, because participation in contract farming is voluntary, the household would have no reason to participate.

2.4 Attribution

When we ask whether contract farming makes people better off, we ask whether contract farming *causes* welfare improvements. In this context, “attribution” is used in the sense given to it in the Roy-Rubin causal model (Rubin 1974). In that model, attributing the impact of contract farming on welfare requires taking the difference between a household's welfare if they are participating in contract farming and the same household's welfare if they were not participating in contract farming. Obviously, this is made impossible by the fact that a household cannot be observed simultaneously participating and not participating in contract farming.

Ideally, a researcher would randomly assign contracts to smallholder farmers. In practice this ideal is complicated by the fact that contract farming is typically a bilateral monopoly in which a grower interacts with a processor, both of whom are aiming to do what is best for themselves. It would thus likely be difficult (if not impossible) to convince a buyer to agree to randomize contract recipients or to convince growers to participate in contracts they are randomly assigned to.

⁶ Wendimu et al. (2016) offer an analysis of a compulsory contract farming scheme in Ethiopia. Using propensity score matching, the authors find a meaningful reduction in income and asset stocks of participants in this "forced" contracting arrangement.

The latter might not altogether spell doom for the identification of causal relationships, for two reasons. First, it would allow getting an intent-to-treat (ITT) estimate by regressing welfare on whether a grower is assigned to participation in contract farming, and second because with enough compliance, it would allow getting an estimate of the local average treatment effect (LATE) of participation in contract farming. That said, in most cases, the ITT and the LATE will be different from the average treatment effect, which is often what policy makers are most interested in.

Still, there likely would be ethical concerns if a researcher were to intentionally prohibit contracting between a grower and a processor, even for legitimate methodological research purposes. If there is no clinical equipoise—that is, if the researcher has a clear prior regarding what she expects to find—and contract farming really does improve welfare, preventing a mutually beneficial arrangement ventures into ethical ambiguity.

Instead, the literature is largely composed of observational studies that aim to understand the effects of contract farming by comparing participants with nonparticipants using methods that vary in their degree of statistical sophistication. This task is not easy since a rigorous assessment of the impact of contract farming on welfare needs a way to get around the issue of selection, which can occur at several phases in contract farming arrangements (Barrett et al. 2012). This identification problem is both pervasive and persistent in the literature, and will serve as a key point of discussion for the remainder of this review.

3. What We (Don't) Know

In this section we discuss what we know, and what we do not know, from the literature on contract farming. Specifically, we divide the literature into two blocks. The first block consists of studies examining the effect of contract farming on household welfare; this section constitutes the bulk of this

review, and it groups the studies in the literature by the empirical method they rely on to make a statement about the welfare impacts of contract farming.

The second block includes studies looking into important factors that condition, mediate, or explain the heterogeneous effects of contract farming; this subsection discusses factors such as the presence of NGOs or government programs, contract enforcement, and institutional or psychological factors.

3.1 Welfare Effects

As previously mentioned, the early work aiming to understand the welfare effects of contract farming simply compared mean outcomes (e.g., income) between participating households with nonparticipating households. Performing a case study analysis across a variety of contexts, Goldsmith (1985) compares the outcomes of growers and non-growers and finds that participation in contract farming is associated with higher incomes and faster adoption of modern production technologies. Using a similar methodological technique, Singh (2002a; 2002b) also finds that participants in contract farming have higher incomes than nonparticipants. The core issue with these studies is that they do not account at all for the fact that growers and processors both choose whether to contract with each other. In short, participation in contract farming is endogenous; therefore these effect estimates are likely biased. In particular, it is not clear whether richer farmers are simply more likely to participate in contract farming. Studies in this category—which we dub “case studies”—include Asokan and Singh (2003), Birthal and Joshi (2009), Dhillon and Singh (2006), Dolan 2002, Hultman et al. (2012), Karuppusamy et al. (2014), Maertens and Swinnen (2012), Moyer-Lee and Prowse (2015), Nolte and Ostermeier (2017), Porter and Phillips-Howard (1995), Ram and Kumawat (2015), Ramamurthy (2011),

Ravikumar (2015), Raynolds (2002), Rohini and Selvi (2015), Sachikonye (2016), Sharma (2014), Singh (2002a, 2002b, 2005, 2008, 2016), Singh and Kaur (2016), Swain (2016), and Wang et al. (2014).⁷

Recognizing the problem with simply comparing contract farming participants with nonparticipants, many studies adopt empirical strategies (i.e., combined estimation and identification strategies) that purport to correct for selection (see, for example, Bellemare 2012; Bolwig et al. 2009; Miyata et al. 2009; Schipmann and Qaim 2010; Warning and Key 2002). These empirical strategies can in principle successfully estimate the causal effect of contract farming on welfare, but—similar to instrumental variable estimation—doing so relies on the identifying variable in the selection regression satisfying the exclusion restriction (Heckman 1979).⁸ The validity of this exclusion restriction is rarely, if ever, testable, which means that it ultimately must be assumed. In many cases, the assumption that the identifying variable in the selection-correction estimator is excludable is difficult to believe. We will discuss some of these studies. Although our discussion includes the weakness of the identifying assumptions, we would rather not lose sight of the fact that many of the studies we discuss were methodological improvements in their time. Our comments do not imply statements about the overall quality of this work, but rather are aimed at informing the confidence we have in the reported results.

Studying contract farming arrangements in Senegal, Warning and Key (2002) use a selection-correction empirical strategy and find that contract farming increases income, even among the poorest households. The identifying variable in their model is a measure of honesty of the household's primary

⁷ We view these case studies as distinct from qualitative evidence, such as that found in studies by Ayako (1989), Ayako et al. (1989), Dunham (1993), Glover (1990), Maganya et al. (1989), Nyirongo and Shula (1989), Porter and Phillips-Howard (1997), and Sithole and Boeren (1989).

⁸ The core insight of Heckman's selection method is to view selection bias as an omitted variable problem. In the context of contract farming, estimating the effect of participating in contract farming on income is potentially biased by the fact that farmers nonrandomly choose to participate in contract farming. Heckman's two-step selection correction approach first estimates the probability of participation in contract farming based on observable characteristics. Next, a selection term derived from this probability—the inverted Mills ratio—is included in the equation estimating the effect of contract farming on income. A requirement for this procedure to adequately control for selection is there must be at least one variable in the first-stage participation equation with a non-zero coefficient that does not appear in the income equation. This "excludable" variable is akin to an instrumental variable, and the whole setup is highly similar to a two-stage least squares setup (2SLS).

decision maker. The identifying assumption in this case is that honesty is both (i) correlated with participation in contract farming, and (ii) not predictive of household income except through participation in contract farming. The authors state that they, "... do not expect honesty to be correlated with income," but do not explain why. In particular, it is not entirely obvious why a household with a more honest decision maker would not be more likely to earn more income overall. It could simply be that honest people prefer working with other honest people, and economic transactions between honest people will produce a higher return than transactions between less honest people. If this were the case, then honesty would be correlated with income outside of participation in contract farming and the estimated effect would be biased.

Another example is Miyata et al. (2009), who study the impact of contract farming on income in China. They use a selection-correction empirical strategy that relies on the excludability of a variable measuring distance between the farm of a household and the farm of a village leader. Using this identification strategy, Miyata et al. (2009) find that contract farming increases per capita household income. The identifying assumption for this study is that distance from the village leader's house is (i) correlated with participation in contract farming, but (ii) not correlated with per capita household income in any other way than through participation in contract farming. The authors claim that "... the village leader plays an important role in selecting farmers for participation in contract farming," thereby providing a good theoretical reasoning for why this is a relevant instrument. It is less clear, however, that this distance does not have an independent effect on income; that is, it is less clear that the exclusion restriction is met here. For instance, soil quality (or other agro-ecological conditions) might be better closer to the village leader's house. If this were the case, the finding that contract farming increases household income is potentially spurious. Studies in this category include Bolwig et al. (2009), Cahyadi and Waibel (2013), Freguin-Gresh et al. (2013), Miyata et al. (2009), Sharma (2008), and Warning and Key (2002).

One strategy for improving estimates of the causal impact of contract farming is to use propensity score matching (PSM) methods. Propensity score matching and other matching techniques are closely related to OLS regression in the sense that identification of treatment effects is based on observable characteristics. The familiar OLS regression framework estimates treatment effects conditional on other observable characteristics, but does not adjust these effect estimates based on a comparable counterfactual. Matching techniques aim to construct reasonable counterfactuals, based on observable characteristics, for the estimation of treatment effects. One benefit of PSM methods is that identification does not rely on an identifying variable satisfying an exclusion restriction. Rather than calculating a predicted probability of participation in contract farming (as in Heckman's selection method), PSM directly calculates household propensity to participate based on observed characteristics.⁹ Mishra et al. (2016) use PSM to estimate the impact of contract farming on the costs, yield, and profits of smallholder farmers in Nepal. The authors find a statistically significant positive impact of contract farming on revenues, profits, as well as yield, and a statistically significant negative impact on costs of production. Additionally, they find that small farms tend to gain most in terms of yield per hectare. The benefit of PSM is that it eliminates the selection bias associated with observable differences between participating and nonparticipating farmers. Concern still persists, however, that unobservable differences associated with participation in contract farming, such as farming ability or motivation, continue to bias the result. Studies in this category include Bannor et al. (2017), Cahyadi and Waibel (2016), Herrmann and Grote (2015), Herrmann (2017), Maertens and Vande Velde (2017), Mishra et al. (2016), Wainana et al. (2014), Wendimu et al. (2016).

Other studies try to overcome the challenge of endogeneity by using instrumental variable estimation (Bellemare 2012; Briones 2015; Simmons et al. 2005). Aiming to improve on the effect of contract farming on household income, Bellemare (2012) uses contingent-valuation methods to control

⁹ See Morgan and Winship (2007) or Li (2012) for more specific details about how PSM differs from OLS regression and Heckman's selection method.

for unobserved heterogeneity among households in Madagascar. Specifically, indicators of willingness to pay (WTP) for contract farming are used as a vector of instrumental variables for participation in contract farming. Using these measures of WTP as an instrumental variable, Bellemare (2012) finds evidence of a positive income effect from participating in contract farming. This method has the advantage of controlling for numerous factors that may be omitted by other studies, such as ambiguity and risk preferences, subjective perceptions about contract farming, entrepreneurial ability, technical ability, time preferences, etc. This identification strategy is not perfect, however. Although selection bias and other issues related to unobserved heterogeneity are plausibly controlled for by the WTP instrument, the presence of reverse causality may still bias estimates of the causal effect of contract farming. For example, richer households may have a higher WTP for contract farming and are therefore more likely to participate in contract farming. If this reverse effect is present, then the findings of a positive effect of contract farming on income may still be biased.¹⁰ Studies in this category include Bellemare (2012), Briones (2015), Mwambi et al. (2016), Narayanan (2014), Ramaswami et al. (2009), Sharma (2008), Simmons et al. (2005), and Trifkovic (2014, 2016).

A number of studies also rely on a selection-on-observables strategy, in which the researchers argue that the observables they account for in their empirical analyses account for grower self-selection into participating in contract farming. Using the WTP experiment in Bellemare (2012) as a control variable instead of as an IV, Bellemare and Novak (2017), Bellemare (2018), and Bellemare et al. (2018) argue that because grower WTP to participate in a hypothetical contract farming agreement is a good proxy for a grower's marginal utility of participating in contract farming, factors that usually go unobserved (e.g., ambiguity aversion, discount rate, entrepreneurial ability, loss aversion, overweighting of small-

¹⁰ Since that 2012 study, Bellemare and coauthors (Bellemare 2018; Bellemare et al. 2018; Bellemare and Novak 2017) have used WTP as a control variable rather than as an instrumental variable. As Henningsen (2017) shows on the basis of Monte Carlo simulations, using WTP as a control variable works better than as an instrumental variable. Moreover, the requirements for a control variable to be valid are considerably less stringent than the requirements for an instrumental variable to be valid.

probability events, risk aversion, technical ability, or any other factor that moves around the marginal utility of participating in contract farming) are accounted for, which obviates the need for more statistical sophistication. Dedehouanou et al. (2013), Lemeilleur (2013), Maertens and Swinnen (2009), and Wendimu et al. (2017) also rely on selection-on-observables designs.

Given the limitations and challenges posed by finding a credible estimate of the causal effect of contract farming, perhaps the best method is to implement a randomized control trial (RCT). Working with a Kenyan NGO that helps farmers export crops, Ashraf et al. (2009) implement an RCT where they randomly assign self-help groups (SHG) to three experimental treatments: (i) offered NGO services with credit, i.e., the first treatment arm, (ii) offered NGO services without credit, i.e., the second treatment arm, and (iii) no services and no credit, i.e., pure control. Rather than work with a for-profit company that buys agricultural commodities, Ashraf et al. (2009) work with non-profit intermediary that has a limited capacity to provide services. Although this design has internal validity and it may side-step ethical concerns with randomizing treatments, this study is limited in its external validity in that it is unlikely to mirror the institutional arrangement of contract farming in other contexts; if anything, most contract farming arrangements do not occur between a grower household and an NGO processor but between a grower household and a private-sector, for-profit processor. With these caveats in mind, Ashraf et al. (2009) find that the impacts of the program on household welfare depend on the definition of the sample. For the full sample, which includes both first-time growers and farmers who may have previously grown export crops, there is a positive but statistically insignificant effect of participation on income. When the sample is limited only to farmers who are growing export crops for the first time, however, the income effect is positive, economically meaningful, and statistically significant. A complication arose, however, a year after the experiment ended. The exporter refused to continue buying the crops grown by NGO participants due to the challenges involved with getting the growers to obtain the proper certification. Therefore, to call this intervention a success may be a bit contrived, and

it looks as though the trade-off between internal and external validity is a binding constraint in his literature.

Despite the methodological issues and the presence of identifying assumptions that may not be valid, it is worth noting that in each of the aforementioned studies, the effect of contract farming on income is positive. Indeed, Wang et al. (2014) report that 75% of studies find a positive income effect. This conclusion is complicated, however, by publication bias (Bellemare, 2015). That is, published studies may themselves be a biased source of information if, within the process of publishing those studies, null or contradictory results are not reported. Additionally, contract farming schemes that may actually lead to a decrease in household income are likely to fail and therefore are less likely to be empirically studied; this phenomenon is generally referred to as survivorship bias. Ton et al. (2018) investigate these dynamics by performing a systematic review of the income effectiveness literature in contract farming. The authors find evidence of both publication and survivorship bias. Ton et al. (2018) assess the presence of publication bias by regressing the effect size measured across various empirical studies on its own standard error. If there is no publication bias then the correlation between the effect size and the standard error should be approximately zero.¹¹ To the contrary, Ton et al. (2018) find positive and statistically significant correlations between measured effect sizes and reported standard errors. Survivorship bias is assessed logically. Since farmers cannot participate in contract farming arrangements that have previously failed, results from these failed arrangements are not included in this literature. Therefore, any broad assessment of the effect of contract farming on income is upwardly biased due to mechanically excluding studies that did not stand the test of time.

¹¹ To clarify this point, consider multiple studies examining the same empirical relationship. In the absence of publication bias and of issues relating to external validity, the effect sizes of these studies should be relatively evenly distributed within the range of the associated standard errors. In this case, the correlation between effect size and standard errors would be statistically zero. The presence of a non-zero correlation supports the presence of selection bias.

We offer a word of caution in taking the findings of survivorship bias too far. Although it is true that some contract farming schemes fail and these rarely appear in the scientific literature, we must be careful about correctly defining what we actually study when we study contract farming. Most studies in this literature define contract farming as an institutional arrangement whereby growers and processors agree to terms of production in regards to some agricultural commodity. Therefore, in impact evaluation parlance the “treatment” is the willing participation of two private actors within a market setting. Studies on the effectiveness of contract farming aim to study how the completion of contracts between growers and buyers impact development outcomes of interest such as poverty reduction among smallholder farmers. Put differently, the treatment effect of interest is the treatment effect on the treated, rather than the ITT. Since the treatment effect on the treated necessarily examines the treatment effect among those growers and buyers who are successfully implementing contracts, survivorship bias should not be viewed as an overwhelming concern in the contract farming literature.

Regardless of our interpretation of the evidence on the effects of contract farming on income, income is only a proxy for welfare. Additionally, it is not necessarily clear that extra income translates into other beneficial outcomes for households. As Bellemare and Novak (2017) note, any extra income gained from participating in contract farming is likely earned at harvest time and necessary expenditures may exist at other times of the year (Duflo, Kremer, and Robinson 2011). Additionally, self-control problems (Banerjee and Mullainathan 2010) and other issues with the institution of saving (Dupas and Robinson 2013) may prevent households from ultimately benefiting from contract farming. As such, some researchers have looked beyond income and at other measures of household welfare.

One candidate for an alternative measure of welfare is the household’s stock of productive assets. Motivated by the lack of a credible instrumental variable or method to control for selection bias, Michelson (2013) constructs an eight-period pseudo-panel of farmers in Nicaragua. The constructed dataset includes recall information on household asset holdings from farmers who supplied modern

supermarket chains and other non-supplier farmers. This sets up the opportunity for a difference-in-differences research design. Using that as her identification strategy, Michelson (2013) finds that farmers who supply supermarkets experience an increase in their stock of household assets of about 16% on average. This is an encouraging finding, because compared to income, only part of which can be saved to become assets, household asset stocks can directly contribute to future household productivity. Thus, at least in the Nicaraguan context, contract farming may have a lasting impact on household welfare. But even this study is not without its methodological weaknesses: although the difference-in-differences strategy represents a substantial methodological improvement over previous studies, Michelson's identification strategy ultimately relies on the assumption that the selection bias based on unobservable characteristics is constant over time. If unobservable factors that influence participation in modern supermarket supply chains are not constant over time, then the results will be biased by this selection effect.

Another dimension of welfare is food security. It is also not immediately clear that increases in income, if they are indeed caused by participation in contract farming, necessarily lead to increased food security and decreased prevalence of malnutrition (Gelli et al. 2015). Income is but one necessary input into improving nutritional and food security outcomes throughout the developing world. Bellemare and Novak (2017) examine the impacts of participation in contract farming of farmers in Madagascar on the duration of a household's hungry season. This paper uses a similar identification strategy to that of Bellemare (2012) where measures of a household's WTP for participation in contract farming controls for unobserved heterogeneity. As we noted earlier, the difference is that Bellemare and Novak (2017) use these WTP measures as control variables rather than as a vector of instrumental variables. The authors find that participation in contract farming reduces the duration of a household's hungry season by about eight days, and increases the likelihood that a household's hungry season ends on average by 18 percentage points. Although, in principle, similar methodological critiques also apply to Bellemare

and Novak (2016), it seems less likely—compared to Bellemare (2012)—that reverse causality seriously biases results. In particular, a household improving their food security and therefore increasing their WTP for contract farming seems to be plausible in only extreme cases.

Though the collection of longitudinal household surveys is now commonplace in developing countries, few articles in this literature rely on panel data. The only exceptions we know of are by Balineau (2013), Brambilla and Porto (2011), Jones and Gibbon (2011), and Muriithi and Matz (2014), but among those studies, only Jones and Gibbon (2011) focus on the welfare impacts of contract farming, finding that participation in contract farming increases incomes via the adoption of improved technology.

A handful of studies have relied on simulation methods to study the welfare impacts of contract farming. Though we count such simulation studies as “empirical” and they certainly have their use when considering a policy intervention *ex ante*, it is hard to assess them in relation with the remainder of the (*ex post*) empirical literature on contract farming, given that simulation studies tend to assume internal validity (i.e., they assume specific causal relationships) but to have little external validity (i.e., it is not clear how applicable their findings are to the real world). Examples of such studies include Arndt et al. (2010), Kabungo and Jenkins (2016), and Schuenemann et al. (2017).

As regards outcomes, some researchers have used subjective evaluations of well-being as an outcome variable to proxy for welfare. These subjective measures may more closely address concerns that the downside of contract farming may not necessarily be in immediate monetary terms but rather along psychological and social dimensions of life (Porter and Phillips-Howard 1997). In a study in Senegal, Dedehouanou et al. (2013) investigate the effect of contract farming, across multiple commodities, on subjective well-being, measured on a one through seven ordinal scale. The authors find that participation in contract farming has a positive and statistically significant effect on subjective well-being. The effects are different, however, for different types of contracts. In particular, the effect persists when

only looking at mango contracts, but not when only looking at green bean contracts. Although this paper provides suggestive evidence in favor of the use of income as a valid approximation for welfare, there are limitations to these results. The first limitation is in regards to endogeneity, since it is not clear how Dedehouanou et al. (2013) account for issues of selection bias and other issues related to reverse causality. The second limitation questions the treatment of the dependent variable measuring subjective well-being on a scale ranging from one to seven; in this case, OLS regression analysis may lead to biased estimates by assuming arbitrary and inaccurate intervals between categories on the ordinal scale (Bloem 2018).

Again, it is challenging to draw broad conclusions from the studies investigating the impact of contract farming on nonincome dimensions of welfare. The studies examine different measures of household welfare across different geographical contexts—assets in Nicaragua (Michelson 2013), food security in Madagascar (Bellemare and Novak 2016), and subjective well-being in Senegal (Dedehouanou et al. 2013)—but still represent a limited outlook, both in terms of the dimensions of welfare that matter and geography. Additionally, previous points about the potential for publication, as made by Ton et al. (2018), continue to apply to these studies as well.

3.2 Conditioning and Mediating Factors

In this section, we review studies that examine the factors that can influence or explain the success of contract farming. Theoretically, a myriad of factors could matter. As Barrett et al. (2012) emphasize, there are multiple stages where agents engaged in contract farming make choices based on conditioning or mediating factors. First, geographic factors and firm attributes influence the firm's choice of procurement location. These factors could include agro-ecological characteristics, transportation costs associated with the location, political-economic institutional arrangements, etc. Second, the firm offers a contract to the growers whom the firm expects will provide profitable returns on investment.

Influential factors in this stage could be observable characteristics of the grower such as farm size, use of irrigation and other modern technology, human capital, etc. (Michelson 2013). Third, growers decide whether or not to accept the contract based on whether they expect entering into a contract is worthwhile compared to all other accessible alternatives. Here factors include risk attitudes, information networks, and psychological factors such as self-efficacy, grit, and aspirations (Wydick and Lybbert 2017; Wuepper and Sauer, 2016). Finally, both the firm and the grower decide to honor the contract or renege on it by side-selling, or “leaking” contracted crops on the spot market (Fafchamps 2004). Factors influencing this decision include social capital, price volatility, legal institutional arrangements, etc. Clearly, a number of factors are potentially important. Although a substantial literature has formed around understanding the important conditional or mediating factors surrounding the effects of contract farming, there is much more to understand and study.

Reporting on results of the aforementioned RCT in Kenya, Ashraf et al. (2009) find that 41% of SHG members assigned to their first treatment arm and 27% of SHG members assigned to their second treatment arm joined the NGO program and began producing crops for export.¹² This suggests that access to credit is a meaningful and potentially limiting factor mediating the participation in contract farming. Abstracting away from the Kenyan RCT context, the idea that access to credit limits participation in (presumably) welfare-enhancing schemes is not very surprising. In particular, the market for contracts seems to already be aware of this idea since the buyer provides most necessary agricultural inputs to growers on credit. This credit is paid back to the buyer at harvest time, when the contract is completed. Indeed other services, such as agricultural insurance, may benefit from applying this lesson to the delivery of their own products (Casaburi and Willis 2017).

Again examining farmers in Madagascar, Bellemare (2010) finds that the number of visits by a technical assistant working for the buyer has a positive effect on agricultural productivity that is both

¹² Recall the difference between these treatment arms is the first received access to credit, while the second did not.

economically and statistically significant. Both the firms who engage in contract farming and the governments who aim to improve agricultural productivity in their country can make use of this finding, though the literature on agricultural extension services notes that producers tend to trust private extension agents more than they do public extension agents in developing countries (Umali-Deininger, 1997). Both of these entities possess the ability to send extension officers out to routinely check on farmers. The use of agricultural extension services alongside contract farming could be a beneficial way to improve the impacts of contract farming. More work examining complimentary policy tools could provide worthwhile policy-relevant insights.¹³

In Nicaragua, Michelson (2013) finds that farmers with “advantageous endowments of geography and water,” such as transportation options, are those that are most likely to (a) participate in supplying modern supermarkets with agricultural commodities, and (b) experience an increase in the stock of household assets. Obviously not all farmers are so uniquely endowed as to be able to participate in supplying modern agricultural value chains, even if they wanted to. Since most measures of poverty likely correlate with natural resource endowments and transportation options, this result casts some doubt on the idea that contract farming is a scalable solution for reducing poverty. This result, however, does allow for a prediction of what geographical locations may be excluded and could motivate the strategy and implementation for other complementary development interventions.

Michelson (2013) also finds that in Nicaragua, there is no differential effect on the accumulation of household assets among NGO-assisted farmers. Some speculate that the programs run by NGOs may be complementary and therefore enhance the benefits of contract farming among poor farmers. On the

¹³ On enforcement, Saenger et al. (2014) also conduct a field experiment wherein randomly selected smallholder dairy farmers in Vietnam are assigned to a treatment in which the quality of their milk is independently verified, thereby curbing the processor’s opportunity to cheat the growers by claiming a lower quality, and thus paying less for the milk. Saenger et al.’s findings are encouraging, as they show that growers produce more when they know the quality of their milk will be independently assessed.

other hand, when services from an NGO are similar to the perceived benefits of contract farming there may be risk that public actions crowd out private activity.

Although Michelson (2013) finds no evidence of a positive or negative effect of NGO assistance on the benefits of supplying supermarkets, Lambrecht and Ragasa (2018) find a negative correlation between the presence of public development projects and participation in contract farming. Moreover, this effect seems to be most likely driven by the presence of agricultural development projects.

Although this is an interesting result with direct policy relevance, it does come with caveats. The first is that Lambrecht and Ragasa (2018) are not able to account for all sources of endogeneity or rigorously pin down causality. Unlike Michelson's (2013) results, the results are merely associations and it could be that development projects are targeted in areas where contract farming is less successful. A likely scenario is development projects are targeted in areas where the circumstances that condition and mediate successful contract farming do not readily exist.

Since contract farming is often a response to transaction costs (Grosch 1994), mediating factors may have psychological or historical roots. Wuepper and Sauer (2016) examine pineapple farmers in Ghana and find that self-efficacy (the belief of an individual to have the ability to be successful) and social capital (the economic value of a person's relationships) are both important factors explaining successful integration into contract farming. Wuepper and Sauer (2016) point out that contract farming, particularly in Ghana, has heterogeneous effects. This heterogeneity is important to understand whether contract farming is going to be an important component to any rural or agricultural development strategy. The identification strategy used by the authors is to use so-called accidents of history—specifically the colonial establishment of Christian missionary schools and cocoa cooperatives—as instruments for self-efficacy and social capital. The authors find that successful integration in contract farming, measured in terms of income, is positively associated with cocoa cooperatives and negatively associated with historic locations of Christian missionary schools. This translates to cooperatives having

a positive effect and to Christian missionary schools having a negative effect on both self-efficacy and social capital. Finally, both self-efficacy and social capital are positively and statistically significantly related to income earned from contract farming.

Other studies looking at what influences or explains the success of contract farming include Key and Runsten (1999), Guo and Jolly (2008), and Kumar et al.'s (2013) observational studies of the determinants of contract fulfillment, as well as Kunte et al.'s (2017) experimental study documenting how relational contracts can help reduce the incidence of breach of contract and Saenger et al.'s (2013) lab-in-the-field experiment looking at different incentives schemes in Vietnamese dairy contracts.

4. A Research Agenda for Contract Farming

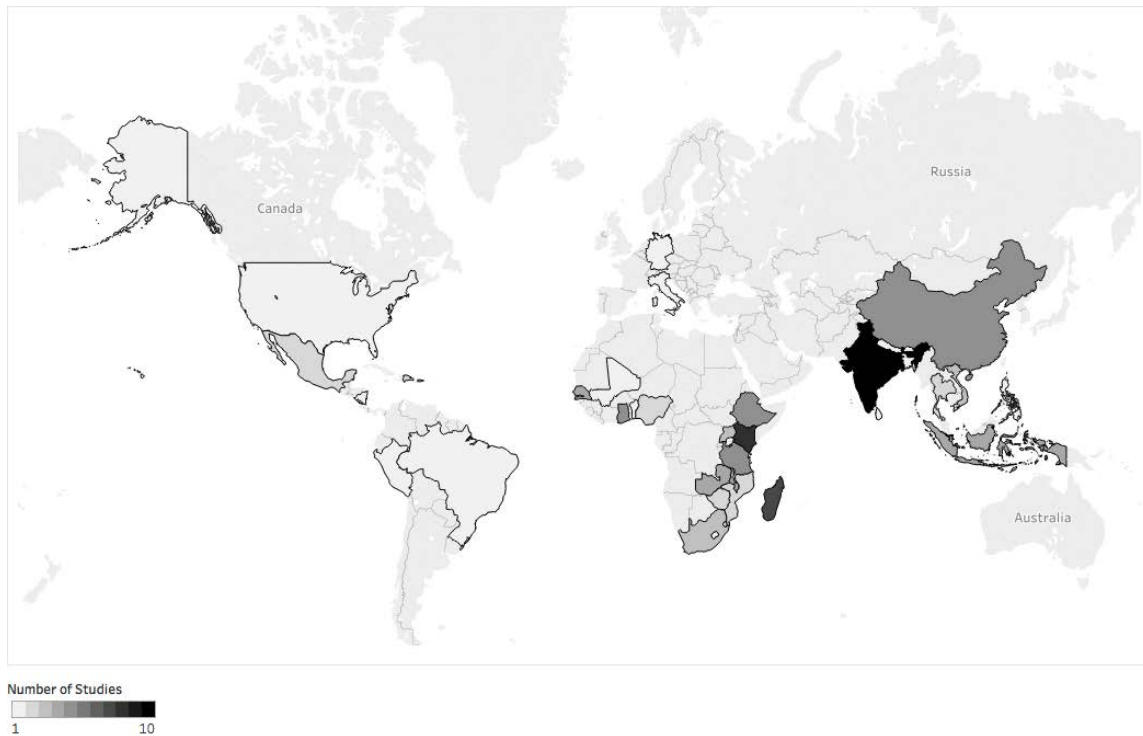
Having taken stock of the state of the empirical economics literature on contract farming, we now provide a discussion of where we believe that literature should be going in the next few years and decades.

From the foregoing, it is clear that the question “Does contract farming improve welfare?” has not yet been answered in a satisfactory fashion, whether one defines “satisfactory” in terms of internal validity, external validity, or both. On the internal validity front, though many studies likely come close to providing the right answer, just how close they get depends on how important grower selection into contract farming is in a given application. Unfortunately, that is something which just cannot be assessed statistically given extant methods and data. To truly make headway in terms of internal validity, our suggestion would be to either conduct an RCT or adopt a difference-in-differences design. Both approaches would require researchers to work with a processing firm. In the former case, the researchers would have to (i) convince the firm to randomize who it selects to contract with in a given community, and most likely (ii) offer those selected growers a randomized encouragement in order to

induce them to contract with the processor. This in principle would allow obtaining ITT and LATE estimates, which respectively would tell us the welfare impacts of being selected to contract with the processor and the welfare impacts of participation in contract farming for those growers whose participation is induced by the randomized encouragement. In the latter case, the researchers would have to ask the firm where and to which households it plans to expand its procurement activities in the future so as to collect pre- and post-treatment data on the households and communities involved in contract farming and on the households and communities not involved in contract farming; this would allow obtaining an ITT estimate, which would tell us the welfare impacts of being selected to contract with the processor. Moreover, knowing who the processor makes contracting offers to and exploiting that information as an instrumental variable would allow obtaining a LATE estimate, which would tell us the welfare impacts of participation in contract farming for those growers whose participation is induced by the processing firm's offer to contract. In both the RCT and the difference-in-differences cases, however, it is unclear just how useful knowing the ITT and the LATE would be to a policy maker interested in fostering the development of contract farming activities, as policy makers tend to be interested in the average treatment effect on the treated—that is, in the impact of contract farming for those households who do participate as growers.

Relatedly, most empirical studies of the impacts of contract farming focus on the benefits of participation, but to our knowledge, few studies have systematically explored the costs associated with participation in contract farming. We realize just how difficult it can be to properly estimate costs of agricultural production in contexts where many inputs (e.g., labor, organic fertilizer) are not purchased. Still, as Suri (2011) did for the literature on technology adoption, if the literature is to make an accurate statement about the welfare impacts of contract farming, we need to have an accurate representation not only of the benefits and costs of participation in contract farming, but also a good idea of how heterogeneous those benefits and costs are.

Figure 1. Heat Map of the Frequency of Studies of Contract Farming by Country.



Note: The number of studies is top-coded at 10 to preserve meaningful variation. India (30 studies) is the only country with more than 10 studies.

On the external validity front, we see two problems. First off, many studies only focus on a handful of commodities, or on a few communities that are relatively close to one another. The only exceptions we know to this are Bellemare (2012, 2018), Bellemare and Novak (2017), and Bellemare et al. (2017) who study contract farming across several regions, firms, and crops in Madagascar. A no-less-important problem is posed by the fact that empirical studies of contract farming have largely focused on a certain set of countries. Figure 1, which shows a heat map of the frequency of contract farming studies by country, highlights the problem by showing that some countries have been clear favorites (with India being the subject of 30 studies, Kenya of being the subject of eight studies, and Madagascar being the subject of seven studies) while entire regions have been practically ignored by the literature (Central and South America, Central Africa and parts of West Africa, as well as the Middle East and North Africa).

The literature should also spend more time investigating the mechanisms whereby contract farming improves welfare. The institution is thought to be able to help resolve a number of market failures, from the absence of agricultural extension services to the lack of adoption of new technologies to missing financial markets (Grosh, 1994), but so far, the literature has focused largely on asking whether contract farming makes growers better off. Beyond impact evaluation, we see a distinct need to bring the economics back into this literature by asking whether the institution does help resolve all those market failures. Should it be found to do so, then there is a much stronger case for policies aimed at fostering the emergence and the development of contract farming. One example of the following is ongoing work by Bellemare et al. (2017), who look at whether participation in contract farming is associated with more stable incomes—with whether contract farming can help partially resolve insurance market failures.

Likewise, although some of the research in this literature examines the spillover effects of contract farming (Minten et al. 2007; Schipmann and Qaim 2010), this is one area where the literature is relatively thin. If contract farming is an institutional arrangement that is part and parcel of the process of structural transformation, then there are likely to be additional effects of contract farming on those who do not directly participate, or on the other things those who participate are doing (or not doing, as it were). An exception to the latter is Bellemare (2018), who looks at the spillover of participation in contract farming on a grower household's various sources of income (i.e., income from livestock, from labor markets, from agricultural sources other than livestock or contract farming, from nonfarm businesses, and unearned income). This analysis focused attention on the spillovers within a household rather than between households. An understanding of the spillover and general equilibrium effects of contract farming will be a welcome addition to future research.

Whether participation in contract farming is sustained or not is unclear and should be investigated in future studies in contract farming, along with whether the institution is growing or shrinking, and for what reasons. Given that good market-level data on food markets in developing countries has become

available in the last 10 years, this and other market-level phenomena should be the subject of empirical industrial organization studies.¹⁴

Finally, there are opportunities for researchers working on contract farming in developing countries to incorporate insights from other areas of investigation. The work done by researchers in the literature we review is very close to what economists working on agribusiness are working on—oftentimes, only the contexts differ. Likewise, the work done by researchers in the literature on contract farming is very close to the work done by researchers in business schools and elsewhere working on supply chains. It would be a worthwhile endeavor to incorporate insights from the research on agribusiness and on supply chains. Likewise, it might be time to look beyond the micro-level (i.e., survey) data, and to look at more macro-level (i.e., trade or growth) data in order to see whether and how much contract farming contributes to economic development.

5. Summary and Concluding Remarks

We have surveyed the empirical economics literature on contract farming. Starting from a search of the EconLit database for articles containing the terms “contract farming,” “outgrower” and its extensions, and “grower-processor,” we initially identified 114 articles on the topic. Because contract farming has often been touted as a potential policy intervention to spark the oft-elusive structural transformation, we looked at how well the literature has done as it has attempted to answer the question “Does contract farming improve the welfare?” after excluding purely theoretical articles and those articles that

¹⁴ The field of development economics has changed considerably over the last 20 years, to the point where few economists are only-development economists anymore. Most development economists nowadays do x and development, where x can be agricultural economics, health economics, labor economics, law and economics, population studies, etc. One of the few fields that has been left alone by development economists is that of industrial organization (IO), most likely because the (structural) empirical methods of IO economists are often viewed with suspicion by mainstream (i.e., reduced-form) development economists. But since the first markets to modernize in developing countries tend to be food markets—as incomes rise, the demand for food both takes off and changes radically (Bennett, 1941)—we believe the next few decades will see an explosion of development-and-IO studies. Good examples of the kind of work we have in mind are Atkin et al. (2018) and Macchiavello and Morjaria (2011).

we deemed irrelevant. Ultimately, we found few if any studies whose findings were both relatively internally and externally valid. Concretely, this means that the answers to the question posed by much of the literature, about whether contract farming makes participating households better off, rarely have a shot at being causal, and that they almost always apply narrowly to a handful of crops, to a restricted geographical area, or to a single year. Similar to reviews of this literature to date by Glover (1984) and Senenayake (2005), we find it difficult to draw any broad policy relevant conclusions from the literature on contract farming.

Our work is not without its limitations. For instance, in order to circumscribe the size of this review within due bounds, we have had to ignore entire literatures, most notably on supermarkets in developing countries,¹⁵ on food standards (e.g., organic, sustainable, FairTrade),¹⁶ food supply chains,¹⁷ and on contract farming in developed countries. We also focused solely on contracts between private agents, and only in developing countries

Finally, our work has more implications for research than it does for policy. On the latter, we devote an entire section of this article to what we see as promising areas for research over the next few decades. We hope younger researchers will take on in earnest the research questions we lay out. On the former, because of the limitations of the literature we review, we caution policy makers against making too much from a series of findings which are, at best, tentative when it comes to making causal statements, and whose applicability remains limited.

¹⁵ See Reardon et al. (2003) for an early discussion of the “supermarket revolution” in developing countries and Demmler et al. (2018) for recent findings on the impact of supermarkets on nutritional outcomes.

¹⁶ See Meemken et al. (2017) on sustainability standards and Meemken and Qaim (2018) on organic standards. On trade standards for food more broadly, see Maertens and Swinnen (2009).

¹⁷For recent contributions to the literature on agri-food supply chains, see Du et al. (2016) and Zilberman et al. (2017).

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