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The Price Effects of Cash Versus In-Kind Transfers

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This article examines the effect of cash versus in-kind transfers on local prices. Both types of transfers increase the demand for normal goods; in-kind transfers also increase supply in recipient communities, which could lead to lower prices than under cash transfers. We test and confirm this prediction using a programme in Mexico that randomly assigned villages to receive boxes of food (trucked into the village), equivalently-valued cash transfers, or no transfers. We find that prices are significantly lower under in-kind transfers compared to cash transfers; relative to the control group, in-kind transfers cause a 4% fall in prices while cash transfers cause a positive but negligible increase in prices. In the more economically developed villages in the sample, households' purchasing power is only modestly affected by these price effects. In the less developed villages, the price effects are much larger in magnitude, which we show is due to these villages being less tied to the outside economy and having less competition among local suppliers.

Key words: General equilibrium, Price effects, Cash, In-kind, Welfare transfers.

JEL Codes: D50, O12, I38

1. INTRODUCTION

A central question in anti-poverty policy is whether transfers should be made in kind or as cash. The rationales for in-kind transfers include encouraging consumption of particular goods or inducing the less needy to self-select out of the programme (Nichols and Zeckhauser, 1982; Besley, 1988; Blackorby and Donaldson, 1988; Besley and Coate, 1991; Bearse *et al.*, 2000). These potential benefits of in-kind transfers are weighed against the fact that cash transfers typically have lower administrative costs and give recipients greater freedom over their consumption.

Another potentially important but less discussed aspect of this policy trade-off is the effect that in-kind and cash transfers have on local prices. Cash transfers increase the demand for normal goods, which will lead to price increases. This prediction holds either with perfect competition and

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The editor in charge of this paper was Stephane Bonhomme.

marginal costs that are increasing in quantity, or with imperfect competition even if marginal costs are constant or decreasing (under certain assumptions about demand, as we discuss in detail later). In-kind transfers similarly increase demand through an income effect, but, in addition, if they increase local supply (*e.g.* the government trucks food aid into a village), then local prices should be lower under in-kind transfers, relative to cash transfers.[1] From local suppliers' viewpoint, an in-kind transfer consists of a negative shock to the residual demand they face because the transfer has met some of consumers' demand, plus a positive demand shock due to consumers having higher income.

The pecuniary effects could potentially be a useful policy lever, as noted by the previous literature.[2] For example, the price declines caused by in-kind transfers could serve as a second-best way to tax producers and redistribute to consumers (Coate *et al.*, 1994). Similarly, Coate (1989) discusses how price effects could make an in-kind food aid programme more effective than a cash programme, depending on the market structure. And even if the main rationale for in-kind transfers is paternalism or self-targeting and the pecuniary effects are an unintended consequence, they might significantly enhance or diminish the programme goal of assisting the poor.[3] Note that under perfect competition, the price effects shift wealth between buyers and sellers, while with imperfect competition and prices above the first-best level, lower prices induced by in-kind transfers could represent an increase in efficiency, relative to cash transfers.

This article tests for price effects of in-kind transfers versus cash transfers in rural Mexico and compares both to the status quo of no transfers. We study a large food assistance programme for poor households, the Programa de Apoyo Alimentario (PAL). When rolling out the programme, the government selected around 200 villages for a village-level randomized experiment. The poor in some of the villages received monthly in-kind transfers of packaged food (rice, vegetable oil, canned fish, etc.) that were trucked in by the government. The market value of the food transfer was about 200 pesos (20 US dollars) per household per month; most of the in-kind transfer was inframarginal to households' consumption.[4] In other villages, the poor households received monthly cash transfers of similar value to the in-kind transfer. A third set of villages served as a control group. The vast majority of households in the villages, 89% on average, were eligible for the programme.

A comparison of the cash-transfer villages to the control villages provides an estimate of the price effect of cash transfers, which should be positive for normal goods since the income effect shifts the demand curve outward. The PAL in-kind transfer has a higher nominal value than the cash transfer (due to the idiosyncratic way that PAL administrators calculated the cost of the in-kind bundle). The in-kind bundle's true value to recipients is, coincidentally, very similar to the cash transfer on average (Cunha, 2014). Therefore, the *income effect* in the in-kind villages should be similar to that in the cash villages, and a comparison of in-kind and cash villages isolates the *supply effect* of an in-kind transfer—the change in prices caused by the influx of

1. Transfers can also take the form of vouchers, as in the U.S. Food Stamp and WIC programs. In this case, the programme increases demand for certain goods but local supply is not directly affected. We are considering in-kind transfers in which the government delivers the goods or services (*e.g.* public housing projects in the U.S., the Head Start programme), rather than providing vouchers. In addition, the type of transfer we consider is one in which the supply is sourced from outside the economy that receives the transfer.

2. We refer to the effects we study as "price effects" or "pecuniary effects". The data do not allow for a full examination of general equilibrium effects including effects outside the market for food or outside the recipient villages.

3. Another rationale for in-kind transfers is to insulate consumers from price volatility. The welfare effects of insurance against price fluctuations are more often discussed in the context of price stabilization policies (Massell, 1969; Deaton, 1989; Newbery, 1989). Lower prices also would further boost consumption of the in-kind goods (for both programme recipients and non-recipients); if encouraging consumption of these items is the paternalistic motive for using in-kind transfers, then the price effects will reinforce this programme goal.

4. Throughout the article, when we calculate the nominal value of the transfer, we use pre-programme unit values.

goods into the local economy. This supply effect should cause a decline in prices. We use pre- and post-programme price data collected from households and food stores to test these predictions.

We find no detectable increase in prices under cash transfers (though the point estimate suggests a small increase), while in-kind transfers cause prices of the transferred goods to fall by 3.7%. Across several specifications, we consistently find that providing transfers in kind rather than as cash causes prices to be lower by 3–4%. These effects are not limited to the short run; over the full range of programme duration in the data, from 8 to 22 months, the effects persist. Thus, the price effects do not appear to be undone by exit or entry of grocery shops in the village or other changes in market structure induced by the intervention, or alternatively, such adjustments take several years to materialize.

Goods that are not part of the transfer programme are also subject to pecuniary effects. The supply influx from the in-kind transfer should lower demand and prices for food items that are substitutes of the in-kind items. Empirically, the price effects for these other goods are small. Therefore, all told, the price effects have only modest implications for many households' purchasing power. This is noteworthy because programme eligibility is very high and the transfer is large relative to food expenditures, both of which result in a large aggregate shock to the local economy. This finding of, on average, small price effects suggests that for typical transfer programs, price effects may not be economically significant in many communities.

The exception is the less developed villages in our sample, as proxied by low average income, small population, and physical remoteness. In fact, the average effects we find are driven almost entirely by the subsample of villages with below-median development.[5] These villages could have larger price effects for at least two reasons. First, their goods markets could be less integrated with the regional or world economy, so local supply and demand determine prices. Second, there could be less competition among local suppliers (*e.g.* among grocery shops or distributors supplying those shops). We find evidence that both mechanisms help explain the result. Furthermore, surveys of store owners in a subsample of villages point to imperfect competition as a key feature of the market structure and an important factor in understanding the pronounced price effects in less developed villages.

For the less developed villages, in-kind transfers cause prices of the transferred goods to fall by 5% relative to cash transfers. In addition, cash transfers lead to a 1.1% increase in overall food prices; this implies an elasticity of prices with respect to income of 0.11, as the cash transfers in less developed villages constitute a 10% increase in aggregate income, on average. Choosing in-kind rather than cash transfers generates extra indirect transfers to consumers in the form of lower prices worth about 13% of the direct transfer itself in less developed villages; these effects have the opposite implication for food-producing households in the recipient villages. We should note that our estimates of the programme's total effects have wide confidence intervals, but they are nonetheless suggestive of quantitatively important price effects in poor communities. Mexico's very poor villages have a similar level of development—income level and physical remoteness—as many villages in Africa, Asia, and Central America (World Bank, 1994). Our results suggest that transfer programs in ultra-poor communities in developing countries may have important pecuniary effects. Meanwhile, if the recipient community is well-connected with larger markets or has a competitive supply side, or in general is more developed, then pecuniary effects are likely to be small relative to the direct benefits of transfer programs.

This article contributes to the literature on in-kind transfers, which has mostly focused on the consumption effects of in-kind transfers and on the political economy of transfer programs. (See Currie and Gahvari (2008) for a nice review of this literature.) Several studies have examined

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5. Consistent with this finding, Angelucci and De Giorgi (2009) do not find price effects of conditional cash transfers in Oportunidades villages in Mexico, which are more developed than the PAL villages.

the consumption effects of the PAL programme in Mexico (Gonzalez-Cossio *et al.*, 2006; Skoufias *et al.*, 2008; Leroy *et al.*, 2010; Cunha, 2014). They broadly find that cash and in-kind transfers lead to similar increases in total expenditures, although of different types of foods and non-foods. There is also extensive work on the consumption effects of other transfer programs, such as the U.S. Food Stamp programme (Moffitt, 1989; Hoynes and Schanzenbach, 2009). Other work examines whether in-kind transfers are effective at self-targeting (Reeder, 1985; Currie and Gruber, 1996; Jacoby, 1997). Another branch of the literature examines the political economy of in-kind programs, including their degree of voter support and how they affect producer rents (De Janvry *et al.*, 1991; Jones, 1996).

Fewer studies provide evidence on the question this article addresses, namely the price effects of in-kind transfers, and those that do often focus on voucher programs in which the government does not act as a supplier (Murray, 1999; Finkelstein, 2007; Hastings and Washington, 2010).[6] Another related literature is on the international food aid and local prices, but few of the papers in this literature aim to establish causality; for example, Levinsohn and McMillan (2007) use estimates of the supply and demand elasticity of food from the literature to gauge the potential price effect of food aid, and Garg *et al.* (2013) examine food aid and prices, but emphasize that their estimates are correlations and not necessarily causal effects.

Our article is also one of the first to measure the price effects of social programs. There is a vast literature that studies the direct effects of social programs, but fewer studies examine the indirect effects of programs and in particular their market-level price effects (Angelucci and De Giorgi, 2009; Lise *et al.*, 2004; Kaboski and Townsend, 2011; Attanasio *et al.*, 2012; Imbert and Papp, 2015; Muralidharan *et al.*, 2017). Our finding that the pecuniary effects of social programs can be quite large in underdeveloped communities is relevant when thinking about the impacts of many other programs in developing countries.[7]

Finally, our findings also contribute to an active area of policy debate. One of the largest and most prominent in-kind programs worldwide, the World Food Programme, is increasingly shifting towards cash transfers, and in many developed and developing countries there is policy debate about providing a universal basic income (UBI) (World Food Programme, 2011). Meanwhile, other major programs are moving away from cash towards in-kind transfers. For example, in the U.S. much of the welfare support under the Temporary Assistance for Needy Families programme is now in the form of child care, job training, and other in-kind services (Pear, 2003). For policy makers choosing between cash and in-kind transfers, our work highlights that their choice could have non-trivial implications for local prices in markets with imperfect competition. Moreover, when local suppliers have market power, changes in local prices are not just pecuniary externalities, but have efficiency implications too. These lessons are relevant in developing countries where most of the poor live in rural villages. They may also be applicable in developed countries: High-poverty neighbourhoods in the U.S. have high participation in transfer programs such as SNAP, and would experience large increases in average income through a UBI programme; meanwhile, they are often characterized as having few grocery stores and high food prices (Bell and Burlin, 1993; Talukdar, 2008).

The remainder of the article is organized as follows. Section 2 lays out the theoretical predictions. Section 3 describes Mexico's PAL programme, other aspects of the context, and

6. Murray (1999) examines the response by private suppliers in a market where the government does provide supply, U.S. public housing. Finkelstein (2007) finds that the Medicare programme caused health care prices to rise, and Hastings and Washington (2010) find that grocery stores in the U.S. set prices higher at the time of the month when demand from Food Stamp recipients is higher.

7. The article is also related to a broader literature on the determinants of prices in isolated markets in developing countries (Jayachandran, 2006; Donaldson, 2018; Atkin and Donaldson, 2015).

the experimental design. Section 4 describes our empirical strategy and data. Section 5 presents the results, and Section 6 offers concluding remarks.

2. CONCEPTUAL FRAMEWORK

In this section, we lay out the predictions about how cash and in-kind transfers affect prices. We do not present a formal model but instead informally derive the predictions that we take to the data.

In a small open economy, changes in the local demand or supply should have no effect on prices since supply is infinitely elastic with prices set at the world level. However, the rural villages that are our focus are more typically partially-closed economies in which prices depend on local conditions. In our empirical application, an economy is a Mexican village, and the main goods we examine are packaged foods. The local suppliers are shopkeepers in the village, and they procure their inventory from outside the village.[8]

We discuss, in turn, two possibilities: that the supply side has perfect or imperfect competition. In our empirical setting, imperfect competition appears to be the more relevant scenario.

2.1. *Perfect competition*

If the local market is perfectly competitive, then if the supply curve is positively sloped—that is, with increasing marginal costs—shifts in the demand for a good will affect its price. For local suppliers in Mexican villages, high transportation costs to other markets is one potential reason for increasing marginal costs, at least in the short run; to meet higher demand, a shopkeeper in a remote village might need to travel to a neighbouring village to buy supply from a shop there.

Figure 1A depicts the market for a normal good in a village. The demand curve represents the aggregate demand faced by local suppliers. The figure shows, first, the effect of a cash transfer: The demand curve shifts to the right via an income effect, and the equilibrium price, p, increases.[9] Denoting the amount of money transferred in cash by X_{Cash}, our first prediction is that a cash transfer will cause prices to rise:

$$\frac{\partial p}{\partial X_{\text{Cash}}} > 0. \tag{1}$$

In-kind transfers also generate an income effect, so demand will again shift to the right. We define the in-kind transfer amount X_{InKind} in terms of its equivalent cash value.[10] Thus the demand shift caused by a transfer amount X is by definition the same for either form of transfer. With an in-kind transfer, however, some of consumers' demand is now provided to them for free by the

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8. There is also a supply side of the market that is outside the local economy, namely the packaged food manufacturers, which are located in urban areas. If by increasing the total demand for the goods from food manufacturers, the government is driving up manufacturers' marginal cost (because they have decreasing returns to scale), then there would also be Mexico-wide price effects of the programme. These effect on prices would be very small since the programme households represent less than 1% of Mexican households, but these small effects would apply to the large set of consumers of the goods. Our focus is the price effects within the villages that receive the programme; thus, we examine only the local price effects in the recipient villages, and not the total price effect of the programme.

9. For inferior goods, demand will shift to the left with the opposite price effect. In unreported results estimating a QUAIDS demand function, we find that all of the food items we study are normal goods except for one (chayote). Similarly, Attanasio *et al.* (2013) find that most food items are normal goods in Mexico.

10. If either the transfer is inframarginal (*i.e.* it is less than the household would have consumed had it received the transfer in cash, valued at the market prices) or resale is costless, the cash value of the transferred goods is simply the market value. If, instead, the transfer is "extramarginal" and resale is costly, then the extramarginal quantity would be valued at between the market price and the resale price.
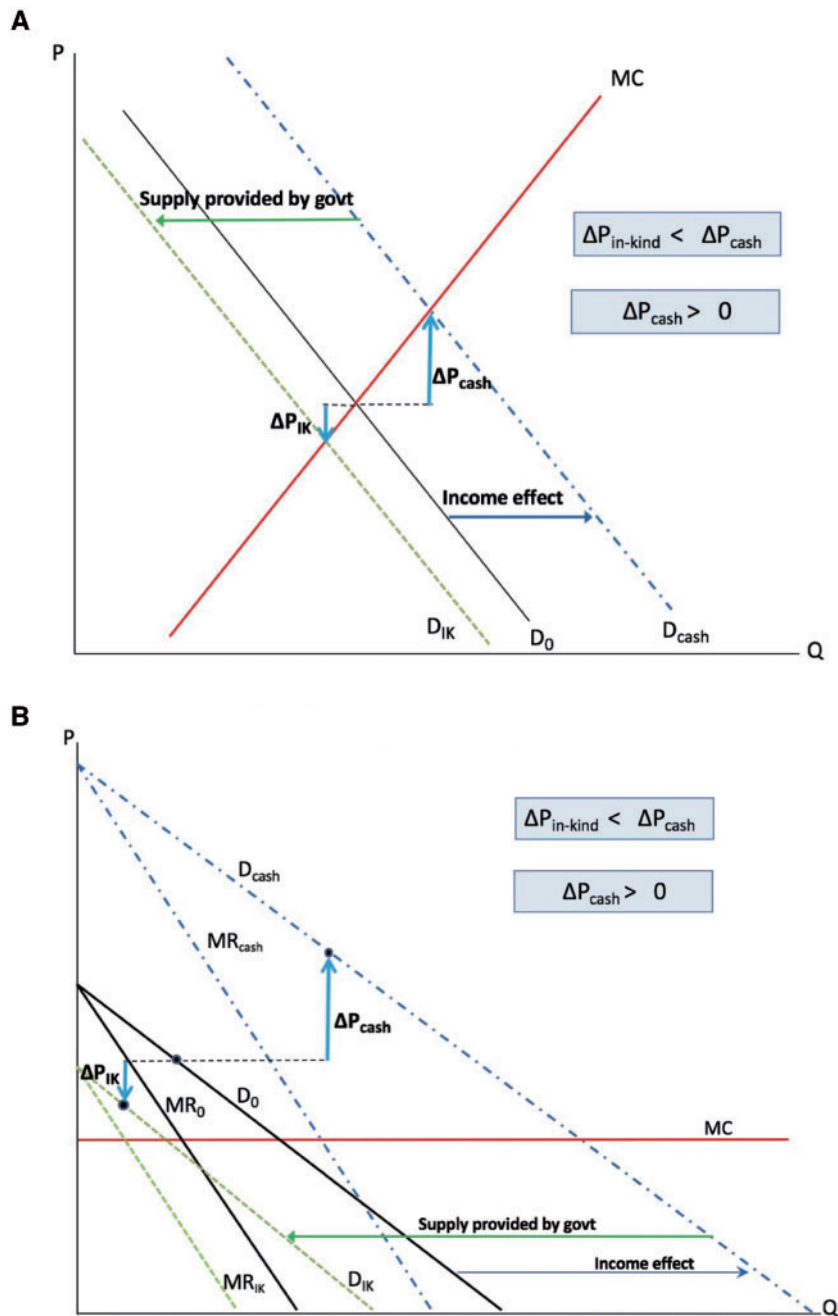
FIGURE 1

Effect of cash and in-kind transfers on prices in different competitive environments. (A) Perfect competition;
(B) Imperfect competition.

government, so the residual demand facing local suppliers shifts to the left by the amount provided in kind. While the net price effect of an in-kind transfer relative to the original market equilibrium is, in general, theoretically ambiguous, one can sign the price effect of in-kind transfers relative to cash transfers.[11] For transferred goods, the price should be lower under in-kind transfers:

$$\frac{\partial p}{\partial X_{\text{InKind}}} - \frac{\partial p}{\partial X_{\text{Cash}}} < 0. \tag{2}$$

Empirically, we will be better positioned to test Prediction (2) than Prediction (1). To detect the effect of the supply influx, we can concentrate on the nine specific goods provided in kind in the Mexican transfer programme we study. In contrast, the increased demand due to income effects will be spread across several food and non-food items. The cash transfer programme we study placed no restriction on how recipients could use the money, and it led to a small amount of extra demand per good, spread across many goods (Cunha, 2014).[12, 13]

2.2. *Imperfect competition*

In the setting we study, the supply side consists of food shops in the village and the distributors who supply the shops, trucking in food from outside the village. There are neither many shops nor distributors serving the typical village, so the degree of competition may be limited.

Predictions (1) and (2) can also hold in the case of imperfect competition. Importantly, in contrast to the case of competitive firms, under imperfect competition, transfer programs can have price effects even if marginal costs are constant. Figure 1B depicts, for simplicity, the case of constant marginal cost for a monopolist facing linear demand, but the same predictions of price effects hold more generally, as we discuss below.

Consider a Cournot–Nash model with N firms that have constant marginal cost c and face linear demand $p = d - Q$, where Q indicates quantity and d represents factors that shift demand. The equilibrium price is $p = (d + Nc)/(N + 1)$. Suppose the transfer changes the amount demanded from the local firms by an amount Δd; Δd is positive for a cash transfer and negative or less positive for an in-kind transfer. Then the change in price is given by $\Delta p/p = \Delta d/(d + Nc)$, which has the property that the higher N is (more competition), the smaller the magnitude of the price effects.

More generally, the price effects under imperfect competition depend on the shape of the demand curve. For example, if the programme causes a multiplicative shift in demand, then there would be no effect on prices in the standard Cournot model (Cowan, 2004). In other cases, an increase in demand can cause oligopolistic prices to fall; greater competition would still dampen the magnitude of the price effects. Appendix A presents a Cournot model with a generalized demand function and shows conditions under which an increase in demand leads to a higher price. A sufficient condition for Predictions 1 and 2 to hold is a downward-sloping demand curve where the transfers represent an additive shift in demand. The price effects then vary with the

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11. For many standard classes of preferences, such as homothetic preferences, prices are predicted to decline with an in-kind transfer relative to no transfer. For the price to increase, an in-kind transfer of a good with aggregate value X would need to increase aggregate demand for the good by more than X; in other words, the good would have to be a strong luxury good.

12. Cunha (2014) shows that 17% of the cash transfer is spent on PAL foods, while 50% is spent on non-PAL foods and 33% is spent on non-food goods.

13. Note that there could be a flypaper effect through which this cash transfer labelled as food assistance stimulated the demand for food more than a generically-labelled transfer would have (Hines and Thaler, 1995; Kooreman, 2000).

degree of competition as follows:

$$\frac{\partial^2 p}{\partial N \partial X_{\text{Cash}}} < 0, \tag{3}$$

and

$$\frac{\partial}{\partial N}\left(\frac{\partial p}{\partial X_{\text{InKind}}} - \frac{\partial p}{\partial X_{\text{Cash}}}\right) > 0. \tag{4}$$

The higher N is (more competition), the smaller in magnitude the price effect of a demand shift.

Note that price effects under perfect and imperfect competition have different efficiency implications. If lack of competition causes prices to be above their efficient level, then in-kind transfers can increase total surplus. Local suppliers' strategic rationing of supply is partly undone by the government provision of goods. (Note, however, that these potential welfare gains could be undone by inefficiencies in how the government runs the transfer programme.)

The discussion above takes the market structure as given. The programme could also affect how many stores stock a given product as well as entry and exit of stores and thus the degree of competition. For example, in response to a supply influx from the government, a shop might stop carrying a product or go out of business, reducing competition and causing prices to return to, or even exceed, the counterfactual price level without the programme. A positive demand shock (*e.g.* due to a cash transfer) could cause stores to open or more stores to stock a given good, increasing competition. The theoretical predictions are not clear-cut in many cases. For example, the in-kind programme also made villagers richer, so the net effect on store entry and exit or inventory decisions is ambiguous. In addition, the price effect of a store beginning to or ceasing to stock a product is not easy to predict because firms do not profit maximize separately for each product. Nonetheless, in general these responses on the supply side would cause price effects to be smaller. These changes would likely not occur immediately, but as they occur, the price effects would fade. Thus, we also examine whether the price effects dissipate over time.

The above are the main testable implications we take to the data. We next describe the transfer programme we study and discuss some of the above assumptions in the context of this programme.

3. DESCRIPTION OF THE PAL PROGRAMME AND CONTEXT

3.1. *PAL programme and experiment*

We study the Programa de Apoyo Alimentario (PAL) in Mexico. Started in late 2003, PAL operates in about 5,000 very poor, rural villages throughout Mexico. Villages are eligible to receive PAL if they have fewer than 2,500 inhabitants, are highly marginalized as classified by the Census Bureau, and do not receive aid from either Liconsa, the Mexican subsidized milk programme, or Oportunidades, the conditional cash transfer programme. Therefore PAL villages are typically poorer and more rural than the widely-studied Progresa/Oportunidades villages.[14] Households within programme villages are eligible to receive transfers if they are classified as poor by the national government.

PAL provides a monthly in-kind allotment consisting of seven basic items (corn flour, rice, beans, pasta, biscuits (cookies), fortified powdered milk, and vegetable oil) and two to four supplementary items (including canned tuna fish, canned sardines, lentils, corn starch, chocolate powder, and packaged breakfast cereal). All of the items are common Mexican brands and are

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14. Villages could be "too poor" to receive Progresa/Oportunidades because a requirement was that they had the capacity to meet the extra demand for prenatal visits and school attendance induced by the programme; villages that lacked adequate health facilities, for example, were ineligible for Progresa/Oportunidades.
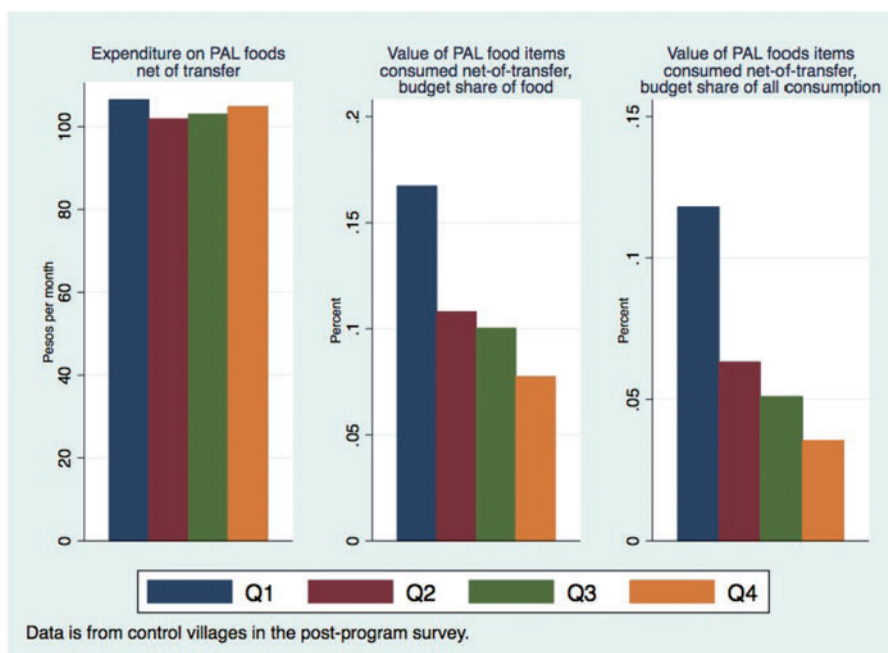
FIGURE 2

Expenditure on PAL goods across households. Means by quartile of per capita expenditure (Q1 are the poorest,
Q4 the richest).

typically available in local food shops. The basic goods are dietary staples for poor households in
Mexico. The supplementary goods are foods typically consumed by fewer households in a village
or less frequently; one goal of the programme was to encourage households to add diversity to
their diet and consume more of these supplementary goods.[15]

Most recipient households consumed a larger quantity of the in-kind items, particularly
the basic goods, than was provided in the transfer. That is, absent the transfer, their monthly
quantity consumed exceeded the PAL in-kind allotment. The fact that recipients made out-of-
pocket purchases of these goods even when receiving the in-kind transfer means that they were
affected by the price effects; otherwise, price effects would only be relevant for non-recipients.
Figure 2 shows the net-of-transfer expenditures on PAL goods (calculated using post-intervention
expenditure in the control group).[16] The poorest quartile of households spends slightly more than
the richest quartile on these items, and spends more as a proportion of total food expenditures.
Most of the PAL items are staple goods, which explains why they comprise a larger share of food
spending for the poor.

PAL is administered by the public/private agency, Diconsa. The Diconsa agency also maintains
subsidized grocery shops in some villages (38% of the villages in our sample), which are run by

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15. Appendix Figures A1–A4 show the PAL box, trucks transporting the boxes to a village, the unloading of the
boxes in the village, and examples of the grocery shops in the villages.

16. We use the good-by-good quantity consumed and subtract the quantity of the PAL allotment for that good, and
then multiply by the price. If the aggregate transfer to a village exceeds the village's aggregate consumption of a good, we
set out-of-pocket spending for the village-good to zero; this allows for within-village resale but assumes there is no resale
outside the village. For two food items (powdered milk and lentils), villages consumed less than the amount delivered in
kind, while for the other goods (*e.g.* vegetable oil, beans), they consumed more per month than the transfer.

a resident of the village. The government provides suggested prices to Diconsa store operators; the Diconsa stores are not obliged to use the suggested prices, but they must maintain prices that are 3–7% lower than market prices. Thus, prices at Diconsa stores should be responsive to market conditions, but to a lesser degree than at fully private stores.[17] The local supply side of the market is mostly composed of small private stores that stock food products, including the packaged foods that PAL provided, as well as sundry items. Small villages typically have one to six of these types of stores. Some households in the village also grow food which is substitutable with the PAL packaged foods.

Concurrent with the national roll-out of the programme, 208 villages in southern Mexico were randomly selected for inclusion in an experiment.[18] Each study village was then randomly assigned to an in-kind treatment arm, cash treatment arm, or the control group; the village-level randomization was not stratified on any characteristics. Eligible households in the in-kind villages received a monthly in-kind food transfer (50% of villages); those in the cash villages received a 150 peso per month cash transfer (25% of villages); and those in the control group villages received nothing (the remaining 25% of villages). About 89% of households in the in-kind and cash villages were eligible to receive transfers (and received them). Due to administrative capacity constraints, experimental villages were rolled into the programme over the course of 14 months, beginning in December of 2003. This gradual rollout creates variation in how long the programme had been running when endline data collection occurred in 2005.

Of the 208 villages in the experiment, 14 are excluded from the analysis. Eight villages do not have follow-up price data; in two villages, the PAL programme began before the baseline survey; two villages are geographically contiguous and cannot be regarded as separate villages; and two villages were deemed ineligible for the experiment because they were receiving the conditional cash programme, Oportunidades, contrary to PAL regulations.[19] Observable characteristics of the excluded villages are balanced across treatment arms. (Results available from the authors.) Of the remaining 194 villages, three received the wrong treatment (one in-kind village did not receive the programme, one cash village received both in-kind and cash transfers, and one control village received in-kind transfers). We include these villages and interpret our estimates as intent-to-treat estimates.

The aggregate impact of the PAL programme on a recipient village was large, both because the eligibility rate was high and because the transfer per household was sizeable. The in-kind transfer represented 18% of a recipient household's baseline food expenditures on average and 11% of total expenditures. Including the ineligible households, the injection of food into the village through the programme was equivalent to 16% of baseline aggregate food expenditures and 10% of total expenditures for the village. Similarly, the cash transfer represented an 8% increase in recipients' income and, in aggregate, a 7% increase in total village income.

In the in-kind experimental villages, the transfer comprised the seven basic items and three supplementary goods: lentils, breakfast cereal, and either canned tuna fish or canned

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17. Diconsa stores receive a government subsidy to cover transportation costs. Unlike fully private shops, they do not allow purchases on credit. After our study period, the government changed the discount that Diconsa stores are supposed to offer to 20% (private communication with programme administrators).

18. The experiment was implemented in eight states: Campeche, Chiapas, Guerrero, Oaxaca, Quintana Roo, Tabasco, Veracruz, and Yucatan. The 208 study villages were randomly chosen from among all PAL-eligible villages in these states, without stratification. See Appendix Figure A5 for the locations of the experimental villages.

19. The contiguous villages are named "Section 3 of Adalberto Tejada" and "Section 4 of Adalberto Tejada," which appear to be part of the same administrative unit. The correlation of baseline unit values between these two villages is 0.92. When we take random draws of pairs of villages in our sample and calculate the correlation of baseline unit values, the 99th percentile is a correlation of 0.51, suggesting that the contiguous pair is an extreme outlier and cannot be treated as two distinct markets. Our results are robust to including them in the analysis, however.

sardines. There is some ambiguity about whether the in-kind villages always received these three supplementary items, so, in some of our analyses, we separate the basic PAL goods from the supplementary ones. Another reason to examine the basic goods separately is that they isolate the simple income and supply effects of in-kind transfers; if the government succeeded in increasing households' taste for the supplementary goods, then the supplementary goods would have an additional effect of changing preferences (which goes in the direction of increasing demand and prices). The market for basic goods is also thicker, so the price effects might be easier to detect for the basic goods.

Both the in-kind and cash transfers were, in practice, delivered bimonthly, two monthly allotments at a time per household. A woman (the household head or spouse of the head) was designated the beneficiary within the household, if possible. The transfer size was the same for every eligible household regardless of family size. Resale of in-kind food transfers was not prohibited, nor were there purchase requirements attached to the cash transfers. The monthly box of food had a market value of about 206 pesos in the programme villages, and the cash transfer was 150 pesos per month, based on the government's wholesale cost of procuring the in-kind items.[20] The items included in the in-kind transfer are not produced locally.[21] Thus, the main welfare effects on the local supply side of the market will be felt by shopkeepers. There will also be welfare effects for local agricultural producers in cases where there is a high degree of substitutability (or complementarity) between the in-kind goods and the local products.

An inconvenient feature of the programme for our purposes is that the cash villages and a randomly selected half of the in-kind villages were assigned to receive health, hygiene, and nutrition classes, as well. This programme feature could create two potential problems for the interpretation of our results. First, the difference between the price effects of cash and in-kind transfers, which we interpret as due to the injection of supply, could be partly driven by differential exposure to the classes. Second, the impact of cash transfers on prices could be partly driven by the classes, rather than being a pure income effect.

These concerns appear to be small in practice. Regarding the first concern (in-kind versus cash), as documented in the Appendix, when we restrict the sample to in-kind villages assigned to receive classes—that is, if we analyse in-kind and cash villages that do not differ in their assignment to classes—the cash-versus-in-kind price effect is very similar to our main results that use all of the in-kind villages. This finding is not surprising given that classes were actually offered in almost all of the in-kind villages assigned not to receive them (Cunha, 2014).[22] Thus, in practice, the cash and in-kind treatment arms were essentially identical vis-à-vis classes, and it seems valid to interpret the in-kind versus cash comparison as due to the supply effect. For the second concern (cash versus control), there is no experimental variation to exploit, but when we compare class attendees to non-attendees in the cash arm, there is no evidence that the classes shifted food consumption, either overall or towards the PAL foods (as shown in the Appendix).

20. The government should have included its transportation costs when calculating the in-kind programme's costs. This oversight attenuates the in-kind-versus-cash price differential that is our main focus; a 206 peso cash transfer would have led to a larger price increase in cash villages, so a larger relative price decline in in-kind villages.

21. We do not observe actual food production, but rather draw this conclusion from household survey data on consumption of own-produced foods. The only PAL good that has auto-consumption in any appreciable quantity is beans (10% of households consume own-produced beans at baseline). There is also relatively little auto-consumption of non-PAL foods. Only 7 out of 60 foods in our analysis have more than 10% of the population producing the good, the largest of which is corn kernels, which 27% of households produce.

22. Based on the household survey data, 76% of respondents attended a class in the in-kind villages assigned to receive classes and 69% attended a class in the in-kind villages assigned to not receive classes. In both cases, average attendance was roughly four classes over the course of the programme. Furthermore, assignment to classes did not affect total food expenditure or the composition of food expenditure (results available from the authors).

This evidence makes us doubtful that the classes affected prices in the cash treatment arm, though attendance is endogenous so this evidence is only suggestive. Therefore, the caveat that the classes may have played some role in the price effect of cash transfers should be kept in mind when interpreting our cash versus control effect as a pure income effect. We abstract from this component of the programme for the remainder of our analysis.

3.2. *Assumption of identical income effects for cash and in-kind transfers*

In Section 2, we expressed the size of the in-kind transfer X_{InKind} in terms of its cash equivalent to recipients. If one compares a cash transfer programme and an in-kind transfer programme, and the cash equivalent of the in-kind transfer is exactly the same amount as the cash transfer, then the income effect for both transfer programs is the same. Coincidentally, this is quite close to being the case in our empirical setting. The market value of the in-kind transfer in the recipient villages averaged 206 pesos (based on pre-programme prices). The in-kind bundle would have had a cash-equivalent value of 206 pesos *if* the transfer was inframarginal to consumption or resale was costless, that is, if the in-kind nature of the transfers did not distort recipients' consumption choices. However, the transfers did alter consumption patterns, so the cash equivalent was less than the nominal value of 206 pesos. We estimate that recipients valued it at 146 pesos on average, or 71 cents on the dollar, as detailed in the next paragraph. The Mexican government made the (peculiar) decision to set the cash transfer in its randomized experiment equal to its wholesale cost of procuring the in-kind goods, which was about 27% lower than the cost at consumer prices in the recipient villages. The government also did not adjust for the fact that its estimated distribution cost was 30 pesos per in-kind box but 20 pesos per recipient for the cash transfer. The cash transfer was set at 150 pesos per month.

There are three conceptually distinct ways that recipients use goods provided to them in kind. First, they consume some amount of it that they would have consumed anyway; they value this inframarginal portion at market prices. By comparing the control group's consumption to transfer recipients' consumption, Cunha (2014) estimates that 116 pesos worth of the 206-peso bundle falls in this category. Second, recipients consume an additional amount of the transferred foods, more than they would have consumed absent the in-kind transfer. PAL recipients consumed an estimated 35 pesos more of food in the transferred categories as a result of the in-kind transfer. Third, recipients received an additional 55 pesos worth of goods that they did not consume and presumably resold instead.[23] For the latter two categories—the "extramarginal" portion—there is deadweight loss, and recipients will value the goods at less than their market value. For the extra goods they consume, they would not have been willing to purchase them at market prices, and for the goods they resell, they likely incur transaction costs. We assume, first, that consumers value the extramarginal consumption at a two-thirds discount relative to its market value, and second, that for goods that are resold, transaction costs erode two thirds of their value. Thus, the 90 pesos of extramarginal transfers are valued at only 30 pesos. Under these assumptions, the PAL in-kind transfer is worth 146 pesos to recipients (116 for the inframarginal portion + 30 for the extramarginal portion).

To recap, while it is impossible to pinpoint the precise value of the in-kind transfer to recipients—its nominal value minus the deadweight loss relative to an unconstrained transfer— the value of the PAL in-kind transfer was likely quite similar to the value of the cash transfer

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23. Households might also store the goods, but since the programme is expected to continue indefinitely, perpetual storage and an accumulating amount of stored goods seems unlikely. In any case, there would also be some deadweight loss from storage.

to which we compare it (146 pesos versus 150 pesos).[24] Moreover, even if consumers place *zero* value on the extramarginal portion of the in-kind transfer, valuing only the 116 pesos of inframarginal consumption, this difference in the income effect is much too small to explain the magnitude of the cash-versus-in-kind price effects that we estimate in Section 5, as we show in that section.

It is also worth noting that flypaper effects could be especially strong when transfers are made in-kind: By giving households particular goods, the government might signal the high quality of these goods (*e.g.* their nutritional value) and also make these items more salient to households. In other words, with an in-kind transfer relative to a cash transfer, not just the supply but also the demand for the transferred goods might increase. This extra effect of in-kind transfers would counteract the supply effect, and our estimated price effects would give a lower bound for the pure supply-shift effect of in-kind transfers.[25]

3.3. *Market structure*

As the data collected by the Mexican government for the PAL experiment did not include information on market structure, we conducted surveys of store owners in a subsample of 52 villages to qualitatively understand the market structure, stores' cost curves, and their price-setting behaviour. (See Appendix B for further details on the data collection.) Several facts are worth highlighting. First, there are few food stores per village. The median number of stores in 2015 was 4, and while respondents could not reliably recall the number of stores at the time the PAL experiment began in 2003, they reported that the number of stores was lower at that time. Second, there are fewer stores in less economically developed villages. Third, marginal cost curves appear to be upward-sloping over the short run (*e.g.* 1 month), but flat over a longer duration. Store owners report that they meet unexpectedly high demand by travelling to a neighbouring village or town to buy goods, which is costly, but for a permanent demand shock, they readjust the amount they procure from their distributors on a regular basis. Finally, store owners report that they adjust their prices quickly in response to increases or decreases in demand, usually within a week.

We interpret these facts as pointing to stores having market power and facing a flat marginal cost curve over the one- to two-year time horizon for which we test for price effects.

4. EMPIRICAL STRATEGY AND DATA

4.1. *Empirical strategy*

Our analysis treats each village as a local economy and examines food prices as the outcome, using variation across villages in whether a village was randomly assigned to in-kind transfers, cash transfers, or no transfers. We begin by focusing on the food items included in the in-kind programme. Our first prediction is that prices will be higher in cash villages relative to control villages since a positive income shock shifts the demand curve out (under the assumption that the items are normal goods). The second prediction is that relative to cash villages, prices will be lower in in-kind villages because of the supply influx.

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24. Another empirical fact that suggests that the income effects are the same for cash and in-kind villages is that we do not observe differential impacts on two categories of goods that are plausibly separable from the PAL food items, namely food expenditure away from home and non-food expenditure. This analysis is presented in Appendix Table A1.

25. A shift in preferences could also have been generated by the hygiene, health, and nutrition classes. However, as mentioned, we find no evidence of class attendance having an effect on overall food consumption or consumption of the PAL food items.

Our main data consists of prices collected in experimental villages both pre- and post-programme. We estimate the following regression where the outcome variable is p_{gsv}, the price for good g at store s in village v:

$$p_{gsv} = \alpha + \beta_1 \text{InKind}_v + \beta_2 \text{Cash}_v + \phi p_{gv,t-1} + \sigma I_{gv} + \varepsilon_{gsv}. \tag{5}$$

Our two predictions correspond to $\beta_2 > 0$ (cash transfers increase prices), and $\beta_1 < \beta_2$ (prices are lower under in-kind transfers than cash transfers). In our main specification, we control for the baseline price, denoted $p_{gv,t-1}$, which does not vary within a village (see below). (The subscript $t-1$ is shorthand for the variable being constructed from the baseline data; the estimation sample is cross-sectional, not a panel over time.) We also include the dummy variable I to indicate whether the pre-programme price is imputed (again, see below). We cluster standard errors at the village level, the level at which the treatment was randomized.

Note that a difference between the two predictions is that the first one—a positive price effect of cash transfers—applies to all normal goods, whereas the second one—a negative price effect of in-kind relative to cash transfers—applies to the goods provided in kind. We therefore have a more focused (and possibly higher-powered) way to test the second prediction, namely by examining the prices of PAL goods rather than all goods.

4.2. *Data*

The data for our analysis come from surveys of stores and households conducted in the experimental villages by trained enumerators from the Mexican National Institute of Public Health both before and after the programme was introduced. Baseline data were collected in the final quarter of 2003 and the first quarter of 2004, before villagers knew they would be receiving the programme. Follow-up data were collected two years later in the final quarter of 2005, one to two years after PAL transfers began in these villages. The Mexican government's purpose in running the experiment was to measure the programme's impacts on food consumption, and what type of data they collected was determined accordingly.

Our measure of post-programme prices comes from a survey of local food stores. From each store, enumerators collected prices for fixed quantities of sixty-six individual food items. They were instructed to first identify all the food stores in the village and then survey a maximum of three stores per village; unfortunately, no data were recorded from the step where they identified all of the stores. If more than three stores existed per village, they were instructed to randomly select three to survey, if possible one from each of three store types: general stores with posted prices, general stores without posted prices (*e.g.* small corner shops, butcher shop, or bakery), and the village market, taken as a unit. For 37% of villages in our sample, one store was surveyed; for 47% of villages, two stores were surveyed; and three stores were surveyed in the remaining 16% of villages.[26] Some of the stores surveyed were part of the Diconsa agency (21%) while the majority were independent stores (79%).

We also use measures of pre-programme food prices. Baseline data collection on store prices are missing for 40% of the sample because, first, data were collected for only forty of the 66 food items, and, second, even among the sampled goods, there are missing data for 19% of village-good observations (see Appendix B for details). Therefore, we also use the household survey to construct the pre-programme unit value (expenditure divided by quantity purchased) for each

26. Many of the shops had posted prices. If prices were not posted, the enumerators were instructed to choose the lowest price available for a given good to maintain consistency.

food item. In each village, a random sample of thirty-three households was interviewed about purchase quantities and expenditures on sixty food items. We use the median unit value among households in the village as a measure of the village's pre-programme price.[27] In cases where the pre-programme village median unit value is missing, we impute it using the median unit value in other villages within the same municipality (or within the same state in the few cases where there are no data for other villages in the municipality). Despite the missing data, we also use pre-programme store prices in some specifications to check the robustness of our results. The data do not allow us to match stores between waves; therefore, we use the median store price within a village and good as a measure of the pre-programme price. When the village median store price is missing, we impute the price using, first, the village median unit value, and then the geographic imputation of village median unit values (as above).

To facilitate comparisons across goods with different price levels, we normalize the price for each good by the sample mean for the good within the control group, by survey wave. (If one good is ten times the price of another good, we would not expect the programme to have the same effect in levels for these two goods, but we would expect it to have the same proportional effect, all else equal.) The mean price for each good is thus roughly 1, and exactly 1 for the control group. The empirical results are nearly identical if we normalize by the mean value across all the villages, but using the control villages seems preferable so that the normalization factor is not affected by the treatments. We also show the results using the logarithm of the price as the outcome.

We exclude some food items from the analysis due to missing data. Among the PAL goods, the store price survey mistakenly did not include biscuits; for the non-PAL items, chocolate powder, nixtamalized corn flour, salt, and non-fortified powdered milk were not included in the household survey and corn starch was not included in the store survey.[28] Finally, two pairs of goods were asked about jointly in the household survey (beef/pork and canned fish) but separately in the store survey (beef, pork, canned tuna, canned sardines). To address this discrepancy, we use the aggregated categories and take the median across all observed store prices for either good as our post-programme price measure. Our final data set comprises six basic PAL goods (corn flour, rice, beans, pasta, oil, fortified milk), three supplementary PAL goods (canned fish, packaged breakfast cereal, and lentils), and fifty-one non-PAL goods. Appendix Table A2 lists all of the goods in our analysis.

Table 1 presents descriptive statistics for the PAL goods. Column 2 shows the quantity per good of the monthly household transfer, and column 3 shows its monetary value measured using our pre-programme measure of prices. Column 4 presents each good's share of the total calories in the transfer bundle. As can be seen, the supplementary items were transferred in smaller amounts with lower value and fewer calories than the basic goods.

There is considerable variation across the PAL goods in the size of the aggregate village-level transfer. One measure of the size of this supply shift is listed in column 5. Here, the village change in supply, ΔSupply, is constructed as the average across in-kind villages of the total amount of

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27. Unit values are observed for households that purchased the good in the past seven days. We do not use unit values for post-programme prices because the programme changes the number and composition of households that purchase items. (Results available from the authors.) If the quality of a good does not vary and there is no price discrimination (*e.g.* bulk discounts), then unit values could still be used as a proxy for post-programme prices. However, if quality varies, then treatment effects estimated with post-programme unit values would reflect changes in both price and quality, and if there is price discrimination across households, then the treatment effects would also reflect changes in the composition of households purchasing a good. While quality is quite homogenous for manufactured items where there are few brands sold, it is heterogeneous for other goods (*e.g.* fresh food). See also McKelvey (2011) on the effect of income and price changes on the interpretation of unit values. Also note that for some goods, there are very few household-level observations of the baseline unit value (*e.g.* lentils, cereal, corn flour), while for others, most households purchased the good

TABLE 1
Summary of PAL food box

Item	Type	Amount per box (kg)	Value per box (pre-programme, in pesos)	Calories, as % of total box	Village change in supply (ΔSupply)
	(1)	(2)	(3)	(4)	(5)
Corn flour	Basic	3	15.7	20	1.00
Rice	Basic	2	12.7	12	0.61
Beans	Basic	2	21.0	13	0.29
Fortified powdered milk	Basic	1.92	76.2	17	8.62
Packaged pasta soup	Basic	1.2	16.2	8	0.93
Vegetable oil	Basic	1 (lt)	10.4	16	0.25
Biscuits	Basic	1	18.7	8	0.81
Lentils	Supplementary	1	10.3	2	3.73
Canned tuna/sardines	Supplementary	0.6	14.8	2	1.55
Breakfast cereal	Supplementary	0.2	9.3	1	0.90

Notes: (1) Value is calculated using the average of pretreatment village-level median unit values. 10 pesos $\approx$ 1 USD. (2) ΔSupply measures the PAL supply influx into villages, relative to what would have been consumed absent the programme. It is constructed as the average across all in-kind villages of the total amount of the good transferred to the village divided by the average consumption of the good in control villages in the post-period. (3) We do not know whether a household received canned tuna fish (0.35 kg) or canned sardines (0.8 kg); the analysis assumes the mean weight and calories throughout. (4) Biscuits are excluded from our analysis as post-programme prices are missing.

a good transferred to the village (*i.e.* average number of eligible households per village times allotment per household) divided by the average consumption of the good in control villages in the post-programme period. For example, there was almost exactly as much corn flour delivered to the villages each month as would have been consumed absent the programme (ΔSupply = 1.00 for corn flour), while the allotment of beans was 29% of what would have been consumed absent the programme (ΔSupply = 0.29 for beans).

Our final data set contains 360 stores in 194 villages and 12,940 good-village-store observations. The number of goods varies by store since many stores sell only a subset of goods. Table 2 presents summary statistics by treatment group. The baseline characteristics are for the most part balanced across groups. For three variables, there are significant differences across groups at the five percent level: The presence of a Diconsa store differs between control and in-kind, the share of food-producing households differs between control and cash and between in-kind and cash, and farm costs differ between control and in-kind and between control and cash. For our primary comparison—between the cash and in-kind treatments—no variable is unbalanced at baseline at the 5% level and only one variable is unbalanced at the 10% level.[29]

In some of our auxiliary analyses, we use household-level data to either construct village-level variables or to estimate household-level regressions. For example, we calculate the median household expenditures per capita in a village at baseline as a measure of the income level in the village. Also, when we test for heterogeneous welfare effects for households that produce agricultural goods, we use household-level outcomes such as farm profits and expenditures per capita. We present more detail on other relevant data as we introduce each analysis in the next section.

(*e.g.* beans, corn kernels, onions). The noisiness of our pre-period price measure will vary with the number of observed unit values.

28. The price of biscuits was intended to be collected, but a mistake in the survey questionnaire led enumerators to collect prices for crackers ("galletas saladas" in Spanish) rather than for biscuits ("galletas" in Spanish).

29. Appendix Table A3 presents additional summary statistics of demographic and consumption variables by treatment group, which further demonstrate balance.

TABLE 2
Baseline characteristics by treatment group

	Control	In-kind	Cash	(1)=(2)	(1)=(3)	(2)=(3)	(1)=(2)=(3)
	(1)	(2)	(3)	*p*-value	*p*-value	*p*-value	*p*-value
Prices, basic PAL goods							
Median village unit-value, normalized	1.00	0.98	0.98	0.28	0.31	0.95	0.48
	(0.014)	(0.012)	(0.015)				
Missing median village unit-value	0.13	0.14	0.13	0.94	0.80	0.72	0.94
	(0.018)	(0.013)	(0.020)				
Observations (good level)	486	1,092	582				
Prices, all PAL goods							
Median village unit-value, normalized	1.00	1.02	1.00	0.39	0.88	0.46	0.64
	(0.017)	(0.016)	(0.016)				
Missing village unit-value	0.18	0.17	0.16	0.72	0.48	0.64	0.77
	(0.016)	(0.013)	(0.021)				
Observations (good level)	729	1,638	873				
Prices, all goods							
Median village unit-value, normalized	1.00	1.02	1.00	0.23	0.98	0.18	0.30
	(0.015)	(0.010)	(0.013)				
Missing village unit-value	0.23	0.23	0.23	0.84	0.99	0.84	0.97
	(0.017)	(0.012)	(0.016)				
Observations (good level)	4,860	10,920	5,820				
Village level characteristics							
Missing median store price	0.13	0.10	0.16	0.69	0.66	0.36	0.65
	(0.048)	(0.034)	(0.046)				
Diconsa store in the village	0.26	0.45	0.39	0.03**	0.16	0.51	0.08*
	(0.71)	(0.049)	(0.068)				
Travel time to nearest market (hours)	0.77	0.69	0.74	0.55	0.86	0.69	0.82
	(0.108)	(0.076)	(0.104)				
Village population	682.8?	580.39	543.96	0.29	0.21	0.70	0.42
	(79.65)	(55.14)	(75.65)				
Number of stores	1.70	1.82	1.8	0.33	0.47	0.88	0.62
	(0.102)	(0.072)	(0.098)				
Median months for which	–	13.21	12.96	–	–	0.52	–
transfers were received		(0.224)	(0.305)				
Observations (village level)	47	96	51				

(continued)

Note that the data collection was designed to measure the PAL programme's impact on food consumption, not its price effects. It is fortunate that the price data from stores were collected, enabling our analysis of the programme's price effects. However, other data that ideally we would have are unavailable, for example, a census of grocery shops in each village. Thus, we do not have data on market structure to include in the empirical analysis. (Our survey of store owners in a subset of the villages, described in Section 3.3, provides a qualitative understanding of the typical market structure in the study villages.)

5. RESULTS

5.1. *Price effects of in-kind transfers and cash transfers*

Table 3, column 1, presents the main specification (equation (5)) using all nine PAL goods. The regression pools the effects for the different PAL food items. (See Appendix Table A4 for the results separately for each PAL good.) For cash villages, the point estimate suggests that the transfer programme caused prices to increase by 0.2% ($\widehat{\beta_2}$), though the coefficient is not statistically significant. In in-kind villages, prices fell by 3.9% relative to the cash villages

TABLE 2
Continued

	Control	In-kind	Cash	(1)=(2)	(1)=(3)	(2)=(3)	(1)=(2)=(3)
	(1)	(2)	(3)	*p*-value	*p*-value	*p*-value	*p*-value
Household level characteristics							
Monthly per capita expenditure (pesos)	570.54	535.10	529.54	0.31	0.26	0.85	0.50
	(29.02)	(18.90)	(21.77)				
Food-producing household	0.68	0.75	0.82	0.11	0.00***	0.05*	0.01***
	(0.04)	(0.02)	(0.03)				
Farm costs (pesos)	413.76	664.92	784.65	0.03**	0.00***	0.32	0.01***
	(82.46)	(76.91)	(93.22)				
Farm profits (pesos)	211.72	319.13	289.61	0.24	0.38	0.70	0.50
	(72.52)	(56.80)	(52.08)				
Asset index	2.24	2.18	2.27	0.78	0.87	0.59	0.86
	(0.16)	(0.10)	(0.13)				
Indigenous household	0.21	0.18	0.15	0.66	0.39	0.56	0.68
	(0.06)	(0.03)	(0.04)				
Household has a dirt floor	0.32	0.31	0.32	0.77	0.95	0.70	0.92
	(0.04)	(0.03)	(0.03)				
Household has piped water	0.65	0.57	0.50	0.23	0.06*	0.33	0.16
	(0.05)	(0.04)	(0.06)				
Observations (household level)	1291	2810	1473				

Notes: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. (1) Standard errors in parentheses. For normalized median village unit values and household level characteristics, standard errors are clustered at the village level. (2) Median village unit values are normalized with the good-specific control group mean and are imputed geographically if missing (see text). (3) Travel time to the nearest market is the time in hours needed to travel to a larger market that sells fruit, vegetables, and meat. It is constructed as the village median of household self-reports. (4) Expenditure is the value of non-durable items (food and non-food) consumed in the preceding month, measured in pesos; six households are missing expenditure data. (5) Food producing households are those that, at baseline, either auto-consume their production or report planting or reaping produce or grain or raising animals. (6) Farm costs and profits are for the preceding year. Samples are trimmed of outliers greater than 3 SDs above the median (about 1% of observations). (7) The asset index is the sum of binary indicators for whether the household owns the following goods: radio or TV, refrigerator, gas stove, washing machine, VCR, and car or motorcycle; two households are missing the asset index. (8) A household is defined as indigenous if one or more members speak an indigenous language.

TABLE 3
Price effects of in-kind and cash transfers

	All PAL goods	Basic PAL goods only	All PAL goods	Basic PAL goods only	All PAL goods	Basic PAL goods only
Outcome =	price	price	price	price	Δprice	Δprice
	(1)	(2)	(3)	(4)	(5)	(6)
In-kind	−0.037*	−0.033	−0.036*	−0.033	−0.062**	−0.025
	(0.020)	(0.020)	(0.020)	(0.020)	(0.029)	(0.024)
Cash	0.002	0.014	0.003	0.012	0.000	0.039
	(0.023)	(0.027)	(0.023)	(0.026)	(0.031)	(0.029)
Lagged normalized unit value	0.027	0.127***				
	(0.021)	(0.042)				
Observations	2,335	1,617	2,335	1,617	2,335	1,617
Effect size: **In-kind - Cash**	**−0.039****	**−0.047****	**−0.038****	**−0.045****	**−0.063****	**−0.064****
H_0: **In-kind = Cash (*p*-value)**	**0.02**	**0.04**	**0.03**	**0.04**	**0.02**	**0.02**

Notes: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. (1) The outcome variable in columns 1–4 is the post-programme price; it varies at the village-store-good level. It is normalized by good; the price is divided by the average price of the good across all observations in the control group. (2) Lagged normalized unit value in columns 1–2 is the village median unit-value, imputed geographically if missing (see text), normalized using the good-specific control group mean; it varies at the village-good level. (3) Columns 3-4 do not control for the lagged normalized unit value. (4) The outcome variable in columns 5–6 is the difference between the normalized post-programme price (the outcome in columns 1–4) and the lagged normalized unit value (the baseline price measure in columns 1–2). (5) Regressions in columns 1–2 and 5–6 include an indicator for imputed pre-programme prices (see text). (6) Standard errors (in parentheses) are clustered at the village level.

$(\widehat{\beta_1} - \widehat{\beta_2})$, with a *p*-value of 0.02; the bottom of the table reports the difference between the in-kind and cash coefficients and the statistical significance of this difference. As mentioned above, theory is ambiguous about whether the supply or demand effect is bigger in magnitude, but unless a good has a particularly high income elasticity of demand, we would expect the supply effect to dominate. Empirically we indeed find that the net effect of the in-kind transfer on prices is negative (3.7% decline, significant at the 10% level).

The in-kind-versus-cash difference is much too large to be due to just the income effect differing between the two types of transfer programs. As discussed in Section 2, recipients valued the in-kind bundle at roughly 146 pesos which is similar to the cash transfer amount of 150 pesos. The coefficient on *Cash* of 0.002 is the effect of a 150 peso income transfer, suggesting that the 4 peso difference would generate an in-kind-versus-cash difference in the income effect on the order of -0.00005. Even if recipients only valued the in-kind goods that were purely inframarginal to their consumption, which account for 116 pesos of the bundle, and they placed zero value on the rest of the food transfer, the resulting 34 peso difference in the value of the in-kind and cash transfer would only lead to a coefficient difference of -0.00045, which is smaller by a factor of 80 than the actual difference of -0.039. Thus, the fact that prices are lower under in-kind transfers compared to cash transfers appears to be driven by the supply influx into the village, not by differing income effects.

In column 2, we estimate the model excluding the supplementary PAL goods. The fact that canned fish, cereal, and lentils may not have been the supplementary goods in some experimental villages should not affect the cash or control villages but might attenuate our estimates of the in-kind-versus-cash effect. In addition, there is low consumption at baseline for the supplementary goods, and for very thin markets, prices are noisier. We find an in-kind-versus-cash coefficient difference that is somewhat larger in magnitude when we exclude the supplementary goods (magnitude of -0.047 with a *p*-value of 0.04).

The remaining columns of Table 3 test the same predictions while varying the specification. In cases such as ours where the outcome variable is autocorrelated but noisy, controlling for the baseline outcome is more efficient than either using only post-programme data or using a difference-in-differences estimator, but we also show the results using these two alternatives (McKenzie, 2012). Columns 3 and 4 do not control for baseline prices, and columns 5 and 6 present the difference-in-differences estimates.

5.2. *Robustness checks*

The results are also robust to using several other specifications, as shown in Appendix Table A5. First, we show that the results are nearly identical when we include good fixed effects. Second, rather than controlling for baseline unit values, we control for baseline store prices, imputing them for the 40% of cases where they are missing.[30] The results are again very similar to the main specification. Third, we show the results using the log of (unnormalized) prices rather than the normalized price level. While the predictions are in terms of price levels rather than the log of prices, this robustness check is helpful to ensure that the results are not driven by outliers. The in-kind versus cash effect is slightly larger in magnitude in this specification and, again, significant at the 5% level. Fourth, we show that regressions that weight each observation by the expenditure share for the good (as observed in the control group post-programme) produce almost identical results. Fifth, we show that the results are similar when we drop half of the in-kind villages and

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30. In these specifications we include two dummy variables, one indicating the village median store price was missing and one indicating the village median unit value was missing (conditional on a missing village median store price).

TABLE 4
Robustness check testing for changes in product quality

Measure of quality variation =	Subjective categorization		Good-specific coefficient of variation of baseline price		Village-good-specific coeff. of variation of baseline price	
	All PAL goods	Basic PAL goods only	All PAL goods	Basic PAL goods only	All PAL goods	Basic PAL goods only
Outcome =	price	price	price	price	price	price
	(1)	(2)	(3)	(4)	(5)	(6)
High-quality variation × In-kind	−0.026	−0.034	−0.001	0.032	0.007	0.021
	(0.025)	(0.027)	(0.029)	(0.033)	(0.024)	(0.037)
High-quality variation × Cash	−0.018	−0.029	−0.006	0.039	−0.004	0.027
	(0.033)	(0.041)	(0.040)	(0.046)	(0.036)	(0.047)
In-kind	−0.022	−0.014	−0.036*	−0.044**	−0.040**	−0.038**
	(0.021)	(0.029)	(0.021)	(0.019)	(0.018)	(0.018)
Cash	0.012	0.030	0.006	0.001	0.004	0.007
	(0.025)	(0.034)	(0.028)	(0.031)	(0.027)	(0.027)
High-quality variation	−0.007	−0.002	−0.012	−0.031	−0.006	−0.002
	(0.021)	(0.023)	(0.026)	(0.029)	(0.019)	(0.031)
Observations	2,335	1,617	2,335	1,617	2,335	1,617
Effect size: **In-kind - Cash**	−0.034*	−0.044*	−0.041*	−0.044	−0.044*	−0.045*
H_0: **In-kind = Cash** (*p-value*)	0.08	0.09	0.08	0.13	0.06	0.06
Effect size: **High-quality var. × In-kind - High-quality var. × Cash**	−0.008	−0.005	0.005	−0.007	0.011	−0.006
H_0: **High-quality var. × In-kind = High-quality var. × Cash** (*p-value*)	0.78	0.9	0.88	0.86	0.73	0.89

Notes: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. (1) The outcome variable is the post-programme price; it varies at the village-store-good level. It is normalized by good; the price is divided by the average price of the good across all observations in the control group. Standard errors (in parentheses) are clustered at the village level. (2) Regressions control for the pre-period normalized unit value and an indicator for imputed pre-programme prices (see text). (3) High-quality variation is defined in three ways. First, we subjectively identified goods that had high-quality variation; these goods are beans, cereal, corn flour, lentils, and pasta soup (columns 1–2). Second, we use the coefficient of variation (C.V.) of pre-period unit values; a high C.V. is one that is above the median. We construct the within-village-good C.V. We average across villages to create a good-specific measure of quality variability (columns 3–4) and also use the village-good-specific measure (columns 5–6). When the village-good C.V. is missing, it is imputed with the good-specific C.V.

focus on the cash and in-kind villages assigned to receive health and nutrition classes. Finally, we show that the results are robust to restricting the sample to privately-owned stores.[31] In addition, the results are remarkably similar if we aggregate the data to the village-good or village level, estimating the model with one observation per village-good or per village (results available from the authors).

We also investigate the potential concern that the effects we estimate reflect changes in quality within a product category—stores might have started stocking higher quality vegetable oil, for example—rather than changes in prices. Note, however, that if households upgrade quality when their income increases, this effect should apply to recipients of both cash and in-kind transfers. Nonetheless, in Table 4, we explore this concern by using proxies for the amount of quality

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31. Some of the stores in our sample are the public/private Diconsa stores, which are allowed to adjust prices based on market conditions, but with some restrictions. Thus, the price effects could be stronger for the fully private non-Diconsa stores than for the Diconsa stores. In the final columns of Appendix Table A5 we estimate equation (5) for the subsample of non-Diconsa stores and find that the positive effect of cash transfers is somewhat larger in this subsample compared to the main specification while the in-kind-versus-cash effect is similar in magnitude to the full sample. When we use the full sample and estimate the interacted model, we cannot reject that the Diconsa stores have the same price responses to the transfer programs as non-Diconsa stores.

variation there is for a good. First, we subjectively categorize the goods as having a high or low degree of product variation (each of the three authors independently categorized the goods, and we use the median of our answers). We categorized cereal, beans, corn flour, lentils, and pasta soup as having high quality variation, and vegetable oil, rice, canned fish, and powdered milk as having low variation. We run an interacted model, testing whether the price effects are driven by goods with more scope for quality upgrading (or downgrading). If quality were the explanation, the effects would be driven by the high-quality-variation goods. As seen in columns 1 and 2, the effects do not seem to vary with the likelihood of quality changes. The coefficient on the interaction of cash villages and quality variation is wrong-signed and insignificant, and the difference in the interaction terms for in-kind and cash villages is close to zero. Meanwhile, even among goods with little quality variation (the main effects), we find significantly lower prices in in-kind villages than in cash villages.

As a second proxy for quality variation, we use data from the household survey on the unit value that different households report paying for the same good and construct the coefficient of variation of unit values for each village-good. The variation in unit values is likely due mostly to measurement error, not quality variation, so this is an imperfect measure, but it has the advantage of being more objective than our subjective categorization. We average the coefficient of variation across villages to create a good-specific measure of quality variation (columns 3 and 4) and also use the village-good-specific measure (columns 5 and 6). We again find that, first, the results are not driven by the goods with more quality variation, and, second, even for the goods with low quality variation, prices are lower in in-kind villages than in cash villages. In short, the price effects we estimate do not appear to be a result of quality upgrading.

To summarize, we find that the influx of supply from in-kind transfers causes prices to fall relative to prices under cash transfers. The result is robust to several alternative specifications and does not appear to be driven by changes in product quality. The point estimates suggest that this price gap between transfer modalities results from in-kind transfers having a net negative effect on prices and cash transfers having a very small positive effect on prices, though these two individual effects relative to the control group are less precisely estimated than the cash-versus-in-kind gap.

5.3. *Persistence of price effects*

In Table 5 we present evidence on whether the price effects dissipate over time, using the variation across villages in when the programme was launched. We calculate the duration of the treatment, which is the difference between the date of the follow-up survey and the start date of benefit receipts. This duration ranges from 8 to 22 months. Note that programme duration is undefined for the control group, so this analysis compares in-kind to cash villages only.

We interact programme duration with the in-kind treatment dummy in Table 5. For ease of interpretation, we use a dummy for above median duration (the average duration is 16 months in above-median villages and 12 in below-median villages), but the conclusion is similar if we use the duration in months: The coefficient on the interaction is insignificant and in fact negative, suggesting that the effects become if anything larger over time. In any case, we find no evidence that the effects fade away. The programme start date is not randomly assigned, so one concern is the endogeneity of the programme duration at follow-up. The one observable characteristic that we find is significantly correlated with programme duration is the level of development of the village (we define our measure of development in the next section). Thus, we reproduce the test above controlling for the level of development and its interaction with the in-kind indicator; as shown in columns 2 and 4, the results are similar.

Many supply-side adjustments such as store owners altering their procurement would likely be complete by the one to two year mark. Thus, these results appear to be inconsistent with the

TABLE 5
Price effects based on duration of intervention

Outcome =	All PAL goods		Basic PAL goods only	
	price	price	price	price
	(1)	(2)	(3)	(4)
In-kind	−0.031	−0.029	−0.038	−0.035
	(0.022)	(0.022)	(0.031)	(0.030)
In-kind × Above median length of treatment	−0.021	−0.021	−0.022	−0.029
	(0.034)	(0.034)	(0.040)	(0.038)
Above median length of treatment	0.004	0.000	0.018	0.015
	(0.028)	(0.027)	(0.033)	(0.029)
In-kind × Development index		0.010		−0.005
		(0.020)		(0.026)
Development index		−0.010		−0.007
		(0.016)		(0.023)
Observations	1,818	1,798	1,258	1,245

Notes: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. (1) The outcome variable is the post-programme price; it varies at the village-store-good level. It is normalized by good; the price is divided by the average price of the good across all observations in the control group. Standard errors (in parentheses) are clustered at the village level. (2) Regressions control for the pre-period normalized unit value and an indicator for imputed pre-programme prices (see text). (3) Length of treatment is defined as the village median number of months for which transfers were received prior to the follow-up survey. (4) The development index is the first principal component from a factor analysis of the following four variables: (1) the time required to travel to a larger market that sells fruit, vegetables, and meat; (2) the distance to the head of the municipality; (3) pre-programme village median monthly expenditure on non-durables, and (4) the village population.

village markets being perfectly competitive, as we would expect the marginal cost curve to be flat over this time span, and with a flat marginal cost curve and perfect competition, there would be no price effects of shifts in demand. Even with imperfect competition, one might expect the effects to fade over time as firms respond by entering or exiting the market, or local agricultural producers change their production levels. These adjustments would likely be underway after two years, so this finding of persistence suggests that such adjustments might not fully undo the price effects of transfer programs, at least in the medium run. Thus, while we cannot look at effects further out than two years, the price effects appear to persist beyond the short run.

5.4. *Heterogeneity by the village's level of development and market structure*

We next test for heterogeneity in the price effects based on the village's level of development. We hypothesize that less developed villages experience larger price effects because they are less integrated with the outside economy and have less competition among local suppliers. Moreover, understanding how the price effects vary with how impoverished the village is of policy interest per se.

We combine several village characteristics to construct a measure of its "development". Specifically, we use the average expenditures per capita, population, average self-reported travel time to a larger market that sells fruit, vegetables, and meat, and distance to the nearest municipality head (calculated using GIS software). We construct the first principal component of these variables. (See Appendix B for details on the construction of this variable.)[32] Essentially, an underdeveloped village is poorer, smaller, and more physically remote. For convenience, we will refer to villages with a development index below the sample median as less developed or underdeveloped.

32. Appendix Table A6 presents balance tests that show that the sample is balanced across treatment groups within both the more- and less-developed subsamples of villages.

Table 6 reports the results on how the price effects vary with development. Column 1 reports the results for less developed villages. In-kind transfers cause a 3.6% price decline, and cash transfers cause a 1.5% increase. The difference is statistically significant at the 5% level. Meanwhile, in more developed villages (*i.e.* above-median development index), in-kind transfers cause a 3.3% decline in prices, while cash transfers cause a 0.7% price decline, with the difference of −0.027 in the predicted direction but insignificant (column 2).[33,34] These findings reveal that the average effects for the cash-versus-in-kind effect (Table 3) are mostly driven by less developed villages.[35] Column 3 reports the interacted model which shows that the interaction is statistically insignificant.

Next, we test for heterogeneity by supply-side factors that theoretically should lead to larger price effects. Specifically, we examine how the price effects vary with local stores' market power and with how integrated local prices are with national prices.

To measure market structure, we would ideally use data on the number of grocery shops and their market share, but a store census was not included in the data collection.[36] Instead, we use an approach derived from Attanasio and Pastorino (2015) that relates the existence of price discounts for larger-quantity purchases to the degree of imperfect competition in the market. (Their study context is also rural Mexico.) In particular, we rely upon their observation that when there is market power among sellers, price discrimination should lead to a negative within-village correlation between prices (unit values) and quantities purchased. We apply this method to measure market power, and then test if the price effects of PAL are larger in villages characterized by market power in the goods market. We use the pre-intervention data for the set of food items examined by Attanasio and Pastorino—rice, beans, sugar, tomatoes, and corn tortillas— and estimate separately for each village the correlation coefficient between the household unit value and the quantity purchased. We categorize a village as having market power if the correlation coefficient is negative.[37]

Table 6, columns 4 and 5, test the hypothesis that the price effects should be larger in villages with more supply-side market power. Indeed, we find that price effects exist only in villages where local stores have market power. In these villages, there is a 5.5% decline in prices under in-kind transfers relative to cash transfers.

We next test for heterogeneity by how tied local prices are to Mexico City prices. If a village market is fully integrated with the national economy, its prices should co-move with national prices. If, instead, a village economy is more closed, local supply and demand determine prices, so there will be less co-movement with outside prices. For each village, we construct a measure

33. We defined the development index based on our ex ante hypotheses about what factors correlate with economic development. When we examine heterogeneous price effects by the individual components of the index, the estimates for the two distance measures and per capita income are in the predicted direction, but the estimate for village size is in the counterintuitive direction.

34. There are more observations in the above-median subsample because there are slightly more stores in those villages, and village-store-good is the level of observation.

35. A larger income elasticity of demand in less developed villages is not a likely explanation for these patterns because such a difference should net out when comparing in-kind to cash villages.

36. A previous version of the paper used the number of surveyed stores as a proxy for the number of stores in the village. We find that villages with fewer stores have larger price effects, but the results are insignificant. In addition in unreported results, we test whether the cash or in-kind programme affected the number of stores in the village, using the store count at endline as the outcome. We find no evidence of an overall effect or heterogeneity by the level of development.

37. The Attanasio and Pastorino (2015) model assumes a representative seller, an assumption that might not be appropriate in villages with more than one store. For robustness, we also estimated the measure of market power controlling for the within-village standard deviation of prices as a proxy for deviations from the representative seller assumption, and our results do not change.

TABLE 6

Heterogeneous price effects by level of village development, market integration, and supply-side competition

	Below-median development	Above-median development	All villages	Villages with market power	Villages without market power	Below-median price correlation	Above-median price correlation	All villages	All villages
Outcome =	price	price	price	price	price	price	price	price	price
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
In-kind	−0.041 (0.030)	−0.031 (0.027)	−0.031 (0.027)	−0.048* (0.025)	−0.005 (0.021)	−0.060** (0.028)	−0.019 (0.028)	0.009 (0.025)	0.018 (0.036)
Cash	0.012 (0.031)	−0.006 (0.037)	−0.006 (0.037)	0.007 (0.029)	−0.005 (0.026)	0.002 (0.032)	−0.014 (0.032)	−0.036 (0.034)	−0.038 (0.041)
Development index below-median × In-kind			−0.010 (0.041)						−0.013 (0.040)
Development index below-median × Cash			0.018 (0.048)						0.006 (0.048)
Market power village × In-kind								−0.039 (0.036)	−0.037 (0.037)
Market power village × Cash								0.029 (0.041)	0.027 (0.044)
Price correlation below-median × In-kind								−0.039 (0.041)	−0.045 (0.042)
Price correlation below-median × Cash								0.022 (0.047)	0.022 (0.048)
Observations	1,194	1,110	2,304	1,733	602	1,115	1,220	2,335	2,304
Effect size: **In-kind - Cash**	**−0.053****	**−0.025**	**−0.025**	**−0.055*****	**0.000**	**−0.063*****	**−0.006**		
H_0: **In-kind = Cash (*p*-value)**	**0.01**	**0.38**	**0.38**	**0.01**	**1.00**	**0.01**	**0.81**		
Effect size: **Development index below-median × In-kind - Development index below-median × Cash**			**−0.028**						**−0.019**
H_0: **Development index below-median × In-kind = Development index below-median × Cash (*p*-value)**			**0.43**						**0.60**
Effect size: **Price correlation below-median × In-kind - Price correlation below-median × Cash**								**−0.061***	**−0.067***
H_0: **Price correlation below-median × In-kind = Price correlation below-median × Cash (*p*-value)**								**0.07**	**0.05**
Effect size: **Market power village × In-kind - Market power village × Cash**								**−0.067***	**−0.064***
H_0: **Market power village × In-kind = Market power village × Cash (*p*-value)**								**0.05**	**0.09**

Notes: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. (1) The outcome variable is the post-programme price; it varies at the village-store-good level. It is normalized by good; the price is divided by the average price of the good across all observations in the control group. Standard errors (in parentheses) are clustered at the village level. (2) Regressions include all PAL goods and control for the main effects of the interaction terms reported, and for the pre-period normalized unit value and an indicator for imputed pre-programme prices (see text). (3) Price correlation is the correlation coefficient of the pre- to post-programme change in village prices with the pre- to post-programme change in prices in Mexico City for all PAL goods, it varies at the village level. (4) The number of stores is the number of stores included in the baseline price survey; a maximum of three stores were surveyed per village. (5) The development index is the first principal component of a factor analysis of the following four variables: the time required to travel to a larger market that sells fruit, vegetables, and meat; the distance to the head of the municipality; pre-programme village median monthly expenditure on non-durables; and the village population.

of how correlated its baseline prices are with Mexico City prices for the same set of goods in the same year. The Mexico City price data were obtained from the Mexican central bank (Bank of Mexico) and are the data used to construct the Mexican consumer price index (see Appendix B for further details on this price correlation measure). Columns 6 and 7 and show that the effects are entirely driven by more isolated markets, that is, ones where prices are not strongly correlated with Mexico City prices.

Column 8 runs a horserace between these two mechanisms, as one might expect that the same villages that have low integration with Mexico City also have low within-village competition. The specification includes interactions of the treatment indicators with both the indicator for having high market power and the indicator for having a low price correlation. We find that both factors matter and, in fact, there is sufficient statistical power to detect that each mechanism has the predicted effect. The in-kind versus cash gap is 6.7% larger in villages with high market power compared to those with low market power. The gap is 6.1% larger in villages with a low price correlation (more closed) relative to villages with a high price correlation (more open). Finally, column 9 shows that these channels "knock out" much of the heterogeneity by village development. In other words, the fact that underdeveloped villages have more closed economies and less within-village competition helps explain why they experience larger price effects.

5.5. *Total pecuniary effects of the PAL programme*

We next examine the price effects for goods not transferred in the PAL bundle. There are two reasons to do so. First, for the cash transfers, there is nothing unique about the PAL goods, and the hypothesized price effects apply equally to the non-transferred goods. Second, to assess the overall price effects in the village, even of the in-kind transfer, it is important to consider effects on all of the goods. By and large, other food items are substitutes for the PAL bundle, so non-PAL food prices are predicted to fall in in-kind villages relative to cash villages.[38]

For the non-PAL goods, we do not find that food prices fall in in-kind villages relative to cash villages (Table 7, column 1). In the less-developed villages (column 2) but not the more-developed villages (column 3), the point estimates match the prediction that prices should fall in in-kind villages relative to cash villages, but the effect is statistically insignificant, and we cannot reject that the patterns are the same in less- and more-developed villages (column 4).

The estimated price effects for the PAL goods reported in Table 3 combined with the results for non-PAL goods in Table 7 allow us to quantify the indirect transfer that occurs through the pecuniary effects. We convert the price changes into the corresponding indirect transfer, measured in pesos, for a consumer household. For example, a price decrease is a positive transfer, the magnitude of which depends on the decline in prices and on the amount households spend on the goods. We then compare the magnitude of the indirect pecuniary transfers to the direct transfer provided by PAL. The imprecise price effects for non-PAL goods imply that the estimated pecuniary effects might be noisily estimated, so we also calculate the bootstrapped confidence interval.

We begin with the PAL goods. In-kind transfer recipients receive a portion of their demand for PAL food items for free. The price effects only impact the residual portion that they purchase. We estimate the out-of-pocket purchases by subtracting the in-kind transfer quantities, good-by-good, from the quantities consumed in control villages at follow-up. (See footnote 16 for more details.) The value of net-of-transfer purchases is 104.2 pesos. Thus, the 3.7% price decrease in in-kind

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38. The price of non-food items, which should not be close substitutes with the PAL bundle, should respond less; unfortunately, the prices of non-food items are not available to test this prediction.

TABLE 7
Price effects for non-PAL goods

	All non-PAL goods			
	All villages	Below-median development	Above-median development	All villages
Outcome =	price	price	price	price
	(1)	(2)	(3)	(4)
In-kind	0.010	−0.006	0.024	0.024
	(0.019)	**(0.029)** ~~(0.030)~~	(0.023)	(0.023)
Cash	0.009	0.011	−0.001	−0.001
	(0.022)	(0.032)	(0.026)	(0.026)
Development index below-median × In-kind				−0.030
				(0.037)
Development index below-median × Cash				0.012
				(0.041)
Observations	10,648	**4,751** ~~4,699~~	**5,780** ~~5,832~~	10,531
Effect size: **In-kind - Cash**	**0.001**	**−0.017**	**0.025**	**0.025**
H_0: **In-kind = Cash (*p*-value)**	**0.95**	**0.62** ~~0.63~~	**0.32** ~~0.31~~	**0.33**
Effect size: **Development index below-median × In-kind - Development index below-median × Cash**				**−0.042**
H_0: **Development index below-median × In-kind = Development index below-median × Cash (*p*-value)**			**0.32** ~~0.33~~	

Notes: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. (1) The outcome variable is the post-programme price; it varies at the village-store-good level. It is normalized by good; the price is divided by the average price of the good across all observations in the control group. Standard errors (in parentheses) are clustered at the village level. (2) Regressions include all fifty-one non-PAL goods and control for the main effects of the interaction terms reported, as well as for the pre-period normalized unit value and an indicator for imputed pre-programme prices (see text). (3) The development index is the first principal component of a factor analysis of the following four variables: (1) the time required to travel to a larger market that sells fruit, vegetables, and meat, (2) the distance to the head of the municipality, (3) pre-programme village median monthly expenditure on non-durables, and (4) the village population.

villages (Table 3, column 1) represents a transfer of ~~3.85~~ **3.82** pesos for every recipient household (about 90% of the households in the villages) that is a pure consumer of these items. Note that we exclude the increase in demand induced by the transfer's income effect when calculating the quantity to which to apply the price change.

The price changes affect all households, not just programme recipients. Non-recipient households (about 10% of the village) spent 206 pesos a month on the food items contained in the PAL bundle, which represents a transfer of 7.6 pesos (206*0.037) per household. For the cash transfers, our point estimate suggests that the price effect is equivalent to a −0.41 peso transfer (206*−0.002) for each recipient or non-recipient consumer household.

The total pecuniary effect of the programme also includes the effects on non-PAL food items. Expenditure on the non-PAL items was 1,096 pesos per month in the control villages. The 1 percent price increase for in-kind transfers (Table 7, column 1) is thus equivalent to a −10.96 peso transfer to a consumer (programme recipients and non-recipients alike), and the 0.9% increase in prices in cash villages is equivalent to about a −9.86 peso transfer.

Combining these numbers, we find that for the overall sample, the pecuniary effects of cash versus in-kind transfers have small impacts for households, equivalent to a ~~−3.43~~ **−3.11** peso transfer. Thus, our first conclusion from this calculation is that, averaged over all villages, the price effects of the PAL programme do not have important implications for households' purchasing power.

The story is fairly different for the subsample of less developed villages. Here, the pecuniary effects are economically important. Doing the same calculation as above but for the less developed subsample, we find that the total pecuniary effects of in-kind transfers relative to cash transfers are equivalent to adding an extra ~~28~~ **26** pesos in indirect transfers for a consumer household, which

represents **13%** ~~14%~~ of the direct transfer amount. Thus, via the channel of price effects, in-kind transfers deliver considerably more to consumer households than cash transfers do, with the converse being true for producer households. In less developed villages, price effects appear to be an important consideration in the cash-versus-in-kind policy decision. A caveat is that these estimates are noisy; the *p*-value for the **26** ~~28~~ peso estimate is 0.51.[39] Also, there are many other considerations such as administrative costs and paternalistic objectives that factor into the policymaker's choice of transfer modality.

5.6. *Effects on food-producing households*

Our last analysis examines effects on households engaged in agricultural production. The packaged goods in the in-kind bundle are not produced in the programme villages, but agricultural households produce items that are substitutable with the in-kind goods. Even for agricultural households who are net consumers of food, in their capacity as food producers the welfare implications of price changes are the opposite of those for their consumption: A price increase (decrease) for food raises (lowers) the value of their production.[40]

Unfortunately, the availability and quality of the data on agricultural production is not ideal. First, there are no data on agricultural yield. Second, the profit variable never takes on negative values, and for the majority of households who state that they engage in food production, profits are identically zero, pointing to considerable measurement error. Finally, there is some baseline imbalance across treatment arms in the proportion of households that engage in agricultural production. For all these reasons, we regard the results below as tentative, providing suggestive evidence on the distributional effects for producing and consuming households.

We begin by examining how farm profits in the past year are affected by the transfer programme, estimating the following equation using the household-level data:

$$\text{FarmProfits}_{hv} = \alpha + \beta_1 \text{InKind}_v + \beta_2 \text{Cash}_v + \phi \text{FarmProfits}_{hv,t-1} + \varepsilon_{hv}. \tag{6}$$

The subscript h indexes the household and v indexes the village. We cluster the standard errors by village and, analogous to our earlier analyses, control for the pre-period outcome variable. Note that price effects are not the only reason that transfers might affect farm production. If farmers are liquidity constrained, then the income effect of the programme might lead to more investment and increased production. This channel would cause an increase in profits (unless the investments pay off only in the long run) for both the cash and in-kind treatments. However, there is no obvious reason that having more liquidity would cause differential effects for cash versus in-kind villages.

As shown in column 1 of Table 8, we find, as predicted, a positive coefficient on *Cash*. Farm profits are higher by 186 pesos in villages where households received cash transfers. We find that the in-kind programme also increases farm profits but not as much; profits are lower in in-kind villages relative to cash villages by 42 pesos, though not statistically significantly. These patterns are consistent with both types of transfer programs increasing farm productivity by making households less credit constrained, but cash transfers leading to relatively higher profits than in-kind transfers because of price effects.

Higher food prices will raise agricultural profits holding quantity fixed, but higher prices might also incentivize farmers to expand production. We do not have data on the quantity produced by a household, but we do have, as a proxy, data on total production costs (column 2). The fact that production costs increase in cash villages compared to in-kind villages is consistent with the

39. The *p*-value is calculated by bootstrapping the estimation process. We redraw villages, with replacement, 5,000 times and recalculate the various inputs into this calculation such as the baseline expenditures and regression coefficients.

40. Ideally, we would also examine effects on grocery shop owners, but the occupation variable in the survey is not specific enough to identify store owners.

TABLE 8
Effects for producer versus consumer households

Outcome =	Farm profits	Farm costs	ln(Expenditure per capita)	ln(Expenditure per capita)	Asset index	Asset index
	(1)	(2)	(3)	(4)	(5)	(6)
In-kind	143.87	134.01	0.115**		0.084	
	(89.839)	(119.511)	(0.046)		(0.075)	
Cash	186.16*	345.32**	0.087		−0.040	
	(106.082)	(140.378)	(0.068)		(0.106)	
Producer × In-Kind			0.001	−0.018	0.077	0.055
			(0.060)	(0.046)	(0.115)	(0.088)
Producer × Cash			0.087	0.015	0.266*	0.229**
			(0.068)	(0.051)	(0.142)	(0.109)
Producer			−0.161***	−0.003	−0.308***	−0.007
			(0.050)	(0.036)	(0.092)	(0.071)
Control for pre-period outcome?	Yes	Yes	Yes	Yes	Yes	Yes
Village FE				Yes		Yes
Observations	4,924	5,038	5,534	5,534	5,571	5,571
Effect size: **In-kind - Cash**	−42.29	−211.31*	−0.086		−0.189	
H_0: **In-kind = Cash (*p*-value)**	0.67	0.08	0.25		0.20	
Effect size: **Producer × In-Kind - Producer × Cash**			−0.086	−0.033	−0.189	−0.174*
H_0: **Producer × In-Kind = Producer × Cash (*p*-value)**			0.13	0.47	0.13	0.07

Notes: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. (1) Observations are at the household level. Standard errors (in parentheses) are clustered at the village level. (2) Profits and costs are measured in pesos and they are for the preceding year; samples are trimmed of outliers greater than three standard deviations above the median (about 1% of observations). (3) Producer is an indicator for households that, at baseline, either auto-consume their production or report planting or reaping produce or grain or raising animals. (4) Expenditure is the value of non-durable items (food and non-food) consumed in the preceding month, measured in pesos. (5) The asset index is the sum of binary indicators for whether the household owns the following goods: radio or TV, refrigerator, gas stove, washing machine, VCR, and car or motorcycle.

effect on profits being partly due to farmers expanding or contracting the quantity they produce in response to the price changes. In other words, in cash villages, a farmer receives higher revenues both because she earns more per unit sold and because she sells more units. The effects are more statistically significant for total costs than for profits, which could reflect the cost data being better measured.

The results in columns 1 and 2 suggest that the PAL transfer programme, through its pecuniary effects, may have had different welfare implications for food-producing households. Households are classified as food producers if, at baseline, they either report planting or reaping produce or grain or raising animals, or consume food from their own production; 75% of households meet this criterion. We first examine heterogeneity in the programme impacts on total expenditures per capita, which serves as a proxy for household welfare and is meant to capture the total programme effect for the household:

$$\text{Expend}PC_{hv} = \alpha + \theta_1 \text{Producer}_h \times \text{InKind}_v + \theta_2 \text{Producer}_{hv} \times \text{Cash}_v$$

$$+ \beta_1 \text{InKind}_v + \beta_2 \text{Cash}_v + \rho \text{Producer}_{hv} + \phi \text{Expend}PC_{hv,t-1} + \varepsilon_{hv}. \quad (7)$$

The predictions are $\theta_1 < \theta_2$ and $\theta_2 > 0$; in-kind transfers compared to cash transfers are relatively less beneficial to producer households, and cash transfers are relatively more beneficial to producer households. The point estimates in column 3 line up with the predictions that cash transfers are more valuable to producer households than to non-producer households (by 8.7 percentage points), and in-kind transfers are relatively less valuable to producer households than to non-producer households (by 8.6 percentage points), but the estimates are imprecise. Any conclusions drawn from this analysis are therefore tentative.

Note the large main effect of *Producer*. The regression controls for the baseline outcome, so this result suggests that producer households have slower expenditure growth than non-producers. To probe this somewhat puzzling coefficient, in column 4 we include village fixed effects and find that the main effect of *Producer* vanishes. It appears that there was slower growth in more agricultural villages rather than producers and non-producers in the same places having divergent growth. With village fixed effects included, we find again that the difference between the producer-in-kind and the producer–cash interactions is negative as predicted but insignificant.

In columns 5 and 6 we examine a second measure of welfare, an asset index that measures how many of the following items the household owns: radio or TV, refrigerator, gas stove, washing machine, VCR, car, or motorcycle. The point estimates suggest that cash transfers are differentially beneficial for producers ($p = 0.06$) and that cash transfers, relative to in-kind transfers, are more helpful for producers ($p = 0.13$). To address the large main effect for producers, we include village fixed effects in column 6. We find that producers are relatively better off with cash transfers, and this finding increases in statistical significance ($p = 0.07$). To summarize, due to their different price effects, cash and in-kind transfers should differ in their welfare implications for producer households versus consumer households. Our estimates investigating this heterogeneity are imprecise but consistent with cash transfers being relatively more beneficial to food producers and in-kind transfers being relatively more beneficial to consumers.

6. CONCLUSION

Government transfer programs often inject a large quantity of goods or services or cash into a community. Through these shifts in supply and demand, transfer programs could have quantitatively important price effects. This article tests for price effects of in-kind transfers versus cash transfers using the randomized design and panel data collected for the evaluation of a large food assistance programme for the poor in Mexico, the Programa de Apoyo Alimentario.

We test two main predictions, first, that cash transfers should lead to price inflation and, second, that prices should fall under in-kind transfers relative to cash transfers. We do not find strong evidence for the first hypothesis, though the point estimates generally match the prediction. We find robust evidence in support of the second hypothesis: Prices are significantly lower with in-kind transfers than cash transfers. For the sample as a whole, the price effects are quite small. Since programme eligibility is high and the transfers are large—that is, the programme injects a large quantity of food or cash into these villages—this finding suggests that in many settings, price effects will have quite negligible consequences for policy decisions.

In less developed villages—poorer and more remote—our results tell a different story: Here, the price effects we estimate are economically significant. In villages with below-median development, the difference in the price effects between in-kind and cash transfers is equivalent to an indirect transfer of ~~28~~ 26 pesos per month for a consumer household, or about ~~14~~ 13% of the direct transfer. While the less developed half of villages in our sample are particularly underdeveloped by Mexico's standards, in many other low-income countries, much of the population lives at this (or a lower) level of development, and our findings suggest that pecuniary effects may be an important component of the total welfare impact of large transfer programs.[41]

Our finding that the price effects are particularly pronounced for poor and geographically isolated villages is consistent with, first, their good markets being relatively closed, and, second, their local suppliers being imperfectly competitive. We find evidence that both of these market characteristics underlie the large price effects in less developed villages. Several additional facts

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41. This finding is also potentially relevant in developed countries. In high-poverty urban neighbourhoods in the U.S., enrollment in transfer programs such as Food Stamps and WIC is high, and these neighbourhoods are often characterized as having few grocery stores (imperfect competition). Transportation costs to other neighbourhoods are often high (*e.g.* because of low car ownership), causing these markets to also be relatively closed (Talukdar, 2008).

also point to imperfect competition as an important factor in explaining the patterns we see. For example, the price effects persist almost two years after the programme is in place, a period over which marginal costs are likely flat. This finding is consistent with imperfect but not perfect competition. Qualitative data on market structure also highlight the limited number of suppliers per village. In terms of normative implications, the dearth of supply-side competition in poor villages suggests then when the government acts as a supplier and provides in-kind transfers, it may not only be creating a pecuniary externality but also reducing deadweight loss from prices being set above their first-best level by imperfectly competitive firms.

One area for further research is to study how the supply-side adjusts when there are long-term in-kind or cash transfer programs in place. We do not observe changes in market structure over the one- to two-year horizon we study, but such effects might materialize in the longer run. We leave this question for future work since the experimental design and available data do not allow for such an analysis.

Policymakers' decision of whether to provide transfers in-kind or as cash includes many other considerations besides price effects. In-kind transfers constrain households' choices, which has costs but also might promote a paternalistic objective. Distributing goods in-kind might also be more expensive than delivering cash, as was the case in our context. Another key consideration is how efficiently the government can produce or procure supply; it is possible that an uncompetitive private sector creates more surplus than the government-cum-supplier if the government's productive efficiency is much lower than the private sector's. In that case, the best way for the government to alleviate supply constraints in poor villages while also providing income support to households might be cash transfers combined with alternative policies to promote supply-side competition.

APPENDIX

A. PRICE EFFECTS WITH IMPERFECT COMPETITION

Consider a simple Cournot-Nash model with N identical stores and indirect market demand for a homogenous good, $p(Q;X)$. Total demand is $Q = \sum_f q_f = Nq$ where $f = 1,...,N$ indexes the store. Each store faces constant marginal costs, $C = cq$. We assume that the demand curve is downward sloping, that is, $\frac{\partial p}{\partial Q} < 0$.

Both an in-kind and cash injection can be represented by a shift in demand. A cash transfer has only an income effect and is equivalent to a positive demand shift (for a normal good). An in-kind transfer entails this income effect and an additional decrease in demand due to the external influx of goods; consumers receive some items for free from the government, so they now demand less from local firms. In this model, such an exogenous change in demand is represented by a change in the demand shifter X, where we define $\frac{\partial Q}{\partial X} > 0$.

Stores maximize profits with respect to quantities taking others' behavior as given (Nash equilibrium):

$$\max_q \Pi = p(Q)q - cq.$$

The first-order condition is $p'q + p - c = 0$, which yields by substitution and differentiation:

$$p = c - \frac{Q(p;X)}{N\frac{\partial Q(p;X)}{\partial p}} \equiv \frac{N\varepsilon c}{N\varepsilon - 1},$$

where $\varepsilon \equiv -\frac{\partial Q}{\partial p}\frac{p}{Q}$ is the price elasticity of demand.

The above equilibrium condition is useful for studying the effect of a shift in demand, for example, $\partial X > 0$, on the equilibrium price. For the class of demand functions that are additive in X of the form $Q = g(p) + X$, we can immediately see that

$$\frac{dp}{dX} = -\frac{1}{N\frac{\partial g(p)}{\partial p}} > 0$$

since $\partial g/\partial p < 0$ from the assumption of a downward-sloping demand curve. A simple example in this class of demand curves is linear demand, for example, $Q = X - \alpha p$.

Thus, for any downward-sloping demand with an additive shifter, we can sign the price effect of a demand shift. For demand functions in this class, a cash transfer will lead to higher prices of normal goods and an in-kind transfer will lead to lower prices than a cash transfer, just as in the case of perfect competition.

The price effect of a demand shift will in general be given by $\frac{dp}{dX} = -Nc\frac{d\varepsilon}{dX}/(N\varepsilon-1)^2$. The sign of $\frac{dp}{dX}$, and hence the sign of the price effects of transfer programs, will depend on the sign of $\frac{d\varepsilon}{dX}$. For example, if transfers have a multiplicative effect on demand (*e.g.* $Q=Xp^{-\alpha}$), there would be no price effects of transfers ($\frac{dp}{dX}=0$) since the elasticity of demand is independent of X.

Appendix Table A1
There is no evidence of a differential impact across in-kind and cash villages in food expenditure away from home or non-food expenditure

Outcome =	Food expenditure away from home per capita	Non-food expenditure per capita	ln(Food expenditure away from home per capita)	ln(Non-food expenditure per capita)
	(1)	(2)	(3)	(4)
In-kind	2.11	9.06	0.03	0.09*
	(3.87)	(13.64)	(0.13)	(0.05)
Cash	1.50	11.73	0.00	0.10*
	(3.90)	(16.46)	(0.18)	(0.06)
Observations	10,985	10,985	1,248	10,907
Effect size: **In-kind - Cash**	**0.61**	**−2.67**	**0.03**	**−0.01**
H_0: **In-kind = Cash (*p*-value)**	**0.84**	**0.84**	**0.87**	**0.84**

Notes: ***$p<0.01$, **$p<0.05$, *$p<0.1$. (1) Each column is a difference-in-differences regression of the outcome on treatment groups and survey waves. The In-kind and Cash coefficients reported are the interactions of those treatment groups with an indicator for the follow-up survey wave. (2) Standard errors (in parentheses) are clustered at the village level.

Appendix Table A2
List of goods used in the analysis

	Goods used in analysis	PAL goods	High-quality variation		Goods used in analysis	PAL goods	High-quality variation
1	Tomato			31	Oats		
2	Onion			32	Soy		
3	Potato			33	Chicken		
4	Carrot			34	Beef and pork		
5	Leafy greens			35	Goat and lamb		
6	Squash			36	Seafood (fresh)		
7	Chayote			37	Canned tuna/sardines	x	
8	Nopale (cactus)			38	Eggs		
9	Fresh chili			39	Milk (liquid)		
10	Guava			40	Yogurt		
11	Mandarin			41	Cheese		
12	Papaya			42	Lard		
13	Oranges			43	Fortified powdered milk	x	
14	Plantains			44	Processed meats		
15	Apple			45	Pastelillo (snack cakes)		
16	Lime			46	Soft drinks		
17	Watermelon			47	Alcohol		
18	Corn tortillas			48	Coffee		
19	Corn kernels			49	Sugar		
20	Corn flour	x	x	50	Corn or potato chips		
21	Bread rolls			51	Chocolate		
22	Sweet bread			52	Candy		
23	Loaf of bread			53	Vegetable oil	x	
24	Wheat flour			54	Mayonnaise		
25	Wheat tortillas			55	Fruit drinks		
26	Dry pasta soup	x	x	56	Consome (broth)		
27	Rice	x		57	powdered Fruit drinks		
28	Breakfast cereal	x		58	Atole (corn based drink)		
29	Beans	x	x	59	Tomato paste		
30	Lentils	x	x	60	Canned chilis		

Note: We identified the set of PAL goods with high quality variation prior to estimating the models discussed in the text. The choice of goods was based solely on our knowledge of Mexican food consumption practices and through discussion with Mexican colleagues.

Appendix Table A3
Additional baseline characteristics by treatment group

	Control	In-kind	Cash	(1)=(2)	(1)=(3)	(2)=(3)	(1)=(2)=(3)
	(1)	(2)	(3)	*p*-value	*p*-value	*p*-value	*p*-value
Household characteristics							
Number of household members	4.78	4.64	4.61	0.40	0.36	0.84	0.61
	(0.14)	(0.10)	(0.13)				
Years of education of household head	4.28	4.24	3.94	0.88	0.17	0.17	0.29
	(0.18)	(0.14)	(0.17)				
Household has running water	0.65	0.57	0.50	0.23	0.06*	0.33	0.16
	(0.05)	(0.04)	(0.06)				
Household has an outside toilet	0.27	0.27	0.26	0.99	0.82	0.77	0.96
	(0.04)	(0.02)	(0.04)				
Household has electric lights	0.82	0.90	0.88	0.16	0.30	0.79	0.36
	(0.05)	(0.02)	(0.04)				
Household owns their own home	0.83	0.83	0.83	0.85	0.78	0.89	0.96
	(0.02)	(0.01)	(0.02)				
Household consumption (monthly pesos per capita)		298.71	288.76				
In-home food consumption	316.38 316.33	298.67	288.73	0.31	0.12	0.48	0.30
	(14.47)	(9.48)	(10.23)				
Non-food consumption	178.54	168.60	171.72	0.51	0.67	0.81	0.80
	(12.64)	(8.15)	(10.19)				
Out-of-home food consumption	14.78	11.70	9.46	0.19	0.03**	0.24	0.10
	(2.00)	(1.21)	(1.46)				
Consumption of PAL in-kind foods	45.08	45.66	45.98	0.81	0.72	0.87	0.94
	(2.09)	(1.24)	(1.44)				
Observations (household level)	1291	2810	1473				

Notes: ***$p<0.01$, **$p<0.05$, *$p<0.1$. Standard errors (in parentheses) are clustered at the village level.

Appendix Table A4
Main specification separately by PAL good

	Corn flour	Rice	Beans	Fortified powdered milk	Packaged pasta soup	Vegetable oil	Lentils	Canned tuna / sardines	Breakfast cereal
Outcome =	price	price	price	price	price	price	price	price	price
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
In-kind	−0.012	−0.004	−0.042	−0.026	−0.070**	−0.003	−0.012	−0.039	−0.062
	(0.019)	(0.028)	(0.033)	(0.143)	(0.034)	(0.020)	(0.061)	(0.024)	(0.099)
Cash	−0.007	0.009	−0.024	0.113	0.035	0.036	−0.053	−0.023	0.027
	(0.023)	(0.029)	(0.038)	(0.183)	(0.083)	(0.029)	(0.068)	(0.027)	(0.121)
Lagged normalized unit value	0.078	0.417***	0.398***	−0.016	0.521***	0.460***	0.004	0.053**	0.003
	(0.052)	(0.103)	(0.074)	(0.049)	(0.137)	(0.116)	(0.067)	(0.024)	(0.027)
Observations	249	317	309	103	316	323	202	313	203
Effect size: **In-kind - Cash**	**−0.005**	**−0.014**	**−0.018**	**−0.140**	**−0.105**	**−0.040**	**0.041**	**−0.016**	**−0.089**
H_0: **In-kind = Cash (p-value)**	**0.80**	**0.62**	**0.55**	**0.28**	**0.18**	**0.12**	**0.47**	**0.47**	**0.30**

Notes: ***$p<0.01$, **$p<0.05$, *$p<0.1$. (1) The outcome variable is the post-programme price; it varies at the village-store-good level. It is normalized by good; the price is divided by the average price of the good across all observations in the control group. Colums 1–6 are the basic PAL goods, columns 7–9 are the supplementary goods. Standard errors (in parentheses) are clustered at the village level. (2) Lagged unit value is the village median unit-value, imputed geographically if missing (see text), and it varies at the village-good level; the normalization uses the good-specific control group mean. (3) Regressions in all columns include an indicator for imputed pre-programme prices (see text).

Appendix Table A5
Price effects of in-kind and cash transfers, alternative specifications

							Expenditure weighted regressions		Excluding in-kind villages without classes		Non-Diconsa stores only	
	All PAL goods	Basic PAL goods only	All PAL goods	Basic PAL goods only	All PAL goods	Basic PAL goods only	All PAL goods	Basic PAL goods only	All PAL goods	Basic PAL goods only	All PAL goods	Basic PAL goods only
Outcome =	price	price	price	price	ln(price)	ln(price)	price	price	price	price	price	price
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
In-kind	−0.037*	−0.033	−0.036*	−0.033	−0.037	−0.015	−0.037*	−0.031	−0.032	−0.017	−0.027	−0.018
	(0.020)	(0.020)	(0.020)	(0.020)	(0.025)	(0.022)	(0.020)	(0.021)	(0.022)	(0.023)	(0.020)	(0.021)
Cash	0.002	0.014	0.003	0.013	0.006	0.039	−0.004	0.003	0.002	0.015	0.014	0.018
	(0.023)	(0.026)	(0.023)	(0.027)	(0.028)	(0.026)	(0.022)	(0.023)	(0.023)	(0.027)	(0.023)	(0.025)
Lagged normalized unit value	0.034	0.128***					0.101***	0.276***	0.025	0.149***	0.022	0.091**
	(0.021)	(0.042)					(0.030)	(0.040)	(0.029)	(0.056)	(0.021)	(0.043)
Lagged normalized store price			0.017	0.034								
			(0.029)	(0.031)								
Lagged ln(unit value)					0.857***	0.861***						
					(0.025)	(0.037)						
Good fixed effects	yes	yes										
Observations	2,335	1,617	2,335	1,617	2,335	1,617	2,335	1,617	1,729	1,197	1,767	1,217
Effect size: In-kind - Cash	−0.038**	−0.047**	−0.039**	−0.046**	−0.044**	−0.054**	−0.033**	−0.034**	−0.034*	−0.032	−0.040**	−0.036*
H_0: In-kind = Cash (*p*-value)	0.03	0.04	0.03	0.04	0.04	0.02	0.00	0.00	0.08	0.21	0.02	0.09

Notes: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. (1) The outcome variable in columns 1–4 and 7–10 is the post-programme price; it varies at the village-store-good level. It is normalized by good; the price is divided by the average price of the good across all observations in the control group. The outcome in columns 5–6 is the logarithm of the post-programme store price, with no normalization. Standard errors (in parentheses) are clustered at the village level. (2) Lagged normalized unit value is the village median unit-value, imputed geographically if missing (see text), and it varies at the village-good level; the normalization of this variable uses the good-specific control group mean; (3) Lagged store price is the village median store price, imputed with the village median unit-value if missing (see text), and it varies at the village-good level; it is normalized using the good-specific control group mean; those in columns 7 and 8 include two imputation indicators. (4) Lagged ln(unit value) is the logarithm of the village median unit-value, imputed geographically if missing (see text); it varies at the village-good level. (5) Regressions in columns 1–2 and 5–8 include one indicator for imputed pre-programme prices; those in columns 3–4 include two such indicators (see text).

Appendix Table A6
Tests of baseline balance by development index

Regressor =	Above median development		In-kind × Above median development		Cash × Above median development		Obs
	Coefficient	(SE)	Coefficient	(SE)	Coefficient	(SE)	
Outcome			Prices, basic PAL goods				
Median village unit-value, normalized	0.012	(0.028)	0.010	(0.036)	−0.044	(0.041)	2,130
Missing median village unit-value	0.081**	(0.033)	−0.063	(0.042)	−0.160***	(0.049)	2,130
			Prices, all PAL goods				
Median village unit-value, normalized	0.040	(0.034)	−0.026	(0.046)	−0.021	(0.046)	3,195
Missing village unit-value	0.015	(0.032)	−0.049	(0.041)	−0.158***	(0.048)	3,195
			Prices, all goods				
Median village unit-value, normalized	0.042	(0.028)	−0.025	(0.035)	−0.022	(0.039)	21,300
Missing village unit-value	−0.083**	(0.032)	−0.029	(0.039)	−0.037	(0.043)	21,300
			Village level characteristics				
Missing median store price	0.076	(0.099)	−0.118	(0.118)	0.100	(0.143)	191
Diconsa store in the village	−0.110	(0.131)	−0.018	(0.167)	0.156	(0.191)	191
Median months for which transfers were received	–	–	−0.940**	(0.434)	−0.123	(0.870)	190
			Household level characteristics				
Food-producing household	−0.191**	(0.093)	−0.073	(0.101)	0.085	(0.111)	1,979
Farm costs (pesos)	−207.942	(296.626)	121.687	(350.373)	114.687	(358.265)	1,903
Farm profits (pesos)	279.562	(226.033)	−164.342	(318.659)	−201.309	(273.831)	1,867
Asset index	1.243***	(0.375)	−0.034	(0.434)	0.173	(0.471)	1,978
Indigenous household	−0.158	(0.149)	−0.130	(0.167)	−0.200	(0.177)	1,979
Household has a dirt floor	−0.151	(0.126)	−0.179	(0.143)	−0.191	(0.148)	1,979
Household has piped water	0.150	(0.125)	0.096	(0.144)	0.146	(0.172)	1,979

Notes: ***$p<0.01$, **$p<0.05$, *$p<0.1$. (1) Each regression also includes indicators for cash and in-kind villages. (2) Above median development is an indicator for a village being above the median of the first principal component of the following four variables: the time required to travel to a larger market that sells fruit, vegetables, and meat; the distance to the head of the municipality; pre-program village median monthly expenditure on non-durables; and the village population. (3) Standard errors in parentheses. For normalized median village unit values and household level characteristics, standard errors are clustered at the village level. (4) Median village unit values are normalized with the good-specific control group mean and are imputed geographically if missing (see text). (5) Travel time to the nearest market is the time in hours needed to travel to a larger market that sells fruit, vegetables, and meat. It is constructed as the village median of household self-reports. (6) Expenditure is the value of non-durable items (food and non-food) consumed in the preceding month, measured in pesos; six households are missing expenditure data. (7) Food producing households are those that, at baseline, either auto-consume their production or report planting or reaping produce or grain or raising animals. (8) Farm costs and profits are for the preceding year. Samples are trimmed of outliers greater than 3 standard deviations above the median (about 1% of observations). (9) The asset index is the sum of binary indicators for whether the household owns the following goods: radio or TV, refrigerator, gas stove, washing machine, VCR, and car or motorcycle; two households are missing the asset index. (10) A household is defined as indigenous if one or more members speak an indigenous language.

Appendix Table A7
Household expenditure in cash villages, class attendees versus non-attendees

Outcome =	Total expenditure (food + non-food) per capita		Food expenditure per capita		Expenditure on PAL in-kind food items per capita		Non-food expenditure per capita	
	Logs	Levels	Logs	Levels	Logs	Levels	Logs	Levels
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Attended classes	0.04	12.55	0.05	10.52	0.07	−0.21	0.06	2.52
	(0.04)	(27.72)	(0.04)	(15.74)	(0.05)	(3.25)	(0.06)	(18.76)
Lagged outcome	0.42***	0.56***	0.38***	0.42***	0.30***	0.42***	0.34***	0.51***
	(0.03)	(0.06)	(0.03)	(0.06)	(0.03)	(0.06)	(0.03)	(0.05)
Observations	1,257	1,257	1,257	1,257	1,252	1,257	1,235	1,257

Notes: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. (1) Observations are at the household level. Sample includes all PAL eligible households in cash villages. (2) Expenditure is the value of goods consumed in the preceding month, measured in pesos. (3) A household is classified as attending classes if they report attending at least one class covering topics in health, hygiene, or nutrition. (4) Village fixed effects included in all regressions. Standard errors (in parentheses) clustered at the village level.

APPENDIX FIGURE A1
Trucks transporting PAL boxes.

APPENDIX FIGURE A2
PAL box of food.

APPENDIX FIGURE A3
Unloading PAL boxes in the village.

APPENDIX FIGURE A4
Grocery shops in PAL villages.
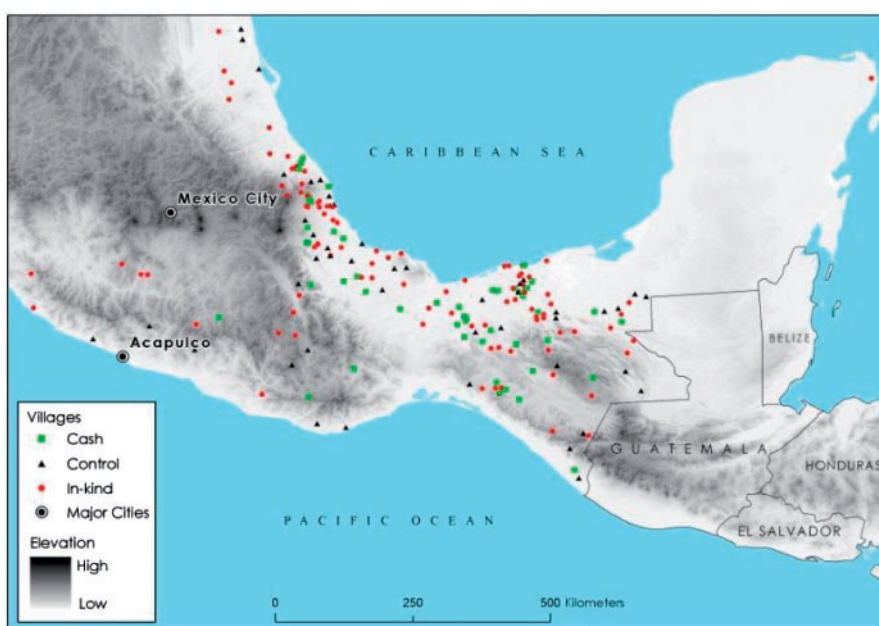
APPENDIX FIGURE A5
Villages in the PAL experiment.

B. DATA APPENDIX

Variable Construction

Post-programme prices.   Post-programme prices come from a survey of local stores; a maximum of three stores were surveyed per village in each survey wave. Prices were collected in common units, for example the price of a 150 milliliter container of yogurt, a "small" loaf of bread, or a kilogram of corn flour. For non-standard units, we converted prices to either kilograms (for solids) or litres (for liquids) using conversion factors supplied by the Mexican government for non-standard units (*e.g.* a "small" loaf of bread weighs 0.68 kg). In most specifications, post-programme prices are normalized by the good-specific mean in the control group.

Pre-programme prices.   The main measure of the pre-programme price is the village-median household unit value. Households reported both expenditure and quantity purchased by good in a seven-day food recall survey, and the household unit-value is defined as the ratio of the two measures. For some goods in some villages, there was no expenditure on a good by any household during the seven-day recall period, and therefore the village-median unit-value for that good is missing. In these cases, we impute the pre-programme price using the median pre-programme price in other villages within the same municipality (or within the same state in the few cases where there are no data for other villages in the municipality). Among all PAL goods, we impute 18% of village-good observations; among basic PAL goods, we impute 14% of village-good observations.

An alternative pre-programme price is the village median store price; we use the village median as there is no store identifier in the data that would allow us to match stores between survey waves. When no price of a good is observed in a village pre-programme, we impute this measure using the village median unit-value (19% and 16% of observations for all PAL goods and basic PAL goods, respectively). When the village median store price and the village median unit-value are missing, we impute geographically as above (11% and 10% of observations for all PAL goods and basic PAL goods, respectively). For both of these measures of pre-programme prices, we normalize in most specifications using the good-specific mean in the control group.

Presence of a Diconsa store.   We identify the presence of a Diconsa store in a village from the names of stores that were surveyed for their prices, coding this variable by hand. There could be false negatives if a Diconsa store was not one of the one to three stores surveyed.

Length of receipt of aid.   Households self-reported to enumerators in the post-treatment survey whether they received transfers in any of the preceding 24 months. Our village-level measure of the length for which aid was received is the village median number of months for which transfers were received.

Variation in product quality.   We define the variation in the quality of PAL goods in two ways. First, we subjectively identified goods that had high quality variation; these goods include beans, cereal, corn flour, lentils, and pasta soup. Second, we calculate the village-good-specific coefficient of variation of pre-period unit values, that is, the coefficient of variation among households in the village that purchased the good. We also average this coefficient of variation across villages to create a good-specific version of this proxy measure of quality variation.

Good- or village-specific influx of in-kind goods (ΔSupply).   ΔSupply is a ratio that measures the size of the supply influx of in-kind goods into programme villages, relative to what would have been consumed in the absence of the PAL programme. We construct a village-good-specific measure: the village aggregate amount of a good that was or would be transferred to the village (based on its eligibility rate) divided by the average consumption of the good at baseline. In the descriptive statistics reported in Table 1, we report the average across in-kind villages of the actual supply influx, by good, where counterfactual consumption is the average across control villages in the post period.

Development index.   The development index, defined at the village level, is constructed as the first principal component of the following variables: pre-intervention average expenditures per capita, village population, median self-reported travel time to a larger market that sells fruit, vegetables, and meat, and distance to the nearest municipality head. Expenditures per capital come from the PAL survey, and village population comes from the Mexican census of 2005. For the self-reported travel time, households were first asked if these fresh foods were sold in the village; if the answer was no, they were then asked to state the time to get to the nearest market using their typical mode of transportation. We use the village median among households that report leaving the village to purchase fresh foods. The distance to the nearest municipality head is measured in kilometers and was constructed using GIS software.

Market power index. We classify villages into those where grocery shops have market power and those where they do not. The measure of market power is the empirical implication of the model developed by Attanasio and Pastorino (2015) relating the existence of price discounts for larger-quantity purchases to the degree of imperfect competition in the market. When there is market power among sellers, there should be a negative within-village correlation between prices (unit values) and quantities purchased. We estimate the correlation between household unit values and and quantities consumed separately for each of the PAL villages using the pre-intervention data for the set of food items examined by Attanasio and Pastorino: rice, beans, sugar, tomatoes, and corn tortillas. A village is defined as having market power if the correlation coefficient is negative; in our sample 75.2% of villages are classified as having market power.

Measure of openness. Our measure of the openness of a village economy is the correlation between pre-programme village prices and prices in the same year in national capital, Mexico City. We calculated good-specific Mexico City prices as the average good-specific Consumer Price Index (CPI) collected by the Bank of Mexico between July of 2003 and July of 2004. Of the 60 food items in our main analysis, nine do not have CPI data: soy, tomato paste, oats, mandarins, lard, canned chilis, atole, goat/lamb meat, and lentils. The openness measure is the correlation coefficient between the prices of fifty-one goods in a village and the same goods in Mexico City.

Total household consumption. *ExpendPC*, or monthly per capita expenditure, is constructed as the sum of monthly household food expenditure, non-food expenditure, and expenditure on food away from home, divided by the number of household members. Food expenditure is the value of food consumed; consumption amounts were collected with a seven-day food recall module (converted to monthly amounts), covering sixty-one food items, and we use village median household unit-values (imputed geographically if missing) to value consumption. Non-food expenditure was reported at the monthly level and covers twenty-six categories designed to capture the extent of non-durable, non-food expenditure (non-food consumption quantities were not collected). Weekly expenditure on food away from home was self-reported by the household, and we convert to monthly amounts.

Farm production measures. We use two measures of farm production: farm profits and farm costs. Both are self-reports from the household surveys. Households were first asked whether any household member planted or reaped produce or grain or raised animals in the past year. If yes, they were asked the total costs involved in these activities and then how much money was left after these costs had been paid (*i.e.* farm profits). At baseline, among households that reported planting or reaping produce or grain or raising animals, 33% stated that farm costs were zero, and 85% stated that farm profits were zero.

Producer household indicator. The variable *Producer* equals one if, at baseline, a household either auto-consumed their production or reported that, within the last year, any household member planted or reaped produce or grain, or raised animals. Auto-consumption data was collected for sixty-one food items in a seven-day food recall module. Households were asked to state the quantities consumed of each item, and how much of that consumption was from own production (auto-consumption). If a household auto-consumed any positive amount of at least one good, we classify them as a producer.

Household asset index. We construct an index of the durable assets a household owns from self-reports in the household questionnaire. Households were asked if they owned each of the following six items: a radio or TV, a refrigerator, a gas stove, a washing machine, a VCR, and a car or motorcycle. We sum the number of items the household reports owning to create the variable *Asset Index*; thus, *Asset Index* ranges from zero to six.

Qualitative Surveys of Food Stores

We conducted two rounds of qualitative surveys of shopkeepers in a total of fifty-two villages in the states of Veracruz, Oaxaca, and Puebla. The first round was in the spring of 2011 and included sixteen villages, 11 of which were PAL experimental villages and five are villages that were incorporated into the programme after the experiment ended. The second was in the fall of 2015 and included thirty-six villages, all of which were PAL experimental villages. In each village, a research assistant interviewed several shopkeepers and asked a series of questions designed to learn about (1) the shape of the marginal cost curve and (2) the degree of competition. Specific topics included: how many stores were in the village, how they procured supply, how they responded to unexpected changes in demand, and when they adjusted prices.

Supplementary Data

Supplementary data are available at *Review of Economic Studies* online.

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