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Abstract

This paper 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 should cause prices to fall relative to cash transfers. We test and confirm this prediction using a program 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 with cash transfers; relative to the control group, in-kind transfers lead to a 4 percent fall in prices while cash transfers lead to a positive but negligible increase in prices. Prices of goods other than those transferred are also affected, but by a small amount. Thus, households' purchasing power is only modestly affected by these price effects, even in this setting where program eligibility is high, the transfer per household is sizeable, and hence the supply influx is large. The exception is in remote villages, where the price effects (both the negative effects of in-kind transfers and the positive effects of cash transfers) are larger in magnitude. The effects do not dissipate over the two years of program duration we observe.

Key words: price, policy

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1. Introduction

A central question in anti-poverty policy is whether transfers should be made in kind or as cash. The oft-cited rationales for in-kind transfers are to encourage consumption of particular goods or to induce less needy individuals to self-select out of the program (Besley, 1988; Nichols and Zeckhauser, 1982; Blackorby and Donaldson, 1988; Besley and Coate, 1991; Bearse, Glomm, and Janeba, 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 tradeoff is the effect that in-kind and cash transfers have on local prices. Cash transfers increase the demand for normal goods, and if supply is not perfectly elastic, prices of these goods should rise. In-kind transfers have a corresponding cash value, so they similarly shift demand through an income effect. But, in addition, an in-kind transfer program increases local supply. If the government injects supply into a partially-closed economy (e.g., trucks food aid into a village), then relative to cash transfers, local prices should fall when transfers are provided in-kind.[1]

These pecuniary effects could potentially be a useful policy lever.[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, as discussed by Coate, Johnson, and Zeckhauser (1994). Similarly, Coate (1989) discusses how price effects could make an in-kind food aid program more effective than a cash program, depending on the market structure. While under perfect competition, price effects are pecuniary externalities that shift wealth between buyers and sellers, with imperfect competition among local suppliers and prices above the first-best level, the lower prices induced by in-kind transfers could represent an increase in efficiency. 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 program goal of assisting the poor.[3]

[1] Transfers can also take the form of vouchers, as in the U.S. Food Stamp and WIC programs. In this case the program increases demand for certain goods but local supply is not 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 program), 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 program recipients and non-recipients); if encouraging consumption of these items is precisely the paternalistic motive for using in-kind transfers, then the price effects will reinforce this program goal.

Price effects of transfer programs are likely to be largest when, first, the size of the individual transfer is large, second, there is high program enrollment within a community, and, third, the economy is not fully integrated with the outside economy so local supply and demand determine prices. Many transfer programs in developing countries meet the first two criteria, and, regarding the third criterion, many program recipients live in rural, often isolated villages (IFAD, 2010).

This paper 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 program for poor households, the Programa de Apoyo Alimentario (PAL). When rolling out the program, 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 percent on average, were eligible for the program.

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

There is no detectable increase in prices under cash transfers, while in-kind transfers cause prices of the transferred goods to fall by 3.7 percent. Across several specifications, we consistently find that providing transfers in kind rather than as cash causes prices to be lower by 3 to 4 percent.

Further, we find that these effects are not limited to the short run; over the range of program duration in the data, from 8 to 22 months, the effects persist as program duration increases. 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 potentially induced by the intervention, or alternatively, such

[4]Throughout the paper, when we calculate the nominal value of the transfer, we use pre-program unit values.

adjustments take several years to materialize.

Goods that are not part of the transfer program 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 most households' purchasing power, even in this setting where program eligibility within a market is very high and the transfer is large relative to food expenditures. This finding suggests that for typical transfer programs, price effects may not be economically significant.

The exception within our setting is the more isolated villages. Isolated markets are hypothesized to have larger price effects because product markets are less integrated with the world economy. In addition, there is likely less competition on the supply side (e.g., among grocery shops), which could also make prices more responsive to transfers. We find that the price effects are much larger in the geographically remote villages, which we define as those with above-median travel time to a large market. In fact, the average effects we find are driven almost entirely by remote villages. We find some suggestive evidence that imperfect competition, as proxied by fewer grocery shops in the village, is part of the explanation for these heterogeneous effects.

For remote villages, in-kind transfers cause prices of the transferred goods to fall by 8 percent relative to cash transfers. In addition, the cash transfers lead to a 5 percent increase in overall food prices; this implies an elasticity of prices with respect to income of 0.64, as the cash transfers constitute an 8 percent increase in aggregate village income, on average. Thus, the price effects of the transfer program have important consequences in remote villages. Choosing in-kind rather than cash transfers generates extra indirect transfers to consumers in the form of lower prices worth over 40 percent of the direct transfer itself; these effects have the opposite implication for food-producing households in the recipient villages. We should note that our estimates of the program's total effects have wide confidence intervals, but they are nonetheless suggestive of quantitatively important price effects in physically isolated communities.

Since poorer villages are typically more isolated, these findings suggest that transfer programs targeting the ultra-poor in developing countries may inherently have important pecuniary effects (World Bank, 1994). Mexico is more developed than many countries, so even the more remote villages in our sample are not extremely remote in an absolute sense; the median travel time to a market is half an hour. As a comparison, a large proportion of people in Africa live in areas more isolated than this, both because of geographic conditions and poor road quality: In Uganda and Kenya, an estimated 50 percent of the population lives more than 2 hours away from the nearest

market, defined as a village with a population of 5,000 or higher (Pozzi and Robinson, 2008). But it bears repeating that our analysis also indicates that if a community is well-connected with larger markets, pecuniary effects are likely to be small relative to the direct benefits of transfer programs.[5]

Using household-level data, we also analyze how the price changes affect consumers and producers differently. The packaged food items provided in kind by PAL are procured from outside the recipient villages, but the crops that agricultural households grow are substitutes for the in-kind goods, so agricultural households should see the prices of their products fall with in-kind transfers. We indeed find that agricultural households seem to be relatively better off under cash transfers (higher food prices) than in-kind transfers (lower food prices), enjoying larger increases in agricultural profits and household wealth. Agricultural households are poorer than non-agricultural households (13 percent lower per capita expenditures at baseline), so a differential welfare gain for agricultural households is progressive in this setting.

This paper 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 the consumption effects of the PAL program in Mexico (Gonzalez-Cossio et al., 2006; Skoufias, Unar, and Gonzalez-Cossio, 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 program (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, Fargeix, and Sadoulet, 1991; Jones, 1996).

Fewer studies provide evidence on the question this paper 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 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

[5] Angelucci and De Giorgi (2009) study the Oportunidades villages in Mexico—villages that are less remote and less poor than the PAL villages—and do not find price effects of cash transfers.

[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 program 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.

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 paper is also one of the first to measure the price effects of social programs in developing countries. There is a rapidly growing set of studies in development economics that study the direct effects of social programs, but fewer studies examine the indirect effects of programs and in particular their market-level price effects (Lise, Seitz, and Smith, 2004; Angelucci and De Giorgi, 2009; Kaboski and Townsend, 2011; Imbert and Papp, forthcoming; Attanasio, Meghir, and Santiago, 2012). Our finding that the pecuniary effects of social programs can be quite large in areas that have limited access to other markets 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 toward cash transfers (World Food Programme, 2011). Meanwhile, other major programs are moving away from cash toward in-kind transfers. For example, in the U.S. much of the welfare support under the Temporary Assistance for Needy Families program is now in the form of child care, job training, and other in-kind services (Pear, 2003). Our work highlights two related lessons for policy makers choosing between cash and in-kind transfers. First, their policy choice could have non-trivial implications for local prices, at least in isolated communities. Second, the communities that have high eligibility for transfer programs may also be the ones with less competition among local suppliers; in this case, changes in local prices are not just pecuniary externalities, but have efficiency implications too. These lessons are very relevant in developing countries where most of the poor live in rural villages. They may also be applicable in developed countries: Inner cities in the United States have high participation in programs such as Food Stamps/SNAP and are often characterized as having few grocery stores and high food prices (Bell and Burlin, 1993; Talukdar, 2008).

The remainder of the paper is organized as follows. Section 2 lays out the theoretical predictions. Section 3 describes Mexico's PAL program and the experiment. Section 4 describes our empirical strategy and data. Section 5 presents the results, and Section 6 offers concluding remarks.

[7]The paper is also related to a broader literature on the determinants of prices in isolated markets in developing countries (Jayachandran, 2006; Donaldson, 2010; Atkin and Donaldson, 2014).

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] 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 reason for increasing marginal costs; to meet higher demand, a shopkeeper in a remote village might need to travel to a neighboring village to buy supply from a shop there.[9] Over time, one might expect the supply curve to become flatter, and thus the price effects to diminish, under perfect competition.

Figure 1 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.[10] Denoting the amount of money transferred in cash by X_{Cash}, our first prediction is that a cash transfer will cause prices to rise:

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

In-kind transfers also generate an income effect, so demand will again shift to the right. We

[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 program. These effects would be very small since the program households represent less than 1 percent of Mexican households, but these small effects would apply to many people. Our focus is the price effects within the villages that receive the program; thus, we examine only the local price effects in the recipient villages, and not the total price effect of the program.

[9]In our qualitative interviews of shopkeepers in the program villages, shopkeepers said that they meet unexpectedly high demand by traveling to a neighboring village or town to buy goods. Shopkeepers can adjust to low demand by allowing inventory to build up. However, shopkeepers face relatively high inventory costs because many are credit constrained and have limited working capital. Other factors cited by shopkeepers in our qualitative fieldwork were the risk of theft or damage to inventory and limited storage capacity.

[10]For inferior goods, demand will shift to the left with the opposite price effect. Attanasio, Di Maro, Lechene, and Phillips (2013) find that food items are typically normal goods in Mexico.

define the in-kind transfer amount X_{InKind} in terms of its equivalent cash value.[11] 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 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.[12] For transferred goods, the price should be lower under in-kind transfers:

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

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 program we study. In contrast, the increased demand due to income effects will be spread across several food and non-food items. The cash transfer program we study placed no restriction on how recipients could use the money, so it likely led to a small amount of extra demand per good, spread across several goods. Note that there could be a flypaper effect through which this cash transfer labeled as food assistance stimulated the demand for food more than a generically-labeled transfer would have (Hines and Thaler, 1995; Kooreman, 2000). This would improve our ability to detect the effects of the cash transfer on food prices, though the effects would still be spread across the over sixty food prices we observe.

Imperfect competition

In the setting we study, the local supply side consists of food shops in the village. There are not many shops in the typical village, so the assumption of perfect competition may be inaccurate.[13]

Predictions (1) and (2) can also hold in the case of imperfect competition. This can also be seen using Figure 1, where now we regard the residual demand curve as that facing, say, a monopolist

[11] If either the transfer is inframarginal (that is, 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. Note that if this latter case pertained (costly resale), then the effective supply influx into the economy from an in-kind transfer would be the actual influx net of any extramarginal transfers that are consumed.

[12] 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.

[13] The distributors that truck supplies into the village are another type of supplier. They often have market power, so they may be the source of imperfect competition and the effective price setter in some cases.

rather than a set of perfectly competitive firms. Note that unlike the case of competitive firms, imperfect competition implies that transfer programs can have price effects even if marginal costs are constant. Thus, price effects could persist over time, even if marginal cost curves flatten as stores are able to adjust their inventory.

To assess how the price effects vary with the degree of competition, 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.[14]

More generally, the price effects under imperfect competition depend on the shape of the demand curve. For example, if the program 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 is a downward-sloping demand curve where the transfers represent an additive shift in demand. One, then, has the following comparative statics for how the price effects vary with the degree of competition:

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

and

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

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

While the comparative statics may be the same with perfect or imperfect competition, the efficiency implications differ. 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 program.)

The discussion above takes the market structure as given. The program could also affect how

[14] Another potential response to the program is that firms might change their degree of price discrimination. In our qualitative interviews with shopkeepers, we found that surprisingly few engage in price discrimination. Most post prices openly and do not vary the price for different customers.

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 program. 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 program 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 within 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.

Openness of the economy

Returning to the competitive case, another testable prediction is that the more inelastic supply is (i.e., the steeper the supply curve is or the lower the elasticity, η_S, is), the more prices will respond to shifts in supply and demand. One factor affecting the elasticity of supply is the degree of openness of the local economy. For example, in our setting, if a shopkeeper responds to an increase in demand by obtaining extra supply from a neighboring village, then the more remote the location of the village, the higher the marginal cost of procuring additional supply, or the steeper the supply curve. In the extreme of a perfectly open economy, with prices exogenous to the village, then neither cash or in-kind transfers into the village should affect prices.

For a cash transfer, the price increase should be smaller the higher η_S is (the more open the economy is or the flatter the supply curve).

$$\frac{\partial^2 p}{\partial \eta_S \partial X_{Cash}} < 0 \tag{5}$$

Comparing in-kind to cash transfers, the (relative) price response should be smaller in magnitude (less negative) when η_S is higher.

$$\frac{\partial}{\partial \eta_S}\left(\frac{\partial p}{\partial X_{InKind}} - \frac{\partial p}{\partial X_{Cash}}\right) > 0 \tag{6}$$

For an in-kind transfer relative to no transfer, the net effect of the income and supply effects is ambiguous as discussed above, but the magnitude of the net effect will be smaller in more open

economies.

In our empirical analysis, to test both the predictions about imperfect competition and about openness, we compare more geographically isolated villages (longer travel time to larger markets) to less isolated villages. Geographic isolation is our proxy for both how closed an economy is (lower η_S) and for how uncompetitive the market is (lower N). We also test more directly for the effects of competition by using a measure of how many grocery shops there are in the village.

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

3. Description of the PAL Program

3.1 PAL program 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 program, or Oportunidades, the conditional cash transfer program. Therefore PAL villages are typically poorer and more rural than the widely-studied Progresa/Oportunidades villages.[15] Households within program 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 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 program was to encourage households to add diversity to their diet and consume more of these supplementary goods.[16]

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

[15] 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 program; villages that lacked adequate health facilities, for example, were ineligible for Progresa/Oportunidades.

[16] Appendix Figures 1 to 4 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.

by 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 three to seven percent 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 comprised 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 program, 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 percent of villages); those in the cash villages received a 150 peso per month cash transfer (25 percent of villages); and those in the control group villages received nothing (the remaining 25 percent of villages). About 89 percent 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 program over the course of 14 months, beginning in December of 2003. This gradual rollout creates variation in how long the program 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 program 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 program, 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

[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 percent (private communication with program 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 5 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.

the program, 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 program 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 percent of a recipient household's baseline food expenditures on average and 11 percent of total expenditures. Including the ineligible households, the injection of food into the village through the program was equivalent to 16 percent of baseline aggregate food expenditures and 10 percent of total expenditures for the village. Similarly, the cash transfer represented an 8 percent increase in recipients' income and, in aggregate, a 7 percent 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 sardines. There is some ambiguity about whether the in-kind villages always received these three supplementary items, so, in our analysis, 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. As mentioned above, the monthly box of food had a market value of about 206 pesos in the program 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,

[20]The government should have included its transportation costs when calculating the in-kind program'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 percent 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 percent of the population producing the good, the largest of which is corn kernels, which 27 percent of households produce.

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 program 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 program 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 analyze 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 toward the PAL foods (as shown in the appendix). 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 program 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 program and an in-kind transfer program, and the cash equivalent of the in-kind transfer is exactly the same amount as the cash transfer, then the income

[22] Based on the household survey data, 76 percent of respondents attended a class in the in-kind villages assigned to receive classes and 69 percent 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 program. Furthermore, assignment to classes did not affect total food expenditure or the composition of food expenditure (results available from the authors).

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-program 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 percent lower than the cost at consumer prices in the recipient villages; 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 to which we compare

[23]Households might also store the goods, but since the program 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.

it (146 pesos versus 150 pesos). 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 another potential mechanism is 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.[24]

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 program. 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.

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

$$p_{gsv} = \alpha + \beta_1 InKind_v + \beta_2 Cash_v + \phi\, p_{gv,t-1} + \sigma I_{gv} + \varepsilon_{gsv} \tag{7}$$

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

[24] 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.

is cross-sectional, not a panel over time.) We also include the dummy variable *I* to indicate whether the pre-program 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 program 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 program. 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 program's impacts on food consumption, and what type of data they collected was determined accordingly.

Our measure of post-program prices comes from a survey of local food stores. From each store, enumerators collected prices for fixed quantities of 66 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 percent of villages in our sample, one store was surveyed; for 47 percent of villages, two stores were surveyed; and three stores were surveyed in the remaining 16 percent of villages.[25] Some of the stores surveyed were part of the Diconsa agency (21 percent) while the majority were independent stores (79 percent).

We also use measures of pre-program food prices. Baseline data collection on store prices are missing for 40 percent of the sample because, first, data were collected for only 40 of the 66 food items, and, second, even among the sampled goods, there are missing data for 19 percent

[25]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 in order to maintain consistency.

of village-good observations (see Appendix B for details). Therefore, we also use the household survey to construct the pre-program unit value (expenditure divided by quantity purchased) for each food item. In each village, a random sample of 33 households was interviewed about purchase quantities and expenditures on 60 food items. We use the median unit value among households in the village as a measure of the village's pre-program price.[26] In cases where the pre-program 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-program 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-program 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 program 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.[27] Finally, two pairs of goods were

[26] Unit values are observed for households that purchased the good in the past seven days. We do not use unit values for post-program prices because the program 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-program prices. However, if quality varies, then treatment effects estimated with post-program 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 (e.g., beans, corn kernels, onions). The noisiness of our pre-period price measure will vary with the number of observed unit values.

[27] The price of biscuits was intended to be collected, but a mistake in the survey questionnaire led enumerators to

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-program 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 51 non-PAL goods. Appendix Table 1 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-program 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, $\Delta Supply$, is constructed as the average across in-kind villages of the total amount of 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-program period. For example, there was almost exactly as much corn flour delivered to the villages each month as would have been consumed absent the program ($\Delta Supply = 1.00$ for corn flour), while there was over eight times as much fortified powdered milk delivered as would have been consumed absent the program ($\Delta Supply = 8.62$ for fortified milk powder).

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 percent level and only one variable is unbalanced at the 10 percent level.

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

collect prices for crackers ("galletas saladas" in Spanish) rather than for biscuits ("galletas" in Spanish).

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.

Note that the data collection was designed to measure the PAL program'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 program's price effects. However, other data that ideally we would have liked to have are unavailable, e.g., a census of grocery shops in a village. We conducted follow-up qualitative fieldwork in 2011 in 16 of the program villages (see Appendix B for further details), interviewing several shopkeepers per village, in order to better understand the market structure and the price-setting behavior of grocery shops. We did not collect retrospective quantitative data, as we found that respondents could not reliably remember details about the market structure from eight years before.

5. Results

5.1 Price effects of in-kind transfers and cash transfers

Table 3, column 1, presents the main specification (Equation 7) using all nine PAL goods. The regression pools the effects for the different PAL food items. (See Appendix Table 2 for the results separately for each PAL good.) For cash villages, the point estimate suggests that the transfer program caused prices to increase by 0.2 percent ($\widehat{\beta}_2$), though the coefficient is not statistically significant. In in-kind villages, prices fell by 3.9 percent relative to the cash villages ($\widehat{\beta}_1 - \widehat{\beta}_2$), 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 percent decline, significant at the 10 percent 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, again much 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-program 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 we find very similar coefficients. Columns 5 and 6 present the difference-in-differences estimate. Here the relevant coefficients are the interactions with the dummy for the post-period.

5.2 Robustness checks

The results are also robust to several other specifications, as shown in Appendix Table 3. 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 percent of cases where they are missing.[28] 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 percent level. Fourth, we show that the results are similar when we drop half of the in-kind villages and focus on the cash and in-kind villages assigned to receive health and nutrition classes.

[28] 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).

Finally, we show that the results are robust to restricting the sample to privately-owned stores.[29] 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 empirically by using proxies for the amount of quality 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 the goods with little variation in quality (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 use the village-good-specific measure (columns 5 and 6) and we again find that, first, the results are not

[29] 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 3 we estimate equation (7) 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. The Diconsa/cash interaction coefficient is -0.012 with a standard error of 0.038, and the Diconsa/in-kind coefficient is -0.002 with a standard error of 0.030.

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 quality upgrading or downgrading. The point estimates suggest that this price wedge is a result of 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 effect.

5.3 Persistence of price effects

In Table 5 we present evidence on whether the price effects dissipate over time. To do so, we exploit variation across villages in when the program 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 program duration is undefined for the control group, so this analysis compares in-kind to cash villages only.

We interact program 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 program start date is not randomly assigned, so one concern is the endogeneity of the program duration at follow-up. The one observable characteristic that we find is significantly correlated with program duration is the remoteness of the village (we define our measure of remoteness formally in the next section). Thus, we reproduce the test above controlling for remoteness 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 cast doubt on the interpretation that these village markets are perfectly competitive, as we would expect the marginal cost curve to become flatter over time. 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 based on remoteness of the village

We next test the prediction that more remote villages—which are both more closed economies and more likely to have imperfectly competitive suppliers—exhibit larger price effects. Our measure of remoteness is the time required to travel to a larger market that sells fruit, vegetables, and meat. For convenience, we will refer to a village as "remote" if it is above the median value of this measure. The measure captures the difficulty of transporting supply to the village and therefore the village's lack of integration with the outside economy. In addition, remote villages are likely to have more market concentration (e.g., fewer shops selling groceries). *Remoteness* is constructed from household-survey self-reports on travel time to the larger market. (See Appendix B for details on the construction of this variable.)

Table 6 reports the results on how the price effects vary with remoteness. Columns 1 to 3 examine all PAL items (basic and supplementary). Column 1 reports the results for remote (i.e., above-median) villages. The point estimates suggest that in-kind transfers cause a 3 percent decline in prices while cash transfers cause a 5 percent price increase. The difference between in-kind and cash is -0.081 and significant at the 1 percent level. Meanwhile in non-remote villages, the point estimate for cash villages is wrong-signed though statistically insignificant, as seen in column 2.[30] There is a negative and sizeable price decline in in-kind villages relative to the control group, but relative to cash villages, there is no significant decline in prices in in-kind villages. In other words, the average effects for the cash-versus-in-kind effect (Table 3) are entirely driven by remote villages. Column 3 reports the interacted model using the continuous measure of remoteness.[31] The interaction of remoteness and in-kind is negative as predicted, and the interaction with cash is positive as predicted. The difference between the in-kind and cash interactions is -0.050 with a p-value of 0.02; this magnitude implies that for a 10 percent increase in driving time, prices fall by 0.5 percent more under in-kind transfers than under cash transfers.

Columns 4 to 6 examine the basic PAL goods only and we find similar patterns. The main

[30] There are more observations in the below-median subsample because there are 34 villages exactly at the sample median of 30 minutes, which we classify as below median. The results are very similar if we classify these 34 villages as above median.

[31] We find similar interaction effects using the above-median dummy measure of remoteness. We report the results with the continuous measure since it uses more of the variation in the data. At the same time, it is helpful when discussing effect sizes to show the results separately for the above- and below-median subsamples. Note that if a survey respondent reports that there is a market for fresh food in the village itself, then she is not asked the travel time to a market, so these villages are not in the regression that uses the log of travel time, but they can be classified as below-median remoteness.

differences are that, first, in the remote villages, cash transfers lead to a significant increase in prices of 6 percent and, second, in the interacted model, the difference between the in-kind and cash interactions is marginally significant (p-value of 0.08 in column 6, compared to p-value of 0.02 in column 3).

Our hypothesis is that the larger price effects in remote villages are due to supply-side differences. A second possibility is that the heterogeneity reflects demand-side differences, for example a different income elasticity of demand in remote villages, although in principle, this type of effect should net out when comparing in-kind to cash villages. In addition, the heterogeneity could be due to an omitted variable correlated with remoteness.

To sort between these explanations, we test whether the remoteness result is driven by other factors correlated with remoteness, with the results reported in Appendix Table 4. The remote villages in our sample are also poorer. Demand-side explanations would likely be due to income rather than remoteness. Thus, we first include interaction terms (and the main effect) of the median expenditure per capita in the village. Controlling for this measure of the village's income level makes the remoteness results stronger. The cash-versus-in-kind coefficient difference is -0.058 (p-value of 0.01) compared to -0.050 (p-value of 0.02) without the controls. This suggests that remoteness is not just proxying for income and makes it less likely that the cash-remoteness interaction is due to a different demand elasticity in remote villages.

Another possibility is that the larger effects are due to the aggregate transfer being bigger in remote villages. The remote villages have higher program eligibility rates, and because these villages are poorer, market size per capita is smaller. Thus, the influx from the transfer leads to a larger supply or demand shock, which could explain the larger price effects. In column 2 of Appendix Table 4, we test this possibility. Because of high program eligibility across the board, the aggregate transfer is actually not that much bigger in these villages and does not explain the heterogeneity patterns that we observe. When we control for interactions of the village-specific size of the in-kind transfer (analogous to $\Delta Supply$ reported in Table 1, but varying by village as well as good), we continue to find that more remote villages have bigger price effects. In other words, conditional on the aggregate size of the transfer into the village, transfers lead to larger price effects in remote villages. We find similar results using the aggregate size of the cash transfer as the proxy for how large the transfer into the village is, as shown in column 3.

Next, we comprehensively test whether any village-level baseline characteristics are driving the remoteness results. Columns 4 to 9 of Appendix Table 4 use the baseline village characteristics from Table 2 that are most strongly correlated with remoteness and add to the regressions these

variables and their interactions with the treatment dummies. The remoteness heterogeneity persists when these various control variables are added. Note that Appendix Table 4 reports the results using all of the PAL goods, and we find similar results when we restrict the sample to the basic PAL goods.

To recap, neither correlates of remoteness nor demand-side differences seem to explain why remote villages experience larger price effects. This points to supply-side differences in remote villages as a likely explanation. First, there could be perfect competition but steeper marginal cost curves in remote villages.[32] Second, there could be fewer stores in remote villages, so less competition. Third, the degree of imperfect competition could initially be similar in remote and non-remote villages, but there might be slower supply-side adjustment to the program in terms of firm entry and exit in remote villages.

The least likely of these explanations seems to be perfect competition. Recall from Section 2 that under imperfect competition, price effects exist when the marginal cost curve is flat, but under perfect competition, a flat marginal cost curve would imply no price effects. Since we observe prices up to two years after the PAL program is in place—when grocery shops likely are able to adjust their procurement quite easily—our evidence of price effects is suggestive of imperfect competition.

Ideally, we would use data on the market structure of grocery shops both before and after the program is in place to test if remote areas have initially fewer grocery shops so less competition or less entry and exit of stores in response to the program. Unfortunately, no store census was conducted. The best proxy we have for the degree of competition is the number of stores in which data were collected. This variable gives a lower bound on the number of grocery shops in the village and is likely correlated with the actual number of grocery shops in the village. However, it is a clearly imperfect proxy both because data were collected from at most three stores and because the number of stores is also a reflection of how thorough the data collection was in the village.

With those important caveats in mind, we test for heterogeneous price effects using this measure of the number of stores in the village. More remote villages indeed appear to have fewer stores; the correlation between remoteness and number of stores at baseline is -0.11. Table 7 presents the heterogeneity results. The prediction is that the price effects should be smaller in

[32] Another difference between remote and non-remote villages under perfect competition is that the in-kind transfer could be of higher value in remote villages because the government is subsidizing distribution and transport costs, which are higher for remote villages. This is equivalent to the cash transfer having less purchasing power in remote villages. However, this difference would predict the opposite signed effect: Remote cash villages would have a smaller income effect in purchasing power terms, leading to a smaller, not larger, gap between the in-kind and cash coefficients in these villages.

magnitude the more stores there are at baseline. The cash interaction coefficient should be negative, and the difference between the in-kind and cash interaction coefficients should be positive. In column 1, we find suggestive support for this prediction; the cash-stores interaction is negative and marginally significant, and the in-kind minus cash interaction difference is positive with a p-value of 0.14. In column 2, we simultaneously include the remoteness interactions and number-of-stores interactions; the number-of-stores interactions are similar to those in column 1 while the remoteness interactions remain significant. Columns 3 and 4 show the results using the basic PAL goods. We find similar patterns, with the most notable difference being that the difference between the in-kind and cash interactions with the number of stores is statistically significant. The fact that the price effects are more muted when there are more stores in the village is consistent with imperfect competition being part of why we observe larger price effects in remote villages. At the same time, the fact that including the number-of-stores interactions does not fully "knock out" the remoteness interactions suggests it may not be the full explanation.

In unreported results, we test whether the cash or in-kind program 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 remoteness. Thus, differential adjustment to the intervention in terms of market structure does not seem to explain the remoteness heterogeneity.

The finding that the price effects of transfers are larger in magnitude in villages that are more isolated from other villages and towns has important implications. Because remote areas tend to be poorer, the results imply that pecuniary effects will often be more pronounced in poorer areas. Thus, for transfer programs aimed at the very poorest of communities, pecuniary effects may be an important component of the total welfare impact of the program. This point likely applies to developing countries quite broadly.[33] Our results also highlight that poorer places may have supply-side imperfections which introduce a different rationale for in-kind transfers than is typically discussed: In-kind transfers could reduce the deadweight loss associated with prices being set above their first-best level by imperfectly competitive firms.

5.5 Total pecuniary effects of the PAL program

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

[33]This point may also be relevant in developed countries. For example, in the U.S., inner cities are particularly poor. Enrollment in transfer programs such as Food Stamps and WIC is high, and these neighborhoods are often characterized as having few grocery stores (imperfect competition). Transportation costs to other neighborhoods are often high (for example because of low car ownership), causing these markets to also be relatively closed (Talukdar, 2008). These factors suggest that there could be important pecuniary effects of transfer programs in these neighborhoods.

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.[34]

For the non-PAL goods, we do not find that food prices fell in in-kind villages relative to cash villages (Table 8, column 1). We find a small positive point estimate in cash villages, similar to the point estimate for the PAL items. When we examine the results for the remote and non-remote subsamples, we find that the predicted patterns are seen more strongly for remote villages (column 2), though the results are insignificant.

The estimated price effects for the PAL goods reported in Table 3 combined with the results for non-PAL goods in Table 8 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 results for non-PAL goods suggest that the point estimates from these calculations might be noisily estimated, so we also present confidence intervals.

We begin with the PAL goods. Recipients' counterfactual expenditures on the items in the in-kind bundle was on average 206 pesos per household per month, which we calculate using the control villages at follow-up. The nominal value of the in-kind bundle was, coincidentally, also 206 pesos. (Good by good, the value of the transfer is sometimes less than counterfactual expenditure and sometimes more, but summing across goods, the totals are the same.) Thus, recipient households did not receive any additional pecuniary transfer due to price changes for the PAL goods in the in-kind villages. 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 program recipients. Non-recipient households spent 206 pesos a month on the food items contained in the PAL bundle, and the 3.7 percent price decrease in in-kind villages (Table 3, column 1) represents a transfer of 7.6 pesos (206*0.037) for every non-recipient household that is a pure consumer of these items. 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.

[34]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.

The total pecuniary effect of the program also includes the effects on non-PAL food items. Expenditure on the non-PAL items was 1096 pesos per month in the control villages. The 1 percent price increase for in-kind transfers (Table 8, column 1) is thus equivalent to a -10.96 peso transfer to a consumer (program recipients and non-recipients alike), and the 0.9 percent 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 negligible implications for households (equivalent to 0.2 pesos). Thus, our first conclusion from this calculation is that, overall, the price effects of the PAL program do not have important implications for households' purchasing power.

The story is fairly different for the subsample of more remote villages. Here, the pecuniary effects are economically important. Doing the same calculation as above but for the remote subsample, we find that the price effects of in-kind transfers are equivalent to adding an extra 1 peso in indirect transfers for a consumer household, and for the cash transfers, the price inflation is equivalent to subtracting 60 pesos from the direct transfer. This large effect is driven by the 4 percent across-the-board increase in food prices in cash villages (Table 8, column 2). Thus, via the channel of price effects, in-kind transfers deliver considerably more—61 pesos—to the average consumer household in a remote village than cash transfers do. The p-value on this point estimate of 61 pesos is 0.15, and the 95% confidence interval is -136.32 to 7.33 pesos.[35] The 61 peso wedge is equivalent to 40 percent of the direct transfer. Meanwhile, for producer households, cash transfers deliver a substantially larger pecuniary benefit than in-kind transfers do. There are also many other considerations such as administrative costs and paternalistic objectives that factor into the cash-versus-in-kind policy decision, but in physically isolated villages, price effects would appear to be important in the decision, given their magnitude.

When estimating price effects, we weight each good equally because prices are measured in the same way across goods and the precision of the measure is unrelated to whether the good is consumed in small or large quantities. However, if there are heterogeneous effects by good, then a preferred way to estimate the total pecuniary effect might be to weight each good by its budget share, analogous to how a consumer price index is calculated. Thus, we also conduct this alternative calculation.[36] Here we estimate that the extra indirect transfer to a consumer household from in-kind relative to cash transfers is 20 pesos with a p-value of 0.53.[37]

[35]The p-value and confidence intervals are calculated by bootstrapping the estimation process. We re-draw villages, with replacement, 10,000 times and re-calculate the various inputs into this calculation such as the baseline expenditures and regression coefficients.

[36]Appendix Table 5 shows regression results using this weighting.

[37]For non-remote villages, the unweighted point estimate is that the indirect transfer from in-kind relative to cash

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 program 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.[38]

Unfortunately, the quality of the data on agricultural production is not ideal. For example, 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. Thus, we regard the results below as tentative but still providing suggestive evidence on the distributional effects of transfer programs.

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

$$FarmProfits_{hv} = \alpha + \beta_1 InKind_v + \beta_2 Cash_v + \phi FarmProfits_{hv,t-1} + \varepsilon_{hv}. \quad (8)$$

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 program might lead to more investment and increased production. This channel would cause an increase in profits for both the cash and in-kind treatments (unless the investments are very long-run). 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 9, we find, as predicted, a positive coefficient on *Cash*. Farm profits are higher by 186 pesos in villages where households received cash transfers (and hence where food prices rose). We find that the in-kind program 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.

transfers is -37 pesos with a p-value of 0.11, while the CPI-weighted estimate is that this relative indirect transfer is 0.5 pesos with a p-value of 0.98.

[38] 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.

Higher food prices will raise agricultural profits simply because revenues increase holding quantities fixed, but higher prices will 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 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. Somewhat surprisingly, 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 program, 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 percent of households meet this criterion. We first examine heterogeneity in the program impacts on total expenditures per capita, which serves as a proxy for household welfare and is meant to capture the total program effect for the household:

$$\begin{aligned} ExpendPC_{hv} &= \alpha + \theta_1 Producer_h \times InKind_v + \theta_2 Producer_{hv} \times Cash_v \\ &\quad + \beta_1 InKind_v + \beta_2 Cash_v + \rho Producer_{hv} + \phi ExpendPC_{hv,t-1} + \varepsilon_{hv} \end{aligned} \tag{9}$$

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. While the results (column 3) are imprecise, they 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).

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-value = 0.06) and cash transfers, relative to in-kind transfers, are more helpful for producers (p-value = 0.13).

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 paper 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 program for the poor in Mexico, the Programa de Apoyo Alimentario (PAL).

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 PAL program eligibility is high and the transfers are large—that is, the program injects a large quantity of food or cash into these villages—this finding suggests that in most settings, price effects will have quite negligible consequences for policy decisions.

Many of the poorest people in the world live in remote villages, and here our results suggest a different story. We empirically verify the hypothesis that price effects are bigger in more physically remote areas, where the markets are less tied to world prices and there is less product market competition. Moreover, in remote villages, the price effects we estimate are economically significant. In villages with above-median travel time to a large market, the difference in the price effects between in-kind and cash transfers is equivalent to an indirect transfer of 60 pesos per month for a consumer household, or about 30 to 40 percent of the direct transfer. While the more remote half of villages in our sample are particularly remote by Mexico's standards, in many other low-income countries, much of the population lives at this (or a higher) level of remoteness.

Our finding that the price effects are particularly pronounced for geographically isolated villages is consistent with these villages being less open to trade and having less market competition. While we cannot decisively test between these explanations, we find suggestive evidence that imperfect competition is part of the explanation. Moreover, the fact that the price effects persist

almost two years after the program is in place is also suggestive that imperfect competition may be at play; even if marginal costs are flat in the long run, with imperfect competition there could be long-run price effects of in-kind transfers since the residual demand facing local suppliers would be permanently lower. The dearth of supply-side competition in remote markets 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 the inefficiency associated with imperfect competition and strategic undersupply.

Our results are also suggestive that agricultural profits increase in cash villages (where food prices rose) more than in in-kind villages (where prices fell). This effect is due both to the change in the price of goods sold, but also to households responding by producing more when the price of what they produce increases. This evidence of local agricultural producers adjusting their supply raises the question of whether there would be further adjustments over time. Similarly, adjustments by grocery shops in the long run would also affect whether and how long price effects persist. We leave this question of long run effects for future work since the experimental design and available data do not allow for such an analysis in this setting.

Of course, 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. Another key consideration is how efficiently the government can provide supply. It could still be the case that an uncompetitive private sector creates more surplus than if the government were to enter the market; the government's productive efficiency could be considerably lower than the private sector's. In that case, the best way for the government to alleviate supply constraints in poor villages while providing income support to households might be cash transfers combined with alternative supply-side policies.

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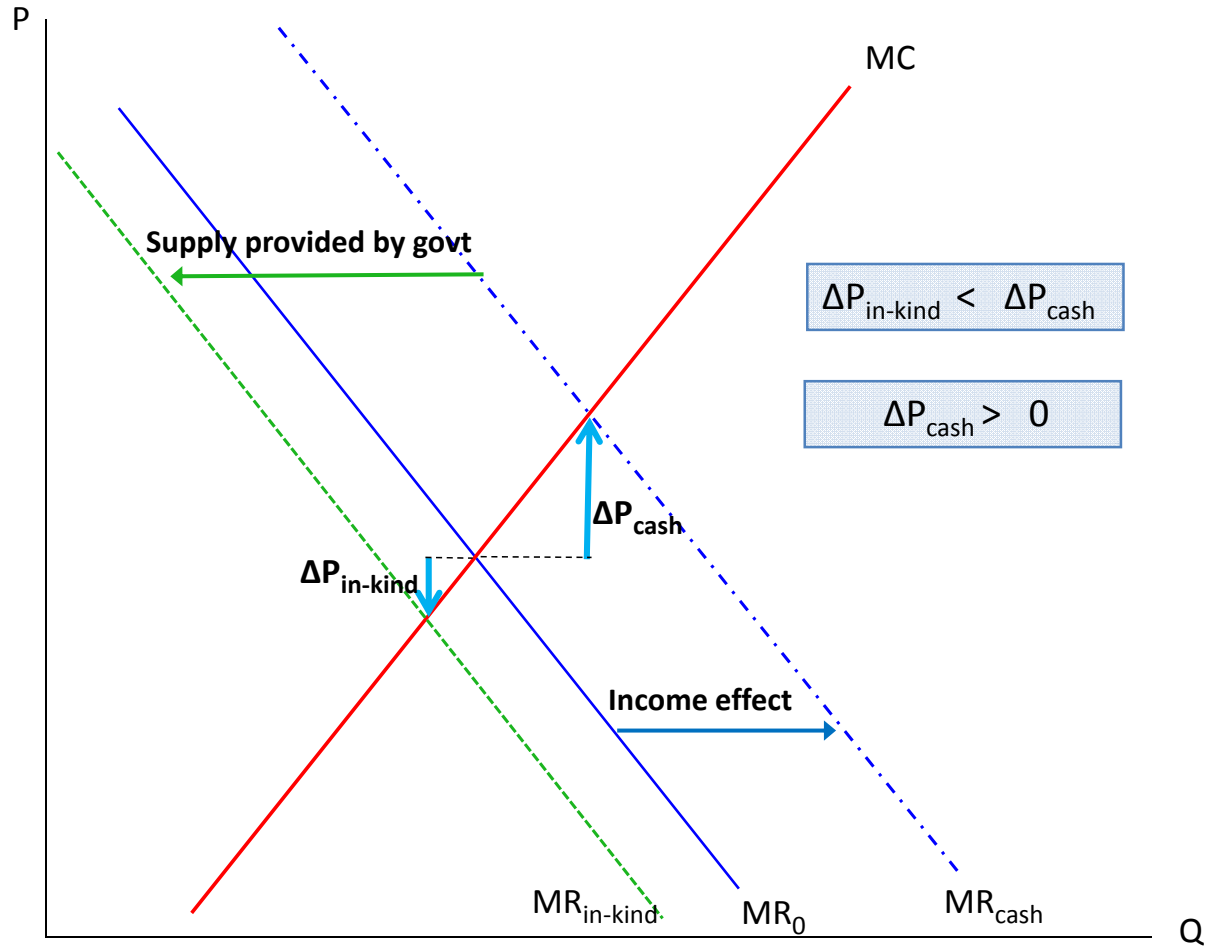
Figure 1: Effect of cash and in-kind transfers on prices



An in-kind transfer has two effects, an increase in the demand facing local suppliers due to an income effect, and a decrease in demand facing local suppliers because the government meets some of consumers' demand via its transfer. The net effect is that the marginal revenue curve shifts from MR to $MR_{in-kind}$. A cash transfer has only the income effect, and the marginal revenue curve shifts to MR_{cash}.

Table 1: Summary of PAL food box

Item	Type	Amount per box (kg)	Value per box (pre-program, 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 pre-treatment village-level median unit values. 10 pesos ≈ 1 USD.

(2) ΔSupply measures the PAL supply influx into villages, relative to what would have been consumed absent the program. 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 (0.35kg) or canned sardines (0.8kg); the analysis assumes the mean weight and calories throughout.

(4) Biscuits are excluded from our analysis as post-program prices are missing.

Table 2: Baseline characteristics by treatment group

	Control	In-kind	Cash	(1)=(2) p-value	(1)=(3) p-value	(2)=(3) p-value	(1)=(2)=(3) 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)				
Observations (good level)	282	576	306				
			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)				
Observations (good level)	423	864	459				
			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)				
Observations (good level)	2820	5760	3060				
			Village level characteristics				
Diconsa store in the village	0.26	0.45	0.39	0.03**	0.16	0.51	0.08*
	(0.071)	(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)				
Median months in which transfers were received	--	13.21	12.96	--	--	0.52	--
		(0.224)	(0.305)				
Observations (village level)	47	96	51				
			Household level characteristics				
Monthly per capita expenditure (pesos)	570.48	535.06	529.51	0.31	0.26	0.85	0.50
	(29.02)	(18.89)	(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) Producer 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 standard deviations 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-treatment 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 1-4 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) The outcome variable in columns 5-6 is the difference between the normalized post-treatment price (the outcome in columns 1-4) and the lagged normalized unit value (the baseline price measure in columns 1-2).

(4) All regressions include an indicator for imputed pre-program prices (see text).

(5) Standard errors are clustered at the village level. 194 villages.

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 x 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 x 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. x In-kind - High quality var. x Cash**	-0.008	-0.005	0.005	-0.007	0.011	-0.006
***H_0 : High quality var.* x In-kind = High quality var. x 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-treatment 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 are clustered at the village level.

(2) Regressions control for the pre-period normalized unit value and an indicator for imputed pre-program 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.

Table 5: Price effects based on duration of intervention

	All PAL goods		Basic PAL goods only	
Outcome =	price	price	price	price
	(1)	(2)	(3)	(4)
In-kind	-0.031	-0.056**	-0.038	-0.056
	(0.022)	(0.026)	(0.031)	(0.035)
In-kind x Above median length of treatment	-0.021	-0.011	-0.022	-0.018
	(0.034)	(0.035)	(0.040)	(0.043)
Above median length of treatment	0.004	-0.002	0.018	0.013
	(0.028)	(0.029)	(0.033)	(0.035)
In-kind x ln(Remoteness)		-0.047**		-0.037
		(0.022)		(0.023)
ln(Remoteness)		0.036**		0.039**
		(0.015)		(0.016)
Observations	1,818	1,665	1,258	1,150

Notes: *** p<0.01, ** p<0.05, * p<0.1

(1) The outcome variable is the post-treatment 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 are clustered at the village level.

(2) Regressions control for the pre-period normalized unit value and an indicator for imputed pre-program prices (see text).

(3) Length of treatment is defined as the village median number of months for which transfers were receieved prior to the follow-up survey.

(4) Remoteness is defined as the time required to travel to a larger market that sells fruit, vegetables, and meat. It is constructed as the village median of household self-reports. It is missing (though below median) if no household in the village reports leaving the village to purchase these foods.

Table 6: Heterogeneous price effects based on remoteness of the village

	All PAL goods			Basic PAL goods only		
	Above-median remoteness	Below-median remoteness	All villages	Above-median remoteness	Below-median remoteness	All villages
Outcome =	price	price	price	price	price	price
	(1)	(2)	(3)	(4)	(5)	(6)
In-kind	-0.030	-0.044*	-0.050	-0.014	-0.045*	-0.033
	(0.033)	(0.024)	(0.030)	(0.027)	(0.027)	(0.031)
Cash	0.050	-0.029	0.013	0.062**	-0.015	0.032
	(0.034)	(0.031)	(0.031)	(0.031)	(0.038)	(0.036)
ln(Remoteness) x In-kind			-0.028			-0.007
			(0.033)			(0.036)
ln(Remoteness) x Cash			0.023			0.033
			(0.033)			(0.037)
Observations	865	1,470	2,130	603	1,014	1,471
***Effect size:* In-kind - Cash**	**-0.081*** **	**-0.015**		**-0.076*** **	**-0.030**	
***H_0:* In-kind = Cash (p-value)**	**0.00**	**0.56**		**0.00**	**0.35**	
***Effect size:* ln(Remoteness) x In-kind - ln(Remoteness) x Cash**			**-0.050****			**-0.040***
***H_0:* ln(Remoteness) x In-kind = ln(Remoteness) x Cash (p-value)**			**0.02**			**0.08**

Notes: *** p<0.01, ** p<0.05, * p<0.1

(1) The outcome variable is the post-treatment 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 are clustered at the village level.

(2) Regressions 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-program prices (see text).

(3) Remoteness is defined as the time required to travel to a larger market that sells fruit, vegetables, and meat. It is constructed as the village median of household self-reports. It is missing (though below median) if no household in the village reports leaving the village to purchase these foods.

Table 7: Heterogeneous price effects based on supply-side competition

	All PAL goods		Basic PAL goods only	
Outcome =	price	price	price	price
	(1)	(2)	(3)	(4)
In-kind	0.010	-0.003	-0.027	-0.033
	(0.052)	(0.059)	(0.055)	(0.064)
Cash	0.100	0.113*	0.092	0.110
	(0.063)	(0.067)	(0.066)	(0.072)
# stores x In-kind	-0.021	-0.023	0.001	0.001
	(0.022)	(0.022)	(0.026)	(0.026)
# stores x Cash	-0.047*	-0.048*	-0.037	-0.038
	(0.025)	(0.025)	(0.028)	(0.027)
ln(Remoteness) x In-kind		-0.030		-0.012
		(0.038)		(0.041)
ln(Remoteness) x Cash		0.022		0.029
		(0.038)		(0.042)
Observations	1,998	1,998	1,375	1,375
***Effect size:* In-kind - Cash**	**-0.090***	**-0.117****	**-0.120****	**-0.144*****
***H_0:* In-kind = Cash (p-value)**	**0.05**	**0.01**	**0.02**	**0.00**
***Effect size:* # stores x In-kind - # stores x Cash**	**0.026**	**0.025**	**0.038***	**0.038****
***H_0: # stores* x In-kind = # stores x Cash (p-value)**	**0.14**	**0.17**	**0.05**	**0.04**
***Effect size:* ln(Remoteness) x In-kind - ln(Remoteness) x Cash**		**-0.052****		**-0.041***
***H_0:* ln(Remoteness) x In-kind = ln(Remoteness) x Cash (p-value)**		**0.02**		**0.08**

Notes: *** p<0.01, ** p<0.05, * p<0.1

(1) The outcome variable is the post-treatment 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 are clustered at the village level.

(2) Regressions 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-program prices (see text).

(3) Remoteness is defined as the time required to travel to a larger market that sells fruit, vegetables, and meat. It is constructed as the village median of household self-reports. The number of stores is the number of stores included in the baseline price survey; a maximum of three stores were surveyed per village.

Table 8: Price effects for non-PAL goods

	All non-PAL goods			
	All villages	Above-median remoteness	Below-median remoteness	All villages
Outcome =	price	price	price	price
	(1)	(2)	(3)	(4)
In-kind	0.010	0.000	0.014	-0.005
	(0.019)	(0.029)	(0.024)	(0.023)
Cash	0.009	0.039	-0.012	0.013
	(0.022)	(0.042)	(0.023)	(0.034)
ln(Remoteness) x In-kind				-0.022
				(0.028)
ln(Remoteness) x Cash				0.014
				(0.032)
Observations	10,648	3,765	6,883	9,698
***Effect size:* In-kind - Cash**	**0.001**	**-0.039**	**0.026**	
***H_0:* In-kind = Cash (p-value)**	**0.95**	**0.34**	**0.24**	
***Effect size:* ln(Remoteness) x In-kind - ln(Remoteness) x Cash**				**-0.036**
***H_0:* ln(Remoteness) x In-kind = ln(Remoteness) x Cash (p-value)**				**0.27**

Notes: *** p<0.01, ** p<0.05, * p<0.1

(1) The outcome variable is the post-treatment 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 are clustered at the village level.

(2) Regressions include all 51 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-program prices (see text).

(3) Remoteness is defined as the time required to travel to a larger market that sells fruit, vegetables, and meat. It is constructed as the village median of household self-reports.

Table 9: 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**	0.064		-0.040	
	(106.082)	(140.378)	(0.052)		(0.106)	
Producer x In-Kind			0.001	-0.018	0.077	0.055
			(0.060)	(0.046)	(0.115)	(0.088)
Producer x 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***	**0.050**		**0.124**	
***H 0 :* In-kind = Cash (p-value)**	**0.67**	**0.08**	**0.25**		**0.20**	
***Effect size:* Producer x In-Kind - Producer x Cash**			**-0.086**	**-0.033**	**-0.189**	**-0.174***
***H 0 :* Producer x In-Kind = Producer x 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 are clustered at the village level.

(2) Profits and costs are measured in pesos and they are for the preceeding year; samples are trimmed of outliers greater than three standard deviations above the median.

(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.

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, i.e., $\frac{\partial p}{\partial Q} < 0$.

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, e.g., $\partial X > 0$, 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, e.g., $Q = X - \alpha p$.

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 B. Data appendix

VARIABLE CONSTRUCTION

Post-program prices

Post-program 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 liters (for liquids) using conversion factors supplied by the Mexican government for non-standard units (e.g., a "small" loaf of bread weighs 0.68kg). In most specifications, post-program prices are normalized by the good-specific mean in the control group.

Pre-program prices

The main measure of the pre-program 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-program price using the median pre-program 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 percent of village-good observations; among basic PAL goods, we impute 14 percent of village-good observations.

An alternative pre-program 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-program, we impute this measure using the village median unit-value (19 and 16 percent 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 percent of observations for all PAL goods and basic PAL goods, respectively). For both of these measures of pre-program 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.

Remoteness measure

Remoteness is constructed from household self-reports on the time it takes to travel to the nearest market where fresh fruit, vegetables, and meat are sold. 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. *Remoteness* is the village median among households that report leaving the village to purchase fresh foods. In some specifications, we split the sample into villages that are above and below the median of *Remoteness*. The sample median is 30 minutes, and twenty percent of villages are at this median value. We classify this twenty percent of villages as being below the median.

Good- or village-specific influx of in-kind goods ($\Delta Supply$)

$\Delta Supply$ is a ratio that measures the size of the supply influx of in-kind goods into program villages, relative to what would have been consumed in the absence of the PAL program. In the robustness check reported in Appendix Table 4, it is constructed as 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.

Number of stores in a village

The number of stores in a village is identified from the number of stores that were surveyed for prices in the baseline survey. Survey protocol had enumerators survey all stores in a village, up to a maximum of three stores. The variable is truncated at 3 stores for larger villages, but we do not know the extent of this truncation. Conversely, some of the villages with fewer than 3 stores in the data actually had at least 3 stores, in all likelihood; according to program administrators, the number of stores surveyed is also a reflection of the completeness of the data collection and could be an underestimate even when the number is below 3.

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 61 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 26 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. We also use the median village monthly per capita expenditure at baseline as a control variable in Appendix Table 4.

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 percent stated that farm costs were zero, and 85 percent 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 61 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 qualitative surveys of shopkeepers in 20 villages in the spring of 2011 in the states of Veracruz, Oaxaca, and Puebla. Eleven of the villages were PAL experimental villages and another five are villages that were incorporated into the program in the past two years. A research assistant interviewed several shopkeepers per village (Diconsa and non-Diconsa) in these 16 villages. One of the coauthors (Jayachandran) conducted similar interviews with shopkeepers in the other 4 villages, which were poor, rural villages but not part of the PAL program. Shopkeepers were asked how they procured supply, how they responded to unexpected changes in demand, when they adjusted prices, whether prices varied by customer (i.e., price discrimination), why they did not stock more inventory, and other questions related to the market structure and pricing.

Appendix Table 1: 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 2: Main specification separately by PAL good

Outcome =	Corn flour price	Rice price	Beans price	Fortified powdered milk price	Packaged pasta soup price	Vegetable oil price	Lentils price	Canned tuna / sardines price	Breakfast cereal 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 - Control**	**-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-treatment 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 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-program prices (see text).

Appendix Table 3: Price effects of in-kind and cash transfers, alternative specifications

							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
Outcome =	price	price	price	price	ln(price)	ln(price)	price	price	price	price
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
In-kind	-0.037*	-0.033	-0.031*	-0.022	-0.037	-0.015	-0.032	-0.017	-0.027	-0.018
	(0.020)	(0.020)	(0.016)	(0.016)	(0.025)	(0.022)	(0.022)	(0.023)	(0.020)	(0.021)
Cash	0.002	0.014	0.003	0.017	0.006	0.039	0.002	0.015	0.014	0.018
	(0.023)	(0.026)	(0.019)	(0.020)	(0.028)	(0.026)	(0.023)	(0.027)	(0.023)	(0.025)
Lagged normalized unit value	0.034	0.128***					0.025	0.149***	0.022	0.091**
	(0.021)	(0.042)					(0.029)	(0.056)	(0.021)	(0.043)
Lagged normalized store price			0.429***	0.493***						
			(0.077)	(0.118)						
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	1,729	1,197	1,767	1,217
***Effect size:* In-kind - Cash**	**-0.038****	**-0.047****	**-0.034****	**-0.039****	**-0.044****	**-0.054****	**-0.034***	**-0.032**	**-0.040****	**-0.036***
***H_0:* In-kind = Cash (p-value)**	**0.03**	**0.04**	**0.01**	**0.01**	**0.04**	**0.02**	**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-treatment 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-treatment store price, with no normalization. Standard errors 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-program prices; those in columns 3-4 include two such indicators (see text).

Appendix Table 4: Heterogeneous effects by remoteness of village: Robustness checks amongst all PAL goods

Cavariate =	ln(Village expenditure)	ΔSupply	ΔSupply, imputed	% of households who produce food	% of indigenous households	% households with dirt floors	% households with running water	Village mean asset index	Village has a Diconsa store
Outcome =	price	price	price	price	price	price	price	price	price
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
ln(Remoteness) x In-kind	-0.028	-0.021	-0.027	-0.024	-0.026	-0.020	-0.026	-0.022	-0.028
	(0.034)	(0.031)	(0.033)	(0.032)	(0.031)	(0.032)	(0.034)	(0.035)	(0.034)
ln(Remoteness) x Cash	0.031	0.021	0.020	0.048	0.017	0.026	0.029	0.029	0.013
	(0.034)	(0.032)	(0.033)	(0.036)	(0.031)	(0.033)	(0.034)	(0.035)	(0.034)
Covariate x In-kind	-0.002	-0.002	-0.001	-0.021	-0.050	-0.064	-0.002	0.009	0.030
	(0.068)	(0.002)	(0.001)	(0.070)	(0.065)	(0.085)	(0.056)	(0.018)	(0.063)
Covariate x Cash	0.052	-0.005	-0.000	-0.127	-0.007	-0.033	0.025	0.010	0.022
	(0.072)	(0.003)	(0.001)	(0.123)	(0.075)	(0.105)	(0.066)	(0.023)	(0.068)
Observations	2,130	1,956	2,130	2,130	2,130	2,130	2,130	2,130	2,130
***Effect size:* ln(Remoteness) x In-kind - ln(Remoteness) x Cash**	**-0.058****	**-0.042****	**-0.048****	**-0.072****	**-0.043***	**-0.045***	**-0.055****	**-0.050****	**-0.041****
***H_0:* ln(Remoteness) x In-kind = ln(Remoteness) x Cash (p-value)**	**0.01**	**0.03**	**0.02**	**0.01**	**0.06**	**0.07**	**0.01**	**0.03**	**0.04**

Notes: *** p<0.01, ** p<0.05, * p<0.1

(1) The outcome variable is the post-treatment 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 are clustered at the village level.

(2) All regressions 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-program prices (see text).

(3) Remoteness is defined as the time required to travel to a larger market that sells fruit, vegetables, and meat. It is constructed as the village median of household self-reports.

(4) Village expenditure is the median per capita household value of all non-durable items (food and non-food) consumed in the preceding month.

(5) ΔSupply is constructed as the total amount of a good transferred to the village (or the amount that would have been transferred in cash or control villages had they received in-kind transfers), divided by the amount of the good consumed pre-program. In some cases, certain goods were not consumed at all in the village pre-program; for these village-goods, we impute pre-program consumption as one-half the minimum village consumption of that good in the sample.

(6) Producer households are those that, at baseline, either auto-consume their production or report planting or reaping produce or grain or raising animals.

(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.

Appendix Table 5: Price effects of in-kind and cash transfers, weighting observations by expenditure share

	Observations weighted by expenditure share					
	All PAL goods			Non-PAL goods		
	All villages	Above-median remoteness	Below-median remoteness	All villages	Above-median remoteness	Below-median remoteness
Outcome =	price	price	price	price	price	price
	(1)	(2)	(3)	(4)	(5)	(6)
In-kind	-0.037*	-0.018	-0.048*	0.003	0.009	0.002
	(0.020)	(0.028)	(0.028)	(0.017)	(0.026)	(0.021)
Cash	-0.004	0.037	-0.030	0.008	0.018	0.006
	(0.022)	(0.034)	(0.030)	(0.020)	(0.032)	(0.025)
Lagged normalized unit value	0.101***	0.119**	0.087***	0.179***	0.213***	0.154***
	(0.030)	(0.048)	(0.032)	(0.039)	(0.072)	(0.045)
Observations	2,335	865	1,470	10,648	3,765	6,883
***Effect size:* In-kind - Cash**	**-0.033****	**-0.055***	**-0.018**	**-0.004**	**-0.009**	**-0.004**
H_0: In-kind = Cash (p-value)	**0.05**	**0.07**	**0.37**	**0.80**	**0.77**	**0.86**

Notes: *** p<0.01, ** p<0.05, * p<0.1

(1) The outcome variable is the post-treatment 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 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) Expenditure shares are calculated using consumption of the control group post-program.

(4) All regressions include an indicator for imputed pre-program prices (see text).

(5) Standard errors are clustered at the village level. 194 villages.

Appendix Table 6: Household expenditure in cash villages, class attendees versus non-attendees.

	Cash villages only							
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

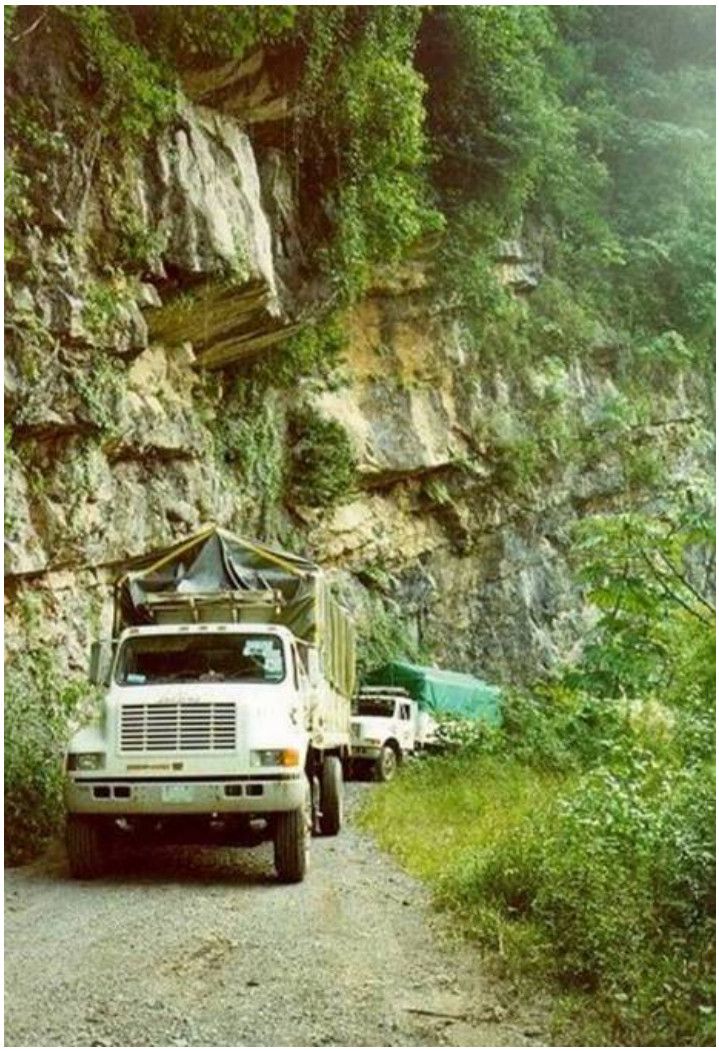
(1) Observations are at the household level. Sample includes all PAL eligible households in cash villages.

(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 clustered at the village level.

Appendix Figure 1: Trucks transporting PAL boxes



Appendix Figure 2: PAL box of food

Appendix Figure 3: Unloading PAL boxes in the village

Appendix Figure 4: Grocery shops in PAL villages

Appendix Figure 5: Villages in the PAL experiment