The Price Effects of Cash Versus In-Kind Transfers*

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Abstract

This paper examines how cash and in-kind transfers into small, partially-closed economies (villages) affect prices. Cash transfers increase the demand for normal goods, causing prices to rise. In-kind transfers generate a similar increase in demand, but they also increase supply (if the goods themselves rather than vouchers are provided). Hence, relative to cash transfers, in-kind transfers should lead to lower prices, which shifts surplus from producers to consumers. Prices should also fall for substitutes of the in-kind goods. We test and find support for these predictions using a transfer program for poor households in rural Mexico that randomly assigned villages to receive in-kind food transfers, equivalently-valued cash transfers, or no transfers. The estimated price effects are quite large in magnitude: the price decline in in-kind villages increases the program's net transfer by 12 percent for a recipient who is a consumer of food. The price increase in cash villages dissipates 11 percent of the transfer. We also find that the pecuniary effects are larger in more remote villages where there is less competition among sellers and the economy is less open.

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1 Introduction

Government transfer programs can have important price effects in addition to their direct effect of increasing recipients' income. Cash transfers increase the demand for normal goods, and if supply is not perfectly elastic, the price 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 can increase local supply.[1] If the government injects supply into a partially-closed local economy (e.g., a village), then relative to cash transfers, prices should fall when transfers are provided in-kind.

These pecuniary effects shift wealth between producers and consumers. With a cash transfer, the price increase for normal goods hurts consumers and favors producers. With in-kind transfers, the additional price decrease from the supply influx helps consumers at the expense of producers. For example, a transfer of packaged food (the in-kind transfer we study in this paper), should result in a lower price for the packaged food in the local economy, relative to a cash transfer. If the poor are net consumers of these goods, then in-kind transfers, via their price effect, will increase the overall transfer to the poor more than cash transfers will.

When there is perfect competition among local producers, these effects are pecuniary externalities. However, if there is imperfect competition among local suppliers—and prices are above the first-best level—then the lower prices induced by in-kind transfers could also reflect an increase in efficiency. In addition, a further effect of the lower prices is that they encourage consumption of the in-kind goods (for both program recipients and non-recipients); if boosting consumption of these items was precisely the paternalistic motive for using in-kind transfers, then the price effects will reinforce the program's goals.

Most of the world's poor live in rural, often isolated villages. In these partially-closed economies, not characterized by the infinitely elastic supply of small open economies, large transfer programs are likely to have quantitatively important price effects. The pecuniary effect of in-kind transfers could be regarded as a useful policy lever, a second-best way to tax producers and redistribute to consumers (Coate, Johnson, and Zeckhauser, 1994). However, the more often cited rationales for in-kind transfers are paternalism, i.e., the government wants to encourage consumption of certain goods (Besley, 1988), and self-targeting, whereby

[1]Transfers can also take the form of vouchers, as in the U.S. Food Stamp program. 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, rather than providing vouchers, e.g food provision or public housing. In addition, the type of transfer we consider is one in which the supply is sourced from outside the economy that receives the transfer.

in-kind transfers cause the less needy to self-select out of the program (Coate, 1989; Besley and Coate, 1991). In this case, the pecuniary effects are an unintended consequence, but one that might significantly enhance or diminish the program goal of assisting the poor.

This paper tests for price effects of in-kind transfers versus cash transfers and compares both to the status quo of no transfers. We study a large food assistance program for the poor in Mexico, 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 in-kind transfers of packaged food (rice, beans, vegetable oil, canned fish, etc.); in other villages they received a similarly valued cash transfer; and the third set of villages served as a control group.

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 schedule outward. The in-kind transfer that we study was designed to be of the same value as the cash transfer, so in the in-kind villages, the income effect 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, according to the standard demand-supply framework. This in-kind-versus-cash estimate is relevant to policy makers deciding whether to provide transfers in kind or as cash. Using panel data (pre- and post-program) from households and food stores in the experimental villages, we find support for these predictions.

Furthermore, the pecuniary effects of transfers are not restricted to just the transferred items. A cash transfer should affect demand for all goods (there are no "transferred items" in this case). In addition, the supply effect of an in-kind transfer should dampen demand and lead to lower prices for goods that are substitutes of the in-kind items. Other food items are the most obvious substitutes for the PAL food items, and we find that prices for these goods fell in the in-kind villages, relative to the cash villages. Meanwhile, cash transfers appear to have caused an increase in overall food prices.

The price effects we find are large in magnitude. For in-kind transfers, the price effect represents an additional indirect benefit for a consumer equal to 12 percent of the direct benefit.[2] The price increase caused by cash transfers offsets the direct transfer by 11 percent,

[2]We multiply our estimated coefficients for the price change in the cash and in-kind villages, relative to the control villages, by average consumption in the control group. For the program participants in the in-kind villages, we net out the quantities given to them for free. The price effects apply to all households, not just program recipients.

though this effect is imprecisely estimated. Choosing in-kind rather than cash transfers in this setting, hence, generates extra indirect transfers to the poor that are worth 23 percent of the direct transfer itself.

For a producer, these welfare effects are of course reversed. The items provided in-kind are procured from outside the recipient villages, but households that grow crops, which are substitutes for the in-kind goods, see the price of their products fall. We find in the data that the net welfare effects of cash (in-kind) transfers indeed seem to be relatively higher (lower) for agricultural households than non-agricultural households.

Finally, we examine how these price effects differ depending on how physically isolated the village is. First, isolated villages are typically less integrated with the world economy, so local supply and demand should matter more in the determination of prices. Second, there is likely to be less competition on the supply side (i.e., among grocery stores) in remote villages, so prices will be more responsive to transfers than if the market were perfectly competitive. For both of these reasons, the price effects of transfers should be more pronounced in remote villages. We confirm this prediction, and we also find suggestive evidence that the driving factor is the lack of supply-side competition in remote villages. Since poorer villages are also typically more isolated (World Bank, 1994), these findings suggest that transfer programs targeting the very poor inherently may have important pecuniary effects.

This paper is related to several areas of research. First, there is an extensive literature comparing in-kind to cash transfers.[3] In addition to the theoretical work cited above, there is empirical evidence on how in-kind transfers affect consumption patterns (Moffitt, 1989; Hoynes and Schanzenbach, 2009), including for the PAL program in Mexico (Skoufias, Unar, and Gonzalez-Cossio, 2008; Cunha, 2010). Other work examines whether in-kind transfers are effective at self-targeting (Reeder, 1985; Currie and Gruber, 1996; Jacoby, 1997). Fewer studies provide evidence on the question this paper addresses, namely the price effects of in-kind transfers (Murray, 1999; Finkelstein, 2007). Second, our work is related to the literature on equilibrium effects of social programs (Lise, Seitz, and Smith, 2004; Angelucci and De Giorgi, 2009; Attanasio, Meghir, and Santiago, 2009). Finally, we add to the evidence on price effects in isolated localities in developing countries (Jayachandran, 2006; Donaldson, 2009).

Section 2 of this paper lays out the theoretical predictions. Section 3 describes Mexico's PAL program and our data. Section 4 presents the empirical strategy and results. Section 5 offers concluding remarks.

[3] Currie and Gahvari (2008) provide an excellent review of this literature.

2 Conceptual Framework

In this section, we use a basic supply and demand framework to discuss how cash and in-kind transfers should affect prices. We do not present a formal model but instead informally derive the predictions that we take to the data.

We begin by describing the case where local suppliers are perfectly competitive and then discuss imperfect competition below. 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. When the supply curve is positively sloped and quantity is increasing in price, shifts in the demand for or supply of a good will affect its price (as well as those of substitutes and complements).

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 the items from outside the village. In effect, we are focusing on the short-run equilibrium of the market, where we assume that local suppliers cannot adjust capacity instantaneously and procuring more supply entails increasing marginal costs. The remoteness of the villages (i.e., high transportation costs to other markets) is one reason that inventory in local stores is unlikely to adjust instantaneously; for example, to meet higher demand, a shopkeeper might need to travel to a neighboring village to buy supply from a store there. In the long run, one might expect the supply curve to be flatter; at the end of the section, we discuss in more detail how the market would likely adjust in the longer run.

Figure 1 depicts the market for a normal good in a village. The figure shows the effect of a cash transfer: the demand curve shifts to the right via an income effect, and the equilibrium price, p, increases.[4] Denoting the amount of money transferred in cash by X_{Cash}, our first prediction is, thus, 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 define the in-kind transfer amount X_{InKind} in terms of its equivalent cash value.[5] Thus

[4]The demand curve also might become steeper if higher-income individuals are less price elastic, but this effect is not important for our purposes. Also note that for inferior goods, demand will shift to the left with the opposite price effect. We focus on normal goods for brevity. In related ongoing work, we formally estimate the income elasticities of the goods in our data and our results confirm the validity of the normality assumption. See also Attanasio, DiMaro, Lechene, and Phillips (2009).

[5]The simplest case to consider is if either the transfer is inframarginal (that is, it is less than the household

the demand shift caused by a transfer amount X is by definition the same for either form of transfer.[6] With an in-kind transfer, however, there is also a shift in the supply curve. For a transferred good, supply shifts to the right by the quantity added to the local economy, as shown in Figure 2. While the net price effect of an in-kind transfer relative to the original market equilibrium is theoretically ambiguous in general, one can sign the price effect of in-kind transfers relative to cash transfers.[7] For transferred goods, the price should be lower under in-kind transfers:

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

In our empirical application, we examine the predictions above in two ways. First, we compare villages that received different forms of transfers (extensive margin) and, second, for Prediction (2) we compare different goods that were transferred in-kind in larger versus smaller amounts (intensive margin).

Imperfect competition

Predictions (1) and (2) also hold in the case of imperfect competition. This can be seen most clearly for the case of a cash transfer and a monopolist: If we relabel the demand curves in Figure 1 as marginal revenue curves and relabel the supply curve as marginal cost, then one obtains the same comparative static that a cash transfer increases the price of normal goods.

To consider in-kind transfers in our graphical framework, it is helpful to depict just the quantity demanded *from local suppliers*. Then, the supply effect of an in-kind transfer is equivalent to a reduction in the demand facing local suppliers, since a portion of total consumer demand is now met by the government transfer. Thus, an in-kind transfer entails an income effect (demand shifts forward, just as with a cash transfer) and a supply effect (demand shifts back), and Prediction (2) holds.

While the basic comparative statics are the same with perfect or imperfect competition,

would have consumed had it received the transfer in cash, valued at the market prices), or resale is costless. In this case, 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. When considering effects on the market for a substitute (complement) good, the effective supply would not be entirely net of extramarginal consumption, because extramarginal consumption of the transferred good would crowd out (in) consumption of a substitute (complement).

[6] As we discuss at the end of this section, this assumption might not hold if there are flypaper-type effects.

[7] For many standard classes of preferences such as homothetic preferences, one would expect the price to fall for 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.

the efficiency implications differ. If lack of competition causes prices to be above their efficient level, then in-kind transfers can increase total surplus (assuming that there are not inherent production inefficiencies in the government sector). Less consumer demand is met inefficiently by the local suppliers because part of the demand is now met by the welfare-maximizing (not profit-maximizing) government.

Another difference is that even if marginal cost is constant, with imperfect competition, shifts in demand will affect prices. Thus, even if the long run, the price effects would likely persist.

A testable comparative static is that the price effects of transfers should be larger the less competition there is. Consider a Cournot-Nash model with N firms that have constant marginal cost c and face linear demand $p = d - Q$. 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 price effects are:

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

and

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

Openness of the economy

Returning to the competitive case, we also have predictions about heterogeneous effects, analogous to predictions (3) and (4), as the elasticity of supply varies. 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 (steeper supply curve).

Figure 3 illustrates the comparative static for a shift in supply in a more open versus closed economy. For a cash transfer, when the demand curve shifts to the right, 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 gives the effect of increased supply, and again the (relative) price response should be smaller in magnitude, or less negative in this case, when η_S is higher.

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

Note that 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 above and the predictions about openness, we compare more geographically isolated villages (longer travel time to major markets) to less isolated villages. Geographic isolation is our proxy for how closed an economy is (lower η_S) and for how uncompetitive the market is (lower N).

Goods not in the transferred bundle

The discussion above focuses on the goods that are transferred in the in-kind bundle, but there are also price effects for other goods. With cash transfers, demand and hence prices for all normal goods should increase. Using the superscript NX to denote goods not transferred, we have the following additional prediction:

$$\frac{\partial p^{NX}}{\partial X_{Cash}} > 0 \tag{7}$$

With in-kind transfers, the influx of supply for certain goods will affect the demand for and prices of substitutes and complements. If the price of the transferred good falls, then demand for its complements should increase and demand for its substitutes should fall. Let D^{NX} be the demand for a non-transferred good, which is a function of the price p of the transferred good (among other prices and factors). We can define the cross-price elasticity for a non-transferred good with respect to the transferred good as $\eta_D^{NX} \equiv \frac{\partial \ln D^{NX}(p)}{\partial \ln p}$. If a good is a substitute (complement) for the transferred goods, then η_D^{NX} is positive (negative).[8] The prediction is that demand for substitutes—and hence their price—should decrease under an in-kind transfer program relative to a cash transfer program:

[8] Note that when a bundle of goods is transferred, the cross-price elasticity would be treating the bundle as a single aggregate good with a single aggregate price.

$$\frac{\partial}{\partial \eta_D^{NX}} \left(\frac{\partial p^{NX}}{\partial X_{InKind}} - \frac{\partial p^{NX}}{\partial X_{Cash}} \right) < 0. \tag{8}$$

The above are the main testable implications we take to the data. We now discuss some assumptions and extensions to the analysis.

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

Above we define the in-kind transfer amount as its cash equivalent, so the income effect is the same for a cash and in-kind transfer. In our setting, the Mexican government set the cash transfer as equal to its cost of procuring the in-kind goods, which was 25 percent lower than the cost at consumer prices. Therefore, the in-kind bundle would have a higher cash-equivalent value than the cash transfer *if* the transfer was inframarginal to consumption or resale was costless, i.e., the in-kind nature of the transfers did not distort recipients' consumption choices. However, some of the transfers were in fact binding on consumption patterns. Cunha (2010) finds that the distortion in consumption is, on average, 17 percent of the in-kind transfer (34 pesos); that is, the transfer was larger than counterfactual consumption of the goods under a cash transfer, and recipients consumed 34 pesos' worth of the extramarginal portion. The deadweight loss is less than this amount since consumers place some value on these goods; for example, if they value the extramarginal consumption at half its market value, on average, the deadweight loss would be 8.5 percent of the transfer.

In addition, there are transaction costs associated with resale of the portion of extramarginal in-kind transfers that is not consumed. On average, 45 percent (90 pesos) of the in-kind transfer is extramarginal, but most of this is not binding on consumption, presumably because the goods are resold (Cunha, 2010).

Putting these pieces together, while it is difficult to pinpoint the precise value of the in-kind transfer to consumers—its nominal value minus the deadweight loss relative to an unconstrained transfer and minus transaction costs of resale—in our setting, the value of the in-kind transfer is likely quite similar to but somewhat larger than the value of the cash transfer to which we compare it. This extra income effect for the in-kind transfer will bias us *against* finding a relative price decline for in-kind transfers relative to cash transfers.

Another important consideration is that the effect of government transfers on demand might differ from the standard income elasticity of demand. For example, there might be a flypaper effect whereby a cash transfer labeled as food assistance stimulates the demand for food more than a generically labeled cash transfer would have. This type of effect is likely 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 result given in (2), and the magnitude we estimate would then represent a lower bound for the pure supply-shift effect of in-kind transfers.

Supply side of the local economy

In our setting, the local supply side of the market comprises mainly shops rather than producers. Most of the items in the bundle are packaged foods, industrially produced elsewhere in urban areas. When we examine effects on other food items that were not transferred in the bundle, some of these items are produced locally (e.g. vegetables).

It is important to note that in the long run, local supply could react to the government-induced extra supply. Local sellers could scale back their procurement of the food items that were in the transferred bundle, or producers of food could cut back production. In the short run, there is limited scope for this adjustment unless the suppliers anticipate the policy.[9] In the longer term, it is quite possible that the price effects would diminish as local supply adjusts. It is ultimately an empirical matter whether the price effects in the short to medium run, which we study in this paper, are economically significant.

Since the goods in our setting are mainly storable goods (e.g., vegetable oil, rice, beans), even in the short run, shopkeepers might be able to adjust supply downward by allowing inventory to build up. In treating the short-run market as a spot market, the implicit assumption is that inventory costs are high. One potential reason for high inventory costs is that shopkeepers are credit constrained and have limited working capital. In addition, there might be a high risk of theft or damage to inventory or limited storage capacity.

Finally, there is also a supply side of the market that is outside the local economy, namely the food manufacturers, which we ignore in our analysis. If by increasing the total demand 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% 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. In other words, we examine only the local general equilibrium effects, and not the total general

[9] According to the administrators of the transfer program that we study, the start of the program was indeed a surprise to the local communities (private communication).

equilibrium effect of the program.

3 Description of the PAL Program and Data

3.1 PAL Program and experiment

We study the Programa de Apoyo Alimentario (PAL) in Mexico. Started in 2004, PAL operates in about 5,000 very poor, rural villages throughout Mexico.[10] Households within program villages are eligible to receive transfers if they are classified as poor by the national government. PAL is administered by the public/private company Diconsa, which also maintains subsidized general stores in these areas.[11]

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 stores. The basic goods are dietary staples for the poor households in Mexico. The supplementary goods are foods usually consumed in smaller quantities; one goal of the program was to encourage households to add diversity to their diet.

Concurrent with the national roll-out of the program, 208 villages in southern Mexico were randomly selected for inclusion in an experiment.[12] The randomization was at the village level, with eligible households in experimental villages receiving either (i) a monthly in-kind food transfer (50 percent of villages), (ii) a 150 peso per month cash transfer (25 percent of villages), or (iii) nothing, i.e., the control group (the remaining 25 percent of villages).[13] About 89 percent of households in the in-kind and cash villages were eligible to

[10] 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 currently receive aid from other food transfer programs. In practice, this last criterion implies that the village is not incorporated in either Liconsa, the Mexican subsidized milk program, or Oportunidades, a conditional cash transfer program (formerly known as Progresa). Therefore PAL villages are largely poorer and more rural than the widely-studied Progresa/Oportunidades villages. Angelucci and De Giorgi (2009) do not find significant price effects of Progresa, consistent with price effects being stronger in smaller, more rural economies.

[11] Diconsa stores set their own prices but receive a government transportation cost subsidy. Unlike fully private stores, they do not allow purchases on credit.

[12] The experiment was implemented in eight states: Campeche, Chiapas, Guerrero, Oaxaca, Quintana Roo, Tabasco, Veracruz, and Yucatan. See Figure 4 for the locations of the experimental villages.

[13] The rationale for making the in-kind treatment arm larger was that there was an orthogonal randomization among the in-kind villages in which they were or were not provided nutrition education classes. We abstract from this component of the experiment in our analysis because we find that a substantial fraction of the villages that should have been excluded from the nutritional classes received them.

receive transfers (and received them). A woman (the household head or spouse of the head) was designated the beneficiary within the household, if possible.

The impact of the PAL program on villages was large, both because the eligibility rate was high and because the transfer per household was sizeable. For example, the in-kind transfer represented 12% of a recipient household's baseline food expenditures on average. Including the ineligible households, the injection of food into the village through the program was equivalent to 11% of baseline aggregate food expenditures. Similarly, the cash transfer represented a 6% increase in total village income.

In the in-kind experimental villages, the transfer comprised the seven basic items and the following three supplementary goods: lentils, breakfast cereal, and either canned tuna fish or canned sardines. However, there is some ambiguity about whether the in-kind villages received different supplementary items in some months. Thus, in our analysis, we also separate the basic PAL goods from the supplementary ones. A second reason to examine the basic goods separately is that they isolate the basic 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 (that goes in the direction of increasing demand and lowering prices).

Of the 208 villages, 15 are excluded from the analysis. Two villages could not be re-surveyed due to concerns for enumerator safety; in two villages, the PAL program began before the baseline survey; four villages received a different treatment than they were assigned in the randomization; and two villages are geographically contiguous and cannot be regarded as separate markets. In five of the remaining villages, no post-program store data were collected. Observable characteristics of excluded villages are balanced across treatment arms. (Results available upon request.)

Both the in-kind and cash transfers were, in practice, delivered bimonthly, two monthly allotments at a time per household. The transfer size was the same for every eligible household, regardless of family size. Resale of in-kind food transfers was not prohibited, nor were there purchase requirements attached to the cash transfers. The monthly box of food had a market value of about 200 pesos (around 20 U.S. dollars). However, the wholesale cost of the food to the government was about 150 pesos per box, and the government used this procurement cost to set the cash transfer at 150 pesos per month.

The items included in the in-kind transfer are not produced locally.[14] Thus, the main

[14]We do not observe actual food production, but rather draw this conclusion from household survey data on consumption of own-produced foods (We discuss the household survey below). The only PAL good that has auto-consumption in any appreciable quantity is beans (10 percent of households consume own-produced

welfare effects on the producer side of the market will be felt by shopkeepers. There will also be welfare effects for local producers in cases where there is a high degree of substitutability (or complementarity) between the in-kind goods and the local products.

3.2 Data

The data for our analysis come from surveys of stores and households conducted in the experimental villages by the Mexican National Institute of 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.[15] Follow-up data were collected two years later in the final quarter of 2005, about one year after PAL transfers began in these villages.

Our measure of post-program prices comes from a survey of local food stores. Enumerators collected prices for fixed quantities of 66 individual food items, from a maximum of three stores per village, though typically data were collected from only one or two stores per village.[16] Some of the stores surveyed were part of the Diconsa agency (13 percent) while the majority were independent stores (the remaining 87 percent). Diconsa stores are owned and run by the local community, and prices are set locally rather than centrally, so prices at these stores should also be responsive to market conditions.[17]

We also use measures of pre-program food prices. Unfortunately, store prices were only collected for 40 items in the baseline survey, and enumerators did a poor job of recording even these data; there is extensive missing data in the pre-program store survey. Therefore, we use the household survey to construct the pre-program unit value (expenditure divided by quantity purchased) for each food item, and we take the village median unit value as our measure of price. In each village, a random sample of 33 households was interviewed about purchase quantities and expenditures on 60 food items, all of which were also asked

beans at baseline). There is also relatively little auto-consumption of non-PAL foods. Only 7 out 57 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.

[15]Household surveys were administered with the stated objective of studying the nutritional status of children and their mothers; intentionally, no mention was made of the experiment, PAL, or Diconsa.

[16]Many of the stores had posted prices, and in cases where they did not, enumerators were instructed to choose the lowest price available for a given good in order to maintain consistency. There will likely be more noise in the price data when prices are not posted (and there are many other sources of noise in the price data). These other sources of variation in prices apply in all of the treatment arms, so they should reduce the precision of the estimates but should not change the coefficients, in expectation.

[17]This fact has been confirmed by program administrators. Unfortunately, we do not have sufficient within-village variation in store type to test this prediction.

about in the store survey.[18] Note that unlike the post-program prices where we have multiple observations per village-good (one for each store-good), the pre-program prices do not vary within a village. In cases where the pre-program village median unit value is missing, we impute the pre-program price using data from the pre-program store price survey, if this information exists.

We exclude some food items from the analysis due to missing data or the low number of households that consumed the item. Among the PAL goods, the store price survey did not include two items, biscuits and corn starch, and the household survey did not collect data on chocolate powder.[19] Among the non-PAL items, nixtamalized corn flour, salt, and non-fortified powdered milk were not included in the household survey. We also exclude three goods that are consumed by less than 5 percent of households (watermelon, goat/sheep, and wheat tortillas) since the unit values for these are very noisy. Finally, two pairs of goods were asked about jointly in the household survey (beef/pork and canned fish) but separately in the store survey (beef, pork, canned tuna, canned sardines). To address this discrepancy, we use the aggregated category and take the median across all observed store prices for either good as our post-program price measure. Our final data set contains 6 basic PAL goods (corn flour, rice, beans, pasta, oil, fortified milk), 3 supplementary PAL goods (canned fish, packaged breakfast cereal, and lentils), and 48 non-PAL goods. Appendix Table 1 lists all of the goods used in our analysis.

Table 1 presents summary 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 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 all in-kind villages of the total amount of a good transferred to the village divided by the average consumption of the good in control villages in the post-period. For example, there was about as much

[18] Unit values are only observed for households that purchased the good in question in the past seven days (the survey recall window). For some goods, there are very few household-level observations of the unit value (e.g., lentils, cereal, and corn flour), while for others, most households purchased the good (e.g., beans, corn kernels, and onions). The noisiness of our pre-period price measure will vary with the number of observed unit values.

[19] The price of biscuits was intended to be collected, but a mistake in the survey questionnaire led enumerators to collect prices for crackers ("galletas saladas" in Spanish) rather than for biscuits ("galletas" in Spanish). We do not know why corn starch and chocolate powder were not included in the data collection.

corn flour delivered to the villages each month as would have been consumed absent the program ($\Delta Supply = 1.05$ for corn flour), while there was over eight times as much fortified powdered milk delivered as would have been consumed absent the program ($\Delta Supply = 8.49$ for fortified milk powder). We use this measure, which we discuss in more detail in the following section, to test for effects along the "intensive margin" of the in-kind program.

Our data set is a good-store-village panel. Since many stores sell only a subset of goods, the number of goods varies by store. Our final data set contains 358 stores in 193 villages and 11,214 good-store observations. Table 2 presents summary statistics by treatment group. The comparison of baseline characteristics across treatment arms confirms that the randomization appears to have been successful. There is some imbalance in the pre-period unit values, though it is not statistically significant for the PAL goods and only marginally significant for the full set of goods. Nonetheless, we also can address the imbalance by controlling for the pre-period unit price.

In some of our auxiliary analyses, we use household level data to either construct village- or good-level variables or to estimate household-level regressions. For example, we calculate the median household expenditures per capita in a village at baseline as a measure of the income level in the village. We also classify goods as locally produced or imported based on household data; we do not have information on production by good, but the consumption module did ask whether any of the consumption of a good was from own production, which we use to infer whether a good is produced locally. Finally, to test for heterogeneous program effects for households that produce agricultural goods, we use household level information on outcomes such as farm profits, expenditures per capita, and labor supply. We present more detail on the relevant data as we introduce each analysis in the next section.

4 Empirical Strategy and Results

4.1 Price effects of in-kind transfers and cash transfers

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 transferred by the government in the in-kind program. Our first testable 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 the supply curve shifts to the right.

We estimate the following regression where the outcome variable is $\ln p_{gsv}$, the log price for good g at store s in village v. Because our predictions are for cash transfers versus the control group and for cash versus in-kind transfers, we use the cash villages as the omitted category in our regression. Our two predictions correspond to $\beta_1 < 0$, and $\beta_2 < 0$. The relative magnitude of β_1 and β_2 is theoretically ambiguous.

$$\ln p_{gsv} = \alpha + \beta_1 InKind_v + \beta_2 Control_v + \phi \ln p_{gsv,t-1} + \sigma X_{gv} + \epsilon_{gsv} \quad (9)$$

The regression pools the effects for the nine different PAL food items. To adjust for the different price levels of different goods and more generally to improve the precision of the estimates, we control for the pre-period log price, denoted $\ln p_{gsv,t-1}$. The variable X is a dummy variable for whether the pre-program price is imputed from store prices because the village-median unit value is missing. We cluster standard errors at the village level.

Table 3, column 1, presents the basic specification. For in-kind villages relative to cash villages, prices are 3.5 percent lower, and the coefficient is significant at the 10 percent level. The interpretation of the negative coefficient is that prices fell due to the supply curve shifting out when the government injected the PAL goods into the economy. The coefficient on control villages implies that in cash villages relative to control villages, prices increased by 0.8 percent. However, this estimate is not statistically significant. As mentioned above, theory does not tell us whether the supply or demand effect should be bigger in magnitude, but empirically we find that the supply effect (in-kind coefficient), based on the point estimate, is about four or five times the magnitude of the income effect (cash versus control comparison).

It is somewhat ambiguous whether canned fish, cereal, and lentils were the supplementary goods throughout the experiment. This should not affect the cash or control villages, but might attenuate our estimates of the in-kind effect. In column 2 we therefore focus on the 6 basic goods.[20] We find coefficients that are somewhat larger in magnitude than those in column 1. The in-kind transfer leads to prices that are 4.8 percent lower than the transfer (significant at the 5 percent level) and the cash transfer leads to prices that are 3.8 percent higher than in the control group (statistically insignificant). These larger effects are also consistent with the program having any additional effect of increasing households' taste for the supplementary in-kind goods, which would cause the prices of the supplementary goods

[20] Another rationale for excluding these goods is that there is low consumption at baseline for them, and for very thin markets, prices are noisier and the neoclassical model might not fit as well.

to be relatively higher.

Next we estimate a before-after version of the model. The coefficient on the lagged price in column (1) was 0.86 and statistically less than 1, but the estimate is consistent with a true coefficient of 1 that is downward biased due to measurement error: A rough calculation of attenuation bias suggests that the coefficient on the lagged price is downward biased by a factor of 0.84.[21] This suggests that the true coefficient is 1, in which case a preferred specification might be to estimate the model in first differences, comparing before and after the program. Since our treatment variables are equal to zero in the pre-period, a model in first differences is equivalent to using the after-minus-before change in log prices (denoted $\Delta \ln p_{gsv}$) as the outcome variable.

$$\Delta \ln p_{gsv} = \alpha + \beta_1 InKind_v + \beta_2 Control_v + \sigma X_{gv} + \epsilon_{gsv} \tag{10}$$

These results, reported in Appendix Table 2, are somewhat larger in magnitude than the ones presented in the main text, but generally quite similar.

Our estimates allow us to quantify the indirect transfer that occurs through the pecuniary effects. Expenditure on the items in the in-kind bundle was on average 289 pesos per household per month in the control villages. Therefore, in-kind recipients spent an additional 89 pesos per month on the food items contained in the PAL bundle in addition to the 200 pesos' worth they received from the program. (We exclude the transfer-induced increase in demand when calculating the quantity to which to apply the price change.) The 3.5 percent price decrease in in-kind relative to cash villages is thus roughly equivalent to a 3 peso transfer for a household that is a net consumer of these items, as most recipients are. Note that the price changes affect all households, not just program recipients.[22] After we scale up for non-recipients, we find that for every 200 pesos the government directly transferred in-kind, the price effect transfers 4.3 pesos, equivalent to 2 percent of the direct transfer, compared to a cash transfer.[23] Using a similar calculation, our point estimate for the cash-transfer effect suggests that the price effect offsets about 1 pesos, or 0.5 percent, of the transfer value. Note that these are not the total pecuniary effects of the program since they

[21] This calculation uses the between-village variation in baseline unit values for a good, which is 0.127, as the estimate of the actual variance (signal) and the within-village variance in prices for a good, which is 0.025, as the estimate of measurement error (noise). The attenuation factor is thus $0.127/(0.127 + 0.025) = 0.84$.

[22] Analyzing consumption at the household level, we find a positive but insignificant increase in consumption of the in-kind goods in the in-kind villages relative to the cash villages among non-recipient households. Results available upon request.

[23] For non-eligibles, we multiply our estimated price effects by 289 pesos of expenditures rather than 89 pesos since non-recipients do not receive any food through the transfer program.

exclude price effects on the rest of households' consumption bundle, i.e. the non-transferred goods. As shown later in this section, the total pecuniary effect is in fact considerably larger.

4.2 Size of the supply influx

A larger influx of supply will cause a larger change in the price, all else equal. In our setting, the supply shift associated with each good in the PAL basket varied in magnitude. Some of the goods were provided in large quantity, measured relative to the baseline market size (e.g., powdered milk) whereas for other goods, a small quantity was transferred (e.g., vegetable oil). We can thus also examine variation across goods in the intensity of treatment.

We quantify the size of the supply shift as the average across all in-kind villages of the total amount of good g transferred to the village divided by the average consumption of the good in control villages in the post-period.[24] We use consumption in the control villages as a proxy for the equilibrium market size for the good in the post-period, absent the program.[25] This normalization gives us a measure of the supply shock that is relative to the market size. For each good, the intensity of the treatment is measured as $\Delta Supply_g \equiv InKindAmount_g/TotalMarketSize_g$, as reported in Table 1, column 5.[26] Using this measure of the size of the in-kind transfer by good, we can test whether the price effects vary by good accordingly.

The variable $\Delta Supply$ measures the intensity of the *in-kind* treatment, and there is no a priori reason that the intensity of the cash treatment will vary with it. Thus, in principle, we could compare the in-kind villages to the pooled cash and control villages. However, since the income effect could be spuriously correlated with $\Delta Supply$, we again will compare in-kind villages to cash-transfer villages. We set $\Delta Supply$ equal to the same value in all villages and construct an interaction term for each of the treatment arms. We estimate the following equation.

$$\begin{aligned} \ln p_{gsv} &= \alpha + \theta_1 \Delta Supply_g \times InKind_v + \theta_2 \Delta Supply_g \times Control_v \\ &\quad + \rho \Delta Supply_g + \pi_v + \phi \ln p_{gsv,t-1} + \sigma X_{gv} + \epsilon_{gsv} \end{aligned} \tag{11}$$

[24]There is also between-village variation in the size of the transfer; villages differ in their baseline consumption of goods and the proportion of households that are program-eligible. We average across villages because of the endogeneity of this between-village variation (for example, it depends on the village's poverty and its taste for a good).

[25]We can alternatively divide by average consumption in the pre-period in the in-kind (or all) villages. Both measures of counterfactual consumption give similar results.

[26]To be more precise, one should net out the amount of binding extramarginal transfers from the supply influx. See footnote 5.

Note that a set of village fixed effects π_v absorbs the main effects of $InKind$ and $Control$. The prediction is that $\theta_1 < 0$, or that the larger the supply shock, the more prices fall in in-kind versus cash villages. Since the regressor varies at the village-good level, we cluster at this level.

Columns 3 and 4 of Table 3 show the results on treatment intensity. The negative coefficient for $\Delta Supply \times InKind$ implies that the larger the supply shock, the bigger the price decline, as one would expect. The coefficient of -0.047 in column 3 means that when the supply shock increases in size by 10 percentage points, measured relative to the baseline market size, the price falls by 0.47 percent more in in-kind villages relative to cash villages. When we restrict the sample to the basic PAL goods (column 4), we find effects that are slightly larger in magnitude. There is no theoretical prediction on $\Delta Supply \times Control$, which measures how the income effect varies by good, but we find a negative coefficient. The likely explanation is that in-kind transfers are (by definition) large relative to the market size (high $\Delta Supply$) when a good is not a staple but is instead less common in the diet, e.g., lentils, breakfast cereal, fortified milk; these non-staples are very likely luxury goods with a high income elasticity. The main effect of $\Delta Supply$ suggests that prices, by happenstance, were increasing over time more for those goods that were transferred in larger amounts by PAL.

These results using $\Delta Supply$ (columns 3 and 4) are identified off of a different source of variation than the earlier results using the treatment indicators (columns 1 and 2). Here we examine the intensive margin of treatment across goods, whereas earlier we examined the extensive margin of treatment across villages. We find it reassuring that the hypotheses about the price effects of in-kind versus cash transfers are confirmed in two independent ways.

4.3 Substitute goods and total pecuniary effect size

Effects on all non-PAL food items

We next test predictions related to substitute goods. First, we examine all the non-PAL food items in our data. By and large, other food items are substitutes for the PAL bundle of food, so in aggregate, non-PAL food prices are predicted to fall in in-kind villages relative to cash villages.[27] As shown in Table 3, column 5, when the transfer is made in-kind rather than in cash, the point estimate suggests a decline in the price of food items not included

[27] Ideally we would have price data on non-food items, which should not be close substitutes with the PAL bundle, and could test whether their prices responded less. Unfortunately this information is not available because non-food consumption is recorded as expenditures only, with no quantity information with which to construct unit values and no price survey conducted.

in the transfer bundle. Surprisingly, this coefficient of 3.5 percent is the same magnitude as for the PAL goods. One possible explanation, though it is speculative, is the effect on preferences mentioned earlier. If the government transfer made salient the PAL goods or signalled their nutritional quality, then the in-kind transfer might have boosted demand for the PAL goods in addition to increasing their supply in the village.

We also find that prices rise in the cash villages for the non-PAL goods, with coefficients similar to our estimate among the PAL goods. For the cash transfer, unlike the in-kind transfer, nothing distinguishes the PAL goods from other food items, so one would indeed predict similar price increases for both sets of goods.

Total pecuniary effects of the program

We can use these estimates for non-PAL food items, combined with our earlier results for the PAL items, to quantify the total pecuniary effect of the program. Expenditure on the non-PAL items was 1193 pesos per month in the control villages. The 3.5 percent price decrease for in-kind versus cash transfers is thus equivalent to an 41 peso transfer, and the 1.7 percent increase in prices in cash villages is equivalent to a negative 20 peso transfer.

Combining the PAL and non-PAL goods, we find that, compared to the control group, pecuniary effects decrease the transfer size by 11 percent in the cash program. Meanwhile, compared to the control group, pecuniary effects increase the value of in-kind transfers by 12 percent. Thus, for the policy decision of whether to provide transfer in-kind or in cash, in-kind transfers deliver 23 percent more to consumer households, based on our estimates. There are of course many other costs and benefits of in-kind transfers that factor into the policy decision, but the pecuniary effects would appear to be quite important in the decision, given their magnitude.

Heterogeneity across goods in their substitutability with the PAL bundle

We next look at heterogeneity across goods in how substitutable they are with the PAL bundle. Note that we must consider substitutability with the aggregate bundle since there are no instances where, say, vegetable oil is transferred but corn flour is not. The larger in magnitude the cross-price elasticity of a good is with one of the PAL items *and* the more of that PAL item transferred *and* the more extramarginal the supply of that PAL item is (essentially, the larger $\Delta Supply$ is), the more the price of that good should fall. To construct a set of hypothesized close substitutes, we first identified corn flour, fortified powdered milk, biscuits, and pasta soup as goods that were transferred in large and extramarginal quantities by the PAL program. We then classified the following goods as their close substitutes: corn grain, corn tortillas, liquid milk, cheese, yogurt, potatoes, and plantains.[28]

Column 6 examines the price effects for the close substitutes. As expected, in in-kind villages we find a larger price decline for goods that the in-kind allotment should crowd out than for the full set of non-PAL goods, and the effect is marginally significant.

4.4 Remoteness of the village

There are two reasons why the price effects might be amplified in more physically remote village. The first is that these villages are more closed economies. In the extreme of a perfectly open economy (horizontal supply curve), prices are exogenous to the village, but if the local supply at least in part determines prices, then one expects that the more disconnected the village is from other markets, the steeper the supply curve will be and the more prices should adjust to supply shocks or demand shocks (see Figure 3).

The second reason is that the supply side of the market is likely to be less competitive in smaller, physically remote villages. In standard models of oligopolistic competition, the less competition there is, the more prices will respond to changes in the amount demanded from local suppliers.

Using village-level measures of how physically remote the locality is, we test whether $\gamma_1 < 0$ and $\gamma_2 < 0$ in the following model.

$$\begin{aligned} \ln p_{gsv} &= \alpha + \beta_1 InKind_v + \gamma_1 Remote_v \times InKind_v + \beta_2 Control_v \\ &\quad + \gamma_2 Remote_v \times Control_v + \rho Remote_v + \phi \ln p_{gsv,t-1} + \sigma X_{gv} + \epsilon_{gsv} \qquad (12) \end{aligned}$$

Our measure of *Remote* is the time required to travel to a larger market. 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 have more market concentration (e.g., fewer shops selling groceries). We use two measures of travel time to the market. The first, *Travel Time*, is constructed from household-survey self-reports on the travel time to a medium-sized market. The second, *Drive Time*, is the estimated driving time to the nearest large market, calculated using GIS data on the village locations, locations of population centers, and the road network. The two measures have a correlation coefficient of 0.69. (See the Appendix for details on the construction of these variables.)

[28]This exercise is a placeholder and will be replaced with a new categorization of substitutes based on a short survey we are conducting in rural Mexico.

Table 4 reports the results on how pecuniary effects vary with remoteness. Column 1 uses the log of $Travel\ Time$. For the in-kind villages, the price effects are indeed stronger in more remote areas. The coefficient of -0.052 on $ln(Drive\ Time) \times InKind$ is significant at the 10 percent level. The coefficient implies that for every extra hour of driving time, prices fall by 5.2 percentage points more under in-kind transfers relative to cash transfers. We do not find an effect for $ln(Drive\ Time) \times Control$.

$Travel\ Time$ is likely correlated with other characteristics of the village. For example, the more remote villages in our sample are also poorer. To partly address this omitted variable problem, column (2) includes interaction terms (and the main effect of) the median expenditure per capita in the village. Somewhat surprisingly, controlling for this measure of the village's income level makes the results stronger. The coefficient on $ln(Drive\ Time) \times InKind$ is -0.065 and significant at the 5 percent level. The coefficient on $ln(Drive\ Time) \times InKind$ is negative, but small and insignificant.[29]

In Columns 3 and 4 of Table 4, we use the log of $Drive\ Time$ as a proxy for $Remote$. As predicted, we find a negative coefficient on the interaction of remoteness with the in-kind dummy and with the control dummy, both with and without controlling for village median expenditures, but the coefficients are insignificant in this case.[30]

Finally, in columns 5 to 8, we repeat the specifications using the non-PAL goods. Note that the predictions should hold equally strongly for PAL and non-PAL goods for the cash villages since no good has special status, but for the in-kind villages, the predictions should hold for non-PAL goods only insofar as they are substitutes for the PAL goods. We find negative coefficients, as predicted, but the coefficients are imprecise.

To summarize, we find suggestive support for the hypothesis that the price effects of transfers are larger in magnitude in villages that are more isolated from other villages and towns. Because more remote areas also 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 are likely to be an important component of the total welfare impact of the program. This point applies not just to Mexico, but to

[29]The smaller price effects in poorer villages are a bit puzzling, but could be due to food expenditures being a larger portion of total expenditures in poor villages and demand being less sensitive to price when there is less discretionary spending on a good.

[30]If there is classical measurement error that is uncorrelated across the two measures of remoteness, then instrumenting one with the other should reduce attenuation bias. We therefore also estimated an IV specification in which $ln(Drive\ Time)$ and its interactions with the two treatment dummies are the three endogenous regressors in the model, and $ln(Travel\ Time)$ and its interactions with the treatment dummies are the three instruments. The IV coefficients are slightly larger in magnitude than those in Columns 1 and 2, with similar p-values. Results available upon request.

developing countries broadly.

Testing between different interpretations of the remoteness results

We next assess whether the larger price effects in isolated villages are due to these areas having fewer grocery stores so less competition on the supply side or to these villages being more closed economies. While both have the same implication that price effects are larger in less developed areas, separating the two interpretations is important as they have different efficiency implications. In addition, under the perfect-competition, closed-economy explanation, one expects the long-run supply curve to be flatter in the long run so the price effects to dissipate, while the imperfect competition explanation would predict more persistent effects.

Ideally, we would have measures of competition to empirically separate these hypotheses, but unfortunately no data on, for example, the number of stores per village are available. Instead, we take the approach of comparing the price effects for different types of goods in order to separate these two interpretations. For goods that are produced locally, the goods market will plausibly be closed to the outside world, whereas if a good is produced elsewhere and imported into the village, then even in remote villages, the closed economy predictions may not hold as strongly. In contrast, if remote villages have less competition, this should be most true for goods that are sold through grocery stores, the concentrated sector; most of the goods produced elsewhere and imported into the village, such as packaged food, are sold exclusively through grocery stores. For locally produced goods, even if the grocery story sector is uncompetitive, there should be more competition overall because the supply side also includes many local producers selling their crops or livestock products. In other words, the signature of the openness interpretation is that the price effects should be larger in remote villages especially for locally produced goods, and the signature of the competition interpretation is the opposite, namely that the effects should be especially strong for imported goods.

We categorize goods (both PAL goods and non-PAL goods) as locally produced if there is any consumption out of own-production in the sample villages.[31] We construct the measure so that it is not village-specific, but instead is defined over the entire sample. About 57% of the goods have some local production. Not surprisingly, the imported goods tend to be packaged foods.

Columns 1 and 2 of Table 5 estimate equation (12) for the locally produced goods, using

[31] We do not have data on production by good, only auto-consumption by good. Note that there may be cases of production that is fully exported that our definition therefore does not capture.

$ln(Travel\ Time)$ as the measure of remoteness. We do not find the negative interaction effects with *Remote* for the locally produced goods, casting doubt on the closedness interpretation. Next, columns 3 and 4 examine the imported goods, and, here, the price effects are indeed larger in magnitude in the remote villages. Thus, the results lend support to the competition interpretation, as the goods sold through grocery stores exhibit especially strong price effects in isolated villages. Columns 5 and 6 estimate the fully-interacted models using all of the goods, and we find that the triple interaction with *ImportedGood* is negative and, for the cash villages, significant at the 1 percent level.

To recap, the fact that the price effects are larger in isolated villages only for goods brought into the village and sold through grocery stores suggests that the lower degree of competition among food suppliers is the reason that prices respond more to cash and in-kind transfers in remote villages.

4.5 Effects on producer households

Our last analysis examines effects on households engaged in agricultural production. Households in the village are consumers of the packaged goods in the in-kind bundle, and most are net consumers of food overall. However, many households produce some agricultural products, and for their production, the welfare implications of price changes are the opposite of those for their consumption: A price increase (decrease) for food raises (decreases) the value of their production.

We begin by examining how farm revenues and profits vary by treatment type, estimating the following equation using household-level data:

$$FarmProduction_{iv} = \alpha + \beta_1 InKind_v + \beta_2 Control_v + \phi FarmProduction_{iv,t-1} + \epsilon_{iv}. \quad (13)$$

The subscript i indexes the household and, as before, v indexes the village type. We cluster the standard errors by village and, analogous to our earlier analyses, control for the pre-period outcome variable. We examine as outcomes farm revenues in the past year, the log of farm revenues, and farm profits.

As shown in column 1 of Table 6, we find, as predicted, a negative coefficient on *Control*: Farm revenues are higher in cash villages (where food prices have risen) relative to control villages by 1500 pesos (about 150 dollars). Similarly, we find that farm revenues are lower in in-kind villages relative to cash villages by 1100 pesos. In percentage terms (column 2), these effects are larger than the price effects we found earlier, which is not too surprising given that farmers can adjust their production. We do not have data on quantity produced, only the monetary value of production, but the fact that profits change by a smaller amount than revenues (column 3) suggests that farmers expanded or contracted the quantity they produced in response to the price changes. In other words, when earning a higher price, a farmer receives higher revenues both because she earns more per unit sold and because she sells more units.[32]

The results in columns (1) to (3) suggest that the PAL transfer program, through its pecuniary effects, has different welfare implications for producer households. To examine the net effect of the program for different types of households, we first classify households as agricultural producers if, at baseline, they either own a farm or consume food from their own production; 65% of households meet one of these two criteria. We then examine 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. While the results (column 4) are imprecise, they line up with the predictions that cash transfers are more valuable to producer households than to non-producer households (by 9 percentage points), and in-kind transfers are less valuable to producer households than to non-producer households (by 6 percentage points).

Finally, we examine how labor supply responds to the program and whether it does so differentially for producer households. All recipient households experience an income effect, so labor supply should decrease if leisure is a normal good.[33] However, because of the pecuniary effects in the goods market, producer households also experience a change in the revenue product of their labor, i.e., their (shadow) wage. Thus, in cash villages, we would expect labor supply to increase for producer households relative to non-producer households (assuming that the labor supply curve is not backward bending). We also expect labor supply to be lower among producers than non-producers in in-kind villages relative to cash villages. In the following estimating equation, these predictions are equivalent to $\beta_2 > 0$

[32] Price effects are not the only reason that transfers might affect farm production. If farmers are credit constrained, then the income effect of the program might lead to more investment and increased production. For both the cash and in-kind treatment, one expects an increase in farm revenues (and either an increase or decrease in profits depending on how long-run the investment is), though there is no obvious reason there would be a differential effect for cash versus in-kind villages.

[33] Because we do not know which households in the control villages would be transfer recipients (these data were not recorded), in the analysis we do not distinguish between the 11 percent of households who are non-recipients and the 89 percent of households who received the transfer.

(income effect), as well as $\theta_2 < 0$ and $\theta_1 < 0$ (wage effect):

$$\begin{aligned} LaborSup_{iv} &= \alpha + \theta_1 Producer_i \times InKind_v + \theta_2 Producer_i \times Control_v \\ &\quad + \beta_1 InKind_v + \beta_2 Control_v + \rho Producer_i + \phi LaborSup_{iv,t-1} + \epsilon_{iv} \quad (14) \end{aligned}$$

As seen in columns (5) and (6) of Table 6, these predictions are generally born out in the data. In cash villages, non-producer households decrease household labor supply by 14%, consistent with higher income leading to more leisure. Among producer households, the food-price-cum-wage effect offsets the income effect, and total labor supply in fact is unchanged by the program. We also find coefficients that fit the predictions for the in-kind versus cash comparison, but these latter coefficients are statistically insignificant.[34]

5 Conclusion

As most of the world's poor live in rural, often isolated villages, large transfer programs to the poor are likely to 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).

The price effects we find are large in magnitude. The price increase caused by cash transfers, based on the point estimates, offsets the direct transfer by 11 percent for recipients who are consumers of these goods. Meanwhile, for in-kind transfers, the price effects represent an indirect benefit equal to 12 percent of the direct benefit. Thus, choosing in-kind rather than cash transfers in this setting generates extra indirect transfers to the poor worth over 20 percent of the direct transfer.

Of course, the welfare implications are reversed if transfers recipients are producers rather than consumers. We find that agricultural revenues increase in cash villages and decrease relatively in in-kind villages. These effects are due both to the change in the price of goods sold, but also to households responding by producing more (less) when the price of what they produce increases (decreases). Labor supply also responds to the transfers heterogeneously, with agricultural households adjusting their work hours not just because of the income effect

[34] In unreported results, we do not find impacts on investment in agriculture such as the purchase of small farm equipment or loan take-up. However, the limitations of the data prevent us from fully testing the prediction that, just as production increases in the short run, longer run investment in production capacity might respond.

of the program but also because pecuniary effects in the goods market change the marginal product of their labor.

The fact that producer households adjust supply raises the question of how long-lasting the price effects would be. It is likely that supply would further adjust in the longer run, at least if there are no barriers to expansion or entry. We leave the question of the long-run effects of the program for future work since the available data do not allow for such an analysis.

Another key finding is that the price effects are particularly pronounced for very geographically isolated villages, where the most impoverished people live. This finding is consistent with these villages being less open to trade and having less market competition. Our suggestive evidence points to imperfect competition as the main explanation. Thus, when the government acts as a supplier and provides in-kind transfers, it may be not only creating a pecuniary externality in these villages but also reducing the inefficiency associated with imperfect competition.

The policy decision of whether to provide transfers in-kind or as cash includes many other considerations besides price effects. For example, in-kind transfers constrain households' choices, which has costs, but also might help policy makers achieve a paternalistic objective. Another important consideration is how efficiently the government can provide supply. It could still be the case that an uncompetitive private sector creates more surplus than when the government enters as a supplier; if the government is an inefficient producer, then the gain in surplus generated by the fact that it maximizes welfare rather than profits may be outweighed by other sources of inefficiency that it introduces.

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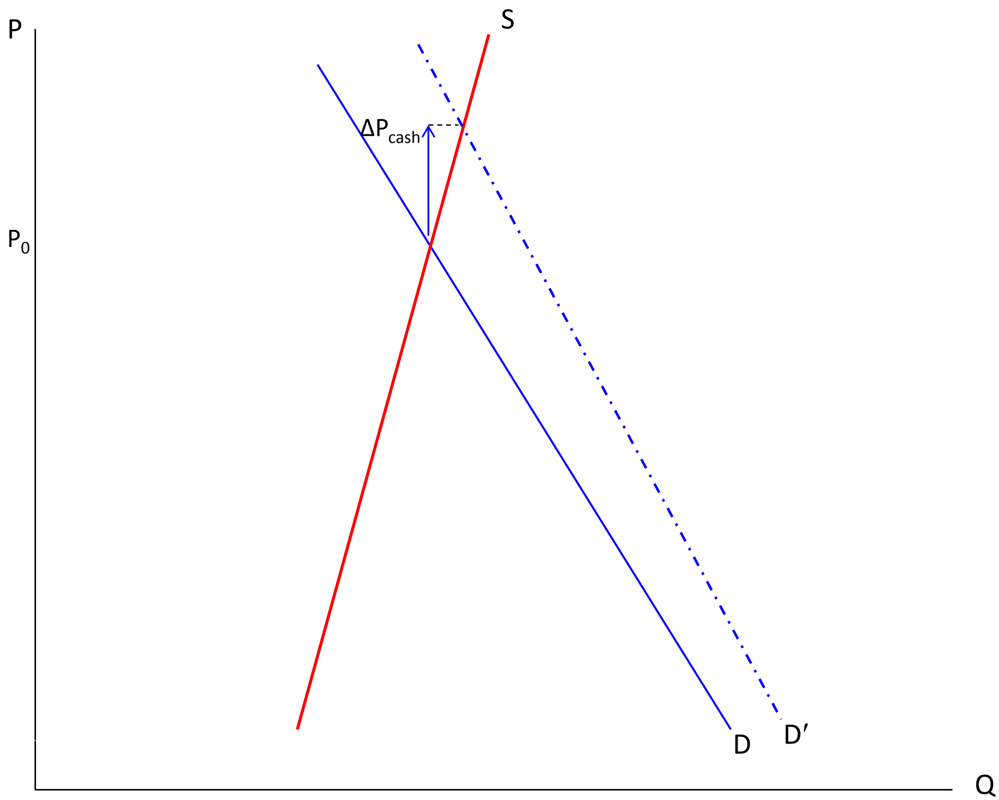
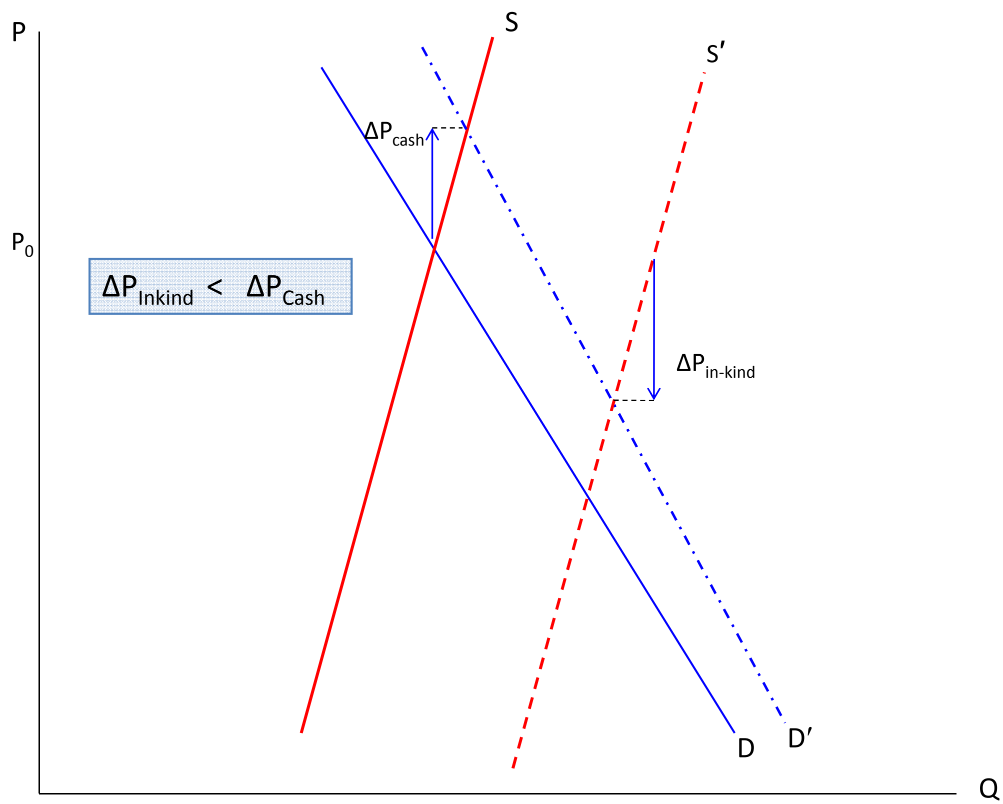
Figure 1: Effect of cash transfers on prices of normal goods



A cash transfer shifts demand to the right from D to D′ for a normal good.

Figure 2: Effect of government-provided supply on prices



An in-kind transfer shifts demand from D to D′ and also shifts supply to the right by the amount of new supply transferred to the economy, from S to S′.

Figure 3: Heterogeneous effects for open versus closed economies

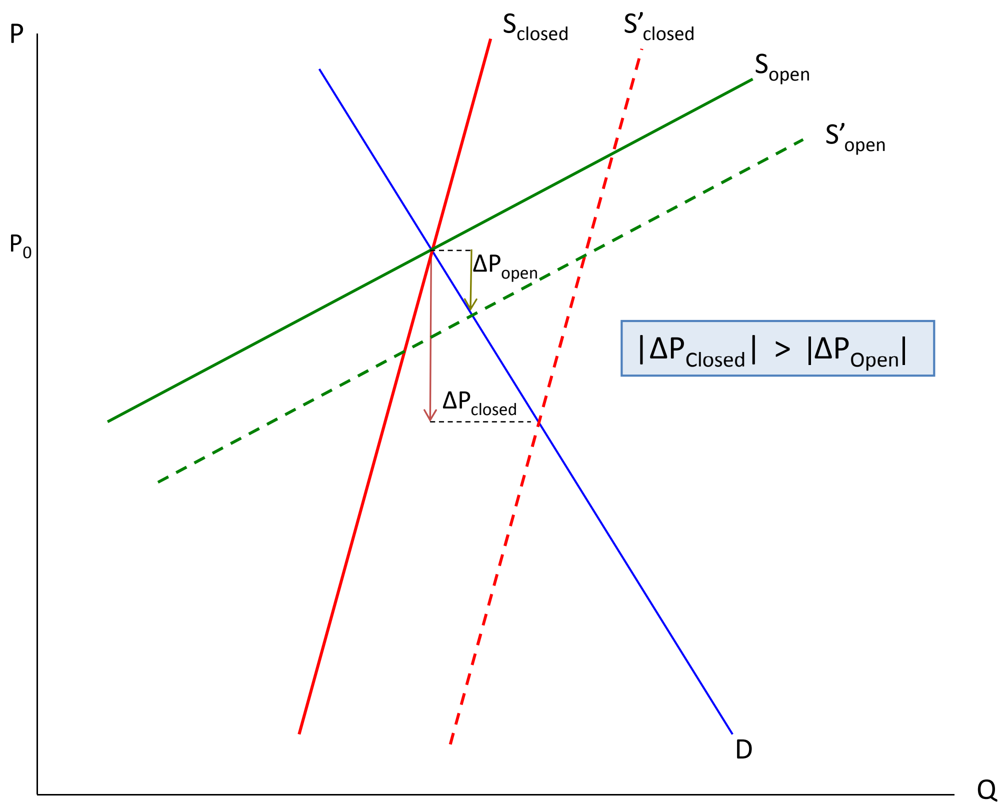


Figure 4: Villages in the PAL experiment

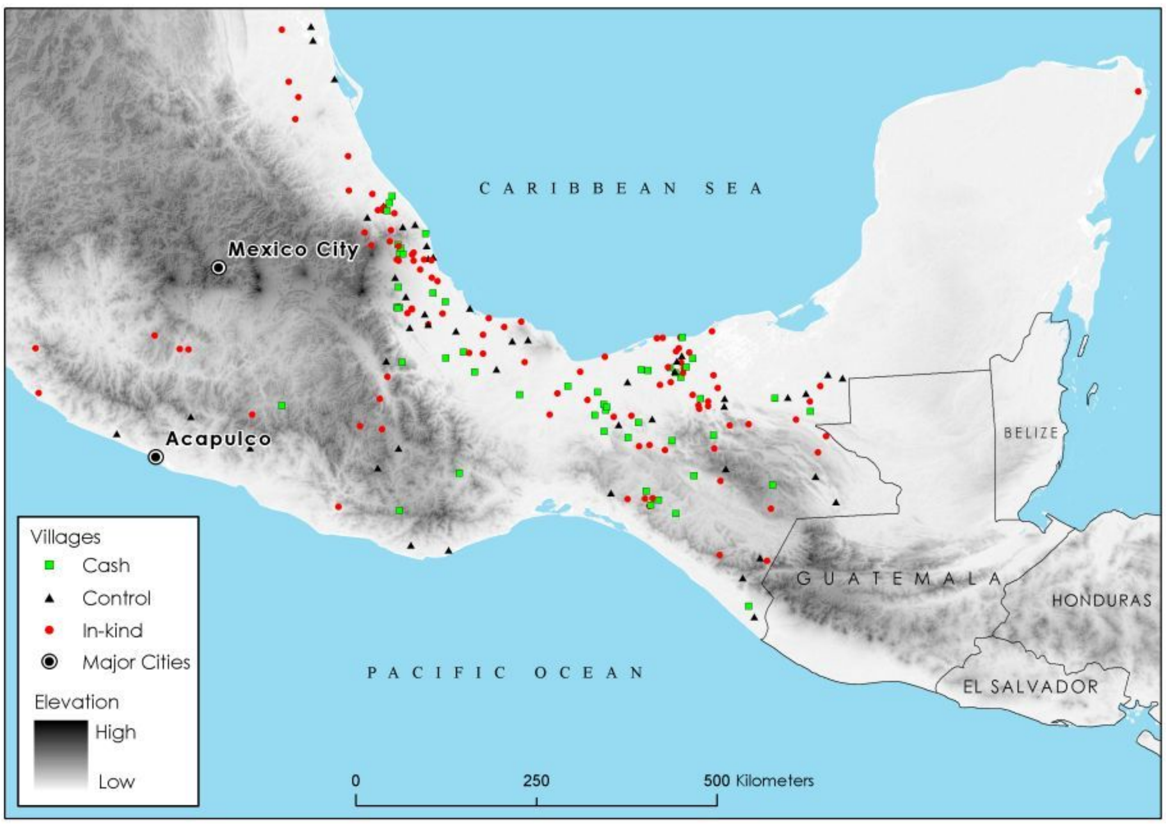


Table 1: PAL food box summary

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	15.0	20%	1.05
Rice	basic	2	12.8	12%	0.58
Beans	basic	2	21.0	13%	0.28
Fortified powdered milk	basic	1.92	82.2	8%	8.49
Packaged pasta soup	basic	1.2	16.2	16%	0.90
Vegetable oil	basic	1 (lt)	10.4	17%	0.25
Biscuits	basic	1	18.5	8%	0.81
Lentils	supplementary	1	9.6	2%	3.44
Canned tuna/sardines	supplementary	0.35	8.7	1%	0.92
Breakfast cereal	supplementary	0.2	8.1	1%	1.02

Notes:
(1) Value is calculated using the average pre-treatment village-level median unit values. 10 pesos ≈ 1 USD. 193 Villages included.
(2) ΔSupply is a measure of 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 a good transferred to the village divided by the average consumption of the good in control villages in the post-period.

Table 2: Baseline characteristics across villages by treatment group

	Control	In-kind	Cash	(1)=(2) p-value	(1)=(3) p-value	(2)=(3) p-value
	(1)	(2)	(3)	(4)	(5)	(6)
PAL goods only						
ln(median village unit-value)	2.49	2.50	2.46	0.74	0.30	0.16
	(0.02)	(0.02)	(0.02)			
N	478	1125	569			
All goods						
ln(median village unit-value)	2.71	2.75	2.70	0.19	0.73	0.09
	(0.02)	(0.02)	(0.02)			
N	2595	5695	2924			
# stores in village surveyed	1.70	1.91	1.90	0.11	0.16	0.98
	(0.10)	(0.07)	(0.10)			
Driving time to nearest city	0.49	0.44	0.53	0.63	0.65	0.30
	(0.07)	(0.05)	(0.07)			
Travel time to nearest market	0.87	0.76	0.84	0.43	0.85	0.56
	(0.11)	(0.08)	(0.11)			
Average HH food consumption (pesos)	7.42	7.34	7.33	0.13	0.13	0.86
	(0.05)	(0.03)	(0.04)			
% HH that farm or raises animals	0.31	0.37	0.44	0.25	0.02	0.15
	(0.04)	(0.03)	(0.04)			
Average # HH members working	1.25	1.27	1.27	0.73	0.76	0.99
	(0.04)	(0.03)	(0.04)			
% HH that are indigenous	0.20	0.18	0.15	0.72	0.50	0.68
	(0.05)	(0.04)	(0.05)			
Average age of HH head	44.50	45.48	45.61	0.28	0.28	0.88
	(0.74)	(0.52)	(0.71)			
% HH with have dirt floor	0.32	0.30	0.34	0.68	0.76	0.44
	(0.04)	(0.03)	(0.04)			
% HH with temporary walls or roof	0.14	0.19	0.16	0.19	0.61	0.46
	(0.03)	(0.02)	(0.03)			
% HH with no separate kitchen	0.26	0.25	0.20	0.71	0.04	0.05
	(0.02)	(0.01)	(0.02)			
% HH with piped water	0.61	0.57	0.52	0.56	0.23	0.42
	(0.06)	(0.04)	(0.06)			
% HH that have refrigerator	0.42	0.46	0.50	0.37	0.16	0.47
	(0.04)	(0.03)	(0.04)			
Number of villages	47	95	51			

Notes:
(1) Standard errors in parentheses. For ln(mean village unit-value), standard errors are clustered at the village level.

Table 3: Price effects of in-kind and cash transfers. Main effects, interactions with the size of the supply influx, and substitutes.

	All PAL goods	Basic PAL goods only	All PAL goods	Basic PAL goods only	All non-PAL goods	Set of PAL substitutes
Outcome =	ln(price)	ln(price)	ln(price)	ln(price)	ln(price)	ln(price)
	(1)	(2)	(3)	(4)	(5)	(6)
In-kind	-0.035*	-0.048**			-0.035	-0.066*
	(0.021)	(0.022)			(0.026)	(0.037)
Control	-0.008	-0.038			-0.017	-0.037
	(0.028)	(0.026)			(0.031)	(0.040)
ΔSupply x In-kind			-0.047*	-0.058**		
			(0.027)	(0.027)		
ΔSupply x Control			-0.054*	-0.071**		
			(0.029)	(0.028)		
ΔSupply			0.066***	0.105***		
			(0.025)	(0.029)		
Lagged ln(price)	0.856***	0.845***	0.867***	0.777***	0.544***	0.957***
	(0.028)	(0.045)	(0.028)	(0.043)	(0.019)	(0.011)
Village FE	no	no	yes	yes	no	no
Observations	2172	1528	2172	1528	9042	1355
R-squared	0.69	0.68	0.72	0.78	0.19	0.87

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

(1) All columns: The outcome is the logarithm of post-treatment store prices (ln price), which varies at the village-store-good level. Lagged ln price is the village median unit-value and varies at the village-good level. Regressions include an indicator for imputed pre-program prices (see text). 193 villages included.

(2) Columns (1)-(2): Standard errors in parentheses clustered at the village level.

(3) Columns (3) and (4): Standard errors in parentheses clustered at the village-good level.

(4) Column (5) includes all 48 non-PAL goods included in the sample.

(5) Column (6) includes 7 items we identified as PAL substitutes: corn tortillas, corn kernels, liquid milk, cheese, yogurt, potatoes, and plantains.

Table 4: Price effects as a function of the remoteness of the village.

	All PAL goods				Non-PAL goods			
	Remote= ln(Travel time)		Remote= ln(Drive time)		Remote= ln(Travel time)		Remote= ln(Drive time)	
Outcome =	ln(price)	ln(price)	ln(price)	ln(price)	ln(price)	ln(price)	ln(price)	ln(price)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Remote x In-kind	-0.052*	-0.065**	-0.015	-0.024	-0.041	-0.048	0.033	0.040
	(0.030)	(0.030)	(0.021)	(0.021)	(0.043)	(0.042)	(0.025)	(0.028)
Remote x Control	0.002	-0.011	-0.014	-0.019	-0.026	-0.058	-0.022	-0.027
	(0.041)	(0.039)	(0.028)	(0.028)	(0.058)	(0.057)	(0.029)	(0.032)
Ln(Village Expenditure) x In-kind		-0.107*		-0.074		-0.057		0.062
		(0.059)		(0.059)		(0.107)		(0.125)
Ln(Village Expenditure) x Control		-0.087		-0.111		-0.203		-0.104
		(0.082)		(0.075)		(0.136)		(0.136)
Observations	1940	1940	2129	2129	8052	8052	8874	8874

Notes: *** p<0.01, ** p<0.05, * p<0.1
(1) All columns: Observations are at the village-store-good level. 193 villages included. Standard errors in parentheses are clustered at the village level. (2) Village expenditure is the median household expenditure per capita in the village.
(3) Regressions control for the main effects of the interaction terms reported, as well as for the pre-period log price and an indicator for imputed pre-program prices (see text).

Table 5: Imperfect competition versus closedness as reason for heterogeneity by remoteness.

	Locally produced goods		Imported goods		All goods	
Outcome =	ln(price)	ln(price)	ln(price)	ln(price)	ln(price)	ln(price)
	(1)	(2)	(3)	(4)	(5)	(6)
Ln(Travel Time) x In-kind	0.006	0.001	-0.104	-0.126*	-0.006	0.001
	(0.033)	(0.031)	(0.069)	(0.075)	(0.032)	(0.031)
Ln(Travel Time) x Control	0.071*	0.060	-0.155*	-0.212**	0.041	0.060
	(0.041)	(0.037)	(0.081)	(0.086)	(0.038)	(0.037)
Ln(Village Expenditure) x In-kind		-0.066		-0.117		-0.066
		(0.100)		(0.149)		(0.100)
Ln(Village Expenditure) x Control		-0.050		-0.375**		-0.050
		(0.114)		(0.176)		(0.114)
Imported x Ln(Travel Time) x In-kind					-0.111	-0.127
					(0.078)	(0.087)
Imported x Ln(Travel Time) x Control					-0.227***	-0.272***
					(0.082)	(0.092)
Observations	5715	5715	4277	4277	9992	9992

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

(1) All columns: Observations are at the village-store-good level. 193 villages and both PAL and non-PAL goods included. Standard errors in parentheses are clustered at the village level. Regressions control for the pre-period price and an indicator for imputed pre-program prices (see text).

(2) Village expenditure is the median household expenditure per capita in the village.

(3) Imported goods are those which no household in the sample consumes out of own-production.

(4) All columns include the main effects of In-Kind, Control, and Ln(Travel Time), as well as Ln(Village Expenditure) in the even columns. The specificiations in Column 5 and 6 interact every variable in, respectively, Column 1 and 2, with Imported and include the main effect of Imported.

Table 6: Effects for food producers.

	(1) Farm revenues	(2) ln(Farm revenues)	(3) Farm profits	(4) ln(Total expenditures per capita)	(5) ln(Hours of Work)	(6) Hours of work
In Kind	-1,122.3**	-0.279**	-318.7	0.030	0.050	-0.374
	(536.708)	(0.135)	(245.574)	(0.039)	(0.062)	(3.089)
Control	-1,504.1***	-0.256	-515.6**	-0.086*	0.144**	1.141
	(559.566)	(0.179)	(249.221)	(0.047)	(0.068)	(3.442)
In Kind * Producer HH				-0.057	-0.050	-0.037
				(0.052)	(0.070)	(3.539)
Control * Producer HH				-0.086	-0.138*	-2.726
				(0.067)	(0.073)	(3.922)
Observations	4918	1004	4918	5506	4396	5538
Control for pre-period outcome?	Y	Y	Y	Y	Y	Y
Main effects of Producer HH?	N	N	N	Y	Y	Y

Notes: *** p<0.01, ** p<0.05, * p<0.1
(1) All columns: Observations are at the household level. Standard errors in parentheses are clustered at the village level.
(2) Producer households are those that, at baseline, either auto-consume their production or own a farm. Revenues, profits, expenditure, and values are measured in pesos. Revenues and profits are for the preceeding year. Hours are for the preceeding week, aggregated for the household.

Appendix A: Variable construction

Openness measures

Our two measures of the physical remoteness of the village, *Drive Time* and *Travel Time*, are constructed as follows. First, *Drive Time* is an approximation of the time it takes to drive from each experimental village to the nearest city with a population of at least 10,000. Our algorithm feeds in the latitude and longitude of each village along with guesses for the driving speeds on each of four road types ("unimproved road," "undivided highway," "paved road, non-highway," and "divided highway") into GIS software that contains the entire road structure of Mexico. We then calculate driving times from each experimental village to all cities in Mexico with over 10,000 inhabitants, and choose the closest one.

Second, *Travel Time* is constructed from household self-reports on the time it takes to travel to the nearest market where fresh food is sold. Household were first asked if fresh foods were sold in the village; then they were asked to state the time to get to the nearest market, regardless of mode of transportation. *Travel Time* is thus the village-median amongst households that report leaving the village to purchase fresh foods.

Appendix Table 1: List of goods used in our analysis.

	All Goods in our analysis	PAL goods		All Goods in our analysis	PAL goods
1	tomato		30	soy	
2	onion		31	chicken	
3	potato		32	beef and pork	
4	carrot		33	seafood (fresh)	
5	leafy greens		34	canned tuna/canned sardines	x
6	squash		35	eggs	
7	chayote		36	milk (liquid)	
8	nopale (cactus)		37	yogurt	
9	fresh chilis		38	cheese	
10	guava		39	lard	
11	mandarin		40	fortified powdered milk	x
12	papaya		41	cold cuts and sausages	
13	orange		42	pastelillo (snack cakes)	
14	plantain		43	soft drinks	
15	apple		44	alcohol	
16	lime		45	coffee	
17	corn tortillas		46	sugar	
18	corn kernels		47	corn or potato chips	
19	corn flour	x	48	chocolate	
20	bread rolls		49	candy	
21	sweet bread		50	vegetable oil	x
22	loaf of white bread		51	mayonnaise	
23	wheat flour		52	fruit drinks	
24	packaged pasta soup	x	53	consome (broth)	
25	rice	x	54	powdered drinks (e.g. Kool-Aid)	
26	breakfast cereal	x	55	atole (masa based hot drink)	
27	beans	x	56	tomato paste	
28	lentils	x	57	canned chilis	
29	oats				

Appendix Table 2: Estimates in first differences: Main effects, interactions with the size of the supply influx, and substitutes.

	All PAL goods	Basic PAL goods only	All PAL goods	Basic PAL goods only	All non-PAL goods	Set of PAL substitutes
Outcome =	Δ ln(price)	Δ ln(price)	Δ ln(price)	Δ ln(price)	Δ ln(price)	Δ ln(price)
	(1)	(2)	(3)	(4)	(5)	(6)
In-kind	-0.041*	-0.054**			-0.060*	-0.066*
	(0.023)	(0.024)			(0.034)	(0.038)
Control	-0.011	-0.046*			-0.018	-0.038
	(0.031)	(0.028)			(0.040)	(0.042)
ΔSupply x In-kind			-0.055*	-0.075**		
			(0.031)	(0.038)		
ΔSupply x Control			-0.061*	-0.087**		
			(0.033)	(0.038)		
ΔSupply			0.058**	0.084**		
			(0.028)	(0.034)		
Village FE	no	no	yes	yes	no	no
Observations	2172	1528	2172	1528	9042	1355
R-squared	0.00	0.01	0.13	0.26	0.00	0.00

Notes: *** p<0.01, ** p<0.05, * p<0.1
(1) All columns: The outcome is the after-minus-before change in ln(price), which varies at the village-store-good level. Regressions include an indicator for imputed pre-program prices (see text). 193 villages included.
(2) Columns (1)-(2): Standard errors in parentheses clustered at the village level.
(3) Columns (3) and (4): Standard errors in parentheses clustered at the village-good level.
(4) Column (5) includes all 48 non-PAL goods included in the database.
(5) Column (6) includes 7 items we identified as PAL substitutes: corn tortillas, corn kernels, liquid milk, cheese, yogurt, potatoes, and plantains.