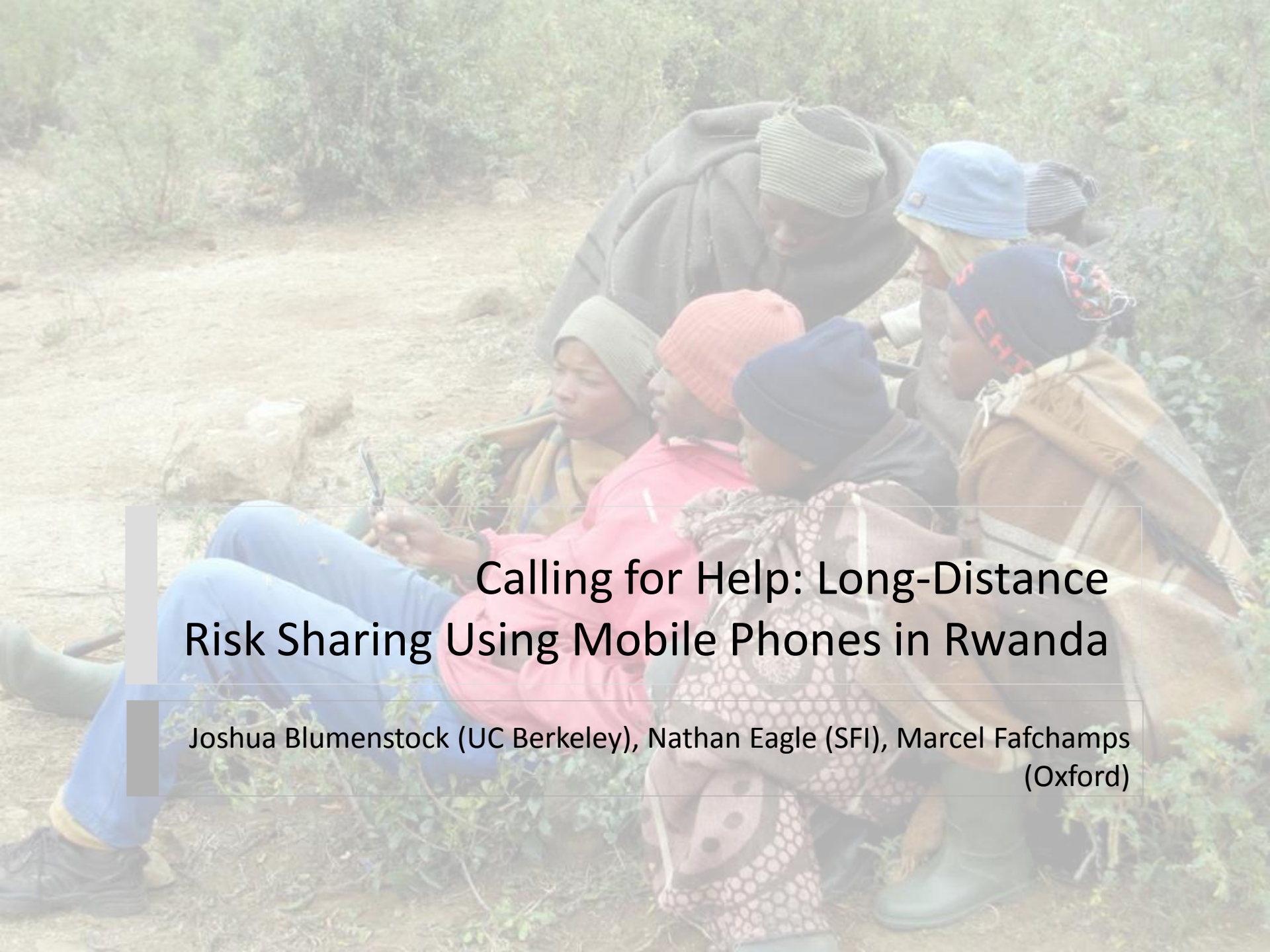
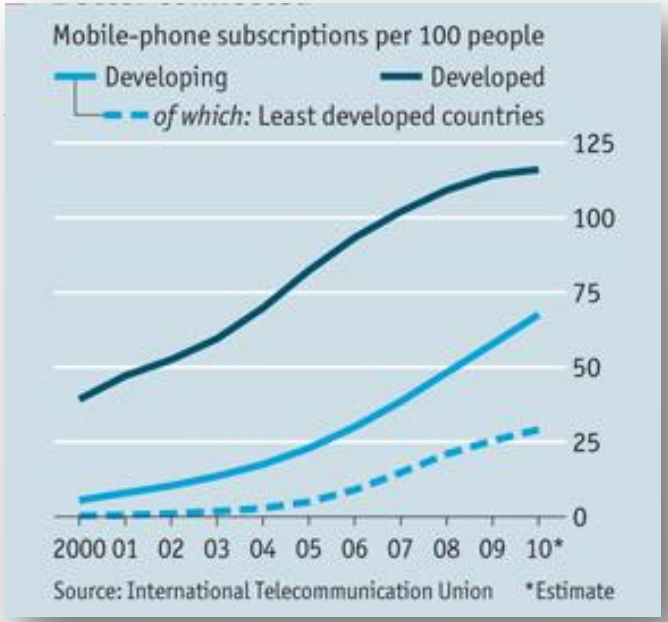


Calling for Help: Long-Distance Risk Sharing Using Mobile Phones in Rwanda

Joshua Blumenstock (UC Berkeley), Nathan Eagle (SFI), Marcel Fafchamps (Oxford)

Motivation & Context



- Rapid uptake of mobile phones in developing countries
  - 2/3 of all mobile phone users live in the developing world
  - **1 new subscriber per second** in Nigeria
- Mobile-based financial services taking off
  - 1.7 Billion people have phones but no bank account
  - Kenya: **$200M *per day*** sent over mobile network
  - Exponential network growth in East Africa
  - Planned launches in 20 countries in Sub-Saharan Africa
- Many Potential Benefits
  - Facilitates payments, markets, trade; Increases savings; enables informal credit (and investment?)
  - **Facilitates risk sharing** between individuals
- But few empirical studies of impacts of mobile phones & mobile banking
  - Micro studies (Jensen 2007, Aker 2008, Goyal 2008, Futch & McIntosh 2009)
  - Cross-country regressions (Waverman et al. 2005, Qiang et al. 2003)
  - Endogeneity, measurement issues, data constraints

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This paper: Risk sharing and mobile phones

- We investigate how mobile banking can help individuals in developing countries share risk over long distances.
- We show that a major earthquake caused a highly significant influx of "mobile money" into the areas affected by the earthquake.

- Key results
  1. A current-day earthquake would cause roughly $20,000 in transfers.
  2. The wealthiest phone owners receive the most.
  3. Empirical evidence indicates that phones are used for reciprocal *risk sharing*, rather than to support distant relatives (remittances) or for dependent relationships (philanthropy/altruism).

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Background: Phones in Rwanda

- Rwanda
  - 10 million people, landlocked, densest nation is SSA
  - Ranked 165th in GDP/capita ($1,041)
- Mobile phones in Rwanda
  - Huge growth in past decade

	Mobile Phones (per 100 people)						Landlines
	2001	**2003**	**2005**	**2007**	**2009**	**CAGR**	**2007**
Rwanda	0.78	1.49	2.47	6.53	24.3	**77.1%**	0.24
USA	44.77	54.90	71.43	83.51	97.1	9.1%	53.35

- Important differences from US market
  - Prepaid, "pay as you go" contracts
  - Caller pays
- Mobile banking since 2006
  - Initially just "Me2U" – send airtime between individuals
  - Now "Mobile Money" – interest bearing accounts, easy withdrawals and deposits

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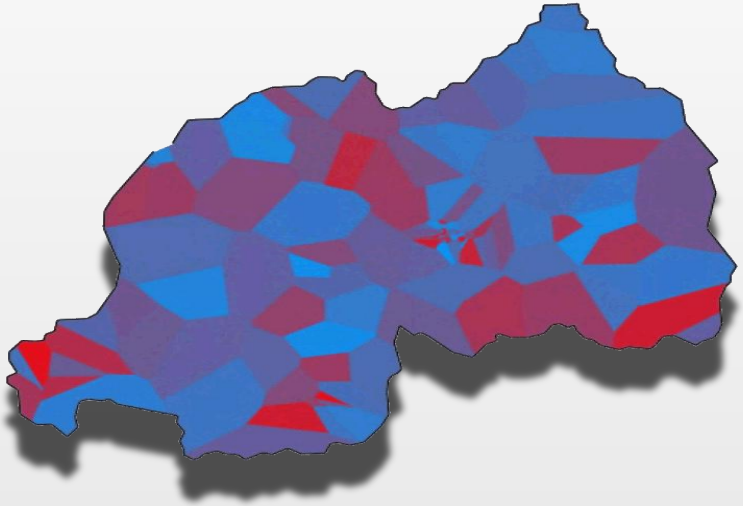
Data: Call Detail Records

- New dataset: records from Rwandan phone company of all "mobile money" transfers from 2005-2009 (and all phone calls).
  - 1.5 million users
  - 300 cell towers
  - 50 billion transactions
  - Calls, SMS's, recharges, …
  - Date/Time/value of all txns
  - 2,300 gigabytes of data



- Includes information on **Me2u** (airtime transfer service)
- Includes **geographic information**: each call is linked to a cell tower with known geo-coordinates
- Can derive **social network information**: number of contacts, number of contacts within radius of *r* km, centrality, clustering, etc.
- But all users are **anonymous** (contracts are pre-paid)

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Identification strategy

- Lake Kivu Earthquake
  - February 3, 2008, 9:30am
  - Magnitude 6
  - 43 dead, 1,090 injured
  - 2,288 houses destroyed
  - Schools closed, power outages
  - Lat: -2.296, Long: 28.900

$$\tau_{d,t} = \alpha + \beta_1 Quake_t + \beta_2 Epicenter_d + \beta_3 (Quake_t * Close_d) + \beta_4 Z_{d,t} \, [+\pi_d + \theta_t] + \varepsilon_{d,t}$$

- $\tau_{d,t}$ measures total airtime transfers sent **into** district d at time t.
- $Quake_t$ is a dummy that takes value 1 on the day of the quake.
- $Epicenter_d$ is a dummy that takes value 1 for towers close to the epicenter.
- $Z_{d,t}$ includes time-variant district-level network attributes [transfers out, total call traffic, …]
- π_d and θ_t are regional and temporal (daily) fixed effects, $\varepsilon_{d,t}$ clustered by district

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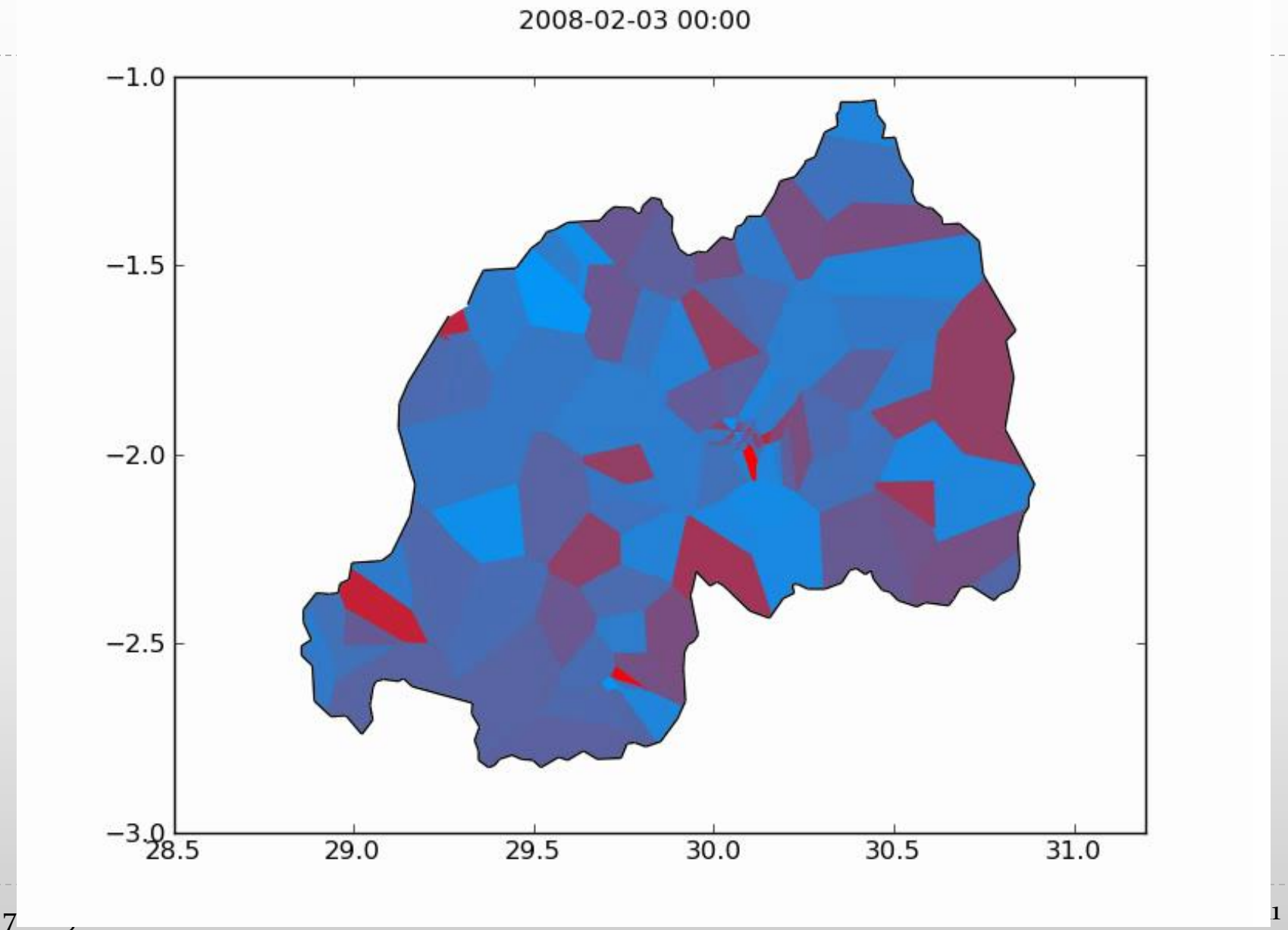
Identification Strategy: Intuition (video)



http://www.youtube.com/watch?v=-uShJaGOkc4

Results - Base Specification (District Level)

	(1) No Controls	(2) Basic Controls	(3) District FE	(4) District/Time FE
shock	16375.150***	13472.260***	13398.199***	13478.897***
	(3321.23)	(1369.93)	(1398.27)	(1364.61)
"day of quake"	-6677.574*	-2125.949**	-2054.505**	
	(2878.47)	(719.14)	(704.93)	
"in quake region"	-19211.078	-706.446		
	(9937.24)	(396.10)		
Total call volume		0.010***	0.010	0.010
		(0.00)	(0.01)	(0.01)
Outgoing transfers		0.904***	0.903***	0.876***
		(0.00)	(0.01)	(0.02)
District FE	NO	NO	YES	YES
Daily FE	NO	NO	NO	YES
_cons	27676.003**	1944.769***	1926.681**	8441.880
r2	0.007	0.983	0.984	0.984
N	19260	17172	17172	17172

Outcome: Value of incoming airtime sent **to** people near the tower (in FRW; US$1=550FRW).
"in quake region" are Nyamasheke and Rusizi districts. Day of quake is 2/3/2008.
Computed over interval from 10/01/2006 through 7/01/2008
Heteroskedasticity-robust SE's in parentheses (clustered at district level).
* p<0.05, ** p<0.01, *** p<0.001.

Unconditional mean of outcome: in earthquake region in two weeks preceding earthquake = 5045.92
in earthquake region = 8480.03; full dataset = 26386.56;

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Lagged effects on transfers and calls

	(1) Me2u Received	(2) Calls Received	(3) Int'l Calls Received
shock	13512.649***	7195.625***	50.477
	(1335.51)	(1926.01)	(49.34)
shock_lag1	-917.294	2260.950***	182.241***
	(1330.88)	(258.39)	(38.09)
shock_lag2	1540.204	773.228	55.742
	(2796.36)	(529.88)	(32.77)
shock_lag3	830.593	588.593**	94.906
	(3157.92)	(163.84)	(52.31)
shock_lag4	-189.597	213.894	61.651
	(1518.35)	(109.26)	(51.99)
shock_lag5	-40.867	489.812***	5.387
	(3028.17)	(124.99)	(65.16)
shock_lead1	810.813	48.257	102.622
	(1732.01)	(171.90)	(63.32)
shock_lead2	1341.489	59.513	63.696
	(1124.93)	(211.16)	(39.21)
shock_lead3	-2460.249	-108.260	66.668
	(2003.26)	(109.59)	(42.24)
_cons	155.928	-299.070***	-45.137
N	16,808	16,808	16,808

"placebo"

Control variables: Daily FE, Tower FE, Me2u sent, calls made, international calls made.

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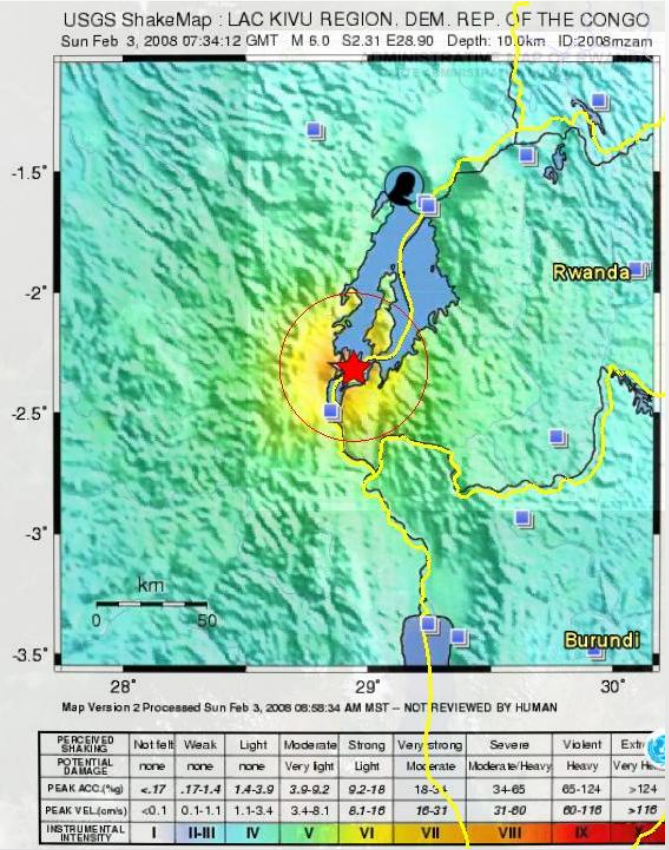
Results - Base Specification (Tower Level)

	(1) Cross-section Day of quake	(3) Basic Controls	(4) Tower FE	(5) Tower/time FE
shock		2819.503***	2787.305***	2710.861***
		(121.33)	(136.18)	(183.53)
day of quake		-1375.294***	-1287.145***	
		(111.76)	(119.77)	
in quake region	1979.840***	-510.751***		
	(227.95)	(73.57)		
calls_gross	0.068***	0.074***	0.064***	0.103***
	(0.01)	(0.00)	(0.01)	(0.01)
me2u_val_out	0.600***	0.677***	0.637***	0.527***
	(0.10)	(0.03)	(0.03)	(0.04)
_cons	-217.559	656.197***	1094.820***	719.627
r2	0.728	0.702	0.729	0.753
N	154	74,895	74,895	74,895



Outcome: Value of incoming airtime sent **to** people near the tower.
"in quake region" is defined as towers within .3 degrees (roughly 20 miles) of the epicenter. Results hold with "near epicenter" re-defined anywhere in interval 10 miles – 50 miles.
Unconditional mean of outcome: in earthquake region over 2 weeks prior=540.9
In earthquake region = 857.20; full dataset = 1183

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Results: Interpretation & Robustness

- Interpreting the regression results
  - If Me2u transfers scale linearly with number of users: we expect **$11,000 - $22,000** to be sent in response to a current-day earthquake in **Rwanda**
    - (2,500 users/day in 2/2008, vs with 50,000 in 12/2008, and 500k-1M now)
  - For a similar earthquake in Kenya: we would expect **$13 million** to be sent in response to a current-day earthquake **in Kenya**
    - ($1,500 transferred per day in RW at time of quake, $200 million per day in Kenya)
- Robustness
  - Robust to a variety of estimation strategies, levels of aggregation, etc.
- In context?
  - The alternatives are expensive, inefficient, and intermittent
  - 90% savings in transaction costs
  - We interpret these effects as a lower bound
    - As mobile money becomes more prevalent, more liquid, we expect observed effects to increase substantially (network effects)

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Heterogeneity: Who benefits?

- Individual-level regression with interaction terms
  - Who receives more *in general*?
  - Who receives more *because of the earthquake*?

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Results: Individual level + heterogeneity

	(1) Basic	(2) degree	(3) Wealth	(4) "Urban" contacts	(5) "Rural" contacts
Shock	10.167***	12.330**	12.226***	16.512	9.669***
	(0.88)	(4.01)	(3.75)	(14.11)	(1.14)
Day of quake	-0.812***	-0.716***	-0.287	-0.741***	-0.786***
	(0.09)	(0.12)	(0.22)	(0.16)	(0.18)
In quake region	-1.946*	-1.731**	-1.990***	-0.459	-2.058*
	(0.80)	(0.55)	(0.32)	(0.54)	(0.92)
Number of contacts		0.012***			
		(0.00)			
Contacts * Shock		0.053			
		(0.04)			
Wealth Index			3.420***		
			(0.15)		
Wealth Index* Shock			20.831***		
			(3.12)		
Kigali contacts				0.014***	
				(0.00)	
Kigali contacts * Shock				0.151	
				(0.14)	
Non-Kigali contacts					0.004***
					(0.00)
Non-Kigali* Shock					0.018***
					(0.00)
F		6455.007	5965.523	1988.535	469.575
N		13092544.000	13092544.000	13085330.000	13085330.000

Notes: Outcome is total value of airtime sent to a person on a single day. Double interaction terms (e.g. Degree * day of quake, Urban * In Quake Region) are omitted for clarity. Heteroskedasticity-robust standard errors in parentheses (clustered at district level). * p<0.05, ** p<0.01, *** p<0.001.

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Heterogeneity: Who benefits?

- Wealth proxy
  - Wealthy people receive more *in general*
  - Wealthy people receive even more *because of the earthquake*
- Size of social network
  - People with lots of contacts receive more *in general*
    - Probably because they are heavy phone users
  - People with lots of contacts receive no more *on the day of the quake*
- Urban/rural differences
  - People with contacts *in Kigali* receive more in general
  - People with contacts *outside Kigali* receive more because of quake
- Geography of social network
  - People with contacts *at any* distance receive more in general
  - People with contacts *close to but not inside the earthquake region* receive more because of the quake

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Summary and conclusions

1. Highly significant amount of mobile money flowed into regions affected by the earthquake
2. "Benefits" are heterogeneous
   - Wealthy receive the most because of the quake
3. Heterogeneity looks like risk sharing
   - Remittances: would expect flows from Kigali->epicenter (not observed)
   - Altruism: would expect flows from rich->poor (not observed)
   - Risk sharing: would expect flows in reciprocal relationships (observed)

- Thanks for listening!

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The Price Effects of Cash Versus In-Kind Transfers*

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Preliminary version

Abstract

This paper examines how cash and in-kind transfers into partially-closed economies 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 are also predicted to fall for goods that are substitutes for 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, given that recipients are net consumers 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.

*We thank Rebecca Dizon-Ross and seminar participants at Stanford, Houston/Rice, and the MOVE Conference on Development for helpful comments. Authors' contact information: jcunha@nps.edu; degiorgi@stanford.edu; jayachan@stanford.edu

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 in a partially-closed economy, where 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 increases local supply.[1] Thus, 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 increase efficiency. A further effect of the lower prices induced by in-kind transfers 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 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 in-kind transfers cause the less needy to self-select out of the program (Coate, 1989; Besley and

[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.

Coate, 1991). In this case, the pecuniary effects are an unintended consequence, one that might significantly enhance or diminish the program goals 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, for an in-kind transfer, the supply influx will also affect the demand for goods that are substitutes or complements with 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 equal to 12 percent of the direct benefit.[2] The price increase caused by cash transfers offsets the direct transfer by 11 percent, though this effect is imprecisely estimated. Choosing in-kind rather than cash transfers in this setting, hence,

[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.

generates extra indirect transfers to the poor that are worth 23 percent of the direct transfer itself.

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 less in the determination of prices. Second, there is likely to be less competition on the supply side in these villages, and prices will be more responsive to shifts in demand 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 for the interpretation that the supply side of the market is less competitive in remote villages. Since poorer villages tend to also be 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, closed economies 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.

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.

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

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 supply and demand. That is, instead of being infinitely elastic, the supply curve is positively sloped, with quantity increasing in price. In such a context, shifts in the demand for or supply of a good will affect its price (as well as those of substitutes and complements).

We begin by describing the perfectly competitive case and discuss imperfect competition below. In addition, we focus on the short-run equilibrium of the market, where we assume that local suppliers cannot adjust capacity instantaneously. 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, where they are manufactured. 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. At the end of the section, we discuss how the market would likely adjust in the longer run.

Figure 1 depicts the market for a normal good in such an economy. 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] Thus, 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 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. 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]We are assuming that 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 that resale is costless. In this case, the cash value of the transferred goods is simply the market value. If 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).

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, one can sign the price effect of in-kind transfers relative to cash transfers. 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, 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 a marginal revenue curves and relabel the supply curve as marginal cost, then one obtains the same comparative static that a cash transfer increases prices.

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 downward shift 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, 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). Part of consumer demand continues to be met inefficiently by the local suppliers, but part is satisfied by the welfare-maximizing (not profit-maximizing) government.

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 who have constant marginal cost c and face linear demand $p = d - Q$. The equilibrium price is $p = (d + Nc)/(N +$

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

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 our benchmark competitive case, another set of testable implications arises from the elasticity of supply. 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 important factor affecting the elasticity of supply is the degree of openness of the local economy. In our setting, more rural and remote villages would likely be more closed economies. 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 these openness-related supply-elasticity predictions and the predictions about imperfect competition above, we compare more geographically isolated villages (longer travel time to major markets) to villages that are closer to major

markets. 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).[7] 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:

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

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 practice 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

[7] 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.

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.

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 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 centers. 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.[8] In the longer term, however, it is quite possible that the price effects would diminish as local supply decreases and net supply is left almost unchanged. 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 in our setting 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.

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 rural villages throughout Mexico.[9] 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.[10]

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

[8] 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).

[9] 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.

[10] Diconsa stores set their own prices but receive a government transportation cost subsidy.

brands and are typically available in local food stores.

Concurrent with the national roll-out of the program, 208 villages in southern Mexico were randomly selected for inclusion in an experiment.[11] 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).[12] Approximately 90 percent of households in the in-kind and cash villages were eligible to receive transfers (and received them).

In the in-kind experimental villages, the transfer comprised the seven basic items and, to the best of our knowledge, the following three supplementary goods: lentils, breakfast cereal, and either canned tuna fish or canned sardines. However, it is possible that in-kind villages received different supplementary items in some months. Thus, in our analysis below, we sometimes separate the basic PAL goods from the supplementary ones.

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 by and large not produced locally.[13] Thus,

[11] 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.

[12] 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.

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

the main 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.[14] 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.

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; the pre-program store price data are often missing. 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 about in the store survey.[15] 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. If 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

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.

[14] 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.

[15] 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.

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.[16] 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 9 PAL goods in our analysis. 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. Note that PAL in-kind transfer is large: On average the in-kind transfer represents 12 percent of household pre-program food consumption. 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 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 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 discuss this measure in more detail in the following section.

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

[16]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.

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 can address any imbalance by controlling for the pre-period unit price.

In some of our auxiliary analyses, we also 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. The omitted category in our regression are the cash villages, so our two predictions correspond to $\beta_1 < 0$, and $\beta_2 < 0$. The relative magnitude of β_1 and β_2 is theoretically ambiguous.

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

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, throughout the experiment, canned fish, cereal, and lentils were the supplementary goods. 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.[17] 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).

Next we estimate a before-after version of the model. The coefficient on lagged prices is 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 is downward biased by a factor of 0.84.[18] This suggests that the true coefficient on lagged prices 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

[17] 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.

[18] 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 $0.127/(0.127 + 0.025) = 0.84$.

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 very similar to the ones presented in the main text.

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 per household, as recipients are net consumers of these items. Note that the price changes affect all households, not just program recipients.[19] 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.[20] 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 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 shift in 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

[19] 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.

[20] 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.

of the good in control villages in the post-period.[21] 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.[22] 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 $\Delta Supply_g \equiv InKindAmount_g/TotalMarketSize_g$.[23] 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. Although no supply was transferred into cash or control 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.

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

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

[21] 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).

[22] 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.

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

explanation is that in-kind transfers are, by definition, large relative to the market size (high $\Delta Supply$) if a good is uncommon rather than a staple, e.g., lentils, breakfast cereal, fortified milk; these goods 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.

Note that these results using $\Delta Supply$ (columns 3 and 4) use a different source of variation than those 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 additional 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.[24] 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 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 only speculative, is the flypaper effect 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

[24] 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.

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.

Column 6 examines these hypothesized close substitutes. As expected, we find a larger price decline for them compared to the full set of non-PAL goods, though the magnitudes of the two coefficients are in fact quite similar. The weakness of these results may be due to our crude way of measuring substitutes.[25]

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. Our analysis views villages as separate closed economies in which local supply determines prices. However, in

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

the extreme of a perfectly open economy (horizontal supply curve), prices are exogenous to the village. In that case, a supply infusion should not affect prices. More generally, the prediction we can test is that the more closed the economy is (i.e., the steeper the supply curve), 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.

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.

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

Our measure of *Remote* is the time required to travel to a larger market. The measure is meant to capture the difficulty of transporting supply to the village and therefore the village's lack of integration with the outside economy. These remote villages also 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.)

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, more remote villages are poorer in our sample. 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 variable 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.[26]

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.[27]

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. More remote areas also tend to be poorer; our remoteness measures are strongly negatively correlated with per capita expenditures and other village-level poverty measures. Thus, the results above 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 developing countries broadly.

Testing between the imperfect-competition and closed-economy interpretations

As mentioned above driving distance to a large market is a proxy for how closed the economy is, but more remote areas also might have fewer grocery stores and less competition. While both have the same implication that, for example, price effects are larger in less developed areas, separating the two interpretations is important as they have different efficiency implications. Ideally, we would have measures of competition to empirically separate these hypotheses, but unfortunately no data on, e.g., 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. A goods market will be more open to the

[26]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.

[27]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.

outside economy if the good is produced elsewhere (or if it is exported). Meanwhile, for goods produced locally, the price effects should match the closed-economy predictions. In contrast, there should be less competition among sellers of packaged goods; these goods are not manufactured in the small villages we study and are sold through a small number of grocery stores in each village. However, there should be more competition for locally produced goods where, even though the grocery-store sector may be uncompetitive, the overall supply side includes many local producers selling their crops or livestock products. To summarize, the signature of the openness interpretation is 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 categorized goods (both PAL goods and non-PAL goods) as locally produced if there is any consumption out of own-production in the sample villages.[28] This measure of a locally produced goods is not village-specific, but instead is defined over the entire sample. By this definition, about 57% of goods have some local production. Columns 1 and 2 of Table 5 estimate equation (12) for the locally produced goods, using $ln(Travel\ Time)$ as the measure of remoteness. We do not find the negative interaction effects with *Remote* for the locally produced goods. 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. 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

[28] 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.

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 on 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 Table 6, column 1 we find that, as predicted, farm revenues are higher in cash villages relative to control villages (the negative coefficient on $Control$) by 1500 pesos (about 150 dollars) and 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. This is not 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 money per unit sold and because she sells more units.

The results in columns (1) to (3) suggest that the transfers have different net effects 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 (column 4). While the results 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 for households 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.[29] However, because of the pecuniary

[29]Because we do not know which households in the control villages would be transfer recipients (these

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 $\beta_2 > 0$ (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), these predictions are generally born out in the data. In cash villages, non-producer households decrease household labor supply by 14%. 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.[30]

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 the 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 (most recipients are consumers of these goods only). 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.

data were not recorded), in the analysis we do not distinguish between the 10 percent of households who are non-recipients and the 90 percent of households who received the transfer.

[30] 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 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 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 this 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 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 be the case that an uncompetitive private sector creates more surplus than if the government entered 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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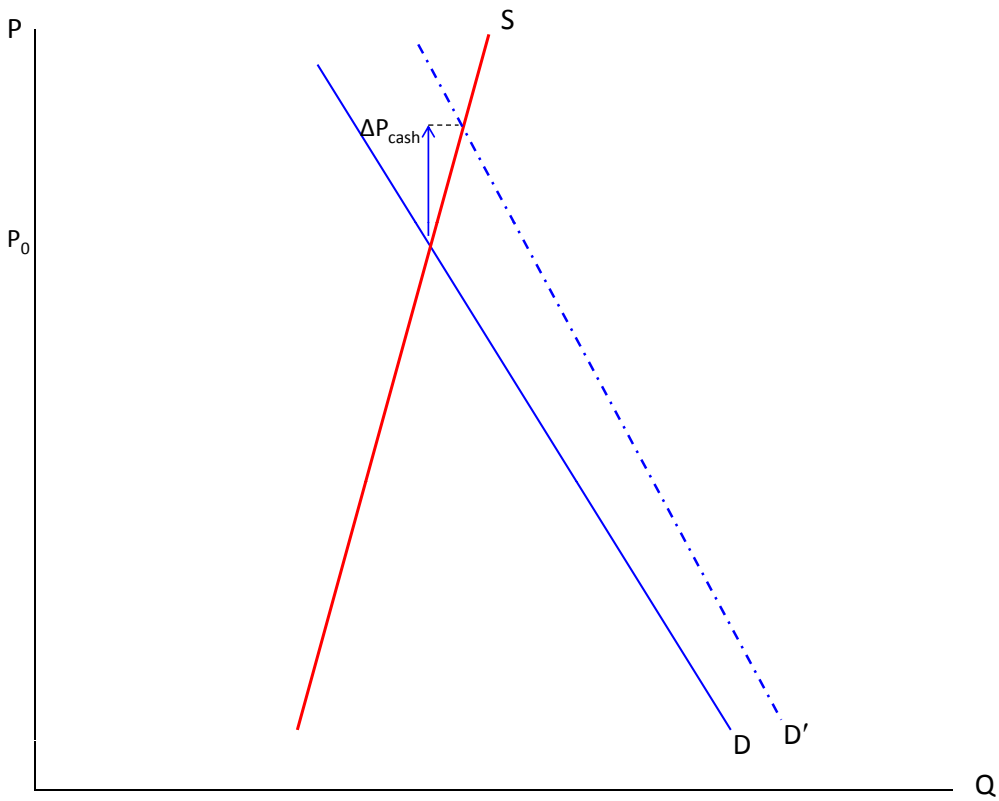
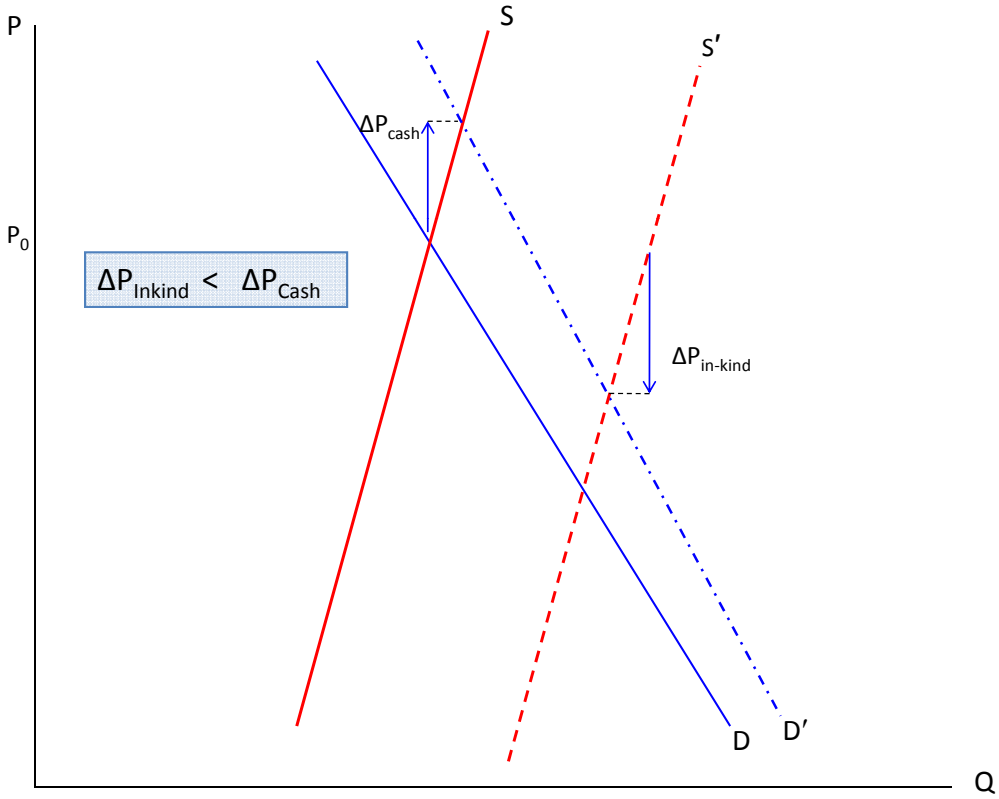
Figure 1: Effect of cash transfers on prices of normal goods



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

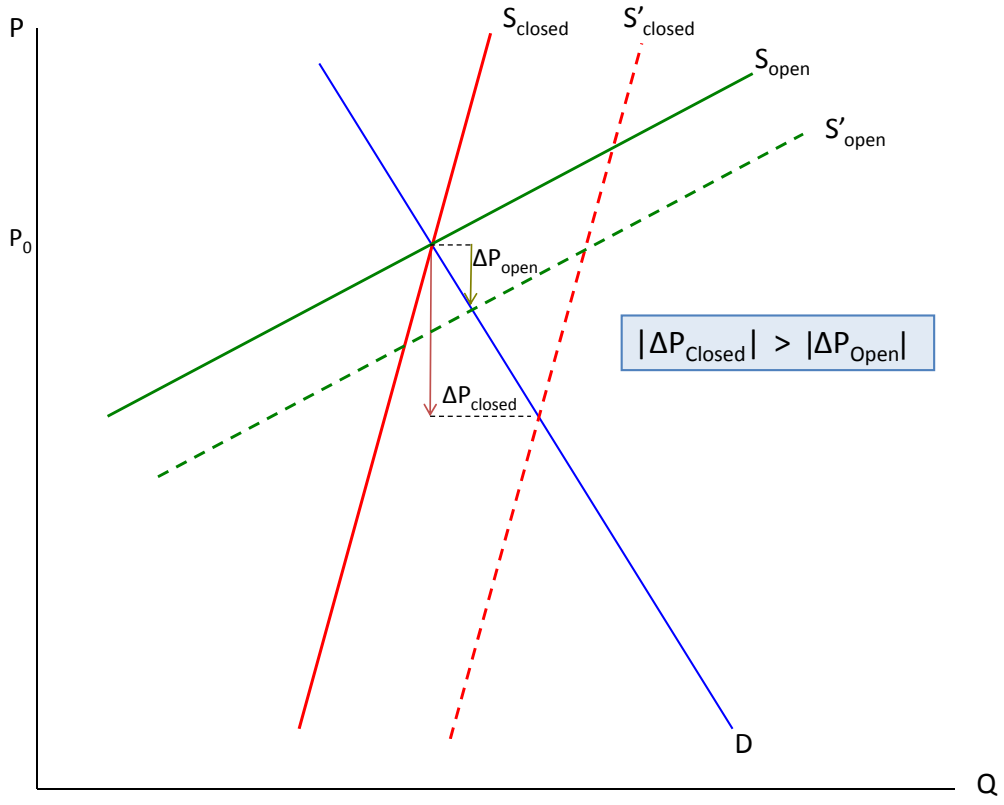


Figure 4: Villages in the PAL experiment

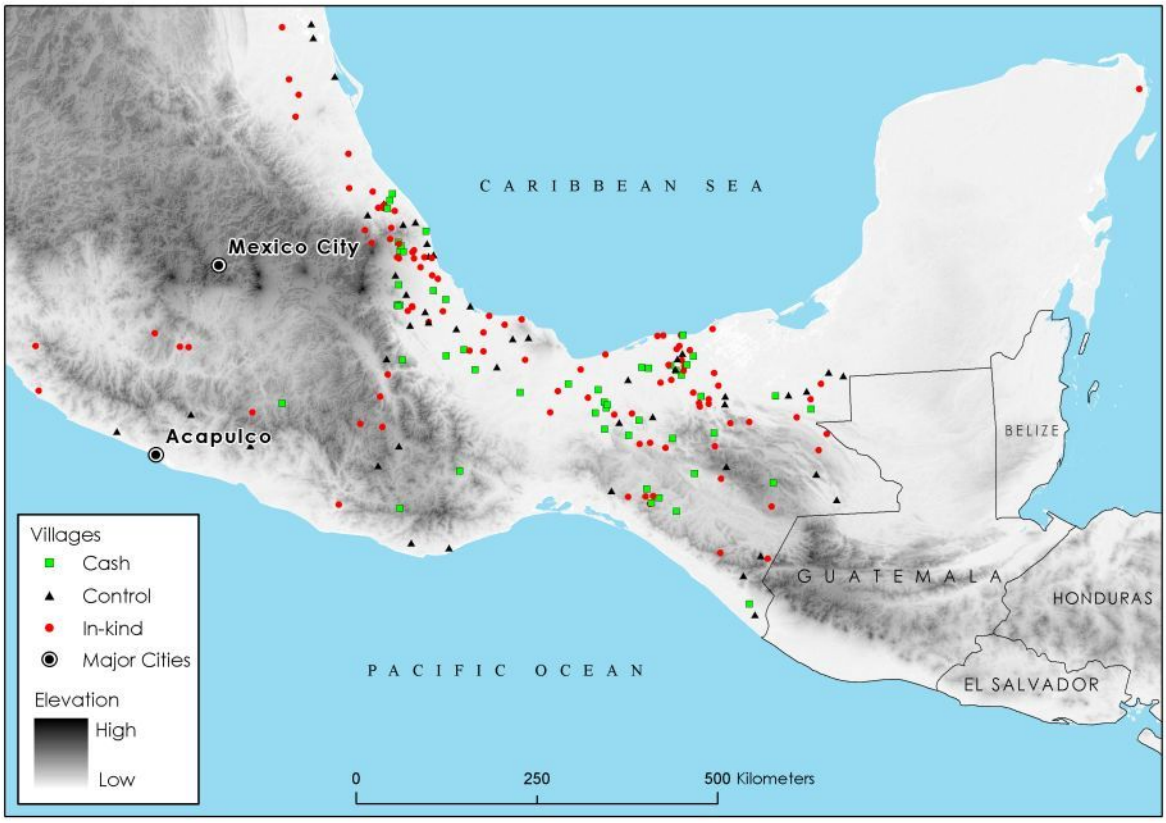


Table 1: PAL food box summary

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)	(2)	(3)	(4)	(5)	(6)
	Farm revenues	ln(Farm revenues)	Farm profits	ln(Total expenditures per capita)	ln(Hours of Work)	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.

Vulnerability of Household Consumption to Village-level Aggregate Shocks in a Developing Country*

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Abstract

Village-level aggregate shocks such as droughts and floods cannot be perfectly insured by risk sharing within a village. Then, what type of households are more vulnerable in terms of a decline in consumption when a village is hit by such natural disasters? This question is investigated in this study by using two-period panel data for the years 2001 and 2004 from rural Pakistan. We propose a methodology to infer the theoretical mechanisms underlying the heterogeneity of households in terms of their vulnerability, and focus on the difference between the across-household-type difference in marginal response to aggregate shocks and that in marginal response to idiosyncratic shocks. The empirical results obtained indicate that the sensitivity of consumption changes to shocks differs across household types, depending on the type of natural disasters. Moreover, land and credit access are effective in mitigating the ill-effects of various types of shocks. Household heads who are educated or elderly and households with a greater number of working members bear a larger burden of the village-level shocks; however, they are not vulnerable to idiosyncratic health shocks. It is revealed that these patterns may be explained by the coexistence of unequal access to credit markets and risk sharing among heterogeneous households in terms of risk tolerance.

JEL classification codes: O12, D12, D91.

Keywords: natural disaster, consumption smoothing, risk sharing, self-insurance, Pakistan.

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1 Introduction

In addressing the issue of poverty in developing countries, due consideration must be given to the vulnerability of households to natural disasters. Poor households are likely to suffer in terms of not only low levels of income and consumption on average but also from fluctuations in their income and consumption in the face of natural disasters. Such households are vulnerable to a decline in their welfare level because they are subject to substantial shocks, such as weather variability, and have limited ability to cope with such shocks (Dercon, 2005; Fafchamps, 2003). These conditions of poor households have led to an emerging literature on vulnerability measures in development economics (Ligon and Schechter, 2003; 2004; Kamanou and Morduch, 2005; Calvo and Dercon, 2005; Kurosaki, 2006a). According to these studies, poor people are considered to be vulnerable to shocks when (i) they cannot mitigate income volatility and (ii) their consumption expenditure is volatile over time (they lack reliable coping mechanisms).

Economic development in South Asia has been characterized by moderate success in economic growth and substantial failure in human development in relation to aspects such as basic health, education, and gender equality (Drèze and Sen, 1995). This characteristic is most apparent in Pakistan (World Bank, 2002). In July-August 2010, Pakistan experienced "the worst floods in its history... The floods have affected 84 districts out of a total 121 districts in Pakistan, and more than 20 million people — one-tenth of Pakistan's population... More than 1,700 men, women and children have lost their lives, and at least 1.8 million homes have been damaged or destroyed" (UN 2010, p.1). In this paper, the case of Pakistan is examined as an example of low-income countries subject to such natural disasters. Although the overall economic growth rates had improved during the 2000s in Pakistan, poverty reduction was slower than expected. Using a two-period panel dataset spanning three years from the North-West Frontier Province (NWFP),[1] one of the four provinces that comprise Pakistan, Kurosaki (2006a) and Kurosaki (2006b) indicated that rural households were indeed vulnerable to substantial welfare fluctuations. In addition, using a three-year panel dataset from Pakistan's Punjab, Kurosaki (1998) showed that farmers' consumption was excessively sensitive to idiosyncratic shocks that hit their non-farm income. Similar findings have been reported for other South Asian countries under agronomic conditions that are comparable to those of Pakistan, such as villages in the Deccan Plateau in India (Townsend, 1994; Kurosaki, 2001).

One shortcoming of existing literature is its focus on the welfare impacts of *idiosyncratic*

[1] In April 2010, the constitution of Pakistan was amended and the former NWFP was renamed "Khyber Pakhtunkhwa." In this paper, since all data correspond to a period before this constitutional amendment, the expression "NWFP" is used to refer to the current province of "Khyber Pakhtunkhwa."

shocks, such as loss due to theft or accidental injury. Such shocks imply that the experience of a particular household is different from and independent of other households. This focus has led to econometric specifications in which all village-level shocks are often controlled through fixed-effects, without fully analyzing information on the village-level co-movement of income and consumption. This is unsatisfactory, particularly when considering the growing influence of aggregate shocks on the welfare of villagers in the process of globalization and global warming. According to Sawada (2007), the impact of idiosyncratic risks and nondiversifiable aggregate risks that characterize a disaster are distinctively different, and the role of self-insurance becomes more important against large-scale disasters because formal or informal mutual insurance mechanisms are largely ineffective. However, research on the heterogeneity of the impact of natural disasters on household welfare and the economic mechanism underlying the heterogeneity is lacking.

This paper attempts to fill this gap in the literature by investigating the following question: Which type of households in rural Pakistan are more vulnerable to natural disasters such as floods and droughts in terms of a decline in their consumption during such disasters? In order to infer the microeconomic mechanisms underlying the heterogeneity of this vulnerability, this paper proposes a methodology that focuses on the difference between the across-household-type difference in the marginal response to aggregate shocks and that in the marginal response to idiosyncratic shocks. Since the marginal response to exogenous shocks is identified by the double difference in household consumption between two time periods as well as across villages, our approach may be considered to be a sort of quadruple differencing. The employment of this methodology is motivated by the possibility of a coexistence of risk sharing among villagers and intertemporal resource allocation using credit markets outside the village. Among existing studies, Asdrubali and Kim (2008) and Morten (2010) also proposed methodologies to analyze consumption smoothing with a focus on the difference between the marginal response to aggregate shocks and that to idiosyncratic shocks. However, we attempt to be explicit in specifying partial risk sharing while Asdrubali and Kim (2008) did not explicitly consider this aspect. Moreover, our approach directly focuses on consumption smoothing while Morten (2010) analyzed remittance transfer.

The remainder of the paper is organized as follows. The data used in this study is described in Section 2. The empirical model and empirical strategy employed for inferring the theoretical mechanism underlying the heterogeneous response of consumption to village-level shocks is presented in Section 3. The econometric results are presented in Section 4 and the conclusion is presented in Section 5.

2 Data

2.1 Characteristics of Pakistan's economy

Pakistan is a federal state comprising the four provinces of Punjab, Sindh, NWFP, and Balochistan. In general, Punjab and Sindh are regarded as economically advanced provinces, while NWFP and Balochistan are regarded as backward provinces. One difficulty in comparing the four provinces is the difference in their sizes. In terms of population as well as production, Punjab is the largest and accounts for over half of the national population. Sindh is the second largest and accounts for 23% of the national population, followed by NWFP, which accounts for 14%. Further, Balochistan is the largest in terms of area (approximately 45% of Pakistan's territory) but the smallest in terms of population (only 4% of the national population). The isolation and remoteness of Balochistan makes it difficult to obtain reliable data for this province.

Another dimension of spatial disparity in Pakistan is the difference in living standards between urban and rural areas. Even after adjusting for differences in prices, income and expenditure levels in urban areas are much higher than in rural areas. The urban-rural disparity is the greatest in Sindh, where the rural regions are lagging behind in terms of income, education, health facilities, and so on, and are characterized by a few big landlords and numerous landless sharecroppers (Naqvi et al., 1989; Perera, 2003).

Although declining, the share of agriculture in Pakistan's GDP continues to be high at over 20% (Government of Pakistan, various issues). There are two main crop seasons: *Kharif* and *Rabi*.[2] Since most land in Pakistan is semi-arid and arid, crop production in both seasons is highly dependent on irrigation. Despite the fact that Pakistan has the largest irrigated agricultural area among developing countries, agricultural output fluctuates substantially (Kurosaki, 1998). This is because the availability of canal water depends on rainfall in the Himalaya, which fluctuates every year; moreover, the availability of irrigation water at the farm level is disrupted frequently due to administration problems in the irrigation system. The majority of agricultural households combine crop farming and livestock raising as their main livelihood. Bullock cattle and she-buffaloes for milk are the most important large livestock animals, while small livestock animals including sheep and goats are important means of saving. In addition to the agricultural sector, the non-agricultural sector includes agro-industries (such as cotton-based textiles) and agro-services (such as trade of agricultural produce). Thus, due to the substantial dependence on the agricultural sector, the performance of Pakistan's macroeconomy as a whole fluctuates substantially, depending

[2]The *Kharif* crop is the monsoon or autumn crop for which harvests come in September-November; rice, cotton, and maize are major *Kharif* crops. The *Rabi* crop is the spring crop of the dry season for which harvests come in March-June; wheat and gram pulse are major *Rabi* crops.

on the weather.

Recent changes in Pakistanis' average consumption, inequality among them, and their poverty levels can be analyzed using repeated cross-section household datasets. For instance, Kurosaki (2009) characterized these changes using four rounds of nationally-representative, repeated cross-section data (PIHS/PSLM data) surveyed by the Federal Bureau of Statistics of the Government of Pakistan for 1998/99,[3] 2001/02, 2004/05, and 2005/06. His results revealed that the average consumption declined initially and increased in the two subsequent periods; the Foster-Greer-Thorbecke (FGT) poverty measures moved in the opposite direction; inequality decreased from 1998/99 to 2001/02, then it increased rapidly from 2001/02 to 2004/05. Nevertheless, since these are based on repeated cross-section data of households, we cannot have an idea of how many households actually experienced improvement in their welfare. For such analysis, we need panel data of households.

2.2 PRHS panel data

In this paper, we employ micro data from the Pakistan Rural Household Survey (PRHS), which is a unique panel dataset from Pakistan with a relatively large sample size. The survey was conducted jointly by the Pakistan Institute of Development Economics and the World Bank. The first survey (PRHS-I) was conducted in the period from September 2001 to January 2002; information was collected on agriculture-related activities for the crop seasons of *Kharif* 2000 and *Rabi* 2000/01 and that on consumption corresponding to the month preceding the survey. Approximately 2,700 rural households in all four provinces of Pakistan were included in the survey.

The second survey (PRHS-II) was conducted three years later in the period from August to October 2004; information was collected on the crop seasons of *Kharif* 2003 and *Rabi* 2003/04, and on consumption in the month preceding the survey. It must be noted that because of security problems and other reasons, sample households in NWFP and Balochistan were not re-surveyed.[4]

From the PRHS panel data, nominal consumption expenditure[5] per capita[6] in Pakistan rupees was calculated and then converted into real terms by dividing this value by the

[3]Pakistan's fiscal year as well as agricultural year begins on July 1 and ending on June 30 of the next year.

[4]In PRHS-I, approximately 450 sample households were surveyed in NWFP and approximately 400 sample households were surveyed in Balochistan.

[5]Since numerous farm households in Pakistan are subsistence-oriented and numerous rural laborer households are occasionally paid in kind, the value of these non-cash transactions were carefully imputed using village-level prices for calculating the consumption expenditure.

[6]To be precise, "per capita" implies "per adult equivalence unit," which is the unit adopted by the Government of Pakistan to establish the official poverty line. Individuals who are 18 years old or above are assigned the weight of 1.0 and others are assigned 0.8.

official poverty line.[7] This is known as the "welfare ratio" and is denoted as c_{it} below, where subscript i refers to individual i and t refers to the survey year. Individuals with $c_{it} \geq 1$ are classified as non-poor and those with $c_{it} < 1$ are classified as poor.

In this paper, a balanced panel of 1,609 households (929 in Punjab and 680 in Sindh) is employed, for which complete consumption information was available in both surveys. In PRHS-I, the number of sample households in Punjab and Sindh with complete consumption information was 1,874, thereby implying an attrition rate of 14%.

In PRHS-I, the sample households were randomly drawn from sample villages and the sample villages were selected as broadly representative of each province. Therefore, if the attrition was purely random, the PRHS panel data are broadly representative of rural Punjab and Sindh. Comparing the panel households with those that were excluded from PRHS-II, we found that the average c_{it} in PRHS-I among the excluded households was 12% lower than that among the households in the panel sample, and the difference was statistically significant (p value = 0.029). On the other hand, household size and composition were similar between the two groups (the difference was statistically insignificant). This suggests a possibility of weak attrition bias in that initially poor households were more likely to be excluded from the sample. Furthermore, those households that were affected so severely by exogenous shocks that they physically disappeared or became unable to be re-surveyed have not been included in the panel data. This implies that the portion of vulnerable households that is worst hit by natural calamities are not included in our dataset.

Table 1 presents three welfare measures based on the PRHS panel data: average of c_{it}, poverty measures, and Atkinson's (1970) inequality measures. Since there is a socioeconomic gap between the northern and southern parts of Punjab, we divide Punjab into two portions.[8] The changes between PRHS-I (2001) and PRHS-II (2004) are similar to the changes between PIHS 2001/02 and PSLM 2004/05, which are nationally representative. The poverty level gauged by three FGT measures decreased substantially from 2001 to 2004. The decrease was slightly larger in Sindh than in northern and southern Punjab, thereby reducing the

[7]The official poverty line of Pakistan is close to the level of 1 PPP$/day (1.25 PPP$/day in 2005 price), which is adopted widely in the international comparisons. The official poverty line was converted into the poverty line for each PRHS round in four steps: First, the poverty headcount rate for rural Punjab and Sindh was estimated at 38.5% using PIHS 2001/02 data and the official poverty line. Second, the poverty line for PRHS-I was fixed in order to generate the same poverty headcount rate using PRHS-I data for rural Punjab and Sindh, including the households that were excluded from PRHS-II. Third, an intertemporal inflation rate of 15.2% between PRHS-I and PRHS-II was estimated by weighting monthly CPIs by the number of observations for each corresponding month for PRHS-I and PRHS-II data. Fourth, the poverty line for PRHS-II was fixed by multiplying the PRHS-I poverty line by the inflation rate.

[8]There is no official division of Punjab into North Punjab and South Punjab. From among 35 districts in Punjab, 6 districts were surveyed in PRHS, and from among these six, 3 districts of Attock, Faisalabad, and Hafizabad are classified as "northern Punjab" and 3 districts of Bahawalpur, Muzaffargarh, and Vehari are classified as "southern Punjab" in this paper. Moreover, from among 22 districts in Sindh, the PRHS data include 4 districts of Badin, Larkana, Mirpur Khas, and Nawabshah.

gap between the two provinces. In both Punjab and Sindh, inequality increased during this period. This is similar to the change observed in nationally representative household surveys between 2001/02 and 2004/05. Thus, it is evident from Table 1 that there is a clear ranking of average economic well-being among the three regions: northern Punjab at the top, Sindh at the bottom, and southern Punjab in between.

2.3 Poverty transition at the household level

In order to utilize the advantage of panel data, Table 2 classifies sample households by their status of poverty *transition*. From among 1,609 sample households, 182 were below the poverty line in both periods ("chronically poor"), 342 were below the poverty line in PRHS-I but above it in PRHS-II ("getting out of poverty"), 176 were above the poverty line in PRHS-I but below it in PRHS-II ("falling into poverty"), and 909 were on or above the poverty line in both periods ("never poor"). In terms of individual population, 13.4% of the PRHS-I individuals belonged to the "chronically poor" households, 23.7% to the "getting out of poverty" households, 11.6% to the "falling into poverty" households, and 51.2% to the "never poor" households.

In terms of transition probability, 65.3% of households who were poor in PRHS-I became non-poor in PRHS-II, while 16.2% of households who were non-poor in PRHS-I became poor three years later in PRHS-II. Therefore, we observe a high level of poverty mobility during the survey periods. The vulnerability measured by the incidence of falls into poverty is thus rather high in rural Pakistan. Further, the transition probability from non-poor to poor was higher in Sindh (23.5%) than in southern Punjab (16.3%) and northern Punjab (9.9%). It must be noted that these falls into poverty occurred when the average poverty headcount ratio decreased. Thus, the aggregate figure conceals, from a micro viewpoint, the fact that certain households suffered from a severe decline in their overall welfare during the survey period.

A comparison of the three regions reveals that dwellers in rural Sindh were more vulnerable than those in rural Punjab. This regional contrast in vulnerability is robust to the application of other methodologies to the same panel data (see, e.g., Arif and Bilquees, 2008; Kurosaki, 2009).

Idiosyncratic and village-level negative shocks may possibly be responsible for the consumption decline of certain households when the nation experienced a consumption increase on average. As an indicator of idiosyncratic shocks, we constructed a dummy variable from the PRHS panel data for households whose members experienced a severe health shock due to injury or sickness that resulted in treatment in medical institutions during the two survey periods. Approximately 7% of the sample households experienced such shocks.

Further, with regard to village-level shocks, 24 variables were available in PRHS-II, all of which assessed the negative impact due to natural disasters on a five-point scale: 0 ("No effect": no report for crop damage), 1 ("Little effect": yield loss up to 10%), 2 ("Moderate": 10-25% loss), 3 ("Severe": 25-50% loss), and 4 ("Disaster": over 50% loss). Three types of disasters were investigated: drought, flood, and pest attack. Eight cropping seasons up to the survey reference period (i.e., from *Kharif* 2000 to *Rabi* 2003/04) were covered. Since we found that drought damage variables in a year are highly correlated with pest attack variables in the same year,[9] we exclude pest attack variables in the analysis below and focus only on droughts and floods.

Table 3 presents the incidence of these disasters from 2000 to 2004. It is evident that droughts are more common than floods — they occurred in all three regions with similar frequency. On the other hand, flood damage was not reported from northern Punjab, and only infrequently from southern Punjab. In other words, floods occurred most frequently in Sindh in the period. It may appear that the variation in drought and flood damage reported at the village level are in effect more aggregate, with little effective variation across villages within a region. In order to investigate whether or not this applies to our data, we examined the spatial correlations of drought and flood variables. For example, only 17.3% (21.3%) of the variation of the drought (flood) damage variable was explained by variation across the three regions. The rest were within-region and between-village variations.[10] Such variation will be utilized in identifying the effects of village-level shocks on overall household welfare.

3 Analytical Framework

3.1 Empirical model

One shortcoming of the transient poverty analysis in Table 2 is that it does not take into account changes in household consumption that may have occurred without crossing the poverty line. The consumption levels of some of the "chronically poor" may have been stable and slightly below the poverty line, while those of others of the "chronically poor" may have been fluctuating annually. In such a case, it may be preferable to regard the latter type as more vulnerable than the former type. Another issue is that it is possible that some of the observed changes in consumption levels were anticipated by the household. If this is the case, the observed changes in consumption must be decomposed into anticipated and unanticipated components. Thus, we regress consumption changes on the initial characteristics

[9] The correlation coefficients between drought damage and pest attacks were in the range from 0.363 to 0.741, all of which were statistically significant at the 1% level, while those between drought and flood damage were in the range from -0.199 to -0.015, all of which were not statistically significant at the 5% level.

[10] The number of sample villages in each region is 23 in northern Punjab, 25 in southern Punjab, and 46 in Sindh.

and variables that capture idiosyncratic and village-level shocks that were unexpected by the household. Since there are only two periods in our panel dataset, the empirical model is given by a cross-sectional regression model for household i:

$$\Delta \ln c_i = X_{1i}b_0 + Z_v X_{2i} b_1 + Z_i X_{2i} b_2 + \epsilon_i, \tag{1}$$

where $\Delta \ln c_i = \ln c_{it} - \ln c_{i,t-1}$; X_{1i} is a vector of household characteristics in period $t-1$ such as physical assets owned by the household, income sources, credit access, education level of the household head, and demographic composition; Z_v is a vector of village-level production shock variables (floods and droughts) for household i living in village v; X_{2i} is a subset of X_{1i} used as a shifter for the household's ability to cope with village-level shocks; Z_i is the idiosyncratic health shock; b_0, b_1, and b_2 are vectors of the parameters to be estimated; and ϵ_i is a zero mean error term. X_{1i} includes the intercept term and region dummies.

When the economy was hit by nation-wide negative shocks, parameter b_0 may be interpreted as a measure of vulnerability since it indicates the household attributes in X_{1i} that are associated with a larger decline in consumption in the face of nation-wide shocks (Ravallion, 1995; Jalan and Ravallion, 1999; Glewwe and Hall, 1998). However, since there was an overall growth in the economy in our dataset, it is preferable to interpret parameter b_0 as an indicator of which households were not able to keep up with the national growth trend.

Further, when only the intercept term is included in X_{2i}, parameter b_1 indicates the double-difference estimator[11] for the impact of drought (flood) on consumption; vector b_1 is expected to be negative. When region dummies are included in X_{2i}, the difference in vulnerability across regions can be examined. Moreover, when households' initial attributes are included in X_{2i}, vector b_1 indicates which household attributes are associated with a larger decline in consumption if the village is hit by a production shock Z_v. Thus, parameter b_1 is of main interest of this paper.

The model presented in equation (1) is an extension of the excess sensitivity of household-level consumption to idiosyncratic shocks after controlling for village-level aggregate shocks (Townsend, 1994; Kurosaki, 2006a). The extent to which household consumption responds to idiosyncratic shocks (parameter b_2) may be interpreted as one measure of vulnerability (Amin et al., 2003; Skoufias and Quisumbing, 2005). However, since our main motivation is to identify the impacts of village-level aggregate shocks, parameter b_2 itself is not of main interest in this paper. Further, since Z_i is orthogonal to Z_v by definition, the entire expression $Z_i X_{2i} b_2$ can be excluded and merged into ϵ_i without affecting our ability to obtain unbiased

[11] The first difference is across time (the dependent variable is the change in consumption) and the second difference is with respect to villages distinguished by drought and flood shocks.

estimates for b_0 and b_1.[12] Nevertheless, we include the term $Z_i X_{2i} b_2$ because it enables us to infer the theoretical mechanisms underlying the heterogeneity of household responses to village-level shocks. The main idea is to examine the difference between the across-household-type difference in marginal response to aggregate shocks (b_1 with respect to some household attribute) and the difference in marginal response to idiosyncratic shocks (b_2 with respect to the same household attribute). This approach may be termed a sort of "quadruple" differencing,[13] which is explained in greater detail below using a simple household model.

3.2 Inference of the theoretical mechanisms underlying the heterogeneous response to village-level shocks

Let W_{it} be the forward-looking welfare of household i in period t based on a standard model defined as

$$W_{it} = U_i(c_{it}) + E_t \left[\sum_{\tau=1}^{\infty} \left(\frac{1}{1+\delta} \right)^{\tau} U_i(c_{i,t+\tau}) \right], \tag{2}$$

where $U(.)$ is an instantaneous utility function that satisfies $U'(.) > 0, U''(.) < 0$, δ is the subjective discount rate, and $E[.]$ is an expectation operator. We assume the following simple budget constraint that comprises

$$y_{it} + (1 + r_{it}) s_{i,t-1} + x_{it} - c_{it} - s_{it} = 0, \tag{3}$$

$$y_{it} = y_i^P + y_t^T(Z_{vt}) + y_{it}^T(Z_{it}), \tag{4}$$

$$x_{it} = g_{it}(\theta_t, H_t), \tag{5}$$

where y_{it} (exogenous income flow to household i) is the sum of non-stochastic income (y_i^P), village-level aggregate transient income (y_t^T) a function of the village-level shock Z_{vt}, and idiosyncratic transient income (y_{it}^T) a function of the idiosyncratic shock Z_{it}; $s_{i,t-1}$ is net saving (credit if negative) from $t-1$ to t; r_{it} is the market interest rate on $s_{i,t-1}$; x_{it} is a net transfer receipt (payment if negative) of household i in period t from other members in the risk-sharing network; and $g(..)$ is a function for determining the net transfer receipt, which has as arguments vector θ_t (rules and institutions that determine the risk-sharing rule) and vector H_t, which includes the history of exogenous shocks in income until period t and the endogenous decisions of consumptions, savings, and transfers until period $t-1$ by all members in the risk-sharing network.[14]

[12] Our regression results indicate that this is true, as shown below.

[13] The third difference is with respect to household types and the fourth difference is between village-level aggregate shocks and idiosyncratic shocks.

[14] See Ligon et al. (2000, 2002) for an example of function $g(..)$ under partial risk sharing due to limited commitment problems. In their cases, the function has a different form in both dimensions of i and t, and H_t includes all the history. In the classic full risk-sharing case analyzed by Townsend (1994), the functional form differs only in the i dimension, and H_t includes only the t period exogenous shocks.

In period t, household i chooses c_{it}, x_{it}, and s_{it} in order to maximize W_{it} subject to budget constraints. The optimal solution (the policy function) for the household regarding consumption may be expressed in the following reduced-form:

$$c_{it}^* = f_{it}(y_i^P, y_t^T, y_{it}^T, r_{it}, \theta_t, H_t). \tag{6}$$

Using the reduced-form expression, we investigate the marginal response of optimal consumption to transient income, $\beta_1 \equiv \partial c_{it}^*/\partial y_t^T$ and $\beta_2 \equiv \partial c_{it}^*/\partial y_{it}^T$, from which we derive empirical predictions regarding b_1 and b_2 in equation (1). Due to risk aversion, households would choose a completely smoothed consumption path even if their income is fluctuating, if such a path is feasible. However, intertemporal transactions are likely to suffer from credit (or liquidity) constraints, which are likely to be binding when the households' cash in hand is low, thereby resulting in non-smooth consumption (Deaton, 1991; 1992). Similarly, risk sharing among villagers in a village may suffer from information asymmetry (Ligon, 1998) and limited commitment (Ligon et al., 2000; 2002), thereby resulting in only partial insurance to idiosyncratic shocks to income because villagers cannot completely pool their income under such conditions.

(i) Hand-to-mouth economy

In an extreme case, when households have no means to smooth consumption (i.e., households do not belong to a risk-sharing network and cannot access any intertemporal resource allocation technology), their optimal consumption c_{it}^* simply equals y_{it} due to the assumption of $U'(.) > 0$. Therefore,

$$\beta_1 = \beta_2 = 1 \tag{7}$$

should hold for all households. Applying this to our empirical model (equation (1)), assuming that the income shock affects the transient income linearly at the same rate among all households, we obtain the empirical relation $b_1^A = b_1^B < 0$ and $b_2^A = b_2^B < 0$; the absolute values obtained for the four parameters are all very large. This implies that due to the absence of consumption-smoothing opportunities, household consumption declines significantly when a household is hit by idiosyncratic or village-level shocks.

(ii) Full risk sharing with no intertemporal technology

We assume the constant relative risk aversion (CRRA) preference, i.e., $U_i(c_i) = \frac{1}{1-R_i} c_i^{1-R_i}$, where R_i is an Arrow-Pratt coefficient of relative risk aversion (heterogeneous risk preference).[15] Under this assumption, the optimal solution under full risk sharing can be charac-

[15] Since Kurosaki (2001) found no evidence for heterogeneous time preference among South Asian households, this paper assumes a homogenous time preference.

terized by

$$\ln c_{it} = -\frac{1}{R_i}\ln\mu_t + \frac{1}{R_i}\ln\lambda_i + \frac{1}{R_i}t\ln\frac{1}{1+\delta} = \alpha_i'\bar{\ln} c_t + \beta_i', \tag{8}$$

where μ_t is the Lagrange multiplier associated with the resource constraint in the risk-sharing group in period t, λ_i is a Pareto-Negishi weight for household i, $\bar{\ln} c_t$ is the group mean of log consumption, and α_i' and β_i' are defined as

$$\alpha_i' \equiv \frac{1}{R_i}\left[\frac{1}{N}\sum_j \frac{1}{R_j}\right]^{-1}, \tag{9}$$

$$\beta_i' \equiv \frac{1}{R_i}\left[\ln\lambda_i - \frac{1}{N}\sum_j \alpha_j \ln\lambda_j\right], \tag{10}$$

where N is the number of households in the risk-sharing group. Equation (8) intuitively indicates that optimal consumption comprises a variable portion that is proportional to the group mean consumption at the rate of α_i' and a fixed portion β_i'. Equation (9) implies that when household i is more risk-averse than the group average in the sense that $\frac{1}{R_i} < \frac{1}{N}\sum_j \frac{1}{R_j}$, α_i' becomes smaller than unity; in other words, the household's share in variable consumption is smaller than the group average. Equation (10) implies that the risk-sharing group allocates consumption to households according to the size of λ_i. Although the weights can assume any positive values under the social planner's optimization framework, there exists a mapping from the consumption allocation under a full-information competitive equilibrium to the consumption allocation under the social planner's problem with a specific vector of λ. Under such competitive equilibrium, wealthier households who can contribute more to the group income pool on average are assigned higher λ_i and hence have higher consumption.

Therefore, if all households in the risk-sharing network have a homogeneous risk preference,

$$\beta_1 = 1, \qquad \beta_2 = 0 \tag{11}$$

should hold for all households. The empirical implication of this is that $b_1^A = b_1^B < 0$ and $b_2^A = b_2^B = 0$.

On the other hand, if the households have a heterogeneous risk preference,

$$\beta_1^A > 1 > \beta_1^B, \qquad \beta_2^A = \beta_2^B = 0 \tag{12}$$

should hold, where household A is relatively risk-loving and B is relatively risk-averse. In our empirical model, $b_1^A < b_1^B < 0$ and $b_2^A = b_2^B = 0$ should hold.

(iii) Access to an external credit market with no risk sharing

Under the permanent income hypothesis with infinite time horizon and a quadratic utility function, the optimal marginal response of consumption to completely transient income (y_t^T

and y_{it}^T) is equal to $r/(1+r)$ (Deaton, 1992). With no risk sharing, the distinction between village-level and idiosyncratic shocks is not significant for the household. Therefore, if all households have homogeneous access to the external credit market,

$$1 > \beta_1 = \beta_2 = r/(1+r) > 0 \tag{13}$$

should hold for all households. Thus, the empirical implication is that $b_1^A = b_1^B < 0$ and $b_2^A = b_2^B < 0$. This is qualitatively similar to case (i); however, the absolute values of the four parameters are all small due to intertemporal smoothing. Therefore, in empirical exercise, this case could be easily distinguished from case (i).

On the other hand, if households have heterogeneous access to the credit market and are faced with different interest rates, effectively,

$$1 > \beta_1^A = \beta_2^A = r^A/(1+r^A) > r^B/(1+r^B) = \beta_1^B = \beta_2^B > 0 \tag{14}$$

should hold, where the interest rate for household A is higher than that for household B. In terms of the empirical model of this paper, $b_1^A < b_1^B < 0$ and $b_2^A < b_2^B < 0$.

Although the above relations are derived under the restrictive assumption of a quadratic utility function and perfect access to an external credit market, their key characteristics — those with better access to credit are better able to mitigate the ill-effects of shocks and this ability should not differ between the ability to deal with village-level aggregate shocks and against idiosyncratic shocks — are likely to hold under less restrictive assumptions as well.[16]

(iv) Combining credit market with risk sharing

It is not an easy task to model situations where risk sharing and intertemporal resource allocation coexists for a household. The simplest case is when households form a full risk-sharing network and have access to the external credit market. In this case, insurable shocks of y_{it}^T are completely smoothed through village-level risk sharing while uninsurable shocks of y_t^T are partially smoothed through intertemporal resource allocation at the lowest interest rate among the villagers (r^*). Thus, the optimal solution should satisfy

$$1 > \beta_1 = r^*/(1+r^*) > \beta_2 = 0 \tag{15}$$

for all households. This is the Pareto optimal allocation with the highest level of consumption smoothing among all the cases considered in this subsection. Qualitatively, its implication is that $b_1^A = b_1^B < 0$ and $b_2^A = b_2^B = 0$, which is the same for the case of full risk sharing among homogeneous households with no access to credit markets; however, the slope of $b_1^A = b_1^B < 0$ is less steeper than the case of risk sharing, thereby distinguishing this case from other cases.

[16] Numerical results showing this are available on request, using the CRRA utility case.

As analyzed by Ligon et al. (2002), access to the external credit market may cause the full risk sharing more difficult to sustain under limited commitment. This is because a household that happens to have a high transient income has an incentive to renege the risk-sharing contract and save the transient income, thereby leaving the risk-sharing network for self-insurance. The case described above is sustainable only in a community with a highly strong ability to avoid such reneges. Without such an ability, the limited commitment is likely to result in partial risk sharing. Under partial risk-sharing regimes with limited commitment, access to the external credit market may worsen the condition of households due to more partial risk sharing (Ligon et al., 2000), while transfers under such partial risk sharing appear as debt contracts (Ligon et al., 2002). In these cases, the relationship among β_1^A, β_1^B, β_2^A, and β_2^B depends on how we model $g(..)$ and the household's credit access.

3.3 Short summary of the empirical strategy

Let us summarize our empirical strategy. First, a simplified version of our empirical model with no cross term identifies the causal effect of natural disasters and health shocks on consumption through double differencing. Then by considering the third differencing with respect to household types and compare the third difference between village-level shocks and idiosyncratic shocks (the fourth difference), we can infer the economic mechanisms underlying incomplete consumption smoothing.

However, the theoretical inference in this paper is incomplete in two senses. First, the restrictions on b_1^A, b_1^B, b_2^A, and b_2^B are necessary conditions, not sufficient conditions. Second, the restrictions are only a partial characterization of possible patterns of coefficients. Thus, it is necessary that the theoretical inference be made more complete. The limitation that restrictions are only necessary conditions implies that the same relationship among b_1^A, b_1^B, b_2^A, and b_2^B could occur under a different mechanism as well. For example, if exogenous income shocks (aggregate or idiosyncratic) affect the household transient income disproportionally, depending on the household type, we may have estimation results that indicate a difference between b_1^A and b_1^B, and between b_2^A and b_2^B. This possibility will also be considered in interpreting the regression results in the next section.

4 Sensitivity of Consumption Changes to Village-level Shocks

4.1 Empirical variables

Since the main objective of this paper is to analyze the vulnerability of households in a low-income country like Pakistan to a decline in consumption due to a natural disaster, we exclude relatively rich households ($c_{i,t-1} > 4$ or $c_{it} > 4$) in the regression analysis. In addition, we also exclude households that experienced a drastic change in their demographic

structure (households in which the change in the number of household members was equal to or over 4). This reduced the sample size from 1,609 (Tables 1 and 2) to 1,293. Thereafter, the consumption was re-calculated after excluding the medical expenditure since it is highly correlated with Z_i (idiosyncratic health shock) and the increase in consumption due to $Z_i = 1$ does not imply an increase in welfare. In other words, the total consumption in the regression analysis is the consumption excluding durables, house rent, and medical expenditures.

As controls for household characteristics, vector X_{1i} includes variables such as physical assets owned by the household (farmland, livestock, sum of the value of durable consumption goods, transportation equipment, house buildings, etc.), income sources (number of male working members engaged in non-farm work, existence of remittance receipts, etc.), credit access, education level of the household head, and demographic composition (number of household members, female ratio among them, and dependency ratio).[17]

After attempting several methods of aggregating the sixteen variables presented in Table 3, we report the results with two aggregated variables for drought and flood in two agricultural years of 2002/03 and 2003/04, normalized between zero and one. The robustness of our results with respect to this definition will be investigated below. Since the consumption data in PRHS-II were collected in August-October 2004, the agricultural output in 2002/03 and 2003/04 should have had the most direct effect on household consumption. Production shocks that occurred before these two years may have affected the consumption level reported in PRHS-I. For this reason, we use the shocks in the last two years as village-level shocks that are exogenous to initial consumption and unanticipated by villagers.

Table 4 presents the summary statistics for empirical variables that have been compiled for this analysis. They are weighted by the household size in order to obtain individual-level means and standard deviations, since the regression analysis is conducted in order to gauge individual welfare.

4.2 Estimation results

4.2.1 Sensitivity of consumption to shocks

Table 5 presents the estimation results of equation (1), both excluding (specification (i)) and including cross-terms (specifications (ii)-(iv)). Examining specification (i), it is evident that among household characteristics X_{1i}, five variables have statistically significant coefficients: the size of owned land (negative), number of small livestock animals (negative),

[17] With regard to education and landholding, the use of dummy variables distinguishing zero and positive years of education or positive acreage of owned land was attempted as well; this yielded results that were similar to those reported in this paper. With regard to the access to non-farm jobs, variables characterizing female workers engaged in non-farm jobs were not included because the average was close to zero and the variation was small.

number of male household members who were employed permanently in regular non-farm jobs (positive), remittance receipt dummy (positive), and dependency ratio (positive). The finding that households with larger landholding or larger livestock were lagging behind in consumption growth appears to suggest that growth from 2001 to 2004 was based on non-agricultural sectors. It may be tempting to interpret this finding to indicate that households in which more members were employed in non-farm permanent employment jobs were less vulnerable to a stochastic decline in consumption. However, the positive coefficient may simply reflect the life-cycle improvement in earnings associated with non-farm permanent jobs (e.g., regular promotion). The positive impact of remittance receipt on consumption growth is also consistent with prior expectation. The finding that households with a greater number of dependent household members experienced higher growth in consumption may simply reflect the fact that children (the majority among the dependent members) require larger amount of consumption after they become older by three years.[18] All other variables are insignificant. The proxy variables for credit constraints have a positive sign, as expected from the theoretical model (Deaton, 1991); however, the coefficients were statistically insignificant. The impact of household characteristics remains qualitatively the same when we introduce the cross-terms of natural disasters and region dummies (see specification (ii) in Table 5). These patterns of parameter estimates for b_0 on X_{1i} are robustly found under different specifications. Therefore, parameter estimates for b_0 are not reported in the following tables in order to save space.

With regard to coefficients on village-level production shocks, the coefficients on natural disasters are all negative in specification (i). However, only the coefficient on floods is statistically significant: it indicates that households had to reduce consumption by 37% ($1 - \exp(-0.4654) = 0.3721$) when their village was hit by a flood that destroyed over 50% of *Kharif* and *Rabi* crops. This implies a substantial decline in welfare. On the other hand, the coefficients on drought damages and health shocks have smaller absolute values and are statistically insignificant. This indicates the existence of some sort of insurance mechanism against droughts and health shocks in the study area on average.

The contrast between droughts/health shocks and floods could be understood by the insurability of shocks within a region. Theoretically, it is easy to insure health shocks within a village since they are idiosyncratic. Drought shocks are more aggregate than health shocks; however, because droughts are highly common in rural Pakistan, villagers may have established an institution to insure against them across villages within a region. On the other hand, it is difficult to insure against floods because they disrupt across-village transportation

[18] When we subdivide the sample into the relatively rich and relatively poor by the median of the welfare ratio, *depratio* has a positive and significant coefficient only among the former. It is negative and statistically insignificant among the poor. This appears to support the life cycle interpretation.

and communication. With disrupted transportation and communication, the institutional arrangement becomes less effective. This is only a speculation and the examination of estimation results using cross-terms will enable us to examine the validity of this speculation.

In order to examine whether there are any regional differences in terms of the extent of consumption smoothing ability against natural disasters, specification (ii) in Table 5 permits the coefficient on Z_v to differ across the three regions. Since no incidence of flood was reported from northern Punjab, the cross-terms that include floods are only for southern Punjab and Sindh. With regard to the effect of droughts and health shocks, all coefficients remain statistically insignificant. Further, with regard to the effect of floods, only the coefficient for Sindh is significant. However, the coefficient for southern Punjab has a large absolute value, thereby suggesting that there is the potential of a negative impact; however, it is not statistically discernible due to the infrequency of floods in this region. In addition, the null hypothesis that the impact of shocks is the same in all regions is not rejected at the 10% level. Therefore, no spatial heterogeneity is found in marginal impacts of natural disasters and idiosyncratic shocks. For this reason, the cross-terms with region dummies are not included in the following specifications.

In order to further examine the heterogeneity in the marginal impact of a natural disaster, household-level characteristics were interacted with village-level shocks (specification (iii) in Table 5). From among the fifteen household-level variables, seven are chosen as potential shifters of the marginal impact. Four of them (size of land holdings, number of household members employed in permanent non-agricultural jobs, dummy for remittance receipts, and dependency ratio) are those variables in X_{1i} in equation (1) that have robustly significant coefficients. The other three (dummy for credit constraint in the formal sector, age of the household head, and education level of the household head) are those variables that were found to be associated with several measures of vulnerability analyzed by Kurosaki (2009). In specification (iii), the regression results including all these cross-terms are reported, while in specification (iv), the model was made parsimonious by deleting statistically insignificant interaction terms.[19]

The following results are revealed from the analysis. More landed households and those with more dependent members were more capable of isolating their consumption from a drought-driven income decline. In addition, the ill-effects of flooding are mitigated if a household is more landed, younger, and more educated. Further, with regard to the impact of idiosyncratic health shocks on consumption, greater land holding and access to formal credit help to mitigate the ill impact of such shocks on consumption. Moreover, access

[19]More concretely, we first retained those cross-terms with the 15% level of significance and re-estimated the model. We then retained those cross-terms with the 10% level in the second analysis and re-estimated the model again. The results of the third regression are reported as the final parsimonious specification.

to formal credit also mitigates the shock due to droughts and floods, although this is not statistically significant in the specifications reported in Table 5.

Now we make inferences on the theoretical mechanisms through the quadruple differencing approach that is explained in Section 3. First, those with relatively less landholdings and limited access to credit are vulnerable to a larger decline in consumption when hit by floods and droughts on the one hand, and by health shocks on the other. This is a pattern consistent with the regime of unequal access to credit markets. It may be interpreted that the amount of landholding has the effect of reducing household vulnerability by improving their ability in intertemporal resource allocation. Since the land sales market is thin in rural Pakistan, it is likely that this ability is due to the collateral and social value of land (Hirashima, 2008). The observed pattern among the coefficient estimates involving land is difficult to explain by the argument based on the heterogenous impact of exogenous shocks on household transient income, since the total income of more landed households must be affected proportionally more by floods and droughts as compared with less landed households. Moreover, the access to formal credit, by definition, improves the ability of households in intertemporal resource allocation; thus, its cross-terms may be interpreted similarly as those for land.

In contrast, households headed by educated and elder household heads and households with a greater number of working members are subject to a larger consumption decline when hit by floods or droughts. From the viewpoint of household ability in intertemporal resource allocation, this appears to be a puzzle. However, this may be clarified from the viewpoint of the theory of full risk sharing. Such households are less risk-averse than other households; thus, it is more efficient for them to bear greater aggregate risk (in return for higher expected values of transfers from the risk-sharing network). With regard to the effect of education, it is found that there is a greater consumption decline due to floods among more educated households, which suggests that educated households are able to behave in a less risk-averse manner in the optimal village-level risk sharing due to the fact that they possess greater human capital. As another support for this interpretation, none of the three shifters (age of household head, education level of household head, and dependency ratio) is statistically significant in $Z_i X_{2i}$. The overall pattern is in favor of the regime under full risk sharing among heterogeneously risk-averse households. At the same time, however, the observed pattern among the coefficient estimates could be consistent with the argument based on heterogenous impact of exogenous shocks on household transient income, if the household income of those with more educated and older household heads, and greater number of working members are less affected by droughts and floods.

Furthermore, the null hypothesis that the impact of village-level shocks is the same across different household characteristics is rejected at the 1% level. Therefore, the marginal

impacts of natural disasters are heterogenous, which is consistent with the co-existence of the unequal credit market access and full risk-sharing models among heterogenous villagers.

4.2.2 Robustness of the empirical results

The results in Table 5 were found to be robust to various alterations.[20] Most importantly, different definitions of natural disaster variables were attempted and yielded similar results (Table 6). In the first group of columns in Table 6, regression results based on the default definition are subtracted from those given in Table 5. In the second group of columns (alternative (1)), the results based on disaster variables corresponding to the larger disaster of the last two years instead of their averages are reported, since it is possible that only major disasters matter. In the last group of columns in Table 6 (alternative (2)), results are based on a specification using only the most recent disasters (indices corresponding to the last agricultural year instead of the averages of the last two years) since the impact of disasters may be short-lived.

Without cross-terms, the estimated patterns are similar to those given in Table 5 — only the flood variable has a statistically significant effect on consumption growth. Thus, our finding that floods are difficult to cope with and have a greater impact on consumption, while droughts and health shocks can be insured within a region is confirmed. The negative impact of floods is estimated with a slightly smaller value under different definitions of the disaster variables. Villagers had to reduce consumption by 32% (alternative (1)) or by 17% (alternative (2)) when their village was hit by a flood that destroy 50% or more of crops. Cross-terms of household attributes and droughts/floods reveal a pattern that is similar to that given in Table 5. Under alternative (2), in addition to health shocks, the damage-increasing impact of credit constraint is statistically significant with respect to droughts.

In order to examine whether our assumption of the orthogonality of health shocks to village-level natural disasters holds, we re-estimate the same regressions after deleting the $Z_i X_{2i} b_2$ term. The results of this re-estimation are similar to those already reported (Appendix Table 1). This is as expected since the health shock variables and flood/drought variables are not correlated (the bivariate correlation coefficient was not statistically significant even at the 20% level). Thus, our assumption appears to be valid.

In a different direction for the robustness check, different weights were employed in running the household-level regression. In the default specifications, we used the number of household members in the initial period as the weight to convert the regression results to become consistent with individual-based aggregates. Since there was a change in household size of certain households between the two surveys, weights based on the second survey

[20] Detailed results of these robustness checks are available on request.

and those based on the average of the two were attempted. The results obtained with this specification were almost identical to those reported here (not reported for the sake of brevity).

4.2.3 Vulnerability of food consumption

In order to infer the underlying mechanisms of the vulnerability of households from a different angle, we re-estimated the regression models by replacing the total consumption (excluding medical expenditure) by only food consumption. The regression results are summarized in Table 7, for which detailed results under the default definition of drought and flood variables are given in Appendix Table 2. As indicated in the lower portion of Table 7, the difference in the marginal impact of shocks across household types is highly similar to the one found for total consumption. In other words, the quadruple difference pattern remains the same even when only food consumption is considered. In this sense, the coexistence of consumption smoothing through credit markets and risk sharing is suggested from the dynamics of food consumption as well.

However, an interesting difference is found in the double difference pattern: the coefficient on floods is no longer significant, and assumes a slightly positive or slightly negative value depending on the definition of village-level shock variables; the coefficient on health shocks is highly positive and statistically significant at the 5% level. This implies that households increased consumption when hit by shocks, which is contrary to expectation. Across regions, the positive coefficient on health shocks was most evident in Sindh (specification (ii) in Appendix Table 2). Moreover, since flood damages were also concentrated in Sindh, the insignificant coefficient on floods is also mostly due to the difference in the food consumption dynamics in Sindh. After a more careful examination of the data, we have the following interpretation.[21]

The positive coefficient on health shocks and insignificant coefficient on floods could be due to a change in preference toward food. It is found that both health shocks and floods cause households to increase their budget share for food. Spending more on high-quality food for household members who have been injured and are seriously sick or taking more calories under exhaustive hygienic conditions when hit by floods appears to be rational behavior. It is likely that within-region and inter-village networks in rural Sindh may have contributed to the increase in food consumption in Sindh when villagers were hit by these disasters. Rural

[21] Another possibility could be the problem in the imputation of own-produced food consumption. Especially regarding floods, the local shock may have risen the local price of foods, resulting in a seemingly increased food consumption expenditure even when food consumption quantity did not change or declined. To check whether this is a serious problem, we re-estimated regressions using region-level prices in imputation or using subsample of households whose share of own-produced food consumption was low. The results were qualitatively similar to those in Table 7.

Sindh is known for the existence of big landlords who are closely connected and the patron-client relationships with such landlords at the top (Perera, 2003; Naqvi et al., 1989). In other words, landlord-based networks of patron-client relationships in Sindh could have served as such risk-sharing networks. Thus, the contrast between the total consumption dynamics and food consumption dynamics also suggests the existence of consumption smoothing through a kind of risk sharing across villages within a region.

5 Conclusion

This paper investigated the type of households in rural Pakistan that are vulnerable to natural disasters in terms of a decline in their consumption when their village was hit by natural disasters such as floods and droughts. The regression results associating observed changes in consumption to household characteristics and village-level disaster variables indicated the following results. The sensitivity of consumption changes to village-level shocks differentiated by the characteristics of households is different from that to idiosyncratic health shocks differentiated by similar characteristics. It was found that more landed households and households with greater access to formal financial institutions were less vulnerable to all these shocks. On the other hand, households in which the household head is educated and elderly as well as households with a greater number of working members bore a larger burden of village-level shocks, while they were not vulnerable to idiosyncratic health shocks. The coexistence of unequal access to credit markets and risk sharing among heterogeneous households in terms of risk tolerance may be responsible for these patterns.

There are several possible extensions that could be attempted in the future with regard to the impacts of village-level shocks. First, empirically distinguishing risk sharing, self-insurance, and the heterogenous impact of shocks on household income remains an important challenge. The evidence provided in this paper is only suggestive. Second, the actual mechanisms that enable intra-region and inter-village risk sharing must be identified. From anthropology literature on the rural society in Pakistan, it may be indicated that landlord-based networks of patron-client relationships are a possible mechanism. Since such networks are strongest in rural Sindh, this interpretation appears consistent with the regional contrast that Sindh villagers were protected against certain types of shocks but they suffered from the lowest average consumption level, while northern Punjab villagers enjoyed the highest average consumption level that was mostly self-insured. Third, the investigation of long-term welfare costs of natural disasters through (human) capital investment is highly recommended for further research.

What are the implications of the findings of this paper for the Pakistani Floods of 2010? Our best estimate for the impact of floods is a 20–40% decline in consumption, which is a

substantial reduction considering the already low levels of initial consumption. Nevertheless, these estimates must be interpreted as the lower bound since they are based on flood data where between-village variation in damages was large. When there were unprecedented floods all over the country and they have a similar effect on a majority of the villages, risk coping across villages becomes highly difficult due to disrupted communication and transportation, thereby resulting in a huge loss of welfare. The contrast found in this paper with regard to the impact of droughts and floods on household consumption indicates this possibility in a qualitative manner.

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Table 1. Average consumption, poverty, and inequality measures based on expenditures in Pakistan

	PRHS-I (2001)	PRHS-II (2004)
1. Average welfare ratio		
Punjab and Sindh pooled (rural only)	**1.465**	**1.846**
	(0.029)	(0.038)
By regions		
Northern Punjab	**1.848**	**2.190**
	(0.064)	(0.070)
Southern Punjab	**1.546**	**1.886**
	(0.065)	(0.099)
Sindh	**1.175**	**1.617**
	(0.028)	(0.043)
2. Poverty Measures		
Punjab and Sindh pooled (rural only)		
Headcount index	**0.372**	**0.259**
	(0.014)	(0.013)
Poverty gap index	0.0950	0.0680
	(0.0047)	(0.0043)
Squared poverty gap index	0.0354	0.0260
	(0.0023)	(0.0022)
Headcount index by regions		
Northern Punjab	**0.196**	**0.154**
	(0.020)	(0.019)
Southern Punjab	**0.361**	**0.267**
	(0.026)	(0.024)
Sindh	**0.490**	**0.318**
	(0.022)	(0.021)
3. Atkinson inequality measures		
Punjab and Sindh pooled (rural only)	**0.359**	**0.425**
	(0.012)	(0.013)
By regions		
Northern Punjab	**0.336**	**0.394**
	(0.019)	(0.022)
Southern Punjab	**0.359**	**0.461**
	(0.027)	(0.032)
Sindh	**0.305**	**0.392**
	(0.015)	(0.016)

Notes: The inequality aversion parameter for Atkinson's inequality measure is set at 3. Conventional standard errors are reported in parenthesis for the average welfare ratio and poverty measures, while bootstrapped standard errors (the number of replications is 500) are reported in parentheses for inequality measures. Statistics are weighted in order to make figures representative of individual-level summary statistics.

Source: Calculated by the author from PRHS panel data (NOB=1,609).

Table 2. Household-level poverty transition in Pakistan from 2001 to 2004

	Status in PRHS-II (2004)		
Status in PRHS-I (2001)	Below *z*	Above *z*	Total
Punjab and Sindh pooled (rural only)			
Number of sample households			
Below *z*	182	342	524
Above *z*	176	909	1,085
Total	358	1,251	1,609
Transition probability (%)			
Below *z*	34.7	65.3	100.0
Above *z*	16.2	83.8	100.0
Northern Punjab			
Number of sample households			
Below *z*	27	58	85
Above *z*	42	383	425
Total	69	441	510
Transition probability (%)			
Below *z*	31.8	68.2	100.0
Above *z*	9.9	90.1	100.0
Southern Punjab			
Number of sample households			
Below *z*	50	80	130
Above *z*	47	242	289
Total	97	322	419
Transition probability (%)			
Below *z*	38.5	61.5	100.0
Above *z*	16.3	83.7	100.0
Sindh			
Number of sample households			
Below *z*	105	204	309
Above *z*	87	284	371
Total	192	488	680
Transition probability (%)			
Below *z*	34.0	66.0	100.0
Above *z*	23.5	76.5	100.0

Note: "*z*" is the poverty line corresponding to the official one (see footnote 7).
Source: Calculated by the author from PRHS panel data.

Table 3. Incidence of village-level production shocks in Pakistan

	Distribution of damage index* in *Rabi* season (%)					Distribution of damage index* in *Kharif* season (%)				
	0	1	2	3	4	0	1	2	3	4
Drought in the last year (Kharif 2003 and Rabi 2003/04)										
Northern Punjab	47.1	7.1	9.5	36.3	0.0	47.9	7.1	12.9	32.2	0.0
Southern Punjab	0.0	34.1	41.4	24.4	0.0	4.8	24.9	45.3	12.9	12.1
Sindh	61.7	4.4	10.3	15.6	8.2	81.5	5.4	3.7	2.9	6.5
Drought in the year before the last year (Kharif 2002 and Rabi 2002/03)										
Northern Punjab	54.4	7.1	6.4	32.2	0.0	50.8	7.1	3.0	35.7	3.3
Southern Punjab	8.7	37.6	16.4	37.3	0.0	8.5	30.2	56.3	5.1	0.0
Sindh	84.0	0.0	4.8	7.5	3.7	76.7	6.7	6.6	4.1	5.9
Drought in Kharif 2001 and Rabi 2001/02										
Northern Punjab	50.8	7.1	3.0	35.7	3.3	47.7	7.1	9.5	35.7	0.0
Southern Punjab	22.6	65.9	7.3	4.2	0.0	29.3	50.5	20.3	0.0	0.0
Sindh	79.2	7.0	4.7	2.0	7.1	79.7	2.5	3.7	2.0	12.1
Drought in Kharif 2000 and Rabi 2000/01										
Northern Punjab	85.0	0.0	0.0	15.0	0.0	85.0	0.0	0.0	15.0	0.0
Southern Punjab	100.0	0.0	0.0	0.0	0.0	100.0	0.0	0.0	0.0	0.0
Sindh	89.8	5.1	1.1	2.0	2.0	89.8	5.1	1.1	2.0	2.0
Flood in the last year (Kharif 2003 and Rabi 2003/04)										
Northern Punjab	100.0	0.0	0.0	0.0	0.0	100.0	0.0	0.0	0.0	0.0
Southern Punjab	100.0	0.0	0.0	0.0	0.0	100.0	0.0	0.0	0.0	0.0
Sindh	94.3	0.0	0.0	0.0	5.7	72.1	5.7	4.0	3.9	14.2
Flood in the year before the last year (Kharif 2002 and Rabi 2002/03)										
Northern Punjab	100.0	0.0	0.0	0.0	0.0	100.0	0.0	0.0	0.0	0.0
Southern Punjab	100.0	0.0	0.0	0.0	0.0	90.7	4.8	0.0	4.5	0.0
Sindh	69.1	8.0	4.5	6.5	11.9	84.9	0.0	2.8	2.4	9.9
Flood in Kharif 2001 and Rabi 2001/02										
Northern Punjab	100.0	0.0	0.0	0.0	0.0	100.0	0.0	0.0	0.0	0.0
Southern Punjab	100.0	0.0	0.0	0.0	0.0	95.8	4.2	0.0	0.0	0.0
Sindh	87.1	0.0	0.0	5.1	7.7	91.2	0.0	2.3	1.1	5.4
Flood in Kharif 2000 and Rabi 2000/01										
Northern Punjab	100.0	0.0	0.0	0.0	0.0	100.0	0.0	0.0	0.0	0.0
Southern Punjab	100.0	0.0	0.0	0.0	0.0	100.0	0.0	0.0	0.0	0.0
Sindh	98.9	0.0	0.0	1.1	0.0	98.9	0.0	0.0	1.1	0.0

Source: Calculated by the author from PRHS panel data (NOB=1,609).
Note: * The index takes 0 (``No effect": no report for crop damage), 1 (``Little effect": yield loss up to 10%), 2 (``Moderate": 10-25% loss), 3 (``Severe": 25-50% loss), and 4 (``Disaster": over 50% loss). Since all of them are mutually exclusive, the sum of the percentage is 100.0.

Table 4. Summary statistics of empirical variables used in the regression analysis

Variable	Definition	Mean	Std.Dev.	Min	Max
Dependent variable					
dlnc	Log difference of the welfare ratio between PRHS-I and PRHS-II (consumption excluding durables, house rent, and medical expenditures).	0.175	0.611	-1.761	2.314
Explanatory variables: Household characteristics and idiosyncratic shocks					
landacre	Size of farmland owned by the household (acres).	4.947	11.679	0	102
livslrg	Number of large livestock animals owned by the household.	2.496	3.019	0	21
livssml	Number of sheep and goats owned by the household.	1.816	3.935	0	50
assets	Value of assets (durable consumption goods, transportation equipment, house buildings, etc.) owned by the household (Rs.1,000).	20.000	56.992	0	2001
nfe_perm	Number of male household members who were employed permanently by the private sector, government, or police.	0.239	0.561	0	5
nfe_casl	Number of male household members who were employed in non-farm activities on daily or contract basis.	0.429	0.742	0	4
remit	Dummy for a household that received remittances from family members living separately.	0.055	dummy	0	1
cc_fml	Dummy for a household that was constrained to the formal credit access.#	0.682	dummy	0	1
cc_inf	Dummy for a household that was constrained to the informal credit access.#	0.101	dummy	0	1
head_age	Age of household head (years).	47.639	14.283	14	99
head_sch	Education level of household head (completed years of schooling).	2.791	3.849	0	21
head_fem	Dummy for a female-headed household.	0.018	dummy	0	1
femratio	The ratio of females in the household size.	0.482	0.143	0	1
depratio	The ratio of dependent members (aged <15 and >60) in the household size.	0.476	0.186	0	1
popwt1	Household size (Nos.).	8.957	4.443	1	42
health_shock	Dummy variable for the household whose members experienced a severe health shock during the two survey periods resulting in medical treatment.	0.071	dummy	0	1
Explanatory variables: Village-level agricultural production shocks					
drought	Index variable* for crop damage due to drought in Rabi 04, Kharif 03, Rabi 03, and Kharif 02.	0.279	0.281	0	1
flood	Index variable* for crop damage due to flood in Rabi 04, Kharif 03, Rabi 03, and Kharif 02.	0.076	0.161	0	0.938

Notes: (1) The subsample used in the regression analyses is those households whose welfare ratio was lower than four in both PRHS-I and PRHS-II and whose size changed by less than or equal to three persons during the two surveys. Due to this selection, the number of households in this table is at most 1,293 (1,290 for cc_fml and cc_inf, and 1,243 for head_sch), against 1,609 in Tables 1 and 2.

(2) Means and standard deviations (Std.Dev.) are weighted by the household size in PRHS 1 in order to obtain individual-level summary statistics.

(3) All household-level variables are taken from the PRHS-I dataset, except for "remit", which corresponds to the remittance receipts in the agricultural year of 2003/04.

Households were regarded as constrained if they needed to borrow from the formal (informal) sector and applied for a loan but rejected; or, if they needed to borrow from the formal (informal) sector but did not apply for the loan because the credit institutions are too far away, there is no guarantee available, no collateral, excessive procedures, etc. The corresponding period for formal loans is "ever until 2000/01" while that for informal loans is "during 2000/01".
* The sum of index variables for the four seasons in the last two years in Table 3 divided by 16.

Source: Calculated by the author from PRHS panel data.

Table 5. Sensitivity of consumption changes to village-level production shocks in Pakistan

	Dependent variable: *dlnc* (change in log consumption)					
	(i) Without cross-terms			(ii) With cross-terms with region dummies		
Explanatory variables	Coef.		S.E.	Coef.		S.E.
Region fixed effects						
intercept	0.0555		(0.1123)	0.0532		(0.1145)
South.Punjab	-0.1097	**	(0.0474)	-0.2412	**	(0.1033)
Sindh	0.2321	***	(0.0508)	0.2544	***	(0.0626)
Household characteristics						
landacre	-0.0062	**	(0.0025)	-0.0058	**	(0.0024)
livslrg	-0.0041		(0.0068)	-0.0030		(0.0068)
livssml	-0.0126	**	(0.0063)	-0.0123	*	(0.0064)
assets	0.0003		(0.0002)	0.0002		(0.0002)
nfe_perm	0.0884	**	(0.0347)	0.0871	**	(0.0351)
nfe_casl	0.0077		(0.0254)	0.0094		(0.0257)
remit	0.1223	*	(0.0726)	0.1238	*	(0.0734)
cc_fml	0.0394		(0.0418)	0.0372		(0.0420)
cc_inf	0.0741		(0.0594)	0.0744		(0.0596)
head_age	0.0016		(0.0013)	0.0015		(0.0013)
head_sch	0.0032		(0.0051)	0.0027		(0.0051)
head_fem	-0.0198		(0.1099)	-0.0140		(0.1122)
femratio	-0.1597		(0.1230)	-0.1550		(0.1230)
depratio	0.2561	***	(0.0944)	0.2501	***	(0.0947)
popwt1	-0.0054		(0.0060)	-0.0059		(0.0060)
Village-level shocks						
drought	-0.0081		(0.0655)			
drought*North.Punjab				0.0193		(0.0926)
drought*South.Punjab				0.3164		(0.1970)
drought*Sindh				-0.1428		(0.1069)
flood	-0.4654	***	(0.1410)			
flood*South.Punjab				-1.0140		(0.9604)
flood*Sindh				-0.4286	***	(0.1450)
Idiosyncratic shocks						
health_shock	-0.0878		(0.0605)			
health_shock*North.Punjab				-0.1028		(0.1151)
health_shock*South.Punjab				-0.0886		(0.0939)
health_shock*Sindh				-0.0605		(0.0980)
F-stat for zero slopes#	4.46	***		3.76	***	
F-stat for homogenous impact#				0.90		
R-squared	0.090			0.093		

Notes: NOB is 1,241 (several households whose "head_sch" was missing were excluded). Estimated by weighted least squares with household size as weights. Huber-White robust standard errors are reported in parenthesis, with * 10%, ** 5%, and *** 1% statistical significance levels.

"F-stat for zero slopes" indicates the F statistics for the null hypothesis that the empirical model has no explanatory power. It is distributed as F(20,1220) for specification (i), F(25,1215) for specification (ii), F(41,1199) for specification (iii), and F(27,1213) for specification (iv), under the null. "F-stat for homogenous impact" indicates the F statistics for the null hypothesis of specification (i) against others. It is distributed as F(5,1215) for specification (ii), F(21,1199) for specification (iii), and F(7,1213) for specification (iv), under the null.

Source: Estimated by the author from PRHS panel data.

Table 5. Sensitivity of consumption changes to village-level production shocks in Pakistan (cont'd)

	Dependent variable: *dlnc* (change in log consumption)					
	(iii) With all cross terms with households' initial attributes			(iv) Parsimonious specification		
Explanatory variables	Coef.		S.E.	Coef.		S.E.
Region fixed effects	(Yes)			(Yes)		
Household characteristics	(Yes)			(Yes)		
Village-level shocks and their cross-terms with household characteristics						
drought	0.0986		(0.2936)	-0.4132	**	(0.1686)
drought*landacre	0.0119		(0.0075)	0.0139	**	(0.0069)
drought*nfe_perm	-0.0187		(0.1232)			
drought*remit	-0.3490		(0.2627)			
drought*cc_fml	-0.1979		(0.1435)			
drought*head_age	-0.0060		(0.0043)			
drought*head_sch	-0.0036		(0.0179)			
drought*depratio	0.5984	*	(0.3205)	0.7181	**	(0.3182)
flood	1.0307	**	(0.4614)	0.6296		(0.4108)
flood*landacre	0.0141		(0.0095)	0.0153	*	(0.0089)
flood*nfe_perm	0.1142		(0.3229)			
flood*remit	0.1280		(0.7966)			
flood*cc_fml	-0.1298		(0.2358)			
flood*head_age	-0.0270	***	(0.0090)	-0.0246	***	(0.0087)
flood*head_sch	-0.0393	*	(0.0227)	-0.0404	*	(0.0223)
flood*depratio	-0.4542		(0.5199)			
Idiosyncratic shocks and their cross-terms with household characteristics						
health_shock	0.0330		(0.2352)	0.0363		(0.0759)
health_shock*landacre	0.0109	**	(0.0051)	0.0111	**	(0.0050)
health_shock*nfe_perm	-0.1484		(0.0990)			(0.0834)
health_shock*remit	0.1932		(0.2002)			
health_shock*cc_fml	-0.2822	***	(0.1066)	-0.2773	***	(0.1004)
health_shock*head_age	0.0000		(0.0032)			
health_shock*head_sch	0.0164		(0.0127)			
health_shock*depratio	-0.0191		(0.2963)			
F-stat for zero slopes#	3.80	***		4.84	***	
F-stat for homogenous impact#	2.49	***		5.03	***	
R-squared	0.120			0.113		

Table 6. Robustness check with respect to the definition of production shock variables

	Default#			Alternative (1)			Alternative (2)		
	Production shock variables corresponding to the average of the last two years (Kharif 2002, Rabi 2002/03, Kharif 2003, and Rabi 2003/04)			Production shock variables corresponding to the larger of the last two years (Kharif 2002 and Rabi 2002/03, or, Kharif 2003 and Rabi 2003/04)			Production shock variables corresponding to the last year (Kharif 2003 and Rabi 2003/04)		
(i) Without cross-terms									
drought	-0.0081		(0.0655)	-0.0017		(0.0596)	-0.0963		(0.0605)
flood	-0.4654	***	(0.1410)	-0.3789	***	(0.0839)	-0.2167	**	(0.1087)
health_shock	-0.0878		(0.0605)	-0.0890		(0.0601)	-0.0799		(0.0606)
(ii) With cross terms with households' initial attributes, parsimonious									
drought*landacre	0.0139	**	(0.0069)	0.0128	**	(0.0054)			
drought*cc_fml							-0.2358	*	(0.1269)
drought*depratio	0.7181	**	(0.3182)	0.6309	**	(0.2914)	0.5174	*	(0.3010)
flood*landacre	0.0153	*	(0.0089)						
flood*head_age	-0.0246	***	(0.0087)				-0.0132	**	(0.0065)
flood*head_sch	-0.0404	*	(0.0223)	-0.0281	*	(0.0159)	-0.0485	***	(0.0185)
health_shock*landacre	0.0111	**	(0.0050)	0.0102	*	(0.0053)			
health_shock*cc_fml	-0.2773	***	(0.1004)	-0.2758	***	(0.1022)	-0.3188	***	(0.1043)

Notes: See Table 5 for the estimation methodology, number of observations, and list of explanatory variables not reported in this table. The mean (standard deviation) of the alternative shock variables are: Alternative (1), drought 0.332 (0.312), flood 0.119 (0.258); Alternative (2), drought 0.306 (0.304), flood 0.060 (0.189).

Specification (i) is subtracted from (i) in Table 5, and specification (ii) is subtracted from (iv) in Table 5.

Table 7. Sensitivity of food consumption changes to village-level production shocks in Pakistan

	Dependent variable: *dlncf* (change in log food consumption)					
	Production shock=Default		=Alternative (1)		=Alternative (2)	
(i) Without cross-terms						
drought	0.0192	(0.0653)	0.0236	(0.0601)	-0.0134	(0.0619)
flood	-0.0089	(0.1396)	-0.1064	(0.0856)	0.1287	(0.1092)
health_shock	0.1380 **	(0.0593)	0.1309 **	(0.0589)	0.1451 **	(0.0593)
(ii) With cross terms with households' initial attributes, parsimonious						
drought*landacre	0.0119 *	(0.0065)			0.0123 ***	(0.0047)
drought*head_age	-0.0120 ***	(0.0043)	-0.0087 **	(0.0039)	-0.0095 **	(0.0041)
flood*landacre	0.0200 **	(0.0079)	0.0161 ***	(0.0058)	0.0134 **	(0.0055)
flood*head_age	-0.0270 ***	(0.0083)	-0.0135 ***	(0.0049)	-0.0192 ***	(0.0063)
flood*depratio					-0.7853 *	(0.4697)
health_shock*landacre	0.0100 **	(0.0040)	0.0106 ***	(0.0036)	0.0099 **	(0.0040)
health_shock*cc_fml	-0.1936 *	(0.1096)			-0.1946 *	(0.1100)

Notes: See Table 5 for the estimation methodology, number of observations, and list of explanatory variables not reported in this table. The mean (standard deviation) of the dependent variable 0.176 (0.635).

Appendix Table 1. Robustness check with respect to the exclusion of idiosyncratic shock variables

	Dependent variable: *dlnc* (change in log consumption)					
	Production shock=Default		=Alternative (1)		=Alternative (2)	
(i) Without cross-terms						
drought	-0.0086	(0.0656)	-0.0025	(0.0597)	-0.0965	(0.0606)
flood	-0.4524 ***	(0.1402)	-0.3722 ***	(0.0835)	-0.2069 *	(0.1082)
(ii) With cross terms with households' initial attributes, parsimonious						
drought*landacre	0.0130 *	(0.0071)	0.0122 **	(0.0056)		
drought*cc_fml					-0.2385 *	(0.1271)
drought*depratio	0.7083 **	(0.3179)	0.6256 **	(0.2915)	0.5374 *	(0.3018)
flood*landacre	0.0153 *	(0.0090)				
flood*head_age	-0.0244 ***	(0.0086)			-0.0131 **	(0.0065)
flood*head_sch	-0.0421 *	(0.0222)	-0.0290 *	(0.0158)	-0.0488 ***	(0.0184)

Notes: See Table 5 for the estimation methodology, number of observations, and list of explanatory variables not reported in this table. All specifications exclude the terms associated with variable *health_shock* from the list of explanatory variables.

Appendix Table 2. Sensitivity of food consumption changes to village-level production shocks

	Dependent variable: *dlncf* (change in log food consumption)					
	(i) Without cross-terms			(ii) With cross-terms with region dummies		
Explanatory variables	Coef.		S.E.	Coef.		S.E.
Region fixed effects						
intercept	0.0071		(0.1151)	0.0054		(0.1157)
South.Punjab	-0.1616	***	(0.0476)	-0.2989	***	(0.1047)
Sindh	0.1969	***	(0.0520)	0.1648	***	(0.0627)
Household characteristics						
landacre	-0.0074	***	(0.0025)	-0.0070	***	(0.0024)
livslrg	-0.0036		(0.0069)	-0.0015		(0.0067)
livssml	-0.0159	***	(0.0056)	-0.0149	***	(0.0056)
assets	0.0002		(0.0002)	0.0002		(0.0002)
nfe_perm	0.0694	**	(0.0346)	0.0688	**	(0.0346)
nfe_casl	-0.0088		(0.0277)	-0.0018		(0.0282)
remit	0.0621		(0.0645)	0.0609		(0.0628)
cc_fml	0.0110		(0.0412)	0.0165		(0.0408)
cc_inf	0.0423		(0.0605)	0.0324		(0.0597)
head_age	0.0011		(0.0014)	0.0009		(0.0014)
head_sch	0.0041		(0.0055)	0.0039		(0.0056)
head_fem	0.0504		(0.0969)	0.0471		(0.0975)
femratio	-0.0739		(0.1264)	-0.0562		(0.1261)
depratio	0.2319	**	(0.0980)	0.2422	**	(0.0974)
popwt1	0.0012		(0.0066)	0.0000		(0.0064)
Village-level shocks						
drought	0.0192		(0.0653)			
drought*North.Punjab				0.0220		(0.0822)
drought*South.Punjab				0.4071	**	(0.2019)
drought*Sindh				-0.1023		(0.1175)
flood	-0.0089		(0.1396)			
flood*South.Punjab				-1.9646	**	(0.8922)
flood*Sindh				0.1109		(0.1429)
Idiosyncratic shocks						
health_shock	0.1380	**	(0.0593)			
health_shock*North.Punjab				0.0059		(0.0895)
health_shock*South.Punjab				-0.0567		(0.0939)
health_shock*Sindh				0.3360	***	(0.0936)
F-stat for zero slopes#	4.47	***		4.56	***	
F-stat for homogenous impact#				2.89	**	
R-squared	0.090			0.104		

Notes: See Table 5.

Appendix Table 2. Sensitivity of food consumption changes to village-level production shocks (cont'd)

	Dependent variable: *dlncf* (change in log food consumption)					
	(iii) With all cross terms with households' initial attributes			(iv) Parsimonious specification		
Explanatory variables	Coef.		S.E.	Coef.		S.E.
Region fixed effects	(Yes)			(Yes)		
Household characteristics	(Yes)			(Yes)		
Village-level shocks and their cross-terms with household characteristics						
drought	0.5372	*	(0.3043)	0.5340	**	(0.2239)
drought*landacre	0.0108		(0.0072)	0.0119	*	(0.0065)
drought*nfe_perm	-0.1096		(0.1160)			
drought*remit	-0.1531		(0.1908)			
drought*cc_fml	-0.1646		(0.1365)			
drought*head_age	-0.0107	**	(0.0044)	-0.0120	***	(0.0043)
drought*head_sch	-0.0140		(0.0183)			
drought*depratio	0.2727		(0.3374)			
flood	1.1117	**	(0.4582)	1.0508	***	(0.3848)
flood*landacre	0.0229	***	(0.0077)	0.0200	**	(0.0079)
flood*nfe_perm	0.0939		(0.3303)			
flood*remit	0.2208		(0.5739)			
flood*cc_fml	0.3851		(0.2391)			
flood*head_age	-0.0287	***	(0.0088)	-0.0270	***	(0.0083)
flood*head_sch	-0.0061		(0.0257)			
flood*depratio	-0.4197		(0.5517)			
Idiosyncratic shocks and their cross-terms with household characteristics						
health_shock	0.1480		(0.2528)	0.2022	**	(0.0882)
health_shock*landacre	0.0113	***	(0.0040)	0.0100	**	(0.0040)
health_shock*nfe_perm	-0.0004		(0.0987)			(0.0834)
health_shock*remit	0.1173		(0.1343)			
health_shock*cc_fml	-0.1164		(0.1120)	-0.1936	*	(0.1096)
health_shock*head_age	-0.0012		(0.0034)			
health_shock*head_sch	0.0241		(0.0166)			
health_shock*depratio	-0.0554		(0.3315)			
F-stat for zero slopes#	4.20	***		5.25	***	
F-stat for homogenous impact#	2.28	***		5.64	***	
R-squared	0.120			0.112		