

Supplemental Appendix

Risk Sharing Tests and Covariate Shocks: Drought, Floods, and Pests in Uganda

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DIRECT EVIDENCE ON THE EFFECTS OF COVARIATE SHOCKS

C1. Introduction

In this appendix we draw on sources of data not needed in the main analysis to try to directly explore the effects of covariate shocks on a variety of outcomes, with the aim of tracing whether and how such shocks affect production, income, and consumption; what mechanisms households use to cope with these shocks; the effects of covariate shocks on local prices (both levels and relative); and finally on food consumption for the household. We provide direct evidence that: (i) Preferences are nonhomothetic (expenditure shares vary with income and prices); (ii) Covariate shocks are indeed spatially correlated, in the sense that variation between clusters is greater than would be expected by random chance; (iii) People who reported having experienced a shock also tended to report declines in agricultural production and income; (iv) People reported coping with covariate shocks by reducing their consumption, using savings, or increasing labor, while they reported coping with idiosyncratic shocks by relying on help from relatives and friends; (v) Farmgate prices of locally produced goods increase significantly when a covariate shock is realized; (vi) Covariate shocks are associated with differences in the relative market prices consumers face; and (v) Covariate shocks of drought, floods, or pests are all associated with statistically significant changes in diet, and in particular a decreased demand for food obtained from animal sources such as dairy products and different kinds of meat.

C2. Preferences are nonhomothetic

With homothetic preferences the composition of the consumption portfolio is irrelevant, but for our approach the composition is central. Can we see direct evidence in the Ugandan expenditure data for or against homotheticity? Figure C.1 uses the fact that homothetic preference structures imply that *aggregate* expenditure shares (of the sort that appear in national income and product accounts) will be the same as *average* expenditure shares. The figure takes logs of each of these, and then differences. It is apparent that these shares are not the same at all, with goods to the right (e.g., beer) playing a larger role in the consumption portfolios of households with larger budgets, and goods to the left (e.g., sorghum) playing a more important role for poorer households.

C3. Incidence and covariance of shocks

INCIDENCE OF SHOCKS

The data we have from the LSMS surveys includes self-reported data on a variety of “shocks” the household may have experienced (Heltberg, Oviedo and Talukdar, 2015, discuss the collection of this sort of data in a variety of different household surveys), generally elicited using the prompt “Did you experience [SHOCK] in the last twelve months?”²² Where the answer is “Yes”, the respondent is asked about the timing of the shock (in what month did the shock occur, and for how long did it last). There is some modest variation across rounds in the language used to describe different sorts of shocks (see Appendix D), but one can distinguish two classes. The first is idiosyncratic, shocks which directly involve a particular household. Frequently reported idiosyncratic shocks include health issues (serious illness or accident), thefts of property, and death (death of “income earners” is reported separately from the death of other household members). The second is covariate, shocks which seem likely to affect many households within a local area, though not necessarily equally. Frequently reported covariate shocks are drought (or “irregular rains”), agricultural pests, floods,

²²There is some variation in the elicitation in different rounds, and the first 2005-06 round in particular uses a longer reporting period.

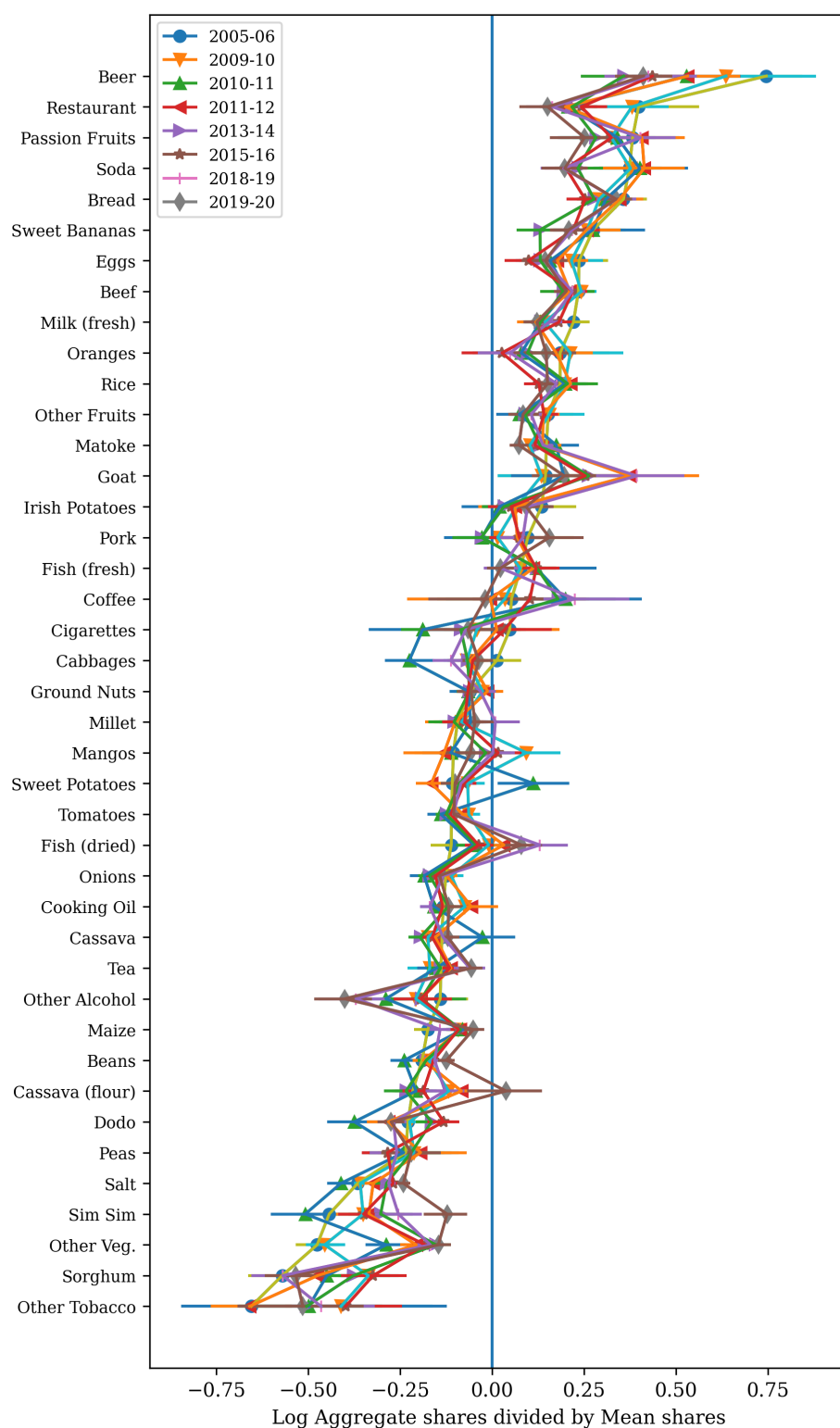


FIGURE C.1. LOG OF AGGREGATE SHARES MINUS LOG OF MEAN SHARES FOR DIFFERENT YEARS (ORDERED BY AGGREGATE SHARE AVERAGED ACROSS YEARS), WITH 95% CONFIDENCE INTERVALS.

and adverse agricultural prices (unusually expensive inputs or unusually low prices for output). Table C.1 reports the incidence of these shocks across different rounds.

TABLE C.1—REPORTED INCIDENCE OF DIFFERENT KINDS OF SHOCKS BY YEAR.

Shock	2005	2009	2010	2011	2013	2015	2018	2019	Pooled
Health	2%	12%	11%	5%	4%	2%	5%	6%	3%
Theft	11%	7%	3%	1%	2%	1%	2%	2%	2%
Death (non-earner)	13%	2%	2%	1%	2%	1%	1%	1%	1%
Death of earner	3%	0%	0%	0%	0%	0%	0%	0%	0%
Drought	39%	45%	26%	19%	29%	18%	22%	17%	13%
Floods	13%	2%	3%	5%	3%	1%	2%	3%	2%
Pests	15%	7%	2%	3%	2%	1%	4%	2%	2%
Prices	2%	3%	2%	2%	2%	0%	2%	0%	1%
<i>N</i>	3122	2974	2685	2843	3117	3305	3241	3076	24363

To get a notion of frequency from Table C.1, there are about 2800 households observed per year. Drought is by far the most frequently reported shock. Meteorologists regard 2005–08 as a period of major drought for Uganda, with 2010–11 and 2014–15 periods of minor drought (Byakatonda et al., 2021), consistent with the household reports in Table C.1. Health shocks (both illness and accidents) are the second most frequently reported shocks, followed by pests (which includes both crop pests and livestock disease), floods, death of non-earner, theft, and adverse changes in agricultural prices (i.e., increases in the costs of inputs; decreases in the value of outputs).

COVARIATE SHOCKS ARE SPATIALLY CORRELATED

We use several distinct sorts of evidence to establish that the “covariate” shocks we have data on do indeed covary. First, we test for geographical “clustering” of reported shocks within the Ugandan LSMS data. Second, we consider similar data from other LSMS surveys in Africa (eight countries in all), and construct maps of the reported incidence of different shocks, which we think provides informal but compelling visual evidence of spatial covariation. Third, we use these same data to estimate spatial autocorrelation functions, and establish that below a certain distance these estimated autocorrelations functions would be very unlikely to be observed under the null of no spatial correlation.

The sampling scheme for the LSMS surveys involve randomly selecting geographical clusters (e.g., villages or parishes), and then randomly selecting households from within those clusters. One would then suppose that the realizations of “covariate” shocks ought to involve significant variation “between” clusters. Here we test that supposition by decomposing the variance of reports of different shocks in each year into variation attributable to variation in reporting “within” clusters and “between” clusters.

Table C.2 provides estimates of the proportion of variance which is “between”, along with the p -values associated with the one-sided null hypothesis that this variance is no greater than would be expected if there was no clustering. Several features of this table are worth mentioning. First, there is statistically significant clustering (between variation) for drought in every year, for floods in most years, for pests in four of eight years, and for prices in two of eight years. Some of these failures to reject the null hypothesis may simply be due to the shock being reported infrequently; for example, there were only 12 reports of adverse agricultural prices in total in 2015–16, so the power of the test in that year is extremely low. Second, while for covariate shocks there’s most

TABLE C.2—PROPORTION OF TOTAL VARIANCE IN SHOCK REPORTS ATTRIBUTABLE TO VARIATION BETWEEN CLUSTERS, BY YEAR. FIGURES IN PARENTHESES ARE p -VALUES[†] ASSOCIATED WITH THE NULL HYPOTHESIS THAT THE “BETWEEN” VARIANCE IS NO GREATER THAN THE POOLED VARIANCE.

Round	Drought	Floods	Pests	Prices	Health	Death (non-earner)	Death (earner)	Theft
2005-06	0.11*** (0.00)	0.09*** (0.00)	0.08** (0.01)	0.06 (0.84)	0.06 (0.70)	0.08** (0.01)	0.07 (0.27)	0.07 (0.15)
2009-10	0.18*** (0.00)	0.12*** (0.00)	0.11*** (0.00)	0.11*** (0.00)	0.08 (0.45)	0.09* (0.09)	0.07 (0.75)	0.09 (0.12)
2010-11	0.21*** (0.00)	0.17*** (0.00)	0.13*** (0.00)	0.09 (0.67)	0.11** (0.02)	0.09 (0.75)	0.10 (0.28)	0.10 (0.27)
2011-12	0.19*** (0.00)	0.15*** (0.00)	0.12*** (0.01)	0.12*** (0.01)	0.09 (0.32)	0.10 (0.14)	0.12** (0.02)	0.11** (0.05)
2013-14	0.34*** (0.00)	0.27** (0.02)	0.19 (0.58)	0.17 (0.81)	0.23 (0.13)	0.22 (0.34)	0.16 (0.77)	0.19 (0.62)
2015-16	0.46*** (0.00)	0.29 (0.16)	0.19 (0.90)	0.25 (0.48)	0.23 (0.67)	0.23 (0.60)	0.17 (0.81)	0.29 (0.22)
2018-19	0.32*** (0.00)	0.27* (0.07)	0.20 (0.85)	0.20 (0.72)	0.24 (0.18)	0.23 (0.42)	0.10 (0.95)	0.23 (0.42)
2019-20	0.29*** (0.00)	0.28 (0.10)	0.19 (0.93)	0.21 (0.73)	0.26 (0.23)	0.30 (0.13)	0.19 (0.66)	0.29 (0.13)

[†] All p -values computed using randomized permutation tests, with 1000 draws.

* $p < 0.10$

** $p < 0.05$

*** $p < 0.01$

N Total observations are 28,588.

T All regressions involve 8 rounds, 4 markets, and an average of 550 clusters per round.

H A total of 5771 distinct households (an average of 3045 households per round).

often unanimity within a cluster (either no one says there’s been a covariate shock or everyone says there has been), so that within-variance is zero for these clusters, there are also many clusters in which some but not all households report experiencing the shock. Third, statistically significant clustering is observed less frequently for the shocks we’ve categorized as “idiosyncratic,” however, we still reject no clustering somewhat more frequently than we would expect under the null. Three of the four reported “idiosyncratic” shocks involve sickness or death, and it’s reasonable to suppose that a spatially correlated spread of disease may sometimes be implicated in these reports.

While there’s important variation across years in the proportion of households that report drought, we would like to know that the data are consistent with those shocks being covariate, in the sense that if a person in one cluster reports a particular sort of shock, then another nearby person is likely to report the same. Figure C.2 documents all the drought reports across the eight different LSMS-ISA countries over all the years (2005–20) for which we have data, while Figure C.3 and Figure C.4 do the same for floods and pests, respectively.²³ Dots correspond to sample clusters, and the “redder” the dot, the higher the proportion of households which reported drought.

Casual observation of Figure C.2 suggests not only that there are many dark dots (high proportions of households within a cluster agreeing that there’s drought), but also that darker dots tend to be close to each other, consistent with the notion that these reports really are reflecting covariate shocks.

²³For our examination of data across African countries we rely on the dataset described by Bentze and Wollburg (2024). Note that this dataset categorizes shocks somewhat differently than we do in our analysis of data from Uganda, and evidently uses a narrower definition of “floods” than we do.

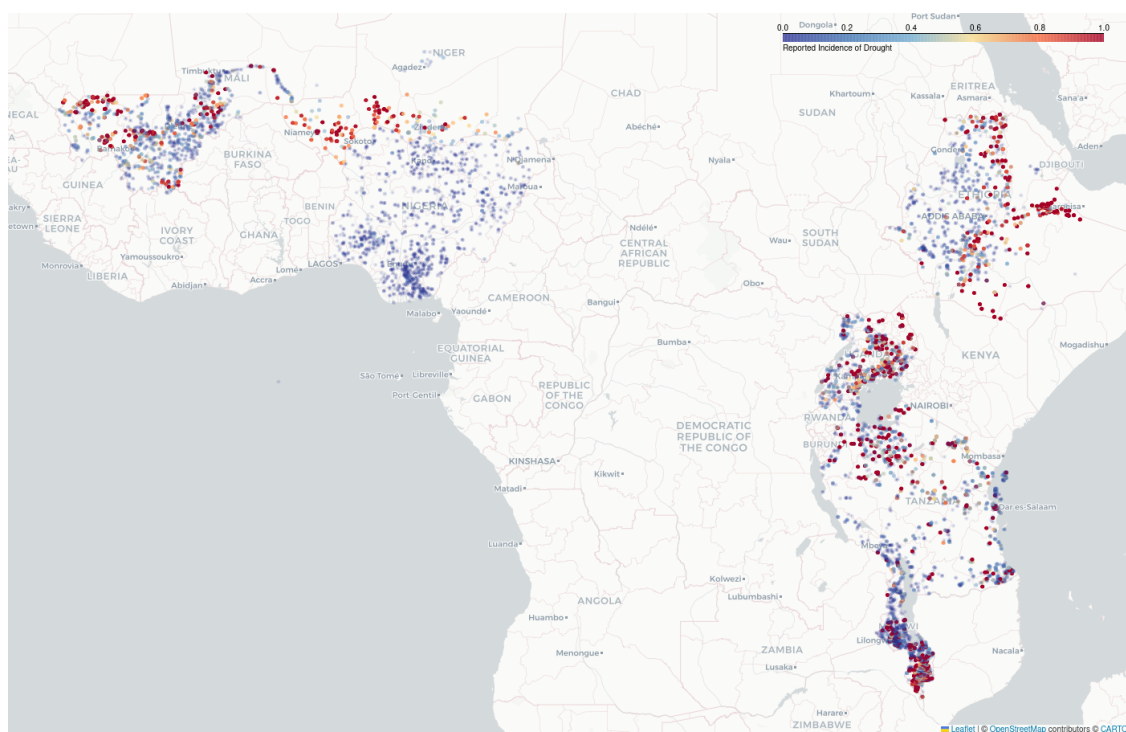


FIGURE C.2. DROUGHT REPORTS IN THE LSMS-ISA DATA ACROSS AFRICA. EACH DOT CORRESPONDS TO A SAMPLE CLUSTER; THE “BLUER” THE DOT THE HIGHER THE PROPORTION OF HOUSEHOLDS IN THE CLUSTER WHICH REPORTED DROUGHT. NOTE THAT OBSERVATIONS FROM LATER SAMPLE YEARS OFTEN PARTLY OBSCURE DATA FROM EARLIER YEARS.

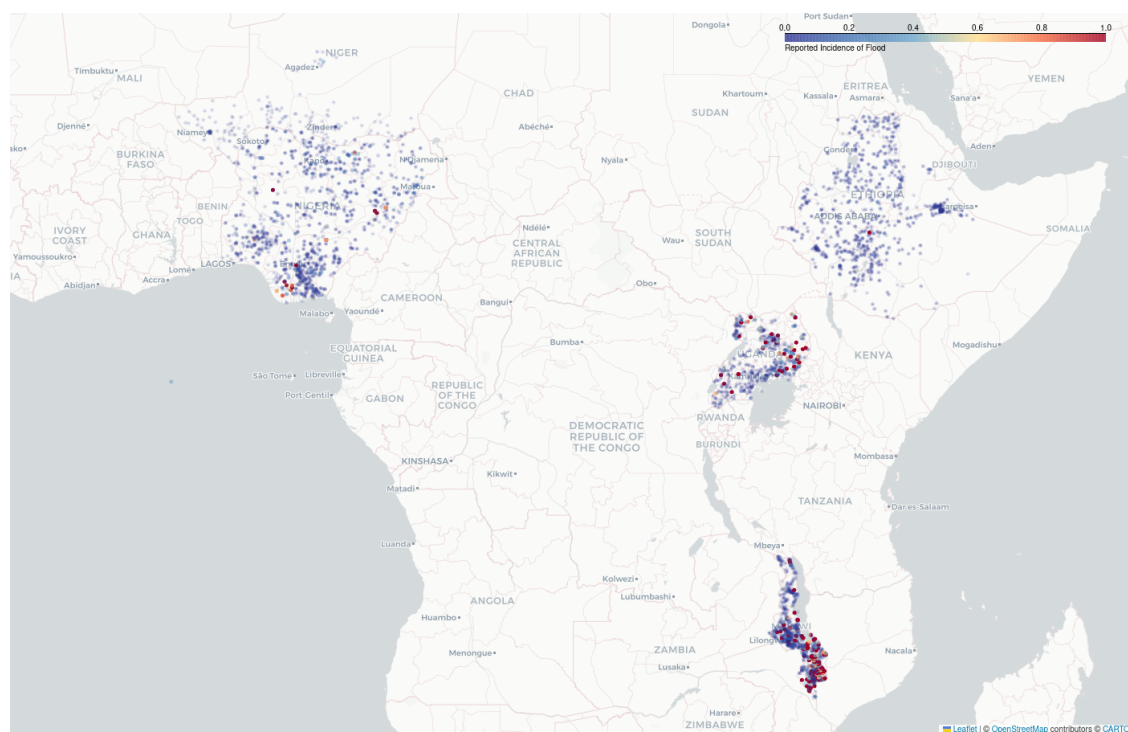


FIGURE C.3. FLOOD REPORTS IN THE LSMS-ISA DATA ACROSS AFRICA. EACH DOT CORRESPONDS TO A SAMPLE CLUSTER; THE “BLUER” THE DOT THE HIGHER THE PROPORTION OF HOUSEHOLDS IN THE CLUSTER WHICH REPORTED FLOODS. NOTE THAT OBSERVATIONS FROM LATER SAMPLE YEARS OFTEN PARTLY OBSCURE DATA FROM EARLIER YEARS.

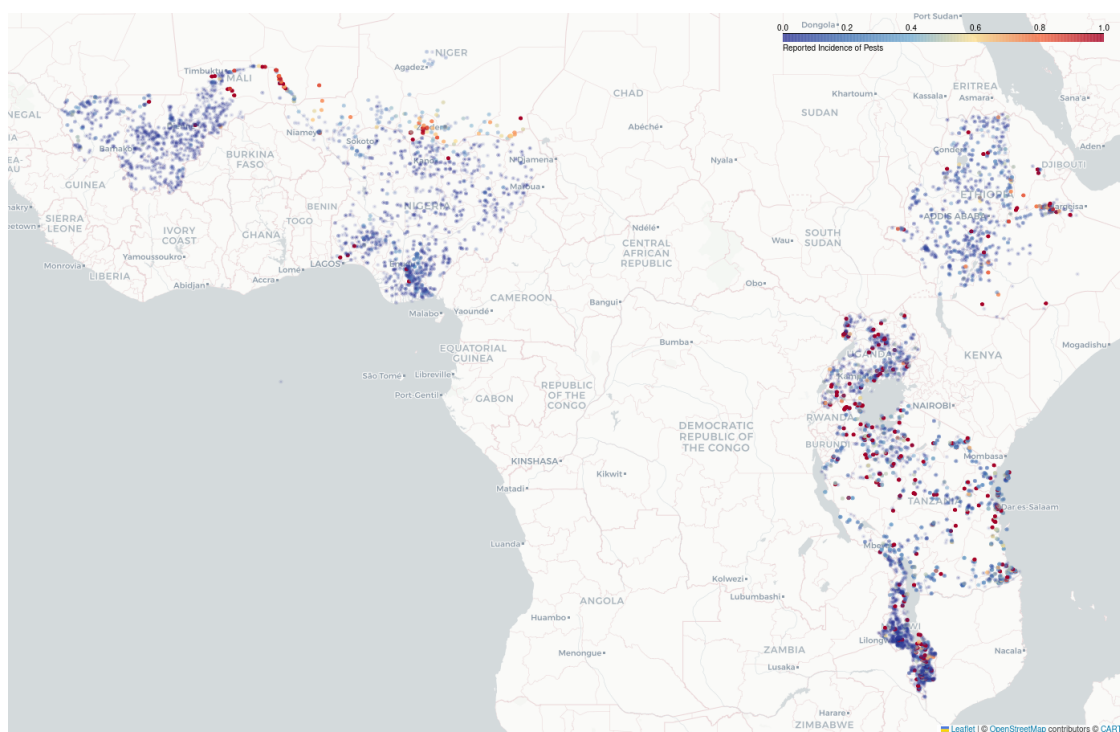


FIGURE C.4. PEST REPORTS IN THE LSMS-ISA DATA ACROSS AFRICA. EACH DOT CORRESPONDS TO A SAMPLE CLUSTER; THE “BLUER” THE DOT THE HIGHER THE PROPORTION OF HOUSEHOLDS IN THE CLUSTER WHICH REPORTED PESTS. NOTE THAT OBSERVATIONS FROM LATER SAMPLE YEARS OFTEN PARTLY OBSCURE DATA FROM EARLIER YEARS.

We’ve seen evidence of correlation in shock reports within clusters (villages or parishes). Is there also correlation across nearby clusters? We have data on cluster location, which we use to take a more formal approach to testing the hypothesis of no spatial dependence across clusters in shock reports. We use the data from shock reports across Africa described above, and then follow Conley and Ligon (2002) in computing non-parametric spatial autocorrelation functions for reports of drought, floods, or pests (adverse price shocks are not reported in this cross-country dataset). This yields pointwise estimates of the correlation of shock reports at different distances; we’re interested in whether or not the observed spatial autocorrelations could have been generated by chance under the null hypothesis of no autocorrelation.

There are two important sources of error one should be aware of. First, the public LSMS data intentionally obscures location in order to protect respondent identity by mapping the location of each household to a single cluster “point”, and then randomly shifting the location across space. However, the rules for this random displacement are clear and well-specified (Burgert et al., 2013), so we are able to take the effects of this displacement on estimation into account in constructing our tests. The second source of error is more profound—the underlying statistical model assumes that while *distance* matters for correlation, *direction* does not (i.e., an assumption of isotropy). At very large scales this is obviously wrong in our data—going in the direction of the Sahara matters for drought. However, the distances we consider are on the order of hundreds of kilometers, so very large-scale features such as the Sahara are in some sense far away from most of the clusters in our data. But isotropy may be a bad assumption even at smaller scales for some shocks such as floods, where the probability of nearby flooding is much greater if one follows the direction of a watercourse than otherwise. Fortunately, Conley and Molinari (2007) provide evidence that hypothesis tests using our non-parametric estimator are quite robust to these sorts measurement error in location.

We proceed by using provided data on cluster locations, then computing the great-circle distance between each pair of clusters in each year. We then use the non-parametric spatial autocovariance estimator of Conley (1999), using a rectangular kernel with a bandwidth distance of roughly 30 kilometers. For each year we obtain an autocorrelation function by dividing the autocovariance function at each distance d by the value of the function at a distance of zero (i.e., by the variance of cluster incidence per year). We use a sort of augmented randomized permutation inference (with 1000 draws) to construct 95% acceptance regions for the estimated functions, and construct an average across years, weighted by the number of observations per year.

The augmentation of our randomized permutation deserves some remark—this is intended to take into account the intentional random displacement of geographical coordinates imposed on the public data (Burgert et al., 2013). There are two key features of this displacement for us: first, one percent of all locations are moved some distance in a random direction, where that distance is drawn from a uniform distribution on $[0, 100]$ kilometers. Then *all* locations are similarly displaced, but only up to five kilometers. Under the null of no autocorrelation location doesn’t matter, so we can treat the observed locations as though they were measured without error, and apply the displacement algorithm to each permutation of locations to discover the effect on inference.

Results are shown in Figure C.5. In our randomized permutation setting distances at which the estimated spatial autocorrelation function lies above the acceptance region are distances at which the p -value associated with the null hypothesis is less 2.5% (for a one-tailed test). Here we see evidence of positive spatial autocorrelation for all three shocks. This evidence is least pronounced for floods, where significant autocorrelation is observed at distances of about 30–60 km. Here the assumption of isotropy may be playing an important role, as two nearby clusters may well be at different elevations. There is evidence of positive autocorrelation at a much larger range of distances for both drought (0–400 km) and pests (0–470 km).

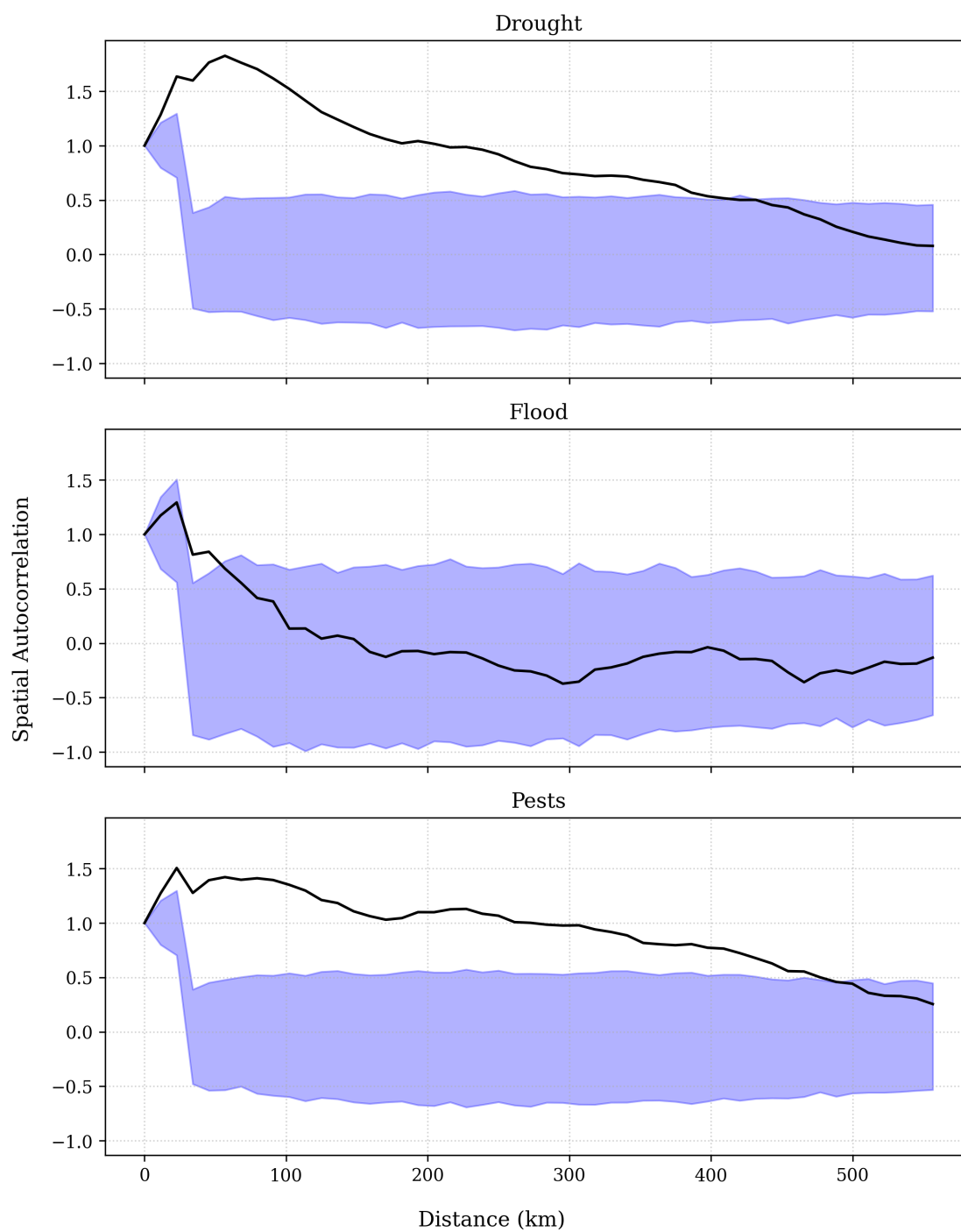


FIGURE C.5. ESTIMATED SPATIAL AUTOCORRELATION FUNCTIONS FOR SHOCK REPORTS. DOTTED AREA INDICATES A POINT-WISE 95% “ACCEPTANCE” REGION OF THE NULL HYPOTHESIS OF NO CORRELATION AT A GIVEN DISTANCE.

C4. Covariate shocks & reported declines in production, income, consumption, and assets

The LSMS survey module on shocks elicits subjective reports on whether the shock resulted in a decline in the outcomes production, income, assets, or consumption. Table C.3 reports the proportion of people who, having reported that they'd experienced a particular sort of shock (such as drought) in the last twelve months, *also* responded affirmatively to the question: "As a result of the [SHOCK] was there a decline in your household's [OUTCOME]?"

TABLE C.3—SUBJECTIVE REPORTS AS TO WHETHER SHOCK RESULTED IN A DECLINE IN PARTICULAR OUTCOMES. THESE REPORTS ARE CONDITIONAL ON THE RESPONDENT REPORTING THAT THEY'D EXPERIENCED THE SHOCK IN THE LAST 12 MONTHS.

Shock Affected	Drought	Floods	Pests	Prices
Production	0.92	0.89	0.73	0.47
Income	0.80	0.67	0.77	0.84
Consumption	0.50	0.41	0.37	0.44
Assets	0.25	0.25	0.35	0.27

Thus, of households who reported drought in the last twelve months, 92% asserted that the drought resulted in a decline in production, and 80% reported a decline in income. The logic of these responses seems fairly clear: a drought will directly affect agricultural production, and so *ceteris paribus* a farmer will have less income from marketing his smaller output.

The remaining two outcomes seem less direct. For a shock seems likely to affect consumption only indirectly, by affecting available resources (such as income or assets) which might be used to finance consumption. And assets could be affected in two different ways: via reduced resources (leading to lower rates of investment or sale of assets), or by directly damaging physical assets.

C5. Covariate shocks increase local farmgate prices

Respondents to the LSMS surveys in Uganda report farmgate prices for goods they produce, and so we can see how these prices respond to covariate shocks. One concern one might have is that respondents who report shocks might also be inclined to report higher prices. We address this by calculating mean prices for every year-market. For each good we then regress changes (over time) in log prices on the proportion of sample households in every market-year that report different aggregate shocks and on a complete set of year effects. The resulting coefficients can be interpreted as the elasticities (of prices to proportions of reporting households).

We observe changes in log mean prices²⁴ for 20 goods over four markets and over seven periods, so the sample is small (just 28 observations of price changes for each good). Nevertheless, results from this exercise are shown in Figure C.6 for the four covariate shocks "drought", "floods", "pests", and adverse "prices". We have farmgate prices in every market-year for 20 different goods. Point estimates on "drought", "pests", and "prices" are positive for all goods; for "floods" they are positive in 9 of 20 cases. Though a *t*-test would reject the null of zero effect in only a handful of cases (12,0,7, and 3, respectively), this is unsurprising given our small sample. A more appropriate test of whether shocks positively affect farmgate prices in general is the non-parametric sign test. Under the null of no effect each point estimate is equally likely to be positive or negative; the *p* values associated with this null hypothesis are 0.41 in the case of floods, and less than 0.000001 for drought, pests, or prices.

²⁴Using log median prices delivers broadly similar results.

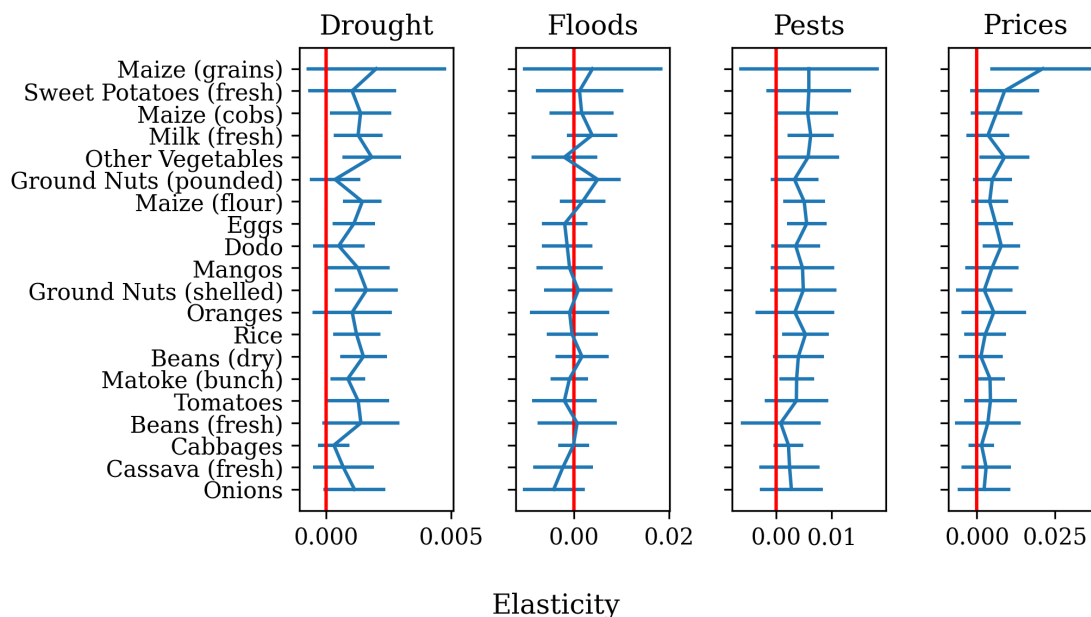


FIGURE C.6. SHOCKS AND FARMGATE PRICES. POINT ESTIMATES FROM A REGRESSION OF CHANGES IN MEAN YEAR-MARKET (FARMGATE) LOG PRICES ON THE PROPORTION OF HOUSEHOLDS REPORTING PARTICULAR COVARIATE SHOCKS IN A GIVEN YEAR-MARKET; INCLUDES YEAR EFFECTS. BARS INDICATE 95% CONFIDENCE INTERVALS. ORDERED BY AVERAGE MAGNITUDE OF COEFFICIENT ACROSS SHOCKS.

C6. Mechanisms used to cope with shocks

For each shock they reported, respondents were asked how they coped with the shock, and offered up to three responses. Table C.4²⁵ indicates the frequency with which different coping strategies were reported for covariate shocks. For example, 51% of respondents who reported experiencing drought said that they coped by reducing consumption, 45% said that they used savings, and 31% said they reduced leisure. All of these mechanisms can be thought of as forms of self-insurance. “Help from relatives/friends” is also commonly invoked (21% of households who experienced one of these covariate shocks reported such help). It should not be surprising that this kind of aid from friends and family is the dominant mechanism for idiosyncratic shocks, reported by 52% (Table C.5) of those who reported experiencing any of the idiosyncratic shocks found in Table C.1—a feature of covariate shocks is that others are likely to also experience them at the same time. And this risk-sharing seems to be fairly effective in addressing idiosyncratic shocks, as fewer than 14% of households who report idiosyncratic shocks say they reduced consumption in response.

C7. Covariate shocks and relative prices

If a covariate shock has a proportionate effect on all prices, then this would be equivalent to a reduction in income, with no change in relative prices. With nonhomothetic preferences this would still result in changes in the composition of expenditures, but these changes would be entirely due to changes in consumers’ MUEs.

However, the kinds of shocks we consider seem likely to change *relative* prices. The exact nature of these changes will be complicated, depending not only on the effects of the shock on the supply of locally produced goods, but on income and price elasticities for all goods. For example, a drought in

²⁵See Table D.3 for details of how responses are coded.

TABLE C.4—HOW DID HOUSEHOLDS COPE WITH DIFFERENT COVARIATE SHOCKS? THESE REPORTS ARE CONDITIONAL ON THE RESPONDENT REPORTING THAT THEY'D EXPERIENCED THE SHOCK IN THE LAST 12 MONTHS. RESPONDENTS COULD REPORT UP TO THREE DIFFERENT COPING MECHANISMS, SO COLUMNS DO NOT SUM TO ONE.

How Coped?	Drought	Floods	Pests	Prices	Any
Reduced consumption	0.51	0.35	0.24	0.21	0.44
Used savings	0.45	0.47	0.38	0.37	0.44
Reduced leisure	0.31	0.26	0.14	0.18	0.28
Help from relatives/friends	0.21	0.20	0.14	0.10	0.20
Nothing	0.16	0.15	0.10	0.15	0.15
Other	0.10	0.12	0.24	0.35	0.13
Changed cropping practices	0.10	0.08	0.21	0.16	0.11
Sold assets	0.08	0.07	0.10	0.06	0.08
Used credit	0.08	0.07	0.05	0.07	0.07
Help from local government	0.03	0.03	0.06	0.01	0.03
Reduced investments in human capital	0.03	0.01	0.02	0.02	0.03
Sent children elsewhere	0.01	0.02	0.01	0.00	0.01
Migrated	0.01	0.01	0.01	0.00	0.01
Rented out assets	0.00	0.00	0.01	0.00	0.00
Average # Coping Methods	2.09	1.84	1.69	1.70	1.99

TABLE C.5—HOW DID HOUSEHOLDS COPE WITH DIFFERENT IDIOSYNCRATIC SHOCKS? THESE REPORTS ARE CONDITIONAL ON THE RESPONDENT REPORTING THAT THEY'D EXPERIENCED THE SHOCK IN THE LAST 12 MONTHS. RESPONDENTS COULD REPORT UP TO THREE DIFFERENT COPING MECHANISMS, SO COLUMNS DO NOT SUM TO ONE.

How Coped?	Health	Theft	Death (non-earner)	Death (earner)	Any
Help from relatives/friends	0.58	0.20	0.74	0.77	0.52
Used savings	0.47	0.34	0.38	0.29	0.40
Other	0.15	0.28	0.10	0.06	0.17
Reduced consumption	0.17	0.13	0.06	0.20	0.14
Nothing	0.12	0.17	0.12	0.12	0.13
Sold assets	0.17	0.04	0.08	0.14	0.11
Used credit	0.13	0.06	0.08	0.07	0.09
Reduced leisure	0.10	0.07	0.03	0.17	0.08
Help from local government	0.03	0.06	0.02	0.03	0.04
Reduced investments in human capital	0.03	0.02	0.02	0.04	0.03
Sent children elsewhere	0.02	0.01	0.02	0.04	0.02
Migrated	0.01	0.01	0.01	0.02	0.01
Rented out assets	0.01	0.00	0.01	0.01	0.01
Changed cropping practices	0.01	0.01	0.00	0.01	0.01
Average # Coping Methods	2.00	1.41	1.66	1.98	1.76

an region where cultivation of maize and beans is important might increase bean prices more than maize, as maize tends to be traded over longer distances. But while a drought might be expected to increase local bean prices (via a shift in local supply), it might also reduce local demand for other goods via an income effect. In fact, if local production is dominated by the production of only a few goods, then we would expect a shock which affected the local supply of those goods to be associated with higher prices for those goods, while for other goods we would tend to expect income effects to reduce demand, causing prices to be lower than otherwise.²⁶

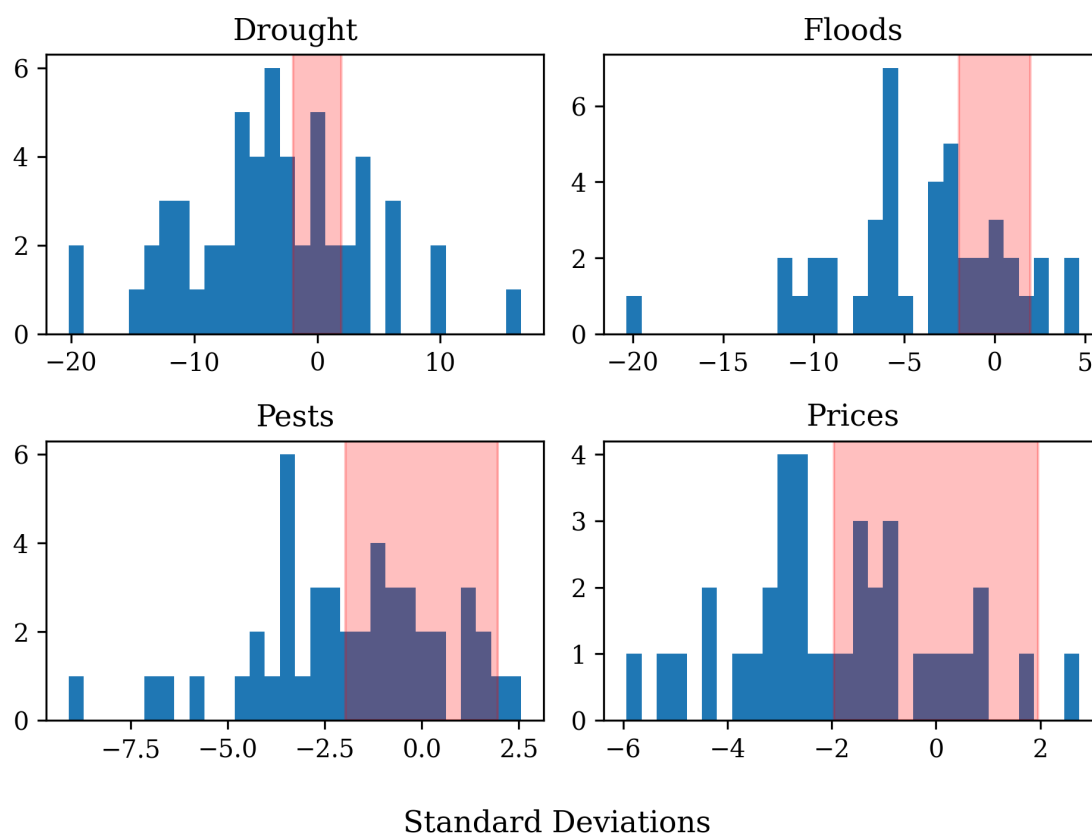


FIGURE C.7. HISTOGRAMS OF t -STATISTICS TESTING HYPOTHESIS THAT COVARIATE SHOCKS HAVE NO EFFECT ON PRICES, BY GOOD (ACROSS MARKET-YEARS). SHADED AREAS ARE 95% ACCEPTANCE REGIONS. GOODS ARE THOSE FOR WHICH WE ESTIMATE DEMANDS, LISTED IN FIGURE 1.

Here we wish to test whether the realization of a market level (regional) covariate shock leads to changes in relative prices. We earlier looked at the effect of covariate shocks on changes in farmgate prices of goods which are locally produced. Because our focus is in part on income effects here we look instead at reported market prices, which encompasses the larger set of goods we use for estimating MUEs, and which are the relevant prices for consumers.

Our approach here is to compute the deviation of mean log price of each good in each market-year from the average country-wide log price. We then divide the sample of prices according to whether households report a particular shock versus households that don't report the shock. We

²⁶However, this simple story is complicated by the possibilities of substitution and inferior goods—for example, an increase in local bean prices might lead to an increase in demand for a substitute such as matoke, or the income effect might result in an increase in demand in an apparently inferior good such as millet.

then simply construct a t -test of the null hypothesis that average log prices are the same across these two samples.

Figure C.7 shows the result of these tests for the four covariate shocks we've considered to this point. Under the null hypothesis of the shock not affecting relative prices we'd expect five percent of computed t statistics to fall outside the acceptance region $(-1.96, 1.96)$. In fact, 82%, 74%, 44%, and 53% of the tests (respectively) reject equality. Further, as suggested above, a negative shock tends to lead to *lower* prices for most goods.

C8. Quantities of food demanded

How does diet respond to shocks? We consider the effects of different covariate shocks on the consumption of different food items by regressing different outcomes on the proportion of households in each market-year who reported a particular shock, along with a complete set of time effects, so that the identifying variation is across market regions within a year.

We consider three related outcomes, all related to what we'll call "food acquired": the sum of all food purchased, consumed out of own production, received as a gift, or consumed outside the household by any household member during the last week. The first outcome is simply quantities of food acquired, including "zeros" for food items that the household didn't report at all. To handle differences in units and scale quantities are standardized. Second, we consider changes in diet on the 'extensive' margin, using an indicator for whether the household reported acquiring any positive amount of food on reports of shocks; one can thus interpret estimated shock coefficients as the probability of consuming a given good conditional on the shock. This set of regressions is of some particular interest because it can be regarded as a measure of how dietary diversity responds to covariate shocks. Third, where amounts are positive we use the logarithm of quantities; the resulting coefficients are interpretable as elasticities related to the 'intensive' margin.

Results are presented in Figures C.8, C.9, and C.10. These three figures report estimated coefficients associated with different shocks, along with horizontal bars that indicate 95% confidence intervals. Goods are ordered according to magnitude of coefficients averaged across different shock regressions.

We first consider the quantity regression; coefficients are reported in Figure C.8. Several points seem worth mentioning. First, one might entertain the hypothesis that a covariate shock might reduce consumption of all kinds of food. This idea is consistent with the logic of homothetic preferences such as the CRRA, where consumption scales linearly with budget and all income elasticities are unity. But we've already seen evidence that there are large differences in the relative income elasticities of different kinds of food. Figure C.8 suggests that the principal effect of a covariate shock is to change the composition of the food bundle, rather than reducing quantities of all foods—just looking at food items, average consumption of roughly half increases in response to shock. And as with prices, patterns of substitution seem to be very important. For example, average *sim sim* (sesame) consumption increases dramatically for all three covariate shocks reported in the figure. From our estimates of the demand system (see Figure 1) *sim sim* is among the less income elastic goods, so we might expect its role in the diet to expand when income is low. But other goods are even less income elastic, such as cassava. It appears that *sim sim* provides a protein-rich complement to cassava and sorghum in some staple dishes, substituting for meat (Wanjala et al., 2016), or being processed to substitute for other cooking oils (one of the more income elastic goods); collectively these patterns of complementarity and substitution may account for the large increase in *sim sim* consumption observed when there's a covariate shock.

We next try to decompose the average dietary changes observed in Figure C.8 into changes on the extensive (Figure C.9) and intensive (Figure C.10) margins. Figure C.9 can be thought of providing estimates of the change in the share of households acquiring a particular food conditional on the proportion of households within its market region reporting a given shock. So, for example, shocks

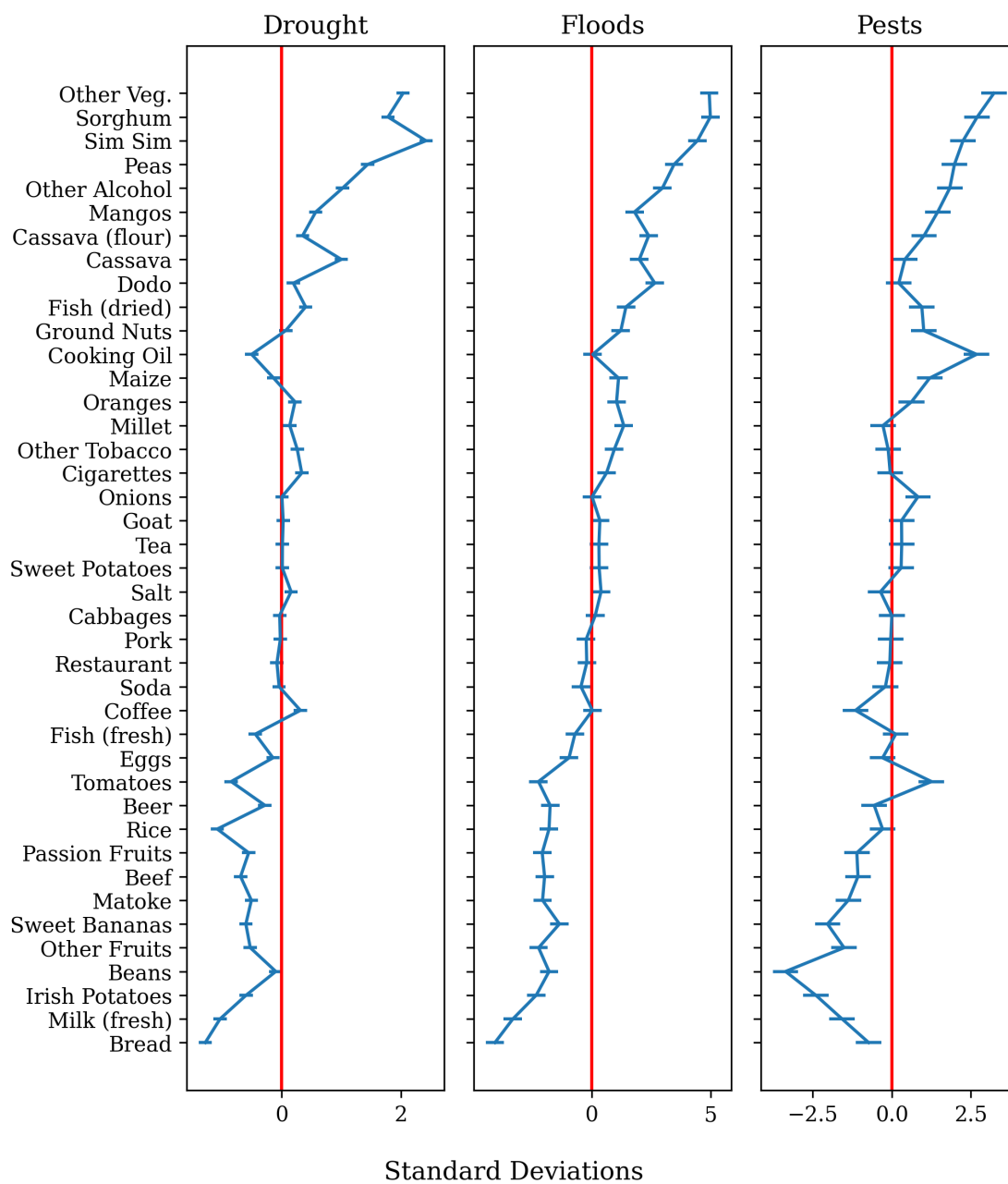


FIGURE C.8. FOOD QUANTITIES AND SHOCKS. ESTIMATED COEFFICIENTS FROM REGRESSION OF STANDARDIZED QUANTITIES OF DIFFERENT FOOD ITEMS ON THE SHARE OF HOUSEHOLDS IN A GIVEN MARKET-YEAR REPORTING GIVEN COVARIATE SHOCK (YEAR EFFECTS INCLUDED). BARS INDICATE 95% CONFIDENCE INTERVALS. GOODS ARE ORDERED ACCORDING TO MAGNITUDE OF COEFFICIENTS AVERAGED ACROSS DIFFERENT SHOCK REGRESSIONS.

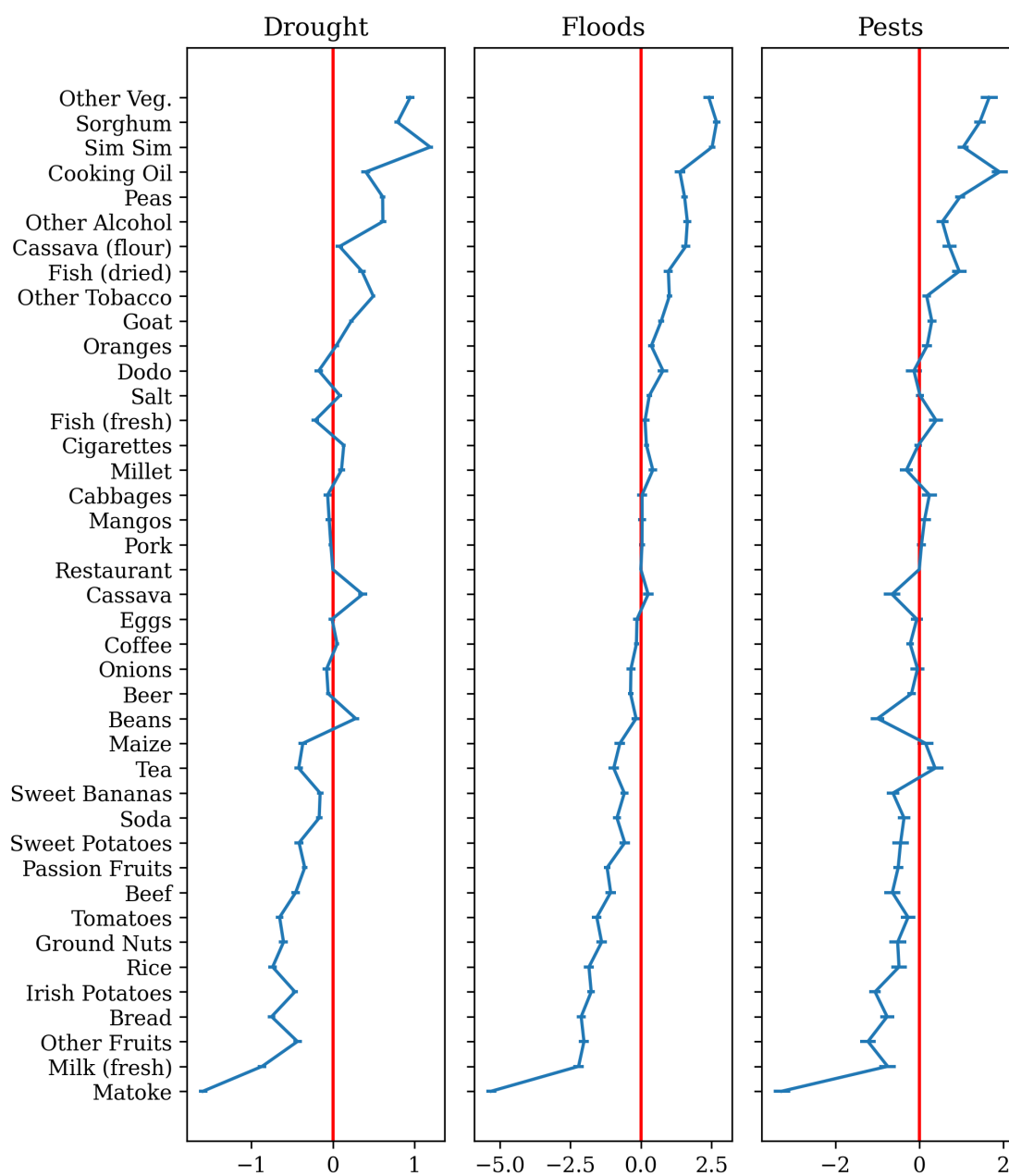


FIGURE C.9. POSITIVE QUANTITIES AND SHOCKS. ESTIMATED COEFFICIENTS FROM REGRESSION OF AN INDICATOR OF POSITIVE QUANTITIES OF DIFFERENT FOOD ITEMS ON THE SHARE OF HOUSEHOLDS IN A GIVEN MARKET-YEAR REPORTING GIVEN COVARIATE SHOCK (YEAR EFFECTS INCLUDED). BARS INDICATE 95% CONFIDENCE INTERVALS. GOODS ARE ORDERED ACCORDING TO MAGNITUDE OF COEFFICIENTS AVERAGED ACROSS DIFFERENT SHOCK REGRESSIONS.

are associated with a larger share of households consuming “other vegetables”, sorghum, or sim sim, and with a dramatically smaller share consuming matoke (cooking bananas, a favored staple), fresh milk, or “other fruit.”

We saw in Figure C.8 that the number of foods that increased in quantity in response to a covariate shock was roughly equal to the number that decreased. That is not the case here: any of the covariate shocks we consider lead to decreases in the expected number of different kinds of food acquired by the household. This tells us that on average these shocks lead to a decrease in dietary diversity, a widely used measure of food security (Hoddinott and Yohannes, 2002).

We next consider the intensive margin. Figure C.10 reports results from regressions of log quantities of different kinds of food on our measure of shocks. Here we regress log quantities (discarding zeros) on a set of time dummies and on the proportion of households in a given market-year reporting a particular shock. A regression of a log quantity on a proportion yields a coefficient which we can interpret as an elasticity. Foods with the largest elasticities include mangoes, different forms of cassava, and oranges. The fact that consumption of mangoes and oranges increases in response to covariate shocks may seem surprising, but both are tree crops widely grown in Uganda and well known for their drought tolerance. Note from C.9 that demand on the extensive margin responds very little to shocks, so we might guess that given that these are available in the market they may serve as substitutes for other foods such as sweet bananas that are not at all tolerant of drought. Another interesting case is “other tobacco”; the prevalence of use increases when there’s a covariate shock, but at the same time the log quantity falls. This suggests that shocks may induce widespread adoption but modest use of this stimulant. We’ve already noted the large extensive response of sim sim consumption to shocks. However, on the intensive margin there’s very little response to shocks, bolstering our interpretation that demand for sim sim is importantly related to substitution and complementarities with other foods.

One general observation about each of these figures is the fact that changes in demand for different goods tend to be highly correlated across the different covariate shocks. Coefficients from both the level and log quantity regressions have a Spearman rank correlation coefficient of 0.92 between drought and flood shocks, for example, while the same correlation in the “positive quantity” regression is 0.91. The correlation between flood and pest coefficients is systematically higher than the correlation between drought and pest, but even the latter ranges from 0.54–0.68. We might read this as evidence that each of these shocks is operating more through its effect on a shared income shock, rather than through prices, since it seems likely that different shocks should have different effects on the local supply of different goods (this is borne out by relatively low correlations between coefficients across shocks in Figure C.6, which range from 0.23–0.61).

Finally, we emphasize that while the results in this section have found evidence that reported covariate shocks affect production, prices, and consumption in ways that seem consistent, that same evidence is all consistent with the hypothesis that the costs of these shocks is *shared* in a fashion consistent with full-insurance. The only evidence we’ve really produced for or against the full insurance hypothesis as it pertains to covariate shocks comes from the tests in Section VI.

DATA ON SHOCKS

Data on self-reported shocks comes from a “shocks” module in different rounds of the Ugandan LSMS surveys. There are minor differences in elicitation and in the list of shocks reported in certain years. Table D.1 reports the slight aggregation of shocks we use, while Table D.3 similarly indicates how we’ve harmonized responses to the question “How did your household cope with this [SHOCK]?”.

Another idiosyncratic variable we’re interested in is income, if only because of how often it’s used in risk-sharing tests. There are three main sources of income for the households in these data: earnings from employment; profits from non-farm enterprises, and income from farming activities.

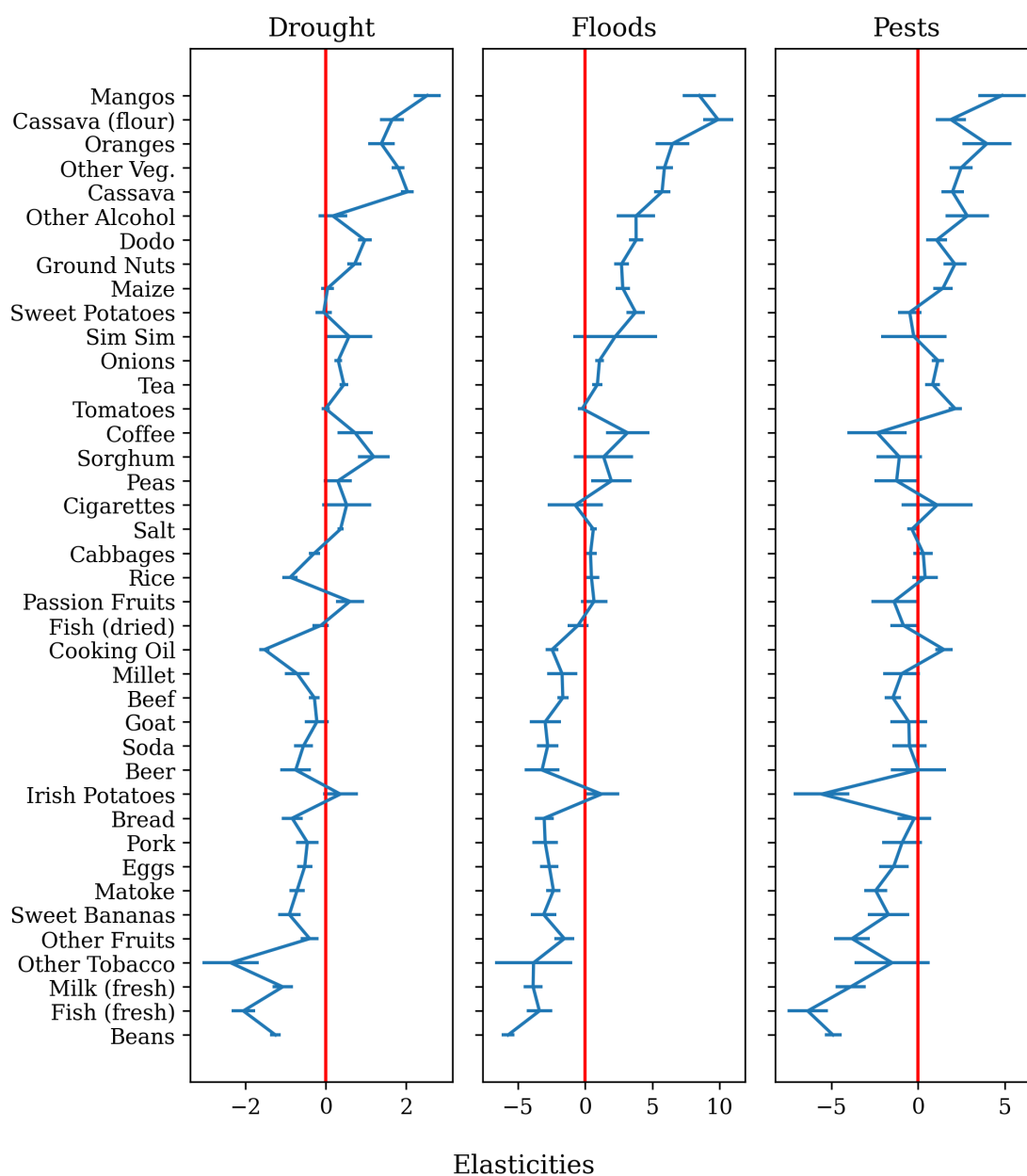


FIGURE C.10. LOG QUANTITIES AND SHOCKS. ESTIMATED COEFFICIENTS FROM REGRESSION OF LOG QUANTITIES OF DIFFERENT FOOD ITEMS ON THE SHARE OF HOUSEHOLDS IN A GIVEN MARKET-YEAR REPORTING GIVEN COVARIATE SHOCK (YEAR EFFECTS INCLUDED; ZEROS DROPPED). BARS INDICATE 95% CONFIDENCE INTERVALS. GOODS ARE ORDERED ACCORDING TO MAGNITUDE OF COEFFICIENTS AVERAGED ACROSS DIFFERENT SHOCK REGRESSIONS.

TABLE D.1—DIFFERENT SHOCK LABELS ACROSS EIGHT ROUNDS OF UGANDAN DATA, WITH HARMONIZED LABELS.

Existing Label	Label
Conflict/Violence	Conflict
Death of Income Earner(s)	Death of earner
Death of Other Household Member(s)	Death (non-earner)
Drought	Drought
Drought/Irregular Rains	Drought
Erosion	Erosion
Fire	Fire
Floods	Floods
Irregular Rains	Drought
Landslides	Erosion
Landslides/Erosion	Erosion
Loss of Employment of Previously Employed Household Member(s) (Not Due to Illness or Accident)	Lost Earnings
Other (Specify)	Other
Reduction in the Earnings of Currently (Off-Farm) Employed Household Member(s)	Lost Earnings
Serious Illness or Accident of Income Earner(s)	Health
Serious Illness or Accident of Other Household Member(s)	Health
Theft	Theft
Theft of Agricultural Assets/Output (Crop or Livestock)	Theft
Theft of Money/Valuables/Non-Agricultural Assets	Theft
Unusually High Costs of Agricultural Inputs	Prices
Unusually High Level of Crop Pests & Disease	Pests
Unusually High Level of Crop Pests & Disease	Pests
Unusually High Level of Livestock Disease	Pests
Unusually Low Prices for Agricultural Output	Prices

TABLE D.2—INCIDENCE OF SHOCKS BY ROUND

Shock	2005	2009	2010	2011	2013	2015	2018	2019
Conflict/Violence	268	34	27	46	11	12	22	25
Death of Income Earner(s)	99	27	17	19	30	19	20	19
Death of Other Household Member(s)	423	74	58	35	66	35	44	49
Drought	1234	—	—	—	735	526	556	360
Drought/Irregular Rains	—	1344	710	560	—	—	—	—
Erosion	—	—	—	—	15	3	19	13
Fire	105	26	21	20	17	18	12	9
Floods	426	61	102	148	98	62	74	117
Irregular Rains	—	—	—	—	179	72	180	169
Landslides	—	—	—	—	1	—	2	2
Landslides/Erosion	—	21	5	17	—	—	—	—
Loss of Employment of Previously Employed Household Member(s) (Not Due to Illness or Accident)	—	9	10	8	5	10	5	6
Other (Specify)	111	101	60	57	62	45	50	75
Reduction in the Earnings of Currently (Off-Farm) Employed Household Member(s)	—	28	3	10	6	4	15	15
Serious Illness or Accident of Income Earner(s)	82	189	152	91	86	56	107	119
Serious Illness or Accident of Other Household Member(s)	—	188	149	65	47	32	83	78
Theft	349	—	—	—	—	—	—	—
Theft of Agricultural Assets/Output (Crop or Livestock)	—	127	48	21	35	27	41	40
Theft of Money/Valuables/Non-Agricultural Assets	—	106	48	34	41	35	34	43
Unusually High Costs of Agricultural Inputs	71	60	19	27	49	7	10	12
Unusually High Level of Crop Pests & Disease	—	137	40	61	54	35	—	—
Unusually High Level of Crop Pests & Disease	292	—	—	—	—	—	120	79
Unusually High Level of Livestock Disease	183	82	37	31	17	18	10	5
Unusually Low Prices for Agricultural Output	—	53	35	38	18	5	68	17

TABLE D.3—DIFFERENT COPING LABELS ACROSS EIGHT ROUNDS OF UGANDAN DATA, WITH HARMONIZED LABELS.

Existing Label	Label
Unconditional help provided by relatives/friends	Help from relatives/friends
Unconditional help provided by local government	Help from local government
Sold land/building	Sold assets
Sold durable household assets (agricultural or nonagricultural)	Sold assets
Sold durable household assets (agricultural or non-agricultural)	Sold assets
Sold assets	Sold assets
Sent children to live elsewhere	Sent children elsewhere
Rented out land/building	Rented out assets
Relied on savings	Used savings
Reduced expenditures on health and education	Reduced investments in human capital
Reduced consumption	Reduced consumption
Other(specify)	Other
Other (specify)	Other
Obtained credit	Used credit
Nothing else	Nothing
More wage employment	Reduced leisure
Increased agriculture labour supply	Reduced leisure
Household member(s) took on more non-farm (wage- or self-) employment	Reduced leisure
Household member(s) took on more farm wage employment	Reduced leisure
Household member(s) migrated	Migrated
HH member took on more non-farm (wage- or self-) employment	Reduced leisure
HH member took on more farm wage employment	Reduced leisure
Distress sales of animal stock	Sold assets
Did nothing	Nothing
Changed dietary patterns involuntarily (Relied on less preferred food options, reduced the proportion or number of meals per day, skipped days without eating, etc)	Reduced consumption
Changed dietary patterns involuntarily	Reduced consumption
Changed cropping practices (crop choices or technology)	Changed cropping practices
Changed cropping practices	Changed cropping practices
Rented out land/building	Rented out assets
99.0	Nothing

The last is particularly difficult to construct,²⁷ and so we restrict our attention to non-farm income, constructed as the sum of earnings and profits from non-farm enterprises. In only 9724 of 22,791 total usable observations do households report any non-farm income; in the remaining cases we treat our measure of income as missing. We would not argue that this is a very good measure of income, but would argue that the full risk-sharing hypothesis implies that variation in this poor measure of income should not affect households MUEs.

²⁷Income from farming activities would in principle include profits from crop and livestock production, but both revenues and costs are poorly measured in these surveys. Revenues are difficult to compute because for many households a considerable amount of production is directly consumed. Though the surveys elicit respondents' assessment of the value of this own-consumption, it would be an obvious mistake to add this to income and then test whether income is correlated with the value of consumption. Costs are also poorly measured, and for a related reason—the on-farm labor of household members is poorly measured and difficult to value.

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