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Working Paper Series

#2021-008

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Published 1 March 2021

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UNU-MERIT Working Papers
ISSN 1871-9872

Maastricht Economic and social Research Institute on Innovation and Technology UNU-MERIT

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COVID-19 Policy Responses, Mobility, and Food Prices: Evidence from Local Markets in 47 Low to Middle Income Countries

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Abstract

Governments around the world have taken drastic measures to contain the spread of the new Coronavirus. Policy responses to the pandemic could affect local food prices in sensitive ways. We hypothesize that mobility restrictions reduce trade, which increases food price dispersion and prices in regionally integrated markets, but not in segmented markets. We use WFP price data of 798 retail markets in 47 low to middle income countries to test if and how food prices were affected by the stringency of COVID-19 measures. We assess market segmentation based on pre-COVID-19 price data and measure government responses using the Oxford Coronavirus Government Response Tracker. Our results show that more stringent policy responses increase food prices for integrated and less remote markets but not for segmented markets. The impact of the stringency of policy reposes on food prices is mediated by reductions in mobility and moderated by markets' pre-Corona dependency on trade.

Keywords: COVID-19; Prices; Food; Market Integration; Public Policy

JEL: H12; D4; Q11;Q18; D04;

1 Introduction

The COVID-19 pandemic has changed the world within a few months. The number of confirmed COVID-19 deaths has surpassed two million and an end is not in sight at the time of writing.[1] Governments around the world have taken drastic measures to contain the spread of the virus: About 190 countries have implemented external border restrictions limiting entry or exit across different sovereign jurisdictions, more than 171 countries have closed their schools in order to reduce the transmission of the virus, and restrictions on non-essential businesses are commonplace (Fang et al., 2020). The fear of an uncontrollable outbreak has also led many governments of low to middle income countries (LMICs) to take drastic lock-down measures even in regions where the occurrence of infections was low. Such policy responses could have a range of sizable side effects. For instance, uncushioned income losses heavily affect households' purchasing power, mobility restrictions disrupt the supply of agricultural labor and limit the ability of market actors to sustain trade (Barrett, 2020; Bene, 2020; Laborde et al., 2020). Predictions on the welfare impacts for vulnerable populations are grim: World Bank growth estimates suggest that COVID-19 could push 71 million people into extreme poverty in 2020 (Global Economic Perspectives, 2020), Guan et al. (2020) predict substantial economic losses as a function of the length, severity and recurrence of lock-down measures, and global figures point out a disproportionate impact on groups that are already vulnerable, such as youth entering the job markets (IMF, 2020).

The pandemic has hit the world at a time in which world hunger was on the rise after decades of steady decline.[2] In many regions, peaking food prices were increasing food insecurity. For vulnerable households that spend most of their budget on food, even small changes in food prices can have severe welfare impacts (Abbott, 2012), which raises concerns that supply chain disruptions and price increments related to the pandemic could further exacerbate this trend. Empirical evidence on the impacts of lock-down measures on food

[1] See https://ourworldindata.org/covid-deaths (access January 19 2021)
[2] See https://unstats.un.org/sdgs/report/2020/ (access October 22 2020)

prices on local markets in lower income countries is scarce. Yet, a local perspective is important as aggregated prices may hide differences related to market characteristics, which could conceal true price effects vulnerable households in some regions are facing. For instance, in exporting markets trade frictions related to COVID-19 reduce local prices as local supply goes up, while in importing markets prices increase because of supply chain disruptions. By looking at price aggregates, we may average out local dynamics and underestimate the risk of price increases in some regions for vulnerable households. We aim to fill this gap by using price data of 798 retail markets in 47 LMICs to test if and how local food prices were affected by COVID-19 policy responses. In particular, we examine whether the same national policy measures had differential impacts on prices depending on the integration of local markets in a wider regional network of markets. We conjecture that more stringent government responses to COVID-19 increase food price dispersion and price levels in regionally integrated markets but not in segmented markets. We regard reductions in local mobility as the underlying mechanism that raises trade costs and dims price signals. To test this, we use monthly price information collected by the World Food Program (WFP) and classify market integration based on pre-COVID-19 data for every market-commodity pair. To measure government responses to COVID-19, we use the Oxford Coronavirus Government Response Tracker and Google's COVID-19 Community Mobility Reports data. Our results show that a one standard deviation (s.d.) increase in the stringency of national COVID-19 measures is associated with a one percentage point (p.p.) increase in monthly changes in food prices in integrated markets, but the effect disappears in segmented markets. This finding is robust to a range of robustness tests and the analysis of the mechanisms suggests that the effect is mediated by reductions in mobility, and the effects are particularly pronounced in less remote areas with low local agricultural activity.

In many regions, governments exempted agricultural trade from mobility restrictions. However, in rural areas agriculture is often the dominating livelihood activity which makes it complicated to design effective exemptions in a way that they do not apply to everyone.

In addition, the closure of informal local markets may increase travel times of households to markets which further impairs access to food (Carlitz and Makhura, 2020). This could suggest that regions that are less dependent on trade may be less affected by mobility restrictions. While this mainly concerns physical access to food, income effects are likely to spill over to economic access to food, reaching rural and urban households through different channels. The economic slow-down and unemployment generated can create excess in supply of labor in rural areas, while the demand of workforce for traditional harvest practices can be limited by distancing measures (Schmidhuber et al., 2020). The change of international remittances, estimated to reduce by 23.1%, can further curtail the resources available to households.[3] These effects reduce purchasing power and feed back into the supply-side, lowering the marketability of agricultural products.

On a global level, there are no signs of significant supply chain disruptions and prices seem to have remained stable internationally (Bene, 2020; Barrett, 2020; Devereux et al., 2020). Yet in Europe, Akter (2020) finds a significant impact of stay-at-home restrictions on national food prices of around 1% over several food categories considered. More granular information on food price developments particularly in LMICs is scarce. Mahajan and Tomar (2020) scrape prices of an online retailer in three cities in India to analyze the availability and prices of selected food items. The authors find a reduction in the availability in foods in the range of 10%, which is mainly driven by the distance to production sites, and lockdown related price changes between -0.8% (edible oils) and 2.4% (cereals). These changes refer to a specific context that may not apply to food insecure households in rural areas, for instance. In this paper, we apply a broad geographical perspective and focus on the main food items in the diet of vulnerable households in each region. On the household level, phone surveys conducted shortly after mobility restrictions were put in place shed first light on the consequences of the pandemic on food insecurity in lower income countries. Preliminary results from six African countries seem to suggest that rising food prices impaired access

[3] See https://www.worldbank.org/en/news/press-release/2020/04/22/world-bank-predicts-sharpest-decline-of-remittances-in-recent-history (access 1 Oct. 2020)

to food. For instance, in Ethiopia for about 90% of households that could not buy enough food, higher prices or less regular income, were the biggest problems (Wieser et al., 2020); in Mali 45% respondents who had no access to basic foods mentioned food price increases as main reason (World Bank, 2020). However, the local context could matter and remote markets with short value chains may be less affected by mobility constraints than more integrated markets. For example, based on phone interviews in two states in India, Ceballos et al. (2020) find that respondents in the region where small-scale farming is more prevalent experienced no significant changes in access to diverse foods after mobility constraints were put in place, whereas in the region with more large farms access to diverse food decreased significantly.

To date, there is little empirical evidence on the side effects of COVID-19 policy responses on prices, but a long-standing literature has analyzed the effect of trade restrictions on international prices and price volatility. To shield domestic prices against global spikes, governments of net-exporting countries can for instance impose export bans or levy export taxes while net-import countries can remove import restrictions. Widespread insulating measures increase prices and price volatility with adverse welfare consequences particularly for food importing countries, as for instance observed during the 2007/8 food price shock (Mitra and Josling, 2009; Anderson, 2012; Abbott, 2012; Götz et al., 2013). Despite clear theoretical predictions, however, informal trade and poor enforcement of trade restrictions may undermine the impacts on cross-border price gaps on local markets (Porteous, 2017). In addition, longer supply chains can absorb global food price spikes before they reach consumers (Abbott, 2012). In a study on local maize prices in Tanzania, Baffes et al. (2019) find that domestic influences explain about two thirds of the variation of local prices while regional and global factors play a subordinate role. We contribute to this literature by showing how local market conditions mediate the effect of national policies on prices. In addition, we identify reductions in local mobility as mechanisms through which policies affect food prices. Previous studies have highlighted the role of infrastructure for food insecurity and the formation of prices (Burgess and Donaldson, 2010; Donaldson, 2018; Shively and Thapa, 2017). In these studies the latent mechanism is mobility and in this paper we have the rare opportunity to examine how sudden changes in mobility affect local prices in the short run. Our results confirm the important role of local mobility for the formation of prices.

The remainder of the article is organized as follows: in the next section we discuss the conceptual framework, followed by a discussion of the data in section 3. In section 4 we outline the empirical strategy, followed by a discussion of the results in section 5 and in section 6 we present concluding remarks.

2 Conceptual Framework

According to Hayek's price theory, economic agents possess particular knowledge that is vital for the functioning of local economies (Hayek, 1945). In market economies the system of prices established by supply and demand ties together this knowledge and decentralizes decisions on how to allocate available resources. Prices shape the allocation of resources, confined by infrastructural and regulatory constraints and the outreach of price signals. That is, an increase in prices in a local market only leads to increased imports from other markets, if there is knowledge about price differentials large enough for arbitrage operations between the markets. In the absence of trade frictions and with complete knowledge, local prices would be perfectly pegged onto international prices. In practice, however, knowledge of regional prices is incomplete (Aker, 2010) and local prices are often found to be a misaligned or independent of international prices (Baffes et al., 2019). We conjecture that COVID-19 policies impact prices through restricted mobility, which increases trade costs and dims price signals.

We focus on short-run price changes due to an exogenous mobility shock without considering general equilibrium effects. To illustrate how mobility restrictions may affect local prices, we develop a simple reduced form framework adapted from Baffes et al. (2019). The price P^i in market i is a function of the external price P^E and costs of trade. We assume that trading costs f are a function of restrictions α imposed to limit the spread of the Coronavirus and the remoteness R^i of a market. We assume that trading costs increase with the stringency of government responses ($\frac{\Delta f}{\Delta \alpha} > 0$), but do not further specify how it interacts with remoteness.[4] Trade with other markets occurs if the autarchy price P^{Ai} is higher than the external price and trading costs (imports) or lower than the external price and trading costs (exports). Otherwise the market price is determined by local demand and supply, which we assume to be unaffected by mobility restrictions in the short run. This leads to three scenarios for the effect of restrictions on the local price P^i:

$$P^i = \begin{cases} P^E + f(\alpha, R^i) \quad if \quad P^{Ai} \geqq P^E + f(\alpha, R^i) \ (1) \\ P^{Ai}\ if\ P^{Ai} \subset (P^E - f(\alpha, R^i)\ , P^E + f(\alpha, R^i)\ )\ (2) \\ P^E - f(\alpha, R^i) \quad if \quad P^{Ai} \leqq P^E - f(\alpha, R^i) \quad (3) \end{cases}$$

The first scenario describes a net importing market in which restrictions increase trading costs and thus the price P^i. The second scenario applies to markets that can meet the demand for commodity i cheaper locally than by importing it from other markets given the trading costs and the autarchy price P^{Ai}. The last scenario refers to exporting markets, in which trade restrictions lead to less exports and excess supply which causes local prices to decrease. Following the framework, an increase in α increases the range at which the autarchy price P^{Ai} is established, thinning trade between connected markets. This categorization is dynamic and may change over the course of a year (depending on local storage capacities), and the commodity considered. For instance, in the lean season and without storage, local demand can only be met by imports from surplus markets which means the price is defined

[4]Some different effects mechanisms are plausible: movement restrictions might increase trading costs with remoteness of markets or could have a decreasing effect in remote areas if restrictions are less likely to be enforced and monitored. We regard this as an empirical question that we analyze in the Results section.

by $P^E + f(\alpha, R^i)$. As trading barriers increase, arbitrage margins decrease, leading to larger price differentials between markets. This leads to the first hypothesis that more stringent COVID-19 policies increase price dispersion.The framework suggests that markets that used to rely on external trade, are more sensitive to mobility restrictions than segmented markets that rely on local supply and demand. Yet, the effect direction is ambiguous. In surplus markets, mobility restriction reduce exports and increase excess supply, which reduces local prices. On the other hand, in net-deficit markets mobility restrictions reduce imports, which leads to excess demand and increasing local prices. In a closed framework in which exports and imports are balanced, positive and negative price effects would cancel each other out. However, most LMIC countries are net-food importers and assuming that this also holds on average for local markets, we expect that mobility restrictions increase prices. This leads to our second hypothesis that stringent COVID-19 policies increase prices in integrated markets but less so in segmented markets.

3 Data

In the main analysis we focus on data covering the months January until October 2020. To construct our market integration indicator, we additionally rely on monthly pre-Corona data covering the period from 2017 to 2019. In the following, we describe the data sources in more detail.

3.1 Market prices

The market price data set, first published in 2009 following the food price crisis of 2007/2008, is a collection of monthly time series coming from 480 primary and secondary sources and accessible publicly from the UN World Food Programme website.[5] It covers mostly markets in the 83 developing countries where WFP operates, also incorporating data available from

[5] See https://dataviz.vam.wfp.org/economic_explorer/prices

UN Food and Agriculture Organization. The information is used by UN organizations, local governments and partners to provide early warnings on economic access to food and complement consumption data in food security analysis (Caccavale et al., 2017). Prices are presented as monthly averages, independently of the original frequency of collection, and are reported at local level. Each market is either an aggregation of different physical locations within multiple settlements or the main retail market in one settlement. In every location, enumerators collect information on the price of a standard retailing or wholesale unit. We converted these units of measurement to either kilograms or liters. From the original data set we applied different criteria to further filter the data, removing national averages, observations that were sparse in time, and data points falling more than 4 standard deviations away from the median.

In order to balance the trade-off between availability of data (and therefore inclusion of markets and countries) and the length of the time series (which allows deal with more degrees of freedom, once modeling), the time series were cut off from January 2017 to the latest available information, up to October 2020. Information from January 2017 to December 2019 is used to estimate the market integration for each market-commodity pair. For the main analysis we focus on 2020, the year in which the pandemic took place, giving us several months before and after the outbreak. The resulting data set is summarized in Table 1 below to provide more insights on the countries considered. It is worth noting that a certain variability in the number of available markets and commodities applies, with extreme cases such as Malawi (118 markets and 4 commodities) or the Syrian Arab Republic (30 markets and 36 commodities) and Bangladesh (7 markets and 6 commodities). We refer to Table 9 in the Annex for a list all commodities included in our data base.

Table 1: Overview of markets, market integration, and mobility indicators by country

Country	**# markets**	**commodities (# min)**	**commodities (# max)**	**Segmentation (0/1)**	**OxCGRT**	**Google**
Afghanistan	8	3	3	0.77	0.36	0.07
Bangladesh	7	1	4	0.53	0.56	0.19
Burkina Faso	60	3	6	0.23	0.39	0.04
Burundi	52	4	13	0.32	0.10	
Cambodia	3	2	3	0.17	0.34	0.17
Central African Republic	11	1	5	0.46	0.40	
Chad	22	1	6	0.49	0.49	
Congo	4	11	12	0.34	0.49	
Cote d'Ivoire	3	3	9	0.85	0.40	
Djibouti	5	7	9	0.81	0.46	
Eswatini	4	10	10	0.58	0.56	
Gambia	8	25	33	0.09	0.53	
Guinea	4	9	9	0.83	0.50	
Haiti	9	7	11	0.33	0.49	0.16
India	55	6	21	0.48	0.59	0.24
Iraq	16	18	19	0.80	0.67	0.11
Jordan	12	24	29	0.29	0.52	0.17
Kazakhstan	3	4	4	0.17	0.63	0.07
Kenya	9	1	2	0.78	0.58	0.08
Kyrgyzstan	18	28	33	0.36	0.53	0.08
Lebanon	24	2	19	0.23	0.50	
Lesotho	10	6	6	0.34	0.26	
Malawi	13	1	4	0.68	0.39	0.06
Mali	64	3	7	0.77	0.38	
Mauritania	6	1	6	0.40	0.58	-0.07
Mongolia	5	5	5	0.68	0.40	0.00
Mozambique	15	1	13	0.89	0.25	0.06
Myanmar	11	2	4	0.77	0.44	0.12
Namibia	8	4	9	0.48	0.23	0.06
Niger	56	2	4	0.62	0.54	0.05
Nigeria	8	18	20	0.28	0.53	0.10
Pakistan	5	13	13	0.66	0.56	0.27
Philippines	6	2	3	0.33	0.58	0.20
Rwanda	14	4	17	0.72	0.39	0.11
Senegal	43	1	6	0.14	0.36	
Sierra Leone	13	8	11	0.87	0.26	
Somalia	9	2	4	0.37	0.53	
South Sudan	7	2	7	0.19	0.55	
State of Palestine	11	25	27	0.51	0.36	0.11
Sudan	12	2	3	0.81	0.43	0.17
Syrian Arab Republic	29	16	19	0.03	0.51	0.13
Tajikistan	5	21	25	0.43	0.60	0.12
Togo	6	3	4	0.29	0.30	-0.01
Turkey	3	35	36	0.47	0.36	0.00
Uganda	6	5	7	0.03	0.46	0.14
Yemen	23	5	15	0.01	0.05	-0.00
Zambia	64	2	6	0.03	0.28	0.03

Note: Google=Google mobility indicator (description in section 5). We re-scaled the original OxCGRT indicator to 0-1 and transfomed the Google mobility indicator so that positive value refer to mobility reductions and divided the original values by 100.

Due to sampling and complexity of collecting such a wide number of time series, missing values are an ever-present issue. To construct market integration indicators with as many time series as possible, we impute missing values in the in the period from 2017 to 2019 using Amelia-II, an R package for imputing missing values (Honaker et al., 2011). Diagnostics on the imputation process have been analyzed with support of different methods. First, we map the missing values against markets and commodities; second, we observe the distributions of imputed and observed values; third we use the model to "over-impute" observed values. These methods, their results, and correlates of missing values are discussed in Annex A. In our sample from 2017 to 2019 about 8.8% of observations are imputed. It is important to note that we do not impute missing values in 2020, the data we use in the main analysis.

To analyze prices in multiple countries we divide the price in local currency by the average official indirect quotation against the U.S. dollar extracted from WFP-Dataviz. This constructs time-series that account for the strategic use of currency reserves in the dynamics of international markets. We rely on this simple method as we don't yet aim to achieve full comparability of price levels or to test purchasing power parity of goods between countries (Marsh et al., 2012).

Figure 1 illustrates the development of prices compared to the previous year in our data. As comparison to international food price developments, we also chart FAO Food Price Index (FFPI) data that measures monthly changes in international prices of a basket of food commodities.[6] On average, the market price increases reported in 2020 were higher than those in any other period since 2018. While market price developments are mostly similar to FFPI changes, we note a divergence starting early 2020. While market prices in our sample increased, the FFPI was declining. This could reflect differences between local and international prices, but could also be due to differences in the dynamics of commodities considered. In Annex B we illustrate price developments separately for maize, wheat, rice, and beans in the data, which shows that developments were not uniform.

[6]See http://www.fao.org/3/ca9509en/ca9509en.pdf#page=78 (access November 2020)

Figure 1: Monthly price development compared to previous year

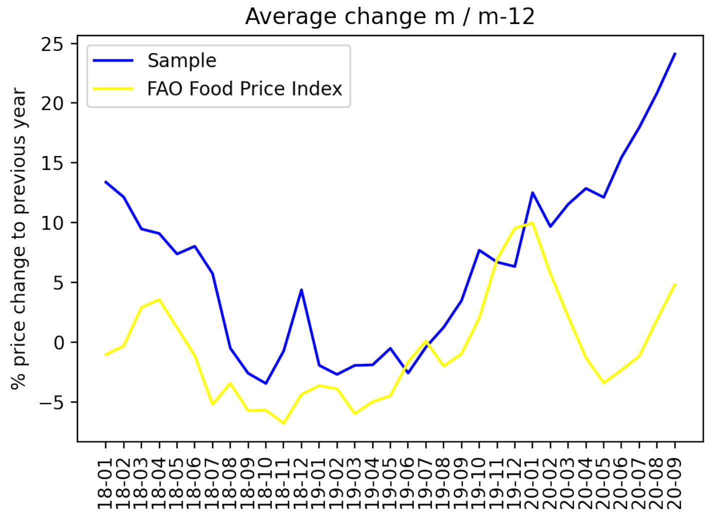
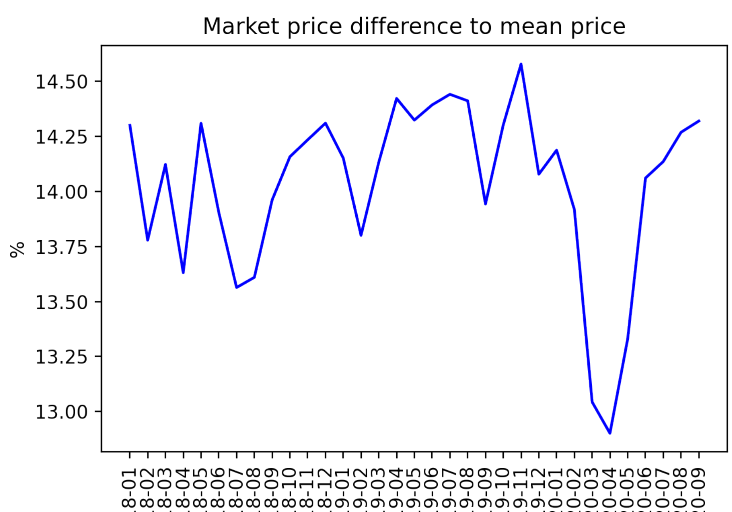


Figure 2 illustrates the absolute percentage difference of prices to the same commodity in the same country and month to obtain an approximation of price dispersion. While price dispersion drops to its lowest levels since 2018 in March, April and May 2020, it rises to high levels again later in the year. However, it bears noting that the extreme points do not differ more than 1.5 percentage points from previous values in the time series.

Figure 2: Price differences compared to national prices



3.2 Market integration

Measuring the impact of mobility restrictions on the process of price formation requires controlling for the embededness of each market in the broader national network before 2020. It has been extensively recognized that spatial convergence has a key role in the formation of prices and this is represented by market integration, or the degree of transmission of price changes between two markets (Enke, 1951). We expect less integrated markets in the regional network to be less impacted by the stringency of COVID-19 responses. Therefore, we classify whether a market is integrated in regional trade using OLS regression methods and market pre-Corona prices between 2017 and 2019.

Market linkages are commonly modeled with transaction costs and demand and supply in distinct markets that jointly determine prices and trade flows (Barrett, 1996). In the absence of trade information, we follow the approach first outlined by Ravallion (1986) to estimate market integration based on price data. Market integration is achieved when there is a degree of transmission of price changes between two markets. Therefore, we use time series of each market-commodity pair before the outbreak of Corona to estimate the effect of prices in a reference market on prices P in month t of that market-commodity pair :

$$P_t = \alpha_0 + \alpha_1 P_{(t-1)} + \beta_0 R_t + \beta_1 R_{(t-1)} + \gamma_{(t,12)} \overline{S}_{(t,12)} + \varepsilon_t \tag{1}$$

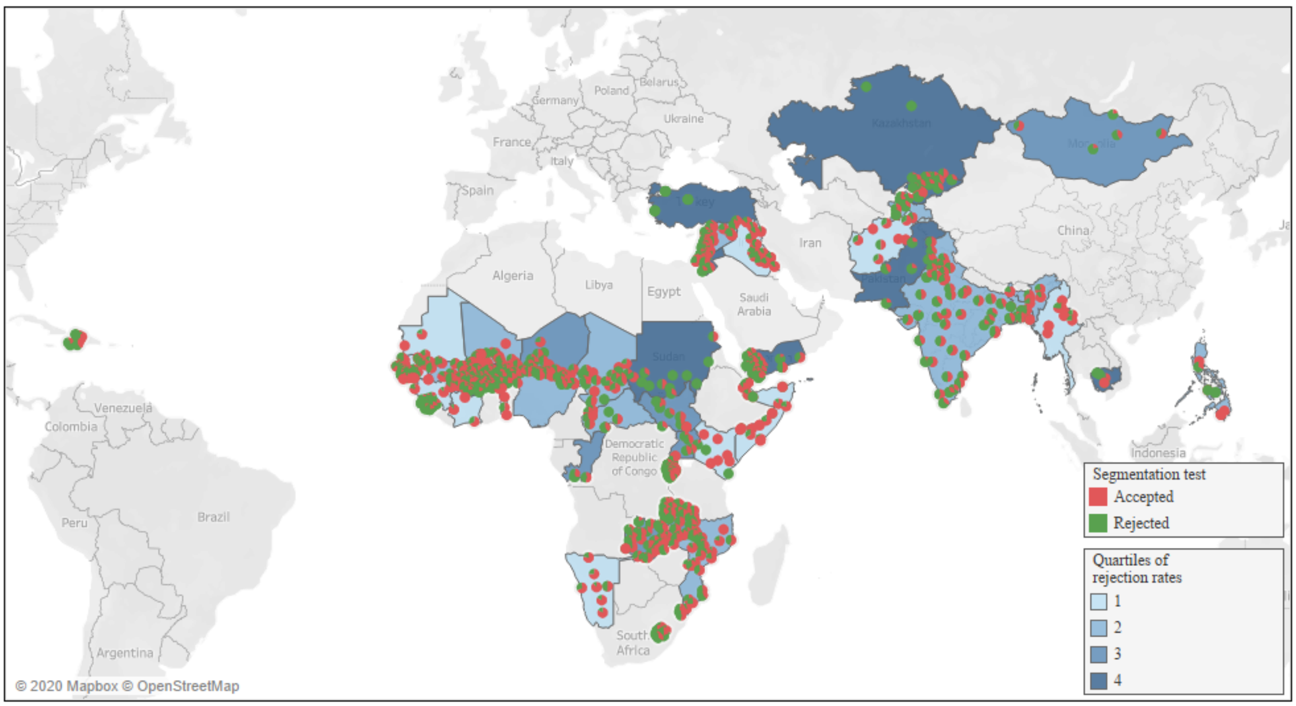
Compared to Ravallion's original framework, we rely on a definition of reference market, in which reference market prices are defined as R, that is more adaptable to the use of large samples. In his seminal work on the 1974 famine that swept Bangladesh, Ravallion defines the market network as having a star-like topology. In other words, each market relates to a central, reference market. This is possible due to the market structures at the time, the different way information and commodities flowed and the availability of anecdotal information, complementing the absence of reliable data on within country trade flows. Given the higher dimensionality we are dealing with, we aggregate prices of the same commodity in multiple neighboring markets to create a reference price series. In the main analysis, we chose up to 10 closest markets in terms of travel distance within 24 hours of driving time, accounting for the presence and conditions of roads and natural barriers (see Annex E for a discussion of variations of this definition). In the estimations we test whether the reference price in the same month R_t and of previous months R_{t-1} is significantly associated with market prices conditional on seasonality $\overline{S}$. Based on that, we classify markets as segmented if we fail to reject the null hypothesis that $\beta_0 = \beta_1 = 0$ at the 5% significance level. We choose to rely on market segmentation as our measure for integration in the main analysis, to clearly identify markets in which prices are independent of reference market prices (pre-Corona). Other indicators derived from the same estimation framework such as long- and short-run integration, further weight in the rate of convergence towards a spatial equilibrium, which is less relevant in our context.[7] The choice of market segmentation is also reinforced by the conceptual framework adopted: our focus is the presence of trade relationships between markets, which are exemplified by the relevance of reference markets in the price determination mechanism. Speed of convergence or strenght of inter-temporal relations, which are better described by other measures, by contrary, are not relevant. We argue that a market has a trade relation with other markets, and therefore is integrated, if the reference market has a significant impact on prices of the local market, regardless of the speed, length and magnitude of that impact.

Other methods to test for market integration have been extensively explored in the past decade, for better accounting information asymmetries, trade costs, inter-temporal variations due to commodity stocks and substitution effects. This comes at the price of higher model complexity and with the requirement of more data, computational capacity and complex model selection algorithms, given the high dimensionality we're dealing with (9,118 different regression models).

[7] As expected, estimation results using long-run integration as indicator for market integration lead to similar results. The selection of indicators, their relationship and the implications for the results are discussed in more detail in Annex E.

We test for market segmentation for each market-commodity pair in the data (using pre-Corona data only). Results are mixed due to the variety given by the different commodities. In total about 44% of market-commodity pairs are classified as segmented. Figure 3 maps all markets in the data base, whether a market is segmented, and the prevalence of segmented markets in a country indicated by the shade of blue. Dividing the results in quartiles of prevalence of segmentation, we find the lowest shares in Rwanda, Burundi, Cambodia, South Sudan, Central African Republic and Zambia. Highest prevalence includes, most notably, Kazakhstan, Afghanistan, Kygyzstan, Pakistan, and Philippines.

Figure 3: Segmentation test rejection shares by commodity and market



3.3 COVID-19 policy response data

To measure government responses to COVID-19, we use Oxford Coronavirus Government Response Tracker (OxCGRT) data. OxCGRT systematically collects cross-national, cross-temporal information on several different common policy responses that governments have taken to respond to the pandemic (Hale et al., 2020). Data is collected and updated in real time and the project tracks governments' policies and interventions across a standardized series of indicators comprising containment responses, economic policies, health system policies and miscellaneous policies from which a set of composite indices is created.[8] We use simple means to bring daily national data to our monthly analysis level. In the main analysis we rely on the stringency index that combines eight measures on containment and closure policies and a measure on public information campaigns. As robustness check we also use single indicators including measures on internal movement restrictions, restrictions of public transport and stay-at-home restrictions (we refer to Figure 13 in the Annex for an overview of these sub-indicators).

The stringency of responses to COVID-19 varies markedly in the sample. Table 1 shows mean government response and mobility indicators by country during the period from February until October 2020. The OxCGRT is highest in Palestine, India and Iraq with an average score above 0.5 and lowest in Yemen and Burundi with scores around 0.1. We present more information on the timely evolution of the stringency indicator and its consequences for local mobility using Google's mobility data in the Result section.

3.4 Other data sources

In addition to price and policy response data, we use a range of georeferenced information to control for the local environment in which markets are located (we refer to Table 10 in the Annex for a list with data sources). To compute the distance between markets in a country, we use the Open Source Routing Machine to estimate the duration of a car ride between every market pair in a country. In the main analysis we approximate remoteness as distance to the capital city and as distance to the next closest market as share of the average market distances in a country. We also created buffer zones of approximately 25km around each market to add worldpop population estimates (Tatem, 2017), density of tertiary roads (Meijer et al., 2018), night time lights (National Oceanic and Atmospheric Administration),

[8] For more information see github page: https://github.com/OxCGRT/covid-policy-tracker/blob/master/documentation/codebook.md

standardized precipitation index (McKee et al., 1993) calculated from the Climate Hazards Group InfraRed Precipitation with Station data (Funk et al., 2014), ICDR Land Cover data to compute the share of cultivated land, and information on the occurrence of armed clashes from the Armed Conflict Location and Event Dataset (Raleigh et al., 2010). To account for the national and regional intensity of the pandemic, we added the number of confirmed Corona cases according to WHO numbers.[9]

In Table 2 we present summary statistics of these variables during the period from January to October 2020. The mean distance to the next closest market is 1,39 hours by car, 240km to the closest capital city and 260km to the next port. Confirmed Corona cases reach on average 73107 in this period, about 40% of land in a radius of 25km around markets is rain fed or irrigated agricultural land, and about 7% of markets were located in a conflict zone (our sample includes Syria and Yemen). Precipitation was normal in most cases and only around 1% of markets experienced extreme rainfall deviations from the mean of below -2 or above 2 s.d. in one of the months in the research period.

[9]See https://covid19.who.int/ (access December 2020)

	N	Mean (Jan-Oct 2020)
Driving time to next market (hours)	774	1.42
		(1.84)
Distance to capital (km)	774	257.58
		(215.62)
Distance to closest port (km)	774	267.63
		(254.64)
Confirmed Corona cases (country/month)	774	73106.66
		(250556.50)
People per 1km grid square (25km radius)	774	397.39
		(954.36)
Standardized Precipitation Index (25km radius)	771	0.40
		(0.63)
Tertiary road density (25km radius)	774	227.58
		(257.52)
Mean night time light (25km radius)	774	4.53
		(9.27)
Armed clashes (25 km radius)	774	1.74
		(18.28)
Share of cultivated land (25km radius)	774	0.40
		(0.30)
International food price index (FFPI)	774	96.09
		(0.54)
Number of reference markets	774	9.03
		(2.11)

Table 2: Summary statistics of other data sources

Note: standard errors in parentheses. See Table 10 for more information on data sources.

4 Empirical Strategy

Our study investigates whether food prices have been affected by the stringency of policy responses to the COVID-19 pandemic and whether the same national measures affected integrated markets differently compared to segmented markets. For the estimation we rely on data from January until October 2020. That is, the data include months before and after the outbreak of the pandemic, where the timing of the outbreak may differ by country. Our baseline specification is the following:

$$\Delta Y_{xit} = \beta_0 + \beta_1 Segmented_{xi} + \beta_2 Stringency_{it} + \beta_2 Segmented_{xi} * Stringency_{it} + \beta_3 X_{xit} + \delta_x + \theta_{it} + \varepsilon_{xit} \quad (2)$$

Where ΔY_{xit} represents the relative change in the price of commodity x, in market i between the month t and $t-1$. Our main independent variables are the stringency indicator and the interaction term of market-commodity segmentation interacted with the (national) stringency index. X_{xit} is a vector that includes a set of time variant control variables comprising the FAO food price index, SPI index, and the occurrence of conflicts. To the best of our knowledge, there are no other relevant time-varying market-level characteristics with a monthly frequency that can and should be included in the model above. However, price changes are known to be driven by several other economic and institutional variables (Baffes et al., 2019; Aker, 2010; Shively and Thapa, 2017). This includes for instance the local infrastructure, distance to other markets, or population density. To account for these factors, we include the distance to next sampled market, local road density, population densities, night time lights, distance to the capital and next port, and the density of sampled reference markets. All time invariant variables are interacted with a linear time trend following the strategy proposed in Manacorda and Tesei (2020). We gradually add these control variables to the estimations to show the robustness of our main independent variables of interest. To capture unobserved heterogeneity at the national and the local level, we further include month varying country θ_{it} and commodity-market δ_x fixed effects. Standard errors are clustered at commodity-market level in the main analysis.

5 Results

We first present the main empirical results, followed by multiple robustness checks to validate the findings and lastly, we explore effect mechanisms.

Main Results

This section presents results from our main specification. Column 1 in Table 3 depicts that the coefficient of the stringency of policy responses to COVID-19 is positive and statistically significant at the 5% level. This means that more stringent policies tend to generate an increase in prices. The coefficient increases only slightly when we add controls to the baseline estimation (Table 3 - Columns 2 and 3). The coefficient of our preferred specification (Table 3 - Column 3) suggests that a one s.d. increase in the stringency index leads to an increase of the price level of approximately one p.p. (0.31*0.03) per month in non-segmented markets. However, Table 3 also shows that market conditions matter. In particular, it seems that this effect almost disappears in segmented markets. The interaction term is negative and statistically significant at the 0.1% level. The coefficient estimate of -0.02 in Column 3 in Table 3 shows that the effect of a one s.d. increase in the stringency index in a segmented market is 0.62 percentage points lower than in non-segmented markets. The overall effect of the stringency of policy responses to COVID-19 is mainly determined by market conditions: more restrictive measures increase prices in integrated markets while they are less important in influencing price changes in segmented markets.

Table 3: Effect of COVID-19 response stringency on prices

	(1) price change	(2) price change	(3) price change
Stringency Index	0.02**	0.03*	0.03*
	(0.01)	(0.01)	(0.01)
Stringency Index * Segmented	-0.02***	-0.02***	-0.02***
	(0.01)	(0.01)	(0.01)
N	51031	50889	50782
N Cluster	6937	6937	6924
Fixed Effects	yes	yes	yes
Controls	no	yes	yes
Additional Controls	no	no	yes

Note: SE in parentheses. * $p<0.05$, ** $p<0.01$. *** $p<0.001$
Controls include distance to next market, FPPI, (log)ntl, (log)road density, (log)population, number of missings in market-commodity pair. Additional controls further include SPI, armed clashes, number of reference markets, (log)distance to capital and closest port.

Following the conceptual framework, we also expect price dispersion to increase in response to an increase in the stringency of COVID-19 measures. In Table 4 we replace the dependent variable and instead of price changes use the relative difference of prices compared to the reference market price. The coefficients suggest no effect of the stringency indicator on food price dispersion in non-segmented markets. However, the interaction term indicates a negative and significant effect in segmented markets. That is, a one s.d. increase in the stringency indicator, reduces the price difference in segmented markets compared to the reference market price by 0.2 percentage points. Compared to the average difference to the reference market price of about 11%, this effect is small. This result seems to indicate that while prices in segmented market remain unaffected by the stringency of measures, reference market prices increase, thus leading to a slight reduction in price gaps.

Table 4: Effect of COVID-19 response stringency on price dispersion

	(1) price dispersion	(2) price dispersion	(3) price dispersion
Stringency Index	0.24	0.29	0.23
	(0.46)	(0.71)	(0.72)
Stringency Index * Segmented	-1.00**	-0.88*	-0.84*
	(0.38)	(0.38)	(0.38)
N	50133	50002	49895
N Cluster	6876	6876	6863
Fixed Effects	yes	yes	yes
Controls	no	yes	yes
Additional Controls	no	no	yes

Note: SE in parentheses. * $p<0.05$, ** $p<0.01$. *** $p<0.001$
Controls include distance to next market, FPPI, (log)ntl, (log)road density, (log)population, number of missings in market-commodity pair. Additional controls further include SPI, armed clashes, number of reference markets, (log)distance to capital and closest port.

Robustness Tests

In this section we deploy a number of tests to assess the validity of the main results. We test the robustness of our result to potential problems related to reverse causality, omitted variable bias, changes in the functional form specification, and sensitivity to alternative estimators. Our main interest lies on price changes, and as price dispersion is a mechanical consequence of differential price changes, we do not explicitly discuss the effects on price dispersion in the following.

i) Reverse causality As source of exogenous variation to explain changes in the level of prices for commodity x after the outbreak of COVID-19, we rely on the interaction between market integration for commodity x before the outbreak of COVID-19 and the stringency indicator in country i implemented after the outbreak of the pandemic. A key assumption with differences-in-differences estimates are parallel pre-trends. In Figure 15 in the Annex, we show that the trends in prices before the outbreak of COVID-19 are parallel in segmented and non-segmented markets. Furthermore, it is reasonable to assume that price changes for a selected commodity after the outbreak of COVID-19 are not affecting market integration for the same commodity before the outbreak of COVID-19. To further corroborate the validity of this assumption, we regress the market integration index for commodity x in country i before the outbreak of COVID-19 on the changes in prices of commodity x in country i between the month t and $t-1$ after the outbreak of COVID-19. The analysis shows that changes in prices are not correlated to market segmentation suggesting that market segmentation mediates the effects of the stringency indicator on prices (see Table 12 in the Annex).

However, our results would still suffer from simultaneous causality if food prices affect the stringency of COVID-19 measures. Therefore, we additionally apply an instrumental variable approach to prove the validity of our main analysis. For each country, the instrument uses the average number of new Corona cases in the sub-region (without cases of the considered country) as a source of exogenous variation that affects the design of national policies. A similar approach was used by Lee and Gordon (2005) who instrument tax rates in one country using average tax rates in other countries weighted by the inverse of the distance between these countries. Martorano (2018) instruments tax/GDP ratio on the average value of the tax/GDP ratio in countries within the same sub-region. Similarly, Collier and Hoeffler (2004) regress national level of military spending on the level of military spending in neighboring countries, while Ebeke and Ngouana (2015) instrument the level of spending on subsidies in a given country considering the level of subsidies in neighboring countries. In all these papers, the choice of the instrument is mainly justified by the effects of spill-over in policy design across countries within the same sub-region. In our case, it would be justified by the fact that changes in the health crisis conditions will lead to changes in the policy responses at regional level. Governments are expected to be more likely to introduce restrictive policies if neighboring countries experienced an increase in the number of new Corona cases. At the same time, we assume that regional Corona cases only affect staple food prices in markets through the stringency of national policies. To reduce the risk that policies of neighbouring countries directly affect prices we additionally use the average number of new COVID-19 cases in the region instead of the sub-region. In both cases we use the number of confirmed cases and the interaction with market segmentation to instrument prices and prices interacted with market segmentation.

Table 13 in the Annex reports the two stage least squares (2SLS) estimates using regional (columns 1-3) and sub-regional (colums 4-6) Corona cases as instrument. Both sets of instruments are relevant as indicated by the very large first stage F-test results and as the equations are exactly identified, we cannot employ overidenification tests. The estimated coefficients are similar to the main results, yet the statistical significance and coefficient size increase in all models. The 2SLS estimations results are similar to the main estimates of Table 3, and we fail to find indication for reverse causality bias in our estimates.

ii) Omitted variable bias A concern with the discussion above is that the analysis might be biased due to omitted variables. We control for possible confounders and add fixed effects at the commodity-market and country level to reduce the risk of omitted variables bias. Furthermore, we test different functional form specifications to see if we omitted non-linear terms of the stringency indicator in the main estimates. To allow for more flexibility, we use a categorical variable showing quintiles of the stringency indicator instead of imposing a linear relationship on the data. The results suggest that the marginal effect of an increase in the stringency indicator diminishes slightly at high levels of the score above about 0.7. Yet the positive and statistically significant effect on prices remains and we fail to reject the hypothesis that the marginal effects are the same at all quintile levels at the 5% level (see Figure 16 in the Annex). Our findings hold if we add quadratic or cubic transformations of the stringency indicator to the model.

iii) Standard errors Lastly, we test whether alternative clustering specifications change the conclusions we draw from the results. In particular, we correct standard errors for spatial correlations to allow errors between markets to be correlated. Therefore, we adopt the method applied in Fetzer (2014) and Hsiang (2010) and assume that spatial dependence is linearly decreasing in the distance from markets up to a cut-off distance of 500 km. The results using different cut-off values are presented in Table 14 in the Annex and are in line with the main estimation results.[10] In addition, if we cluster standard errors at the market or different regional levels, the coefficient of the interaction remains significant at the 5% level in all specifications, yet the statistical significance stringency indicator coefficients drops to the 10% level in most specification.

Mechanisms and heterogeneous effects

Our main analysis establishes a significant relationship between the stringency of COVID-19 policies and changes in prices. However, this effect is conditional on the market structure: more restrictive measures increase prices in integrated markets while they are less important in influencing prices in segmented markets. In this section, we extend the main analysis by first analyzing the potential pathways through which policy responses may influence price levels and thereafter explore heterogeneous effects of the stringency of policy responses to COVID-19 on prices. We identified, in particular, changes in local mobility as main mediator through which the stringency of measures affects prices and, following the conceptual framework, we examine differential effects depending on the level of local agricultural activity, a market's remoteness, and by commodity group.

i) Stringency and mobility

COVID-19 policy responses are only expected to impact food prices if they are enforced. In regions where the state is weak, the OxCGRT stringency indicator may not reflect restrictions on the ground. Therefore, we also consider Google's COVID-19 Community Mobility Reports as measure for changes in mobility. The reports chart movement changes compared to a reference period across different categories of places including retail and recreation, groceries

[10] Note that because we are using a different estimator, the coefficients of the stringency indicator change in the spatial autoregressive models.

and pharmacies, parks, transit stations, workplaces, and residential (Aktay et al., 2020).[11] Data is available at the national, admin-1 and admin-2 level and we match mobility data to markets on the lowest geographic entity for which Google provides data. In 54% of markets we have admin-1 data, in 10% admin-2 level data, and in 36% national information.

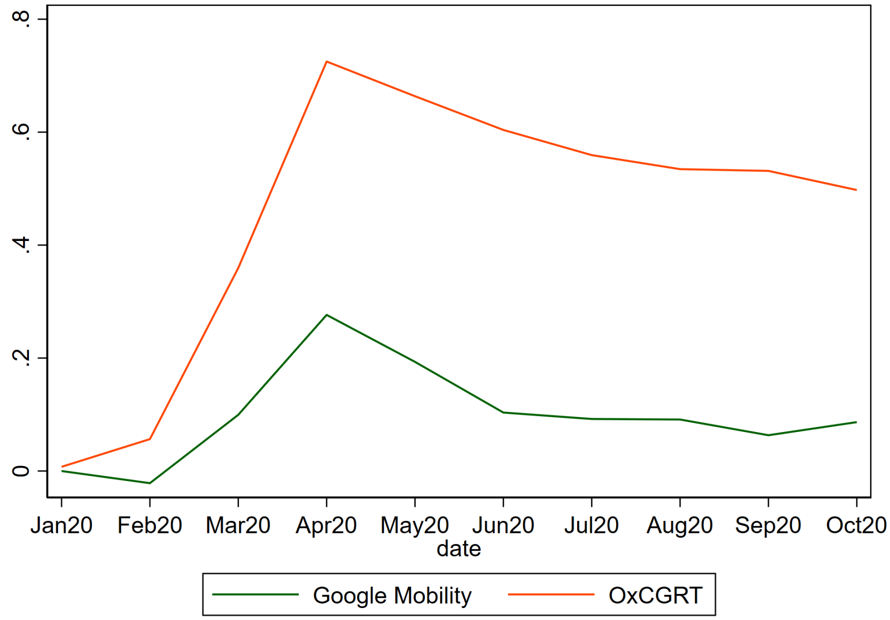
Figure 4 shows the development of the government response index and google mobility data since January 2020. The graph indicates a steep increase in stringency of government's responses from February until April in our sample and a slow and steady decrease thereafter. This trend in policies is clearly mimicked by the mobility data that show the largest reduction in April and a steady recovery thereafter.[12] The recovery in mobility after April seems, however, to happen a bit faster than the decline in the response indicator. The correlation between both measures is high, reaching 0.65 in our sample. An overview of policy responses of sampled countries by geographic regions is presented in Figure 14 in the Annex.

To understand where and under which circumstances the stringency of COVID-19 responses is associated with changes in mobility, we regress the Google indicator on the stringency indicator interacted with a range of market characteristics using OLS models (Table 11 in the Annex). Besides large differences by geographic region, the stringency of measures is more strongly associated with mobility reductions the higher the number of confirmed Corona cases, a smaller effect the further away the region from the capital, and it increases in conflict regions. The effects are more pronounced if we only focus on regions where sub-national Community Mobility Reports were available.

[11] Data available at https://www.google.com/covid19/mobility/

[12] Mobility changes in sub-categories such as markets and pharmacies are very similar to overall mobility changes including all categories.

Figure 4: Government COVID-19 responses and mobility changes



To explore the role of changes in mobility for the development of prices, we re-estimate our baseline estimations, replacing the stringency of responses to COVID-19 with the mobility index. Again, the sign and statistical significance of the coefficient remains similar to the baseline results. Table 5 shows a significant relationship between mobility restrictions and changes in the price level. On average, a one s.d. reduction in mobility increases the price by just short of one p.p. (0.15*0.04) in non-segmented markets, which is very close to the effect size we observed using the stringency indicator. The effect is significant at the 0.1% level. As before, the effect disappears in segmented markets. The results indicate that the effect of the stringency of policy measures on food prices is mediated by changes in mobility, which however, is moderated by the pre-Corona trade dependency of markets.

It should be noted that the Google indicator is not available for all countries in our data set (see Table 1), but the results are not driven by difference in the composition of the sample. To test this we ran the main estimates of Table 3 only for markets with Google mobility data and obtained similar results. In addition, the results hold if we restrict the estimation sample to markets with sub-national mobility data.

Table 5: Effect of mobility reductions on prices

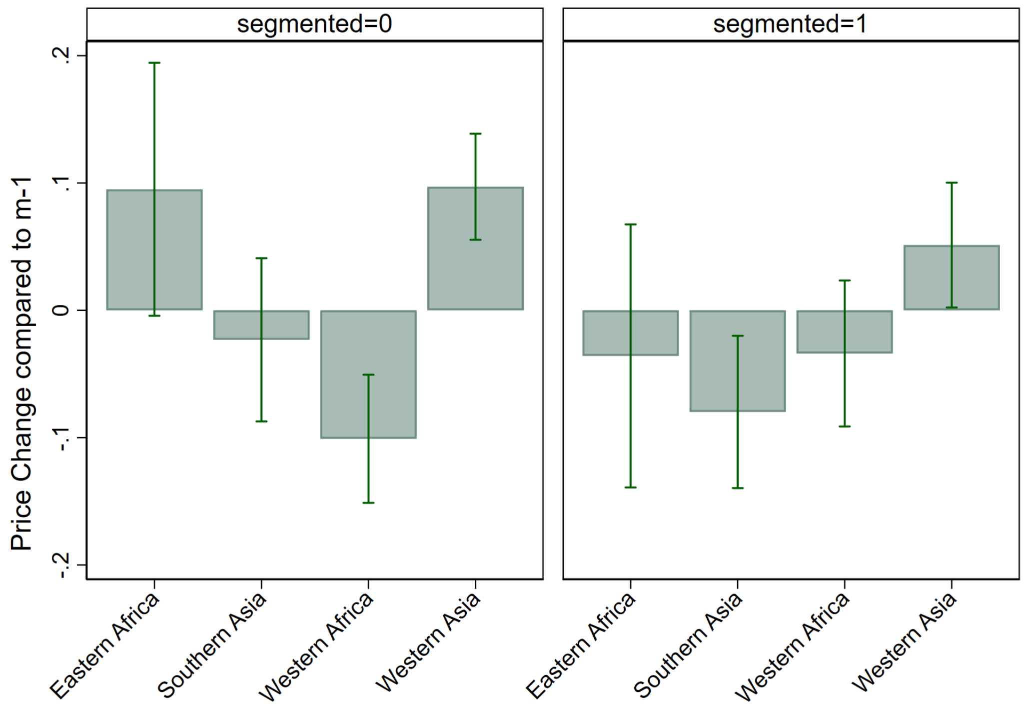
	(1) price change	(2) price change	(3) price change
Mobility Reduction	0.04^{**}	0.06^{***}	0.05^{***}
	(0.01)	(0.01)	(0.01)
Mobility Reduction * Segmented	-0.05^{***}	-0.04^{***}	-0.04^{***}
	(0.01)	(0.01)	(0.01)
N	39801	39736	39629
N Cluster	5255	5255	5242
Fixed Effects	yes	yes	yes
Controls	no	yes	yes
Additional Controls	no	no	yes

Note: SE in parentheses. * p<0.05, ** p<0.01. *** p<0.001
controls include distance to next market, FPI, (log)ntl, (log)road density, (log)population, number of missings in market_commodity pair. Addiotional controls further include SPI, number of battles, number of reference markets, (log)distance to capital and closest port.

As the relationship between the stringency indicator and mobility reductions differs significantly by geographic region, we plot the estimated effect of a unit change in mobility on prices for the four sub-regions that cover more than 90% of the observations in our data.[13] Figure 5 suggests that prices in segmented markets (right panel) are less sensitive to mobility changes compared to non-segmented markets (left panel) except for markets in Southern Asia. Prices in non-segmented (sampled) markets in Western Asia and Eastern Africa are positively affected by changes in mobility, while the effect is negative in Western Africa and there is no effect in sampled markets in Southern Asia. These differences could be related to dependencies on imports and commodity specifities, which we further examine in the next section. However, it is important to bear in mind that the sampled markets are not necessarily representative for these regions.

[13]In the estimations we add the sub-region indicator to the interaction term segmented*stringency and report the effect of a unit change in the mobility indicator on food prices for each sub-region.

Figure 5: Effect of unit reduction in mobility on price changes by sample region (95% CI)



ii) Heterogeneous effects

Our final research objective is to explore other factors that may moderate the relationship between restrictive policy measures and food prices. Therefore, we examine differential effects in areas with high/low agricultural activity, market remoteness, and by commodity.

In the conceptual framework we postulate that the effect direction of restrictive COVID-19 measures on prices depends on whether local markets are net-importers or exporters. We expect increasing prices in importing markets because of decreasing supply and decreasing prices in exporting markets because of excess supply. We don't have trade data for local markets, but we matched information on the amount of agricultural land surrounding markets, which we regard as a prerequisite for the local cultivation of foods. We use the median prevalence of irrigated or rain-fed agricultural land in a radius of 25km of each market as cutoff to distinguish between markets with low and high agricultural activity and run our preferred specification for each sub-set separately (see column 1 and 2 of Table 6). In more agricultural-oriented areas, the implementation of more restrictive measures decreases prices and particularly so in segmented markets. By contrast, stringent policies significantly increase the level of prices in less agricultural-oriented areas. Similarly, using regional night-time-lights to classify more urban, industrial areas suggests that price increases were more pronounced in these markets (see Table 15 in the Annex).[14] As additional robustness check, we match FAO data on the national share of imports in cereals to split the sample into high and low national dependency on imports. The result are similar indicating that the effects of the stringency indicator on prices is driven by markets in more import dependent countries (see Table 15 in the Annex). While our indicators are far from perfect, the results support our hypothesis that policy responses have different effects on prices depending on the structure of markets and in this case, the local availability of agricultural produce.

In the conceptual framework we refer to the role of remoteness for the impact of COVID-19 measures on food prices, yet without specifying impact directions. Previous results suggest that more stringent measures affect reductions in mobility differently in areas further away from the capital which could suggest that prices are less impacted by COVID-19 policies in remote areas. However, distance to other markets could increase transportation costs and lead to supply disruptions for instance if inter-regional traffic is banned. We approximate remoteness as distance to the capital city and the results for the sub-set of markets closer (further) to the capital than the median market is shown in column 3 (and 4) of Table 6. The results suggests that the positive increase in prices is driven by less remote markets. This could imply that, as mobility is less affected in remote areas, prices are less sensitive to the stringency of measures. Using the distance to the closest market as share of the mean distances of markets in a country as robustness check leads to very similar result (see Table 15 in the Annex). However, the distance to the capital may also pick up other market characteristics (e.g. agricultural orientation) which is why the result needs to be interpreted

[14] We use the median and 75th percentile of night-time-lights in the 25km buffer zone around each market as cuttoffs to classify urban regions.

with caution.

Lastly, we examine if effects differ by commodity. Therefore, we group all varieties of rice, wheat, beans, and maize and run the estimations for each group separately. The time series chart of prices in our sample indicated different dynamics in 2020 (see Figure 11 in the Annex), which is also reflected in the estimates (see columns 5-8 of Table 6). The effect of the stringency indicator is positive in all models, but much larger for maize than for wheat. The second largest effect is on beans, yet, the coefficients fails the 5% significance level. As the number of observation drops quite significantly, it is not surprising that standard errors increase. If we group all these staples and distinguish them from other foods, the results suggest that the stringency indicator affects staples more strongly and that the differential effect for segmented markets is more pronounced for non-staples (see Table 15 in the Annex). However, these results need to be interpreted with caution as this definition of staples is quite arbitrary and in principle, all sampled commodities are selected because of their importance in the diet of vulnerable households in the respective regions.

The results of the heterogeneous effects analysis help to better understand where and under which circumstances the stringency of policy responses to the pandemic led to price increases. Impacts seem to be more pronounced in less remote, urban areas and less important for prices in segmented and agricultural areas. While these results are in line with the conceptual framework, the results are only suggestive and we cannot fully rule out that the interacted proxy indicators pick up other important local aspects which could confound the analysis.

6 Concluding remarks

We analyze how COVID-19 policies affect food prices in 47 LMICs, focusing particularly on how the integration of markets in a larger network mediates this effect. The results point at increases in food prices with more stringent policy measures, but the effects are driven

Table 6: Effect of COVID-19 response by prevalence of local agricultural land

	Agr.Area>Median	Agr. Area<Median	Not Remote	Remote	Rice	Wheat	Maize	Beans
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	price change	price change	price change	price change	price change	price change	price change	price change
Stringency Index	-0.05**	0.04**	0.04**	0.00	0.03	0.02	0.20*	0.08
	(0.02)	(0.02)	(0.01)	(0.02)	(0.03)	(0.04)	(0.10)	(0.05)
Stringency Index * Segmented	-0.04***	0.00	-0.02**	-0.02**	-0.02	0.00	-0.04	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)
N	25152	25549	24639	26105	5901	2192	4755	3742
N Cluster	3396	3513	3408	3512	827	299	676	519
Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes	yes	yes	yes

Note: SE in parentheses. * $p<0.05$, ** $p<0.01$. *** $p<0.001$
Controls include distance to next market, FPPI, (log)ntl, (log)road density, (log)population, number of missings in market-commodity pair.
Additional controls further include SPI, armed clashes, number of reference markets, (log)distance to capital and closest port.

by integrated markets that are more dependent on trade with other markets. The effect size in integrated markets is considerable: a one s.d. increase in the stringency of policies, increases monthly price changes by one p.p.. This effect seems to be mediated by changes in local mobility, and the effects are particularly pronounced in less remote regions with low local agricultural activity. The results hold against several robustness tests, but these alternative specifications should be taken with caution because all empirical strategies we employed have certain weaknesses. However, the consistency of the results across different estimators reassures us about the validity of our findings.

The results imply that the impact of COVID-19 policy responses on food security are far from uniform. Vulnerable households in less remote and trade dependent regions are more severely affected by price increments than households in the vicinity of segmented markets. This is the opposite of the effect of other types of shocks on prices such as natural disasters (Hill and Porter, 2016) or positive income shocks in the form of cash transfers (Cunha et al., 2018; Filmer et al., 2018) but similar to the effects of trade shocks (Porteous, 2017; Anderson et al., 2013). Our results imply that, in the face of far-reaching mobility reductions, short-term relief programs should account for the unequal effects on food prices that may also benefit producers. Indiscriminately scaling up existing safety nets, which often target rural populations, might fail to address the needs emerging from stringent policy responses to the pandemic for households in less agricultural and integrated market catchment areas. However, we analyze the short-run impacts of COVID-19 policies, which could substantially differ from the mid- and long-run impacts of the pandemic on food security.

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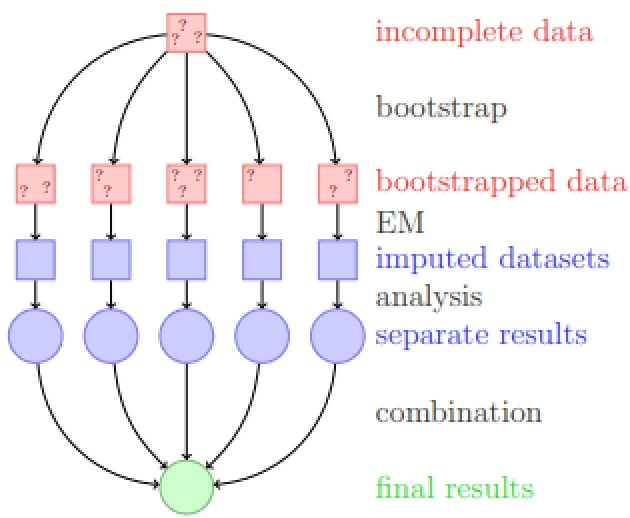
Annex

A

The imputation model assumes that the distribution of missing value is random, where complete data D is distributed as a multivariate normal $D \sim \aleph(, \Sigma)$. Distribution of missing data M, given complete data D is equal to the distribution of missing data M given observed data: $p(MD) = p(MD_{obs})$. The expectation maximization algorithm, based on bootstrapped data, is illustrated in 6.

Figure 6: Data imputation algorithm treeview - Amelia R package



Source: Amelia R package vignette (Honaker et al., 2011).

The ratio for each country is based on the base model below.

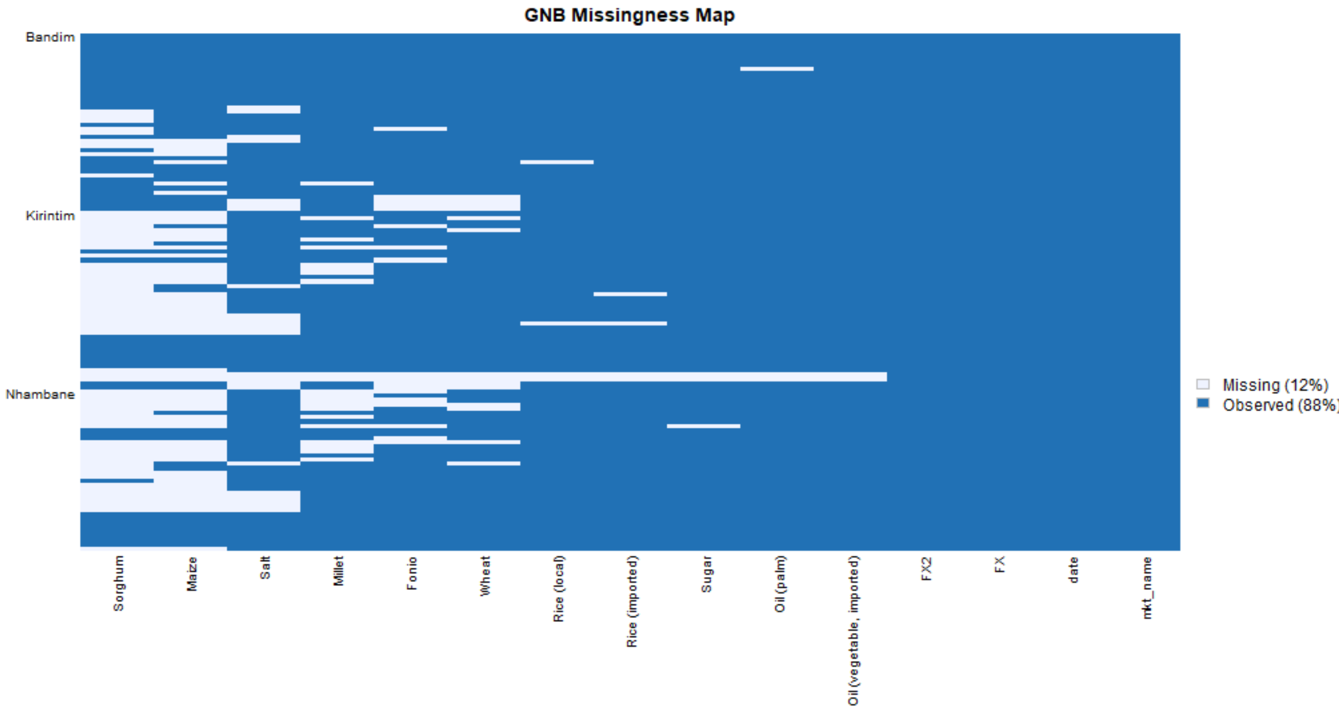
$P_{(t,c,m)}\alpha_{(0,c,m)} + \overline{\alpha}_{(1,c,m)}\overline{P}_{(t+1,\bar{c},m)} + \beta_{(1,c)}X_{(t-1)} + \beta_{(2,c)}X_{(t+1)} + \beta_{(3,c)}X^2_{(t-1)} + \beta_{(4,c)}X^2_{(t+1)} + \varepsilon_{(t,c,m)}$

Prices for each commodity and market over time $P_{(t,c,m)}$ are imputed using lagged and forward values of exchange rates to US Dollar X_t and their square, with a different constant for each market and future price of other commodities in the same market $\overline{P}_{(t+1,\bar{c},m)}$. Moreover, we constrained the model to impute only results in the interval between 0.5 times the minimum and 1.5 times the maximum value for each of the commodities observed in a country. Albeit lacking some obvious checks (such as the absence of additional lagged values) and the presence of multi-collinearity, the model chosen allows estimation of missing values in six datasets with reasonable time (~ 36 hours) and without accounting for each specificity or exogenous variables. The coverage of such variables for more than 2000 locations globally would easily complicate the imputations, while, as mentioned by Honaker et al. (2011), the parsimony or use of future values are in line with the predictive objective. The advantage of this model is its capability to infer a missing price of a commodity in a market, thanks to the information set available of prices of other commodities in the same market or the price of different commodities in other markets, even if those time series are, in turn, incomplete. We excluded from our analysis time series that had convergence issues, specifically 16 series from 7 countries and two markets (Nairobi (Kenya) and Conakry (Guinea)). Later, we further cleaned the database removing markets in a country with more than 2/3 of the series missing (for all commodities) and all commodities with more than 2/3 of the series missing (for all markets in a country).

After this, we also removed series which had null variance in the period of interest. Finally, if a market has more than 5 commodities, we randomly sub-sampled 5 commodities to be included as part of the lead prices in the model. As additional parameters of the imputation method, we limit iterations of the expectation maximization algorithm between 5 and 10,000 executions and create 5 imputed sets for each data point. The entire process is repeated in order to increase the sample-size and ensure our results are not strongly dependent on the random sub-sampling applied. The 10 sets of imputed values resulting from this process are later aggregated with arithmetic average for each data point (commodity, market and date) to give the final imputed value.

Different diagnostics part of the R - Amelia II package are launched during this process. Before running imputations, a missingness map is generated for each country, visually highlighting where missing values are concentrated the most in market/commodity combinations (see Figure 7).

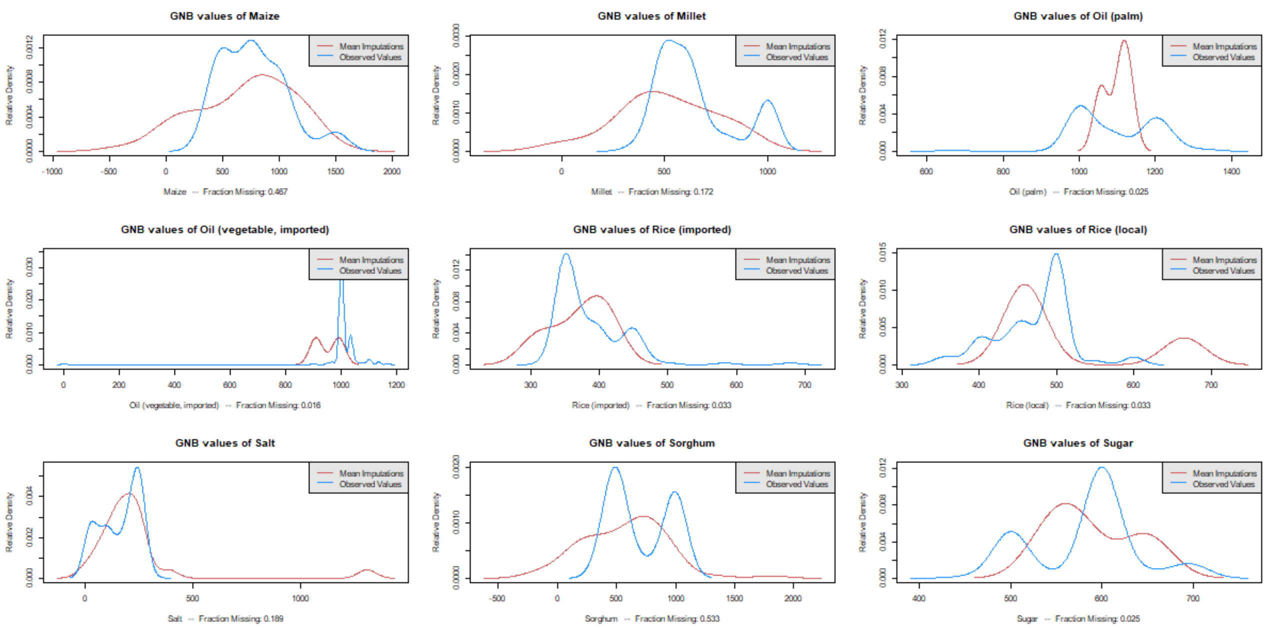
Figure 7: Missingness map, price data, Guinea-Bissau



In this example we see how missing values are spread among commodities (on the horizontal axis) including exchange rates (FX and its squared values) and the remaining three markets (on the vertical axis, combined with time). In this graph, Sorghum and Maize have evidently more missing values than other commodities, and Kirintim market has a number of missing values for all commodities, meaning it was excluded from data collection for multiple months (which further analysis reveals to be three consecutive months in 2019).

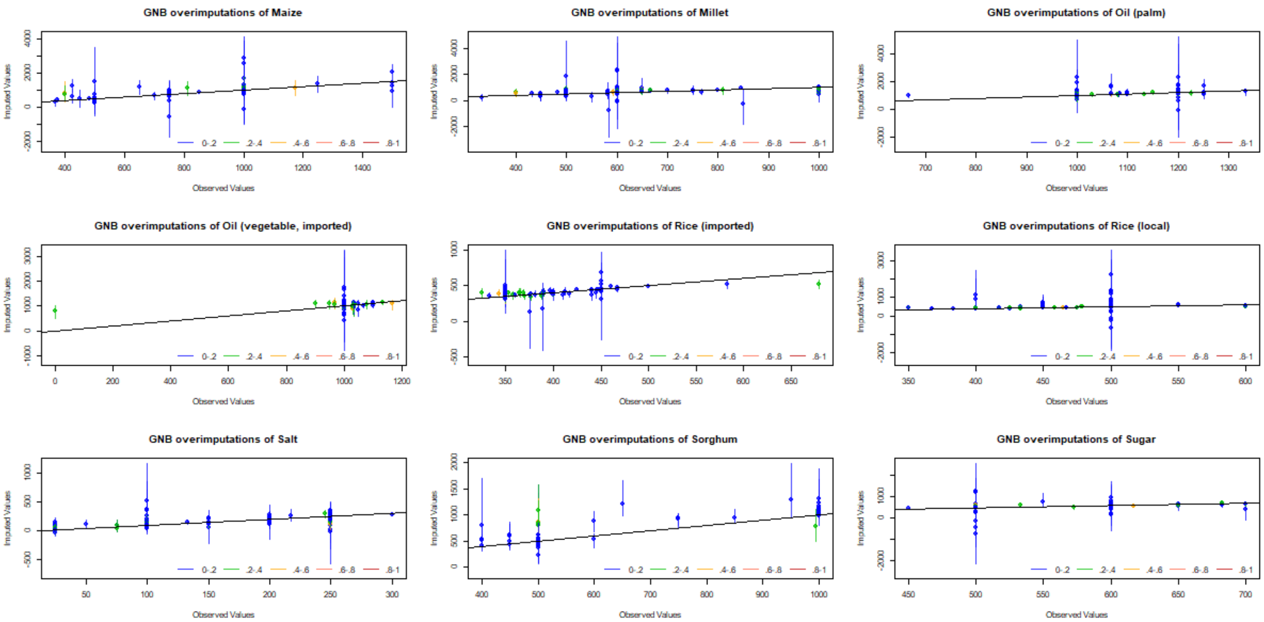
As part of the imputation process, we try to visually assess the accuracy of our estimates through kernel densities of imputed and observed values (see Figure 8).

Figure 8: Density comparison of imputed and observed values by commodity, Guinea-Bissau



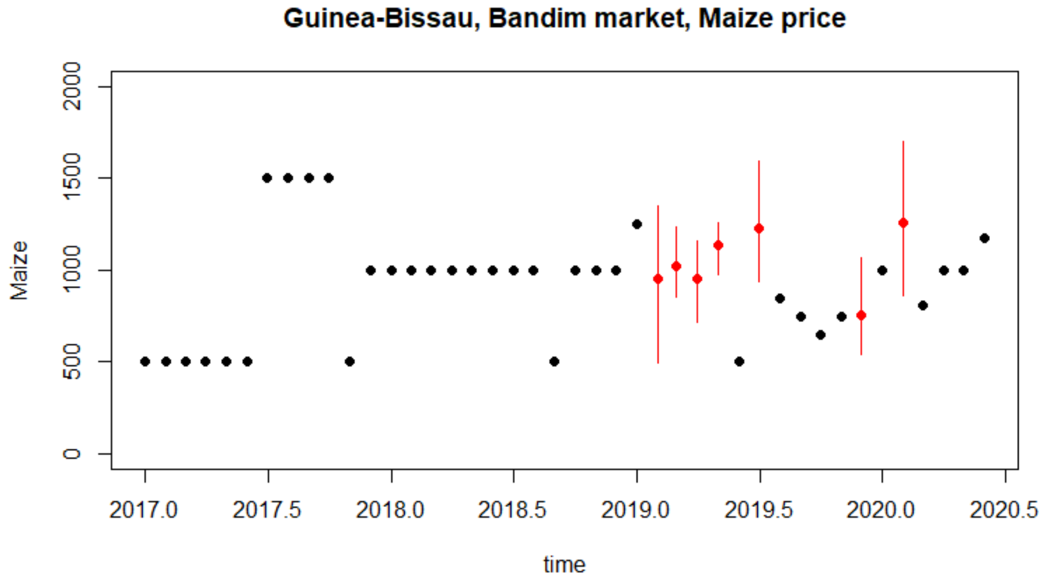
Finally, because of the nature of the missing data mechanism, it's not possible to judge if the prediction of the imputation model is close to the unobserved value we're trying to reconstruct. To overcome this limitation, through multiple imputations, we're able to construct 90% confidence intervals around the average estimation of observed values, that are sequentially treated as missing. The colour of the points is classified in the graphs based on the share of missing values in each overimputed series. This technique, overimputation, allows to graph both observed and imputed value of each price data point for all markets in which we have observed price data of a commodity (see Figure 9).

Figure 9: Overimputation of observed values by commodity, Guinea-Bissau



In these graphs we're able to intercept those unique time series that resulted in overimputed values whose confidence interval doesn't include the observed price value. As a last check for the quality of our imputations, we observe time series graphs with imputed and observed values. Here, as an example, we look at Maize prices, which have more missing values than other commodities in Guinea-Bissau, the country selected for the above examples (see Figure 10).

Figure 10: Maize price in LCU, Bandim market



Noting the imputation process is only used to inform the estimation of the Market Integration measures, we attempt to produce, as a robustness check for data imputation, linear interpolation for all missing values (Becker et al., 1988). Relying on a linear trend constructed on the time variable, we obtain new estimates for 2,668 unique series out of 6,024 missing values. We then proceed to re-estimate the main three market integration measures on these series. Using the same model specification described in our main analysis, we obtain new regression coefficients; still maintaining the same algorithms as described in Annex E, we perform F-tests for segmentation, long-term integration and short-term integration on the newly estimated coefficients. The rejection of the test is different from the result obtained using the Amelia-II method for 345 (3.7%), 309 (3.4%) and 267 (2.9%) series (out of a total of 9,118 series used in the analysis). The average prevalence of segmented, long-term integrated and short-term integrated series is mostly unchanged for all three indicators (respectively -0.8, -1.1 and +0.6 percentage points).

Table 7: Market integration indicators by imputation method

Market Integration Indicator	Data Imputation method	N	Rejection rate (only series with missing values)	# Se
Segmentation	Amelia-II	6.024	40,8%	
	Interpolation	2.668	40,0%	
Long-term integration	Amelia-II	6.024	21,8%	
	Interpolation	2.668	20,7%	
Short-term integration	Amelia-II	6.024	79,2%	
	Interpolation	2.668	79,7%	

Table 8: Correlates of imputed observations

	(1) imputed (OLS)	(2) imputed (OLS)	(3) imputed (logit)	(4) imputed (logit)
main				
Distance to closests market	-0.00	-0.00	-0.01	-0.01
	(-1.05)	(-1.06)	(-1.32)	(-1.36)
Distance capital	-0.00	-0.00	-0.00	-0.00
	(-0.73)	(-0.75)	(-0.56)	(-0.58)
Distance port	-0.00	-0.00	-0.00	-0.00
	(-1.46)	(-1.48)	(-1.15)	(-1.16)
Agricultural activity	0.04	0.04	0.43	0.42
	(1.37)	(1.34)	(1.08)	(1.07)
Armed coflicts	-0.00**	-0.00**	-0.00	-0.00
	(-2.89)	(-2.86)	(-1.91)	(-1.90)
NTL	-0.00	-0.00	-0.03	-0.03
	(-0.77)	(-0.75)	(-0.89)	(-0.86)
StD NTL	-0.01	-0.01*	-0.09	-0.09
	(-2.00)	(-2.02)	(-1.68)	(-1.71)
Road density	0.00	0.00	0.00	0.00
	(0.11)	(0.13)	(0.06)	(0.07)
StD Road density	0.00	0.00	0.00	0.00
	(0.68)	(0.66)	(0.11)	(0.10)
Population density	0.00	0.00	0.00	0.00
	(1.25)	(1.21)	(1.47)	(1.44)
StD Population density	-0.00	-0.00	-0.00	-0.00*
	(-1.82)	(-1.79)	(-1.94)	(-1.96)
SPI	0.01	0.01	0.06	0.06
	(0.52)	(0.53)	(0.34)	(0.37)
Other NAs in timeseries		-0.00		-0.00
		(-0.41)		(-0.39)
Month, Year FE	yes	yes	yes	yes
N	51126	51126	50816	50816

t statistics in parentheses
Note:
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

B

Figure 11: Monthly price development compared to previous year

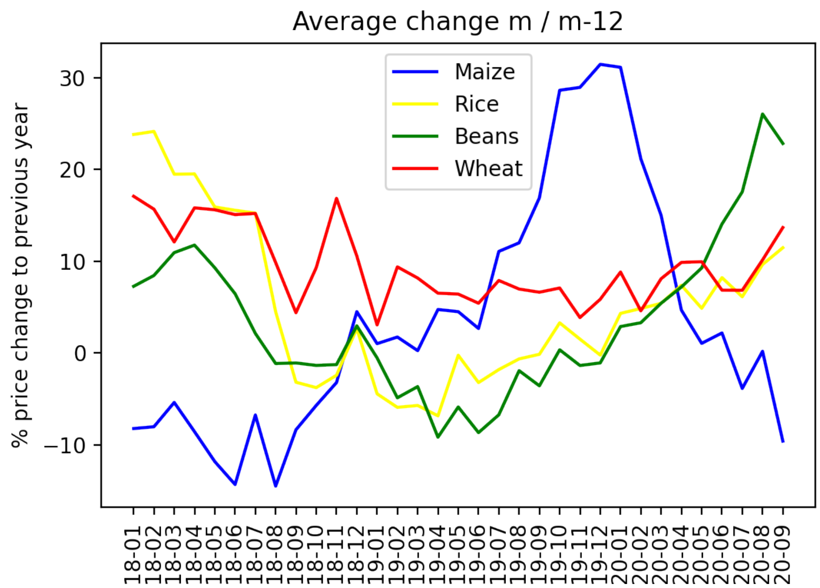


Figure 12: Monthly price dispersion compared to previous year

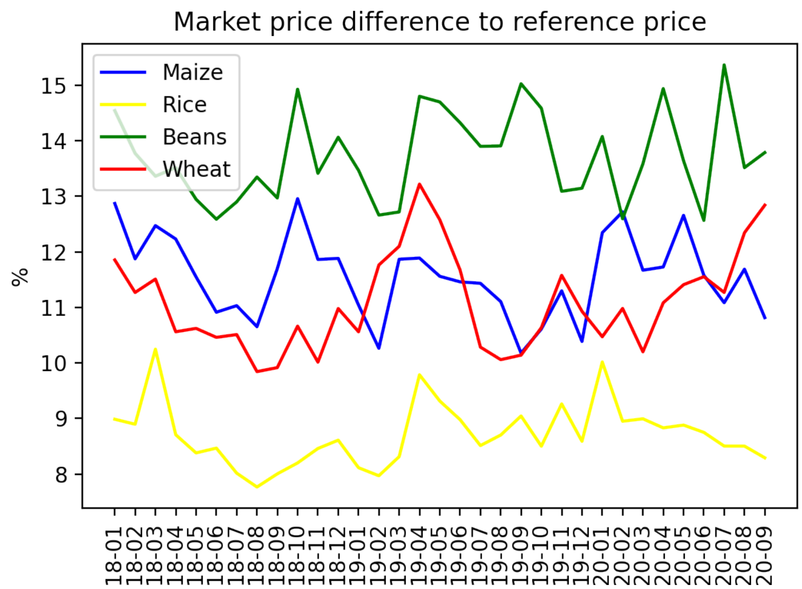


Table 9: Sampled commodities

commodity	markets	commodity	markets	commodity	markets
Millet	282	Cabbage	26	Groundnuts	8
Rice (imported)	264	Fish (frozen)	25	Rice (medium grain imported)	8
Tomatoes	216	Bread (bakery)	25	Oranges	8
Sugar	213	Meat (beef canned)	24	Maize meal (white with bran)	8
Maize (white)	212	Fish (dry)	24	Rice (long grain imported)	7
Rice (local)	207	Beans (red)	24	Bread (vetkoek)	7
Wheat flour	204	Cassava	23	Rice (mixed low quality)	7
Sorghum	193	Milk (non-pasteurized)	23	Cassava (fresh)	7
Onions	180	Fish	23	Fish (fresh)	7
Potatoes	157	Maize meal	22	Cassava leaves	7
Maize	153	Lentils (red)	22	Pulses	7
Rice	150	Rice (imported Egyptian)	22	Rice (milled superior)	6
Lentils	146	Beans (kidney red)	22	Rice (regularmilled)	6
Eggs	125	Pasta (spaghetti)	21	Bread (brotchen)	6
Beans (niebe)	123	Tomatoes (paste)	20	Sorghum (white imported)	6
Meat (beef)	123	Peas (yellow split)	20	Meat (pork)	6
Salt	115	Bulgur (brown)	20	Tea (green)	5
Oil (vegetable)	113	Maize (imported)	20	Sorghum (food aid)	5
Salt (iodised)	110	Apples	19	Beans(mash)	5
Oil (palm)	110	Rice (low quality)	19	Rice (tchako)	5
Beans (white)	105	Wheat flour (local)	19	Ghee (artificial)	5
Groundnuts (shelled)	97	Peas	18	Maize meal (imported)	5
Groundnuts (unshelled)	96	Butter	18	Poultry	5
Oil (sunflower)	93	Walnuts	18	Peas (green dry)	5
Wheat	91	Buckwheat grits	18	Oil (cooking)	5
Beans (dry)	90	Bread (first grade flour)	18	Oil (cotton)	5
Beans	86	Lentils (green)	18	Beans (haricot)	5
Bananas	85	Semolina	18	Beans (sugar)	4
Milk (pasteurized)	79	Kefir	18	Beans (niebe white)	4
Chickpeas	79	Rice (medium grain)	18	Wheat flour (high quality)	4
Tea (black)	77	Fish (bonga)	18	Bread (traditional)	4
Cassava flour	76	Rice (small grainimported)	17	Oil (mixed imported)	4
Sweet potatoes	71	Livestock (sheep med male)	16	Sugar (brown)	4
Meat (chicken)	68	Cheese (local)	15	Oranges (big size)	4
Potatoes (Irish)	68	Apples (dried)	15	Chickpeas (local)	4
Lentils (masur)	64	Oil (vegetable local)	15	Eggplants	3
Maize meal (white breakfast)	63	Bread (khoboz)	15	Milk (powder infant formula)	3
Lentils (moong)	60	Cheese (low-fat)	15	Meat (veal)	3
Sorghum (white)	59	Oil (vegetable imported)	15	Meat (pork with fat)	3
Maize flour	58	Cucumbers (greenhouse)	14	Fish (smoked)	3
Fish (tuna canned)	58	Groundnuts (small shelled)	14	Beans (magnum)	3
Bread	56	Bread (brown)	14	Zucchini	3
Lentils (urad)	55	Beans (butter)	14	Coffee	3
Milk (powder)	55	Cowpeas	14	Cocoa (powder)	3
Sugar (jaggery/gur)	54	Maize meal (white first grade)	14	Tea (herbal)	3
Ghee (vanaspati)	54	Beans (catarino)	13	Bread (common)	3
Oil (mustard)	53	Cheese (fat)	13	Sorghum (taghalit)	2
Meat (goat)	52	Chili (red dry raw)	13	Cornstarch	2
Livestock (sheep 2yrs male)	51	Spinach	12	Rice (denikassia imported)	2
Rice (low quality local)	48	Meat (chicken whole)	12	Sesame	2
Rice (high quality local)	47	Cheese (white boiled)	12	Yam (florido)	2
Oil (soybean)	45	Beans (fava dry)	12	Fish (appolo)	2
Maize meal (white roller)	45	Livestock (goat medium male)	12	Watermelons	8
Pasta	44	Tomatoes (greenhouse)	11	Coffee (instant)	8
Dates	44	Onions (dry local)	11	Rice (paddy long grain	8
Bulgur	43	Pigeon peas	11	Yam	8
Oil (groundnut)	42	Potatoes (medium size)	11	Groundnuts (large shelled)	8
Yogurt	42	Rice (coarse)	11	Sorghum (red)	29
Oil	41	Maize meal (white wto bran)	11	Parsley	29
Garlic	40	Meat (goat with bones)	11	Fish (lates dry local)	27
Milk	39	Oil (maize)	11	Oil (olive)	26
Tea	38	Bananas (medium size)	11	Wheat flour (first grade)	26
Meat (mutton)	38	Labaneh	11	Meat (beef minced)	29
Cheese (picon)	34	Cauliflower	11	Meat (lamb)	29
Maize (local)	33	Beans (sugar-red)	10	Bread (shop)	29
Cheese	32	Cassava (cossette)	10	Rice (basmati broken)	9
Cucumbers	32	Cheese (goat)	10	Wheat flour (imported)	9
Carrots	31	Cassava meal (gari)	10	Beans (black)	9
Apples (red)	31	Peas (split dry)	10		
Bread (pita)	30	Maize meal (local)	9		
Sugar (white)	29	Meat (chicken frozen)	9		

Table 10: Data sources and variable definitions

variable	description	source
Stringency	Average of 9 indicators of government measures in response to COVID.	github.com/OxCGRT/covid-policy-tracker / blob/master/ documentation / index_methodology.md
Mobility	Indicator showing reductions in mobility to a benchamrk period. Transfored so that positive values reflect reductions in mobility.	https://www.google.com/ covid19/mobility/
Cases confirmed	Total COVID case load in the world region. Log transformed in 2SLS model.	https://covid19.who.int/table
SPI	Standardized precipitation index, the number of standard deviations that observed precipitation cumulated over 3 months deviates from the climatological average in a 25 km radius. Monthly averages matched to market data.	Funk et al., 2014
Armed clashes	Number of battles per month, defined as violent interactions between two politically organized armed groups, in a 25 km radius. Monthly averages matched to market data.	https://acleddata.com/
FPPI	FAO food price index.	http://www.fao.org /worldfoodsituation/ foodpricesindex/en
Mean night time light	Index of electric night time lights, based on 2013 data, in a 25 km radius. Annual average matched to market data and log-transformed in main estimations.	https://sos.noaa.gov/ datasets/nighttime-lights/
Road density	Density of tertiary roads in a 25 km radius. Average matched to market data and log-transformed in main estimations.	https://www.globio.info/ download-grip-dataset
Population density	Average population density in a 25 km radius. Log-transformed in main estimations.	https://www.worldpop.org/
Driving time to next market	Distance of the closest market (in driving hours) as percent of the average of the distances between all of the country's markets	http://project-osrm.org/docs/v5.22.0/ api/#route-service
Share of cultivated land	Average of rain-fed and irrigated farm land in 25km radius (constant).	https://cds.climate.copernicus.eu /cdsapp#!/ dataset/satellite-land-cover?tab=form

C

Figure 13: OxCGRT mobility restrictions

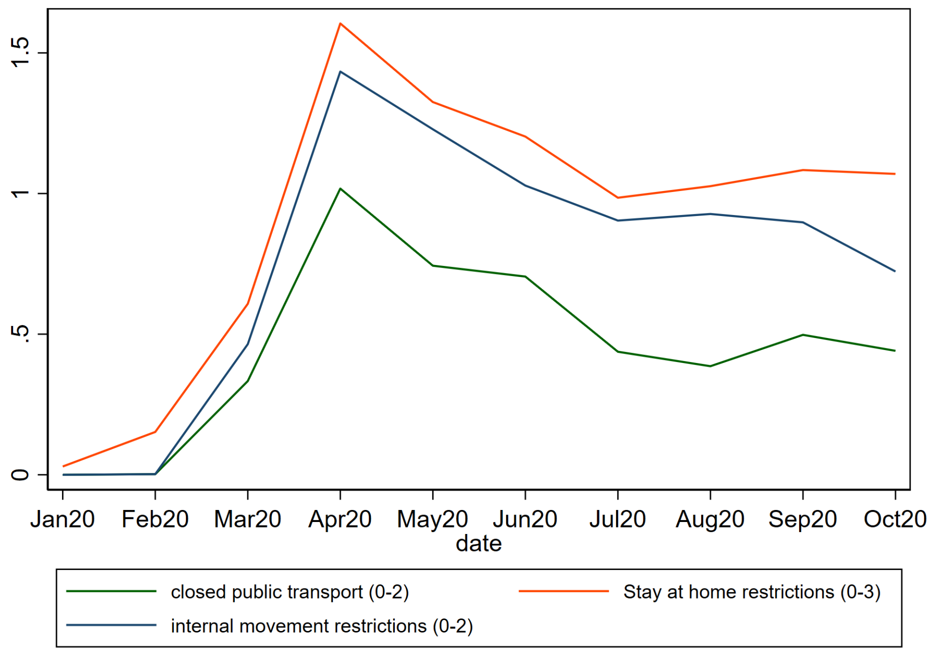


Figure 14: Government responses and mobility changes in the sample by region

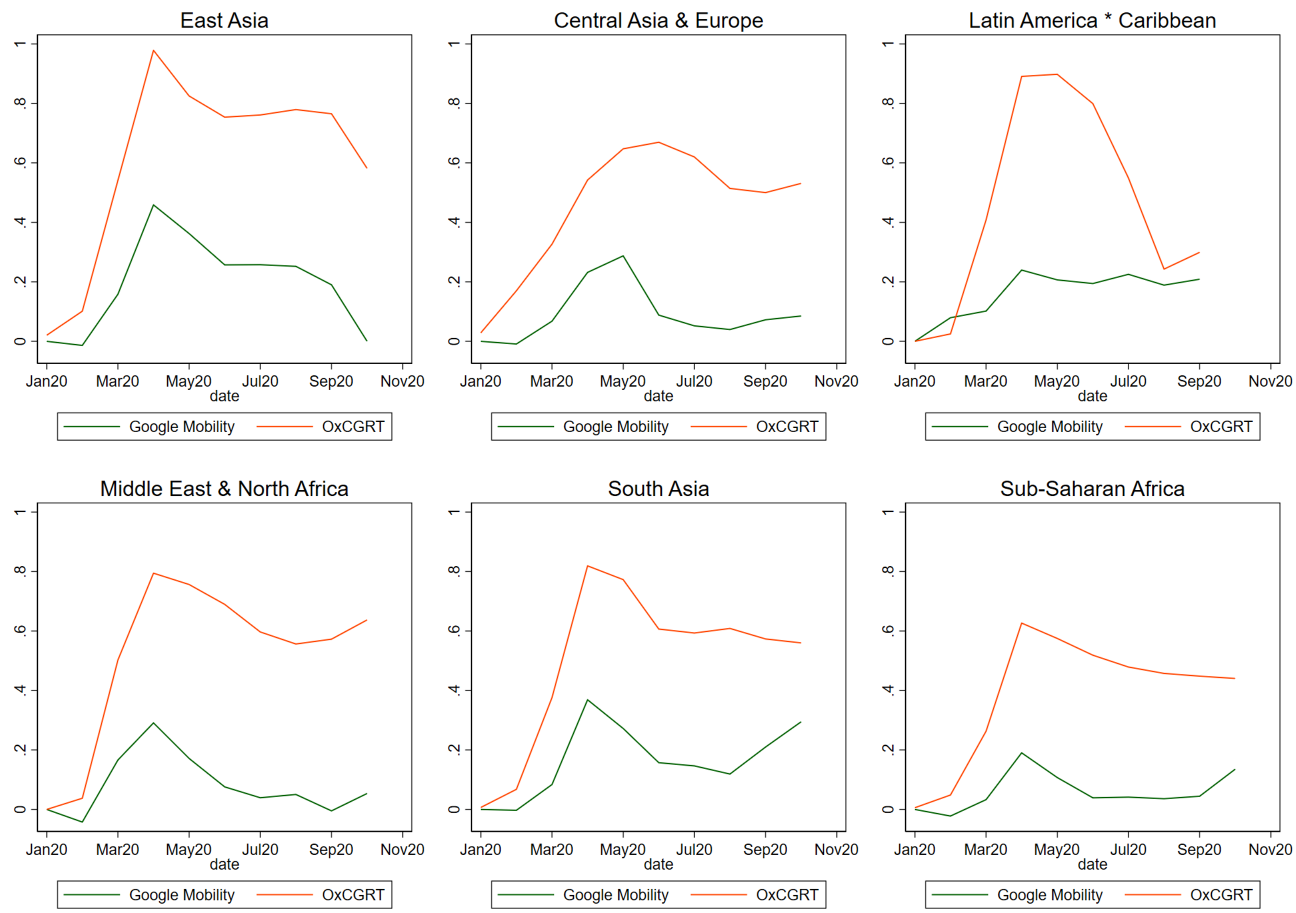


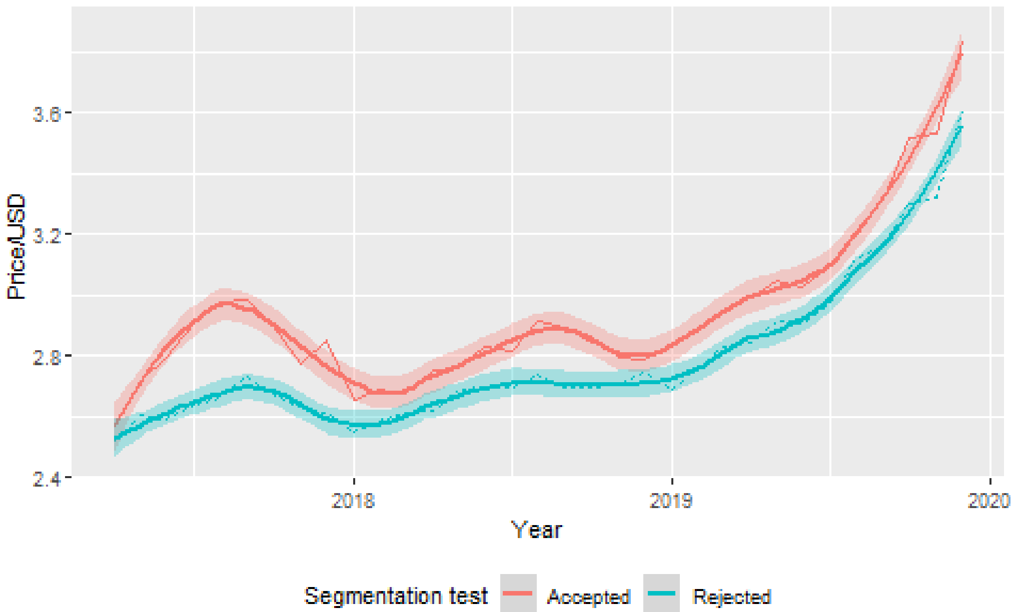
Table 11: Correlates of Mobility Reductions

	All (1) Mobility	Interacted (2) Mobility	Subnational (3) Mobility
StringencyIndex	46.47***	68.33***	44.21***
	(0.52)	(3.05)	(4.75)
SPI	0.01	0.10	0.16
	(0.05)	(0.07)	(0.09)
Armed Clashes	0.00***	0.00	0.02***
	(0.00)	(0.00)	(0.00)
Corona Cases	0.00***	-0.00***	-0.00***
	(0.00)	(0.00)	(0.00)
StringencyIndex × segmented		0.63*	0.56
		(0.27)	(0.30)
StringencyIndex × SPI		-0.13	-0.19
		(0.12)	(0.14)
StringencyIndex × Armed Clashes		0.00***	0.26***
		(0.00)	(0.05)
StringencyIndex × (log)NightTimeLights		11.35	-0.06
		(10.99)	(16.53)
StringencyIndex × (log)RoadDensity		0.15	-0.63**
		(0.18)	(0.24)
StringencyIndex × (log)PopulationDensity		0.03	0.09
		(0.24)	(0.26)
StringencyIndex × (log)Distance to Capital		-1.15***	-1.53***
		(0.11)	(0.13)
StringencyIndex × Share agricultural Land		-0.85	-1.16
		(0.53)	(0.61)
StringencyIndex × Corona Cases		0.00***	0.00***
		(0.00)	(0.00)
CA× StringencyIndex		-36.07***	
		(2.53)	
LAC× StringencyIndex		-34.76***	-9.98*
		(2.67)	(4.56)
MENA× StringencyIndex		-18.74***	13.29**
		(2.24)	(4.59)
SA× StringencyIndex		-6.15**	24.56***
		(2.31)	(4.36)
SSA × StringencyIndex		-19.54***	13.10**
		(2.29)	(4.36)
N	48651	48651	41029
Fixed Effects	yes	yes	yes

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

D

Figure 15: Average prices by segmentation



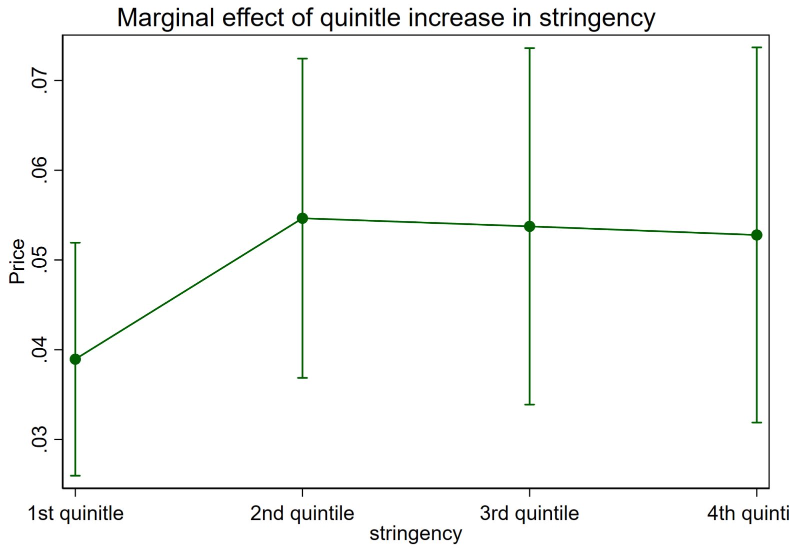
note: average price by segmentation presented with shaded LOESS smothing, optimal SSE alpha=0.3.

Table 12: Effect of COVID-19 response and price variation on segmentation

	(1) segmented
Stringency Index	-0.00
	(-0.13)
price change	0.03
	(1.30)
Controls, FE	yes
N	40925.00

t statistics in parentheses
Note:
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure 16: Non-linear effects of stringency indicator on price changes



Note: Binary indiators for each stringency quintile included as single and interacted coefficients with whole set of control variables. Quintile refer to stringency indicator values of 0.02, 0.2, 0.5, 0.68, 0.84.

Table 13: Effect of COVID-19 response stringency on prices (2SLS)

	(1)	(2)	(3)	(4)	(5)	(6)
	price change	price change	price change	price change	price change	price change
Stringency Index	0.05***	0.18***	0.18***	0.03**	0.09***	0.08***
	(0.01)	(0.03)	(0.03)	(0.01)	(0.02)	(0.02)
Stringency Index*Segmented	-0.03***	-0.03***	-0.04***	-0.03**	-0.03***	-0.03***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
N	51031	50889	50782	51031	50889	50782
N Cluster	6937	6937	6924	6937	6937	6924
F Stringencyindex	2697	1406	1092	5943	3811	3563
F Stringencyindex*Segmented	13503	13788	13434	16837	16909	16331
IV (COVID-cases)	reg-cntry	reg-cntry	reg-cntry	subreg-cntry	subreg-cntry	subreg-cntry
Fixed Effects	yes	yes	yes	yes	yes	yes
Controls	no	yes	yes	no	yes	yes
Additional Controls	no	no	yes	no	no	yes

Note: SE in parentheses. * p<0.05, ** p<0.01. *** p<0.001
Controls include distance to next market, FPPI, (log)ntl, (log)road density, (log)population, number of missings in market-commodity pair. Additional controls further include SPI, armed clashes, number of reference markets, (log)distance to capital and closest port.

Table 14: Standard error cluster specification

	Market	ADM2	ADM1	500 km	250 km	125 km
	(1)	(2)	(3)	(4)	(5)	(6)
	price change	price change	price change	price change	price change	price change
Stringency Index	0.03	0.04	0.03	0.07	0.07*	0.07**
	(0.02)	(0.02)	(0.02)	(0.04)	(0.03)	(0.03)
Stringency Index*Segmented	-0.02***	-0.02***	-0.02*	-0.02**	-0.02**	-0.02***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
N	50782	41876	50632	50830	50830	50830
Fixed Effects	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes	yes

Note: SE in parentheses. * $p<0.05$, ** $p<0.01$. *** $p<0.001$
Adm refer to administrative level 2 and 3 and km refer to cutt-off distances in spatial autoregressive models. Controls include distance to next market, FPPI, (log)ntl, (log)road density, (log)population, number of missings in market-commodity pair. Additional controls further include SPI, armed clashes, number of reference markets, (log)distance to capital and closest port.

Table 15: Heterogeneous effects by cereal import dependency, night time lights, and staple foods

	high cereal imp.	low cereal imp.	high NTL	low NTL	Staple	NoStaple
	(1)	(2)	(3)	(4)	(5)	(6)
	price change	price change	price change	price change	price change	price change
Stringency Index	0.07***	0.04	0.07***	0.00	0.01	0.03
	(0.01)	(0.05)	(0.02)	(0.02)	(0.02)	(0.02)
Stringency Index * Segmented	-0.01*	-0.02**	-0.03***	0.01	-0.03***	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
N	26513.00	24269.00	27196.00	23475.00	34185.00	16595.00
N Cluster	3758.00	3166.00	3477.00	3433.00	4603.00	2321.00
Fixed Effects	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes	yes	yes

Note: SE in parentheses. * $p<0.05$, ** $p<0.01$. *** $p<0.001$
Controls include distance to next market, FPPI, (log)ntl, (log)road density, (log)population, number of missings in market-commodity pair. Additional controls further include SPI, armed clashes, number of reference markets, (log)distance to capital and closest port.

E

Expanding on the creation of market integration indices, we can underline the nature of the four measures derived from Timmer's modification of Ravallion's approach in the context of this research. In the simplified notation of equation (1) we estimate price variations as a function of historical prices, immediate and historical variation of reference prices. In this regression we consider each coefficient of these three elements as the transmission effect of a characteristic of the market structure. Three measures, segmentation, long-run and short-run integration, are directly linked with the estimated value of the coefficients, while the fourth, Inter-Market Connectdness (IMC) is constructed from long-run and short-run integration. Our measure for the main analysis, segmentation, assesses if the price value has no significant correlation with the price of the reference market, having both concurrent and historical price transmission coefficients non significantly different from zero, or rejecting the null hypothesis that $\beta_0 = \beta_1 = 0$ (see Table 16). In order to test for long-run integration, we assume that the transmission of changes in the reference market is the primary determinant of local prices. Therefore the tested hypothesis is that the level of prices in the reference market is not significantly different from zero. This can be expressed, in the simplified notation, by testing the null hypothesis that $\alpha_1 + \beta_0 + \beta_1 = 1$. Rejecting the F-test leads to assuming that reference prices determine local prices irrespectively of the level of the latter. A more stringent condition, short-term integration, is verified whenever there is perfect transmission of reference price variations to local market, with no dependency from price levels or, in other terms, no lagged effects on prices in the future. In our simplified notation, this is tested through the null hypothesis that jointly $\beta_0 = 1, \beta_1 = \alpha_1 = 0$.

IMC, also known as Timmer's index, is the only one of these measures that doesn't depend on statistical testing but it's rather built on the relationship among coefficients. It was constructed to capture the relative magnitude of the contributions of local and reference market price history to the formation of the current local price level (Heytens, 1986). This is simply expressed as $IMC = \frac{\alpha_1}{\beta_0+\beta_1}$. The construction of the four market integration measures

Table 16: Correlation matrix, market integration measures

	(1)			
	segmented	Short-run int.	Long-run int.	IMC
segmented	1.00			
Short-run integration acceptance	0.13***	1.00		
Long-run integration acceptance	0.01*	0.23***	1.00	
IMC	0.01	0.01	-0.01	1.00
Observations	64837			

Note: SE in parentheses. * p<0.05, ** p<0.01. *** p<0.001

that we considered, was later expanded to account for the significance of additional lags and seasonal variables, chosing the regression form with the lowest AIC among equation 1 with the addition of one or two lags and seasonal dummies. The model is chosen independently for each market and commodity combination.

In order to compare the different measures we examined main statistics by country and the correlation among measures. For brevity we reported aggregated results in the table below (see Table 16). We can observe how variables are significantly incorrelated or correlation is non-significantly greater than zero. This shouldn't surprise us, as all measures are built on coefficients obtained from an OLS regression, where orthogonality is a condition imposed by construction. Yet, structural characteristics of the historical time series allow long and short-term integration to coexist, as absorption of the impulse coming from the reference market is unlikely perfect.

In particular for the first three measures of integration, it is possible to comment some interesting findings already from the summary statistics, where the average representes the prevalence of rejection of the null hypothesis of, respectively segmentation (44.4%), short-run

Table 17: Summary statistics, market integration measures

Market integration indicator	N	Mean	Variance	Min	Max
Segmentation	9.228	0,446	0,247	0	1
Long-run integration	9.228	0,230	0,177	0	1
Short-run integration	9.228	0,811	0,153	0	1
IMC	9.228	0,224	11822,100	-7150,965	1316,584

integration (78.9%) and long-term integration (23.6%) (see Table 17).

In regards to IMC, we observe a much higher variability. Most markets have a positive IMC value, which represents an higher incidence of short-term effects on price transmission compared to long-term effects. A negative value can occur only if long-term effects are more predominant in the market and there is no historical convergence of prices towards equilibrium in the observed market. An average value of the IMC of 2.92, while having no value per-se, gives an indication that prices in our sample could be mostly locally determined.

A last check we're strongly interested in, regarding these measures, is their performance in the main analysis. Results of the analysis with the four different indicators is reported below, including all control variables as per the main analysis, and using the same sample (see Table 18). We find results on the effects of interaction between the three different test-based measures are consistent in sign, even if magnitude varies, particularly when observing the coefficient of the interaction term of long-run integration, which is also non-significant. The IMC measure also fails to capture price variability per se, driven by the strong variability that this indicator is subject to.

The values of the segmentation index, as well as those of the other secondary integration measures, are still dependent on our definition of reference market. The parameters chosen for defining this aggregation are indeed arbitrary and require more discussion and testing. The parameters under examination are the maximum hours of driving distance for reaching other markets, and the maximum number of markets in the same country that we want to aggregate to obtain the reference price. Other elements to keep into account are that: the market coverage of the original database is coming from a non-probabilistic sampling and is

Table 18: Effects of COVID-19 response stringency on price variations, four market integration measures

	(1) price change	(2) price change	(3) price change	(4) price change
Stringency Index	0.03*	0.04**	0.02	0.02
	(0.01)	(0.01)	(0.01)	(0.01)
Stringency Index * Segmentation	-0.02***			
	(0.01)			
Stringency Index * Short-run integration		-0.03***		
		(0.01)		
Stringency Index * Long-run integration			-0.01	
			(0.01)	
Stringency Index * IMC				0.00
				(0.00)
N	50782.00	50782.00	50782.00	50782.00
N Cluster	6924.00	6924.00	6924.00	6924.00
Fixed Effects	yes	yes	yes	yes
Controls	yes	yes	yes	yes
Additional Controls	yes	yes	yes	yes

Note: SE in parentheses. * $p<0.05$, ** $p<0.01$. *** $p<0.001$
Controls include distance to next market, FPPI, (log)ntl, (log)road density, (log)population, number of missings in market-commodity pair. Additional controls further include SPI, armed clashes, number of reference markets, (log)distance to capital and closest port.

far from exahusting the total number of markets in a country; we need at least one market to create the reference price; and that we need to limit those cases that have a high concentration of markets that are satisfying the driving distance condition. Our preferred option was to use maximum 24 hours, considering that is an affordable travel for medium/large traders, and 10 markets, to avoid (in certain extreme cases, such as Malawi) to use information from up to 50 markets to construct the reference price. In order to test if those choices influenced our final results, we conduct the main analysis again using different parameters for the estimate of the reference price. The first new segmentation test is conducted after we reduce the driving distance to maximum 6 hours, then estimating again the regression model presented in section 3.2. The second new segmentation test is conducted after reducing the maximum number of markets included in the aggregation as reference price to 5. The share of segemented markets increases if we only consider the closests 5 markets instead of 10 (49.7%) and decreases if we limit distances to 6 instead of 24 hours driving time (35.65%). Even though limiting markets distances to 6 hours is a too narrow reference market definition in our opinion, overall we find the results to be robust to changes in the reference market definition paramters.

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