Price Controls in Liberia

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I. Introduction

The theoretical predictions of government-imposed price ceilings are a staple element of every first-year economics course. For instance, one of the authors' undergraduate microeconomics textbooks warned that "[a]lthough adoption of such policies may be based on noble motives, the controls deter long-run supply responses and create welfare losses for both consumers and producers" (Nicholson 1997, 488). Beyond the supply constraints, nonbinding price ceilings may reduce welfare by helping producers set a collusive focal point at a price above the competitive equilibrium (Haucap and Müller 2012). Moreover, in the presence of weak institutions, controls may be set as a means for bureaucrats and price inspectors to extract rents from businesses (Shleifer and Vishny 2002). Even with such warnings, price controls are still used today, and the question of whether and when to use them remains a contentious issue. Policy makers point to price controls as a tool for restricting monopoly pricing and protecting low-income groups in uncompetitive markets (Swamy 1994). In spite of this universally taught model, there is very little empirical evidence of the effects of applying price ceilings in the real world to support either side of the debate.

This paper provides a rare example of empirical evidence into the efficacy of price controls. Liberia's extensive price controls regime and their stagewise removal in 2009 allow us to observe the effectiveness of price controls at sup-

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pressing prices and quantities against a plausible counterfactual of goods that were never subject to controls. As far as we are aware, this is the first time that such an extensive price control regime has been evaluated. It is also the first time that the effectiveness of price controls can be tested across different types of market and different allowable maximum markups.

Micro-level empirical examinations of price controls are limited to a small number of articles relying primarily on data from developed countries. Knittel and Stango (2003) consider state-level price ceilings in credit card markets in the United States and find that nonbinding state-level price ceilings serve as a focal point for tacit collusion among firms. This leads to higher prices than under a competitive equilibrium. A similar conclusion is suggested by Sen, Clemente, and Jonker (2011), who exploit a natural experiment of differing legislation across eight cities in eastern Canada to test the effects of a retail gasoline price ceiling. The authors find that the enactment of such legislation is correlated with higher prices. By contrast, Blinder and Newton (1981) found that wage price controls enacted in the United States in 1971 led to a reduction in the nonfood, nonenergy price level by 3%–4% and that the removal corresponded with a period of double-digit catch-up inflation. Kyle (2007) finds in pharmaceutical markets that companies avoid price-controlled markets and are less likely to introduce products in additional markets after entering a price-controlled country.

Other research has focused on how the level of market competition influences the effectiveness of price controls. Sheahan (1961) observed that far-reaching price controls enacted by the French government following World War II were targeted toward highly concentrated industries. Sheahan finds that during the recession of 1952–7, "controls helped move prices more in the manner they would have if they were set under competitive condition" (351). Sheahan concludes that arbitrarily set prices applied on competitive industries "distort resource allocation and lessen efficiency," whereas for uncompetitive industries, set prices "may well have held prices and wages in a pattern more nearly consistent with efficient resource allocation than would have resulted in the absence of controls" (355). A similar conclusion is derived by Helpman (1987), who writes a macroeconomic model to explain the unexpected results of Israel's price control stabilization program of 1985. Helpman shows that in a competitive market, price controls lead to shortages and lower output. However, under imperfect competition, moderate price controls do not lead to shortages, and output—which is demand determined—increases.

Today, price ceilings are surprisingly common in developing countries; price control regimes exist in Venezuela, Kenya, Vanuatu, Zimbabwe, the Philippines, Thailand, and Bangladesh (Thuraisingham 2010). The prevalence of price ceil-

ings is particularly high among countries with weak institutions and is seen to increase around the time of food price spikes. For example, during the 2006–8 food price crisis, Sri Lanka and Malaysia announced a retail and wholesale maximum price for all varieties of rice. Simultaneously, Senegal announced price controls on assorted grains, while Malawi announced that maize prices would be fixed and sold through the state marketing corporation. Other countries (including Mexico, Jordan, and Burkina Faso) negotiated with importers, retailers, and wholesalers and announced agreed indicative prices for key staples. In reviewing these policies, Prakash (2011, 179) concludes that "such measures, while popular with the public, were likely to reduce private storage or marketing activities and reduce incentives for producers."

Although price controls are commonplace, it is surprisingly difficult to come by details of how they are calculated in different countries. Where we have been able to observe details, the most common method appears to be regulating markups on firms profits, as was the case in Liberia.[1] If calculated correctly, this would appear to be the least likely to cause a collapse in the market.

In countries with weak institutions, price ceilings can be seen as a double-edged sword. On the one hand, critics of price ceilings point to the increased opportunities for corruption and rent seeking. For example, in Venezuela two high-ranking government officials were arrested in 2013 for extorting bribes from businesses in exchange for letting them sell goods at prices above government ceilings (Ellsworth 2014). During World War II, evasion of price controls and rationing instituted in the United States and the United Kingdom were said to be widespread. Black market dealers channelled goods into black market retail stores, and operators sold lower-quality goods to increase profits for the same price (Wallace 1951; Mills and Rockoff 1987). In Egypt and India, Deacon and Sonstelie (1989) observe that the actions undertaken by buyers as a consequence of shortages induced by price controls result in rent dissipation. Tarr (1994) estimated the welfare loss from price controls on Polish color televisions in 1989 to be about 10 times the standard estimates of distortion costs and more than 100% of the total value of domestic producers' sales. On the other hand, countries with weak institutions have limited capacity for antitrust enforcement, and controlling prices may be easier than the policing of anticompetitive behavior, representing a plausible second-best policy.

In Liberia, price ceilings were in place on 333 categories of goods sold on the Liberian market between 1988 and 2009. These categories covered a wide

[1] For instance, markups in Greece on fruits and vegetables, China on gas production and transportation, the European Union on pharmacists' fees, and Venezuela in the Fair Prices Act (Genakos, Koutroumpis, and Pagliero 2014; El Universal 2015).

range of goods across staple, industrial, and luxury items. Maximum prices were calculated by the Ministry of Commerce and Industry and agreed with the Liberian Chamber of Commerce on the basis of import prices, estimated costs, and an allowable percentage markup. Enforcement was conducted by Ministry inspectors. In April 2009, this policy was removed in favor of a period of semiliberal monitoring of a smaller list of essential commodities. In February 2010, the regulation was further liberalized, with the Ministry moving to just analyzing and observing but not in any way regulating prices.

The data allow us to conduct several counterfactual exercises. First, we test whether and by how much goods prices increased following the removal of price controls. Second, we test whether there was a supply response to the removal of controls, as proxied for by Liberian import data. Finally, we consider whether the effects varied across different market structures, the size of the maximum allowable markup, different types of good, and rural and urban consumption baskets. In each case, we examine goods subject to price controls against a counterfactual of goods that were never subject to price controls. We then conduct robustness tests against the same goods modeled in the US consumer price index (CPI) survey and conduct a placebo experiment.

We find that across all goods and all markets, price controls suppressed prices and increased the volume supplied of goods in the treatment group compared with the control group. This first finding goes against the hypothesis that all price controls end up in shortages as well as the hypothesis that price controls in a country like Liberia would simply enable corruption without affecting market outcomes. The price effect that we observe was attenuated by the level of allowable profit markup, indicating that the ceilings were nonbinding for higher levels of markup. This finding goes against the hypothesis that price controls were used as a collusive focal point to enable higher prices. We do not find a statistically significant difference between the suppression in prices in retail and local markets, although we find that the consumption basket of rural consumers was more influenced by the reduction in price controls than that of urban consumers. Altogether, these results fit with a model of monopoly pricing among firms in Liberia that can be counteracted with at least some effectiveness by government. Our results also provide a positive insight into how, despite weak institutions and incentives for corruption and collusion, there is evidence of state functionality and nonvenal business government relations.

The remainder of the paper is organized as follows. Section II presents a brief overview of price controls adopted in Liberia. Section III presents a theoretical framework. Section IV discusses the data and empirical specifications. Section V presents results. Section VI discusses conclusions and policy implications.

II. Liberia and Price Controls

Liberia is a small country of four million people situated on the west coast of Africa. Its economy is exceptionally open and dependent on international trade and investment. As of 2014, Liberia had an import bill of more than 65% of gross domestic product, which includes items that are essential to the stability of the country, such as rice, cement, and petroleum (IMF 2014). This is financed through two means: the export of primary commodities (iron ore, rubber, and logs), which are predominantly owned and operated by foreign businesses; and extensive foreign aid, including a UN peacekeeping mission.

Private consumption in Liberia functions as a dual economy. The capital city, Monrovia, houses four main outdoor markets, each servicing predominately poor Liberians with cheap goods at low profit margins. The other group of consumers are predominately wealthy Liberians, foreign aid workers, and foreign business owners who shop in retail stores and supermarkets owned by a small group of interconnected businessmen. These retail stores provide food and beverages, consumer goods, electronics, and other items used by consumers and businesses. Anecdotal evidence suggests that local markets are highly competitive, while the small number of retail stores, supermarkets, and importers may be colluding in order to increase profits.[2] Although no study has previously been able to directly observe this in Liberia, it is well established that microenterprises in sub-Saharan African markets may represent something approaching perfect competition (Fafchamps 1994). This is no less true in Liberia, where these markets are characterized by many buyers and sellers, freedom of entry and exit, homogeneous products, mobile factors of production, and close to perfect knowledge among consumers.

In 1988, in an effort to limit the maximum price of goods, the Ministry of Commerce and Industry began imposing price ceilings on 333 categories of goods sold in the Liberian market. The price ceilings were imposed on specific commodities, covering everything from staple goods (e.g., milk, rice, fruit, and vegetables) to industrial goods (e.g., cement, building materials, machines, and transportation equipment) to luxury goods (e.g., artificial flowers and musical instruments). Government price analysts calculated maximum prices using an algorithm including import prices, estimated taxes and costs, and an allowable percentage markup that varied according to the product. Markups were agreed upon by the Liberian Chamber of Commerce. Enforcement depended on businesses self-enforcement through the Chamber of Commerce and the policing capacity of Ministry inspectors. Inspections were said to take place more frequently and were enforced more vigorously in foreign-owned supermarkets

[2] Authors' discussions with Liberian government officials, 2009–12.

and retail stores compared with urban street markets, although in some instances inspectors may have taken bribes in exchange for not enforcing ceilings.[3]

In April 2009, the Ministry of Commerce stated that in order to move into line with "international best practices," "encourage imports into the commerce of Liberia that would create market competition between sellers," and "contribute to the reduction in prices to consumers," the government should liberalize the market.[4] In April 2009, price controls were removed on all goods, with the exception of petroleum products.[5] In place of this policy, a smaller list of essential commodities was monitored by the Ministry's Division of Price Analysis. The division was in charge of issuing recommendations if prices grew beyond a reasonable level.[6] In practice, this period is best thought of as an adjustment period, moving away from strict controls to semiliberal monitoring with particular focus on category A political commodities.

In February 2010, the regulation was further liberalized, with the Ministry moving to just analyzing and observing but not in any way regulating prices. If prices breached the ceiling calculated from import invoices, the Ministry would observe, discuss with retailers, and broker a price reduction. Even collusive pricing was tolerated if sufficient supply was available in the market.

Despite recent improvements, following a long civil war and a history of kleptocratic rule, the Liberian state remains both low in capacity and prone to corruption and elite capture (Werker and Pritchett 2017). Civil servants are often poorly paid, and government jobs are frequently seen as tools for personal enrichment (Chessen and Krech 2006). This makes Liberia an ideal location to study the effects of price controls in a weak institutional environment.

III. Theoretical Framework

Standard firm theory predicts several alternative hypotheses for how the market will respond to the removal of a binding price ceiling, depending on the level of competition and degree of corruption. Given that our data come from imports, which we assume to be perfectly elastic, we model supply with con-

[3] Authors' discussions with Liberian government officials, 2009–12.

[4] This is stated in the introduction to the revised markup document issued by the Ministry of Commerce in April 2009 and from authors' discussions with Liberian government officials, 2009–12.

[5] The Ministry continues to issue maximum prices for petroleum products in collaboration with the Liberia Petroleum Refining Corporation; a recent example can be found at http://www.lprclib.com/others.php?&7d5f44532cbfc489b8db9e12e44eb820=MzE0.

[6] This appears to be largely up to the discretion of those doing the analysis.

stant marginal cost.[7] This has the immediate consequence of ensuring that price ceilings would have no impact under competitive markets with a fixed cost of operating.[8] Therefore, we instead focus on outcomes under imperfect competition. For the purposes of this paper, we present a graphical analysis, although a full mathematical treatment can be found in most graduate microeconomic textbooks.[9]

A. Monopoly and Cournot Competition

In a monopoly market, the sole producer's profit maximization decision is to set the quantity (y^M) supplied such that marginal revenue (MR) equals marginal cost (MC), as shown in figure 1. The consequence on prices and quantities supplied of the removal of a price ceiling will depend on where the price ceiling ($\bar{p}$) was set with respect to the profit maximization price (p^M) and the price at the intersection of the demand and marginal cost curves (p^*).

Under Cournot competition, firms consider their optimum level of output by considering the best response to other firms' output choices. Like the case of monopoly, Cournot competition predicts that the removal of a price ceiling will take effect only if the ceiling was set at a level below the Cournot equilibrium price but above the intersection of the demand and marginal cost curves.

Scenario 1. $\bar{p} \geq p^M$; no change in equilibrium price or quantity.

Scenario 2. $p^* < \bar{p} < p^M$; prices increase and quantity decreases as outcomes move to the monopolistic equilibrium.

Scenario 3. $\bar{p} < p^*$; if enforced, then the market will collapse under the price ceiling $y^M \to 0$. Therefore, its removal would lead to new entry.

[7] We assume marginal costs are constant because Liberia is a small open economy making import prices exogenous to the economy. Additionally, almost all shipments are for a variety of products, such that importers trade off imports of one product versus another. Thus, storage and transportation marginal costs might not be constant across an importer, but from the perspective of any single product they might realistically be treated as such.

[8] This is for the obvious reason that a binding price ceiling could not be sustained because firms would be operating at a loss.

[9] An alternative framework might embed the threat of exit via a one-time deviation from a price control against the discounted sum of remaining in the market. We decided against following this approach for the following reason. In our view, the stakes in the price control regime, at least during the period of the data, were not high enough to generate a threat of exit. Rather, the threat that the inspectors probably had was of extracting some bribes and causing trouble, which in enough cases might have been easier to comply to some extent with lower prices. Moreover, given that our data on the quantity side are limited to importers—which are a proxy for retail traders—it would be difficult to empirically test the results of this more nuanced model.

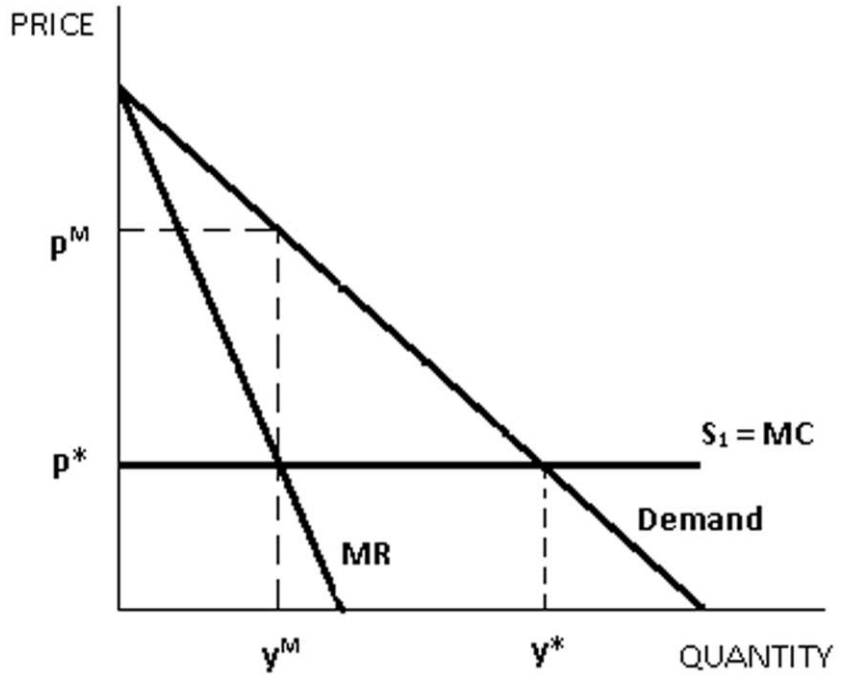

Figure 1. Monopoly.

B. *Collusion*

An alternative possibility discussed in the literature is that firms may use the government price ceiling as a focal point in order to solve the coordination problem of how to set a collusive price above the competitive equilibrium price (p^*; Haucap and Müller 2012). There are three possible scenarios for the consequences of removing a ceiling, depending on whether the ceiling was binding.

Scenario 1. $\bar{p} > p^*$; prices decrease and quantity increases as firms can no longer utilize the ceiling to find a tacit focal point.
Scenario 2. $\bar{p} = p^*$; no change in equilibrium price or quantity.
Scenario 3. $\bar{p} \leq p^*$; if enforced, then the market will collapse under a price ceiling. Removal of the ceiling should lead to firm entry.

C. *Corruption*

Under conditions of weak institutions, it is likely that firms may pay a per-unit bribe (B) in order to avoid having to meet the price ceiling. If the price ceiling is binding, the bribe is equivalent to a vertical shift in the supply curve. We as-

sume under this scenario that firms are monopolistic; under a competitive outcome, firms would face no pressure to pay the bribe. The consequences of a removal of price ceilings is displayed in the following scenarios (fig. 2):

Scenario 1. If $\bar{p} < p_1$ and $\bar{p}\bar{y} < p_2 y_2 - (\mathrm{MC} + B)y_2$, then under a price ceiling it will be economical for the firm to pay a per-unit bribe to inspectors in order to avoid adhering to the price ceiling. Therefore, the removal of this option would shift the marginal cost curve down ($S_2 \rightarrow S_1$) and lead to a lower price and higher quantity supplied.

Scenario 2. If $\bar{p} \geq p_1$, the price ceiling was not binding, and firms will set prices according to market forces. Therefore, the removal of the ceiling should have no effect.

Scenario 3. If $\bar{p} < p_1$ and $\bar{p}\bar{y} < p_2 y_2 - (\mathrm{MC} + B)y_2$, the bribe is not economical to pay, so the firm will set prices according to one of the previous models of competition.

The theoretical predictions of removing a binding (nonbinding) price ceiling on prices (dp/dt) and quantities (dq/dt) are summarized in table 1. Under competitive markets, we expect price ceilings to have no effect on prices or quantities. However, if we assume firms have market power to restrict quantity

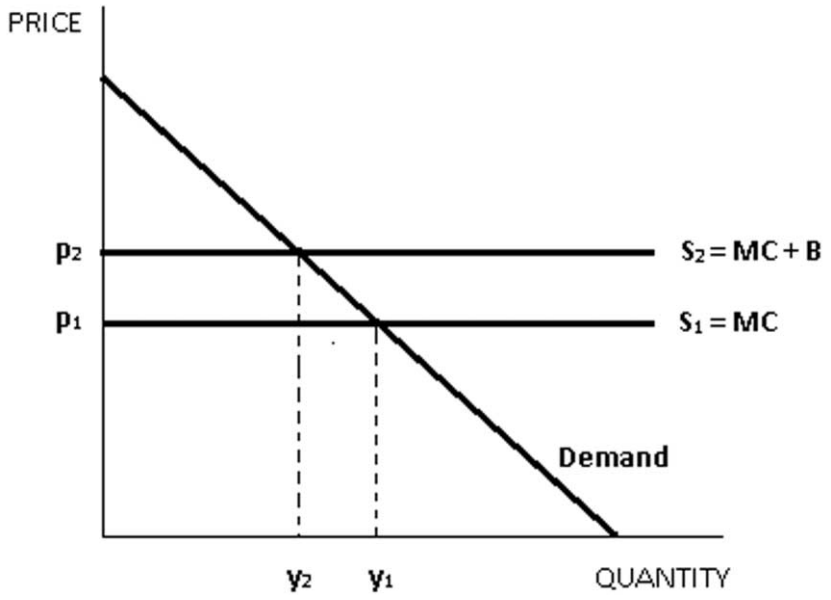

Figure 2. Corruption.

TABLE 1
SUMMARY OF THEORETICAL PREDICTIONS OF REMOVAL OF A BINDING (NONBINDING) PRICE CONTROL

	Competitive Market	Cournot Competition	Monopoly	Collusion	Corruption
dp/dt	0 (0)	>0 (0)	>0 (0)	Variable (<0)[a]	<0 (0)
dy/dt	$0 \rightarrow y$ (0)	Ambiguous (0)	Ambiguous (0)	Variable (>0)[a]	>0 (0)

[a] In the case that the ceiling is nonbinding, the predictions will depend on the level of market competition.

supplied, a binding price ceiling should result in lower prices and higher output. By contrast, under corruption and collusion, a price ceiling may increase prices and lower output. The removal of these ceilings should have the opposite effect.

IV. Data and Empirical Specifications

A. Data

In order to test the effects of price controls in Liberia, we construct two panel data sets. For Liberian price data, we use the repeated panel of price quotes collected by the Liberian Institute for Statistics and Geo-Informational Services and the Central Bank of Liberia, primarily collected for the Liberian harmonized consumer price index (HCPI). The price data set contains a monthly collection of prices spanning a period from January 2008 to February 2011 (38 months). We also use an earlier sample from January 2006 to February 2009 for the purpose of a placebo experiment. Price quotes on 236 goods are sampled twice per month from four street markets, retail stores, and supermarkets in Monrovia. The same good is measured either in the street market or a formal retail outlet but never both. Prices are averaged across the four street markets and the two time periods to arrive at a monthly average price. In addition to the price quote, for each observation enumerators record product information coded with a unique classification of individual consumption by purpose (COICOP) identifier, weight, good origin, and whether the price is recorded in a local or retail store.[10] Following Bertrand, Duflo, and Mullainathan (2004), we collapse the data to look at annual changes so as to avoid issues of serial correlation over time and to correct within product-year correlation across observations.

We then match whether the goods in the HCPI survey appear on the Ministry of Commerce and Industry's price control schedule. For each good subject to a price ceiling, the schedule records the maximum allowable markup at the wholesale and retail level. These markups range from a minimum of 8% to a maximum of 50%. We assume that goods subject to lower markups are more likely to have a binding price ceiling. For the purposes of our analysis, we multiplied the wholesale and retail markups to set a total markup and

[10] COICOP is the standard classification system used in CPI surveys.

split the results into three categories to allow for a nonparametric approach. In total, 131 goods in the CPI price survey were subject to price ceilings compared with 105 that were never controlled. In tables 1–4 in the appendix (appendix is available online), we include a table of *t*-tests for pretreatment characteristics of goods with and without price controls. As expected, given price controls were negotiated between the Ministry of Commerce and the Liberian Chamber of Commerce, price controls were more likely to be imposed on certain types of goods. We find that goods sold in retail stores, goods that are deemed essential by the Liberian Ministry of Commerce, and industrial goods are more likely to be subject to price controls. This may be a concern for our identification strategy if these goods fail the common trends assumption. We address this by running a placebo experiment and by using data from the US CPI survey as an alternative counterfactual.

One potential shortcoming with this data is that suppliers might report prices below the price ceiling to avoid enforcement from inspectors. In this case, we do not believe this to be a major concern because data were collected by an institution different from the inspectors and reports are held anonymously and confidentially. During this period, enumerators were instructed by the Central Bank to inform respondents that no data would be shared with inspectors. These facts were confirmed by a qualitative survey undertaken by the authors with respondents at different retail outlets surveyed in the data set.

The Liberian statistics bureau has made several edits to the raw data that we have kept for the purposes of our analysis. First, all price quotes are changed into prices per unit in order to avoid problems related to different packaging or weights of goods. Second, in order to avoid censored price spells, missing prices are replaced with information from the previous month. Third, in instances of unusually large monthly changes in prices, recorded prices are replaced with those of the previous month. If the higher price continues to the next month, the price is changed to the new higher price from that point onward. This is conducted by the statistics bureau on the basis of local knowledge of prices and underlying cost factors.[11]

In order to provide a second international counterfactual, we match the Liberian price quotes with those collected during the same period by the US Bureau of Labor Statistics as part of the US CPI survey. Inevitably, there are some differences in the size and scope of the baskets of goods collected in Liberia and the United States, making direct matching difficult.[12] We were able

[11] These cleaning methodologies do not make a difference in results, as shown in table 2 in the appendix.

[12] The US basket of goods is much smaller than Liberia's, and many goods are not measured across both countries.

to match 24 goods, of which 15 were subject to Liberian price controls and nine were not.

Our second data set is used to consider the impact of price controls on the quantity supplied. In Liberia, imports represent an excellent proxy for total quantity supplied, given the economy's heavy dependence on imported goods. One potential concern with this approach would be that domestic goods serve as substitutes for imported goods and thus the removal of price controls might influence this substitution. We do not consider this to be a major issue in the period of our analysis for two main reasons. First, Liberia's lack of private sector development means that there are very few import-competing sectors. Instead, the majority of imports are for manufactured goods that would be prohibitively expensive to produce locally. Second, where import-competing sectors exist, it tends to be in agricultural goods. However, given the unanticipated nature of the price control removal and lag before smallholder farmers are likely to be able to identify price movements and switch crops produced, we do not anticipate a major substitution between imported and domestic agricultural goods during the period of analysis. Indeed, in many discussions with Ministry officials there was never any mention of domestic import-competing sector growth as a response to the removal of price controls. In fact, discussion on the lack of import-competing sectors and lack of market competition was a far more common topic of conversation.[13] This is further highlighted by the lack of change in imports as a percentage of gross domestic product over the past five years (IMF 2014). However, as a precaution, we drop all Harmonized System (HS4) lines that are included in the Ministry of Commerce and Industry's industrial survey (2013).[14] This is the only comprehensive survey of manufactured and industrial goods produced in Liberia.[15]

In order to mitigate the effects of large outliers, we remove all product categories with zero imports in any one of the three time periods (such as capital equipment for mining). We also remove observations that experience import swings of more than 10 times or less than one-tenth of the previous period's results, since these movements are almost certainly unrelated to price controls.

[13] When the current Minister of Commerce and Industry (who was not in charge when controls were removed) was presented with this work, he remarked that he was fully aware that firms were colluding to fix prices in the Liberian market and was unsurprised that the removal of ceilings appears to have increased this behavior.

[14] The Harmonized System of tariff nomenclature is the standard international system for classifying traded goods. The more digits in the code, the higher the level of disaggregation; e.g., HS4 code 22.03 is beer made from malt, whereas HS8 code 22.03.00.90 specifies that the beer made from malt is either stout or porter.

[15] A full list of the products excluded is given in the appendix.

TABLE 2
DESCRIPTIVE STATISTICS

Variable	Mean	SD	*N*
Price:			
ln price	5.031	2.016	702
Had a price ceiling	.560	.497	702
Sold in a retail store	.741	.438	702
Essential commodity	.085	.28	702
Maximum markup:			
8%–20%	.201	.401	702
25%–30%	.205	.404	702
35%–50%	.111	.314	702
Quantity:			
ln import quantity by HS4 code	10.246	2.964	2,397
Had a price ceiling	.542	.498	2,397
Sold in a retail store	.690	.463	2,397
Essential commodity	.153	.361	2,397
Maximum markup:			
8%–20%	.188	.391	2,397
25%–30%	.172	.377	2,397
35%–50%	.182	.386	2,397

Note. HS = Harmonized System.

Finally, we Winsorize at 5%. In all quantity regressions, we run these cleaning methods alongside robustness checks using alternative cutoffs. This leaves a total of 799 import categories, of which 402 were subject to price ceilings and 373 were not. Table 2 describes both the price and quantity data that we use in the paper.

In all regressions, we aggregate results over time periods aligning with the policy changes. Period 1 covers January 2008–April 2009, the period when controls were in place. Period 2 covers May 2009–March 2010, the period of semiliberal monitoring of just essential commodities. Period 3 spans April 2010–February 2011, when price controls were further liberalized. We then take the natural log of the price and the volume.[16] In figures 3–6, we collapse to quarterly changes and index the price and volume with the initial observation set as the base period and given a value of 100.

We linked the two data sets in order to observe whether the effects of the price ceilings were the same across retail and street markets. In practice, this was difficult because of the lack of clearly identifiable categories across both the price and the quantity data sets, leading to fewer observations. In the linked data set, we created a dummy variable ("retail") coded 1 if the good were recorded to be sold in a retail store/supermarket in the CPI survey or 0 if it were sold in a local street market. We use this as a proxy for the level of market power

[16] As a robustness check, instead of using log values we use indexes; results are not qualitatively different and are available from the authors on request.

for both the price and quantity regressions. We also create a dummy variable ("essential") coded 1 if the good is an essential commodity and therefore received closer attention during period 2.

Finally, we were able to obtain details on the consumption basket of rural and urban consumers, which was calculated from household data by de Melo and Mancellari (2014). A full breakdown is shown in table 3. Given that rice is such an important expenditure item to the consumption basket, we also calculate a consumption basket net of rice. We give all line items not included in the household survey an arbitrarily small value of 0.0005.

B. Empirical Specifications

To test the effects of the price controls on market prices and import volumes, we would ideally randomly assign the stepped removal of price controls and observe the difference in the change in average retail prices and average import volume between the groups. Random removal of controls would ensure that

TABLE 3
BUDGET SHARE BY EXPENDITURE CATEGORY

	With Rice		Without Rice	
Category	Rural	Urban	Rural	Urban
Rice	.281	.137	0	0
Fish	.099	.115	.137	.133
Other vegetables	.074	.053	.103	.061
Meat (including pork)	.061	.038	.084	.044
Personal hygiene	.057	.109	.08	.126
Fruit	.057	.038	.08	.044
Garments and food	.055	.046	.077	.053
Cassava roots	.053	.023	.073	.027
Palm oil	.052	.047	.072	.054
Fuel	.034	.185	.048	.214
Smoked fish	.032	.016	.044	.018
Alcohol and tobacco	.032	.026	.044	.03
Chicken	.029	.039	.04	.045
Onions, potatoes	.024	.024	.033	.028
Soft drinks	.01	.025	.014	.029
Cassava flour	.009	.008	.013	.009
Flour	.007	.013	.01	.015
Condiments	.007	.006	.009	.007
Eggs	.006	.008	.009	.009
Dairy	.006	.018	.009	.021
Home equipment	.005	.015	.006	.017
School material	.004	.006	.006	.007
Live animals	.004	.002	.005	.002
Entertainment	.002	.006	.003	.007

Note. Budget shares calculated from household data set by de Melo and Mancellari (2014). Budget share calculations done with and without rice because of its importance. All other items not included in table are given an arbitrarily small value of 0.0005.

all endogenous factors determining retail prices and volumes are orthogonal to the variables of interest. Given that randomization is not possible, we instead use the list of goods that have never had price controls (the control group) as counterfactuals for those that were previously controlled (the treatment group). This allows for pre-post analysis of the effect of price controls across goods that were—and those that were never—subject to price controls. We conduct the same analysis across prices and quantities. While in our case selection to having a price ceiling was nonrandom, this is unlikely to create a substantial omitted variable bias once we control for product-specific fixed effects. This is achieved by using the standard difference-in-difference design:

$$\boldsymbol{z_{it}} = \beta_0 + \beta_1 \text{ceiling}_{it} + \boldsymbol{B_i} + \boldsymbol{B_t} + u_{it}, \tag{1}$$

where $\boldsymbol{z}$ is a vector of the outcome of interest (prices and quantities) for good i in time t, ceiling_{it} is a dummy for whether good i is subject to a ceiling in time t, and $\boldsymbol{B}$ is a vector of product and period fixed effects.

In this and all subsequent regressions, we use product period as the unit of observation. We do this to avoid serial correlation in standard errors over multiple periods, a common concern in difference-in-difference designs (Bertrand, Duflo, and Mullainathan 2004). We also present results graphically at the product quarter level because we think this yields a clearer depiction of the results of the changes over time and demonstrates that there is no Ashenfelter dip. In figures 3–6, we have indexed prices and quantities and include fixed effects because this is more visually interpretable. Regressions use the log value of the dependent variable.

Our second specification disaggregates the effects of the price controls into individual maximum markups to test whether more binding constraints effect prices. This is achieved with the following specification:

$$\boldsymbol{z_{it}} = \beta_0 + \beta_1 \mathbf{markup}_{it} + \boldsymbol{B_i} + \boldsymbol{B_t} + u_{it}, \tag{2}$$

where $\mathbf{markup}_{it}$ is a vector of three dummy variables for whether good i is subject to a given maximum permitted markup at time t.

Our third specification considers the effects of competition on the effectiveness of price controls. We include an interaction between a dummy variable for whether the good is sold in a retail market:

$$\boldsymbol{z_{it}} = \beta_0 + \beta_1 \text{ceiling}_{it} + \beta_2 \text{retail}_{it} + \beta_3 \text{retail}_{it} \times \text{ceiling}_{it} + \boldsymbol{B_i} + \boldsymbol{B_t} + u_{it}, \tag{3}$$

where retail is a dummy variable for whether good i is sold in a retail market at time t and subject to price ceiling c.

Our fourth specification identifies whether essential commodities that were only fully liberalized in period 3 had a delayed effect from the removal of price controls. Unfortunately, there is not an adequate counterfactual of essential commodities that were never subject to price controls. Instead, we run specification 4 only on the set of essential commodities and those goods that were never subject to price controls:

$$\boldsymbol{z_{it}} = \beta_0 + \beta_1 \text{essential}_{it} + \boldsymbol{B_i} + \boldsymbol{B_t} + u_{it}. \tag{4}$$

Our fifth and sixth specifications attempt to identify whether there is a differential effect of the removal of controls on different goods and different types of consumers. In specification 5, we categorize goods into three categories: staple, luxury, and industrial.[17] We then run separate regressions for each good type. In specification 6, we use inverse probability weights to weight our sample by the importance to the consumption basket for rural and urban consumers, as shown in table 3.

Our last three specifications attempt to address potential alternative hypotheses. In specification 7, we further address the issue of endogeneity of good selection by including the price of like goods as measured by the US Bureau of Labor Statistics as a robustness test. We use the US results first as a control variable, effectively removing any variation that might come from any differences in international cost factors. We then use the US goods as a direct counterfactual comparing the change in the treatment group of Liberian goods subject to price ceilings with the control group of the same goods priced in the United States.

In specification 8, we make one final assurance that the selection of goods subject to price controls does not lead to a failure of the common trends assumption in our results; we run a placebo difference-in-difference experiment. This involves running a fake evaluation of a policy change before the policy change took place, that is, running the same regressions but on data that is 3 years older.[18] If our treatment and control groups are on different trends, we would expect the placebo experiment to reveal a difference in means between the two groups.

[17] Staple goods include basic food items, clothing, and cheap hygiene products. Luxury goods include entertainment items, expensive food items, and expensive hygiene products. Industrial goods include building products, services used by firms, and machinery.

[18] This experiment works only if there is no change in policy over the period of the placebo experiment. We understand through discussions with Ministry staff that no policy change took place during this time.

Finally, in specification 9, we rerun specifications 1 and 2 using the import price as the dependent variable.[19] This is to address the potential concern that our results are being driven by coincidental external factors driving import prices but not actually changing firm markups. If, however, it is markups that are changing as a result of the change in government price control regulation, then we would expect no change in import prices.

V. Results

A. Price

We find evidence that price controls were effective in their primary goal of suppressing prices. Figure 3 compares the average monthly price index of goods that were originally subject to price controls (treatment) with those that never faced the ceilings (control). The first vertical line (in quarter 2 of 2009) shows the first policy change of the removal of price ceilings. The second vertical line (in quarter 1 of 2010) shows the second policy change of a move to further liberalization. During the first period, rates of growth of prices in the treatment and control group followed an almost identical trend. However, after the first policy change, while the control group continues at a similar rate of quarterly inflation of 1.1%, the treatment group average price index increases three times as quickly at 3.3% per quarter.

Figure 4 suggests that this aggregate effect is more heavily driven by sales in the less competitive retail stores than goods sold on the local market. In figure 5, we restrict the sample of goods to prices collected just in retail stores. The treatment and control group prices sharply diverge after the removal of the controls. In figure 6, we limit the sample to goods sold in street markets. The prices of goods in the treatment group increase faster than the control group in street markets. However, the effect is less clear than in the retail store case, given that the price of goods sold in street markets rises before treatment and then levels.

This evidence provides an initial indication that across all goods and all markets, price controls were effective in suppressing prices below the free market outcome. Moreover, this effect appears stronger in retail stores than in street markets. This fits with the prediction that price controls are more effective at constraining established businesses and those businesses that may have some degree of market power. It also suggests that any corruption among inspectors did not completely negate the effects of the price controls.

[19] Import prices are calculated from trade data as the cost, insurance, and freight value over the import quantity for each HS4 digit item.

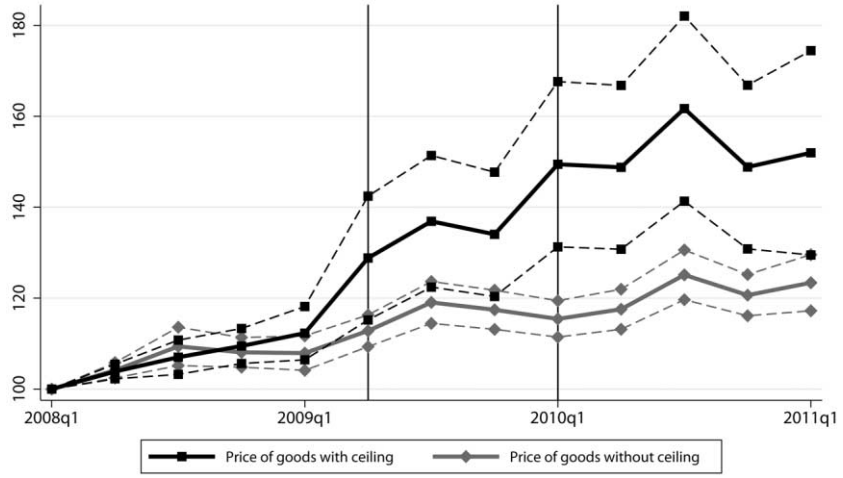

Figure 3. Treatment and control average price indexes for all goods, 2008–11. Shown is the change in the indexed price of goods in treatment and control groups over time. Dashed lines show 95% confidence intervals. Image is for the purpose of visualizing trends; however, we encourage readers to use the main regression specification to interpret significance.

The results of the fixed effects difference-in-difference estimates in column 1 of table 4 support these conclusions. One year after the removal of the ceiling, prices in the treatment group were on average 10% higher than the counterfactual of no controls, significant at the 1% level. During the third period, this difference rose to almost 13%, significant at the 1% level.

We find that this effect is attenuated with the maximum allowable markup, such that more restrictive markups restrict prices by a greater margin, but less restrictive markups do not restrict prices at all. Column 2 of table 4 breaks the price ceiling maximum markup into three dummy variables based on multiplying the maximum allowable retail and wholesale markups. In both time periods after the removal of price ceilings, the most restrictive markup category has the highest average price increase, followed by the second-most restrictive. However, in the least restrictive category, prices of the treatment group are not significantly different from those of the control group. Across all degrees of permitted markup, we do not find any evidence of a decrease in prices following the removal of price ceilings. Contrary to previous empirical studies, this suggests that even when price ceilings were nonbinding, there was no collusive focal point that, when removed, ended collusion.

Table 5 shows the differential effect of removing price controls for essential commodities and goods sold in retail stores. We can see from column 2 of table 5 that, relative to a control group of goods never subject to price controls, essential commodity prices did not significantly rise during the period of semi-

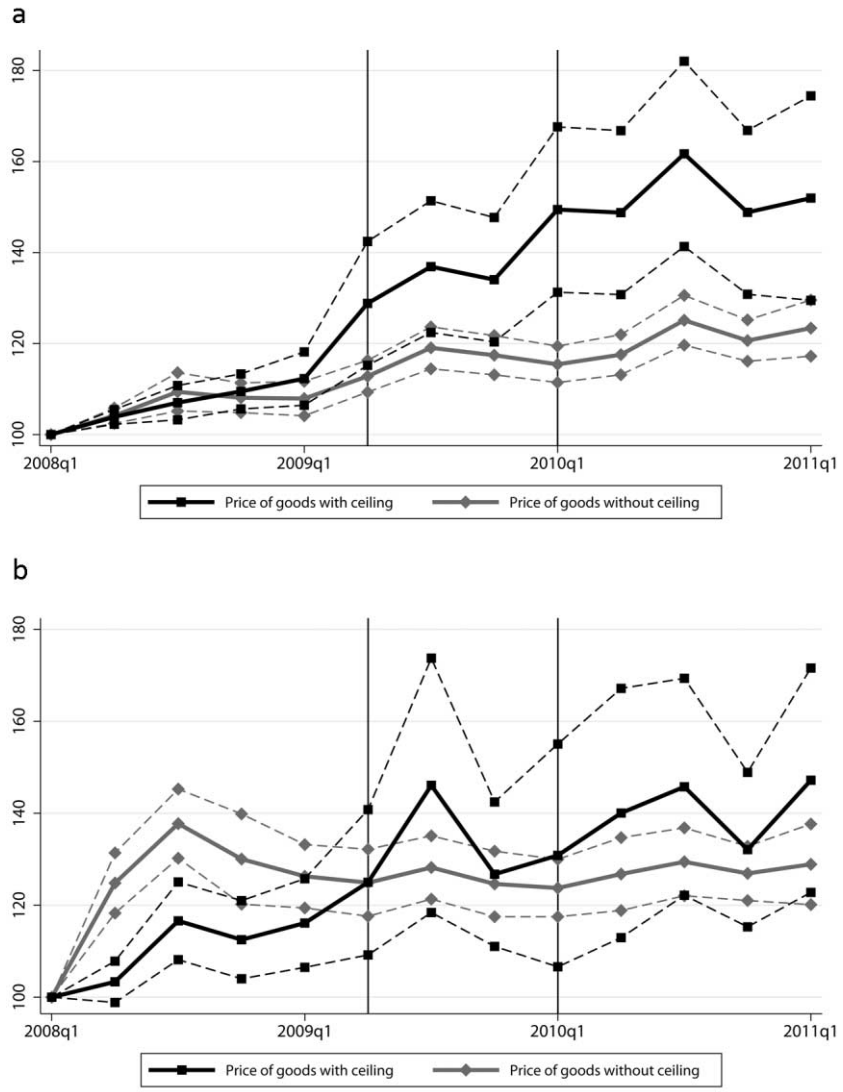

Figure 4. Treatment and control average price indexes in retail stores and street markets for all goods, 2008–11. *a*, Retail stores. *b*, Street markets. Shown is the change in the indexed price of goods in treatment and control groups over time in the subsample of goods sold in local markets and in retail stores. Dashed lines show 95% confidence intervals. Image is for the purpose of visualizing trends; however, we encourage readers to use the main regression specification to interpret significance.

liberal monitoring. However, after the complete relaxation of controls in period 3, prices rose substantially in line with the other set of goods, which were subject to price controls. Column 1 of table 5 shows that retail goods with a ceiling grew on average more than local goods with a ceiling, although this is not statistically significant.

TABLE 4

DIFFERENTIAL EFFECT OF REMOVING PRICE CONTROL FOR ESSENTIAL COMMODITIES AND GOODS SOLD IN RETAIL STORES WITH LOG OF LIBERIAN PRICES AS DEPENDENT VARIABLE (LN PRICE)

	(1)	(2)
Period 2	.0532*	.0635**
	(.0278)	(.0267)
Period 3	.0805***	.0894***
	(.0285)	(.0278)
Had a ceiling:		
Period 2	.0918***	
	(.0344)	
Period 3	.120***	
	(.0426)	
Had multiplied maximum markup:		
Period 2:		
80%–375%		.113
		(.0721)
400%–500%		.0702**
		(.0301)
525%–1,050%		.0352
		(.0387)
Period 3:		
80%–375%		.182**
		(.0861)
400%–500%		.0967***
		(.0295)
525%–1,050%		.0162
		(.0485)
Observations	702	702

Note. Standard errors are in parentheses. Robust standard errors clustered at the classification of individual consumption by purpose (COICOP) four-digit level. All specifications include product fixed effects.

* $p < .10$.

** $p < .05$.

*** $p < .01$.

Table 6 breaks goods into three categories. Even with the smaller sample size, we still find strong positive effects of the removal of price controls on prices in all three goods types. The largest effect sizes are for staple goods and luxury goods, suggesting that the effect is likely to hit all consumers. The effect on industrial goods is smaller.

Finally, table 7 weights the results by the importance to the consumption basket of rural and urban consumers. Once we take into account the importance of different goods in the consumption basket, we find that the effect size increases, suggesting large impacts from the removal of price control on consumers. The effect size increase on prices is strongest on rural consumers, but the effect on rural versus urban consumers is not statistically distinguishable from one another.

TABLE 5
LIBERIAN PRICE CONTROLS INDEX WITH INDEX OF LIBERIAN PRICES AS DEPENDENT VARIABLE (LN PRICE)

	All Goods (1)	Essential Commodities and Goods Never Subject to Price Controls (2)
Period 2	.00238	.0607***
	(.0401)	(.0168)
Period 3	.0123	.0894***
	(.0239)	(.0191)
Had a ceiling:		
Period 2	.0965	
	(.0661)	
Period 3	.145***	
	(.0494)	
Sold in a retail store:		
Period 2	.0805	
	(.0510)	
Period 3	.108**	
	(.0418)	
Had a ceiling and sold in a retail store:		
Period 2	−.0246	
	(.0788)	
Period 3	−.0570	
	(.0819)	
Essential commodity:		
Period 2		.0860
		(.0641)
Period 3		.173**
		(.0745)
Observations	702	363

Note. Standard errors are in parentheses. Robust standard errors clustered at the COICOP four-digit level.
** $p < .05$.
*** $p < .01$.

B. Quantity

As displayed in figure 5, after the removal of the price ceiling, the supply of goods in the treatment group grew less quickly than the supply of goods in the control group. While not as stark as the figures of price growth, this is consistent with the results on the price side and provides evidence that after the removal of price ceilings, some businesses may have reduced supply in order to increase prices with the purpose of gaining monopolistic rents. It is certainly inconsistent with the hypothesis that the price controls had restricted supply and resulted in pent-up demand.

Column 1 of table 8 presents the results from the difference-in-difference fixed effects regression, which supports this conclusion. One year after the removal of the ceiling, quantities in the treatment group were on average 11%

TABLE 6
LIBERIAN PRICE DISAGGREGATED BY GOOD TYPE WITH LOG OF PRICES AS DEPENDENT VARIABLE (LN PRICE)

	Staple (1)	Industrial (2)	Luxury (3)
Period 2	.0245	.0420	.114*
	(.0411)	(.0259)	(.0595)
Period 3	.0399	.118***	.105
	(.0350)	(.0363)	(.0695)
Had a ceiling:			
Period 2	.0802	.0910**	.0923
	(.0579)	(.0336)	(.101)
Period 3	.154*	.0379	.186
	(.0794)	(.0480)	(.112)
Observations	231	288	183

Note. Robust standard errors are in parentheses clustered at the COICOP four-digit level. All specifications include product fixed effects.
* $p < .10$.
** $p < .05$.
*** $p < .01$.

lower than the counterfactual of no controls. This difference increased to 32% after 2 years and is significant at the 5% level. The further reduction in quantity among treatment goods between periods 2 and 3 would be consistent with firms stockpiling goods in response to the slower movement of goods at the now higher price. This would lead to a delay in the effect of the treatment as firms run down their existing stock.

TABLE 7
LIBERIAN PRICE INDEX WEIGHTED BY RURAL AND URBAN BASKETS WITH INDEX OF LIBERIAN PRICES AS DEPENDENT VARIABLE (LN PRICE)

	With Rice		Without Rice	
	Rural Basket (1)	Urban Basket (2)	Rural Basket (3)	Urban Basket (4)
Period 2	−.000732	.0261	−.00492	.0248
	(.0446)	(.0570)	(.0454)	(.0579)
Period 3	.0340	.0671	.0299	.0658
	(.0329)	(.0506)	(.0335)	(.0514)
Had a ceiling:				
Period 2	.230***	.129	.121**	.0570
	(.0901)	(.107)	(.0517)	(.0813)
Period 3	.248***	.146	.125***	.0689
	(.0932)	(.103)	(.0449)	(.0697)
Observations	702	702	681	681

Note. Standard errors are in parentheses. Robust standard errors are clustered at the COICOP four-digit level. All specifications include product fixed effects. Results are weighted according to inverse probability weights calculated from rural and urban expenditure shares. Results are calculated with and without rice because this is by far the most important line item.
** $p < .05$.
*** $p < .01$.

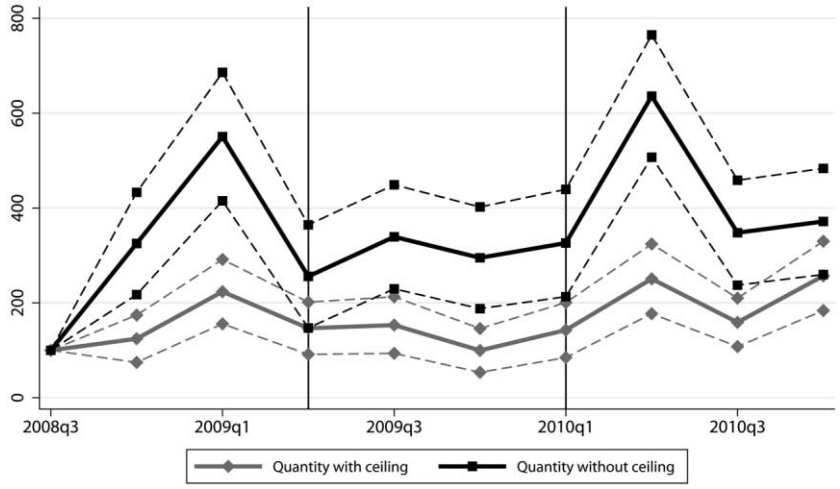

Figure 5. Comparing quantity treatment and control groups for all goods, 2008–11. Shown is the change in indexed quantity of imported goods in treatment and control groups over time. Dashed lines show 95% confidence intervals. Image is for the purpose of visualizing trends; however, we encourage readers to use the main regression specification to interpret significance.

The effects of price controls on import quantity across different maximum allowable markups are less clear than in the price case. However, across all categories there are no statistically significant positive results predicted by a collusive focal point theory or by the economics 101 prediction that price controls restrict supply.[20]

In column 1 of table 9, we run the regression including the linked dummy variable of whether the good is recorded in a retail market in the HCPI survey. We do not find any statistically significant difference on quantity of goods sold in retail and market stores with and without a ceiling, although similar to the price side, the negative coefficients on periods 2 and 3 with a ceiling and sold in a retail store are consistent with the effects being strongest in retail stores. As shown in column 2 of table 9, there is no reduction in the quantity of essential commodities until period 3 when these commodities were fully liberalized. This is consistent with the results on the price side.

[20] We were also interested in other outcome variables that might influence welfare, such as entry or exit of suppliers proxied through the number of importers. However, we learned from ministry officials that importers often use a variety of companies for different orders, thus making it impossible to trace ultimate ownership and to measure market competition in product markets. When estimating the effect on number of importers, we found a statistically significant increase in the number of importers in some specifications. This could indicate more product market competition; however, we are cautious in interpreting it this way in case it was due to an unrelated change in the strategy of incorporation of subsidiaries by key importers. Results are available from the authors on request.

TABLE 8
LIBERIAN INDEXED IMPORT VOLUMES WITH INDEX OF LIBERIAN IMPORT VOLUMES BY HS4 CATEGORY AS DEPENDENT VARIABLE (LN QUANTITY)

	(1)	(2)
Period 2	−.286*	−.286*
	(.148)	(.148)
Period 3	.960***	.960***
	(.157)	(.157)
Had a ceiling:		
Period 2	−.120	
	(.164)	
Period 3	−.384**	
	(.177)	
Had multiplied maximum markup:		
Period 2:		
8%–20%		.0514
		(.207)
25%–30%		−.335
		(.233)
35%–50%		−.0919
		(.199)
Period 3:		
8%–20%		−.320
		(.248)
25%–30%		−.397
		(.241)
35%–50%		−.440*
		(.224)
Observations	2,397	2,397

Note. Standard errors are in parentheses. Robust standard errors clustered at the HS2 level. All specifications include product fixed effects.
* $p < .10$.
** $p < .05$.
*** $p < .01$.

In table 10, we weight the results by the importance to consumption basket. Similar to the price case, we find that the effect size increases, although the difference between effects on the rural and urban baskets is less clear. In general, the results on the quantity side provide evidence that after the removal of price ceilings, firms may have restricted the supply of imports in order to push up prices to accrue monopolistic rents.

C. *Alternative Hypothesis*

There are several alternative hypotheses that might explain our results. First, as noted by Mills and Rockoff (1987), some shops may lower the quality of goods but still charge the same price. This is unlikely to be the case in Liberia, given that price ceilings were calculated as profit markups that include the cost of the good. Since import duties are levied, the incentive to overreport prices is substantially reduced.

Second, it might be possible that our results are driven by selection into the treatment group. Figure 6 and table 11 provide evidence against this hypoth-

TABLE 9
LIBERIAN INDEXED IMPORT VOLUMES WITH INDEX OF LIBERIAN IMPORT VOLUMES BY HS4 CATEGORY AS DEPENDENT VARIABLE (LN QUANTITY)

	(1)	(2)
Period 2	−.288*	−.288*
	(−1.91)	(−1.94)
Period 3	.966***	.965***
	(6.10)	(6.16)
Had a ceiling:		
Period 2	−.123	−.126
	(−.76)	(−.77)
Period 3	−.362*	−.365**
	(−1.94)	(−2.02)
Sold in a retail store:		
Period 2	.0889	
	(.40)	
Period 3	.00639	
	(.03)	
Had a ceiling and sold in a retail store:		
Period 2	−.0724	
	(−.27)	
Period 3	−.183	
	(−.75)	
Essential commodity:		
Period 2		.355**
		(2.16)
Period 3		−.608***
		(−3.15)
Had a ceiling and essential commodity:		
Period 2		−.196
		(−1.08)
Period 3		−.0170
		(−.03)
Observations	2,397	2,397

Note. *t*-statistics are in parentheses. Robust standard errors are clustered at the HS2 level.
* $p < .10$.
** $p < .05$.
*** $p < .01$.

esis. In columns 1 and 2 of table 11, we again test to see whether there is a difference between the Liberian treatment and control groups using the US price as a control variable to remove variation due to international price factors. Again, and even with this restricted sample, we find that the average price of the treatment group is higher than the control group, significantly so at the 10% level by the end of the third period. The effect size is only slightly reduced by including the US control. Figure 6 compares the average prices in Liberia and the United States for the subset of goods in our treatment group that were also covered in the US CPI survey. While prices of goods sold in Liberia accelerate steeply after the removal of the price ceilings, American prices were basically flat over the period. This result is shown in column 3 of table 11, where the difference in means between the two groups is significant in both periods.

TABLE 10
LIBERIAN PRICE INDEX WEIGHTED BY RURAL AND URBAN BASKETS WITH INDEX OF LIBERIAN PRICES AS DEPENDENT VARIABLE (LN QUANTITY)

	With Rice		Without Rice	
	Rural Basket (1)	Urban Basket (2)	Rural Basket (3)	Urban Basket (4)
Period 2	−.489*	−.469**	−.489*	−.469**
	(.259)	(.226)	(.259)	(.226)
Period 3	.930***	.926***	.932***	.928***
	(.148)	(.141)	(.148)	(.141)
Had a ceiling:				
Period 2	.269	.201	.185	.160
	(.327)	(.299)	(.357)	(.314)
Period 3	−.459*	−.502**	−.286	−.440**
	(.253)	(.202)	(.233)	(.209)
Observations	2,397	2,397	2,385	2,385

Note. Standard errors are in parentheses. Robust standard errors are clustered at the HS two-digit level. All specifications include product fixed effects. Results are weighted according to inverse probability weights calculated from rural and urban expenditure shares. Results are calculated with and without rice because this is by far the most important line item.
* $p < .10$.
** $p < .05$.
*** $p < .01$.

Third, it is possible that control and treatment groups do not follow a common trend. This is a necessary assumption for identification in the difference-in-difference specifications. Table 12 presents results from a placebo difference-in-difference experiment that provides evidence in support of this assumption.

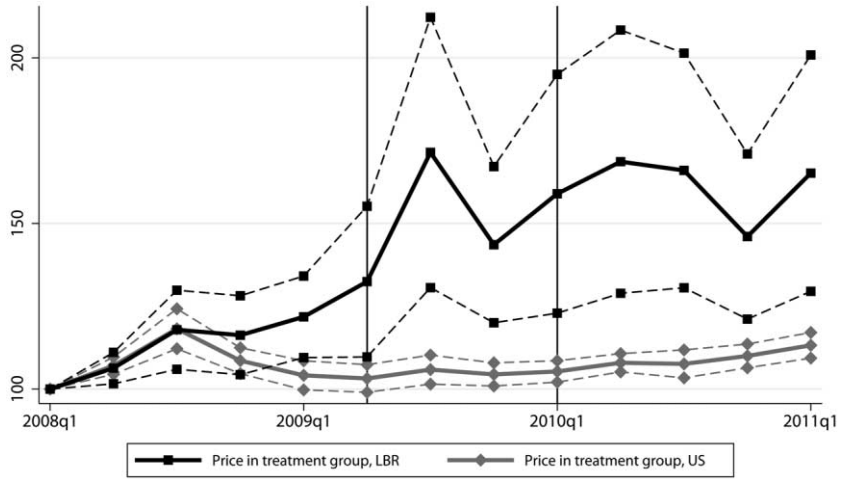

Figure 6. Comparing US and Liberian (LBR) prices in the treatment group, 2008–11. Shown is the change in indexed price of goods in treatment group in Liberia and control group of the same goods in the United States. Dashed lines show 95% confidence intervals. Image is for the purpose of visualizing trends; however, we encourage readers to use the main regression specification to interpret significance.

TABLE 11
EFFECT OF REMOVING PRICE CONTROLS WITH US COUNTERFACTUAL

	Liberian Prices: Goods Present in US CPI Basket		Liberian − US Prices: Goods in US CPI Basket and Treatment Group
	(1)	(2)	(3)
Period 2	.0923**	.108	.187
	(.0393)	(.0675)	(.105)
Period 3	.0471	.0398	.193*
	(.0316)	(.0432)	(.0952)
Had a ceiling:			
Period 2	.0686	.0715	
	(.127)	(.131)	
Period 3	.161	.157*	
	(.103)	(.0841)	
ln US price		.711	
		(.728)	
Observations	69	71	45

Note. Standard errors are in parentheses. Robust standard errors are clustered at the COICOP four-digit level. All specifications include product fixed effects. CPI = consumer price index.
* $p < .10$.
** $p < .05$.

Using data from 2006–9, the placebo experiment highlights no major differences between the two groups.

A connected concern is that the variation in prices between treatment and control groups might be driven not by the removal of price controls but instead by changes in import prices, which could be influenced by external pressures on the Liberian dollar's exchange rate. This is especially pertinent given that the Liberian authorities utilize import prices directly into the formula to calculate maximum markups.

Since we have access to detailed customs data, we can observe import prices by taking the mean of import value over import quantity for each HS4 category in each period. We then take the natural log, as done in the main specifications. We can therefore directly test this hypothesis by running the same specification and replacing retail prices with import prices. If the theoretical predictions are correct, we would not expect to see any impact of price controls on import prices, as price ceilings bind only the maximum allowable markup. Table 13 supports this hypothesis, suggesting that the source of the difference between treatment and control groups is not coming through higher import prices but instead through firms increasing markups.

VI. Conclusions

This paper provides a rare empirical window into the effectiveness of price controls. Liberia's removal of an extensive and differentiated price control re-

TABLE 12
PLACEBO EXPERIMENT WITH LOG OF LIBERIAN PRICES AS DEPENDENT VARIABLE, 2006–9 (LN PRICE)

	(1)	(2)
Period 2	.107**	.0941**
	(.0425)	(.0401)
Period 3	.235***	.224***
	(.0623)	(.0581)
Had a ceiling:		
Period 2	−.0582	
	(.0414)	
Period 3	−.122*	
	(.0630)	
Had multiplied maximum markup:		
Period 2:		
8%–20%		−.0312
		(.0735)
25%–30%		−.0518
		(.0504)
35%–50%		−.0241
		(.0701)
Period 3:		
8%–20%		−.0648
		(.0789)
25%–30%		−.151**
		(.0725)
35%–50%		−.113
		(.0996)
Observations	704	704

Note. Standard errors are in parentheses. Robust standard errors are clustered at the COICOP four-digit level. All specifications include product fixed effects.
* $p < .10$.
** $p < .05$.
*** $p < .01$.

gime allows us to credibly observe the efficacy of price controls against a set of counterfactuals.

We find strong evidence that price controls in Liberia were associated with reduced prices and evidence that they corresponded to a higher quantity of goods sold. We find that this effect seems to be more prevalent in the retail sector as compared with local street markets. This corroborates the view that firms in Liberia act in a monopolistic manner, artificially reducing quantity in order to raise the price above the competitive equilibrium. It also contradicts the assertion that corruption among inspectors completely negates the effects of the price ceiling.

We find that the price effects are attenuated with the maximum allowable markup; more restrictive markups increase prices, whereas less restrictive markups have no effect on prices. This suggests that even if price ceilings are non-

TABLE 13
ROBUSTNESS CHECK OF IMPORT PRICES WITH LOG OF LIBERIAN IMPORT VOLUMES BY HS4 CATEGORY AS DEPENDENT VARIABLE (LN IMPORT PRICE)

	(1)	(2)
Period 2	−.476***	−.476***
	(.161)	(.161)
Period 3	.753***	.753***
	(.145)	(.146)
Had a ceiling:		
Period 2	−.0330	
	(.157)	
Period 3	−.162	
	(.173)	
Had multiplied maximum markup:		
Period 2:		
8%–20%		.149
		(.212)
25%–30%		−.270
		(.311)
35%–50%		−.0146
		(.190)
Period 3:		
8%–20%		−.0614
		(.266)
25%–30%		−.168
		(.238)
35%–50%		−.256
		(.240)
Observations	2,086	2,086

Note. Standard errors are in parentheses. Robust standard errors are clustered at the HS2 level. All specifications include product fixed effects. Import price calculated by taking the average free-on-board value over the average quantity in a given period.
*** $p < .01$.

binding, this has not led to collusive focal points. While this paper does not test the welfare effects of price controls and so should not be thought of as support for their use, it does raise some important issues for the practical application of business policy in weakly institutionalized environments. In Liberia, a country infamous for the fragility of its state institutions, price controls appear to have been neither a constraint to supply nor completely negated by corruption. Indeed, they may have successfully lowered prices at the expense of monopolistic rent. This poses a couple of interesting challenges to the usual conceptualization of fragile states. One, it suggests a recasting of what it means to lack government capacity in these countries (UNDP 2010). Two, while price controls and other distorting policies may not be a best solution in countries with well-governed markets, the efficacy of more blunt, second-best interventions in countries like Liberia might benefit from a closer look.

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