Do Microenterprises Maximize Profits?
A Vegetable Market Experiment in India

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July 14, 2023

Abstract

We ran a market-level experiment in Kolkata vegetable markets in which we subsidized some vendors to sell additional produce. The vendors earned over 60% higher profits, after excluding the value of the subsidy. Nevertheless, after the subsidy ended vendors largely stopped selling the additional produce. Vendors had knowledge of the opportunity and demonstrated they were capable of exploiting it without assistance. We conclude that their behavior significantly diverges from profit maximization, which may reflect social or private preferences. We draw implications for development research and policy.

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1 Introduction

Microenterprises often fail to grow (Hsieh and Olken, 2014). Economists have predominantly focused on external constraints to growth such as lack of capital (De Mel et al., 2008), labor (De Mel et al., 2019; Hardy and McCasland, forthcoming), managerial skill (Drexler et al., 2014), and information (Hanna et al., 2014) as explanations for this phenomenon. We identify a setting in which none of these constraints bind. Microentrepreneurs have an opportunity to materially increase their profits – by over 60% – for which they have access to the necessary capital, labor, skill, and information, and the opportunity poses little additional risk, yet it remains unexploited. We rule out typically hypothesized external constraints and are left with a stark result: these enterprises are simply not profit-maximizing, either at the individual or the group level.

We work with fruit and vegetable vendors in India. These vendors typically operate in densely populated markets and their capacity is often underutilized – common features of informal markets in developing countries (Lewis, 1954). Indeed, our first contribution is to describe novel census data on the location, inventory, and sales volume of nearly 1,500 fruit vendors in Delhi, and document that they operate in close proximity to one another, and have underutilized capacity.

We then report on a non-randomized experiment in 20 Kolkata vegetable markets in which we subsidized vendors to expand their product offerings and utilize some of their spare capacity. We recorded prices and quantities for all vendors in all markets for three weeks. Then, in three markets we offered vendors three-week subsidies to procure and stock carrots and peas. We offered the carrot subsidy to all vendors and the pea subsidy only to those vendors who were not previously frequent pea sellers. The status quo prevailed in the remaining 17 markets. Following the removal of the subsidy, we recorded prices and quantities in all markets for a final two weeks. We use a difference-in-differences approach to estimate the contemporaneous and persistent effects of the subsidies, and for small-N inference we use the wild bootstrap and permutation tests.

Our first finding is that vendors who received the subsidies stocked more peas and carrots during the subsidy period. Vendors tended to sell their expanded stock without cutting prices. As a result, the profits of treated vendors rose by more than 60%, not including the value of the subsidy. Importantly, vendors who received the subsidy had to provide the additional capital up-front and had to procure the additional produce on their own; they were only reimbursed for their purchases later in the day. Hence, by design, all vendors who exploited the opportunity to sell additional peas and carrots must have had access to the capital and knowledge necessary to do so. That vendors can, in partial equilibrium, significantly increase their profits by increasing their inventory is an important finding in its own right, given that there are over five million street vendors in India alone.

Strikingly, after the subsidy period concluded, treated vendors reduced pea and carrot procurement almost to pre-intervention levels. That is, despite having experienced higher profits when they stocked the new products, and despite having the knowledge, capital, skill, and labor required to do so, most vendors reverted to their prior scale of operation and refrained from exploiting this profitable opportunity that they just experienced.

Could it be that despite having experienced higher profits from expanding their offerings, vendors failed to realize that their profits increased? We view this as unlikely, as this is a case where it is straightforward to verify that profits have increased. So long as revenues from sales of a product exceed the cost of acquiring it, and so long as vendors have spare capacity to stock the additional products – two features satisfied by our environment – then stocking the additional products increases total profits. For a vendor to verify that he sold peas or carrots at a profit does not require complex counterfactual reasoning. We also rule out risk and loss aversion as likely explanations. At the vendor-by-week level, stocking the additional peas and carrots results in lower profits less than 1% of the time, greatly reducing the specters of risk and loss.

Having established the existence of a very profitable, low-risk deviation from current business practices, and having ruled out a large set of external constraints, we view the most likely remaining explanation to be that vendors do not seek to maximize their profits, either at the individual- or group-level. This could operate at the level of individual preferences, e.g. the effort required to procure and sell additional produce, and the stress of running a larger, more active business looming too large in their objective function relative to the reward of additional profits. This explanation may be particularly plausible given evidence that many self-employed workers in the developing world are "forced entrepreneurs" (Breza et al. 2021) – such entrepreneurs might be less likely to pursue business growth at any price.

Alternatively, non-maximizing behavior may operate at the market-level if norms preclude people from expanding their inventories and selling too many products in direct competition with their nearby neighbors. In this latter case, while such norms resemble collusion, we argue that they differ in a crucial way: "classical" collusion would lead to maximized group profits. Here, we observe that vendors are not even approximately maximizing their joint profits at the market level.

We present qualitative evidence in support of each of these channels. Regardless of the channel, given the magnitude of profits being left on the table — greater than 60% of vendor profits, on average — and given the level of idle time we document in our survey, the evidence strongly suggests that these microentrepreneurs are not even approximately maximizing their profits.

This observation has important implications for how development economists and policy-makers understand microentrepreneurship. A great deal of work, a small sampling of which was cited in this introduction, has examined factors limiting the business growth of microentrepreneurs and has identified interventions to promote business growth. Our results call into question whether promoting business growth is an unambiguously positive goal, if there is some non-pecuniary cost to the microentrepreneur from managing a larger enterprise.

More specifically, many papers have identified large impacts of cash grants on microenterprise growth (De Mel et al., 2008; McKenzie and Woodruff, 2008; Fafchamps et al., 2014).

These results are often interpreted as evidence of credit constraints, as the implied returns to capital significantly exceed prevailing interest rates. The possibility of a significant non-pecuniary cost of business growth provides an alternative explanation – vendors could expand without the cash drops (i.e. they are not credit-constrained), but choose not to because of the non-pecuniary costs of expansion. Expansion is worthwhile if the necessary capital is provided for free, but not if it is provided at market-interest rates. A similar argument can also explain the divergence in impacts on microenterprise growth of cash grants versus credit.

Our results also speak to a longstanding puzzle within development economics. As early as 1954, Nobel Laureate Arthur Lewis noted the ubiquity of small firms operating side-by-side in densely packed urban markets, often seeming to operate below their capacity (Lewis, 1954), as is the case in the vegetable markets we study. Lewis conjectured that consumers would be no worse off if many traders left the market, leaving others to expand. Why this does not occur is a puzzle insofar as firms' natural desire is to grow their businesses and take market share, thereby driving some out of the market. However, our results may partially resolve this puzzle by casting doubt on the presumption that vendors uniformly seize opportunities to increase their scale and profitability.

Beyond the papers cited above, our paper is related to the literature that documents the failure of some firms to maximize profits. Cho and Rust (2010), Atkin et al. (2017), and DellaVigna and Gentzkow (2019) document various failures of profit maximization due to the organizational complexity of large firms. In contrast, the microenterprises we study are overwhelmingly sole proprietorships. Beaman et al. (2014) and Gertler et al. (2022) document failures by small and microenterprises to adopt business practices that increase profits by 3 to 8%. These authors attribute the phenomenon to limits of attention, memory and trust. In contrast, we identify a failure to adopt business practices that are far more profitable as a percentage of baseline earnings, and failures of attention, memory and trust are not plausible explanations.[1]

[1]Our paper also relates to the literature examining the extent to which micro and small business growth comes at the expense of competing businesses (De Mel et al., 2008; McKenzie and Woodruff, 2008; Drexler et al., 2014; Cai and Szeidl, 2022). Most closely related is McKenzie and Puerto (2021), which examines the impact of business

2 Spatial Competition of Fruit Vendors in South Delhi

We first document several facts about the spatial competition of produce markets, drawn from a census of fruit vendors in South Delhi. Because of the scale and completeness of these data, we are able to draw uniquely confident inferences on several important descriptive facts regarding informal markets. Together these facts will motivate the opportunity that vendors have to expand their scale of operation and increase their profits.

We conducted the census from November 2018 to February 2019 and surveyed all contiguous neighborhoods in a 135 square kilometer area. Our census included 1,179 street vendors and 309 vendors operating in designated weekly markets. We asked each vendor to carry out a 15-minute survey covering demographics, fruit variety, daily profits and revenues, bargaining, and fruit-level questions on procurement costs, selling prices, and quantities. 80% of vendors consented to the survey. For those that did not consent, we nevertheless have their locations geo-referenced and data on the fruits they were selling based on surveyor observation. We use the census to document four facts that suggest that expansion may be feasible and simple.

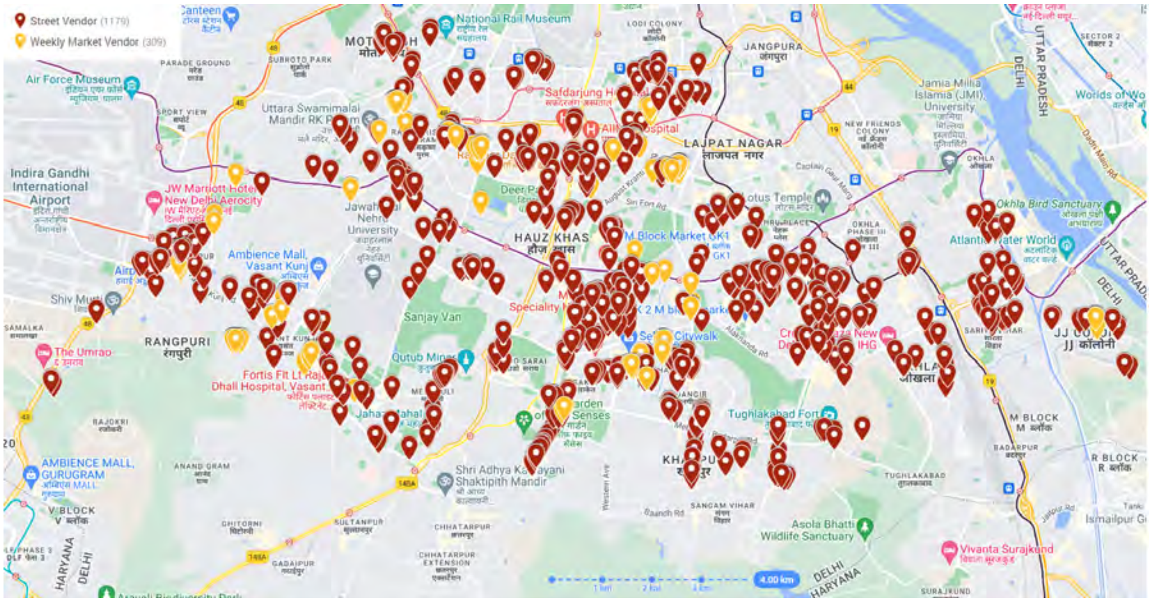
Fact 1: Vendors exhibit a high degree of spatial clustering

Figure 1 maps the universe of vendors in our census area (see Figure A1 for our survey catchment area). Vendors have on average 3.8 other vendors selling fruit within a 25-meter radius, and 1.1 within a 10-meter radius. Furthermore, 27% of vendors have at least one other vendor selling fruit within a five-meter radius. Density is higher for weekly market vendors than street vendors. For example, while 64% of market vendors have at least one other vendor within a ten-metre radius, only 43% of street vendors face such competition.

The high density of fruit vendors suggests that a given vendor could scale by attempting to acquire the market share of their nearby competitors.

training on female vendors in rural markets. In that setting, providing training to some entrepreneurs did not negatively impact competitors; rather profits increased at the market level. Similarly, we find that our intervention causes profits to increase at the market level, and we find no evidence of negative spillovers on vendors who did not receive the pea subsidy.

Figure 1: Fruit Vendor Locations



Notes: The figure denotes the exact locations of 1,179 street vendors (red) and 309 weekly market vendors (yellow). The figure includes vendors that did not consent to answer the census survey. The rectangular area includes some locations outside of our census catchment area. For the exact catchment area, see Figure A1.

Fact 2: Vendors charge non-trivial markups over their marginal cost

Despite operating in close proximity, vendors charge meaningful markups over their procurement costs. Using almost 5,000 vendor-fruit-level observations, we find that the average markup is 29%, measured as the stated selling price of the fruit less the stated procurement cost, as a fraction of the stated procurement cost. After accounting for vendors' expectations about discounts given to customers, markups are still 21% on average. Figure A2 plots the distribution of markups in our data.

While it is difficult to gauge whether these markups are big or small in an absolute sense, they are sufficiently large that vendors have room to lower their prices to undercut their competitors, if they so desired. Nevertheless, we will see below that vegetable vendors in Kolkata expanded their inventory without lowering their prices.

Fact 3: A large fraction of vendors' time is spent sitting idly

As part of our census, we asked how many customers vendors served at various times throughout the day for each day of a typical week. Vendors report a large range of typical customers per hour, with Saturdays busier than weekdays on average, and evenings the busiest, followed by mornings and then afternoons. When considering the maximum typical customers per hour across all three slots, vendors still report a large range, with a median of 15 customers per hour, and a 95th percentile of 42 customers per hour. Figure A3 plots the distribution of customers per hour in our data.

Assuming that all vendors have the capacity to operate at the capacity of the 95th percentile vendor, this suggests that even at their busiest hours the median vendor is operating at less than half capacity. Most vendors can then increase their scale without needing to hire employees.[2]

Fact 4: Nearby vendors maintain a significant degree of product differentiation

Despite a high degree of spatial clustering amongst fruit vendors, the degree of clustering at the fruit-level is considerably smaller. Specifically, averaging over all vendor-fruit-level observations, for any given fruit a vendor sells there is 1 other vendor selling the same fruit within a 25-meter radius, and 0.3 within a ten-meter radius. Only 10% of fruits have a vendor selling the same fruit within a five-meter radius. In other words, while fruit vendors are often stationed in close proximity, they are much less often selling identical fruits.

Together these four facts suggest that expansion may be feasible: vendors have the spare capacity and opportunity to expand their inventory, potentially lower their prices, and increase their sales volume. We conducted an experiment to test whether expansion is feasible and profitable in practice.

[2]While this conclusion derives from the assumption that all vendors have the capacity of the 95th percentile vendor, our experimental evidence below is in support: vegetable vendors in Kolkata manage to expand their operations without hiring employees.

3 An Experiment to Induce Vegetable Vendors to Increase Their Scale

In this section we document the existence and nature of a profitable deviation from vendors' business practices. In the following section we discuss potential mechanisms that may be responsible for vendors' failure to adopt it.

3.1 Experiment Design

Timeline and Market Selection. Our experiment took place from December 2018 to March 2019 in 20 vegetable markets around Kolkata. Due to the cost of market-wide subsidy interventions we could only intervene in three markets. With so few treated units, we did not randomize. Instead, we chose three markets with two criteria in mind. First, we chose markets of roughly medium size when compared with all 20 markets. Second, we chose markets with relatively little price volatility for peas and carrots. This reduces the possibility of idiosyncratic market-level shocks confounding the subsidy intervention.[3] Our three intervention markets are Charu Market ($n = 45$ vendors), Sarkar Bazar ($n = 73$), and Alam Bazar ($n = 85$).[4] Figure 2 presents a map of our treatment and control markets.

We break our analysis into three periods: pre-subsidy, subsidy, and post-subsidy. The pre-subsidy period lasted three weeks from December 15, 2018 to January 4, 2019; the subsidy period lasted three weeks from February 23, 2019 to March 15, 2019; and, the post-subsidy period lasted two weeks from March 16, 2019 to March 31, 2019. In each period we collected daily data from all vendors in all 20 markets. The data includes the quantity of all vegetables

[3]Given that there are more markets in our control group than our treatment group, idiosyncratic variation in our outcome variables is more likely to average out in our control group.

[4]Table A1 presents some descriptive statistics on each of our three intervention markets and our 17 control markets. Our intervention markets had 67 vendors on average while our control markets had an average of 85 vendors. Vendors in our intervention markets earned an average of Rs.355/day compared to vendors in control markets with an average daily profit of Rs.520/day. 57% (50%) of vendors in our intervention markets sold peas (carrots), while the corresponding number in control markets is 61% (54%).

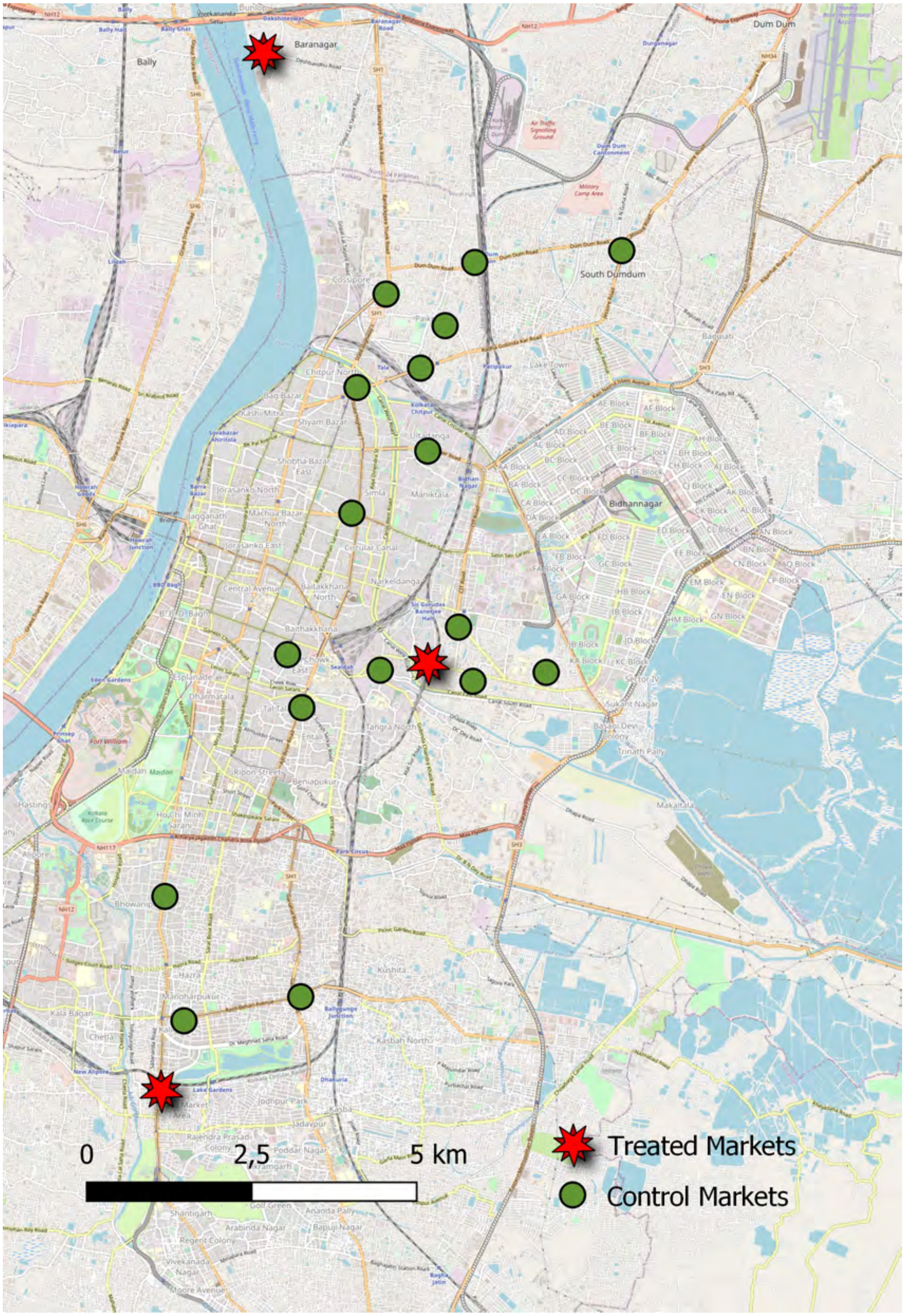


Figure 2: This map shows the location of the treated markets (in red) and control markets (in green) in our Kolkata sample.

procured each morning, the quantity sold during the previous day, and the sale price and procurement cost of each vegetable. Given our non-randomized approach, we use this panel data for a difference-in-differences strategy, checking throughout that pre-subsidy period trends of key outcomes are parallel.

Subsidy Intervention. The 17 control markets received no intervention during any of the periods. During the subsidy period, we offered a subsidy to all vendors in the three intervention markets to procure carrots. The subsidy took the form of a cash payment delivered to vendors each morning if they had procured carrots that day. The subsidy value was equal to Rs.20/kg, which was the median procurement cost of carrots during the pre-subsidy period. The maximum quantity subsidized was randomized at the vendor-level each week to be either low or high. Vendors who received the low subsidy were compensated for a maximum of 2kg of carrots, while the high quantity was set at the median of the distribution of daily wholesale purchases of carrots during the pre-subsidy period, for each market (7kg in Charu market, and 5kg in Sarkar Bazar and Alam Bazar).

While we offered the carrot subsidy to all vendors in intervention markets, we only offered the pea subsidy to infrequent pea sellers – the 40% of vendors who sold peas in fewer than eight of the days during the pre-subsidy period. The pea subsidy value was equal to Rs.30/kg, which was the median procurement cost of peas during the pre-subsidy period, and once again the subsidized volume was either low or high. Like carrots, the low quantity was set at 2kg, and the high quantity was set at the median of the distribution of daily wholesale purchases of peas during the pre-subsidy period, for each market (8kg in Charu market, 6kg in Sarkar Bazar, and 10kg Alam Bazar).

The weekly randomization of subsidized quantity was intended to investigate whether vendors with greater opportunity to stock the new produce would exhibit more persistent adoption of these products in the post-subsidy period. Unfortunately our intervention did not induce enough variation in the number of weeks a vendor was exposed to the high subsidies, and as a result this analysis is under-powered. Therefore we pool vendors who received a high versus low subsidy and focus only on the market-level variation in whether vendors were offered the subsidy or not.

Finally, we introduced one universal (carrots) and one non-universal (peas) subsidy for two reasons. First, we wanted to ensure that all vendors in intervention markets received at least one subsidy to minimize the likelihood of vendors feeling they were treated unfairly. Second, the two different subsidies allow us to explore the effects of two margins of vendor expansion. The pea subsidy effectively stimulates the "entry" of new vendors who previously did not sell the product and allows us to explore business stealing or other spillover effects on incumbent vendors. In contrast, the carrot subsidy also induces incumbents to expand their inventory on the intensive margin, which illuminates whether vendors are effectively constraining the supply of even the goods they have chosen to sell.

3.2 Empirical Approach

We estimate the following specification:

$$y_{imt} = \alpha + \beta_1 During_t + \beta_2 Post_t + \beta_3 Treat_m + \gamma_1 During_t \times Treat_m + \gamma_2 Post_t \times Treat_m + \varepsilon_{imt} \quad (1)$$

where y_{imt} is the outcome of interest for vendor i in market m on day t. $During_t$ is a dummy taking a value of one if day t was during the subsidy period, $Post_t$ is a dummy taking a value of one if day t was after the subsidy period, and $Treat_m$ is a dummy taking a value of one if market m is one of the three intervention markets. This is a difference-in-differences model where our coefficients of interest are γ_1, capturing the effect of our subsidies during the subsidy period, and γ_2, capturing the persistent effect of our subsidies after the subsidy period had ended.

Because we only have twenty markets with three treated, traditional econometric inference based on large-sample asymptotics is unlikely to perform well in our setting. Instead, we report p-values and confidence intervals computed using the wild bootstrap (Cameron et al., 2008;

Roodman et al., 2019), and p-values computed using Fisher's permutation test (Fisher, 1936; Young, 2019),[5] both using markets as the relevant cluster unit. While our tables report the results from both inference approaches, the two approaches largely coincide. Given this, we report the wild bootstrap estimates in the text, and note the permutation test p-values only when the two methods differ in statistical significance at conventional levels.

[5] With 20 markets, there are 1,140 possible combinations of three intervention markets. For the permutation test, we re-run a given regression 1,140 times, each time using a different unique combination of hypothetical intervention markets. We then calculate p-values as the fraction of t-statistics larger in magnitude than the t-statistic from the original regression.

4 Results

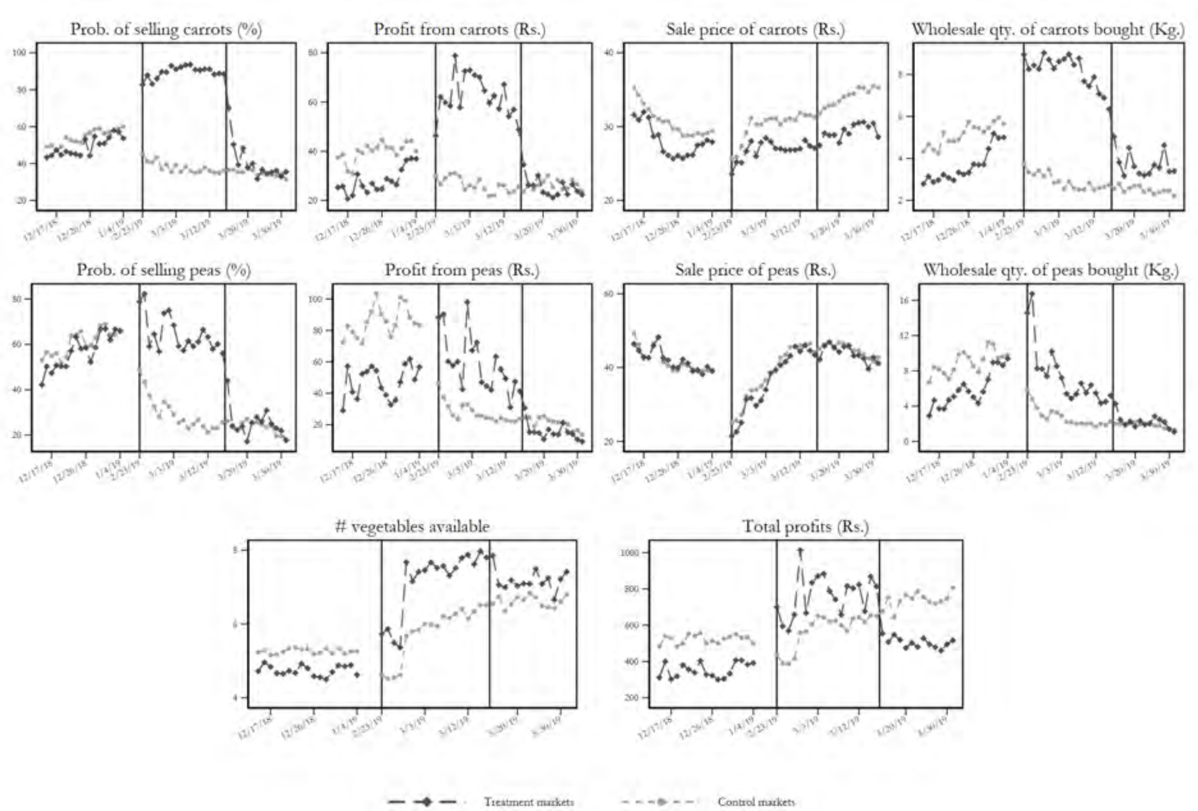


Figure 3: The first row plots the probability a vendor sells carrots, the daily profits accruing from the sales of carrots, the vendor's sale price of carrots, and the quantity of carrots procured in kilograms. The second row plots the same four outcomes for peas. The third row plots the number of types of vegetables a vendor stocks on a given day, and the vendor's daily total profits. The first vertical line demarcates the start of the subsidy period and the second line demarcates the end of the subsidy period. Profits are calculated as: (amount of vegetable at the start of the day - amount left over at the end of the day)* sale price - (amount procured at the start of the day * procurement cost). On days where the amount left over was not observed, we impute the vendor's average amount left over all days in which it was measured. Our measure of profit does not include the subsidy vendors received as part of our intervention.

Graphical Summary. Figure 3 summarizes our main findings graphically. First, outcomes in the intervention markets trend similarly to those in the control markets in the pre-subsidy period. We never reject the null that the trends are parallel (Table A2). Second, the subsidies had an important effect during the subsidy period. Vendors in intervention markets were more likely to sell peas and carrots and had higher average profits from the sales of peas and carrots. They also had higher overall profits during the subsidy period. In contrast, prices in intervention markets trended similarly to those in control markets. Third, the effects of our subsidies largely disappeared in the post-subsidy period.

Before turning to the regression results, we note that the figures appear to exhibit a discontinuity for many of the outcomes in control markets between the pre-subsidy and subsidy periods. This is because one and a half months elapsed between our pre-subsidy period and our subsidy period, and it reflects that the aggregate sales of both peas and carrots declined somewhat in that intervening period due to seasonal variation. Nevertheless, neither vegetable went "out of season" during our study period, with at least 20% of vendors selling each vegetable at all points throughout the study.

The Subsidy Period. During the subsidy period, vendors in intervention markets were 57 percentage points more likely to sell carrots on any given day (95% CI: 40pp – 73pp, $\hat{\gamma}_1$ in column 1, Table 1) and 39 percentage points more likely to sell peas (95% CI: 16pp – 64pp, column 5). On average vendors in intervention markets procured an extra 6.0kg of carrots per day (95% CI: 3.8kg – 7.7kg, column 3) and an extra 6.7kg of peas (95% CI: 4.0kg – 10.0kg, column 7). These are substantial increases relative to average procurement volumes of 3.4kg of carrots and 5.9kg of peas in intervention markets in the pre-subsidy period.[6]

Importantly, profits for vendors in intervention markets increased during the subsidy period, even after subtracting the value of their subsidy. Profits from carrots increased by Rs.44.8 per day (95% CI: Rs.21.1 – Rs.60.1, column 4) compared to an average profit from carrots of Rs.25.4 per day in the pre-subsidy period. Profits from peas increased by Rs.59.7 per day (95% CI: Rs.10.2 – Rs.98.0, column 8) compared to an average profit from peas of Rs.47.7 per day in

[6]Indeed, these point estimates suggest that vendors increased their average procurement of peas and carrots by more than the average subsidized quantity. This may be because once a vendor is induced to procure any positive quantity of peas or carrots (or induced to continue procuring peas or carrots if they would otherwise have ceased doing so), they find it worthwhile to procure more than the subsidized quantity. This would be reasonable behavior, for example, if they have negotiated a temporary exemption from a collusive norm during the experiment, and want to take full advantage of it.

the pre-subsidy period.

In addition, there is no evidence that our intervention caused sale prices for peas and carrots to decline (columns 2 and 6), indicating that vendors had not been meeting customers' full demand prior to our intervention. Thus vendors can expand profitably without reducing prices.

After the Subsidy Ended. The impacts of the subsidy diminished or fully disappeared after the subsidy period ended. There is no statistically significant increase in the likelihood that vendors in intervention markets sell additional carrots or peas ($\hat{\gamma}_2$ in columns 1 and 5, Table 1). Vendors in intervention markets only procured an additional 1.9kg of carrots per day (95% CI: -0.2kg – 4.0kg and only procured an additional 2.6kg of peas (95% CI: -0.9kg – 6.5kg). These are an increase relative to the preperiod but a roughly two-thirds drop relative to the subsidy period. And additional profits from selling carrots and peas fell even farther: profits from carrots were only Rs.8.8 higher per day (95% CI: Rs.-14.1 – Rs.35.0) and Rs.26.5 higher per day for peas (95% CI: Rs.-19.6 – Rs.66.9). All of these figures are statistically significantly lower than the corresponding estimates during the subsidy period.

Table 1: Subsidy Impacts: Carrots and Peas

	Carrot				Peas			
	Prob. of selling (%)	Sale price (Rs.)	Wholesale qty. bought (kg)	Profits (Rs.)	Prob. of selling (%)	Sale price (Rs.)	Wholesale qty. bought (kg)	Profits (Rs.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β_3 Treat	–0.05	–2.62	–1.40	–12.71	–0.04	–0.01	–2.53	–33.29
	[–0.287, 0.191]	[–6.699, 3.012]	[–3.615, 1.010]	[–26.549, 7.743]	[–0.319, 0.264]	[–3.260, 2.661]	[–7.632, 1.997]	[–82.570, 26.028]
	{ 0.651 }	{ 0.086 }	{ 0.303 }	{ 0.307 }	{ 0.563 }	{ 0.986 }	{ 0.089 }	{ 0.082 }
	⟨ 0.602 ⟩	⟨ 0.147 ⟩	⟨ 0.264 ⟩	⟨ 0.141 ⟩	⟨ 0.604 ⟩	⟨ 0.989 ⟩	⟨ 0.147 ⟩	⟨ 0.049 ⟩
γ_1 Treat × During Subs	0.57	–0.44	5.99	44.75	0.39	–0.81	6.73	59.67
	[0.398, 0.725]	[–3.122, 2.035]	[3.758, 7.725]	[21.061, 60.080]	[0.160, 0.644]	[–4.005, 2.755]	[3.956, 10.007]	[10.170, 97.985]
	{ 0.002 }	{ 0.617 }	{ < 0.001 }	{ 0.002 }	{ 0.018 }	{ 0.757 }	{ 0.016 }	{ 0.039 }
	⟨ < 0.001 ⟩	⟨ 0.685 ⟩	⟨ < 0.001 ⟩	⟨ < 0.001 ⟩	⟨ < 0.001 ⟩	⟨ 0.686 ⟩	⟨ < 0.001 ⟩	⟨ 0.002 ⟩
γ_2 Treat × After Subs	0.10	–2.04	1.94	8.79	0.05	–0.87	2.58	26.52
	[–0.075, 0.273]	[–7.815, 5.228]	[–0.162, 3.975]	[–14.129, 34.962]	[–0.187, 0.274]	[–4.614, 2.027]	[–0.936, 6.475]	[–18.532, 63.079]
	{ 0.410 }	{ 0.160 }	{ 0.069 }	{ 0.245 }	{ 0.453 }	{ 0.586 }	{ 0.063 }	{ 0.075 }
	⟨ 0.354 ⟩	⟨ 0.187 ⟩	⟨ 0.192 ⟩	⟨ 0.200 ⟩	⟨ 0.546 ⟩	⟨ 0.532 ⟩	⟨ 0.068 ⟩	⟨ 0.055 ⟩
Pre-subsidy intervention market mean	0.490	27.914	3.433	25.407	0.569	41.798	5.939	47.727
Wild Bootstrap p-value: $\gamma_1 = \gamma_2$	<0.001	0.311	0.001	0.001	0.003	0.965	0.004	0.009
Fisher p-value: $\gamma_1 = \gamma_2$	<0.001	0.357	<0.001	<0.001	<0.001	0.976	0.002	0.023
Number of Vendors	1631	1470	1631	1631	1631	1489	1631	1631
Number of Observations	55218	25073	55218	55213	55243	22657	55243	55241

Notes: This table estimates specification 1 on our full sample. Coefficients for During and Post not shown. 95% wild bootstrap confidence intervals are in [], wild bootstrap p-value is in { }, and Fisher permutation p-value is in ⟨⟩. Columns 1 - 4 present outcomes for carrots, and 5 - 8 for peas. The outcome in columns 1 and 5 is whether the vendor sells carrots or peas on the given day, the outcome in columns 2 and 6 measure the vendor's anticipated sale price for the relevant vegetable, the outcome in columns 3 and 7 measure the wholesale quantity procured of the relevant vegetable, and the outcome in columns 4 and 8 measure the daily profits accrued from the relevant vegetables. Profits are calculated by computing (amount of vegetable at the start of the day - amount left over at the end of the day)*anticipated sale price - (amount procured at the start of the day * procurement cost). On days where amount left over was not observed, we impute the vendor's average amount left over all days in which it was measured. Our measure of profit does not include the subsidy vendors received as part of our intervention.

We note that 100% (99%) of vendors in intervention markets who sold carrots (peas) experienced positive profits from sales of those vegetables during the subsidy period. Hence these results are not driven by the possibility that a majority of vendors found it marginally unprofitable to sell peas and carrots and only those who experienced the profit increase continued selling peas and carrots. Rather, many vendors who experienced positive profits from sales of peas and carrots nevertheless chose to stop selling these vegetables after our intervention concluded.

Beyond Carrots and Peas. Table 2 presents the impact of our subsidies on aggregate vendor outcomes, rather than those corresponding to either carrots or peas. The aggregate picture is largely consistent with the results from the individual vegetables. During the subsidy period, total costs of wholesale purchases in intervention markets rose by Rs.690 per day (95% CI: Rs.234 – Rs.1,149, column 1) compared to an average cost of wholesale purchases of Rs.825 in intervention markets in the pre-subsidy period. Average vendor profits rose by Rs.228 per day (95% CI: Rs.-52 – Rs.531, column 3) compared to an average profit of Rs.342 in the pre-subsidy period. On average vendors stocked an additional 2.0 (95% CI: 0.5 – 3.4, column 4) types of vegetables during the subsidy period compared to an average of 4.7 products stocked per vendor in the pre-subsidy period. Once again, these effects either diminish or disappear after our subsidy concluded, with no statistically significant increase on any of the aforementioned outcomes.

Table 2: Subsidy Impacts: Vendor-Level Outcomes

	Aggregate			
	Total Cost of Wholesale Purchases (Rs.)	Sales (Rs.)	Profits (Rs.)	# vegetables available
	(1)	(2)	(3)	(4)
β_3 Treat	–448.49	–589.02	–140.53	–0.50
	[–1073.389, 58.919]	[–1203.300, 46.070]	[–385.185, 114.969]	[–2.525, 1.784]
	{ 0.058 }	{ 0.052 }	{ 0.128 }	{ 0.547 }
	⟨ 0.032 ⟩	⟨ 0.030 ⟩	⟨ 0.075 ⟩	⟨ 0.471 ⟩
γ_1 Treat × During Subs	689.60	917.98	228.32	1.97
	[234.192, 1149.070]	[275.331, 1496.020]	[–52.334, 530.698]	[0.506, 3.388]
	{ 0.027 }	{ 0.033 }	{ 0.066 }	{ 0.031 }
	⟨ < 0.001 ⟩	⟨ 0.005 ⟩	⟨ 0.025 ⟩	⟨ 0.012 ⟩
γ_2 Treat × After Subs	527.12	466.27	–60.84	1.16
	[–126.081, 1079.258]	[–303.151, 1240.917]	[–414.986, 250.636]	[–0.552, 2.651]
	{ 0.266 }	{ 0.322 }	{ 0.325 }	{ 0.309 }
	⟨ 0.268 ⟩	⟨ 0.342 ⟩	⟨ 0.403 ⟩	⟨ 0.228 ⟩
Pre-subsidy intervention market mean	825.121	1167.304	342.183	4.733
Wild Bootstrap p-value: $\gamma_1 = \gamma_2$	0.573	0.166	0.033	0.060
Fisher p-value: $\gamma_1 = \gamma_2$	0.582	0.139	<0.001	0.059
Number of Vendors	1628	1628	1628	1628
Number of Observations	52898	52898	52898	52898

This table estimates specification 1 on our full sample. Coefficients for During and Post not shown. 95% wild bootstrap confidence intervals are in [], wild bootstrap p-value is in {}, and Fisher permutation p-value is in ⟨⟩. The outcome in column 1 is the total cost of wholesale purchases on a given day, the outcome in column 2 is the vendor's total revenues on a given day accruing from all produce, the outcome in column 3 is the daily profits accrued from all produce, and the outcome in column 4 is the number of distinct types of vegetables a vendor has available on a given day. Profits are calculated by computing (amount of vegetable at the start of the day - amount left over at the end of the day)*anticipated sale price - (amount procured at the start of the day * procurement cost). On days where amount left over was not observed, we impute the vendor's average amount left over all days in which it was measured.

Interestingly, the effect of our intervention on total profits is larger than the sum of the effects on the profits from sales of peas and carrots. This difference is only statistically significant at the 10% level when using the wild bootstrap, and not statistically significant at the 10% level using the Fisher permutation test. Similarly, the effect on the cost of total wholesale purchases is larger than the sum of the effect on the costs of purchases of peas and carrots. This difference is statistically significant at the 10% level using both the wild bootstrap and Fisher permutation tests. Hence these results leave open the possibility that our subsidy "crowded in" the sale of complementary produce.

Pea-Subsidy Impacts by Eligibility. Recall that while everyone in intervention markets received a carrot subsidy, only infrequent peas sellers were eligible for the pea subsidy. We now turn to the differential effects of the pea subsidy on vendors in intervention markets who did and did not receive the subsidy to focus on the extensive margin. These are presented in Table 3, which again uses specification 1, but now splits the sample by pea subsidy eligibility.

Table 3: Subsidy Impacts: By Pea Subsidy Eligibility

	Eligible				Ineligible			
	Prob. of selling (%) (1)	Sale price (Rs.) (2)	Wholesale qty. bought (kg) (3)	Profits (Rs.) (4)	Prob. of selling (%) (5)	Sale price (Rs.) (6)	Wholesale qty. bought (kg) (7)	Profits (Rs.) (8)
β_3 Treat	–0.03	0.38	–0.10	–17.39	0.01	–0.05	–2.87	–37.71
	[–0.062, 0.035]	[–9.990, 14.333]	[–2.347, 0.354]	[–50.080, 17.587]	[–0.113, 0.120]	[–3.125, 3.090]	[–9.707, 3.420]	[–96.298, 41.895]
	{ 0.114 }	{ 0.835 }	{ 0.081 }	{ 0.098 }	{ 0.755 }	{ 0.944 }	{ 0.319 }	{ 0.086 }
	⟨ 0.053 ⟩	⟨ 0.908 ⟩	⟨ 0.060 ⟩	⟨ 0.033 ⟩	⟨ 0.793 ⟩	⟨ 0.961 ⟩	⟨ 0.267 ⟩	⟨ 0.104 ⟩
γ_1 Treat × During Subs	0.67	4.35	7.84	65.59	0.16	–1.996	5.37	50.90
	[0.538, 0.743]	[0.652, 9.503]	[4.120, 10.338]	[32.976, 96.680]	[–0.084, 0.483]	[–5.239, 1.326]	[0.993, 11.342]	[–24.166, 106.831]
	{ < 0.001 }	{ 0.036 }	{ < 0.001 }	{ 0.004 }	{ 0.074 }	{ 0.282 }	{ 0.038 }	{ 0.075 }
	⟨ 0.002 ⟩	⟨ 0.181 ⟩	⟨ 0.005 ⟩	⟨ 0.008 ⟩	⟨ 0.124 ⟩	⟨ 0.311 ⟩	⟨ 0.007 ⟩	⟨ 0.018 ⟩
γ_2 Treat × After Subs	0.10	0.25	1.74	19.36	–0.00	–0.56	2.66	27.51
	[–0.008, 0.219]	[–3.815, 3.286]	[0.264, 2.838]	[–14.929, 52.739]	[–0.168, 0.223]	[–4.875, 2.785]	[–3.304, 7.159]	[–41.816, 85.503]
	{ 0.064 }	{ 0.909 }	{ 0.039 }	{ 0.108 }	{ 0.952 }	{ 0.798 }	{ 0.249 }	{ 0.175 }
	⟨ 0.120 ⟩	⟨ 0.913 ⟩	⟨ 0.010 ⟩	⟨ 0.013 ⟩	⟨ 0.969 ⟩	⟨ 0.754 ⟩	⟨ 0.274 ⟩	⟨ 0.250 ⟩
Pre-subsidy intervention market mean	0.176	39.381	1.635	10.564	0.847	42.154	8.982	73.993
Wild Bootstrap p-value: $\gamma_1 = \gamma_2$	<0.001	0.257	<0.001	0.001	0.027	0.611	0.043	0.045
Fisher p-value: $\gamma_1 = \gamma_2$	<0.001	0.539	0.006	0.008	0.025	0.554	0.043	0.060
Number of Vendors	562	480	562	562	1069	1009	1069	1069
Number of Observations	19763	3687	19763	19761	35480	18970	35480	35480

This table estimates specification 1 on our full sample. Coefficients for During and Post not shown. 95% wild bootstrap confidence intervals are in [], wild bootstrap p-value is in {}, and Fisher permutation p-value is in ⟨⟩. The outcome in column 1 is the total cost of wholesale purchases on a given day, the outcome in column 2 is the vendor's total revenues on a given day accruing from all produce, the outcome in column 3 is the daily profits accrued from all produce, and the outcome in column 4 is the number of distinct types of vegetables a vendor has available on a given day. Profits are calculated by computing (amount of vegetable at the start of the day - amount left over at the end of the day)*anticipated sale price - (amount procured at the start of the day * procurement cost). On days where amount left over was not observed, we impute the vendor's average amount left over all days in which it was measured.

The qualitative patterns for eligible pea vendors are the same as in the previous analyses. During the subsidy period, eligible vendors in intervention markets were 66 percentage points (95% CI: 54pp – 74pp) more likely to stock peas on any given day during the subsidy period, they procured an extra 7.8kg of peas per day (95% CI: 4.1kg – 10.3kg), and earned an extra Rs.65.6 per day (95% CI: Rs.33.0 – Rs.96.7) from the sale of peas. Unlike in the previous analysis, there is evidence of a price increase during the subsidy period of Rs.4.4/kg (95% CI: Rs.0.7 – Rs.9.5), statistically significant at the 5% level using the wild bootstrap, but not when using the permutation test. Qualitative evidence we collected suggests this may be because vendors substituted towards higher quality peas. Once again, all of these effects diminish considerably after our subsidy was removed.

We find no evidence of business stealing effects. In fact, the patterns for vendors who were ineligible for the pea subsidy are largely the same as the patterns for eligible vendors. These vendors procured more peas and earned higher profits from the sale of peas (and higher profits overall, not reported in the table) during the subsidy period, despite not having access to a pea subsidy. Qualitative evidence we collected after the intervention suggests that this is due to informal arrangements between vendors who sold peas prior to our intervention, typically larger vendors, and vendors who did not. Namely, these larger vendors would procure and transport additional peas at the wholesale market and then sell them to vendors who received a subsidy. Note however that this remains consistent with our basic narrative. It is possible for many vendors to increase their sales volume and profits by purchasing and selling more carrots and peas, but even after directly verifying and experiencing these opportunities firsthand, they refrained from exploiting them after our intervention.

For completeness, in Appendix Table A3 we present analogous results from the carrot subsidy, disaggregated by vendors who were frequent or infrequent carrot sellers in the pre-subsidy period. The results are qualitatively the same. Both types of vendors experienced an increase in sales and profits of carrots during the subsidy period, and then these increases largely disappeared in the post period.

Therefore, on the extensive margin, subsidizing the entrance of new pea vendors increased their profits while also increasing the average profits of incumbent pea vendors. On the intensive margin, inducing existing carrot vendors to expand their supply, while also inducing the entry of new carrot vendors, increased the profits of both groups. In both cases, the additional sales and profits largely dissipated after our intervention concluded.

Accounting For Potential Spillovers From Treatment to Control Markets. An identifying assumption of our difference in differences approach is that our intervention in treatment markets did not influence outcomes in our control markets. This assumption would be violated if vendors who expanded their scale in treated markets diverted customers from control markets.

To rule out this possibility, we re-estimate Specification 1, but remove from the analysis the control markets that are most likely to be affected by our intervention in treatment markets. Specifically, within each market i we asked all vendors which nearby market customers would be most likely to shop from if they were not to shop at market i. For each treatment market i, we drop any market from our sample that is in the top three markets that are most frequently listed as likely competitors. This results in dropping three markets from the analysis, as most of the frequently listed competitor markets are not within our sample to begin with.

The results are presented in Tables A4 and A5. Importantly, none of the patterns are qualitatively altered. Vendors are significantly more likely to sell peas and carrots during the subsidy period and earn significantly higher profits from the sale of peas and carrots, as well as total profits. Again, all of these effects diminish significantly after the subsidy is removed.

Measurement Error in Profits. To compute profits from a particular vegetable we multiply a vendor's sale price times the volume of the good sold minus the procurement price times the the volume of the good procured. This measure omits several important factors in a vendor's profits, such as the cost of renting their spot in a market, the cost of transporting their goods from the wholesale market to their stall, and the opportunity cost of their labor.

Nevertheless, under reasonable assumptions, the exclusion of these costs serves to downwardly bias our main result – that subsidizing the procurement of peas and carrots increased vendors' profits by more than 60%. We formalize this argument through a simple model.

Suppose that a vendors' baseline profits, without scaling their business to include additional peas and carrots, generates r_0 revenue and c_0 expenses from vegetable procurement (i.e. the revenues and costs that we measure). And suppose that conditional on scaling their business to include additional peas and carrots it would generate $r_1 = sr_0$ revenue and $c_1 = sc_0$ expenses from vegetable procurement, for some scalar $s > 0$. Then our treatment effect corresponds to

$$\frac{(r_1 - c_1) - (r_0 - c_0)}{r_0 - c_0} = s \equiv \hat{\tau}.$$

Now consider any unmeasured fixed cost f – i.e. costs that do not scale with vegetable procurement – such as the rent expenses of a vendor's market stall. Accounting for these fixed costs f would serve to increase our estimate of the impact of scaling the vendor's business on her profits to

$$\frac{(r_1 - c_1) - (r_0 - c_0)}{r_0 - c_0 - f} > \hat{\tau}.$$

Having established that properly accounting for fixed costs would serve to increase our estimated treatment effect, we now assume them to be 0 and turn to unmeasured variable costs v, which scale with the amount of produce procured. These would include the cost of transporting the additional produce as well as the opportunity cost of the vendor's labor required to procure and sell the additional produce. We assume that the total unmeasured variable cost is vc_0 at the baseline scale, and is vc_1 at the larger scale induced by our intervention. Then accounting for these variable costs would not change the estimate of our impact. That is,

$$\frac{(r_1 - (1+v)c_1) - (r_0 - (1+v)c_0)}{r_0 - (1+v)c_0} = \hat{\tau}$$

If instead of scaling in proportion to the cost of vegetable procurement c the variable costs scaled less than proportionately, accounting for the variable costs would only serve to increase our estimated treatment effect. Therefore, the only unaccounted for expenses that could weaken our results are variable expenses that scale *more* than proportionately with the cost of vegetable procurement.

We do not believe these types of variable expenses are likely. Qualitative evidence we collected suggests that transport costs from the wholesale market to the vendors' stalls scale less than proportionately with the cost of vegetable procurement. Once a vendor commissions a truck for transport, marginally increasing the vendor's allotted space on the truck is a relatively small expense. Selling additional produce likely requires additional labor, but it is nearly infeasible that the vendor's supplied hours of labor could increase by more than 60%. Utilizing data from our baseline survey, we note that the distribution of working hours is narrow. Moving a vendor from the 20th percentile of working hours to the 90th percentile of working hours would correspond to an increase from 6 hours worked per day to 9 hours worked per day – an extreme movement in the distribution that would correspond to only a 50% increase in working hours. Moreover, while we do not have data on hours worked in the subsidy and post-subsidy periods, we did collect data on whether the vendor was present in the market on any given day – we find no treatment effect of our intervention on the number of days a vendor is present in the market.

While it is extremely unlikely that the number of hours worked increased in proportion to the increase in scale and profits, it is possible that vendors exhibit upward sloping labor supply curves and that the marginal hours of labor required to service a larger business are extremely costly to supply. We return to this possibility in the following section where we consider mechanisms that drive our results.

5 Why Don't Vendors Exploit Their Opportunity For Increased Scale and Profits?

Thus far we have established that vendors have an opportunity to significantly increase their profits – on average by more than 60% – yet they do not exploit it, either before or after our intervention. In this section we consider a number of mechanisms that may drive this phenomenon.

Do vendors know that selling (more) peas and carrots would increase their profits? Our experiment strongly suggests that a lack of knowledge about the profitability of selling peas and carrots is not the inhibiting factor. Vendors in our treatment markets experienced higher profits from selling peas and carrots for three weeks, and despite this, they largely ceased selling the additional products once the subsidy was removed.

While in principle it is possible that vendors did not realize they were earning higher profits than in their unsubsidized counterfactual, we do not believe this is likely. This is a setting in which learning that it is profitable to sell a new product does not require complicated inference about a counterfactual scenario. So long as the revenues generated by selling peas and carrots exceed the cost of procuring them – a fact that is clearly satisfied in our setting – and so long as vendors have sufficient excess capacity to stock additional produce without removing any of their existing produce, then selling the additional products should result in increased profits. This latter fact is confirmed in Table 2, demonstrating that the sales of peas and carrots complemented, rather than displaced sales of existing produce. And it is further supported by our analysis of Delhi fruit vendors in Section 2.

For these reasons, it does not seem likely that vendors ceased selling peas and carrots for lack of knowledge that doing so would be profitable.

Do vendors have the necessary skill, labor, and capital to successfully stock and sell peas and *carrots?* Our experimental design ensures that vendors who procured additional peas and carrots during the subsidy period had access to all of the necessary capital, labor, and skill required to do so. Specifically, each day the subsidy was delivered to vendors only after they procured the additional vegetables on their own. Therefore a lack of any of these factors must not be the explanation for why vendors did not continue to stock peas and carrots after the intervention concluded.

Can risk- or loss aversion explain why vendors do not continue to stock peas and carrots? In principle, a sufficiently high degree of risk or loss aversion can explain any failure to adopt profitable business practices (e.g. Kremer et al., 2013). But we emphasize that stocking peas and carrots offered a very high return with very little risk. In treatment markets during the subsidy period, vendors who sold carrots earned positive profits from doing so on 96.1% of the vendor $\times$ days in which they stocked carrots, and 99.6% of the vendor $\times$ weeks. 100% of vendors earned positive profits over the full subsidy period. The analogous numbers for peas are 96.2%, 99.1%, and 98.9%.

Given these statistics, even an extremely risk averse vendor ought to find it worthwhile to stock at least a small amount of peas and carrots, yet in our experiment we found that the probability a vendor stocked any carrots or peas fell to just 10% and 5% respectively. Therefore risk and loss aversion are unlikely to be the driving force for vendors' failure to continue stocking carrots and peas.

Vendors' objectives significantly deviate from profit-maximization. We have established that stocking additional peas and carrots is an extremely profitable, low risk opportunity, that vendors are aware of the opportunity, and that it is feasible for them to exploit this opportunity without outside assistance. Therefore we view the most likely explanation as to why they do not exploit the opportunity to be that vendors' objectives are not well approximated by profit-maximization.

This deviation from profit maximization may operate either at the individual- or group-level. At the individual-level, running a larger and more active business entails significantly more stress and physical exertion. This could manifest itself through vendors working harder during their ordinary hours of operation (e.g. by serving more customers per hour) and through vendors working for longer hours. Though in this latter case, we emphasize that it is virtually certain that vendors did not work more hours in proportion to their earnings increase. Rather it may be that vendors face an upward-sloping cost of labor supply, and their marginal hours of labor are extremely costly to supply.

At the group-level, there may be norms or implicit or explicit agreements that discourage vendors from selling the same produce as their nearby competitors. To the extent that such norms or agreements exist they diverge significantly from "classical" collusion, in which vendors within a market jointly suppress sales volume to maximize their collective profits (e.g. Tirole, 1988). An explicit agreement to supress sales volume would be difficult to sustain. The markets are informal, have many vendors (median of 73) who could all potentially sell the same product, and almost all have excess capacity. Moreover, almost all sellers operate very small businesses, making it less likely that they have the deep pockets needed to undertake a price war to punish a deviant seller.

More importantly, when we induced some vendors to stock additional peas and carrots, profits went up for the entire market on average (column 3, Table 2). That is, there was unmet demand at the market level, and vendors who began selling peas and carrots were able to serve this demand and increase aggregate profits.[7]

Moreover, these norms would need to be flexible enough that our intervention had meaningful effects: quantities sold could change temporarily without generating any observable punishment for violating the norms. While norms like these may facilitate a form of collusion,

[7]Note that there was unmet demand at the market level does not necessarily imply that consumers' demand was not being met prior to our intervention. Vendors in treated markets who expanded their inventory may have partially been serving demand that was previously served outside the market. However we also note there is little evidence of spillovers from treatment to control markets, as we document in Section 4.

they sustain an outcome that leaves significant profits on the table even for the market as a whole; that is, the group of vendors is behaving in a way that significantly deviates from profit maximization.

After our subsidy concluded, we conducted a qualitative survey with all vendors in intervention markets who stopped selling peas and carrots. We read a list of 16 reasons a vendor may have stopped selling a product, and for each asked them to tell us whether this reason had no influence, a small influence or a big influence on their decision. While we did not design the survey with this explanation in mind, many of the vendors' responses were indicative of both individual- and group-level departures from profit maximization.

Regarding their individual preferences, many vendors indicated that the additional profits from selling peas and carrots were not sufficient to induce them to do so. For instance, with regards to why vendors stopped selling carrots, 63% said that they were too costly to procure, 63% said there was insufficient demand to make it worth their while to stock it, and 19% said the market price was too low. The analogous figures were 54%, 65%, and 27% for peas.

Several responses also indicated the possibility of norms that discourage vendors to sell the same produce as their nearby neighbors. For instance, 9% of vendors who stopped selling carrots, and 8% of vendors who stopped selling peas said that they feared other vendors would be angry at them for continuing to do so. Further, 37% of vendors who stopped selling carrots, and 38% of vendors who stopped selling peas indicated that there were too many other vendors in the market already selling these products. Although this final response may also be partially indicative of the former channel, that the profits from selling these products was insufficient to induce vendors to do so.

While it is qualitatively unsurprising that vendors do not single-mindedly maximize their profits, the magnitude of money left on the table suggests that their preferences diverge very significantly from profit maximization.

6 Implications for Development Research and Policy

6.1 The Returns to Capital in Microenterprises

A large body of academic research studies the returns to capital in microenterprises. A consistent finding in this work is that cash grants have large impacts on business profits (e.g. De Mel et al., 2008; McKenzie and Woodruff, 2008; Blattman et al., 2016; Karlan et al., 2019), while access to microcredit has more muted impacts (e.g. Banerjee et al., 2015a; Meager, 2019), though there are prominent exceptions to both patterns. Our findings have three important implications for this literature. These implications are formalized in Appendix Section A.

The first implication is that the welfare impact of these interventions may not be well approximated by impacts on profits or consumption. To the extent that there are unmeasured, non-pecuniary costs associated with inducing microenterprises to expand, the welfare impacts may be significantly smaller than increases in profits. Of course, this observation extends beyond capital interventions, to any program meant to promote business expansion amongst microenterprises. While recent work emphasizes the non-pecuniary *benefits* of some work relative to no work (Hussam et al. 2022a), future work might characterize the nonpecuniary costs of intensive work, relative to the status quo.

Second, the large impacts of cash grants cited above are often interpreted as evidence of credit constraints, as the implied returns to capital far exceed the prevailing cost of credit (e.g. De Mel et al., 2008; McKenzie and Woodruff, 2008; Fafchamps et al., 2014). However, the large impacts of cash grants may instead be indicative of the non-pecuniary costs of business growth, or "as-if" non-pecuniary costs in the case of norms discouraging business growth. That is, it may be worthwhile to grow one's business when offered the means to do so for free, but not at the prevailing cost of capital. Therefore unexploited profitable opportunities for business growth may not be the results of credit constraints.

Third, the wedge between the returns to microcredit and the returns to cash grants is often interpreted as evidence that the structure of microcredit is not well-suited to help microenterprises exploit their most profitable investment opportunities (e.g. Field et al., 2013; Green and Liu, 2021; Liu and Roth, 2022; Hussam et al., 2022b). But, related to the previous point, this wedge could also arise if the non-pecuniary costs of business expansion deter entrepreneurs from availing credit to expand their businesses. In particular, non-pecuniary costs of business expansion could mean that some entrepreneurs would grow their business if given a grant "for free", but not if they are offered the same amount of capital at market interest rates. Then, on average, grants would induce more business growth than credit. Moreover, this narrative would predict that credit would only impact entrepreneurs whose opportunities to increase their profits were strong enough to outweigh both the cost of interest and the non-pecuniary costs of growth, suggesting that microcredit would induce a small set of entrepreneurs to grow their profits a lot. This prediction is born out in some evaluations of microcredit (e.g. Banerjee et al., 2015b; Crépon et al., 2015).

6.2 The Industrial Organization of Microenterprises

Development economists have long noted the ubiquity of small firms operating side-by-side in densely packed urban markets. Lewis (1954) observes that petty retail trading in developing countries is dominated by crowded markets and traders making only a few sales each. According to Lewis, consumers would be no worse off if many traders left the market, leaving others to expand. More recently, motivated in large part by the question of why microenterprises do not grow, potentially either employing neighboring competitors or driving them out of the market, economists have evaluated the impact of helping microenterprises hire additional employees (e.g. Hardy and McCasland, forthcoming; De Mel et al., 2019).

The non-pecuniary costs of business expansion that we identify may be a principle explanation for why many markets can sustain so many microenterprises, seemingly operating significantly below capacity, without "natural forces" inducing the most efficient of them to grow and the least efficient to exit. It may be that the modal microenterpreneur's objectives are sufficiently misaligned with profit-maximization that the prospect of growing their business and competing their neighbors out of the market is simply not attractive. Indeed, our study takes place precisely in such a market. Our intervention induced vendors to compete more aggressively with their neighbors, it induced vendors to earn significantly higher profits (without even inducing any apparent cost on their neighbors), and after our subsidy ended, vendors revealed the prospect of maintaining this expansion to be unattractive.

7 Discussion and Conclusion

We conducted an experiment demonstrating that vegetable vendors in Kolkata could stock additional peas and carrots and increase their profits by over 60%. No external constraint prevented vendors from exploiting this opportunity. And even after we subsidized vendors to stock additional produce, allowing them to directly verify the degree to which doing so would increase their profits, almost all vendors reduced or stopped the sale of these produce altogether upon the subsidy's removal.

Instead, our results are best explained by the fact that vendors do not seek to maximize their profits, at the individual- or group-level. At the individual-level this may be attributable to the additional stress or effort required to sustain a larger business, and at the group-level it may be attributable to norms that discourage vendors from maintaining an inventory that is too like that of their close neighbors. In the latter case we emphasize that while these norms may facilitate a form of collusion, it is one that induces vendors to leave significant profits on the table, even as a group. We provide qualitative evidence consistent with both the individual preferences and group norms that induce important deviations from profit-maximizing behavior.

Finally, we draw important implications of these results for development research and policy. Our results provide a new perspective on the returns to cash grants and credit on microenterprises, and on a long-standing puzzle regarding the preponderance of small vendors operating in densely packed urban markets in many developing countries. Most importantly, policy should be guided by microentrepreneurs' revealed preferences rather than merely maximizing their household incomes. Our results suggest that these two goals may be quite divergent.

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A Formalizing the Implications from Section 6.1

In this section we formalize the implications of our experimental results, discussed informally in Section 6.1.

Consider an entrepreneur i who has a lumpy investment opportunity requiring $\bar{k}$ capital. If she invests, she receives a financial return r_i and suffers a non-pecuniary cost ψ. This cost represents the friction identified in our experiment that prevents an entrepreneur from growing her business. It may represent the additional effort or stress required of the vendor to run a larger business, or the social or psychological cost of violating a group norm discouraging her from growing her business. Any uninvested capital, and any returns from her investment are consumed with linear utility of consumption. We normalize any other income to zero.

Time unfolds in two periods, without discounting. In the first period the entrepreneur makes her investment decision and in the second period she consumes any output.

Formally, an entrepreneur with a budget of $\bar{k}$ capital must decide whether to consume it, delivering $\bar{k}$ utility, or to invest it, delivering $\bar{k}r_i - \psi$ utility.

Suppose further that the entrepreneur has no capital endowment, but can borrow $\bar{k}$ capital from a lender at interest rate $\bar{r}$ — that is, if she borrows $\bar{k}$ capital then in the second period she must repay $\bar{r}\bar{k}$, and consumes the residual. If she chooses not to borrow, her utility will be zero and if she borrows her utility will be $\bar{k}(r_i - \bar{r}) - \psi$.

Implication 1.

The first implication from Section 6.1 is that the welfare impacts of giving an entrepreneur a cash grant of size $\bar{k}$ for investment may not be well approximated by the impact on her income. This is a straightforward consequence of the fact that inducing a borrower to make her investment will increase her income by $r_i\bar{k}$, yet will only increase her utility by $r_i\bar{k} - \psi$. If the non-pecuniary cost ψ is large, these two measures will diverge significantly.

Implication 2.

The second implication from Section 6.1 is that an entrepreneur given access to a cash grant of size $\bar{k}$ may experience a return $r_i > \bar{r}$ without being credit constrained.

To see this, note that in line with the discussion above, an entrepreneur given a cash grant will invest it if and only if

$$\bar{k}r_i - \psi > \bar{k} \iff r_i > 1 + \frac{\psi}{\bar{k}}$$

If the same entrepreneur were denied the grant, she would choose to borrow on the capital market if and only if

$$\bar{k}r_i - \psi > \bar{k}\bar{r} \iff r_i > \bar{r} + \frac{\psi}{\bar{k}}$$

The cash grant is pivotal for the entrepreneur's investment decision if and only if $r_i \in \left[1 + \frac{\psi}{\bar{k}}, \bar{r} + \frac{\psi}{\bar{k}}\right]$. For $\psi > 0$, some of these entrepreneurs will have a return $r_i > \bar{r}$ despite there being no credit constraints in the model. The non-pecuniary cost is necessary for this result in the sense that if $\psi = 0$, no entrepreneur who would make an investment only if they received the grant would have a return $r_i > \bar{r}$.

Implication 3.

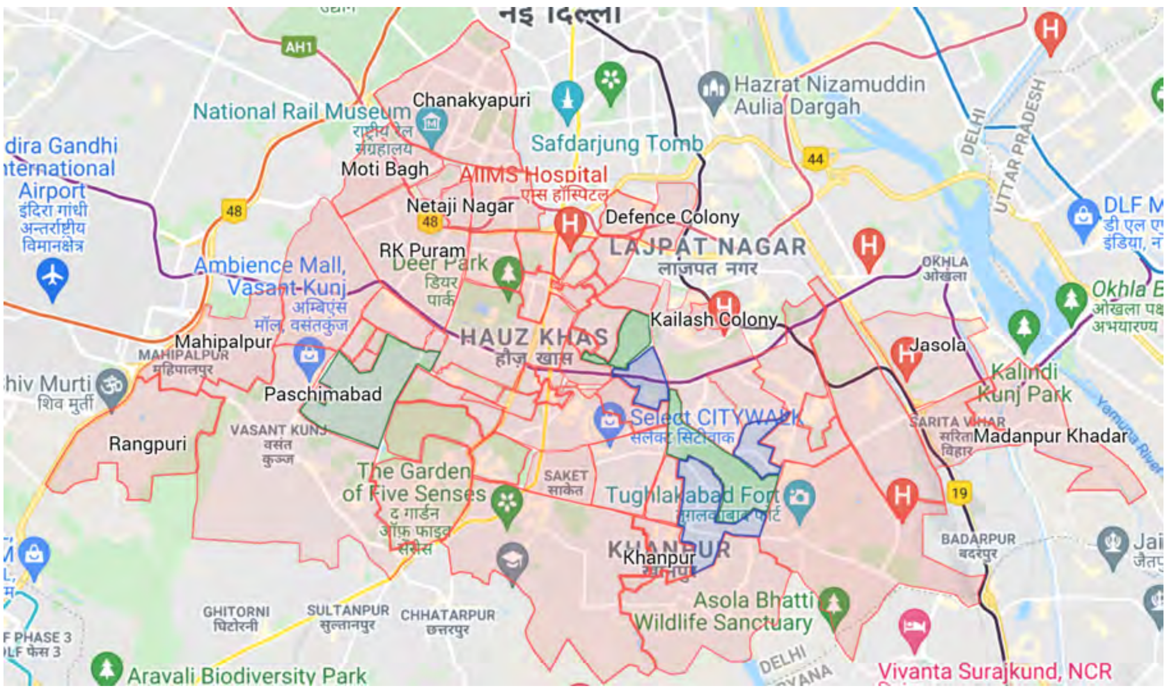
The third implication from Section 6.1 is that the impacts of cash and credit interventions may considerably diverge. The analysis that leads to this implication is virtually identical to that of Implication 2. The principle difference is that in order for a credit intervention to have any impact, it must be that similar credit was not previously available to the entrepreneur at the same interest rate. So, we now assume there is no credit available to the entrepreneur at baseline.

As above, an intervention that provides her access to a cash grant of size $\bar{k}$ will induce her to invest if and only if $r_i > 1 + \frac{\psi}{\bar{k}}$. An intervention providing her access to a loan of size $\bar{k}$ at interest rate $\bar{r}$ will induce her to invest if and only if $r_i > \bar{r} + \frac{\psi}{k}$. The cash grant will have a bigger impact on business growth insofar as more entrepreneurs will choose to invest if given access to the cash grant. Specifically, parallel to the analysis for Implication 2, entrepreneurs whose returns lie within $r_i \in \left[1 + \frac{\psi}{k}, \bar{r} + \frac{\psi}{k}\right]$ would invest in their business if and only if they are given access to the cash grant. The larger is ψ, the larger are the returns that lie within this range and the larger is the potential divergence in impacts between cash grants and credit.

Finally, we noted that several RCTs evaluating the impact of microcredit found that credit has the biggest impacts on the right tail of entrepreneurial incomes (e.g. Banerjee et al., 2015b; Crépon et al., 2015). Within the framework outlined in this section, the only entrepreneurs impacted by access to credit are those for whom their return is sufficiently high to justify incurring the non-pecuniary cost of investment, i.e. for whom $r_i > \bar{r} + \frac{\psi}{k}$. For large non-pecuniary cost ψ, this would imply that only the right tail of investment opportunities are impacted by access to credit.

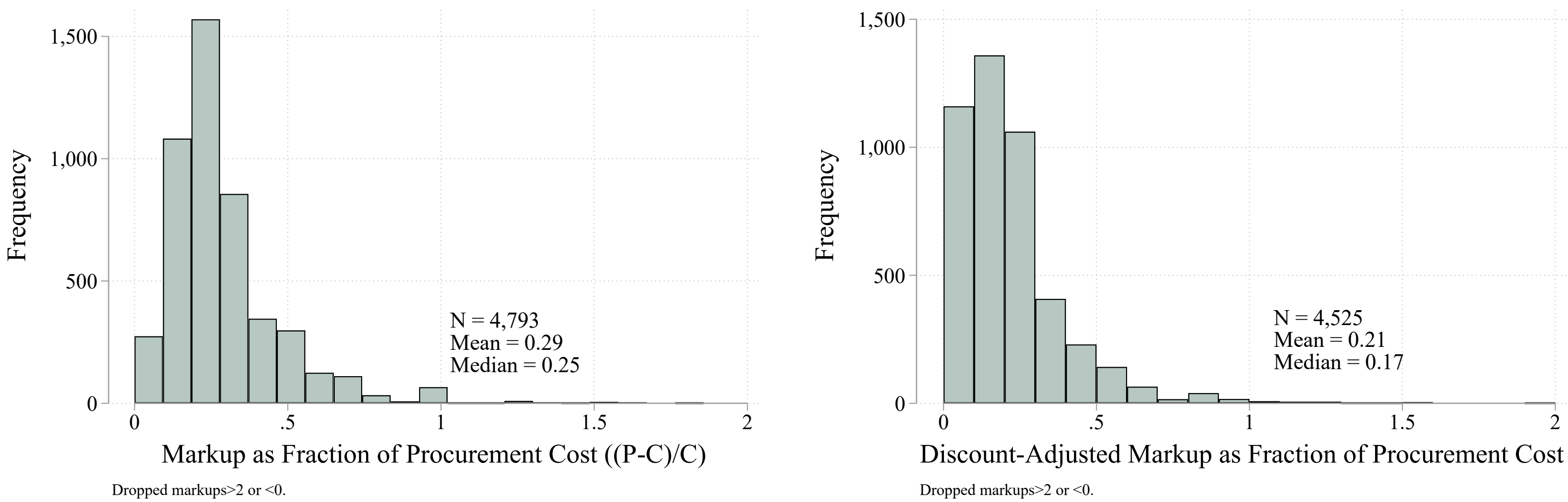
B Appendix Figures and Tables

Figure A1: Fruit Vendor Census Area



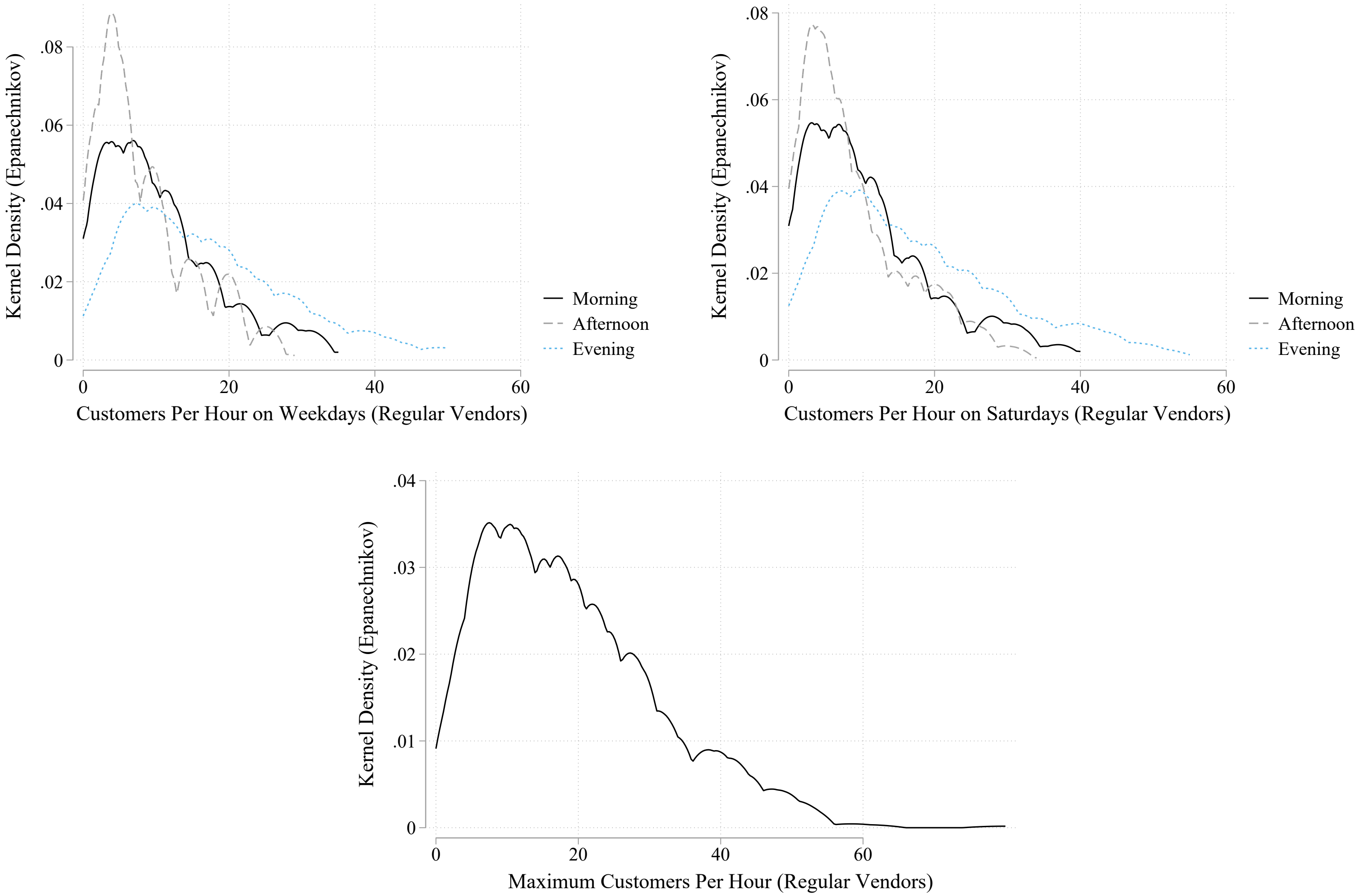
Notes: This figure shows the contiguous 135 square kilometer area of South Delhi covered by our vendor census. The red polygons cover 125 square kilometers and were successfully surveyed, the green polygons (Jawaharlal Nehru University, Hauz Khas Forest, and Jahanpanah Forest) are non-commercial areas and so were not surveyed, while the three blue polygons were erroneously missed.

Figure A2: The Distribution of Fruit-Level Markups



Notes: The figure shows the distribution of fruit-level markups as measured in the vendor census survey. In the left panel, the markup is measured as the stated selling price less the stated procurement cost, as a fraction of the stated procurement cost. In the right panel, we discount-adjust each markup by subtracting the vendor's stated typical discount (with the discount only measured at the vendor-level, rather than fruit-by-fruit). In both cases, we drop outliers above two or below zero (25 dropped from the left panel, 246 dropped from the right).

Figure A3: Many Vendors Are Not Very Busy



Notes: The top panel shows kernel densities of regular vendor answers to the question "what is the number of customers you serve during one hour of operations during each of these time periods on a [weekday/Saturday]?" In each case, the figure excludes any outlier answers at or above the 99th percentile. The bottom panel shows the kernel density of the maximum answer given to the six previous questions (weekday/Saturday-by-morning/afternoon/evening), as well as the equivalent questions for Sunday, which were only asked in the rare case that vendors said Sunday demand was different to Saturday demand.

Table A1: Descriptive Statistics for Intervention and Control Markets

	Charu Market (1)	Sarkar Bazar (2)	Alam Bazar (3)	Control markets (4)
Mean # vendors in census	45.0	73.0	85.0	85.8
Mean # vendors present per day	34.5	57.4	71.7	86.7
Mean profits per vendor (Rs.)	448.9	344.9	314.6	519.6
Mean # vegetables available per vendor	5.7	5.1	4.0	5.2
% of present vendors selling peas	62.9	59.9	51.8	61.0
% of present vendors selling carrots	65.1	52.2	39.1	54.1
Mean total cost of daily purchases (Rs.)	969.0	894.4	748.3	1,396.8
Mean value of Sales (Rs.)	1,417.9	1,239.4	1,062.9	1,917.0
Mean number of years selling in market	22.0	22.9	26.6	23.9
Mean age	44.2	48.7	49.1	47.6
% of female vendors	33.3	47.9	12.9	23.3

Notes: This table presents summary statistics averaged for each intervention market in columns 1 - 3 and averaged over all control markets in column 4. All statistics are calculated using data from the pre-subsidy period. Each of the control markets is assigned equal weight in the reported mean. Profits are calculated by computing (amount of vegetable at the start of the day - amount left over at the end of the day)*anticipated sale price - (amount procured at the start of the day * procurement cost). On days where amount left over was not observed, we impute the vendor's average amount left over all days in which it was measured.

Table A2: Testing for Parallel Trends in the Pre-Subsidy Period

	Carrot				Peas			
	Prob. of selling (%) (1)	Sale price (Rs.) (2)	Wholesale qty. bought (kg) (3)	Profits (Rs.) (4)	Prob. of selling (%) (5)	Sale price (Rs.) (6)	Wholesale qty. bought (kg) (7)	Profits (Rs.) (8)
Panel A: Carrot and Peas								
β_3 Treat	–0.05	–3.21	–1.51	–13.06	–0.08	–0.44	–3.92	–32.10
	[–0.248, 0.193]	[–7.777, 0.852]	[–3.496, 0.580]	[–28.925, 7.708]	[–0.360, 0.220]	[–5.608, 4.298]	[–8.219, -0.212]	[–68.302, 1.433]
	{ 0.700 }	{ 0.085 }	{ 0.135 }	{ 0.294 }	{ 0.543 }	{ 0.753 }	{ 0.045 }	{ 0.056 }
	⟨ 0.606 ⟩	⟨ 0.181 ⟩	⟨ 0.218 ⟩	⟨ 0.182 ⟩	⟨ 0.504 ⟩	⟨ 0.789 ⟩	⟨ 0.013 ⟩	⟨ 0.021 ⟩
γ_1 Treat × Day	–0.00	0.08	0.04	0.17	0.01	0.06	0.19	–0.22
	[–0.014, 0.009]	[–0.313, 0.532]	[–0.071, 0.140]	[–1.575, 2.185]	[–0.005, 0.016]	[–0.296, 0.414]	[–0.131, 0.540]	[–3.323, 3.374]
	{ 0.981 }	{ 0.696 }	{ 0.433 }	{ 0.718 }	{ 0.594 }	{ 0.701 }	{ 0.628 }	{ 0.747 }
	⟨ 0.991 ⟩	⟨ 0.626 ⟩	⟨ 0.529 ⟩	⟨ 0.747 ⟩	⟨ 0.446 ⟩	⟨ 0.605 ⟩	⟨ 0.517 ⟩	⟨ 0.839 ⟩
Pre-subsidy intervention market mean	0.488	27.881	3.398	24.946	0.566	41.749	5.715	44.499
Number of Vendors	1591	1361	1591	1591	1591	1373	1591	1591
Number of Observations	20040	10675	20040	20040	20040	12053	20040	20040

	Cost of wholesale purchases (Rs.)	Sales (Rs.)	Profits (Rs.)	# vegetables available
Panel B: Aggregate				
β_3 Treat	–550.19	–689.20	–139.01	–0.40
	[–1189.504, 3.171]	[–1201.552, -52.897]	[–305.242, 50.550]	[–2.780, 1.980]
	{ 0.051 }	{ 0.046 }	{ 0.204 }	{ 0.631 }
	⟨ 0.069 ⟩	⟨ 0.052 ⟩	⟨ 0.154 ⟩	⟨ 0.644 ⟩
γ_1 Treat × Day	13.02	12.88	–0.14	–0.01
	[–8.966, 33.517]	[–14.810, 40.492]	[–9.291, 10.210]	[–0.091, 0.076]
	{ 0.505 }	{ 0.632 }	{ 0.980 }	{ 0.334 }
	⟨ 0.367 ⟩	⟨ 0.517 ⟩	⟨ 0.970 ⟩	⟨ 0.455 ⟩
Pre-subsidy intervention market mean	825.121	1167.304	342.183	4.733
Number of Vendors	1591	1591	1591	1591
Number of Observations	20040	20040	20040	20040

Notes: This table estimates the following specification: $y_{it} = \alpha + \beta_1 Day_t + \beta_2 Treat_m + \beta_3 Day_t \times Treat_m + \varepsilon_{imt}$ on our sample during the pre-subsidy period. Coefficients for Day not shown. 95% wild bootstrap confidence intervals are in [], wild bootstrap p-value is in {}, and Fisher permutation p-value is in ⟨⟩. In Panel A, outcomes are specific to peas or carrots. The outcome in columns 1 and 5 is whether the vendor sells carrots or peas on the given day, the outcome in columns 2 and 6 measure the vendor's anticipated sale price for the relevant vegetable, the outcome in columns 3 and 7 measure the wholesale quantity procured of the relevant vegetable, and the outcome in columns 4 and 8 measure the daily profits accrued from the relevant vegetables. Profits are calculated by computing (amount of vegetable at the start of the day - amount left over at the end of the day)*anticipated sale price - (amount procured at the start of the day * procurement cost). On days where amount left over was not observed, we impute the vendor's average amount left over all days in which it was measured. Our measure of profit does not include the subsidy vendors received as part of our intervention. In Panel B the outcomes correspond to aggregate measures. The outcome in column 1 is the total cost of wholesale purchases on a given day, the outcome in column 2 is the vendor's total revenues on a given day accruing from all produce, the outcome in column 3 is the daily profits accrued from all produce, and the outcome in column 4 is the number of distinct types of vegetables a vendor has available on a given day.

Table A3: Subsidy Impacts on Carrots: By Pre-Period Carrot Sales

	Eligible				Ineligible			
	Prob. of selling (%)	Sale price (Rs.)	Wholesale qty. bought (kg)	Profits (Rs.)	Prob. of selling (%)	Sale price (Rs.)	Wholesale qty. bought (kg)	Profits (Rs.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β_3 Treat	–0.03	0.14	–0.88	–7.44	–0.01	–3.06	–1.32	–13.12
	[–0.101, 0.040]	[–8.571, 11.088]	[–2.271, 0.142]	[–20.559, 3.236]	[–0.151, 0.140]	[–7.143, 2.125]	[–5.089, 2.331]	[–41.382, 16.092]
	{ 0.203 }	{ 0.939 }	{ 0.057 }	{ 0.072 }	{ 0.934 }	{ 0.090 }	{ 0.167 }	{ 0.270 }
	⟨ 0.439 ⟩	⟨ 0.947 ⟩	⟨ 0.006 ⟩	⟨ 0.039 ⟩	⟨ 0.928 ⟩	⟨ 0.086 ⟩	⟨ 0.267 ⟩	⟨ 0.211 ⟩
γ_1 Treat × During Subs	0.70	–1.87	5.85	44.58	0.45	0.16	6.00	44.78
	[0.582, 0.751]	[–5.377, 2.017]	[4.757, 6.764]	[29.547, 54.127]	[0.229, 0.662]	[–3.024, 3.344]	[3.750, 8.648]	[17.312, 69.357]
	{ < 0.001 }	{ 0.554 }	{ < 0.001 }	{ 0.002 }	{ 0.017 }	{ 0.816 }	{ 0.003 }	{ 0.026 }
	⟨ < 0.001 ⟩	⟨ 0.361 ⟩	⟨ < 0.001 ⟩	⟨ 0.003 ⟩	⟨ < 0.001 ⟩	⟨ 0.865 ⟩	⟨ < 0.001 ⟩	⟨ 0.002 ⟩
γ_2 Treat × After Subs	0.12	–1.59	1.85	11.21	0.09	–2.11	1.86	6.94
	[0.045, 0.176]	[–8.995, 4.909]	[0.615, 2.576]	[2.744, 18.805]	[–0.157, 0.340]	[–9.917, 6.610]	[–1.607, 5.068]	[–34.375, 43.041]
	{ 0.019 }	{ 0.275 }	{ 0.018 }	{ 0.032 }	{ 0.399 }	{ 0.205 }	{ 0.101 }	{ 0.379 }
	⟨ 0.049 ⟩	⟨ 0.466 ⟩	⟨ 0.012 ⟩	⟨ 0.004 ⟩	⟨ 0.482 ⟩	⟨ 0.240 ⟩	⟨ 0.312 ⟩	⟨ 0.482 ⟩
Pre-subsidy intervention market mean	0.151	27.917	0.844	6.739	0.785	27.913	5.687	41.665
Wild Bootstrap p-value: $\gamma_1 = \gamma_2$	<0.001	0.925	<0.001	<0.001	0.006	0.164	0.019	0.040
Fisher p-value: $\gamma_1 = \gamma_2$	<0.001	0.941	<0.001	0.002	0.002	0.165	<0.001	<0.001
Number of Vendors	629	517	629	629	1002	953	1002	1002
Number of Observations	22045	4408	22045	22044	33173	20665	33173	33169

Notes: This table estimates specification 1 on our full sample. Coefficients for During and Post not shown. 95% wild bootstrap confidence intervals are in [], wild bootstrap p-value is in {}, and Fisher permutation p-value is in ⟨⟩. Columns 1 - 4 present outcomes for vendors who sold carrots on less than 8 days during the pre-subsidy period (analogous to the pea subsidy eligibility criterion) and 5 - 8 present outcomes for vendors who sold carrots on 8 or more days during the pre-subsidy period. The outcome in columns 1 and 5 is whether the vendor sells carrots on the given day, the outcome in columns 2 and 6 measure the vendor's anticipated sale price for carrots, the outcome in columns 3 and 7 measure the wholesale quantity of carrots procured, and the outcome in columns 4 and 8 measure the daily profits accrued from carrots. Profits are calculated by computing (amount of carrots at the start of the day - amount left over at the end of the day)*anticipated sale price - (amount procured at the start of the day * procurement cost). On days where amount left over was not observed, we impute the vendor's average amount left over all days in which it was measured. Our measure of profit does not include the subsidy vendors received as part of our intervention.

Table A4: Subsidy Impacts: Carrots and Peas, Selected Control Markets

	Carrot				Peas			
	Prob. of selling (%) (1)	Sale price (Rs.) (2)	Wholesale qty. bought (kg) (3)	Profits (Rs.) (4)	Prob. of selling (%) (5)	Sale price (Rs.) (6)	Wholesale qty. bought (kg) (7)	Profits (Rs.) (8)
β_3 Treat	–0.04	–2.41	–1.29	–10.66	–0.04	0.40	–2.53	–30.34
	[–0.242, 0.195]	[–6.057, 1.717]	[–3.354, 1.312]	[–23.827, 9.896]	[–0.338, 0.215]	[–2.229, 2.834]	[–7.383, 1.816]	[–66.253, 14.929]
	{ 0.685 }	{ 0.088 }	{ 0.295 }	{ 0.311 }	{ 0.542 }	{ 0.614 }	{ 0.088 }	{ 0.072 }
	⟨ 0.666 ⟩	⟨ 0.131 ⟩	⟨ 0.312 ⟩	⟨ 0.168 ⟩	⟨ 0.618 ⟩	⟨ 0.654 ⟩	⟨ 0.150 ⟩	⟨ 0.066 ⟩
γ_1 Treat $\times$ During Subs	0.58	0.00	5.99	44.95	0.40	–0.59	6.84	59.02
	[0.397, 0.751]	[–2.287, 2.445]	[3.717, 7.707]	[21.165, 57.910]	[0.204, 0.641]	[–3.864, 3.092]	[4.073, 11.220]	[16.323, 100.547]
	{ 0.002 }	{ 1.000 }	{ < 0.001 }	{ 0.001 }	{ 0.013 }	{ 0.810 }	{ 0.012 }	{ 0.032 }
	⟨ 0.001 ⟩	⟨ 1.000 ⟩	⟨ 0.001 ⟩	⟨ 0.001 ⟩	⟨ < 0.001 ⟩	⟨ 0.775 ⟩	⟨ 0.001 ⟩	⟨ 0.001 ⟩
γ_2 Treat $\times$ After Subs	0.11	–1.08	2.01	11.29	0.06	–0.66	2.58	25.22
	[–0.063, 0.262]	[–3.624, 1.669]	[0.004, 4.060]	[–3.038, 24.198]	[–0.226, 0.316]	[–4.191, 2.214]	[–1.707, 7.546]	[–32.502, 75.147]
	{ 0.339 }	{ 0.309 }	{ 0.048 }	{ 0.173 }	{ 0.433 }	{ 0.662 }	{ 0.076 }	{ 0.091 }
	⟨ 0.297 ⟩	⟨ 0.312 ⟩	⟨ 0.176 ⟩	⟨ 0.100 ⟩	⟨ 0.504 ⟩	⟨ 0.600 ⟩	⟨ 0.093 ⟩	⟨ 0.075 ⟩
Pre-subsidy intervention market mean	0.490	27.914	3.433	25.407	0.569	41.798	5.939	47.727
Wild Bootstrap p-value: $\gamma_1 = \gamma_2$	<0.001	0.425	<0.001	<0.001	0.002	0.972	0.002	0.013
Fisher p-value: $\gamma_1 = \gamma_2$	0.001	0.460	0.001	<0.001	0.001	0.974	<0.001	0.007
Number of Vendors	1477	1329	1477	1477	1477	1351	1477	1477
Number of Observations	50042	22173	50042	50037	50060	20334	50060	50058

Notes: This table replicates Table 1 excluding control markets that were frequently cited as a likely substitute for each treatment market. Substitute control markets were defined as any of top three responses by vendors in treatment markets to this question: 'If customers were not buying from this market, where would they buy?' Relative to our full sample, this sample excldues three control markets, as the majority of responses to this question were markets that are not in our sample. Coefficients for During and Post not shown. 95% wild bootstrap confidence intervals are in [], wild bootstrap p-value is in {}, and Fisher permutation p-value is in ⟨⟩. Columns 1 - 4 present outcomes for carrots, and 5 - 8 for peas. The outcome in columns 1 and 5 is whether the vendor sells carrots or peas on the given day, the outcome in columns 2 and 6 measure the vendor's anticipated sale price for the relevant vegetable, the outcome in columns 3 and 7 measure the wholesale quantity procured of the relevant vegetable, and the outcome in columns 4 and 8 measure the daily profits accrued from the relevant vegetables. Profits are calculated by computing (amount of vegetable at the start of the day - amount left over at the end of the day)*anticipated sale price - (amount procured at the start of the day * procurement cost). On days where amount left over was not observed, we impute the vendor's average amount left over all days in which it was measured. Our measure of profit does not include the subsidy vendors received as part of our intervention.

Table A5: Subsidy Impacts: Aggregate, Selected Control Markets

	Aggregate			
	Total Cost of Wholesale Purchases (Rs.)	Sales (Rs.)	Profits (Rs.)	# vegetables available
	(1)	(2)	(3)	(4)
β_3 Treat	–439.42	–563.24	–123.82	–0.46
	[–1063.251, 18.057]	[–1290.848, -49.077]	[–333.440, 154.260]	[–2.551, 1.973]
	{ 0.050 }	{ 0.047 }	{ 0.181 }	{ 0.570 }
	⟨ 0.049 ⟩	⟨ 0.047 ⟩	⟨ 0.107 ⟩	⟨ 0.491 ⟩
γ_1 Treat × During Subs	712.69	952.08	239.33	2.06
	[261.979, 1186.478]	[291.745, 1620.608]	[–68.883, 518.041]	[0.758, 3.195]
	{ 0.025 }	{ 0.027 }	{ 0.059 }	{ 0.017 }
	⟨ 0.001 ⟩	⟨ 0.004 ⟩	⟨ 0.021 ⟩	⟨ 0.012 ⟩
γ_2 Treat × After Subs	561.93	542.41	–19.52	1.26
	[–100.174, 1103.259]	[–216.245, 1166.595]	[–294.265, 208.509]	[–0.466, 2.744]
	{ 0.234 }	{ 0.291 }	{ 0.687 }	{ 0.280 }
	⟨ 0.221 ⟩	⟨ 0.304 ⟩	⟨ 0.776 ⟩	⟨ 0.200 ⟩
Pre-subsidy intervention market mean	1117.143	1556.492	439.340	5.493
Wild Bootstrap p-value: $\gamma_1 = \gamma_2$	0.559	0.252	0.013	0.037
Fisher p-value: $\gamma_1 = \gamma_2$	0.597	0.201	0.003	0.053
Number of Vendors	1474	1474	1474	1474
Number of Observations	48098	48098	48098	48098

This table replicates Table 2 excluding control markets that were frequently cited as a likely substitute for each treatment market. Substitute control markets were defined as any of top three responses by vendors in treatment markets to this question: 'If customers were not buying from this market, where would they buy?' Relative to our full sample, this sample excldues three control markets, as the majority of responses to this question were markets that are not in our sample. Coefficients for During and Post not shown. 95% wild bootstrap confidence intervals are in [], wild bootstrap p-value is in {}, and Fisher permutation p-value is in ⟨⟩. The outcome in column 1 is the total cost of wholesale purchases on a given day, the outcome in column 2 is the vendor's total revenues on a given day accruing from all produce, the outcome in column 3 is the daily profits accrued from all produce, and the outcome in column 4 is the number of distinct types of vegetables a vendor has available on a given day. Profits are calculated by computing (amount of vegetable at the start of the day - amount left over at the end of the day)*anticipated sale price - (amount procured at the start of the day * procurement cost). On days where amount left over was not observed, we impute the vendor's average amount left over all days in which it was measured.