Essays on Public Policy and Firm Behavior

by

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ABSTRACT

Private sector plays a key role in job-creation and is central to economic development. Governments around the world use various tools and policies to foster growth of private enterprises. My dissertation aims to shed light on how firms respond to large-scale regulatory policy reforms and its aggregate implications for economic growth and productivity.

Chapter I investigates the impact of input-output linkages on aggregate productivity gains from reducing distortions that implicitly tax larger and more productive firms. Specifically, I study the elimination of firm-size restrictions on a set of product markets in India during the 2000s using a rich firm-level data. I show that such a reform propagates to upstream suppliers as a demand shock and to downstream customers as an input cost shock. Upon reform, there is an increase in aggregate productivity within the directly exposed and linked markets. These gains are primarily driven by reallocation of inputs to larger and more productive firms. However, productivity gains are attenuated when linked markets are highly concentrated. More productive firms in concentrated linked markets raise their markups in response to the demand or supply shock, thereby increasing misallocation. The adjustment of markups is consistent with models where demand elasticity decreases with firm productivity and underlines the substantial bias from ignoring market structure in linked markets when assessing the impact of piecemeal reforms. Conditional on the supply-chain being sufficiently competitive, linkages can amplify the overall gains from reforms that reduce distortions in a market.

Chapter 2 evaluates the impact of a capital subsidy on the ability of small firms to cope with electricity shortages. Infrastructure development has become a critical policy issue in many countries. With public resources strained, attention has increasingly turned to mobilizing private investment in infrastructure. Exploiting a policy within a state in India that offered subsides to acquire generators for firms below a capital threshold, I study the effectiveness of reducing self-generation costs. Using the triple differences approach, I find that a 25% subsidy on the capital increased

the probability of self-generation by 14%. Smaller firms with higher marginal cost of self-generation and lower credit costs are the key beneficiaries of this subsidy. Secondly, firms investing under the subsidy also have lower rates of self-generation, indicating capacity under-utilization. I estimate returns to infrastructural investment of at least 11%. Hence, despite low utilization and high marginal costs, a capital subsidy can incentivize firms on the margin to make large remedial infrastructure investments that yield net positive returns.

In Chapter 3, with Luiza Antoun de Almeida, we analyze the effects of selected structural reforms on output and employment in the short and medium term using comprehensive cross-country firm-level dataset covering both advanced and emerging market economies over the period 2003–14. In line with previous studies, we find that structural reforms have in general a positive impact on output and employment in the medium term. Furthermore, we find evidence that firm characteristics such as size, leverage, profitability, and sector influence the effectiveness of structural reforms. These findings have relevant policy implications as they help policymakers tailor the design of structural reforms to maximize their payoffs, taking into account their distributional impact on firms.

Chapter I

Bottlenecks vs ripple effect: The role of linkages in the India's Product Market Liberalization

Introduction

Misallocation of inputs from high- to low-productivity firms and industries has been identified as an important driver of productivity losses in developing countries (Hsieh and Klenow, 2009). Sources of such misallocations include domestic policies, institutions and market frictions that implicitly subsidize smaller and less productive firms.[1] Most markets in developing countries potentially suffer from some form of this distortion. The question then arises as to how reducing distortions in one market affects misallocation in linked upstream supplier and downstream customer markets. On the one hand, correcting for distortions in one small market can potentially have large spillover effects that result in improved allocation across the supply-chain. On the other hand, a second-best world offers no assurance of efficiency gains from selective interventions in one sector if uncorrectable market failures are present in linked markets. My paper examines how market power and the resulting markup distortions in linked markets produces different gains in response to a regulatory regime change. Specifically, I focus on the interaction of two deviations from an efficient economy: imperfect competition and regulations that restrict firm-size, both creating production bottlenecks in the supply-chain. In other words, this paper addresses two related questions: How do IO linkages amplify or attenuate the gains from reducing distortions within one market and what role does market power play in this transmission?

I leverage the removal of a policy that restricted firm size in a set of product markets in India as a quasi-natural experiment to shed light into the propagation of

[1]Examples of market imperfections that are costly for large firms include poor quality of the managerial delegation environment (Akcigit et al., 2016), frictions in financial markets (Midrigan and Xu, 2014; Gopinath et al., 2017), rent-seeking and unequal regulation enforcement (Almeida et al., 2011), markup dispersion (Peters, 2013)

distortions through IO linkages. This *reservation* of products for exclusive manufacturing by Small-scale industries (SSI) in India (i.e., establishments with plant and machinery value less than Rs. 10 million) is an example of a regulation that distorts the size-distribution of firms.[2] The main advantage of this setting is that the policy affected only around 20 percent of all products and a quarter of the establishments in India, leaving a large subset of firms to serve as control group as well as to identify the effects on linked markets. A growing literature finds such regulations reduce aggregate productivity by restricting the growth of productive firms (Restuccia and Rogerson (2008), Guner et al. (2008)). In addition, they fail to protect employment, deter investment and technology upgrades (Bertrand and Kramarz (2002)). Consistent with studies on the effects of such distortions, Indian reservation episode resulted in sub-optimal firm-size with substantial output and productivity loss in the reserved markets (Garcia-Santana and Pijoan-Mas (2014)). While removing distortions within these markets enhances its productivity, such improvements do not occur without large reallocations and displacements of inputs. From a policy perspective, it is therefore important to evaluate the incidence of productivity gains across the supply-chain. The goal of this paper is to provide such an evaluation.

My analysis of the ramifications of India's removal of reservation proceeds as follows. First, I analyze the effect of size distortionary polices on misallocation and productivity in directly affected markets. I do so by comparing the evolution of reserved and non-reserved product markets before and after the liberalization. The plausibly exogenous variation generated by the liberalization offers new evidence on the predominant role of misallocation in driving productivity using relatively weak assumptions.[3] Second, I evaluate the spillovers of this intervention, tracing how the deregulation propagates through the supply-chain. I disentangle the effects on directly linked markets exploiting the availability of detailed product-firm-level data on prices and quantities. I compare markets by the strength of their linkages to the reserved markets. Using a structural model of production, I isolate the markup component of product price from marginal costs and estimate heterogenous

[2]Such size distortionary policies are popular across the world including product market regulations that restrict size in the Japanese retail sector (Lewis, 2004), labor regulations that are only applicable for large firms in many countries (Garicano et al. (2016)), financial subsidies for Small and Medium Enterprises (SMEs) in Korea and India (Rotemberg (2017)). Even a regulation that applies uniformly to all firms within a market may generate misallocation. For example, tariffs applied to narrowly defined categories of goods will differentially affect importing firms relative to ones that source domestically. Martin et al. (2017), Tewari and Wilde (2018), Galle (2018) are other papers that use the Indian reservation episode to quantify the costs of size-distortionary policies on affected markets.

[3]Existing literature that estimates the effects of market distortions generally identifies misallocation by comparing productivity distribution between less developed and a counterfactual frontier of more developed country. Here, any deviation from the frontier is attributed to misallocation and ignores the intrinsic differences between countries or other supply and demand shocks that can drive the gap.

responses by firm productivity and market concentration. This firm-level evidence captures reallocation gains from supply/demand shocks that extend beyond output growth and price changes and illustrates the mitigating role of market power. Finally, I estimate the size of the multiplier effect on aggregate productivity growth from linked markets. I also decompose the productivity growth into within-firm productivity gains, improved allocation in the market and the efficiency loss from market concentration to understand the main mechanisms through which reducing distortionary policies operate.

To guide the empirical analysis, I present a framework where production is organized in a vertical supply-chain setting with three markets: upstream, reserved and downstream. Each market has a finite number of heterogeneous firms with endogenous markups. Firms in the reserved market also face an output distortion that limits their size. When this distortive policy is eliminated, output increases and prices decrease as production is shifted from smaller, less productive firms to larger, more productive firms who were previously constrained by the policy.[4] These changes in prices and output propagate via the supply-chain through two channels. First, distorted firms purchase less-than-optimal amounts of inputs, thereby depressing the sales of these upstream suppliers.[5] When this distortion is removed, factor demand increases leading to higher output and potentially higher prices in imperfectly competitive upstream markets. If the upstream firms respond heterogeneously, the resulting reallocation would amplify the productivity gains if inputs are shifted to more productive firms. The resulting price changes could also have feedback effects as the marginal costs of reserved firms change. Second, the fall in prices reduces the marginal costs of downstream customers that use the reserved product as an input in production. By reallocating inputs to more productive firms, reforms in linked markets may have a cascading effect that reduces misallocation in downstream markets as well. When downstream markets are perfectly competitive, these customers completely pass-through the cost reductions. However, imperfect competition could mitigate the reallocation gains when markup dispersion increases from incomplete pass-through. Thus, the overall gains from removing a regulatory bottleneck is theoretically ambiguous, and consequently requires an empirical exploration.

Using firm-level panel data from the Annual Survey of Industries, I show that eliminating size restrictions significantly increases output and decreases prices within the reserved markets. This policy change induces a positive demand shock for upstream suppliers and lowers input

[4]The reallocation from removing size distortions is driven by both an increase in the expected value of entry leading to a larger mass of firms in the economy (Bento and Restuccia (2017)) and the expansion of the most efficient firms, which in equilibrium improves the selection of producers in the market (Bartelsman et al. (2013))

[5]Downstream production bottlenecks acts like taxes on upstream firms' final output as identified by Jones (2011a,b). It reduces the value the firm receives from producing a given level of output.

costs for downstream customers. In both the linked markets, output growth is driven by larger and more productive firms when the markets are competitive. However, in concentrated markets, more productive firms raise their markups in response to the demand/supply shock, resulting in a substantial increase in aggregate markups and dispersion. Consequently, as market concentration increases, output growth is driven be relatively less productive firms in these linked markets. The increase in markups from incomplete pass-through also abates output growth in concentrated downstream markets. These results are consistent with a large class of models where more productive firms face lower demand elasticities and charge higher markups.[6]

Aggregating the nationally representative firm-level data to the product-level, I quantify the aggregate productivity implications of these firm-level responses and the resulting redistribution. Within reserved markets, the intervention led to productivity growth of 18% with 82% of this growth driven by reallocation of inputs to more productive firms and the remaining from within-firm productivity growth. Hence, reallocation stressed in heterogeneous firm models plays a dominant role here, relative to the channels stressed in homogenous firm theories (such as better incentives to adopt new technologies). Productivity increased by an average of 5% in upstream and -0.5% in downstream markets. These gains diminish with increasing market concentration as inputs are reallocated to initially low-markup, less productive firms. In addition, market-power induced distortions have feedback effects by reducing the productivity gains within markets that were directly affected by the reform itself. In total, this reform increased productivity of the manufacturing sector by 2.3% without accounting for linkages. With a competitive supply-chain, the growth in aggregate productivity is augmented by directly linked markets with an estimated multiplier effect of around 1.9 (i.e. productivity increases by 4.2%). However, these multiplier effects fully dissipate when linked markets are highly concentrated. The empirical estimates in this paper suggest that the cascading effects of improved allocation dominates the efficiency loss from variable markups when the supply-chain is sufficiently competitive.

Therefore, the overall gains from correcting for distortions in one market depends on the underlying market structure of the linked markets. Industrial policies that open up particular product markets can exacerbate the efficiency loss in linked industries by increasing the average markups and dispersion.[7] This paper is one of the first to explore this allocative efficiency channel in an industrial policy context, specifically focusing on transmission through demand and supply shocks induced by eliminating size distortionary policies. Given that many

[6] See Atkeson and Burstein (2008), Melitz and Ottaviano (2008) and Dhingra and Morrow (2015).

[7] See Dhingra and Morrow (2015), Arkolakis et. al (2015), Epifani and Gancia (2011), Edmond et al. (2015, 2018) and Holmes et al. (2014) for welfare implications of variable markups in international trade context.

developing countries adopt industrial policies that direct resources towards selected sectors, it is critical to account for the vertical linkages when deciding which sectors to promote. The findings here emphasize the importance of accounting for market-power induced inefficiencies in determining the overall gains from industrial-policy interventions. My results suggest that removing distortionary policies in parts of a sufficiently competitive production network can trigger reallocation of resources across producers in the supply-chain and lead to large overall gains in the economy.

The paper contributes to several strands of literature. First, the paper is related to the large body of macro-development literature on the misallocation of resources. This research finds that microeconomic distortions that induce misallocation of resources across firms and sectors are an important source of cross-country productivity differences (e.g., Banerjee and Duflo (2005), Hsieh and Klenow (2009), Restuccia and Rogerson (2008), Midrigan and Xu (2014)). My paper draws on this literature but shifts the focus to the spillover effects of a policy distortion that affects particular markets and highlights the role of market power in this transmission. Instead of treating distortions as a black box, I explicitly identify and study the interaction between two sources of prevalent distortions. More importantly, IO linkages are an important channel to explore because industrial development process requires the growth of entire distribution of suppliers and downstream producers, not just particular firms or product markets (Hirschman (1958)).[8] Even a regulation that is applied to all markets within the supply-chain may generate more misallocation within some relative to others, thereby creating bottlenecks.

My paper builds on the burgeoning literature that studies the transmission of distortions through IO linkages[9]. In related work, Liu (2018) looks at production networks under market imperfections where these distortions accumulate through backward demand linkages, thereby generating aggregate sales distortions that are largest in the most upstream markets. Recent work by Grassi (2017) studies the dampening effects of competition on transmission of idiosyncratic productivity shocks, while Baquee (2018) presents a framework where deviations from perfect competition affects the amplification of shocks by generating firm entry and exit across the network. In contrast, this paper empirically evaluates the role of market power in mitigating the inter-industry propagation of efficiency gains from alleviating a prevalent policy distortion. To the best of my knowledge, this is also the first paper to study both the upstream and downstream effects of market failures utilizing a unique natural experiment

[8]Modeling firms' behavior along a vertical channel has important implications for the analysis of price dynamics in the economy as a whole (Chevalier, Kashyap and Rossi (2003)) and for the passthrough effects of both foreign-trade policy (Amiti and Konings (2007)) and industrial policy (Lane (2017)).

[9]Key theoretical references include Basu and Fernald (2002), Acemoglu et al. (2012), Jones (2013), Bartelme and Gorodnichenko (2015), Baqaee (2017a), Caliendo et al. (2018)

that translates nicely to available firm balance sheet data and produces more direct empirical estimates. The setup is rich enough to test the impact on firm-level distribution and its aggregate productivity implications within directly and indirectly exposed markets. This provides a deeper understanding of how these frictions propagate and impact other firms, an aspect missing from the existing literature that uses aggregated IO tables to mainly estimate the role of firm-specific or sector-specific shocks in generating aggregate fluctuations.

An important contribution of my paper is to connect the above literature on propagation of distortions through IO linkages to the recent international trade literature that explicitly models the welfare implications of variable markups (see Footnote 7). In these models, a policy is welfare improving when inputs are allocated to firms with higher markups. Hence, markups are not only costly because they act like a uniform output tax, but the resulting markup dispersion misallocates factors of production. There are relatively few papers in the empirical literature that have tested the theoretical predictions of this class of models, and my paper addresses this gap.[10] My empirical evidence of increased misallocation in concentrated markets that face demand/supply shocks using disaggregated firm-level data is broadly consistent with the findings of these papers.

Finally, there has been renewed interest in studying firm market power, due to several secular trends such as increasing concentration and long- run rise in average markups (see DeLoecker and Eeckhout (2017)). Barkai (2017) and Autor et al. (2017) explain the secular declines in labor share from the increase in sector-level and firm-level concentration. Gilbukh and Roldan (2018) attribute the increase in dispersion at the top of the markup distribution to the decline in business dynamism. In this paper, I document increasing markup dispersion from deregulation in related markets. My paper also relates to the pass-through literature. De loecker et al. (2016) focuses on incomplete pass through of input tariff shocks that reduce the welfare gains (of lower prices and higher average productivity) from import competition in the output sectors. Relatedly, Amiti et al. (2012) find that the most productive firms import the most and also have the lowest pass-through. I find evidence of heterogeneous pass-through that lower the gains from industrial policy interventions.

The remainder of the paper proceeds as follows. Section 2 provides background on the Small-Scale Industry Reservation in India. In Section 3, I set up a stylized framework in a supply chain setting with heterogeneous firms and endogenous markups to provide intuition on

[10]Weinberg (2017) is the closest to my paper. He finds sectors using imported inputs become more misallocated relative to exporting sectors that face more competition from exchange rate appreciations in Chile. However, he does not find any significant effects from tariff changes, a policy distortion. Moreover, as emphasized by Restuccia and Rogerson (2013), changes in misallocation measured at business cycle frequency or exchange rate fluctuations need to be treated with extreme caution as these measures can be heavily driven by adjustment costs, and not a true measure of misallocation.

how the removal of distortions propagate through IO linkages and its aggregate productivity implications. Section 4 describes the data, the construction of the exposure measures, and proceeds to present the identification strategy. Section 5 presents tests of the main identifying assumptions. Section 6 analyzes the impact of the Indian reform across the firm distribution and surveys different theoretical mechanisms through which these patterns can be realized. Section 7 discusses the effects on aggregate productivity and provides empirical estimates. Section 8 concludes.

2. Background on the Small Scale Reservation in India

The reservation and de-reservation of products for exclusive manufacturing by small firms has several features that allow me to explore the transmission of policy distortions through IO linkages.

The small-scale industry (SSI) in India contributes almost 40 percent to gross industrial value-added and is the second largest employer after agriculture. The development of SSI has been an economic priority because of its potential to generate employment and a more equitable distribution of income. Promotion measures included subsidies for credit, energy and capital, technical assistance and excise tax exemption. With the aim of protecting SSI from competition and fostering labor-intensive growth, India implemented a policy of reservation of certain products for exclusive manufacturing by SSI firms in 1967, i.e. only firms with historic plant and machinery value of less than Rs. 10 million were permitted to operate in these product markets.[11][12] The number of reserved products had grown from 47 products to over 1000 products in 1996. Production of these reserved items accounted for about 13 percent of total manufacturing output in India in 1989 (Mohan, 2002). The choice of products to be reserved and the timing of dereservation was itself reportedly arbitrary according to the policy notes (Hussain (1997), Mohan (2002)). The only criterion was the ability of SSI to manufacture such items. Hence, the number of reserved products and the timing of removal varied greatly across industries. Figure 1 shows the trends of dereservation episodes as well the variation in reservation across industries.

The timing of the elimination of these reservations alleviates concerns of other large-scale economic reforms around the time driving the effects. India began economic liberalization in the 1990s following an economic crisis in 1991 and this included the widely studied episodes of delicensing and trade liberalization. One of the last remnants of the protectionist era was

[11] Firms could expand beyond this threshold if they export at least 50% of their output. Also, firms that exceeded this threshold prior to the policy implementation could continue production but were restricted from expanding.

[12] This capital threshold expanded over the 30 years until it reached the 10 million limit in 1997.

this reservation. Growing concerns about SSI's ability to compete with imported goods and produce high-quality goods that meet growing consumer demands nudged the dismantling of this policy.[13] Beginning in 1997, the products were dereserved in a staggered manner with the pace accelerating to around 253 products dereserved at the end of 2007 and the final set of 20 products being dereserved in 2015. My analysis using this regulatory reform mainly utilizes a difference-in-difference strategy. This methodology requires that there are no differences in trends of outcome variables of interest between the control and treatment groups prior to the reform (i.e. parallel trend assumption). Existing literature studying these dereservation episodes in other contexts lends confidence to the plausible exogeneity of the reform itself. Martin et al. (2017) and Tewari and Wilde (2016) find no evidence of differences in pre-trends for output growth, labor growth and product variety at different levels of aggregation. Following the reform, output and employment increased in these markets due to increased entry and expansion of firms that were previously constrained by the capital threshold. More relevant to this paper, Galle (2018) finds a fall in markups post dereservation without any pre-trends, resulting in pro-competitive gains. Singh (2018) provides evidence of a significant fall in output prices both in short-term and medium-term post reform (using the CMIE Prowess data of only publicly listed firms). This fall in prices could be driven by both increasing competition and increase in quantity supplied. My paper leverages the dereservation episode to study the spillover effects of such distortions on linked markets.

The dereservation provides us with a rich variation in policy-induced misallocation across time and industries. Availability of detailed firm-level data allows me to identify both the distributional effects and the channels through which distortionary policies lead to lower aggregate productivity while controlling for firm and industry fixed effects, as well as state and product-specific trends. As this paper also studies the suppliers' and customers' reaction to such policy changes, it exploits the variation in upstream and downstream exposure as well. Of the 1300 reserved products, nearly 50 percent of the products (658 products) were reported as being used as intermediate inputs in production, helping identify the downstream effects. Of the firm-input observations, 27 percent of the observations are from the reserved product markets. In the upstream, 40 percent of never reserved firm-output observations supply inputs to the reserved markets. I find no pre-trends in output or prices in linked markets prior to the reform. Finally, with price and quantity data for both inputs and outputs at the detailed product-firm-year level, I back out markups and productivity using the production-function approach to study the reallocation gains and role of market power.

[13] Reservation was intended to protect SSI from competition. But, with the possibility that large undertakings could operate in the market under certain conditions, there was also concern of market structure tilting to a dominant firm with competitive fringe, creating deadweight loss.

This Indian case thus offers an excellent setting to explore the role of linkages in bolstering the effects of deregulation.

3. Conceptual Framework

Before presenting the empirical analysis, l describe a simple partial equilibrium model to build intuition on how a bottleneck in the supply chain can have distributional consequences when market structure is heterogeneous across related markets. Without loss of generality, I assume there are only three levels of imperfectly competitive markets that are combined in sequence to make a final good: an upstream supplier market u, intermediate reserved market r facing size-restrictions and downstream customers d, and where both basic inputs and final retailing are separated from firms that integrate intermediate inputs within the supply-chain.[14] My goal is to show how eliminating size-restrictions in R can transmit upstream and downstream, with ambiguous responses. With this aim in mind, I combine insights from canonical model of supply chain proposed by Spengler(1950) with variable markups. The network of interest is fixed (i.e. downstream firms transform the upstream good in fixed proportions into a final product) and substitution effects from outside this network may be ignored.[15] Finally, there is no monopsony power whereby downstream firms do not exhibit market power in the upstream market.[16]

Intermediate Goods market

The production side of the economy consists of m intermediate goods markets, each with a finite number of firms. Here, I employ a few simplifying assumptions. Firms are profit maximizing and labor is the only primary input. The firm e in market i combines labor L and other market goods x_j to produce q_{ie} units using the following Cobb-Douglas production function:

[14] I expect the transmission mechanisms proposed in the paper to work by the same channels through higher order linkages. For clarity and brevity, I focus only on the first order linkages. Investigating the effects across the whole economy is a subject for future research, although it would be complicated by the Leontief inverse matrix mixing together ω's from all sectors.

[15] Here the IO linkages are interpreted as technology. I abstract from general equilibrium considerations that would introduce interactions in factor markets across supply-chain. See Goulder and Williams III (2003) to derive empirically implementable formulas for incidence in the presence of pre-existing distortions in all other markets and formulate the general equilibrium effects.

[16] I find no evidence of monopsony power in this setting. Additionally, this is a standard assumption in the literature on vertical oligopolies, e.g. Ghosh and Morita (2007). This property approximately holds if the upstream sector serves a large number of independent downstream markets so that a quantity change by a firm in one of these downstream markets has a negligible effect on the price of its input.

$$q_{ie} = z_{ie} L_{ie}^{1-\gamma_i} (\prod_{j=1}^{m} x_{ije}^{\omega_{ij}})^{\gamma_i} \qquad (1)$$

where x_{ije} are the intermediate goods from market j used by firm e in market i with ω_{ij} capturing the fixed input share of market j's goods needed to produce market i's production, z_{ie} is the firm-specific hicks-neutral productivity and $\gamma_i = \sum_{j=1}^{n} \omega_{ij}$ (i.e. production function exhibits constant returns to scale).[17] Market real output is given by $Q_i = (\sum_{e=1}^{E} q_{ie}^{\frac{\sigma_i - 1}{\sigma_i}})^{\frac{\sigma_i}{\sigma_i - 1}}$. Labor supply is exogenous and labor market clearing is given by $L = \sum_i \sum n L_{ie}$.

In this framework, as larger firms are more likely to be regulated and penalized, the cost of reservation can be approximately written as an output distortion:

$$\max \ \pi_{ie} = (1 - \tau_{ie}(q_{ie})) P_{ie} q_{ie} - w_i L_{ie} - \sum_{j=1}^{m} P_j x_{ije}$$

where $\tau_{ie} \geq 0$ if a firm-specific wedge that distorts output and is increasing in its quantity.[18] Solving for firm's problem, the price-setting rule is standard markup over marginal cost with the firm-specific elasticity of demand $\epsilon_{ie} = -\frac{\partial q_{ie}}{\partial P_{ie}} \frac{P_{ie}}{q_{ie}}$ and markup elasticity $E_{ie} = \frac{\partial \epsilon_{ie}}{\partial p_{ie}} \frac{p_{ie}}{\epsilon_{ie}}$[19]:

$$P_{ie} = \underbrace{\frac{\epsilon_{ie}}{\epsilon_{ie} - 1}}_{\text{Markup}} \ \underbrace{\frac{1}{1 - \tau_{ie}}}_{\text{Policy distortion}} \ \underbrace{[(\frac{w_i}{1 - \gamma_i})^{1-\gamma_i}][(\prod \frac{P_j}{\omega_{ji}})^{\omega_{ji}}]^{\gamma_i} [\frac{1}{z_{ie}}]}_{\text{Marginal cost}} \qquad (2)$$

There are three important takeaways from this solution. Firstly, without a size distortionary policy, more productive firms producing a differentiated good with a lower marginal cost will produce more output within a market. Second, larger policy distortions or market power would result in firms charging a higher price and producing less output. Finally, a positive τ exerts a disproportionately higher penalty on high-productivity firms. The key result is that size-constraints create a production bottleneck by restricting growth of productive firms. A point to note is that these distortions are not taxes - there is no revenue to rebate - rather frictions that prevent firms from equalizing marginal costs and marginal products.

Let $\phi_{ie} = \frac{1 - \tau_{ie}}{\mu_{ie}}$ be firm-specific wedge between its marginal cost and marginal revenue where μ_{ie} is a firm-specific markup that captures the market-power of the firm. Here, high

[17] Labor can be thought of as encompassing all the primary inputs including capital K_{ie} with the following setup $L_{ie}^{\alpha_i} K_{ie}^{1-\alpha_i}$. I suppress capital to simplify the notation.

[18] τ_{ie} is continuous function that does not exactly capture the policy studied in this paper. However, the policy was not a rigid cutoff as observed in both the degree of flexibility in imposed capital constraints and the resulting firm-size distribution. In addition, Hsieh and Olken (2014) find no bunching at the size threshold for other regulations as well.

[19] E is positive (negative) when demand is strictly convex (concave) and zero for linear demand or CES preferences. Markup elasticity of 0 implies invariant markups.

(marginal) product can be indicative of both market power and constraining output distortions. A producer with monopoly power may produce less than the efficient level and charge a higher markup. An increase in ϕ_{ie} such that $\phi_{ie} \rightarrow 1$ indicates lower distortions, either through lower τ or markups, and results in higher output.

Testable Prediction 1 - *(Reserved markets): Removal of size-restrictions will reduce output distortions τ_{re}. This reallocates production towards the larger, most productive firms that were constrained by the policy. Market-level aggregate output increases and prices decrease.*

The above prediction is conditional on markup distortions remaining exogenous or endogenously decreasing. The effects on markups are theoretically ambiguous as competition could increase or decrease within the market. Existing empirical evidence and the institutional context points to increased competition once the size-constraints are removed (Galle (2018) and Singh (2018)). This comes from increased entry of large firms as well as the growth of constrained firms (see Appendix).

With constant returns to scale, market-level marginal cost is also equal to the average cost $mc_i = \sum_{e=1}^{E} mc_{ie}s_{ie}$. The aggregate wedge for market i ϕ_i is given by:

$$\phi_i = (\sum_{e=1}^{E} \phi_{ie}^{-1} s_{ie})^{-1} \qquad (3)$$

The aggregate wedge is a revenue-weighted harmonic mean of firm-level markups and output distortions where $s_{ie} = \frac{p_{ie}q_{ie}}{P_i Q_i}$ is the market share of the firm. Here, the price index of market i, P_i is given by:

$$logP_i = log\phi_i + logw_i + \sum_{j \neq i} \omega_{ji} log(\phi_j mc_j) \qquad (4)$$

Therefore, when production involves more than one vertically-related step, the relationship between costs and final prices will depend on the combined effect of the separate distortions faced and markups set by the vertically-related firms. This is commonly referred to as "double-marginalization" in the supply-chain literature. If upstream and downstream markets are close to perfectly competitive (i.e. $\mu \rightarrow 1$) and there are no policy distortions (i.e. $\tau \rightarrow 0$), these higher-order effects can be ignored. Otherwise, when linked markets are themselves distorted, then aggregate effects from removing distortions in one market would depend on the responses of these linked markets.

To capture the upstream demand and downstream supplier effects, I generalize the notation as follows. An individual producer in each vertically linked market faces an inverse demand function given by p where:

$$p = P(q, \alpha)$$

where p is the price, q is quantity, α contains exogenous variables that affect demand such that $P_q < 0$ and $P_\alpha > 0$. Each firm faces a constant marginal cost $c = C(z, \eta)$ where z is the unobserved productivity and η is the intermediate input costs with $C_z < 0$ and $C_\eta > 0$.

Taking the uniform input prices as given, the resulting first-order condition for profit maximization is:

$$\frac{d\pi_{ie}}{dq_{ie}} = \underbrace{P(q, \alpha) + P_q(q, \alpha)q}_{\text{Marginal revenue}} - \underbrace{c}_{\text{Marginal cost}} = 0$$

where $q = Q(\alpha, z, \eta)$ is firm output. The usual regularity conditions imply that marginal revenue is decreasing in q and increasing in α.

Upstream effects

Aggregating the profit maximization condition for firms in reserved market r:

$$P^u = \gamma_r \omega_{ur} (\phi_r \frac{P^r Q^r}{x_{ru}})$$

When $\phi_r \to 1$, there is a positive demand shock for upstream markets u whereby the inverse-demand function P^u shifts out as sales in reserved market increases. Let α be the firm-specific demand shock for upstream suppliers such that $P^u_\alpha > 0$. Then,

$$q^u_\alpha = -\frac{MR^u_\alpha}{MR^u_q} = -\frac{P^u_\alpha + q^u P^u_{q,\alpha}}{P^u_q(2 - E^u)}$$

Firms respond heterogeneously to their demand shock depending on their elasticity of demand and demand curvature, with firms facing less elastic demand and higher markup elasticity having smaller output growth.[20] Correspondingly, firm-specific markups would also change:

$$\mu^u_\alpha = \underbrace{\frac{P^u_\alpha}{c^u}}_{\text{Direct effect}} + \underbrace{\frac{P^u_q q^u_\alpha}{c^u}}_{\text{Indirect effect}}$$

The first term captures the positive direct effect on prices via a shift in the demand function while the second term is the negative indirect effects from an increase in production that decreases prices. I assume that marginal costs are fixed. Hence, the effects of demand shocks

[20]The sufficient condition for this to hold is the log-concavity of demand as well as the slope of inverse demand should be non-decreasing in α.

on markups are theoretically ambiguous. Markups will increase as long as the ratio of the elasticities of the inverse demand function and of output both with respect to demand shock is larger than the inverse of markup elasticity. Here, market power channel has important implications for output and prices. Under many demand systems, productive firms face lower demand elasticities and hence feature higher markups and markup elasticity. This would then imply higher markup increase for larger and more productive firms in response to a demand shock. In addition, if a demand shock affects firm's demand elasticity, this can potentially generate feedback effects on the markup and markup elasticity.

Testable Prediction 2 *(Upstream Markets): When distortions in the reserved market decreases, upstream suppliers experience a positive demand shock. Output increases. More productive firms will increase prices relatively more than less productive firms.*

Note the prices will increase if and only if the price elasticity falls and there is no firm entry. These effects will be stronger in upstream markets that are more exposed to reserved markets as inverse demand shifts out furthe.[21] If market shares capture the market power of firms (as in Atkeson and Burstein (2008)), then larger, more productive firms will have higher market power in more concentrated markets where they face less elastic demand. These firms would respond to the demand shock by increasing markups more and consequently have lower output growth.

Downstream effects

When $\phi_r \to 1$, there is a uniform input cost shock for downstream customers d as price of intermediate inputs decreases (see Equation 4). Summarizing the size of input cost shock by η, downstream customers have the following markup response:

$$\mu^d_\eta = \mu \frac{C^d_\eta}{c^d} + \frac{P^d_q q^d_\eta}{c^d}$$

The first term is the direct effect from reducing marginal costs and is positive. The second term is the indirect effect of increase in production that reduces prices. Again, the effect on markups is theoretically ambiguous in this case as well. However, there is consistent evidence in the literature of increasing markups indicating incomplete pass-through. The pass-through elasticity of a cost reduction for d is given by:

$$\frac{\partial p^d}{\partial p^r}\frac{p^r}{p^d} = (\frac{1}{1 + \frac{E^d}{(\epsilon^d - 1)}})\frac{\omega^{dr} p^r}{\omega^{dr} p^r + w^d}$$

Firms facing lower demand elasticity and higher markup elasticity would have incomplete

[21]This is in line with the centrality measure emphasized by Brassi (2018), Baquee (2018) and Acemoglu et al. (2012), whereby markets that are important customers or suppliers will have large reallocation effects.

pass-through. They would charge higher markups and exert market power via final consumers with uniform intermediate input cost reductions. For example, if indeed demand elasticity decreases with firm productivity, then a cost shock could lead to markup adjustments and lower pass-through for more productive firms. See Appendix C for proof using the Atkeson and Burstein (2008) model. This heterogenous cost-pass through then logically generates heterogenous quantity response to a supply shock.

Testable Prediction 3 *(Downstream Markets): When distortions in the reserved market decreases, downstream marginal costs decreases. Output prices decrease while quantity increases. A reduction in intermediate input prices increases markups due to incomplete pass-through with stronger effects for more productive firms.*

Again, the effects for downstream markets increase with exposure to the reserved market, measured by ω_{dr}. The markup variations could lead to efficiency loss within the market if the dispersion in markups is large enough to reallocate production to initially low markup firms because they increase markup by less. The framework in Appendix C in which marketshares determine markups would suggest that more concentrated markets will see larger markup dispersions. In the next subsection, I show how any changes in the markups and the resulting markup dispersion in linked markets can diminish the aggregate productivity gains.

Aggregate productivity implications

The impact of reducing distortions on aggregate TFP can be broken down into two components as in Chari et al. (2007). First, some firms become more productive and increase their output given the initial distribution of resources, thereby increasing the average productivity.[22] This can be interpreted as the reduction in labor wedge between the marginal product of labor and the real wage that acts like an input tax and reduces output relative to the efficient level. Second, the distribution of resources across producers shifts in response to the reform, leading to reduced output for previously over-producing firms and increasing output for previously under-producing firms. This change in allocative efficiency wedge that is a result of misalloaction of inputs also affects aggregate TFP. In particular, variable markups is important because of its implications for allocation. Recall, *Labor wedge* ϕ_i for market i is given by:

$$\phi i = (\sum_{e=1}^{E}(\frac{\mu_{ie}}{1-\tau_{ie}})s_{ie})^{-1}$$

Then, as in Hsieh and Klenow (2009), any policy that increases the dispersion marginal products reduces the aggregate TFP by increasing the dispersion of revenue productivity (TFPR):

[22] Within-firm productivity growth could arise from optimizing product mix, technology adoption, economies of scale etc.

$$TFPR_e = P_e z_e = P_e \frac{Q_e}{L_e^{1-\gamma} x_e^{\gamma}} \propto (\frac{MPRL_e}{1-\gamma})^{1-\gamma} (\frac{MPRM_e}{\gamma})^{\gamma} \quad (5)$$

where $MPRL_{ie}$ denotes marginal revenue product of labor and $MPRM_{ie}$ denotes marginal revenue product of materials, both derived from Equation (1):

$$MRPL_{ie} = (1-\gamma_i)\frac{P_{ie}q_{ie}}{L_{ie}} = w_i \frac{\mu_{ie}}{1-\tau_{ie}} \text{ and } MRPM_{ie} = \gamma_i \frac{P_{ie}q_{ie}}{X_{ie}} = p_j \frac{\mu_{ie}}{1-\tau_{ie}}$$

Market frictions distort the use of sectoral inputs yielding higher marginal products and resulting in less-than-optimal output. $TFPR$ summarizes the impact of distortions on an establishment with higher revenue productivity than industry average indicating a higher level of distortions. With a constant-elasticity-of-substitution aggregation:

$$TFP_i \propto [\sum_e (z_{ie} \frac{\phi_{ie}}{\phi_i})^{\sigma-1}]^{\frac{1}{\sigma-1}}$$

where σ is the elasticity of substitution between different varieties. If the increase in output across the supply-chain is driven by reallocation of inputs to more productive firms (i.e. $\phi_{ie} \to 1$ for most productive firms), then aggregate TFP increases. However, as previously shown, firms in linked markets could respond by changing their markups. If the increased markup dispersion is driven by most productive producers and a less-than-optimal share of inputs is allocated to them, TFP gains will diminish. That is, if the reallocation of inputs across the supply-chain is driven by increasing market share of profitable firms who are also more productive, then the productivity gains would be smaller.

4. Empirical Strategy

In this section, I present the data and the empirical specification to quantify the first-order effects of liberalization. First, I identify the effects on output prices and quantity in reserved market. Then, I proceed to describe the methodology for identifying upstream and downstream firms and present evidence that confirms our predictions of transmission to downstream markets as a supply shock and to upstream markets as a demand shock. Finally, I describe the methodology for calculating markups and productivity that will be used to study the distributional effects.

Data The empirical analysis mostly relies on the Annual Survey of Industries of India (ASI) from 2000-2001 through 2012-2013.[23] The ASI is a repeated cross section representative

[23] I will refer to these years as 2000 through 2012 hereon. ASI uses the accounting year, which runs from April 1st to March 31st. For products de-reserved towards to end of an account year, I do not count these

of formal establishments. with large establishments surveyed each year and smaller establishments surveyed with a probability that depends on their specific state and 4-digit industry block, with a minimum sampling probability of 15%.[24] Recent changes in policy have allowed researchers to track establishments who were sampled multiple times, thereby creating a panel version of the ASI that goes back 1998. I use this panel version for the establishment-level analysis and aggregate the firm-level data using the sampling weights for the product-level analysis. [25] Finally, using the panel version of the data to examine misallocation mitigates concerns about measurement errors as well as the potential for adjustment costs to generate the cross-sectional dispersion in the marginal product across firms rather than idiosyncratic policy distortions.

The basic unit of observation in the ASI is an establishment. I treat each establishment as a separate firm as most establishments are the only plant in their firm.[26] The main advantage of the ASI is that establishments report products they produce and inputs they use at the detailed 5-digit product classification (ASICC), which has about 5,500 possible products comparable to the 5-digit SIC product definition collected for U.S. manufacturing plants and is at the level of product reservation. Each establishment reports the quantity of the product it uses and produces and its respective value (before taxes and distribution expenses) which can be used to compute prices. Each product quantity is supposed to be reported for a standardized unit (kilograms, numbers, etc.) allowing for comparison across firms within product markets and easier aggregation.[27]

The dataset contains around 30,000 establishments per year with 385, 873 unique firm-year observations in total. For these establishments, we have a total of 1,436,347 raw material input entries, which is 3.72 entries per firm on average over the entire period. These input-using

products as de-reserved until the following year.

[24]Formal establishments include manufacturing plants employing twenty or more workers and not using electricity, or employing ten or more workers and using electricity. Large establishments are those with 200 or more workers until 2003-2004, and 100 or more since then.

[25]Firm-level regressions may not be ideal because smaller establishments are sampled less frequently than larger ones. However, 57% of the establishments observed making a reserved product have at least one observation both pre and post de-reservation. I use these incumbent firms for the firm-level DID estimates ensuring the composition of individuals of the two groups remain unchanged over time. Secondly, there is no reason to expect that there is differential sampling by firm size between reserved and never-reserved markets. Furthermore, the SSI cutoff is capital-based whereas the sampling frame is labor-based. Hence, I still observe firms that have less capital but a large labor more frequently. Finally, I complement the analysis with product-level analysis and the results remain robust.

[26]ASI contains very little information about each establishment's parent firms. Less than 5% of the firms do joint returns.

[27]Another project that uses this information is Kothari (2013). In the ASI, all plants reporting a certain product are supposed to report quantities in the same units. However, there are clear cases in which plants are misreporting quantity units. I use a modified version of his algorithm to address the misreporting issues. More details in the Appendix I.B.

firms also report data on sales quantities and values with 573,595 output entries, which is 1.5 entries per firm on average. 94% of the firms report using more than one input and 43% of the firms sell more than one output.

Data on the dereservation dates and their respective product list is readily available from the Ministry of Micro, Small and Medium Enterprises.

Specification

EVENT STUDY DESIGN

I now describe the empirical strategy to test the dynamic effects of dereservation. To start, I am mainly interested in studying the overall quantity and prices effects as this will confirm if dereservation had a substantial impact on the product markets. I start with the following event-study on dereservation equation of the following form where V_{iet} is the log of outcomes (unit prices, output, revenue, markups, market shares) of market i in establishment e in year t:

$$V_{iet} = \alpha_0 + \sum_{\tau=-4}^{5} \beta_m 1[t = E_{iet} + \tau] + \alpha_{ie} + \alpha_t + \epsilon_{iet} \qquad (6)$$

I define the year at which the establishment e is first exposed to the dereserved product market directly or indirectly as E_{iet}. I also bin up the end-points and normalize $\beta_{-1} = 0$. The control group serves to absorb any secular trends in demand/supply side of the Indian economy. Aggregate product-level output is constructed using reported output at the establishment-level and applying the sampling multipliers to all infrequently sampled firms. For the event-study specification, I restrict the sample to a balanced sample of incumbent plants, which includes all plants whose main product category was reserved prior to reform.

DIFFERENCE-IN-DIFFRENCE SETUP

$$V_{iet} = \alpha_0 + \beta_{iet} Dereserv_{it} \text{ x } Exposure_i + \alpha_{ie} + \alpha_t + \epsilon_{iet} \qquad (7)$$

$Dereserv_{it}$ is an indicator that that equals 1 if the product is exposed to the dereserved market directly or indirectly (through IO linkages). All regressions include Firm- Product fixed effects and Year fixed effects and weighted by the inverse sampling weights, provided by the ASI. $Exposure_i$ is a measure of the upstream/downstream exposure to the reserved markets and is determined using the pre-dereservation firm-level input-output correspondence table.[28] For upstream and downstream markets, I account for demand side shocks that could be correlated with dereservation by eliminating firms that are selling products on

[28] $Exposure_i = 1$ for the reserved product markets.

the output side in the reserved market. Errors are clustered at the firm level to adjust for heteroskedasticity and within-firm correlation over time. For the aggregate product-level analysis, I run the same regression as above but with product fixed effects instead of firm fixed effects weighted by the initial labor shares and errors clustered at the product-level.

Identifying IO linked markets

To study the transmission through IO linkages, I need to identify firm-outputs that are made from reserved inputs for downstream customers and vice versa for upstream firms. Input-output table at the detailed product level data is not available. Firm-level data lists inputs used and products sold separately. Therefore, I construct a binary input-output correspondence that specifies whether an input is used in the production of a particular output or not by using data only from the single-product firms. This is based on the assumption that all the inputs that single-product firms report to use enter the production of this single output. I map this correspondence to the multi-product firms.

A natural measure for a market's exposure to the reform is its share of input costs from reserved markets to total costs in the case of downstream effects, and the share of upstream sales that is used as inputs in manufacturing by reserved markets. Using the approach in Acemoglu et al. (2015) and leveraging the detailed product -level data at the firm-level, I construct measures of upstream and downstream exposure to reserved markets. To account for the fact that I do not observe every plant in India in every year, I combine the samples from every year, and for each firm keep its most recent pre-dereservation observation. Given the design of the ASI, this should reflect a census of all manufacturing firms, albeit a census taken over several years (since there are 6 years of pre-program data for most of the reserved products and with a rotating sampling frame, each existing firm should have been surveyed at least once in the period). All the exposure measures are calculated prior to the first episode of dereservation for the relevant market.

For upstream markets, I measure exposure of product market u as the lagged share of input usage that goes into reserved product markets r. Here, $Sales_u$ refers to the sum of sales for r from output that is directly consumed as intermediate inputs by downstream firms with $Sales_{ur}$ going into the reserved product market:

$$Exposure_u^{Upstream} = \sum_r \frac{Sales_{ur}}{Sales_u}$$

Average exposure of upstream markets is 25% with a median of 8%. For downstream markets, more exposure to reserved supplier market r should be more inhibiting for firms in downstream markets d. I utilize similar method as above to determine the exposure for downstream markets d:

$$Exposure_d^{Downstream} = \sum_r \frac{Sales_{rd}}{Sales_d}$$

Average exposure of downstream firms is 16% with a median of 6%. This measure can be calculated at both the firm-level and at the product-market level. I use the firm-level data for the main analysis and provide robustness check using the product-market level measure. Around 10% of firm-product observations are in both markets (i.e. R and D, R and U, U and D) with 3% of observations in all three markets. For the empirical analysis of linked markets, I drop observations that also belong to the reserved market.[29]

Note that downstream and upstream industries could be exposed to multiple reserved products with different dereservation dates. I sum up the exposure to all reserved products prior to the first dereservation episode. Secondly, the upstream and downstream exposures are fixed at the pre-dereservation era so the effects by exposure are simply a function of shocks in connected markets working through a pre-determined IO network. This assumption is reasonable in the short to medium-run where elasticity of substitution between different inputs is close to 0 (see Boehm et al. (2018) for example). Furthermore, aggregate IO tables have not shown significant modifications in the long-run. Finally, I standardize the exposure variables so that a unit increase corresponds to a one standard-deviation change in the positive direction (i.e. more exposure).

Production function estimation and markups

The analysis of distribution effects and the resulting allocative efficiency gains would require estimates of markups and productivity.[30] I compute time-varying, firm-product level markups using structural methods developed by De Loecker and Warzynski (2012) and De Loecker et al. (2017). These estimates combine plant-level production data with assumptions on firm cost minimization to back out marginal costs as the difference between the price and markup. The main advantage of this methodology is that it does not require functional-form assumptions about preferences or the competitive environment. Here, a firm's first order condition under Cobb-Douglas production function implies that the plant's markup equals

[29] Around 34% of observations are purely in control group. Given that 40% of the establishments are multiproduct, then it is possible that multiproduct firms react differently post-deservation. Tewari and Wilde (2018) shows the effects to be stronger for multiproduct firms as these firms are able to reoptimize their product scope.

[30] Markups informs us that the margin of revenue over variable costs has increased. It does not necessarily imply that firms are making higher economic profits, a necessary condition for market power. For example, if a technological change increases the fixed cost but reduces the variable costs, then the rise in markups would not imply higher market power. I find lower volumes with higher markups only in more concentrated markets, lending confidence to a market power interpretation

the output elasticity of a variable input like materials divided by the revenue share of that input:

$$\mu_{iet} = \frac{\theta_{iet}^x}{\alpha_{iet}^x}$$

where θ is output elasticity of materials and α is cost share of materials. Intuitively, conditional on the output elasticity of materials, firms that spend a higher share of their revenue on materials set lower markups. The first step is to estimate the output elasticity of a variable input. Output elasticities are estimated by proxying for unobserved productivity with the firm's demand for material inputs (Levinsohn and Petrin, 2003; Ackerberg, Caves and Frazer, 2015) and a flexible translog, gross-output production function. TFP is the residual of this estimated production function. Details are presented in Appendix I.D.

Table I.D.2 shows the production function coefficients and the median markup by NIC 2-digit industry. The median markup for the whole sample is 21%, consistent with past estimates. In the first step, I establish that higher markups are correlated with both higher TFP, higher market share and more employment. Lower marginal costs are correlated with higher TFP as well as higher market share. Finally, using the regression setup of Equation (7), I find that downstream markets report a significant fall in marginal costs while upstream markets report a significant rise in markups with no changes in marginal costs, giving confidence to the proposed transmission mechanism.

5. First-stage effects

Effects on reserved markets

Firstly, I present evidence on the impact of dereservation on both output and prices in the dereserved markets using the event-study specification. Figure I.1 demonstrates an increase in output with a corresponding fall in output prices following the reform. As soon as dereservation is enforced, there is an immediate and significant decline in prices with output growing more slowly. In subsequent periods, the changes in output prices and sales becomes stronger reflective of the time needed for establishments to expand and enter a new product market. These results are in line with the theoretical prediction of fall in prices, driven by both increasing competition and fall in distortions.[31] Finally, there are no significant differences between treated and control group prior to dereservation, lending confidence to the identification set-up.

Next, I aggregate the outcome variables to includes all establishments in a given year.

[31] In the appendix, I present results of falling HHI within the dereserved product markets that suggests increased competition

Aggregated product-level effects of dereservation are presented in Table I.1. De-reservation leads to an average increase of 32% in quantity and 24% increase in revenue. The fact that quantity increased more than revenue suggests that prices fell by nearly 8%, consistent with the estimates of input prices reported by downstream firms as well the firm-level evidence presented in Figure 1. These large effects are all significant at the 10% level. Secondly, the increase in labor is less than the increase in output, leading to a increase in labor productivity. Finally, there is substantial growth in the number of establishments operating within the dereserved market. These results suggest that dereservation lead to the entry and growth of more productive firms, providing support for my hypothesis that reservation reduced misallocation.[32] The magnitudes reported here are similar to results presented in Martin et al. (2017), although I use a longer sample that captures the medium-run effects,[33] These results are robust to the inclusion of statexyear FE as well as NIC3digit x year FE.

Impact on IO linked markets

Theoretically, the increase in output and decrease in output prices for reserved markets, reduces the marginal cost of downstream sectors and acts as a supply shock. As output increases, there is increasing demand for intermediate inputs resulting in a demand shock for upstream sectors. In this section, I verify the propagation of this reform episode through IO linkages holds empirically.

UPSTREAM SUPPLIERS: First, I look at the upstream markets identified as inputs by establishments producing a single reserved product. When I classify an establishment as making an output that is used by reserved market as input, the output could go to multiple reserved markets with different years of dereservation. Hence, in the event-study specification, the indicator is equal to 1 starting from the first year of deresrevation.[34] I find an increase in output price and quantity post-dereservation in the upstream market. These effects become stronger over time and there is a jump in coefficients three years after dereservation, in line with increased entry and falling HHI observed in the reserved market. Again, the magnitudes are consistent with the effects reported by reserved markets as well, lending support for

[32] Exit is difficult to measure in this dataset because of the rotational panel.

[33] Their analysis uses data from 2000 - 2007. With most of the dereservation episodes happening between 2006 - 2008, they only capture the immediate effects. Additionally, their analysis is at the firm-level contrary to my analysis at the firm-product level. This can explain the relatively smaller magnitudes in establishment growth and a fall in labor productivity they observe.

[34] This seems reasonable as most of the dereservation over a span of 5 years between 2003 and 2008, hence there is substantial overlap in exposure to the reform. In addition, the results are robust to alternate specification such the using dereserved year of the main customer market and median dereservation year for each product market. Results are available upon request.

evidence presenting using this data and methodology.

In the event-study specification, I imposed a dummy variable i.e. any non-zero exposure to the dereserved sector would be identified in the treatment group post-dereservation. I pursued this approach because of concerns with misreporting. Here, I study an alternate measure of treatment that estimate the effects by the intensity of exposure. This allows me to ensure results from the event-study is robust to accounting for exposure.

The coefficient of interest is the interaction of log measure of $Exposure^{Upstream}$ with the $UpDereserv$ dummy that equals to 1 from the year the first reserved customer market is dereserved. For the upstream markets, a 1 S.D. increase in exposure to reserved market (i.e.18% of intermediate input sales is to the reserved market) leads to 6% increase in sales with 2% resulting from increase in prices and the rest driven by increase in quantity at 4%.

DOWNSTREAM CUSTOMERS: The first step for downstream market is to determine the effects on input prices and quantity consumed. For this purpose, a dummy variable on dereservation is a sufficient measure because this is a direct supply shock and we have information on the product-wise input prices and quantity. I compare values or prices of inputs have been dereserved to control group of inputs that have no history of dereservation before and after the reform episode.

I need a comparable control group of firm-input observations to test the effects on input prices and quantity. For this, I do a matching between treated and untreated inputs at the firm-input level. In the matching, I verify that the treated inputs in treated firms are similar to treated inputs in control firms on two dimensions: same trend in input use in terms of quantity and value and belong to same 3-digit NIC industry. In addition, by including firm - input fixed effects, I account for observable and unobservable sources of heterogeneity at the firm-input level that may affect raw material input usage. My aim is not to explain input levels, but instead study changes in input usage from dereservation.

I find a 9% fall in input prices reported by downstream firms once the intermediate input is dereserved. The effects become stronger over time and are similar in magnitude and trend to the output prices reported by the reserved markets. Looking at input quantities, there is a 24% increase in input quantity consumed by downstream firms. These results give confidence that the data quality is consistent across the vertical chain as the input trends reported by downstream markets matches the output trends reported in reserved markets.

Given that firms report fall in input prices and increase in input consumption, then one can expect changes in the prices and quantity produced as well. If there is complete - passthrough, then fall in output prices capture the magnitude of fall in marginal costs. However, given the extensive evidence of incomplete pass-through in various contexts (including in India using the same dataset), it would be reasonable to be expect the observed changes in prices to reflect only a fraction of the changes in marginal costs. [35] Table I.2 presents the results using the exposure measure for downstream firms with diff-in-diff setup. The coefficient of interest is the interaction of log measure of $Exposure^{Downstream}$ with the $InputDereserv$ dummy that equals to 1 from the year the first reserved input is dereserved and 0 otherwise. I find that firms in downstream market with 1 S.D. greater exposure to reserved supplier (i.e. 17% of input share is from the upstream market) experience 1% growth in sales that is driven by a 4% increase in quantity and a fall in output prices of 3%.

Hence, the upstream and downstream effects of dereservation are robust to the definition of the treatment variable. Finally, it is plausible that upstream and downstream industries connected to the reserved product market might be facing other correlated demand and supply shocks that differently affects its performance and input/output choices. We find no substantial trends in the input/output choices prior to dereservation, but firms reallocate their inputs (outputs) towards dereserved products in the downstream (upstream) sectors after dereservation.

Feedback effects from linked markets

Finally, as a first pass, I test if the output growth varies by market power in the upstream suppliers and downstream customers. If firms in these vertically linked markets have market power, they could extract a larger share of the Ricardian rent and the resulting feedback effect potentially could reduce the gains from dereservation. I interact the $Dereserv$ dummy with the pre-dereservation weighted upstream and downstream HHI. The HHI are weighted by the input shares (as a fraction of total costs) from upstream suppliers and input shares for downstream customers. Results are presented in Table I.4. When suppliers are concentrated, output growth for dereserved markets is significantly smaller. This is suggestive of increasing input costs mitigating the growth. There are no significant feedback effects from downstream, perhaps because relatively fewer firms are customers of reserved products.

6. Distributional effects

In this section, I test the model's predictions about the distributive effects of dereservation on firms in the production network. When firms differ in productivity, the distribution of inputs across firms affects allocative efficiency within the market. Two sources of misallocation - policy distortions that implicitly taxes large firms and markups - drive a wedge between the revenue and output as larger and more productive firms underproduce relative to smaller

[35] Technically, pass-through can be more than 100 percent with log-convex demand. However, there is consistent empirical evidence that counters this theoretical prediction including the vast literature on exchange rate pass-through. See Hellerstein (2008), Nakamura and Zerom (2010) and Feenstra, Gagnon and Knetter (1996) for example.

and less productive firms. I use a diff-in-diff equation of the following form for establishment e in market i and year t:

$$V_{iet} = \alpha_0 + \beta_1 Dereserv_{it} X_e + \beta_2 Dereserv_{it} + \alpha_{ie} + \alpha_t + \epsilon_{iet} \tag{8}$$

Here, I interact firm-level productivity X_e with the dummy for dereservation $Dereserv$ from the previous section. For upstream (downstream) markets, I replace $Dereserv$ indicator with $UpDereserv$ ($DownDeresrev$) dummies respectively. The results are restricted to incumbent plants in the production network. All regressions include year FE and product-firm FE. Errors are clustered at the firm-level.

Reserved Market

To casually determine if there is reallocation of market shares towards more productive firms, I interact firm-level productivity prior to dereservation with $Dereserv_s$ and examine its effects on the market share. Results are presented in Table 5. I find that amongst the incumbent firms, there is evidence of market reallocation as the most productive firms experience the sharpest increase in their market revenue share: a firm with 10 percent higher productivity witnesses a 0.8 percent increase in market share.[36]These results are robust to including firm-level controls such as capital, age and labor as well as state-specific trends that capture the local demand effects and industry-year FE that control of any factors that affect demand or supply of all firms within a industry in a year (e.g. trade shocks).

Effects of market power - Upstream

I have found evidence thus far for the first order comparative static predictions, including the positive demand and supply shocks for downstream and upstream markets as well as reallocation in the reserved markets itself. The next step is to determine if there is any redistribution in upstream markets. In particular, I analyze if the strategic decisions on pricing and output of upstream suppliers depends on their market power. For the rest of analysis, I use the fact that productivity of the firm is the source of its ability to price over the marginal cost and the HHI of the market captures the market's potential to raise markups. Firstly, I split the sample of firms by the median HHI (for statistical power) at the nic2digit industry level to test if there are any differences in firm behavior following a demand shock by market concentration.[37] In examining the reallocation measures below, I focus on the

[36]I find that for firms that are 10 percent more productive within an upstream market, their market share increases by 0.4 percent post-dereservation but I find no such effects for downstream firms. Results are not presented here for brevity but are consistent with the rest of the analysis.

[37]HHI is calculated as the sum of squared market shares that are each weighted by the sampling multipliers from the ASI in the base year. While I use HHI calculated at the product-level, the results are robust to using more aggregated HHI at the NIC 5-digit and NIC 4-digit level that is an annual census of manufacturing in

effects of dereservation on reallocation across incumbent firms. While this is undoubtedly missing the dynamic effects of reserved markets if more firms enter or exit, as shown in Table I.E.10, I do not observe significant growth in the number of establishments for upstream or downstream firms. This alleviates concerns of entry driving the effects and instead points to reallocation amongst incumbents as the dominant effect.

In Table I.6, I find that average output prices increase, but the effects are significantly and statistically larger in more concentrated markets. Average output increases in all markets with no significant differences by market concentration. This suggests that demand is highly inelastic for intermediate inputs that despite the increase in the prices, the output remains constant. This is consistent with evidence in the literature that finds inputs to be Leontief in production and elasticity of substitution being close to 0 (Boehm et. al 2018). Hence, more concentrated upstream markets are just extracting a larger share of the rent from reserved markets. In the Appendix Figure E.5, I plot the event study specification for 25th and 75th percentile HHI markets that illustrates no pre-trends in the upstream markets. Following dereservation, the most concentrated markets increase their prices more than quantity.

Next, I test if the effects are different by firm's productivity within the product market. I interact the firm's initial productivity with the indicator $UpDereserv$. Results are presented in Table I.7. Firstly, heterogeneity in productivity implies that better performing suppliers expand production following a demand shock. In fact, the growth in output is driven by a small portion of these high-performance firms. The effects on prices and markups are also heterogeneous by productivity. I find productive firms increase their markups and prices relatively more when the market concentration increases. Hence, while output is reallocated to more productive firms in the upstream supplier market, this reallocation effect is weaker in concentrated markets. These results suggest that reducing a production bottleneck generates substantial efficiency gains through upstream linkages, and this effect is stronger in more competitive upstream markets.

Effects of market power - Downstream

I repeat the above analysis for downstream markets. The median HHI for an average downstream industry is 10.7. Firstly, more concentrated downstream markets raise their markups in response to lower marginal costs from dereservation. This incomplete pass-through aligns with the significantly lower quantity growth in concentrated downstream markets.[38] Accounting for the fall in prices reported in the previous section, these results suggest that

India. The median inverse HHI for upstream industries is 17.1.

[38]Event-study specification of these results in the appendix Figure I.E.6 show that only less concentrated markets exhibited a decline in prices with no specific pre-trends prior to dereservation.

marginal costs fell by around 8% due to dereservation, consistent with the observed price trends.

Next, I test if the effects are different by firm's productivity within the downstream markets. There is indeed evidence of reallocation in Table I.9. Larger firms in more competitive markets increase their quantity more in response to the reduced input costs. However, in concentrated downstream markets, larger firms optimally choose to partially absorb cost reduction in their markups with no evidence of reallocation.[39] Hence, welfare gains from removing production bottlenecks is higher when downstream markets are more competitive, arising from both higher output and reallocation gains. The intuition for this mechanism is as follows: because distortive policies tax firms within a layer heterogeneously, then it is reasonable to expect the related input taxes to fall heterogeneously on firms in downstream product markets. Imagine within each layer, there are firms with varying marginal products. An input tax that is imposed by distorting suppliers means that these firms have lower profit margins. Moreover, they might not have access to quality inputs or even quantity needed to grow. This means that their intermediate inputs are distorted, stifling their growth.

Additional Robustness Checks

In this section, I document a series of checks to checks the robustness of results.

EXPOSURE MEASURES - There is significant heterogeneity in the reported input usage at the firm-level within each product market. This can be attributed to the highly disaggregated nature of the product classification of ASI and the resulting misreporting. Using the reported input usage aggregated at the product-level rather than the firm-level measure of exposure used above, I find the above effects to be consistent using the aggregated measure of downstream exposure as well.[40]. Results are shown in the Appendix.

ADDITIONAL CONTROLS- I test the robustness of the results to controlling for a variety of additional characteristics including the nic3digit Industry x year dummies that controls for any changes that affect all product markets within the industry; State x year dummies to account for potential concentration of certain product markets in particular states that experience differential growth; initial age x year dummies and log of capital for

[39]These results are consistent with the trade literature that finds exchange rate shocks tend to affect markups and not export volumes (see Berman et al., 2012).

[40]This in particularly concerning for analysis focusing on the outcome variables for downstream firms because the control group could potentially include firms that failed to report intermediate inputs from reserved sectors. The degree of bias is unclear but assuming there is no significant heterogeneity in the misreporting across product markets mitigates some of the concerns. The fact that firms fail to report an input would suggest that they do not consider it to be an important input and hence, the bias should be small.

differential growth rates in the firm-level regressions. Results are similar to baseline findings.

PRODUCT-LEVEL TIME TRENDS - Another concern for the upstream and downstream markets is that the timing of exposure to dereservation policy coincides with different product-specific growth rates. While I do not find any product-level pre-trends, I implement another robustness checks whereby I include separate time-trends for every production. The results remain robust.

DROPPING OUTLIERS- While the firm-level analysis excludes observations above the 97th percentile and below the 3rd percentile, there is still the concern that product markets might have outliers that are driving the results. That is, there are product markets with high concentration, close to 1, whose effects might by biasing my results. I test the robustness of my results by excluding product markets with concentration higher than 0.90. The results remain robust after eliminating the monopolized markets as well.

ALTERNATE MEASURES OF FIRM PERFORMANCE- While this paper is interested in studying the reallocation effects by firm performance, the measure of productivity used in the baseline analysis is itself estimated and can be prone to measurement errors. In the appendix, I present results using alternate measures of firm performance including market shares which are directly observed. The assumption here is that more productive firms have larger (revenue-based) market shares. Evidence suggests that this assumption is reasonable in my case as more productive firms increase their market share once the capacity constraints are removed in the reserved markets. Results are presented in the Appendix and are similar to the baseline findings. Secondly, I also use a measure of labor productivity (defined as log of real value added per employee) directly calculated from the balance-sheet data and find the results to be very close to the baseline results as well. Results are available upon request.

Mechanisms

Several mechanisms can generate the reallocation observed in linked markets following liberalization. Imposing firm-size restrictions works similarly to an input tariff for downstream customers. Topalova and Khandelwal (2011) finds that access to intermediate inputs improved productivity following trade liberalization in India. I find that removing capacity constraints has similar effects for downstream customers. While individual firms can overcome lack of quality products by importing inputs from abroad, the existence of transport and local coordination costs suggest that a lack of locally available high-quality inputs is likely to hinder the ability of productive firms to grow. Relatedly, the upgrading of downstream producers is likely to generate pressure on local suppliers to improve quality, much like foreign direct investment may lead to productivity improvements among domestic suppliers (Kugler(2006)). Apart from more or better buyers and suppliers, liberalization might also lead to improved matching between suppliers and buyers and enable the larger, more productive upstream and downstream firms to expand. In addition, lower costs of vertical integration could incentivize more productive firms to enter linked markets and increase their production. These results empirically corroborate the importance of production network on firm-size dispersion as illustrated in Bernard et al. (2018). Hence, size-distortionary policies in linked markets can explain some of the left-skewed firm-size distribution observed in developing countries.

Firm-level evidence of endogenously variable markups can be explained by a large class of models including imperfect competition a la Cournot where more productive firms have larger market shares and face lower demand elasticity (Atkeson and Burstein (2008)), linear demand system with horizontal differentiation where demand elasticity increases with prices (Melitz and Ottaviano(2008)) and monopolistic competition with non CES demand and variable elasticity of substitution (Dhingra and Morrow (2016)). The key feature in all these models is that demand elasticity varies with firm productivity, and therefore markups vary within a market. Faced with a supply or demand shock, firms react by increasing their markup the higher their productivity. Here, when elasticities vary, market allocations reflect the distortions of imperfect competition. In an influential work, Arkolakis et al. (2012a,b) show richer models of firm heterogeneity and variable markups are needed for these micro-foundations to determine the welfare gains from trade. Recent work by Edmond et al. (2018) studies the welfare costs of markups using these heterogeneous markup models and points to potential for industrial policies to reduce these costs. The empirical results presented in this paper illustrate that endogenous markups materially affect the gains from regulatory regime changes on productivity gains across the supply-chain. Unlike a perfectly competitive model, shocks to industries and firms have different effects on aggregate output and productivity that depends on the market structure of linked markets.

A model that closely aligns with the propagation mechanisms identified in this paper is Baquee (2016). He presents a framework where changes in the mass of varieties in each industry through firm entry and exit can propagate both upstream and downstream. He uses monopolistic competition, free-entry and IO linkages via constant-elasticity-of-substitution (CES) production functions. He shows changing dynamics between intertwined industries that are sufficiently uncompetitive.

This paper is also consistent with the classic double-marginalization problem analyzed in Spengler (1950) and the subsequent literature that studies the effects of market structure on pass-through. In this strand of literature, the effective demand elasticity for upstream firms depends on downstream pass-through. Here, markups are strategic substitutes in the sense of Bulow et al. (1985): upstream firm raising its mark-up, which is equivalent to imposing a tax on the other firm, induces the downstream firm to absorb this increase and lower its markup. As average market power of firms in upstream market increases, they increase their prices and lower their quantity when downstream taxes decreases. This leads to an increase in the input price that is further shifted to consumers by firms at the downstream layer. Thus, consumers are likely to enjoy a smaller fraction of the welfare gains when linked firms are less competitive. The resulting overestimation would to be large if upstream or downstream market tend to be less competitive and for most cases if the demand curve is relatively inelastic.

7. Aggregate Productivity implications

In this section, I present the aggregate productivity implications of dereservation. The firm-level redistribution of inputs from less- to more-productive firms identified in the last section points to allocative efficiency gains driving productivity growth. I verify these results at the product-level in this section. I use two different growth accounting procedures from the literature to measure allocative efficiency gains. The first is a Melitz-style reallocation where the aggregate productivity is the sum of unweighted average productivity and covariance of market share and firm productivity. Second method follows Petrin and Levinsohn (2012) where aggregate productivity growth is stronger when inputs assigned to firms with higher social value (higher markups) would increase welfare in the economy. The latter method allows to estimate the efficiency loss from variable markups.

Improved allocation in reserved markets

This subsection outlines some of the first evidence of misallocation resulting from a policy change. Following Equation 5, output misallocation is large, if the revenue productivities are dispersed such that reallocating the production across firms significantly increases aggregate gross output. In this economy, resources are allocated optimally when all firms face the same or no distortions in output, i.e. there is no dispersion in returns to factors. First, I compute the variance in revenue productivity within each product market. Using a diff-in-diff specification, I find a significant fall in revenue productivity dispersion by 6% within reserved product markets. Results are presented in Table I.10. The results are robust to inclusion of state x year fixed effects and 3-digit industry x year fixed effects as well. Secondly, I plot the distribution over productivity before and after dereservation in Fig I.4. The left tail of productivity distribution is less thicker post liberalization, consistent with reservation favoring the survival of inefficient plants. In fact, while the productivity distribution becomes less dispersed, it also becomes left skewed suggesting within-firm productivity improvement and selection effects as potential channels as well.

Next, I decompose productivity growth to identify the magnitude of gains from reduced misallocation. Generally, misallocation is measured as the distance between aggregate productivity and a counterfactual frontier (usually another more developed country). Instead of determining the frontier that requires additional assumptions, I estimate misallocation by comparing aggregate productivity before and after resources are reallocated due to the removal of size restrictions in India. Removing capacity constraints increases the industry's aggregate sales by allowing existing firms to expand and new firms to enter the market.[41] In the absence of within-firm productivity improvement, least productive firms within the market will incur a loss in market share and the revenue distribution of surviving firms will shift rightward especially at the right tail. Then, in this case, misallocation is the difference in weighted average of establishment-level productivities, where the scaling factors are market shares $S_{ie} = P_{ie}q_{ie}/P_iQ_i$, i.e. establishment's value added relative to the value added of the industry. The intuition for the weights is as follows: when the output distortion faced by an establishment increases relative to the average distortion in the industry, its size decreases. Since a highly distorted establishment becomes more integral to industry productivity when its distortions are removed, the extent of misallocation depends on which establishments bear the greatest distortions. When the most productive establishments also bear the largest distortions, overall productivity gains associated with the removal of distortion is higher than if less productive establishments bear the same distortions.

$$\Phi_i = \frac{TFP_{i,post}}{TFP_{i,pre}} = \frac{\sum_{e=1}^{N} Z_{ie}S_{ie,post}}{\sum_{e=1}^{N} Z_{ie}S_{ie,pre}}$$

TFP in the reserved product markets falls by 45% with $TFP_{pre} = 0.51$ and $TFP_{post} = 0.74$. There is significant variation in gains across markets suggesting that reservation was not uniformly binding for all markets.[42] To ensure these results are at least partially driven by dereservation, I conduct a series of tests. Firstly, both upstream and downstream firms are exposed to the dereservation but only through IO linkages. Hence, we would expect the gains in TFP to be significantly smaller in these linked markets. I find upstream TFP to increase by 24% and downstream TFP to increase by 21%, significantly less than the TFP growth observed in the dereserved product markets. Secondly, because most of the dereservation happened post 2004, I compare changes in TFP before and after 2004 for reserved vs never reserved product markets and find a 20% greater fall in TFP amongst reserved product markets. While these estimates are correlational, it provides a rough sense of the productivity gains from dereservation. In the next step, I decompose the productivity

[41] This result holds as long as $\epsilon > 1$ resulting in a smaller fall in aggregate price.

[42] An advantage of this measure of misallocation is that it allows for changes in entry and exit of establishments by allowing $S_{ie} = 0$ in the periods of non-operation, thereby capturing effects of turnover on productivity as well.

gains to determine the gains from reallocation using diff-in-diff. Following Olley and Pakes (1996), I decompose the weighted productivity measure TFP for a market i in year t into two parts: the unweighted technical productivity measure $\bar{Z}$ and the total covariance between a firm's share of the market output and its productivity:

$$TFP_{it} = \sum_{ie} S_{iet} Z_{iet} = \bar{Z}_{it} + \sum_{ie} (S_{iet} - \bar{i}_t)(Z_{iet} - \bar{Z}_{iet}) \qquad (9)$$

Productivity growth Φ_s is then given by:

$$\Phi_i = \Delta \bar{Z}_{it} + \Delta \sum_{e} (S_{iet} - \bar{S}_{it})(Z_{iet} - \bar{Z}_{it})$$

First term is the contribution of within-firm productivity improvements amongst incumbents (due to increase in scale, innovation, access to higher quality intermediate inputs, optimal product mix etc.) and the selection effect with regards to both the survival of incumbents and entry of new firms.[43] The second term captures improvements in allocation of factor inputs that contribute to aggregate productivity gains. I compute the covariance at the product-level each year using the establishment-level weights provided in the ASI survey. I find the allocative efficiency improves by 10% when the product market is dereserved. In contrast, the average productivity for the dereserved product market increases by 3%. Hence, a large share (74%) of the TFP gains for dereserved markets comes from reallocation rather than the growth in mean productivity. The relatively small within-firm productivity growth is consistent with findings in Martin et.al (2017) and Tewari and Wilde (2018). These results emphasize the need to focus on reallocation effects rather than the average effects when studying the gains from alleviating distortionary policies.

Productivity effects on linked markets

Firm-level evidence in linked markets points to improved allocation as more productive firms expand. However, while markup dispersion decreases in reserved markets, it increases in both upstream and downstream markets post liberalization. This is consistent with firm-level evidence where more productive firms increase markups in linked markets. These results suggest efficiency losses in concentrated linked markets due to elimination of reservation. In Table I.12, I test these predictions using the covariance of productivity and market shares from Equation 9. Allocative efficiency improves in less concentrated markets while deteriorating in more concentrated upstream and downstream markets, thereby reducing the aggregate productive gains from dereservation. A question then arises if this worsening allocation

[43] Selection effects include the gap in productivity between surviving and exiting plants, between entering and exiting plants and the gap between surviving and entering plants.

is indeed due to variable markups and is it large enough to mitigate all the gains from eliminating a bottleneck i.e. are we just shifting bottlenecks with no real gains? The next subsection attempts to answer this question.

Aggregate Productivity Growth

A critique of the approach using TFP is that it is a production concept and has nothing to do with the value of additional output associated with technical efficiency gains. Markups capture the value of the additional output. Hence, in this spirit of Basu and Fernald (2002) and Petrin and Levinsohn (2012), let aggregate productivity growth (APG) be the extra value of output going to final demand net of any extra primary input costs:

$$APG = \sum_e (\bar{D}_e p_e \Delta ln Y_e) - \sum_e \sum_{X=K,M,L} D_e [\sum_k (s_{X_e}) \Delta ln X_e] \qquad (10)$$

where $D_e = Y_e / \sum_e VA_e$ is the Domar-weight of firm e with D_e being the average of firm e's value added share weights and s_{input_e} is the revenue share of input X.[44] Y_e is the gross output of firm e with p denoting the firm's real prices. This measure estimates the Domar-weighted changes in final demand, and hence captures the welfare gains from dereservation. An advantage of this approach is that both output and the revenue shares can be calculated directly from the data, avoiding the need to estimate firm-specific production function elasticities and productivity as in the previous section. To estimate APG, I take the averages of all relevant variables within pre-reform period and post-reform period separately and compute the differences at the firm-level before weighting and aggregating up to the product-level.[45]

Assuming fixed technology (i.e. constant input coefficients) and no distortions in the primary and intermediate input markets, APG can be decomposed as follows:

$$APG = \underbrace{(\bar{\mu} - 1) \sum_{e=1}^{E} D_e \sum_{X=K,M,L} [\sum_k (s_{X_e}) dln X_e]}_{\text{Average markup effect}} +$$

$$\underbrace{\sum_{e=1}^{E} D_e (\mu_e - \bar{\mu}) \sum_{X=K,M,L} [\sum_k (s_{X_e}) dln X_e]}_{\text{Allocative efficiency gains}} + \underbrace{\sum_{e=1}^{E} D_e dln Z_e}_{\text{Technical efficiency gains}} \qquad (11)$$

where $\bar{\mu} = \sum_{e=1} D_e \mu_e$ and μ_e is the firm-specific markup. See Appendix A for the

[44]The appropriate weight to trace out the final impact of firm-level gains is the Domar weight, as an increase in intermediate input supply would lead to more output some of which may go directly to final demand and some that go off to intermediate input consumption further downstream.

[45]To deal with firms who have no reported output in pre or post period, I assume logs of a small positive output of 0.1.

derivation of this decomposition. The intuition of this expression is as follows: suppose a firm has a higher-than-average markup. Then, relative to the social optimum, that firm produces even less output than does the average firm. Productivity increases even more if input use rises in this high-markup firm relative to the average firm. Higher average markup also distorts the labor-leisure choice and leads to underproduction in the market. When markets are perfectly competitive, these markup adjustment channels do not exist. However, there can still be reallocation effects if there are other distortions in the linked markets. For example, downstream customers could be facing intermediate input distortion whereas upstream suppliers face output distortions that misallocate all their inputs. Because I observe changing markups and divergence of markups and quantity in more concentrated linked markets, I test these implications at the aggregate level using ΔAE:

$$\Delta AE \propto \Delta \sum_{e=1}^{E} D_e(\mu_e - \bar{\mu}) \sum_{Input=K,M,L} \sum_{k} (s_{input_e})(X_e - \bar{X}) \tag{12}$$

where allocative efficiency from markup dispersion AE is the covariance of input shares and markups. Like the previous decomposition, I focus on measuring this object in each year using the establishment-level weights provided in the ASI survey.

APG increases by 18% in the reserved market. With a technical productivity growth of 3% in the previous section, 82% of the APG is a result of reduced markups and improved allocative efficiency. In Table I.15, I present the results on APG in linked markets. In upstream markets, there is a substantial APG that is attenuated with increasing market concentration. The significant growth in aggregate productivity for upstream markets, much larger than the allocative efficiency gains suggests that demand shocks mainly operate by alleviating other input distortions and increasing technical productivity. However, in highly concentrated markets, market power induced efficiency loss attenuates the productivity gains. Divergence in the covariance of markups and inputs can explain nearly all of the fall in APG in highly concentrated markets. In the downstream markets, there is no significant growth in APG within less concentrated markets despite a small improvement in allocative efficiency. From a social perspective, these high-markup firms were too small to begin with. Hence, the reallocation of factors towards these firms improves allocative efficiency. A rough intuition for small but insignificant APG is that the resulting increase in average markup abates any gains in allocation. Within more concentrated markets, both APG and allocative efficiency worsens. Here, the reallocation was insufficient to overcome the losses from increasing markups and markup dispersion.

Aggregation Results thus far provide estimates for the nature and intensity of propagation of deregulation through IO linkages. I conclude this section by using the above findings to obtain a back-of-the-envelope estimate of the overall productivity impact of dereservation and the role of linkages. I compute the *APG* under different scenarios using estimates from the previous sections weighted by sector-level Domar weights. Results are presented in Table I.16.

Recall that reserved markets accounted for 13% of manufacturing GDP in India prior to dereservation. The reserved markets experienced an average productivity growth of 18% following the removal of capacity constraints. Absent any propagation or amplification mechanism, dereservation accounts for 0.13 x 0.18 = 0.023 percentage increase in aggregate productivity in the manufacturing sector. These results are consistent with the structural estimates of Garcia-Santana and Pijioan-Mas (2013) using data prior to dereservation and a Lucas span-of-control model. APG was around 6.7% a year in India during the period of dereservation as estimated by Nishida et al. (2016). They also find that these gains were primarily driven by reallocation of intermediates. My estimates suggest that the dereservation can account for 1/3 of the productivity growth in India during this period. However, it cannot account for the large gap in manufacturing TFP existing between the US and India.

Next, I sum the estimates for the average intensity of IO transmission weighted by the corresponding sector-level Domar weights (18% for downstream and 35% of upstream). Taking these weights as a baseline and holding all else constant, I find that the direct and indirect propagation of the shock over IO linkages can account for a 4.25% point growth in aggregate productivity when the linked markets have low concentration. Propagation through IO linkages can account for roughly 45% of the aggregate gains with a significant fraction of these gains from upstream transmission.

In the final row, I subtract the efficiency loss from variable markups that occurs within highly concentrated markets. When the linked markets are highly concentrated, the share of APG from linked markets is attenuated. There are also feedback effects from rising input costs and lower demand growth from incomplete-passthrough that can mitigate the APG for reserved markets itself. Results are presented in Table I.14. I find evidence of strong forward propagation from upstream whereby concentrated supplier markets decrease APG for reserved markets. This is line with evidence of lower output and labor growth in reserved markets when upstream market concentration is higher. With sufficiently competitive markets, IO linkages can amplify the gains from reducing distortions. As a final remark, I note that these effects are a lower bound for the real effects as I restrict my analysis to the immediate suppliers and customers of reserved markets. The intensity of propagation diminishes as the shock travels through the chain.

Discussion

Evidence of reduced misallocation and increased productivity in dereserved markets suggests that SSI reservation was indeed an important source of misallocation affecting a large share of manufacturing in India. Contrarily, Hsieh and Klenow (2009) found that misallocation in India worsened over the period from 1987 to 1994 during aother large reform episode that included trade liberalization and delicensing of the "raj" system. This puzzling evidence was further corroborated by Bollard, Klenow, and Sharma (2013) who found that although this period witnessed rapid productivity growth for their sample of very large firms, little of the productivity growth was due to changes in misallocation. They all conclude that APG was primarily driven by within-plant increases in technical efficiency and not between-plant reallocation of inputs.

Contrary to modelling that assumes size-distortions result in an increase in the equilibrium number of establishments, a decline in the average establishment size and concentration, I find the number of establishments increases post-reservation as larger multiproduct firms could enter the market and vertically integrate production. In fact, Tewari and Wilde (2018) find that multiproduct firms are the main entrants into newly de-reserved markets, increasing their product scope and productivity. This suggests that that structural modelling that ignores multiproduct establishments is misleading. Dereservation may have affected the optimal behavior of multiproduct establishments in linked markets, resulting in improved allocation. In addition, instead of an increase in concentration and decrease in competition, the dynamism from increased entry had the opposite effects.

Finally, my results suffer from limitations that can be addressed in future work. My data does not allow to identify the input-supplying firm, hence I do not observe whether matching between firms change. This would require a detailed firm-level transaction data but can offer insights on the channels of rellocation. Additionally, this study ignores the reallocation of resources across product markets as emphasized by Epifani and Gancia (2011). This could be a potential area of future study.[46] It would also be fruitful to study the interaction of trade and industrial policy in this context. For example, do exports grow in the dereserved markets? is there a change in imported input consumption? Finally, the analysis in this paper is partial equilibrium as I am not exploring the effects on higher order or horizontally related markets. I also abstract away from the entry and innovation effects from changing markups as well as labor market effects.[47] A dynamic general equilibrium framework that captures the aggregate effects would require one to construct detailed product-level IO table as well as estimate parameters like the centrality of the affected market, elasticity of substitution and

[46] Existing literature points to substantially smaller reallocation effects across sectors relative to reallocation within sector. See Berman et. al (2012) for an exploration in the exchange rate pass-through context.

[47] I am only focusing on the static gains.This is a reasonable assumption given that I find no significant changes in the number of establishments within vertically linked markets over the medium-term.

upstream and downstream markups. This could be an interesting avenue for future work especially if the estimate can provide a bound on the IO multiplier by market power.

8. Conclusion

This paper studies the aggregate productivity effects of reducing size-distortionary polices that constrain larger and more productive firms. These types of policies are common across the world and have been shown to reduce productivity and output by increasing misallocation. My results showcase the importance of accounting for IO linkages when implementing selective reforms that affect a subset of markets. Leveraging a natural experiment in India, I show that eliminating size-restrictions in one market affects productive firms across the supply-chain. Consequently, piecemeal reforms have a much larger impact on the macroeconomy as it increases the aggregate productivity of not only directly exposed markets, but also of those that are connected to it. Nearly all of these productivity gains are driven by selection and market reallocation of inputs to more productive firms.

However, I find that response of the aggregate economy to reforms can be different depending on the micro-structure of the underlying network. I find that eliminating a size restriction propagates to upstream suppliers and downstream customers by triggering large reallocations of factors of production within competitive linked markets. However, when linked markets are concentrated, their efficiency worsens as inputs are reallocated to less productive firms with lower markups. Uncompetitive markets would become the new binding bottlenecks in the supply-chain due to increasing markups driven by larger and more productive firms.

My empirical results have nuanced consequences for policymakers. A government that ignores the fact that the upstream or downstream market is imperfectly competitive and thus ignores the resulting markup distortions, will make imprecise predictions concerning the gains from selective policy interventions. These results also underscore the role of IO linkages in shaping the overall market structure including the firm-size and markup distribution and demonstrates the possibility that a small industry can have an arbitrarily large effect on the economy.

Table I.1: Impact of dereservation on product-level outcomes

	ln(revenue)	ln(quantity)	ln(labor)	ln(estab)
Dereserv	0.243^{**}	0.329^{*}	0.180^{*}	0.201^{***}
	(0.115)	(0.175)	(0.104)	(0.0549)
Product FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Products	4088	4088	4088	4088
Observations	22647	19591	22647	22647

Note: The table displays resuls from the product-level regressions using (2). *Dereserv* is a dummy variable that takes a value 1 when the product is removed from the list of reserved products. Regressions are weighted by the initial labor shares and errors are clustered at the product-level. $^{*}p < 0.10$, $^{**}p < 0.05$,$^{***}p < 0.01$

Table I.2: Impact on downstream market by exposure

	(1)	(2)	(3)
	ln(revenue)	ln(price)	ln(quantity)
$DownDereserv * Exposure^{Down}$	0.0106^{***}	-0.0331^{*}	0.0428^{**}
	(0.00235)	(0.017)	(0.0198)
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Number of Clusters	94362	94434	94362
Observations	239893	240098	239893

Note: The table displays resuls from the firm-level regressions using (2). *DownDereserv* is a dummy variable that takes a value 1 when the first reserved input is removed from the list of reserved products. Regressions are weighted by the sampling multipliers from ASI and errors are robust clustered at the firm-level. $^{*}p < 0.10$, $^{**}p < 0.05$,$^{***}p < 0.01$

Table I.3: Impact on upstream market by exposure

	(1) ln(revenue)	(2) ln(price)	(3) ln(quantity)
$UpDereserv * Exposure^{Up}$	0.0571***	0.0167***	0.0447**
	(0.013)	(0.0054)	(0.013)
FirmxProduct FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Number of Clusters	3065	3065	3065
Observations	94788	94049	93948

Note: The table displays resuls from the firm-level regressions using (2). *UpDereserv* is a dummy variable that takes a value 1 when the first reserved output market is removed from the list of reserved products. Regressions are weighted by the sampling multipliers from ASI and errors are robust clustered at the firm - level. $^{*}p < 0.10$, $^{**}p < 0.05$,$^{***}p < 0.01$

Table I.4: Impact on reserved markets by linked market concentration

	ln(revenue)	ln(quantity)	ln(labor)	ln(estab)
Dereserv	0.243**	0.329*	0.180*	0.201***
	(0.115)	(0.175)	(0.104)	(0.0549)
Dereserv*Upstream HHI	-0.0409**	-0.0252	-0.0285*	-0.0121**
	(0.0197)	(0.0325)	(0.0168)	(0.00555)
Dereserv*Downstream HHI	-0.0445	-0.0294	-0.000309	-0.00184
	(0.0198)	(0.0300)	(0.0171)	(0.00813)
Product FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Products	4088	4088	4088	4088
Observations	32704	32704	32704	32704

Note: Results from product-level regressions. *Dereserv* is a dummy that equals 1 when the reserved product market is dereserved and 0 otherwise. HHI is the concentration of the product market. Upstream HHI is measured as the weighted input shares of upstream products and Downstream HHI is the weighted output sale share of downstream markets. Errors are robust clustered at the product-level. Standard error in parentheses.$^{*}p < 0.10$, $^{**}p < 0.05$,$^{***}p < 0.01$

Table I.5: Marketshares of productive firms under dereservation

	(1) ln(marketshare)	(2) ln(marketshare)	(3) ln(marketshare)
$Productivity_{Pre} * Dereserv$	0.0905*	0.0809*	0.0787*
	(0.0504)	(0.0466)	(0.0450)
$Dereserv$	0.0416	0.0326	0.0180
	(0.0397)	(0.0409)	(0.0323)
Firm x Product FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Firm -level controls	No	Yes	Yes
IndustryxYear FE	No	No	Yes
State-specific Trend	No	No	Yes
Products	4288	4288	4288
Observations	201254	192694	192694

Note: Results from establishment-level regressions and restricted to incumbents. The dependent variable is (ln) marketshare of the firm. $Dereserv$ is a dummy that equals 1 when the product market is dereserved and 0 otherwise. Productivity is demeaned. Column 2 includes firm-level controls - capital, age and labor while Column 3 includes the regional trends and industry (3-digit) level x year FE. Errors are robust clustered at the product- level. The regressions exclude outliers in the top and bottom 3rd percent of the productivity. All regressions include firm–product and year fixed effects. Standard error in parentheses.$^{*}p < 0.10$, $^{**}p < 0.05$,$^{***}p < 0.01$

Table I.6: Upstream effects by concentration

	ln(price) *Low HHI*	ln(price) *High HHI*	ln(output) *Low HHI*	ln(output) *High HHI*
$UpDereserv$	0.0119	0.0451**	0.0621*	0.0502**
	(0.00772)	(0.0196)	(0.0368)	(0.0183)
Firm x Product FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Products	2288	2467	2288	2467
Observations	152812	140929	152812	140929

Note: Results from establishment-level regressions and restricted to incumbents. $UpDereserv$ is a dummy that equals 1 when the reserved product market that uses intermediate inputs from the upstream is dereserved and 0 otherwise. Errors are robust clustered at the product-level. The regressions exclude outliers in the top and bottom 3rd percent of the output price within a product-year. Standard error in parentheses.$^{*}p < 0.10$, $^{**}p < 0.05$,$^{***}p < 0.01$

Table I.7: Upstream effects by productivity

	ln(price) *Low HHI*	ln(price) *High HHI*	ln(markup) *Low HHI*	ln(markup) *High HHI*	ln(output) *Low HHI*	ln(output) *High HHI*
UpDereserv	0.0378***	0.0536**	0.0199***	0.0140	0.0114	0.0233 **
	(0.00888)	(0.00905)	(0.00646)	(0.0169)	(0.0156)	(0.00853)
UpDereserv*Productivity	0.00721	0.0158***	0.0211***	0.0569***	0.128***	0.0836**
	(0.00684)	(0.00603)	(0.00765)	(0.00462)	(0.0207)	(0.0271)
Firm x Product FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	174354	193200	174354	193200	174354	193200

Note: Results from establishment-level regressions and restricted to incumbents. *UpDereserv* is a dummy that equals 1 when the reserved product market that uses intermediate inputs from the upstream is dereserved and 0 otherwise. Productivity is standardized. Regressions are split by the median HHI. Errors are robust clustered at the firm - level. The regressions exclude outliers in the top and bottom 3rd percent of markups. Standard error in parentheses.$^{*}p < 0.10$, $^{**}p < 0.05$,$^{***}p < 0.01$

Table I.8: Downstream effects by concentration

	(1)	(2)	(3)	(4)
	ln(markup)	ln(markup)	ln(output)	ln(output)
	Low hhi	*High hhi*	*Low hhi*	*High hhi*
DownDereserv	0.0151	0.0460*	0.0930*	0.0492**
	(0.0318)	(0.0250)	(0.0267)	(0.0188)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Number of Clusters	2316	2839	2316	2839
Observations	82868	105709	82868	105709

Note: Results from establishment-level regressions and restricted to incumbents. *DownDereserv* is a dummy that equals 1 when the intermediate input market is dereserved and 0 otherwise. Errors are robust clustered at the firm- level. The regressions exclude outliers in the top and bottom 3rd percent of the markups within a product - year market. Standard error in parentheses.$^{*}p < 0.10$, $^{**}p < 0.05$,$^{***}p < 0.01$

Table I.9: Downstream effects by productivity

	ln(price)	ln(price)	ln(markup)	ln(markup)	ln(output)	ln(output)
	Low HHI	*High HHI*	*Low HHI*	*High HHI*	*Low HHI*	*High HHI*
DownDereserv	-0.0281***	-0.0118	-0.0185	0.00615	0.0419***	0.0385**
	(0.00838)	(0.0126)	(0.0115)	(0.0137)	(0.0155)	(0.0110)
DownDereserv*Productivity	0.0141**	-0.0139	0.0303***	0.120***	0.0671**	-0.0179
	(0.00702)	(0.0102)	(0.0112)	(0.0225)	(0.0291)	(0.0252)
Firm x Product FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	165855	170894	165855	170894	165855	170894

Note: Results from establishment-level regressions and restricted to incumbents. *DownDereserv* is a dummy that equals 1 when the intermediate input market is dereserved and 0 otherwise. Errors are robust clustered at the firm- level. The regressions exclude outliers in the top and bottom 3rd percent of the markups within each product-year market. Standard error in parentheses.$^{*}p < 0.10$, $^{**}p < 0.05$,$^{***}p < 0.01$

Table I.10: TFPR variation in reserved market

	(1) TFPR dispersion	(2) TFPR dispersion	(3) TFPR dispersion
Dereserv	-0.0523**	-0.0638*	-0.0424*
	(0.0284)	(0.0364)	(0.0238)
State x year FE	No	Yes	Yes
Industry x year FE	No	No	Yes
Number of Clusters	4727	4727	4727
Observations	27295	27295	27295

Note: The table reports the effect of dereservation on the CV of TFPR. *Dereserv* is a dummy that equals 1 when the product market is dereserved. Regressions are at the product-level and weighted by the initial labor shares. Firm-level TFPR is trimmed above and below the 3rd and 97th percentiles before calculating the variation. All regressions include Year and Product level FE. Errors are robust clustered at the product-level.

Table I.11: Decomposition of TFP for reserved market

	(1) tfp	(2) correlation	(3) meanproductivity
Dereservation	0.132**	0.0974***	0.0330***
	(0.0525)	(0.0305)	(0.00489)
Year FE	Yes	Yes	Yes
Product FE	Yes	Yes	Yes
Observations	29250	29250	29250

Note: Results from product-level regressions. *Dereserv* is a dummy that equals 1 when the product market is dereserved and 0 otherwise. All regressions include product and year fixed effects. Regressions are weighted by the initial labor share and errors are robust clustered at the product level. Standard error in parentheses.$^{*}p < 0.10$, $^{**}p < 0.05$,$^{***}p < 0.01$

Table I.12: Reallocation effects in linked markets

	Correlation *Low HHI*	Correlation *High HHI*	Correlation *Low HHI*	Correlation *High HHI*
$UpDereserv * Exposure^{Up}$	0.00998*	-0.0194***		
	(0.00560)	(0.0160)		
$DownDereserv * Exposure^{Down}$			0.00533***	-0.0107***
			(0.00168)	(0.00425)
Observations	14627	24164	14877	24547

Note: Results from product-level regressions. Dependent variable is the $Corr(productivity, marketshare)$ from Equation 4. *Exposure* is standardized. Regressions are weighted by the initial labor shares and errors are robust clustered at the product- level. Standard error in parentheses.$^{*}p < 0.10$, $^{**}p < 0.05$,$^{***}p < 0.01$

Table I.13: Aggregate productivity growth within reserved market

	APG_i	APG_i	APG_i
Dereserved	18.8^{**}	19.9^{**}	16.4^{**}
	(4.55)	(4.75)	(4.73)
Products	4046	4046	4046
Product controls	No	Yes	Yes
Industry x Year FE	No	No	Yes

Note: Results from product-level regressions. Dependent variable is APG_i within a market inflated by the sampling weights from the ASI. *Dereserv* is a dummy that equals 1 when the reserved product market is dereserved and 0 otherwise. HHI is the concentration of the product market. UpstreamHHI and DownstreamHHI are measured the weighted input shares of upstream products and weighted output sale share of downstream markets. Regressions are weighted by the initial labor shares and errors are robust clustered at the product-level. Standard error in parentheses.$^{*}p < 0.10$, $^{**}p < 0.05$,$^{***}p < 0.01$

Table I.14: APG in reserved markets by market concentration

	APG_i	APG_i	APG_i	APG_i
Dereserv	18.82^{***}	20.50^{**}	18.23^{***}	18.26^{***}
	(4.55)	(6.83)	(4.11)	(4.11)
Dereserv*HHI		-4.46^{***}		
		(1.72)		
Dereserv*Upstream HHI			-2.27^{*}	
			(1.19)	
Dereserv*Downstream HHI				-7.62
				(5.48)
Products	4742	4742	4742	4742

Note: Results from product-level regressions. Dependent variable is APG_i of Eq 10 within a market inflated by the sampling weights from the ASI. *Dereserv* is a dummy that equals 1 when the reserved product market is dereserved and 0 otherwise. HHI is the concentration of the product market. UpstreamHHI is weighted by input shares of upstream products and Downstream HHI is weighted by output sale share of downstream markets. Regressions are weighted by the initial labor shares and errors are robust clustered at the product-level. Standard error in parentheses.$^{*}p < 0.10$, $^{**}p < 0.05$,$^{***}p < 0.01$

Table I.15: APG in linked markets

	Low HHI APG_i	*High HHI* APG_i	*Low HHI* APG_i	*High HHI* APG_i
$UpDereserv * Exposure^{Up}$	7.284**	3.821**		
	(3.305)	(1.738)		
$DownDereserv * Exposure^{Down}$			1.295	-2.243***
			(0.843)	(0.504)
	Allo. Efficiency	Allo. Efficiency	Allo. Efficiency	Allo. Efficiency
$UpDereserv * Exposure^{Up}$	1.619*	-2.378*		
	(0.835)	(0.758)		
$DownDereserv * Exposure^{Down}$			0.698***	-0.572**
			(0.423)	(0.264)
Products	2665	2716	2665	2716
Product controls	Yes	Yes	Yes	Yes
Industry x Year FE	Yes	Yes	Yes	Yes

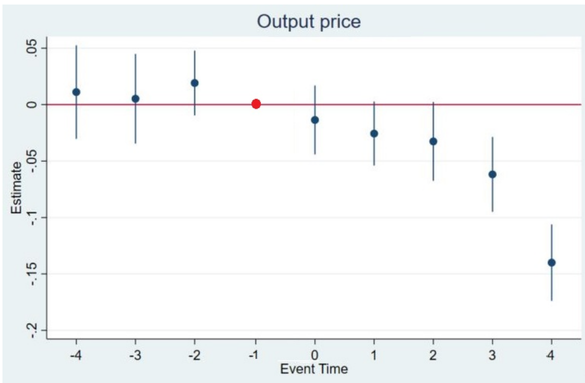
Note: Results from product-level regressions. Dependent variables are APG_i of Eq 10 and covariance term from Eq 11 within each market inflated by the sampling weights from the ASI. Regressions are weighted by the initial labor shares and errors are robust clustered at the three-digit level to allow for arbitrary error correlations within larger industries over time. Standard error in parentheses.$^*p < 0.10$, $^{**}p < 0.05$,$^{***}p < 0.01$

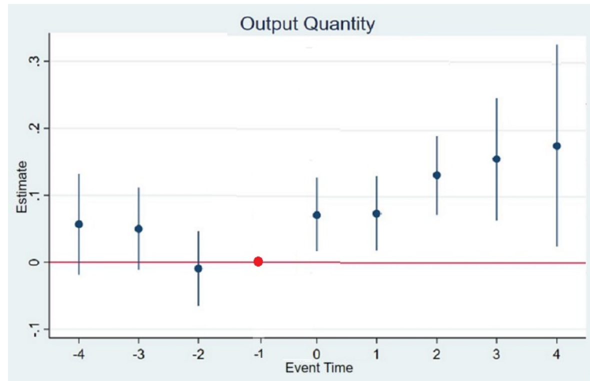
Table I.16: Aggregate Productivity Growth Estimates

	Upstream (1)	Reserved (2)	Downstream (3)
Ignoring IO linkages		2.34 %	
IO linkages with Low HHI	+ 1.82%		+ 0.09%
IO linkages with High HHI	- 0.86%		- 0.25%

Note: The table displays domar weighted APG for the manufacturing sector. The first row measures APG if only the reserved markets were affect by reservation. The second row estimates the additional APG from linked markets if they are not concentrated. The third row estimates the loss in addtional APG from the second row if the upstream and downstream markets were concentrated instead.

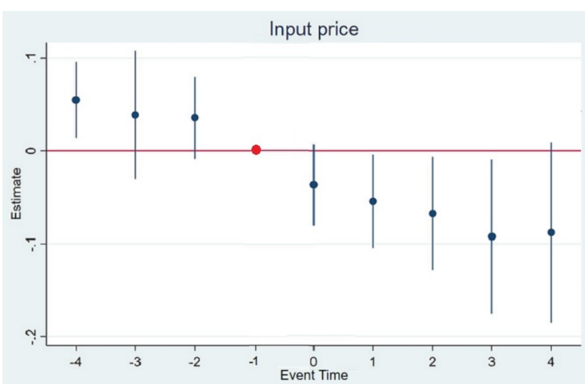
Figure I.1: Effects on dereserved product markets

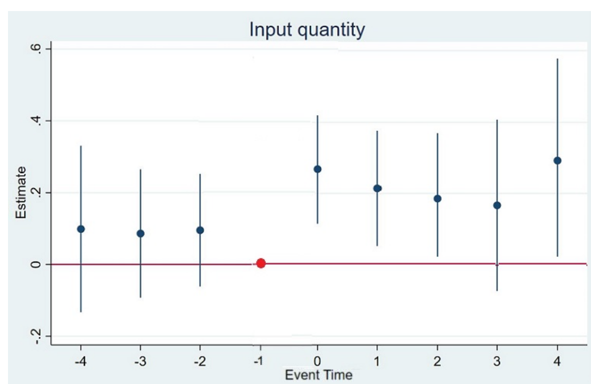




Note: The figures display the coefficients and 95 percent confidence intervals for the β_m coefficients in using the event-study regression specification (1). Output prices are at the firm-level while quantity is aggregated to the product-level using the weights provided in the ASI survey. Outcome variables are logged and observations are trimmed above and below the 3rd and 97th percentiles for output prices within each market. I impose the normalization that $\beta_{-1} = 0$ and errors are clustered at the product -level.

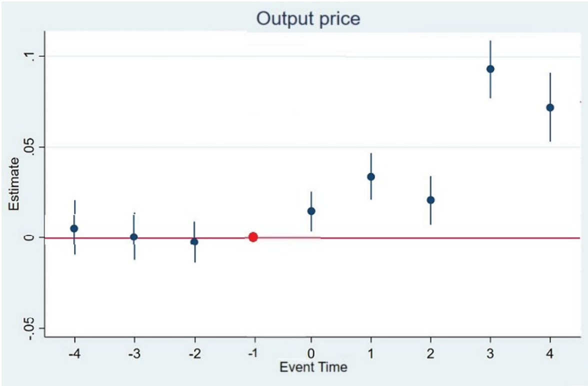
Figure I.2: Effects on downstream inputs

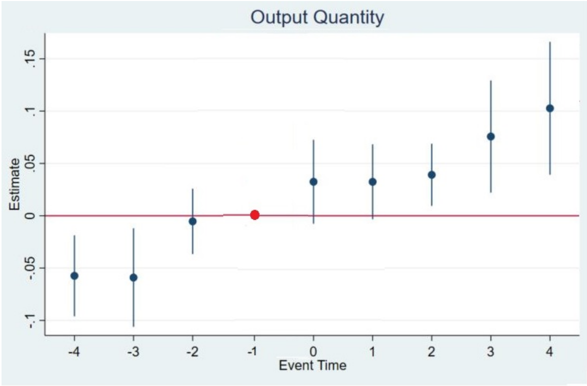




Note: The figures display the coefficients and 95 percent confidence intervals for the β_m coefficients in using the event-study regression specification (1). Here, regressions include firmxinput fixed effects and weighted in the inverse propensity score matching. I impose the normalization that $\beta_{-1} = 0$ and errors are clustered at the firm-level.

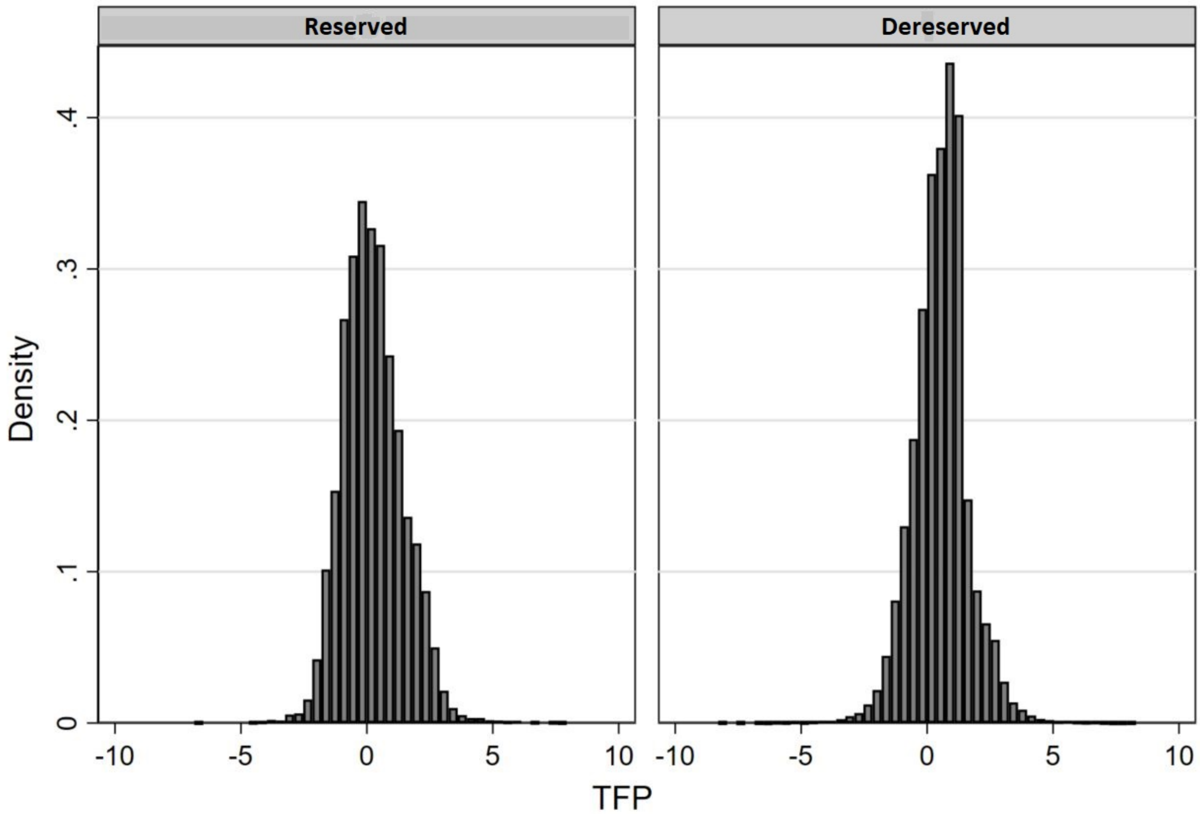
Figure I.3: Effects on upstream suppliers





Note: The figures display the coefficients and 95 percent confidence intervals for the β_m coefficients in using the event-study regression specification (1). I impose the normalization that $\beta_{-1} = 0$ and errors are clustered at the firm-level.

Figure I.4: TFP Histogram for reserved market



Note: The figures display the distribution of firm-level TFP within reserved markets weighted by the sampling multipliers.

Appendix

I.A: Proof for Aggregate Productivity Growth measure In this section, I provide the derivation for Equation 11 that decomposes the aggregate productivity growth measure. Assume each firm e has a production function for gross output given by:

$$Y_e = F^e(K_e, L_e, M_e, Z_e)$$

where K_e is the capital, L_e is labor, M_e is the intermediate inputs and Z_e is firm-specific productivity. This production function encompasses the cobb-douglas production from Equation 1. I assume constant-returns-to-scale with firms having market power in the output market but no monopsony power. Furthermore, I assume that each establishment uses materials in fixed proportions to output, i.e. there is no technological change. In addition, there are no factor market frictions and any wedge arises from variable markups. First, rewriting the output growth in terms of input growth and technology [48]:

$$dy_e = \frac{F^e_L L_e}{F^e} dl_e + \frac{F^e_K K_e}{F^e} dk_e + \frac{F^e_M M_e}{F^e} dm_e + \frac{F^e_Z Z_e}{Z^e} dz_e$$

First order condition of cost-minimization implies for any input J:

$$\frac{F^e_J J_e}{F^e} = \mu_e s_{Je}$$

noindent where input share in nominal gross output is given by $s_{Je} = \frac{P_J X_J}{PQ}$ and μ_e is the firm-specific markup. Then, it follows that

$$dy_e = \mu_e[s_{L_e} dl_e + s_{K_e} dk_e + s_{M_e} dm_e] + \frac{F^e_Z Z_e}{Z^e} dz_e$$

Then, substituting the above equation to the definition of aggregate productivity:

$$APG = \sum_{e=1}^{E} D_e(\mu_e - 1)dx_e + \sum_{e=1}^{E} D_e dz_e$$

where D_e is the firm's share of nominal value added. Decomposing the equation further to capture allocative efficiency:

$$APG = (\bar{\mu} - 1)\sum_{e=1}^{E} D_e dx_e + \sum_{e=1}^{E} D_e(\mu_e - \bar{\mu})dx_e + \sum_{e=1}^{E} D_e dz_e$$

[48] See Basu and Fernald (2002) for an explanation on how this productivity growth measure is proportional to welfare gains. See Petrin and Levinsohn (2012) for a decomposition with market frictions besides market power.

where $\bar{\mu} = \sum_{e=1}^{E} D_e \mu_e$. As discussed in Hulten (1978), if all firms were perfectly competitive and there are no factor-market distortions, only the last term of technical efficiency changes matter. The second term, markup-reallocation term, represents shifts of inputs toward uses with higher social valuations, i.e. reallocating inputs from low- to high-markup firms shifts resources towards uses where consumers value them more highly. If there is correlation between market power and firm's inputs, then imperfect competition affects aggregate productivity even if the average markup is small. Finally, if average markup is greater than one, then the markups distort labor-leisure choice. Consumers would prefer to provide more capital and labor and consume the extra goods they could produce, since the utility value of these goods exceeds the disutility of producing them. Note that the final term can be decomposed further like Olley and Pakes (2002) with an average technical efficiency measure and a correlation term. There are one diffrence though: the weights are based on firm's share of value added instead of revenue share. Hence, firms that use relatively more intermediate inputs will have smaller weights now. Finally, I ignore the distortions in input markets in the above proof because of my focus on the role of markup distortions. In fact, changes in APG between concentrated and competitive markets could be largely explained by markup reallocation alone. A reduction in primary input distortions from demand/supply shocks where input distortion term R_x is given by:

$$R_x = (\bar{\mu_e} - 1) \sum_{e=1}^{E} D_e s_{X_e} \frac{P_{xe} - P_x}{P_{xe}} dx_e$$

Here, aggregate productivity increases when inputs are reallocated to firms with higher marginal products.

I.B: Data Cleaning

I drop firm observations that do not report any labor, inputs or outputs in a given year. Second, I also drop establishments that report less than 4 employees with the idea that these are self-employed businesses. Next, I exclude extreme outliers in terms of key production variables such as revenue, employment, and input use. To calculate labor share, material share and capital share for each product within a multiproduct firm, I use the revenue share of the product. For firms that report multiple price for the narrow product category within a year, I take the average. I treat each observation as a firm with the caveat that there is a small possibility that it is part of a multi-unit firm. Capital is measured at the average of the starting and ending value within a given year. Material inputs include expenditures on fuel and electricity. Labor includes temporary contract workers. All aggregated material input costs are deflated using the IO table weighted nic2digit input index. Wages are delfated with GDP deflator and Capital is deflated with capital cost deflator. All product and industry codes are in the ASICC - 04 and NIC - 04 classification respectively using concordance tables provided by the Central Statistics office.

Misreporting in prices - Even though there is supposed to be consistency in the unit of measurement of a narrowly defined 5-digit product, firms use other units while reporting that leads to abnormally high or small price values. For example, some firms might report price of milk in liters even though the questionnaire asks for kiloliters. To address this discrepancy, I initiate an algorithm based on Agarwal(2016) that addresses the misreporting as follows: 1) For each product in a given year, I drop the values that are smaller than 3rd percentile and larger than 97th percentile. 2) For each firm with at least 2 observations in product x prices, I drop firms whose prices exhibit a 10 fold increase. 3) I rank the prices of each product in a given year and calculate the difference. If the difference between two price observations is higher than 20x, I split the observations into two different product categories. Results are robust to alternate cutoffs.

Appendix I.C: Model of Variable Markups

In the section, I present a model that can explain the observed distributional effects in this paper. Within each market, a finite number of heterogeneous firms are engaged in imperfect competition with variable markups and input costs à la Atkeson and Burstein (2008). It includes a generalized version of quantity competition in which firms do not fully pass-through changes in their marginal costs to their prices because their optimal markup depends on their market share. Here, I am assuming production functions and consumer preferences are Cobb-Douglas in line with the benchmark model in this literature.

Firms play a static game of quantity competition. Specifically, each firm $e = 1, ..., E$ chooses its quantity q_{ie} sold in market i taking as given the cost of intermediate inputs from upstream market j, final consumption price p and quantity Q of the final product and a size-dependent distortions.[49] Note that under this assumption, each firm does take into account the fact that it has market power and that market price index P_i and quantities Q_i vary when that firm changes its quantity q_{ie} where:

$$Q_i = (\sum_{e=1}^{E} {q_{ie}}^{\frac{\sigma_i - 1}{\sigma_i}})^{\frac{\sigma_i}{\sigma_i - 1}}$$

$$P_i = (\sum_{e=1}^{E} P_{ie}^{1-\sigma_i})^{\frac{1}{1-\sigma_i}}$$

Given these preferences, each firm faces the demand curve $q_{ie} = {P_{ie}}^{-\sigma_i} P_i^{\sigma_i - 1} Q_i$.

With cournot competition, Atkeson and Burstein (2008) implies the following:

$$\mu_{ie} = \frac{\epsilon(s_{ie})}{\epsilon(s_{ie}) - 1} = \frac{\sigma_i}{\sigma_i - 1} \frac{1}{1 - s_{ie}}$$

where $s_{ie} = \frac{p_{ie} q_{ie}}{P_i Q_i}$ is the market share of the firm and $\epsilon_{ie} > 1$ is the subjective demand elasticity [50]. In this framework, demand elasticities vary with quantity and firms vary in productivity leading to resource allocation across different types of firms and variable markups within the market. The important take-away from this is that more productive firms will produce higher quantity but also charge higher markups as a larger share of market's revenue s_{ie} implies lower demand elasticity [51].

[49]Most of the size-dependent distortions are on either capital or labor. To simply the model, here I am assuming that a unit of output q requires a unit of capital or labor that is constant and varies with firm productivity. Hence, a tax of capital or labor can be throught of as a tax on output.

[50]The subjective demand elasticity is the weighted average of the elasticity of substitution across varieties σ_i and the elasticity of substitution across sector which is here equal to one.

[51]This implication that inverse demand elasticity is increasing with quantity is common in the literature using other models as well (see Mrazova and Neary (2013) and Krugman (1979) for example.)

Proposition 1: *Market share s_{ie} captures the market power of firm within a market in terms of its ability to charge a markup*

The intuition as follows: Large firms have a higher market share and thus they can use this higher market power to charge even higher markups, which in turn aggregate to a higher industry-level markup. Then, pass-through elasticity is given by:

$$\frac{\partial p_{de}}{\partial p_r}\frac{p_r}{p_{de}} = \underbrace{(\frac{\sigma_d(1-s_{de})+s_{de})}{\sigma_d})}_{\text{markup adjustment}} \underbrace{(\gamma_{ie}\omega_{rd})}_{\text{marginal cost share of input}}$$

Then, when there is an uniform input-cost shock such that $p_r \to 0$:

$$\frac{\partial \mu_{de}}{\partial p_r} < 0 \text{ and } \frac{\partial \mu_{ue}^2}{\partial p_r \partial s_{ue}} > 0$$

Following an input cost decrease, markups increase and firms with larger market share increase their markups more leading to higher average markup and markup dispersion.This heterogeneous response to input cost shocks would then logically generate a heterogeneous quantity response whereby quantity dispersion falls. While the framework presented in this paper relies on the strong Cobb-Douglas or the constant elasticity-of-substitution functional assumptions commonly imposed in the literature, these results are not unique to the model presented here. A monopolistic competitive framework as in Krugman would yield similar results albeit through a different mechanism as well as complex variable elasticity of subsitution models (Arkolakis et al. (2015), Dhingra and Morrow, (2012)).

I.D: Production function estimation

I use the "supply-side" approach based on Hall(1986), De Loecker and Warzynski(2012) and De Loecker et al.(2016) to calculate markups. The main advantage of this methodology is that it does not commit to a particular demand or market structure but adopt an empirical specification that nests the main models used in trade and industrial organization, including those that generate variable markups. Hence, it captures the reallocation effects as well as changes in residual demand and costs on markups. Apart from cost minimization, this method assumes that input adjustment is costless and that firms in the same industry face exogeneous input prices. Let Y_{it} be the deflated firm-level sales. I use a flexible translog, gross-output production function whereby the coefficients are the same across firms within an industry but the output elasticities, and consequently markups, vary by plant and by year within an industry. Taking logs of the gross output, translog production function:

$$y_{it} = \beta_l l_{it} + \beta_{ll} l_{it}^2 + \beta_k k l_{it} + \beta_{kk} k_{it}^2 + \beta_x x_{it} + \beta_{xx} x_{it}^2 + \beta_{lk} l_{it} k_{it} + \beta_{lx} l_{it} x_{it} + \beta_{kx} k_{it} x_{it} + \beta_{lkx} l_{it} k_{it} x_{it} + z_{it} + \epsilon_{it}$$

where z_{it} is firm's quantity - productivity TFP at time t and ϵ_{it} the measurement error. I follow the literature and control for the simultaneity and selection bias inherently present in the estimation of the above equation [52]. I rely on a control function approach, paired with an AR(1) process for productivity to estimate the output elasticity of the inputs [53]. The resulting product-firm-year variation in markup is driven by both changes in plant-level revenue share for the variable input and changes in the mix of inputs used for production. Following Ackerberg et al. (2015), all production function parameters are estimated using second-stage moments for each 2-digit industry separately. Importantly, I use control function to correct for endogeneity in input prices of materials as in Deloecker et al. (2016). I incorporate dummies for firms around the capital cutoff as well as dummy for reserved status as state variables to the firms' production decisions. This allows firms that are not SSI or reserved to have different production technology. These dummy variables are used in the first step of the production function estimation. Given the large share of multiproduct firms, I also control for probability of being multiproduct and follow the approach in Deloecker et al. (2016) to allocate input shares to the respective products. The derived estimates are quantitatively similar to estimates from Deloecker et al. (2016) who uses the Prowess Public firm-level data

[52] Simulatenity bias arises for example if productivity decrease leads to decrease in production inputs. Conversely, if an establishment's factor productivity increases, it will lead to increase in the used inputs over the same period.

[53] Fixed effects method resolves the problem of simultaneousness by fixing in the panel sample the error term in the observed time interval, whereas the instrumental variables method avoids the correlation between productivity and input choices.

from India.

Table I.D.1: Average Output elasticities, By Sector

Sector	Labor	Capital	Material	Returns to Scale
15 Food products and beverages	0.18	0.10	0.74	0.97
17 Textiles	0.093	0.091	0.79	1.00
18 Wearing apparel	0.10	0.098	0.81	1.03
21 Paper and paper products	0.18	0.056	0.79	1.02
23 Coke, refined petroleum products	0.038	0.12	0.87	1.02
24 Chemicals	0.20	0.10	0.70	0.99
25 Rubber and Plastic	0.10	0.13	0.84	1.06
26 Non-metallic mineral products	0.47	0.09	0.48	1.04
27 Basic metal	0.06	0.09	0.83	0.98
28 Fabricated metal products	0.15	0.083	0.77	1.04
29 Machinery equipment	0.24	0.036	0.80	0.98
31 Electrical machinery	0.20	0.091	0.75	1.05
34 Motorvehicles	0.16	0.097	0.76	1.07
Total	0.21	0.094	0.72	1.00

Note: The table reports the average output elasticities by sector with respect to each factor of production for the translog production function for all firms for the sample 2000 - 2013. The last column report the returns to scale which is the just of the output elasticities. Observations are trimmed above and below the 3rd and 97th percentiles for returns to scale within each sector.

Table I.D.2: Markups, By Sector

Sector	Mean	Median
15 Food products and beverages	1.37	1.06
17 Textiles	1.99	1.54
18 Wearing apparel	2.35	1.81
21 Paper and paper products	1.32	1.06
23 Coke, refined petroleum products	2.15	1.74
24 Chemicals	1.71	1.12
25 Rubber and Plastic	1.50	1.15
26 Non-metallic mineral products	2.14	1.55
27 Basic metal	1.76	1.20
28 Fabricated metal products	1.72	1.20
29 Machinery equipment	1.57	1.17
31 Electrical machinery	1.60	1.13
34 Motorvehicles	1.86	1.34
Total	1.69	1.21

Note: The table reports the mean and median markups by sector for the sample 2000 - 2013. Observations are trimmed above and below the 3rd and 97th percentiles for markups within each sector.

Table I.D.3: Markups and marginal costs of IO linked markets

	(1)	(2)
	log (marginal cost)	log (markup)
Outputshock	0.0310	0.0211***
	(0.0392)	(0.00796)
Inputshock	-0.0601**	-0.00508
	(0.0294)	(0.00328)
Number of Clusters	2232	2232
Observations	31049	31049

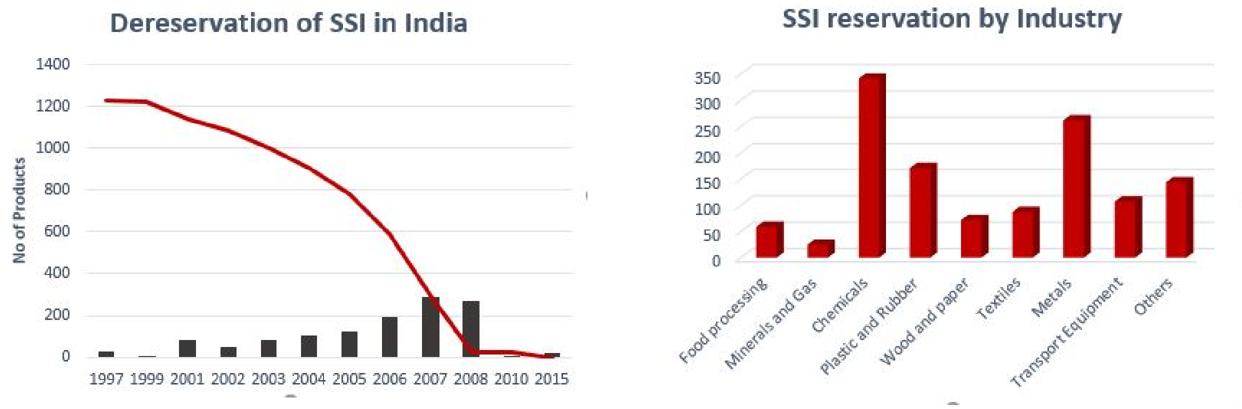
Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table displays resuls from the firm-level regressions using (2). *UpDereserv* is a dummy variable that takes a value 1 when the first reserved output market is removed from the list of reserved products. Regressions are weighted by the sampling multipliers from ASI and errors are clustered at the product - level. $^{*}p < 0.10$, $^{**}p < 0.05$,$^{***}p < 0.01$

I.E: Additional Tables

Figure I.E.1: Timeline of Dereservation Policy



Note: The figures displays the timeline of De-reservation and the variation in reserved products across industries in India. A total of 1256 products were reserved for SSI before 1997.

Table I.E.2: Pairwise Correlation Matrix across firm performance variables

	Productivity	Marketshare	Marginal Costs	Markups	Prices
Productivity	1.00				
Marketshare	0.16	1.00			
Marginal Costs	-0.33	-0.12	1.00		
Markups	0.12	0.070	-0.22	1.00	
Prices	0.036	0.095	0.97	0.070	1.00

Note: The table reports the pairwise correlation across the following firm performance variables: productivity, marketshare, marginal costs, markups and prices. All variables are expressed in logs. Markups, prices and marginal costs vary at the firm-product level, marketshare and productivity vary at the firm-level. Observations are trimmed above and below the 3rd and 97th percentiles for output prices within each sector.

Table I.E.3: HHI, By Sector

Sector	Mean	Median
15 Food products and beverages	6.54	1.42
17 Textiles	11.16	3.67
18 Wearing apparel	6.77	1.37
21 Paper and paperr products	11.19	2.63
23 Coke, refined petroleum products	17.15	7.13
24 Chemicals	18.21	6.56
25 Rubber and Plastic	14.16	4.53
26 Non-metallic mineral products	9.27	1.95
27 Basic metal	13.90	4.99
28 Fabricated metal products	15.77	6.25
29 Machinery equipment	16.94	8.06
31 Electrical machinery	13.50	4.09
34 Motorvehicles	17.06	5.83
Total	12.55	3.69

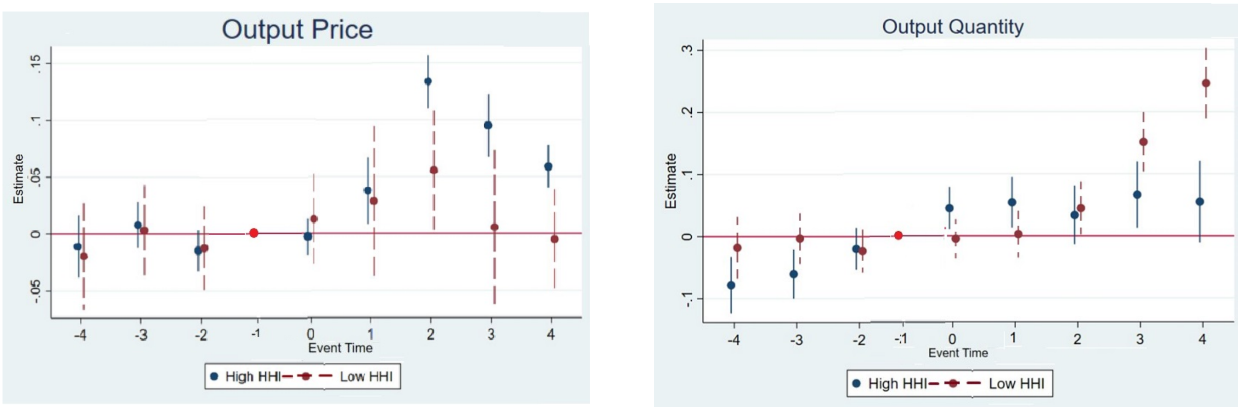
Note: This table presents the average and median HHI for a product market within each industry.

Table I.E.4: Market shares

Sector	Mean	Median
Market share	0.044	0.0025
Mean highest share	0.21	0.17
75th p.c. share	0.018	
95th p.c. share	0.125	
99th p.c. share	0.328	

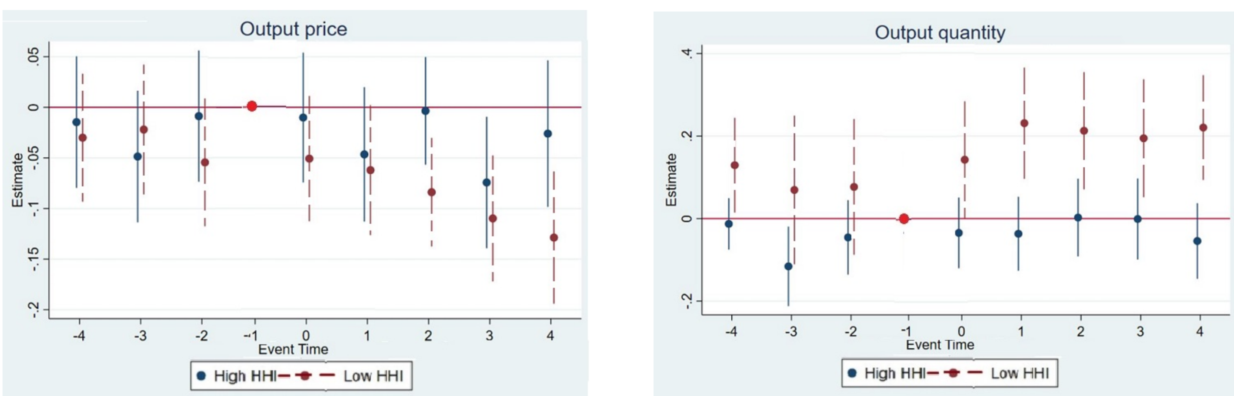
Note: This table presents some summary statistics on the market share distribution for the whole sample

Figure I.E.5: Upstream Effects by concentration



Note: The figures display the coefficients and 95 percent confidence intervals for the β_m coefficients in using the event-study regression specification (1) with $UpDereserv$ dummy as the independent variable. Low HHI includes all markets with HHI below 25th percentile and high HHI includes all markets with HHI above 75th percentile. Output prices and quantity are at the firm-level. Outcome variables are logged and observations are trimmed above and below the 3rd and 97th percentiles for output prices within each market. I impose the normalization that $\beta_{-1} = 0$ and errors are clustered at the product -level.

Figure I.E.6: Downstream Effects by concentration



Note: The figures display the coefficients and 95 percent confidence intervals for the β_m coefficients in using the event-study regression specification (1) with $DownDereserv$ dummy as the independent variable. Low HHI includes all markets with HHI below 25th percentile and high HHI includes all markets with HHI above 75th percentile. Output prices and quantity are at the firm-level. Outcome variables are logged and observations are trimmed above and below the 3rd and 97th percentiles for output prices within each market. I impose the normalization that $\beta_{-1} = 0$ and errors are clustered at the product -level.

Table I.E.7: Downstream effects by the product-level exposure

	(1)	(2)	(3)	(4)
	ln(quantity)	ln(revenue)	ln(quantity)	ln(revenue)
$DownDereserv * Exposure^{Down}$	0.00299***	0.000817	0.00312***	0.000956***
	(0.000720)	(0.0006202)	(0.000721)	(0.000619)
Year FE	Yes	Yes	Yes	Yes
State-specific trend	No	No	Yes	Yes
Number of Clusters	4155	4155	4155	4155
Observations	239893	239893	239893	239893

Note: Results from establishment-level regressions and restricted to incumbents. *DownDereserv* is a dummy that equals 1 when the input measured at aggregated product-level is dereserved irrespective of the firm resporting the input and 0 otherwise. Columns (3) and Colums (4) include state specific linear time trends. Errors are robust clustered at the product- level. Standard error in parentheses.$^{*}p < 0.10$, $^{**}p < 0.05$,$^{***}p < 0.01$

Table I.E.8: Upstream effects by market share

	(1)	(2)	(3)	(4)	(5)	(6)
	ln(price)	ln(output)	ln(price)	ln(output)	ln(price)	ln(output)
	All firms	*All firms*	*Low HHI*	*Low HHI*	*High HHI*	*High HHI*
UpDereserv	0.0193***	0.0538	0.0119	0.0353	0.0478*	0.0798**
	(0.00718)	(0.0343)	(0.00960)	(0.0465)	(0.00250)	(0.0315)
UpDereserv*Mrktshare	0.00206	0.0420***	0.00687	0.0577***	0.00664	0.0353***
	(0.00287)	(0.00708)	(0.00466)	(0.0125)	(0.00472)	(0.00973)
Firm x Product FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Clusters	5010	5010	4071	4071	4300	4300
Observations	180324	181974	100833	92415	79491	89559

Note: Results from establishment-level regressions and restricted to incumbents. *UpDereserv* is a dummy that equals 1 when the reserved product market that uses intermediate inputs from the upstream is dereserved and 0 otherwise. Marketshare is standardized. Errors are robust clustered at the product- level. The regressions exclude outliers in the top and bottom 3rd percent of the markups within each year-product market. Standard error in parentheses.$^{*}p < 0.10$, $^{**}p < 0.05$,$^{***}p < 0.01$

Table I.E.9: Downstream effects by marketshare

	(1)	(2)	(3)	(4)	(5)	(6)
	ln(markup)	ln(output)	ln(markup)	ln(output)	ln(markup)	ln(output)
	All firms	*All firms*	*Low HHI*	*Low HHI*	*High HHI*	*High HHI*
DownDereserv	-0.00407	0.0467***	0.0104	0.0479***	-0.00898	0.0327**
	(-0.64)	(4.83)	(0.92)	(3.00)	(-0.94)	(2.26)
DownDereserv*Mrktshare	0.00558	0.00589	0.01016	0.1057***	0.00931*	0.00361
	(1.52)	(1.37)	(0.48)	(3.39)	(1.70)	(0.84)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Clusters	44331	52791	23413	28695	26210	31398
Observations	118032	150014	48775	60140	51421	64496

Note: Results from establishment-level regressions and restricted to incumbents. *DownDereserv* is a dummy that equals 1 when the intermediate input market is dereserved and 0 otherwise. Errors are robust clustered at the firm-level. The regressions exclude outliers in the top and bottom 3rd percent of the markups within each year-product market. Standard error in parentheses.$^{*}p < 0.10$, $^{**}p < 0.05$,$^{***}p < 0.01$

Effects on firm entry

My analysis assumes static gains i.e. there is no substantial entry that changes market structure. Looking at the total number of establishments within a product market, I do not find significant changes either in the upstream or downstream but dereserved markets witness an increase in the number of establishments. The change in establishments both capture the exit of firms and entry of new firms. It is possible that for the upstream and downstream, while there was no net entry, there was some exit and entry. To rule this out, I look at the entry rate of firms into a product market in the survey before and after dereservation. I again find no evidence of increased entry except for the dereserved markets itself.[54] These results lend credibility to my assumption of fixed competitive structure in the linked markets.

Table I.E.10: Impact of dereservation on firm turnover

	(1)	(2)	(3)
	ln(estab)	ln(estab)	ln(estab)
UpDereserv	0.0134		
	(0.0199)		
Dereserv		0.0868**	
		(0.0383)	
DownDereserv			-0.0118
			(0.0140)
	ln(entryrate)	ln(entryrate)	ln(entryrate)
UpDereserv	0.0102		
	(0.0100)		
Dereserv		0.0806**	
		(0.0382)	
DownDereserv			0.00626
			(0.00826)
Products	4368	4368	4368
Observations	27683	27683	27683

Note: Results from product-level regressions. The dependent variable $ln(estab)$ is the total number of establishments making a product. *Dereserv* is a dummy variable that takes a value 1 when the product is removed from the list of reserved products. *DownDereserv* is a dummy that equals 1 when the downstream customer uses a dereserved input and 0 otherwise. *UpDereserv* is a dummy that equals 1 when the reserved product market that uses intermediate inputs from the upstream is dereserved and 0 otherwise. Errors are robust clustered at the product- level and weighted by the initial labor shares. Standard error in parentheses.$^{*}p < 0.10$, $^{**}p < 0.05$,$^{***}p < 0.01$

[54] I also check if the share of sales by entrants is different and find no significant differences in the linked markets.

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Chapter II

Sparking private investment in infrastructure: Evidence from a generator subsidy program

1. Introduction

Developing countries often suffer from poor infrastructure as a result of insufficient public investment. This has stifled their ability to sustain economic growth and development. One such example of poorly managed and underfunded infrastructure is public utilities. Insufficient electricity generation and inadequate transmission infrastructure combined with growing demand has resulted in chronic electricity shortages in many developing countries. The resulting power blackouts are cumbersome for everyone, particularly the manufacturing firms. Energy is necessary for the operation of productive capital in the manufacturing sector. Without continuous access to electricity, firms halt production and reallocate inputs or resort to self-generation. These firms face significant output and productivity losses as a result. In addition, fluctuations in voltage causes machine damage, material losses, and variations in product quality, resulting in higher manufacturing costs. While self-generation mitigates many of the negative impacts associated with unreliable power supply, its adoption has not been homogenous. Smaller firms are more adversely affected by power shortages as they are less likely to own and operate generators. Incidentally, developing countries also have large number of small firms holding most of the employment. For example, in India, 80 percent of employment is in unregistered manufacturing sectors where firms have less than 10 employees. The various disadvantages that small firms in India face in terms of operational, financial and technological constraints are potentially impeding their ability to grow under substandard quality of public infrastructure. Hence, understanding the obstacles to private infrastructure investment and ways to overcome those constraints is critical to securing private enterprise growth and sustainability.

This paper studies the impact of a generator subsidy program in Tamil Nadu, India. The design of the program provides clean quasi-experimental variation in the availability of the subsidy to some buyers and not others, allowing for a transparent treatment-to-control comparison. Importantly, the subsidy was directed towards Small and Medium Enterprises (SME), thereby shedding light into the low rates of investment in private remedial infrastructure amongst small firms.

To motivate the empirical analysis, I present a model of investment in remedial infrastructure that separates the fixed cost and marginal cost of adoption which vary with firm's financial health and electricity intensity of the industry respectively. Firms also face differential probability of a binding electricity shortage that can explain the lower capacity utilization within smaller firms. Using the Annual Survey of Industries (ASI) firm-level panel data and exploiting the launch of a generator subsidy program, I test the predictions of this model. I use a triple difference estimator that exploits geographic, temporal and subsidy-exposure variation. I find that the probability of firms self-generating any electricity increases by 14 % amongst the intent-to-treat firms. Firms that adopt generators under subsidies are generally smaller, more energy intensive and have lower financing costs with longer operating cycles. Blackouts induced delay in completing the production batch reduces the number of goods produced by less in firms with longer operating cycles. These firms consequently have relatively lower rates of capacity utilization and lower returns (exclusive of the subsidy). Lowering the fixed costs by 25 % allows these firms to attain the same level of returns to investment as firms that invest without subsidies. These results highlight the potential for a fixed cost subsidy to mitigate the high marginal costs and smaller potential gains from self-generation that impede smaller firms from investing in remedial infrastructure. However, because the fixed cost of a generator is large, a partial subsidy is insufficient to overcome higher financing costs that small firms face in developing countries. Finally, I find no significant crowding out effects from this investment. Hence, one-time subsidies can be effective in nudging firms on the margin to invest in remedial infrastructure. These results are consistent with existing evidence on the impact of power shortages on various measures of firm performance and underline the potential for targeted policies to alleviate the costs of unreliable power supply.

The contribution of this paper is two-fold. Firstly, the existing literature has identified the negative impacts of unreliable power supply and increased energy costs on firm productivity. For example, Allcott, Collard-Wexler and O'Connell (2015) estimate that India's average reported level of shortages reduces the average plant's revenues and producer surplus by five to ten %.[55] There is also consistent evidence of higher costs from power shortages for smaller

[55]In addition to lost output, electricity shortages have also been shown to reduce firm investment (Reinikka

firms and firms in energy intensive sectors.[56] Contrary to the existing literature that focuses on costs of unreliable electricity supply and the resulting firm behavior, my paper looks at how reducing costs of remedial infrastructure affects firm behavior. To my knowledge, this is the first paper to conduct impact evaluation of a policy that can address the differential costs of electricity shortages and increase the competitiveness of smaller firms. The program was expanded to a bigger subsidy and capacity limit in 2014, allowing more firms to adopt or expand their generator capacity. The state government has spent around Rs. 895 million in the generator subsidy program between 2010-11 and 2014-15. Shortages are a problem but developing countries like India typically have numerous pressing policy issues. Quantifying the effectiveness of this subsidy program will help to allocate the limited funds optimally.

Secondly, this paper explores the lack of investment amongst small enterprises. In fact, Foster and Steinbuks (2009) find that firm characteristics are more important in determining own generation rather than unreliable power supply. Differential rates of generator adoption amongst firms has also been attributed to credit constraints, firm size, productivity, price of electricity, industries' electricity–intensity and competition.[57]. This paper attempts to disentangle the factors that explain the low rates of investment in complementary infrastructure within small firms. For example, existing firm-level studies focusing on returns to capital have attributed the low levels of investment amongst small enterprises to credit constraints and find high rates of return on capital for these firms.[58] Contrarily, studies also argue that small firms in fact are not limited by constraints, but it is the large firms that face binding constraints in developing countries (Hsieh and Klenow (2009)). I do not find evidence of high returns to remedial capital for small firms. This can explain the lack of investment in private complementary infrastructure amongst these firms and mitigates concerns of lack of credit or irrational firm behavior such as time inconsistency or naivete as the binding constraints. These results have policy implications for countries, who are endowed with a large share of small firms, to revive or accelerate investment.

With the rapid growth and development of many Asian and African countries, their intrinsic infrastructural drawbacks have become an important issue. Literature has focused on the economic and welfare losses of these failures including but not limited to transportation, property rights, labor regulations, financial and energy infrastructure - both at the aggregate

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and Svensson (2002)), outsource the production of intermediate goods (Fisher-Vanden, Mansur and Wang (2012)), reallocate production to non-shortage periods (Zuberi 2012) and switch to less electricity-intensive production processes that are less technologically advanced (Abeberese, 2016).

[56]See Allcott, Collard-Wexler and O'Connell (2015), Alam (2013) and Alby, Dethier and Straub (2012)

[57]See for example Allcott, Collard-Wexler and O'Connell (2015), Steinbucks (2008), Rud(2012b) and Alby, Dethier and Straub(2010)

[58]See for example Banerjee (2001), Banerjee and Duflo (2014), Beck et. al (2005), de Mel et al. (2008)

level and more recently, focusing on within-country variation using micro data. [59] My paper is related to the larger literature of infrastructural failures in the emerging economies and factors that drive firm-level investment and growth in this context.

The findings of this paper are also related to the literature on factors that drive growth and profitability of small firms in general (see Svyverson (2011), de Mel et al. (2008), Bruhn, Karlan and Schoar (2013)) and the work on effects of institutional distortions and structural reforms. Shaun Mcrae (2013) finds that subsidies are distortionary as they discourage investment in infrastructure and trap households and firms in a nonpaying, low-quality equilibrium in the electricity sector in Colombia. In this paper, I do not find evidence of such distortionary effects under fixed cost subsidies for private investment.

The rest of the paper is organized as follows. Section 2 provides the context with a brief overview of the electricity sector in India and the subsidy program in particular. Section 3 describes the conceptual framework. Section 4 describes the data and outlines the empirical strategy. Section 5 presents the empirical results. Section 6 concludes with policy implications.

2. Context

Indian Electricity Sector

India has experienced power shortages averaging at around 7 percent of demand for more than two decades. Frequent load shedding means that many consumers go without power for hours. While the average electricity situation has improved over the past few years, reliability issues still exist and improvements have not been uniform across regions. Figure II.1 shows the variability in energy shortages across states over time. In the World Bank Enterprise Survey 2014, 16% of Indian business managers stated electricity as the biggest obstacle to the operations of their establishment. Around 47% of the business managers consider electricity to be at least a moderate obstacle to their operations. Compared to the WBES 2004 where 56% of business managers mentioned electricity to be a moderate obstacle, there seems to have been little improvement in the electricity situation in India over a decade in the perception of business managers.

GDP of India is growing at an average of eight % and reliable power supply is essential to maintaining this momentum. Supply has been unable to keep up with the growing demand resulting in chronic electricity shortages. Infrastructural changes that would allow for reliable power supply comparable to the developed world would require large investments and take a long time to materialize. In fact, historically, India has struggled to meet the capacity targets and achieve reforms in the electricity sector. For example, after the 1991 liberalization, 200

[59] See for example Calderon and Serven (2004), Dinkelman(2011), Donaldson(2016), Jensen (2007), Lipscomb et. al (2013), Rud(2012a), Straub(2008)

Memoranda of Understanding were signed between the government and investors to build 50 gigawatts of generation capacity, but less than four gigawatts of this was actually built (Bhargava and Subramaniam (2009)). Similarly, only half of the 71 gigawatts of capacity targeted to be built between 1997 and 2007 was actually achieved (CEA 2013a). It is critical to help manufacturing firms survive, thrive and grow in the meanwhile. In FY 2012-13, firms reported average loss in sales of around 5 percent due to power outages with 26 outages per year and each outage lasting about 2 hours on average. State governments regularly pursue short term solutions to alleviate the costs such as purchasing electricity from the private sector or from other states at high prices, subsidizing grid electricity and giving diesel subsidies.

Many of these subsidy programs are recurring and can become costly over time. Another approach to help firms deal with blackouts is to enable them to generate electricity themselves, and a one-time subsidy to acquire the capital for self-generating could be an effective solution. Self-generation is often considered as the second-best solution to coping with electricity shortages because of the relatively high costs of operating them and associated negative externalities.[60] However, it is noteworth that because these generators only operate a small fraction of the time, they do not greatly affect the overall average cost of power to industry. Thus, while self-generation is not an alternate to reliable power supply from public grid, it can be a viable alternate to unreliable power supply. Secondly, it is a readily available technology that is critical for the short term. While subsidies are generally seen as distortionary, the laissez faire approach to coping with electricity shortages would result in small firms disproportionally bearing most of the burden, leading to firm exit and increasing the inequality between firms. On the other hand, generator subsidies can lead to over-adoption and utilization of generators. The lessons learnt from the generator subsidy policy in India can have policy implications for other developing countries which continue to face severe and worsening power shortages. For example, in Pakistan, the percentage of firms considering electricity shortage as the largest obstacle increased from 44% in 2007 to 66% in 2010 according to the WBES. More importantly, my findings can provide insights into the investment behavior of firms, particularly in private infrastructure and policies that can drive investment within these firms.

Generator Subsidy Program in Tamil Nadu

In this paper, I study a subsidy program in Tamil Nadu (TN), a state in India. This generator subsidy program is a part of the continuing high subsidy support provided by the

[60] Bhattacharya and Patel (2007) estimate the cost per kilowatt-hour incurred by a firm in generating its own electricity in India is 24% higher than the price paid for the electricity provided by power utilities. Foster and Steinbucks (2009) find the costs of own-generation are about three times as high as the price of purchasing (subsidized) electricity from the public grid in Africa.

TN Government. Historically, the state has a two-party system: DMK and AIADMK and until 2016, the parties have alternated into power every 5 years, due to the anti-incumbency voting by the citizens. This has incentivized the parties in power to seek short-term solutions to problems, particularly providing many subsidies and "freebies". In 2008, with pressing electricity shortages, the DMK government began offering subsidies on the fixed cost of generator adoption for their captive use to all new and existing Micro, Small and Medium enterprises (MSMEs). The subsidy was set at 25 % with a maximum of Rs. 150,000 and 125 KvA for any generator purchased after 11.11.2008. The subsidy is not binding for most MSMEs as their generator requirements are usually less than 125 kVA. Furthermore, a 125 kVA generator costs around Rs. 600,000 indicating that most firms will receive the maximum subsidy rate of 25 %. MSMEs could use the subsidy to buy generators for the first time, add to the capacity or replace old generators. For additional capacity, the subsidy would be limited to the proportionate cost of the excess capacity of the new generator over and above the capacity of the replaced one.[61] The subsidy was well- publicized and the program was structured to make evasion difficult. Application for generator subsidy should be within six months from the date of purchase of the generator set or date of installation of the generator i.e. the date of issue of safety certificate by the Electrical Inspectorate Department, whichever is later. Subsidy amount would be released to the financial institution in case a term loan is availed by the MSMEs. For self-financed enterprises, the subsidy shall be directly released to the enterprises. AIADMK came into power in the 2011 elections and the program was further expanded in 2014 to cover up to Rs. 500,000 and 320 KvA for a 25% subsidy. The government has spent around Rs. 420 million in 2015-16 on the generator subsidy program.

TN offers a proper setting for the empirical analysis. Firstly, it is the second largest economy in India, contributing to around 8% of India's GDP (Ministry of Statistics and Program Implementation, 2015). Tamil Nadu has a strong and diversified manufacturing sector ranging across automobiles and auto components, chemicals, textile products, etc. In fact, Tamil Nadu ranks first among the Indian states in terms of number of factories and industrial workers (India Brand Equity Foundation, 2015). Hence, it offers a large and diversified sample of firms to study the heterogeneous impact of subsidies. Additionally, the state has a generally well-developed infrastructure with an excellent road and rail network. Electricity shortages seem to be the most prominent infrastructural failure in this state. Finally, I control for confounding effects of electricity shortages using data on peak demand and supply and synthetic control to get a control group of states where firms face similar

[61]Unfortunately, the data does not allow me to capture the effects on existing generators. If the program allowed firms to expand their capacity or upgrade generators, then my estimates can be thought of as the lower bound as I mainly focus on the newly adopted firms

trends in electricity shortages. As an additional robustness check, I set firms from Karnataka as the only control group since Karnataka exhibits similar trends to Tamil Nadu in terms of energy shortages (see Fig II.1).[62]

SME Sector in India

MSMEs are an integral and important contributor of economic growth and development in India. According to the estimates of the Ministry of MSME, Government of India, the sector generates around 100 million jobs through over 46 million units situated across the country. They play a salient role in the social and economic restructuring of India, contributing to 38% of the nation's GDP and 40% and 45% of the overall exports and manufacturing output, respectively. MSMEs are engaged in the manufacturing of over 6,000 products ranging from traditional to hi-tech items. This electricity shortage is particularly devastating for these small – scale manufacturing firms who rely on the power grid for production and lack the resources to insure themselves against these shocks.

MSMEs are classified based on their historical value of investment in plant and machinery. In manufacturing, micro enterprises are firms with investment up to Rs. 2.5 million, small enterprises are firms with investment above Rs. 2.5 million up to Rs. 50 million and medium enterprises are those with investment above Rs. 50 million and up to Rs. 10 million The generator subsidy program is a significant capital infusion, supporting at least 24% of its total investment for micro enterprises and up to 1.2 % for small enterprises.

3. Conceptual Framework

The model is an extension of the firm investment decision framework from Rust (1984) with firms facing constraints from two channels: credit market frictions and uncertainty of realizing the gains from investment. These are two of the most common explanations considered in the literature on firm investment behavior. Uncertainty is particularly relevant for remedial infrastructure investment decisions because this infrastructure is intended to insure firms against the risk of poor public power supply but its returns also depend on the uncertainty. Uncertainty in my model comes from binding electricity shortages and demand fluctuations. Remedial infrastructure is unique in that firms can operate without investing in this costly technology and incur the associated output losses. Hence, firms face a tradeoff of higher marginal costs and higher output with investment and lower marginal cost and lower output without investment. I present a static version of the model to get some testable predictions.

[62]This similarity arises from the fact that they are neighboring states with similar climates and annual rainfall that is essential for hydro power generation

Setup

Firms are profit maximizing risk neutral agents who have to decide whether to acquire self-generation capacity, $i \in 0, 1$. Each firm uses capital K, labor L, material M and electricity E with productivity *(A)* to produce Q units of output. I assume the production function is Leontif in electricity and a Cobb-douglas aggregate of K,L,M,A:[63]

$$Q = min\{AK^{\alpha_{\mathrm{K}}}M^{\alpha_{\mathrm{M}}}L^{\alpha_{\mathrm{L}}}, \frac{1}{\lambda}E\} \qquad (1)$$

I assume constant returns to scale on the aggregate, $AK^{\alpha_{\mathrm{K}}}M^{\alpha_{\mathrm{M}}}L^{\alpha_{\mathrm{L}}}$ such that so $\alpha_{\mathrm{K}} + \alpha_{\mathrm{M}} + \alpha_{\mathrm{K}} = 1$.

Electricity Shortages

Firms believe there will be a power outage with probability $\theta \subset [0, 1]$ in a given day and they observe this power outage before setting out production for the day. Firms can purchase electricity from the grid at price $P^{E,G}$ when there are no blackouts. To cope with electricity shortages, firms can self-generate electricity at price $P^{E,S}$ such that $P^{E,S} > P^{E,G}$. Furthermore, this involves a large fixed cost investment for the generators, F. Firms without generators will have no access to electricity during an outage, and thus zero output during these periods.

Given θ, firms also differ in their probability of experiencing a constraining shortage, $\mu \subset [0, 1]$ such that $\mu \leq \theta$. This captures the firm's ability to reutilize capacity, i.e. avoid production during blackouts by shifiting production to times when power off the grid is available or outsource certain stages of production, thereby reducing the dependency and losses from electricity. Zuberi (2013) finds that costs of electricity shortages are largest for firms facing high production targets (demand) during a period of frequent outages. All the firms in his sample are large scale manufactures with generator capacity. He suggests that firms with lower production targets are more likely/able to shift production to times with grid electricity, allowing them to lower costs by reducing the use of self-generation without losing sales. Secondly, Alam (2013) finds that firms in some industries are better suited to deal with chronic electricity shortages as they can shift their production technology. Hence, μ is a function of the industry in which the firm operates. The ability to reutilize capacity combined with the expensive generator operation can make self-generation an unattractive substitute to poor infrastructure quality for SMEs. I assume the costs of capacity requtilization is always lower than cost of self-generation so firms will always maximise capacity reuitlitzation before shifting to self-generation.

[63]See Allcott, Collard-Wexler and O'Connell (2016) for details on this production function and an alternative of Cobb-douglas aggregate of all inputs including electricity

Financial Constraints

Much of the literature focusing on investment has focused on financial constraints as the driver. There is also evidence that smaller firms are usually more constrained, hence we include this constraint in our model as well. Financing constraints are introduced from the model of Kaplan and Zingales (1997). The fixed cost of a generator, F, can be financed either with internal funds (A) or with external funds ($E = F - A$). The cost of internal funds equals the opportunity cost of capital, r, which is the market interest rate and set to equal to 1 (numeraire). If F $\geq$ A, then firms will have to borrow funds. Because of financial market imperfections, it is assumed that the use of these external funds generates an additional cost. The reduced form of this cost function is represented by g(E;η), where η is an unobservable measure of a firms wedge between the internal and the external costs of funds, reflecting the extent of agency or information problems. It is assumed that the total cost of raising external funds is convex in the amount of funds raised and it increases in η (i.e. $g_\eta > 0$. For the model to be well behaved, it is also assumed that cross-partial derivative of the total cost of raising external funds with respect to the amount of funds raised and the extent of the agency or information problems is positive $g_{E\eta} > 0$. The following is the cost of acquiring generator, $c(F)$:

$$c(F) = g(E;\eta) + F, \text{ where } F = A + E \qquad (2)$$

Subsidies will lower the fixed cost for firms that have sufficient internal funds by 0.25F. Also, for firms that have to borrow externally, subsidies will lower costs as the size of funds that need to be borrowed is smaller. However, if smaller firms face higher borrowing costs $g(.)$, then a sufficiently small subsidy might not be enough to overcome this impediment.

Firm's Problem

Firms make investment decisions under the following profit maximizing conditions:

$$\Pi(q,0) = (1-\theta\mu)[1-\gamma(0)-\nu]q^G$$

$$\Pi(q,1) = (1-\theta\mu)[1-\gamma(0)-\nu]q^G + \theta\mu[1-\gamma(1)-\nu]q^S - c(F)$$

where

$$\gamma(0) = \lambda P^{E,G} \text{ and } \gamma(1) = \lambda P^{E,S}$$

ν is the marginal cost of operation that excludes the price of electricity and varies with firm. γ is the marginal cost from electricity usuage with $\gamma(0)$ capturing the marginal cost from power off the grid and $\gamma(1)$ being the marginal cost from self-generation. λ is the electricity intensity of the industry. q^G is the quantity produced on the grid and q^S is the quantity produced with self-generation of electricity.

Then, in a static model, the firm's decision to invest in self-generation is given by:

$$V(q,s) = max\{\Pi(q,0), \Pi(q,1)\}$$
$$s.t. \quad q^G + q^S \geq \zeta Q$$

Q is the production target. I assume that firms only produce $q^S \geq 0$ if $q^G \leq Q$, i.e. firms will always maximize quantity produced on the grid before running generators. Then,

$$q^G = (1 - \theta\mu)Q$$
$$q^S = (\theta\mu)Q$$

Hence, the firms will invest in a generator as long as:

$$\Delta\Pi = [1 - \gamma(1) - \nu](\theta\mu)Q - c(F) \geq 0$$

Substituting Equation 2:

$$\Delta\Pi = [1 - \gamma(1) - \nu](\theta\mu)Q - g(F - A; \eta) - F \geq 0$$

The one-time fixed cost subsidy mainly works by reducing $c(F)$, both through internal funds and external funds. θ is the same across all firms within the state but binding shortages μ is firm-specific. Differentiating $\Delta\Pi$ in equation (1) with respect to each of its arguments gives several interesting comparative statics. *Testable Prediction 1*: As $c(F)$ falls, the threshold for profitable investment falls for all firms and consequently, generator adoption should increase.

This will be particularly important for firms whose borrowing costs were previously too high to allow for investment. To test if financial constraints are binding, I utilize proxies to capture the variables of the fixed cost function, $c(F)$. As assets A decreases, the cost of borrowing increases. We would see more leveraged (indebted) firms increase their investment if A was the binding constraint. Secondly, firms that have more liquid assets would have lower agency costs η. Hence, firms with less liquidity could be nudged by the subsidy. Alternately, at the sector level, we would see that financially dependent manufacturing sectors would increase the generator adoption if in fact the fixed cost of a generator was the binding constraint. Following Rajan and Zingales (1998), in a setting such as India, with under developed financial markets, financial constraints become especially binding in sectors with high levels of financial dependence. Specifically, industrial financial dependence is measured as:

$$Findep_s = \frac{\text{Capital Expenditures}_s - \text{Cash Flows}_s}{\text{Capital Expenditures}_s}$$

based on data for US sectors over the entire 1980's. Here, *Findep* captures the share of external finance in a firm's investments in a setting with highly developed financial markets, namely the US. [64]

Testable Prediction 2: $\gamma(1)$ reflects the electricity intensity of the manufacturing process that varies with the industry. A fixed-cost subsidy does not affect the per - unit marginal cost of self-generation $P^{E,S}$ that is homogenous across all firms. However, industries with higher electricity intensive manufacturing process will suffer from relatively higher marginal cost of production when self-generating. If higher marginal cost of self-generation is a binding constraint, then lowering $c(F)$ allows firms in industries with higher $\gamma(1)$ to adopt generators as it reduces the average cost of self-generation.

Testable Prediction 3: Firms that face a smaller constraining shortage,μ, would generally have lower gains from investing in this remedial infrastructure. If this is infact a binding constraint, these firms would find it profitable to invest in a generator when a subsidy is offered. Furthermore, the quantity produced through self-generation, q^s, reduces along with μ suggesting that both rates of self-generation and returns from self-generation would be lower amongst these firms that adopt generators with subsidy.[65]

To test if smaller constraining shortages, μ, can explain lower rates of generator adoption, I borrow the concept of operating cycle from the trade credit and working capital management literature (e.g. Jose et al., 1996; Petersen and Rajan, 1997). In this paper, operating cycles refers to the time between receiving raw materials and shipping final goods. One would expect that firms in industries with longer operating cycles, particulalry in the production stage, are able to reutilize production more effectively. The operating cycle for each industry is defined as the median of firm-level operating cycle, OC_i where:

$$OC_i = \frac{Inventory_i}{Salesi} * 365$$

[64] I use the original Rajan and Zingales (1998) measures of financial dependence for ISIC Rev.2 sector definitions. These sector definitions match closely with India's NIC 1987 sector definitions. The concordance between ISIC Rev.2 and NIC 1987 is provided by the Indian Statistical Office.

[65] Firms would have lower returns if they have higher marginal costs of self-generation as well. To isolate the differences, I will proxy for μ with a variable that is unrelated to the marginal costs.

In the following section, I will go over the data and methodology for testing the above predictions.

4. Data and Empirical Strategy

Firm data

I use firm-level data from the Annual Survey of Industries (ASI) data from 2004 - 2011 for the empirical analysis. It covers all factories registered under Sections 2m(i) and 2m(ii) of the Factories Act, 1948 i.e. those factories employing 10 or more workers using power; and those employing 20 or more workers without using power. The reporting period is the Indian fiscal year, which runs from April 1st through March 31st of the following year. Hence, when we refer to an individual survey year, we refer to the calendar year when the fiscal year begins. The survey extends to the entire country and is split into two schemes: Census and Sample. In each year, the census scheme surveys all manufacturing establishments with over 100 workers, while the sample scheme surveys a rotating sample of one-third of establishments with less than 100 workers. This dataset is sufficiently large for this study as it includes many MSME's and all large firms.[66] Unfortunately, the effects of the generator subsidy program on many micro enterprises (with less than 10 employees) cannot be determined. This is a drawback because micro enterprises are the least likely to own generators a priori and the subsidy is a large shock to their capital stock. Our results might be underestimating the effects of subsidy. However, since size is a strong determinant of generator ownership, we would expect the subsidy to be most effective for relatively larger microenterprises that are part of ASI and any resulting bias would not be significant. In addition, the generator subsidy was offered by the capital threshold whereas the survey sampling was determined by labor threshold. Hence, we would still be capturing enterprises that are "micro" by capital standards but with more than 10 employees. The firms in ASI have assigned IDs that can be connected over years to create long panels. However, the panel is unbalanced for the sample scheme since the firms in this group rotate and the criteria has changed over the years. I balance the panel by dropping any firms that do not have at least one survey both before and after 2008. This allows us to perform firm-level fixed effects panel analysis comparing the same firms pre and post treatment.

While the threshold for establishing a MSME technically applies to the historical value of plant and machinery, I use the reported book value of plant and machinery in the survey and therefore likely to understate the historical value.[67] Most firms close to the threshold

[66]Informal firms generally do not qualify for subsidies, hence lack of their data is not an issue in this context.

[67]An "industrial undertaking" may include more than one establishment. Therefore when measuring plant

are already generator users. Hence, the subsidy program is intended for firms way below the threshold. This fact motivates our reduced-form setup of difference-in-difference rather than exploring a regression discontinuity.

One of the benefits of the ASI over other manufacturing datasets from India and other countries is that we observe the physical quantity of each plant's total electricity purchases as well as self-generation in each year. The sum of these two variables, minus reported sales of electricity, yields electricity consumed. Self-Generation share equals electricity self-generated divided by electricity consumed. Electricity intensity is the amount (kilowatt hour) of electricity consumed divided by revenue. A firm has generator capacity if it is both self-generating and consuming grid electricity in positive amounts for at least one year in a given period. A firm has no generator capacity if self-generation is a missing variable and electricity consumption off the grid is non-zero for all years in a given period. Operating costs are the cost of material inputs plus wage costs but not overhead and depreciation. Cost of financing of a firm is measured by access to credit, leverage and liquidity. Leverage is the ratio of debt to total current assets. Liquidity is the ratio of working capital to current liabilities. Access to credit is equal to 1 if the firm has access to an overdraft facility or line of credit. All financial amounts and electricity costs are deflated to constant 2004 Rupees using the National GDP Deflator from the World Economic Database Outlook. The energy and fuel costs are deflated by the price index for "Fuel, Power, Light, and Lubricants" the input and output costs are deflated using the product specific deflators constructed by Allcott, Collard-Wexler and O'Connell (2015). Profits are calculated as the difference between total revenue and operating costs. To ensure my results are not driven by outliers, I replace the 99th percentile of all continous outcome variables as missing.

Electricity Shortage Data

Because electricity shortages is an important determinant of generator ownership, I use state level annual data on electricity demand and production to control for any changes in electricity shortages that would drive generator adoption. Specifically, I use two measures: Peak Demand Shortage and Energy Shortage. The data comes from the annual reports published by the Central Electricity Authority in India. The shortages are in percentages with negative referring to excess requirement over availability at the state level. The electricity shortage data is an integral component in selecting the sample of control states.

Identification Strategy

My analysis follows a two-step procedure. First, I look at the first-order effects of the subsidy program to determine if the subsidy program was indeed effective in promoting self-generation.

and machinery for firms that report more than one establishment in our dataset, we use the total value across establishments.

I pursue a difference-in-difference (DID) strategy comparing MSMEs in TN to firms in states that do not offer a subsidy. A key assumption in using DID is the parallel trend assumption. We need a comparable control group of firms in states that have similar trends to treatment group prior to 2008, the year subsidy program was initiated. For this, I use synthetic control method to find a weighted average of non-treated states with similar characteristics to Tamil Nadu in terms of size, electricity intensity and age of firms, level of electricity shortages and GDP growth rates prior to treatment. These variables are chosen based on evidence from the existing literature on the determinants on generator ownership (Reinikka and Svensson (2002) and Steinbuks and Foster (2010)). The reason for including the plant and machinery as a proxy for firm size into the regression comes from the existing empirical evidence that a larger firm is more likely to invest in generator.[68]The summary statistics of sample firms from Tamil Nadu and the synthetic control is presented in Table II.1. Figure II.2 shows the dynamics of the treatment effect on generator ownership across the treatment and control over our sample period. In general, there exists a similar trend in generator ownership amongst all firms prior to 2008 with firms in treatment having a higher share of firms with generator ownership. After 2008, the control group experiences a declining rate of generator ownership while MSMEs in TN have an uptick in generator ownership. In fact, firms in TN have higher likelihood of self-generating post 2008 relative to the average over the entire period. This gives credibility to our parallel trend assumption. Because crippling electricity shortages was the driving force behind this policy, I present the trends in electricity shortages between the treatment and synthetic group as well in Figure II.3. I find that they exhibit similar trends so any effects captured here can be atrributed to the subsidy itself with more confidence. In the following section, I present additional robustness checks for this parallel trend assumption as well as treatment identification. The empirical specification for the first step is as follows:

$$G_{ijst} = \beta(D_{is} \times T_{it}) + X_{it-1} + \gamma_t + \phi_i + \eta_{st} + \delta_{js} + \epsilon_{ist} \qquad (3)$$

where G_{ist} is the generation capacity of firm (i) in industry j, state s and year t. I employ two modelling approaches to test the treatment effectiveness. The first follows the binary choice model where G is a dummy equal to 1 if firm has positive self-generation and 0 otherwise, measuring the proability that firm i has self-generation capacity.[69]. The second approach employs censored regression approach where G is the logarithm of the firm's self-generating rate (left-censored at 0). The coefficent β will reflect the intent-to-treat estimate - the average

[68]I use plant and machinery value because the subsidy is for a machinery and the program allocates subsidies based on the historical value of the plant and machinery. Results are robust to an alternate measure of firm size - number of manufacturing employees.

[69]Because the probabilities are moderate in our sample (between 0.2 and 0.8 or slightly more), the linear and logistic models fit about equally well, and the linear model is favored for its ease of interpretation

increase in the number of self-generating firms residing in TN.

D_{is} is a dummy indicating whether the firm is in Tamil Nadu and T_{it} is an indicator that equals 1 if the year is post 2008 and 0 otherwise. The coefficient of interest is β. X_{it-1} is the set of firm-level controls that were used for the synthetic control. In addition, I also control for financial constraints with proxies (leverage, liquidity and access to credit) as it has been identified as one of the key determinants for investing in a generator (Steinbuks, 2011; Alby et al, 2011). The regressions also include the urban vs. rural location of plants to account for any differences in electricity shortages across locations. This is important because our electricity shortages data is at the state level and there is evidence of urbanization of small firms with migration of large firms to rural location (Ghani, Goswami and Kerr, 2012). Finally, the effect of age is more ambiguous. On one hand, an old firm is more likely to have installed a generator many years ago. Contrarily, a mature firm will also learn to deal with the electricity outages through other methods such as adopting more energy efficiency techniques (Reinikka and Svensson, 2002), which can reduce the probability of owning a generator. All the firm-level controls are from the pre-treatment to alleviate any spurrious correlations. State-specific linear trend (η_{st}) equals to the time index in a given year (i.e. time trend variable equals 1 for 2004, 2 for 2005, etc.) and varies with the state. This captures the differential growth rates and technical progress of states. Finally, I also include state-level controls of energy shortage, peak demand shortage to capture the energy reliablity issues and gdp growth rate to proxy for the demand effects.[70]

The subsidy program would nudge firms with different characteristics to adopt generators. To test the heterogeneous effects of the subsidy program, I interact the detrended firm-level characteristics X_{it-1} with treatment variable $D_{is} \times T_{it}$.

In the second step, I determine the average treatment effect on the treated (ATT) for the firms that adopt generators under the subsidy regime. Outcomes of interest include costs, investment and profits. This analysis quantifies the performance outcomes for firms who select into generator adoption under the new program. My strategy to identify the effects of the subsidy program on firm performance uses a triple-differenced specification comparing treated and untreated firms, in and out of Tamil Nadu, before and after the start of the program. The treated firms are MSMEs without generator capacity prior to 2008 in Tamil Nadu who adopt generators after 2008. The untreated firms are MSMEs outside Tamil Nadu differentiated by whether a generator was adopted post 2008 and MSMEs in Tamil Nadu with generator capacity pre 2008 or no generator capacity throughout our sample period.[71]

[70] The regressions at the firm-level with the assumption that they are too small to influence these state-level controls.

[71] The control group includes firms who already have generators, but these firms could potentially be treated by the subsidy as it allows them to expand capacity or update technology. However, the fact that

Differencing across firms and time within Tamil Nadu controls for other factors that would affect the outcome of all firms similarly irrespective of their initial generator ownership as well as biases from comparisons over time in the treatment group that could be the result of trends. However, a potential problem with this analysis is that firms who already own generators (incumbents) may have secular trends that are different from firms that did not own self-generation capacity prior to 2008 (non-incumbents). Hence, changes in other factors unrelated to the state's new policy or state-specific changes in the economy might affect these firms differently in Tamil Nadu. By using both a control group within the treatment state and a different state, I obtain more robust results that controls for two potentially confounding trends: changes in performance outcomes amongst MSMEs with no prior self-generation capacity across states and changes in outcomes of all firms within Tamil Nadu. The main empirical specification for the outcome variable of interest y is as follows:

$$y_{ist} = \alpha(D_{is} \times T_{it} \times A_i) + +X_{ijst-1} + \gamma_t + \eta_{st} + \delta_{sj} + \phi_i + \epsilon_{ist} \qquad (4)$$

whereA_i is an indicator of the firm's generator adoption status that equals to 1 if the firm has no generator capacity prior to 2008 but adopts generator afterwards and 0 otherwise. I include year fixed effects (γ) to control for any common time effects across states such as the global financial recession of 2009. The state x industry (at the 2-digit NIC level) fixed effects (δ) controls for any time-invariant state and industry specific factors that affect the demand for self-generation. I also control for state x year fixed effects (η) to absorb shocks that affect all firms in a particular state. Additionally, changes in policy in a state may be correlated with changes in other unobserved state variables which also affect firm outcomes. If these state variables affect all firms in the same way, controlling for state-specific year effects may help address this issue. It allows us to control for the exogenous increase in the dependent variable which is not explained by other variables. Hence, this would account for any political and economic changes within the state as well as changes in electricity shortages. Plant-level fixed effects, (ϕ), control for any systematic differences across firms for all periods (including the location of the plant, age etc.). The main coefficient of interest is α. Errors are robust and clustered at the state level, the variation level of the treatment.

As previously mentioned, I expect the firms that adopt generators in Tamil Nadu post treatment to be inherently different from firms that adopt generators in the synthetic control group and firms that choose not to self-generate ever. Absent control for these differences, the endogeneity can lead to inappropriate inferences about treatment. To address this issue, I use propensity score matching at the firm-level to get a comparable set of treated and

these firms already had generators implies lower returns from this subsidy. Moreover, I am interested in studying the effects of subsidies on new entrants only so this assumption is reasonable.

untreated firms in evaluating the treatment effects (apart from including the firm - level controls). Firm - level control variables that have been previously identified in the literature as determinents of generator adoption are used for the matching. This would reduce the selection bias by balancing observable covariates that predict receiving the treatment. The errors are bootstrapped to account for estimation errors from the first stage. Finally, to determine if the generator subsidy program is beneficial for firms, I use back of envelope calculations to estimate the capital investment from the generator subsidy program and estimate the marginal returns to generator ownership by regressing log profits on the interaction of treatment with log generator investment value. I also study the crowding out effects of this program on firm-level investment.

4. Empirical Results

Treatment effects

In this section, I study the effects of the subsidy program on generator adoption amongst MSMEs in TN relative to the synthetic control state. As previously discussed, I compare all MSMEs from TN with a synthetic control group of firms and identify the changes in probability of having generator capacity for these firms well as any changes in the distribution of generator ownership by firm characteristics.

Generator Adoption

Table II.2 presents results of Equation (3) with outcome y being an indicator for generator ownership. I find that among the MSMEs in Tamil Nadu, probability of generator ownership increases by 8% relative to the untreated states. This represents a 20% increase in probability of generator adoption amongst MSME's in Tamil Nadu post 2008 (given a generator ownership rate of 50% pre-2008). In column 2, the results are robust with the inclusion of measures of electricity shortages and gdp.The results are robust to including firm-level controls and state-year trend in column 3 as well. Our results are consistent with the existing evidence that firm characteristics, particularly firm size, is a more important determinant of generator ownership than electricity shortages. Results suggest that older and bigger firms are more likely to self-generate with other controls being statistically insignificant. Firm-level controls and electricity shortage proxies should be considered as control variables for the sake of robustness, and their respective coefficients should not be interpreted as causal because specific endogeneity concerns arise from omitted variable bias and unobserved effects, such as entrepreneurial skills. I find these magnitudes to be similar to regressions that do not include firm FE but state FE and firm-level controls. These results are not presented here for brevity but the main takeaway from this excercise is that the set of firm-level observable covariates that predict generator ownership are captured in my firm-level controls as these characteristics account for most of the variation captured by firm - fixed effects.

Self-Generation rates

The above section looks at the extensive margin of generator adoption with an indicator for self-generation as the outcome variable. In Table II.3, I present the results of the Equation (1) with outcome variable being the logarithm of the firm's self-generation rate. Similar to the previous results, we find that treatment lead to significant increase in self-generation rates amongst MSMEs in Tamil Nadu. However, once firm-level fixed effects are included, these effects become smaller and statistically insignificant. This is indicative of consistent rates of self-generation amongst already self-generating firms with low rates of self-generation amongst firms that newly adopt generators (i.e. there is not much variation at the firm-level so including firm fixed effects absorbs much of the differences). It is also interesting to note that in line with hypothesis 2, firms with higher electricity intensity actually have lower rates of self-generation. This is perhaps not surprising because higher electricity intensity is associated with higher marginal costs of self-generation. Consequently, even if these firms have generator capacity, they limit its usage (consisent with evidence of Zuberi (2013) and the assumptions in the model). Finally, while older firms are more likely to self-generate, they also have smaller rates of self-generation suggesting that they are either better at reutilizing capacity or face higher costs of self-generation (due to older inefficient technology).

Heterogeneous effects by firm characteristics

In Table II.4, I explore the heterogeneous effects of the treatment on generator adoption by using the same specification as Table 2 but including interactions of the firm-level characteristics with the treatment variable. Firstly, I find that smaller firms (measured by plant and machinery value) have a relatively larger increase in generator adoption under the subsidy program. This suggests that the program reached its intended recipients of smaller firms below a exogenously determined capital threshold. However, firms with more employees increase their generator adoption under the subsidy. Considering that unlike materials, labor is less flexible, then these results suggest that smaller firms whose production process involves relatively more semi-flexible labor inputs are the most likely to adopt generators when subsidies are offered. In other words, firms who face more difficulties with reallocating input (i.e. labor) are most likely to adopt generators under the subsidy, in line with the predictions of the conceptual framework. These results suggest that smaller firms with are unable to reallocate inputs effectively are the ones nudged by subsidies to invest in remedial infrastructure.

From the model, I hypothesized that more electricity intensive sectors would find self-generation relatively more expensive because of the higher marginal costs of operating generators. They are more likely to find alternatives and less likely to adopt generators. We would expect these firms to adopt generators if the fixed cost of generators fall as this reduces the average cost of self-generation. Our results are consistent with the model predictions. I find that the subsidies increased generator adoption rates amongst firms in more electricity intensive industries confirming that high marginal costs is a critical factor in limiting adoption of self-generation.

Apart from high marginal costs, lower potential gains can hamper investment in remedial infrastructure. Firms who can realloacte production away from blackouts more effectively would have lower gains from adopting generators. In line with this argument, I find firms in industries with longer operating cycles to invest in generators when subsidies are offered. This suggests that firms who face less binding electricity shortages are partially deterred by the high fixed costs of self-generation as their returns would be lower.

Shifting focus to financial proxies, I find that financial constraints is an important determinant of investment in remedial infrastructure. Results are presented in Table II.5. Highly leveraged firms are less likely to adopt generators under the subsidy. Similarly, firms with more liquid assets and access to credit increase adoption. This is because the subsidy only covers 25% of the generator and the remaining 75% is a significant capital cost that can be burdensome for firms with high costs of credit. Therefore, the subsidy was not substantial enough to allow firms with relatively higher fixed costs of generation (due to financing costs) to invest. Instead, my results indicate that the subsidies worked by allowing firms with relatively higher marginal costs to invest in generator but not financing costs itself. In other words, these results suggest that financing cost is not the binding constraint to remedial infrastructure investment.

In conclusion, these results indicate that a one-time fixed cost subsidy can be effective in inducing smaller firms to invest in remedial infrastructure.

Firm-level Effects

The generator subsidy program had the intended result of increasing generator adoption amongst MSMEs without prior generation capacity. In the next step, I sutdy if the increase in adoption was indeed beneficial for the firms. Do the firms use the generators? What are the effects on costs and profits Finally, are the returns to adopting generators worth the cost of capital To determine these effects, I compare firms that adopted generators under the subsidy to firms that already had generator capacity prior to 2008 within and outside TN. To address this selection bias, I use propensity score nearest neighbor matching within calipers (of 0.02) to get a sample of comparable firms from the synthetic control state to the treated group using the set of firm level covariates from the previous section. I use three methods of weighting the sample using the calculated propensity scores to ensure the results are not sensitive to the type of weighting. The first method uses the propensity score by itself with random sampling within strata. In the second method, I assigns weights of 1 or 0 to each observation. If a given observation is in the selected sample, it gets a weight of 1, while if it is not, a weight of 0 is assigned to it. In the third method, the weighting is based on inverse of propensity scores to weigh each observation in the treated group, and one minus the inverse of the propensity score (i.e., the propensity of NOT being in the treated group) in the controls.

Capacity utilization

Focusing on just the intensive margin, I want to compare if firms that self-generate only with subsidy have different rates of utilization relative to firms that acquired generator capacity without the subsidy. This analysis focuses on the hypothesis that firms who expect lower rates of capacity utilization during shortages are the ones incentivized under subsidies to take up self-generation. Capacity utilization refers to the intensity with which the resources of the firm are employed (per unit of time). I proxy for the utilization during shortages with self-generation rates. I cannot directly compare the utilization rates of firms that adopted generators with and without subsidy over our time period because these firms face different rates of electricity shortages and potentially differing economic conditions.[72] Instead, I use DDD comparing rates of self-generation between firms who self-generate both pre and post treatment to the treated firms who only self-generate post treatment in treatment and control states. Results are presented in Table II.7. I find that firms in Tamil Nadu who adopt generators under the subsidy program self-generate at a significantly lower rate than firm who adopt generators without subsidy relative to firms who are already self-generating. These firms inherently use the generators less, possibly because of the higher marginal costs or greater flexibility in production. Hence, the additional output from owning generators might not be large enough to cover costs of generation without a subsidy. When the cost of generator ownership falls, the average cost of generator usage falls and these firms may now find it

[72] I find that firms who adopt generators after 2008 in TN self-generate 1% less than firms in other states. This mitigates concerns that higher electricity shortages in TN induced firms to adopt generators rather than subsidies. If this was the case, then we would expect firms in TN to have higher self-generation rates.

profitable to invest in generators.

Cost Effects

Table II.7 presents the effects of generator adoption on costs. In this specification, I compare firms that adopt generators after 2008 to all other firms with the aim of determining the cost effects of self-generation. The independent variable of interest is an indicator that equals 1 if firm adopts generator post 2008 and 0 otherwise. As expected, material costs, wages and fuel costs increased for firms that selected to self-generate after 2008 relative to firms that were already self-generating in our sample period. The increase in fuel costs is substantial, even as a share of revenue, alluding to the fact that the generators are diesel operated and require relatively expensive fuel to produce electricity. Firms experience increased material costs and labor costs because these firms can now operate during power outages rather than shutting down production temporarily. There are no significant effects on material costs or wages per worker, suggesting that the increase in costs comes from increase in capital productivity (a fixed input) rather than from an increase in labor productivity (semi-flexible input). Finally, there are no significant changes in electricity intensity after acquiring generators, suggesting that firms do not change their production process in ways that would increase their dependence on electricity. This is reasonable because per unit cost of electricity self-generated is almost twice as expensive as power off the grid, allowing firms to use the generators solely as standby. This reinforces our assumption that firms strictly prefer to use electricity off the grid maximising the production in this system and will only use self-generation when demand exceeds the maximum quantity that can be produced with grid electricity. Results are robust to the weighting method used.

We are interested in studying the differences between firms that adopt generators under subsidy and firms that adopt without subsidy. Hence, using Equation 2, I study the effects of the subsidy regime on costs. Results are presented in Table II.8. There are no statistically significant differences in changes in costs between the firms that adopted generators under subsidy and without subisdy. However, the coefficients on fuel costs are negative aligning with evidence of lower capacity utilization during blackouts. Additionally, the growth in wage costs are negative (albeit insignificant). In the previous section, I find that firms with more employees are incentivized to adopt generators under subsidies and argued that this is due to the semi-flexible nature of labor inputs. Perhaps, self-generation capacity allows firms to optimize the labor inputs and reduce its associated costs.

Estimating Returns to Generator Capacity

To estimate the returns from owning a generator, I need a monetary value of generator capacity. Because the ASI does not include data on generator capacity, I construct a back-of-the-envelope estimate of each plant's capacity requirement and using data on generator prices, I estimate the cost of generator. First, I transform a plant's total electricity consumption (in kiloWatt-hours) from ASI into required generation capacity assuming that plants use a constant flow of power while operating a fixed number of hours per day for all the manufacturing days. I change the assumption about the number of operating hours per day and rescale the generator cost estimates accordingly. The number of hours increases in increments of six hours, starting at 6 hour per day and going up to 24 hours per day. As hours increase, the cost of generator needed to run the operations decrease. Hence, estimates with 24 hours per day can be interpreted as the maximum returns from generator capacity as this assumes that in the days the manufacturing firm was operating, there were no power shortages and the firm operated 24 hours a day. In reality, power shortages averaged 7% over the sample period so we would be underestimating the generator capacity needed when we assume full operation. To convert the generation capacity in kiloWatts (kW) to kilo Volt Amperes (kVA), I assume power factor (a measure of efficiency) of 0.8. The price of generator is derived from online sellers price for generators of different capacity. There is an approximately linear relationship between the generator capacity and price, with 1 kVA costing about 2008 rupees.[73]

In the following regressions, the dependent variable is firm profits while the coefficient of interest comes from the dependent variable that is the cost of generator capacity K_{it} interacted with the treatment indicator T_{it} that equals 1 if firm adopted generator after 2008. The following is the setup:

$$Profits_{it} = \beta_0 + \beta 1 K_{it} \times T_{it} + X_{ijt-1} + \gamma_t + \delta_j + \phi_i + \epsilon_{it} \tag{5}$$

Profits and capital can be measured in either levels or logs, reflecting a linear or CES production function, respectively. I present results using real levels. Because I expect heterogeneous returns to capital, β_1 provides a local average treatment effect (LATE), which is a weighted average of the marginal returns to capital with the weight being the cost of generator.

Firstly, I compare firms that adopt generators after 2008 to firms that already have generators and firms that have no self-generation in the sample period within TN. Here,

[73]The market prices are obtained from www.aqprice.com for Kiroslar Diesel Generators in New Delhi in 2017 and deflated to the 2008 prices.

treatment indicator equals 1 if generator is adopted post 2008 and 0 otherwise. Results of the coefficient on the value of generator capacity are presented in Table II.9, Column 1. There are significant positive effects on profit across all generator capacity estimates with the largest returns for 24 hour assumption. The gross returns are higher for firms that require a smaller generator capacity and the subsidy increases the returns substantially, with larger effects for less expensive generators. Given that smaller firms usually operate fewer hours within a day, this would suggest that the generators on average would have returns that are between 8.5% and 17% without subsidy. With the subsidy, the returns are higher (between 11.35 and 22.70 %) and are at least equal to the market interest rate for most firms. As the generator capacity requirement increases, the subsidy would not be large enough for some firms to make the investment profitable and in fact, might be less than market interest rate. Generators that are used as standby have diminishing value annual depreciation rate of 8% (NZ Inland revenue, 2017). Under standard profit maximization, we need to have that the increase in profits is at least as large as the sum of depreciation and real interest rate for the net marginal returns to generator capacity to be positive. The subsidy on generators allow firms to achieve positive marginal returns from generator capacity if they have a sufficiently small generator capacity requirement. Contrary to evidence in the existing literature that finds much higher returns to capital, my results suggest that returns to remedial infrastructure are relatively modest for small formal enterprises. There are many plausible reasons for this including lack of economies of scale, more flexible production targets, less market power leading to lower barriers to entry.[74] I find some evidence suggesting that MSMEs are able to better utilize capacity when connected to grid, reducing the need for self-generation and lower rates of capacity utilization during power shortages. In fact, in WBES, larger firms report greater percentage of electricity shortages, suggesting that these firms faced more binding electricity shortages. Small firms are more likely to operate a limited set of hours and days in a week whereas large firms operate throughout. This gives smaller firms greater flexibility in dealing with electricity shortages by having labor work overtime, work additional shifts or change shift timings (GhausPasha, 2009).

In Table II.9, Column 2 and Column 3, I compare returns from generator capacity for firms that invest in remedial infrastructure with and without subsidy (i.e. firms within TN and outside TN). Here, the depenedent variable is the cost of generator (without the subsidy) for firms that invest in generator post 2008 and 0 otherwise. I find that firms that adopt generators in TN post 2008 have significantly lower returns from generator adoption. When the cost of generator is reduced by subsidy, then the returns are statistically indifferent from the returns for firms in the control state. These results confirm that the subsidy was essential

[74]See Rud(2012b)

in nudging the firms on the fence to invest in remedial infrastructure.

Finally, it is important to note the above analysis only looks at measurable returns in the short run. However, there can be other indirect effects from the subsidy program that can bias our estimates. For example, it is possible that the owners become more motivated or upgrade the machinery that can contribute to some of the increase in returns. Generator adoption could allow for more labor to be hired as suggested in Table II.6 where wage expenses increase, as well as fewer machine breakdowns and better product quality. There are also longer run effects such as the increased growth potential from the ability to export or supply to larger firms (that require attaining certain standards including having generator capacity). Unfortunately, many of these potential changes are unobservable, making it difficult for us to identify the postive spillovers. Furthermore, there are negative externalities from generator adoption that are not accounted for as well. On the other hand, running generators would lead to faster rate of depreciation of productive capital and environmental damages. Irrespective of this, the conclusion is that generators increase profits and the net returns can be positive for firms that receive subsidies.

Effects on firm investment

A potential distortionary effect of the subsidy program is the crowding out of investment in other productive capital. Reinikka and Svensson (2002) find that poor complementary public capital significantly reduces private investment. When firms compensate for deficient public services by investing in complementary capital themselves, less productive capital is installed. Contrarily, when firms have generator capacity, the returns to having additional machinery increases as they can operate even during shortages. To test the effects under the subsidy, I use Equation (3) with the outcome variable y_{ist} being capital investment in a given year. For the firms who adopted generators post 2008, I subtract the estimated values of generator capacity (excluding the subsidized value for firms in TN) from the investment reported in ASI. In Table II.10 Row 1, I restrict the sample to firms in TN and compare firms that adopt generator post 2008 to other firms. Contrary to the evidence in existing literature, I find no evidence of crowding out. The coefficients on indicator for generator adoption is large but statistically insignificant. These results suggest that remedial infrastructure and productive capital are in fact complementary. To test if subsidy had a differential effect on investment, I use triple differencing strategy of Equation (4). These results are presented in Table II.10 Row 2. Again, I find no substantial differences in investment between firms that invest in generator capacity with and without subsidy.

Robustness Checks

I perform series of checks to ensure the results are robust to different specifications of the data. Firstly, I address the timing of the subsidy by documenting that there are no pre-treatment trends indicating higher or lower generator adoption before the subsidy program was initiated. I conduct two falsification tests. The first test assigns false subsidy program to the large firms that did not qualify for the subsidy program and conducts a DID for only large firms, comparing TN with the synthetic control state. I find no significant differences in generator adoption or fuel costs, reaffirming that subsidies were effective in nudging small fims. Secondly, to ensure there were no changes in trends prior to 2008 that could drive the results, I test the effects on the sample by restricting to years before the intervention in 2008 and imposing a false treatment in Tamil Nadu from 2006, 2 years prior to the actual subsidiy program in Equation (3). The results of this falsification test are presented in the Appendix. The false subsidy shows no effects generator ownership or revenue. Finally, it is possible that electricity shortages are driving the increase in self-generation and these are the effects that are attributed to the subsidy treatment effect. For this, I restrict the control group to Karnataka that has the same trends to Tamil Nadu in terms of electricity shortages during our sample period and re-run Equation (3). These results confirm our findings that the subsidy program was indeed effective and alleviates concerns of endogeneity.

While I use propensity score matching to address selection bias as many of the covariates that determine generator adoption are observable for all firms, there is still the possibility that unobservable characteristics could be driving the selection, and the performance results could be biased. As a robustness, I use the Heckman inverse-Mills-ratio (IMR) method to address selection bias from unobservable firm characteristics. I follow the two step Heckman procedure to evaluate the effect of treatment on profits (i.e. the same outcome and variable of interest at Table 9). The results are presented in the appendix and are similar to the returns estimated using propensity score matching.

Cost-benefit Analysis

We find the program to be profitable for the treated firms. Here we determine if this program was worth the fiscal costs for the TN government using back - of-the-envelope calculations. It is beyond the scope of this to paper to determine if there are better policies that can attain the same or more effective results.This policy was rather simple to implement as it is one-time and involved a well-established arbitrary cutoff based on firm size. Other policies such as subsidies on marginal costs would be more cumbersome to target and enact. Undoubtedly, the first-best policy would be to overhaul the energy infrastructure and eliminate unreliable electricity provision. But, given the lack of funding and the length of time required, I find a fixed-cost subsidy program to be an effective remedy for the short-term. Assuming 12 hours of operation, the total estimated subsidy costs for the government over the 3 years from the launch of the program is approximately R. 89 million in our sample. The additional tax revenue (at a corporate tax rate of 35%) from the increase in profits for the sample is Rs. 667 million over the same period.[75] However, much of this growth can be attributed to external factors and internal firm-level characteristics independent of the generating capacity itself. To control for this, I look at firms that did not adopt generators in this time period and calculate the average profit growth for these firms. I find these firms to have a growth rate that is on average lower by 10%. Hence, of the tax revenue growth of Rs. 667 million, around Rs. 67 million can be attributed to the generator subsidy program itself. Given a depreciation rate of 7%, my results suggest that the goverment would recoup the expenditures on the subsidy program within four years (without any improvements in overall energy infrastructure).

6. Conclusion

Poor quality of infrastructure is a hindrance to sustainable growth in many developing countries. Improving the infrastructure through public investment is a costly and time consuming endeavor. Meanwhile, the negative effects of unreliable infrastructure such as electricity is larger for some firms compared to others. For example, small firms are the most affected under chronic power shortages as they do not have remedial infrastructure that can help them cope with the blackouts.

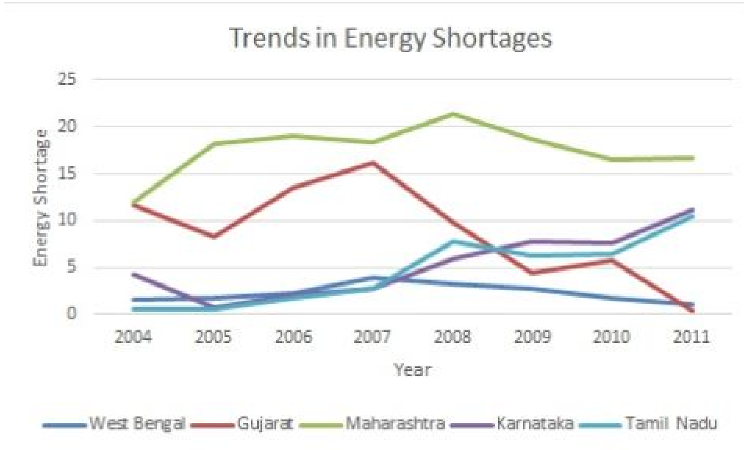
We find that a one-time fixed cost subsidy can be effective in mitigating the losses from unreliable electricity supply for smaller firms. While higher financing costs and higher marginal costs of operating generators are both impediments to generator adoption, subsidies are effective in primarily counteracting the latter effect. This is because the difference that a firm needs to pay to acquire the infrastructure after a fixed cost capital subsidy is still substantially large for most small firms. Furthermore, these firms have lower rates of capacity utilization alluding to both higher marginal costs and potential ability of smaller firms to reallocate inputs. Either way, these firms have relatively lower returns to remedial capital and the subsidy is essential to nudging these firms into positive returns (in line with the returns of firms who invest without generator subsidy).

While we observe positive returns from generator adoption, there are other externalities that are not quantifiable with our existing data but can have significant long run effects. For example, there is evidence that frequent power blackouts has led many small enterprises to reduce their labor and even shut down. Offering subsidies can help these firms survive periods of severe electricity shortages and have positive distributional effects by supporting the majority of workforce that work in these enterprises. In fact, we find that firms with

[75] Here profits are net of capital depreciation as firms can discount their capital depreciation for taxes

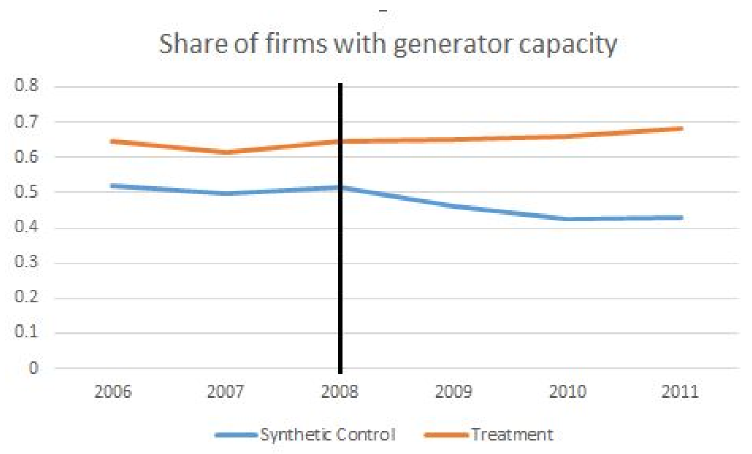
more workers are more likely to adopt generators under subsidies, allowing them to better utilize labor. These firms increase their wage expenditure following adoption. Furthermore, export status of a firm is an important determinant of generator adoption. Having access to generators can allow these firms to compete and export as well. Finally, while I identified some policy implications for inducing private infrastructure investment amongst small firms, I did not decompose the different factors that can explain the observed rate of returns from remedial capital for these firms. Insights into the magnitudes of these channels can help us device more effective policies that target firms under constrained public infrastructure.

Fig II.1: Electricity shortages in the firve most industralized states in India



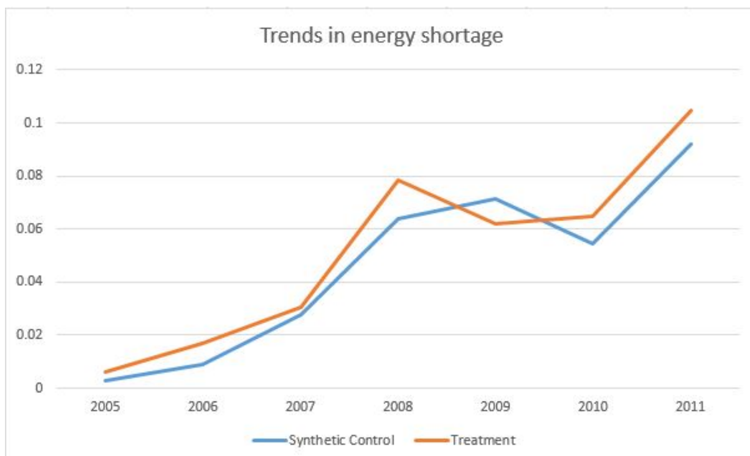
Source: Central Electricity Authority, India

Fig II.2: Share of MSMEs with self-generation capacity



Source: Annual Survey of Industries, India 2003 - 2011

Fig II.3: Electricity shortages of treatment vs. synthetic control states



Source: Central Electricity Authority, India

Table II.1: Summary Statistics with synthetic control

	Mean	Std.Dev.	Min	Max	N
Tamil Nadu					
1(Self-Generator)	0.62	0.49	0	1	12691
Self-Generation Share	0.06	0.13	0	1	12691
Age	19.73	14.87	0	95	12626
1(Urban)	0.49	0.50	0	1	12691
Total Employees	169.16	217.80	4	2446	12581
Plant and Machinery (million Rupees)	13.35	21.28	0.01	99.96	12444
Revenues (million Rupees)	157.65	560.12	0	25200.23	7481
Material Costs (million Rupees)	68.95	156.32	0	3620.27	7630
Fuels Purchased (million Rupees)	1.06	2.97	0	53.54	12291
Electric Intensity (KWH/Rupees)	0.01	0.02	0	0	7300
Leverage	1.38	1.37	0	12	11229
Liquidity	1.50	4.48	-1	57	11156
Access to Credit	0.85	0.36	0	1	12691
1(Census Scheme)	0.66	0.47	0	1	12691
Synthetic Control					
1(Self-Generator)	0.51	0.50	0	1	23172
Self-Generation Share	0.06	0.15	0	1	23172
Age	20.31	16.92	0	95	22785
1(Urban)	0.63	0.48	0	1	23168
Total Employees	159.87	278.85	4	2492	22543
Plant and Machinery (million Rupees)	10.94	19.53	1620	99.94	22773
Revenues (million Rupees)	175.73	1840.58	0	143000.46	17020
Material Costs (million Rupees)	77.89	176.82	0	3620.39	17453
Fuels Purchased (million Rupees)	0.96	3.06	13	79.60	21093
Electric Intensity (KWH/Rupees)	0.01	0.02	0	0	16286
Leverage	1.26	1.32	0	12	21773
Liquidity	1.73	4.87	-3	58	21685
Access to Credit	0.92	0.27	0	1	23172
1(Census Scheme)	0.50	0.50	0	1	23172

Note: Mean and median for the synthetic control is weighted by the synthetic weights.

Table II.2: Effects on Generator Ownership - Binary Choice

	(1)	(2)	(3)	(4)
	1(Self-Generator)	1(Self-Generator)	1(Self-Generator)	1(Self-Generator)
Treatment	0.0739***	0.0794***	0.0741**	0.0681*
	(0.0263)	(0.0272)	(0.0363)	(0.0401)
Energy shortage		0.000314		-0.0178
		(0.00533)		(0.0143)
Peak demand shortage		-0.00170		-0.00326
		(0.00305)		(0.00432)
GDP		-0.0113*		-0.00345
		(0.00629)		(0.00952)
Age			0.0194*	0.0194*
			(0.0105)	(0.0105)
1(Urban)			0.0165	0.0168
			(0.0135)	(0.0136)
Size			0.0560***	0.0561***
			(0.00914)	(0.00914)
Leverage			0.00848	0.00849
			(0.00751)	(0.00751)
Liquidity			-0.00303	-0.00304
			(0.00316)	(0.00316)
Number of Clusters	11314	11314	9658	9658
Observations	35863	35863	24208	24208
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State x Industry FE	Yes	Yes	Yes	Yes
State - Year Trend	No	No	Yes	Yes

Note: *Treatment* is an indicator that equals 1 if firm is in TN and 0 otherwise. Sample is restricted to firms in TN and synthetic control state and firms are weighted by the state-level synthetic weights. Column (1) has no controls and linear time trend for states. Column(2) adds state-level controls that account for the trends in gdp and energy shortage. Column(3) instead includes the linear time trend for states combined with firm-level controls. Column(4) includes all the variables from column (3) and columnd (2). Errors are robust clustered at the state level. Standard error in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$,$^{***}p < 0.01$

Table II.3: Effects on Generator Ownership - Censored Regression

	(1) Self-gen rate	(2) Self-gen rate	(3) Self-gen rate
Treatment	0.246***	0.471***	0.0370
	(0.0326)	(0.105)	(0.262)
Age		-0.0647***	0.0734
		(0.00998)	(0.0975)
1(Urban)		-0.177***	0.0856
		(0.0285)	(0.0834)
Size		0.0205	0.0110
		(0.0153)	(0.0678)
Leverage		-0.0494	0.0725
		(0.0490)	(0.0571)
Liquidity		0.00236	0.0193
		(0.0144)	(0.0228)
Electric Intensity (KWH/Rupees)		-11.90***	2.156
		(1.692)	(2.909)
Energy shortage		-0.00226	-0.0882
		(0.0479)	(0.121)
GDP		-0.0344	0.0652
		(0.0272)	(0.0718)
Firm FE	No	No	Yes
State FE	Yes	Yes	Yes
State x Industry FE	Yes	Yes	Yes
State - Year Trend	Yes	Yes	Yes
Number of Clusters	6	6	6
Observations	15906	7931	7931

Note: *Treatment* is an indicator that equals 1 if firm is in TN and 0 otherwise. Sample is restricted to firms in TN and synthetic control and firms are weighted by the state-level synthetic weights. Column (1) has no controls and firm FE. Column(2) adds firm-level controls and state-level controls. Column(3) includes firm FE and includes controls. Errors are robust clustered at the state level. Standard error in parentheses. Errors are robust clustered at the state level. Standard error in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$,$^{***}p < 0.01$

Table II.4: Effects on Generator Ownership by firm characteristics

	(1)	(2)	(3)	(4)
	1(Self-Generator)	1(Self-Generator)	1(Self-Generator)	1(Self-Generator)
Treatment	0.0967***	0.102***	0.0987***	0.106***
	(0.0125)	(0.0141)	(0.0125)	(0.0138)
Treatment*Plant	-0.00984***			
	(0.000797)			
Treatment*Labor		0.0108***		
		(0.000712)		
Treatment*Intensity			0.00322**	
			(0.000872)	
Treatment*Operating Cycle				0.00895***
				(0.000451)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State x Industry FE	Yes	Yes	Yes	Yes
State - Year Trend	Yes	Yes	Yes	Yes
Number of Clusters	6	6	6	6
Observations	17143	17137	17143	16946

Note: Note: *Treatment* is equal to 1 if firm is in TN and 0 otherwise. Sample is restricted to firms in TN and synthetic control and firms are weighted by the state-level synthetic weights. Interactions of firm characteristics with *treatment* is logged and detrended except for credit. All regressions include firm-level and state-level controls. Errors are robust clustered at the state level. Standard error in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$,$^{***}p < 0.01$

Table II.5: Effects on Generator Ownership by firm financial characteristics

	(1)	(2)	(3)
	1(Self-Generator)	1(Self-Generator)	1(Self-Generator)
Treatment	0.0951***	0.101***	0.0577***
	(0.0124)	(0.0125)	(0.0131)
Treat*Leverage	-0.0140***		
	(0.00140)		
Treat*Liquidity		0.0108***	
		(0.000366)	
Treat*Credit			0.0427***
			(0.00208)
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
StatexIndustry FE	Yes	Yes	Yes
State - Year Trend	Yes	Yes	Yes
Number of Clusters	6	6	6
Observations	17143	17143	17143

Note: *Treatment* is equal to 1 if firm is in TN and 0 otherwise. Sample is restricted to firms in TN and synthetic control and firms are weighted by the state-level synthetic weights. Interactions of firm characteristics with *treatment* is logged and detrended except for credit. All regressions include firm-level and state-level controls. Errors are robust clustered at the state level. Standard error in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$,$^{***}p < 0.01$

Table II.6: Effects on Capacity Utilization

	(1) Self-Generation Share	(2) Self-Generation Share	(3) Self-Generation Share
Treated	-0.0338***	-0.0392***	-0.0291***
	(-6.12)	(-7.03)	(-4.36)
Peak Shortage	0.000363***	0.000197	0.000416
	(4.35)	(1.69)	(1.36)
GDP	-0.000460***	-0.000263	-0.000217
	(-3.51)	(-1.76)	(-0.37)
Size	0.00761***	0.00778***	0.0122***
	(5.98)	(6.13)	(4.51)
Electric Intensity (KWH/Rupees)	0.397***	0.395***	0.310***
	(9.20)	(9.11)	(3.31)
Observations	77187	77187	26162
Number of clusters	22	22	6
State x Year Trend	NO	YES	YES
Synthetic Control	NO	NO	YES

Note: In this regression, treated is an indicator that equals 1 if the firm is small, in TN and adopted generator post 2008 and 0 otherwise. This is the DDD specification comparing firms that adopt generators with subsidy to firms that already have generator and firms that adopt generators without subsidy. The first two columns includes all states with available data whereas the last column restricts sample to firms in TN and synthetic control. Firms are weighted by the interaction of synthetic control weights and inverse propensity score. All regressions include firm, year, state-year and state-Industry FE. Errors are robust and clustered at the state-level. Standard error in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$,$^{***}p < 0.01$

Table II.7: Effects on Costs of operation

	(1)	(2)	(3)
	Random Sampling	Matching	IPW
Outcome Variable			
ln(materialcosts)	0.174*	0.180*	0.269*
	(2.27)	(2.51)	(2.49)
ln(materialcosts per worker)	0.0525	0.0549	0.145
	(0.74)	(0.82)	(1.39)
ln(fuelcosts)	0.474***	0.577***	0.559***
	(5.05)	(3.20)	(5.73)
ln(fuelcosts as share of revenue)	0.272**	0.285**	0.273*
	(2.82)	(3.20)	(2.37)
ln(wages)	0.150**	0.163***	0.133*
	(2.89)	(4.23)	(2.32)
ln(wages per labor)	0.0382	0.0531**	0.0209
	(1.48)	(2.81)	(0.66)
Electric Intensity	0.000582	0.000280	0.00125
	(0.61)	(0.34)	(0.61)

Note: Sample is restricted to firms in TN comparing firms that invest in generators post 2008 to firms that do not change their generator status. Rows are the depedent variables that measure different factors of firm costs. Columns represent the different weights from the propensity score method to find a matched control sample of firms. All regressions include firm, industry and year FE. Errors are robust and clustered at the firm-level. Standard error in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$,$^{***}p < 0.01$

Table II.8: Effects on Generator Ownership between Treated and Control group

	(1) Random Sampling	(2) Matching	(3) IPW
Outcome Variable			
ln(materialcosts)	0.0126	0.0246	0.0411
	(0.11)	(0.30)	(0.29)
ln(materialcosts per worker)	0.104	0.0636	0.152
	(-0.93)	(0.81)	(1.01)
ln(fuelcosts)	-0.133	-0.0656	-0.0604
	(-0.93)	(-0.73)	(-0.39)
ln(fuelcosts as share of revenue)	-0.166	-0.108	-0.151
	(-1.02)	(3.20)	(-0.84)
ln(wages)	-0.145	-0.0910	-0.165
	(-1.82)	(-1.91)	(-1.86)
ln(wages per labor)	0.000969	0.0248	-0.0103
	(0.02)	(1.07)	(-0.23)
Electric Intensity	0.000543	0.000681	0.00267
	(0.36)	(0.66)	(0.86)

Note: Rows are the dependent variables that measure different factors of firm costs. These are coefficients of the independent variable measuring treatment that equals 1 if firm in small, in TN and adopts generator post 2008 and 0 otherwise. Control group includes firms within TN who do not change their generator status and firms outside TN who adopt generator within the same period and firms who dont. This is the DDD specification. All regressions include firm-level controls, Year, State x Industry, State x Year and Firm FE. Errors are robust clustered at the state level. Standard error in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$,$^{***}p < 0.01$

Table II.9: Estimation of returns to investment

	Total Profits *Without Subsidy in TN*	Total Profits *With Subsidy in TN*	Total Profits *Outside TN*
Price of generator			
6 Hours	8.470*	11.29*	10.84***
	(2.11)	(2.11)	(4.36)
12 Hours	16.94*	22.79*	18.37***
	(2.11)	(2.11)	(4.36)
18 Hours	25.41*	34.05*	32.69***
	(2.11)	(2.11)	(4.36)
24 Hours	33.88*	45.17*	43.37***
	(2.11)	(2.11)	(4.36)
Observations	28080	28080	29464

Note: Rows are the dependent variables that measure different factors of firm costs. These are coefficients of the independent variable measuring treatment that equals 1 if firm in small, in TN and adopts generator post 2008 and 0 otherwise. Control group includes firms within TN who do not change their generator status and firms outside TN who adopt generator within the same period and firms who dont. This is the DDD specification. All regressions include firm-level controls, Year, State x Industry, State x Year and Firm FE. Errors are robust clustered at the state level. Standard error in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$,$^{***}p < 0.01$

Table II.10: Effects of generator adoption on investment

	(1)	(2)	(3)	(4)
	ln(investment)	ln(investment)	ln(investment)	ln(investment)
	6 hours	12 hours	18 hours	24 hours
Adopted Generator - within TN	0.281	0.485	0.411	0.422
	(0.300)	(0.274)	(0.269)	(0.257)
Observations	14945	14976	14992	15003
Adopted Generator - DDD	-0.0624	0.124	0.0229	-0.0291
(With subsidy)	(0.223)	(0.210)	(0.215)	(0.213)
Observations	23316	23341	23356	23369
Number of Clusters	6	6	6	6

Note: The sample for first row includes all firms within TN. *Adopted Generator* is an indicator that equals 1 if a small firm adopts generator post 2008 and 0 otherwise. Regressions include Firm, Industry and Year FE and firm-level controls. Dependent variable is the log of net investment (investment in capital - cost of generator) and the columns represent different potential costs of generator. Errors are robust and clustered at the firm-level. Second row is DDD specification whereby *Adopted Generater(With Subsidy)* is equal to 1 if the firm is small, in TN and adopted a generator post 2009 and 0 otherwise. Control group includes firm in TN that did not change their generator status and firms outside TN (including those who changed their generator status. Apart from firm FE, these regressions also include State x industry and State x Year FE and firm-level controls. Errors are robust and clustered at the state-level. Standard error in parentheses.
$^{*}p < 0.10$, $^{**}p < 0.05$,$^{***}p < 0.01$

Appendix

Robustness Checks

Table II.A.1: Effects on generator ownership of large firms

	(1)	(2)	(3)	(4)
	1(Self-Generator)	ln(selfgenrate)	ln(fuelcosts)	ln(output)
Placebo Treatment	0.0268	0.194	0.0203	-0.0284
	(0.0285)	(0.118)	(0.211)	(0.0837)
Year FE	Yes	Yes	Yes	Yes
State x Industry FE	Yes	Yes	Yes	Yes
State - Year Trend	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Number of Clusters	6	6	6	6
Observations	3048	2179	2236	3048

Note: Sample is restricted to large firms who do not qualify for subsidy. *Placebo Treatment* is an indicator that equals 1 if the firms is in TN and post 2008 and 0 otherwise. Errors are robust and clustered at the state level. Standard error in parentheses. ${}^{*}p < 0.10$, ${}^{**}p < 0.05$,${}^{***}p < 0.01$

Table II.A.2: Effects on generator ownership prior to 2008

	(1)	(2)	(3)
	1(Self-generator)	ln(selfgenrate)	ln(revenue)
False Treatment	0.00000442	0.00351	-0.00334
	(1.37)	(0.28)	(-0.06)
Observations	18363	18363	12546

Note: Sample is restricted to firms pre-2008 (before subsidy). *False treatment* is an indicator that equals 1 if firm is in TN and year > 2006 and 0 otherwise. All regressions include Year FE, StatexYear FE and StatexIndustry FE. Errors are robust and clustered at the state level. Standard error in parentheses.
${}^{*}p < 0.10$, ${}^{**}p < 0.05$,${}^{***}p < 0.01$

Table II.A.3: Effects on generator ownership with KA as control

	(1)	(2)	(3)
	1(Self-Generator)	1(Self-Generator)	1(Self-Generator)
Treatment	0.0578***	0.0363**	0.0888**
	(0.0000745)	(0.000314)	(0.00626)
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State - Year Trend	No	Yes	Yes
Controls	No	No	Yes
Observations	19325	19325	12598

Note: Sample is restricted to firms in TN and KA with KA serving as the control group. *Treatment* is an indicator that equals 1 if firm is in TN and year > 2008 and 0 otherwise. Errors are robust and clustered at the state level. Standard error in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$,$^{***}p < 0.01$

Table II.A.4: Returns estimated using the IMR ratio

	(1)	(2)
Outcome: Profits	Without Subsidy	With subsidy
6 Hours	4.274*	5.699*
	(0.94)	(1.25)
12 Hours	8.549*	13.529*
	(1.88)	(2.48)
18 Hours	12.824*	11.399*
	(2.82)	(2.50)
24 Hours	17.099*	22.799*
	(3.75)	(5.00)
Observations	64692	64692
Censored Observations	4465	4465

This set of regressions use the heckman procedure to determine the control group rather than the propensity score method used in the rest of the paper All regressions include Year FE, Firm FE, State x Year FE, State x Industry FE and firm-level controls. *Treatment* is an indicator that equals 1 if firm is in TN, adopting generator in year > 2008 and 0 otherwise. Errors are robust and clustered at the state level. Standard error in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$,$^{***}p < 0.01$

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Chapter III

On the Impact of Structural Reforms on Output and Employment: Evidence from a Cross-Country Firm-Level Analysis

1. Introduction

Since the global financial crisis policy-makers across the world have been grappling with subdued growth. Data suggest that the recent growth slowdown was mainly the result of a deceleration in productivity growth combined with other secular forces, such as low population growth and structural transformation (IMF 2015, 2017; Eichengreen, 2015). While the productivity slowdown could already be observed in advanced economies before the crisis, the slowdown in emerging markets became more evident after 2010. Reviving productivity growth is particularly important for emerging markets to allow for convergence toward higher living standards and lift their population out of poverty. In this context, structural reforms have been identified as a key remedy to lift potential growth over the medium term by boosting productivity.

Structural reforms cover a wide range of measures that, in principle, can tackle obstacles to an efficient resource allocation. Thus, by increasing the flexibility of the economy to respond effectively to changes in economic fundamentals, structural reforms can raise productivity, employment, investment, and efficiency in the production of goods and services, thereby ultimately making a country more resilient to shocks (Culiuc and Kyobe, 2017). Such reforms are usually undertaken in labor and product markets, trade, and institutional framework. Where high cost of labor dismissal prevents firms from adjusting to economic downturns, labor market reforms that reduce those costs can make labor markets more adaptable to changes in the economy and lead to higher employment in the long run. Product market reforms that, for example, reduce the entry barriers in network sectors, such as electricity and telecommunications, typically increase competition and reduce price mark-ups, decreasing the rents of a few protected firms (Blanchard and Giavazzi, 2003), while allowing for cheaper and often better goods and services provision, thereby also benefiting consumers. Trade reforms, such as the reduction in tariffs and the loosening of non-tariff barriers, can also improve resource allocation across countries, reduce rents, and promote technology transfers (Alfaro et al., 2009). Institutional reforms, such as a stricter enforcement of contracts and property rights, a faster resolving of insolvencies, and reductions in red tape and corruption, can also lead to more investment by reducing firms' costs of adjusting the capital stock in reaction to changes in fundamentals (Alesina et al, 2005). That said, the payoffs from alternative structural reforms depend on various country-specific factors, including the stage of development (Dabla-Norris, Ho and Kyobe, 2016; IMF, 2015; and Prati et al., 2013). In particular, previous studies suggest that advanced economies tend to generate larger marginal gains from financial market deepening and product and labor market reforms, while developing countries benefit more from trade liberalization and less state intervention in agriculture. This indicates that countries should prioritize structural reforms according to their level of development.

While related studies have assessed the effects of structural reforms, the role of sectoral and firm characteristics in affecting these payoffs has been less explored, which is the main focus of this paper. This paper studies the effects of labor, product market, trade, and institutional reforms on firm-level output and employment growth using a local projection method. It uses a firm-level panel dataset covering 30 advanced and emerging economies over the period 2003-2014, with over 4 million firm-year observations. Beyond revisiting the role of standard macroeconomic variables, we also analyze whether the effects of structural reforms depend on firm characteristics, such as size, profitability, leverage, and sector. Our goal is to better understand the mechanisms through which structural reforms operate, as well as uncover the heterogeneous effects of structural reforms on different firm types, which would help improve the design of these reforms. Finally, the granularity of our firm-level data allows us to control for unobserved variables that affect productivity and structural reforms simultaneously at the country level through a richer set of fixed effects.

In line with previous studies, our results indicate that structural reforms do have a positive effect on output and employment, particularly over the medium term. The effect of structural reforms on output tends to be larger than on employment, suggesting that structural reforms increase firm productivity. Furthermore, we find differential payoffs from different reforms. Product market and institutional reforms seem to have higher payoffs than labor market and trade reforms during our relatively short sample period. However, this may be so because labor market reforms were identified during economic downturns in our sample and the effects of labor reforms were previously found to be procyclical (IMF, 2016). At the same time, most trade reforms were already implemented before 2004.

Our findings also suggest that the effects of structural reforms vary with firm characteristics. Specifically, labor market reforms are more beneficial for large, high-skill, and low labor-intensive firms. Intuitively, larger firms have a greater likelihood of being subject to organized labor and thus, labor regulations are generally more binding for them. Large firms tend also to be more flexible to benefit from less stringent labor regulations by reallocating resources to increase productivity. Product market reforms have stronger positive effects on more leveraged, low-skill, and service-oriented firms, signaling that the benefits of this reform act through lower input prices for firms relying more on intermediate inputs from network sectors. Trade reforms appear to provide additional benefits to more profitable, leveraged, and service firms. This may be because profitable firms are more able to compete with international competitors and leveraged firms benefit from the financial sector liberalization embedded in trade reforms. Moreover, the recent trade liberalization experience across most countries in our sample had a higher focus on services. Finally, the effects of institutional reforms are larger for service firms. In general, leveraged and service firms tend to profit more from structural reforms. Leveraged firms profit more from lower input prices and broader access to financial services, while recent structural reforms have focused more on service sectors, as these are usually more regulated than manufacturing sectors. These results emphasize that structural reforms affect different types of firms unevenly and have important heterogenous effects that policymakers should be aware of.

The remainder of the paper is structured as follows. Section 2 summarizes the literature on structural reforms. Section 3 discusses the methodology. Section 4 describes the data on structural reform indices, including how the structural reform shocks are identified, as well as the firm-level data. Section 5 presents the results and conducts an analysis of the mechanisms through which structural reforms operate by comparing how the effects of structural reforms vary across different firm characteristics. Finally, Section 6 concludes.

2. Literature Review

The recent decline in productivity has ignited a rising interest on the effects of structural reforms. Several studies at the sector (Dabla-Norris et al., 2015; Bouis, Duval and Eugster, 2016) as well as firm level (Bertrand and Kramarz, 2002; Fabrizio et al., 2007; Goolsbee and Syverson, 2008; Schivardi and Viviano, 2011; Arnold et al., 2016; Gal and Hijzen, 2016; IMF, 2016; Lanau and Topolova, 2016) have shown that structural reforms increase productivity and that their effects take time to materialize. These studies are, however, largely focused on advanced countries.

For example, Bassanini and Duval (2009) find that labor productivity tends to be weaker in industries with more stringent employment protection. Henrekson and Johansson (2010)'s results suggest that less stringent labor market institutions increase aggregate productivity by reallocating labor to more productive firms and through firm entry and exit. The critical role of timing and type of reform is highlighted in the work of IMF (2016), Adascalite and Morano (2016), and Duval and Furceri (2017). Using a sample of advanced economies, they show effect of different types of labor market reforms depend on business cycle conditions. Reductions in labor tax wedges and increases in public spending on active labor market policies have larger effects during contractionary periods, while the effects of lowering employment protection and benefits are procyclical.

For product market reforms, IMF (2016) shows that such reforms lead to output and employment growth, independent of the business cycle. Lanau and Topolova (2016) combine firm-level data from Italy with product market reform indicators during 2003–13 to find that deregulation in network sectors had a positive impact on the value added and productivity of firms in these sectors, as well as on firms using these intermediate inputs in their production process. They also find that these effects are stronger in Italian provinces with more efficient public administration, emphasizing the need for complementary policies to augment the benefits of structural reforms. Gal and Hijzen (2016) use firm-level data for OECD countries to document the positive impact of product market reforms on employment, capital, firm entry, and output of firms in the deregulated sectors. They find that the positive effects are weakened for credit-constrained firms, larger firms in network industries, and smaller firms in retail trade.

There is an extensive literature that studies the effects of trade liberalization on firms, with consistent evidence pointing to a reallocation towards more productive firms and products (Pavknick, 2002; Melitz, 2003; Muendler, 2004; Amiti and Konings, 2007; Fernandes, 2007; Bernard et al., 2007; Topalova and Khandelwal, 2010; Eaton, Kortum and Kramarz, 2011). The evidence suggests that firms' initial level of productivity is an important determinant of the extent of gains from trade reforms. There is also evidence that other firm characteristics, such as size, import intensity of inputs, and financial health, play an important role. For example, Nataraj (2011) finds that India's unilateral reduction in final goods tariffs increased the average productivity of small, informal firms, whereas reduction in input tariffs led to an increase in productivity among larger, formal firms. Furthermore, liquidity constraints of firms can severely impede their ability to export both at the extensive margin and intensive margin (see for example Minetti and Zhu, 211; Manova, 2012; Chaney, 2016).

The quality of institutions has been established as a critical driver of private investment and entrepreneurship. Seminal work by Acemoglu, Johnson and Robinson (2005) suggests that differences in economic institutions are the fundamental cause of differences in economic development. Laeven and Woodruff (2007) show that improvements in the quality of legal systems lead to increased firm size by reducing the idiosyncratic risk faced by firm owners. Beck, Demirgüç-Kunt, and Maksimovic (2005) find that effects of financial, legal and corruption constraints on firm's growth is exacerbated for the smallest firms.

Against this burgeoning literature, the main contribution of our paper is twofold. First, we extend the analysis beyond advanced economies by including firms from emerging markets in our sample. Our findings suggest that the macroeconomic effects of structural reforms on emerging markets are broadly similar to the effects in advanced economies. Second, we analyze in a comprehensive manner the effect of different types of structural reforms on output and employment at the firm level. These findings provide a richer perspective on the effects of reforms based on firm-level characteristics. In particular, the paper highlights the heterogeneous effects of alternative structural reforms, which are important to keep in mind in order to design measures that can deliver strong and inclusive growth.

3. Methodology

Baseline specification

We are interested in assessing the impact of structural reforms on firms' output and employment growth over time. For this purpose, we derive impulse response functions using the local projection method (Jordà, 2005; Teulings and Zubanov, 2014).[76] Specifically, our main regression specification is:

$$y_{isc,t+h} - y_{isc,t-1} = \beta_{0,h} + \beta_{1,h} R_{ct} + \beta_{2,h} X_{isc,t-1} + \beta_{3,h}(gdp_{c,t+h} - gdp_{c,t-1}) + \mu_s + \theta_c + \omega_t + \epsilon_i sc_{t+h}$$

$$\forall h = 0, 1, 2, 3, 4, 5 \qquad (1)$$

where the subscript isc,t refers to firm i, in sector s, country c, and year t. y is the log of output (log of employment); R refers to a structural reform shock as defined below and is an indicator variable that equals one in the event of a major structural reform, and 0 otherwise; X are firm-specific characteristics that may influence output growth such as size (measured taking the log of total assets); $(gdpc, t + h - gdpc, t - 1)$ is a country's cumulative GDP growth; and μ_s, θ_c and ω_t are sector, country, and year fixed effects, respectively.[77] Finally, h

[76]A key advantage of the LPM over a VAR is that the former is more robust to misspecification, as it does not require identification restrictions. Furthermore, non-linear specifications can be easily introduced.

[77]To avoid endogeneity issues, these firm characteristics are fixed and taken from the year previous to the shock.

is the horizon of the impulse response function.

We control for firm size as this variable is likely to have a large influence on a firm's productivity by being correlated to access to finance, export status, and leverage. We also include a country's cumulative growth rate over the period of interest to control for changes in the economy-wide business conditions and alleviate concerns of endogeneity with the timing of the structural reforms, as higher GDP growth may influence the likelihood of reforms and increase firm output at the same time. Sector and country fixed effects capture time-invariant characteristics, such as the education level of the workforce, distance to frontier, trade openness, and regulations in place that will influence firms' output growth, while year fixed effects capture common trends across countries, such as the global financial crisis. A robustness check considers the inclusion of a richer set of fixed effects.

We are mainly interested in the sign and significance level of the coefficients $\beta_{1,h}$, i.e. we want to know whether structural reform shocks have a significant impact on output (employment) growth at different horizons, and whether it is positive or negative. Standard errors are robust and clustered at the frequency of the structural reform shock, our variable of interest. Thus, standard errors are clustered by country and year in the case of labor market, trade, and institutional reform shocks, and by country and sector in the case of product market reforms.[78] The equation above is estimated for each horizon of $h \in 0, .., 5$ and impulse response functions are computed using the estimated coefficient $\beta_{1,h}$ with the corresponding 90 percent confidence band level associated with the estimated standard errors of $\beta_{1,h}$. We assume that the shock happens at period t=0.

Interaction of structural reforms with firm characteristics

In a second step, we modify the baseline specification to allow the effect of structural reforms to vary with firm characteristics in order to get a better understanding of the mechanisms through which structural reforms operate and their heterogeneous impact. Specifically, for every firm characteristic (size, profitability, leverage, sector, skill level of the labor force, labor intensity, and technological intensity) we calculate a dummy which indicates whether the firm is above the median of this firm characteristic in its country and interact this dummy with the structural reform shock variable in the following specification:[79]

$$y_{isc,t+h} - y_{isc,t-1} = \beta_{0,h} + \beta_{1,h} R_{ct} + \beta_{2,h} R_{c,t} x d_i + \beta_{3,h} d_i + \beta_{4,h} X_{isc,t-1} + \beta_{5,h} (gdp_{c,t+h} - gdp_{c,t-1})$$

[78] The results do not change if the standard errors are clustered by country only.

[79] Again to avoid endogeneity issues, this firm characteristic dummy does not change over time and is based on values one year prior to the structural reform shock.

$$+\mu_s + \theta_c + \omega_t + \epsilon_i sc_{t+h}, \forall h = 0, 1, 2, 3, 4, 5 \qquad (2)$$

We then check the sign and significance of the coefficient $\beta_{2,h}$ to determine whether a specific firm characteristic influences the magnitude of the effect of structural reforms.

4. Data

Structural Reforms

Data on structural reforms come from a variety of sources. For labor market regulations, we focus on dismissal protection reforms, more specifically on the *mandated cost of worker dismissal* from the World Bank's Ease of Doing Business Report, as this index has the widest country coverage. For product market reforms, we use the *OECD Regulatory Impact index* which measures the cost of anti-competitive regulation in upstream network sectors (energy, transport, and communication) on 37 downstream sectors that use intermediate inputs from these network sectors in their production. When calculating the cost of regulation this index takes into account the input-output linkages between sectors. For trade regulations, we use the index measuring the *non-tariff regulatory trade barriers* from the Fraser Institute. The focus on non-tariff trade barriers is mainly because most tariff reductions happened already in the 1990s and have been extensively studied in the literature.[80] For institutional reforms, we take the *legal rights* index from the Fraser Institute. This measure has been commonly used in the literature studying the effects of sound institutional and legal systems, including IMF (2015) and Dabla-Norris et. al (2016). Quality institutions can promote investment by reducing uncertainty and improving the efficiency of resource allocation, thereby boosting productivity growth. All reform indices are available at an annual frequency. Product market indices are available from 1975 to 2013 while all others are available from 2000 to 2014.[81] However, since firm-level data are available only from 2003 to 2014 and we want to measure the effect of structural reforms over a 5-year period, we consider structural reforms between 2004 and 2009. We have to start in 2004 because the firm characteristics used as controls are lagged. Appendix and Figure III.1 provide more details on the structural reform dataset.

We transform all indices so that a higher value implies more liberalization, i.e. lower costs,

[80] Tariff measures have commonly been used to study the effects of trade reforms on productivity. However, much of the tariff reductions took place between the 1980s and 1990s. The tariff rates are already at low rates, close to the frontier, so any changes we capture in the 2000s would be small. In fact, we find no significant evidence of lower average tariff rates having any positive effects on firm output or productivity in line with previous studies (IMF, 2015; Dabla-Norris et al., 2016). Hence, given our focus on regulations, we study the effects of deregulating non-tariff trade barriers instead.

[81] Mandated cost of dismissal, trade, and institutional reform indices are available at a five-year frequency between 1970 and 2000.

less regulation and government intervention, and lower trade barriers. The reform indices change little from year to year. Following previous studies, in particular Bouis et al. (2012a, 2012b), a reform shock is identified as an increase in the structural reform index that exceeds two standard deviations of the change in the indicator over all observations in the sample, and the reform variable is defined as a dummy variable which takes a value of one when a reform shock is observed.[82] The focus on large episodes allows us to treat them as a shock and to estimate impulse responses using a dynamic specification.[83] Further, the identification strategy also reflects evidence that the marginal effect of reforms tend to be greater when the policy reform is large. While discretizing the reform measure neglects its intensity, it also partially attenuates endogeneity issues. In robustness checks, we repeat the analysis with two alternative identification methodologies: one that takes the 5 percent largest changes in the reform indices, and another that uses the absolute changes in the reform indices, exploiting the variations in the size of the reform.

The incidence of reform shocks is relatively rare. Table III.1 shows the identified reform shocks for the countries in our firm-level sample. In our sample the incidence of reform shocks is on average 4 percent of all potential country-year (or country-sector-year in the case of PMR) observations. Reforms reducing the cost of worker dismissal were the most observed in this period with a 6 percent incidence, while the other reform shocks have an incidence of around 3 percent. All labor market reforms in our sample happened in 2009, following the outbreak of the financial crisis. In our sample most product market reforms happened in Korea, Slovenia, and Spain. Korea experienced the highest number of structural reforms shocks in our sample, with reforms in all areas, except in the labor market. As a check, we were able to validate the reform shocks identified with our approach of using two standard deviations with evidence from news articles and IMF staff reports. For instance, Bosnia and Herzegovina joined the CEFTA trade agreement in 2006. Similarly, Bulgaria and Romania liberalized trade, particularly in services, in addition to lowering trade tariffs in the years prior to their ascension to the EU in 2007. Hungary also privatized several companies in upstream sectors in the period 2007-09, such as the national airline company and the rail freight of the national railway company.

Firm-level data

The firm-level data come from the Orbis dataset. The sample covers the years 2003-14 and contains around 4 million firm-year observations coming from 30 countries. Of these 30

[82]For OECD product market reforms, we use 1 standard deviation as the cutoff for reform shocks to have a large enough sample that allows for identification of effects.

[83]See Bordon, Ebeke, and Shirono (2016) for more details on the measure of reform shock.

countries, 11 are emerging markets which account for around 40% of the firm-year observations. This sample is constructed after using several cleaning criteria: i) we drop micro firms with less than 4 employees; ii) we restrict the sample to firms from non-agricultural market sectors (ISIC 5-82), iii) only firms present in the dataset in at least 5 consecutive years are kept so that we can analyze the impact of reforms 5 years ahead and the panel is locally balanced; iv) firms' financial accounts need to refer to the whole fiscal year; v) the financial accounts are either unconsolidated, consolidated but without an unconsolidated counterpart, or the consolidation status is unknown; vi) all firms in the sample need to have non-missing and strictly positive values for employment, assets and gross output (measured in terms of operating revenue) in all years; and vii) a country is included in the sample if it has at least 1,000 firm observations in every year of the sample. We deflate all nominal variables using the WEO GDP deflators. We opt for aggregate deflators to keep our sample as large as possible. A robustness check using sector-level deflators from the WIOD SEA and still keeping 75 percent of the original sample shows that results are robust to the choice of deflators.[84] The coverage of the firm-level data is presented in Table III.2.

The outcome variables are firms' output and employment growth, whereby output is proxied by a firm's operating revenue and employment by the number of employees. Ideally, one would use TFP growth instead of output growth as an outcome variable, but to calculate TFP growth we would need data on a firm's value added and capital stock, which would in turn considerably reduce the size of our sample. In addition, there is considerable uncertainty about how to measure TFP. As a robustness check, we show that the results hold if we use labor productivity growth, defined as output divided by the number of employees, instead of output growth.

A key question addressed in the paper is whether the effect of structural reforms vary with different firm characteristics. In this regard, we focus on several firm characteristics that can potentially attenuate or strengthen the effects of structural reforms:

Size. Size is proxied by the log of a firm's total assets. It is important to understand whether small or large firms profit more from structural reforms to understand structural reforms' effects on a country's enterprise landscape and employment, and to design potential compensating measures. In many countries a large share of the population is employed in smaller firms, thus reforms that have an adverse impact on small firms could have a significant impact on aggregate employment and welfare. When studying the effects of product market reforms on the firms being deregulated, Gal and Hijzen (2016) find that smaller firms benefit more in network sectors, while larger firms benefit more in retail sectors. They argue that network sectors tend to be characterized with greater market concentration, therefore removal

[84]World Input-Output Database, Socio Economic Accounts, (Timmer et al., 2015).

of entry barriers forces larger firms to cut back on investment and employment so as to defend their market share (thereby gaining relatively less than smaller firms). In contrast, in the retail sector, large competitive firms extend their operations further after liberalization, while traditionally-operated smaller firms suffer. In general, if smaller firms experience higher post-reform growth, this suggests that smaller firms are constrained by the structural rigidities while larger firms are able to overcome these constraints by themselves.

Profitability. Profitability is proxied by return on assets, with return measured by the EBITDA of a firm. Structural reforms can increase aggregate profitability either by increasing the profitability of the most productive firms and/or by closing the profitability gap across firms. Structural reforms are in general expected to increase profitability by lowering input prices. At the same time, the firms directly affected by deregulation will experience stronger competition and only the most productive firm will likely benefit more.

Leverage. Leverage is proxied by the ratio of total debt to assets. Several studies have shown that firms' financial health, leverage, and access to external financing are important determinants of their behavior (Aivazian et al., 2005; Margaritis and Psillaki, 2010; Coricelli et al., 2012; Campello, 2006; Harrison and McMillan, 2003). Nucci et al. (2005) find that leverage has a negative impact on productivity, while Margaritis and Psillaki (2010) and Coricelli et al. (2012) find a positive impact, the latter showing that the effect of leverage on productivity vary with the leverage level. Gal and Hijzen (2016) find that credit constraints (defined as dependence on external financing) can play an important role in weakening the positive impact of product market reforms on investment. It is not clear in which direction leverage should affect the impact of structural reforms on output. On the one hand, high leverage may signal access to credit and good investment opportunities what would enhance the effects of structural reforms. On the other hand, high leverage may also hinder future access to credit and weaken the impact of structural reforms. At the end, it is an empirical question and past studies have found evidence on both sides of the debate.

Sector. Some countries rely more on manufacturing than services and vice-versa and this could lead to different payoffs from structural reforms. In addition, the share of the service sector tends to increase with a country's development. With manufacturing typically more open to trade and subject to less regulation in most economies, there may be more scope for deregulation in the service sector. Finally, manufacturing and service sectors may differ in the intensity of the inputs used from the deregulated sectors. Looking at the effect of product market reforms, Gal and Hijzen (2016) find that manufacturing firms profit more than service firms. They argue that competition is stronger in manufacturing sectors leading to a higher output-price elasticity, what amplifies the effects of lower input prices on output. In addition, they argue that manufacturing sectors use more inputs from network sectors for production.

Labor force skill level. Labor force skill level is proxied by the average wage level a firm pays its employees. Without distortions, a higher wage would indicate a higher marginal productivity of labor and more skilled labor. If structural reforms are more beneficial for firms with more skilled labor, reallocation towards more productive firms would result in higher aggregate productivity. In contrast, higher payoffs from structural reforms to firms with low skilled labor is indicative that removing structural rigidities allow firms with low labor productivity to grow.

Labor intensity. Labor intensity is proxied by dividing total costs of employees by operating revenues. Structural reforms that benefit more labor-intensive firms can lead to higher employment and are particularly important for countries with high unemployment rates.

Technology intensity. Technology intensity is proxied by dividing the consumption of ICT intermediate inputs by total gross output in the firm's sector.[85] Firms with more use of ICT intermediate inputs are likely more productive. We would thus expect such firms to benefit more from structural reforms.

5. Results

Baseline Results

Figures III.2 and III.3 report our baseline results, showing the estimated impulse response functions of the effects of structural reforms on output and employment growth, respectively. The vertical axis shows the cumulative change measured in simple units, i.e. an increase of 0.05 indicates a 5 percent cumulative increase. The baseline results suggest that structural reforms have a significant positive effect on output and that this effect tends to materialize with some lag. The effects on employment are insignificant, suggesting that structural reforms increase labor productivity.

The results in Figure III.2 indicate insignificant output effects in the short term from labor market reforms. Instead, the positive output effects materialize only 5 years after the reform implementation. The effect of labor market reforms on employment growth is insignificant in the short and medium term (Figure III.3). IMF (2016) shows that employment protection reforms have a procyclical effect on output and employment growth, i.e. they increase output during upturns and decrease output during downturns. Without differentiating between

[85]Data on the consumption of ICT intermediate inputs at the sectoral level come from the World Input-Output Database (Timmer et al., 2015). This measure is at the sector level because of data constraints. ORBIS does not have data on the ICT inputs.

upturns and downturns, IMF (2016) also finds that labor market reforms have no significant effect on output. Since labor market reform shocks were only identified during downturns in our sample, our results are unable to shed light on whether the effects of labor market reforms are positive and significant during upturns.

On the other hand, product market reforms have a significant positive effect on output growth and this effect strengthens over time, indicating that it takes time for the benefits of structural reforms to materialize. The effects of product market reforms on employment growth are not significant, indicating that these reforms likely operate through increased labor productivity. These results are in line with Gal and Hijzen (2016) and IMF (2016), emphasizing the benefits of reduced input prices from network sectors and the better provision of goods and services.

Trade reforms that reduce the non-tariff regulatory trade barriers have positive but statistically insignificant effects on both output and employment in the short and medium terms. The result is not surprising as most trade reforms took place in the 80s and 90s, implying that regulatory trade barriers represent no longer a significant hindrance to firm growth in emerging and advanced economies. Our results are consistent with Dabla-Norris, Ho and Kyobe (2016)'s findings that reducing trade barriers is effective for mostly low-income countries.

Finally, institutional reforms have a positive effect on output growth with no significant effect on employment. These results suggest that firms expand their operations as there is less uncertainty with improved legal systems. Hail and Luez (2006) find that firms under strong legal institutions experience significantly lower cost of capital and invest more. By the same token, Kumar, Rajan and Zingales (2002) also find that improved legal systems increase investment, particularly of firms which rely on intangible assets and the protection of intellectual property.

Heterogeneous effects of structural reforms

One of the advantages of working with firm-level data is that it enables us to analyze how the effects of structural reforms vary with firm characteristics and thereby infer potential heterogeneous effects of reforms. We consider the following firm characteristics: size, profitability, leverage, sector, the skill of the labor force, and capital, labor, as well as technology intensity. Figure III.4 shows how the effect of structural reform shocks on output growth varies with firm characteristics. More precisely, it shows the magnitude of the effect of structural reforms on output growth for each type of firm in the period in which the effect peaks (usually t=5). The significance signs indicate whether the effect of structural reforms is statistically different for different types of firms.

Labor market reforms that reduce the cost of worker dismissal yield stronger returns for larger, high-skilled labor, and low labor-intensive firms. All other firm characteristics do not seem to represent a significant channel through which labor market reforms operate. Large firms are more likely to be constrained by labor contracts and more flexible to benefit from less stringent labor regulations by reallocating resources to increase productivity. On the other hand, low-skill, and high labor-intensive are more likely to adjust (reduce) activities after a lowering of dismissal costs. All labor market shocks in our sample were identified in 2009 and probably led to a reduction in operations. Furthermore, as previously mentioned, there is evidence that the effects of labor market reforms that lower employment protection are procyclical (IMF, 2016). Hence, it is possible that firms which were more exposed to the reform (such as low-skilled and labor-intensive firms) experienced less output growth because the reform was implemented during an economic downturn.

Product market reforms that lower the costs of inputs in production are found to be more beneficial for more leveraged, service-oriented, and low-skilled labor firms. Product market reforms deregulate network sectors increasing competition and ultimately lowering input costs, such as electricity, transport, and telecommunication costs. The decrease in input costs alleviate liquidity constraints that leveraged firms face. In practice, we find that the deregulation benefits the sector that is being deregulated relatively more (i.e., the service sector). More specifically, the firms being deregulated are the ones which face the highest cost of deregulation and thus the ones which should profit more from deregulation. If we exclude firms from the deregulated sectors (ISIC codes 40-1 and 60-4) from our sample, we find that manufacturing firms profit more from the deregulation, consistent with the findings of Gal and Hijzen (2016) who also excluded the deregulated firms from their sample.

Trade reforms (which have tended to focus on services during the sample period of our study) appear to provide greater benefits to more profitable, leveraged, and service firms. This may reflect the relatively greater ability of high-profitability firms to compete with international firms following trade reforms and benefit from the advantages of global value chains. Lowering non-tariff trade barriers lowers the fixed cost of trading, possibly allowing the most leveraged firms who could not previously engage in trade to do so. Furthermore, this reform often also relates to liberalizing the financial sector, which also typically benefit more leveraged firms.

Finally, institutional reforms result in stronger gains for service firms. Institutional reforms, as defined by the strengthening of the legal system, tend to reduce the uncertainty in doing business in a country and increase the protection of intellectual property, thereby generating more investment. These results are in line with Kumar, Rajan and Zingales (2002) who find that the positive effects of legal institutions on firm size in Europe are especially pronounced in industries characterized by low levels of capital intensity. They argue that all legal systems in Europe are of high enough quality to protect investment in physical capital, hence the effects show up through intangible assets such as intellectual property. We find that these results hold in a sample including emerging markets as well.

These results need to be interpreted with some caution, given standard shortcomings in the data which were also found in other related studies. First, the ORBIS firm-level dataset is not necessarily representative for every country. We tried to mitigate this challenge by including in our sample only countries which have at least 1,000 firm observations per year. The results presented here show thus the effect for an average firm in our sample. Second, one of the main channels through which structural reforms work is via entry and exit of firms from the economy, particularly for product market reforms, which cannot be explored as the dataset does not have information on entry and exit dates. If a firm disappears from the ORBIS dataset, it is difficult to ascertain whether the firm has ceased to exist or whether it was simply not sampled in that particular year. In order not to bias our results in different horizons of the local projections, we also require that firms in our sample exist in the dataset for at least 5 consecutive years. Since we do not take both entry and exit into account, it is not clear whether our results are biased downwards or upwards. Third, structural reforms are usually undertaken in packages in different areas of the economy, whereas they are considered in our analysis only one at a time with unclear implications for the direction of bias of the results. Fourth, due to the short availability of firm-level data, we had to restrict our study on the effects of structural reforms to a relatively short period from 2004 to 2009. Finally, this paper estimates the time it takes for structural reforms to have an effect on the economy once they are implemented, but it does not consider that it usually takes years to implement structural reforms. These caveats however are not specific to our findings but are a concern for the broader empirical literature on structural reforms using firm-level data.

Robustness checks

ALTERNATE MEASURES OF REFORM SHOCKS: In the baseline, the occurrence of a structural reform shock is identified if the increase in the structural reform index is larger than two standard deviations. As shown previously, this leads to a shock incidence ranging from 2 to 6 %, depending on the reform shock. We check now whether our results are robust to changing the shock identification. More precisely, we follow Gal and Hijzen (2016) to identify as a structural reform shock the largest 5th percentile changes in each structural reform index in our sample. Figures III.5 and III.6 show that our results are robust to this change. Another way of measuring structural reforms is to take the absolute change of the reform index as the structural reform variable instead of a dummy to account for the reform intensity. Table III.3 shows that our results remain robust.

SECTORAL DEFLATORS: We deflate firms' output growth using sector-level deflators from the WIOD SEA (Timmer et al., 2015). The sample is then reduced by 25%. Results are very similar to the ones using aggregate deflators.

RICHER SET OF FIXED EFFECTS: Our baseline regressions include sector, country, and year fixed effects. In this robustness check we include a richer set of fixed effects by including firm fixed effects and sector-specific time trends. Figures III.7 and III.8 show that our results are robust to the inclusion of a stricter set of fixed effects. The main difference is that now product market reforms have a negative impact on employment. Bassanini (2015) also finds a negative impact of product market reforms on employment.

LABOR PRODUCTIVITY GROWTH: In line with previous studies, we analyze the effect of structural reforms on output and employment growth. Since the effect on output growth is larger than on employment growth, we infer that structural reforms increase output by raising labor productivity. Alternatively, we can directly measure the effect of structural reforms on labor productivity growth, defined as the change in revenue by employee. Figure III.9 confirms that indeed structural reforms increase labor productivity growth.

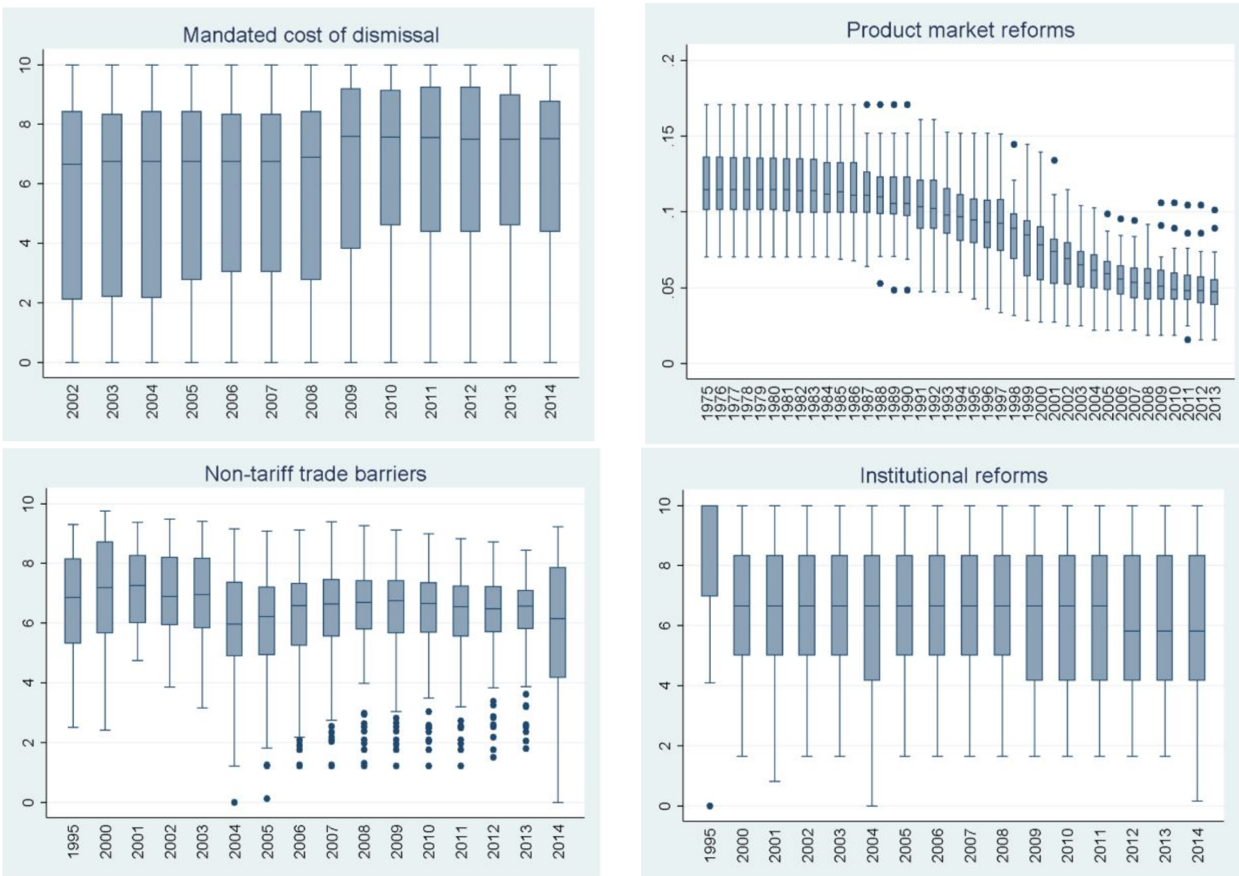
DELETING PREDOMINANT COUNTRIES FROM THE SAMPLE: There are four countries (Japan, Russia, Spain, and Ukraine) representing each more than 10% of the firm-year observations in our sample that may be driving the results. If we repeat our baseline estimations excluding these four countries from our sample, we arrive at very similar results to the baseline model.

6. Conclusion

This paper assesses the impact of a diverse set of structural reforms on firms' output and employment growth in emerging and advanced economies using a firm-level dataset with over four million observations. We find that structural reforms lead to higher output at the firm level and that these effects take time to materialize. The results suggest that structural reforms act through an increase in productivity, as the effects of structural reforms are larger and more significant for output growth than for employment growth. Product market and institutional reforms seem to have higher payoffs than labor market and trade reforms during our relatively short sample period (2004-2009). However, this may be so because labor market reforms were identified during economic downturns in our sample and the effects of labor reforms were previously found to be procyclical (IMF, 2016). At the same time, most trade reforms were already implemented before 2004. Our findings emphasize the role that structural reforms can play in lifting potential growth by increasing firms' productivity.

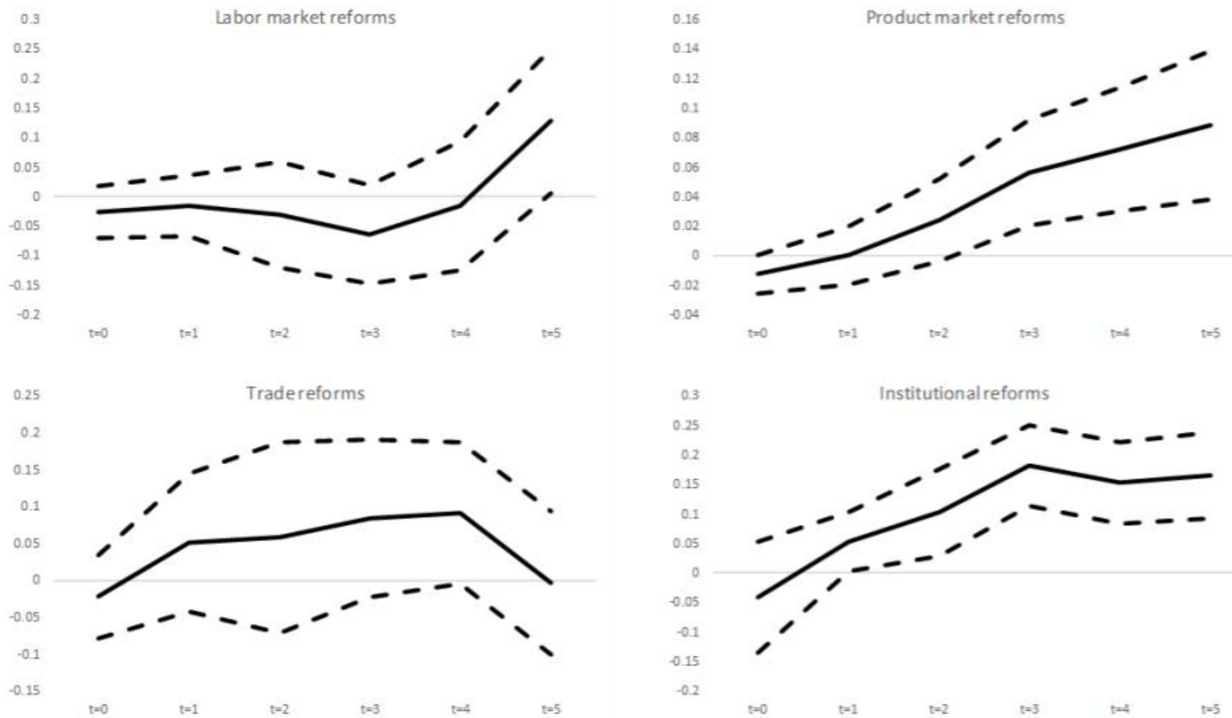
Furthermore, we find that the benefits of structural reforms on firms' output depend on firm-level characteristics, such as size, profitability, leverage, and sector. These results point to the importance of targeting structural reforms to economies based on their firm and sector characteristics to maximize their returns. They also emphasize the usefulness of complementary policies that can mitigate the costs that may arise from the redistribution.

Fig III.1: Trends in Structural reform indices



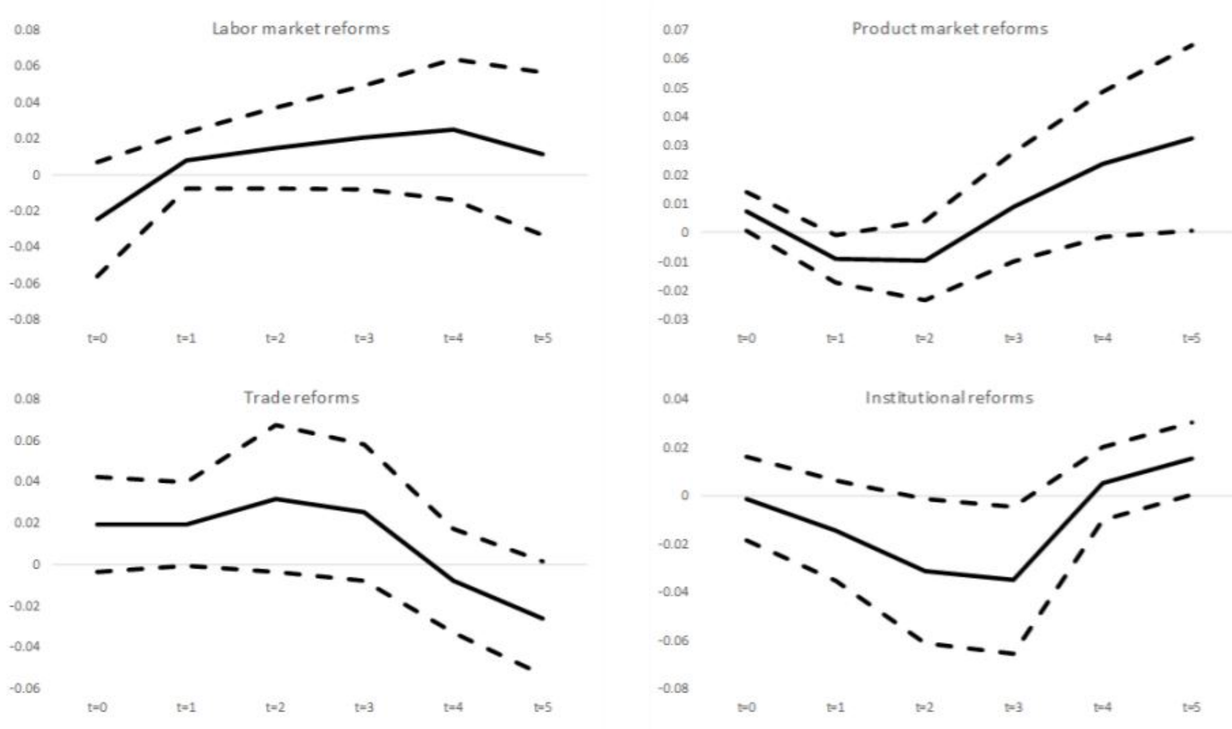
Note: The box plots show the three quartiles of the distribution of the structural reform indices with their lower and upper adjacent values. The circles represent outliers. The labor market, trade, and institutional reform indices are standardized on a scale from 0 to 10, representing the distribution of the underlying data. The product market reform index quantifies the costs of anti-competitive regulation in upstream sectors (electricity, transportation, and communication) on 37 downstream sectors that use the output of these sectors as intermediate inputs.

Fig III.2: Effect of structural reforms on output growth



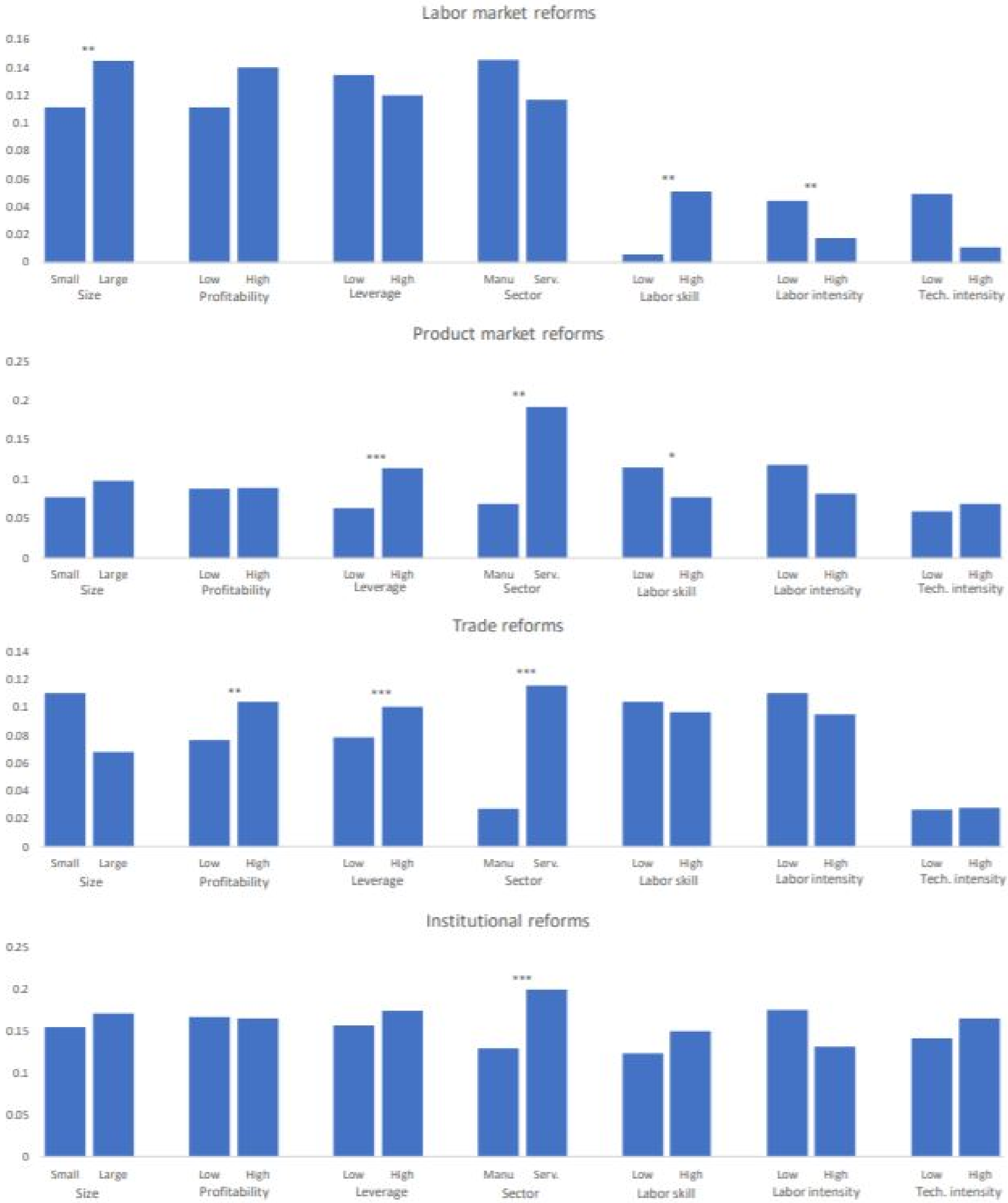
Note: Results are from the coefficient $\beta_{1,h}$ in Equation 1 with output value as the outcome variable. t=0 is the year of the shock. The solid lines denote the response to a major structural reform shock and the dashed lines denote the 90 percent confidence bands.

Fig III.3: Effect of structural reforms on employment growth



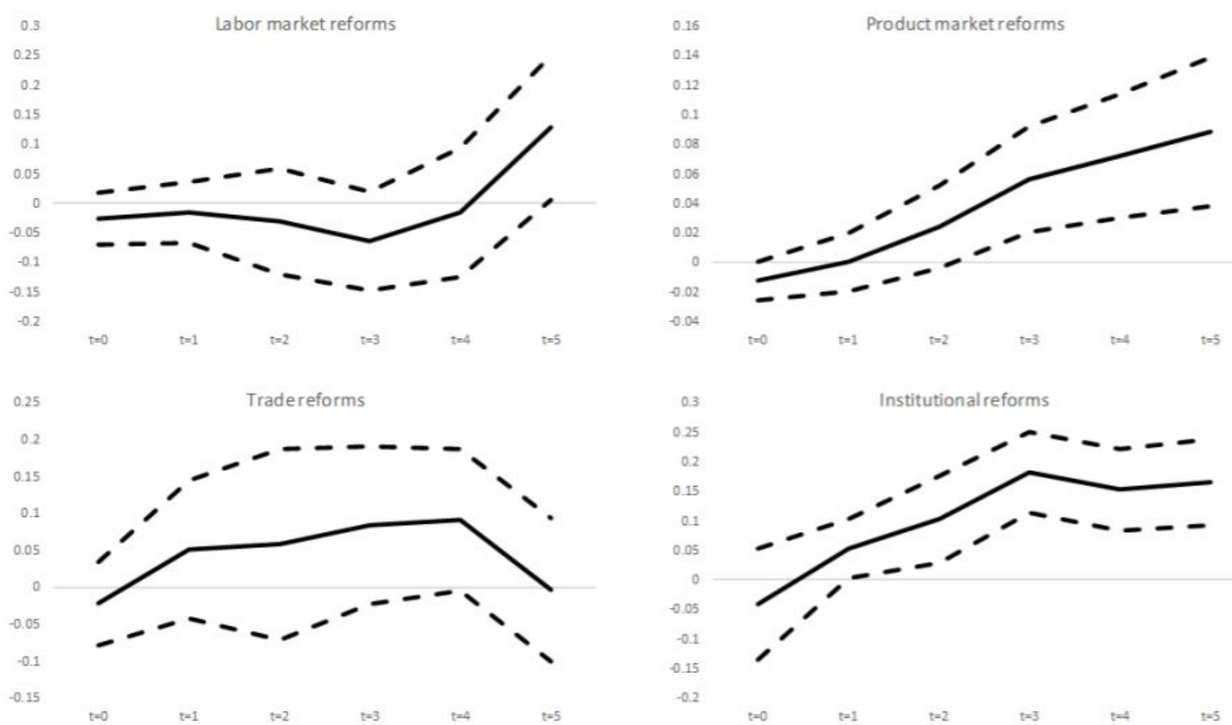
Note: Results are from the coefficient $\beta_{1,h}$ in Equation 1 with number of workers as the outcome variable. t=0 is the year of the shock. The solid lines denote the response to a major structural reform shock and the dashed lines denote the 90 percent confidence bands.

Fig III.4: Effect of structural reforms by firm characteristics



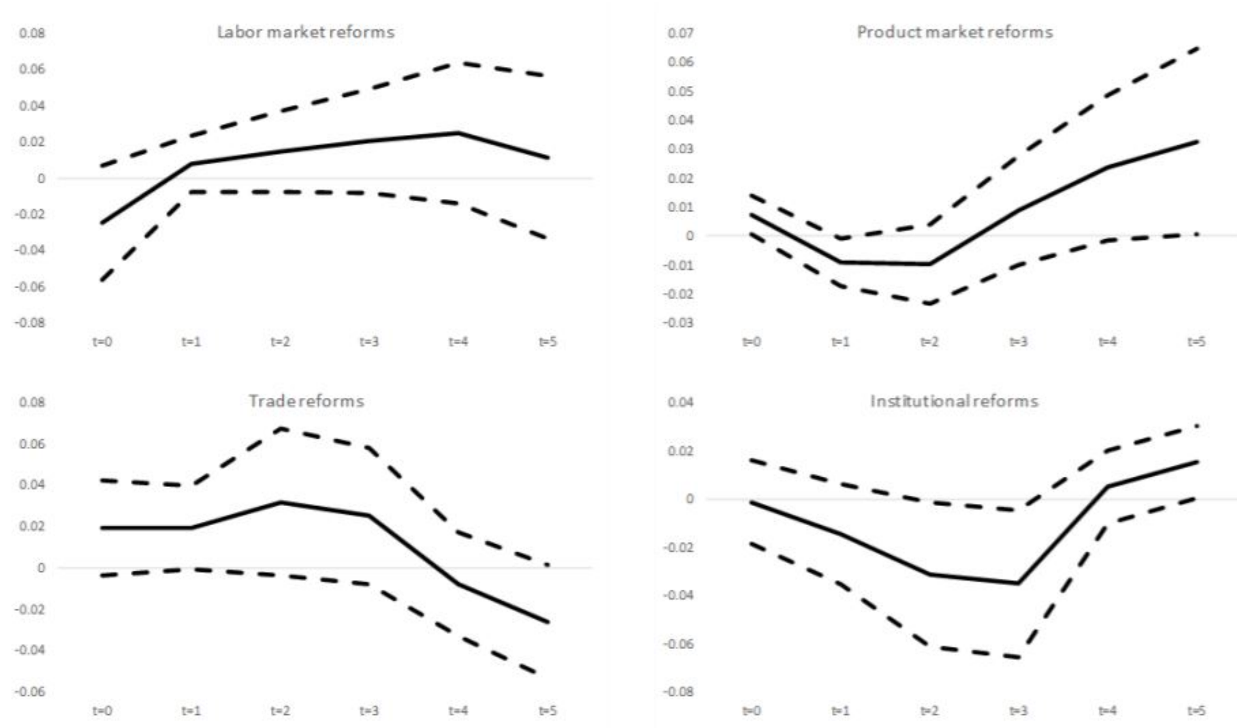
Note: The bar charts show the coefficient size $\beta_{2,h}$ in Equation 2. It measures the effect of structural reforms on output growth at t+5 for firms which are below or above the median of each firm characteristic. ***, **, * indicate whether the coefficients for firms below and above the median of each characteristic are significantly different from each other at a 10, 5, and 1 percent significance level, respectively.

Fig III.5: Effect of structural reforms on output growth - Alternate measure



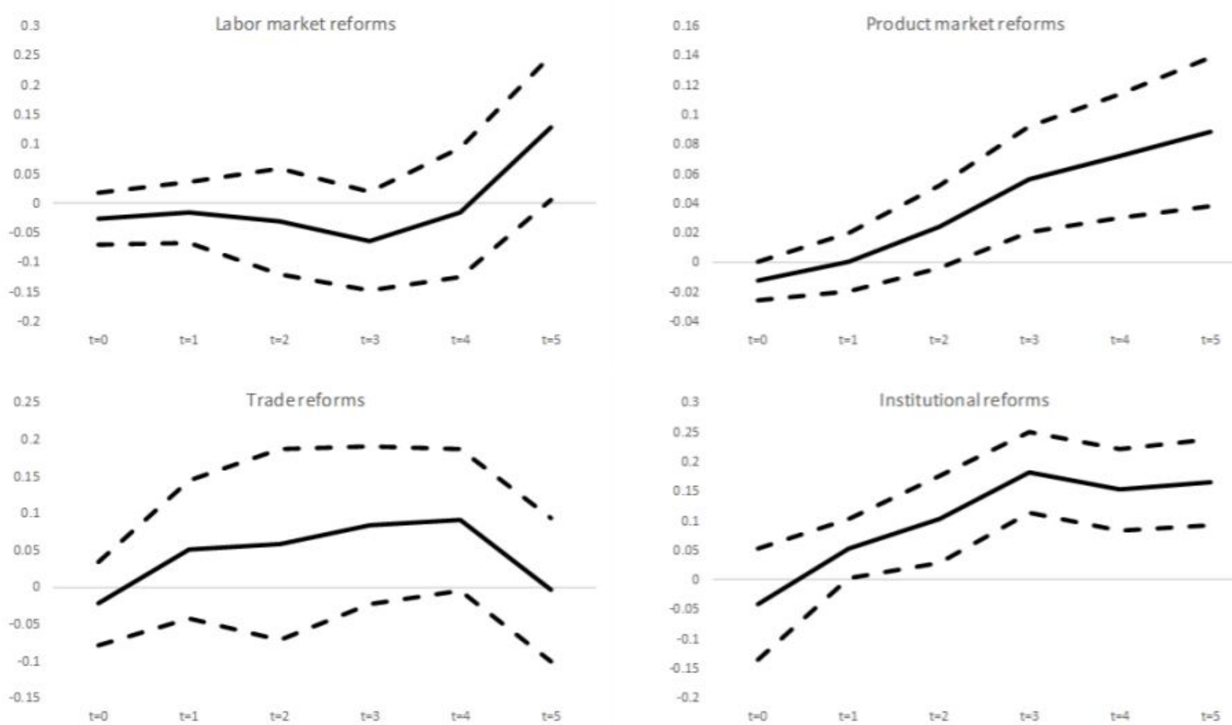
Note: Results are from the coefficient $\beta_{1,h}$ in Equation 1 with output value as the outcome variable. t=0 is the year of the shock. Here, R is a dummy that equals 1 if the change in structural reform index is higher than the 5th percentile. The solid lines denote the response to a major structural reform shock and the dashed lines denote the 90 percent confidence bands.

Fig III.6: Effect of structural reforms on labor growth - Alternate measure



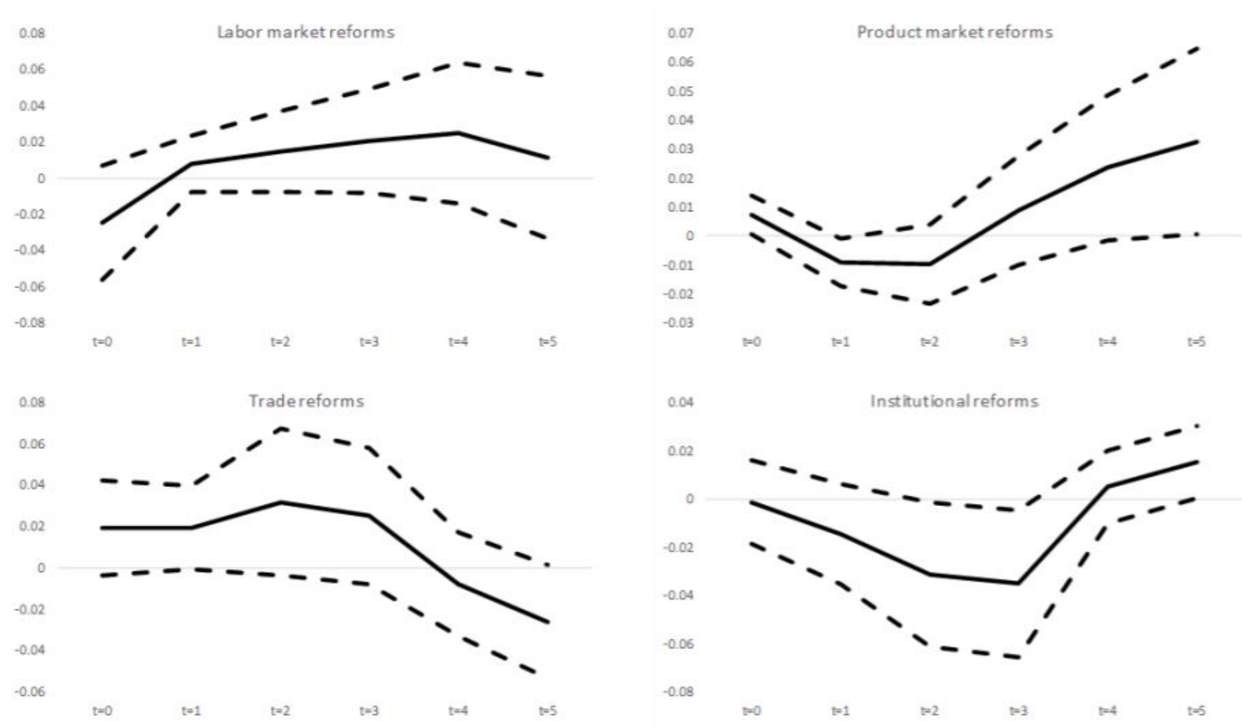
Note: Results are from the coefficient $\beta_{1,h}$ in Equation 1 with number of workers as the outcome variable. t=0 is the year of the shock. Here, R is a dummy that equals 1 if the change in structural reform index is higher than the 5th percentile . The solid lines denote the response to a major structural reform shock and the dashed lines denote the 90 percent confidence bands.

Fig III.7: Effect of structural reforms on output growth - Richer FE



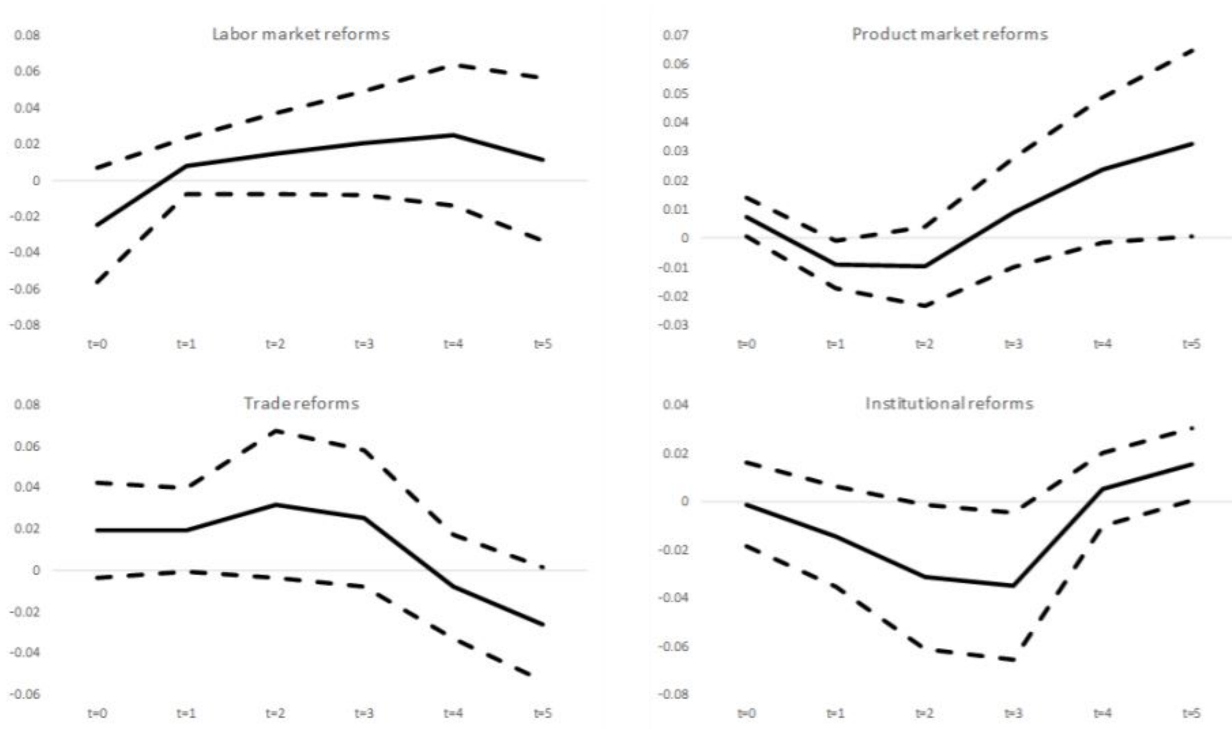
Note: Results are from the coefficient $\beta_{1,h}$ in Equation 1 with output value as the outcome variable. t=0 is the year of the shock. In addition to the country, sector and time fixed effects, these regressions include firm fixed effects and sector-specific time trends. The solid lines denote the response to a major structural reform shock and the dashed lines denote the 90 percent confidence bands.

Fig III.8: Effect of structural reforms on labor growth - Richer FE



Note: Results are from the coefficient $\beta_{1,h}$ in Equation 1 with number of workers as the outcome variable. t=0 is the year of the shock. In addition to the country, sector and time fixed effects, these regressions include firm fixed effects and sector-specific time trends. The solid lines denote the response to a major structural reform shock and the dashed lines denote the 90 percent confidence bands.

Fig III.9: Effect of structural reforms on labor productivity growth



Note: Results are from the coefficient $\beta_{1,h}$ in Equation 1 with labor productivity as the outcome variable. t=0 is the year of the shock. The solid lines denote the response to a major structural reform shock and the dashed lines denote the 90 percent confidence bands.

Table III.1: Reform Shocks by Country period 2004 – 2009

	Labor Market	Product Market	Trade	Institutional
BEL	1(2009)	2(2005, 2006)		
BGR			1(2006)	
BIH			1(2006)	
CZE		2(2005, 2006)		
DEU	1(2009)	1(2005)		
DNK		2(2005, 2006)		
ESP		3(2005, 2007, 2008)		
EST	1(2009)	2(2008, 2009)		
FIN	1(2009)	1(2006)		
FRA		2(2005, 2006)	1(2005)	
GBR	1(2009)	1(2008)		
GRC		2(2005, 2007)		1(2005)
HRV	1(2009)		1(2005)	
HUN	1(2009)	1(2007)		
IRL		1(2007)		
ITA	1(2009)	1(2005)		1(2005)
KOR		3(2005, 2006, 2007)	1(2006)	1(2009)
POL	1(2009)	1(2005)		
ROU			1(2005)	
SVK	1(2009)	2(2005, 2007)		
SVN		3(2005, 2007, 2008)		
SWE	1(2009)	2(2005, 2009)		
Total	11	18	6	3
Incidence	6%	3%	3%	2%

Note: The frequency of product market reforms is measured by the number of years a reform shock is observed. However, when calculating the incidence, we divide all reform incidence per sector x country by all (sector x country x year) observations. The other incidences have a country x year variation.

Table III.2: Firm-level data from Orbis by Country

Countries	Frequency	Percent	Cumulative
BEL	53,723	1.27	1.27
BIH	26,135	0.62	1.89
BGR	57,805	1.37	3.26
CHN	32,414	0.77	4.03
HRV	69,662	1.65	5.68
CZE	80,538	1.91	7.59
DNK	14,469	0.34	7.93
EST	36,827	0.87	8.8
FIN	36,836	0.87	9.67
FRA	155,486	3.68	13.36
DEU	36,950	0.88	14.23
GRC	60,306	1.43	15.66
HUN	18,750	0.44	16.1
IRL	5,299	0.13	16.23
ITA	295,133	6.99	23.22
JPN	450,855	10.68	33.9
KOR	101,892	2.41	36.31
LVA	9,609	0.23	36.54
LTU	11,713	0.28	36.81
POL	34,457	0.82	37.63
PRT	175,340	4.15	41.78
ROU	264,936	6.27	48.06
RUS	617,032	14.61	62.67
SRB	55,568	1.32	63.99
SVK	32,169	0.76	64.75
SVN	23,680	0.56	65.31
ESP	721,526	17.09	82.4
SWE	244,984	5.8	88.2
UKR	402,939	9.54	97.74
GBR	95,270	2.26	100
Total	4,222,303	100	

Note: These are firm x year observations.

Table III.3: Effect of structural reforms using absolute change in reform index

	Employment growth					
	t=0	t=1	t=2	t=3	t=4	t=5
Labor market reforms	-0.0167	0.0079	0.0040	-0.0053	0.0201	0.0104
	0.0182	0.0207	0.0287	0.0257	0.0488	0.0376
Product market reforms	-0.6111***	0.2743	0.8351	1.1452***	1.5440***	1.4608**
	0.2283	0.3608	0.5804	0.5917	0.6601	0.7632
Trade reforms	0.0105	-0.1267	-0.1246	-0.0531	-0.0890	-0.0055
	0.0418	0.0817	0.0898	0.0760	0.0909	0.0659
Institutional reforms	-0.0039	-0.0016	-0.0147	0.0038	0.0177**	0.0046
	0.0102	0.0110	0.0140	0.0165	0.0083	0.0075
	Output growth					
	t=0	t=1	t=2	t=3	t=4	t=5
Labor market reforms	-0.0167	0.0079	0.0040	-0.0053	0.0201	0.0104
	0.0182	0.0207	0.0287	0.0257	0.0488	0.0376
Product market reforms	-0.6111***	0.2743	0.8351	1.1452***	1.5440***	1.4608**
	0.2283	0.3608	0.5804	0.5917	0.6601	0.7632
Trade reforms	0.0105	-0.1267	-0.1246	-0.0531	-0.0890	-0.0055
	0.0418	0.0817	0.0898	0.0760	0.0909	0.0659
Institutional reforms	-0.0413	0.0179	0.0684**	0.1145***	0.1024***	0.1135***
	0.0325	0.0302	0.0355	0.0367	0.0348	0.0321

Note: Results from Equation 1 where R is the absolute change in reform index rather than a dummy variable measuring a shock. Standard error are under the coeffecient. $^{*}p < 0.10$, $^{**}p < 0.05$,$^{***}p < 0.01$

Appendix

Definition of Structural Reforms

Labor Market We use the mandated cost of worker dismissal based on the World Bank's Doing Business report which gives a lower rating to countries with costly requirements for advanced notice, severance payments, and penalties due when dismissing a redundant worker.

Product market Reforms We use the OECD indicators on regulatory impact (RI) which quantifies the potential costs of anti-competitive regulation in upstream sectors (electricity, transportation, and communication) on 37 downstream sectors that use the output of these sectors as intermediate inputs. The measures are available in 32 OECD and 2 non-OECD countries on an annual basis starting in 1975. The cost of regulation in downstream sectors are captured by the Energy, Transport, and Communication Regulation (ETCR) index which covers the degree of liberalization in the telecommunication and electricity markets, including the extent of competition in the provision of these services, the presence of an independent regulatory authority, and privatization.

Trade Regulations We use the regulatory trade barriers index from the Fraser Institute which is an average of the non-tariff trade barriers index from the World Economic Forum's Global Competitiveness Report, the compliance cost of importing and exporting index from World Bank's Doing Business Report and the black-market exchange rates index from the MRI Bankers' Guide to Foreign Currency that gives high score to countries with a domestic currency that is fully convertible without restrictions.

Institutional reforms Institutional reforms are measured by the integrity of the legal system index from the Fraser Institute Economic Freedom Index which is composed using the PRS Group's International Country Risk Guide measure of risk component. This risk component is an average of law and order, with the "law" sub-component measuring the strength and impartiality of the legal system and the "order" sub-component measuring the popular observance of the law.

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