Equilibrium Effects of Firm Subsidies

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Abstract

Subsidy programs affect firms through two countervailing forces: direct gains for eligible firms and indirect losses for firms whose competitors are eligible. I develop a framework to capture the relationship between these two effects, allowing for multi-product firms and heterogeneous sectors. I exploit an expansion of eligibility for small-firm subsidies in India in 2006 to estimate (i) the growth of newly eligible firms, (ii) the extent of crowd-out, and (iii) the program's impact on factor misallocation. Newly eligible firms benefited from the policy change, as their sales grew by roughly 30% after gaining access to the subsidies. Indirect losses vary substantially: I find almost complete crowd-out for products that are less internationally traded, but little crowd-out for more traded products. Relaxing the eligibility criteria for subsidies improved the within-sector allocation of factors; the reduction in misallocation increased aggregate productivity by around .1%.

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I Introduction

Many governments support small firms using a variety of mechanisms, including directed lending, investment subsidies, export assistance, technical training, and preferential procurement.[1] These types of industrial polices are often justified by overarching goals to increase aggregate output and productivity,[2] and can benefit targeted firms substantially. However, the effect that these types of programs have on aggregate output depends on the extent to which eligible firms expand at their competitors' expense. These equilibrium effects will also depend on the characteristics of the targeted firms, as those producing globally traded goods are likely to have different effects on their competitors than firms who are competing in small local markets.

A growing body of work suggests that within-sector resource misallocation is an important source of productivity differences across countries (Hsieh and Klenow 2009; Hopenhayn 2014). Programs supporting small firms may be second-best solutions to pre-existing distortions, such as those in credit markets (Banerjee and Duflo 2014). However, if the eligible firms are relatively unproductive, these types of programs may be the cause of the misallocation of productive factors.[3] Empirically, the effect that these types of programs have on productivity depends on how well targeted they are.

In this paper, I study small firm subsidies in India, leveraging a 2006 policy change relaxing the eligibility requirements for a variety of programs. [4] The newly eligible firms represented around 15% of the formal manufacturing sector. Most sectors included some newly eligible firms, and there was substantial heterogeneity in the extent to which different sectors were exposed.

[1] For instance, other than the UK, each of the G8 countries have state-backed institutions designed to support small firms (Greene and Patel 2013). Bannock 1997 argues that, for all regions, loan guarantee programs are "the rule rather than the exception." See Mor et al. (2013) for a recent report on the specific programs in India, and De Rugy (2006) for the United States.

[2] For instance, this is the motivation for a recent "Call for Innovative Ideas on SME Growth and Entrepreneurship" from the World Bank at http://goo.gl/SQ4kOR (Accessed 11/04/2014)

[3] Guner et al. (2006); Restuccia and Rogerson (2008); Gourio and Roys (2014); Garicano et al. (2012), and García-Santana and Ramos (2013) make versions of this argument.

[4] India also strictly regulates the production of certain products (such as plastic buttons) by firms with assets above a cutoff, a program known as the Small Scale Reservation laws (Mohan 2002; Martin et al. 2014; García-Santana and Pijoan-Mas 2014; Tewari and Wilde 2014) but the eligibility criteria for these did not change in 2006.
Historically, there have been even more strict policies regulating firms' ability to produce certain products in certain locations (see Panagariya (2008); Kochhar et al. (2006); Aghion et al. (2008) and Reed (2014) for further discussion of the history of industrial licensing in India).

My empirical strategy is derived using a Melitz-style framework, with multi-product firms. I assume that the policy change led the newly eligible firms to face lowered costs of inputs, leading to two effects on firm performance. The first, which I term the *direct* effect, reflects the gains to newly eligible firms. The second, the *indirect* effect, captures the equilibrium effects resulting from newly eligible firms' growth. While only newly subsidized firms are directly affected, all firms may be indirectly affected.[5] Using data that is representative of all manufacturing activity in India, I leverage variation in time and firm characteristics in order to separately identify the direct and indirect effects of the policy change. While I apply the technique to estimate the effects of subsidies for small Indian firms, I derive a more general relationship capturing the effect on a firm's revenue of a change in their competitors' prices. This technique is therefore applicable to a wide variety of settings where researchers are interested in understanding the extent of product market spillovers. This strategy allows for a direct calculation of the extent of equilibrium effects, as the estimated indirect effect is a sufficient statistic for the elasticity of aggregate growth with respect to private growth.

The resulting estimation strategy is intuitive: the indirect effect on each firm from an expansion of eligibility for a subsidy program will be a weighted average of the size of each firm gaining access to the subsidies. The effect on each each firm is a function of (a) the product mix of that firm; (b) for each of those products, the share of value produced by newly subsidized firms; and (c) the products' characteristics such as the elasticity of substitution or if the products are traded internationally. The model predicts neither the sign nor the magnitude of these spillovers: depending on the values of the parameters, it is consistent with a range of equilibrium effects including complete crowd-out and positive agglomeration spillovers. Understanding the aggregate effects of the eligibility expansion therefore requires an empirical analysis.

I find that the programs had large direct effects, as newly eligible firms increased their sales

[5] My framework abstracts from other potential general equilibrium effects. For instance, three possible sources of these other effects are a) if firms distorting their size in order to maintain eligibility, b) if the policy change affected the prices paid by firms whose eligibility status was unchanged, and c) if the costs of raising revenue to pay for the subsidies affected the economy as a whole. In Appendix Table 1, I find no evidence of distortions in the firm-size distribution around the cutoff. In Table 8, I find no evidence that the policy change affected the input prices of the newly eligible firms' competitors. In all of the regressions, I include fixed effects for each year in order to control for economy-wide general equilibrium effects.

by around 30%. This finding is consistent with Banerjee and Duflo (2014), Sharma (2005), and Kapoor et al. (2012), which study earlier eligibility changes for a similar set of programs in India. Calibrating the model with the estimated coefficients can explain around 2/3 of the actual policy-induced growth of the newly eligible firms.

I also find large indirect effects, with around two-thirds of subsidized firms' growth coming at the expense of their within-state competitors. I explore different mechanisms to uncover the sources of crowd-out, and find that international trade played an important role. For traded products, there were no negative competitive effects; the estimates are consistent with positive spillovers. This result is compatible with the argument that local demand shocks will have a limited effect on local production of traded goods (Matsuyama 1992; Magruder 2013). For non-traded products, the private gains from the subsidy programs were completely eliminated on aggregate.

While these findings suggest that estimates of direct effects are not sufficient for understanding the aggregate effects of subsidy programs, including the estimates of indirect effects is not sufficient either. The results are consistent with, for instance, Cobb-Douglas preferences, where the expenditure shares in each sector is constant, regardless of any within-sector shocks. As a result, I consider the program's effect on aggregate productivity through its within-sector reallocation of factors, in the spirit of Hsieh and Klenow (2009) and Petrin and Levinsohn (2012). Firms adjust their capital and labor after gaining access to subsidies, and the first-order conditions of the model allow me to use those adjustments to estimate the effective change in input prices for the newly eligible firms, and through the model, I calibrate the effect that those price changes have on aggregate productivity. Had the eligibility requirements been relaxed five years earlier, or if they never changed, misallocation would increase by around .1%, with around half of the gains effectively coming from the program's effect on the newly eligible firms' relative price of labor, and half from the effect on the relative prices of capital. I also show that, given the size and scope of the policy change, the maximum theoretically possible TFP gains were around .7%.

My paper builds on three different strands of literature in economics. First, my study of the direct effects of the program contributes new evidence to the development economics literature

focusing on firms. The papers most similar to mine study industrial policies[6] and firms' access to credit and capital.[7] I also add to the literature discussing small firms, and in particular the policy effects of programs which differentially favor small firms.[8] My work is in the spirit of Abbring and Heckman (2007), who develop a simple theoretical model to argue that finding that a program that has a large direct effect motivates testing its equilibrium effects. I focus on a particular equilibrium channel, within-product-market competition, and find large equilibrium effects, dramatically changing the policy interpretation relative to just looking at the direct effects.

Second, I contribute to the empirical literature studying peer effects (Manski 1993; Angrist 2014), in particular among firms.[9] The urban economics literature focuses on Marshallian channels, through which firms interact with each other through goods, workers, and ideas (Ellison et al. 2007; Hanlon and Miscio 2013). A related body of work studies how firms are affected by increased competition following trade shocks (De Loecker et al. 2012; Sivadasan 2006), FDI (Aitken and Harrison 1999; Keller and Yeaple 2003), and research and development (Jaffe 1986; Bloom et al. 2013). Of particular relevance to my work, Burke (2014) studies the equilibrium effects of credit programs which help farmers store their crops, Busso and Galiani (2014) study the effect of entry in retail markets on prices and quality, and Acemoglu et al. (2012) and Acemoglu et al. (2014) study how shocks to particular industries affect the economy as a whole through input-output networks. I show that the effect of competition due to a policy shock can be estimated using strategies similar to "linear-in-means" models often used to study peer effects in other settings, since the share of subsidized activity is the source of the indirect effect. I have collected detailed data allowing me to study product markets instead of the more commonly used industry codes,

[6] For instance, Sivadasan (2006); Bollard et al. (2013); Chamarbagwala and Sharma (2011) and McCaig and Pavnick (2014) study how firms respond to trade liberalization.

[7] Banerjee et al. (2014); Galindo et al. (2007); Crepon et al. (2014) and Angelucci et al. (2013) analyze access to credit in developing countries affects firm performance and de Mel et al. (2008) give capital to measure its returns.

[8] A large literature in the United States discusses the Birch (1979) hypothesis that small firms are the engine of economic growth (Neumark et al. 2011; Young et al. 2014; Haltiwanger et al. 2013; Krishnan et al. 2014), and a similar research agenda focuses on the developing world (Karlan et al. 2012; Martin et al. 2014).

[9] Peer effects have been studied in a variety of contexts, including deworming (Miguel and Kremer 2004), labor markets (Duflo 2004; Crépon et al. 2013), local economies (Rosenstein-Rodan 1943; Murphy et al. 1989; Crisculo et al. 2012; Kline and Moretti 2014; Autor et al. 2014), and idiosyncratic individual economic behavior (Townsend 1994; Deaton 1990; Buera et al. 2012; Angelucci and Giorgi 2009). While the empirical papers studied on different topics, their reduced-form empirical strategies tend to take a similar form to mine. Baird et al. 2014 and Sinclair et al. 2012 formalize the design of experiments to identify spillovers, and present more complete literature reviews.

and I show that this is essential for studying the aggregate effects of subsidies, since measurement error on the extent of competition would cause a regression to understate the magnitude of the indirect effects relative to the direct effects.

Third, my work complements a literature that has tried to understand the mechanisms through which different types of firm-level distortions can lead to aggregate losses.[10] I develop a methodology for calibrating the aggregate productivity gains from firm-specific shocks, such as a change in eligibility for subsidies. I formalize the intuition that subsidies for firms facing relatively larger distortions can increase aggregate TFP. These gains are complementary with each firm's productivity, as the aggregate TFP effects are magnified for subsidies to larger firms. I use the model both to calibrate the aggregate TFP gains from the program, which are around .1%, as well as relate these gains to those of counterfactual policy changes that targeted random subsets of firms. In the most years, the estimated aggregate effects of the true policy are larger than most of the counterfactual policies.

Programs which subsidize targeted firms can have significant effects on aggregate output and productivity, and this paper empirically studies the magnitude of these effects in the context of a large-scale policy change in India. In Section 2, I describe small firm subsidies in India. In Section 3, I develop a model for estimating the aggregate effects of a change in targeted subsidies, and in particular show how to decompose the aggregate effect of subsidies into direct and indirect components. Section 4 describes the data, the construction of the exposure measures, and the identification strategy. Section 5 estimates the direct effects of the policy change, and Section 6 expands the analysis to estimate the indirect effects. Section 7 discusses the effect of the program on aggregate productivity, and Section 8 concludes.

II Institutional Background

II.A Overview of small-scale firm policies in India

The Indian government has had a ministry dedicated to small-scale enterprises since 1954. Eligible firms were originally those with under 500,000 rupees in fixed assets. Over time, as can be seen in Figure 2, the real fixed asset cut-off has changed, although most of the policy changes until the late

[10] Banerjee and Duflo (2005); Hopenhayn (2014); Midrigan and Xu (2014); Asker et al. (2014); and Ziebarth (2013).

1990s were to keep pace with inflation. Banerjee and Duflo (2014) and Kapoor et al. (2012) study the policy change in 1999, when the office was renamed the Ministry of Small Scale Industries and Agro and Rural Industries and the eligibility criteria were tightened. In 2001, that Ministry was split into two distinct units, the Ministry of Small Scale Industries and the Ministry of Agro and Rural Industries. I start my empirical analysis in this year.

With the passage of the Micro, Small, and Medium Enterprises Development Act of 2006, the federal government's small firm programming was consolidated once again, into the Ministry of Micro, Small, and Medium Enterprises. The Act expanded the definition of who was eligible for small-firm programs, and introduced several new programs. Before the 2006 policy change, establishments with a value of under ten million rupees in nominal investment in plants & machinery were eligible;[11] the Act raised the size cutoff to fifty million rupees. Eligibility for the programs is exclusively determined by an establishment's nominal accumulated capital investment (ignoring depreciation), limiting establishments' ability to use accounting tricks in order to subvert the intent of the process.[12,13] Before the policy change, establishments who were above the cutoff in 2006 likely did not expect to gain eligibility without selling assets. At the time, newly eligible establishments represented around 15% of all formal manufacturing output, and, the majority of firms faced some competitors whose eligibility status changed in 2006.

The Reserve Bank of India (RBI) manages "Priority Sector Lending," which directs banks to provide 32% to 40% of their loan portfolio to clients designated eligible.[14] While the RBI maintained administrative control of the Priority Sector program, they tend to defer to the Ministry of Micro, Small, and Medium Enterprises for determining the eligibility criteria in manufacturing.[15] Public banks also have sub-targets: 45% of the priority sector credit must go to agriculture, and

[11] This cutoff is roughly $200,000 in 2013 dollars.

[12] Eligibility is at the *establishment* level, so a multi-plant firm could potentially have both eligible and ineligible plants.

[13] In other settings, industrial policies often base their eligibility criteria on information such as the industry or the firm's location, and these types of programs are likely to have equilibrium effects through other channels. Burgess and Pande (2005) and Chaurey (2013) discuss the effects of place-based programs in India, and Amirapu and Gechter (2014) discuss small favoring programs in India with employment-based criteria.

[14] The smaller number is for foreign-owned banks with fewer than 20 branches.

[15] The RBI has, however, changed the set of eligible borrowers outside of manufacturing, such as in 2007 limiting banks' ability to include loans to micro-finance institutions (see "Master Circular - Lending to Micro, Small & Medium Enterprises (MSME) Sector" dated July 1, 2013, and "Guidelines of Lending to Priority Sector – Revised" dated April 30, 2007).

25% to weaker sections. In general, the targets and sub-targets are binding (Nathan India 2013).[16]

In addition to directed credit, there are a variety of other programs designed to benefit small firms. Many states have developed preferential procurement policies for local governments, forcing them to make purchases from small firms. Furthermore, the Ministry of Micro, Small, and Medium Enterprises manages a large portfolio of its own activities - in 2012/2013 it had a budget of just under $130 million. Of that, 45% went to "Quality of Technology Support Institution & Programmes," which includes programs such as advising on new manufacturing techniques, granting access to material testing facilities, product design, and training programs, 15% went to "Promotional Services Institutions and Programmes," 13% to "Infrastructure Development & Capacity Building," and the rest to a variety of other programs including training, export subsidies, credit guarantees, and the development of an agency to keep credit scores for small firms.[17] Since the 2006 policy change changed the eligibility requirements for all of these programs, I do not attempt to estimate the impact of the specific components separately.

III Analytical Framework

In this section, I develop a partial-equilibrium model with heterogeneous firms (Hopenhayn 1992; Melitz 2003) who produce multiple products (Allanson and Montagna 2005; Bernard et al. 2010, 2011), with firm-specific distortions on the cost of capital and labor (Hsieh and Klenow 2009, which for this section I abbreviate as HK), but without entry or exit from product markets. The framework does not generate ex-ante comparative statics on the effects of subsidies (depending on parameter values, there may be either crowd-out or agglomeration); it is used to derive the sources of equilibrium effects for the empirical specifications.

III.A Direct and Indirect Effects Considering a Single Product

I first demonstrate the relationship between the direct and indirect effects within a single product. I derive the static equilibrium, then discuss the relationship between the growth rate of each firm

[16] Figure 2 shows how lending to the priority sector has evolved since 1999, respectively as a share of overall credit and in raw values. While is no clear large jump around 2006/2007 in lending, in the empirical analysis, I include fixed effects for each year, and therefore do not estimate the effects that relaxing the program requirements had on the economy as a whole.

[17] The MSME Annual Report 2006 provides a more complete description of the many activities undertaken by the agency.

and the growth of subsidies. I assume that in each sector, a single final good Q_s is produced by a representative firm in a perfectly competitive market. The utility function of the representative consumer over the S sectors is

$$U = \sum_{s=1}^{S} Q_s^{\phi} + c,$$

where c is consumption of the outside good, whose price is normalized to one, and the post-tax income of the consumer is assumed to be I (in partial equilibrium).[18] In Appendix A, I show that similar predictions to the ones in this subsection can be derived (i) in a Lucas span-of-control style model, with decreasing returns to scale and homogenous output in each sector, and (ii) when the consumer has CES preferences over the final goods. I choose this specific form so as to avoid any cross-industry (demand) spillovers in the predictions.[19] The first-order condition of the final-good consumer ensures that the revenue in sector s will be

$$Y_s = P_s Q_s = \left(\frac{P_s}{\phi}\right)^{\frac{\phi}{\phi-1}}. \tag{1}$$

In each sector, this firm combines the output q_{js} of each of the N intermediate goods producers with a CES production function:

$$Q_s = \left(\sum_{j=1}^{N} q_{js}^{\frac{\sigma-1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}}.$$

The final good producer profit-maximizing ensures that the price of the final good in each sector P_s will be the following function of the intermediate goods producers' prices:

$$P_s = \left(\sum_{j=1}^{N} p_{js}^{1-\sigma}\right)^{\frac{1}{1-\sigma}}. \tag{2}$$

[18] This assumption differs from HK, who assume a Cobb-Douglas utility function. I make this choice because when the representative consumer has Cobb-Douglas utility, total revenue for the final good producer in sector s sector not a function of that producer's price, an undesirable property for evaluating crowd-out. For a similar reason, I assume that I is large enough to guarantee an interior solution.

[19] Regardless, with year fixed effects in the regressions, I unable to separately identify spillovers which affect aggregate demand from other shocks to India as a whole.

Each intermediate good producer has a Cobb-Douglas production function of capital and labor,[20]

$$q_{js} = A_{js} K_{js}^{\alpha} L_{js}^{1-\alpha}, \tag{3}$$

where A_{js} is firm-specific TFP, and $\alpha \in (0,1)$ is the capital intensity. Following HK, I allow for distortions which change the marginal products of capital $\left(\tau_{k_j}\right)$ and labor $\left(\tau_{l_j}\right)$ for each firm, respectively the "capital wedge" and the "labor wedge."[21] A growing literature seeks to micro-found these distortions (Peters 2013; Buera et al. 2011); in this paper I focus on the change in distortions arising from subsidies for which only some firms are eligible.

As a result of the distortions, firm j's profits in sector s are given by

$$\pi_{js} = p_{js} q_{js} - \left(1 + \tau_{K_j}\right) R K_{js} - \left(1 + \tau_{L_j}\right) w L_{js},$$

where w and R reflect the prices of the factors of production.[22] I assume that firms take the price index as given, so profit maximization implies that

$$p_{js} = \frac{\sigma}{\sigma - 1} \left(\frac{R}{\alpha}\right)^{\alpha} \left(\frac{w}{1-\alpha}\right)^{1-\alpha} \frac{\left(1 + \tau_{K_j}\right)^{\alpha} \left(1 + \tau_{L_j}\right)^{1-\alpha}}{A_{js}}, \tag{4}$$

which, as is standard, is a constant markup over the firm's marginal cost. Revenue for each intermediate good producer will be a function of a) their own price, b) the prices of their competitors

[20] The distinction between the "final-goods" and "intermediate-goods" produces is made to the make the exposition clearer for each hierarchy of how firms interact with each other (within and across sectors). In the empirical analysis, all of the plants correspond to the "intermediate goods" producers of the model.

[21] I differ from HK, who discuss a "capital wedge," which differentially lowers the *relative* marginal product of capital and an "output wedge," which changes the marginal product of the two inputs by the same proportion. I do this since the changing the "output wedge" will mechanically affect revenue in the absence of behavioral effects. Note that a firm with equal τ_{k_j} and τ_{l_j} will behave identically to a different firm with output subsidies of the same magnitude, but no subsidies to its inputs.

When computing how misallocation matters for aggregate TFP, focusing the notation on labor instead of output wedges is without loss of generality, as HK discuss (in fact, as Vollrath (2014) notes, that the wedges notation is not needed for calculating the role micro-distortions have in causing misallocation).

[22] With homogenous inputs, endogenous factor price changes will affect each sector equally, and therefore in the empirical section would be absorbed by the fixed effects.

in the sector, and c) total revenue in the sector:

$$\begin{aligned} y_{js} =& p_{js} q_{js} \\ =& \left(p_{js}^{1-\sigma}\right) \cdot \left(P_s^{\sigma-1}\right) \cdot \left(\frac{P_s}{\phi}\right)^{\frac{\phi}{\phi-1}}. \end{aligned} \tag{5}$$

Firm size is determined by a mix of each firm's wedges and underlying productivity: increased productivity will increase firm size, as will lower "wedges." Holding P_s fixed, and combining equations 4 and 5, the growth of firm size with respect to productivity and the distortions is:

$$\begin{aligned} \frac{\partial \ln\left(y_{js}\right)}{\partial A_{js}} =& \sigma - 1, \\ \frac{\partial \ln\left(y_{js}\right)}{\partial \tau_{K_j}} =& \alpha\left(1-\sigma\right), \\ \frac{\partial \ln\left(y_{js}\right)}{\partial \tau_{L_j}} =& \left(1-\alpha\right)\left(1-\sigma\right). \end{aligned}$$

In the following subsection, I show how firm size changes as a function of all firms' subsidies, taking into account the equilibrium effects on price index.

III.B The Effect of Changing Subsidies

These properties derived in equation 5 allow for simple equations relating firm growth to increasing subsidies (which would imply a decline in τ_{K_j} and τ_{L_j}) in an economy. I assume throughout that expanding the set of eligible firms will change those firms' relative prices of inputs, but will have no effect on other firms.[23] From equations 4, 2, and 5 the growth rates of the final good

[23] Ex-ante it is not clear if expanding eligibility will increase other distortions (because there are more firms to be subsidized) or decrease them (since the share of eligible firms is converging towards 1). The subsequent sections provide three empirical justifications for focusing on the equilibrium effects in competitive markets caused by changes to the newly eligible firms. First, in all regressions I include fixed effects for each year, which absorbs common changes in the distortions due to the program change (such as a changes in the interest rate for borrowing and saving). Second, in Table 8, I find evidence that newly eligible firms behave as if their relative input prices changed, but no evidence that their competitors do as well. Third, I find similar heterogeneous treatment effects when comparing regressions on aggregate outcomes to those using firm-level data.

producer's price, and the revenue and price of the intermediate good producer satisfy:[24]

$$\begin{aligned}
\hat{y}_{js} &= (1-\sigma)\,\hat{p}_{js} + \left(\sigma - \frac{1}{1-\phi}\right)\hat{P}_s, \\
\hat{P}_s &= \sum_{j=1}^{N}\left[\hat{p}_{sj}\frac{y_{js}}{Y_s}\right], \\
\hat{p}_{js} &= \alpha\left(\widehat{1+\tau_{K_j}}\right) + (1-\alpha)\left(\widehat{1+\tau_{L_j}}\right).
\end{aligned}$$

The change in each firm's revenue as a function of the changing wedges is therefore:

$$\begin{aligned}
\hat{y}_{js} = &(1-\sigma)\left(\alpha\left(\widehat{1+\tau_{K_j}}\right) + (1-\alpha)\left(\widehat{1+\tau_{L_j}}\right)\right) \\
&+ \left(\sigma - \frac{1}{1-\phi}\right)\sum_{j=1}^{N_s}\left[\left(\alpha\left(\widehat{1+\tau_{K_j}}\right) + (1-\alpha)\left(\widehat{1+\tau_{L_j}}\right)\right)\frac{y_{js}}{Y_s}\right]. \qquad (6)
\end{aligned}$$

The first line reflects the direct effect of the program. As inputs are relatively more subsidized (lowering the wedges), revenue will increase. Each firm's growth as a function of growth in subsidies is independent of that firm's pre-existing productivity or "wedges."

The second line reflects the indirect effect of the program, which captures how each firm's change in price changes the overall price index. While firms are not indifferent to their competitors getting access to subsidies programs, conditional on the share of the competition with access, firms are indifferent as to who gets access, similar to what Hudgens and Halloran (2008) define as *stratified interference*.[25] Conditional on size, competitors' productivity or pre-existing wedges do not predict the size of the indirect effect.

Aggregating over all of the firms in each sector gives

$$\hat{Y}_s = \left(1 - \frac{\sigma - \frac{1}{1-\phi}}{\sigma - 1}\right)(\sigma-1)\sum_{j=1}^{N_s}\left[-\left(\alpha\left(\widehat{1+\tau_{K_j}}\right) + (1-\alpha)\left(\widehat{1+\tau_{L_j}}\right)\right)\frac{y_{js}}{Y_s}\right]. \qquad (7)$$

The total change in revenue in a sector will be weighted average sum of the direct and indirect

[24] The notation $\hat{x} = \frac{\dot{x}}{x}$ represents the log-linearization of the growth of x over time.
[25] This result is similar in spirit to Hirth (1999), Kosova (2010) and Kovak (2013).

effects.[26]

To simplify the notation, I define $e_j = 1$ if firm j gained access to subsidies as a result of the policy change and $\mathbf{e}$ as the vector of all of the e_j's. Furthermore, I define

$$\beta \equiv (\sigma - 1),$$

$$\theta \equiv \left(\frac{\sigma - \frac{1}{1-\phi}}{\sigma - 1} \right),$$

and

$$\mu_s \equiv \sum_{j=1}^{N_s} \left[- \left(\alpha \left(\widehat{1 + \tau_{K_j}} \right) + (1 - \alpha) \left(\widehat{1 + \tau_{L_j}} \right) \right) \frac{y_{js}}{Y_s} \right] = \frac{\sum_{j=1}^{N_s} e_j \times y_{js}}{Y_s}.$$

β reflects the private growth from the program, θ the extent of crowd-out from that growth,[27] and μ_s the share of output in sector s produced in newly subsidized firms. As a result we can condense equation 7 to:

$$\hat{Y}_s = \beta\mu_s - \theta\beta\mu_s = (1 - \theta)\beta\mu_s. \tag{8}$$

Aggregate growth in a sector is the sum of the aggregate direct effect, the private growth from the program times the share of newly eligible firms, minus the aggregate indirect effect, which is the aggregate direct effect times the crowd-out parameter θ. Defining private growth in a sector due to the program change as $\hat{Y}_{ps} = \beta\mu_s$, the elasticity of aggregate growth with respect to private growth is $\frac{\hat{Y}_s}{\hat{Y}_{ps}} = (1 - \theta)$. Estimating this elasticity is one of the primary empirical goals of this paper.

As the across and within sector elasticities of substitution (respectively captured by ϕ and σ) change, so too will the indirect effect. As $\phi \to 0$ or $\sigma \to 1$, the indirect effect approaches 1, which implies complete crowd-out.[28] As ϕ increases, the indirect effect shrinks, such that there is no

[26] Equation 7 is identical to what would be found by calculating the growth rates directly from equation 1. However, that would not allow for a decomposition of the direct and indirect effects.

[27] The θ notation is used by Spence (1984) to denote knowledge spillovers.

[28] The extreme of $\phi_s = 0$ implies that the consumer will never buy anything from the sector, and the derivations assumed that $\phi_s > 0$.

indirect effect if $\phi = \frac{\sigma-1}{\sigma}$, and positive spillovers if ϕ is larger. Furthermore, as σ_s increases (the good becomes more substitutable), the direct effect increases.

III.C Multi-Product Firms

The previous subsection considered each sector separately, but in the data most plants produce multiple products. In this subsection, I adapt equation 6 to account for firms who are affected through multiple products. I assume that the production function in equation 3 is true for firm j in *each* sector s in which it produces (Bernard et al. 2010; Valmari 2014). Defining $\omega_{js} = \frac{y_{js}}{y_j}$ as the share of firm j's revenue in sector s, a multi-product firm's growth due the subsidy program is

$$\hat{y}_j = \beta e_j - \theta\beta \left(\sum_{s=1}^{S} \omega_{js} \cdot \mu_s \right). \tag{9}$$

As in equation 6, each firm's growth can be is linearly decomposed into direct effect - β if the firm is newly eligible - and indirect effects, where the indirect effect on each firm is now a weighted average of their exposure to the program through all sectors, where the weights are determined by each firm's product mix.

The primary structure of the empirical analysis of section 6 will be to estimate the effect on firm revenue on (i) if their eligibility status changed and (ii) the weighted-average share of their competitors who gained eligibility.

In the data, $\left(\sum_{s=1}^{S} \omega_{js} \cdot \mu_s\right)$ can be calculated using a natural analogue to the input-output tables used to calculate inter-industry spillovers. Consider the $N \times N$ *output-output* matrix (denoted $\mathbf{A}$), where element

$$a_{jk} = \sum_{s=1}^{S} \left(\omega_{js} \frac{y_{ks}}{y_s} \right) \tag{10}$$

corresponds to the weighted average share of firm $j's$ products produced by firm k.[29] The vector of the growth rate for each firm coming from equation 9 as

$$\xi = \beta \left(\mathbf{I} - \theta \mathbf{A} \right)' \mathbf{e}, \tag{11}$$

[29] The more standard input-output measure captures how industries are related to each other through vertical relationships. The output-output matrix captures how firms are related to each other through horizontal relationships.

where $\mathbf{I}$ is the identity matrix.[30] $\beta\mathbf{Ie}$ is the vector of direct effects, and $\theta\mathbf{A}'\mathbf{e}$ is the vector of indirect effects.

III.C.1 Trade and Observed Heterogeneous Product Characteristics

The transnational crowd-out parameter θ may vary for different types of sectors. In particular, for more traded products the estimated θ may be smaller, since the true y_s is not just made up of output in India but worldwide output.[31] If producers have the ability to sell the good abroad if they desire, then the domestic price for exportable goods will be less responsive to increased domestic production. To account for this, define $x_s = 1$ if production in sector s is exported, θ^d as the competitive effect in sectors where products are sold domestically, and θ^x as difference in the competitive effect in the more-traded versus less-traded sectors. As a result, I can extend equation 9 to

$$\hat{y}_j = \beta e_j - \theta^d \beta \left(\sum_{s=1}^{S} \omega_{js} \cdot \mu_s \right) + \theta^x \beta \left(\sum_{s=1}^{S} \omega_{js} \cdot \mu_s \cdot x_s \right). \tag{12}$$

Define the $N \times N$ *traded-output-output* matrix (denoted $\overset{\mathbf{x}}{\mathbf{A}}$), where element

$$\overset{x}{a}_{jk} = \sum_{s=1}^{S} x_s \cdot \left(\omega_{js} \frac{y_k}{y_s} \right).$$

The vector of growth rates coming from equation 12 can be extended to include separately the effect of more and less internationally traded products:

$$\xi^x = \beta \left(\mathbf{I} - \theta^d \mathbf{A} - \theta^x \overset{\mathbf{x}}{\mathbf{A}} \right)' \mathbf{e}. \tag{13}$$

A similar logic applies when there are many relevant sector characteristics. For instance, suppose some products have different elasticity of demand, where $\theta^{d,\,h}$ is the competitive effect in non-traded and high elasticity sectors , $\theta^{x,\,l}$ is the competitive effect in traded and low elasticity

[30] This notation is similar to Acemoglu et al. (2014), who use input-output tables to study how shocks to one industry affect the whole economy.

[31] If firms compete on a product which is sold on international markets, then a large (by Indian market standards) policy shock my be a small one (by world market standards), and therefore there will be limited competitive effects on Indian firms. From the perspective of the firms, this corresponds to a a high elasticity of substitution ϕ, since decreases in the price of the sectoral good lead to corresponding increases in demand.

sector, and so on. Furthermore, assume that there is no effect of the elasticity of demand for traded sectors: $\theta^{x,l} = \theta^{x,h} = \theta^{x}$.

Defining $l_s = 1$ if sector s has a low elasticity of substitution, I can extend equation 12 to

$$\hat{y}_j = \beta e_j - \theta^{d,h}\beta\left(\sum_{s=1}^{S}\omega_{js}\cdot\mu_s\right) + \theta^{x}\beta\left(\sum_{s=1}^{S}\omega_{js}\cdot\mu_s\cdot x_s\right) + \theta^{d,l}\beta\left(\sum_{s=1}^{S}\omega_{js}\cdot\mu_s\cdot l_s\right). \tag{14}$$

Defining the $N \times N$ *low-elasticity-output-output* matrix (denoted $\overset{\mathbf{l}}{\mathbf{A}}$), where element

$$\overset{l}{a}_{jk} = \sum_{s=1}^{S} l_s \cdot \left(\omega_{js}\frac{y_k}{y_s}\right),$$

we can extend the vector of growth rates coming from equation 14 as

$$\xi^{x,l} = \beta\left(\mathbf{I} - \theta^{d,h}\mathbf{A} - \theta^{x}\overset{\mathbf{x}}{\mathbf{A}} - \theta^{d,l}\overset{\mathbf{l}}{\mathbf{A}}\right)'\mathbf{e}. \tag{15}$$

III.C.2 Location of Sales and Unobserved Heterogeneous Product Characteristics

In many empirical settings, separate geographic regions are treated as separate markets (such as when trying to estimate the effect of trade shocks). In particular, many researchers have argued that the states of India have relatively unintegrated markets (Topalova 2010; Hasan et al. 2012; Kothari 2013). A difficulty with testing this assumption is that firms rarely report the location of their sales.

A potential test of the separability of state markets is if the indirect effects of subsidies vary across state lines. That is to say, if a firm's growth crowds-out its within-state competitors, but is irrelevant for the producers located outside the state, that would suggest that states are different markets. Even without information on the location of firms' sales, it is possible to identify the within-state and the outside-state indirect effects of subsidy programs. Define ς_{jk} as an indicator for if firm j and k are in the same state, θ^{ς} as the competitive effect for within state competition, and θ^{o} as the competitive effect for out-of-state competition. Furthermore, define $\mu^{\varsigma}_{js} \equiv \frac{\sum_{j=1}^{N_s}\varsigma_{jk}\times e_j\times y_{js}}{\sum_{k=1}^{N_s}\varsigma_{jk}\times y_{ks}}$ as the share of within-state competition in sector s for firm j, and $\mu^{o}_{js} \equiv \frac{\sum_{j=1}^{N_s}\left(1-\varsigma_{jk}\right)\times e_j\times y_{js}}{\sum_{k=1}^{N_s}\left(1-\varsigma_{jk}\right)\times y_{ks}}$ as the

non-state share. Including the geography of sales adjusts equation 9 to:

$$\hat{y}_j = \beta e_j - \theta^{\varsigma}\beta \left(\sum_{s=1}^{S} \omega_{js} \cdot \mu_{js}^{\varsigma} \right) - \theta^{o}\beta \left(\sum_{s=1}^{S} \omega_{js} \cdot \mu_{js}^{o} \right). \tag{16}$$

This is similar to equation 12, with one crucial difference: instead of calculating how the indirect effect differs for traded and non-traded products, I instead must calculate the effects separately for within and outside state sales. The difference between θ^{ς} and θ^{o} informs how affected firms are by within state and outside state competition.

In the subsequent sections, I use the theory to estimate the aggregate effects of the 2006 policy change expanding access to subsidies for small firms. In Section 4, I discuss the data, and how I use it to generate the measures of indirect exposure that are crucial for estimating crowd-out and the aggregate gains from targeted subsidies. In Sections 5 and 6, I study the direct and indirect effects of the program. In Section 7, I consider how the program's reallocation of economic activity affected aggregate productivity.

IV Data and Identification Strategy

The empirical analysis mostly relies on the Annual Survey of Industries of India (ASI), which is produced by the Ministry of Planning and Statistics (MOPSI). The ASI is a repeated cross section representative of formal establishments, (stratified at the state by 4 digit industry level). The cross section is designed as follows: large establishments, which are those with 200 or more workers until 2003-2004, and 100 or more since then, are surveyed each year (with about 10% non-reporting each year). Smaller establishments are surveyed with a probability which depends on their specific state and industry block, with a minimum sampling probability of 15%.[32] MOPSI has recently allowed researchers to track establishments who were sampled multiple times, in what is known as the "Panel" version of the ASI.[33] I have collected the surveys taken in 2001-2011.

The ASI asks establishments not only the net value of owned fixed assets, but also the historical value, broken down into several categories. As a result, I observe each establishment's eligibility

[32] The smaller establishments are surveyed on a rotating basis with additional surveys undertaken randomly to increase precision.

[33] Researchers have started to take advantage of this change, for examples see Allcott et al. 2014 and Martin et al. 2014.

for small-firm subsidies in each year. The ASI does not ask firms if they specifically take advantage of any small-firm specific programming, so I am unable to present any results showing what percent of eligible firms actually take advantage of those programs. Furthermore, while the ASI contains very little information about each establishment's parent firm, most establishments are the only plant in their firm. For most of the analysis, I treat each establishment as a separate firm, but the direct effect is similar when I constrain the sample to only single-plant firms.[34]

I augment the ASI with the 2006 round of the National Sample Survey Organization's information on unorganized manufacturing establishments (NSS), which are explicitly the non-ASI firms in India.[35] It is designed to be a representative cross-section of those firms, and therefore combing the two datasets allows for a representative sample of all manufacturing activity in India.[36] Unlike the ASI, the NSS is only undertaken every 5 years, and establishments cannot be tracked over time. As a result, I use the information for understanding exposure to the policy change, but not for understanding its effects. While informal firms represent an enormous share of manufacturing establishments in India (around 99%), their shares of employment (80%) and revenue (16%) are lower (Ghani et al. 2014a).

Firms in the ASI and NSS report not only total sales, but also sales broken down by product. As a result, with the provided sampling weights it is possible to calculate the total revenue for each product, as well as the revenue from newly eligible firms, which will be essential for constructing each firm's exposure through product markets. Since a primary goal of the paper is estimating the effect of small firm subsidies on output, firm sales is the primary outcome of interest. The other

[34] Eligibility for all of the "small" firm programs in India are at the establishment level, although interviews suggest that there has been some confusion on this point.
[35] The dataset is the NSS round 62, schedule 2.2.
[36] Several other projects have combined the datasets, such as Nataraj (2011); Chatterjee and Kanbur (2013); Kothari (2013); Ghani et al. (2014b) and García-Santana and Pijoan-Mas (2014).

outcomes I use are total liabilities, total costs,[37] and if a plant continues to exist.[38,39,40] Firms also report quantities and prices whenever possible.[41]

IV.A Constructing Measures of Exposure

To analyze the policy change, I classify a firm's value of assets in the last year it appears in the ASI before the policy change. This gives an equivalent to an "intent-to-treat" estimate of the effect of small firm subsidies, and avoids issues with firms' changed behavior as a result of policy change itself (such as growing because of eligibility, or deliberately shrinking in order to gain access). Firms below the original cutoff of 10 million rupees in that year are considered always eligible for the small-scale government programs, regardless of their actual past or future sizes. Firms with over 50 million rupees in assets that year are likewise considered never eligible. The rest of the firms are considered eligible starting in 2007, and ineligible beforehand. In order to have a consistent description, I define "micro" firms as those who who were always eligible, "small" firms as those who were newly eligible, and "large" firms as those who were always ineligible. A firm's category is fairly stable over time: for firms who appear in the sample twice before the policy change, 93% are in the same category in the second-to-most recent year as in the most recent one. For firms who appear in the sample three times, 90% have the same classification in the third-to-most recent year as in the most recent one.

[37] I follow Nishida et al. (2013) and calculate the flow costs of capital as .15*fixed assets. I then impute primary input costs as the (flow costs of capital) + (rented capital costs) + (total wages), and total costs as (primary input costs) + (cost of materials).

[38] In principle, firms remain in the sample even if they close, and enumerators manually note the closure. The sampling set is not updated very quickly, and so (closed) firms continue to be asked for responses, to the point where there exists a specific code for the enumerators to signify that the plant has already been denoted as closed in a previous survey. However, the firm status variable is somewhat inaccurate, as some firms who are marked as having exited report positive assets, sales, and employment both for the year that they "exited" and in subsequent years. Following Martin et al. 2014, I only denote a firm as having exited if a) its enumerator-reported "unit-status" is consistent with having exited, b) it reports no revenues, material input costs, labor, or months in operation, and c) it never again reports revenues, material input costs, labor, or months in operation.

[39] To avoid measurement error coming from reporting error (Bollard et al. (2013); Hsieh and Klenow (2009)), in each year outcomes are trimmed at the 99th percentile, although the results are not especially sensitive to this. Allcott et al. (2014) and Martin et al. (2014) also undergo exercises to remove plants who report probably incorrect values (such as those who report increasing sales by three log points in one year and then shrinking back the subsequent year). Applying either of their strategies, or both, also does not substantively change the results. I am happy to share these tables by request.

[40] Occasionally - and particularly in 2011 - existing firms did not fully complete the survey. I drop these firms from the regressions when the specified outcome is missing.

[41] Most three-digit product aggregations contain designations for goods which are not elsewhere classified, and those are the ones that do not have corresponding units.

For similar types of questions, researchers tend to use industry codes as a proxy for competition. Likely this is due to data issues, since product-level data is often unreported. For estimating the aggregate effects of firm-specific shocks, using industry codes is likely to lead to biased results, since they are not supersets of product codes. For instance, in 2006, each 5-digit product code[42] was produced in a median of three 5-digit industries, and over 95% of output was of products produced in multiple industries. Figure 3 Column 1 shows the distribution of the number of industries producing each product. Using more coarse industry codes does not alleviate the problem, as the median product is produced in two 3-digit industries. Figure 3 Column 2 plots the total share of revenue from products produced in different number of 3-digit industries. Only 25% of revenue is from products produced in a single-3-digit industry products. Scrap iron, for instance, is sold by firms in 160 5-digit industries, and 32 3-digit industries.[43] In order to avoid these concerns, I construct the exposure measures at the product level. In Appendix B, I derive the sources of bias when using industry codes instead of products to estimate the effects of competition, and show empirically that it will lead to understating the magnitude of the indirect effects.

Crucially, the data is informative about the exposure shares from the perspective of the product. If instead the data was of firms with no sense of the population weights, then it would be difficult to generate unbiased estimates of the spillovers measures. When constructing the exposure measures detailed in equation 11 and equation 38, I must account for the fact that I do not observe every plant in India in every year. However, in 2006 I do observe a representative cross-section of all establishments. As a result, I can approximate the true exposure measures using the sampling weights. There are around 100,000 establishments for whom I observe their investment in plants and investment, with just over a third of them from the Annual Survey of Industries, and the rest informal firms.[44] In estimating the share of newly eligible firms for each sector, I use these

[42] Products are reported in ASICC codes, of which there are around 5000.

[43] One reason for this could be that industry codes are self-reported; there exist pairs of firms who produce the exact same products but who nevertheless report being in different industries. If this were the only problem, then given access to firm's products researchers could construct "new-industry" classifications with the desirable property that if a given firm is in a given "new industry," all of the firms who produce the same products as that firm are also in that "new-industry." I created the largest possible such industry classification for India in 2006, and generated 256 "new-industries." However, over 99% of revenue was concentrated in just one of them. Note that if there only existed single-product firms this would not be an issue, but in India the median establishment produces multiple products, which often would be intuitively considered to belong to different industries.

[44] In the empirical section, including information on the informal firms does not substantially change the regression

establishments. There are roughly another 50,000 establishments in the ASI who were not sampled in 2006, but were observed before the policy change. As a result, the output-output matrix A (and the equivalent matrices for different product characteristics) includes a row and a column for each firm, and only the first 100,000 columns contain non-zero values. For those columns, I augment equation 10 with inverse probability weights $\frac{1}{p_j}$ so that

$$\tilde{a}_{jk} = \sum_{s=1}^{S} \left(\frac{\omega_{js} \cdot y_{ks} \cdot \frac{1}{p_k}}{\sum_{l=1}^{N} \frac{1}{p_l} \cdot y_{ls}} \right).$$

From there, it is straightforward to calculate $\tilde{\mathbf{A}}'\mathbf{e}$ as the data approximation to $\mathbf{A}'\mathbf{e}$. A similar strategy is used to impute the other exposure measures.[45,46] Figure 4 Panel A is a scatter plot of each firm's exposure to the policy through within-state output competition and not-within-state output competition.[47] The correlation of the two measures is .13, suggesting that firms who make the same types of products as newly eligible firms do not have some peculiar trait, since the empirical extent of that sameness depends on the geography considered.

In this paper, I focus on product-market competition. However, there are many measures of exposure to the program which may affect firms, such as within-local market competition for primary inputs. In Appendix C, I discuss this briefly, as well as various empirical issues which make geographic indirect effects of subsidies difficult to identify in this context. In Figure 4 Panel B, I

results. This is likely because a) informal firms are generally a relatively small share of output, and b) fewer than one third of the products produced by formal firms are also produced by informal firms, suggesting that including informal firms they will not affect the spillovers measures for most firms. This fact also somewhat alleviates concerns about if there is incomplete coverage of the Indian economy from combining the two datasets.

[45] A concern with this strategy may be that it is an inappropriate use of the probability weights, since the survey was only designed to be representative at a more aggregate level. As an alternate strategy, I also create an exposure measure where, instead of using the weights to estimate exposure in 2006, I combine the samples from every year, and for each firm keep its most recent pre-program 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 and a rotating sampling frame, each existing firm should have been surveyed at least once in the period). I then ignore the sampling weights and calculate directly the exposure shares in this constructed census. The alternate exposure measure has a correlation over .7 with the value I use in the paper. Furthermore, in the appendix I show that regressions using the alternate measure leads to similar conclusions, in spite of the fact that this exercise naively abstracts from firm growth.

[46] I only include firms who report assets. Furthermore, I cannot calculate this measure for the firms who do not report sales-by-product in this calculation, and so those firms are dropped in the regressions including exposure measures (even if those firms did report overall sales).

[47] There is a mechanical correlation between the within-state and all-India output competition measures, since a firm in the same state is also in the same country. Figure 4 avoids this problem by comparing the within-state output competition measure to the outside state measure.

plot the relationship between exposure through in-state product markets and in-district exposure through (imputed) primary inputs. The correlation of the two measures is .1, suggesting that it may not be the case that firms who make the same products as small firms are also in the same districts.

IV.B Product Characteristics

As discussed in the previous section, the competitive effects of exposure to the program may vary by product characteristic. The primary characteristic I focus on is trade, since products which are exported may not impose as much pressure on the domestic price index. To test this, I construct a measure of how "traded" each product is. For each product I calculate the share of exports in the year before the policy change (from the Department of Commerce)[48] over total domestic production of that product (estimated by combining the NSS and the ASI).[49,50,51]

I also generate measures of capital intensity (the capital/labor ratio), and loan intensity (liabilities divided by flow costs of primary inputs) which I use as a proxy for external finance dependance (Rajan and Zingales 1998; Gupta and Yuan 2009; Levchenko et al. 2009). These measures are generated at the firm level, and then I calculate, for each product, the weighted average values over all of its producers to generate product level information on the expected characteristic of a producer. Finally, I use measures of the elasticity of substitution across products from Broda and Weinstein (2006). For each characteristic, I split the products by their median value, so for instance less-traded (or "non-traded") products are the ones with below-median export shares.

IV.C Identification Strategy

The first part of the estimation strategy is to estimate relative effects, using a difference-in-differences approach. The only firms for whom I have panel information are the formally registered ones, and the regressions are restricted to the firms in the ASI. Defining $\tilde{priority}_{it}$ for firm i taking advantage

[48] http://commerce.nic.in/eidb/default.asp. Accessed 07/07/14

[49] I constructed a concordance from the HS 6-digit codes reported on the site to the ASICC product codes used by the ASI, and am happy to provide this concordance table upon request. Creating the concordance was only possible without hand-coding because in 2011 the ASI switched to a new product coding scheme and (effectively) provided a concordance from the ASICC to the Central Product Classification (CPC), which can be concorded to HS codes using tables provided by the UN.

[50] Kothari (2013) and Mian and Sufi (2014) generate similar measures at the industry level.

[51] As a robustness check, I have also calculated exports+imports over total production.

of small-firm subsidies in year t, I follow equation 9 (for now, ignoring the indirect effect):

$$\ln\left(y_{jt}\right) = \beta \tilde{priority}_{jt} + \sum \gamma_{it} X_i + \eta_j + \eta_t + \epsilon_{jt},$$

where the X_i are time-invariant (as determined before the policy change) characteristics of the firm. However, $\tilde{priority}_{it}$ is not observed,[52] and firms who are eligible but do get subsidies may be different than those who do. As a result, I instead estimate

$$\ln\left(y_{jt}\right) = \beta Post_t \times Small_j + \sum \gamma_{it} X_i + \eta_j + \eta_t + \epsilon_{jt} \tag{17}$$

where $Small_i$ is determined by the plant's last observed size before the policy change, essentially serving as an intent-to-treat estimate. Using a change in the program's eligibility requirements allows for plausibly more exogenous measures of the direct effect, since the size of each firm had not yet responded to the policy change.

Post is a dummy indicating a survey taken after the policy change. In each specification I control for a cubic polynomial in the running variable, the firm's historical value of capital immediately before the policy change. For each outcome for the direct effects I run four regressions: one with all establishments and controls for assets, one with additional fixed effects for $state \times Post\ Reform$ and $3-digit-industry \times Post\ Reform$, these same two specifications, but restricting the analysis to single-plant firms. I always include firm and year fixed effects, and observations are weighted by the inverse of their sampling probability, provided by the ASI.[53] Standard errors are clustered at the firm level to adjust for heteroskedasticity and within-firm correlation over time.

A firm's competitors gaining access to the program may also have effects on growth. To understand how exposure to the program through competitive channels matter,[54] I leverage the fact that

[52] The National Small Industries Corporation Ltd. maintains a registry of small firms, but unregistered firms looking to take advantage of a program may prove their eligibility on a case-by-case basis, and many take advantage of this opportunity.

[53] A firm's sampling probability is not constant over time. For instance, a firm who grows from 90 employees in 2007 to 110 in 2008 would go from being sampled roughly every 3 years to being sampled every year. I use the endogenous sampling weights in the regressions in order to achieve consistent estimates (Solon et al. 2014; Wooldridge 1999)

[54] In ongoing work, study up and downstream effects.

the share of production by newly eligible firms varies dramatically at the product level, as shown in Figure 4. For each exposure measure, I calculate the weighted average share of a firm's sectors which are newly eligible. For instance, for the indirect effects over all products, the exposure calculation is $\sum_s \omega_{js} \cdot \mu_s$, as outlined in Section 3.3. To estimate the indirect effect for internationally traded products, I also include the exposure measure $\sum_s \omega_{js} \cdot \mu_s \cdot l_s$. To estimate the corresponding θs, I augment equation 9:

$$\ln\left(y_{jt}\right) = \beta Post_t \times Small_j + \sum_k \Theta^k Post_t \times Exposure_j^k + \sum \gamma_{jt} X_j + \eta_j + \eta_t + \epsilon_{jt}. \tag{18}$$

For instance, if just considering the magnitudes of crowd-out for all types of competition jointly, the regression would be $\ln\left(y_{jt}\right) = \beta Post_t \times Small_j + \Theta Post_t \times \left(\omega_{js} \cdot \mu_s\right) + \sum \gamma_{jt} X_j + \eta_j + \eta_t + \epsilon_{jt}$.

Θ estimates the effect that exposure has on firms' growth; in other words it is an estimate for $-\theta\beta$. As a result, the test of complete crowd-out ($\theta = 1$) is if $\Theta = -\beta$. When including multiple sector characteristics, then the test of complete crowd out is if the sum of the the relevant Θ^ks equals $-\beta$. As with the direct effects, I proxy for each firm's exposure to the policy change using their product mix before the policy change. This avoids issues with firms changing their product mix in response to the program.

Finally, Table 2 shows that firm exit is correlated with eligibility, as firms who gain access to subsidies are more likely to continue production than their peers. Since $\ln(0)$ is undefined, I use two approaches: adding one to the outcome before taking the log,[55] or using an use an inverse hyperbolic sine transformation of the outcome.[56] In the paper, I report results from the former (and for notational convenience I omit the $+1$), but the results are almost identical if I use the latter approach.[57]

[55] The implicit model is that had the firm stayed open, it would have had one (real) rupee of each outcome.
[56] See Woolley 2014; Burbidge et al. 1988 and Carroll et al. 2003 for further information about this approach.
[57] The correlation of $ln(sales + 1)$ and the inverse hyperbolic sine of sales is over .99

V Estimating the Direct Effect of Eligibility

In this section, I begin by demonstrating that firms who gained eligibility expanded relative to the other formal firms in the economy. Variation in eligibility comes from the historical value of capital at each firm before the policy change. One concern with this strategy would be if firms of different qualities manipulated their sizes, so that part of the effect of policy change would come from from the less-distorted behavior of particular firms, instead of the policy change itself (Lee and Lemieux 2009; McCrary 2008). In order to test for this, Figure 5 shows the distribution of log plants and machinery, the criteria determining firm eligibility, immediately before the policy change, as well as the old and new boundaries for eligibility in 2006 around the cutoff. Any discontinuity at the old firm size cut-off is reasonably small - it is a gap of similar magnitude to other jumps at other, policy-irrelevant, sizes - and there is no evidence that firms anticipated the new policy change and and bunched around the future cutoff in an anticipatory fashion. Appendix Table 1, Panel A, tests for bunching around the cut-off formally, following McCrary (2008). There is no significant break in the firm-size distribution at the old or new size-cutoff, neither before nor after the policy change. Panel B reports the results of regression discontinuity estimates of sales, liabilities, and employment costs, again around the two cut-offs, before and after the program change.[58] None of the 12 estimates are statistically significant. Table 1 shows summary statistics for the main outcome and explanatory variables in the paper for each year in its most recent pre-program observation. The newly eligible firms are, by definition, larger than the always eligible establishments, and smaller than the never eligible ones. They are most exposed to the program through product markets, which is consistent with the fact that firms are exposed to themselves. The always and never eligible plants share a similar exposure to the policy change of about 20%.

V.A Plots of Program Effects

To start, I estimate an event study regression predicting the firm's sales,

$$\ln\left(y_{jt}\right) = \sum_{t=2002}^{2011} \beta_t Small + \sum \gamma_{jt} X_i + \eta_j + \eta_t + \epsilon_{jt}, \tag{19}$$

[58] I use the default option - local linear regressions - of the "rdrobust" package, described in Calonico et al. (2014).

with controls for year trends times a cubic polynomial of the firm's assets. I plot the β_ts in Figure 6 to show the growth trends of the small firms relative to the rest. There do not appear to be significantly positive pre-trends of the newly-eligible firms relative to their peers. Furthermore, the program had a fairly small relative effect on firm outcomes in 2007, which is not unexpected, since the policy change was enacted in the final quarter of 2006 and the survey only covered through the first quarter of 2007. There is a jump in 2008 which persists through 2011. Not only do the newly eligible firms benefit from the policy change relative to their peers, but the gains are persistent.[59]

V.B Effects of the Program on Firm-level Economic Outcomes

Table 2 estimates equation 17 for sales, continued production, liabilities, and (imputed) costs:

$$\ln(y_{it}) = \beta Post \times Small_i + \sum \gamma_{it} X_i + \eta_i + \eta_t + \epsilon_{it}.$$

Panel A of Table 2 shows that gaining eligibility predicts an increase in establishment size of 25-35%, and is significantly different from zero. Each specification controls for a cubic polynomial for the historical value of capital in 2006. Columns 3 and 4 look only at single plant firms, and the even columns include fixed effects for post×state and post×3-digit industry, to guard against omitted correlations between location and production driving the results. The results are consistent across the four specifications. Some of the effect on increased sales is driven by the extensive margin, shown in Panel B of Table 2. Newly eligible firms are 3-4% more likely to exist in a given year when they are surveyed. As with sales, this result is consistent across the four specifications.

If the primary effect of the subsidy program were the government buying a small quantity of goods at inflated prices from eligible firms, then revenues of those firms may increase without corresponding increases in costs, since the effect of the program would be infra-marginal. To test this, Panels C and D look at the relative effect of the program on the input choices of newly eligible firms. Banerjee and Duflo (2014) argue that the most effective small-firm favoring program in India is the Priority Lending Sector (run by the Reserve Bank of India), and that increased

[59] The "shock" in this instance is not a one-time occurrence, but potentially continued eligibility. The fact that the results persist over time is consistent with this.

borrowing allows firms to expand. Panel C of Table 2 is consistent with their result: newly eligible firms expand their borrowing, relative to their peers, by around 30%. Panels D suggest that the increase in borrowing is not due to an infra-marginal adjustment in funding sources, as firms adjust their inputs in response to the program, by a similar magnitude (the point estimates are insignificantly larger than those for sales) to their adjustment in sales and liabilities.

VI Estimating the Indirect Effect of Eligibility Through Competition in Output Markets

VI.A The Effects of Output Competition, Treating All Products Similarly

Following equation 18, I run a firm-level regression of the following form:

$$\ln\left(y_{jt}\right) = \beta Post_t \times Small_j + \sum_k \Theta^k Post_t \times Exposure_j^k + \sum \gamma_{jt} X_j + \eta_j + \eta_t + \epsilon_{jt},$$

adding the weighted-average competitive exposure measures generated in Section 3 to the difference-in-differences regressions of the previous section. In this subsection, I include two exposure measures, one for in-state output competition, the other for outside-state competition.

Table 3 presents the effects of output exposure on firm performance. Columns 1 and 2 present the exposure effect treating each state/product combination as a separate market. For sales, the coefficient on within-state output exposure is around 70% (in magnitude) of the coefficient of newly eligible, and has the opposite sign. Since, as outlined in equation 9, $\hat{y}_j = \beta e_j - \theta\beta\left(\sum_{s=1}^{S} \omega_{js} \cdot \mu_s\right)$, the aggregate gains, $(1-\theta)\,\beta$, can be calculated by adding the coefficients on the direct and indirect effects. Therefore, Table 3 Column 1 implies that 30% of the private gains from the program are translated into aggregate gains (for instance, the regressions predict that if *every* firm gained access to the subsidies, every firm would expand by 10%). The relative magnitude is consistent across tables.[60]

Columns 3 and 4 include both within-state and outside-state competition. Within-state exposure to the program is substantially more important to firms than exposure from firms in different states: the coefficient on across-state exposure is close to zero, and the magnitude and precision of the effect of within-state competition remains reasonably unchanged with the inclusion of the

[60] In the model this would imply that $\sigma = \frac{1+2\phi}{1-\phi}$. For $\sigma = 5$, this would imply a $\phi = \frac{2}{7}$.

outside-state exposure measure. As outlined above, since location of sales is unobserved, it is difficult to distinguish if the small magnitude on across-state competition is due to the fact that firms in different states produce fundamentally different products even if they share a product code, or if firms do not produce much to sell in different states. However, the fact that firms are not significantly affected by their out-of-state competitors gaining access to subsidies suggests that treating each state as a separate market is reasonable.

Panel B shows that within-state output exposure predicts firm closure as well: if every one of a firm's competitors gained access, it would be 1-2% less likely to continue producing. Again, outside-state competition has low predictive power. In both panels, the coefficient on gaining access increases slightly relative to Table 2 - consistent with the logic of the model that firms will also indirectly affect themselves. Panels C and D look at the effect of exposure on firms' costs. Within-state competitor's access to the subsidies predicts a substantial decline in firm size, both for costs and for liabilities, and across-state competitors do not exert significant competitive pressures.[61]

VI.B Trade and Output Competition

Increased competition may matter less for products which are traded, much like the logic that production of traded products are less sensitive to changes in local demand (Matsuyama 1992; Magruder 2013). I augment equation 18 by estimating

$$\ln\left(y_{jt}\right) = \beta Post_t \times Small_j + \sum_k \Theta^k Post_t \times Exposure_j^k + \sum \gamma_{jt} X_j + \eta_j + \eta_t + \epsilon_{jt} \tag{20}$$

where now the indirect measures used are within and outside state output markets, and those markets for traded products. This regression has a similar motivation to a triple interaction, since the goal is to test if the difference-in-difference effects of output exposure is different for products which are traded and those which are not. However, since firms cannot be separated into those who produce only traded goods and those who produce only non-traded goods, it cannot be estimated using a standard difference-in-difference-in-differences approach. In keeping

[61] In Appendix Table 2, I instead use exposure measures calculated at the industry level. As expected, this overstates the aggregate effects of the program; the output exposure measure are positive and close 0, which would beneficial and small indirect effects.

with the spirit of the triple differences regression and to control for differences between firms who produce more traded goods and those who produce fewer, I create a measure for each firm capturing the share of its outputs which (before the policy change) are traded.[62] I then include $Post \times share\_traded$ as a control.

Table 4a shows the coefficients from estimating equation 20 on firms' intensive and extensive margin production choices. Column 1 includes exposure measures for "within-state" output competition and "within-state output competition" for traded products, and is my preferred specification for understanding the indirect effects of the policy change. The sum of the two exposure measure coefficients is close to (and is never statistically distinguishable from) 0 , suggesting no crowd-out for more-traded goods.[63] Conversely, the coefficient on overall exposure is almost identical to the direct effect (and again their sum is not statistically distinguishable from 0), which implies almost complete crowd-out for less-traded goods. Column 2 shows that this result holds even within 3-digit industries and states.

Columns 3 and 4 include the coefficients for outside-state competition as well, both for traded and non-traded goods. The coefficients on within-state exposure are of similar magnitudes and precisions, and the coefficients on outside-state competition insignificant and smaller than those for within-state competition, especially with fixed effects for each industry. The qualitative patterns are similar on the extensive margin, shown in Panel B, although with less statistical precision. Panels C and D, in Table 4b, show similar findings for credit and costs. However, the coefficient on traded output exposure - while consistently of similar magnitude to the coefficient on general exposure - is only significant for sales.[64]

[62] To be clear, this is a measure of if the firm produces *products* which are traded, not if the firm itself exports them (which is not reported in the ASI. In 2011 the survey asked the share of output which was *directly* exported, but only four firms reported non-zero shares.).

[63] The sum of the coefficients is positive but insignificant. This is weakly suggestive of positive agglomeration spillovers for traded products, a common argument for subsidizing exports (Rodrik 2008; Krueger and Tuncer 1982; Clerides et al. 1998; Klenow and Rodriguez-Clare 2005; Ohashi 2005)

[64] Appendix tables 4a and 4b demonstrate a similar pattern when using the alternate measure of product market exposure discussed above, which is created using a "census" of firms by combining each year of the ASI. Appendix Table 4c demonstrates the robustness of the three main specifications for the effects of the program on sales – a) the direct effects, b) the direct and indirect effects, and c) the indirect effects split by how traded the good is. Panel A does not trim sales before taking the log, Panel B uses an inverse hyperbolic transformation of sales (without trimming), and Panel C does not use the sampling weights. The alternative measures of sales growth, in Panels A and B, lead to qualitatively similar results to those in the main tables of the paper. Ignoring the sampling weights, in Panel C, gives point estimates which are consistent with complete crowd-out for less-traded products, and no crowd-out for

The difference-in-difference effect of output exposure can be visually seen by plotting the coefficients on the exposure measures for each year. Given the within-state output exposure to small firms for each firm, I estimate the Θ_t coefficients of the following extension of equation 18:

$$\ln\left(y_{jt}\right) = \sum_{t=2002}^{2011} \beta_t Small_j + \sum_{t=2002}^{2011} \Theta_t Exposure_j + \sum_{t=2002}^{2011} \Theta_t^x Traded\_Exposure_j + \sum \gamma_{jt} X_j + \eta_j + \eta_t + \epsilon_{jt}.$$

In Figure 7 Panel A, I plot the coefficients and 95% confidence-intervals for the exposure coefficients, which reflect the effect of the program on less-traded goods. Much like in Figure 6, there does not appear to be a pre-program trend in the effect of exposure to the program. However, after the implementation, exposed firms lose sales. Panel B plots the estimates and standard errors of $\Theta_t + \Theta_t^x$, representing the indirect effect of the program change through traded products. In each post-program year, the effect is weakly positive, consistent with the results in Table 4a.

VI.C Permutation Tests

In the spirit of Fisher (1935), I undertake three different permutation tests in order to examine how unlikely the regression results would be if there were no true effect of the program (Rosenbaum 2002; Ho and Imai 2006; Sinclair et al. 2012; Shue 2013). Research on peer effects are a natural setting for permutation tests, since one can permute a) the source of the shock, b) the connections of the network, and c) the characteristics of the network. Using Monte Carlo simulations of 1000 iterations, these tests construct placebo estimates around the null hypothesis that the subsidies do not matter, that output competition does not matter, and that trade does not matter. I report the results in Table 5.

In the first set of tests, for each iteration establishments are randomly assigned to the "newly eligible" group, regardless of their true assets in 2006, maintaining the same share of newly-eligible firms as in the real data. Furthermore, given the placebo eligibility changes, I construct each firm's (placebo) exposure through output competition, maintaining the products that the firms actually produce in the data and if those products are traded. I then re-estimate the effects of eligibility

more traded products. However, the estimates of the direct and indirect effects are both smaller than those in the main results of the paper, and the indirect competitive effects of more-traded products (relative to less-traded ones) is statistically insignificant.

$\hat{\beta}^{placebo}$, as well as $\hat{\theta^d}^{placebo}$ and $\hat{\theta^x}^{placebo}$, using equation 20, as outlined in the previous subsection.

The second set of tests undertakes a similar procedure, but instead constructs placebo indirect effects while maintaining the true eligibility changes. Specifically, for each product that a firm produces, I assign it a placebo product code (and each placebo product code gets a corresponding indicator for if it is "traded"). I maintain of the characteristics of production with each state: the number of products produced by formal and informal firms, the overlap between the two sectors, and the share of products which are traded.

The third set of tests is similar, but instead of testing the effect of the shock of the network, or the connections of the network, it tests the effect of heterogeneous network connections. In particular, in each of the permutations, a placebo for each product's "traded" status is generated, while maintaining the true eligibility changes and each firm's product mix.

Table 5 shows the means and standard deviations for these regressions, presented next to the results from Table 4a Panel A Column 1 (without stars). Column 2 shows the results from permuting the assignment of eligibility. The estimates are all small and close to 0; neither placebo eligibility nor exposure to placebo eligible firm predicts a change in firm behavior. Column 3 shows the results from permuting the network of production. The estimates on new eligibility are similar for those from Table 2,[65] while the estimates on the effect of placebo exposure are close to 0. Column 4 shows the results from permuting the tradability of products. The mean estimated coefficient on new eligibility and exposure are reasonably similar to from Table 3, but the estimates on placebo tradability are close to 0. In all cases, the coefficients coming from permuted data are are closer to 0 than their real world counterparts in over 99% of iterations.

VI.D Other Product Characteristics and Output Competition

There are a variety of product characteristics which could affect both the direct and indirect effects of the subsidy program. Furthermore, it may be that traded products have some particular feature which make them less affected by the program, rather than that they are actually traded. To test for this potential omitted variables bias, I focus on three product characteristics besides the tradability:

[65] Recall that in the data, including the spillover measures pushed the coefficient on the direct effect up slightly. That did not tend to happen in the permutation tests, as the estimated β stayed reasonably similar to the estimates which did non include the indirect effects.

the elasticity of substitution across products (from Broda and Weinstein 2006), the capital/labor ratio (calculated in the Indian data), and a measure of external finance requirements (the fraction of total liabilities divided by imputed flow costs). For the latter two measures, I first calculate firm-level measures of the capital/labor or liabilities/primary input cost ratios. For each product, I calculate the weighted average of these measures, where the weights are the share of the product produced by each firm. I then split the products by their median values.

Table 9 examines heterogeneity in the direct effect of subsidies through the four product-level types on sales, adapting equation 13 to various permutations of the form

$$\begin{aligned}\ln\left(y_{jt}\right) =& \beta_1 Post_t \times Small_j + \beta_2 Post_t \times Small_j \times type_j + \beta_3 Post_t \times type_j + \sum \gamma_{it} X_i \\ &+ \Theta Post_t \times Exposure_{jt} + \sum_{types} \Theta^{type} Post_t \times Exposure_j^{type} + \eta_j + \eta_t + \epsilon_{it}\end{aligned} \tag{21}$$

and reporting β_1, β_2 and the Θs. The results are reported in Table 6, and I only report effects on sales. Columns 1 and 2 test if firms producing higher shares of tradable products benefit relatively more from access to the program. The results are positive although statistically insignificant, weakly suggesting that exports may be an important margin through which credit constraints are binding (Beck 2003; Manova 2012). This is consistent with Kapoor et al. (2012), who find that Indian firms increased their exports in response to the 1999 policy change. Columns 3 and 4 include interactions for the various firm level measures with new eligibility. Capital intensity strongly negatively predicts benefiting from the program, and credit intensity has a strong and positive relationship. The extreme magnitudes may be mechanical, as the two measures are highly correlated (the correlation is .784), and the direct effect does not change substantially (although the precision of the estimate decreases substantially), but the latter result is consistent with theories of financial dependance (Rajan and Zingales 1998; Gupta and Yuan 2009), and the former may be because, for instance, capital is collateralizable, but labor is not, so increased access to credit is more helpful for firms reliant on labor.

Columns 5 and 6 add heterogeneous indirect effects for the sector characteristics. The original indirect exposure measure's magnitude remains reasonably unchanged, as does the coefficient on

exposure through traded products (although again the precision of the estimates decreases). The decline in firm sales is relatively larger from exposure through borrowing intensive products - which may be a function of the larger direct effect - and less through exposure of products with low elasticities of substitution, consistent with the model.

Appendix Table 5 reports the effects of interacting the direct and indirect effects with each firm's assets before the policy change (un units such that the smallest newly eligible firms have an assets value equal to 1). None of the interactions significantly predict firm outcomes.

VI.E Aggregate Effects of the Policy Change

While it is not possible to separately identify the direct and the indirect effects of the eligibility expansion by looking at the aggregate effects, it is possible to examine the joint effect. I create an empirical analogue to equation 8 by estimating equations of the form

$$\ln\left(Y_{st}\right) = \sum_{k} \beta_{\theta^k} Post_t \times \mu_s^k + \eta_s + \eta_t + \epsilon_{st} \tag{22}$$

in order to identify if sectors which are relatively more exposed to the policy change grow relatively quicker, where each separate state & product group is its own sector. In addition to being unable to separately identify the effects of the program, the aggregate effects are potentially less informative than the firm level regressions in the presence of product switching. If firms change their products in response to the policy change, then the effect on that firm's sales will potentially be very different than the estimated effect on that firm's old products. However, there are two appealing features of the aggregate regressions. First, these regressions account for entry and exit dynamics, since I estimate the change on all output, not just for firms whose eligibility status is known ex-ante. Second, I can estimate the effects not only on the total value of production, but also on quantities and prices, which is difficult to conceptualize for multi-product firms (especially those who switch products). For each outcome, I run four regressions: two only looking at the share of newly eligible firms, and two running a triple differences regression interacting the share of newly eligible firms with if the product is categorized as more-traded. Within each set, one of the regressions additionally controls for three-digit product and state fixed effects.

Table 7 Panel A tests the effects of exposure on the value of sectoral output, and finds effects consistent with the firm-level regressions presented earlier. Aggregate output is predicted to grow by around 10% if a sector went from having no subsidized firms to only subsidized firms, and the estimate is not significantly different from 0. The triple-differences regression shows that there are no predicted output gains for less-traded products, and output gains of around 40% for more-traded products, again consistent with the results in Table 4a. Panel B tests the effect of the program change on firm quantities. Increased program exposure increases the quantities produced in the sector by around 35%, with most of the effects from the more-traded products. Panel C tests the effects of the average price of the product. A fully exposed products is predicted to have a price around 25% lower than a non-exposed product. Unexpectedly, the lower prices are (weakly) concentrated in the more-traded products.[66] Across all specifications, the inclusions of product and state by time period fixed effects does not qualitatively change the results.

VII Effects of the Reallocation of Economic Activity

It has been argued that within-sector factor misallocation is key cause of low productivity in developing countries, and a common argument for small-firm subsidies is that they increase aggregate productivity (and a common argument against them is that they decrease aggregate productivity). In the spirit of Hsieh and Klenow (2009), I examine the effects of productivity coming from one specific channel: the change in the variance of productivities across firms. Subsidies can either increase the variance of revenue productivity across firms (lowering aggregate TFP), or the opposite. In order to make the productivity results tractable, and to make my results directly comparable to Hsieh and Klenow (2009), I adopt several of their assumptions. First, I treat each establishment as producing a single product in its self-reported industry. Second, the appropriate CES aggregation is at the 3 or 4 digit industry level. Third, the consumer has Cobb-Douglas utility on consumption from each industry: $U = \sum Y_i^{\chi_i}$ (this last assumption is somewhat supported in the data, since there is complete crowd-out for non-traded products). Furthermore, as in the model section, I

[66] Note that this is the average price of the firms in the data, not the P_s in the model, which is price index faced by the consumers, which is a weighted average of all varieties, including imports.

abstract from effects of the policy beyond the change in subsidies for the newly eligible firms.[67]

Under these alternate assumptions, the distortions and productivities can be identified in the data by the first-order conditions of the intermediate good producers (Hsieh and Klenow 2009; Chari et al. 2007):

$$\begin{aligned}
\left(1+\tau_{L_j}\right) =& \frac{\sigma}{\sigma-1}(1-\alpha)\frac{y_j}{wL_j} \\
\left(1+\tau_{K_j}\right) =& \frac{\sigma}{\sigma-1}(\alpha)\frac{y_j}{RK_j} \\
A_j \equiv TFPQ_j =& c_s \frac{\left(y_j\right)^{\frac{\sigma}{\sigma-1}}}{K_j^{\alpha} L_j^{(1-\alpha)}} \\
p_j A_j \equiv TFPR_j \propto& \left(1+\tau_{K_j}\right)^{\alpha}\left(1-\tau_{L_j}\right)^{1-\alpha}
\end{aligned} \tag{23}$$

where c_s is a sector specific constant.[68] Since the production function is Cobb-Douglas and demand is CES, it is possible to back out marginal cost given the price, and therefore quantity productivity given total inputs and outputs.[69]

VII.A Effect of the Policy Change on Firm Productivity and Relative Prices

In order to show that these measures are reasonable approximations of firm characteristics, Appendix Table 5 shows the correlation of a given measure for a firm in year t and that measure the previous time the firm was observed in the data. Column 1 demonstrates the correlations without trimming, and suggests that outliers are extremely important for this calculation, particularly for the labor wedge, where the correlation over time is less than .2. Column 2 trims the 1% outliers from the sample, and takes logs, which is the same outcome used in the empirical specification. The persistence of the measures is substantially higher. To show the effect of the policy change on productivity and the relative cost of factors, Table 8 demonstrates the results of estimating

[67] For instance, I do not account for misallocation caused by firms distorting their size in response to the program, described by Garicano et al. (2012), since in Appendix Table 1 I find no evidence of manipulation.

[68] Recall that the "capital wedge" in this paper changes the *absolute* cost of capital, while in Hsieh and Klenow (2009) it changes the *relative* cost of capital, which is why equation 23 is different than equation 17 in Hsieh and Klenow (2009). The distinction between a firm's physical productivity and its revenue productivity (Foster et al. 2008) is the crucial mediator of how the capital and labor wedges change aggregate TFP (in the model, in the absence of distortions every firm in a sector would have the same $TFPR_j$).

[69] In the calculations, I assume that $\sigma_s = 3$ and $\alpha = \frac{1}{3}$ in all sectors.

equations of the form

$$\ln(\cdot) = \beta Post_t \times Small_j + \sum_k \Theta^k Post_t \times Exposure_j^k + \sum \gamma_{jt} X_j + \eta_j + \eta_t + \epsilon_{jt}$$

for the still-existing firms. Panel A demonstrates the results on structurally-estimated quantity productivity (TFPQ), both for direct and indirect access. The coefficients are consistently low and insignificant: there do not seem to be large productivity adjustments in response to competition. Panel B looks at revenue productivity, assuming a Cobb-Douglas production function. While the indirect effects do not predict significant changes to revenue productivity, the direct effect does: after the program change, small firms have lower revenue productivity, by around 4-5%. Figure 8 plots event-study coefficients of the effect of being small on revenue productivity, following equation 19. Again, there does not appear to be a significant pre-trend in TFPR, nor a large effect in the April 2007 survey, but a sustained decreased afterwards.[70]

The change to TFPR is decomposed in Panels C and D. Panel C demonstrates that firms' capital wedge decreases by around 3.5% when they gain access to subsidies, although the coefficient tends to be marginally insignificant. The labor wedge of the newly eligible firms falls by around 5%. For both outcomes there are no statistically significant (or large) indirect effects. This is evidence consistent with the modeling assumption that the program change affects the prices paid by the newly eligible firms, without directly affecting the prices paid by their competitors. The fact that the labor wedge changes by somewhat more than the capital wedge is consistent with the finding in Table 6 that firms who produce products associated with lower capital/labor ratios benefit relatively more from access to subsidies. Recall that there is a latent "output wedge" which is unidentified in the data but also potentially changing. The estimates are consistent with, for instance, a 3.5% increase in output subsidies and, in addition, labor being 1.5% cheaper for newly eligible firms.

The estimated weighted average of the growth in input wedges is similar to the estimated growth in revenue productivity. Furthermore, with $\widehat{\left(1+\tau_{L_j}\right)} \approx -.05$, $\widehat{\left(1+\tau_{K_j}\right)} \approx -.035$, $\alpha = \frac{1}{3}$,

[70] While the coefficient on 2011 is lower than the rest, the gap is not significant (with a Wald test, $p \approx .12$) and does not correspond to any policy changes.

and $\sigma = 5$, the predicted direct effect of the program coming from the change in the wedges, $(1-\sigma)\left(\alpha\widehat{\left(1+\tau_{K_j}\right)} + (1-\alpha)\widehat{\left(1+\tau_{L_j}\right)}\right)$, equals .18, around 2/3 of the actual change in revenue.

VII.B Aggregate TFP Growth

This section extends the logic in Domar (1961) and Petrin and Levinsohn (2012) in order to explicitly consider the effects of changing relative prices on allocative efficiency. TFP in a sector is calculated as

$$TFP_i = \frac{Q_i}{K_i^{\alpha} L_i^{1-\alpha}} = \left(\frac{Y_i}{K_i}\right)^{\alpha}\left(\frac{Y_i}{L_i}\right)^{1-\alpha}\frac{1}{P_i}$$

where P_i is defined as the ideal price index in industry i, as in equation 2. Some algebra yields

$$TFP_i = \frac{\sigma}{\sigma-1}\left(\frac{\overline{MPRK_i}}{\alpha}\right)^{\alpha}\left(\frac{\overline{MPRL_i}}{1-\alpha}\right)^{1-\alpha}\frac{1}{P_i}$$

where $\overline{MPRK_i} = \frac{R}{\sum \frac{1}{\left(1+\tau_{k_j}\right)}\frac{y_j}{Y_i}}$ and $\overline{MPRL_i} = \frac{w}{\sum \frac{1}{\left(1+\tau_{l_j}\right)}\frac{y_j}{Y_i}}$.

A first-order approximation of growth in industry TFP, as a function of the changed subsidies, can therefore be written as

$$\widehat{TFP_i} = \sum\left[\alpha\widehat{\left(1+\tau_{k_j}\right)}\left(\frac{\frac{1}{\left(1+\tau_{k_j}\right)}\cdot\left(y_j\right)}{\sum\frac{1}{(1+\tau_m)}\cdot(y_m)} - \frac{y_j}{Y_i}\right) + (1-\alpha)\widehat{\left(1+\tau_{l_i}\right)}\left(\frac{\frac{1}{\left(1+\tau_{l_j}\right)}\cdot\left(y_j\right)}{\sum\frac{1}{(1+\tau_{l_m})}\cdot(y_m)} - \frac{y_j}{Y_i}\right)\right]. \tag{24}$$

Defining $\frac{1}{\overline{\left(1+\tau_{k_i}\right)}} \equiv \sum\frac{1}{\left(1+\tau_{k_j}\right)}\cdot\frac{\left(y_j\right)}{Y_i}$, and its equivalent for the weighted average labor wedge, $\frac{1}{\overline{\left(1+\tau_{l_i}\right)}} \equiv \sum\frac{1}{\left(1+\tau_{l_j}\right)}\cdot\frac{\left(y_j\right)}{Y_i}$, equation 24 can be rewritten as

$$\widehat{TFP_i} = \sum\left[\alpha\widehat{\left(1+\tau_{k_j}\right)}\left(\left(\frac{\overline{\left(1+\tau_{k_i}\right)}}{\left(1+\tau_{k_j}\right)} - 1\right)\frac{y_j}{Y_i}\right) + (1-\alpha)\widehat{\left(1+\tau_{l_i}\right)}\left(\left(\frac{\overline{\left(1+\tau_{l_i}\right)}}{\left(1+\tau_{l_j}\right)} - 1\right)\frac{y_j}{Y_i}\right)\right]. \tag{25}$$

Equation 25 formalizes the intuition that subsiding the inputs for distorted firms can increase aggregate productivity. For instance, capital subsidies for firm j will increase productivity in an industry iff the firm is facing relatively high distortions (iff $\left(1+\tau_{k_j}\right) > \overline{\left(1+\tau_{k_i}\right)}$), with a similar argument for labor subsidies. Knowing a firm's revenue productivity is not sufficient for knowing the correct productivity-enhancing subsidy. Subsidizing an input for firms who have relatively

low costs for that input will decrease TFP overall, even if generally those firms have relatively high TFPR (and so are relatively smaller than they would be in the absence of any distortions). Furthermore, information on a firm's productivity is uninformative (on its own) on the sign of the aggregate TFP change resulting from a targeted subsidy.

Overall TFP in manufacturing is $TFP = \prod TFP_i^{\chi_i}$, and so therefore

$$\widehat{TFP} = \sum \chi_i \widehat{TFP_i}. \tag{26}$$

I conduct the following counter-factual exercises to estimate how much productivity would change under different policy regimes: For 2001-2006, following equation 26, I estimate would would have happened had small firms in fact gained eligibility in 2001 instead of in 2007, by lowering their capital and labor costs by the values found in Table 8 Panel B (a 3.6% decline in the capital wedge, and a 5% decline in the labor wedge). For 2007-2011, conversely, I simulate the effect of no policy change by instead increasing the relative input costs by the same amount (effectively "undoing" the program). Equation 25 also provides bounds on the maximal possible TFP gains from a policy having that effect on input prices for 15% of firms: $\alpha * .036 * .15 + (1-\alpha) * .05 * .15 \approx .07$. In the data, had the subsidies instead gone to the 15% with the goal of increasing TFP the most,[71] the TFP gains would have been around 1.5-2%.

In Table 9, Panel A, I show the results, which are consistent across the two regimes and the two different aggregations of industry codes. Just by changing the relative distortions, introducing the policy change earlier would have increased TFP in the affected years by .05 - .1%, whereas removing the policy change would have lowered TFP by around a similar magnitude. The mapping is not perfect in each year, for instance the effect is relatively larger in 2001 and relatively smaller (in fact the opposite sign) in 2006, but is broadly consistent across the 11 years.[72] In Table 9, Panel B, I decompose the gains into those coming from changes to capital prices versus those

[71] That is, targeting the 15% of firms with the smallest $\left(\alpha\left(\frac{\overline{(1+\tau_{k_i})}}{(1+\tau_{k_j})}-1\right)+(1-\alpha)\left(\frac{\overline{(1+\tau_{l_i})}}{(1+\tau_{l_j})}-1\right)\right)\frac{y_j}{Y_i}$.

[72] Overall net value added in formal manufacturing in India was around 1.8 trillion rupees in the 2011 ASI (net value added was around 350 billion rupees), so a heroic back of the envelope calculation suggests that the program increased output by roughly $30 million (and net value added by $5.5 million).

for labor prices. While I do not map specific policies to specific changes in prices, it is still valuable to discuss the effects of hypothetical policy changes which shut down one of the price channels. Each input's price change explains around half of the aggregate TFP gains. This suggests that the expanding eligibility requirements for small firm subsidies was a small step towards lowering the 40-60% TFP gap between India and the United States found by Hsieh and Klenow (2009).[73]

In order to calculate how well-targeted the newly eligible firms were (if the only policy goal of relaxing the eligibility criteria was to reduce the within-sector variation in distortions), I estimate the distribution of potential TFP changes, using 1000 permutation tests as in Section 6.3. I assign the true change in wedges to random subset of firms, and the values in parenthesis represent the share of those estimates with TFP more positive (negative) the true change in the years before (after) the policy change. Panel A shows that the true change is larger than most of the counterfactual estimates in most years. Panel B shows that this effect is largely driven by the capital subsidies, where the true effect is larger than the placebo effects over 98% of the time.

VIII Conclusion

In this paper, I study the aggregate effects of programs which subsidize small firms, by leveraging a large-scale policy change. These types of programs are popular around the world, and are often justified by their effects on aggregate output and productivity. I focus my analysis on those two outcomes. My empirical analysis leverages a large-scale weakening of eligibility criteria for firm subsidies in India, dramatically shrinking the set of ineligible firms.

I make two methodological contributions. First, using standard assumptions in the trade literature, I show how changed input prices for some firms lead to changes in aggregate output, decomposing the effects into direct and indirect effects. The growth rate of a firm's sales through the indirect effect is linear: it will be twice as large if twice the share of activity in their sector is subsidized. The measure of indirect effects I generate can be used to calculate the (policy-relevant) elasticity of aggregate growth with respect to private growth. Second, I show how to adapt a canonical measure of misallocation to estimate the productivity effects of these types of

[73] An earlier draft of the paper found dramatically larger gains, using a different and less precise estimation strategy. Given that the policy change lowered the input costs of 15% of firms by around 5%, a .1% increase in TFP is a relatively large change.

input price shocks. I formalize the logic that within-sector misallocation will decrease iff the firms facing lower input prices are those who originally faced relatively high prices (compared to their sector).

I apply the model to detailed firm-level data in order to analyze the aggregate effects of firm subsidies. Datasets of this type are becoming more common in economics research, and I show that this type of information, in particular information on products, can be used for more than just increasing power for studying external shocks. In particular, I use product-level information to generate measures of how exposed firms are to each other, and therefore am able to estimate how firms affect their competitors. I also show that industry codes alone are not able to answer these types of questions.

My empirical results have nuanced consequences for policymakers. Gaining eligibility for small-firm subsidies predicts large gains in firm output, and increases the likelihood that a firm survives. However, crowd-out absorbed around two-thirds of the direct effects. The extend of crowd-out depends on sector characteristics, as all of the aggregate gains were concentrated in sectors with more internationally-traded products. Properly estimating crowd-out is therefore crucial for understanding the aggregate effects of firm level shocks, and for giving policy advice on the types of sectors where these types of subsidies can increase overall domestic output.

While I do not find evidence that non-traded sectors more exposed to the program change grew relatively more, there may have been aggregate increases in output from improved allocation of factors within-sectors. I calibrate that this mechanism increased aggregate productivity by around .1%. While this estimate is two orders of magnitude lower than the naive estimate of increased growth given by just the direct effects, the effects are reasonably large given the program's size and scope: around of half of the possible gains from targeting the most distorted firms.

These results alone are not enough for policy recommendations, since I abstract from potential costs of the program. While I do not find evidence that firms manipulate their size in response to the policy, nor do I find that the newly eligible firms' competitors behave as though they are newly taxed, subsidies for small firms may have equilibrium effects beyond the scope of the competitive effects studied in this paper, in addition to the costs of implementation and oversight. An analysis

which incorporates these channels, as well as the underlying political economy considerations, is a promising avenue of future research.

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A Alternate Models for Generating β and θ

A.A Lucas Span-of-Control Model

In this section, I show that the direct and indirect effects derived in Section 3.1 also follow from a decreasing returns to scale model *a la* Lucas, Robert E (1978). When possible, I try to keep notation for the relevant parameters the same as in the main text. I maintain the assumptions that in each sector, a single final good Q_s is produced by a representative firm in a perfectly competitive market, and that the utility function of the representative consumer is

$$U = \sum_{s=1}^{S} Q_s^{\phi} + c,$$

where c is consumption of the outside good, whose price is normalized to one, and the post-tax income of the consumer is assumed to be I (in partial equilibrium). Demand for the final good for each sector must satisfy

$$Q_s = \left(\frac{P_s}{\phi}\right)^{\frac{1}{\phi-1}}, \tag{27}$$

where P_s is the price charged by the final good producer.

The final goods producers in each sector produce their goods treating the output from each intermediate good producer as homogenous:

$$Q_s = \sum_{j=1}^{N_s} q_{js}. \tag{28}$$

Each intermediate good producer has a decreasing returns to scale production function in labor,

$$q_{js} = A_{js} L_{js}^{\alpha}, \tag{29}$$

where A_{js} is firm-specific TFP, and $\alpha \in (0,1)$ is the capital intensity. There is an output subsidy (τ_y), adjusting the relative price received by each firm, so firm j's revenue in sector s are given by

$$y_{js} = \left(1 + \tau_{y_j}\right) P L_j^{\beta}, \tag{30}$$

and profits are given by

$$\pi_{js} = \left(1 + \tau_{y_j}\right) p_{js} y_{js} - w L_{js}$$

where w reflects the wage. Since the intermediate goods in each sector are homogenous, in equilibrium they will all charge the same price, which will be the same price charged by the final good producer, P_s. Each intermediate good firm profit-maximizing in each sector chooses labor to satisfy

$$L_j = \left(\frac{w}{\left(1 + \tau_{y_j}\right) P_s A_{js} \alpha} \right)^{\frac{1}{\alpha - 1}}. \tag{31}$$

Plugging equations 31 and 28 into equation 27 and taking the growth rates yields

$$\hat{P} = \frac{\frac{\alpha}{\alpha-1}}{\left(\frac{\alpha}{1-\alpha} + \frac{1}{1-\phi}\right)} \sum_{j=1}^{N_s} \left[\left(\widehat{\frac{1}{\left(1 + \tau_{y_j}\right)}} \right) \frac{q_{js}}{\sum_{k=1}^{N_s} q_{ks}} \right]. \tag{32}$$

Plugging into equation 30 allows us to generate how the revenue of each firm grows as the firm-specific subsidies grow:

$$\hat{y}_j = \frac{2-\alpha}{1-\alpha} \frac{\frac{\alpha}{\alpha-1}}{\left(\frac{\alpha}{1-\alpha} + \frac{1}{1-\phi}\right)} \sum_{l=1}^{N_s} \left[\left(\widehat{\frac{1}{\left(1 + \tau_{y_j}\right)}} \right) \frac{q_{ls}}{\sum_{k=1}^{N_s} Q_{ks}} \right] - \frac{2-\alpha}{1-\alpha} \left(\widehat{\frac{1}{\left(1 + \tau_{y_j}\right)}} \right) \tag{33}$$

and

$$\hat{Y}_s = \left(1 - \frac{\frac{\alpha}{\alpha-1}}{\left(\frac{\alpha}{1-\alpha} + \frac{1}{1-\phi}\right)} \right) \frac{2-\alpha}{1-\alpha} \sum_{l=1}^{N_s} \left[- \left(\widehat{\frac{1}{\left(1 + \tau_{y_j}\right)}} \right) \frac{q_{ls}}{\sum_{k=1}^{N_s} q_{ks}} \right]$$

is the change in total revenue. The direct effect corresponding to β is $\frac{2-\alpha}{1-\alpha}$, the indirect effect corresponding to θ is $\frac{\frac{\alpha}{\alpha-1}}{\left(\frac{\alpha}{1-\alpha} + \frac{1}{1-\phi}\right)}$, and knowing those parameters plus the share of output in each sector with access to the subsidies is sufficient for calculating the aggregate change in output due to a change in firm-specific subsidies.

A.B CES utility over final goods

In this section, I show that the direct and indirect effects derived in Section 3. I only diverge from the baseline model by assuming that the representative consumer has CES utility over the final goods. In each sector, a single final good Q_s is produced by a representative firm in a perfectly competitive market. The utility function of the representative consumer (who has exogenous income I) is therefore

$$U = \left(\sum_{s=1}^{S} Q_s^{\frac{\phi-1}{\phi}} \right)^{\frac{\phi}{\phi-1}},$$

where now ϕ is the same for each good, and represents the cross-sector elasticity of substitution. Given price P_s in each sector, the aggregate price index is

$$P = \left(\sum_{s=1}^{S} \left(P_s^{1-\phi} \right) \right)^{\frac{1}{1-\phi}}$$

The revenue in sector S will therefore be

$$Y_s = P_s Q_s = P_s^{1-\phi} P^{\phi-1} I.$$

Revenue for each intermediate good producer will be

$$\begin{aligned} y_{js} =& p_{js} q_{js} \\ =& \frac{P_s^{1-\phi} P^{\phi-1} I}{(P_s)^{1-\sigma}} p_{js}^{1-\sigma}. \end{aligned} \tag{34}$$

Given (as in the main text) CES production from the representative final goods firms in each sector, Cobb-Douglas production from each intermediate goods producer, and firm-specific wedges of capital and labor, the growth rates of the final good producer's price, and the revenue and price

of the intermediate good producer satisfy:[74]

$$\begin{aligned}
\hat{y}_{js} &= (1-\sigma)\,\hat{p}_{js} + (\sigma-\phi)\,\hat{P}_s + (\phi-1)\,\hat{P} \\
\hat{P}_s &= \sum_{j=1}^{N_s}\left[\hat{p}_{sj}\frac{y_{js}}{Y_s}\right] \\
\hat{p}_{js} &= \alpha\left(\widehat{1+\tau_{K_j}}\right) + (1-\alpha)\left(\widehat{1+\tau_{L_j}}\right).
\end{aligned}$$

While $\hat{P}$ can be decomposed in a similar fashion to $\hat{P}_s$, it will affect each sector equally, and therefore will be absorbed by the time fixed effects in the regression. As a result, I omit its derivation. The change in each firm's revenue as a function of the changing wedges is therefore:

$$\begin{aligned}
\hat{y}_{js} = &(1-\sigma)\left(\alpha\left(\widehat{1+\tau_{K_j}}\right) + (1-\alpha)\left(\widehat{1+\tau_{L_j}}\right)\right) \\
&+ (\sigma-\phi)\sum_{j=1}^{N_s}\left[\left(\alpha\left(\widehat{1+\tau_{K_j}}\right) + (1-\alpha)\left(\widehat{1+\tau_{L_j}}\right)\right)\frac{y_{js}}{Y_s}\right]. \\
&+ (\phi-1)\,\hat{P}
\end{aligned}$$

The first line still reflects the direct effect of the program, which are unchanged relative to the main text. As inputs are relatively more subsidized (lowering the wedges), revenue will increase. The second and third lines reflect the indirect effect of the program, which captures how each firm's change in price changes the overall price index. As before, as σ increases, the indirect effect will be relatively larger, and the derivations of β and θ are simple.

B An Issue with Industry Codes

In many settings researchers use industry codes instead of product codes, since product codes are unavailable. In other settings, Delgado et al. (2014) and Hoberg and Phillips (2010) argue that industry codes are not the optimal way to cluster product markets In this section, I show that using industry codes as a proxy for competition will lead to biased estimates for the effects of competitive exposure, as firms will be assigned too much exposure to firms within their industry, and too little exposure to firms outside the industry. To see this, rewrite equation 9 (with an

[74] The notation is $\hat{x} = \frac{\dot{x}}{x}$ represents the growth of x over time.

indicator $i_k = 1$ for firm k self-reporting as being in industry i) as

$$\hat{y}_j = \beta e_j - \beta\theta \sum_{s=1}^{S} \omega_{js} \left[\frac{\sum_{k=1}^{N_s} i_k \times e_k \times y_{ks}}{Y_s} + \frac{\sum_{k=1}^{N_s} (1 - i_k) \times e_k \times y_{ks}}{Y_s} \right].$$

If instead I used the share of a firm's industry exposed to competition, I would generate

$$\hat{y}'_j = \beta e_j - \beta\theta \frac{\sum_{k=1}^{N} i_k \times e_k \times y_k}{Y_i}. \tag{35}$$

The difference between the two measures is

$$\hat{y}_j - \hat{y}'_j = \beta\theta \left[\sum_{s=1}^{S} \frac{\omega_{js}}{Y_s} \left(\left(\left(\sum_{k=1}^{N} i_k \times e_k \times y_k \right) \frac{Y_s}{Y_i} - \left(\sum_{k=1}^{N_s} i_k \times e_k \times y_{ks} \right) \right) - \left(\sum_{k=1}^{N_s} (1 - i_k) \times e_k \times y_{ks} \right) \right) \right]. \tag{36}$$

The first term $\left(\left(\sum_{k=1}^{N} i_k \times e_k \times y_k \right) \frac{Y_s}{Y_i} - \left(\sum_{k=1}^{N_s} i_k \times e_k \times y_{ks} \right) \right)$, can be decomposed further, to

$$\left(\left(\sum_{s' \neq s}^{S} \sum_{k=1}^{N} i_k \times e_k \times y_{ks'} \right) \frac{Y_s}{Y_i} - \left(\left(\frac{Y_s}{Y_i} - 1 \right) \sum_{k=1}^{N_s} (i_k \times e_k \times y_{ks}) \right) \right). \tag{37}$$

$\left(\sum_{s' \neq s}^{S} \sum_{k=1}^{N} i_k \times e_k \times y_{ks'} \right) \frac{Y_s}{Y_i}$ captures the fact that each industry produces products which are produced in other industries. As a result, given the original model, there will be indirect effects which industries do not impose on themselves. This will would lead one to overestimate the aggregate effects of firm specific programs if one estimated equation 35.

$\left(\left(\frac{Y_s}{Y_i} - 1 \right) \sum_{k=1}^{N_s} (i_k \times e_k \times y_{ks}) \right)$captures the fact that each firm may not produce the same set of products as its own industry. This effect would lead one to underestimate the aggregate effects of firm specific programs if one estimated equation 35.

Finally, $\left(\sum_{k=1}^{N_s} (1 - i_k) \times e_k \times y_{ks} \right)$ captures the fact that there may be firms in other industries who produce the same products as firm j. This effect will lead one to overestimate the aggregate effects if one estimated one estimated equation 35. In Appendix Table 2, I estimate the "effects" of the program change following equation 35. The results in the table incorrectly imply that there were no negative competitive spillovers due to the program change.

In ongoing work, I am working on constructing "industry-overlap" matrices, which can alle-

viate this issue even in settings where researchers only have access to industry codes.

C Within-District Exposure

While the notation used in section 3 used sector to mean output product markets, that is not necessary. In particular, I also focus on **local markets** for primary inputs. As eligible firms expand their primary inputs, the total change of formal manufacturing employment within a district may not be equal to the sum of the relative growth of eligible firms. Similar to the previous subsection, define $d \in \{1, D\}$ as the districts of India,[75] d_j as firm j's district, w_k as the cost of primary inputs at firm k, β^e as the relative effect in increasing primary input use, and θ^d as the competitive mediator. As a result the $N \times N$ *primary-input-primary-input* matrix (denoted $\mathbf{B}$), where element

$$b_{jk} = \left(\frac{(d_j = d_k) \cdot w_k}{\sum_{l=1}^{N} \left((d_j = d_k) \cdot w_l \right)} \right)$$

corresponds to the share of primary input costs in firm j's district paid by k, and the vector

$$\xi^d = \beta \left(\mathbf{I} + \theta^d \mathbf{B}' \right) \mathbf{e} \tag{38}$$

represents how each firm is affected by $\mathbf{e}$ relative to no policy through local employment markets.

C.A Estimating the Indirect Effect of Eligibility Through Local Markets

There are many markets through which local competition matters for inputs. In principle, formal manufacturing firms could compete with other formal firms, informal firms, agriculture, and services for access to both workers and capital. There are therefore many researcher degrees of freedom: in this section, I consider competition with other formal firms through (imputed) primary inputs, but the results are somewhat noisy both for this measure and for other measures, and so I do not highlight these results. Furthermore, the "panel" version of the ASI does not contain information about which district each establishments is in. I collected older versions of the 2006, 2009, and 2010 ASIs which do contain district information, but do not observe the location of

[75] In the data I have, there are 539 consistently defined districts with positive reported formal manufacturing output.

firms not surveyed in those years.[76] Figure 1 plots the distribution of the share primary input costs in each district newly eligible as of 2006. Another concern with geography-based identification is that districts with higher shares of newly eligible firms tend to be in the south, along the costs, or near major cities.[77]

As in section 6, I estimate 18 with a regression of the following form:

$$\ln\left(y_{jt}\right) = \beta Small_{jt} + \Theta Exposure_{jt} + \sum \gamma_{jt} X_j + \eta_j + \eta_t + \epsilon_{jt}, \tag{39}$$

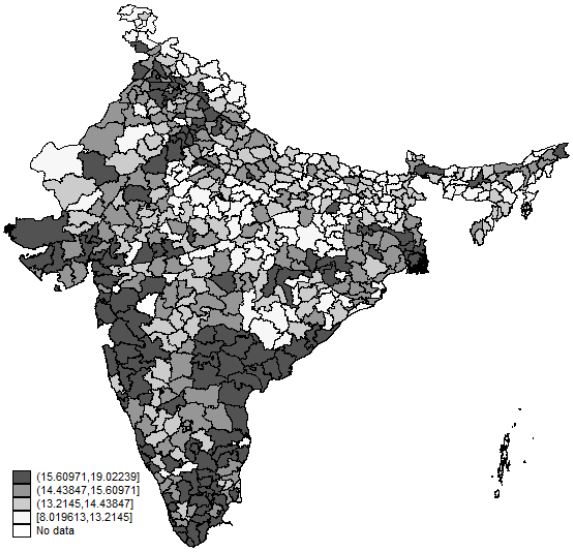
where the exposure measure comes from the share of a firm's district newly eligible for small firm benefits.

Appendix Table 3 presents the regression results. Focusing on column 4, which includes both the local and product-based exposure measures, as well as post reform X state and industry fixed effects, the effects of local competition appear broadly similar to those for product market competition, as the coefficients on the exposure measures are reasonably similar in magnitude to those for the direct effects. Without the state and industry fixed effects, in column 3, the coefficient declines to being close to zero. Due to the variability in the coefficients, it is difficult to know the extent to which a firm gaining eligibility for subsidies affect its neighbors, although this is a promising area for future research.

[76] For those years, there was a 100% matching rate from the panel version of the ASI to the older version. However, almost half of the firms in the sample were not surveyed in those years, and I do not know their district.

[77] Other concerns to geography-based identification are other place-based policy changes in India around the time. For instance, in 2005 the RBI changed its the branching requirements, effectively banks to expand to certain districts (Young 2014), and the National Rural Employment Guarantee Act was enacted, introducing a large workfare program in some districts before others.

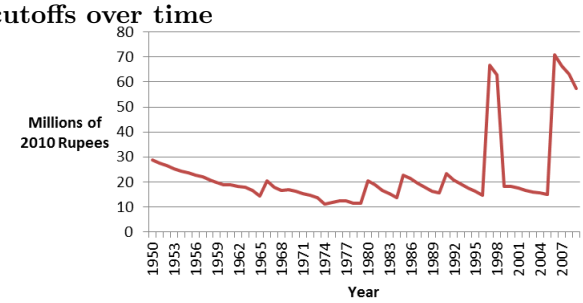
Figure 1: Distribution of Small Firms Across India



This figure plots the share of imputed primary input costs in 2006 at "small" formal firms in each district of India for which there is data in the ASI.

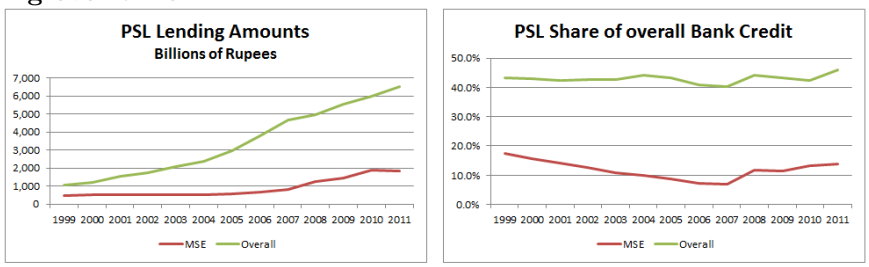
Figure 2: Small Firm Subsidies in India

Panel A: Maximal size cutoffs over time



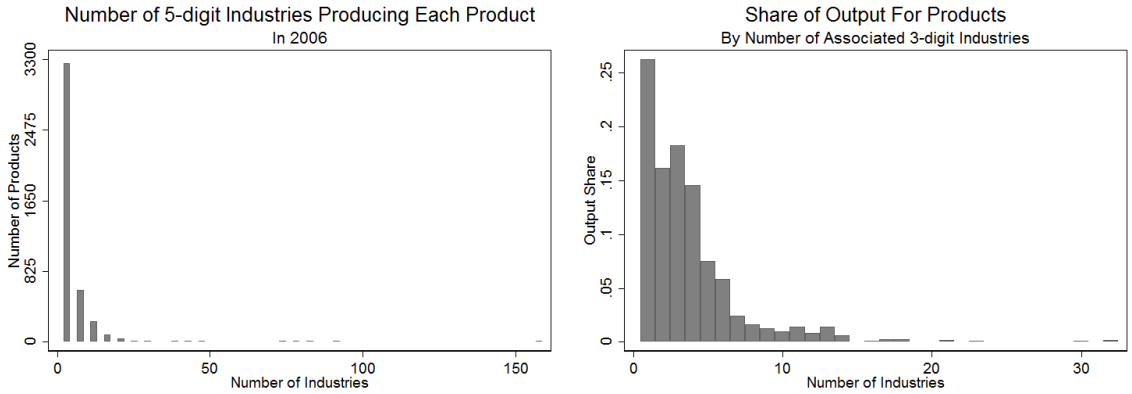
Plot of the change in the eligibility requirements for small firm benefits in India. All establishments whose (nominal) stock of plants and machinery are below the line are eligible. The manufacturing data in this paper covers 2001-2011, which is after the first spike and covering the second. Source: various Reserve Bank of India circulars.

Panel B: Lending over time



Plot of total value and the share of overall bank credit to "Priority Sector" borrowers, and to Micro,Small, and Medium Enterprise (MSE) borrowers. Source: Reserve Bank of India.

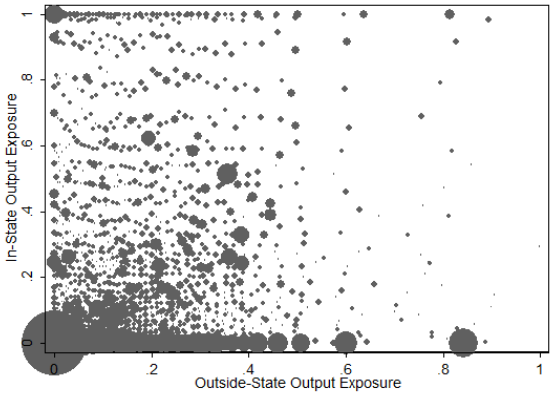
Figure 3: Distribution of Multi-Industry Products



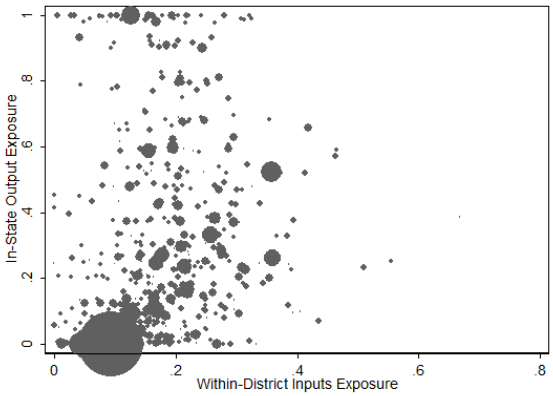
Column 1 plots of the distribution of how many 5-digit industries each product was produced in, in India in 2006. Column 2: Plot of the distribution of the share of total output coming from products produced in different number of 3-digit industries. I show the results for different aggregations to show that the overlap holds for multiple levels of industry codes. Plots use firm's self-identified industry classifications. Source: ASI

Figure 4: Scatterplots of Different Measures of Exposure to the Change in Eligibility

Panel A: In-State and Outside State Product Market Competition

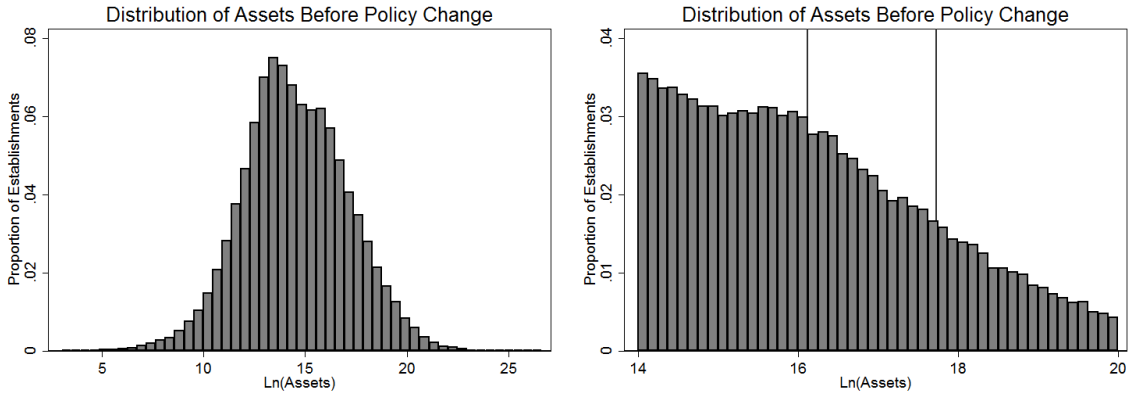


Panel B: In-State Product Market and In-District Input Market Competition



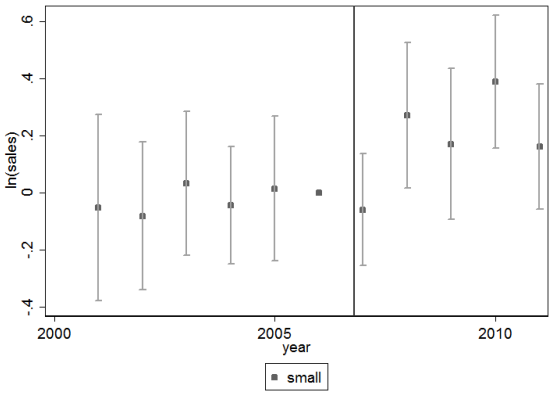
As outlined in the text, I calculate a exposure to the change in program eligibility for each firm in 2006. I generate 50 bins of inside-state outside-state, and inside district exposure measures. Each dot represents one combination of bins, The area corresponds to the number of firms in the group, and the location corresponds to the median value of exposure in the group. See text for construction of the exposure measures. Source: ASI

Figure 5: Distribution of Nominal Assets Before Policy Change in 2006



This figure demonstrates the distribution of nominal assets for all firms immediatly before the policy change. Appendix Table 1 uses the test developed by McCrary to formally test for a break in the firm-size distribution around the cut-offs. Source: ASI

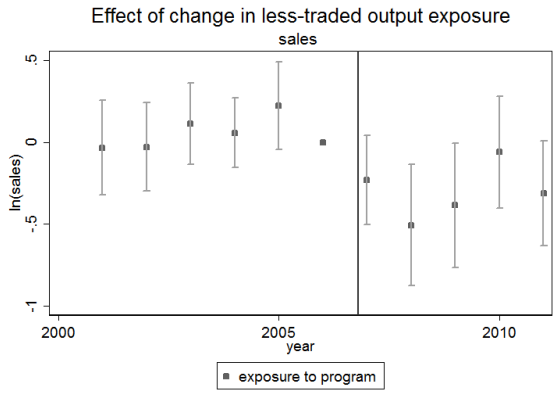
Figure 6: Event-Study Plot of Coefficients: Effect of Being Small on Sales



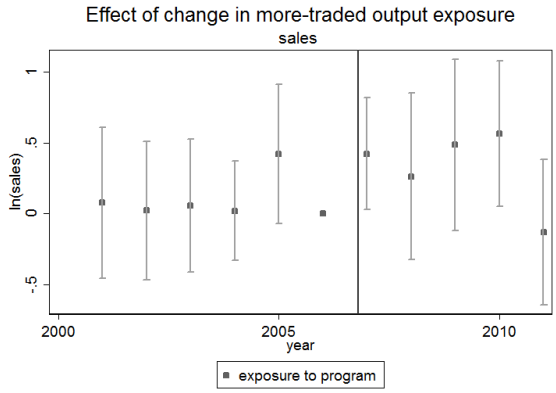
The sales of small (newly eligible in 2007) firms over time, relative to their peers. Each of the points comes from one pooled regression with time and firm fixed effects. The 95% confidence intervals are constructed using robust standard errors clustered by firm. The vertical line between 2006 and 2007 indicates the policy change, and 2006 was the omitted year in the regression. Source: ASI

Figure 7: Event-Study Plot of Coefficients: Effect of Product Market Competition

Panel A: Exposure Through Less-Traded In-State Product Markets

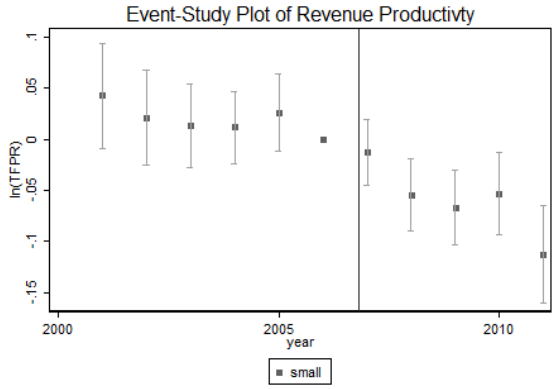


Panel B: Exposure Through More-Traded In-State Product Markets



The sales of firms with higher shares of exposure to small firms, relative to their peers. The exposure measures are constructed for each firm as a function of the (weighted) share of competitors who got access to the program, with specific details in the text. All of the points comes from one pooled regression with time and firm fixed effects, and separate coefficients for size and exposure in each year. The 95% confidence intervals are constructed using robust standard errors clustered by firm. The vertical line between 2006 and 2007 indicates the policy change, and 2006 was the omitted year in the regression. Source: ASI

Figure 8: Event-Study Plot of Coefficients: Effect of Being Small on Revenue Productivity



The sales of small (newly eligible in 2007) firms over time, relative to their peers. Each of the points comes from one pooled regression with time and firm fixed effects. The 95% confidence intervals are constructed using robust standard errors clustered by firm. The vertical line between 2006 and 2007 indicates the policy change, and 2006 was the omitted year in the regression. Revenue Productivity is caluculated assuming Cobb-Douglas production functions, with a capital share of 1/3. Source: ASI

Table 1: Summary Statistics for firms in most recent pre-program obervation

	Micro	Small	Big	Overall Mean	Overall SD
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Number of Firms	93586	20068	14237	127891	-
Range of historical value of fixed assets (million rupees)	<10	10-50	>50	-	-
"Census" scheme dummy	0.17	0.33	0.61	0.24	0.43
probability weight	4.28	3.19	2.07	3.86	3.01
years in data	2.55	3.26	5.22	2.96	2.31
ln(assets)	13.25	16.85	19.01	14.46	2.79
ln(sales)	14.62	16.60	18.16	15.40	2.40
ln(liabilities)	13.07	15.48	17.16	13.94	2.57
ln(total costs)	14.42	16.65	18.26	15.20	2.15
ln(units of firm)	1.21	1.73	2.79	1.47	8.67
in-state output exposure	0.15	0.40	0.14	0.19	0.27
in-state traded output exposure	0.03	0.09	0.02	0.04	0.36
share of firm's output from traded products	0.18	0.17	0.19	0.18	0.15
ln(TFPQ)	8.84	9.84	10.75	9.24	1.68
ln(TFPR)	1.28	1.09	1.05	0.46	0.97
ln(labor wedge)	1.65	1.96	2.14	-0.39	0.98
ln(capital wedge)	0.93	-0.27	-0.76	0.59	1.62

Notes: Summary statistics for all factories are based on ASI data. Only firms who declared assets before the policy change are reported, and this is the firm's value in the most recent pre-program year. Micro firms are imputed as being always eligible, small firms as newly eligible, and big firms as never eligible. Monetary values are denoted in real (2004) rupees. Sampling multipliers were not used, since every (formal) firm should be in the data.

"Census" scheme firms are those with employment over 100 workers, and therefore are sampled with certainty. Probability weight is the inverse sampling probability. Total costs imputed, as described in the data. The construction of the output exposure measures is discussed in section 4, and corresponds to the (weighted average) share of a firm's competitors who were newly eligible. "Traded" is defined as "above median share of production exported." The construction of TFPQ, TFPR, and the wedges is described in section 7. TFPQ is calculated by assuming a constant firm markup given CES utility and a Cobb-Douglas production function, TFPR is TFPQ*price, and each input wedge corresponds to a calcuation of how much "extra" the firm pays for each input. It is reported without normalizing for industry.

Table 2: Differences in Differences Estimates of the Direct Effect of Firm Subsidies

	Sample: All establishments		Sample: Single-plant establishments	
	(1)	**(2)**	**(3)**	**(4)**
A. Effect on ln(sales)				
Post Reform * Small Firm	0.276***	0.313***	0.326***	0.388***
	(0.071)	(0.072)	(0.079)	(0.081)
Number of Plant-Year Observations	298137	298137	245657	245657
R-squared	0.778	0.774	0.774	0.769
B. Effect on firm continuing to exist				
Post Reform * Small Firm	0.036***	0.040***	0.040***	0.045***
	(0.004)	(0.004)	(0.004)	(0.004)
Number of Plant-Year Observations	365019	365019	305663	305663
R-squared	0.634	0.624	0.637	0.627
C. Effect on ln(total liabilities)				
Post Reform * Small Firm	0.297***	0.338***	0.325***	0.392***
	(0.064)	(0.065)	(0.071)	(0.073)
Number of Plant-Year Observations	326800	326800	272163	272163
R-squared	0.772	0.767	0.768	0.763
D. Effect on ln(total costs)				
Post Reform * Small Firm	0.312***	0.355***	0.349***	0.415***
	(0.065)	(0.066)	(0.073)	(0.07)
Number of Plant-Year Observations	341513	341513	284371	284371
R-squared	0.748	0.741	0.742	0.735
Cubic Controls for Post Reform*Assets	Y	Y	Y	Y
Fixed Effects for Post Reform * 3-digit Industry and Post Reform*State	N	Y	N	Y

Notes: "Small" firms are those who gained eligibility in 2006. Each panel runs a difference in diffferences specification (with firm and year fixed effects), predicting the indicated outcome variable. Columns 3 and 4 correspond to 1 and 2, but only including the sample of single-plant establishments. The observations are weighted by their inverse sampling probability, and robust standard errors clustered by firm are reported in parentheses. *** denotes statistical significance at the 1% level, ** at 5%, and * at 10%. Source: ASI

Table 3: Differences in Differences Estimates of the Direct and Indirect Effects of Firm Subsidies

	(1)	(2)	(3)	(4)
A. Effect on ln(sales)				
Post Reform * Small Firm	0.332***	0.358***	0.329***	0.356***
	(0.078)	(0.079)	(0.078)	(0.079)
Post Reform * In-State Exposure	-0.233**	-0.226**	-0.208*	-0.217*
	(0.111)	(0.113)	(0.111)	(0.114)
Post Reform * Outside-State Exposure			-0.08	0.136
			(0.148)	(0.154)
Number of Plant-Year Observations	274724	274724	272843	272843
R-squared	0.751	0.751	0.751	0.751
B. Effect on firm continuing to exist				
Post Reform * Small Firm	0.332***	0.358***	0.329***	0.356***
	(0.078)	(0.079)	(0.078)	(0.079)
Post Reform * In-State Exposure	-0.233**	-0.226**	-0.208*	-0.217*
	(0.111)	(0.113)	(0.111)	(0.114)
Post Reform * Outside-State Exposure			-0.08	0.136
			(0.148)	(0.154)
Number of Plant-Year Observations	271921	271921	270088	270088
R-squared	0.729	0.725	0.729	0.725
C. Effect on ln(total liabilities)				
Post Reform * Small Firm	0.036***	0.038***	0.036***	0.038***
	(0.004)	(0.004)	(0.004)	(0.004)
Post Reform * In-State Exposure	-0.018***	-0.013*	-0.016***	-0.013*
	(0.006)	(0.007)	(0.006)	(0.007)
Post Reform * Outside-State Exposure			-0.010	0.012
			(0.009)	(0.009)
Number of Plant-Year Observations	298426	298426	296373	296373
R-squared	0.622	0.616	0.623	0.617
D. Effect on ln(total costs)				
Post Reform * Small Firm	0.360***	0.383***	0.362***	0.382***
	(0.075)	(0.076)	(0.075)	(0.076)
Post Reform * In-State Exposure	-0.243**	-0.243**	-0.221**	-0.236**
	(0.104)	(0.106)	(0.104)	(0.106)
Post Reform * Outside-State Exposure			-0.152	0.048
			(0.147)	(0.154)
Number of Plant-Year Observations	274724	274724	272843	272843
R-squared	0.751	0.748	0.752	0.748
Cubic Controls for Post Reform*Assets	Y	Y	Y	Y
Fixed Effects for Post Reform * 3-digit Industry and Post Reform*State	N	Y	N	Y

Notes: "Small" firms are those who gained eligibility in 2006. Exposure is calculated using a) each firms product mix in its most recent pre-program observation and b) the share of products produced by "small" firms in 2006. For firms who produce only products produced in their state, "outside-state output exposure" is undefined. Each panel runs a difference in diffferences specification (with firm and year fixed effects), predicting the indicated outcome variable. The observations are weighted by their inverse sampling probability, and robust standard errors clustered by firm are reported in parentheses. *** denotes statistical significance at the 1% level, ** at 5%, and * at 10%. Source: ASI

Table 4a: Differences in Differences Estimates of the Direct and Indirect Effects of Firm Subsidies With Differential Effects for Traded Products

	(1)	(2)	(3)	(4)
A. Effect on ln(sales)				
Post Reform * Small Firm	0.334***	0.354***	0.331***	0.426***
	(0.078)	(0.079)	(0.078)	(0.09)
Post Reform * In-State Exposure	-0.366***	-0.338***	-0.332***	-0.322**
	(0.125)	(0.129)	(0.126)	(0.14)
Post Reform * In-State	0.545**	0.471**	0.546**	0.537**
Traded Exposure	(0.236)	(0.24)	(0.237)	(0.265)
Post Reform * Outside-State Exposure			-0.186	0.152
			(0.172)	(0.199)
Post Reform * Outside-State			0.449	0.096
Traded Exposure			(0.343)	(0.389)
Number of Plant-Year Observations	274724	274724	272843	222870
R-squared	0.751	0.748	0.752	0.743
B. Effect on firm continuing to exist				
Post Reform * Small Firm	0.036***	0.037***	0.036***	0.043***
	(0.004)	(0.004)	(0.004)	(0.005)
Post Reform * In-State Exposure	-0.023***	-0.016**	-0.021***	-0.016**
	(0.007)	(0.007)	(0.007)	(0.008)
Post Reform * In-State	0.021	0.013	0.022	0.016
Traded Exposure	(0.014)	(0.014)	(0.014)	(0.016)
Post Reform * Outside-State Exposure			-0.014	0.013
			(0.01)	(0.012)
Post Reform * Outside-State			0.019	0.004
Traded Exposure			(0.02)	(0.023)
Number of Plant-Year Observations	274724	274724	272843	222870
R-squared	0.751	0.748	0.752	0.743
Cubic Controls for Post Reform*Assets	Y	Y	Y	Y
Fixed Effects for Post Reform * 3-digit Industry and Post Reform*State	N	Y	N	Y

Notes: "Small" firms are those who gained eligibility in 2006. Exposure is calculated using a) each firms product mix in its most recent pre-program observation and b) share of products produced by "small" firms in 2006, and c) the share of each product which is exported. For firms who produce only products produced in their state, "outside-state output exposure" is undefined. Each panel runs a difference in diffferences specification (with firm and year fixed effects), predicting the indicated outcome variable. The observations are weighted by their inverse sampling probability, and robust standard errors clustered by firm are reported in parentheses. *** denotes statistical significance at the 1% level, ** at 5%, and * at 10%. Source: ASI

Table 4a: Differences in Differences Estimates of the Direct and Indirect Effects of Firm Subsidies With Differential Effects for Traded Products

	(1)	(2)	(3)	(4)
C. Effect on ln(total liabilities)				
Post Reform * Small Firm	0.355***	0.379***	0.355***	0.437***
	(0.071)	(0.072)	(0.071)	(0.082)
Post Reform * In-State Exposure	-0.336***	-0.332***	-0.305***	-0.282**
	(0.113)	(0.116)	(0.114)	(0.126)
Post Reform * In-State	0.230	0.182	0.227	0.169
Traded Exposure	(0.219)	(0.224)	(0.221)	(0.246)
Post Reform * Outside-State Exposure			-0.265*	-0.005
			(0.16)	(0.184)
Post Reform * Outside-State			0.359	0.094
Traded Exposure			(0.316)	(0.356)
Number of Plant-Year Observations	274724	274724	272843	222870
R-squared	0.751	0.751	0.751	0.751
D. Effect on ln(total costs)				
Post Reform * Small Firm	0.347***	0.366***	0.346***	0.431***
	(0.074)	(0.075)	(0.074)	(0.086)
Post Reform * In-State Exposure	-0.374***	-0.345***	-0.342***	-0.317**
	(0.118)	(0.122)	(0.12)	(0.132)
Post Reform * In-State	0.400*	0.326	0.402*	0.365
Traded Exposure	(0.228)	(0.233)	(0.23)	(0.257)
Post Reform * Outside-State Exposure			-0.213	0.091
			(0.165)	(0.192)
Post Reform * Outside-State			0.391	0.039
Traded Exposure			(0.33)	(0.374)
Number of Plant-Year Observations	278865	278865	276948	226031
R-squared	0.751	0.751	0.751	0.751
Cubic Controls for Post Reform*Assets	Y	Y	Y	Y
Fixed Effects for Post Reform * 3-digit Industry and Post Reform*State	N	Y	N	Y

Notes: "Small" firms are those who gained eligibility in 2006. Exposure is calculated using a) each firms product mix in its most recent pre-program observation and b) share of products produced by "small" firms in 2006, and c) the share of each product which is exported. For firms who produce only products produced in their state, "outside-state output exposure" is undefined. Each panel runs a difference in diffferences specification (with firm and year fixed effects), predicting the indicated outcome variable. The observations are weighted by their inverse sampling probability, and robust standard errors clustered by firm are reported in parentheses. *** denotes statistical significance at the 1% level, ** at 5%, and * at 10%. Source: ASI

Table 5: Permutation Tests of Differences in Differences Estimates of a Placebo Effect of Firm Subsidies

		Placebo:		
	Real Data	New Eligibility	Products Produced	Tradability of Products
	(1)	**(2)**	**(3)**	**(4)**
A. Effect on ln(sales)				
Post Reform * Small Firm	0.334	0.002	0.277	0.331
	(0.078)	(0.079)	(0.027)	(0.002)
Post Reform * In-State Exposure	-0.366	-0.014	-0.027	-0.240
	(0.125)	(0.161)	(0.118)	(0.096)
Post Reform * In-State Traded Exposure	0.545	0.020	0.004	0.015
	(0.236)	(0.290)	(0.163)	(0.203)
Number of Firm-Year Observations	274724	274724	274724	274724
Number of Iterations	-	1000	1000	1000

Notes: Column 1 corresponds to the real data, predicting the indicated outcome in difference in diffferences specification (the same regression as table 4A, column 1). Columns 2-4 report the mean and standard deviation of 1000 runs of a difference-in-difference regressions, using different placebo treatements. Column 2 permutes which firms gained access to subsidies in 2006, while maintaining actual products produced and the tradability of those products. Column 3 maintains the the new eligible firms in the data, but permutes which products each firm produces, and therefore each firm's indirect exposure to the policy change. Column 4 maintains both the new eligibe firms and the products they produce, but permutes which products are "traded." Each regression included cubic controls for Post Reform * Assets. Stars are omitted. Source: ASI

Table 6: Differences in Differences Estimate of the Direct and Indirect Effects of Firm Subsidies
With Differential Effects Across Firm and Product Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
Effect on ln(sales)						
Post Reform * Small Firm	0.321***	0.322***	0.276*	0.243	0.271*	0.271
	(0.061)	(0.088)	(0.157)	(0.161)	(0.164)	(0.167)
Post Reform*Small Firm*share output	0.076	0.188	0.070	0.159	0.052	0.052
Traded	(0.152)	(0.221)	(0.222)	(0.224)	(0.222)	(0.224)
Post Reform*Small Firm*share output			0.124	0.030	0.056	0.056
Low Elasticity of Substitution			(0.157)	(0.16)	(0.172)	(0.174)
Post Reform*Small Firm*share output			-0.356*	-0.343*	-0.388*	-0.388*
Capital Intensive			(0.196)	(0.198)	(0.22)	(0.221)
Post Reform*Small Firm*share output			0.368*	0.469**	0.461**	0.461**
High Borrowing Intensity			(0.197)	(0.202)	(0.218)	(0.221)
Post Reform * In-State Exposure	-0.360***	-0.324**	-0.350***	-0.324**	-0.331	-0.331
	(0.089)	(0.13)	(0.127)	(0.131)	(0.237)	(0.24)
Post Reform * In-State	0.509***	0.383	0.491*	0.381	0.531**	0.531*
Traded Exposure	(0.187)	(0.269)	(0.265)	(0.269)	(0.267)	(0.272)
Post Reform * In-State					0.224	0.224
Low Elasticity of Substitution Exposure					(0.229)	(0.234)
Post Reform * In-State					0.055	0.055
Capital Intensive Exposure					(0.26)	(0.266)
Post Reform * In-State					-0.281	-0.281
High Borrowing Intensity Exposure					(0.265)	(0.272)
Number of Plant-Year Observations	271921	271921	271921	271921	271921	271921
R-squared	0.729	0.729	0.729	0.729	0.729	0.729
Cubic Controls for Post Reform*Assets	Y	Y	Y	Y	Y	Y
Fixed Effects for Post Reform * 3-digit Industry and Post Reform*State	N	Y	N	Y	N	Y

Notes: "Small" firms are those who gained eligibility in 2006. Exposure is calculated using a) each firms product mix in its most recent pre-program observation and b) share of products produced by "small" firms in 2006, and c) for each category, the share of each products above the median value, as described in the text. Each panel runs a difference in diffferences specification (with firm and year fixed effects), predicting the indicated outcome variable. The observations are weighted by their inverse sampling probability, and robust standard errors clustered by firm are reported in parentheses. *** denotes statistical significance at the 1% level, ** at 5%, and * at 10%. Source: ASI

Table 7: Differences in Differences Estimates of the Effect of the Program Change on Outcomes at the State/Product Level

	(1)	(2)	(3)	(4)
A. Effect on Total Value of Production: ln(Y)				
Post Reform * Small Firm	0.103	0.087	-0.058	-0.060
	(0.096)	(0.097)	(0.134)	(0.135)
Post Reform * In-State Exposure			0.418**	0.403**
			(0.201)	(0.2)
Number of Plant-Year Observations	35201	35201	31165	31165
R-squared	0.016	0.041	0.017	0.046
B. Effect on Total Quantity of Production: ln(Q)				
Post Reform * Small Firm	0.367***	0.398***	0.026	0.081
	(0.136)	(0.138)	(0.176)	(0.178)
Post Reform * In-State Exposure			0.766***	0.738***
			(0.284)	(0.283)
Number of Plant-Year Observations	33010	33010	29815	29815
R-squared	0.013	0.038	0.014	0.042
C. Effect on Product's Price: ln(P)				
Post Reform * Small Firm	-0.243**	-0.272***	-0.083	-0.114
	(0.104)	(0.105)	(0.13)	(0.132)
Post Reform * In-State Exposure			-0.339	-0.359*
			(0.215)	(0.219)
Number of Plant-Year Observations	33010	33010	29815	29815
R-squared	0.008	0.030	0.010	0.031
Fixed Effects for Post Reform * 3-digit Product and Post Reform*State	N	Y	N	Y

Notes: Each panel runs a difference in diffferences specification (with state/product and year fixed effects), predicting the indicated outcome variable for each state/product combination. The odd columns also control for post reform*if the product is traded. Total output and quantities is calculated using the firm-level information provided in the ASI, accounting for the sampling weights, the price is calculated by dividing the total value of output by the total quantity. *** denotes statistical significance at the 1% level, ** at 5%, and * at 10%. Source: ASI

Table 8: Differences in Differences Estimates of the Direct and Indirect Effects of Firm Subsidies On Productivity and Distortions to Capital and Output

	(1)	(2)	(3)	(4)	(5)	(6)
A. Effect on Quantity Productivity: ln(TFPQ)						
Post Reform * Small Firm	0.026	0.037*	0.015	0.021	0.015	0.019
	(0.022)	(0.022)	(0.024)	(0.024)	(0.024)	(0.024)
Post Reform * In-State Exposure			(0.043)	0.061*	0.02	0.048
			(0.033)	(0.034)	(0.037)	(0.038)
Post Reform * In-State					0.093	0.06
Traded Exposure					(0.072)	(0.072)
Number of Plant-Year Observations	260379	260379	239675	239675	239675	239675
R-squared	0.831	(0.832)	(0.822)	0.823	0.822	0.823
B. Effect on Revenue Productivity: ln(TFPR)						
Post Reform * Small Firm	-0.049***	-0.033**	-0.050***	-0.040**	-0.051***	-0.040**
	(0.015)	(0.015)	(0.016)	(0.016)	(0.016)	(0.016)
Post Reform * In-State Exposure			0.011	0.029	0.000	0.024
			(0.022)	(0.023)	(0.025)	(0.026)
Post Reform * In-State					0.046	0.023
Traded Exposure					(0.049)	(0.049)
Number of Plant-Year Observations	257233	257233	236655	236655	236655	236655
R-squared	0.771	0.773	0.758	0.760	0.758	0.760
C. Effect on capital wedge: ln(1+τ_k)						
Post Reform * Small Firm	-0.036*	-0.026	-0.034	-0.028	-0.034	-0.028
	(0.019)	(0.019)	(0.021)	(0.021)	(0.021)	(0.021)
Post Reform * In-State Exposure			-0.002	0.014	-0.006	0.017
			(0.031)	(0.031)	(0.035)	(0.035)
Post Reform * In-State					0.016	-0.014
Traded Exposure					(0.066)	(0.066)
Number of Plant-Year Observations	257509	257509	236899	236899	236899	236899
R-squared	0.855	0.856	0.847	0.848	0.847	0.848
D. Effect on labor wedge: ln(1+τ_l)						
Post Reform * Small Firm	-0.050***	-0.032**	-0.055***	-0.042***	-0.056***	-0.043***
	(0.015)	(0.015)	(0.016)	(0.016)	(0.016)	(0.016)
Post Reform * In-State Exposure			0.023	0.042*	0.007	0.031
			(0.023)	(0.023)	(0.026)	(0.026)
Post Reform * In-State					0.067	0.046
Traded Exposure					(0.05)	(0.05)
Number of Plant-Year Observations	261019	261019	240242	240242	240242	240242
R-squared	0.778	0.780	0.764	0.766	0.764	0.766
Cubic Controls for Post Reform*Assets	Y	Y	Y	Y	Y	Y
Fixed Effects for Post Reform * 3-digit Industry and Post Reform*State	N	Y	N	Y	N	Y

Notes: "Small" firms are those who gained eligibility in 2006. Exposure is calculated using a) each firms product mix in its most recent pre-program observation and b) share of products produced by "small" firms in 2006, and c) the share of each products which are exported. For firms who produce only products produced in their state, "outside-state output exposure" is undefined. Each panel tests runs a difference in diffferences specification (with firm and year fixed effects), predicting the indicated outcome variable. The outcome variables are calculated following Hsieh and Klenow (2009), as described in the text. The observations are weighted by their inverse sampling probability, and robust standard errors clustered by firm are reported in parentheses. *** denotes statistical significance at the 1% level, ** at 5%, and * at 10%. Source: ASI

Table 9: Counterfactual Change in Aggregate TFP from the Policy Change

A. % TFP Change from Counterfactual Subsidiy Regimes

	Subsidizing Firms Earlier						Never Introducing Subsidies				
level of aggregation	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011
3-digit	0.12%	0.10%	0.10%	0.06%	0.06%	-0.04%	-0.07%	-0.11%	-0.01%	-0.14%	-0.06%
Industries	(0.02)	(0.02)	(0.01)	(0.07)	(0.12)	(0.87)	(0.13)	(0.11)	(0.58)	(0.25)	(0.41)
4-digit	0.10%	0.09%	0.09%	0.04%	0.04%	-0.04%	-0.06%	-0.02%	-0.02%	-0.17%	-0.06%
Industries	(0.02)	(0.02)	(0.01)	(0.16)	(0.23)	(0.84)	(0.14)	(0.36)	(0.36)	(0.04)	(0.43)

B. Decomposition of % TFP Change from Counterfactual Subsidiy Regimes

		Subsidizing Firms Earlier						Never Introducing Subsidies				
	level of aggregation	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011
Only Change Price of Labor	3-digit	0.07%	0.04%	0.05%	0.01%	0.02%	-0.09%	-0.03%	-0.07%	0.03%	-0.09%	-0.03%
	Industries	(0.13)	(0.17)	(0.06)	(0.42)	(0.50)	(0.97)	(0.39)	(0.31)	(0.88)	(0.52)	(0.61)
	4-digit	0.06%	0.04%	0.05%	0.00%	0.00%	-0.07%	-0.03%	0.01%	0.01%	-0.13%	-0.03%
	Industries	(0.09)	(0.14)	(0.05)	(0.58)	(0.55)	(0.95)	(0.34)	(0.62)	(0.65)	(0.18)	(0.57)
Only Change Price of Capital	3-digit	0.05%	0.06%	0.05%	0.05%	0.05%	0.05%	-0.04%	-0.04%	-0.04%	-0.04%	-0.03%
	Industries	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
	4-digit	0.04%	0.05%	0.04%	0.04%	0.04%	0.03%	-0.03%	-0.03%	-0.03%	-0.04%	-0.02%
	Industries	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)

Notes: This table estimates counterfactual changes in aggregate TFP under different policy regimes. For 2001-2006, I calculate the TFP change had eligbility for subsidies been granted earlier. For 2007-2011, I calculate the TFP change had the eligibility never been expanded. In Panel B, I (exactly) decompose the changes into those coming from changes to each input's prices. The values in parenthesis are calculated running 1000 permutation tests on granting subsidies to random firms, and I show the proportion of those iterations with larger gains (for 2001-2006) or larger losses (2007-2011).

Appendix Table 1: Tests for Firm-Size Manipulation

Panel A. McCrary Density Tests

	2006 cutoff	2011 cutoff
	(1)	**(2)**
Firm Size Before	-0.017	0.017
Policy Change	(0.02)	(0.03)
Firm Size After	0.036	0.02
Policy Change	(0.02)	(0.03)

Panel B. Regression Discontinuity Tests

	Effect in 2006		Effect in 2011	
	2006 cutoff	2011 cutoff	2006 cutoff	2011 cutoff
	(1)	**(2)**	**(3)**	**(4)**
ln(sales)	0.05	-0.06	0.15	0.04
	(0.08)	(0.08)	(0.12)	(0.07)
ln(total liabilities)	0.03	-0.02	-0.05	0.09
	(0.04)	(0.12)	(0.77)	(0.09)
ln(employment costs)	0.07	0.02	0.07	0.12
	(0.04)	(0.08)	(0.10)	(0.08)

Notes: Panel A presents four McCrary tests of the distribution of firm sizes around the asset size-based eligibility criteria in India. The first column is at the older, lower, cutoff, and the second column is at the new eligibility criteria. The first row counts each firm once, in its most recent pre-program observation, and the second row looks at firms in their most recent post-program observation. Standard errors in parenthesis. Panel B presents regression discontinuity (following Calonico et al. 2014) estimates of the "effect" of being just a certain firm size, again using the old and new cutoffs and before and after the policy change.

Appendix Table 2: Differences in Differences Estimate of the Direct and Indirect Effects of Firm Subsidies, (incorrectly) Using Industry Codes as Measure of Exposure

	(1)
A. Effect on ln(sales)	
Post Reform * Small Firm	0.273***
	(0.072)
Post Reform * In-State Exposure	0.057
(Generated with Industry Codes)	(0.172)
Number of Plant-Year Observations	298043
R-squared	0.778
B. Effect on firm continuing to exist	
Post Reform * Small Firm	0.030***
	(0.004)
Post Reform * In-State Exposure	0.020*
(Generated with Industry Codes)	(0.011)
Number of Plant-Year Observations	298043
R-squared	0.719
C. Effect on ln(total liabilities)	
Post Reform * Small Firm	0.290***
	(0.067)
Post Reform * In-State Exposure	0.209
(Generated with Industry Codes)	(0.162)
Number of Plant-Year Observations	293257
R-squared	0.797
D. Effect on ln(total costs)	
Post Reform * Small Firm	0.294***
	(0.071)
Post Reform * In-State Exposure	0.069
(Generated with Industry Codes)	(0.17)
Number of Plant-Year Observations	297122
R-squared	0.785
Cubic Controls for Post Reform*Assets	Y
Fixed Effects for Post Reform * 3-digit Industry and Post	N

Notes: "Small" firms are those who gained eligibility in 2006. Exposure is calculated using a) each firm's self-reported industry code in its most recent pre-program year of observation, and b) the share of that industry which was newly eligible in 2006. The observations are weighted by their inverse sampling probability, and robust standard errors clustered by firm are reported in parentheses. *** denotes statistical significance at the 1% level, ** at 5%, and * at 10%. Source: ASI

Appendix Table 3: Differences in Differences Estimate of the Direct and Indirect Effects of Firm Subsidies, Using Local Primary Input Exposure as a Measure of Indirect Effects

	(1)	(2)	(3)	(4)
A. Effect on ln(sales)				
Post Reform * Small Firm	0.221***	0.254***	0.254***	0.288***
	(0.08)	(0.081)	(0.089)	(0.09)
Post Reform * In-District Exposure	-0.011	-0.391	0.04	-0.374
	(0.243)	(0.281)	(0.244)	(0.281)
Post Reform * In-State			-0.208*	-0.217*
			(0.111)	(0.114)
Number of Plant-Year Observations	271921	271921	270088	270088
R-squared	0.729	0.725	0.729	0.725
B. Effect on firm continuing to exist				
Post Reform * Small Firm	0.033***	0.037***	0.033***	0.035***
	(0.004)	(0.005)	(0.005)	(0.005)
Post Reform * In-District Exposure	0.002	-0.017	-0.013	-0.029*
	(0.015)	(0.017)	(0.015)	(0.017)
Post Reform * In-State			-0.016***	-0.013*
			(0.006)	(0.007)
Number of Plant-Year Observations	298426	298426	296373	296373
R-squared	0.729	0.725	0.729	0.725
C. Effect on ln(total liabilities)				
Post Reform * Small Firm	0.237***	0.285***	0.294***	0.333***
	(0.072)	(0.073)	(0.081)	(0.082)
Post Reform * In-District Exposure	0.015	-0.408*	0.030	-0.451*
	(0.216)	(0.25)	(0.224)	(0.257)
Post Reform * In-State			-0.255**	-0.282***
			(0.102)	(0.104)
Number of Plant-Year Observations	274724	274724	272843	272843
R-squared	0.729	0.725	0.729	0.725
D. Effect on ln(total costs)				
Post Reform * Small Firm	0.261***	0.306***	0.303***	0.330***
	(0.073)	(0.074)	(0.086)	(0.087)
Post Reform * In-District Exposure	0.114	-0.346	0.046	-0.426
	(0.244)	(0.276)	(0.256)	(0.289)
Post Reform * In-State			-0.221**	-0.236**
			(0.104)	(0.106)
Number of Plant-Year Observations	280741	280741	278811	278811
R-squared	0.729	0.725	0.729	0.725
Cubic Controls for Post Reform*Assets	Y	Y	Y	Y
Fixed Effects for Post Reform * 3-digit Industry and Post Reform*State	N	Y	N	Y

Notes: "Small" firms are those who gained eligibility in 2006. Exposure is calculated using a) each firms product mix in its most recent pre-program observation and b) the share of products produced by "small" firms in 2006, both for each state-product and within each district. Primary input costs are imputed from firms capital, employment, and materials use. Each panel tests runs a difference in diffferences specification (with firm and year fixed effects), predicting the indicated outcome variable. The observations are weighted by their inverse sampling probability, and robust standard errors clustered by firm are reported in parentheses. *** denotes statistical significance at the 1% level, ** at 5%, and * at 10%. Source: ASI

Appendix Table 4a: Differences in Differences Estimate of the Direct and Indirect Effects of Firm Subsidies With Differential Effects for Traded Products, using an alternate measure of exposure

	(1)	(2)	(3)	(4)
A. Effect on ln(sales)				
Post Reform * Small Firm	0.346***	0.358***	0.338***	0.357***
	(0.079)	(0.079)	(0.079)	(0.08)
Post Reform * (Alternate) In-State	-0.450***	-0.388***	-0.405***	-0.379***
Exposure	(0.131)	(0.134)	(0.134)	(0.136)
Post Reform * (Alternate) In-State	0.687***	0.591**	0.676***	0.608**
Traded Exposure	(0.253)	(0.256)	(0.254)	(0.257)
Post Reform * (Alternate) Outside-State			-0.228	0.037
Exposure			(0.166)	(0.176)
Post Reform * (Alternate) Outside-State			-0.445	-0.578*
Traded Exposure			(0.339)	(0.347)
Number of Plant-Year Observations	271921	271921	271921	271921
R-squared	0.743	0.743	0.743	0.743
B. Effect on firm continuing to exist				
Post Reform * Small Firm	0.037***	0.038***	0.036***	0.037***
	(0.004)	(0.004)	(0.004)	(0.004)
Post Reform * (Alternate) In-State	-0.028***	-0.017**	-0.024***	-0.015*
Exposure	(0.008)	(0.008)	(0.008)	(0.008)
Post Reform * (Alternate) In-State	0.026*	0.016	0.025*	0.015
Traded Exposure	(0.015)	(0.015)	(0.015)	(0.015)
Post Reform * (Alternate) Outside-State			-0.020**	-0.011
Exposure			(0.01)	(0.011)
Post Reform * (Alternate) Outside-State			-0.023	-0.021
Traded Exposure			(0.021)	(0.021)
Number of Plant-Year Observations	298426	298426	298426	298426
R-squared	0.743	0.743	0.743	0.743
Cubic Controls for Post Reform*Assets	Y	Y	Y	Y
Fixed Effects for Post Reform * 3-digit Industry and Post Reform*State	N	Y	N	Y

Notes: "Small" firms are those who gained eligibility in 2006. The alternate exposure measures is calculated using a) each firms product mix in its most recent pre-program observation and b) share of products produced by "small" firms before the program change, where each firm is counted once and no weights are used, and c) the share of each products which are exported. For firms who produce only products produced in their state, "outside-state output exposure" is undefined. Each panel tests runs a difference in diffferences specification (with firm and year fixed effects), predicting the indicated outcome variable. The observations are weighted by their inverse sampling probability, and robust standard errors clustered by firm are reported in parentheses. *** denotes statistical significance at the 1% level, ** at 5%, and * at 10%. Source: ASI

Appendix Table 4b: Differences in Differences Estimate of the Direct and Indirect Effects of Firm Subsidies With Differential Effects for Traded Products, using an alternate measure of exposure

	(1)	(2)	(3)	(4)
C. Effect on ln(total liabilities)				
Post Reform * Small Firm	0.371***	0.383***	0.364***	0.382***
	(0.072)	(0.073)	(0.073)	(0.073)
Post Reform * (Alternate) In-State	-0.444***	-0.393***	-0.402***	-0.378***
Exposure	(0.119)	(0.122)	(0.122)	(0.124)
Post Reform * (Alternate) In-State	0.414*	0.341	0.395*	0.341
Traded Exposure	(0.233)	(0.237)	(0.234)	(0.237)
Post Reform * (Alternate) Outside-State			-0.218	-0.025
Exposure			(0.151)	(0.16)
Post Reform * (Alternate) Outside-State			-0.357	-0.413
Traded Exposure			(0.313)	(0.32)
Number of Plant-Year Observations	274724	274724	274469	274469
R-squared	0.751	0.751	0.751	0.751
D. Effect on ln(total costs)				
Post Reform * Small Firm	0.358***	0.369***	0.349***	0.367***
	(0.075)	(0.076)	(0.076)	(0.076)
Post Reform * (Alternate) In-State	-0.460***	-0.399***	-0.411***	-0.383***
Exposure	(0.125)	(0.128)	(0.127)	(0.129)
Post Reform * (Alternate) In-State	0.584**	0.488**	0.566**	0.495**
Traded Exposure	(0.242)	(0.245)	(0.243)	(0.246)
Post Reform * (Alternate) Outside-State			-0.262*	-0.024
Exposure			(0.159)	(0.168)
Post Reform * (Alternate) Outside-State			-0.373	-0.497
Traded Exposure			(0.326)	(0.333)
Number of Plant-Year Observations	278865	278865	278603	278603
R-squared	0.751	0.751	0.751	0.751
Cubic Controls for Post Reform*Assets	Y	Y	Y	Y
Fixed Effects for Post Reform * 3-digit Industry and Post Reform*State	N	Y	N	Y

Notes: "Small" firms are those who gained eligibility in 2006. The alternate exposure measures is calculated using a) each firms product mix in its most recent pre-program observation and b) share of products produced by "small" firms before the program change, where each firm is counted once and no weights are used, and c) the share of each products which are exported. For firms who produce only products produced in their state, "outside-state output exposure" is undefined. Each panel tests runs a difference in diffferences specification (with firm and year fixed effects), predicting the indicated outcome variable. The observations are weighted by their inverse sampling probability, and robust standard errors clustered by firm are reported in parentheses. *** denotes statistical significance at the 1% level, ** at 5%, and * at 10%. Source: ASI

Appendix Table 4c: Robustness Checks on the Differences in Differences Estimates of the Direct and Indirect Effects of Firm Subsidies on Firm Sales

	(1)	(2)	(3)	(4)	(5)	(6)
A. Ln(sales): No Trimming						
Post Reform * Small Firm	0.311***	0.334***	0.371***	0.379***	0.372***	0.378***
	(0.077)	(0.077)	(0.087)	(0.086)	(0.087)	(0.086)
Post Reform * In-State Exposure			-0.218*	-0.205*	-0.341**	-0.318**
			(0.12)	(0.121)	(0.136)	(0.137)
Post Reform * In-State					0.488*	0.460*
Traded Exposure					(0.25)	(0.251)
Number of Plant-Year Observations	298137	298137	271921	271921	271921	271921
R-squared	0.769	0.772	0.717	0.720	0.717	0.720
B. Sinh^{-1}(sales): No Trimming						
Post Reform * Small Firm	0.293***	0.316***	0.350***	0.360***	0.352***	0.358***
	(0.074)	(0.074)	(0.084)	(0.083)	(0.084)	(0.083)
Post Reform * In-State Exposure			-0.211*	-0.197*	-0.331***	-0.306**
			(0.115)	(0.116)	(0.13)	(0.132)
Post Reform * In-State					0.474**	0.446*
Traded Exposure					(0.24)	(0.241)
Number of Plant-Year Observations	298137	298137	271921	271921	271921	271921
R-squared	0.775	0.778	0.724	0.728	0.724	0.728
Panel C. Ln(sales): No Sampling Weights						
Post Reform * Small Firm	0.118***	0.149***	0.189***	0.195***	0.19***	0.194***
	(0.043)	(0.044)	(0.053)	(0.053)	(0.053)	(0.053)
Post Reform * In-State Exposure			-0.219***	-0.169**	-0.272***	-0.209**
			(0.075)	(0.076)	(0.083)	(0.085)
Post Reform * In-State					0.225	0.167
Traded Exposure					(0.146)	(0.147)
Number of Plant-Year Observations	298137	298137	271921	271921	271921	271921
R-squared	0.722	(0.725)	(0.680)	0.684	0.68	0.684
Cubic Controls for Post Reform*Assets	Y	Y	Y	Y	Y	Y
Fixed Effects for Post Reform * 3-digit Industry and Post Reform*State	N	Y	N	Y	N	Y

Notes: "Small" firms are those who gained eligibility in 2006. Exposure is calculated using a) each firms product mix in its most recent pre-program observation and b) share of products produced by "small" firms in 2006, and c) the share of each products which are exported. For firms who produce only products produced in their state, "outside-state output exposure" is undefined. Each panel tests runs a difference in diffferences specification (with firm and year fixed effects), predicting sales, using different specifications than those in the main text, as indicated in the panel headings. For Panels A and B, the observations are weighted by their inverse sampling probability, and for all panels, robust standard errors clustered by firm are reported in parentheses. *** denotes statistical significance at the 1% level, ** at 5%, and * at 10%. Source: ASI

Appendix Table 5: Differences in Differences Estimate of the Direct and Indirect Effects of Firm Subsidies With Differential Effects for Firms of Different Size

	(1)	(2)	(3)	(4)	(5)	(6)
Effect on ln(sales)						
Post Reform * Small Firm	0.185*	0.306**	0.219	0.294*	0.292*	0.292*
	(0.108)	(0.15)	(0.154)	(0.152)	(0.152)	(0.152)
Post Reform * In-State Exposure			-0.098	-0.017	-0.116	-0.116
			(0.112)	(0.113)	(0.127)	(0.127)
Post Reform * In-State Traded Exposure					0.405*	0.405*
					(0.229)	(0.229)
Post Reform * Small Firm * Assets	0.056	0.055	0.062	0.061	0.061	0.061
	(0.04)	(0.055)	(0.055)	(0.055)	(0.055)	(0.055)
Post Reform * In-State Exposure * Assets			-0.010	-0.007	-0.007	-0.007
			(0.007)	(0.007)	(0.007)	(0.007)
Post Reform * In-State Traded Exposure * Assets					-0.001	-0.001
					(0.001)	(0.001)
Number of Plant-Year Observations	303327	303327	273430	273430	273430	273430
R-squared	0.773	0.776	0.681	0.686	0.686	0.686
Cubic Controls for Post Reform*Assets	Y	Y	Y	Y	Y	Y
Fixed Effects for Post Reform * 3-digit Industry and Post Reform*State	N	Y	N	Y	N	Y

Notes: "Small" firms are those who gained eligibility in 2006. Exposure is calculated using a) each firms product mix in its most recent pre-program observation and b) share of products produced by "small" firms in 2006, and c) for each category, the share of each products above the median value, as described in the text. Each panel runs a difference in diffferences specification (with firm and year fixed effects), predicting the indicated outcome variable. The observations are weighted by their inverse sampling probability, and robust standard errors clustered by firm are reported in parentheses. Assets based on each firm's size in the last reported pre-2006 year, in tens of millions of rupees (so the smallest newly eligible firms have a value of 1 for assets, and the largest have a value of 5). *** denotes statistical significance at the 1% level, ** at 5%, and * at 10%. Source: ASI

Appendix Table 6: Within-Firm Correlation of Productivity Measures Over Time

	Correlation of $measure_{jt}$ and $measure_{j(t-1)}$	
	raw data	trimmed & logs
	(1)	**(2)**
TFPQ	0.3501	0.7315
TFPR	0.2101	0.5362
capital wedge	0.4426	0.7787
labor wedge	0.1902	0.6178

Notes: This table presents the correlation for each firm of their estimated productivities and distortions over time. For each firm, I calculate the measure following Hsieh and Klenow (2009), as outlined in the text, and then correlate the measure for each firm in t and t-1. I include all possible pairs of firms, both before and after the policy change. Probability weights were not used.