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Equilibrium Effects of Firm Subsidies†

By Martin Rotemberg*

Subsidy programs have two countervailing effects on firms: direct gains for eligible firms and indirect losses for those whose competitors are eligible. In 2006, India changed the eligibility criteria for small-firm subsidies, and the sales of newly eligible firms grew by roughly 35 percent. Competitors of the newly eligible firms were affected, with almost complete crowd-out within products that were less internationally traded, but little crowd-out for more-traded products. The newly eligible firms had relatively high marginal products, so relaxing the eligibility criteria for subsidies increased aggregate productivity by around 1–2 percent. Targeting different firms could have led to similar gains. (*JEL* D22, D24, H25, L25, L52, L60, O14)

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 the overarching goals of increasing aggregate output and productivity,[2] and can benefit targeted firms substantially. However, the effects 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 firms producing globally traded goods are likely to have different effects on their (domestic) competitors than firms who are competing in small local markets.

In this paper, I study small firm (priority sector) subsidies in India, leveraging a 2006 policy change that relaxed the eligibility requirements for a variety of

* Economics Department, New York University, 19 West 4th Street, New York, NY 10012 (email: mrotemberg@nyu.edu). Penny Goldberg was the coeditor for this article. I am especially grateful to my advisors Shawn Cole, Rick Hornbeck, Michael Kremer, and Rohini Pande for their advice and encouragement, and to the editor and referees for their detailed comments. This project greatly benefited from helpful discussions with Natalie Bau, Dan Bjorkegren, Pallavi Chavan, Abhiman Das, Rafael Di Tella, Mike Egesdal, Joan Farre-Mensa, James Feigenbaum, Siddarth George, Ben Hebert, Bill Kerr, Pete Klenow, Asim Khwaja, Sara Lowes, Brian McCaig, Marc Melitz, Virgiliu Midrigan, Eduardo Montero, Nathan Nunn, Steve O'Connell, Mikkel Plagborg-Mller, Ariel Pakes, N. R. Prabhala, Raghuram Rajan, Tristan Reed, Julio Rotemberg, Alex Roth, Frank Schilbach, Alex Segura, Andrei Shleifer, Bryce Millett Steinberg, Andrew Weiss, T. Kirk White, and Jack Willis, as well as various seminar participants. Hunt Alcott, John Baldisserotto, D. R. Dagar, and Richard Lesage generously answered my questions about the data. I am grateful to the National Science Foundation for a generous graduate research fellowship, as well as funding from a Pellegrini grant. All errors are my own.

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[1] For instance, each of the G8 countries have state-backed institutions designed to support small firms. 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.

[2] For example, this is the motivation for a recent "Call for Innovative Ideas on SME Growth and Entrepreneurship" from the World Bank, available at http://goo.gl/SQ4kOR (accessed November 4, 2014).

government programs.[3] The newly eligible firms represented around 15 percent 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 to the policy change.

The aggregate effects of these types of programs will depend on the marginal products of the firms who are directly subsidized and the firms who are indirectly affected through product market competition. 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), which implies that there can be aggregate gains from reallocating output. Programs supporting small firms may be second-best solutions to preexisting distortions, such as those in credit markets (Banerjee and Duflo 2014). However, if the eligible firms are less productive than their competitors on the margin, then these types of programs may be the cause of the misallocation of productive factors. The effect that these types of programs have on productivity depends on if the firms directly caused to grow have higher marginal products than their competitors who are indirectly caused to shrink. In this context, ignoring spillovers increases the measured gains from the program by a factor of three.

The direct effects of the policy can be estimated using relatively standard tools. Those tools are not immediately applicable for understanding indirect exposure, since it is not explicitly recorded in firm surveys. I develop a Melitz-style framework with multi-product firms to translate what firms do report (their product mix) into an estimating equation for how subsidizing some firms can directly and indirectly affect all firms' inputs and outputs. The framework generates an intuitive and simple prediction: the indirect effect on each firm will be a weighted average of the program's direct effects. A firm's indirect exposure to the program is a function of (i) the product mix of that firm; (ii) the share of each product produced by newly subsidized firms; and (iii) the products' characteristics, such as where the products are made or sold. A commonly used alternative to products produced (often for data constraints) is measuring overlap using firms' self-reported industry. However, even within relatively narrowly-defined industries, firms in the same industry often produce different products, and firms producing the same product are often in different industries. The correlation of the exposure measure using products versus industries is only around 0.3–0.4, depending on the level of industry aggregation (although for some aggregations the estimated indirect effects are similar).

The model predicts neither the sign nor the magnitude of the spillovers: depending on the values of the parameters, it is consistent with a range of equilibrium effects including complete crowd-out and agglomeration. Understanding the aggregate effects of the eligibility expansion therefore requires an empirical analysis.

The empirical analysis is at the firm level, but the structure of the model allows me to use the estimates to calculate aggregate effects of the program: the estimated indirect effect is a sufficient statistic for the elasticity of aggregate growth with respect to private growth. Using data that are representative of all manufacturing

[3] Historically, there have been strict policies regulating firms' ability to produce certain products in certain locations (see Panagariya 2008 and Chari 2011 for further discussion of the history of industrial licensing in India).

activity in India, I leverage variation in time and firm characteristics to separately identify the direct and indirect effects of the policy change.[4]

I find that newly eligible firms increased their sales by around 35 percent. Although both the timing and the eligibility criteria of the policy are not random, I find that the newly eligible firms behaved similarly to their peers before the policy change. The magnitude and lack of pre-trends are in line with Banerjee and Duflo (2014); Sharma (2005); and Kapoor, Ranjan, and Raychaudhuri (2017), who study earlier eligibility changes for a similar set of programs in India.

There were large indirect effects, with around two-thirds of subsidized firms' growth coming at the expense of their within-state competitors. However, for traded products, there were no negative competitive effects and the estimates are consistent with positive spillovers. This result supports the argument that local demand shocks will have a limited effect on local production of traded goods (Matsuyama 1992, Mian and Sufi 2014). For non-traded products, the direct output increases caused by the subsidy programs were completely counteracted by the indirect effects.

Although nontrivial to measure, the mere fact of equilibrium effects is not surprising (McKenzie and Woodruff 2014). The ultimate effect on aggregate productivity, however, is ex ante ambiguous. I estimate that the expansion of the priority sector increased aggregate productivity in manufacturing by around 1 percent. The measured gains come from reallocation: I find no evidence that the program increases firm TFPQ either directly or indirectly. However, there were many firms with similarly high marginal products: had the government randomly targeted firms, my estimates suggest that the aggregate productivity gains would have been at least as large around 20 percent of the time.

The papers most similar to mine study the direct effect of firms' access to credit and capital (Banerjee et al. 2015) and the policy effects of programs which differentially favor small firms (Birch 1979; Haltiwanger, Jarmin, and Miranda 2013; Brown and Earle 2017).[5] A related literature studies inter-firm spillovers due to trade shocks (Sivadasan 2006; De Loecker et al. 2016; Bollard, Klenow, and Sharma 2013), FDI (Aitken and Harrison 1999), and research and development (Jaffe 1986; Bloom, Schankerman, and Van Reenen 2013). Acemoglu et al. (2012) discuss how industry-specific shocks affect the economy as a whole through input-output networks.[6] My model for understanding how firms compete with each other within each

[4] My framework abstracts from other potential general equilibrium effects. For instance, three possible sources of these other effects are (i) firms distorting their size in order to maintain eligibility, (ii) the policy change affecting the prices paid by firms whose eligibility status was unchanged, and (iii) the costs of raising revenue to pay for the subsidies affecting the economy as a whole. In online Appendix Table 1, I find little evidence of distortions in the firm-size distribution around the cutoff. In Table 4, 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 state/year and industry/year in order to control for general equilibrium effects. Furthermore, given the structure of the Indian economy, it is unlikely that a policy that affects a small part of the formal manufacturing sector will have a large effect on wages (Lewis 1954).

[5] A series of experiments has found mixed evidence for competitive spillovers in retail trade in developing countries (de Mel, McKenzie, and Woodruff 2008; Busso and Galiani 2019; McKenzie and Puerto 2017).

[6] More broadly, a recent series of papers have discussed how Hulten's (1978) theorem may break down in the presence of frictions and linkages (Bigio and La'O 2017; Baqaee and Farhi 2019; Liu 2019). My work complements this literature by studying within-sector effects. I follow Hulten's (1978) approach as an *accounting* identity, but use structure to argue that even within that setup an underlying shock to one firm can have first-order consequences to the behavior of other firms and therefore on aggregate productivity growth if frictions cause the envelope theorem not to hold.

sector generates similar predictions to those used for understanding cross-industry spillovers. More broadly, this project is in the spirit of Abbring and Heckman (2007), who argue that finding that a program that has a large direct effect motivates testing its equilibrium effects.

I. Institutional Background

The Indian government has had a ministry dedicated to supporting small-scale enterprises since 1954. In the model section I describe the bundle of programs it runs as potentially lowering input costs (for labor, capital, and materials) and increasing productivity. In this section, I describe the history of the ministry and its largest programs.

Eligibility for the programs is exclusively determined by a cutoff for an establishment's nominal accumulated capital investment.[7] Eligibility is at the *establishment* level, so a multi-plant firm can have both eligible and ineligible plants. At first, only establishments with under 500,000 rupees in fixed assets were eligible. The fixed asset cutoff has changed roughly every six years (shown in Figure 1), although most of the policy changes until the late 1990s were implemented in order to keep pace with inflation. Banerjee and Duflo (2014); Sharma (2005); and Kapoor, Ranjan, and Raychaudhuri (2017) study a 1999 policy change, which created the Ministry of Small Scale Industries and Agro and Rural Industries and lowered the eligibility criteria. 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 that year. At the time, manufacturing establishments with a value of under 10 million rupees in nominal investment in plants and machinery were eligible.

With the passage of the Micro, Small, and Medium Enterprises Development Act of 2006 (the "Act"),[8] the federal government's small firm programming was reconsolidated into the Ministry of Micro, Small, and Medium Enterprises (MSME). The Act raised the size cutoff to 50 million rupees and introduced several new programs, including one to help small firms get timely payments. At the time, newly eligible establishments represented around 15 percent of all formal manufacturing output, and the majority of firms had competitors whose eligibility status changed.

Eligible establishments have access to a wide variety of programs run by the MSME.[9] Several programs promote employment generation through training and worker subsidies (for instance, for hiring members of "special categories" such as scheduled caste/scheduled tribe), which together make up around 20 percent of the MSME's budget (which is over \$100 million). The only larger program category is credit guarantees and support (around 70 percent of the budget) with programs that help firms both with short-term and long-term loans. Other programs include

[7] Some programs are additionally targeted at "Traditional Industries" such as handicrafts.

[8] The first version of the bill was introduced in May 2005 and it was passed with few changes in May 2006. I have not seen any evidence that the bill was introduced in response to specific new demands from the firms who would become newly eligible.

[9] The 2017–2018 Annual Report of the Ministry describes 22 major schemes (see https://msme.gov.in/sites/default/files/MSME-AR-2017-18-Eng.pdf).

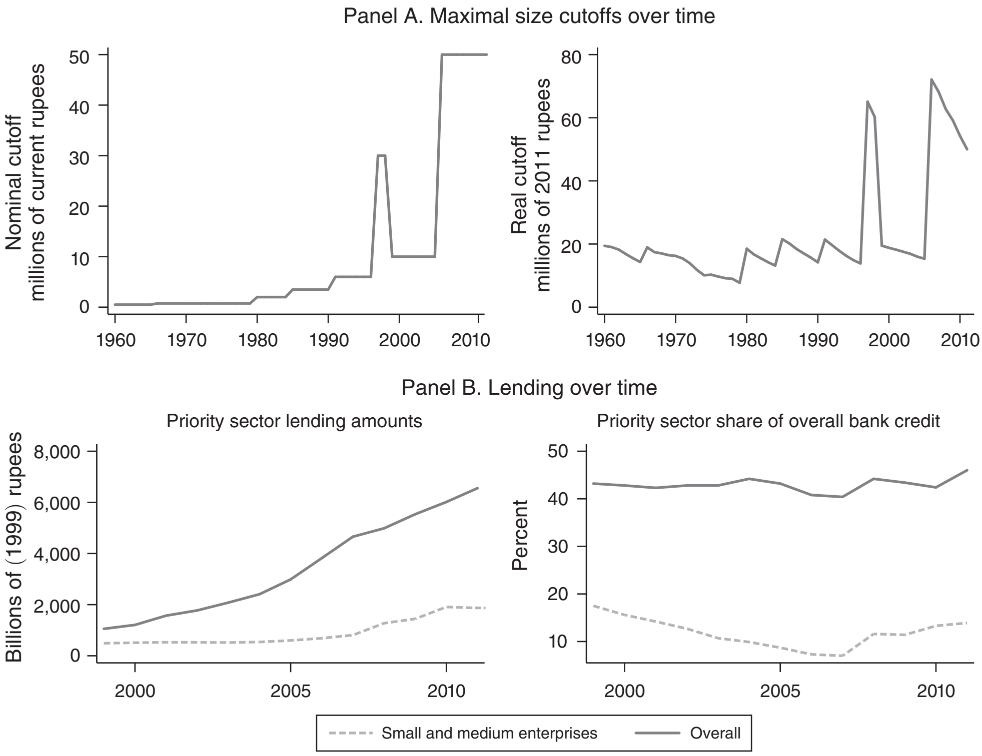

Figure 1. Small Firm Subsidies in India

Notes: Panel A: plot of the change in the eligibility requirements for small firm benefits in India. For the left panel, all establishments whose (nominal) stock of plants and machinery are below the line are eligible. The right panel deflates the cutoffs using the GDP deflator from the WDI. The manufacturing data in this paper cover 2001–2011, which is after the first spike and covering the second. Panel B: plot of total value and the share of overall bank credit to Priority Sector borrowers, and to Micro, Small, and Medium Enterprise (MSE) borrowers.

Sources: Various Reserve Bank of India circulars. Reserve Bank of India Statistical Tables Relating to Banks in India.

trainings for quality and safety control, managerial development, and marketing assistance (Ministry of Micro, Small and Medium Enterprises 2011).

Other government programs also support small firms, such as preferential procurement from local governments. Furthermore, the Reserve Bank of India manages "Priority Sector Lending," which directs banks to provide 40 percent of their loan portfolios to "small" clients at fair rates.[10]

Historically, India has also strictly regulated the production of certain products (such as plastic buttons) by firms with assets above a cutoff, a regime known as the Small Scale Reservation laws (Mohan 2002, Tewari and Wilde 2017). Martin, Nataraj, and Harrison (2017) and Balasundharam (2019) study the direct and indirect effects of removing this regulation, which has happened sporadically over the

[10] The targets are considered binding (Nathan 2013). Foreign-owned banks with fewer than 20 branches only need 32 percent of their portfolio in the Priority Sector establishments. Around one-half of total Priority Sector lending is to manufacturing firms.

last 20 years. The eligibility cutoff for the remaining products also changed in 2006, and my results are similar if I drop firms producing reserved products (by 2000, reserved products represented a relatively small share of overall formal manufacturing output).

II. Analytical Framework

I define aggregate productivity as aggregate output minus aggregate input costs (Solow 1957, Hulten 1978, Basu and Fernald 2002, Petrin and Sivadasan 2013), what Jorgenson and Griliches (1967) described as the "conventional" definition. Mechanically, aggregate productivity is a function of firm inputs and outputs. In order to understand the effect of the policy change on aggregate productivity, I need a measure of how each firm's inputs and outputs were affected, both for the firms that were directly targeted by the policy change as well as firms that were potentially indirectly affected. While the tools for identifying the direct effects of these types of programs are well established, a model is needed to measure how competition leads to spillovers, particularly since establishments produce multiple products with multiple characteristics.

I develop estimating equations using a model of consumer behavior to predict how firms could be affected by the program. In particular, I develop a partial-equilibrium model with heterogeneous firms (Hopenhayn 1992, Melitz 2003) that produce multiple products (Bernard, Redding, and Schott 2011), with firm-specific distortions on the cost of capital and labor (Hsieh and Klenow 2009—henceforth, HK).

With additional assumptions (those needed for estimating production functions), aggregate productivity growth can be further decomposed into (i) a term capturing within-firm productivity improvements and (ii) reallocation between firms with potentially different marginal products. The framework captures these forces as well: I model the program as potentially affecting both establishment productivity and distortions that may prevent firms from equating their marginal products with their marginal costs. A growing literature seeks to micro-found these distortions, such as markups or credit constraints (Buera, Kaboski, and Shin 2011; Peters 2018); in this paper I focus on policies which *change* preexisting distortions.

A. *Aggregate Productivity Growth and Firm Behavior*

In this subsection, I describe the aggregate productivity growth decomposition from Petrin and Levinsohn (2012). By definition, the change in aggregate productivity is the difference between (the changes in) output and input costs:

$$dAP \equiv \sum_{j=1}^{N} p_j dY_j - \sum_{j=1}^{N} \sum_{Input \in K,M,L} p_{Input} dInput_j,$$

where Y_j is the gross output of firm j, and the ps denote the firm's real prices.

Given the repeated-cross sectional sampling frame, only around one-third of establishments surveyed in a given year are also surveyed in the subsequent year (there is a time-consistent firm identifier), making it difficult to fully characterize the contribution of entry and exit to aggregate productivity growth (APG). Setting aside entry and exit, some algebra (Petrin and Levinsohn 2012) yields a decomposition of APG into a weighted sum of firm-level growth rates,

$$APG = \sum_{j=1}^{N} D_j \left[p_j d\ln Y_j - \sum_{Input \in K,M,L} s_{Input_j} d\ln Input_j \right], \tag{1}$$

where s_{Input_j} is the input's firm-level revenue share, and $D_j \equiv y_j / \sum_{j'} VA_{j'}$ is the firm's Domar (1961) weight. Equation (1) corresponds exactly to changes in

$$\text{“}AP\text{”} = \sum_{j=1}^{N} D_j \left[p_j \ln Y_j - \sum_{Input \in K,M,L} s_{Input_j} \ln Input_j \right]. \tag{2}$$

The Domar weights and revenue shares can be calculated directly in the data. How firm output and inputs are affected by the program is derived using the framework developed in the next subsection. For firms that report being closed, I impute 0 for ln output and inputs, since those firms are not contributing to aggregate productivity; for notational convenience I describe the estimated coefficients as percent changes.

Estimating the effect of the program on aggregate productivity growth does not require estimating a production function, which is difficult given the data.[11] The value of estimating production functions is that APG can then be decomposed into two components, a "technical efficiency" term capturing within-firm productivity improvements, and a "reallocation" term, which can appear whenever a firm's input use changes and the marginal product of that input (measured using its production function elasticity) is not equal to its marginal cost (measured with its revenue share as in Hall 1988):

$$APG = \sum_j \left(D_j d\ln A_j \right) + \sum_{j=1}^{N} \sum_{Input \in K,M,L} D_j \left[\sum_k \left(\alpha_{Input_j} - s_{Input_j} \right) d\ln Input_j \right], \tag{3}$$

where A_j is the firm's productivity and α_{Input_j} is the firm-specific elasticity of output with respect to the input. Each firm/input's "gap," $\left(\alpha_{Input_j} - s_{Input_j} \right)$, is a measure of the marginal product for that firm's input: the marginal change in output minus the marginal change in cost. Nishida et al. (2017) estimate that average productivity growth was around 6.7 percent a year in India over the period studied in this paper, primarily driven by reallocation.

Motivated by equation (2), in the next subsection I derive how a targeted subsidy program can affect firms' inputs and outputs. In partial equilibrium, the subsidies affect firm productivity and lower firm/input-specific distortions, which in turn affect both that input's revenue share (which increases) and usage (which also increases). In equilibrium, this behavioral change may also affect other firms through competitive forces, causing them to shrink. In the next subsection, I describe a simple version of the model, and in the subsequent subsections show that the intuition of the model carries through when including more realistic features such as multi-plant firms, tradable products, and transportation costs.

[11] Heterogeneous (time-varying) frictions and multi-product firms each introduce challenges for estimating production functions: see De Loecker et al. (2016), Shenoy (2018), and Orr (2018) for potential solutions.

B. *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 and the growth of subsidies. I assume that in each sector, a single 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 $\phi > 0$ and c is consumption of the outside good. The post-tax income of the consumer is assumed to be I.[12] The first-order condition of the final-good consumer ensures that the revenue in sector s will be

$$Y_s = P_s Q_s = \left(\frac{P_s}{\phi}\right)^{\frac{\phi}{\phi-1}}. \tag{4}$$

In each sector, this firm combines the output q_{js} of each of the N intermediate goods producers with a constant elasticity of substitution (CES) production function, $Q_s = \left(\sum_{j=1}^{N} q_{js}^{\frac{\sigma-1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}}$. The final good producers' profit-maximizing ensures that the price of the final good in each sector P_s will be the a CES aggregator of the intermediate goods producers' prices:

$$P_s = \left(\sum_{j=1}^{N} p_{js}^{1-\sigma}\right)^{\frac{1}{1-\sigma}}. \tag{5}$$

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

$$q_{js} = A_j K_j^{\alpha_{K_j}} L_{js}^{\alpha_{L_j}} M_{js}^{\alpha_{M_j}}, \tag{6}$$

where A_{js} is firm/sector-specific total factory productivity (TFP). The Cobb-Douglas assumption is more restrictive than more flexible approaches such as translog, but makes the aggregation to multi-product firms substantially more straightforward.

In the spirit of HK, I allow for distortions which change the marginal products of capital $\left(\tau_{K_j}\right)$, labor $\left(\tau_{L_j}\right)$, and materials $\left(\tau_{M_j}\right)$ for each firm. These distortions reflect frictions which prevent firms from equalizing marginal costs and marginal products, such as credit constraints. I normalize a potential "output wedge" to 1 to maintain similarity to equation (1); focusing the notation on inputs instead of output wedges is without loss of generality, as HK discuss (the four potential wedges are collinear).

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

$$\pi_{js} = p_{js} q_{js} - \sum_{Input \in K,M,L} \left(1 + \tau_{Input_j}\right) p_{Input} Input_{js},$$

[12] While the highest utility nest is often assumed to be Cobb-Douglas, I avoid this choice since it would imply that total revenue for the final good producer in sector s is 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. In online Appendix Section 1, 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 homogeneous output in each sector, and (ii) when the consumer has CES preferences over the final goods.

where p_{Input} reflect the price of that factor of production. I assume firms take the price index as given, so profit maximization implies a constant markup over the firm's marginal cost:

$$p_{js} = \frac{\sigma}{\sigma-1}\left(\prod_{Input \in K,M,L}\left(\frac{p_{Input}}{\alpha_{Input_j}}\right)^{\alpha_{Input_j}}\right)\frac{\left(\prod_{Input \in K,M,L}\left(1+\tau_{Input_j}\right)^{\alpha_{Input_j}}\right)}{A_{js}}. \tag{7}$$

If the firm reports input costs exclusive of the distortions, then

$$s_{Input_j} = \alpha_{Input_j}\frac{1}{\left(1+\tau_{Input_j}\right)}. \tag{8}$$

Revenue for each intermediate good producer will be a function of (i) their own price, (ii) the prices of their competitors in the sector, and (iii) total revenue in the sector:

$$y_{js} = p_{js}q_{js} = \left(p_{js}^{1-\sigma}\right)\cdot\left(P_s^{\sigma-1}\right)\cdot\left(\frac{P_s}{\phi}\right)^{\frac{\phi}{\phi-1}}. \tag{9}$$

Firm size is determined by a mix of each firm's wedges and underlying productivity: both increasing productivity and decreasing the wedges will increase firm size. Holding P_s fixed, and combining equations (7) and (9), the growth of firm size with respect to productivity and the input distortions is $\partial \ln\left(y_{js}\right)/\partial A_{js} = \sigma - 1$ and $\partial \ln\left(y_{js}\right)/\partial \tau_{Input_j} = \alpha_{Input_j}(1-\sigma)$. In the following subsection, I show how a firm's size changes as a function of all firms' subsidies (I use the term subsidies loosely here, as firm productivity may also be affected by eligibility).

C. *The Effect of Changing Subsidies*

In this subsection, I build on equation (9) to derive equations relating firm growth to increasing subsidies. I assume throughout that expanding the set of eligible firms will (i) directly change those firms' relative prices of inputs and (ii) potentially increase their technical efficiency. Any other effects on firms are due to the changes in the price index (for instance, I assume that ineligible firms do not experience a change in input prices). While this is a strong assumption, the subsequent sections provide two empirical justifications for the important assumption that the Priority Sector only directly affects eligible firms. First, in all regressions I include fixed effects for each industry/year and state/year, which absorbs common changes in local and industrial distortions due to the program change (such as changes in local wages and in the interest rate). Second, in Table 4, I find evidence that newly eligible firms behave as if their relative input prices changed, but no evidence that their competitors do as well. In order to rationalize errors in the regression, I assume that the growth of firm productivity is

$$\widehat{A_{js}} = \xi_{js} - \epsilon_{js}, \tag{10}$$

where ϵ_{js} is mean-zero and normally distributed, and potentially autocorrelated within a firm or industry; ξ_{js} is the potential direct effect that program eligibility has on firm TFPQ, and is only nonzero if a firm's access to the program changes.

Combining equations (5), (7), (9), and (10), the change in each firm's revenue as a function of the changing wedges is

$$(11)\quad \hat{y}_{js} = (1-\sigma)\Bigg(\sum_{Input\in K,M,L}\alpha_{Input_j}\big(\widehat{1+\tau_{Input_j}}\big) - \xi_{js} + \epsilon_{js}\Bigg) + \left(\sigma - \frac{1}{1-\phi}\right)\sum_{j'=1}^{N_s}\left[\Bigg(\sum_{Input\in K,M,L}\alpha_{Input_{j'}}\big(\widehat{1+\tau_{Input_{j'}}}\big) - \xi_{j's} + \epsilon_{j's}\Bigg)\frac{y_{j's}}{Y_s}\right].$$

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 preexisting 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. A (naïve) estimate of the direct effects violates the stable-unit-treatment-value assumption typically needed for comparing a treatment and control group (Rubin 2005). This is because the potential outcomes for firm j are not stable as the treatment status of other firms change. However, the model generates an estimating equation which does satisfy stable unit treatment value assumption (SUTVA); while firms are not indifferent to their competitors gaining access, conditional on the share of the competition with access firms are indifferent as to which competitors get access.[13] Aggregating over all of the firms in each sector gives

$$(12)\quad \hat{Y}_s = \left(1 - \frac{\sigma - \frac{1}{1-\phi}}{\sigma - 1}\right)(\sigma-1)\sum_{j=1}^{N_s}\left[-\Bigg(\sum_{Input\in K,M,L}\alpha_{Input_j}\big(\widehat{1+\tau_{Input_j}}\big) - \xi_{js} + \epsilon_{js}\Bigg)\frac{y_{js}}{Y_s}\right].$$

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

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_js. Furthermore, I define $\mu_s \equiv \left(\sum_{j=1}^{N_s} e_j \times y_{js}\right)/Y_s$, $\theta \equiv \left(\sigma - \frac{1}{1-\phi}\right)/(\sigma-1)$, and

$$\beta \equiv (1-\sigma)\Bigg(\Bigg(\sum_{Input\in K,M,L}\alpha_{Input}\big(\widehat{1+\tau_{Input}}\big)\Bigg) - \xi\Bigg),$$

where β reflects the private growth from the program,[14] θ the extent of crowd-out from that growth,[15] and μ_s the share of output in sector s produced in newly subsidized firms. With this notation, we can condense equation (12):

$$(13)\qquad \hat{Y}_s = \beta\mu_s - \theta\beta\mu_s + \theta\beta\sum_{j=1}^{N_s}\epsilon_{js}\frac{y_{js}}{Y_s} \equiv (1-\theta)\beta\mu_s + \epsilon_s.$$

[13] Hudgens and Halloran (2008) define this property as *stratified interference*. Kosová (2010) and Kovak (2013) present models with similar predictions. Note that the CES structure imposes the strong assumption of perfect pass-through, which is likely not generically true across sectors (Weyl and Fabinger 2013, Casaburi and Reed 2017). However, the model's predictions on competitive spillovers are not sensitive to this, as (i) the direct effect of subsidies is a function of how much targeted firms lower their price, regardless of their underlying change in costs, and (ii) the indirect effect is a function only of that change in price.

[14] A constant β requires assuming all firms to have the same production function elasticties and the same productivity gain across sectors, which isn't needed for the theory (such as in equation (12)) but does keep the notation going forward substantially cleaner.

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

Aggregate growth in a sector due to the subsidies 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 (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 expected elasticity of aggregate growth with respect to private growth is $\hat{Y}_s/\hat{Y}_{ps} = (1-\theta)$. 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. As ϕ increases, the indirect effect shrinks, such that there is no indirect effect if $\phi = (\sigma-1)/\sigma$, and positive spillovers if ϕ is larger. While in the model ϕ represents preferences, in the data positive spillovers could also reflect agglomeration spillovers on the production side, and I am not able to distinguish the two. Furthermore, as σ_s increases (the good becomes more substitutable), the direct effect increases.

D. *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 (11) to account for firms that are affected through multiple products. I assume that the production function in equation (6) holds for firm j in *each* sector s in which it produces (Bernard, Redding, and Schott 2010). Defining $\omega_{js} = y_{js}/y_j$ as the share of firm j's revenue in sector s, a multi-product firm's growth after the subsidy program is

$$\hat{y}_j = \beta e_j - \theta\beta\left(\sum_{s=1}^{S}\omega_{js}\cdot\mu_s\right) + \left(\sum_{s=1}^{S}\omega_{js}\cdot\left(\epsilon_{js} - \theta\beta\epsilon_s\right)\right). \tag{14}$$

As in equation (11), each firm's growth after the introduction of subsidies can be linearly decomposed into a direct effect (β if the firm is newly eligible), 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), and a mean-zero normally distributed disturbance term. The primary structure of the empirical analysis will be to estimate the effect on a firm's revenue on (i) if its eligibility status changed and (ii) the weighted-average share of its competitors that gained eligibility.

Firm Productivity.—With multi-product firms (with potentially product-specific productivity), firm-TFPQ can suffer from well-known aggregation biases (Leontief 1947, Felipe and Fisher 2003). However, the assumptions thus far—Cobb-Douglas production functions, and constant (i) within-firm production function elasticities, (ii) distortions, and (iii) input prices—allow us to aggregate firm/sector TFPQ to an overall firm measure. Firm production is $q_{js} = A_{js}K_{js}^{\alpha_{K_j}}L_{js}^{\alpha_{L_j}}M_{js}^{\alpha_{M_j}}$, and optimization implies that inputs are allocated according to their revenue share (for instance, $K_{js} = \omega_{js}\cdot K_j$). We can therefore drop some subscripts,

$$q_{js} = A_{js}K_j^{\alpha_{K_j}}L_j^{\alpha_{L_j}}M_j^{\alpha_{M_j}}\omega_{js}^{\alpha_{K_j}+\alpha_{L_j}+\alpha_{M_j}}, \tag{15}$$

and adding up gives

$$\sum_s q_{js} = \left[\left(\sum_s \left(A_{js}\omega_{js}^{\alpha_{K_j}+\alpha_{L_j}+\alpha_{M_j}}\right)\right)\right] K_j^{\alpha_{K_j}} L_j^{\alpha_{L_j}} M_j^{\alpha_{M_j}}.$$

Defining Q_j as the sum of product quantities and $A_j = \sum_s \left(A_{js}\omega_{js}^{\alpha_{K_j}+\alpha_{L_j}+\alpha_{M_j}}\right)$ as a weighted average of product TFPQ, the overall firm production function is

$$Q_j = A_j K_j^{\alpha_{K_j}} L_j^{\alpha_{L_j}} M_j^{\alpha_{M_j}}. \tag{16}$$

With constant returns to scale, firm TFPQ is exactly the revenue-weighted average of firm/sector TFPQ (as returns to scale increase, the weights larger sectors increase). Measured changes in overall firm productivity captures a combination of some products' productivity changing and production reallocating to relatively more (or less) productive sectors.

Trade and 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 worldwide output, not just output in India.[16] To account for the dampening of the competitive effect for traded products, define $x_s = 1$ if production in sector s is traded internationally, θ^d as the competitive effect in sectors where products are produced and sold domestically, and θ^x as difference in the competitive effect in the more-traded versus less-traded sectors. Equation (14) extends naturally to include the effects of trade:

$$\hat{y}_j = \beta e_j - \theta^d \beta \left(\sum_{s=1}^{S} \omega_{js} \cdot \mu_s\right) - \theta^x \beta \left(\sum_{s=1}^{S} \omega_{js} \cdot \mu_s \cdot x_s\right) + \epsilon_{js}^x, \tag{17}$$

where $\epsilon_{js}^x = \left(\sum_{s=1}^{S} \omega_{js} \cdot \left(\epsilon_{js} - \beta\left(\theta^d + \theta^x x_s\right)\epsilon_s\right)\right)$. A similar logic applies when there are many relevant sector characteristics.

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, Van Leemput 2016). A difficulty with testing this assumption is that firms rarely report the location of their sales.

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. A potential test of the separability of state markets is if the indirect effects of subsidies vary across state lines. If states are relatively independent markets, then a firm's growth will crowd out its within-state competitors but not significantly

[16] If firms compete on a product which is sold on international markets, then a large (by Indian market standards) policy shock may 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 high (sectoral) elasticity of substitution ϕ, since decreases in the price of one firm in the sector leads to a large overall increase in sales in the sector.

affect producers located outside its state. 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 $\mu_{js}^{\varsigma} \equiv \left(\sum_{j=1}^{N_s} \varsigma_{jk} \times e_j \times y_{js}\right) / \left(\sum_{k=1}^{N_s} \varsigma_{jk} \times y_{ks}\right)$ as the share of within-state competition in sector s for firm j, and $\mu_{js}^{o} \equiv \left(\sum_{j=1}^{N_s} \left(1 - \varsigma_{jk}\right) \times e_j \times y_{js}\right) / \left(\sum_{k=1}^{N_s} \left(1 - \varsigma_{jk}\right) \times y_{ks}\right)$ as the non-state share. Including the geography of sales adjusts equation (14) to

$$(18) \qquad \hat{y}_j = \beta e_j - \theta^{\varsigma} \beta \left(\sum_{s=1}^{S} \omega_{js} \cdot \mu_{js}^{\varsigma} \right) - \theta^{o} \beta \left(\sum_{s=1}^{S} \omega_{js} \cdot \mu_{js}^{o} \right) + \epsilon_{js}^{o},$$

where

$$\epsilon_{js}^{o} = \left(\sum_{s=1}^{S} \omega_{js} \cdot \left(\epsilon_{js} - \beta \left(\theta^{o} \frac{\sum_{j=1}^{N_s} \left(1 - \varsigma_{jk}\right) \times e_j \times y_{js}}{\sum_{k=1}^{N_s} \left(1 - \varsigma_{jk}\right) \times y_{ks}} + \theta^{\varsigma} \frac{\sum_{j=1}^{N_s} \varsigma_{jk} \times e_j \times y_{js}}{\sum_{k=1}^{N_s} \varsigma_{jk} \times y_{ks}} \right) \right) \right).$$

This is similar to equation (17), 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. Since products that are more likely to be traded on international markets are also more likely to be traded across state lines, I also consider how international trade mediates the effect of geography.

III. Data and Identification Strategy

The empirical analysis relies on the 2001–2010 Annual Surveys of Industries of India (ASI), which is produced by the Ministry of Planning and Statistics (MOPSI). The ASI sampling frame is representative of formal establishments, stratified by state by 4-digit industry. The sampling frame is designed as follows: large establishments, which are those with 200 or more workers until 2003–2004, and 100 or more after 2004, are always surveyed (with about 10 percent non-reporting each year). Smaller establishments are surveyed with a probability which depends on their specific state and industry, with a minimum sampling probability of 15 percent.[17] MOPSI has recently allowed researchers to track establishments that were sampled multiple times, in what is known as the "Panel" version of the ASI.

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 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. While the ASI contains very little information about each establishment's parent firm, most establishments are the only plant in their firm. Eligibility for all of the "small" firm programs in India are at the

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

Table 1—Summary Statistics

	Unweighted		Regression coefficient on "small"	
	Mean	SE	β	SE
Survey weight	3.17	0.010	−0.61	0.024
Number of times in survey	4.65	0.014	0.84	0.034
"Small"	0.15	0.002		
ln real output	5.59	0.009	1.24	0.021
ln real wages	11.29	0.012	1.98	0.027
ln real (flow) capital	12.70	0.011	1.56	0.026
ln real intermediates	13.22	0.010	1.53	0.026
ln real total input costs	13.06	0.010	1.64	0.024
Labor revenue share	0.10	0.001	−0.03	0.002
Capital revenue share	0.04	0.000	0.01	0.001
Intermediates revenue share	0.71	0.001	0.00	0.003
Total inputs revenue share	0.86	0.001	−0.02	0.003
In-state exposure	0.16	0.001	0.27	0.003
In-state traded exposure	0.04	0.001	0.07	0.002
Outside-state exposure	0.05	0.001	0.01	0.001
Share of output traded	0.26	0.002	−0.01	0.005

Notes: Summary statistics for all factories in ASI data. Sample is firms who appear in the data both before and after the policy change (and who reported assets before the policy change). Values are for firms in the year that they were categorized to small or not (before 2006), and real values are in 2004 dollars.
The survey weight is the inverse sampling probability. Capital flow costs are imputed using capital stocks (and rentals) following Nishida et al. (2017), and total input costs are the sum of the wage bill, intermediates, and capital flow. The construction of the output exposure measures is discussed in Section IV, 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."

establishment level, although interviews suggest that there has been some confusion on this point. The summary statistics for firms in their final pre-program year are in Table 1, and Figure 2 shows the distribution of eligible firms over India.

I augment the ASI with the 2006 round of the National Sample Survey Organization's data on unorganized manufacturing establishments (NSS), which are explicitly the non-ASI firms in India.[18] The NSS is designed to be a representative cross section of informal firms, and combining the NSS and the ASI allows for a representative sample of all manufacturing activity in India.[19]

Unlike the ASI, the NSS is only undertaken every five years, and establishments cannot be tracked over time. As a result, I use the information in the NSS to measure exposure to the policy change, but not to understand the effects of the policy change. While informal firms represent an enormous share of manufacturing establishments in India (around 99 percent), their shares of employment (80 percent) and revenue (16 percent) are lower. The results do not change dramatically when calculating exposure to the policy change while ignoring informal firms: for the

[18] The dataset is the NSS round 62, schedule 2.2.

[19] Several other projects have combined the datasets, such as Nataraj (2011) and Ghani, Kerr, and Segura (2014), or done something similar in other contexts such as Brazil (Dix-Carneiro and Kovak 2019).

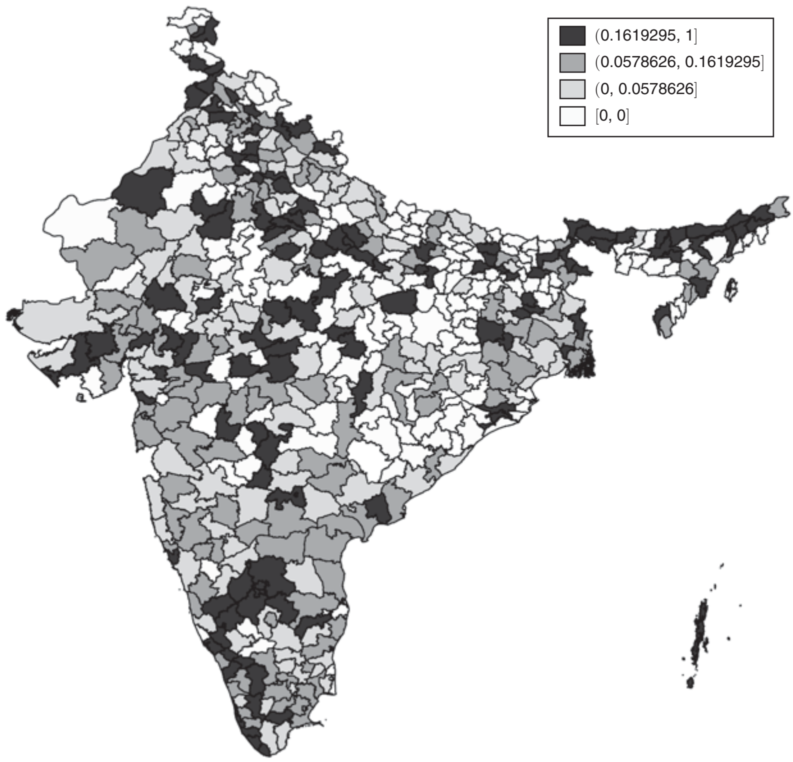

Figure 2. Distribution of Small Firms across India

Notes: This figure plots the share of gross output at "small" formal firms in each district of India for which there are data in the ASI. Firms are counted in their final pre-program year.

sales-weighted-average product produced by formal firms, only 3 percent of production comes from the informal sector.

Since a primary goal of the paper is estimating the effect of small firm subsidies on aggregate productivity (as in equation (1)), the primary outcomes of interest are the ones that matter for aggregate productivity: sales and the costs of labor, materials, and capital (and if a plant continues to exist). I also show results for imputed total flow costs of the firm, following Nishida et al. (2017) and imputing the flow value of capital as equal to $0.15 \times$ fixed assets.[20,21] Firms also report quantities and

[20] 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 that are marked as having exited report positive assets, sales, and employment both for the year that they "exited" and in subsequent years. As in Martin, Nataraj, and Harrison (2017), I only denote a firm as having exited if (i) its enumerator-reported "unit-status" is consistent with having exited, (ii) it reports no revenues, material input costs, labor, or months in operation, and (iii) it never again reports revenues, material input costs, labor, or months in operation.

[21] To avoid measurement error coming from reporting error, various researchers using the ASI trim outliers in various ways. For instance, some researchers (Bollard, Klenow, and Sharma 2013; Hsieh and Klenow 2009), trim relevant outcomes at the ninety-ninth percentile each year. Allcott, Collard-Wexler, and O'Connell (2016) and Martin, Nataraj, and Harrison (2017) undergo exercises to remove plants that report probably incorrect values (such as those that report increasing sales by three log points in one year and then shrinking back the subsequent

prices whenever possible. However, starting in 2005 prices (and therefore quantities) were sometimes imputed (with no edit flags), making it impossible to use the data to study if the subsidy program directly (or indirectly) affected prices.

Crucially from the perspective of the model, 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 the sampling frame of the ASI is a random cross section, it is unfortunately not well suited to studying entry (since it may take many years before a new establishment shows up in the data). Similarly, it is difficult to identify the exact year that a plant closes. However, for any given sector/year it is possible to calculate the share of production coming from new firms (or from firms new to that particular sector).[22]

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 potential issues that firms might endogenously adjust their size as a result of the policy (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 nomenclature, I define "small" firms as those that were newly eligible. A firm's category is fairly stable over time: for firms that appear in the sample at least twice before the policy change, 95 percent are in the same category in the second-to-most recent year as in the most recent one. For firms that appear in the sample four times, 90 percent have the same classification in the fourth-to-most recent year.

Crucially, the data is informative about the exposure shares from the perspective of each product, since the true exposure measures can be approximated using the sampling weights.[23,24] Panel A of Figure 4 is a scatter plot of each firm's exposure

year). Applying either of their strategies, or both, also does not substantively change the results. Similarly, for the relevant regressions I drop respondents that report a revenue share for a single input above 1.

[22] Using an earlier version of the data (without time-consistent firm identifiers), Chari (2011) uses the structure of a Melitz and Ottaviano (2008)-style model to back out entry from aggregate changes; it is now possible to measure entering firms' share of output directly.

[23] 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. I then ignore the sampling weights and calculate directly the exposure shares in this constructed census. In online Appendix Table 12, I show that the alternate exposure measure have correlations around 0.7 with the value I used in the main results.

[24] I only include firms that report assets. Furthermore, I cannot calculate this measure for the firms that do not report sales-by-product in this calculation, and so those firms are dropped in the regressions, even if those firms did report overall sales.

to the policy through within-state output competition and not-within-state output competition. The correlation of the two measures is 0.19, suggesting that firms that make the same types of products as newly eligible firms do not share some peculiar trait (although Table 1 shows that the newly eligible firms face somewhat higher out-of-state exposure).

B. *Industry versus Product Codes*

For similar types of questions, industry codes are often used to measure the extent of the market. Likely this is due to data constraints, since product-level data are often unreported. Unfortunately, since industry codes are not supersets of product codes, measuring competition with industry codes may to lead to biased results. For comparison, there are around 5,000 product codes, 700 5-digit industry codes, 130 4-digit industry codes, and 50 3-digit industry codes. Aggregating over firms in their final pre-program observation (and removing nonspecific codes such as "Other basic items"), there is both a substantial amount of overlap of industries within products and products within industries. Each 5-digit product code was produced in a median of three 4-digit industries. Not surprisingly, the overlap is exacerbated for 5-digit industries (products are produced in an average of five 5-digit industries), and somewhat mitigated for 3-digit industries. Considering the share of each product produced in each industry, the median Herfindahl-Hirschman Index (HHI) (over the products) for 4-digit sectors is around 7,500, 6,000 for 5-digit sectors, and 8,500 for 3-digit sectors. For the average firm, around 90 percent of output in its 4-digit industry is in products it does not produce, and it is around 75 percent for 5-digit sectors and 95 percent for 3-digit sectors.

The products produced in multiple industries tend to be large. For instance, weighting by sales the median product is produced in eight 4-digit industries. Overall, around 95 percent of output was of products produced in multiple industries, regardless of the industry aggregation.

Figure 3 shows the breakdown of (i) the number of industries producing each product and (ii) the share of output from products producing each quantity of output. While the modal number of industries associated with a product is one (both for 4- and 3-digit industries), those products are relatively small; for both aggregations there is more rupee output in two-industry products than single-industry products. While many of the products in multiple industries may be secondary products and others are somewhat vague, many seem to be plausibly producible by many different types of firms.[25]

Industry codes are self-reported; there exist pairs of firms that produce the same products but report different industry codes. In addition to using each firms' self-reported industry, I impute new industry codes for firms based on their primary product. If reporting error was the only problem, then researchers could construct "new-industry" classifications with the desirable property that each industry contains its entire penumbra: if a given firm is in a given "new-industry," all of the firms

[25] Examples of each category include, respectively "scrap, iron/steel" and "general purpose machinery/tools, components, not elsewhere classified"; "components, plastic"; and "pipes, tubes, and poles, steel," all of which are produced in over 35 industries.

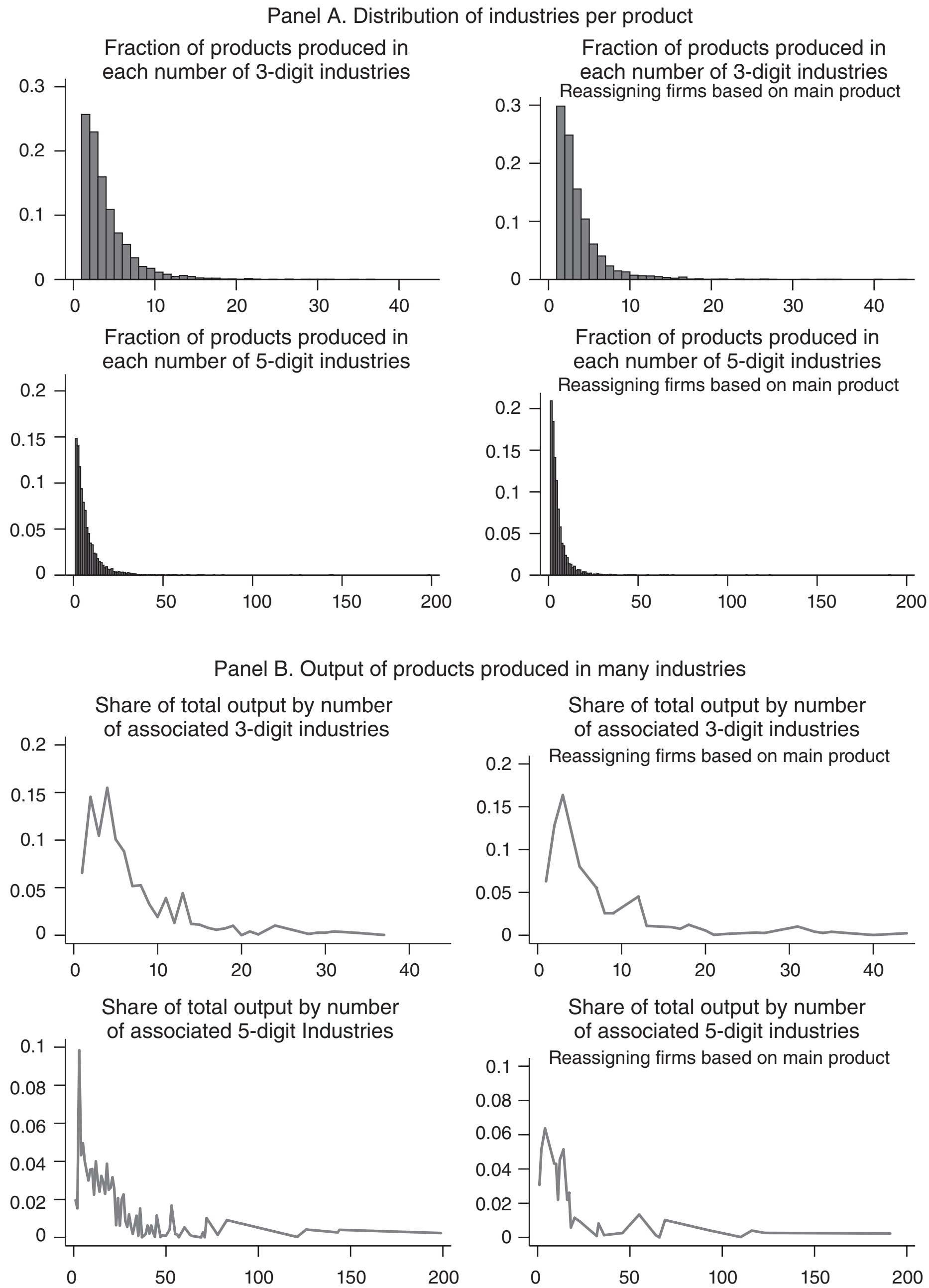

Figure 3. Distribution of Multi-Industry Products

Notes: Panel A: plot of the distribution of how many industries each product was produced in. The industry classifications used are 3- and 5-digit, with the left column representing the firms' self-identified industry, and the right column reassigning firms based on their major product. Firms are counted in their final pre-program year. Panel B: plot of the distribution of the share of total output coming from products produced in different number of industries. The industry classifications used are 3- and 5- digit, with the left column representing the firms' self-identified industry, and the right column reassigning firms based on their major product. Firms are counted in their final pre-program year.

Source: ASI

that produce the same products as that firm are also in that "new-industry." I created the largest possible such industry classification system for India (including all firms in their final pre-program observation), and generated roughly the same number of "new-industries" as in the 4-digit classification. However, over 99 percent of revenue was concentrated in just one of them, and most of the other industries contain exactly one product. The extremely high concentration remains even if I construct "new-industries" after dropping NSS firms and products containing "not elsewhere classified" or "scrap" in their descriptions.

In order to avoid these concerns, I construct the exposure measures at the product level. In online Appendix Section 2, I derive the sources of bias when using industry codes instead of products to estimate the effects of competition, and show empirically that using industry codes to measure competition instead of actual products produced can understate the magnitude of the indirect effects.

C. *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 how traded products are, since products which are exported (or imported) 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 value of exports in the year before the policy change[26] over total domestic production, and split by the median.[27]

I also generate (product-level) measures of capital intensity (the average capital/labor ratio), loan intensity (average liabilities divided by flow costs of primary inputs), and size (average assets). These measures are generated at the firm level, and I then 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).

D. *Identification Strategy*

The first part of the estimation strategy is to estimate relative effects in the ASI, using a difference-in-differences approach. Defining $\widetilde{priority}_{it}$ for firm i taking advantage of small-firm subsidies in year t, I follow equation (14) (for now, ignoring the indirect effect):

$$\ln(y_{jt}) = \beta \widetilde{priority}_{jt} + \sum \gamma_t X_j + \eta_j + \epsilon_{jt},$$

[26] From the Department of Commerce website: http://commerce.nic.in/eidb/default.asp (accessed July 7, 2014).

[27] Although a straightforward exercise, there are several choices that needed to be made along the way. The most important choices are (i) if the correct trade flow is exports or exports plus imports, and (ii) using outcomes in the most recent pre-program year, or the average over the previous five years. While my main analysis uses the former choice from each category, I show in the online Appendix that the results are similar with any of the other three combinations.

where the X_j are time-invariant (as determined before the policy change) characteristics of the firm (in the main regression, the firm's state), and the γ_t vary over time. In each specification, in addition to the state/year fixed effects, I include industry-year fixed effects (for around 130 consistent industries), as well as firm fixed effects. However, $\widetilde{priority}_{it}$ is not observed,[28] and firms that are eligible and actually take advantage of the subsidies may be different than those that do not. As a result, I instead estimate

$$\ln(y_{jt}) = \beta Post_t \times Small_j + \sum \gamma_t X_j + \eta_j + \epsilon_{jt}, \tag{19}$$

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 the 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. Observations are weighted by the inverse of their sampling probability.[29] Standard errors are clustered at the firm level and year/industry level to adjust for the within- and between-firm correlation in errors described in the previous section.

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, I leverage the fact that the share of production by newly eligible firms varies dramatically at the product level, as shown in Figure 4.

The main estimating equation is

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

For instance, if just considering the magnitudes of crowd-out for all types of competition jointly, the regression would be $\ln(y_{jt}) = \beta Post_t \times Small_j + \Theta Post_t \times \left(\sum_s \omega_{js} \cdot \mu_s\right) + \sum \gamma_{jt} X_j + \eta_j + \epsilon_{jt}$. This has the same form as equation (11) (with the addition of controls):

$$\hat{y}_j = \beta e_j - \theta\beta \sum_s (\omega_{js}\mu_s) + \left(\sum_s (\epsilon_{js} + \epsilon_s)\right).$$

The estimated β corresponds to β in the model, and the estimated Θ corresponds to $-\theta\beta$.[30] As with the direct effects, I proxy for each firm's exposure to the policy change using their product mix before the policy change.

[28] 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 do so.

[29] A firm's sampling probability is not constant over time. For instance, a firm that 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, Haider, and Wooldridge 2015; Wooldridge 1999).

[30] The logic extends to multiple sector characteristics, for instance the test of complete crowd-out is if the sum of the relevant Θ^ks equals $-\beta$.

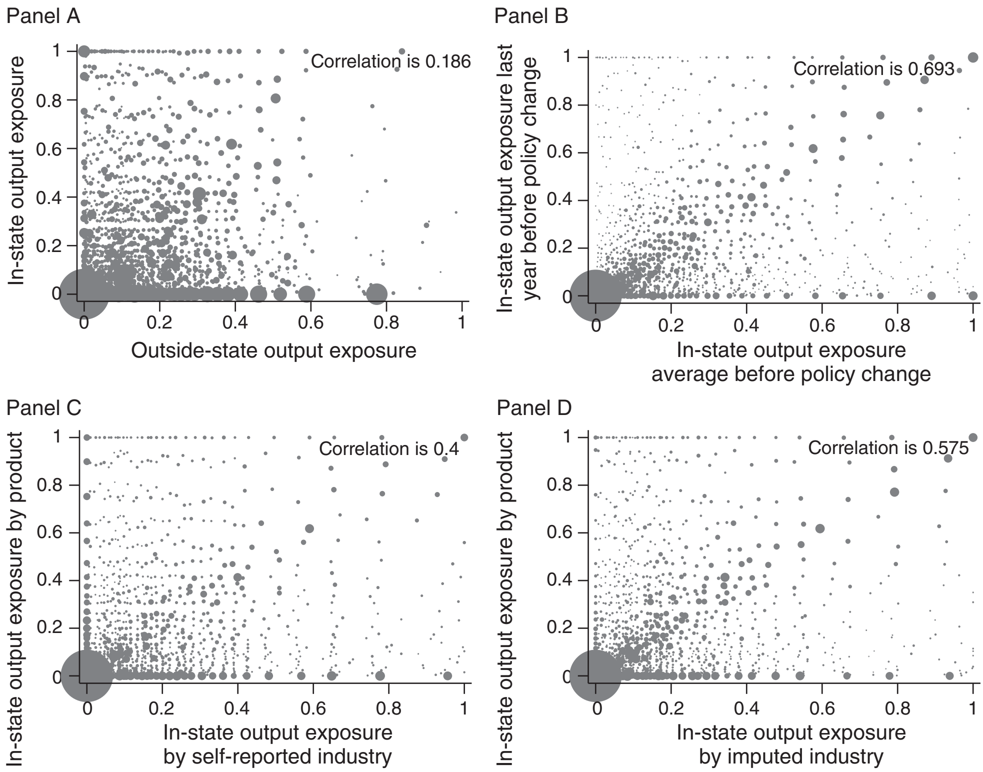

FIGURE 4. SCATTER PLOTS OF RELATIONSHIP BETWEEN EXPOSURE MEASURES

Notes: This figure plots the relationship between how exposed firms are through product markets to the policy change, comparing within-state exposure (measured in the final pre-program year) to (panel A) outside-state, (panel B) average pre-program, (panel C) (self-reported) 5-digit-industry exposure, and (panel D) imputed 5-digit-industry exposure. To make the figures, I generate 50 bins of inside-state and the alternative 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

IV. Estimating the (Naïve) Direct Effect of Eligibility

In this section, I begin by demonstrating that firms that 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 the less-distorted behavior of particular firms, instead of the policy change itself (McCrary 2008; Cattaneo, Jansson, and Ma forthcoming). Panel A of Online Appendix Table 1 tests for bunching around the cutoff formally, following Cattaneo, Jansson, and Ma (forthcoming). I compare the firm-size distribution at the old and new size-cutoff, both before and after the policy change. Figure 5 plots these distributions. There is weak evidence of manipulation before the policy change (firms distorting their size in order to remain eligible), but not at the other years. Table 1 shows summary statistics for the main outcome and explanatory variables in the paper for each firm in its most recent pre-program observation. The lack of bunching around the cutoff is useful

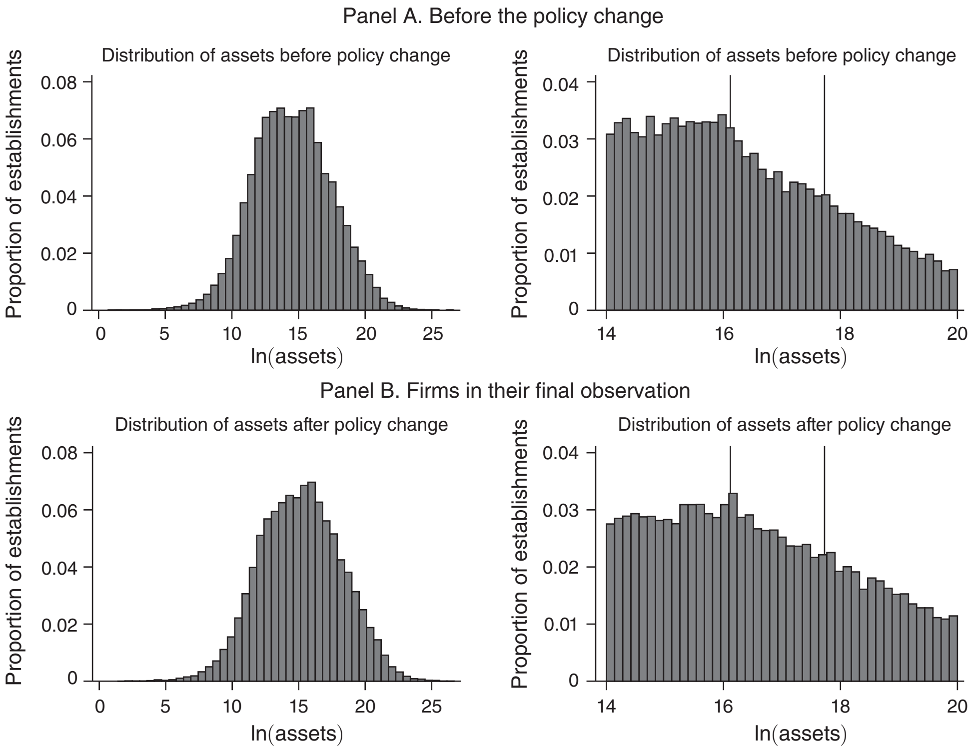

Figure 5. Distribution of Nominal Assets

Notes: This figure demonstrates the distribution of nominal assets for all firms around the policy changes. Panel A shows the firm size distribution for firms in their most recent pre-2006 observation, and panel B the distribution for firms in their most recent observation. Online Appendix Table 1 follows Cattaneo, Jansson, and Ma (forthcoming) to test formally for a break in the firm-size distribution around the cutoffs.

Source: ASI

for identification but initially somewhat puzzling: if access to the Priority Sector benefits firms, then they should be willing to distort their capital stock in order to take advantage. However, given the difficulty of verifying exact eligibility for the program, qualitative interviews suggest that there is no discrete change in a firm's ability to access the program around the eligibility cutoff, precluding the use of a regression-discontinuity type of design.

A. *Plots of Program Effects*

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

$$\ln\left(y_{jt}\right) \;=\; \sum_{t=2001}^{2010} \beta_t Small + \sum \gamma_t X_j + \eta_j + \epsilon_{jt}. \tag{21}$$

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

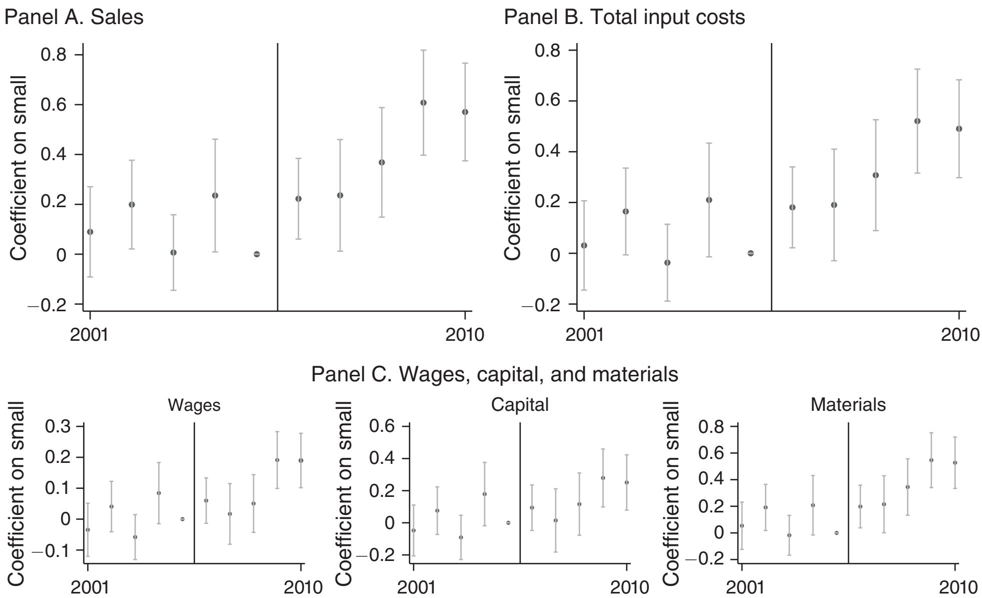

Figure 6. Event-Study Plot of Coefficients: Effect of Direct Eligibility

Notes: These figures plot the outcomes 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 percent confidence intervals are constructed using robust standard errors clustered by firm and industry/year. The vertical line between 2005 and 2006 indicates the policy change, and 2005 was the omitted year in the regression.

Source: ASI

first quarter of 2007. The coefficients are subsequently all significantly larger than zero (as well as the largest pre-2006 coefficient). Not only do the newly eligible firms benefit from the policy change relative to their peers, but the gains are persistent.[31] In addition to providing evidence on medium-run dynamics, the relatively long panel also allows me to average over a fair amount of variation (Rosenzweig and Udry forthcoming): F-tests reject equality of the coefficients before and after the policy change (for sales, respectively, $F(3,1{,}321) = 2.76$ and $F(4,1{,}321) = 5.68$).

B. *Effects of the Program on Firm-Level Economic Outcomes*

Table 2 estimates equation (19) for output and inputs,

$$\ln(y_{it}) \;=\; \beta Post \times Small_i + \sum \gamma_t X_j + \eta_i + \epsilon_{it}.$$

Column 1 of Table 2 shows that gaining eligibility predicts an increase in establishment sales of 25–35 percent, and is significantly different from zero. 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

[31] The "shock" in this instance is not a one-time occurrence, but potentially continued eligibility. The fact that the results increase over time (as firms learn about eligibility) is consistent with this interpretation.

Table 2—Naïve Direct Effects of Priority Sector

	Sales (1)	Wages (2)	Capital (3)	Materials (4)	Total flow inputs (5)
Post × *small*	0.31	0.09	0.13	0.30	0.28
	(0.07)	(0.03)	(0.06)	(0.06)	(0.06)
Controls for:					
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
State/year fixed effects	Yes	Yes	Yes	Yes	Yes
Industry/year fixed effects	Yes	Yes	Yes	Yes	Yes
Firms	51,549	52,321	52,485	52,198	51,916
Firm/year observations	218,086	223,148	224,139	221,980	220,552

Notes: Small firms are those who gained eligibility in 2006. Each column represents a difference-in-differences specification, predicting the indicated outcome variable. The predicted variable is how the (real) outcome affects aggregate productivity: ln(outcome) if positive, and 0 for closed establishments. Observations are weighted by their (potentially time-varying) inverse sampling probability, and robust standard errors clustered by firm and industry/year are reported in parentheses.

Source: ASI

corresponding increases in costs, since the effect of the program would be infra-marginal. However, column 4 shows that the flow costs of inputs increase by an amount similar to sales. This effect is primarily driven by intermediates, but there is also a significant increase in firms' wage bills and capital usage after gaining eligibility.

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 borrowing allows firms to expand. I find evidence supporting this argument in online Appendix Table 2, column 3, since liabilities significantly increase for the newly eligible firms once they gain access to the program.

V. Estimating the Indirect Effect of Eligibility through Competition in Output Markets

To start, I estimate the effects of output competition treating all products similarly. Following equation (20), I run a firm-level regression of the following form:

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

adding the weighted-average competitive exposure measures generated in Section III to the difference-in-differences regressions of the previous section.

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 75 percent (in magnitude) of the coefficient of newly eligible, and has the opposite sign. Since, as outlined in equation (14),

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

TABLE 3—DIRECT + INDIRECT EFFECTS OF PRIORITY SECTOR

	Sales (1)	Wages (2)	Capital (3)	Materials (4)	Total flow inputs (5)
Post × *small*	0.38	0.11	0.17	0.35	0.33
	(0.07)	(0.03)	(0.06)	(0.07)	(0.07)
Post × within-state exposure	−0.24	−0.07	−0.17	−0.23	−0.22
	(0.10)	(0.04)	(0.09)	(0.10)	(0.10)
Controls for:					
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
State/year fixed effects	Yes	Yes	Yes	Yes	Yes
Industry/year fixed effects	Yes	Yes	Yes	Yes	Yes
Firms	51,549	52,321	52,485	52,198	51,916
Firm/year observations	218,086	223,148	224,139	221,980	220,552

Notes: Small firms are those who gained eligibility in 2006. Each column represents a difference-in-differences specification, predicting the indicated outcome variable. The predicted variable is how the (real) outcome affects aggregate productivity: ln(outcome) if positive, and 0 for closed establishments. Observations are weighted by their (potentially time-varying) inverse sampling probability, and robust standard errors clustered by firm and industry/year are reported in parentheses.

Source: ASI

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 25 percent 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 25 percent). The relative magnitude is similar for total input costs, as well as for each component of inputs.[32]

The magnitude on the direct effects increases slightly when including measures of indirect effects. This is consistent with the intuition of omitted variables: indirect exposure is positively correlated with direct exposure (since firms are in their own sector), which should bias downward the effect of direct exposure in the naïve regression. This is derived in online Appendix Section 4.

The difference-in-differences effect of output exposure can be graphed 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 (20):

$$\ln(y_{jt}) = \sum_{t=2001}^{2010} \beta_t Small_j + \sum_{t=2002}^{2011} \Theta_t Exposure_j + \sum \gamma_j X_j + \eta_j + \epsilon_{jt}.$$

In panel A of Figure 7, I plot the coefficients and 95 percent 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 shrink both their output and inputs. In Figure 8, I plot a similar regression, where

[32] In the model this would imply that $\sigma = (1+2\phi)/(1-\phi)$. For $\sigma = 5$, this would imply $\phi = 2/7$.

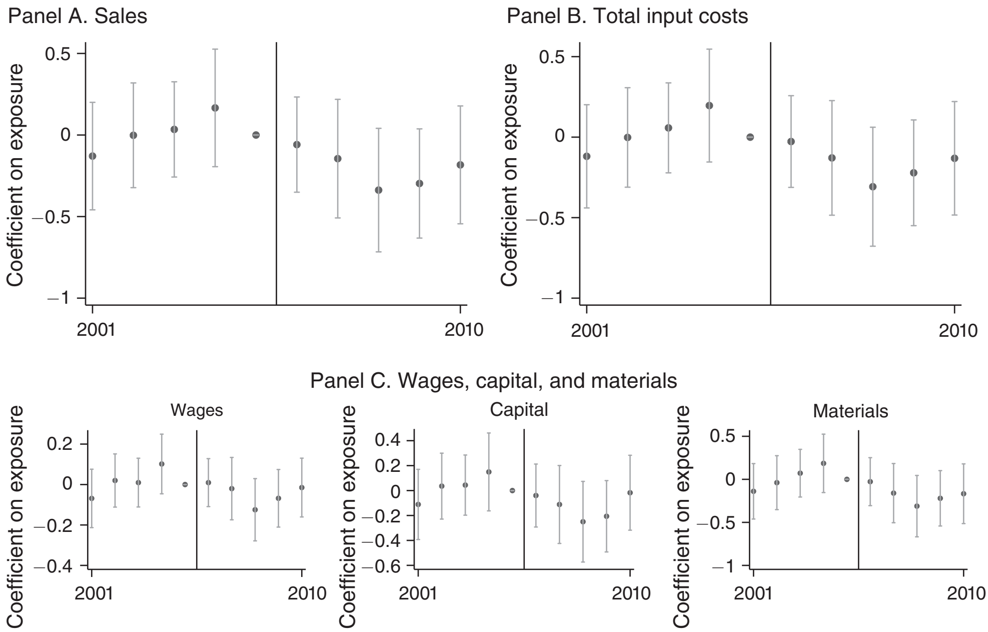

FIGURE 7. EVENT-STUDY PLOT OF COEFFICIENTS: EFFECT OF PRODUCT MARKET COMPETITION

Notes: These figures plot the outcomes of indirectly exposed firms over time, relative to their peers (the calculation of the exposure measure is described in the text). Each of the points comes from one pooled regression with time and firm fixed effects, as well as the direct exposure measures. The 95 percent confidence intervals are constructed using robust standard errors clustered by firm and industry/year. The vertical line between 2005 and 2006 indicates the policy change, and 2005 was the omitted year in the regression.

Source: ASI

instead the x-axis is the extent of exposure to the program (times $Post_t$). Consistent with the theory, firms that are more exposed to the program shrink more.

In online Appendix Table 3, I calculate the exposure measure using industry codes instead of the product codes. The coefficients on exposure (for sales) are qualitatively similar, but for most industry aggregations are smaller in magnitude and less precise. This finding is consistent with the argument that industry codes measure how firms compete with each other, but with more error than actual products do. Online Appendix Table 16 shows the correlation of exposure measures using product codes versus industries is around 0.4.

In the model, the three mechanisms for affecting firm size are the price index (which I do not directly observe), input distortions, and firm productivity. In Table 4, I test the second mechanism, studying the direct and indirect effects of the policy on revenue shares. Consistent with the model, firms that gain access to the program see an increase in the revenue shares of their inputs by around one percentage point, driven primarily by labor (the change in the capital share is negative, but insignificantly so, and both the coefficient for capital and material's revenue share are smaller in magnitude than that for labor). This finding is consistent with program eligibility both lowering barriers for firms to hire workers and additional costs of training. Unlike for sales and inputs, there is no corresponding significant decrease

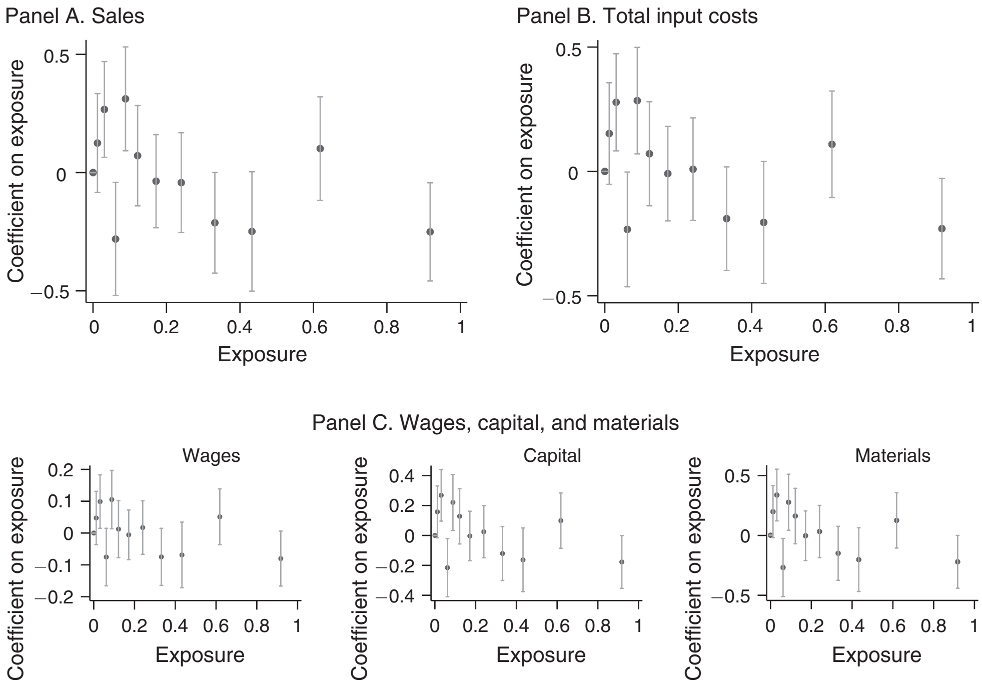

Figure 8. Plot of Coefficients: Effect of Variation in Product Market Competition

Notes: The firms are binned by the share of their product markets exposed to the policy change, and a separate coefficient for *Post* is estimated for each bin (relative to those that were not exposed). Each of the points comes from one pooled regression with time and firm fixed effects, as well as the direct exposure measures. The 95 percent confidence intervals are constructed using robust standard errors clustered by firm and industry/year. The vertical line between 2005 and 2006 indicates the policy change, and 2005 was the omitted year in the regression.

Source: ASI

Table 4—Mechanisms through Which the Priority Sector Affects Eligible Firms (Distortions)

	Revenue share for			
	Wages	Capital	Materials	Total flow inputs
	(1)	(2)	(3)	(4)
Post × *small*	0.007	−0.001	0.002	0.008
	(0.002)	(0.002)	(0.003)	(0.004)
Post × within-state exposure	−0.003	0.002	0.004	0.003
	(0.002)	(0.002)	(0.004)	(0.005)
Controls for:				
Firm fixed effects	Yes	Yes	Yes	Yes
State/year fixed effects	Yes	Yes	Yes	Yes
Industry/year fixed effects	Yes	Yes	Yes	Yes
Firms	46,773	47,014	39,496	39,192
Firm/year observations	201,459	202,930	165,537	164,065

Notes: Small firms are those who gained eligibility in 2006. Each column represents a difference-in-differences specification, predicting the indicated outcome variable. The predicted variable is the (nominal) revenue share for each input. Observations are weighted by their (potentially time-varying) inverse sampling probability, and robust standard errors clustered by firm and industry/year are reported in parentheses.

Source: ASI

in the revenue shares for indirectly exposed firms, and the overall revenue share for inputs is insignificantly increasing with exposure.

What matters for aggregate productivity is not the revenue share, but the gap between the production function elasticity and the revenue share. In practice, the estimates would be identical had the outcomes been the gap for each input. Traditional (Cobb-Douglas) production function estimation is at the industry level (or occasionally industry/year), and the estimates are therefore constant at the industry level (and therefore will get absorbed by the industry/year fixed effects). Even firm-specific production function estimates would be absorbed by the firm fixed effects.

Estimating production functions does allow me to test the third mechanism, that firm productivity itself is affected by the program. Methods for estimating firm production function struggle when firms face idiosyncratic frictions on their input choices. My main solution, as in HK, is to assume no distortions in US data and use cost-shares from Becker, Gray, and Marvakov (2013). For completeness, I also use a variety of measures on Indian data as well: cost shares, Levinsohn and Petrin (2003) and Wooldridge (2009) (known colloquially as W-LP), and De Loecker et al. (2016) (both their measure estimated using Prowess data and from the ASI as in Balasundharam 2019).[33] The methods produce fairly correlated TFPQ estimates (shown in online Appendix Table 17).[34] In Table 5, I show that across all five measures, there is no evidence that the program increased firm size through improvements in productivity (nor were there spillovers on competitors).[35]

A. *Trade and Output Competition*

Increased competition may matter less for products which are traded. I augment equation (20) by estimating

$$\ln\left(y_{jt}\right) = \beta Post_t \times Small_j + \sum_k \Theta^k Post_t \times Exposure_j^k + \sum \gamma_j X_j + \eta_j + \epsilon_{jt}\,, \tag{22}$$

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-differences effects of output exposure is different for products which are traded and those which are not. However, since firms cannot be separated into those that produce only traded

[33] Since the Indian data are a repeated cross section, W-LP, which requires relatively long panel information on firms' output and inputs, is limited to estimates on a small part of the data (the firms that happen to be surveyed in repeated years) and the resulting estimated elasticities are often negative (or larger than 1) for some inputs. For those sectors, I use the cost share estimates.

[34] Following HK, I distinguish between TFPR and TFPQ and estimate TFPQ as $Y_j^{\frac{\sigma}{\sigma-1}} / \left(K_j^{\alpha_{K_j}} L_{js}^{\alpha_{L_j}} M_{js}^{\alpha_{M_j}}\right)$. I assume $\sigma = 3$; the estimated effects on TFPQ are smaller with $\sigma = 5$. The derivation in HK assumes single-product plants, I show in online Appendix Section 5 that with constant returns to scale a similar intuition holds for multi-product plants.

[35] While the model predicts that the only reason why firm sales increase is due to changes in the revenue shares, this is not completely consistent with the data. The average revenue share for firms is around 86 percent (reported in Table 1), so a 1 percent increase in the total revenue share due to the program would represent only a 1–1.5 percent decrease in the marginal cost of the inputs. Given the CES structure, this would lead to a σ–1.5σ percent increase in sales, which for most estimates of elasticities of substitution would be less than 30 percent (Broda and Weinstein 2006).

Table 5—Mechanisms Through Which the Priority Sector Affects Eligible Firms (TFPQ)

	Cost shares (US)	Cost shares (India)	W-LP	W-LP (weighted)	DGKP (prowess)	DGKP (ASI)
	(1)	(2)	(3)	(4)	(5)	(5)
Post × *small*	0.027	0.011	0.010	0.021	0.021	0.017
	(0.025)	(0.016)	(0.018)	(0.017)	(0.028)	(0.019)
Post × within-state exposure	−0.020	0.011	0.020	0.007	0.039	0.030
	(0.036)	(0.020)	(0.022)	(0.022)	(0.038)	(0.025)
Controls for:						
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State/year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry/year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firms	35,335	47,211	47,211	47,211	35,245	35,245
Firm/year observations	134,988	204,013	204,013	204,013	137,658	137,658

Notes: Small firms are those who gained eligibility in 2006. Each column represents a difference-in-differences specification, predicting the indicated outcome variable. The outcomes are (log and industry demeaned) firm productivity, estimated using cost shares (in the ASI or NBER/CES US data), Levinsohn and Petrin (2003) and Wooldridge (2009), and De Loecker et al. (2016) (from the Prowess dataset or the ASI). Observations are weighted by their (potentially time-varying) inverse sampling probability, and robust standard errors clustered by firm and industry/year are reported in parentheses.

Source: ASI

goods and those that produce only non-traded goods, it cannot be estimated using a standard difference-in-difference-in-differences approach. In keeping with the spirit of the triple differences regression and to control for differences between firms that produce more traded goods and those that produce fewer, I create a measure for each firm capturing the share of its outputs which (before the policy change) are traded. I then include *Post* × *share_traded* as a control.

Table 6 shows the coefficients from estimating equation (22) on firm outcomes, including exposure measures for within-state output competition and within-state output competition for traded products. For sales, 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.[36] 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. Again, the results are similar both for overall flow input costs, as well as costs broken down by input. Figure 9 plots the corresponding event-study coefficients, which are insignificant and close to zero before the policy change, and positive afterward. Both in Table 6 and Table 3, the coefficient on the direct effect of eligibility is (insignificantly) larger than when I only considered direct effects in Table 2. This would also be predicted by the model: when a firm lowers its price, it partially cannibalizes its own sales by affecting the sectoral price index, and is derived formally in online Appendix D.

[36] 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; Clerides, Lach, and Tybout 1998).

Table 6—Direct + Indirect Effects of Priority Sector, Heterogeneous Effects of Trade

	Sales (1)	Wages (2)	Capital (3)	Materials (4)	Total flow inputs (5)
Post × *small*	0.37	0.11	0.17	0.35	0.33
	(0.07)	(0.03)	(0.06)	(0.07)	(0.07)
Post × within-state exposure	−0.40	−0.13	−0.30	−0.37	−0.36
	(0.12)	(0.05)	(0.10)	(0.11)	(0.12)
Post × within-state traded exposure	0.56	0.22	0.46	0.50	0.49
	(0.19)	(0.08)	(0.16)	(0.18)	(0.19)
Controls for:					
Share of firm's products that are traded × post	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
State/year fixed effects	Yes	Yes	Yes	Yes	Yes
Industry/year fixed effects	Yes	Yes	Yes	Yes	Yes
Firms	51,549	52,321	52,485	52,198	51,916
Firm/year observations	218,086	223,148	224,139	221,980	220,552

Notes: Small firms are those who gained eligibility in 2006. Each column represents a difference-in-differences specification, predicting the indicated outcome variable. The predicted variable is how the (real) outcome affects aggregate productivity: ln(outcome) if positive, and 0 for closed establishments. Observations are weighted by their (potentially time-varying) inverse sampling probability, and robust standard errors clustered by firm and industry/year are reported in parentheses.

Source: ASI

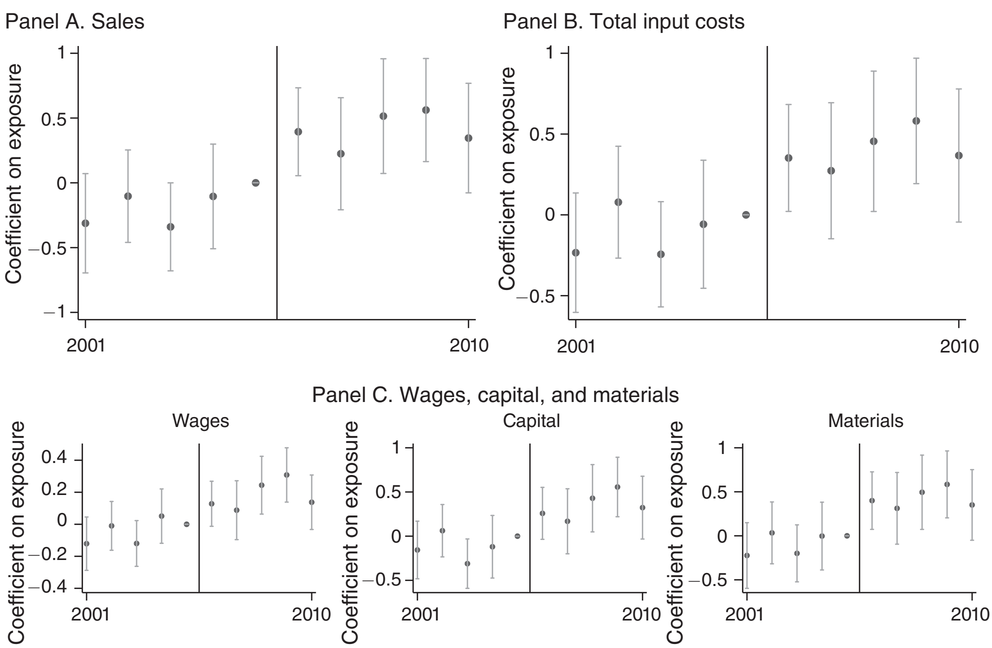

Figure 9. Event-Study Plot of Coefficients: Mediating Effect of Trade on Product Market Competition

Notes: These figures plot the outcomes of indirectly exposed firms through traded products over time, relative to their peers (the calculation of the exposure measure is described in the text). Each of the points comes from one pooled regression with time and firm fixed effects, as well as the main indirect and direct exposure measures. The 95 percent confidence intervals are constructed using robust standard errors clustered by firm and industry/year. The vertical line between 2005 and 2006 indicates the policy change, and 2005 was the omitted year in the regression.

Source: ASI

In online Appendix Table 5, I consider alternative product characteristics that could also mediate the effect of product market competition on sales. While the point estimates are consistent with arguments that product market competition is weaker for products that are produced by firms with higher capital/sales and liabilities/assets ratios, the interactions are not precise nor large in magnitude. The point estimate on the elasticity of substitution is consistent with what would be expected—sectors with low calibrated elasticities of substitution in Broda and Weinstein (2006) have weaker product market competition—but is also insignificant, with a standard error larger than the point estimate.[37] In online Appendix Section 3, I take a complementary approach looking at product-level aggregates, and although underpowered the results are qualitatively similar: the subsidy program increased output by more for traded products, and sectors with more newly eligible firms did not grow faster.

Table 7 augments the analysis to include measures of within-state and outside-state competition, following equation (18). The coefficients on direct and within-state exposure remain similar to before. The coefficients on outside exposure show that firms are (insignificantly) *positively* affected if their far-away potential competitors gain access to the program, both for non-traded products and additionally so for traded ones. As a result, there is no evidence for strong product market competition across state lines in India.

Online Appendix Table 4 shows the effects of direct, indirect, and indirect trade exposure (from Kothari 2014) using industry codes. As with online Appendix Table 3, the estimated industry effects are smaller and less precise than those using products (here there is the additional effect that industries contain both traded and non-traded products, and so the classification is noisier).

B. *Permutation Tests*

In the spirit of Fisher (1935) and Ho and Imai (2006), 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. Peer effects are a natural setting for using permutation tests, since one can permute (i) the source of the shock, (ii) the connections of the network, and (iii) the characteristics of the network. Using Monte Carlo simulations of 500 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 for predicting sales in Table 8, where column 1 reproduces the results from column 1 of Table 6.

[37] In the online Appendix, I provide several robustness checks for Table 6. In online Appendix Table 6, I show that the results are similar with state by industry by year fixed effects, not just state by year and industry by year. In the spirit of regression discontinuities, in online Appendix Table 7 I show that the results are robust to controlling for a cubic polynomial in the assignment variable (original value of plants and machinery) interacted with each year separately. Online Appendix Table 8 controls for the additional characteristics from online Appendix Table 5. In online Appendix Tables 9, 10, and 11, I respectively show the estimated effects using total flows, average exports, and average total flows in order to measure the indirect effects of the policy change, and in online Appendix Table 12 I show that the exposure measures are strongly correlated with each other. Online Appendix Table 14 drops establishments that produce products "reserved" for small firms (Martin, Nataraj, and Harrison 2017); the results are relatively unchanged. Online Appendix Table 15 does not include the informal firms when calculating indirect exposure, which also does not affect the results.

Table 7—Direct + Indirect Effects of Priority Sector, Heterogeneous Effects by Geography

	Sales (1)	Wages (2)	Capital (3)	Materials (4)	Total flow inputs (5)
Post × *small*	0.37	0.11	0.17	0.35	0.33
	(0.07)	(0.03)	(0.06)	(0.07)	(0.07)
Post × within-state exposure	−0.41	−0.13	−0.31	−0.37	−0.37
	(0.12)	(0.05)	(0.10)	(0.11)	(0.12)
Post × within-state traded exposure	0.55	0.21	0.45	0.49	0.48
	(0.20)	(0.08)	(0.16)	(0.18)	(0.19)
Post × outside-state exposure	0.16	0.01	0.13	0.18	0.18
	(0.17)	(0.07)	(0.14)	(0.16)	(0.16)
Post × outside-state traded exposure	0.32	0.20	0.22	0.27	0.27
	(0.17)	(0.07)	(0.14)	(0.16)	(0.16)
Controls for:					
Share of firm's products that are traded × post	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
State/year fixed effects	Yes	Yes	Yes	Yes	Yes
Industry/year fixed effects	Yes	Yes	Yes	Yes	Yes
Firms	51,549	52,321	52,485	52,198	51,916
Firm/year observations	218,086	223,148	224,139	221,980	220,552

Notes: Small firms are those who gained eligibility in 2006. Each column represents a difference-in-differences specification, predicting the indicated outcome variable. The predicted variable is how the (real) outcome affects aggregate productivity: ln(outcome) if positive, and 0 for closed establishments. Observations are weighted by their (potentially time-varying) inverse sampling probability, and robust standard errors clustered by firm and industry/year are reported in parentheses.

Source: ASI

Table 8—Permutation Tests on the Effects of the Priority Sector on Sales

		Permuting		
	Observed (1)	Newly eligible (2)	Products produced (3)	Product characteristics (4)
Post × *small*	0.37	−0.02	0.31	0.38
	(0.07)	(0.07)	(0.00)	(0.00)
Post × within-state exposure	−0.40	0.08	0.06	−0.24
	(0.12)	(0.14)	(0.13)	(0.08)
Post × within-state traded exposure	0.56	0.15	0.15	−0.02
	(0.19)	(0.24)	(0.24)	(0.20)
Controls for:				
Share of firm's products that are traded	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
State/year fixed effects	Yes	Yes	Yes	Yes
Industry/year fixed effects	Yes	Yes	Yes	Yes

Notes: Small firms are those who gained eligibility in 2006. Column 1 represents a difference-in-differences specification, predicting how (real) sales affects aggregate productivity: ln(sales) if positive, and 0 for closed establishments. The permutation tests replace the observed characteristics with permuted ones: eligibility for the program in column 2, the share of each state/product produced by *small* firms in column 3, and which products are tradable in 4. Within each of the 500 permutations, all characteristics are held consistent, so in column 2 both the direct and exposure measures are counterfactual. In all regressions, observations are weighted by their (potentially time-varying) inverse sampling probability. Robust standard errors clustered by firm and industry/year are reported in parentheses in column 1, and the standard deviation of the permutations is reported in columns 2–4.

Source: ASI

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 within each state as observed in the data. 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 $\hat{\beta}^{placebo}$, as well as $\hat{\theta}^{d^{placebo}}$ and $\hat{\theta}^{x^{placebo}}$, using equation (22), as outlined in the previous subsection. The results are shown in column 2. The estimates are all small and close to 0; neither placebo eligibility nor exposure to placebo eligible firms predicts a change in firm behavior. The coefficient on the effect of (counterfactual) competition on traded products is somewhat more positive, but around 20 percent of its observed counterpart.

The second set of tests undertakes a similar procedure, but instead constructs placebo indirect effects while maintaining the true eligibility changes. Specifically, while I use firms' actual eligibility in order to estimate the direct effects, I use the permuted exposure measures from the previous column.[38] Again, I maintain the characteristics of production. The results are shown in column 3. The estimated direct effect is similar to that in Table 2, consistent with maintaining the true eligibility change. The estimated coefficients for placebo exposure to the newly eligible firms are, as in column 2, close to zero.

The third set of tests is similar, but instead of testing the effect of the shock to the nodes 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. The results are shown in column 4. The estimated direct and indirect effects are similar to that in Table 3, consistent with maintaining the true eligibility change and product mix. The estimated coefficients for how placebo product characteristics interact with indirect exposure are close to zero. In all cases, the coefficients coming from permuted data are closer to 0 than their real world counterparts in over 95 percent of iterations.

VI. Effects of the Reallocation of Economic Activity

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), a policy argument that dovetails with the academic debate on the roles of within-sector factor misallocation and aggregate productivity. In this section, I show how to use the reduced-form estimates and equation (1) to compute the gains (or losses) from targeted subsidy programs. I estimate effects of the program by (i) adding the program earlier (in each firm's final pre-program observation) and (ii) removing it afterward (in each firm's final observation in the data). Essentially, the exercise takes the estimated effects of product market competition seriously, and asks if the firms that were induced to grow by the program have larger or smaller "gaps" than those induced

[38] This strategy yields some the appealing features of block randomization, since firms that produce similar products (in the same state) will continue to have similar exposure measures in each permutation.

to shrink. Introducing a subsidy policy will potentially change output and inputs both through the direct and indirect effects. I rewrite equation (1) to account for the fact that firm's output and inputs are a function of the policy change: specifically, I use firms' measured direct (e_j) and indirect exposure (μ_s) to the subsidy program, the indirect exposure through traded products (x_s) as well as the estimated direct (β) and indirect (θ^d, θ^x) effects of the program from Table 6:

$$(23)\quad APG(Subsidies) = \sum_j \Big(D_j d\ln Y_j\big(e_j, \mu_s, x_s, \beta, \theta^d, \theta^x\big)\Big) - \sum_{j=1}^{N} \sum_{Input \in K,M,L} D_j \Big[\sum_k \big(s_{Input_j}\big) d\ln Input_j\big(e_j, \mu_s, x_s, \beta, \theta^d, \theta^x\big)\Big].$$

The Domar weights and revenue shares are calculated directly in the data. I report the evaluation of equation (23) in Table 9. The first row shows that the program increased aggregate productivity in Indian manufacturing by around 1–2 percent. In order to demonstrate the importance of calculating product-market crowd-out for understanding the effects that subsidy programs have on aggregate productivity, I undertake two further counterfactual exercises. In the second row, I ignore the effects of trade and use the estimates in Table 3 for estimating one indirect effect of the program (θ), plugging in the estimates to

$$APG(Subsidies\ in\ autarky) = \sum_j \Big(D_j d\ln Y_j\big(e_j, \mu_s, \beta, \theta\big)\Big) - \sum_{j=1}^{N} \sum_{Input \in K,M,L} D_j \Big[\sum_k \big(s_{Input_j}\big) d\ln Input_j\big(e_j, \mu_s, \beta, \theta\big)\Big].$$

Here, measured aggregate productivity gains are an order of magnitude lower, as reported in the second row.[39] However, the estimated productivity gains are positive, partially because since (θ) implies incomplete crowd-out since it is a weighted average of the (complete) crowd-out in domestic markets with (negligible) crowd-out for traded products, and partially because the newly eligible firms had relatively high marginal products. In the third row, I completely ignore the effects of competition, and use the estimates in Table 2 in the following equation:

$$APG(Subsidies\ in\ autarky) = \sum_j \Big(D_j d\ln Y_j\big(e_j, \beta\big)\Big) - \sum_{j=1}^{N} \sum_{Input \in K,M,L} D_j \Big[\sum_k \big(s_{Input_j}\big) d\ln Input_j\big(e_j, \beta\big)\Big].$$

Without accounting for competition, the measured gains from the program would have been 3–4 percent, several times larger than the estimates which account for competition.

In order to estimate how well-targeted the program is, I estimate the aggregate productivity gains in settings where the Indian government had instead subsidized

[39] And in this case, the estimated gains would have been close to those from the previous version of the paper.

Table 9—Percent Change in Aggregate Productivity Due to the Priority Sector

	Observed data		Permuted data	
	Adding program earlier (1)	Removing program afterward (2)	Adding program earlier (1)	Removing program afterward (2)
Overall	1.57 percent	−2.31 percent	1.47 percent (0.41) [0.38]	−1.62 percent (0.41) [0.06]
Assuming no trade	0.30 percent	−1.05 percent	0.15 percent (0.13) [0.12]	−0.26 percent (0.23) [0.00]
Ignoring all spillovers	3.38 percent	−4.20 percent	4.55 percent (0.89) [0.96]	−4.54 percent (0.74) [0.63]

Notes: Each firm's contribution to aggregate productivity is affected by the Priority Sector through three mechanisms in this table: direct size gains for the newly eligible, and indirect size losses for those who compete with the newly eligible, an effect which is mitigated by international trade. The table takes the coefficients in Table 5 and calculates the total effect on aggregate productivity from equation (1). Column 1 calculates the gains from introducing the program earlier (in each firms last pre-program observation), and column 2 calculates the gains from removing the program in each firms final observation. Columns 3 and 4 show the corresponding estimates for what would have counterfactually happened had different firms been affected, assuming that the underlying direct and indirect of the program remained the same. The standard deviation of placebo estimates is in parentheses. The share of calibrated permuted gains which are larger than those observed in the data (or more negative for removing the program) are in brackets.

different firms. In particular, as in Section VB, I permute the set of newly eligible firms, holding fixed the products (and the characteristics of those products) that firms make and the estimated effects of the program. The program was reasonably well-targeted, in that it normally would lead to higher aggregate productivity gains than a random selection of firms. However, over 10–20 percent of the permutations led to larger gains than what was observed in the data.

The effect of the program on aggregate productivity can be decomposed into a within-firm "Technical Efficiency" component and a between-firm "reallocation" component, as in equation (3). The within-firm component is

$$TE = \sum_j \Big(D_j \, d\ln A_j\big(e_j, \mu_s, \xi\big)\Big),$$

where I estimate A_j as described in subsection IIA. The point estimates in Table 5 are consistently small and insignificant. If we take the "Technical Efficiency" component to be zero, then all of the gains to aggregate productivity are due to reallocation.

VII. Conclusion

In this paper, I study the aggregate effects of programs that subsidize small firms, which are popular around the world. The effect of a subsidy program on aggregate productivity depends on the extent to which it reallocates activity across firms. My empirical analysis leverages a large-scale weakening of eligibility criteria for firm subsidies in India, which increased the set of eligible firms.

I make three methodological contributions. First, using standard assumptions, I show how changed input prices for some firms lead to changes in aggregate output. The effects come from direct effects on the affected firms, and indirect effects on their competitors. The growth rate of a firm's sales through the indirect effect is linear: it will be twice as large (in magnitude) if twice the share of activity in their sector is subsidized. The estimate of indirect effects using firm-level data can be used to calculate the elasticity of aggregate growth with respect to private growth. Second, I show how to adapt a canonical measure of reallocation to estimate the productivity effects of these types of input price shocks. I formalize the logic that within-sector misallocation will decrease if the firms that face lower input prices because of the program are those that originally faced relatively high distortions (compared to their sector). Third, I show how to account for heterogeneous sectoral characteristics, in particular the role of international trade. The indirect effects for traded products are conceptually likely to be small, since the market is much larger than a single state in India.

I apply the model to detailed firm-level data in order to analyze the aggregate effects of firm subsidies. 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. Empirically, I show that in the Indian context, industry codes alone may not be able to answer these types of questions, since they measure the products that firms produce with a substantial amount of error. Most products are produced in firms in multiple sectors, and this issue cannot be fixed by creating new product/industry concordances.

My empirical results have nuanced consequences for policymakers.[40] Gaining eligibility for small-firm subsidies predicts large gains in firm output. However, crowd-out absorbed around two-thirds of the direct effects. The extent of crowd-out depends on sector characteristics, and I find complete crowd-out for products that are not traded internationally. Properly estimating crowd-out is therefore crucial for understanding the aggregate effects of firm level shocks. Naïvely estimating perfect crowd-out will understate gains from well-targeted programs, while ignoring crowd-out entirely will dramatically overstate the gains from these types of policies.

I estimate that the expansion of the Indian Priority Sector increased aggregate productivity in manufacturing by around 1 percent, with much of the gains concentrated in the traded sectors that exhibit little crowd-out. While this estimate is two times lower than the naïve estimate of increased growth given by just the direct effects, I show empirically there still can be productivity benefits from well-targeted industrial policies.

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[40] These results alone are not enough for policy recommendations, since I abstract from potential costs of the program. While I do not find strong evidence that firms manipulate their size to take advantage of 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.

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