Essays on Industrial Policy and Communication

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Abstract

In my dissertation, I study government policies and their effects on the behavior of agents in the economy. Chapter 1 discusses the effects that subsidies for small firms have on aggregate output and productivity, with an empirical application to policies in India. Chapter 2 studies the effects of lowering communication costs on structural transformation, with an empirical focus on the roll out of Rural Free Delivery in the United States. Chapter 3 presents a method to estimate the effects that increased transparency have on deliberation, in particular that of the Federal Reserve Bank.

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I would like to dedicate my dissertation to my wife, Alex Roth, who is the best.

Preface

Chapter 1 considers the effects of programs which favor small firms, a common instrument of industrial policy around the world. I develop a theoretical framework to capture competitive equilibrium effects of these types of policies, and then use it to study a specific set of programs in India. In that context, I find that eligible firms benefit from the programs, but their competitors are hurt substantially.

Specifically, in 2006, the Indian government dramatically increased the set of firms eligible for a variety of small-firm-favoring programs, including directed lending, technical trainings, and investment subsidies. Most sectors contained some newly eligible firms, but there sectors varied in their exposure to the program. I exploit this variation, both across products and time, in order to separately identify the direct and indirect effects of the program, using difference-in-differences style techniques. I find that these programs "work": the sales of newly eligible manufacturing establishments grew by 30% relative to their peers. However, two-thirds of those gains were through business stealing; firms shrank when their competitors gained eligibility. There was substantial heterogeneity by sector characteristic: for products that tend to be sold locally, crowd-out was complete and there were no aggregate gains. For products which are more internationally traded, there was little crowd-out, at least domestically. Overall, I estimate that this reallocation of activity improved aggregate productivity in manufacturing in India by around .1%

Chapter 2 (based on work co-authored with James Feigenbaum), focuses on the role that information and communication technologies can play in promoting structural transformation. In order to make sales, producers first have to find buyers and discover their willingness to pay. If manufactured goods are more differentiated than agricultural commodities, then the equilibrium price of those goods will increase relatively more after the introduction of communication technologies. We test these predictions using a novel dataset on the roll-out of free postal delivery in rural communities in the United States at the turn of the 20th century. We use newspaper subscriptions as a proxy first stage, and find that access to new post office services increased newspaper circulation. Investment in manufacturing significantly increased in counties with more new free delivery routes.

Chapter 3 (based on work co-authored with Michael Egesdal and Michael Gill), studies the effect that transparency has on government agencies. We develop new methods for analyzing speech, in order to study how the behavior of the Federal Open Market Committee changed after the statutory enforcement of transparency laws in 1993. We develop "dictionary"-based methods for text analysis, which allow both for analysis of technical language and for easy-to-understand comparisons across settings. Additionally, we develop a theoretical model to explain why committee members' speech changes after an increase in transparency in light of career concerns. We examine the text of various Federal Reserve documents from 1976-2007, covering years in which the FOMC knew its deliberations would eventually be made public, and years in which it believed the transcripts would be kept private. We show that the introduction of transparency led meeting discussions to be more similar to the always-public press releases, and that only some of this change can be explained by a shift away from non-economic conversation in the meetings.

1 Equilibrium Effects of Firm Subsidies

1.1 Introduction

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

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

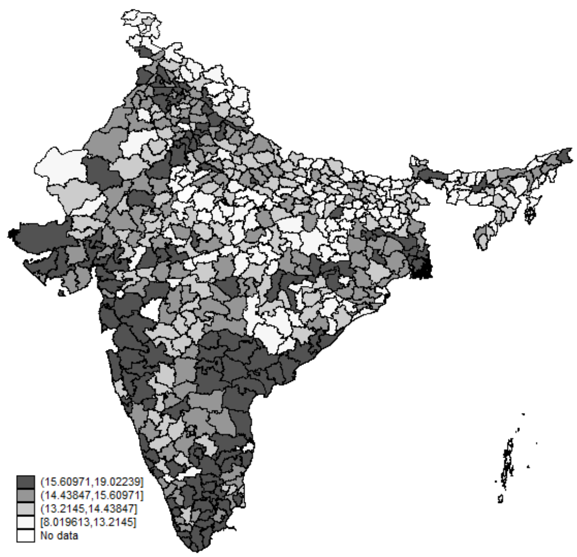
[1] For instance, other than the UK, each of the G8 countries have state-backed institutions designed to support small firms (Greene and Patel 2013). Bannock 1997 argues that, for all regions, loan guarantee programs are "the rule rather than the exception." See Mor et al. (2013) for a recent report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

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

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

In this paper, I study small firm subsidies in India, leveraging a 2006 policy change relaxing the eligibility requirements for a variety of programs.[4] The newly eligible firms represented around 15% of the formal manufacturing sector. Most sectors included some newly eligible firms, and there was substantial heterogeneity in the extent to which different sectors were exposed. Figure 1 shows the distribution of newly-eligible firms throughout India.

Figure 1: Distribution of Small Firms Across India



This figure plots the share of output in 2006 produced at "small" formal firms for each district of India covered by the ASI

My empirical strategy is derived using a Melitz-style framework, with multi-product firms. I assume that the policy change led the newly eligible firms to face lowered costs of inputs, leading to two effects on firm performance. The first, which I term the *direct* effect, reflects the gains to newly eligible firms. The second, the *indirect* effect, captures the equilibrium effects resulting from newly eligible firms' growth. While only newly

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

subsidized firms are directly affected, all firms may be indirectly affected.[5] Using data that is representative of all manufacturing activity in India, I leverage variation in time and firm characteristics in order to separately identify the direct and indirect effects of the policy change. While I apply the technique to estimate the effects of subsidies for small Indian firms, I derive a more general relationship capturing the effect on a firm's revenue of a change in their competitors' prices. This technique is therefore applicable to a wide variety of settings where researchers are interested in understanding the extent of product market spillovers. This strategy allows for a direct calculation of the extent of equilibrium effects, as the estimated indirect effect is a sufficient statistic for the elasticity of aggregate growth with respect to private growth.

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

I find that the programs had large direct effects, as newly eligible firms increased their sales by around 30%. This finding is consistent with Banerjee and Duflo (2014), Sharma

[5] My framework abstracts from other potential general equilibrium effects. For instance, three possible sources of these other effects are a) if firms distorting their size in order to maintain eligibility, b) if the policy change affected the prices paid by firms whose eligibility status was unchanged, and c) if the costs of raising revenue to pay for the subsidies affected the economy as a whole. While I cannot reject these argument,s I find no evidence of distortions in the firm-size distribution around the cutoff, nor that the policy change affected the input prices of the newly eligible firms' competitors. In all of the regressions, I include fixed effects for each year in order to control for economy-wide general equilibrium effects.

(2005), and Kapoor et al. (2012), which study earlier eligibility changes for a similar set of programs in India. Calibrating the model with the estimated coefficients can explain around 2/3 of the actual policy-induced growth of the newly eligible firms.

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

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

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

Second, I contribute to the empirical literature studying peer effects (Manski 1993; Angrist 2014), in particular among firms.[9] The urban economics literature focuses on Marshallian channels, through which firms interact with each other through goods, workers, and ideas (Ellison et al. 2007; Hanlon and Miscio 2013). A related body of work studies how firms are affected by increased competition following trade shocks (De Loecker et al. 2012; Sivadasan 2006), FDI (Aitken and Harrison 1999; Keller and Yeaple 2003), and

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

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

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

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

research and development (Jaffe 1986; Bloom et al. 2013). Of particular relevance to my work, Burke (2014) studies the equilibrium effects of credit programs which help farmers store their crops, Busso and Galiani (2014) study the effect of entry in retail markets on prices and quality, and Acemoglu et al. (2012) and Acemoglu et al. (2014) study how shocks to particular industries affect the economy as a whole through input-output networks. I show that the effect of competition due to a policy shock can be estimated using strategies similar to "linear-in-means" models often used to study peer effects in other settings, since the share of subsidized activity is the source of the indirect effect. I have collected detailed data allowing me to study product markets instead of the more commonly used industry codes, and I show that this is essential for studying the aggregate effects of subsidies, since measurement error on the extent of competition would cause a regression to understate the magnitude of the indirect effects relative to the direct effects.

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

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

Programs which subsidize targeted firms can have significant effects on aggregate output and productivity, and this paper empirically studies the magnitude of these effects in the context of a large-scale policy change in India. In Section 1.2, I describe small firm subsidies in India. In Section 1.3, I develop a model for estimating the aggregate effects of a change in targeted subsidies, and in particular show how to decompose the aggregate effect of subsidies into direct and indirect components. Section 1.4 describes the data, the construction of the exposure measures, and the identification strategy. Section 1.5 estimates the direct effects of the policy change, and Section 1.6 expands the analysis to estimate the indirect effects. Section 1.7 discusses the effect of the program on aggregate productivity, and Section 1.8 concludes.

1.2 Institutional Background

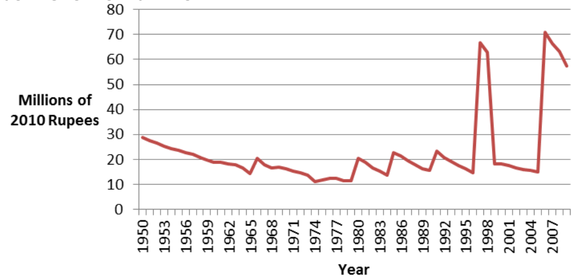
1.2.1 Overview of small-scale firm policies in India

The Indian government has had a ministry dedicated to small-scale enterprises since 1954. Eligible firms were originally those with under 500,000 rupees in fixed assets. Over time, as can be seen in Figure 2, the real fixed asset cut-off has changed, although most of the policy changes until the late 1990s were to keep pace with inflation. Banerjee and Duflo (2014) and Kapoor et al. (2012) study the policy change in 1999, when the office was renamed the Ministry of Small Scale Industries and Agro and Rural Industries and the eligibility criteria were tightened. In 2001, that Ministry was split into two distinct units, the Ministry of Small Scale Industries and the Ministry of Agro and Rural Industries. I start my empirical analysis in this year.

With the passage of the Micro, Small, and Medium Enterprises Development Act of 2006, the federal government's small firm programming was consolidated once again, into the Ministry of Micro, Small, and Medium Enterprises. The Act expanded the definition of who was eligible for small-firm programs, and introduced several new programs.

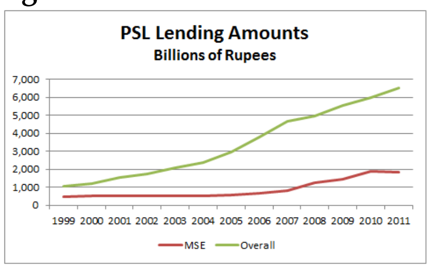
Figure 2: Small Firm Subsidies in India

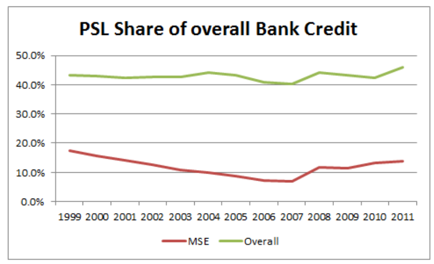
Panel A: Maximal size cutoffs over time



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

Panel B: Lending over time





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

Before the 2006 policy change, establishments with a value of under ten million rupees in nominal investment in plants & machinery were eligible;[11] the Act raised the size cutoff to fifty million rupees. Eligibility for the programs is exclusively determined by an establishment's nominal accumulated capital investment (ignoring depreciation), limiting establishments' ability to use accounting tricks in order to subvert the intent of the process.[12,13] Before the policy change, establishments who were above the cutoff in 2006 likely did not expect to gain eligibility without selling assets. At the time, newly eligi-

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

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

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

ble establishments represented around 15% of all formal manufacturing output, and, the majority of firms faced some competitors whose eligibility status changed in 2006.

The Reserve Bank of India (RBI) manages "Priority Sector Lending," which directs banks to provide 32% to 40% of their loan portfolio to clients designated eligible.[14] While the RBI maintained administrative control of the Priority Sector program, they tend to defer to the Ministry of Micro, Small, and Medium Enterprises for determining the eligibility criteria in manufacturing.[15] Public banks also have sub-targets: 45% of the priority sector credit must go to agriculture, and 25% to weaker sections. In general, the targets and sub-targets are binding (Nathan India 2013).[16]

In addition to directed credit, there are a variety of other programs designed to benefit small firms. Many states have developed preferential procurement policies for local governments, forcing them to make purchases from small firms. Furthermore, the Ministry of Micro, Small, and Medium Enterprises manages a large portfolio of its own activities - in 2012/2013 it had a budget of just under $130 million. Of that, 45% went to "Quality of Technology Support Institution & Programmes," which includes programs such as advising on new manufacturing techniques, granting access to material testing facilities, product design, and training programs, 15% went to "Promotional Services Institutions and Programmes," 13% to "Infrastructure Development & Capacity Building," and the rest to a variety of other programs including training, export subsidies, credit guarantees, and the development of an agency to keep credit scores for small firms.[17] Since the

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

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

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

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

2006 policy change changed the eligibility requirements for all of these programs, I do not attempt to estimate the impact of the specific components separately.

1.3 Analytical Framework

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

1.3.1 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 final good Q_s is produced by a representative firm in a perfectly competitive market. The utility function of the representative consumer over the S sectors is

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

where c is consumption of the outside good, whose price is normalized to one, and the post-tax income of the consumer is assumed to be I (in partial equilibrium).[18] In Appendix Section 5.1.1, I show that similar predictions to the ones in this subsection can be

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

derived (i) in a Lucas span-of-control style model, with decreasing returns to scale and homogenous output in each sector, and (ii) when the consumer has CES preferences over the final goods. I choose this specific form so as to avoid any cross-industry (demand) spillovers in the predictions.[19] The first-order condition of the final-good consumer ensures that the revenue in sector s will be

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

In each sector, this firm combines the output q_{js} of each of the N intermediate goods producers with a CES production function, adjusting for the (fixed) quality a_{js} of each of the intermediates:

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

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

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

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

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

where A_{js} is firm-specific TFP, and $\alpha \in (0,1)$ is the capital intensity. Following HK, I allow for distortions which change the marginal products of capital $\left(\tau_{k_j}\right)$ and labor $\left(\tau_{l_j}\right)$ for

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

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

each firm, respectively the "capital wedge" and the "labor wedge."[21] A growing literature seeks to micro-found these distortions (Peters 2013; Buera et al. 2011); in this paper I focus on the change in distortions arising from subsidies for which only some firms are eligible.

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

$$\pi_{js} = p_{js}q_{js} - \left(1 + \tau_{K_j}\right) RK_{js} - \left(1 + \tau_{L_j}\right) wL_{js},$$

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

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

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

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

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

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

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

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

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

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

1.3.2 The Effect of Changing Subsidies

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

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

good producer satisfy:[24]

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

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

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

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

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

Aggregating over all of the firms in each sector gives

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

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

The total change in revenue in a sector will be weighted average sum of the direct and indirect effects.[26]

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

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

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

and

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

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

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

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

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

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

As the across and within sector elasticities of substitution (respectively captured by ϕ and σ) change, so too will the indirect effect. As $\phi \to 0$ or $\sigma \to 1$, the indirect effect approaches 1, which implies complete crowd-out.[28] As ϕ increases, the indirect effect shrinks, such that there is no indirect effect if $\phi = \frac{\sigma-1}{\sigma}$, and positive spillovers if ϕ is larger. Furthermore, as σ_s increases (the good becomes more substitutable), the direct effect increases.

1.3.3 Multi-Product Firms

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

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

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

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

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

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

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

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

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

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

1.3.4 Trade and Observed Heterogeneous Product Characteristics

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

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

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

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

versus less-traded sectors. As a result, I can extend equation 9 to

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

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

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

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

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

A similar logic applies when there are many relevant sector characteristics. For instance, suppose some products have different elasticity of demand, where $\theta^{d,\, h}$ is the competitive effect in non-traded and high elasticity sectors , $\theta^{x,\, l}$ is the competitive effect in traded and low elasticity sector, and so on. Furthermore, assume that there is no effect of the elasticity of demand for traded sectors: $\theta^{x,l} = \theta^{x,h} = \theta^x$.

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

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

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

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

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

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

1.3.5 Location of Sales and Unobserved Heterogeneous Product Characteristics

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

Nevertheless, if a firm's growth crowds-out its within-state competitors, but is irrelevant for the producers located outside the state, that would suggest that states are different markets. It is therefore possible to identify the within-state and the outside-state indirect effects of subsidy programs even without information on the location of firms' sales,. 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 $\mu_{js}^{\varsigma} \equiv \frac{\sum_{j=1}^{N_s} \varsigma_{jk} \times e_j \times y_{js}}{\sum_{k=1}^{N_s} \varsigma_{jk} \times y_{ks}}$ as the share of within-state competition in sector s for firm j, and $\mu_{js}^{o} \equiv \frac{\sum_{j=1}^{N_s} (1-\varsigma_{jk}) \times e_j \times y_{js}}{\sum_{k=1}^{N_s} (1-\varsigma_{jk}) \times y_{ks}}$ as the non-state share. Including the geography of sales adjusts equation 9 to:

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

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

1.4 Data and Identification Strategy

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

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

I augment the ASI with the 2006 round of the National Sample Survey Organization's information on unorganized manufacturing establishments (NSS), which are explicitly

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

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

[34] Eligibility for all of the "small" firm programs in India are at the establishment level, although interviews suggest that there has been some confusion on this point.

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

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

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

[36] Several other projects have combined the datasets, such as Nataraj (2011); Chatterjee and Kanbur (2013); Kothari (2013); Ghani et al. (2014b) and García-Santana and Pijoan-Mas (2014).

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

1.4.1 Constructing Measures of Exposure

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

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

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

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

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

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

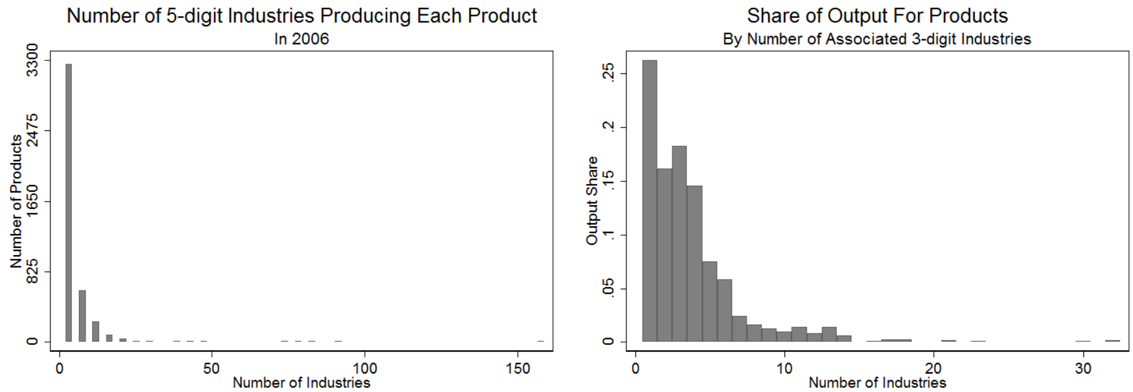
fairly stable over time: for firms who appear in the sample twice before the policy change, 93% are in the same category in the second-to-most recent year as in the most recent one. For firms who appear in the sample three times, 90% have the same classification in the third-to-most recent year as in the most recent one.

For similar types of questions, researchers tend to use industry codes as a proxy for competition. Likely this is due to data issues, since product-level data is often unreported. For estimating the aggregate effects of firm-specific shocks, using industry codes is likely to lead to biased results, since they are not supersets of product codes. For instance, in 2006, each 5-digit product code[42] was produced in a median of three 5-digit industries, and over 95% of output was of products produced in multiple industries. Figure 3 Column 1 shows the distribution of the number of industries producing each product. Using more coarse industry codes does not alleviate the problem, as the median product is produced in two 3-digit industries. Figure 3 Column 2 plots the total share of revenue from products produced in different number of 3-digit industries. Only 25% of revenue is from products produced in a single-3-digit industry products. Scrap iron, for instance, is sold by firms in 160 5-digit industries, and 32 3-digit industries.[43] In order to avoid these concerns, I construct the exposure measures at the product level. In Appendix Section 5.1.2, I derive the sources of bias when using industry codes instead of products to estimate the effects of competition, and show empirically that it will lead to understating the magnitude of the indirect effects.

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

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

Figure 3: Distribution of Multi-Industry Products



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

Crucially, the data is informative about the exposure shares from the perspective of the product. If instead the data was of firms with no sense of the population weights, then it would be difficult to generate unbiased estimates of the spillovers measures. When constructing the exposure measures detailed in equation 11 and equation 47, I must account for the fact that I do not observe every plant in India in every year. However, in 2006 I do observe a representative cross-section of all establishments. As a result, I can approximate the true exposure measures using the sampling weights. There are around 100,000 establishments for whom I observe their investment in plants and investment, with just over a third of them from the Annual Survey of Industries, and the rest informal firms.[44] In estimating the share of newly eligible firms for each sector, I use these establishments. There are roughly another 50,000 establishments in the ASI who were not sampled in

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

2006, but were observed before the policy change. As a result, the output-output matrix A (and the equivalent matrices for different product characteristics) includes a row and a column for each firm, and only the first 100,000 columns contain non-zero values. For those columns, I augment equation 10 with inverse probability weights $\frac{1}{p_j}$ so that

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

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

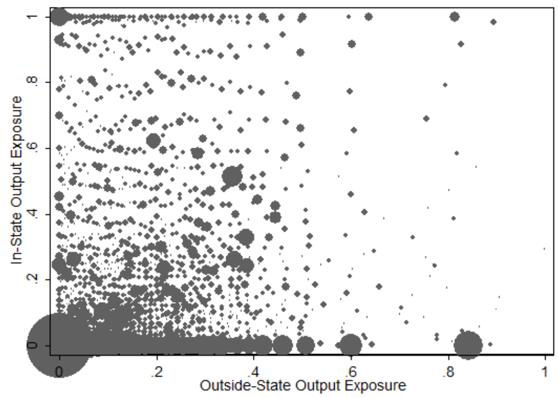
[45] A concern with this strategy may be that it is an inappropriate use of the probability weights, since the survey was only designed to be representative at a more aggregate level. As an alternate strategy, I also create an exposure measure where, instead of using the weights to estimate exposure in 2006, I combine the samples from every year, and for each firm keep its most recent pre-program observation. Given the design of the ASI, this should reflect a census of all manufacturing firms, albeit a census taken over several years (since there are 6 years of pre-program data and a rotating sampling frame, each existing firm should have been surveyed at least once in the period). I then ignore the sampling weights and calculate directly the exposure shares in this constructed census. The alternate exposure measure has a correlation over .7 with the value I use in the paper. Furthermore, in the appendix I show that regressions using the alternate measure leads to similar conclusions, in spite of the fact that this exercise naively abstracts from firm growth.

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

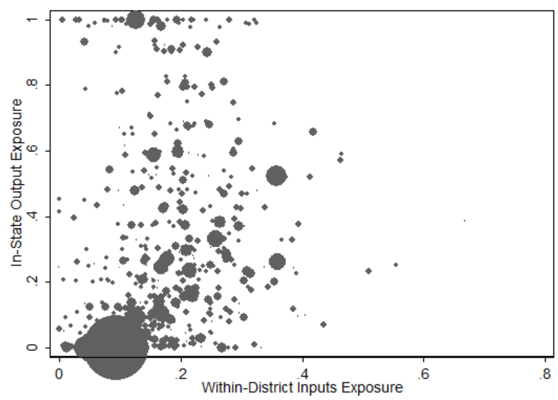
[47] There is a mechanical correlation between the within-state and all-India output competition measures, since a firm in the same state is also in the same country. Figure 4 avoids this problem by comparing the within-state output competition measure to the outside state measure.

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

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



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



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

In this paper, I focus on product-market competition. However, there are many measures of exposure to the program which may affect firms, such as within-local market competition for primary inputs. In Appendix Section 5.1.3, I discuss this briefly, as well as various empirical issues which make geographic indirect effects of subsidies difficult to identify in this context. In Figure 4 Panel B, I plot the relationship between exposure through in-state product markets and in-district exposure through (imputed) primary inputs. The correlation of the two measures is .1, suggesting that it may not be the case that firms who make the same products as small firms are also in the same districts.

1.4.2 Product Characteristics

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

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

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

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

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

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

1.4.3 Identification Strategy

The first part of the estimation strategy is to estimate relative effects, using a difference-in-differences approach. The only firms for whom I have panel information are the formally registered ones, and the regressions are restricted to the firms in the ASI. Defining $\tilde{priority}_{it}$ for firm i taking advantage of small-firm subsidies in year t, I follow equation 9 (for now, ignoring the indirect effect):

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

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

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

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

Post is a dummy indicating a survey taken after the policy change. In each specification I control for a cubic polynomial in the running variable, the firm's historical value of capital immediately before the policy change. For each outcome for the direct effects I run four regressions: one with all establishments and controls for assets, one with additional fixed effects for $state \times Post\ Reform$ and $3 - digit - industry \times Post\ Reform$, these same

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

two specifications, but restricting the analysis to single-plant firms. I always include firm and year fixed effects, and observations are weighted by the inverse of their sampling probability, provided by the ASI.[53] Standard errors are clustered at the firm level to adjust for heteroskedasticity and within-firm correlation over time.

A firm's competitors gaining access to the program may also have effects on growth. To understand how exposure to the program through competitive channels matter,[54] I leverage the fact that the share of production by newly eligible firms varies dramatically at the product level, as shown in Figure 4. For each exposure measure, I calculate the weighted average share of a firm's sectors which are newly eligible. For instance, for the indirect effects over all products, the exposure calculation is $\sum_s \omega_{js} \cdot \mu_s$, as outlined previously. To estimate the indirect effect for internationally traded products, I also include the exposure measure $\sum_s \omega_{js} \cdot \mu_s \cdot l_s$. To estimate the corresponding θs, I augment equation 9:

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

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

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

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

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

sure to the policy change using their product mix before the policy change. This avoids issues with firms changing their product mix in response to the program.

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

Table 1 shows summary statistics for the main outcome and explanatory variables in the paper for each year in its most recent pre-program observation. The newly eligible firms are, by definition, larger than the always eligible establishments, and smaller than the never eligible ones. They are most exposed to the program through product markets, which is consistent with the fact that firms are exposed to themselves. The always and never eligible plants share a similar exposure to the policy change of about 20%.

1.5 Estimating the Direct Effect of Eligibility

In this section, I begin by demonstrating that firms who gained eligibility expanded relative to the other formal firms in the economy. Variation in eligibility comes from the historical value of capital at each firm before the policy change. One concern with this strategy would be if firms of different qualities manipulated their sizes, so that part of the effect of policy change would come from from the less-distorted behavior of particular firms, instead of the policy change itself (Lee and Lemieux 2009; McCrary 2008). In order to test for this, Figure 5 shows the distribution of log plants and machinery, the criteria determining firm eligibility, immediately before the policy change, as well as the old and new boundaries for eligibility in 2006 around the cutoff.

[55] The implicit model is that had the firm stayed open, it would have had one (real) rupee of each outcome.

[56] See Woolley 2014; Burbidge et al. 1988 and Carroll et al. 2003 for further information about this approach.

[57] The correlation of $ln(sales+1)$ and the inverse hyperbolic sine of sales is over .99

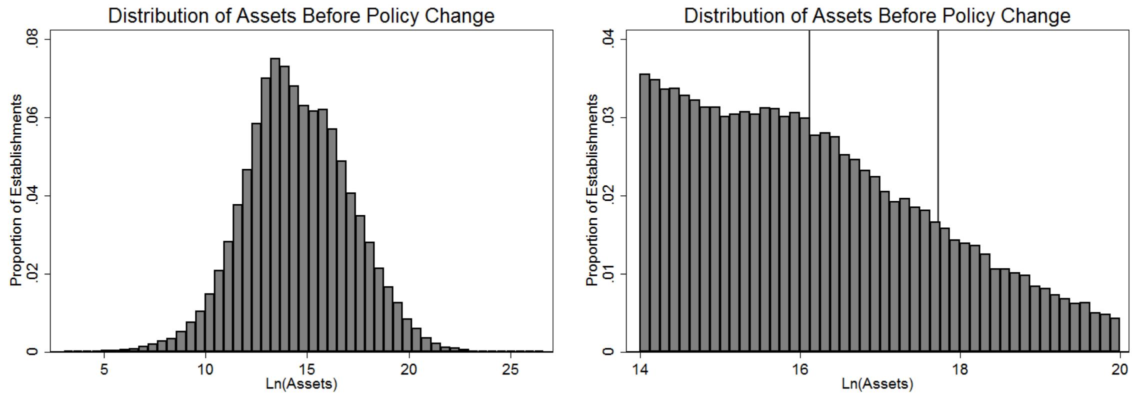
Table 1: Summary Statistics

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

Summary statistics for all factories are based on ASI data. Only firms who declared assets before the policy change are reported, and this is the firm's value in the most recent pre-program year. Micro firms are imputed as being always eligible, small firms as newly eligible, and big firms as never eligible. Monetary values are denoted in real (2004) rupees. Sampling multipliers were not used, since every (formal) firm should be in the data. ""Census"" scheme firms are those with employment over 100 workers, and therefore are sampled with certainty. Probability weight is the inverse sampling probability. Total costs imputed, as described in the data. The construction of the output exposure measures is discussed in the text, and corresponds to the (weighted average) share of a firm's competitors who were newly eligible. "Traded" is defined as "above median share of production exported." The construction of TFPQ, TFPR, and the wedges is described in section 1.7. TFPQ is calculated by assuming a constant firm markup given CES utility and a Cobb-Douglas production function, TFPR is TFPQ*price, and each input wedge corresponds to a calculation of how much ""extra"" the firm pays for each input. It is reported without normalizing for industry.

Any discontinuity at the old firm size cut-off is reasonably small - it is a gap of similar magnitude to other jumps at other, policy-irrelevant, sizes - and there is no evidence that firms anticipated the new policy change and and bunched around the future cutoff in an anticipatory fashion. Table 2, Panel A, tests for bunching around the cut-off formally, following McCrary (2008). There is no significant break in the firm-size distribution at the old or new size-cutoff, neither before nor after the policy change. Panel B reports the

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



This figure demonstrates the distribution of nominal assets for all firms immediately before the policy change. Table 2 uses the test developed by McCrary to formally test for a break in the firm-size distribution around the cut-offs. Source: ASI

results of regression discontinuity estimates of sales, liabilities, and employment costs, again around the two cut-offs, before and after the program change.[58] None of the 12 estimates are statistically significant.

1.5.1 Plots of Program Effects

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

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

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

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

Table 2: Tests for Firm-Size Manipulation

Panel A. McCrary Density Tests

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

Panel B. Regression Discontinuity Tests

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

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

2008 which persists through 2011. Not only do the newly eligible firms benefit from the policy change relative to their peers, but the gains are persistent.[59]

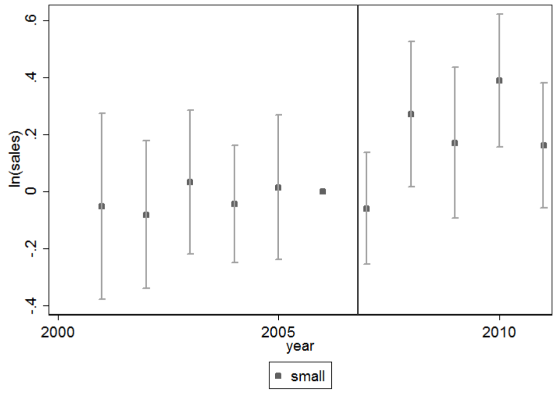
1.5.2 Effects of the Program on Firm-level Economic Outcomes

Table 3 estimates equation 17 for sales, continued production, liabilities, and (imputed) costs:

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

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

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



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

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

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

Table 3: Differences in Differences Estimates of the Direct Effect of Firm Subsidies

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

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

C of Table 3 is consistent with their result: newly eligible firms expand their borrowing, relative to their peers, by around 30%. Panels D suggest that the increase in borrowing is not due to an infra-marginal adjustment in funding sources, as firms adjust their inputs in response to the program, by a similar magnitude (the point estimates are insignificantly larger than those for sales) to their adjustment in sales and liabilities.

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

1.6.1 The Effects of Output Competition, Treating All Products Similarly

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

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

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

Table 4 presents the effects of output exposure on firm performance. Columns 1 and 2 present the exposure effect treating each state/product combination as a separate market. For sales, the coefficient on within-state output exposure is around 70% (in magnitude) of the coefficient of newly eligible, and has the opposite sign. Since, as outlined in equation 9, $\hat{y}_j = \beta e_j - \theta\beta\left(\sum_{s=1}^{S}\omega_{js}\cdot\mu_s\right)$, the aggregate gains, $(1-\theta)\,\beta$, can be calculated by adding the coefficients on the direct and indirect effects. Therefore, Table 4 Column 1 implies that 30% of the private gains from the program are translated into aggregate gains (for instance, the regressions predict that if *every* firm gained access to the subsidies, every firm would expand by 10%). The relative magnitude is consistent across tables.[60]

Columns 3 and 4 include both within-state and outside-state competition. Within-state exposure to the program is substantially more important to firms than exposure from firms in different states: the coefficient on across-state exposure is close to zero, and the magnitude and precision of the effect of within-state competition remains reasonably unchanged with the inclusion of the outside-state exposure measure. As outlined above, since location of sales is unobserved, it is difficult to distinguish if the small magnitude

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

Table 4: Differences in Differences Estimates of the Indirect Effects of Firm Subsidies on Output

	(1)	(2)	(3)	(4)
A. Effect on ln(sales)				
Post Reform * Small Firm	0.332***	0.358***	0.329***	0.356***
	(0.078)	(0.079)	(0.078)	(0.079)
Post Reform * In-State Exposure	-0.233**	-0.226**	-0.208*	-0.217*
	(0.111)	(0.113)	(0.111)	(0.114)
Post Reform * Outside-State Exposure			-0.08	0.136
			(0.148)	(0.154)
Number of Observations	274724	274724	272843	272843
R-squared	0.751	0.751	0.751	0.751
B. Effect on firm continuing to exist				
Post Reform * Small Firm	0.332***	0.358***	0.329***	0.356***
	(0.078)	(0.079)	(0.078)	(0.079)
Post Reform * In-State Exposure	-0.233**	-0.226**	-0.208*	-0.217*
	(0.111)	(0.113)	(0.111)	(0.114)
Post Reform * Outside-State Exposure			-0.08	0.136
			(0.148)	(0.154)
Number of Observations	271921	271921	270088	270088
R-squared	0.729	0.725	0.729	0.725
Controls for Post Reform*Assets	Y	Y	Y	Y
Fixed Effects for Post Reform * 3-digit Industry and State	N	Y	N	Y

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

on across-state competition is due to the fact that firms in different states produce fundamentally different products even if they share a product code, or if firms do not produce much to sell in different states. However, the fact that firms are not significantly affected by their out-of-state competitors gaining access to subsidies suggests that treating each state as a separate market is reasonable.

Panel B shows that within-state output exposure predicts firm closure as well: if every one of a firm's competitors gained access, it would be 1-2% less likely to continue

Table 5: Differences in Differences Estimates of the Indirect Effects of Firm Subsidies on Inputs

	(1)	(2)	(3)	(4)
A. Effect on ln(total liabilities)				
Post Reform * Small Firm	0.036***	0.038***	0.036***	0.038***
	(0.004)	(0.004)	(0.004)	(0.004)
Post Reform * In-State Exposure	-0.018***	-0.013*	-0.016***	-0.013*
	(0.006)	(0.007)	(0.006)	(0.007)
Post Reform * Outside-State Exposure			-0.010	0.012
			(0.009)	(0.009)
Number of Observations	298426	298426	296373	296373
R-squared	0.622	0.616	0.623	0.617
B. Effect on ln(total costs)				
Post Reform * Small Firm	0.360***	0.383***	0.362***	0.382***
	(0.075)	(0.076)	(0.075)	(0.076)
Post Reform * In-State Exposure	-0.243**	-0.243**	-0.221**	-0.236**
	(0.104)	(0.106)	(0.104)	(0.106)
Post Reform * Outside-State Exposure			-0.152	0.048
			(0.147)	(0.154)
Number of Observations	274724	274724	272843	272843
R-squared	0.751	0.748	0.752	0.748
Controls for Post Reform*Assets	Y	Y	Y	Y
Fixed Effects for Post Reform * 3-digit Industry and State	N	Y	N	Y

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

producing. Again, outside-state competition has low predictive power. In both panels, the coefficient on gaining access increases slightly relative to Table 2 - consistent with the logic of the model that firms will also indirectly affect themselves. Table 5, Panels A and B look at the effect of exposure on firms’ costs. Within-state competitor’s access to the subsidies predicts a substantial decline in firm size, both for costs and for liabilities, and across-state competitors do not exert significant competitive pressures.[61]

1.6.2 Trade and Output Competition

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

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

where now the indirect measures used are within and outside state output markets, and those markets for traded products. This regression has a similar motivation to a triple interaction, since the goal is to test if the difference-in-difference effects of output exposure is different for products which are traded and those which are not. However, since firms cannot be separated into those who produce only traded goods and those who produce only non-traded goods, it cannot be estimated using a standard difference-in-difference-in-differences approach. In keeping with the spirit of the triple differences regression and to control for differences between firms who produce more traded goods and those who produce fewer, I create a measure for each firm capturing the share of its outputs which (before the policy change) are traded.[62] I then include $Post \times share\_traded$ as a control.

Table 6 shows the coefficients from estimating equation 20 on firms' intensive and extensive margin production choices. Column 1 includes exposure measures for "within-

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

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

Table 6: Differences in Differences Estimates of the Indirect Effects of Firm Subsidies on Output and Trade

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

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

state" output competition and "within-state output competition" for traded products, and is my preferred specification for understanding the indirect effects of the policy

Table 7: Differences in Differences Estimates of the Indirect Effects of Firm Subsidies on Inputs and Trade

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

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

change. The sum of the two exposure measure coefficients is close to (and is never statistically distinguishable from) 0 , suggesting no crowd-out for more-traded goods.[63] Conversely, the coefficient on overall exposure is almost identical to the direct effect (and again their sum is not statistically distinguishable from 0), which implies almost complete crowd-out for less-traded goods. Column 2 shows that this result holds even within 3-digit industries and states.

Columns 3 and 4 include the coefficients for outside-state competition as well, both for traded and non-traded goods. The coefficients on within-state exposure are of similar magnitudes and precisions, and the coefficients on outside-state competition insignificant and smaller than those for within-state competition, especially with fixed effects for each industry. The qualitative patterns are similar on the extensive margin, shown in Panel B, although with less statistical precision. Panels A and B in Table 7 show similar findings for credit and costs. However, the coefficient on traded output exposure - while consistently of similar magnitude to the coefficient on general exposure - is only significant for sales.

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

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

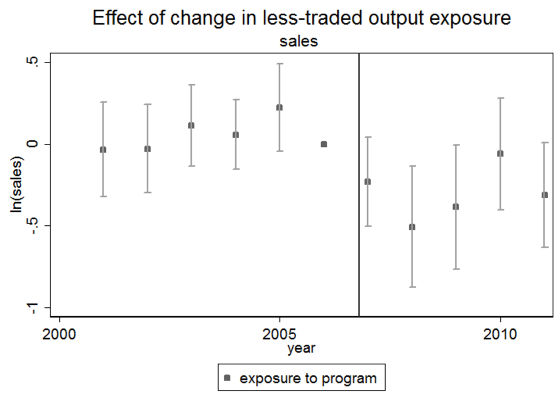
In Figure 7 Panel A, I plot the coefficients and 95% confidence-intervals for the exposure coefficients, which reflect the effect of the program on less-traded goods. Much like in Figure 6, there does not appear to be a pre-program trend in the effect of exposure to the

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

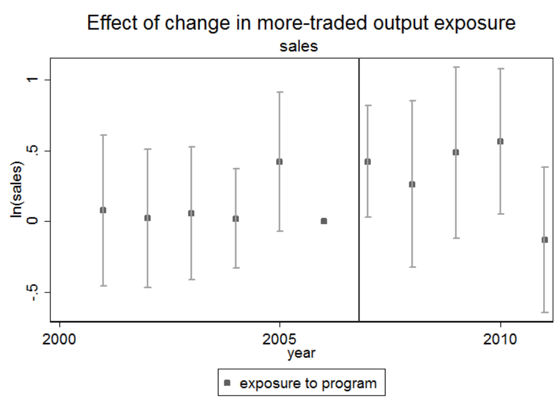
program. However, after the implementation, exposed firms lose sales. Panel B plots the estimates and standard errors of $\Theta_t + \Theta_t^x$, representing the indirect effect of the program change through traded products. In each post-program year, the effect is weakly positive, consistent with the results in Table 6.

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

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



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



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

1.6.3 Permutation Tests

In the spirit of Fisher (1935), I undertake three different permutation tests in order to examine how unlikely the regression results would be if there were no true effect of the program (Rosenbaum 2002; Ho and Imai 2006; Sinclair et al. 2012; Shue 2013). Research on peer effects are a natural setting for permutation tests, since one can permute a) the source of the shock, b) the connections of the network, and c) the characteristics of the network. Using Monte Carlo simulations of 1000 iterations, these tests construct placebo estimates around the null hypothesis that the subsidies do not matter, that output competition does not matter, and that trade does not matter. I report the results in Table 8.

Table 8: Permutation Tests of Differences in Differences Estimates of Placebo Firm Subsidies

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

Column 1 corresponds to the real data, predicting the indicated outcome in difference in differences specification (as reported in Table 6, column 1). Columns 2-4 report the mean and standard deviation of 1000 runs of a difference-in-difference regressions, using different placebo treatments. Column 2 permutes which firms gained access to subsidies in 2006, while maintaining actual products produced and the tradability of those products. Column 3 maintains the the new eligible firms in the data, but permutes which products each firm produces, and therefore each firm's indirect exposure to the policy change. Column 4 maintains both the new eligible firms and the products they produce, but permutes which products are "traded." Each regression included cubic controls for Post Reform * Assets. Stars are omitted. Source: ASI

In the first set of tests, for each iteration establishments are randomly assigned to the "newly eligible" group, regardless of their true assets in 2006, maintaining the same share of newly-eligible firms as in the real data. Furthermore, given the placebo eligibility changes, I construct each firm's (placebo) exposure through output competition, maintaining the products that the firms actually produce in the data and if those products are traded. I then re-estimate the effects of eligibility $\hat{\beta}^{placebo}$, as well as $\hat{\theta^d}^{placebo}$ and $\hat{\theta^x}^{placebo}$, using equation 20, as outlined in the previous subsection.

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

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

Table 8 shows the means and standard deviations for these regressions, presented next to the results from Table 6 Panel A Column 1 (without stars). Column 2 shows the results from permuting the assignment of eligibility. The estimates are all small and close to 0; neither placebo eligibility nor exposure to placebo eligible firm predicts a change in firm behavior. Column 3 shows the results from permuting the network of production. The estimates on new eligibility are similar for those from Table 3,[64] while the estimates on the effect of placebo exposure are close to 0. Column 4 shows the results from permuting the tradability of products. The mean estimated coefficient on new eligibility and exposure

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

are reasonably similar to from Table 4, but the estimates on placebo tradability are close to 0. In all cases, the coefficients coming from permuted data are are closer to 0 than their real world counterparts in over 99% of iterations.

1.6.4 Other Product Characteristics and Output Competition

There are a variety of product characteristics which could affect both the direct and indirect effects of the subsidy program. Furthermore, it may be that traded products have some particular feature which make them less affected by the program, rather than that they are actually traded. To test for this potential omitted variables bias, I focus on three product characteristics besides the tradability: the elasticity of substitution across products (from Broda and Weinstein 2006), the capital/labor ratio (calculated in the Indian data), and a measure of external finance requirements (the fraction of total liabilities divided by imputed flow costs). For the latter two measures, I first calculate firm-level measures of the capital/labor or liabilities/primary input cost ratios. For each product, I calculate the weighted average of these measures, where the weights are the share of the product produced by each firm. I then split the products by their median values.

I examine heterogeneity in the direct effect of subsidies through the four product-level types on sales by adapting equation 13 to various permutations of the form

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

and reporting β_1, β_2 and the Θs.

The results are reported in Table 9, and I only report effects on sales. Columns 1 and 2 test if firms producing higher shares of tradable products benefit relatively more from access to the program. The results are positive although statistically insignificant, weakly suggesting that exports may be an important margin through which credit constraints are

Table 9: Differences in Differences Estimates of Heterogeneous Effects of Firm Subsidies

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

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

binding (Beck 2003; Manova 2012). This is consistent with Kapoor et al. (2012), who find that Indian firms increased their exports in response to the 1999 policy change. Columns 3 and 4 include interactions for the various firm level measures with new eligibility. Capital intensity strongly negatively predicts benefiting from the program, and credit intensity has a strong and positive relationship. The extreme magnitudes may be mechanical, as the two measures are highly correlated (the correlation is .784), and the direct effect does not change substantially (although the precision of the estimate decreases substantially), but the latter result is consistent with theories of financial dependance (Rajan and Zingales 1998; Gupta and Yuan 2009), and the former may be because, for instance, capital is collateralizable, but labor is not, so increased access to credit is more helpful for firms reliant on labor.

Columns 5 and 6 add heterogeneous indirect effects for the sector characteristics. The original indirect exposure measure's magnitude remains reasonably unchanged, as does the coefficient on exposure through traded products (although again the precision of the estimates decreases). The decline in firm sales is relatively larger from exposure through borrowing intensive products - which may be a function of the larger direct effect - and less through exposure of products with low elasticities of substitution, consistent with the model.

1.6.5 Aggregate Effects of the Policy Change

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

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

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

Table 10 Panel A tests the effects of exposure on the value of sectoral output, and finds effects consistent with the firm-level regressions presented earlier. Aggregate output is predicted to grow by around 10% if a sector went from having no subsidized firms to only subsidized firms, and the estimate is not significantly different from 0. The triple-differences regression shows that there are no predicted output gains for less-traded products, and output gains of around 40% for more-traded products, again consistent with the results in Table 4. Panel B tests the effect of the program change on firm quantities. Increased program exposure increases the quantities produced in the sector by around 35%, with most of the effects from the more-traded products. Panel C tests the effects of the average price of the product. A fully exposed products is predicted to have a price around 25% lower than a non-exposed product. Unexpectedly, the lower prices are (weakly) concentrated in the more-traded products.[65] Across all specifications, the inclusions of product and state by time period fixed effects does not qualitatively change the results.

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

Table 10: Differences in Differences Estimates of the Aggregate Effects of Firm Subsidies

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

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

1.7 Effects of the Reallocation of Economic Activity

It has been argued that within-sector factor misallocation is key cause of low productivity in developing countries, and a common argument for small-firm subsidies is that they increase aggregate productivity (and a common argument against them is that they decrease aggregate productivity). In the spirit of Hsieh and Klenow (2009), I examine the effects of productivity coming from one specific channel: the change in the variance of productivities across firms. Subsidies can either increase the variance of revenue productivity across firms (lowering aggregate TFP), or the opposite. In order to make the productivity results tractable, and to make my results directly comparable to Hsieh and Klenow (2009), I adopt several of their assumptions. First, I treat each establishment as producing a single product in its self-reported industry. Second, the appropriate CES aggregation is at the 3 or 4 digit industry level. Third, the consumer has Cobb-Douglas utility on consumption from each industry: $U = \sum Y_i^{\chi_i}$ (this last assumption is somewhat supported in the data, since there is complete crowd-out for non-traded products). Furthermore, as in the model section, I abstract from effects of the policy beyond the change in subsidies for the newly eligible firms.[66]

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

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

[66] For instance, I do not account for misallocation caused by firms distorting their size in response to the program, described by Garicano et al. (2012), since in Table 2 I find no evidence of manipulation.

where c_s is a sector specific constant.[67] Since the production function is Cobb-Douglas and demand is CES, it is possible to back out marginal cost given the price, and therefore quantity productivity given total inputs and outputs.[68]

1.7.1 Effect of the Policy Change on Firm Productivity and Relative Prices

To show the effect of the policy change on productivity and the relative cost of factors, Tables 11 and 12 demonstrates the results of estimating equations of the form

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

for the still-existing firms.

Table 11 Panel A demonstrates the results on structurally-estimated quantity productivity (TFPQ), both for direct and indirect access. The coefficients are consistently low and insignificant: there do not seem to be large productivity adjustments in response to competition. Panel B looks at revenue productivity, assuming a Cobb-Douglas production function. While the indirect effects do not predict significant changes to revenue productivity, the direct effect does: after the program change, small firms have lower revenue productivity, by around 4-5%. Figure 8 plots event-study coefficients of the effect of being small on revenue productivity, following equation 19. Again, there does not appear to be a significant pre-trend in TFPR, nor a large effect in the April 2007 survey, but a sustained decreased afterward.[69]

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

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

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

Table 11: Differences in Differences Estimates of the Effects of Firm Subsidies on Firm Productivity

	(1)	(2)	(3)	(4)	(5)	(6)
A. Effect on Quantity Productivity: ln(TFPQ)						
Post Reform * Small Firm	0.026	0.037*	0.015	0.021	0.015	0.019
	(0.022)	(0.022)	(0.024)	(0.024)	(0.024)	(0.024)
Post Reform * In-State Exposure			(0.043)	0.061*	0.02	0.048
			(0.033)	(0.034)	(0.037)	(0.038)
Post Reform * In-State					0.093	0.06
Traded Exposure					(0.072)	(0.072)
Number of Observations	260379	260379	239675	239675	239675	239675
R-squared	0.831	(0.832)	(0.822)	0.823	0.822	0.823
B. Effect on Revenue Productivity: ln(TFPR)						
Post Reform * Small Firm	-0.049***	-0.033**	-0.050***	-0.040**	-0.051***	-0.040**
	(0.015)	(0.015)	(0.016)	(0.016)	(0.016)	(0.016)
Post Reform * In-State Exposure			0.011	0.029	0.000	0.024
			(0.022)	(0.023)	(0.025)	(0.026)
Post Reform * In-State					0.046	0.023
Traded Exposure					(0.049)	(0.049)
Number of Observations	257233	257233	236655	236655	236655	236655
R-squared	0.771	0.773	0.758	0.760	0.758	0.760
Controls for Post Reform*Assets	Y	Y	Y	Y	Y	Y
Fixed Effects for Post Reform * 3-digit Industry and State	N	Y	N	Y	N	Y

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

The change to TFPR is decomposed in Table 12. Panel A demonstrates that firms' capital wedge decreases by around 3.5% when they gain access to subsidies, although the coefficient tends to be marginally insignificant. The labor wedge of the newly eligible firms falls by around 5%. For both outcomes there are no statistically significant (or large) indirect effects. This is evidence consistent with the modeling assumption that the program change affects the prices paid by the newly eligible firms, without directly affecting the prices paid by their competitors. The fact that the labor wedge changes by some-

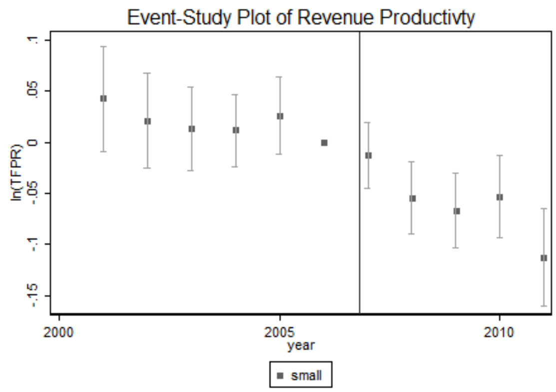
Table 12: Differences in Differences Estimates of the Effects of Firm Subsidies on Firm Input Costs

	(1)	(2)	(3)	(4)	(5)	(6)
A. Effect on Capital Wedge: ln$(1+\tau_k)$						
Post Reform * Small Firm	-0.036*	-0.026	-0.034	-0.028	-0.034	-0.028
	(0.019)	(0.019)	(0.021)	(0.021)	(0.021)	(0.021)
Post Reform * In-State Exposure			-0.002	0.014	-0.006	0.017
			(0.031)	(0.031)	(0.035)	(0.035)
Post Reform * In-State					0.016	-0.014
Traded Exposure					(0.066)	(0.066)
Number of Observations	257509	257509	236899	236899	236899	236899
R-squared	0.855	0.856	0.847	0.848	0.847	0.848
B. . Effect on Labor Wedge: ln$(1+\tau_l)$						
Post Reform * Small Firm	-0.050***	-0.032**	-0.055***	-0.042***	-0.056***	-0.043***
	(0.015)	(0.015)	(0.016)	(0.016)	(0.016)	(0.016)
Post Reform * In-State Exposure			0.023	0.042*	0.007	0.031
			(0.023)	(0.023)	(0.026)	(0.026)
Post Reform * In-State					0.067	0.046
Traded Exposure					(0.05)	(0.05)
Number of Observations	261019	261019	240242	240242	240242	240242
R-squared	0.778	0.780	0.764	0.766	0.764	0.766
Controls for Post Reform*Assets	Y	Y	Y	Y	Y	Y
Fixed Effects for Post Reform * 3-digit Industry and State	N	Y	N	Y	N	Y

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

what more than the capital wedge is consistent with the finding in Table 9 that firms who produce products associated with lower capital/labor ratios benefit relatively more from access to subsidies. Recall that there is a latent "output wedge" which is unidentified in the data but also potentially changing. The estimates are consistent with, for instance, a 3.5% increase in output subsidies and, in addition, labor being 1.5% cheaper for newly eligible firms.

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



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

The estimated weighted average of the growth in input wedges is similar to the estimated growth in revenue productivity. Furthermore, with $\left(\widehat{1+\tau_{L_j}}\right) \approx -.05$, $\left(\widehat{1+\tau_{K_j}}\right) \approx -.035$, $\alpha = \frac{1}{3}$, and $\sigma = 5$, the predicted direct effect of the program coming from the change in the wedges, $(1-\sigma)\left(\alpha\left(\widehat{1+\tau_{K_j}}\right)+(1-\alpha)\left(\widehat{1+\tau_{L_j}}\right)\right)$, equals .18, around 2/3 of the actual change in revenue.

1.7.2 Aggregate TFP Growth

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

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

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

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

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

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

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

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

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

Equation 25 formalizes the intuition that subsiding the inputs for distorted firms can increase aggregate productivity. For instance, capital subsidies for firm j will increase productivity in an industry iff the firm is facing relatively high distortions (iff $\left(1+\tau_{k_j}\right) > \overline{(1+\tau_{k_i})}$), with a similar argument for labor subsidies. Knowing a firm's revenue productivity is not sufficient for knowing the correct productivity-enhancing subsidy. Subsidizing an input for firms who have relatively low costs for that input will decrease TFP overall, even if generally those firms have relatively high TFPR (and so are relatively smaller than they would be in the absence of any distortions). Furthermore, information on a firm's productivity is uninformative (on its own) on the sign of the aggregate TFP change resulting from a targeted subsidy.

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

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

I conduct the following counter-factual exercises to estimate how much productivity would change under different policy regimes: For 2001-2006, following equation 26, I estimate would would have happened had small firms in fact gained eligibility in 2004 instead of in 2007, by lowering their capital and labor costs by the values found in Table 12 (a 3.6% decline in the capital wedge, and a 5% decline in the labor wedge). For 2007-2019, conversely, I simulate the effect of no policy change by instead increasing the relative input costs by the same amount (effectively "undoing" the program). The results are similar if I extend the analysis for all of the years for which I have data, 2001-2011. Equation 25 also provides bounds on the maximal possible TFP gains from a policy having that effect on input prices for 15% of firms: $\alpha * .036 * .15 + (1-\alpha) * .05 * .15 \approx .07$. In the data, had the subsidies instead gone to the 15% with the goal of increasing TFP the most,[70] the TFP gains would have been around 1.5-2%. In Table 13, Panel A, I show the results, which are consistent across the two regimes and the two different aggregations of industry codes.

Just by changing the relative distortions, introducing the policy change earlier would have increased TFP in the affected years by .05 - .1%, whereas removing the policy change would have lowered TFP by around a similar magnitude. The mapping is not perfect in each year, for instance the effect is relatively larger in 2001 and relatively smaller (in fact the opposite sign) in 2006, but is broadly consistent across the 11 years.[71] In Table 13, Panel B, I decompose the gains into those coming from changes to capital prices versus those for labor prices. While I do not map specific policies to specific changes in prices, it is still valuable to discuss the effects of hypothetical policy changes which shut down one of the price channels. Each input's price change explains around half of the aggregate TFP

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

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

Table 13: Differences in Differences Estimates of the Effects of Firm Subsidies on Firm Input Costs

A. % TFP Change from Counterfactual Subsidy Regimes

level of aggregation	Subsidizing Firms Earlier 2004	2005	2006	Never Introducing Subsidies 2007	2008	2009
3-digit	0.06%	0.06%	-0.04%	-0.07%	-0.11%	-0.01%
Industries	(0.07)	(0.12)	(0.87)	(0.13)	(0.11)	(0.58)
4-digit	0.04%	0.04%	-0.04%	-0.06%	-0.02%	-0.02%
Industries	(0.16)	(0.23)	(0.84)	(0.14)	(0.36)	(0.36)

B**. Decomposition of % TFP Change from Counterfactual Subsidy Regimes**

	level of aggregation	Subsidizing Firms Earlier 2004	2005	2006	Never Introducing Subsidies 2007	2008	2009
Only	3-digit	0.01%	0.02%	-0.09%	-0.03%	-0.07%	0.03%
Change	Industries	(0.42)	(0.50)	(0.97)	(0.39)	(0.31)	(0.88)
Price of	4-digit	0.00%	0.00%	-0.07%	-0.03%	0.01%	0.01%
Labor	Industries	(0.58)	(0.55)	(0.95)	(0.34)	(0.62)	(0.65)
Only	3-digit	0.05%	0.05%	0.05%	-0.04%	-0.04%	-0.04%
Change	Industries	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Price of	4-digit	0.04%	0.04%	0.03%	-0.03%	-0.03%	-0.03%
Capital	Industries	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)

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

gains. This suggests that the expanding eligibility requirements for small firm subsidies was a small step towards lowering the 40-60% TFP gap between India and the United States found by Hsieh and Klenow (2009).[72]

In order to calculate how well-targeted the newly eligible firms were (if the only policy goal of relaxing the eligibility criteria was to reduce the within-sector variation in distortions), I estimate the distribution of potential TFP changes, using 1000 permutation tests as in the previous subsection. I assign the true change in wedges to random subset of

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

firms, and the values in parenthesis represent the share of those estimates with TFP more positive (negative) the true change in the years before (after) the policy change. Panel A shows that the true change is larger than most of the counterfactual estimates in most years. Panel B shows that this effect is largely driven by the capital subsidies, where the true effect is larger than the placebo effects over 98% of the time.

1.8 Conclusion

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

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

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

My empirical results have nuanced consequences for policymakers. Gaining eligibility for small-firm subsidies predicts large gains in firm output, and increases the likelihood that a firm survives. However, crowd-out absorbed around two-thirds of the direct effects. The extend of crowd-out depends on sector characteristics, as all of the aggregate gains were concentrated in sectors with more internationally-traded products. Policy advice for firm subsidies therefore requires careful understanding the characteristics of affected sectors in the economy.

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

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

2 Communication and Manufacturing: Evidence from the Expansion of Postal Services

2.1 Introduction

Even after a good has been produced, its producer still needs to find someone willing to purchase it, ideally the person who will pay the most, net of transport costs. Even in a world with costless trade, if there are costs of communication, it may not be the case that the person who ends up purchasing the good is the one willing to pay the most. Communication costs will have a relatively larger effect on the expected price for products with a higher variance of demand. In places where it is more costly to contact potential buyers, therefore, producers will have relatively larger incentives to try to sell goods for whom information is less important. As a result, lowering communication frictions will not only lead to more production overall, for the same reasons as regular iceberg costs, but the increase will be relatively larger for more information-intensive goods. We use a natural experiment in roll-out of postal services in the late 1800s to demonstrate the differential impacts cheaper communication has for producers in isolated, rural areas. In particular, using an instrumental variables approach, we find a relatively larger increase in manufacturing relative to agriculture. This suggests that the ability to cheaply communicate is essential the process of development, because it encourages specialization and structural transformation.

In 1896, the United States Postal Service introduced a program of daily, free home delivery and pick-up of the mail, known as Rural Free Delivery (RFD), to rural towns and counties across the country. Before the introduction of RFD, areas of an average of 100 square miles shared one central post office with no direct home or business delivery services. The time costs of using the central post office had been sufficiently large that most farmers and other rural citizens did not check their mail more than once a week (Fuller, 1959). Enough rural areas qualified for RFD that the roll-out, especially in the first decade, was reasonably arbitrary. Using information we digitized on the spread of services, we show that there are no systematic differences in the pretreatment outcomes or covariates of counties with varying RFD implementation timing; nor is it possible to predict using census data or political information when a county would get RFD service. We find large reduced form impacts of cheaper communication on manufacturing investment at the county level. These large effects are not surprising; historian Wayne E. Fuller described the introduction of RFD to rural America as "as much a revolution in communication as the telegraph had been" (Fuller, 1964, p. 294).

The impacts of cheaper communication are similar in spirit (although to a much more aggregate level of data) to what would be predicted at an aggregate level by Melitz (2003b) and Bernard et al. (2003). While they examine in detail the intra-sectoral impacts of reducing trade barriers-exporters (the most productive firms) gain increased market shares and aggregate productivity increases as a consequence, we focus more broadly on more classical effects of sectoral choices.

There are two natural experiments that have been used to examine phenomena most similar to ours in terms of how producers respond to changing trade costs. Trefler (2004) and Lileeva and Trefler (2010) use the introduction of the Canada-US free trade agreement to determine the impacts on firms on the economy. Trefler (2004) finds that industries with more tariff reductions saw a larger cut in employment in the short-run, but higher productivity gains in the long run. These long run gains were driven by favorable plant turnover and technological upgrading. Lileeva and Trefler (2010) find that this gain is concentrated among the firms who were induced to export by the lower tariff costs, as predicted by Melitz (2003b). Bustos (2011) uses the reduction of tariffs from the introduction of Mercosur and finds similar effects. Sectors with larger tariff reductions had more firms start exporting and invest more.

There have been several recent papers exploring the relationship between modern communication technologies and agricultural production. Jensen (2007) randomized experiments giving fishermen cell phones, Aker (2010) uses a natural experiment on access to cell services and grain markets, and Goyal (2010) implemented randomized experiment using internet kiosks to help run auctions for soy beans. These papers strongly suggest that price dispersion is not purely due to trade costs, the classical explanation, but also due to the difficulty and cost of communication between buyers and sellers spread over space.[73] Once cheaper communication methods are put in place, price dispersion across space shrinks (or vanishes almost entirely as in Jensen, 2007 and Goyal, 2010). However, the reduction in communication costs and price dispersion should have longer term effects on the producers in these areas—agents should factor these new, lower costs into production decision. Indeed, Goyal (2010) presents suggestive evidence that another impact of the project was that more farmers chose to plant soybeans. We extend that logic across sectors and test it formally in the context of rural producers in the US in the 1890s.

There have also been two recent papers identifying the effects of information costs on trade. Allen (2011) implicitly lays out a theory of why reductions in trade costs can have differential impacts across industries. His model describes the role information frictions pay in trade of homogenous goods, such a rice. He argues that they play a quantitatively large role in explaining trade flows and price dispersion, not only internationally but even internationally. Steinwender (2014) uses a natural experiment with the placement of the trans-Atlantic telegram to see how it changes the trade of cotton. She shows that in spite of the fact that cotton is a durable, price dispersion falls between London and New York, and trade flows increase. However, they do not consider the case how information frictions matter more for particular goods, which we explicitly model and find empirical support for. Furthermore, we look at the interaction of communication and trade costs.

[73] As first suggested by Heckscher (1916).

Baldwin (2011) argues that railroads and steamships allowed production and consumption to be feasibly separated, leading to the industrial revolution (what he calls the first unbundling). He continues to argue that we are undergoing an information and communication technology (ICT) revolution, which in turn matters for the form of production, as headquarters and manufacturing can be separated (what he calls the second unbundling). An extension of our model, although without data to test empirically, suggests that an important part of the first unbundling was also access to cheaper communication, since it can decrease the relative cost of communicating over long distances, allowing producers and consumers to recognize the potential for benefits from trade.

The paper proceeds as follows. In Section 2.2, we describe the historical context of our study, detailing the rural economy of late nineteenth century America and the introduction and expansion of rural free delivery (RFD). Based on a close reading of the history of the RFD program and empirical analysis, we argue that the routes established between January 1898 and May 1900 were established in an arbitrary manner, at least among the towns applying to get RFD in this period. Section 2.3 presents a model of search and advertising. When search costs fall (as a result of the introduction of rural free delivery), firms will search more and advertize more. This will increase the expected price and expected revenue in the sector with more price dispersion and unknown good quality (manufacturing) relative to the sector with less price dispersion and commodity goods (agriculture). Thus, firms will transfer investment between sectors when search costs fall. Section 2.4 presents our data sources and our empirical method. Crucially, the 1900 census measures variables of interest to our model, like capital investment in both the agricultural and manufacturing sectors, as of April 1900. Thus, we observe counties with varying numbers and sizes of towns treated with between zero and 26 months of rural free delivery. We can exploit variation in both the timing and intensity of treatment to measure the effects of RFD. However, we rely on the arbitrary ordering of route establishment to identify the effects on investment of access to less expensive information (as provided by RFD). We also present a number of tests of balance that suggest against selection into earlier treatment. In Section 2.5, we present our results. Relative to counties with treated with RFD later, we find that the early RFD counties investment in 1900 increases in manufacturing and decreases in agriculture, as predicted by our model. A placebo test of investment in earlier periods (1880 and 1890) strengthens our argument that the investment is caused by RFD. Preliminary results suggest, however, that there is no first-mover advantage, as we find no significant difference in manufacturing or agricultural output in 1920.[74] Section 2.6 presents empirical results about the effect of RFD on the spread of newspapers, which we consider as a proxy first stage, since it suggests that communication did increase after the implementation of RFD. Section 2.7 concludes.

2.2 Historical Background

In 1863, Congressional action granted free city delivery to all towns requesting the service with population larger than 10,000. Within a year, 65 cities were served by the program, growing to 104 cities by 1880. However, for the majority of the American population living in rural areas, mail service continued as it had since the colonial era.[75] Under this system, the postal service was responsible only for transporting mail between post offices. Individuals and firms could then travel to the closest central post office location to retrieve and send their mail. In many rural districts, the general store in town served as the post office. Contemporary reports suggest that much of the rural population, who

[74] This longer run analysis is complicated by two facts. First, the census of manufacturing is not available at the county level after 1900 until 1920. Second, the 1920 census of manufacturing is quite different from past censuses; it does not contain data on capital at the county level, nor is the definition of the manufacturing sector consistent with earlier years. Thus, any conclusions about the long term effects of cheaper communications or the first-mover advantage of establishing manufacturing in rural areas should be taken as preliminary.

[75] Fuller (1964) notes that in 1890 only 19 million of 76 million Americans received direct home or business mail delivery.

often lived more than three miles from a post office, picked up their mail once a week (Fuller, 1955).

In 1890, a joint resolution passed Congress, authorizing the Postmaster General to experiment with "county free delivery:" the extension of the free-delivery system to small towns and villages. The service only provided home delivery and pick-up of letters, as parcel post would not be introduced for over 20 years. The post office experimented with 46 communities, with populations ranging from 300 to 5,000 people. The Postmaster General reported in 1891 that the experiment was successful. The post office made net proceeds of $3,600 on the $10,000 appropriated thanks to increased business. One community went so far as to arrange for the continuation of the service, whether or not it would be funded by the post office. Despite the initial popularity of the program, it was not expanded, due to concerns about up-front costs.[76]

Upon the succession of William L. Wilson to Postmaster General in 1895, the post office began another rural free delivery experiment with a congressional appropriation of $40,000. A native of Jefferson County, West Virginia, Wilson chose to start the experiment on October 1, 1896 in three towns in that county: Charles Town, Halltown, and Uvilla. According to the Annual Report of 1897, "Congress desired rural free delivery to be thoroughly tested. The Department has endeavored to comply with this request."[77] RFD tests were expanded to 44 routes in 29 states by the end of 1897.

The post office reported to Congress that the trials were a tremendous success and highly popular. As the first assistant Postmaster wrote in 1897, "There has been nothing in the history of the postal service of the United States so remarkable as the growth of the rural free delivery system."[78] The program was expanded in 1898, with the goal to service

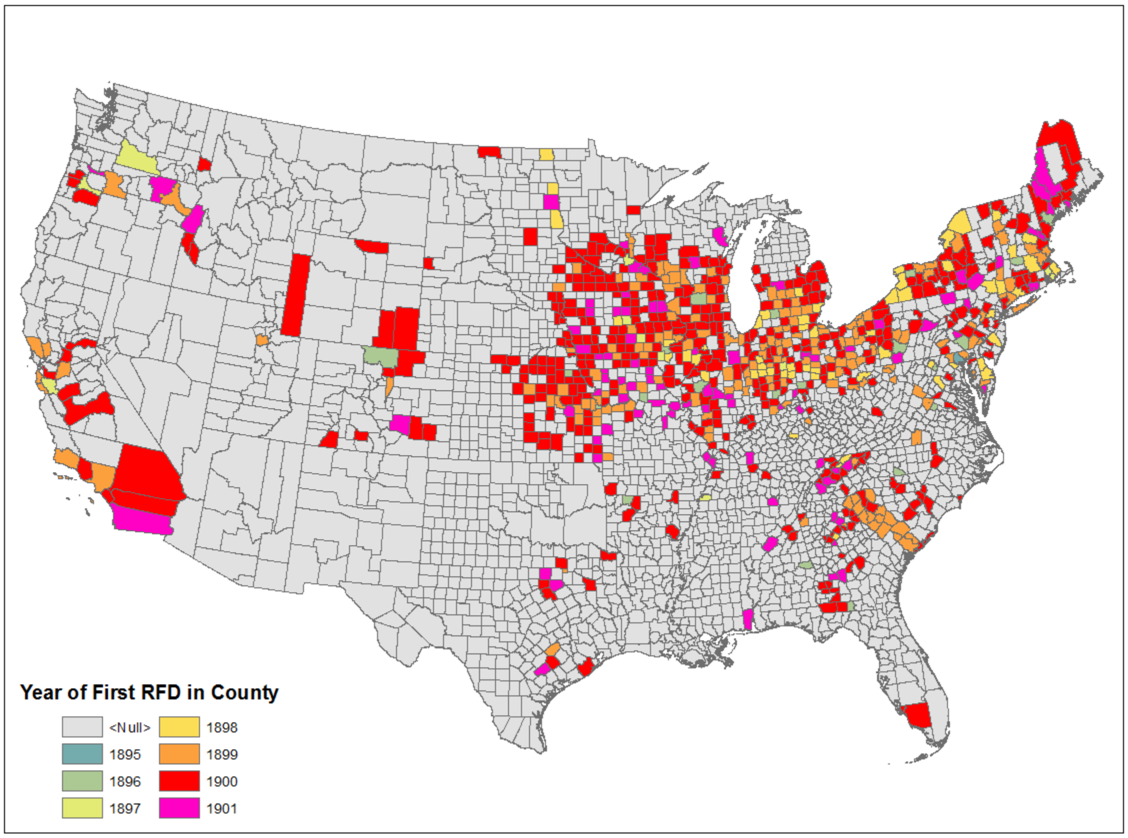
[76] Congressman James O'Donnell of Michigan introduced "A Bill to Extend the Free Delivery System of Mails to Rural Communities" in 1892. However, due to the $6 million projected cost, it was rejected by the House Committee on Post Office and Post Roads.

[77] Annual Report of the Postmaster General 1897, p 105.

[78] Annual Report, 1899, p 196.

any town who applied for a route. The applications required only 100 signatures to petition for service and the post office was quickly inundated with requests. As Fuller (1964, p 42) colorfully noted, “Congress could as easily have stopped an Oklahoma tornado as to have stemmed the demand for rural delivery.” By 1899, 383 counties in 40 states had RFD. Figure 9 maps the expansion of rural free delivery through 1901. According to the 1901 Annual Report of the Postmaster General, 6,000 routes had been organized while more than 6,000 applications were still pending and awaiting action.[79]

Figure 9: Year of First Rural Free Delivery Route by County



In order to qualify for rural free delivery, a potential route only needed to have at least 100 people along a route of 25 miles, with roads which were good enough to travel. While this is only a select group of rural counties at the time, there is some evidence that,

[79] Annual Report of the Postmaster General 1901, 25.

especially at first, many qualified towns did not receive service due to a lack of funds and a lack of knowledge about the service (Annual Report of the Postmaster General, 1898).[80] The petitions for new routes filled several rooms at the Post Office Department and routes were "laid out through the countryside in a helter-skelter fashion" (Fuller, 1964, p. 43-48). The Post Office Department spent a great deal of resources responding to citizen complaints, often delaying the establishment of services by weeks, months, or even years (Fuller, 1964, p. 97). The service was requested by so many communities that in 1900, the Post Office ran out of funds to establish new routes in April 1900. The administration of the RFD application system was reorganized in in May 1900 and petitions required the endorsement of congressmen. From this point on, political considerations mattered greatly in the distribution of RFD routes Kernell (2001). Kernell and McDonald (1999) argue that freshman Republican Congressmen were more likely to receive postal routes than Democrats, as the Republicans had control of Congress, and therefore the post office budget. As this could be a concern for identification, our results include fixed effects for each congressional district.

Contemporary reports were particularly to bullish about the positive effects of RFD on the rural economy. According to the Annual Report of 1900, thanks to RFD, "[a] more accurate knowledge of ruling markets and varying prices is diffused, and the producer, with his quicker communication and larger information, is placed on a surer footing." An earlier report had suggested that "whenever the system has been judiciously inaugurated... it has been followed by these beneficial results... Enhancement of the value of farm lands... A moderate estimate is from $2 to $3 per acre... Better prices obtained for farm products..."[81]

[80] There may have been unqualified towns receiving service as rural agents of the Post Office often granted routes with far fewer than 4 families per mile (Fuller, 1964).
[81] Annual Report of the Postmaster General 1899, p 197.

The expansion of RFD fueled the growth of catalogs, including the giants of the era, Sears and Roebuck and Montgomery Ward. In 1897, for instance, Sears distributed around 360,000 catalogs in the US. Within 10 years, the number grew to over 3.6 million catalogs, a growth caused in large part by the spread of rural free delivery. Furthermore, over the time period Sears added a color section, specialty catalogs, the guarantee "Your money back if you are not satisfied," and sample books for paints and wallpapers (Gordon, 1990). While RFD made it easier for the catalogs to ship, the actual delivery cost of goods was not determined by the existence of an RFD route in a particular town. In addition to the large companies, smaller firms also introduced catalogs for local customers. For instance, U.N. Roberts, a sawmill in Davenport, Iowa, introduced a mail-order catalog of millwork and building material sin 1900, the same year RFD was expanded to their county. While we focus on the short-term effects of RFD on production in affected areas, we recognize that the most dramatic long-term effect of RFD may have been an increase in catalog sales. As a result, while the long-run effect of RFD was probably still a shift in production towards differentiated products, the places where those goods were produced may not have been the places which got RFD itself.

A number of now famous firms emerged in the early years of rural free delivery, attributing their growth to the lower costs of communicating with customers. Vick's Chemical had been founded in Selma, North Carolina in 1890. The firm originally sent salesmen to neighboring counties to advertise and sell their products. In 1903, the first RFD route was established in Selma. Two years later, Vick's developed the "VapoRub" product and began manufacturing it on a large scale. At the same time, the firm used the RFD system to send mail to all nearby counties in order to cheaply access and advertise to potential customers. In fact, Vick's pioneered the use of sending advertisements to "box holder" rather than to named addressees, a practice now known as "junk mail." In the next section, we develop a theoretical framework to explain this shift towards manufacturing.

2.3 Communication and Sector-Specific Investments

In this section, we lay out a partial equilibrium model of how producers make decisions in light of transport and information costs, in order to present micro-foundations which explain our empirical findings. We focus on producers, taking the structure of demand as given, abstracting from any competitive consequences of communication. The producers choose ex-ante the sector they want to be in (agriculture or manufacturing), and also how much to produce in that sector. As in Allen (2011), once they have produced, they search for one buyer who buys all of their product, and who does not have decreasing marginal utility from goods (the notation would be more complicated if they searched for buyers separately for each unit, but the comparative statics would remain unchanged). We consider how the decision making changes, both on the intensive and extensive margins, as communication and transportation costs change. Furthermore, we account for the fact that lowered communication costs may be relevant not only for information acquisition, but also for advertising. While our data do not allow us to distinguish between the channels, they are both potentially first order and there is no ex-ante reason to think that one effect is the dominant one in this setting.

2.3.1 Setup

Each producer, indexed i, can produce in either sector, choosing an amount of agricultural output A_i or manufacturing output M_i. For simplicity, we assume that there is a common convex costs for all producers in the agricultural sector, $F_A + \phi(A_i)$. We assume that agents know the price of agricultural goods, p_A, in advance. In the manufacturing sector, each producer faces the same fixed cost of entry and proportional variable costs, but the degree or proportionality is individual specific. That is to say, the convex costs of manufacturing for producer i is $F_M + \alpha_i \varphi(M_i)$, with $0 < \alpha_i < 1$.

In the agricultural sector, there are no costs to finding buyers or selling the product[82]. The price of agricultural goods is p_A. However, in the manufacturing sector, producers have to pay a cost $S > 0$ in order to contact each potential buyer (who are indexed k). Searching for a buyer serves two functions. First, it allows sellers to discover a buyer's willingness to pay, $p_{M_k} \sim g(p_M)$, which is distributed according to some distribution function which we are agnostic about other than $g'(p_m) < 0$. $E(p_M(M_i))$ is therefore the expected price for producing M_i units of manufactured goods, taking into account the endogenous change in effort as the amount produced changed. Second, it may be possible to shift the buyer's willingness to pay by $\eta(\sigma) \geq 1$. We call this channel advertising, where σ indexes the cost of advertising. We assume the advertising production function, η, satisfies Inada conditions.[83]

As in (Allen, 2011), within each year, producers try to maximize expected earnings, without discounting the future of that year. However, producers do not take into account any consequences that one year's search behavior has on subsequent year's prices. At the start of each year producers make decisions both along the extensive (industry choice) and intensive (quantity produced) margins. This can be represented as a choice of

$$\arg\max_{A,M} \left\{ \begin{array}{c} \arg\max\limits_{A_i} \left(p_A A_i - (F_A + \phi(A_i)) \right), \\ \arg\max_{M_i} \left(E(p_M) M_i \eta(\sigma) - (F_m + \sigma + \alpha_i \varphi(M_i)) \right) \end{array} \right\}. \tag{27}$$

Producers in the agricultural sector, whose profits are $p_A A_i - (F_A + \phi(A_i))$, therefore produce an amount A_i such that

$$\phi'(A_i) = p_A$$

whereas

[82] A potential motivation for this is that, in each community, there is costless entry to becoming a middleman for agricultural commodities

[83] That is, $\eta'(0) \to \infty$ and $\eta'(\infty) \to 0$

Producers can solve this problem with backwards induction, first determining the production decisions and profits conditional on choosing a sector, and then picking the sector which offers relatively higher profits.

2.3.2 Communication Costs

Theorem 1. *The expected profits from manufacturing are increasing as search costs decrease.*

Proof. Before engaging in search, a manufacturing wants to maximize expected profits

$$E(p_M)M_i\eta(\sigma) - (F_m + \sigma + \alpha_i\varphi(M_i))$$

so the amount they produce will equalize the marginal gains and costs

$$\left(\frac{\partial E(p_M)}{\partial M_i}M_i + E(p_M)\right)\eta(\sigma) + \frac{\partial\eta(\sigma)}{\partial M_i}E(p_M)M_i = \alpha_i\frac{\partial\varphi(M_i)}{\partial M_i}. \tag{28}$$

.

A producer who has produced M_i, has put forth a level of advertising intensity σ, and whose highest price seen so far is $\hat{p}_m(M_i)$, has the value function

$$V_i(\hat{p}_m) = \max\{\hat{p}_m(M_i)M_i\eta(\sigma), E(V_i) - S\}$$

This is a stationary problem and so there will exist a critical stopping price $p_m^*(M_i)$ and level of advertising $\overline{\sigma}$, which solve

$$\eta(\overline{\sigma})\ \underbrace{p_m(M_i)M_i}_{\text{Revenue if stop}} = \underbrace{-(S+\overline{\sigma})}_{\text{search+advertising}} + \underbrace{\int_{p_m(M_i)}^{p_m^*(M_i)} \eta(\overline{\sigma})p_m^*(M_i)M_i dG(p_m)}_{\text{Expected revenue if continue next period}}$$

$$+\underbrace{\int_{p_m^*(M_i)}^{\infty} \eta(\overline{\sigma}) p_m(M_i) M_i dG(p_m)}_{\text{Revenue if sell next period}} \tag{29}$$

The producer is indifferent to stopping when the benefits to search are equal to the costs

$$S = \eta(\overline{\sigma}) \int_{p_m^*(M_i)}^{\infty} \left(p_m(M_i) - p_m^*(M_i) \right) M_i dG(p_m), \tag{30}$$

which implicitly defines the stopping values. We can therefore determine comparative statics:

$$\begin{aligned}
\frac{\partial p_m^*(M_i)}{\partial(-S)} > 0, \quad & \frac{\partial \overline{\sigma}}{\partial(-S)} > 0 \Rightarrow \frac{\partial ER(M_i)}{\partial(-S)} > 0 \\
\frac{\partial p_m^*(M_i)}{\partial(M_i)} > 0, \quad & \frac{\partial \overline{\sigma}}{\partial(M_i)} > 0 \Rightarrow \frac{\partial ER(M_i)}{\partial(M_i)} > 0 \\
\frac{\partial^2 p_m^*(M_i)}{\partial M_i \partial(-S)} > 0, & \frac{\partial^2 \overline{\sigma}}{\partial M_i \partial(-S)} > 0 \Rightarrow \frac{\partial ER(M_i)}{\partial M_i \partial(-S)} > 0
\end{aligned} \tag{31}$$

which imply when search costs go down, the expected revenue from each amount of production are increasing. The producer, when choosing where to invest, anticipates this. As a result, for producers who were already manufacturing M_i manufactured goods

$$\frac{\partial \left(\left(\frac{\partial E(p_M)}{\partial M_i} M_i + E(p_M) \right) \eta(\sigma) + \frac{\partial \eta(\sigma)}{\partial M_i} E(p_M) M_i \right)}{\partial(-S)} > 0.$$

Given 28, the the ex-ante production choice

$$\frac{\varphi'(M_j)}{\partial(-S)} > 0$$

implying that

$$\Longrightarrow \frac{\partial M_j}{\partial(-S)} > 0 \tag{32}$$

$\square$

Together, these equations imply that lowering search costs will cause manufacturers to produce more and wait for a higher price, both of which cause higher profits. Define $\Delta ER_i(S, S')$ as the change in net revenue from manufacturing as the search cost changes from S to S'.

Lemma 1. *The total amount of manufacturing will increase as search costs decrease*

Proof. Equation 32 shows that there will be movement along the intensive margins - those who were already producing manufactured goods will produce more as search costs decrease. Furthermore, there are some producers for whom

$$\begin{aligned}\Delta ER_i \quad \geq \quad & \arg\max_{A_i} \left(p_A A_i - (F_A + \phi(A_i)\right) \\ & - \arg\max_{M_i} \left(E(p_M)M_i - (F_m + \alpha_i \varphi(M_i))\right).\end{aligned}$$

These producers will switch products as search costs decrease. As a result, lowering search costs unambiguously increases the extent of manufactured products in a region.

$\square$

2.4 Data and Empirical Strategy

We draw data from two primary sources. First, to identify the towns and counties with rural free delivery, we have digitized the roll out schedules as presented in the Annual Reports of the Postmaster General, from 1900 and 1901. From these records, we are able to record the location of each RFD route, the earliest establishment date of each route, the length of the route, the population and area served, and the number of carriers assigned.[84] The Annual Reports also include the volume of mail delivered on each route

[84] We use a variety of sources, including other postal service records and GIS software, to match each route to its county.

and applications for money orders, which we hope to make use of in future analysis.[85] Figure 10 presents a sample page of our raw historical data.

Figure 10: Data on RFD Roll Out from the Annual Report of the Postmaster General, 1900

Post-office.	County.	State.	When established.	Population served.	Area, square miles.	Number of carriers.	Length of routes.	Pieces of mail delivered and collected during fiscal year 1898–99.
Abbeville[1]	Abbeville	S. C.	May 1, 1899	2,000	57	1	46	4,089
Ada	Hardin	Ohio	May 1, 1899	1,000	42	2	42	19,423
Aiken[1]	Aiken	S. C.	May 15, 1899	1,300	55	1	48	1,357
Albany	Delaware	Ind	Nov. 8, 1897	500	10	1	20	28,217
Allensville[2]	Todd	Ky	Jan. 11, 1897	220	24	3	45	21,752
Anderson[1]	Anderson	S. C.	May 1, 1899	800	60	1	50	3,936
Antwerp	Jefferson	N. Y.	May 2, 1898	500	50	1	25	24,247
Archbold	Fulton	Ohio	May 1, 1899	1,500	50	2	44	7,818
Athens	Clarke	Ga.	Feb. 1, 1899	800	45	1	27	10,893
Athol	Worcester	Mass	Aug. 1, 1898	500	12	2	18	24,955
Atoka	Tipton	Tenn	Jan. 11, 1897	300	10	1	20	17,866
Auburn	Sangamon	Ill	Nov. 23, 1896	1,000	47	3	39	76,995
Baldwinsville	Onondaga	N. Y.	July 5, 1898	2,500	45	4	80	153,024
Baltimore	Baltimore	Md						
Powhatan station	do	do	Feb. 20, 1899	600	5	1	20	13,170
St. Denis station	do	do	May 15, 1899	1,600	23	1	20	3,381
Bamberg[1]	Bamberg	S. C.	Apr. 3, 1899	3,000	82	1	51	5,674
Barker	Niagara	N. Y.	Oct. 3, 1898	1,000	30	1	20	29,568
Belton	Anderson	S. C.	May 1, 1899	1,000	21	1	23	2,842
Benson	Douglas	Nebr	June 1, 1899	800	8½	1	20½	2,571
Benton Harbor	Berrien	Mich	Oct. 15, 1898	2,100	55	2	44	106,545
Bernardston	Franklin	Mass	Nov. 2, 1896	450	17	1	17	38,344
Berryessa	Santa Clara	Cal	Sept. 20, 1898	600	9	1	24	28,539
Bonner Springs	Wyandotte	Kans	Oct. 26, 1896	700	36	3	64	81,569
Bowling Green	Wood	Ohio	Oct. 3, 1898	2,000	56	2	44	66,458
Bradfordsville	Marion	Ky	Jan. 1, 1898	1,200	20	2	48	26,273
Branford	New Haven	Conn	June 1, 1898	1,200	3	1	10	52,576
Brattleboro	Windham	Vt	Mar. 2, 1898	400	20	1	16	23,405
Brooklyn	Poweshiek	Iowa	Dec. 16, 1897	700	60	2	39	145,792
Extension	do	do	Aug. 15, 1898	325	22	1	22	
Brunswick	Cumberland	Me	Dec. 7, 1896	250	16	1	13	42,133
Extension	do	do	May 1, 1899	1,700	45	3	85	
Burlington	Des Moines	Iowa	Aug. 1, 1898	500	18	1	24	254,197
Extension	do	do	Sept. 1, 1898	1,500	80	5	130	
Burlington	Chittenden	Vt	July 20, 1898	1,300	20	2	42	93,484
Cairo	Randolph	Mo	Oct. 15, 1896	1,000	36	3	54	50,923
Campbell	Santa Clara	Cal	Feb. 1, 1897	1,600	22	3	75	205,080
Canton	Stark	Ohio	Jan. 3, 1899	3,900	93	4	80	65,278
Chardon	Geauga	do	Apr. 3, 1899	2,500	46	2	44	32,053
Charlestown	Jefferson	W. Va.	Oct. 1, 1896	600	36	3	54	54,589
Chatham	Morris	N. J	Nov. 1, 1898	1,375	21	1	21	13,709
Chillicothe	Livingston	Mo	May 15, 1899	715	18	1	24½	3,640
China Grove	Rowan	N. C	Oct. 23, 1896	500	40	1	25	19,653
Circleville	Pickaway	Ohio	Oct. 24, 1898	700	15	1	26	29,825

Second, we draw our main outcomes of interest and control variables from the United States Census records, including the Census of Population, the Census of Agriculture, and the Census of Manufacturing, for the years 1870 through 1920 (Haines, 2010). Census data is available at the county level. Though county boundaries are relatively stable over this time period for most of the eastern and Midwestern states, we adjust our data to account for county boundary changes.[86] Again, it is vital to our empirical strategy that the 1900 census measures capital investment in the agricultural and manufacturing sectors, as well other outcomes and controls, as of April 1900. Thus, we observe counties with varying numbers and sizes of towns treated with between zero and 26 months of rural free

[85] Unfortunately, we are unable to directly test the effects of cheaper communication on communication itself as we do not have data on mail before the roll out of RFD. However, given the large, discrete reduction in communication costs, in money, time, and effort, it seems unlikely that the mechanism driving effects of RFD on firms is anything other than cheaper communication.

[86] Specifically, we follow Hornbeck (2010) and use historical county boundary maps, intersecting the boundaries in our base year, 1900, with earlier and later boundaries.

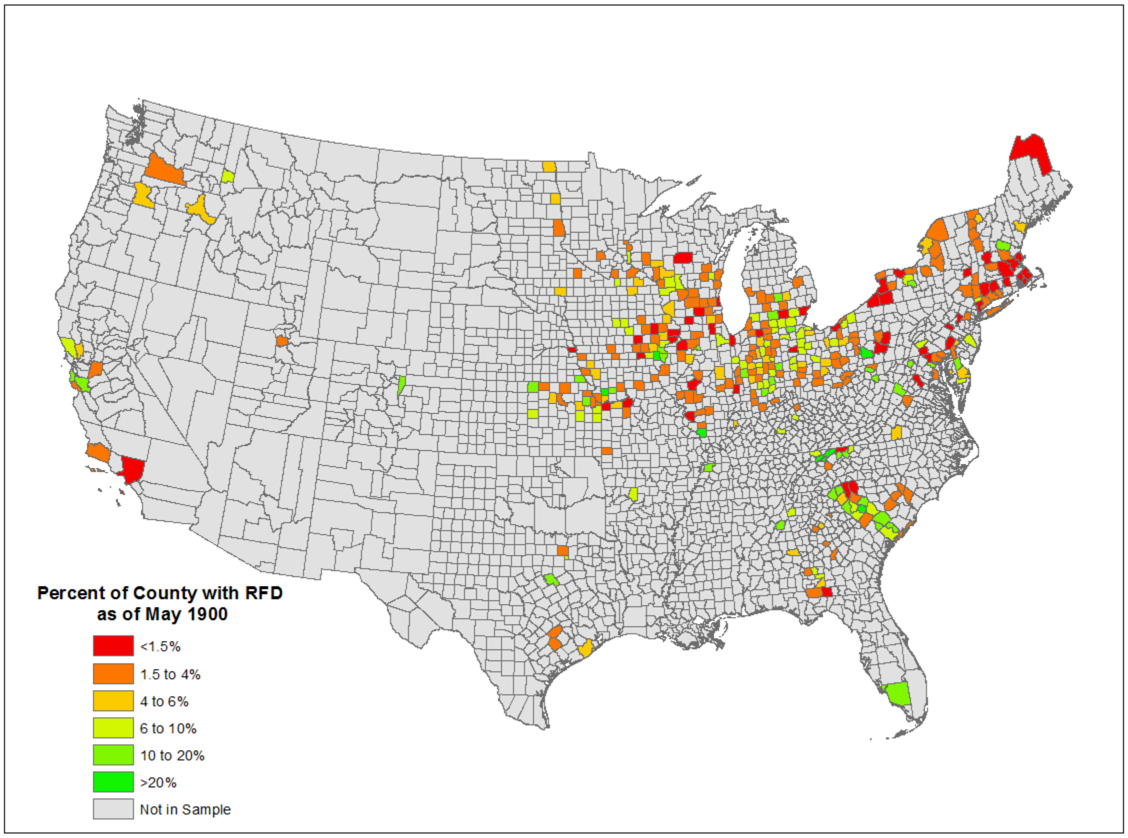
delivery. We rely on the arbitrary ordering of route establishment to identify the effects on investment of access to less expensive information (as provided by RFD), exploiting variation in both the timing and intensity of treatment. However, it is clear that getting access to RFD is not exogenous, since only certain places were even eligible.

Our identifying assumption is that, conditional on getting an RFD route relatively quickly, a town getting the route early or later is arbitrary, determined by the idiosyncratic timing application review in the office of the Postmaster General. By only comparing communities who got RFD, we overcome many of the fundamental selection problems. However, we still have to account for the fact that the establishment of rural free delivery routes occurred at the town level, while both our outcome and control variables were measured at the more aggregate county level.[87] Even if applications had been approved with a lottery, treatment intensity at the county level may be directly correlated with outcomes, since counties with more applications are different than those with fewer, and in expectation will also have more routes. In order to overcome this issue, we use measures of treatment intensity within the treated group as an instrument for total treatment intensity.

Our IV approach splits the sample into three groups: early (getting RFD before a certain date), late (getting RFD after a certain date), and never (not getting RFD in our sample). With this approach, counties, indexed j, have characteristics X_j and outcomes Y_j. Let t_j be the number of people in the county who got an RFD service early, c_j be the number who got it late, and z_j be the number who are not in the experiment at all. Define $n_j \equiv t_j + c_j + z_j$ as the total number of people in the county. The share of the population treated is $\frac{t_j}{n_j}$, and the share eligible for treatment is $\frac{t_j+c_j}{n_j}$. In Figure 11, we present a map of the treatment intensity with in a county, $\frac{t_j+c_j}{n_j}$.

[87] Technically, our treatment is at the post office or route level, but it is not a semantic distinction which changes the underlying econometrics.

Figure 11: Year of First Rural Free Delivery Route by County



The *potential* outcomes for town j given different levels of treatment can therefore be written as $y_j\left(\frac{t_j}{n_j}\right)$. As a result, the theoretical marginal effect of increased RFD is

$$\frac{\partial E\left(y\left(\frac{t}{n}\right)\right)}{\partial\left(\frac{t}{n}\right)}.$$

However, in the data, towns with more RFD are different not just because they happened to get more towns treated, but potentially also because there were more towns eligible. As a result, the gradient observed in the data is a function of both the theoretical effect of interest as well as a selection effect, since counties with fewer z_j towns are likely to also be on a different growth path. Denoting $\tilde{E}$ as the mean in the data,

$$\frac{\partial \tilde{E}\left(y\right)}{\partial\left(\frac{t}{n}\right)} = \underbrace{\frac{\partial E\left(y|\frac{t+c}{n}\right)}{\partial\left(\frac{t}{n}\right)}}_{\text{Effect of RFD}} + \underbrace{\frac{\partial E\left(y\left(\frac{t}{n}\right)\right)}{\partial\left(\frac{t+c}{n}\right)}\frac{\partial\left(\frac{t+c}{n}\right)}{\partial\left(\frac{t}{n}\right)}}_{\text{Selection Bias}}, \tag{33}$$

In order to how this selection bias shows up in our data, we perform the following test. For three of the outcomes we look at for the paper (share of a county's capital in manufacturing, log manufacturing wages per capita, and share of output in manufacturing), we predict the 1900 levels using the lagged outcomes from 1890, 1880, and 1870 (for all of the outcomes in the paper[88]), as well as political vote shares. We then regress the share of a country treated $\left(\frac{t}{n}\right)$and the share of a county eligible for treatment $\left(\frac{t+c}{n}\right)$ on these predicted outcomes, following (Card et al., 2007).

Table 14: Effect of Percent Treated on Predicted Manufacturing Outcomes in 1900

	Mfg Capital Share (Fitted Values)		Mfg Wages per Capita (Fitted Values)		Mfg Output Share (Fitted Values)	
	(1)	(2)	(3)	(4)	(5)	(6)
t+c/n	-1.002***		-87.49***		-1.057***	
	(0.235)		(21.49)		(0.280)	
t/n		-0.240		-39.29*		-0.0747
		(0.204)		(21.83)		(0.284)
Observations	326	326	326	326	326	326
R^2	0.056	0.002	0.056	0.007	0.050	0.000

Outcomes in 1900 are regressed on the share of counties eligible for Rural Free Delivery, as well as the share of the county who actually got a route early in the process. Standard Errors are clustered at the Congressional District level.

The results are presented in Table 14. Places which were relatively more eligible for treatment (the first row) are correlated with significantly smaller predicted manufacturing sectors, which leads to places which were relatively more treated (the second row) also

[88] Which involve output, wages, and capital, both overall and manufacturing's share.

being correlated with smaller predicted manufacturing sectors. As a result, we need an empirical strategy which can account for this selection effect.

Our identifying assumption is that conditional on the percent of the county treated, the potential outcomes are not a function of the actual percent treated, drawing on the fact that the placement of routes was "helter-skelter." Formally, our assumption is

$$E\left(\left((Y(\frac{t}{n})|\frac{t}{n}=a\right)|\frac{t+c}{n}\right)=E\left(\left((Y(\frac{t}{n})|\frac{t}{n}=b\right)|\frac{t+c}{n}\right),\ \forall a,b.$$

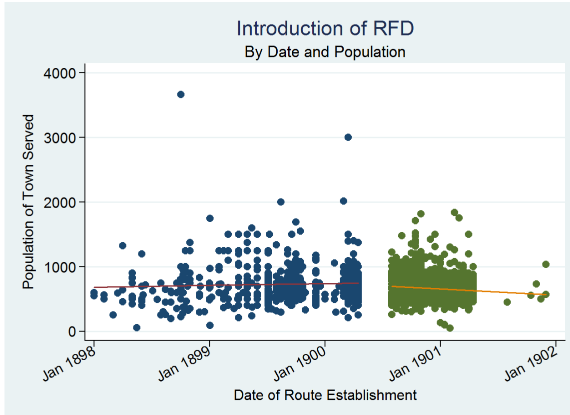
Since $\frac{\partial E\left(y\left(\frac{t}{n}\right)|\frac{t+c}{n}\right)}{\partial\left(\frac{t+c}{n}\right)}=0$, conditioning equation 33 gives

$$\frac{\partial \tilde{E}\left(y\left(\frac{t}{n}\right)|\frac{t+c}{n}\right)}{\partial\left(\frac{t}{n}\right)}=\underbrace{\frac{\partial E\left(y\left(\frac{t}{n}\right)\right)}{\partial\left(\frac{t}{n}\right)}}_{\text{Effect of RFD}}. \tag{34}$$

Our identification strategy relies on the claim that, given a town applied for rural free delivery, the establishment date is arbitrary. Earlier, we presented historical evidence on how the Postmaster General and his staff handled applications. To buttress this claim, we present empirical evidence on the (lack of) differences between towns and counties with earlier or later RFD routes. Figure 12 graphs the population of towns served by routes against the date RFD routes were established. As is apparent in the figure, the initially there is no relationship between town size and the date of establishment. However, following April 1900, where we see the break in route establishment after the post office ran out of funds, larger towns tended to get routes earlier. Roughly, this trend break coincides with the politicization of the RFD application process. After April 1900, towns needed the support of their local congressmen to be granted an RFD route.[89]

[89] A similar trend break appears in measure of area served.

Figure 12: Relationship Between RFD and Local Population



The towns and counties treated with RFD routes before the break, before April 1900, will be our t_j and c_j groups. In these results, we choose July 1899 as the cut off between the t_j and c_j populations. Our qualitative results are not sensitive to shifting this cut-off by a month in either direction. In a web appendix, we employ a more continuous measure of treatment, using the total number of person-days with access to rural free delivery in a county.

There were not enough RFD routes for us to be able run regressions which have fixed effects for the percent of a county getting RFD, as would be suggested by equation 34. We employ two strategies in order to deal with this issue. The first strategy is to instead control for a cubic polynomial in $\frac{t}{t+c}$, in order to approximate the appropriate controls. However, we present these results more for their suggestive nature, as they are unable to fully control for selection effects. Our preferred strategy is an instrumental variables approach, taking advantage of the fact that the percent of the eligible group who ends up actually receiving a route is orthogonal to the potential outcomes given the identification assumption. More formally,

$$E\left(Y(\frac{t}{n})|\frac{t}{t+c}=a\right)=E\left(Y(\frac{t}{n})|\frac{t}{t+c}=b\right) \quad \forall a,b.$$

As a result, $\frac{t_j}{t_j+c_j}$ can be used as an instrument for $\frac{t_j}{n_j}$. In order to give one test of the exclusion restriction, we show that this measure is not correlated with observables using the same (Card et al., 2007) test as before. The results are presented in Table 15. Only in the IV specification is there a small and insignificant correlation between the observables and access to RFD, suggesting that $\frac{t+c}{n}$ is plausibly orthogonal to the potential outcomes.

Table 15: Effect of Instrument on Predicted Outcomes in 1900

	Mfg Capital Share (Fitted Values)		Mfg Wages per Capita (Fitted Values)		Mfg Output Share (Fitted Values)	
	(1)	(2)	(3)	(4)	(5)	
t/n	0.560*		22.95		0.863**	
	(0.310)		(25.24)		(0.389)	
t/t+c		0.0465		2.303		0.0565
		(0.0301)		(2.538)		(0.0347)
Controls for (t+c)/n	Yes	No	Yes	No	Yes	No
Observations	326	323	326	323	326	323

Predicted Outcomes in 1900 are regressed on the share of counties eligible for Rural Free Delivery, as well as the share of the eligible in the county who actually got a route early in the process. Standard Errors are clustered at the Congressional District level.

For the IV specification, we run a first stage regression of the form

$$\frac{t_j}{n_j}' = \beta_0 + \beta_1 \frac{t_j}{t_j + c_j} + \lambda X_j + \epsilon_j'.$$

which we present in Table 16. Standard errors are clustered at the congressional district level. Clearly, the percent of the experiment treated, $\frac{t_j}{t_j+c_j}$ is a very strong predictor of the percent of the county treated, $\frac{t_j}{n_j}$. Importantly for the first stage of an IV regression, the F-statistics is over 10, suggesting than any bias from a weak instrument is likely to be extremely small.

Table 16: Effect of Percent Treated on Predicted Outcomes in 1900

	Percent County Treated
	(1)
t/t+c	0.0640***
	(0.00655)
Observations	323
R^2	0.531
F	12.42

The share of each county receiving Rural Free Delivery is regressed on the share of the eligible in the county who actually got a route early in the process. The regression includes controls for lagged outcomes in 1890, 1880, and 1870 for all of the variables of interest in the paper. Standard Errors are clustered at the Congressional District level.

2.4.1 Newspapers

In the various Annual Reports of the Postmaster General, comments from RFD patrons and beneficiaries are included, perhaps in an attempt to elicit support from members of Congress for further expansion of RFD efforts. We find one such comment, submitted by Nathan Nicholson of Newcastle, IN in 1898, to be particularly revealing. Of the the personal benefits of RFD that Nicholson had experienced, he wrote (emphasis added):

> You want to know about the free delivery of mail. I do not know of anything that the United States could do for that small amount of money that is doing as much good as the free delivery. Nearly everybody is taking more mail than they did before the free delivery started, so it must be about self-supporting. ***I am taking two daily papers now and took none before***. I send and get more letters since this has started. We can keep better posted on the war, markets, weather, politics, etc. It has got me spoiled. I would rather it had not started if it is going to stop now. ***If I was going to buy a farm I would give more per acre on a free-delivery route than I would where there was not any***. Let it come. My neighbors and I are willing to pay our part.[90]

[90] Annual Report of the Postmaster General, 1898 p 240

We turn towards Nicholson's contention that one first-order effect of RFD should be to increase newspaper delivery. While it would be ideal to have an "economic" first stage in which we show the relationship between the introduction of rural free delivery and usage of the mail, newspapers provide a reasonable proxy for this. Given that the model in this paper suggests that easier communication led to changing the structure of the economy in affected regions, before showing the results on the economy it seems prudent to show that RFD also changed communication patterns. We test this using data from Gentzkow et al. (ming). Drawing on data from published advertisers catalogs of newspapers, the data from Gentzkow et al. (ming) reports both the number of daily papers and the circulation of those papers in each county.[91] As presented in Table 17, the IV estimates suggest that giving everyone in a rural community RFD for a year would double the total newspaper circulation, consistent with the anecdotal evidence. Conversely, the results also suggest that the expansion of RFD does not significantly change the number of daily newspapers in a given county. With the high fixed costs of newspaper printing in this era, it is not unreasonable that the response to RFD would be on printing and circulating more papers rather than founding new papers. It is also theoretically unclear if decreased communication costs would allow for higher-quality papers to dominate the market, a theoretical question well beyond the scope of the current paper.

2.5 Results

Our main specification is the effect of RFD, measured as a treatment intensity at the county level, on investment. From the census, we measure both manufacturing capital

[91] As the original emphasis in Gentzkow et al. (ming) is on the effects of newspapers on elections, they collect data every four years, coinciding with the Presidential election years. Thus, we use data from 1892, 1896, and 1900.

Table 17: Effect of Percent Treated on Newspapers in 1900

	Published Newspapers		Circulation	
	(1)	(2)	(3)	(4)
	IV	OLS	IV	OLS
t/n	-2.308	-1.389	6274.8*	3160.3
	(2.416)	(1.921)	(3559.4)	(3948.8)
Observations	332	335	332	335
R^2	0.881	0.883	0.948	0.949

Each outcome in 1900 is regressed on the share of the county receiving RFD. The regressions includes controls for lagged outcomes in 1890, 1880, and 1870 for all of the variables of interest in the paper. The number of newspapers is measured per 10,000 eligible voters. Standard Errors are clustered at the Congressional District level.

per capita and farm capital per acre.[92] The models take the form of an IV specification:

$$Y_{j,1900} = \beta_0 + \beta_1 \frac{t_j}{n_j}' + \sum \gamma_{jt} controls_{jt} + \epsilon_j$$

and an OLS specification:

$$Y_{j,1900} = \beta_0 + \beta_1 \frac{t_j}{n_j}' + \sum_{k=1}^{3} \theta_k \left(\frac{t_j + c_j}{n_j} \right)^k + \sum \gamma_{jt} controls_{jt} + \epsilon_j$$

where we include controls for all of the (lagged) variables of interest in the paper, as well as fixed-effects for each congressional district, and the first stage is as described in the previous section. For outcomes involving how important manufacturing is to the local economy, our model suggests that $\beta_1 > 0$. Our main results are presented in Table 18.

The coefficient in column 1 suggests that going from no access to full RFD access would triple the share of manufacturing capital in a county, from about 25% to 75%, an increase which is significant at the 5% level. The coefficient on wages, in column 2, is insignificant, although the point estimate is in line with that of capital, with about a doubling of manufacturing wages. The coefficient on output in column 3, also signifi-

[92] Measuring the farm capital per acre is a more natural number than farm capita per capita, but results are not sensitive to this definition.

Table 18: Effect of Rural Free Delivery on Manufacturing in 1900

	Mfg Capital Share		Mfg Wages per Capita		Mfg Output Share	
	(1)	(2)	(3)	(4)	(5)	(6)
	IV	OLS	IV	OLS	IV	OLS
t/n	0.582**	0.298	18.24	8.979	0.643**	0.366
	(0.277)	(0.220)	(22.13)	(19.42)	(0.306)	(0.286)
Controls for (t+c)/n	No	Yes	No	Yes	No	Yes
Observations	323	326	323	326	323	326
R^2	0.615	0.627	0.554	0.559	0.582	0.592

Each outcome in 1900 is regressed on the share of the county receiving RFD. The regressions includes controls for lagged outcomes in 1890, 1880, and 1870 for all of the variables of interest in the paper. Standard Errors are clustered at the Congressional District level.

cant at the 5% level, is somewhat difficult to take literally, as it would suggest that the share of manufacturing output would double from just over 50% to just over 100% of total output. As that is impossible, we take these results more for the comparative-static use, demonstrating a economically and statistically significant relationship between RFD and manufacturing in a county.

2.5.1 Placebo Tests

Another concern might be that RFD was given to regions which would have grown regardless. One test of this is to use a placebo test using 1890 outcomes. As a result, we can run the same specification as in Table 18, only using the outcomes in 1890 (before RFD) on the left hand side, and using controls from 1880 and 1890. The coefficients in Table 19 are consistently small and insignificant, and often of the opposite sign as in the main results, suggesting that our measure of RFD access is not merely capturing pre-existing growth trends.

2.6 Discussion and Conclusion

Before the introduction of postal delivery, it is reasonable to think that search costs were convex with distance, since while it was relatively easy to send someone door-to-door

Table 19: Effect of Future Rural Free Delivery on Manufacturing in 1890

	Mfg Capital Share		Mfg Wages per Capita		Mfg Output Share	
	(1)	(2)	(3)	(4)	(5)	(6)
	IV	OLS	IV	OLS	IV	OLS
t/n	-0.0700	-0.0370	-29.45	-5.902	-0.654	-0.270
	(0.210)	(0.205)	(37.85)	(33.82)	(0.516)	(0.514)
Controls for (t+c)/n	No	Yes	No	Yes	No	Yes
Observations	333	336	333	336	333	336
R^2	0.956	0.960	0.922	0.927	0.938	0.944

Each outcome in 1890 is regressed on the share of the county about to receive RFD. The regressions includes controls for lagged outcomes in 1880 and 1870 for all of the variables of interest in the paper. The number of newspapers is measured per 10,000 eligible voters. Standard Errors are clustered at the Congressional District level.

within a small geographic area, it would be difficult to send someone further away. However, the price of postage was independent of the distance. As a result, the introduction of RFD led to a relative decrease in search costs far away. There will be a larger geographic spread of production and consumption as search costs decrease, which will be particularly relevant for differentiated products. This suggests that postal and telegraph services were important to the spread of intranational trade. This suggests an explanation for the stylized fact that the growth of catalog sales came not after the spread of railroads in the early 19th century, but after the spread of Rural Free Delivery in the early 20th century.

3 How Federal Reserve Discussions Respond to Increased Transparency

> *"People think reading the raw transcripts is a way of learning things; I would suggest that if they spend six or eight months reading through some of this stuff, they won't like it."*
> – Alan Greenspan, 1993

> *"Quicker and more complete disclosure already has changed the nature of the Committee's deliberations. I am for the disclosure that we do, but we should not mislead ourselves about how it has changed the nature of these proceedings. I recall participating in routine, vigorous, and freewheeling debates in this room before we decided to release transcripts. Now, most of us read prepared remarks about our Districts and the national economy and even our comments on near-term policy sometimes are crafted in advance. Prepared statements were the rare exception rather than the rule until we started to release transcripts."*
> – Ed Boehne, President of the Philadelphia Federal Reserve Bank, June 1998 Transcript

Imposing transparency on an organization will have two major effects. The first is the transparency itself - outsiders will know what the organization is doing. The second is the endogenous response to transparency, as members of the organization may change their behavior in light of the fact that it will be observed. In spite of its potential importance, empirical studies of the latter effect are scant, perhaps because almost tautologically information on organizational behavior before transparency rarely exists.

We study the effects of transparency on the deliberations of the Federal Reserve Open Market Committee (FOMC). Starting in 1976, archivists at the Federal Reserve kept recordings of FOMC meetings, without the knowledge of most of the participants. Furthermore, the Federal Reserve publicly denied the existence of those recordings, and only publicly released meeting summaries. When the existence of the recordings was discovered in 1993, the Federal Reserve agreed to release all of the transcripts from the earlier meetings. Furthermore, the FOMC agreed to continue to releases summaries soon after meetings, but to also release meeting transcripts with a five year lag, an arrangement which continues. We therefore have one set of documents which were always released to the public (the summaries), and one set of documents which only in the recent period were created knowing that they would be publicly available. For our empirical analysis, we exploit variation within document types over time and across document types within meetings in the public's access.

On its own, text data is appealing, but difficult to use in a systematic and replicable manner. There are over 30,000 words used in the documents, potentially leaving us with an almost overwhelming number of choices to make in our analysis. Furthermore, different words often have the same meaning: just because two documents use literally different language does not mean that they are about different topics. In order to overcome these obstacles, a variety of strategies have been developed in the text analysis literature, ranging from focusing analysis on words identified ex-ante to using clustering methods on the data itself in order to model how "topics" evolve over time. We develop new methods to evaluate the evolution of language over time, which allows us to identify the words and topics most responsible for the evolution of language within documents.

In order to account for the fact that words sometimes have overlapping meanings, we leverage the fact that the Oxford University Press publishes dictionaries with those meanings. We use the relationships of words in the dictionary to create symmetric measures which relate how similar two words' meanings are. We show how to adapt standard measures of similarity, which traditionally treat each word as orthogonal, to account for these weights. This allows us to distinguish a change in word choice from an actual change in content. We document an increase in the similarity of the private and public texts after transparency reforms, even when accounting for words' meanings.

Our third contribution is an exact decomposition from the change in similarity over time into changes in word-level behavior. This decomposition does not requiring specifying particular words in advance: we show how to uncover each word's contribution to the change in similarity, and therefore can create rankings of the words which are most responsible for changes. We find that the words most responsible for the change tend to be economically meaningful, such as "inflation" and "growth." Furthermore, the word "think" was used substantially less in the meetings following transparency reforms, consistent with qualitative evidence that FOMC members started to prepare speeches after the transparency reform.

Merely observing a change in behavior as a result of transparency does not suggest unintended negative consequences. International (2012) argues that transparency for central banks would limit the undue influence of private interest groups. In more theoretical work, the disciplining and effort-enhancing effects of giving a principal more information about an agent's behavior come out of many canonical models of career concerns, such as in Fama (1980) and Holmstrom (1979). However, in line with the Boehne quote at the top of the paper, other models suggest that transparency leads to less risk-taking. Prat (2005) argues that one version of this comes from pooling on public signals. If only actions are observed, agents may choose the actions which are optimal under public information even when they are not optimal given the agent's private signals, since they are worried that they would be considered totally uninformed. In Appendix 5.2.1, we demonstrate another argument in this vein, focusing on horizontal rather than vertical differentiation. If agents get credit for focusing on what ex-post turn out to have been correct topics, ex-ante they will overly focus their attention to issues which are more likely after transparency. Similarly, they will focus their attention on topics which are more salient to the public, even if ultimately they are not as important. However, we are unable to distinguish if certain topics are discussed more due to a reallocation of effort or an increase in overall effort. As a result, the techniques developed in this paper are not sufficient for a cost-benefit analysis.

It is not surprising that this policy change has been studied before in the social science literature, both because it relates to a fundamentally important economic institution and because it provides unusually good data both before and after transparency. Woolley and Gardner (2009) use a measure of "deliberation" which is speakers per 100 words in the transcripts, and find that it declines after transparency. They only use time-series variation, and argue that the underlying trend break may have occurred in 1996 and not 1993. Meade and Stasavage (2008), similarly motivated by a model of career concerns, argue that after transparency led FOMC members to dissent less with Alan Greenspan. Their argument was that people are less willing to disagree with a known expert when their reputation would be on the line. An informative equilibrium, in which members reveal their private information, is more likely when the meetings are not made public. In the paper most similar to ours, Hansen et al. (2014) revisit Meade and Stasavage (2008) using a difference-in-differences design, leveraging the fact that models of career concerns tend to predict that more senior FOMC members have more established reputations, and therefore are influenced less by career concerns. They find that more inexperienced members shift their topics to be more similar to those of Chairman Greenspan. However, their statements become more influential on other members. In a different setting, Dranove et al. (2003) finds that when hospitals become publicly graded on the success of the surgeries which take place, they stop performing more risky surgeries since it will hurt their grade. This is similar to the model in Appendix 5.2.1, which predicts that agents will discuss uncertain issues less, since they do not want to be embarrassed.

Furthermore, the FOMC transcripts have been widely studied. Romer and Romer (2004) manually go through each transcript to determine federal funds rate that the FOMC intended to prevail at the time of the meeting, and use this to develop a measure of monetary shocks. Schonhardt-Bailey (2013) relies on both interviews and textual analysis in her book, which examines the deliberative process of the Federal Reserve. Fligstein et al.

(2014) also study FOMC transcripts, and argue that the organizational use of "macroeconomics" to make sense of the economy made it difficult for them to see the financial crisis coming. To the best of our knowledge, ours is the first study to explicitly consider the relationship of the language of the FOMC meetings to their corresponding public summaries.

3.1 Historical Background

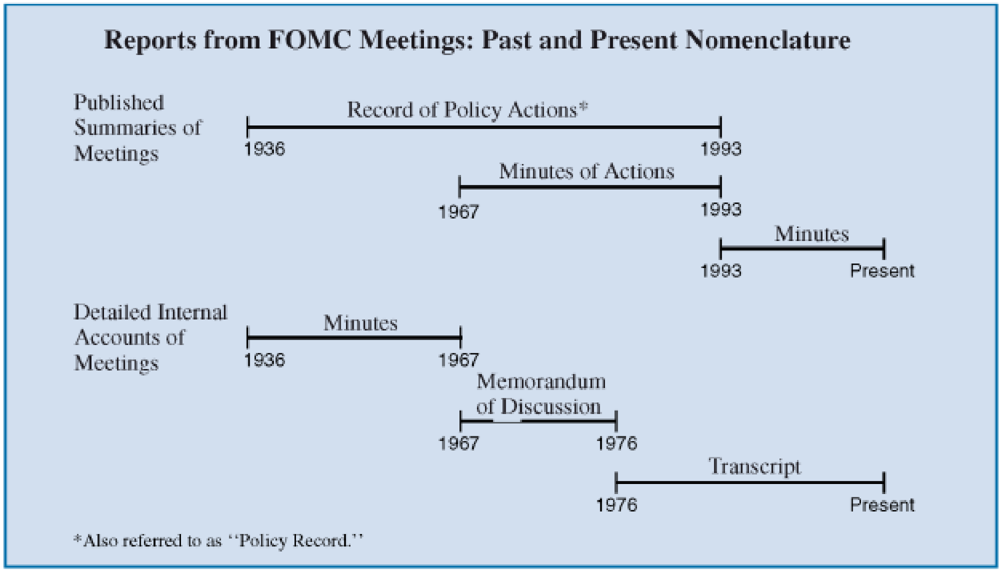
3.1.1 Data availability

The Federal Open Market Committee, formed in 1935, publicly released "Records of Policy Actions" for most of its existence; these were at first released only once a year. The Committee also maintained private records called "Minutes," which contained, for each meeting, details on attendance, discussions, and decisions. In 1967, the Records of Policy Actions started to be released roughly ninety days after each meeting. The Minutes were split into two parts, with the new second document, called the "Minutes of Actions," made available to the public; the other document, called the "Memorandum of Discussion," was kept private. The delay on the release of the public documents was further cut to 45 days in 1975; this was quickly followed by a decrease to 30 days in 1976.

In 1976, Congress passed the Government in the Sunshine Act which said that "The [Fed] shall make promptly available to the public, in a place easily accessible to the public, the transcript, electronic recording or minutes of the meeting." The law was targeted at government agencies more broadly, but probably covered the Federal Reserve as well. Subsequently, in a 10-1 vote, the FOMC voted to discontinue the keeping of transcripts, to make them impossible to release publicly.

In 1993, the House Banking Committee, led by Henry B. Gonzalez, a democrat from Texas, uncovered that transcripts did exist. An agreement was reached where the FOMC would release lightly edited transcripts with a 5 year lag. On a conference call four days

Figure 13: Federal Reserve Documents from 1936 - Present



From the FOMC Spring Bulletin, 2005.

before the meeting, Alan Greenspan made it clear that he would try to prevent Congress from learning about the transcripts (Auerbach 2011), consistent with our argument that the change was an unanticipated shock. The new transparency rules were recognized in the popular press. For instance, Friedman and Schwartz (1993) wrote about it in the Wall Street Journal. As a result, it was well-known to members of the FOMC that there was an important reform, and that the transcripts would be seen by critical readers.

3.1.2 FOMC Members

There are generally 19 FOMC members, which take place 8 times a year (4 is the statutory minimum). There is a chairperson, who typically serves for about a decade, and 6 other Governors based in Washington, DC. Furthermore, there are 12 regional banks, who send their President to the meetings. Although all of the members speak, only a subset of the regional presidents have voting power.[93] While being on the FOMC is a highlight of any career, some of the members are only there for a short period of time

[93] The President of the New York Fed always has a vote, the Chicago and Cleveland presidents each vote every other year, and the rest of the members rotate in to vote for one out of every three years.

(many academics only serve until their universities threaten to pull their tenure), while others end up advancing through the Fed - all of the FOMC chairpersons during the authors' lifetimes were first on the FOMC. Figure 14 shows the duration of membership of appointive FOMC members.

While there were several FOMC members whose tenure spanned both sides of the transparency reform, we do not constrain our analysis to just those members. As can be seen in figure 16, the relationship of interest in this paper - the similarity of the transcripts and public documents - was fairly stable both in a pre and in the post period. As a result, we study the FOMC as a whole instead of the behavior of particular members.

3.1.3 Data

The data for our analysis consist of 270 publicly available transcripts from 1976 to 2007, and their corresponding public summaries.[94] These summaries are called "Records of Policy Action" prior to 1993, and "Minutes" thereafter.[95] We used the OCR software ABBYY FineReader to convert these documents into text files. We use the Oxford Dictionary of Economics (ODE) to determine economic topic clusters, and to define links between words; this aids us in constructing our similarity metric. The ODE contains detailed definitions of over three thousand terms related to economics; see Section 3.2.7 for further details. In addition to an analysis constraining the analysis to words in the ODE, we also use the Harvard General Inquirer (GI) dictionary list of "economics" language.

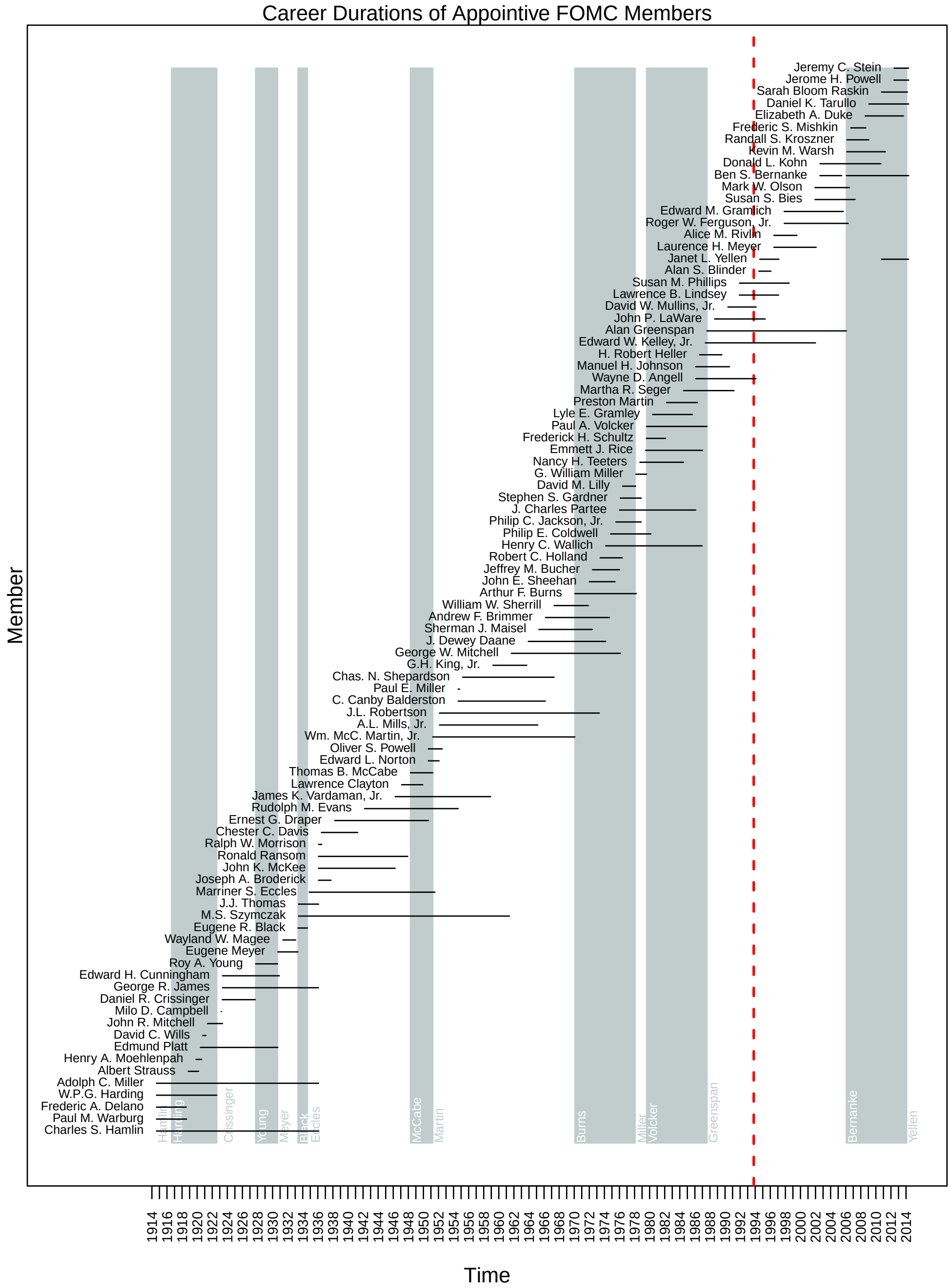
3.2 Methodology

Standard text analysis involves converting documents into vectors of word counts, and then using these vectors to look for patterns in the text. This known as a "bag of words" approach. We convert all transcripts and public summaries into vectors of word counts;

[94] See `http://www.federalreserve.gov/monetarypolicy/fomchistorical.htm`

[95] Despite having different names, Records of Policy Action and Minutes are "functional equivalents," according to the Federal Reserve. See, for example, Danker and Luecke (2005).

Figure 14: Career Durations of Appointive FOMC Members.



then we "stem" the words and remove "stop" words.[96] Our methodological contribution comes from generating measures for describing the evolution of text over time, in particular the relationship between different types of documents. We begin by describing properties that we would like a similarity metric to satisfy, and then propose a particular metric, "weighted cosine similarity," which does. We also describe other properties of the metric which we exploit in our empirical analysis.

3.2.1 Notation

Following standard terminology, we let $w \in \mathcal{D} = \{w^1, \ldots, w^D\}$ denote a *word* for a given dictionary $\mathcal{D}$, and $\delta = (d_1, \ldots, d_N)$ denote a *document*. We then denote a collection of documents, called the *corpus*, by $\mathcal{C} = \{\delta_1, ..., \delta_M\}$. We let n_i^j denote the number of times w^j appears in δ_i, and $d_i = \left(n_i^1, \ldots, n_i^D\right)'$ denote the *document-term vector* for document i.

Define Ω to be a $D \times D$ matrix capturing the relationship of words for a given dictionary $\mathcal{D}$, where $\omega^{ij} \in [0,1]$ is the *relationship* between words w^i and w^j. As words become more related, ω^{ij} is increasing; in particular, $\omega^{ii} = 1$. We impose symmetry on the relationship between words, so that $\omega^{ij} = \omega^{ji}$. $R_{ij}^{\mathcal{D}\Omega}$ is the *similarity metric* between documents d_i and d_j using dictionary $\mathcal{D}$ and relationship matrix Ω. We drop the superscripts for clarity except when they are needed.

3.2.2 Similarity Metric Axioms

In this section we present axioms that we would like our similarity metric to satisfy.

Scale

1. Scope: $R_{ij} \in [0,1]$.

2. Identity: $\forall d_j, d_i$, $R_{ij} \leq R_{ii}$, $R_{ii} = R_{jj} = 1$, and $R_{ij} = R_{ji}$.

3. Orthogonality: If $\exists\, w^k$ such that $\min\left\{n_i^k, n_j^k\right\} > 0$, then $R_{ij} > 0$.

[96] We use the set of Snowball stop words, and the Porter (1980) stemming algorithm.

4. Unitless: $R(c_1 d_i, c_2 d_j) = R(d_i, d_j)$ for positive constants c_1 and c_2.

Rank Preserving

1. Addition: For document-term vectors d_i, d_j, d_k, d_l where $R_{ij}^{\mathcal{D}\Omega} \geq R_{kl}^{\mathcal{D}\mathbf{\Omega}}$, if we add a word w^{D+1}, not contained in any of the relevant documents, to our dictionary to form $\mathcal{D}'$, then $R_{ij}^{\mathcal{D}'\Omega} \geq R_{kl}^{\mathcal{D}'\Omega}$.

2. Monotonicity: If we add the same positive vector d_{noise} to documents $d_i \neq d_j$ to form documents $d_{i'}$ and $d_{j'}$, then $R_{ij} < R_{i'j'}$.

3. Within-word similarity: For documents d_i and d_j, which are identical but for word w^k, then $R_{i\ell} < R_{j\ell}$ iff $||d_i - d_\ell|| < ||d_j - d_\ell||$.

4. Cross-word similarity: Suppose we have documents d_i and d_j, two weight matrices Ω and Ω' which are identical but for $\omega^{kl} > \omega'^{kl}$, and $R_{ij}^{\mathcal{D}\Omega'} < 1$. This implies that $R_{ij}^{\mathcal{D}\Omega} \geq R_{ij}^{\mathcal{D}\Omega'}$, with equality iff $\min \left\{ n_i^k, n_j^l \right\} + \min \left\{ n_i^l, n_j^k \right\} = 0$.

5. Synonym invariance: For documents d_i and d_j for which $n_i^r = n_j^r \; \forall r \notin \{\ell, m\}$, if $n_i^\ell + n_i^m = n_j^\ell + n_j^m$ and $\omega^{\ell s} = \omega^{ms} \; \forall s$, then for any third document d_k, $R_{ik}^{\mathcal{D}\Omega} = R_{jk}^{\mathcal{D}\Omega}$.

These properties are a natural allegory to the properties of idea similarity proposed in Bloom et al. (2013).

3.2.3 Cosine Similarity

For two document-term vectors d_1 and d_2, the *cosine similarity* of d_1 and d_2 is defined as

$$CS(d_1, d_2) = \frac{< d_1, d_2 >}{||d_1|| \cdot ||d_2||}$$

This similarity metric is simple to calculate and satisfies all axioms other than cross-word similarity and synonym invariance. It has the following additional attractive property, which is useful for data dimension reduction: If $d_1 = (d_{11}, d_{12})$ and $d_2 = (d_{21}, d_{22})$, where $||d_{11}|| \geq c||d_1||$ and $||d_{21}|| \geq c||d_2||$, then $|CS(d_1, d_2) - CS(d_{11}, d_{21})| \leq 1 - c^2$. We prove this result in Appendix 5.2.11, and correspondingly trim words who appear fewer than 5 times from the sample.

3.2.4 Generalized Cosine Similarity

For two document-term vectors d_1 and d_2, the *generalized cosine similarity* of d_1 and d_2 is defined as

$$CS_\Omega(d_1, d_2) = \frac{< d_1, d_2 >_\Omega}{||d_1||_\Omega \cdot ||d_2||_\Omega}$$

For symmetric positive definite Ω, this similarity metric satisfies all of the axioms. We present a brief proof for synonym invariance, as the rest are straightforward. It is sufficient to show that for documents d_i and d_j for which $n_i^r = n_j^r \; \forall r \notin \{\ell, m\}$, if $n_i^\ell + n_i^m = n_j^\ell + n_j^m$ and $\omega^{\ell s} = \omega^{ms} \; \forall s$, then for any document d_k, $< d_i, d_k >_\Omega = < d_j, d_k >_\Omega$. This condition will give equality for the numerator of the cosine similarity, and also give $< d_i, d_i >_\Omega = < d_j, d_i >_\Omega = < d_j, d_j >_\Omega$, letting d_k be d_i or d_j. This is equivalent to showing that $< \Omega d_i, d_k > = < \Omega d_j, d_k >$, for which it is sufficient to show that $\Omega d_i = \Omega d_j$. For element s, we have that $\sum_t \omega^{st} n_i^t = \omega^{s\ell} n_i^\ell + \omega^{sm} n_i^m + \sum_{t \notin \{\ell, m\}} \omega^{st} n_i^t = \omega^{s\ell}(n_i^\ell + n_i^m) + \sum_{t \notin \{\ell, m\}} \omega^{st} n_j^t = \omega^{s\ell} n_j^\ell + \omega^{sm} n_j^m = \sum_t \omega^{st} n_j^t$, which completes the proof.

3.2.5 Growth in Cosine Similarity

In much of our analysis, we focus not only on the overall similarity of the public and private documents, but also decompose the change in similarity into word-level changes. In this section, we show this decomposition. For notational convenience, we demonstrate how to decompose growth rates for unweighted cosine similarity; the steps are similar for the generalized version. Given $CS(d_1, d_2) = \frac{< d_1, d_2 >}{||d_1|| \cdot ||d_2||}$, the growth rate of the similarity measure can be decomposed to

$$\widehat{CS(d_1, d_2)} = <\widehat{d_1, d_2}> - \widehat{\|d_1\| \cdot \|d_2\|}.$$

Some algebra yields

$$\begin{aligned} \widehat{CS(d_1, d_2)} &= \sum_{j=1}^{D} \widehat{n_1^j} \left[\frac{n_1^j \cdot n_2^j}{< d_1, d_2 >} - \frac{\left(n_1^j\right)^2}{\|d_1\|^2} \right] \\ &\quad + \sum_{j=1}^{D} \widehat{n_2^j} \left[\frac{n_1^j \cdot n_2^j}{< d_1, d_2 >} - \frac{\left(n_2^j\right)^2}{\|d_2\|^2} \right]. \end{aligned} \tag{35}$$

As a result, the change in similarity can be decomposed into the growth rates of the respective words. For a change in, for instance, in n_1^j, there are four options:

$$\widehat{n_1^j} > 0, \left[\frac{n_1^j \cdot n_2^j}{< d_1, d_2 >} - \frac{\left(n_1^j\right)^2}{\|d_1\|^2} \right] > 0$$

$\Rightarrow$ increasing similarity from increasing already-underrepresented words,

$$\widehat{n_1^j} < 0, \left[\frac{n_1^j \cdot n_2^j}{< d_1, d_2 >} - \frac{\left(n_1^j\right)^2}{\|d_1\|^2} \right] < 0$$

$\Rightarrow$ increasing similarity from increasing already-overrepresented words,

$$\widehat{n_1^j} > 0, \left[\frac{n_1^j \cdot n_2^j}{< d_1, d_2 >} - \frac{\left(n_1^j\right)^2}{\|d_1\|^2} \right] < 0$$

$\Rightarrow$ decreasing similarity from increasing already-overrepresented words, and

$$\widehat{n_1^j} < 0, \left[\frac{n_1^j \cdot n_2^j}{< d_1, d_2 >} - \frac{\left(n_1^j\right)^2}{\|d_1\|^2} \right] > 0$$

$\Rightarrow$ decreasing similarity from decreasing already-underrepresented words.

For any similarity mapping, it is possible to run numerical counterfactual simulations to estimate the effect that each word's evolution has on the aggregate change. For the cosine similarity, the word-level derivatives are analytically straightforward and relate to a clear intuition. If a word's use in a document increases, this leads to an increase in similarity if the word had previously been relatively underrepresented in that document. The magnitude of the effect is increasing in the size of the under-representation.

3.2.6 Growth in Cosine Similarity in the Data

In the data, we are less interested in growth over time, and more interested growth relative to the baseline. In particular, in line with the model, there will be some set of discussion of topics given transparency, and a different set with privacy, and we are interested in understanding how similarity changes between the two regimes. We approximate the growth rates in similarity (which in the previous subsection are in continuous time) with discrete approximations from baseline. In particular, for private statements p and press releases q, we calculate

$$\begin{aligned}
\widehat{CS(q_k, p_k)} &\approx \frac{2\left[CS(q_k, p_k) - CS(q_0, p_0)\right]}{CS(q_k, p_k + CS(q_0, p_0)} \\
&= \sum_{j=1}^{D} \left(\frac{n^j_{q_k} - n^j_{q_0}}{n^j_{q_0}} \right) \left[\frac{n^j_{q_0} \cdot n^j_{p_o}}{< q_0, p_0 >} - \frac{\left(n^j_{q_0}\right)^2}{\|q_0\|^2} \right] \\
&+ \sum_{j=1}^{D} \left(\frac{n^j_{p_k} - n^j_{p_0}}{n^j_{p_0}} \right) \left[\frac{n^j_{q_0} \cdot n^j_{p_o}}{< q_0, p_0 >} - \frac{\left(n^j_{p_0}\right)^2}{\|p_0\|^2} \right].
\end{aligned}$$

This allows us decompose, for each word-document dyad, the source of the change in similarity. q_0 and p_0 are calculated as the average word shares for the respective documents for the final 50 meetings of the the pre-period (for those meetings, the baseline is calculated leaving the own meeting out of the baseline).

3.2.7 Constructing the Term-Relationship Weight Matrix

There are many possible weight matrices. We introduce a new method, a *dictionary-based* approach, as the natural successor the the General Inquirer lists generated in the 1960s. Instead of relying on experts to categorize words as being part of certain topics or relating certain sentiments, we develop an algorithm which translates how words relate to each other using their definitions. In this particular context, we take advantage of the Oxford

Dictionary of Economics (ODE), which contains 3,423 entries (all of which are related to economics in some way). This allows us to construct a weight-matrix which is not dependent on the specific data we analyze, and therefore would be the same for all research questions studying the language choices of economists. Furthermore, since the purpose of dictionaries is to relate words to each other, it seems like a natural tool to leverage as the ideological success to the General Inquiry lists generated in the 1960s

For example the entry for *inflation* is

> A persistent tendency for nominal prices to increase. Inflation is measured by the proportional changes over time in some appropriate price index, commonly a consumer price index or a GDP deflator. Cost inflation is started by an increase in some element of costs, for example the oil price explosion of 1973–4. Demand inflation is due to too much aggregate demand. Once started, inflation tends to persist through an inflationary spiral, in which various prices and wage rates rise because others have risen. Hyperinflation is extremely rapid inflation, in which prices increase so fast that money largely loses its convenience as a medium of ex change.

We construct a weight matrix Ω by taking the cosine similarity of stems (the term columns) from the ODE document-term matrix, with the intuition being that words which show up in definitions together are more similar. If this matrix fails to be positive definite, we suggest scaling the off diagonals by some $\omega < 1$.[97] There are 4,798 stems in the ODE, 4,032 of which appear in the documents of our analysis.

[97] Any off-diagonal entry equal to one must be decreased by an arbitrarily small positive number, due to Sylvester's criterion for positive-definiteness. This only occurs for 4 stems in our data.

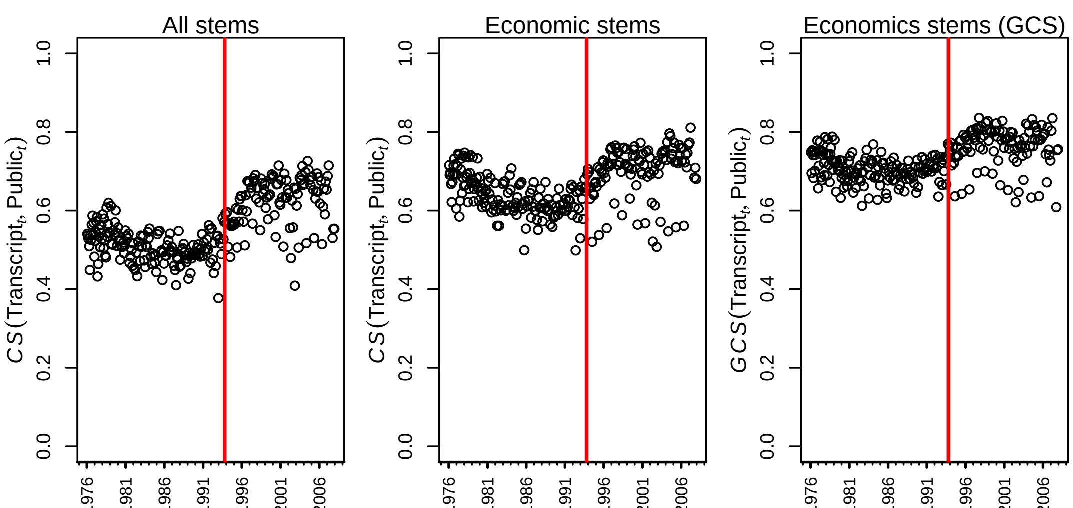
3.3 Results

In this section, we discuss the effects of the transparency reform on the similarity of the transcripts and public documents. For the most part, we do so graphically, plotting over time how similarity evolves.

3.3.1 The Evolution of Language after Transparency

To start, we take the standard approach, which is to plot how the aggregate similarity evolves over time. In the left panel Figure 15 shows that that the similarity of the FOMC meeting transcripts and corresponding public summaries increased following the unexpected enforcement of the Sunshine Act. In the middle panel, we restrict the dictionary to terms in the Oxford Dictionary of Economics, and find a similar increase in cosine similarity following transparency. In the right panel, we allow for cross-word interactions using our term-relationship weight matrix, and, while there is a slightly higher overall similarity, we again see a rise in similarity following late 1993.

Figure 15: Cosine Similarity of FOMC Transcripts and Corresponding Public Summaries from 1976-2007

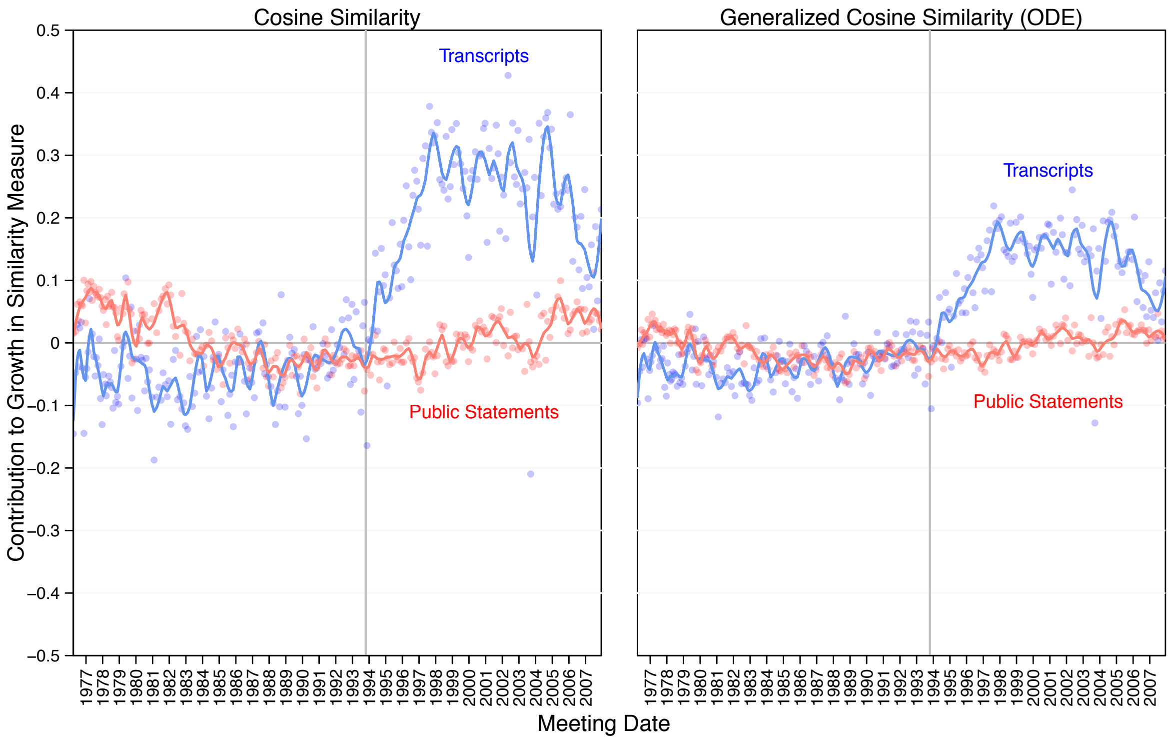


In all cases, the increase in similarity after the transparency reform is not immediate, but gradual, taking several years before reaching a new steady state. Since the transcription process changed after the reform, it is reassuring that the increase in similarity therefore cannot only be due to a change in method. The gradual increase is also consistent with a transition path, as the FOMC may not have immediately known its preferred response to transparency.

The increase in similarity per se is not informative about its underlying cause. One explanation could be that the meeting changed, as FOMC members adjusted their language to be more public-friendly, in the same way that the press releases were always designed with the public in mind. An unrelated story would be that the press release had previously not been a complete representation of the FOMC discussions, and changed after they became ex-post verifiable. A well known issue with vector similarity is its inability to distinguish between these two types of stories, which we overcome with the decomposition derived in the previous section.

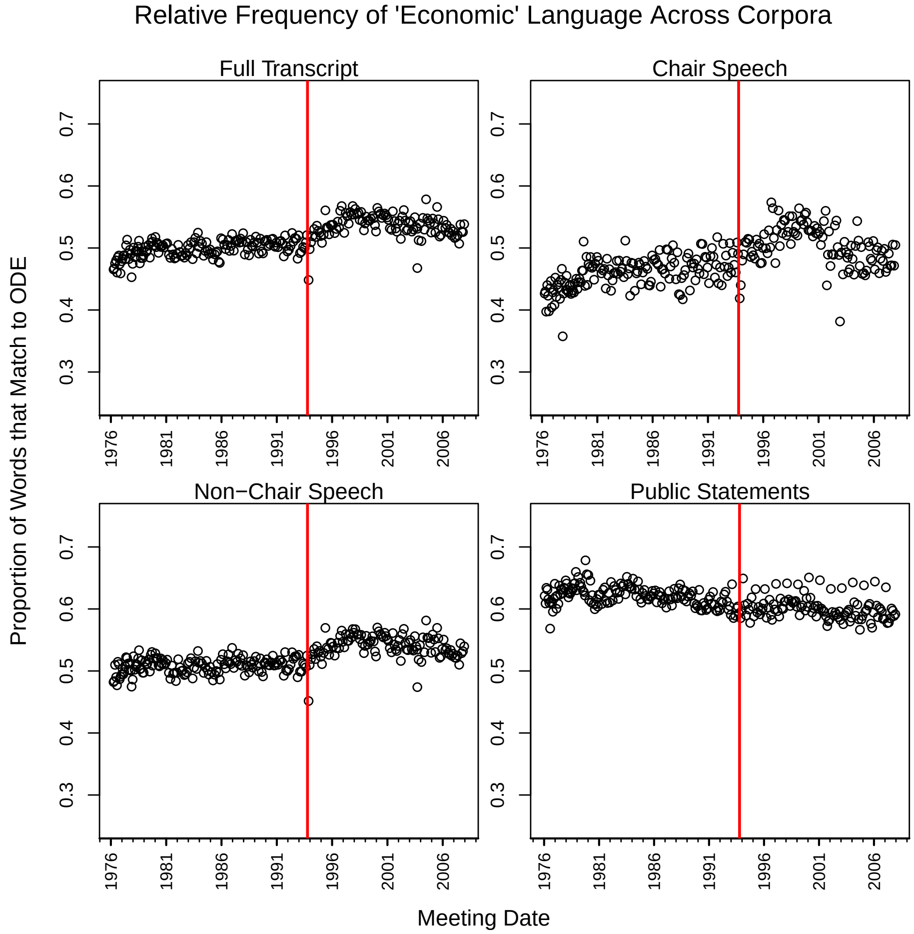
In figure 16, we decompose the change in similarity at the document level, in order to show how the relationship between the documents would have changed if only the transcripts (blue line) or public statements (red line) evolved (holding fixed the other document). Both within the just-economics words (left panel) and including the weights (right panel), it is clear that almost the entirety of the increase in similarity comes through changes in the transcripts, and not because the public statements become more like the transcripts. Using the generalized cosine similarity the result is qualitatively similar, although the effect is scaled down slightly.

Figure 16: Decomposition of similarity growth into public and private contribution



Another way to show that the change in language is being driven by economic words in the language is to see how the proportion of words matching to the Oxford Dictionary of Economics changed over time. In figure 17, the proportion of these economic words increased in the transcripts after transparency was enforced, both for the chair and non-chair participants. In the public minutes, however, the use of economic language remained roughly constant over time, with a slight decrease in the overall proportion of economic words over the sample period. While this reinforces the argument that the language change after transparency was in the meetings, like the similarity measure it is not a methodology which allows us to determine if any specific words were most responsible for the evolution of language.

Figure 17: Economics language in speech over time



Using our growth decomposition does allow us to identify the specific most important words. In Figure 18, we show the 30 words most (and least) responsible for the growth in similarity in the transcripts and public statements, calculated using equation 35. We also distinguish words whose use declined from those whose use increased. The left panel shows the results using the cosine similarity measure, the right panel adds in the weights for the generalized cosine similarity measure. As shown in the previous figures in this section, it is clear that most of the change is coming from the transcripts. Furthermore, there are a few words which are substantially responsible for the change. The words responsible for positive changes in growth and whose usage increased were mostly related to economics, such as "growth," "market," and "price" (in other words, before the reform "growth" had been underused in the transcripts relative to the public statements). The words responsible for positive changes in growth but whose usage decline were mostly not related to economics, such as "think," "that," and "don't." Using our generalized cosine similarity measure that accounts for cross-word similarities does not substantially change which words are the major contributors to similarity growth over time.

The decline in the word think is consistent with anecdotal evidence that the FOMC meetings became less of a conversation after transparency, with members bringing in speeches of their own. The increase in common economics words suggests that the type of language that FOMC members decided to prepare was much more in line with the public statements along certain identifiable dimensions.

Figure 18: Word similarity growth contribution for generalized cosine similarity

3.4 Conclusion

We develop two novel textual methods to examine how the sudden enforcement of the Sunshine Act in 1993 affected communication in FOMC meetings. Our goal is to identify a few words or topics who were most responsible for the change. Instead of first grouping the language into a few clusters, and then focusing our analysis on those clusters (as is often done in computational linguistics, e.g. Hansen et al. (2014)), we instead group the data by how much it was affected by the policy change, and find that a few words were primarily responsible for the changed behavior after transparency reform. Furthermore, we develop methods which allow for semantic similarity across words, using the definitions in the Oxford Dictionary of Economics. We use our measure to show that the change in language also corresponded with a change in meaning more broadly.

In particular, we find that transparency led the previously private FOMC meeting conversations to become more similar to the always-released public statements. In our setting, we found that the proportion of speech related to economics increased after the policy change for both the chair and the non-chairs. To uncover the dimensions of this change, we decomposed the change in cosine similarity of the public and private documents into word level contributions. We found that most of the change in behavior came from FOMC members shifting their speech towards popular economic topics, such as "inflation" and "growth," and away from hedging language such as "think." These results are robust to restricting our analysis to terms in the Oxford Dictionary of Economics, and to allowing our similarity metric to account for cross-word similarities with a relationship weight matrix. Our proposed methods extend and add robustness to any analysis considering similarity of agents over time.

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5 Appendix

5.1 Appendix: Equilibrium Effects of Firm Subsidies

5.1.1 Alternate Derivation of Direct and Indirect Effects.

In this section, I show that the direct and indirect effects derived in the main body of text also follow from a decreasing returns to scale model *a la* Lucas, Robert E (1978), as well as a nested CES framework. When possible, I try to keep notation for the relevant parameters the same as in the main text.

For the span-of control framework, I maintain the assumptions that in each sector, a single final good Q_s is produced by a representative firm in a perfectly competitive market, and that the utility function of the representative consumer is

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

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

$$Q_s = \left(\frac{P_s}{\phi}\right)^{\frac{1}{\phi-1}}, \tag{36}$$

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

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

$$Q_s = \sum_{j=1}^{N_s} q_{js}. \tag{37}$$

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

$$q_{js} = A_{js} L_{js}^{\alpha}, \tag{38}$$

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

$$y_{js} = \left(1 + \tau_{y_j}\right) P L_j^{\beta}, \tag{39}$$

and profits are given by

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

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

$$L_j = \left(\frac{w}{\left(1 + \tau_{y_j}\right) P_s A_{js} \alpha} \right)^{\frac{1}{\alpha - 1}}. \tag{40}$$

Plugging equations 40 and 37 into equation 36 and taking the growth rates yields

$$\hat{P} = \frac{\frac{\alpha}{\alpha - 1}}{\left(\frac{\alpha}{1-\alpha} + \frac{1}{1-\phi}\right)} \sum_{j=1}^{N_s} \left[\left(\widehat{\frac{1}{\left(1 + \tau_{y_j}\right)}} \right) \frac{q_{js}}{\sum_{k=1}^{N_s} q_{ks}} \right]. \tag{41}$$

Plugging into equation 39 allows us to generate how the revenue of each firm grows as the firm-specific subsidies grow:

$$\hat{y}_j = \frac{2-\alpha}{1-\alpha}\frac{\frac{\alpha}{\alpha-1}}{\left(\frac{\alpha}{1-\alpha}+\frac{1}{1-\phi}\right)}\sum_{l=1}^{N_s}\left[\left(\widehat{\frac{1}{\left(1+\tau_{y_j}\right)}}\right)\frac{q_{ls}}{\sum_{k=1}^{N_s} Q_{ks}}\right] - \frac{2-\alpha}{1-\alpha}\left(\widehat{\frac{1}{\left(1+\tau_{y_j}\right)}}\right) \tag{42}$$

and

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

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

For the nested-CES framework, I only diverge from the baseline model by assuming that the representative consumer has CES utility over the final goods. In each sector, a single final good Q_s is produced by a representative firm in a perfectly competitive market. The utility function of the representative consumer (who has exogenous income I) is therefore

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

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

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

The revenue in sector S will therefore be

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

Revenue for each intermediate good producer will be

$$\begin{aligned} y_{js} =& p_{js} q_{js} \\ =& \frac{P_s^{1-\phi} P^{\phi-1} I}{(P_s)^{1-\sigma}} p_{js}^{1-\sigma}. \end{aligned} \tag{43}$$

Given (as in the main text) CES production from the representative final goods firms in each sector, Cobb-Douglas production from each intermediate goods producer, and firm-specific wedges of capital and labor, the growth rates of the final good producer's price, and the revenue and price of the intermediate good producer satisfy:[98]

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

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

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

wedges is therefore:

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

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

5.1.2 An Issue with Industry Codes

In many settings researchers use industry codes instead of product codes, since product codes are unavailable. In other settings, Delgado et al. (2014) and Hoberg and Phillips (2010) argue that industry codes are not the optimal way to cluster product markets In this section, I show that using industry codes as a proxy for competition will lead to biased estimates for the effects of competitive exposure, as firms will be assigned too much exposure to firms within their industry, and too little exposure to firms outside the industry. To see this, rewrite equation 9 (with an indicator $i_k = 1$ for firm k self-reporting as being in industry i) as

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

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

$$\hat{y}_j' = \beta e_j - \beta\theta \frac{\sum_{k=1}^{N} i_k \times e_k \times y_k}{Y_i}. \tag{44}$$

The difference between the two measures is

$$\hat{y}_j - \hat{y}_j' = \beta\theta \left[\sum_{s=1}^{S} \frac{\omega_{js}}{Y_s} \left(\left(\left(\sum_{k=1}^{N} i_k \times e_k \times y_k \right) \frac{Y_s}{Y_i} - \left(\sum_{k=1}^{N_s} i_k \times e_k \times y_{ks} \right) \right) - \left(\sum_{k=1}^{N_s} (1 - i_k) \times e_k \times y_{ks} \right) \right) \right]. \tag{45}$$

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

$$\left(\left(\sum_{s' \neq s}^{S} \sum_{k=1}^{N} i_k \times e_k \times y_{ks'} \right) \frac{Y_s}{Y_i} - \left(\left(\frac{Y_s}{Y_i} - 1 \right) \sum_{k=1}^{N_s} (i_k \times e_k \times y_{ks}) \right) \right). \tag{46}$$

$\left(\sum_{s' \neq s}^{S} \sum_{k=1}^{N} i_k \times e_k \times y_{ks'} \right) \frac{Y_s}{Y_i}$ captures the fact that each industry produces products which are produced in other industries. As a result, given the original model, there will be indirect effects which industries do not impose on themselves. This will would lead one to overestimate the aggregate effects of firm specific programs if one estimated equation 44.

$\left(\left(\frac{Y_s}{Y_i} - 1 \right) \sum_{k=1}^{N_s} (i_k \times e_k \times y_{ks}) \right)$captures the fact that each firm may not produce the same set of products as its own industry. This effect would lead one to underestimate the aggregate effects of firm specific programs if one estimated equation 44.

Finally, $\left(\sum_{k=1}^{N_s} (1 - i_k) \times e_k \times y_{ks} \right)$ captures the fact that there may be firms in other industries who produce the same products as firm j. This effect will lead one to overestimate the aggregate effects if one estimated one estimated equation 44.

5.1.3 Within-District Exposure

While the notation used previously described each sector sector as output product markets, that is not necessary. In particular, I also focus on **local markets** for primary inputs.

As eligible firms expand their primary inputs, the total change of formal manufacturing employment within a district may not be equal to the sum of the relative growth of eligible firms. Similar to the previous subsection, define $d \in \{1, D\}$ as the districts of India,[99] d_j as firm j's district, w_k as the cost of primary inputs at firm k, β^e as the relative effect in increasing primary input use, and θ^d as the competitive mediator. As a result the $N \times N$ *primary-input-primary-input* matrix (denoted $\mathbf{B}$), where element

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

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

$$\xi^d = \beta \left(\mathbf{I} + \theta^d \mathbf{B}' \right) \mathbf{e} \tag{47}$$

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

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

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

those years.[100] Figure 1 plots the distribution of the share primary input costs in each district newly eligible as of 2006. Another concern with geography-based identification is that districts with higher shares of newly eligible firms tend to be in the south, along the costs, or near major cities.[101]

As in the earlier sections, I estimate 18 with regressions of the following form:

$$\ln\left(y_{jt}\right) = \beta Small_{jt} + \Theta Exposure_{jt} + \sum \gamma_{jt} X_j + \eta_j + \eta_t + \epsilon_{jt}, \tag{48}$$

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

Tables 20 and 21 presents the regression results. Focusing on column 4, which includes both the local and product-based exposure measures, as well as post reform X state and industry fixed effects, the effects of local competition appear broadly similar to those for product market competition, as the coefficients on the exposure measures are reasonably similar in magnitude to those for the direct effects. Without the state and industry fixed effects, in column 3, the coefficient declines to being close to zero. Due to the variability in the coefficients, it is difficult to know the extent to which a firm gaining eligibility for subsidies affect its neighbors, although this is a promising area for future research.

5.2 Appendix: How Federal Reserve Discussions Respond to Increased Transparency

5.2.1 Model

In order to take advantage of the richness provided by being able to analyze how members behavior changes on specific topics, we extend existing models on career concerns

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

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

Table 20: Differences in Differences Estimates of the Local Effects of Firm Subsidies on Output

	(1)	(2)	(3)	(4)
A. Effect on ln(sales)				
Post Reform * Small Firm	0.221***	0.254***	0.254***	0.288***
	(0.08)	(0.081)	(0.089)	(0.09)
Post Reform * In-District Exposure	-0.011	-0.391	0.04	-0.374
	(0.243)	(0.281)	(0.244)	(0.281)
Post Reform * In-State			-0.208*	-0.217*
			(0.111)	(0.114)
Number of Observations	271921	271921	270088	270088
R-squared	0.729	0.725	0.729	0.725
B. Effect on firm continuing to exist				
Post Reform * Small Firm	0.033***	0.037***	0.033***	0.035***
	(0.004)	(0.005)	(0.005)	(0.005)
Post Reform * In-District Exposure	0.002	-0.017	-0.013	-0.029*
	(0.015)	(0.017)	(0.015)	(0.017)
Post Reform * In-State			-0.016***	-0.013*
			(0.006)	(0.007)
Number of Observations	298426	298426	296373	296373
R-squared	0.729	0.725	0.729	0.725
Controls for Post Reform*Assets	Y	Y	Y	Y
Fixed Effects for Post Reform * 3-digit Industry and State	N	Y	N	Y

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

and transparency. Meade and Stasavage (2008) and Hansen et al. (2014) carefully lay out the literature on why agents may herd in order to appear smart. Ottaviani and Sø rensen (2001), for instance, lay out propositions where agents who are confident in their self-knowledge have incentives to deviate from public signals, while those who are not herd. Regardless of the direction of herding, the literature is in agreement that if career concerns play a role, more transparency leads agents to pay more attention to public signals

Table 21: Differences in Differences Estimates of the Local Effects of Firm Subsidies on Inputs

	(1)	(2)	(3)	(4)
A. Effect on ln(total liabilities)				
Post Reform * Small Firm	0.237***	0.285***	0.294***	0.333***
	(0.072)	(0.073)	(0.081)	(0.082)
Post Reform * In-District Exposure	0.015	-0.408*	0.030	-0.451*
	(0.216)	(0.25)	(0.224)	(0.257)
Post Reform * In-State			-0.255**	-0.282***
			(0.102)	(0.104)
Number of Observations	274724	274724	272843	272843
R-squared	0.729	0.725	0.729	0.725
B. Effect on ln(total costs)				
Post Reform * Small Firm	0.261***	0.306***	0.303***	0.330***
	(0.073)	(0.074)	(0.086)	(0.087)
Post Reform * In-District Exposure	0.114	-0.346	0.046	-0.426
	(0.244)	(0.276)	(0.256)	(0.289)
Post Reform * In-State			-0.221**	-0.236**
			(0.104)	(0.106)
Number of Observations	280741	280741	278811	278811
R-squared	0.729	0.725	0.729	0.725
Controls for Post Reform*Assets	Y	Y	Y	Y
Fixed Effects for Post Reform * 3-digit Industry and State	N	Y	N	Y

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

before selecting their action, in particular for agents for whom career concerns play a more important role.

We take a somewhat different approach, where there is no ex-ante public signal in our model. In general, when agents know that their actions will be revealed with a delay, the correct "career concern" statistic is likely to the expectation of future opinions, not concurrent public opinion. This is an important distinction since each agent's expectation of future public opinion may be a private signal. While agents will adjust their actions in response to increased public oversight, it will be in the direction of each agents private signal. However, in the data we are unable to identify each FOMC member's private signal. As a result, we add another way where the public affects agent's behavior, in that there are topics which the public cares more about some issues others. As a result, regardless of the strength of each agent private signals of what topics are important, they will shift their attention to the issues more salient to the public.

Furthermore, in the context of the FOMC is is not clear which agents have higher career concerns. In our interviews with central bankers, one concern with transparency was that it might affect those who are looking to be written about "in the history books," who may not be identifiable from ex-ante characteristics.

FOMC members get to choose two actions. The first action is how much they speak about each issue in the meetings, and the second is how much they address each issue in the press release. In the model in the main text, we assume that the FOMC members come to the meeting with some amount of information which is unrelated to the degree of transparency. In appendix 5.2.5, we show that allowing effort to respond in the face of transparency will lead to increased effort, but it will not lead to a convergence of public and private speech, which is what we observe in the data.

Members have signals about the true state of the world, and discuss what they think. In both models, speech has a reduced-form effect on the policy reached, where speech which is closer to the truth leads to better policy outcomes. Before transparency, the only goal for the FOMC members in the meetings is to get a good policy outcome given the information they have.

5.2.2 Setup of FOMC member's motivations

The vector $\mathcal{S} = (s_1, s_2, \ldots, s_N)'$ contains the issues s_i which may be relevant to FOMC policy. These theoretically include issues such as asset market bubbles, unemployment, and so on. In the data section, we describe how we distinguish the topics using dictionary-based methods. After the meetings, the vector $\boldsymbol{\gamma} = (\gamma_1, \gamma_2, \ldots, \gamma_N)'$ shows if an issue turned out to be important or not, with $\gamma_i = 1$ if s_i turns out to be important, and 0 otherwise. While the actual FOMC policy can mitigate the consequences of important features of the economy, $\boldsymbol{\gamma}$ is independent of the actual policy taken. At the start of each FOMC meeting, each member m has an information set $\boldsymbol{\beta}_m = (\beta_{m1}, \beta_{m2}, \ldots, \beta_{mN})'$, with $\beta_{mi} = \mathbb{E}_m(\gamma_i)$. Depending on the model, $\boldsymbol{\beta}_m$ may be endogenous. During the meeting, they have communication $\boldsymbol{\alpha}_m = (\alpha_{m1}, \alpha_{m2}, \ldots, \alpha_{mN})'$, with $\alpha_{mi} \geq 0$ the forcefulness with which issue s_i is discussed. α_{mi} is a composite of both length and certainty, that is to say α_{mi} is increasing in both how long the member talks about the issue, and how certain she is about it, with the two being compliments. The FOMC member's speech has the potential to change monetary policy. In a reduced-form way, the FOMC member has incentives to be correct, that is to say her realized utility over the monetary policy decision can be expressed as

$$-\sum_{j=1}^{N}\left[\left(\gamma_j - \alpha_{mj}\right)^2\right].$$

For notational simplicity we drop the m subscript throughout, since each FOMC member is going be behave in a similar fashion given their information. We do not write out a game where the career concern is more of a tournament - trying to become the Fed Chair - and there may be who in equilibrium use low expected value but high risk strategies. Instead, we assume that each agent is judged solely on the basis of their own actions, and there are sufficient successful outcomes (such as University Professorships) that one agents success in no way limits the others. Furthermore, the public may care about some issues more than others. The vector $\boldsymbol{\sigma} = (\sigma_1, \sigma_2, \ldots, \sigma_N)$, where $\gamma_i \in (0, 1)$ contains the weight of each issue in the public's imagination. Regardless of the importance of an issue, the public rewards agents who talk more about issues the public cares more about.

5.2.3 Credit-Seeking

Suppose that the FOMC members trade off their desire to make the "right" policy for the country and their desire to be recognized for being correct. Those goals potentially diverge because the decision making is ex-ante, whereas credit is given ex-post. The public appreciates public officials who are more forceful about issues which ex-post were actually important, and those who focus on issues more salient to the public. τ is the weight that the FOMC member puts on the public's opinion. Without transparency, $\tau = 0$, since the public cannot infer anything. The Fed also has the opportunity to communicate with the public using the minutes. The minutes contain communications $\boldsymbol{\theta} = (\theta_1, \theta_2, \ldots, \theta_N)'$, with $\theta_i \geq 0$ the forcefulness with which issue s_i is discussed. As a result θ_i is the public analog of α_i. Regardless of transparency, the public knows what the public statements say, and judge the FOMC members accordingly. Since the public responds to the release of the minutes, the FOMC also has an incentive to release public information which is correct.

As a result, the ex-post utility of the FOMC member is

$$U(\boldsymbol{\alpha}) = \tau\left(2\gamma'\boldsymbol{\alpha} + \boldsymbol{\sigma}'\boldsymbol{\alpha}\right) - (1-\tau)(\gamma - \boldsymbol{\alpha})'(\gamma - \boldsymbol{\alpha}) + \left(2\gamma'\boldsymbol{\theta} + \boldsymbol{\sigma}'\boldsymbol{\theta}\right) - (\gamma - \boldsymbol{\theta})'(\gamma - \boldsymbol{\theta}).$$

The expected utility is therefore

$$\begin{aligned}\mathrm{E}\left(U(\boldsymbol{\alpha})\right) =& \tau\left(2\boldsymbol{\beta}'\boldsymbol{\alpha} + \boldsymbol{\sigma}'\boldsymbol{\alpha}\right) - (1-\tau)(\boldsymbol{\beta} - \boldsymbol{\alpha})'(\boldsymbol{\beta} - \boldsymbol{\alpha}) + \left(2\boldsymbol{\beta}'\boldsymbol{\theta} + \boldsymbol{\sigma}'\boldsymbol{\theta}\right) - \\ & (\boldsymbol{\beta} - \boldsymbol{\theta})'(\boldsymbol{\beta} - \boldsymbol{\theta}) + (2-\tau)\boldsymbol{\beta}'(\mathbf{1} - \boldsymbol{\beta}).\end{aligned}$$

The first-order condition for an interior α_k is

$$\alpha_k = \beta_k + \frac{1}{2}\frac{1}{1-\tau}\left(\tau\beta_k + \sigma_k\right).$$

As a result,

$$\alpha_k = \begin{cases} \left(1 + \frac{1}{2}\frac{\tau}{1-\tau}\right)\beta_k + \sigma_k & \text{if } \beta_k \leq \frac{1-\tau}{1-\frac{1}{2}\tau} - \sigma_k \\ 1 & \text{otherwise} \end{cases}$$

where $\boldsymbol{\alpha} = \boldsymbol{\beta}$ if $\tau = 0$. Similarly

$$\theta_k = \begin{cases} \left(1 + \frac{1}{2}\right)\beta_k - \sigma_k & \text{if } \beta_k \leq \frac{2}{3} - \sigma_k \\ 1 & \text{otherwise} \end{cases}$$

5.2.4 Predictions of credit-seeking model

While we can only observe measures of $\boldsymbol{\alpha}$, $\boldsymbol{\theta}$, and $\boldsymbol{\sigma}$ in the data, the model laid out above still makes stark predictions about how they change as τ increases.

Proposition 1: $\forall k$, α_k is weakly increasing when τ increases .

Proof:

$$\frac{\partial \alpha_k}{\partial \tau} = \begin{cases} \frac{1}{2}\frac{1}{(1-\tau)^2}\beta_k & \text{if } \beta_k \leq \frac{1-\tau}{1-\frac{1}{2}\tau} \\ 0 & \text{otherwise} \end{cases}$$

Therefore, regardless of the initial level of β_k, α_k is weakly increasing in τ.

Lemma 2: The increase in α_k as τ increases will be larger for higher β_k, as long as $\beta_k \leq \frac{1-\tau}{1-\frac{1}{2}\tau}$.

Proof:

$$\frac{\partial \alpha_k}{\partial \tau \partial \beta} = \begin{cases} \frac{1}{2}\frac{1}{(1-\tau)^2} & \text{if } \beta_k \leq \frac{1-\tau}{1-\frac{1}{2}\tau} \\ 0 & \text{otherwise} \end{cases}$$

As a result, there will also be a widening of topics - some will be discussed more, and others will be discussed less. Furthermore, this result holds for each member of the committee. If people changed their speech in response to what the chair says, then the chair's speech will not be affected, which is testable.

Lemma 3: The increase in α_k as τ increases will be larger for higher σ_k.

Proof:

$$\frac{\partial \alpha_k}{\partial \tau \partial \sigma} = \frac{1}{2} \frac{1}{(1-\tau)^2}$$

As a result, issues which the population finds more salient will be referenced more after transparency.

Proposition 3: $|\alpha_k - \theta_k|$ is weakly decreasing in τ if $\tau < \frac{1}{2}$.

Proof: For each k, $|\alpha_k - \theta_k| = \left| \min\{ \left(1 + \frac{1}{2}\frac{\tau}{1-\tau}\right) \beta_k, 1\} - \min\{ \left(1 + \frac{1}{2}\right) \beta_k, 1\} \right|$. Suppose $\tau < \frac{1}{2}$. Then

$$\frac{\partial |\alpha_k - \theta_k|}{\partial \tau} = \begin{cases} -\frac{1}{2}\frac{1}{(1-\tau)^2} & \text{if } \beta_k \leq \frac{1-\tau}{1-\frac{1}{2}\tau} \\ 0 & \text{otherwise} \end{cases}$$

As a result, this model predicts that after transparency there will be more talk about economic topics generally, the increase will be relatively larger for "important" topics, and the increase will lead to increased similarity between the speech of the transcripts and of the public documents, both overall and within topics.

5.2.5 Effort Model

In traditional principle-agent models, increased transparency is designed to lead to increased effort. As a result, we replace the twist in the previous model - where FOMC members have an extra incentive to focus on the correct issues - with one where their prior information on γ is a function of the effort they put forth, with increased effort leading to a more precise and accurate assessment. If the public can observe that the FOMC member's signal was more precise, then increased transparency will increase the amount of effort. She puts in effort e, and the return to effort is a set of independent signals about the N states of the world, $S_i = \frac{B_i}{\mathcal{E}_i}$, where $\mathcal{E}_i$ is a Poisson(e) random variable. Suppose that $B_i|\beta_i, \mathcal{E}_i \sim \text{Bin}(\mathcal{E}_i, \beta_i)$ and that $\beta_i|B_i, \mathcal{E}_i \sim \text{Beta}(1 + B_i, 1 + \mathcal{E}_i - B_i)$. Finally, assume that the prior on β_i is Beta$(1, 1)$, which is a uniform distribution. The agent's problem has two stages:

Second Stage: In this stage, $(\mathcal{E}_i, S_i)_{i=1}^{n}$ are known to the agent. The agent minimizes the expected sum of squares $\sum_{i=1}^{N} E\left((\alpha_i - \gamma_i)^2 | B_i, \mathcal{E}_i\right) = \sum_{i=1}^{N} \left(E((\alpha_i - \beta_i)^2 | B_i, \mathcal{E}_i) + E(\beta_i(1 - \beta_i) | B_i, \mathcal{E}_i)\right)$ with respect to α. This leads her to report the posterior mean $\alpha_i(B_i, \mathcal{E}_i) = \frac{1+B_i}{2+\mathcal{E}_i}$.

First Stage: In this stage, the agent maximizes her expected utility $-\tau \sum_{i=1}^{N} E\left((\alpha_i(B_i, \mathcal{E}_i) - \gamma_i)^2\right) - ce^2 = -\tau \sum_{i=1}^{N} \left(E\left((\alpha_i(B_i, \mathcal{E}_i) - \beta_i)^2\right) + E\left(\beta_i(1 - \beta_i)\right)\right) - ce^2$ with respect to e. The solution to her problem is obtained by minimizing $E\left(\frac{N\tau}{12+6\mathcal{E}_i}\right) + ce^2$.

5.2.6 Predictions of effort model

Proposition 4: The optimal level of effort is increasing in τ.

Proof: We have that $\frac{\partial}{\partial e} E\left(\frac{1}{12+6\mathcal{E}_i}\right) < 0$ and $\frac{\partial^2}{\partial e^2} E\left(\frac{1}{12+6\mathcal{E}_i}\right) > 0$; see the next subsection . The result follows, since the first order condition is $FOC(\tau, e) = \frac{\partial}{\partial e} E\left(\frac{N\tau}{12+6\mathcal{E}_i}\right) + 2ce = 0$

and $\frac{de^*}{d\tau} = -\frac{\frac{\partial FOC(e^*,\tau)}{\partial \tau}}{\frac{\partial FOC(e^*,\tau)}{\partial e}} > 0.$

Proposition 4 implies that the agent will get a more precise signal of whether something is likely to be important after the policy change. Topics which are likely to be important will be spoken of with more certainty. Furthermore, if only a small proportion of topics are in fact likely to be important, there will be a decrease in the number of topics spoken about forcefully. As a result, proposition 1 in its exact form from the previous model does not hold, but a looser form of it might. Proposition 2 does hold if there is in fact variation in the likelihood of topics being important or unimportant; proposition 3 should not hold in this model, since there is no incentive for private and public speak to differ, conditional on the signal received. This implies that we are able to distinguish a model of credit-seeking from one of effort.

5.2.7 Effort model derivations

In this subsection, we prove the Proposition in the previous subsection.

Second Stage

From the first order condition and the expectation of a beta random variable, we have that $\alpha_i = E(\beta_i | B_i, \mathcal{E}_i) = \frac{1+B_i}{2+\mathcal{E}_i}$.

First Stage

The derivation follows from iterated expectations:

$$
\begin{aligned}
E(\alpha_i(B_i,\mathcal{E}_i)|\,\beta_i,\mathcal{E}_i) &= \frac{1+\mathcal{E}_i\beta_i}{2+\mathcal{E}_i} \\
Var(\alpha_i(B_i,\mathcal{E}_i)|\,\beta_i,\mathcal{E}_i) &= \frac{\mathcal{E}_i\beta_i(1-\beta_i)}{(2+\mathcal{E}_i)^2} \\
E((\alpha_i(B_i,\mathcal{E}_i)-\beta_i)^2|\,\mathcal{E}_i) &= E(Var(\alpha_i(B_i,\mathcal{E}_i)|\,\beta_i,\mathcal{E}_i)+(E(\alpha_i(B_i,\mathcal{E}_i)|\,\beta_i,\mathcal{E}_i)-\beta_i)^2|\,\mathcal{E}_i) \\
&= E\left(\left.\frac{\mathcal{E}_i\beta_i(1-\beta_i)+(1-2\beta_i)^2}{(2+\mathcal{E}_i)^2}\right|\,\mathcal{E}_i\right) \\
&= \frac{1}{12+6\mathcal{E}_i}
\end{aligned}
$$

5.2.8 Topic modeling with a dictionary

In this section. we consider generating economics clusters (or "topics") in the dictionary, as another way of identifying what types of language was most responsible for the increase in similarity. Running our analysis at the dictionary level allows our results to be replicated in other settings, since the particular set and order of topics will not change.

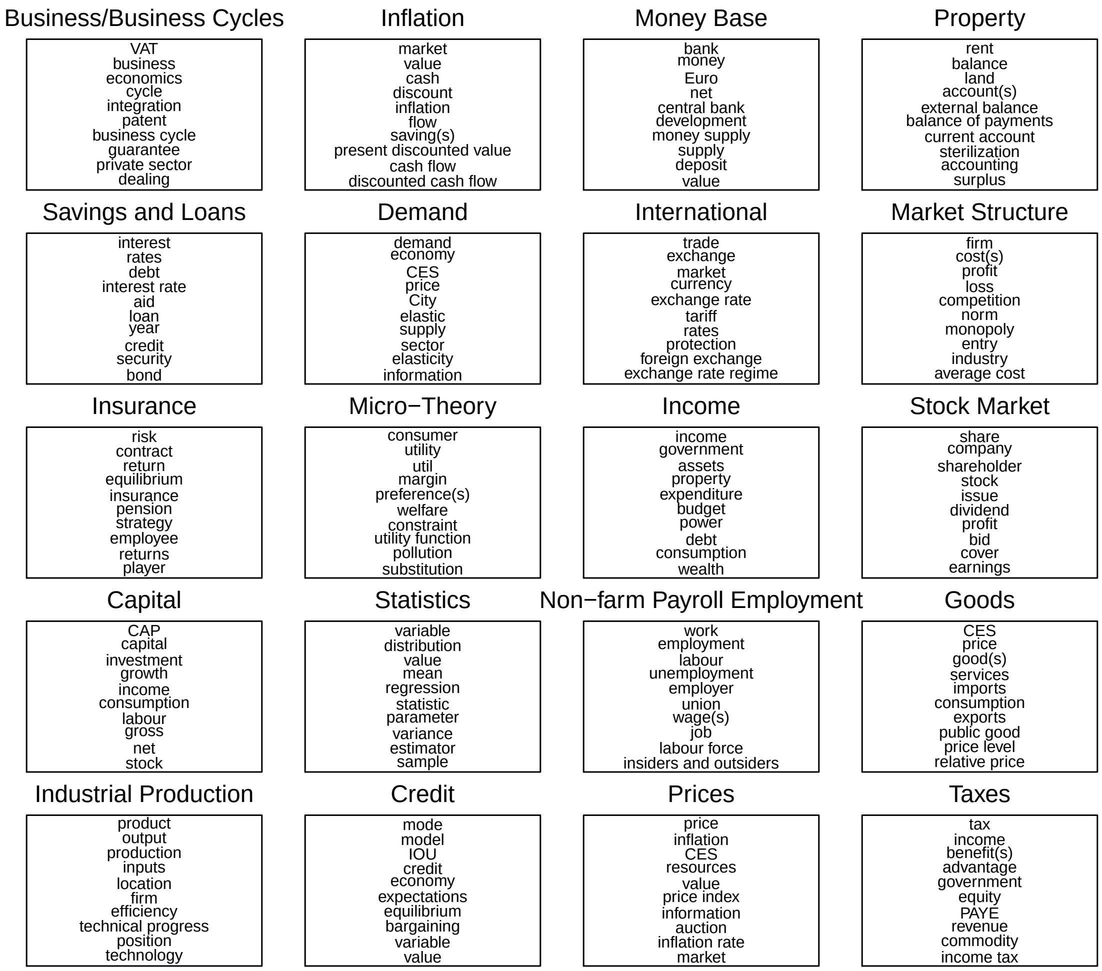
5.2.9 Topic Models on Out-of-Sample Dictionaries

To test the credit-seeking model's prediction of fewer topics, we use Latent Dirichlet Allocation (Blei et al. (2003)) on the ODE document-term matrix constructed from the dictionary's definitions. LDA has been used extensively in the text analysis literature to extract topics from a set of documents. The model has latent topics that are chosen with probabilities following a Dirichlet distribution, and multinomial choice probabilities for word choice conditional on a topic. The estimated model gives us, among other things, the multinomial probabilities for all words within each topic, as well as the posterior distribution of topics conditional on a certain word. More precisely, the setup from Blei et al. (2003) has the number of words N in a document be Poisson(ξ), the latent topic probabilities θ be Dirichlet(α), the topics z_n be Multinomial(θ), and the words w_n be Multinomial(β), conditional on z_n. Then, with M documents, they have that

$$p(\mathcal{C}|\alpha,\beta) = \prod_{d=1}^{M} \int p(\theta_d|\alpha) \left(\prod_{n=1}^{N_d} \sum_{z_{dn}} p(z_{dn}|\theta_d) p(w_{dn}|z_{dn},\beta) \right) d\theta_d$$

Computational difficulties arise in this setting, we follow the suggestions in Blei (2013). With 20 topics in the ODE, the top 10 words, as well as a suggested category title, are presented in Figure 19.

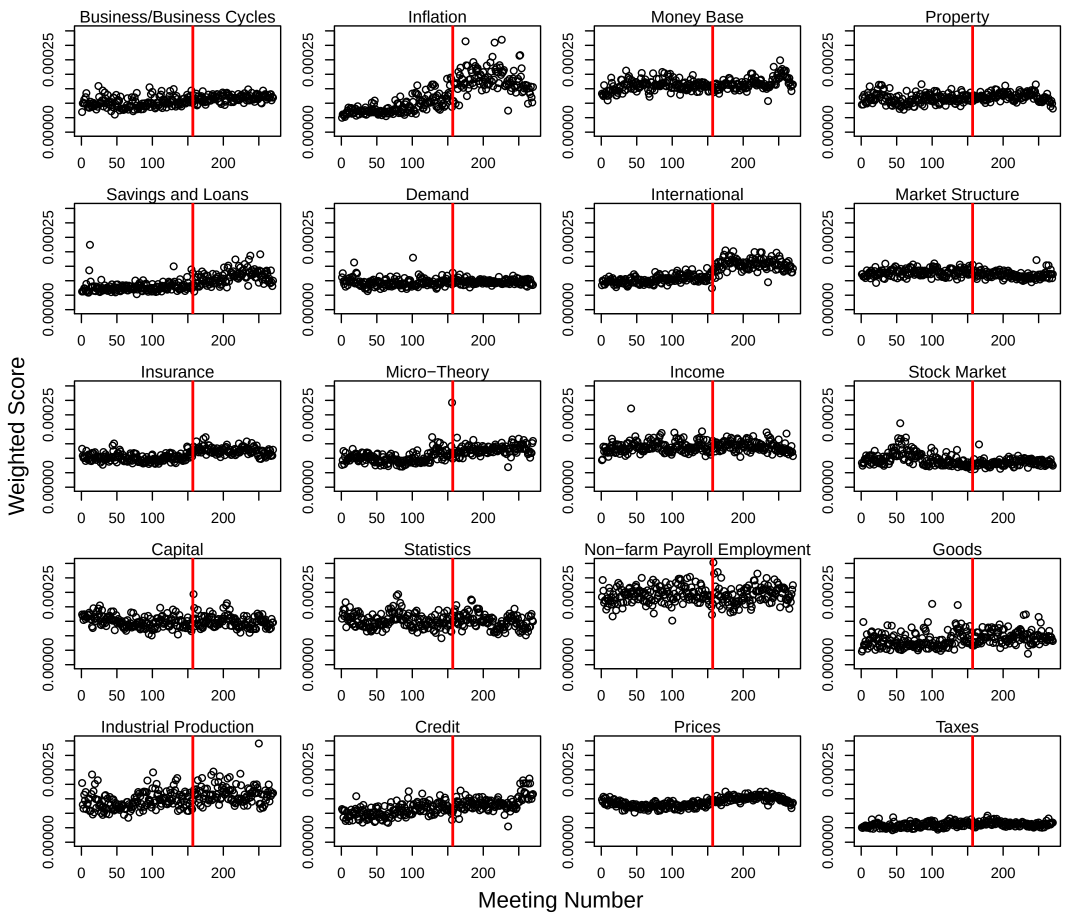
Figure 19: Topics in the ODE



5.2.10 Economic topics in the transcripts over time

In order to uncover the dimensions of economic language that were responsible for the overall increase in the proportion of economic words, we use the topics generated with LDA from the definitions in the Oxford Dictionary of Economics. In figure 20, we find that the use of words in the "Inflation" topic rose around the time of the enforcement of the Sunshine Act, as did the use of words in the "International" topic. This is consistent with a career concerns model in which FOMC members discuss more popular topics when the public will eventually find out what was discussed.

Figure 20: ODE topics over time



5.2.11 A bound on vector similarity when trimming vectors

In this section, we provide a proof that dropping rare words will have a limited effect on our results, while increasing computation efficiency substantially.

Proposition 1: Suppose $x = (x_1', x_2')'$ and $y = (y_1', y_2')'$ are vectors in $\mathbb{R}_+^n$, where $||x|| \geq ||x_1|| \geq c||x||$ and $||y|| \geq ||y_1|| \geq c||y||$. Then $|CS(x,y) - CS(x_1,y_1)| \leq 1 - c^2$.

Proof: First, note that $||x_2|| \leq \sqrt{1-c^2}||x||$ and $||y_2|| \leq \sqrt{1-c^2)}||y||$. $|CS(x,y) - CS(x_1,y_1)| = |\frac{<x,y>}{||x||\cdot||y||} - \frac{<x_1,y_1>}{||x_1||\cdot||y_1||}| = |\frac{<x_1,y_1>+<x_2,y_2>}{||x||\cdot||y||} - \frac{<x_1,y_1>}{||x_1||\cdot||y_1||}| = |\frac{||x_1||\cdot||y_1||\cos(\theta_1)+||x_2||\cdot||y_2||\cos(\theta_2)}{||x||\cdot||y||} - \frac{||x_1||\cdot||y_1||\cos(\theta_1)}{||x_1||\cdot||y_1||}|$
$= |\frac{(||x_1||\cdot||y_1||-||x||\cdot||y||)}{||x||\cdot||y||}\cos(\theta_1) + \frac{||x_2||\cdot||y_2||}{||x||\cdot||y||}\cos(\theta_2)| \leq \max\{\frac{(||x||\cdot||y||-||x_1||\cdot||y_1||)}{||x||\cdot||y||}\cos(\theta_1), \frac{||x_2||\cdot||y_2||}{||x||\cdot||y||}\cos(\theta_2)\}$
$\leq \max\{(1-c^2)\cos(\theta_1), (1-c^2)\cos(\theta_2)\} \leq 1 - c^2$.

Proposition 2: Suppose $x = (x_1', x_2')'$ and $y = (y_1', y_2')'$ are vectors in $\mathbb{R}_+^n$, where $||x_1|| = c||x||$ and $||y_1|| = c||y||$. Then $|CS(x,y) - CS(x_1,y_1)| = |(1-c^2)(CS(x_2,y_2) - CS(x_1,y_1))|$.

Proof: First, note that $||x_2|| = \sqrt{1-c^2}||x||$ and $||y_2|| = \sqrt{1-c^2)}||y||$. $|CS(x,y) - CS(x_1,y_1)| = |\frac{<x,y>}{||x||\cdot||y||} - \frac{<x_1,y_1>}{||x_1||\cdot||y_1||}| = |\frac{<x_1,y_1>+<x_2,y_2>}{||x||\cdot||y||} - \frac{<x_1,y_1>}{||x_1||\cdot||y_1||}| = |\frac{||x_1||\cdot||y_1||\cos(\theta_1)+||x_2||\cdot||y_2||\cos(\theta_2)}{||x||\cdot||y||} - \frac{||x_1||\cdot||y_1||\cos(\theta_1)}{||x_1||\cdot||y_1||}|$
$= |(c^2-1)\cos(\theta_1) + (1-c^2)\cos(\theta_2)| = |(1-c^2)(CS(x_2,y_2) - CS(x_1,y_1))|$.