The Impacts of Managerial Autonomy on Firm Outcomes

Namrata Kala*

Abstract

The allocation of decision rights within organizations can significantly impact firm outcomes. This paper uses a natural experiment to uncover the causal effects of granting managers of State Owned Enterprises (SOEs) more autonomy over several strategic decisions in India. I find that managers meaningfully exercise this autonomy and their decisions result in greater sales and profits. The paper makes two contributions. First, I empirically validate the theoretical prediction that delegation can be a superior alternative to communication within the firm. Second, I show that large gains in SOE performance (driven by managers' career concerns) are possible without ownership changes.

Keywords: delegation, managerial autonomy, State Owned Enterprises (SOEs), India, career concerns

*MIT Sloan School of Management; NBER; BREAD; J-PAL. Email: kala@mit.edu.

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1 Introduction

The information required for critical decisions is typically dispersed within an organization– therefore, firm outcomes can be significantly impacted by who has decision rights. What are the consequences of an uninformed principal (e.g. a firm owner, a board of directors) granting decision rights to a better informed agent (e.g. manager) who has different objectives? Giving managers autonomy can allow them to make decisions based on information that they privately possess. However, managerial choices driven by career concerns may not achieve the objectives of the firm. In this paper, I empirically untangle these effects by exploiting an earned-autonomy program that granted managers decision rights in Indian State Owned Enterprises (SOEs).

While there is a large theoretical literature that studies when and how managerial authority should be delegated, the set of accompanying empirical work is much smaller. This is because quasi-random or random variation in the allocation of decision rights is relatively rare, and also because changes in decision rights are often the result of broader reorganizations in firms, and so the effect of managerial autonomy can be conflated with other reforms (such as wage changes).

This paper overcomes these challenges by combining newly collected data from several sources on Indian SOEs with existing data to estimate the effects of an earned autonomy program on managerial decisions and firm outcomes over an 18-year period. Specifically, the program gave the board of directors (henceforth, referred to as managers) of profitable SOEs more autonomy over strategic decisions such as capital expansion, hiring and the formation of joint ventures. Each SOE in India is housed in a particular ministry. Before the program was introduced, these decisions were taken by a committee that included officials from the governing Ministry, and in some cases (depending on the magnitude of the decision) also higher levels of government. Importantly, autonomy affected neither incentives within the firm nor the set of available options for managers; it only meant that committee approval for certain decisions was no longer required.

Why should delegation affect firm outcomes, when the manager could just recommend to the committee the decisions that she would have made given autonomy? Since Aghion and Tirole (1997), the theoretical literature has recognized that firm outcomes depend on the "allocation of formal authority (the right to decide) and real authority (the effective control over decisions)." In my context, prior to autonomy, managers communicated their recommendations to the Ministry who would then decide whether or not to approve them. Strategic managers, with different objectives to the government, will distort the recommendations they make in order to ensure that they are not denied. Theoretically, these recommendations should differ from decision making under autonomy when the manager is free to make her preferred choice (subject to the imposed constraints). I empirically validate this central theoretical prediction which forms the point of departure for a good proportion of the delegation literature.

When does delegation work? My results additionally validate a second theoretical prediction which states that autonomy is the superior organizational structure for the principal when the divergence between her objectives and those of the manager are relatively small (Dessein, 2002). I show that the program was successful (from the government's perspective) and, with descriptive

evidence, that the managerial decisions reflect small incentive conflicts (since both managers and the government care about firm profitability).

My empirical strategy uses differences-in-differences and event studies to estimate the impact of the autonomy program. The program started in 1997 and gave SOEs that earned profits for three continuous years and had a positive net worth the right to apply for autonomous status. I construct a pre-program measure of eligibility to apply for this status: a binary variable that equals 1 if a SOE earned profits for three years continuously and had a positive net worth *before* 1997, the year of the program introduction, and 0 otherwise. I use this measure of program eligibility as a *proxy* for receiving autonomy, to sidestep the endogeneity concerns around the government picking firms for autonomy that may have the highest potential returns from this program. Using a differences-in-differences framework, I then test whether SOEs that were *eligible* pre-program performed differentially after 1996 relative to SOEs that were not. I also estimate their performance relative to comparable private firms (that earned profits for three years continuously and had a positive net worth before 1997).

I find that earned autonomy resulted in greater profitability, productivity, and sales. To uncover the direct mechanisms, I examine the program details. The program gave managers autonomy over three decisions: capital expansion, labor restructuring, and engaging in joint ventures and subsidiaries. Firms who receive autonomy seem to exercise them along most of these decisions: treated firms have greater capital expansion and are more likely to form strategic partnerships such as joint ventures or subsidiaries after the program. These effects persist for thirteen years after the program was implemented (the entire duration my data covers), indicating that they led to a long-term shift in the way these SOEs were managed. Autonomy also leads to more hiring (though the effects are more imprecisely estimated), indicating that treated firms were under-utilizing *both* capital and labor.

The success of this program from the government's perspective is perhaps most directly reflected in the fact that autonomy was never reversed. Specifically, the objectives of SOEs are not just to generate profits but also to serve as a vehicle to employ constituents (Azmat et al. (2012)), and the program achieved both these goals. That said, the data reveals a conflict between the preferred hiring pattern of politicians and managers. The average SOE increases hiring in the year before an election whereas treated SOEs spread their increased hiring smoothly over the electoral cycle. I interpret these results to suggest that incentive conflicts are present but that the program was successful because the divergence of preferences was not destructively large.

Finally, I show descriptive evidence that career concerns outside the firm can simultaneously explain both the exercised agency and the (small) incentive conflicts, specifically, their desire to join the board of a private sector firm. I show that the probability I match a SOE director to a private firm board of directors increases after the SOE gets autonomy. Demonstrating competence outside the firm involves generating profits even if they are at the expense of other government goals.

In addition to testing theories of the internal organization of the firm, my paper also informs

the policy debate on the privatization of SOEs. It is well-established that government ownership is correlated with lower returns to capital and profitability (Dollar and Wei, 2007; Megginson and Netter, 2001). It is hence both natural and policy-relevant to ask whether, instead of ownership changes, firms' outcomes (such as productivity) can be improved by improving the allocation of decision rights within the firms' hierarchy.

A potential concern with the baseline empirical strategy I employ is that earning profits for three continuous years may put a firm on a differential growth trajectory (in other words, autonomy itself has no effect). To show that this is not driving the results, I use a second DID framework that includes both the pre-program eligibility measure (the proxy for being treated) interacted with the post-1996 dummy variable and the treatment dummy variable interacted with the post-1996 dummy variable (as well as sector-by-year and firm fixed effects). This is meant to test whether pre-program eligibility has any additional effects on the outcomes of interest after controlling directly for treatment assignment. I find that controlling for the interaction of the treatment dummy variable interacted with the post-1996 dummy variable causes the effects on the interaction between pre-program eligibility and the post-1996 dummy variable to be statistically insignificant and much smaller. This indicates that pre-program eligibility is a plausible proxy for treatment, and does not have independent effects on firm outcomes conditional on the controls included in the regression.

I conduct several other robustness checks. I rule out that the effects are driven by strategic reporting of profits, outliers, or by spillovers on ineligible SOEs. I also show that the results are robust to considering only SOEs that reported positive profits at least once during the sample period, the inclusion of more stringent sector-by-year fixed effects, and estimating the effects relative to comparable private firms. I consider alternative specifications, including generalized differences in differences (using the receipt of autonomy as the treatment), generalized differences in differences including only pre-program eligible firms, and a matched generalized differences in differences specification. Finally, I show that government ownership does not change on average during the sample period, indicating that the results are not driven by privatizing firms that received autonomy.

Related Literature

I begin by briefly listing the features of the autonomy program I study as it will allow for a clean comparison with the existing literature. (i) Perhaps most importantly, it provides a natural experiment which allows me to estimate the causal effects of managerial autonomy. (ii) The program only altered decision rights and did not explicitly change any within-firm financial incentives of managers. (iii) The data allows me to infer the incentives of both the principals (in this case, the relevant politician) and the managers.

This paper builds on three literatures. The first is the theoretical literature (following Aghion and Tirole (1997)) that examines the differences in firm outcomes when managers makes recommendations as opposed to having actual decision-making authority. As mentioned above, perhaps

the closest paper in this literature to this one is Dessein (2002). To the best of my knowledge, this paper is the most direct empirical test of this theory because of the unique properties (ii)-(iii) of the data. The fact that the program did not alter financial incentives of managers is especially important because this feature, assumed frequently delegation theory literature, allows for clean predictions of the effect of organizational form on outcomes. Moreoever, as I discuss in the next section, the program I study takes the form of "interval delegation" which Alonso and Matouschek (2008) argue should be the form of the optimal delegation policy when the incentive conflict between the principal and agent is small (a fact consistent with my data). Since this paper is empirical, I do not provide a detailed description of this large theory literature and instead direct the reader to the excellent survey of Bolton and Dewatripont (2011).

The second literature empirically examines the impact of within-firm decentralization and managerial autonomy on firm decisions and outcomes.[1] An important finding of this literature is that decentralization within private firms increases their ability to withstand negative shocks (see, for instance, Aghion et al. (2017); Nagar (2002)). The bulk of this literature focuses on private firms. Perhaps the most substantive difference of my paper from the literature is the fact that I study a natural experiment that granted autonomy to the firms' managers.

In the context of SOEs, Xu (2000) studies a combination of reforms in China in the 1980s and Groves et al. (1994) find that autonomy and incentives together increased SOE productivity in China also in the 1980s, when SOEs produced the bulk of industrial output in China. These reforms were focused around increasing competition to SOEs and an important aspect of them (in addition to managerial discretion) was to allow firms to sell part of their output in the open market. In other words, decentralization was coupled with a significant increase in market access and changes to market structure, and the introduction of performance pay.

It is worth stressing that, while determining the causal effects of autonomy is an important organizational economics question in general, the public sector context is particularly policy relevant. This is because earned autonomy is a widespread policy that aims to promote efficiency and accountability in public organizations across a variety of settings including the health sector, school reforms, and public procurement. A related though distinct literature estimates the effects of autonomy across diverse settings such as schools and public procurement. Clark (2009) finds positive effects of school autonomy on educational achievement in the UK, though Hanushek et al. (2013) document that the returns to school autonomy are negative in developing countries, and positive in developed countries. In other related work, Rasul and Rogger (2018) find that bureaucratic autonomy is associated with project completion in Nigeria. More recently, Bandiera et al. (2020) conduct a randomized control trial in Pakistan and show that increasing procurement officers' autonomy reduces procurement prices with no quality reductions. The contexts, the agency problems and the associated effect of autonomy are very different in their setting. In Bandiera et al. (2020), the main agency problem is that of moral hazard because procurement officers may not choose the lowest price either due to a lack of effort or for financial gain. Their actions are

[1] I use the terms autonomy and firm decentralization interchangeably throughout the paper.

not per se restricted in any way (unlike my setting) but are they are overseen by monitors who in turn introduce additional distortions. By removing these, autonomy may be a net positive. By contrast, agents in my setting are upper-level management and the agency problem is entirely different: they possesses private information but have different preferences. Here the tradeoff is that autonomy leads to more informed decisions but these may not conform with the preferences of the governing ministry.

There has also been increasing policy interest in the role of autonomy for SOE performance- the OECD guidelines for corporate governance in SOEs emphasize that managers should be given operational autonomy (OECD, 2014), but there is little evidence of whether such autonomy affects SOE outcomes. In contrast, this paper focuses on the impacts of giving managers more autonomy in firms operated by the public sector. The specific context of this paper is important because SOEs in India constitute a significant fraction of the economy, and have co-existed with the private sector since about 1950, but are not as well-studied as SOEs in other countries (especially China). Despite India's substantial private-sector reforms in the 1980s and early 1990s, SOEs employ large numbers of workers - in 2009, the year my data ends, central government SOEs employed over 1.14 million people. Thus, policies that impact their profitability and expansion decisions have potentially large aggregate effects.[2]

2 India's Earned Autonomy Program

The earned autonomy policy was instituted in 1997, after privatization goals set in the early 1990s were largely unmet.[3] The goal of the program was to mitigate political interference to SOE functioning, which was widely cited as an impediment to effective management of these firms, while making them less dependent on the government for financing. The government, in an attempt to reduce SOEs' losses and budgetary outlays for capital expenditure, as well as increase firms' profitability, implemented the autonomy program that only better-performing SOEs could access. Policy discussion has suggested that the program was successful even though it did not include performance pay for managers or workers (IMF, 2005).

If an SOE fulfilled certain criteria, their board of directors (who we refer to as managers to avoid confusion) were granted autonomy over several significant strategic decisions. There were three levels of autonomy awarded in the period I study; each was conditional on increasingly stringent criteria. The first level was called "Mini-Ratna" Category-II. This, least level of autonomy, was given to firms that had earned positive profits for three consecutive years, and had positive net worth. The second level, "Mini-Ratna" Category-I, was awarded to firms that, in ad-

[2]The paper also relates to work on the effects of changes in ownership on SOE profitability and productivity (Barberis et al., 1996; Bartel and Harrison, 1999; Berkowitz et al., 2017; Estrin and Pelletier, 2018; Gupta, 2005; Hsieh and Song, 2015; Megginson and Netter, 2001). It is important to understand reforms that can improve performance without changing ownership because the latter fundamentally changes the objectives of the firm and SOEs exist precisely because their raison d'être is not profit maximization alone.

[3]Only about 3-4 SOEs were actually privatized, with a majority of the government's equity being sold to the private sector.

Table 1

	Mini-Ratna Cat-II	Mini-Ratna Cat-I
Positive net profits for each of the last 3 Years	✓	✓
Positive net worth	✓	✓
Do not require budgetary support from the government	✓	✓
Pre-Tax Profit of at least 30 crore in one year		✓

dition to the above Category-II criteria, also earned a profit of at least Rs.300 million in one of the three years. The highest level of autonomy (called "Navratna") was granted subject to the most stringent criteria. These changed over time, including eventually requiring a SOE to have been at a lower level of autonomy for a certain number of years.

SOEs that fulfilled the relevant criteria could apply to their governing Ministry for the corresponding status. Once granted, in principle, they had to include at least 3 independent directors on their board before exercising autonomy. In practice, several of these board seats remain vacant for long periods of time - for instance, in 2003, 6 years after the program had begun (when the board of directors data begins), only 11% of SOEs reporting data reported having an independent director. This indicates that the the results of autonomy program reflect changes in the behavior of existing managers, rather than the addition of new ones. If at any point they preferred to exchange this autonomous status for governmental support once again, they could do so but none of the SOEs in the data ever exercised this option.[4] Once status was granted, the board of directors could exercise autonomy over the following decisions.

1. Capital Expenditure: The board of directors could undertake capital expenditures (upgrading or purchasing new capital) up to a limit which was an increasing function of firm's net worth. These expenditures were financed out of retained earnings and commercial borrowing; the latter took the form of debt as SOEs could not sell equity.

2. Labour training and retirement schemes: The SOE board could introduce human resource management initiatives, training, and retirement schemes. Given that SOEs are large employers, and laying off workers in these firms can be politically sensitive, this may have given them more flexibility to restructure their labor force. There were no changes in the

[4]If a firm gave up its autonomy status, it would have to reapply after re-establishing eligibility.

process to hire workers, so changes in the composition of the labor force would reflect the firm's ability to train and manage workers, and incentivize some workers to retire early.

3. Ability to float joint ventures and subsidiaries: These were also subject to a value cap, about 5% of the net worth of the SOE.

Instead of requesting the government for permission on any of these decisions, the manager was only required to notify the government. For SOEs not granted this autonomy status, the process for approval to undertake any of these decisions was the same as before, as discussed in the introduction. This included requesting approval from the governing Ministry, and the decision was taken by a committee comprising Ministry officials. In cases of projects that required large amounts of government funds, the decision could additionally be subject to a parliamentary vote. The full details of the program, including benefits conferred on firms with different types of autonomy, are detailed in Appendix B.

3 The Theoretical Framework

In this section, I provide an overview of the theoretical framework which provides the context for the empirical exercise. Specifically, the theory highlights the distinct tensions between the firm and the manager in the two organizational forms before, and after, earning autonomy and provides conditions under which an autonomy program (like the one I study) is successful (from the firm's perspective). The full model is in Dessein (2002) but I include a brief summary here so that the paper is self contained.

Consider a firm that needs to make a strategic decision $d \in \mathbb{R}$. The firm has an objective function which depends on her decision but also on an unobserved state of the world θ which is drawn from a distribution supported on an interval $[-L, L]$ where $L \in \mathbb{R}_{++}$. The manager knows this state of the world but has a different objective function to the principal. The principal's objective function is given by $\pi(\theta) - \ell_f(|d - \theta|)$ and the manager's is given by $u(\theta + b) - \ell_m(|d - (\theta + b)|)$ where $b > 0$.

The firm's objective can interpreted as follows. The optimal decision for the firm is to match the state (so $d = \theta$) and this yields the highest possible payoff $\pi(\theta)$; this interpretation implicitly implies $\ell_f(0) = 0$. If the firm chooses any other decision $d \neq \theta$ she incurs a "loss" $\ell_f(|d - \theta|)$ which (symmetrically) increases the further the decision is from the optimal one and is assumed to be strictly convex ($\ell_f''(\cdot) > 0$). The manager's payoff can be analogously interpreted (with similar assumptions $\ell_m(0) = 0$ and $\ell_m''(\cdot) > 0$ imposed) with the sole difference that her optimal decision is $\theta + b$. This b captures the incentive conflict or "bias" in that managers always want to choose a higher action than the firm.

The manager is the informed party and knows the state θ but does not have decision making authority. The firm can either decide to ask the manager to reveal the state or delegate decision making to the manager. The former case becomes the classic cheap talk game of Crawford and

Sobel (1982) where, because of the bias, the manager has a strategic incentive to convince the firm that the state is higher than the true state. As Crawford and Sobel (1982) show, communication always takes the form of *coarse messages*—the manager truthfully tells the firm that the state lies within an interval of values—and therefore the firm will incur a loss because the true state will never be communicated and the firm optimal decision will hence (almost) never be chosen. The quality of communication (measured by the most informative equilibrium for the firm) improves as the bias shrinks and, in the limit when $b = 0$, communication can be perfect because the incentives of the manager and the firm are aligned.

Two important questions can be addressed by comparing the above outcomes from communication to those under delegation.

1. Will decision making differ under delegation?

   When the manager has decision rights (or, equivalently, autonomy), she will always pick the decision $d = \theta + b$. Thus the decision will change *continuously* with the state but will always be strictly higher than the optimal decision for the firm. This immediately demonstrates that, *when there is a bias, the set of implemented decisions differ based on who has authority.*

2. When does delegation dominate communication?

   Dessein (2002) shows that *when the bias b is sufficiently small, autonomy is the superior organizational form for the firm.* Conversely, if the bias is sufficiently large, communication dominates delegation.

My data allow me to test both the above theoretical predictions. To see this, I now map the model to the empirical exercise allowing it to serve as a lens to contextualize my findings. First observe that the organizational structure pre- and post-autonomy mimic those in the theoretical framework. Prior to autonomy, managers would communicate their decisions to the governing Ministry and, conversely, managers in firms granted autonomy had decision making authority delegated to them. Therefore moving decision rights from the Ministry to the managers would only result in changes to the firms' outcomes if the politicians' and managers' preferences were misaligned.

This is indeed the case. Politicians that control the firm are driven by electoral incentives and so, in addition to profits, want to use the SOE as a vehicle to generate employment (and thereby votes). The government's objective function is then to maximize some combination of profits and employment, and would be more sensitive to the electoral cycle than the manager would prefer. The firm decision d can be thought of as the (single-dimensional sufficient statistic of the) choice of the combination of inputs like capital and labor.[5]

Managers are in charge of running the firm efficiently. In addition, if they have career ambitions in the private sector, this will manifest in the form of a preference that puts a greater weight on profits relative to employment. In other words, they prefer a different combination of inputs

[5]The fact that d can take negative values is immaterial and d can be restricted to $\mathbb{R}_+$. I chose not to do so in the description to maintain the elegant symmetry of the model.

than the politician which is captured by their bias b. The optimal choice for both parties depend on the underlying market conditions (the state of the world θ) and, since the politician is not involved in operating the firm, it is reasonable to assume that the manager is the informed party.

As I will show in the sections that follow, I provide causal evidence that the autonomy program changed decision making by managers which resulted in greater profits and, importantly, did not do so at the cost of employment. In this sense, the program was a success for the government. As mentioned above, the theory predicts these outcomes when there is an incentive conflict but it is not large. I provide supplementary evidence that shows precisely this. Specifically, I will show that managers' decisions are driven by their desire to earn seats on private sector boards and so they prefer to increase profits more than maximize employment. While employment increases, the timing of hiring is different for treated SOEs. While the average SOE increases hiring in the year before an election, managers with autonomy spread their increased hiring smoothly over the electoral cycle. Thus, the natural experiment combined with data on localized hiring allow for a clean comparison between these two ubiquitously observed organizational forms.

4 Data

The paper combines data from several volumes of the Public Enterprise Survey Reports with existing data sources.[6] These reports are published annually by the Department of Public Enterprises in India, which is responsible for reporting information on SOE financial performance, expenditures, and labor composition. I was able to access these volumes from 1994 to 2009. These reports also contain a subset of the data from the previous two years; as a result, for certain variables, such as those available in financial statements, the data covers the years 1992-2009. The universe of all SOEs in which the Central Government of India has a majority stake are included in the data: in an average year, the data covers approximately 220 firms.

4.1 Firm Returns, Inputs, Borrowing and Profit Allocation

The annual financial statements of the SOEs cover the period from 1992 to 2009. These include information available in the profit and loss accounts and balance sheets for each firm. To ensure that the results I estimate are not driven by entry or exit, I restrict the sample to SOEs that report data for at least 5 years before (starting in 1992) and at least 5 years after (until 2002) the program (a sample of about 190 firms per year).[7] I have three measures of firm returns— profits, value added, and sales— as well as two measures of productivity— sales per employee and value added per employee.[8]

[6]The data appendix presents all variables used in the analysis, the level of measurement (e.g. whether the data are available at the firm-year level, firm-level, etc.), temporal coverage, and source.

[7]In Appendix table A5, I present results with the entire sample, including firms that began reporting after 1992 or stopped reporting before 2002, to show that the estimates are consistent with the main results.

[8]I calculate value-added by subtracting expenditures on raw material, power and fuel, from sales.

The statements also include information on capital assets (the sum of fixed assets, capital works in progress, and other long-term investments), the number of employees, and the wage bill. These variables, along with whether a SOE participates in a joint venture or subsidiary, are potentially direct mechanisms via which autonomy might impact firm returns and productivity (in Section 4.4, I describe how the data for participation in a joint venture or subsidiary is constructed). I digitize information on total loans, as well as interest payments. For the years 1994-2009, I have separate information on the level of borrowing from both government and non-government sources.

To test whether autonomy changes the allocation of the surplus generated by a SOE, I use information on the distribution of profits into dividends and retained earnings. This captures the amount that the government benefits from autonomy via receiving greater dividends, and how much of their profits SOEs are able to retain.

4.2 Autonomy Status, Labor Composition, and Spatial Presence

The reports include information on the autonomy status of each SOE since the beginning of the program in 1997. This includes whether a SOE has autonomy, and if so, which category (Mini-Ratna category I, Mini-Ratna Category II or Navratna). In addition, I digitize data available from 1994-2007 on the labor composition of the SOEs. For all years except 1999, this contains the number of managers, supervisors, workers, and casual workers. In 1999, only information on the number of managers and non-managers is available. To be consistent, I combine data from the remaining years into these two categories. These data allow me to test whether SOEs respond to autonomy by changing the labor composition of their workforce. Additionally, from 1999, data on each SOE's state-level employment and capital presence is available. This allows me to test whether hiring follows electoral cycles and if autonomy impacts these decisions.

4.3 Sectoral Codes, Board of Director Names, and Private Sector Firm Data

I combine the digitized data with the Prowess database, collected by the Centre for Monitoring the Indian Economy (CMIE). The database includes financial statements for about 50,000 firms (including SOEs and private firms), as well as information on the board of directors of about 41,500 firms. I match SOEs to the Prowess database to get information on their 5-digit National Industrial Classification (NIC) product codes.[9]

Additionally, I use information on the names of the board of directors in SOEs and private firms to test whether SOE managers from firms granted status are differentially more likely to get private sector board seats after autonomy is granted. If so, this would uncover one source of incentive conflicts between managers and the Ministry: managers do not only care about retaining their public sector position but also have private sector ambitions. As described in the theoretical framework (Section 3), an incentive conflict is necessary for managers to act on autonomy.

[9]Of about 230 SOEs operating before 1997, I was unable to find sector codes for only about 10 SOEs in the database. While the Prowess database includes reliable cross-sectional information on these SOEs in the 1990s, consistent annual financial information is not available across years, necessitating the separate digitization of annual financial statements.

Data on SOE board members is available only for a fraction of firms: 100 SOEs consistently report the names of the Board of Directors after 2002. Coverage in the Prowess database on the board of directors improves markedly after 2003, which is why I focus on 2003-2010 for this part of the analysis. I construct a director-year panel, that includes a binary variable that takes the value 1 if there is a match for a SOE director name amongst the director names of private firms in a given year, and 0 otherwise. I can also test whether the propensity of the director to be present on private sector boards varies by whether the SOE director was on the SOE board before the firm received autonomy (which is a measure unaffected by private managers being more likely to join SOE boards after the program).

The Prowess data also includes data on profits, sales, and value added for private firms at an annual level, which allow me to estimate the effects of the autonomy program relative to private firms.[10] To ensure that I am comparing firms that operate in similar conditions, I only include private firms that are in the same 5-digit NIC codes as SOEs, that were in operation before 1997 and report data for at least five years after 1997 (similar to the SOE main sample).

4.4 Participation in a Subsidiary or Joint Venture Project

To construct a measure of whether a SOE had a subsidiary or participated in a joint venture, I combine the CMIE database with the reports from the Department of Public Enterprises. The CMIE database reports whether a SOE had a subsidiary. The annual reports from the Department of Public Enterprises include a paragraph summarizing each SOE's activities over the course of the year. I searched all years of the report for mentions of new joint venture projects, and construct a binary variable that equals 1 if a SOE reported a new joint venture, and 0 otherwise. This variable is likely measured with some error, since a SOE may not choose to report a joint venture for some reason, and the data does not contain a good measure of when a joint venture ends.[11] With this information, I construct another binary variable that takes the value 1 if a SOE either reported a subsidiary (from the CMIE database) or a joint venture in either of the two data sources, and is 0 otherwise.

4.5 State Assembly Election Timing

To test for electoral cycles in SOE hiring, I collected data on the timing of the assembly elections (to elect representatives to the state legislature) in each state between 1999-2009. This data is available from the website of the Election Commission of India, and lists the state and year for each state's assembly election.

[10] The database does not report employment for most of the sample, so I cannot estimate the effects on productivity (sales per employee and value added per employee).

[11] When an SOE only reports the number of joint ventures, this variable is 1 in a given year if the number of joint ventures in that year exceeds the number of joint ventures in previous years.

4.6 Summary Statistics

The main sample comprises of data from 193 firms. 95 firms were eligible before 1997 to apply for autonomy, of which 65 received it at some point between 1997 and 2009. In total, 73 unique firms received autonomy during the sample period.[12] Table 2 presents the summary statistics for SOE inputs and outcomes, as well as outcomes for the private firms used in the analysis. These summary statistics are over the entire sample period. In addition, for all outcome variables, the regression tables report the mean for each outcome variable.

5 Empirical Strategy

5.1 The Main Specification: Direct Effects of Autonomy on SOEs

Recall from Section 3, the first theoretical prediction we aim to test is whether or not autonomy resulted in managers altering decision making in turn leading to different outcomes. To do so, I use a difference-in-differences (DID) framework. I evaluate all firms post-1996, the year before the policy was first implemented. The DID framework allows me to test for parallel trends in the outcomes of interest. However, it is possible that (time-varying) factors that are observed by SOE managers or the government but not by the econometrician are correlated with the decision to apply for or grant autonomy. Therefore, I use the profitability and net worth criteria to generate a pre-program eligibility measure. I construct a variable that takes the value 1 if a firm earned profits for 3 consecutive years and had positive net worth before 1997, the year of the program implementation, and is zero otherwise.[13]

Pre-program eligibility is highly correlated with being awarded autonomy. Of the 95 firms in the data that are eligible before 1997, 65 received autonomy during my sample period (i.e. over 68%), 47 within the first three years of the program. That is, about 72% of pre-program eligible firms that were awarded autonomy in my sample, received it within three years of the program's introduction. Within six years of the program's introduction, 52 of these firms had received autonomy. Being eligible pre-program explains about 40% of the variation in autonomy status: a regression of the treatment dummy variable on the pre-program eligibility dummy variable has a R-squared of 0.4.

The main specification is chosen to confront two issues. First, if a firm decides to change their behavior in order to receive autonomy, they would be labeled as control in this specification. Second, the specification avoids the endogeneity of the timing of receiving autonomy; for instance, that a firm might apply for autonomy as demand for their product is increasing. The fact that the

[12] 19 firms received Navratna status between 1997-2009, 50 firms received Mini-ratna category-I status, and 17 firms Mini-ratna category-II status. These numbers include 13 firms that graduated to a higher level of autonomy during the sample period.

[13] In Tables 14 and A8, and Figures A1 through A5, I present generalized difference in differences results, which evaluate the effects of the program after an eligible firm actually receives autonomy, and show that they are consistent with the main results.

generalized DID results in Tables 14 and A8 are consistent with the results in this specification indicate that these issues are not driving the results, but I nonetheless choose the main specification to be robust to these potential concerns.

Because I use the eligibility measure as a proxy for the treatment, I estimate

$$y_{ijt} = \alpha + \alpha_i + \gamma_t\phi_j + \beta\left(\mathbb{1}(\text{post 1996})_t * \mathbb{1}(\text{eligible})_{ij}\right) + \mu\left(\mathbb{1}(\text{pre 1996})_t * \mathbb{1}(\text{eligible})_{ij}\right) + \epsilon_{ijt}, \tag{1}$$

where y_{ijt} = outcome for firm i in sector j in year t (such as sales or profits), α_i = firm fixed effect (FE), $\gamma_t\phi_j$=2-digit sector by year FE, and $\mathbb{1}(\text{eligible})_{ij}$= 1 if firm i was eligible pre-program, and 0 otherwise. $\mathbb{1}(\text{pre 1996})_t$ is an indicator variable that is 1 for years 1992-1995 and 0 otherwise, and $\mathbb{1}(\text{post 1996})_t$ is an indicator variable that is 1 for years 1997 and later, and 0 otherwise. β is the parameter of interest, and the hypothesis $\mu = 0$ tests for pre-trends in the outcomes of interest. I omit interactions of $\mathbb{1}(\text{eligible})_{ij}$ with the year 1996 to estimate effects relative to the year before the program was implemented. Standard errors are clustered at the firm-level.

I additionally present event study estimates with year by year interactions of pre-program eligibility, showing impacts for 5 years before (when the data begins) and 10 years after 1997 (these analogously omit the interaction of pre-program eligibility with the dummy variable that is 1 for the year 1996, the year before program introduction, and 0 otherwise). I estimate this specification for both the main outcomes of interest, such as profitability, as well as direct mechanisms i.e. the strategic decisions allowed under the autonomy program, such as capital investment.

5.2 Direct Effects of Autonomy Relative to Comparable Private Firms

As mentioned in the introduction, this paper aims to address the effects of delegation not just from a general organizational economics perspective but also specifically within the context of SOEs.

Specifically, I contrast the profits and sales of treated firms to comparable private sector firms by estimating the triple difference regression

$$\begin{aligned} y_{ijt} =& \alpha + \alpha_i + \gamma_t\phi_j + \psi\left(\mathbb{1}(\text{post-1996})_t * \mathbb{1}(\text{eligible})_{ij} * \mathbb{1}(\text{SOE})_{ij}\right) + \nu\left(\mathbb{1}(\text{pre-1996})_t * \mathbb{1}(\text{eligible})_{ij} * \mathbb{1}(\text{SOE})_{ij}\right) \\ &+ \theta\left(\mathbb{1}(\text{post-1996})_t * \mathbb{1}(\text{eligible})_{ij}\right) + \kappa\left(\mathbb{1}(\text{pre-1996})_t * \mathbb{1}(\text{eligible})_{ij}\right) + \zeta\left(\mathbb{1}(\text{post-1996})_t * \mathbb{1}(\text{SOE})_{ij}\right) \\ &+ \tau\left(\mathbb{1}(\text{pre-1996})_t * \mathbb{1}(\text{SOE})_{ij}\right) + \epsilon_{ijt}, \end{aligned} \tag{2}$$

where α_i = a firm FE, $\gamma_t\phi_j$=2-digit sector by year FE, and $\mathbb{1}(\text{eligible})_{jt}$= a firm that earned positive profits for 3 years and has a positive net worth pre-1997. As in Section 5.1, 1996 is the omitted year in all interaction terms. $\mathbb{1}(\text{pre 1996})_t$ is an indicator variable that is 1 for years 1992-1995 and 0 otherwise, and $\mathbb{1}(\text{post 1996})_t$ is an indicator variable that is 1 for years 1997 and later, and 0 otherwise. $\mathbb{1}(\text{SOE})_{ij}$ is an indicator variable that takes the value 1 if firm i in sector j is an SOE, and is 0 otherwise. ψ compares pre-program eligible SOEs with comparable private firms after

1996, and $\nu = 0$ tests for pre-trends. Standard errors are clustered at the firm-level.

5.3 Career Concerns: SOE Board of Directors

To test if career concerns in the private sector are a motivation, I examine whether managers of treated firms are more likely to appear on the boards of private sector companies post-autonomy. I use information on the names of SOE managers between 2003 and 2010. (As mentioned in Section 4.3, data coverage is very sparse before 2003.) In 2003, about 55% of SOEs in the main sample report manager names, and from 2004 onward, that increases to about 66-74%.

I create a cross-sectional manager-level dataset that includes their name, whether they manage a treated firm, and if so, the year in which their SOE received autonomy. Using this information, I create a manager-year level binary variable that takes the value 1 if their name appears on the board of directors of a private firm in a particular year. Since this data only begins after the program was announced, I estimate two separate specifications.

The first specification, which includes all the data, is

$$\mathbb{1}(\text{SOE manager matched to private board})_{it} = \alpha + \alpha_i + \beta \left(\mathbb{1}(\text{SOE has autonomy})_{it}\right) + \delta_t + \epsilon_{it}, \tag{3}$$

where the variable $\mathbb{1}$(SOE has autonomy) is 1 if the manager's firm has autonomy and 0 otherwise (for firms that received autonomy before 2003, it is always 1).[14] α_i is the manager name FE, and δ_t is a year FE. Standard errors are clustered at the manager name level.

While the above specification uses all the data, its limitation is that I cannot test for pre-trends. I therefore additionally estimate the following specification, which drops firms that received autonomy prior to 2005:

$$\begin{aligned}\mathbb{1}(\text{SOE manager matched to private firm board})_{it} = \alpha + \alpha_i + \beta \left(\mathbb{1}(\text{post autonomy})_t * \mathbb{1}(\text{treatment})_i\right) \\ + \mu \left(\mathbb{1}(\text{pre autonomy}) * \mathbb{1}(\text{treatment})_i\right) + \beta_2 \left(\mathbb{1}(\text{post autonomy})_t\right) + \mu_2 \left(\mathbb{1}(\text{pre autonomy})_t\right) + \delta_t + \epsilon_{it}.\end{aligned} \tag{4}$$

Here, $\mathbb{1}$(treatment)=1 if a firm was ever granted autonomy status in 2005 or later and 0 otherwise, α_i is the manager name FE, and δ_t is a year FE. The hypothesis $\beta > 0$ tests whether SOE managers are more likely to be matched to a private sector board after autonomy, and $\mu = 0$ tests for pre-trends.

I estimate both specifications on two different samples: all SOE managers between 2003 and 2010, and only the incumbent managers who were present on SOE boards before 2005.

[14] When a director's name shows up on both treated and untreated SOEs, I consider them to be a treated director.

5.4 Electoral Cycles in Hiring

If the preferences of the managers and politicians coincided, both the amount and the *pattern* of hiring between the average and treated SOE should not differ. One way in which this would manifest in the data is in the form of increased hiring before elections.

I use two specifications analogous to those in Section 5.3 to test whether autonomy changes these hiring patterns. The first specification (as in Section 5.3) which uses all the data is

$$\begin{aligned}&\text{(Proportion of employment)}_{ijkt} = \alpha + \alpha_i + \beta_1 \mathbb{1}\left(\text{Year before a state assembly election}\right)_{kt} \\ &+ \beta_2 \mathbb{1}\text{(SOE has autonomy)}_{ij} + \beta_3 \left(\mathbb{1}\left(\text{Year before a state assembly election}\right)_{kt} * \mathbb{1}\text{(SOE has autonomy)}_{ij}\right) \\ &\gamma_t \phi_j + \psi_k + \epsilon_{ijkt}\end{aligned} \tag{5}$$

where $\text{(Proportion of employment)}_{ijkt}$ is the proportion of employment of firm i in state k at time t. $\mathbb{1}\left(\text{Year before a state assembly election}\right)_{kt}$ is a dummy variable that takes the value 1 if the year is one year prior to state k's assembly election, and 0 otherwise. $\mathbb{1}\text{(SOE has autonomy)}_{ij}$ is a dummy variable that is 1 if a SOE i in sector j already has autonomy, and 0 otherwise. $\gamma_t \phi_j$ denote sector by year fixed effects, ψ_k are state fixed effects, and α_i are firm fixed effects. β_3 tests whether SOEs with autonomy have differential hiring patterns than SOEs without autonomy in the year before an election. Since these data are at the firm-state-year level, I can additionally include firm by year FEs as an additional robustness check. I restrict the sample to firm-state combinations where a firm ever reported positive employment in a state.

I also estimate the following specification, which drops firms that received autonomy prior to 2005, and allows me to test for pre-trends:

$$\begin{aligned}&\mathbb{1}\text{(Proportion of employment)}_{ijkt} = \alpha + \alpha_i + \beta_1 \mathbb{1}\left(\text{Year before a state assembly election}\right)_{kt} \\ &+ \beta_2 \left(\mathbb{1}\text{(Treatment)}_{ij} * \mathbb{1}\left(\text{Year before a state assembly election}\right)_{kt} * \mathbb{1}\left(\text{Pre-autonomy}\right)_t\right) \\ &+ \beta_3 \left(\mathbb{1}\text{(Treatment)}_{ij} * \mathbb{1}\left(\text{Year before a state assembly election}\right)_{kt} * \mathbb{1}\left(\text{Post-autonomy}\right)_t\right) \\ &+ \beta_4 \left(\mathbb{1}\left(\text{Year before a state assembly election}\right)_{kt} * \mathbb{1}\left(\text{Pre-autonomy}\right)_t\right) \\ &+ \beta_5 \left(\mathbb{1}\left(\text{Year before a state assembly election}\right)_{kt} * \mathbb{1}\left(\text{Post-autonomy}\right)_t\right) \\ &+ \beta_6 \left(\mathbb{1}\text{(Treatment)}_{ij} * \mathbb{1}\left(\text{Pre-autonomy}\right)_t\right) + \beta_7 \left(\mathbb{1}\text{(Treatment)}_{ij} * \mathbb{1}\left(\text{Post-autonomy}\right)_t\right) \\ &+ \beta_8 \mathbb{1}\left(\text{Pre-autonomy}\right)_t + \beta_9 \left(\text{Post-autonomy}_t\right)_t + \gamma_t \phi_j + \psi_k + \epsilon_{ijkt}.\end{aligned} \tag{6}$$

β_3 captures whether autonomy changes hiring during the electoral cycle, and $\beta_2 = 0$ tests for pre-trends. In addition to all relevant double interaction terms, the equation includes firm FEs α_i, sector by year FEs $\gamma_t \phi_j$, and state FEs ψ_k.

6 Main Results

I begin by showing that the data confirm the first theoretical prediction: autonomy materially changed decision making by managers which in turn led to different firm outcomes. This shows that politicians and managers have diverging objectives and, therefore, that organizational structure has important implications for SOE functioning. Importantly, outcomes do not run afoul of broad government objectives: profits, productivity increase but are additionally accompanied by increased hiring. Moreover, higher profits are not driven by price increases alone (without an accompanying increase in quality).

Importantly, treated firms perform well even when compared with their private sector counterparts. This shows that SOE performance can be improved by internal organizational reform and that privatization is not the only way to improve efficiency.

I then unpack the source of the divergence in preferences between managers and politicians. Specifically, I show that private sector career concerns motivate managers so maintaining public sector employment is not their sole objective. This manifests in one key way: hiring patterns are less affected by electoral cycles. The latter benefits politicians but has no ostensible benefit for productivity which is what the private sector firms care about. Taken together, I interpret these results to suggest that incentive conflicts, while present, are not large. The second theoretical prediction formalizes this as an explanation for the success of the autonomy program.

The remainder of this section details these findings. I first discuss firm outcomes then the production decisions and finally the diverging preferences of managers and politicians.

6.1 Direct Impacts on Firm Outcomes: Profitability, Sales, and Productivity

Autonomy has large positive effects on profitability and sales, as well as productivity. Tables 3 presents results for all three measures of firm returns: annual revenue, value added, and profits. Column 1 of Table 3 shows that firms that were eligible to apply for the program before 1997 have greater sales by about Rs. 8.82 billion after the program, a large effect in magnitude relative to mean sales of about Rs. 28 billion. The difference between eligible and ineligible firms before 1996 is less than 5% of that magnitude, negative and not statistically significant, indicating the absence of pre-trends. Furthermore, firms that were eligible for autonomy before the program was announced have a higher value added of about Rs. 4.2 billion after the program is implemented, which is approximately a 41% increase over the mean value added. They also have higher profits by about Rs. 1.05 billion, a 58% increase relative to mean profits. Pre-trends are not significantly different from zero for any of the outcome variables.

Table 4 presents results for both measures of labor productivity: sales per employee and value added per employee. For both measures, I find large post-program implementation increases for pre-program eligible firms: an increase in Rs. 4.5 million for sales per worker and an increase in Rs. 1.086 million for value added per worker. I do not find any evidence of pre-trends in either of the measures.

These effects are substantial but cumulative over time (as shown in the event studies, discussed later in the paper). How does the magnitude of these effects compare with recent studies on firm interventions to increase productivity, such as the provision of consulting? In terms of magnitudes, these results are in line with results from interventions such as Bruhn et al. (2018), which finds that consulting increases productivity by 0.2 standard deviations. The results on value added per worker presented in Table 4 are similar (about 0.2 standard deviations) but accrue slowly for up to after 10 years of the program.

What drives the change in revenues: increased production, greater quantity, higher prices or a combination of these? It is important to address this question because higher revenues from increased pricing alone would reduce consumer welfare and could also have proved to a political liability. While I do not have separate data on output prices and quantities, I run several tests to capture the differential impact of pricing and production. First, note that the expansion of capital and labor use (documented in Table 5 and further discussed below in Section 6.2) strongly suggest that firms changed their production. More definitively, I show that total costs of production increase; clearly, if all firms did was raise prices with no change in output or quality, these costs should be unchanged. Specifically, Column 1 of Table 12 shows that costs of production (computed as the sum of purchase of finished goods, raw materials, wage bill, power and fuel expenses, depreciation, interest payments, and miscellaneous expenses), increase substantially after the program. Though the effects are slightly noisily estimated (the double interaction of pre-program eligibility with the post-1996 dummy has a p-value of 0.13), the magnitude of the effect is substantial: the average increase is about Rs.6.2 billion, a 25% relative to mean costs. This, along with increased capital and labor use shown in Table 5, indicates that the changes in revenues are not driven by increases in output prices without any corresponding increase in output quantity or quality (where quality is defined as a higher marginal cost).

It is also possible that firms achieved higher sales and profits by increasing both prices and quantity, while either keeping quality constant or lowering quality (in order for this to be the case, the demand curve facing these firms must be inelastic). Under the assumption of a constant returns to scale (CRS) production function, I can test whether the data are consistent with higher prices with no change in quality. The ratio of gross profits (profits before depreciation, interest, and taxes) to revenues is given by $\frac{pQ - cQ}{pQ} = \frac{(p-c)}{p}$, where p is the output price, Q is the output quantity, and c is the average cost per unit (and in the case of a CRS production function, also the marginal cost). If I do not find any changes in the profit to revenues ratio as a result of the program, I can rule out that p increased but c either did not change or decreased (quality stayed constant or deteriorated). Column 2 of Table 12 shows that this ratio did not change as a result of the program: the coefficient of the interaction between eligibility and the pre-program dummy variables is identical to the interaction term between eligibility and the post-program dummy variable (they are also quite imprecisely estimated). These results indicate that the large and statistically significant increase in profitability and productivity shown in Tables 3 and 4 are unlikely to be solely driven by higher prices without an accompanying increase in quality.

6.1.1 Direct Impacts on Firm Outcomes Relative to the Private Sector

As discussed above, the autonomy program increases sales, value added, and profitability relative to other SOEs. How does autonomy affect SOE performance to the private sector? To answer this question, I estimate Equation 2, and report the results in Table 8. I consider both the full sample (results presented in rows 1-3) and a sample that drops small private firms to ensure a more comparable sample to the SOEs; to construct this, I only consider private firms with average sales before 1997 equal to or greater than average sales by SOEs in the same 5-digit industry code. Results using this sample are presented in columns 4 through 6 of Table 8. Before 1996, the public-private difference for pre-program eligible firms is are not statistically different, but after 1996, this difference is positive and statistically significant. Note that this result is not indicating that SOEs outperform the private sector, only that the SOE-private firm difference is positive for eligible firms (relative to ineligible firms) after 1996.

6.2 Mechanisms

6.2.1 Inputs and Strategic Ventures

Managers use most levers of the autonomy granted to them and, importantly, they increase employment. As we have argued above, the goal of employment generation is one important way in which SOEs differ in their objectives from private sector firms.

Table 5 presents the results on firms' production decisions: this includes capital assets, employment levels and wages, as well as engagement in joint ventures and subsidiaries. Capital assets are the sum of the book value of fixed assets, capital works in progress, and investments. I find a substantial increase in capital assets for pre-program eligible firms post-1996: the effect size is about Rs. 6.4 billion higher borrowing, relative to mean capital assets of Rs. 273 billion. The wage bill also increases significantly, by about Rs. 0.9 billion (about 5% relative to the mean). The effects on employment are very similar in terms of magnitude relative to the mean, but are noisier (the p-value on the interaction between the pre-program eligibility dummy-variable and the post-treatment dummy variable is about 0.16). The fourth column indicates the probability that a firm reports a subsidiary or participates in a joint venture also increases with autonomy by nearly 7 percentage points, an effect that is about 35% relative to mean participation in such ventures.

6.2.2 Borrowing and Surplus Division with the Government

How was the above increase in capital and labor expenditure funded? Did manager's use their increased discretion to redirect firm profits away from government dividends towards expansions? I show that the percentage of profits returned to the government by treated SOEs did not change and that they relied less on the government for funding. In other words, from a purely financial perspective, the program was an unambiguous success for the government.

SOEs, like private firms, retain part of their earnings and distribute the rest as dividends which

are largely received by the government, since it is the majority shareholder. Table 6 reports the effect of the program on the division of profits into retained earnings versus dividends. Both components increase at approximately the same rate: the increase in dividends is about 54% relative to the mean, and the increase in retained earnings is about 55% relative to the mean. Treated SOEs (due to their autonomy status) can use profits (instead of borrowing from the private sector or the government) to finance capital expansion. The results indicate that there is no substantial change in the percentage of profit sharing between the firms and the government and so both parties benefit from the program.

Taken together, the results presented in Panel B of Table 6 (available only from 1994-2009) indicate that increased production expenses were instead funded by higher borrowing and that the source of these funds shifted away from the government. This latter result is consistent with the conditions for capital expansion under the autonomy program (that it be financed out of non-governmental sources). Specifically, pre-program eligible firms increase borrowing by about Rs. 2.94 billion, about 18% relative to the mean, though the effect is not statistically significant. Government loans (defined as loans extended by the Central government), fall substantially, by Rs. 3.3 billion (mean government borrowing is Rs. 24.95 billion), an effect that is statistically significant at the 5% level. Non-government loans increase by Rs. 6.3 billion, though the effect is not statistically significant.

The third column of Panel A indicates that the change in interest payments as a result of the autonomy program is very similar in magnitude to the increase in total borrowing as a percent relative to the mean (about 18% relative to the mean). This indicates that firms did not use autonomy to increase their high-interest borrowing.[15].

6.2.3 Incentives for SOE Managers

What is the source of the incentive conflict that results in different decisions post autonomy? I show that a consistent explanation is that SOE managers are partly driven by private sector career concerns. Specifically, autonomy allowed managers to take decisions that increased profits, thereby signaling their quality to the market which, in turn, resulted in a greater likelihood of winning seats on the boards of private sector firms.

To show this, I use data on SOE managers from the Prowess database, and estimate Equations 3 and 4. Results are presented in Table 7. Columns 1 and 2 present the estimates from Equation 3: the former includes all SOE directors, the latter only includes incumbent directors (those individuals who are on SOE boards before 2005). I show that the probability a SOE director is matched to a private firm board is higher by about 3-4 percentage points for firms with autonomy. Columns 3 through 6 present results after dropping firms that received autonomy before 2005, which allows me to test for pre-trends. Columns 3 and 4 include all SOE directors, and Columns 5 and 6 only

[15]I also test whether interest payments per rupee of borrowing change, by using the ratio of interest payments to total borrowing as an outcome variable. These too do not change. Results are omitted for brevity, and are available upon request.

incumbent SOE directors. Results are similar across specifications, and indicate that SOE directors from treated firms are more likely to be matched to private firm boards. Importantly, they are no more likely to be matched to private firm boards before autonomy is granted (so there are no statistically significant pre-trend effects). The magnitude of the effects is about 6-8 percentage points, which is approximately 10-13 percent relative to the mean probability of a matched name. Unfortunately, the data do not contain demographic variables or details on compensation or tenure, so I cannot test whether these effects are stronger for short vs. long tenure managers, or vary by managerial age. Given these unfortunate limitations of the data, and the fact that the data only begin after the beginning of the program, these results should be considered to be descriptive.

6.2.4 Electoral Cycles in Hiring

While autonomy on average leads to greater hiring as shown in Table 5, it is possible that SOEs and the government differ in the timing of their preferred hiring namely, the government wants to hire more right before an election, while the SOE does not. I use data on annual state-level employment presence for each SOE (available between 1999-2009). I estimate Equations 5 and 6 and present the results in Table 9. Columns 1 and 2 use the entire sample and show results from estimating Equation 5. Columns 3 and 4 drop firms that received autonomy before 2005, and allows me to test for pre-trends. The results are consistent with those in columns 1 and 2, though they are more imprecisely estimated since I use fewer firms for this estimation. The results indicate that firms with autonomy have a lower proportion of their employment in a state the year before that state has an assembly election, relative to firms without autonomy. I interpret this to capture the divergence of preferences of the managers and the government. In light of the above outcomes and the relatively small magnitude of pre-election hiring in untreated firms, I interpret this incentive conflict to be 'small.' As the theoretical framework argued, this provides a unified explanation for both the changes due to and the success of the autonomy program.

7 Robustness Checks and Additional Outcomes

In this section, I first show that the main results are robust and then report some additional outcomes, namely, employment composition and the volatility of firm returns.

7.1 Robustness Checks

7.1.1 Program eligibility versus achieving autonomy

The main empirical strategy uses eligibility for the program as a proxy for treatment. A concern with this strategy is that eligibility alone (earning profits for three years continuously and a positive net worth) has a direct impact on firm outcomes independent of receiving autonomy. To show

this is not the case, I estimate

$$y_{ijt} = \alpha + \alpha_i + \gamma_t\phi_j + \beta_1\left(\mathbb{1}(\text{post 1996})_t * \mathbb{1}(\text{eligible})_{ij}\right) + \mu_1\left(\mathbb{1}(\text{pre 1996})_t * \mathbb{1}(\text{eligible})_{ij}\right) + \beta_2\left(\mathbb{1}(\text{post 1996})_t * \mathbb{1}(\text{received autonomy})_{ij}\right) + \mu_2\left(\mathbb{1}(\text{pre 1996})_t * \mathbb{1}(\text{received autonomy})_{ij}\right) + \epsilon_{ijt}. \quad (7)$$

Table A1 reports the results. Once autonomy status is controlled for, pre-program eligibility has no marginal effect on firms' returns or productivity. Hence, all outcomes are driven by actually receiving autonomy and becoming eligible alone has no effect.

7.1.2 Negative spillover effects on ineligible firms

Instead of having a positive effect on treated firms, it is possible that autonomy had negative spillovers on non-treated firms. Table A2 shows that this is not the case. It reports results from five-digit sectors in which either all or none of the firms were eligible for autonomy before 1997. Since spillover effects are likely to occur within the same sector, including only sectors with all or no pre-program eligible firms leaves a sample with the least amount of potential for spillover effects. This halves the sample size, but the results, while nosier due to the smaller sample size, are very similar to those reported in Tables 3 and 4.[16]

7.1.3 Manipulation of reported profits

Firms that make small losses might be able to falsely report small positive profits instead to increase their eligibility probability. Because I consider firms that were already eligible before the program as treated, this ensures that the results are not driven by such misreporting (if it exists). To further test that results do not change if firms around the zero profit threshold are removed, Table 10 presents the results from a "donut" estimator. Panel A reports results after removing 10 firms around the zero profits threshold in each year (as well as all firms reporting exactly zero profits), and Panel B reports results removing 15 firms around the zero profits threshold in each year (as well as all firms reporting exactly zero profits). The results are quite similar to those in Tables 3 and 4, and consistent across both panels.

7.1.4 Results including entry and exit, including more stringent fixed effects and alternate sample

In my main specifications, I restrict the sample to SOEs that reported data five years before and at least five years after the program. In Table A5, I report results using the entire sample (as well as entry and exit results), to show that the results are not sensitive to accounting for entry and exit.

[16] I residualize sector by year and firm FEs in the whole sample before running regressions on this sample to ensure that I am controlling for similar sectoral-year effects as in the whole sample. Results are the same if I simply estimate Equation 1 on this restricted sample.

Estimates are once again similar to those reported in Tables 3 and 4, both in statistical significance and magnitude.

Table A3 reports results for firm returns and productivity including three-digit NIC sector by year fixed effects and firm fixed effects instead of two-digit NIC sector by year fixed effects and firm fixed effects. Table A4 reports the main results dropping SOEs that never report positive profits between the sample period (1992-2009). Both sets of results are also consistent with those in Tables 3 and 4.

7.1.5 Generalized difference-in-differences and results by grade of autonomy

Table 14 presents results from a generalized difference-in-difference estimation that directly tests whether firms performed differently after receiving autonomy. The results are positive, statistically significant, and similar to those in Tables 3 and 4, though larger in magnitude. This is to be expected, since I am directly testing for the effects of autonomy rather than proxying for treatment status with pre-program eligibility, and estimating effects after the firm actually receives autonomy, rather than post-1996, when the program was first implemented.

As mentioned in section 2, the grades of autonomy that a firm was granted determined the level of capital expenditure the firm could undertake without government approval, as well as the funds allocated to a subsidiary or joint venture. Given this, it is interesting to test whether a higher grade of autonomy shows larger effects of the program. In addition, Table A8 presents generalized difference-in-difference results by grade of autonomy. Panel A presents the results for all firms. The treatment variable in this case is a categorical variable that takes the values 0, 1, 2, or 3. It is 0 for control firms, 1 for the least level of autonomy (Mini-Ratna Category-II) and 3 for firms with the most level of autonomy (Navratna).[17] Panel B additionally presents the results for treated firms only; that is, conditional on being treated, it tests whether the effects of autonomy varied by autonomy level received. I find that the grade of autonomy mattered for profitability, sales, and value added. Interestingly, conditional on being treated, labor productivity increases do not depend on the level of autonomy received, as shown in columns 4 and 5 in Panel B.

7.2 Additional Results

7.2.1 Additional Measure of Productivity

Table 13 presents results using additional measures of productivity, measured using the Ackerberg et al. (2015) estimation procedure. The estimation uses employment as the free variable, and raw materials as the intermediate variable. I calculate two alternate measures of productivity, using both sales and value added as left hand side variables. Capital assets are the state variable. I show results both using the main specification, as well as the generalized differences-in-differences. The

[17] 13 of 73 firms that received autonomy upgraded autonomy status (for example, went from being a Mini-Ratna Category-II to a Mini-Ratna Category-I). To ensure a consistent sample, I assign these firms to the first (least) level of autonomy they received.

pattern is consistent with the main results, showing no evidence of pre-trends and an increase in productivity post-program. They are also consistent with the hypothesis that eligibility in absence of autonomy does not increase productivity. These further bolster the claim that assigning decision rights to managers led to increases in productivity.

7.2.2 Volatility of outcomes

To determine the effect of the program on the volatility of outcomes, I estimate the firm-level standard deviation of each of the main outcomes over three 5-year periods- 1992-1996 (before the program), 1997-2001 (shortly after the program), and 2002-2006 (longer term after the program). This gives a firm-year panel that comprises of three data points (1996, 2001, and 2006) for each firm, each of which is the standard deviation of the outcome over the last 5 years. I then test whether the firm-level standard deviation of profits, sales and value added changed in the short term (in the 5 year period immediately after the program, between 1997 and 2001), and in the longer term (in the 5 year period between 2002 and 2006), relative to 5 years before the program (1992-1996). Results are presented in Table 11. I find no difference in volatility in the short-term, but a much higher volatility across outcomes in the longer-term. This shows these firms' returns, while higher on average, were also accompanied by greater volatility.

7.2.3 Government Ownership

If giving firms autonomy was accompanied by changes in government ownership, it is possible that this was a mechanism for changes in the outcomes. However, government ownership was not affected by the autonomy program. I estimate Equation 1 using the proportion of central government's equity holdings as the outcome variable, which is available between 1994 and 2009. Results are presented in Table A6, and show that government equity did not change as a result of the program. I also show that the results are robust to including state government holdings in the definition of government holdings (though state government holdings in these centrally owned SOEs is very small, on average less than 2% of equity).

7.2.4 Effects on employment by worker type

Table A7 reports results for three types of employees separately (available between 1994 and 2009): managerial and supervisory, non-managerial and non-supervisory, and non-permanent employees. The latter is a measure of the propensity of firms to outsource work, possibly to less well-paid workers with fewer protections under labor laws. Similar to the results for overall employment, I find positive, but noisy effects on all three categories of employment. The results indicate that both managerial and non-managerial employment increased by about 13% relative to mean employment levels (an increase of about 239 managers on average and about 1500 non-managers on average).

7.2.5 Robustness Checks Regarding Outliers

In Table A9, I present results using different methods to identify and remove outliers in the profitability and sales variables. I use four different strategies: trimming the values at the 1st and 99th percentile, winsorizing them at the 1st and 99th percentile, removing values with a z-score greater than 3 or less than -3, and removing values with a z-score greater than 2.5 or less than -2.5.[18] The results are consistent across these different strategies, indicating that extreme values in these variables are not driving the results.

7.2.6 Event studies

I present event study estimates for 10 years after the program in Figures A1 through A10 and additional figures C1 through C8, which are consistent with the regression tables. While the timing of the impacts is noisier when the main specification is used (Figures A6 through A10), this is driven by timing of receiving autonomy differing for firms. Figures A1 through A5 present the generalized DID impacts for pre-program eligible firms only, and show that the effects begin right after autonomy is granted, and increase over time.

7.2.7 Effects across the Outcome Distribution

Table 14 presents results from estimating the effects across the outcome distribution for the three main outcomes of interest, using the Athey and Imbens (2006) estimator. I show results for mean and median effects, as well as by tercile, using the same fixed effects as the main specification. The results at the median are substantially smaller than mean effects for all three outcomes. While the program led to improvements in firm performance across the distribution, the estimates for the first tercile are much larger, indicating that the program had larger effects for smaller firms.

8 Conclusion

While there is a rich theoretical literature, there is a relative paucity of empirical evidence on the causal effects of delegation in firms. At a basic level, the point of departure of the theory is that delegation can lead to distinct outcomes because the preferences of the privately informed agent may differ from those of the uninformed principal. If the principal asks the agent to communicate her private information, she will strategically misreport to ensure the principal follows her advice. Conversely, if the principal delegates authority, the agent can act on her information but will do

[18] While using log transformations of these variables would be a different technique for this purpose, differences-in-differences imposes an additivity assumption. That is, it assumes that outcomes are additive in a time effect, a group effect, and an unobservable that is independent of both of these (Heckman, 1996). This implies that results may be different with nonlinear transformations in outcomes, and trends in such nonlinear transformations may not be balanced (Athey and Imbens, 2006). Since the pre-trends in levels are balanced, I use levels, and use these alternative checks to test for outlier effects.

so in accordance with her (and not the principal's) preference. Which of these is superior for the principal depends on the extent of the incentive conflict with the agent.

I study a program in India where precisely the above organizational change occurred in some but not all SOEs. I show that awarding autonomy to managers leads to a change in decision making which in turn benefits the government. My descriptive evidence simultaneously uncovers the incentive conflict and shows that it can be considered to be small. Specifically, I find that autonomy increases profits, sales, and productivity significantly. These results hold for about 13 years after the program, the entire length of the sample period. These changes are driven by increases in both capital and labor expenditures. I show that private sector career concerns are a factor driving SOE managers' and that, while hiring increased, it is less responsive to electoral cycles (in contrast to untreated firms).

The specific context I study is important for several reasons. The first is the reform only awarded autonomy and was not accompanied by other organization reforms that could conflate the estimated effects. Secondly, studying the impact of earned autonomy programs for public sector organizations is particularly important. This is because these firms constitute a substantial fraction of both developing and developed economies and autonomy programs are a ubiquitously used reform to improve productivity across a variety of different settings from natural resource management and manufacturing to health and education. Finally, the results show that large gains in SOE performance are possible by organizational reform without changes to ownership.

These results contribute to understanding why autonomy affects organizational outcomes and when it can be an effective reform. However, the policy does not allow me to separately test the impact of quasi-randomly or randomly giving autonomy to *all* firms. Autonomy may have heterogeneous returns; for instance, consistently loss-making SOEs may lack the organizational or managerial capacity to benefit from independent decision making. Second, since the program I study allows managers to take several important strategic decisions, I cannot disentangle the effects of autonomy for each decision separately. Third, I cannot test the extent to which the program motivated the managers of ineligible SOEs to improve firm performance and become eligible. These and related questions, including whether similar programs generate positive impacts in other settings, remain important questions for future work.

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Table 2: Summary Statistics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Whole Sample			*Firms With Autononomy*			*Pre-Program Eligible*		
Number of SOEs	193			73			95		
	N	Mean	SD	N	Mean	SD	N	Mean	SD
Total Loans (00,000 Rs.)	3,342	16,963.45	89,298.44	1,303	29,953.92	130,059.30	1,679	23,575.83	117,448.70
Interest Payments (00,000 Rs.)	3,342	11,994.68	37,691.79	1,303	17,645.75	46,497.23	1,679	15,324.83	44,196.68
Net Profit (00,000 Rs.)	3,342	18,090.33	101,533.20	1,303	50,643.46	150,170.00	1,679	37,285.52	134,685.00
Gross Sales (00,000 Rs.)	3,342	282,763.90	1,432,490.00	1,303	621,153.40	2,206,133.00	1,679	487,473.30	1,960,220.00
Value Added (00,000 Rs.)	3,342	99,627.92	375,939.40	1,303	218,157.40	570,131.20	1,679	171,327.10	511,355.80
Sales Per Employee (00,000 Rs.)	3,301	58.56	202.67	1,291	102.98	263.85	1,641	95.54	261.24
Value Added Per Employee (00,000 Rs.)	3,264	17.47	48.71	1,277	31.16	64.75	1,619	28.98	64.53
Retained Profit (00,000 Rs.)	3,341	10,934.56	65,708.59	1,302	32,936.25	90,620.45	1,678	24,134.37	83,105.16
Dividend Paid (00,000 Rs.)	3,341	6,312.73	36,057.27	1,302	15,675.13	56,421.10	1,678	11,597.86	49,349.76
Capital Assets (00,000 Rs.)	3,338	273,303.10	1,066,236.00	1,300	573,913.00	1,614,447.00	1,675	458,874.00	1,455,359.00
Number of Employees	3,338	8,459.91	21,106.89	1,301	12,936.53	25,096.83	1,676	9,353.59	21,747.74
Wage Bill (00,000 Rs.)	3,342	17,986.60	51,863.87	1,303	32,053.38	70,353.95	1,679	24,486.10	63,692.94
	Private Sector Firms						*Pre-Program Eligible Private Firms*		
Number of Private Firms in the Same 5-digit Sectors as SOEs	1,426						993		
	N	Mean	SD				N	Mean	SD
Net Profit (00,000 Rs.)	23,208	1,776.96	26,045.53				17,550	2,324.84	29,890.11
Gross Sales (00,000 Rs.)	20,555	25,468.16	258,928.60				15,878	31,015.50	294,084.20
Value Added (00,000 Rs.)	15,928	14,733.21	102,882.00				12,550	17,641.54	115,601.50

Notes: Pre-program eligible firms are those that earned positive profits for three consecutive years and had a positive net worth before 1997.

Table 3: Sales, Value Added, and Profit

	(1)	(2)	(3)
	Sales (Rs. 00,000)	Value Added (Rs. 00,000)	Profit (Rs, 00,000)
1(Eligible Pre-Program)*1(Year<1996)	-4,118	-5,193	-2,358
	(14,353)	(5,543)	(2,035)
1(Eligible Pre-Program)*1(Year>1996)	88,115**	42,017***	10,561**
	(44,400)	(15,812)	(5,320)
Controls	Firm FE, NIC 2-digit X Year FE		
Observations	3,342	3,342	3,342
R-Squared	0.863	0.832	0.726
Mean of Dependent Variable	282,764	99,628	18,090

Notes: Standard errors clustered at the firm level in parentheses (*** p<0.01, ** p<0.05, * p<0.1). 1(Eligible Pre-Program)*1(Year=1996) is the omitted category.

Table 4: Labor Productivity

	(1)	(2)
	Sales Per Employee (Rs. 00,000)	Value Added Per Employee (Rs. 00,000)
1(Eligible Pre-Program)*1(Year<1996)	1.622	-2.444*
	(5.570)	(1.457)
1(Eligible Pre-Program)*1(Year>1996)	45.20	10.86*
	(28.22)	(5.724)
Controls	Firm FE, NIC 2-digit X Year FE	
Observations	3,301	3,264
R-Squared	0.678	0.767
Mean of Dependent Variable	58.56	17.47

Notes: Standard errors clustered at the firm level in parentheses (*** p<0.01, ** p<0.05, * p<0.1). 1(Eligible Pre-Program)*1(Year=1996) is the omitted category. Sales per employee and profit per employee are trimmed at the 1st and 99th percentile.

Table 5: Capital, Labor and Participation in Joint Ventures/Subsidiaries

	(1)	(2)	(3)	(4)
	Capital Assets (Rs. 00,000)	Wage Bill (Rs. 00,000)	Number of Employees	1(SOE Reported a Subsidiary or a Joint Venture)
1(Eligible Pre-Program)*1(Year<1996)	-15,999	-917.1	-368.5	-0.0130
	(13,250)	(1,157)	(364.2)	(0.0435)
1(Eligible Pre-Program)*1(Year>1996)	64,001**	9,119*	1,554	0.0686*
	(28,878)	(4,727)	(1,109)	(0.0401)
Controls		Firm FE, NIC 2-digit X Year FE		
Observations	3,338	3,342	3,338	3,342
R-Squared	0.849	0.834	0.976	0.717
Mean of Dependent Variable	273303	17,987	8,460	0.193

Notes: Standard errors clustered at the firm level in parentheses (*** p<0.01, ** p<0.05, * p<0.1). 1(Eligible Pre-Program)*1(Year=1996) is the omitted category. Capital assets include the book value of fixed assets, investments, and capital works in progress.

Table 6: Borrowings and Profit Utilization

	(1)	(2)	(3)
Panel A	Retained Profits (Rs. 00,000)	Dividends (Rs. 00,000)	Interest Payments (Rs. 00,000)
1(Eligible Pre-Program)*1(Year<1996)	-1,497	-957.0**	52.44
	(1,753)	(468.6)	(1,468)
1(Eligible Pre-Program)*1(Year>1996)	6,118	3,592**	2,191
	(3,874)	(1,587)	(4,432)
Controls		Firm FE, NIC 2-digit X Year FE	
Observations	3,341	3,341	3,342
R-Squared	0.698	0.690	0.807
Mean of Dependent Variable	10,935	6,313	11,995
Panel B	Total Loans (Rs. 00,000)	Government Loans (Rs. 00,000)	Non-Government Loans (Rs. 00,000)
1(Eligible Pre-Program)*1(Year<1996)	-9,226	-2,759	-6,467
	(6,587)	(2,639)	(5,825)
1(Eligible Pre-Program)*1(Year>1996)	29,462	-33,142**	62,604
	(70,737)	(16,794)	(75,060)
Controls		Firm FE, NIC 2-digit X Year FE	
Observations	2,685	2,685	2,685
R-Squared	0.765	0.718	0.734
Mean of Dependent Variable	166,295	24,946	141,350

Notes: Standard errors clustered at the firm level in parentheses (*** p<0.01, ** p<0.05, * p<0.1). 1(Eligible Pre-Program)*1(Year=1996) is the omitted category. Variables in Panel B are available only from 1994-2009.

Table 7: SOE Board of Directors Matched to Private Firm Boards

	(1)	(2)	(3)	(4)	(5)	(6)
	1(Matched to a Private Firm Board of Directors)					
1(Firm Has Autonomy)	0.0425***	0.0336**				
	(0.0142)	(0.0156)				
1(Treatment)*1(Year<Year Before which Autonomy Received)			-0.0106	-0.00327	-0.000404	0.00253
			(0.0122)	(0.0139)	(0.0161)	(0.0198)
1(Treatment)*1(Year>=Year in which Autonomy Received)			0.0794***	0.0655***	0.0657***	0.0619***
			(0.0153)	(0.0168)	(0.0173)	(0.0221)
Controls	Year FE		Director Name FE			
			1(Pre-autonomy), 1(Post-autonomy)	1(Pre-autonomy), 1(Post-autonomy), Year FE	1(Pre-autonomy), 1(Post-autonomy)	1(Pre-autonomy), 1(Post-autonomy), Year FE
Sample	Full		Dropping Firms that received autonomy before 2005			
Sample	All SOE Directors	Only SOE Directors Before 2005	All SOE Directors		Only SOE Directors Before 2005	
Observations	56,709	34,437	25,516	25,516	16,397	16,397
R-Squared	0.78	0.811	0.781	0.787	0.81	0.811
Mean of Dependent Variable	0.681	0.683	0.576	0.576	0.594	0.594

Notes: Standard errors clustered at the director name level in parentheses (*** p<0.01, ** p<0.05, * p<0.1). 1(Treatment)*1(Year=One Year Before Autonomy Received) is the omitted category in columns 3 and 4. For the control group, the omitted year is 2005. The data for whether a SOE director is matched to a private firm board is available from 2003-2010.

Table 8: Sales and Profits Effects Relative to the Private Sector

	(1)	(2)	(3)	(4)	(5)	(6)
	Sales (Rs. 00,000)	Value Added (Rs. 00,000)	Net Profit (Rs. 00,000)	Sales (Rs. 00,000)	Value Added (Rs. 00,000)	Net Profit (Rs. 00,000)
1(SOE)*1(Eligible Pre-Program)*1(Year<1996)	-41,720	-10,819	-3,449*	-47,162	-5,332	-785.5
	(32,249)	(7,317)	(1,833)	(50,097)	(10,711)	(2,247)
1(SOE)*1(Eligible Pre-Program)*1(Year>1996)	222,436**	59,100*	17,146*	153,105**	56,626**	16,384**
	(96,771)	(30,730)	(8,853)	(77,542)	(26,956)	(8,211)
1(SOE)*1(Year<1996)	-19,908*	-5,850	-142.2	-24,392	-6,172	-1,608
	(10,993)	(3,702)	(1,018)	(19,624)	(5,862)	(1,458)
1(SOE)*1(Year<1996)	39,135	143.6	3,135	25,698	-3,830	2,116
	(25,060)	(8,461)	(3,162)	(32,199)	(11,672)	(4,303)
1(Eligible Pre-Program)*1(Year<1996)	-7,123***	-4,323***	-337.2	-4,837	-7,926**	-2,156**
	(2,599)	(1,165)	(364.6)	(7,823)	(3,465)	(1,043)
1(Eligible Pre-Program)*1(Year>1996)	5,217	7,300**	1,889**	17,583	9,915**	1,199
	(8,912)	(3,390)	(950.2)	(12,857)	(4,031)	(1,519)
Controls			Firm FE NIC 2-digit X Year FE			
Sample Restriction		None		SOEs and Private Firms with Pre-Program Sales Greater than Mean Sectoral Sales		
Observations	23,785	19,158	26,438	8,917	7,978	10,032
R-Squared	0.729	0.753	0.591	0.792	0.787	0.646
Mean of Dependent Variable	59173	29624	3843	125656	55756	7791

Notes: Standard errors clustered at the firm level in parentheses (*** p<0.01, ** p<0.05, * p<0.1). 1(SOE)*1(Eligible Pre-Program)*1(Year=1996), 1(SOE)*1(Year=1996), and 1(Eligible Pre-Program)*1(Year=1996) are the omitted categories.

Table 9: Autonomy Effects on Electoral Cycles in Hiring Decisions

	(1)	(2)	(3)	(4)
	Percentage of Employment in a State			
1(Firm Has Autonomy)	0.00109			
	(0.000710)			
1(Year Before An Election)	0.00391*	0.00411*	-0.00992	-0.00990
	(0.00204)	(0.00213)	(0.00961)	(0.00971)
1(Firm Has Autonomy)*1(Year Before An Election)	-0.00514*	-0.00542*		
	(0.00309)	(0.00323)		
1(Treatment)*1(Year<Year Before which Autonomy Received)*1(Year Before An Election)			-0.0171	-0.0184
			(0.0484)	(0.0499)
1(Treatment)*1(Year>Year in which Autonomy Received)*1(Year Before An Election)			-0.0593	-0.0624
			(0.0555)	(0.0571)
Controls	Firm FE, State FE	State FE	Firm FE, State FE	State FE
	NIC 2-digit X Year FE	Firm X Year FE	NIC 2-digit X Year FE	Firm X Year FE
Sample	Full		Dropping Firms that received autonomy before 2005	
Observations	21,186	20,990	43,491	43,491
R-Squared	0.311	0.188	0.37	0.402
Mean of Dependent Variable	0.0915	0.0830	0.188	0.188

Notes: Standard errors clustered at the director name level in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Interactions with 1(Year=One Year Before Autonomy Received) is the omitted category in columns 3 and 4. For the control group, interactions with a dummy variable that is 1 if year is 2005 and 0 otherwise are the omitted categories. The data on SOE employment presence in a state each year available from 1999-2009. Columns 3 and 4 additionally include the control variables 1(Year<Year Before which Autonomy Received)*1(Year Before An Election), 1(Year>Year Before which Autonomy Received)*1(Year Before An Election), 1(Year<Year Before which Autonomy Received), 1(Treatment)*1(Year<Year Before which Autonomy Received), 1(Treatment)*1(Year>Year Before which Autonomy Received), 1(Year>Year Before which Autonomy Received) and 1(Treatment)*1(Year Before An Election).

Table 10: Firm Returns and Productivity Using a "Donut" Estimator

	(1)	(2)	(3)	(4)	(5)
Panel A	Sales (Rs. 00,000)	Value Added (Rs. 00,000)	Profits (Rs. 00,000)	Sales Per Employee (Rs. 00,000)	Value Added Per Employee (Rs. 00,000)
1(Eligible Pre-Program)*1(Year<1996)	9,783	-8,510	-2,673	2.443	-2.061
	(25,881)	(5,752)	(2,666)	(6.661)	(1.389)
1(Eligible Pre-Program)*1(Year>1996)	105,764***	45,924***	11,786**	53.49**	13.04**
	(36,216)	(17,080)	(5,904)	(26.30)	(6.085)
Controls	Firm FE, NIC 2-digit X Year FE				
Sample Restriction	Dropping 10 Firms Each Around Zero Profits and Firms With Zero Profits				
Observations	2,935	2,935	2,935	2,894	2,858
R-Squared	0.864	0.839	0.729	0.719	0.781
Mean of Dependent Variable	305686	110,331	20602	64.69	19.32
Panel B	Sales (Rs. 00,000)	Value Added (Rs. 00,000)	Profits (Rs. 00,000)	Sales Per Employee (Rs. 00,000)	Value Added Per Employee (Rs. 00,000)
1(Eligible Pre-Program)*1(Year<1996)	5,624	-8,096	-2,467	10.40	-1.740
	(20,581)	(6,814)	(3,025)	(11.73)	(1.534)
1(Eligible Pre-Program)*1(Year>1996)	110,028***	48,419***	12,595**	60.40**	13.47**
	(36,853)	(17,076)	(5,988)	(27.86)	(6.506)
Controls	Firm FE, NIC 2-digit X Year FE				
Sample Restriction	Dropping 15 Firms Each Around Zero Profits and Firms With Zero Profits				
Observations	2,756	2,756	2,935	2,715	2,679
R-Squared	0.865	0.840	0.729	0.741	0.787
Mean of Dependent Variable	324,373	117208	20,602	67.52	20.34

Notes: Standard errors clustered at the firm level in parentheses (*** p<0.01, ** p<0.05, * p<0.1). 1(Eligible Pre-Program)*1(Year=1996) is the omitted category. Value added per employee and sales per employee are trimmed at the 1st and 99th percentile.

Table 11: Variability of Firm Returns Over Time

	(1)	(2)	(3)
	Standard Deviation of Sales (Rs.,00,000)	Standard Deviation of Value Added (Rs.,00,000)	Standard Deviation of Profits (Rs.,00,000)
1(Eligible Pre-Program)*1(Year=2001)	2,429	3,453	-2,626
	(4,762)	(3,756)	(2,008)
1(Eligible Pre-Program)*1(Year=2006)	40,159**	15,037*	5,289
	(19,880)	(9,049)	(5,388)
Controls		Firm FE, NIC 2-digit X Year FE	
Sample Restriction		None	
Observations	554	554	554
R-Squared	0.914	0.849	0.781
Mean of Dependent Variable	56,684	20370	8,171

Notes: Standard errors clustered at the firm level in parentheses (*** p<0.01, ** p<0.05, * p<0.1). There are three data points per firm. Standard deviation of sales for a firm comprises the standard deviation of sales between 1992-1996, 1997-2001, and 2002-2007. Similarly, the standard deviation of profits comprises the standard deviation of profits between 1992-1996, 1997-2001, and 2002-2007. Standard deviation of value added comprises the standard deviation of value added between 1992-1996, 1997-2001, and 2002-2007. 1(Eligible Pre-Program)*1(Year=1996) is the omitted category.

Table 12: Costs of Production and Net Profit Over Sales

	(1)	(2)
	Costs of Production	Gross Profit/Total Sales
1(Eligible Pre-Program)*1(Year=2001)	3,309	-0.160
	(20,777)	(0.289)
1(Eligible Pre-Program)*1(Year=2006)	102,489	-0.210
	(76,363)	(0.269)
Controls	Firm FE, NIC 2-digit X Year FE	
Sample Restriction	None	
Observations	3,342	3,206
R-Squared	0.858	0.53
Mean of Dependent Variable	390,512	-0.0942

Notes: Standard errors clustered at the firm level in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Gross Profit is profit before depreciation, interest and taxes. Gross Profit/Total Sales is trimmed at the 1st and 99th percentile. 1(Eligible Pre-Program)*1(Year=1996) is the omitted category. Cost of Production is the sum of purchase of finished goods, raw materials, wage bill, power and fuel expenses, depreciation, interest payments, and miscellaneous expenses.

Table 13: Impact on Productivity

	(1)	(2)	(3)	(4)
	Productivity (estimated using Ackerberg, Caves and Frazer (2015))			
1(Eligible Pre-Program)*1(Year<1996)	-298.8	9,072	1,321	8,803
	(6,019)	(7,443)	(6,122)	(7,204)
1(Eligible Pre-Program)*1(Year>1996)	30,776*	-8,849	26,360	-12,269
	(18,210)	(20,347)	(16,684)	(20,596)
1(SOE Received Autonomy)*1(Year<1996)		-17,177*		-13,884*
		(9,063)		(7,258)
1(SOE Received Autonomy)*1(Year>1996)		67,334**		65,646**
		(27,583)		(27,288)
Dependent Variable in Productivity Estimation	Value Added		Sales	
Controls	Firm FE, NIC 2-digit X Year FE			
Observations	3,246	3,246	3,246	3,246
Mean of Dependent Variable	0.655	0.659	0.742	0.744
R-Squared	19,964	19,964	24,394	24394

Notes: Standard errors clustered at the firm level in parentheses (*** p<0.01, ** p<0.05, * p<0.1). 1(Eligible Pre-Program)*1(Year=1996) and 1(SOE Received Autonomy)*1(Year=1996) are the omitted categories.

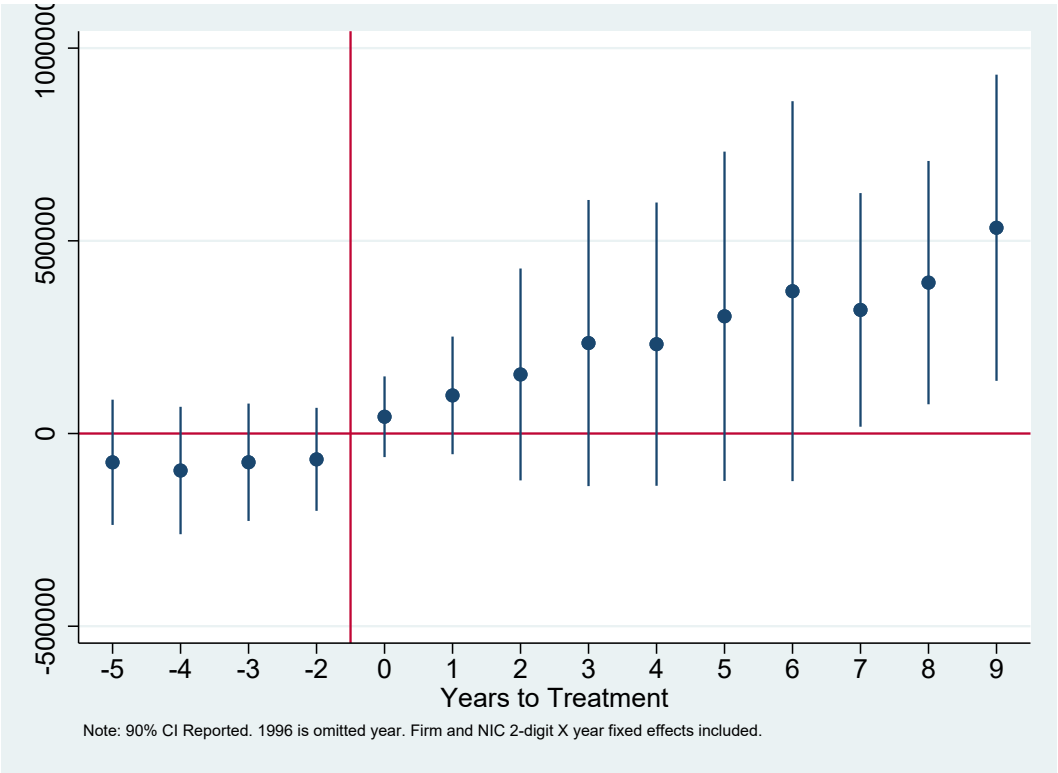
Table 14: Effects at Different Parts of the Outcome Distribution

	(1)	(2)	(3)	(4)	(5)	(6)
	Sales (Rs. 00,000)		Value Added (Rs. 00,000)		Net Profit (Rs. 00,000)	
	Mean	***Median***	***Mean***	***Median***	***Mean***	***Median***
1(Eligible Pre-Program)*1(Year>1996)	91,407***	49,131**	46,169***	24,432***	12,446***	6,456***
	(24,552)	(20,169)	(9,114)	(7,505)	(3,402)	(2,488)
			Terciles			
First Tercile: 1(Eligible Pre-Program)*1(Year>1996)	65,421***		31,265***		8,977***	
	(20,775)		(7,643)		(3,210)	
Second Tercile: 1(Eligible Pre-Program)*1(Year>1996)	38,768**		17,636***		4,355*	
	(16,610)		(6,250)		(2,451)	
Controls			Firm FE, NIC 2-digit X Year FE			
Observations	3,342	3,342	3,342	3,342	3,342	3,342
	Sales (Rs. 00,000)		Value Added (Rs. 00,000)		Net Profit (Rs. 00,000)	
	Mean	***Median***	***Mean***	***Median***	***Mean***	***Median***
Generalized Difference-in-Difference Results	248,550***	190,850***	120,807***	90,546***	43,976***	34,619***
	(38,634)	(33,474)	(15,975)	(13,553)	(5,625)	(5,028)
			Terciles			
First Tercile: Generalized Difference-in-Difference Results	191,809***		94,120***		37,143***	
	(30,451)		(13,262)		(5,673)	
Second Tercile: Generalized Difference-in-Difference Results	176,575***		85,800***		25,327***	
	(32,965)		(13,532)		(5,641)	
Controls			Firm FE, NIC 2-digit X Year FE			
Observations	3,342	3,342	3,342	3,342	3,342	3,342

Notes: Standard errors clustered at the firm level in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

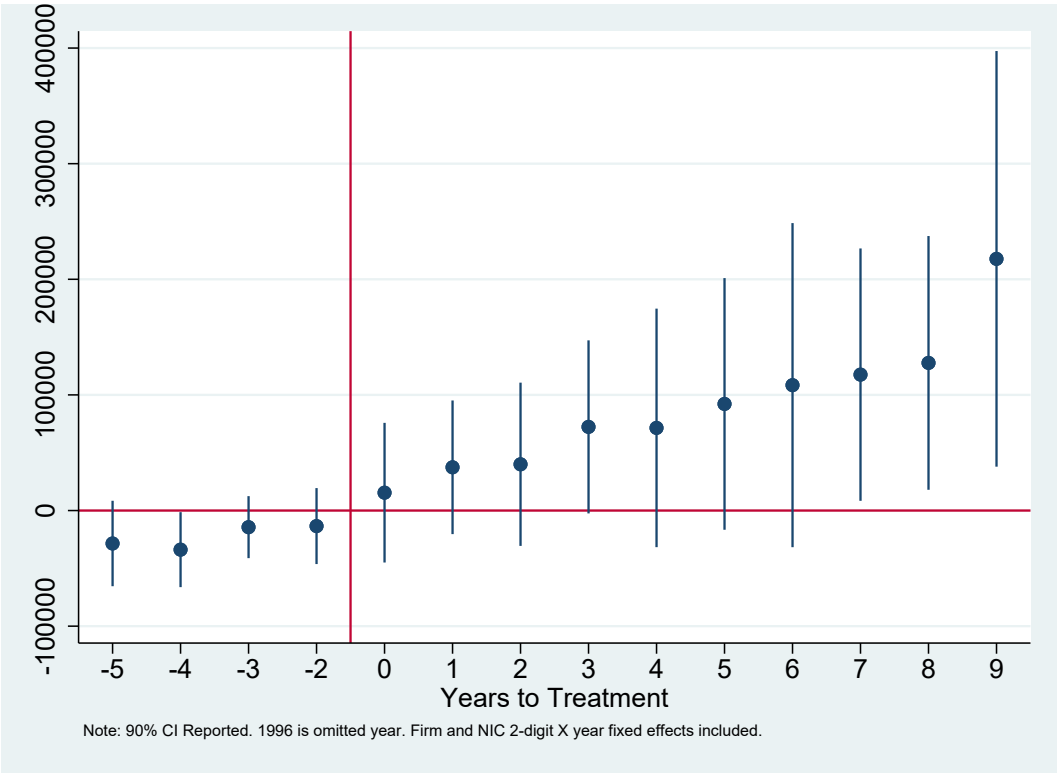
Appendix A

Figure A1: Sales (Rs. 00,000): Generalized Difference in Difference With Pre-Program Eligible Firms Only



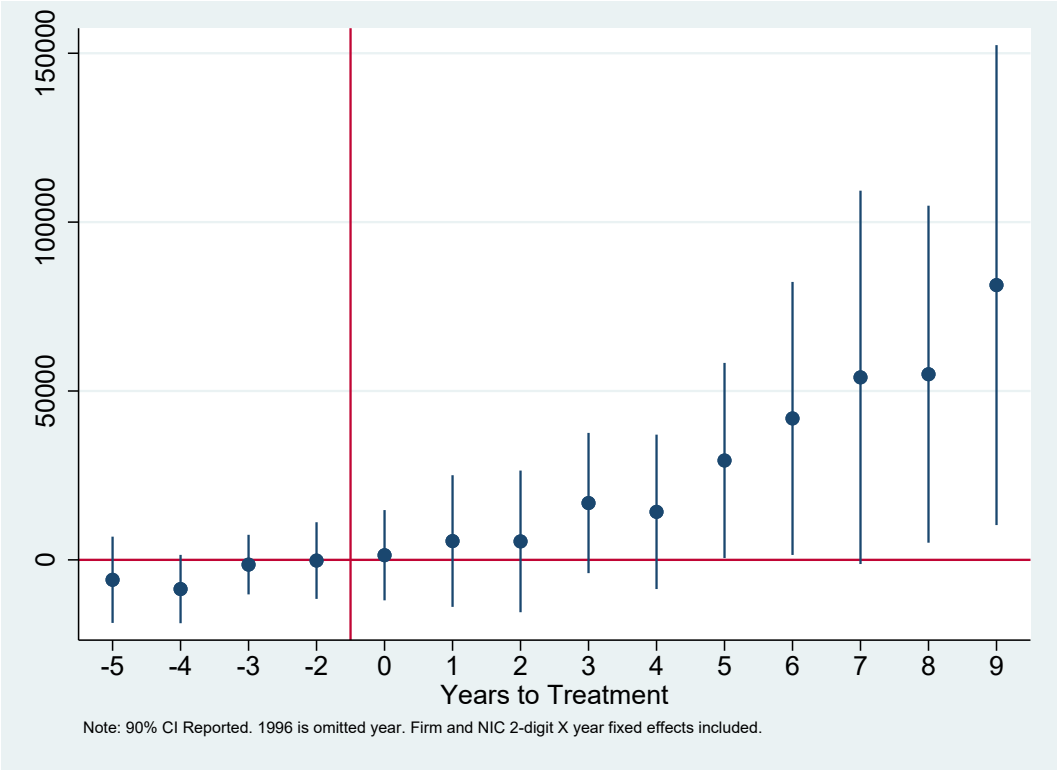
This figure plots the generalized difference in difference estimate for each year. The interaction of the treatment dummy variable with the year prior to treatment is omitted.

Figure A2: Value Added (Rs. 00,000): Generalized Difference in Difference With Pre-Program Eligible Firms Only



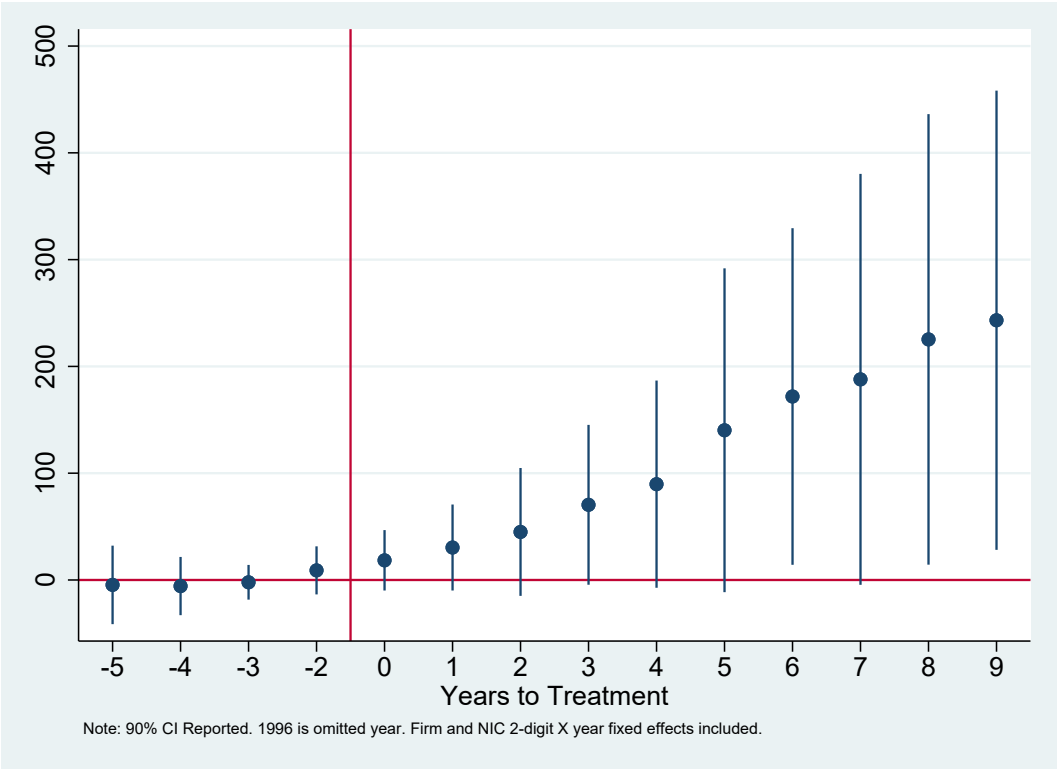
This figure plots the generalized difference in difference estimate for each year. The interaction of the treatment dummy variable with the year prior to treatment is omitted.

Figure A3: Net Profit (Rs. 00,000): Generalized Difference in Difference With Pre-Program Eligible Firms Only



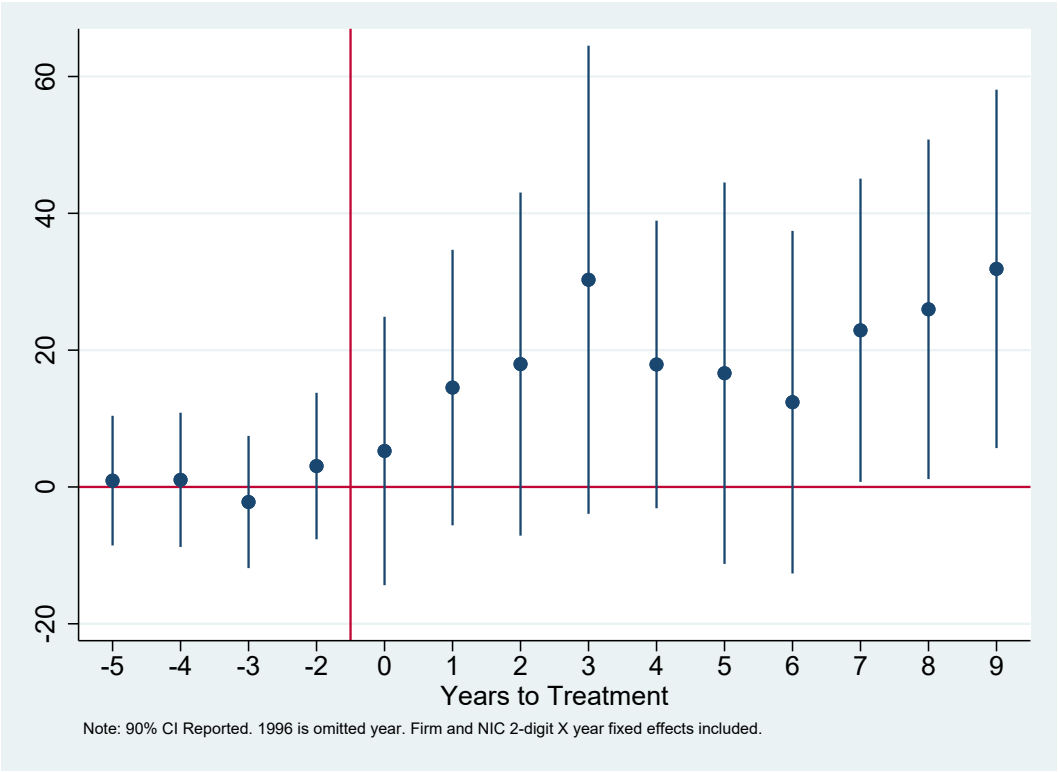
This figure plots the generalized difference in difference estimate for each year. The interaction of the treatment dummy variable with the year prior to treatment is omitted.

Figure A4: Sales Per Employee (Rs. 00,000): Generalized Difference in Difference With Pre-Program Eligible Firms Only



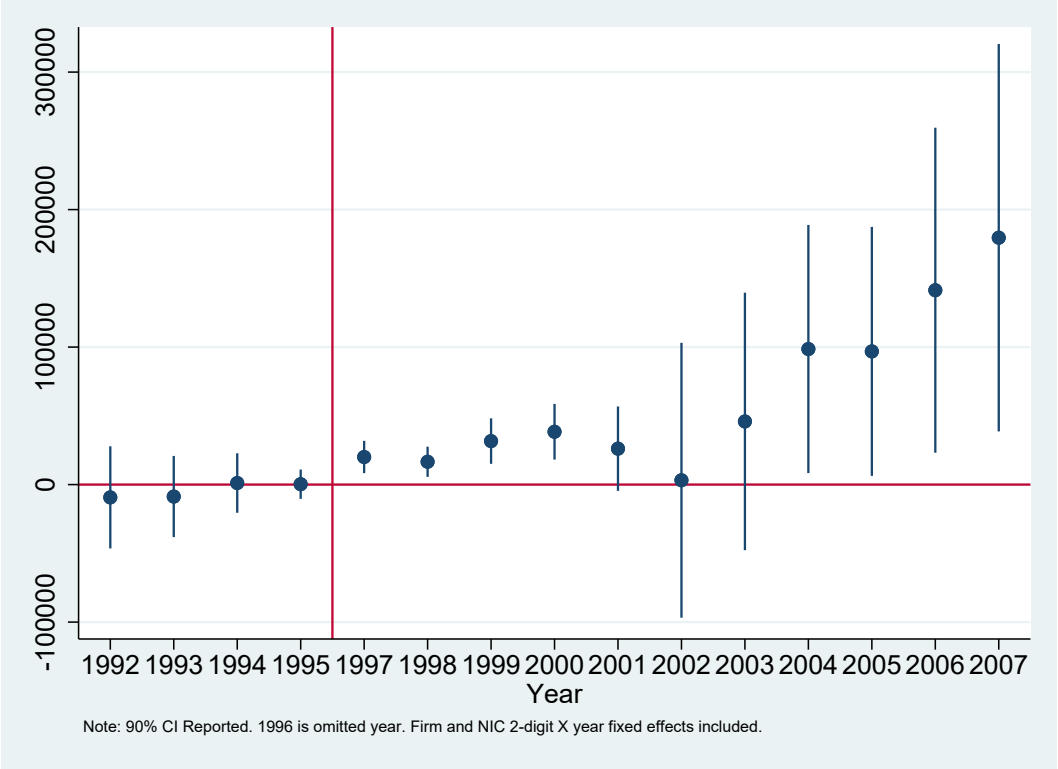
This figure plots the generalized difference in difference estimate for each year. The interaction of the treatment dummy variable with the year prior to treatment is omitted.

Figure A5: Value Added Per Employee (Rs. 00,000) : Generalized Difference in Difference With Pre-Program Eligible Firms Only



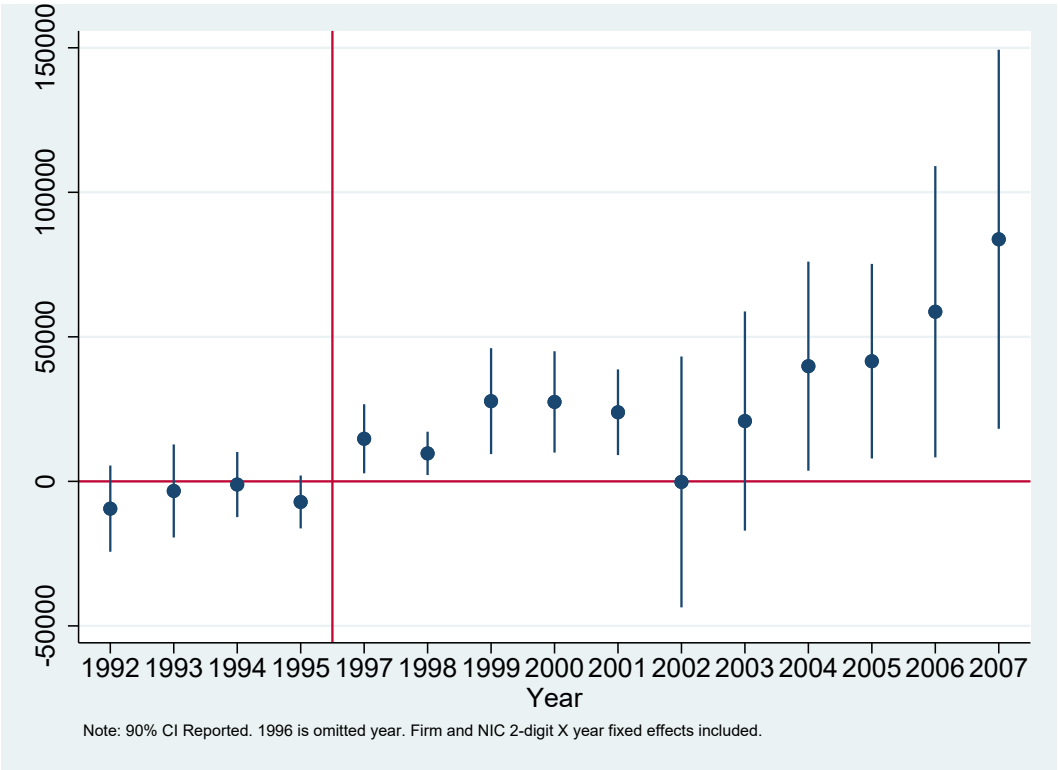
TThis figure plots the generalized difference in difference estimate for each year. The interaction of the treatment dummy variable with the year prior to treatment is omitted.

Figure A6: Sales (Rs. 00,000)



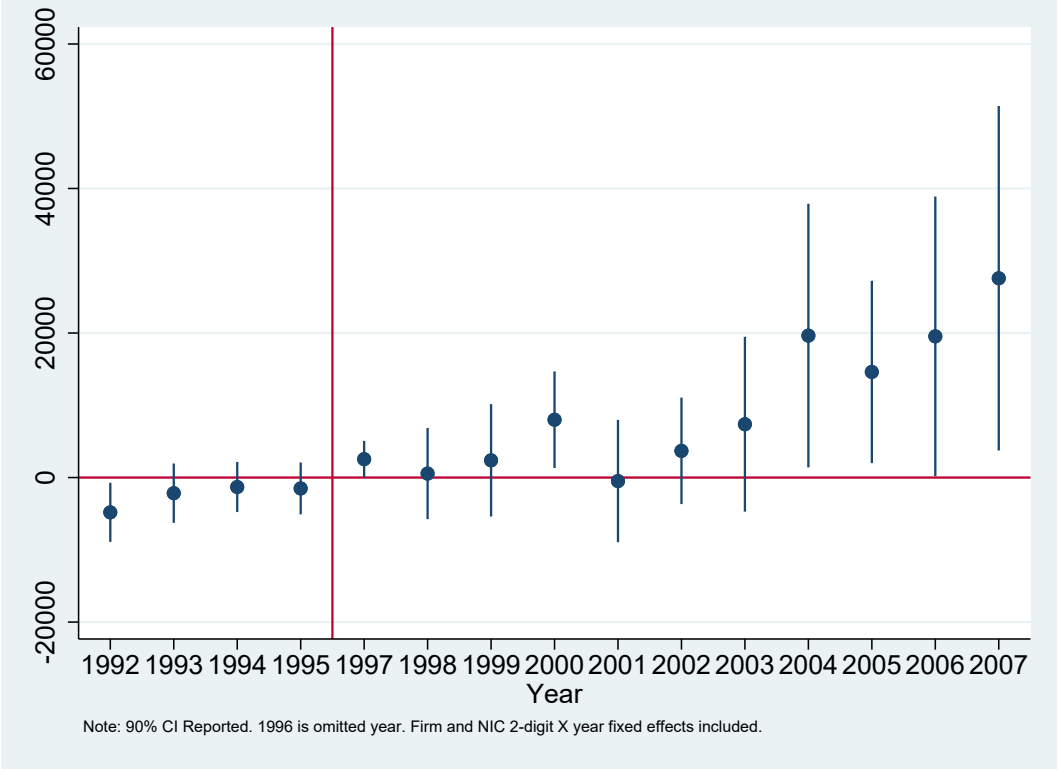
This figure plots the interaction coefficients of the pre-program eligibility dummy variable with year dummy variables. The interaction of pre-program eligibility with the 1996 year dummy variable is omitted.

Figure A7: Value Added (Rs. 00,000)



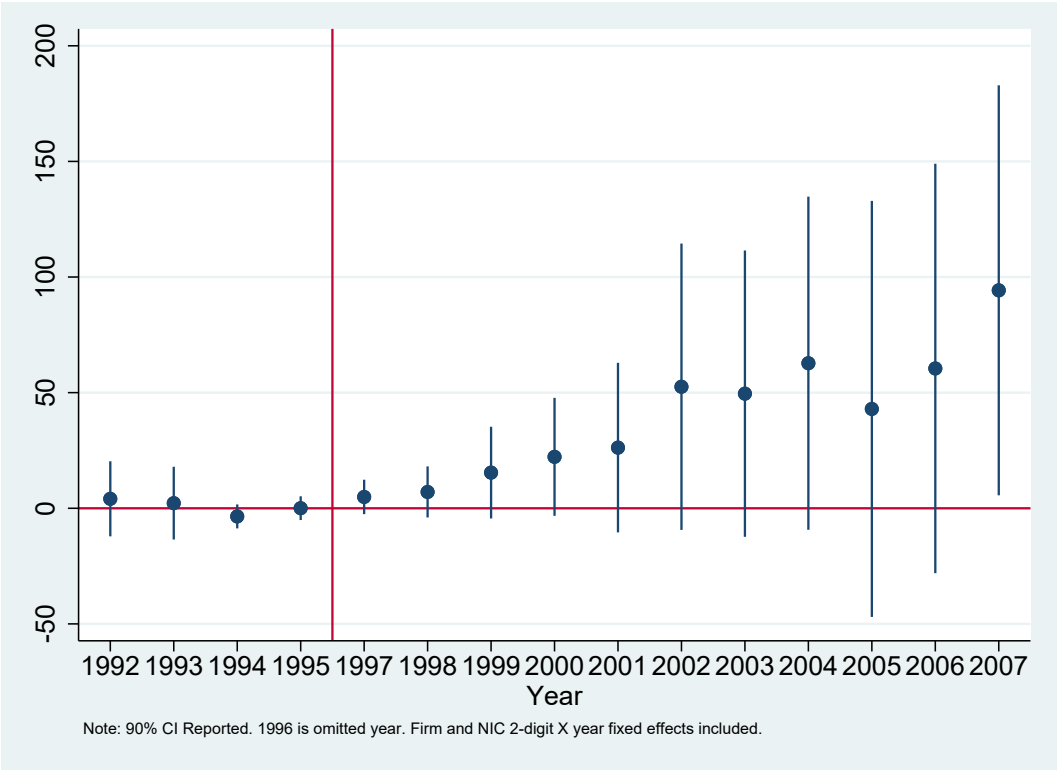
This figure plots the interaction coefficients of the pre-program eligibility dummy variable with year dummy variables. The interaction of pre-program eligibility with the 1996 year dummy variable is omitted.

Figure A8: Net Profit (Rs. 00,000)



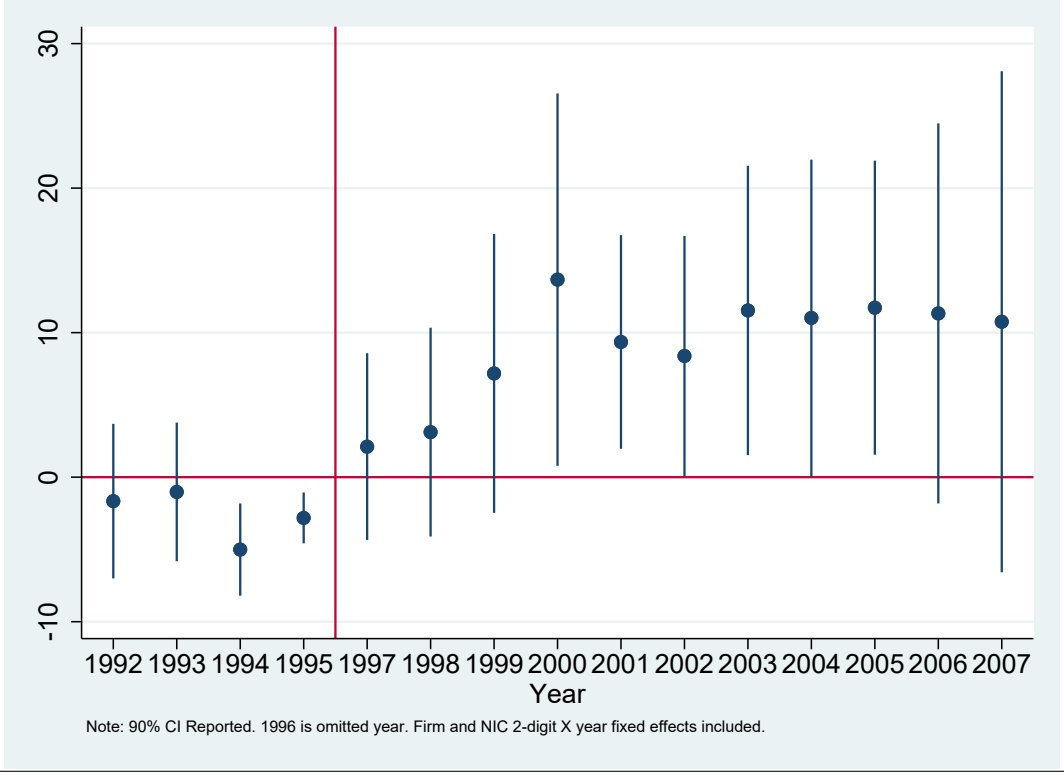
This figure plots the interaction coefficients of the pre-program eligibility dummy variable with year dummy variables. The interaction of pre-program eligibility with the 1996 year dummy variable is omitted.

Figure A9: Sales Per Employee (Rs. 00,000)



This figure plots the interaction coefficients of the pre-program eligibility dummy variable with year dummy variables. The interaction of pre-program eligibility with the 1996 year dummy variable is omitted.

Figure A10: Value Added Per Employee (Rs. 00,000)



This figure plots the interaction coefficients of the pre-program eligibility dummy variable with year dummy variables. The interaction of pre-program eligibility with the 1996 year dummy variable is omitted.

Table A1: Testing Whether Pre-Program Eligibility Affected SOE Outcomes Independent of Autonomy

	(1)	(2)	(3)	(4)	(5)
	Sales (Rs. 00,000)	Value Added (Rs. 00,000)	Net Profits (Rs. 00,000)	Sales Per Employee (Rs. 00,000)	Value Added Per Employee (Rs. 00,000)
1(Eligible Pre-Program)*1(Year<1996)	24,996	6,849	3,083	0.763	-2.025
	(23,161)	(6,966)	(2,685)	(4.577)	(1.471)
1(Eligible Pre-Program)*1(Year>1996)	-35,409	-19,157	-15,463	8.771	0.593
	(58,177)	(28,337)	(10,441)	(22.60)	(6.010)
1(SOE Received Autonomy)*1(Year<1996)	-49,824	-20,610***	-9,311***	0.786	-0.897
	(30,926)	(7,738)	(3,001)	(6.332)	(1.797)
1(SOE Received Autonomy)*1(Year>1996)	210,306***	104,165**	44,310***	61.37*	17.29**
	(78,837)	(42,767)	(15,906)	(32.72)	(7.756)
Controls		Firm FE, NIC 2-digit X Year FE			
Observations	3,342	3,342	3,342	3,301	3,264
R-Squared	0.864	0.834	0.732	0.68	0.77
Mean of Dependent Variable	282,764	99,628	18,090	58.56	17.47

Notes: Standard errors clustered at the firm level in parentheses (*** p<0.01, ** p<0.05, * p<0.1). 1(SOE Received Autonomy)*1(Year=1996) and 1(Eligible Pre-Program)*1(Year=1996) are the omitted categories. Sales per employee and profit per employee are trimmed at the 1st and 99th percentile.

Table A2: SOEs in Five-Digit Sectors Where All Firms or No Firms Were Eligible Pre-Program

	(1)	(2)	(3)	(4)	(5)
	Sales (Rs.,00,000)	Value Added (Rs.,00,000)	Profits (Rs.,00,000)	Sales Per Employee (Rs. 00,000)	Value Added Per Employee (Rs. 00,000)
1(Eligible Pre-Program)*1(Year<1996)	3,813	-3,794	0.378	-3.039	-3.812*
	(31,390)	(10,870)	(2,657)	(2.648)	(1.975)
1(Eligible Pre-Program)*1(Year>1996)	82,204	39,755*	13,776	49.25	10.36***
	(81,550)	(20,128)	(8,306)	(40.15)	(3.668)
Controls	Firm FE, NIC 2-digit X Year FE				
Sample Restriction	Only sectors with all or no eligible firms				
Observations	1,781	1,781	1,781	1,766	1,755
Mean of Dependent Variable	406,144	121,622	28,423	59.75	15.81

Notes: Standard errors clustered at the firm level in parentheses (*** p<0.01, ** p<0.05, * p<0.1). 1(Eligible Pre-Program)*1(Year=1996) is the omitted category. Value added per employee and sales per employee are trimmed at the 1st and 99th percentile.

Table A3: Including Three-Digit Sector by Year Fixed Effects

	(1)	(2)	(3)	(4)	(5)
	Sales (Rs.,00,000)	Value Added (Rs.,00,000)	Profits (Rs.,00,000)	Sales Per Employee (Rs. 00,000)	Value Added Per Employee (Rs. 00,000)
1(Eligible Pre-Program)*1(Year<1996)	-12,685	-7,788	-3,522	3.442	-1.322
	(8,442)	(4,803)	(2,445)	(6.843)	(1.523)
1(Eligible Pre-Program)*1(Year>1996)	104,337***	41,469**	9,019	53.21*	10.47
	(34,541)	(16,676)	(5,586)	(28.51)	(7.114)
Controls			Firm FE, NIC 3-digit X Year FE		
Observations	3,342	3,342	3,342	3,301	3,264
R-Squared	0.867	0.840	0.74	0.75	0.787
Mean of Dependent Variable	282,764	99628	18,090	58.56	17.47

Notes: Standard errors clustered at the firm level in parentheses (*** p<0.01, ** p<0.05, * p<0.1). 1(Eligible Pre-Program)*1(Year=1996) is the omitted category. Value added per employee and sales per employee are trimmed at the 1st and 99th percentile.

Table A4: Sample of Firms That Earned Positive Profits At Least Once Between 1992-2009

	(1)	(2)	(3)	(4)	(5)
	Sales (Rs.,00,000)	Value Added (Rs.,00,000)	Profits (Rs.,00,000)	Sales Per Employee (Rs. 00,000)	Value Added Per Employee (Rs. 00,000)
1(Eligible Pre-Program)*1(Year<1996)	-2,129	-4,929	-2,360	1.122	0.205
	(16,149)	(6,133)	(2,219)	(4.956)	(0.942)
1(Eligible Pre-Program)*1(Year>1996)	82,705*	42,091**	9,239*	36.75	3.521
	(48,092)	(16,497)	(5,555)	(27.44)	(2.674)
Controls	Firm FE, NIC 2-digit X Year FE				
Sample Restriction	Only Firms That Earned Positive Profits At Least Once Between 1992-2009				
Observations	3,064	3,064	3,064	3,023	3,000
R-Squared	0.863	0.831	0.727	0.683	0.747
Mean of Dependent Variable	308,175	108601	20,314	63.55	4.352

Notes: Standard errors clustered at the firm level in parentheses (*** p<0.01, ** p<0.05, * p<0.1). 1(Eligible Pre-Program)*1(Year=1996) is the omitted category. Value added per employee and sales per employee are trimmed at the 1st and 99th percentile.

Table A5: All Firms, Including Those That Began Reporting Data After 1992 or Stopped Before 2002

	(1)	(2)	(3)	(4)
	Sales (Rs.,00,000)	Value Added (Rs.,00,000)	Profits (Rs.,00,000)	Sales Per Employee (Rs. 00,000)
1(Eligible Pre-Program)*1(Year<1996)	-12,221	-7,073	-3,065*	1.701
	(16,894)	(4,837)	(1,637)	(4.980)
1(Eligible Pre-Program)*1(Year>1996)	79,725**	37,125***	8,601*	41.14
	(40,005)	(14,079)	(4,917)	(25.20)
Controls		Firm FE, NIC 2-digit X Year FE		
Observations	3,728	3,728	3,728	3,686
R-Squared	0.863	0.832	0.723	0.674
Mean of Dependent Variable	257,092	91545	16,447	54.56
	Value Added Per Employee (Rs. 00,000)	1(Entry)	1(Exit)	1(Entry or Exit)
1(Eligible Pre-Program)*1(Year<1996)	-2.709*	0.00440	0.0135	0.0179
	(1.453)	(0.00862)	(0.0142)	(0.0167)
1(Eligible Pre-Program)*1(Year>1996)	9.355*	0.00890	-0.00874	0.000157
	(5.289)	(0.00948)	(0.0171)	(0.0196)
Controls		Firm FE, NIC 2-digit X Year FE		
Observations	3,650	3,728	3,728	3,728
R-Squared	0.76	0.209	0.392	0.382
Mean of Dependent Variable	16.59	0.000805	0.02	0.02

Notes: Standard errors clustered at the firm level in parentheses (*** p<0.01, ** p<0.05, * p<0.1). 1(Eligible Pre-Program)*1(Year=1996) is the omitted category. Value added per employee and sales per employee are trimmed at the 1st and 99th percentile. Entry is a binary variable that takes the value 1 if the firm started reporting data that year (it is 0 for all firms in the first year of data). Exit is a binary variable that takes the value 1 if the firm stopped reporting data that year (it is 0 for all firms in the last year of data). 1(Entry or Exit) is a binary variable that takes the value 1 if the firm started or stopped reporting data that year.

Table A6: Impact on Government Ownership

	(1)	(2)
	Proportion of Government Equity (Excluding State Government Holdings)	Proportion of Government Equity (Including State Government Holdings)
1(Eligible Pre-Program)*1(Year<1996)	0.00740*	0.00697
	(0.00430)	(0.00423)
1(Eligible Pre-Program)*1(Year>1996)	-0.00900	-0.00764
	(0.00866)	(0.00938)
Controls	Firm FE, NIC 2-digit X Year FE	
Observations	2,871	2,871
R-Squared	0.855	0.887
Mean of Dependent Variable	0.91	0.93

Notes: Standard errors clustered at the firm level in parentheses (*** p<0.01, ** p<0.05, * p<0.1). 1(Eligible Pre-Program)*1(Year=1996) is the omitted category.

Table A7: Effects on Employment Composition

	(1)	(2)	(3)
	Number of Managerial and Supervisory Employees	Number of Non-Managerial and Supervisory Employees	Number of Non-Permanent Employees
1(Eligible Pre-Program)*1(Year<1996)	-47.58	-160.1	-183.5
	(137.1)	(196.1)	(234.4)
1(Eligible Pre-Program)*1(Year>1996)	239.0	1,516	271.3
	(283.2)	(1,107)	(349.8)
Controls		Firm FE, NIC 2-digit X Year FE	
Observations	2,872	2,872	2,685
R-Squared	0.845	0.959	0.379
Mean of Dependent Variable	1,853	7,105	543

Notes: Standard errors clustered at the firm level in parentheses (*** p<0.01, ** p<0.05, * p<0.1). 1(Eligible Pre-Program)*1(Year=1996) is the omitted category.

Table A8: Generalized Difference-in-Difference Effects by Grade of Autonomy

	(1)	(2)	(3)	(4)	(5)
Panel A	Sales (Rs. 00,000)	Value Added (Rs. 00,000)	Net Profits (Rs. 00,000)	Sales Per Employee (Rs. 00,000)	Value Added Per Employee (Rs. 00,000)
Grade of Autonomy*1(Year<Year Before Autonomy Received)	-17,416	-6,817	-4,810	3.359	-0.879
	(19,771)	(7,923)	(4,407)	(7.364)	(1.813)
Grade of Autonomy*1(Year>Year Before Autonomy Received)	248,566***	99,980***	31,150***	33.50*	11.20**
	(60,448)	(29,212)	(10,293)	(17.99)	(5.034)
Controls	Firm FE, NIC 2-digit X Year FE, 1(Year<Year Before Autonomy Received), 1(Year>Year Before Autonomy Received)				
Sample Restriction	None				
Observations	3,342	3,342	3,342	3,301	3,264
R-Squared	0.867	0.841	0.74	0.68	0.773
Mean of Dependent Variable	282,764	99,628	18,090	58.56	17.47
Panel B	Sales (Rs. 00,000)	Value Added (Rs. 00,000)	Net Profits (Rs. 00,000)	Sales Per Employee (Rs. 00,000)	Value Added Per Employee (Rs. 00,000)
Grade of Autonomy*1(Year<Year Before Autonomy Received)	-130,172	-3,048	-12,854	67.96	34.53
	(129,096)	(36,458)	(13,660)	(59.70)	(24.01)
Grade of Autonomy*1(Year>Year Before Autonomy Received)	1236000***	388,125***	103,070***	30.13	27.33
	(447,063)	(128,828)	(37,327)	(81.90)	(22.08)
Controls	Firm FE, NIC 2-digit X Year FE, 1(Year<Year Before Autonomy Received), 1(Year>Year Before Autonomy Received)				
Sample Restriction	Treatment Firms Only				
Observations	1,303	1,303	1,303	1,291	1,277
R-Squared	0.818	0.871	0.818	0.781	0.857
Mean of Dependent Variable	50,643	218,157	50,643	103	31.16

Notes: Standard errors clustered at the firm level in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Grade of Autonomy is a categorical variable that takes the values0, 1,2, or 3, with 0 for control firms, 1 for firms with the least level of autonomy (Mini-ratna category-II) and 3 for firms with the highest level of autonomy (Navratna). Grade of Autonomy*1(Year=Year Before Autonomy Received) and 1(Year=Year Before Autonomy Received) are the omitted categories. Sales per employee and profit per employee are trimmed at the 1st and 99th percentile.

Table A9: Robustness to Dropping Outliers

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A	Sales (Rs. 00,000)	Value Added (Rs. 00,000)	Net Profit (Rs. 00,000)	Sales (Rs. 00,000)	Value Added (Rs. 00,000)	Net Profit (Rs. 00,000)
1(Eligible Pre-Program)*1(Year<1996)	-4,140	-5,174	-2,417	-4,144	-5,169	-2,427
	(14,350)	(5,553)	(2,041)	(14,331)	(5,553)	(2,039)
1(Eligible Pre-Program)*1(Year>1996)	97,829***	33,837***	7,560*	99,570***	31,871***	7,543*
	(32,510)	(9,868)	(4,550)	(26,494)	(9,138)	(4,413)
Controls	Firm FE					
	NIC 2-digit X Year FE					
Outlier Check	Remove values with z-score greater than 3 or less than -3			Remove values with z-score greater than 2.5 or less than -2.5		
Observations	3,296	3,295	3,295	3,286	3,289	3,284
R-Squared	0.812	0.838	0.732	0.833	0.841	0.735
Mean of Dependent Variable	150678	63107	9020	137967	61170	8250
Panel B	Sales (Rs. 00,000)	Value Added (Rs. 00,000)	Net Profit (Rs. 00,000)	Sales (Rs. 00,000)	Value Added (Rs. 00,000)	Net Profit (Rs. 00,000)
1(Eligible Pre-Program)*1(Year<1996)	-4,134	-8,111**	-2,340	-4,122	-5,376	-2,341
	(14,363)	(4,101)	(2,046)	(14,349)	(5,448)	(2,034)
1(Eligible Pre-Program)*1(Year>1996)	95,047**	37,242***	7,897*	90,022**	40,711***	9,436**
	(39,827)	(11,756)	(4,444)	(43,044)	(13,498)	(4,682)
Controls	Firm FE					
	NIC 2-digit X Year FE					
Outlier Check	Trim 1st and 99th percentile			Winsorize 1st and 99th percentile		
Observations	3,309	3,276	3,276	3,342	3,342	3,342
R-Squared	0.807	0.848	0.733	0.875	0.876	0.756
Mean of Dependent Variable	169329	70412	11629	220434	85560	14873

Notes: Standard errors clustered at the firm level in parentheses (*** p<0.01, ** p<0.05, * p<0.1). 1(Eligible Pre-Program)*1(Year=1996) is the omitted category.

Appendix B: Details of the Autonomy Program Benefits Between 1997-2009

1. *Capital Expenditure*: Between 1997-2005, Mini-Ratna category-I enterprises could undertake capital expenditure on new projects, modernization or purchase of equipment without government approval up to Rs. 3 billion, or equal to their net worth, whichever was lower. This expenditure was for each project, not each year (so a firm could undertake multiple projects each year). For Mini-Ratna category-II enterprises, this amount was Rs. 1.5 billion, or up to 50% of their net worth. In 2005, these amounts were revised upward. Between 2005-2009, Mini-Ratna category-I enterprises could spend up to Rs. 5 billion per project, or up to their net worth, whichever was lower. Mini-Ratna category-II enterprises could spend up to Rs. 2.5 billion per project, or up to 50% of their net worth, whichever was lower. Throughout this period, Navratna enterprises could undertake capital expenditure without any ceiling. They could also (unlike the Mini-ratna enterprises) establish offices abroad without the government's permission.

2. *Labor Restructuring*: All firms with autonomy could implement initiatives around personnel training, and voluntary or compulsory retirement schemes to restructure their labor force. Navratna enteprises could additionally create and fill vacancies in the firm without any government involvement, up to the level of the board of directors (not including the directors themselves).

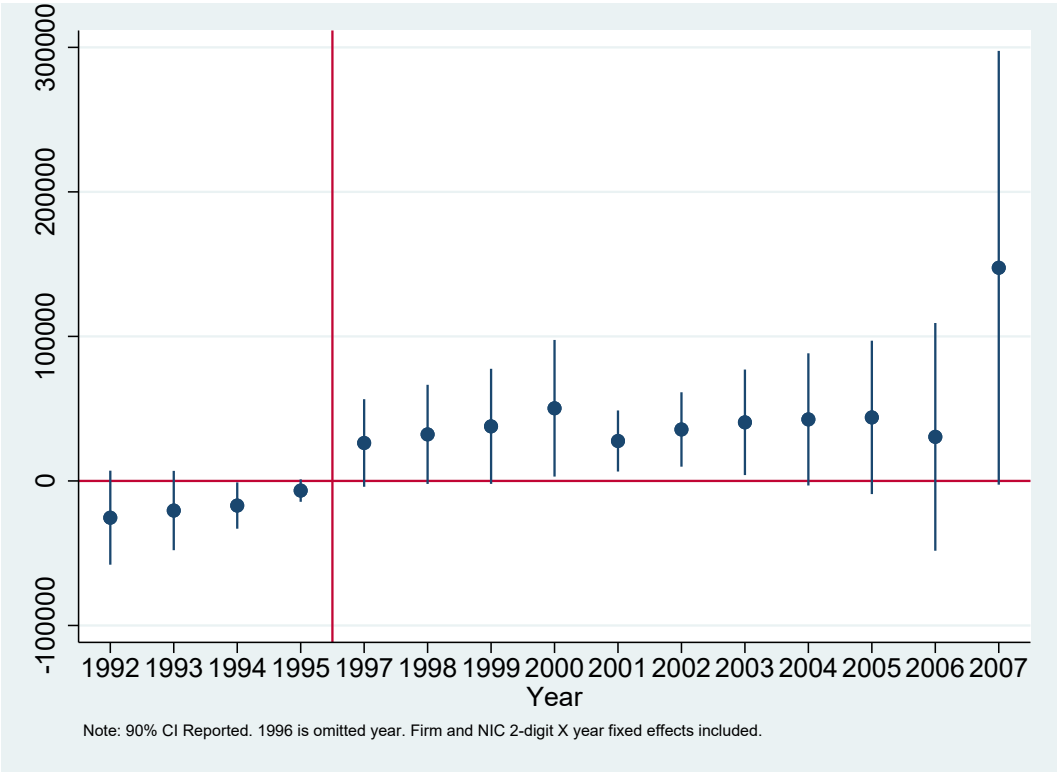
3. *Joint Ventures and Subsidiaries*: Between 1997-2005, Mini-Ratna category-I enterprises could establish joint ventures and subsidiaries (in India) as long as the equity investment of the firm was capped at Rs. 1 billion or 5% of the firms net worth , whichever was lower. For Mini-Ratna category-II enterprises, this amount was Rs. 0.5 billion, or up to 5% of the firms net worth per project, whichever was lower. For Navratna enterprises, this amount was Rs. 2 billion, or up to 5% of the firms net worth per project, whichever was lower. The total equity investment could not exceed 15% of the firms net worth across all joint ventures or subsidiaries in any firm with autonomy (regardless of the type of autonomy).

   In 2005, the cap on the value of these projects was increased - Mini-Ratna category-I enterprises could now invest equity up to Rs. 5 billion or 15% of the firms net worth per project, Mini-Ratna category-II enterprises could now invest equity up to Rs. 2.5 billion or 15% of the firms net worth per project, and Navratna enterprises could now invest equity up to Rs. 10 billion or 15% of the firms net worth per project. Across all types of autonomy, total investment in such ventures was capped at 30% of the firms net worth. In 2005, all firms with autonomy were also allowed to enter into mergers and acquisitions subject to the same value caps, and subject to these activties being in the SOE's core area of functioning.

4. All firms with autonomy were encouraged into strategic alliances such as technology joint ventures, though there were no specific guidelines around this.

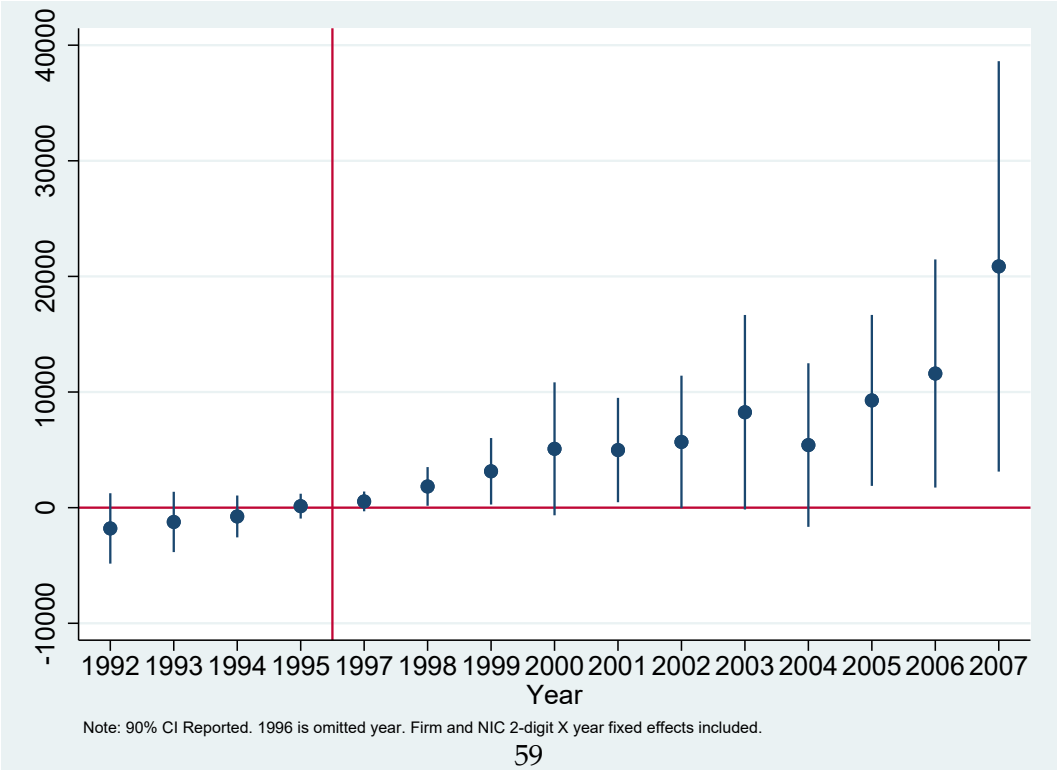
Online Appendix C: Additional Event Studies

Figure C1: Capital Assets (Rs. 00,000)



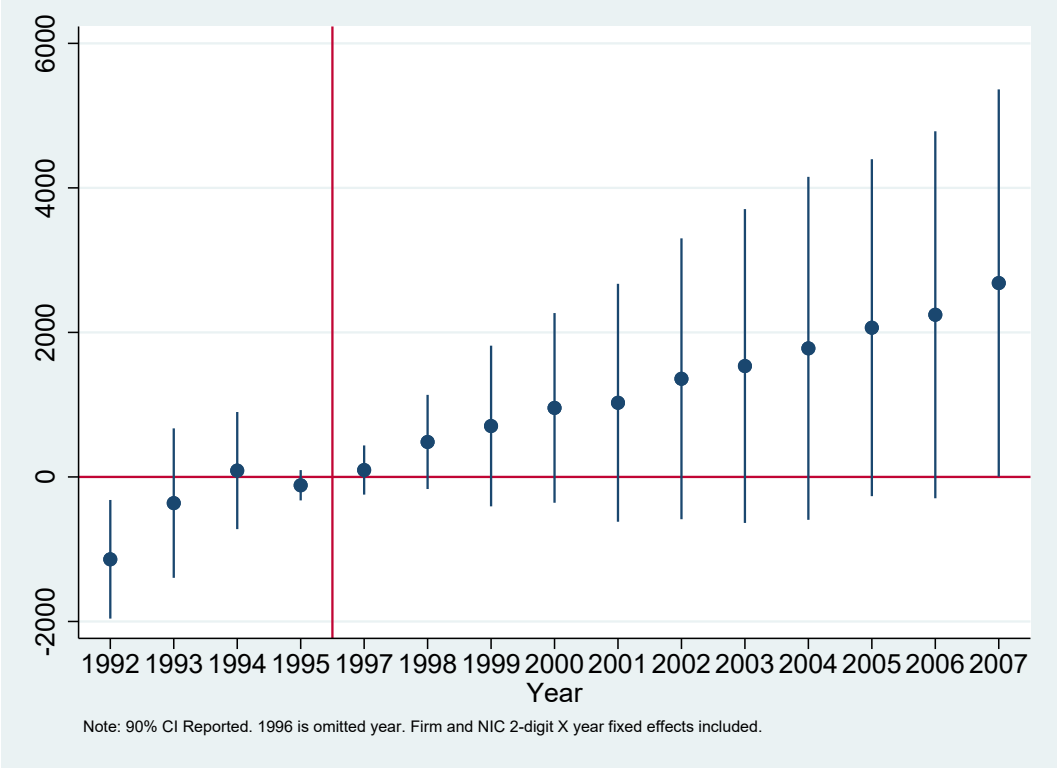
This figure plots the interaction coefficients of the pre-program eligibility dummy variable with year dummy variables. The interaction of pre-program eligibility with the 1996 year dummy variable is omitted.

Figure C2: Wage Bill (Rs. 00,000)



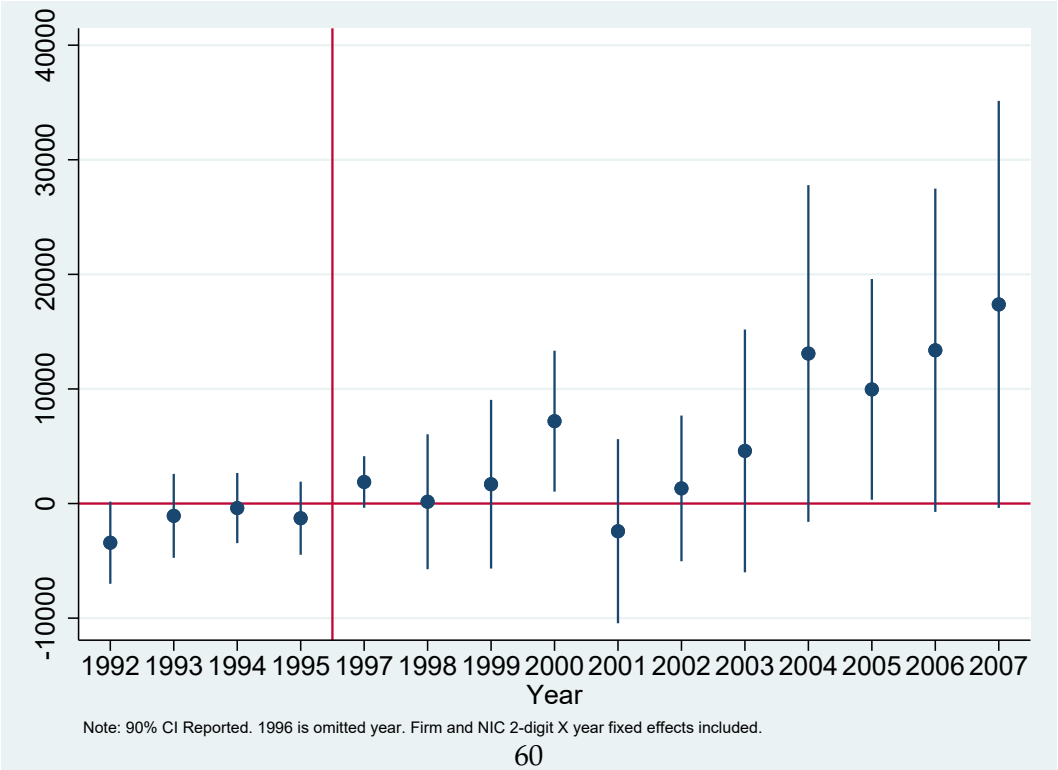
This figure plots the interaction coefficients of the pre-program eligibility dummy variable with year dummy variables. The interaction of pre-program eligibility with the 1996 year dummy variable is omitted.

Figure C3: Number of Employees



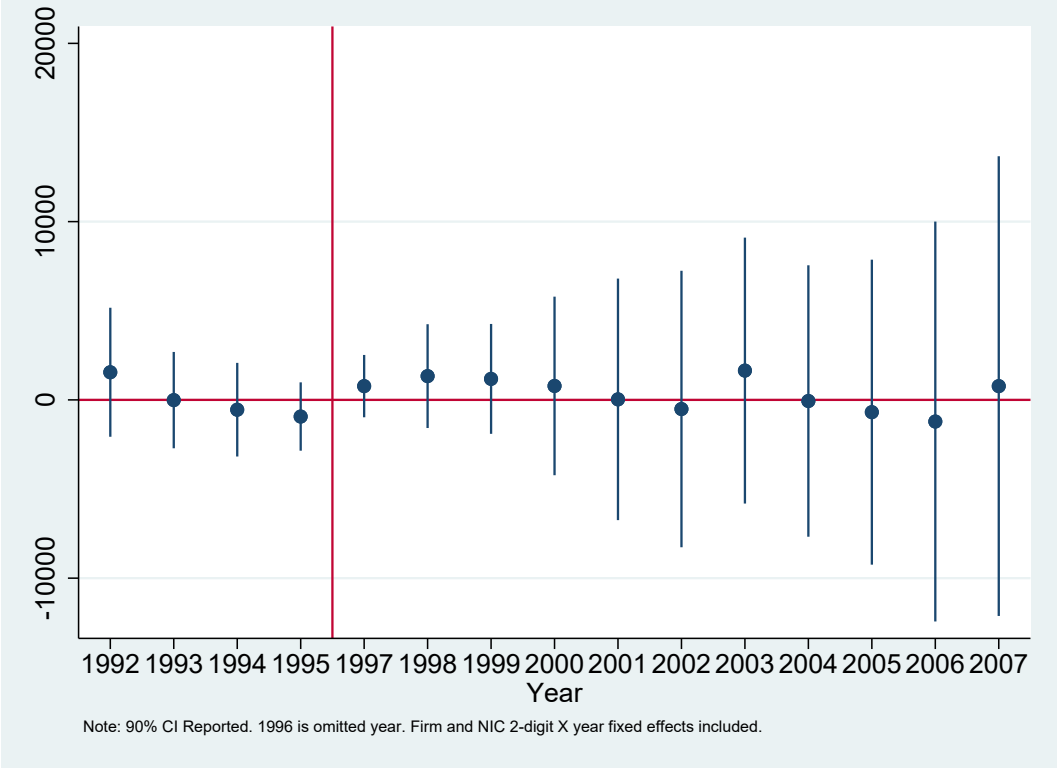
This figure plots the interaction coefficients of the pre-program eligibility dummy variable with year dummy variables. The interaction of pre-program eligibility with the 1996 year dummy variable is omitted.

Figure C4: Retained Profits (Rs. 00,000)



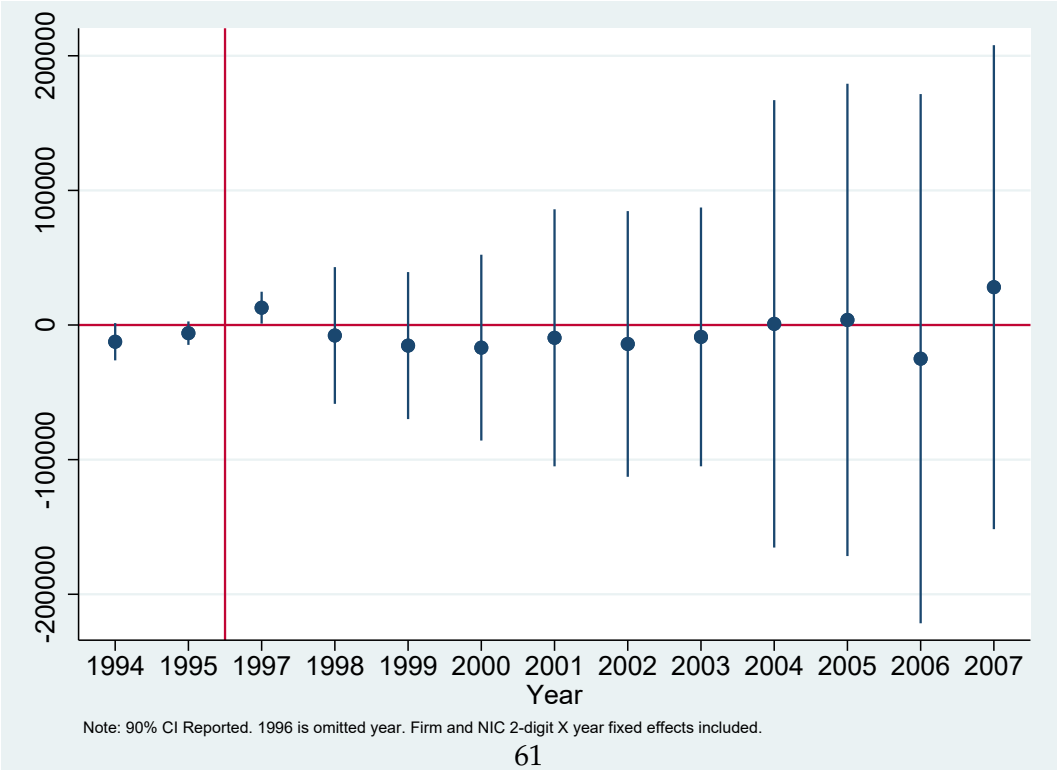
This figure plots the interaction coefficients of the pre-program eligibility dummy variable with year dummy variables. The interaction of pre-program eligibility with the 1996 year dummy variable is omitted.

Figure C5: Interest Payments (Rs. 00,000)



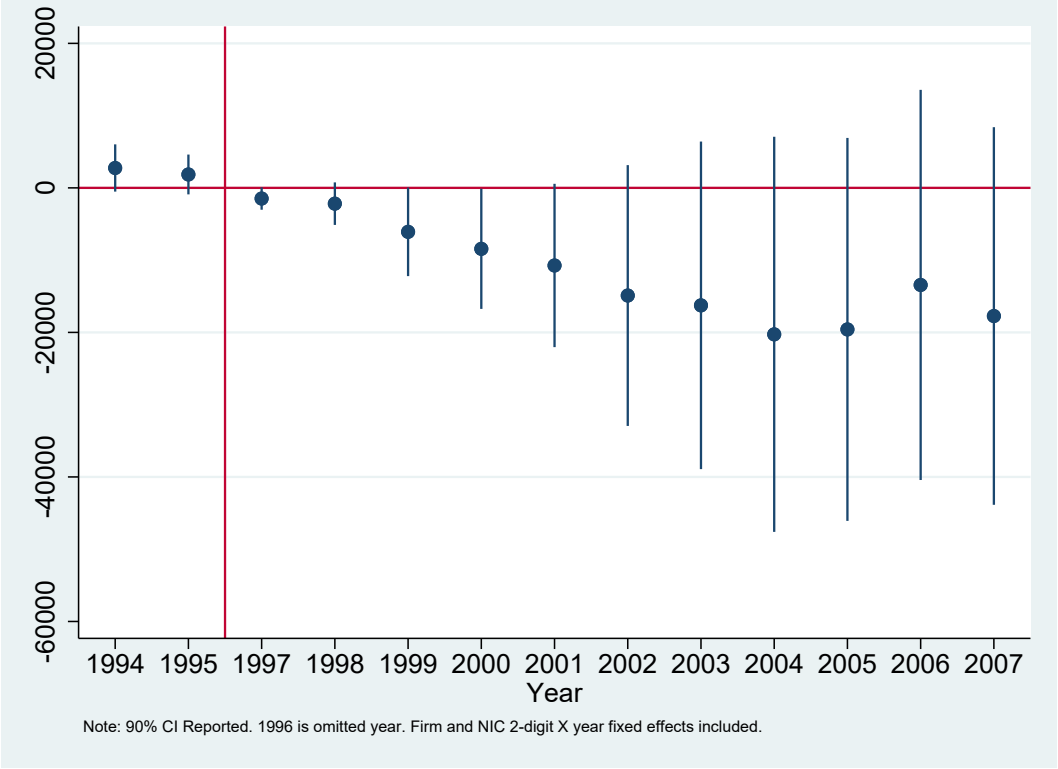
This figure plots the interaction coefficients of the pre-program eligibility dummy variable with year dummy variables. The interaction of pre-program eligibility with the 1996 year dummy variable is omitted.

Figure C6: Total Loans (Rs. 00,000)



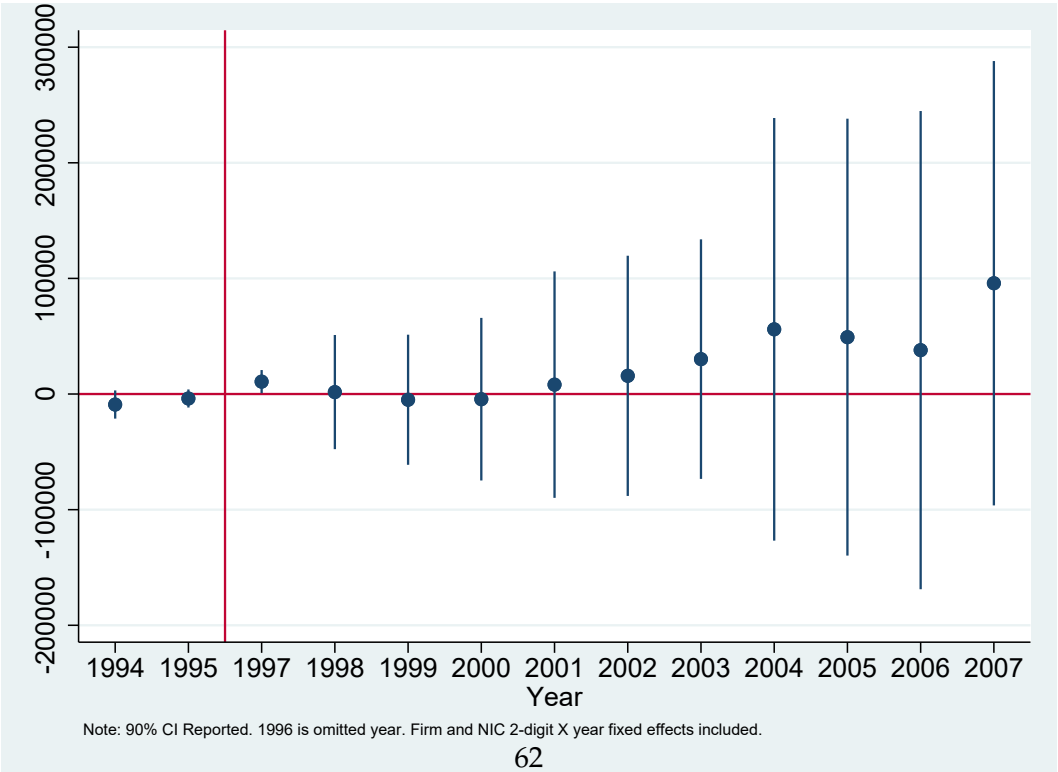
This figure plots the interaction coefficients of the pre-program eligibility dummy variable with year dummy variables. The interaction of pre-program eligibility with the 1996 year dummy variable is omitted.

Figure C7: Government Borrowing (Rs. 00,000)



This figure plots the interaction coefficients of the pre-program eligibility dummy variable with year dummy variables. The interaction of pre-program eligibility with the 1996 year dummy variable is omitted.

Figure C8: Non-Government Borrowing (Rs. 00,000)



This figure plots the interaction coefficients of the pre-program eligibility dummy variable with year dummy variables. The interaction of pre-program eligibility with the 1996 year dummy variable is omitted.

Appendix D: Online Data Appendix

Variable	Years Available	Frequency	Source
Net Profit	1992-2009	Firm-year	Financial Statements, Dept. of Public Enterprises (DPE)
Sales	1992-2009	Firm-year	Financial Statements, DPE
Value Added	1992-2009	Firm-year	Financial Statements, DPE
Sales Per Employee	1992-2009	Firm-year	Financial Statements, DPE
Value Added Per Employee	1992-2009	Firm-year	Financial Statements, DPE
Capital Assets	1992-2009	Firm-year	Financial Statements, DPE
Wage Bill	1992-2009	Firm-year	Financial Statements, DPE
Number of (Permanent) Employees	1992-2009	Firm-year	Financial Statements, DPE
Retained Profit	1992-2009	Firm-year	Financial Statements, DPE
Dividends	1992-2009	Firm-year	Financial Statements, DPE
Interest Payments	1992-2009	Firm-year	Financial Statements, DPE
Total Loans	1992-2009	Firm-year	Financial Statements, DPE
Government Loans	1994-2009	Firm-year	DPE Annual Report
Non-Government Loans	1994-2009	Firm-year	DPE Annual Report
Number of Managerial and Supervisory Employees	1994-2009	Firm-year	DPE Annual Report
Number of non-Managerial and Supervisory Employees	1994-2009	Firm-year	DPE Annual Report
No. of Non-Permanent Workers	1994-2009	Firm-year	DPE Annual Report
Pre-Program Eligibility	1992-2009	Firm	Constructed from Financial Statements
Autonomy Status	1992-2009	Firm-year	DPE Annual Report
Government Equity Holdings	1994-2009	Firm-year	DPE Annual Report

Variable	Years Available	Frequency	Source
Participation in Joint Venture/Subsidiary	1992-2009	Firm-year	Center for Monitoring the Indian Economy/ DPE Annual Report
Sector Codes	1992-2009	Firm-year	CMIE
Private Sector Profit	1992-2009	Firm-year	CMIE
Private Sector Value Added	1992-2009	Firm-year	CMIE
Private Sector Sales	1992-2009	Firm	CMIE
SOE Board of Director Names	2003-2010	Director-level	CMIE
Private Firm Board of Director Names	2003-2010	Director-level	CMIE
SOE State-level Employment	1999-2009	Firm-state-year	DPE Annual Report
Compensation Schedule	2005	Firm	DPE Annual Report
Costs of Production	1992-2009	Firm-year	Financial Statements, DPE
Standard Deviation of Profits, Sales, and Value Added	1996, 2001, 2006	Firm-year	Calculated from Financial Statements, DPE
State Election Timing	1992-2009	State-Year	Election Commission of India