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THE IMPACTS OF MANAGERIAL AUTONOMY ON FIRM OUTCOMES

NAMRATA KALA
Sloan School of Management, MIT

The allocation of decision-making power is a critical choice that organizations make to mitigate agency problems and information frictions. This paper investigates the role of delegation for organizations where the agency problem is both pervasive and has potentially high welfare consequences: state-owned enterprises (SOEs). I use a natural experiment in India to uncover the causal effects of granting SOE managers more autonomy over strategic decisions. Managers meaningfully exercise this autonomy, which results in greater value added, but also a reduced emphasis on outcomes valued by the government, such as a reduction in worker amenities (employee housing), and an increase in markups. Returns to autonomy are higher for firms with higher baseline incentive conflict.

KEYWORDS: Delegation, managerial autonomy, state-owned enterprise, productivity, decision rights, India.

1. INTRODUCTION

THE ALLOCATION OF DECISION-MAKING power is a critical choice that organizations make to mitigate agency problems and information frictions.[1] For instance, a canonical trade-off hypothesized by the literature is that giving managers more authority may align the best information available to make that decision with the power to decide, but comes at the cost of the managers maximizing their own objectives, which might be different than the organization's. Furthermore, delegation is ultimately at the discretion of the authority who is conducting the delegation, since it can be reversed or interferred with (Baker, Gibbons, and Murphy (1999)), rendering the ultimate impact of allocating formal delegation rights ambiguous.

This paper investigates the role of delegation for organizations where the agency problem is both pervasive and has potentially high welfare consequences: state-owned enterprises (SOEs). SOEs are among the largest firms in the world, accounting for nearly a quarter of the Fortune 500 firms, with assets worth 50% of global GDP (IMF (2020)). Furthermore, their significance for the global economy, both in terms of number and size, has increased over the last few decades. At the same time, their objective function is a combination of what private firms seek to do; namely, be profitable, as well as fulfill other government objectives, such as creating well-paying jobs and generating revenue for the government. The emphasis placed on these different objectives, and the consequent impacts on SOE outcomes, then may depend on who in the hierarchy—a politician or a professional manager—is making decisions for these firms.

Namrata Kala: kala@mit.edu

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[1]See, for example, the recent survey by Garicano and Rayo (2016).

I combine newly collected data on the universe of federally owned Indian SOEs with a natural experiment to estimate the impacts of managerial autonomy on managerial decisions and firm outcomes over an 18-year period. Specifically, I study an earned autonomy program, which gave the board of directors (henceforth referred to as managers) of profitable SOEs more autonomy over strategic decisions such as capital expansion and hiring. Each SOE in India is housed in a particular ministry. Before the program was introduced, managers' decisions required approval from 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. I show that autonomy led to greater value added, but also increased markups and lowered investment in employee housing (an amenity valued by the government but not managers). Autonomy has greater effects for SOEs in which there are bigger incentive conflicts. I proxy for this conflict by whether, prior to autonomy, dividends are always paid out from profits as preferred by the government (as opposed to profits being retained and reinvested, which is preferred by the managers). These findings are consistent with a simple theoretical framework that identifies the key agency problem in my setting, as well as the impact of autonomy.

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 autonomy 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 3 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 and event study framework, I then test whether SOEs that were *eligible* pre-program performed differentially after 1996 relative to SOEs that were not. Using pre-program eligibility as a proxy for treatment implies that my results are not driven by selection into autonomy by the firms, or by the government's choice to award autonomy. Controlling for sectoral trends ensure that the results are not driven by firms in faster growing sectors also being more likely to be eligible pre-program, and event study estimates show this is not the case, conditional on these fixed effects. Pre-program eligibility has a strong positive effect on the receipt of autonomy, indicating that it is a suitable proxy for treatment.

I find that earned autonomy resulted in greater value added, but no changes to TFPR (a value added-based measure of total factor productivity). 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. Managers who receive autonomy exercise it for most of these decisions: treated firms spend more on both capital and labor. These effects persist for up to 12 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.

I rule out that the effects are driven by strategic reporting of profits, and are also robust to considering only SOEs that reported positive profits at least once pre-program before 1997. I also show that the results are robust to using alternative specifications, including generalized differences in differences using eligibility in any year rather than pre-program eligibility as a proxy for treatment. Finally, I show that government ownership and managerial turnover does not change on average during the sample period. This indicates that

the results are neither driven by privatizing the firms that received autonomy, nor by autonomy differentially bringing in new managers. Rather, the results are consistent with existing managers changing their behavior in response to the autonomy program.

This paper builds on three literatures. The first is the literature on the role of the allocation of decision-making authority on organizational outcomes. A large theoretical literature (following Aghion and Tirole (1997)) examines the differences in firm outcomes when managers make recommendations that need approval (real authority) as opposed to having actual (formal) decision-making authority.[2] Within this literature, the main tradeoff in my setting is most closely modeled by Dessein (2002) and my theoretical framework builds on this paper (the details are in Section 3). There are two empirical papers that are most closely related. Bandiera, Best, Khan, and Prat (2021) conduct a randomized control trial in Pakistan that increased procurement officers' autonomy. They find that greater autonomy reduces procurement prices with no quality reductions. The context and agency problem in their study is different from mine; this is reflected in the results as I show that autonomy leads to not only to higher value added, it also changes other outcomes valued by the government such as increased markups and reduced investment in employee housing.[3] Aghion, Bloom, Lucking, Sadun, and Van Reenen (2021) use a panel data set for private firms in OECD countries and show that in sectors that were hit harder by the financial crisis, decentralized firms outperformed their centralized rivals.[4]

Second, this paper contributes to the literature on SOE performance. It is well-established that government ownership is correlated with lower returns to capital and profitability (Megginson and Netter (2001), Dollar and Wei (2007)).[5] It is hence both natural and policy-relevant to ask whether, instead of changing ownership, firms' outcomes can be improved by restructuring how decisions are made within the organization.[6] Specifically, there has 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), Frederick (2011)), but there is little evidence of whether or how such autonomy affects SOE outcomes.

Third, this paper relates to the largely qualitative literature on earned autonomy. Across the world, in diverse settings, giving better performing public sector units more operational autonomy has been practiced for decades, such as the National Health Service in the UK (Hoque, Davis, and Humphreys (2004), Mannion, Goddard, and Bate (2007)), schools in the Netherlands (Thoonen, Sleegers, Oort, and Peetsma (2012)), and SOEs

[2]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).

[3]In Bandiera et al. (2021), there is a double agency problem where procurement officers and monitors are the two agents, and the government is the principal. Agents in my setting are upper-level management facing a more standard single principal-agent problem. Here, the tradeoff is that autonomy leads to more informed decisions but these may not conform with the preferences of the governing ministry.

[4]A related but distinct literature examines what *determines* firms' decentralization decisions. Prior work has identified the importance of local information (Huang, Li, Ma, and Xu (2017), Acemoglu, Aghion, Lelarge, Van Reenen, and Zilibotti (2007), coordination (Dessein, Lo, and Minami (2019)), trust (Bloom, Sadun, and Van Reenen (2012)), firm size (McElheran (2014)), how valuable the input is (Alfaro et al. (2024)), and product market competition (Bloom, Sadun, and Van Reenen (2010)).

[5]For the effects of changes in ownership on SOE profitability and productivity, see also Bartel and Harrison (1999), Hsieh and Song (2015), Berkowitz, Ma, and Nishioka (2017), Gupta (2005), Barberis, Boycko, Shleifer, and Tsukanova (1996), Estrin and Pelletier (2018).

[6]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.

in India. However, observed outcomes of earned autonomy programs could be purely driven by selection into who earns autonomy. I show that earned autonomy has tangible treatment effects on outcomes.

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.[7] 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 to 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 change the financial incentives for either managers or workers (IMF (2005)).

Only profitable SOEs were eligible for autonomy. There were obvious downsides to granting blanket autonomy as SOEs faced a soft budget constraint with the government, and the government had to bail out the SOE if it made bad investments. For instance, in 2010, the government announced a $170 million bailout for the government owned airline to be disbursed over 10 years. Thus, the risk in letting loss-making SOEs decide which projects to undertake was considerable, relative to profit-making SOEs who had demonstrated their ability to choose profitable projects.

If an SOE fulfilled certain criteria, their board of directors (referred 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, lowest 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 addition to the above Category-II criteria, also earned a profit of at least ₹300 million in one of the 3 years (see Table I for full eligibility details). 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 three independent directors on their board before exercising autonomy. In practice, several of these

TABLE I

ELIGIBILITY FOR MINI-RATNA STATUS.

	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 ₹300 million in 1 year		✓

[7]Only about 3–4 SOEs were actually privatized, that is, a majority of the government's equity was sold to the private sector.

board seats remain vacant for long periods of time: for instance, in the data from the Center of Monitoring Economy's Prowess database (CMIE (2021)), which includes information on the board of directors of private and public firms, in 2003, 6 years after the program had begun, only 11% of SOEs reporting data reported having an independent director. Once status was granted, managers could exercise autonomy over the following decisions:

- Capital Expenditure: Managers could undertake capital expenditures (upgrading or purchasing new capital) up to a limit which was an increasing function of the firm's net worth. These expenditures were to be financed out of retained earnings and commercial borrowing; the latter took the form of debt, as SOEs could not sell equity.
- Labor training and retirement schemes: Managers 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 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.
- 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, managers were only required to notify the government. Autonomy was an absorbing state, and once earned, the firm did not have to continue to be profitable to retain it.[8] 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 government approval at higher levels. The full details of the program, including benefits conferred on firms with different types of autonomy, can be found in Supplemental Appendix C (Kala (2024a)).

3. THEORETICAL FRAMEWORK

This section sets out a theoretical framework that provides context for the empirical results by explaining why and how autonomy impacts managerial decisions, as well as the key agency problem in my setting. In the model, prior to autonomy, the manager can recommend decisions that need government approval; post autonomy, the manager can make these decisions unilaterally. This is consistent with the setting and the policy I study, where pre-autonomy the manager had to ask for permission from the committee, and the government would refuse approval for decisions they did not agree with. Post-autonomy the managers had the authority to make these decisions for themselves. The model is inspired by Dessein (2002), with two main differences. The first is that I consider qualitatively different objective functions for both players that are tailored to this setting, allowing me to highlight the relevant agency problem in this context. The second is that I study *how* the decisions of the manager change as a result of autonomy; conversely, Dessein (2002) derives conditions under which autonomy benefits the principal (in this case, the government).

[8] A firm could give up autonomy, but would have reearn it if it wanted to exercise the benefits of autonomy again—in the data, I never see a firm give up autonomy. One firm had its status revoked by the government midway, and I assign the firm as treated through the entire period of the analysis.

There are two players: an informed manager of a firm and the uninformed government. The firm has resources $\overline{w} > 0$ that it can use toward inputs or for other causes such as employee perks, paying dividends, etc. The manager's preference U_M depends on the quantities of capital $k \geq 0$ and labor $\ell \geq 0$, and is given by

$$U_M(k, \ell) := Ak^{\alpha}\ell^{\beta} + \lambda_M\big[\overline{w} - (c_k k + c_\ell \ell)\big]. \tag{1}$$

Here, $\alpha, \beta > 0$, $\alpha + \beta < 1$ (i.e., the Cobb–Douglas production has decreasing returns to scale) and $c_k, c_\ell > 0$ are the costs of capital, labor, respectively. $\lambda_M > 1$ captures the marginal value that the manager receives from allocating the firm's resources toward nonproduction causes such as using them for other government projects, making dividend payouts or providing nonproductive benefits to employees (such as housing). It is assumed to be greater than 1 because this obviates the need to separately include the cost of the inputs. A is the manager's private information and it captures both their knowledge about total factor productivity and the market conditions; this should be clear from the above formulation that interprets $Ak^{\alpha}\ell^{\beta}$ as the revenue of the firm. A can take one of two values so $A \in \{\underline{A}, \overline{A}\}$ where $0 < \underline{A} < \overline{A}$; the prior probability of $\overline{A}$ is $\overline{p}$ (and so the probability of $\underline{A}$ is $1 - \overline{p}$).

The government's utility

$$Ak^{\alpha}\ell^{\beta} + \lambda_G\big[\overline{w} - (c_k k + c_\ell \ell)\big] \tag{2}$$

looks identical to that of the manager's but with the difference that $\lambda_G > \lambda_M$, that is, that the government assigns a higher priority toward noninput spending. As an example, the government prefers to use dividends from the firm to spend on other projects outside the SOE, while the manager would prefer to reinvest profits in the firm. This difference in preferences is the key friction in the model.

I now describe the game that captures the interaction between the manager and the government *prior* to the granting of autonomy. Recall that, in this case, the manager had to make recommendations that are approved by the government. In the model, I capture this via a cheap talk game: the manager reports a messages to the government that contains information about A, and upon receipt of this message, the government forms a belief about A and chooses the levels of the inputs. Note that this is equivalent to the manager recommending how to allocate resources and the government accepting/rejecting this recommendation. This is because any recommendation from the manager would signal information about A and the projects that do not maximize the government's utility (evaluated with respect to their posterior belief about A) could always be rejected.

The following makes the timing of the pre-autonomy cheap talk game explicit.

- The manager sends a message $m \in \mathcal{M}$ to the government where the message space $\mathcal{M}$ consists of a finite set of messages.
- The government forms a posterior belief $p(m) \in [0, 1]$ that is the probability that $A = \overline{A}$.
- The government chooses the inputs $k, \ell \geq 0$.

The strategy $\sigma : \{\underline{A}, \overline{A}\} \to \Delta(\mathcal{M})$ of the manager assigns a probability distribution $\sigma(A)$ over the set of messages as a function of her private information A. The strategy of the government determines the level of capital and labor in response to each message $m \in \mathcal{M}$. The solution concept we employ is perfect Bayesian equilibrium (PBE). This requires that the strategies of both players are mutual best responses and that the government's beliefs for all on path messages (those chosen with positive probability by the

manager) are derived by Bayes' rule. I do not need to impose a more stringent refinement in this simple setting because the statement of my result (which refers to all PBE) is starker with a weaker solution concept.

Post autonomy, inputs are determined by a simple decision problem for the manager because she no longer needs governmental approval for production decisions. For each realized value of A, the manager solves

$$\max_{k,\ell\geq 0}\{Ak^{\alpha}\ell^{\beta}+\lambda_M[\overline{w}-(c_k k+c_\ell \ell)]\} \quad \text{subject to } c_k k+c_\ell \ell\leq \overline{w}. \tag{3}$$

Since the objective function is quasilinear, I will make an assumption to ensure the solution is interior. It suffices to assume that the firm has enough resources $\overline{w}$ to guarantee that (3) has an interior solution for $A=\overline{A}$.[9] This assumption is imposed in what follows.

In order to state the result, I need to define one final term. Suppose pre-autonomy, the equilibrium reporting strategy for the manager is $\hat{\sigma}$ and the level of inputs chosen by the government are $\hat{k}(m)$, $\hat{\ell}(m)$ for every message $m\in\mathcal{M}$. Conversely, post-autonomy, the manager chooses $k(A)$, $\ell(A)$ for each $A\in\{\underline{A},\overline{A}\}$. The value of autonomy is given by

$$\begin{aligned}&\overline{p}\big(U_M\big(k(\overline{A}),\ell(\overline{A})\big)-\mathbb{E}_{\sigma(\overline{A})}\big[U_M\big(\hat{k}(m),\hat{\ell}(m)\big)\big]\big)\\&\quad+(1-\overline{p})\big(U_M\big(k(\underline{A}),\ell(\underline{A})\big)-\mathbb{E}_{\sigma(\underline{A})}\big[U_M\big(\hat{k}(m),\hat{\ell}(m)\big)\big]\big)\end{aligned} \tag{4}$$

in which the expectation is taken with respect to the distribution over messages induced by the manager's reporting strategy. This is simply the difference between the manager's highest utility post autonomy and the utility she gets from a given pre-autonomy equilibrium.

The main theoretical insight from this section is that autonomy leads to higher average input usage and profits (which are given by $Ak^{\alpha}\ell^{\beta}-c_k k-c_\ell \ell$). Moreover, the value of autonomy is increasing in λ_G, which captures the extent of the difference between the government's preferences and those of the manager.

CLAIM 1: *The average expenditure on both inputs* (*capital and labor*) *and average profits are strictly higher post-autonomy than in any PBE of the pre-autonomy cheap talk game*.

Moreover, if $\lambda_G>\frac{\overline{A}}{\underline{A}}\lambda_M$, *then there is a unique PBE of the cheap talk game* (*the babbling equilibrium*) *and the value of autonomy to the manager is strictly increasing in* λ_G.

PROOF: Presented in Appendix B. *Q.E.D.*

Note that the second part of the claim implies that, as the government's value from utilizing resources outside the SOE increases, the value of autonomy to the firm increases. I use the baseline level of differences in the frequency of dividend payouts as a proxy for this parameter; this allows me to test whether autonomy has larger effects for firms, which were having their profits paid out as dividends to a greater degree.

Finally, observe that Claim 1 does not hinge on there being two inputs. For instance, we could have defined revenue with an additional input as $Ak^{\alpha}\ell^{\beta}r^{\gamma}$ (with $\alpha+\beta+\gamma<1$) where r is the quantity of raw materials employed and autonomy still leads to strictly

[9] In terms of the model parameters, this assumption states that $\overline{w}>\left(\frac{\overline{A}(\alpha+\beta)\gamma}{\lambda_M}\right)^{\frac{1}{1-(\alpha+\beta)}}$ where γ is defined in equation (8) in Supplemental Appendix B.

higher average expenditure on inputs and average profits. Moreover, we could define a measure of value added as revenue less the cost of raw materials ($Ak^{\alpha}\ell^{\beta}r^{\gamma} - c_r r$ where $c_r > 0$ is the unit cost of raw materials) and (following similar arguments to the proof of the claim) it is easy to show that autonomy also results in strictly higher average value added.

4. DATA

The paper combines data from several volumes of the Public Enterprise Survey Reports with existing data sources. 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 other outcomes, such as investments in employee housing. I was able to access these volumes from 1994 to 2009. These reports also contain a subset of the data from the previous 2 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. *Financial Statements for SOEs*

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. I use value added as a primary outcome measure, which I construct by subtracting expenditures on raw material, power, and fuel, from sales.

The statements also include information on capital assets (the sum of fixed assets and other long-term investments) and the wage bill. I also digitized the information on total loans, as well as interest payments.

4.2. *Other Outcomes*

I also use additional (nonfinancial) statements included in these reports. These include information on the autonomy status of each SOE since the beginning of the program in 1997, as well as the category (Mini-Ratna category I, Mini-Ratna Category II, or Navratna).[10] In addition, I digitize data available from 1994–2009 on the the number of temporary employees (missing for the year 1998), capacity utilization for manufacturing SOEs (available between 1993–2006), government equity percentage (available 1994–2009) as well as number of houses constructed or under construction for employees each year for all SOEs (available between 1994 and 2006, but missing for the years 2003 and 2005).

Finally, I digitize data on job vacancies listed for these managers between 1994–2009, and create a measure of managerial turnover, which is the probability a vacancy was listed for a firm-year observation for Director, Chairman, or Managing Director (i.e., any position on the Board of Directors). The data source for these vacancies is the archival issues of the weekly magazine that lists government job vacancies (called Employment News).

As detailed in the previous paragraph, data availability for each outcome variable varies, but all are available between 3 and 5 years pre-program, and until 10 to 12 years post-program.

[10]Unfortunately, the data does not allow me to distinguish between a SOE that does not apply for autonomy, and one that applies but is denied. But it is worth noting that, even if I could observe this, applying for autonomy may be an equilibrium response and firms that believe they may be rejected could choose not to apply.

4.3. *Sectoral Codes 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). I match SOEs to the Prowess database to get information on their National Industrial Classification (NIC) product codes.[11]

The Prowess data also includes data on profits, sales, and value added for private firms at an annual level, which allow me to obtain production function estimates to estimate TFPR and markups for the SOEs. I use both this data set and the SOE data in the production function estimation because the latter has very few firms in each sector, making sector-wise production function estimation challenging (of the 38 two-digit NIC codes in the SOE data set, the median number of firms in each sector is 3, and 27 of these sectors have 5 or fewer firms). To ensure that I am comparing firms that operate under similar conditions, I only include private firms that are in the same 5-digit NIC codes as SOEs, and that reported data consistently between 1992 and 2009 (to match the SOE balanced panel sample).

Using this combined sample, I obtain production function estimates for a Cobb–Douglas production using the Ackerberg, Caves, and Frazer (2015) approach, and use them to calculate firm-year level TFPR, with value added (rather than sales) as the left-hand side variable (though results are similar if I use sales in this estimation).[12] I also estimate markups using the De Loecker and Warzynski (2012) approach, again using a Cobb–Douglas production function.[13]

4.4. *Summary Statistics*

The main sample is a balanced panel of 165 firms that reported data between 1992 and 2009. Eighty-nine firms were eligible before 1997 to apply for autonomy (eligible *pre-program*), of which 67 received it at some point between 1997 and 2009. In total, 77 unique firms received autonomy during the sample period.

Table II presents the summary statistics for inputs and outcomes. 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. All outcomes except those expressed in percentages (such as capacity utilization and percent of government equity) are winsorized at the 1st and 99th percentile. The average SOE has about ₹9.6 billion in value added, about ₹22.6 billion in capital assets, and about 865 employees. Forty-seven percent of the entire sample received autonomy, and 75% of pre-program eligible firms did so.

[11] 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 such as sector codes, consistent annual financial information is not available across years, necessitating the separate digitization of annual financial statements.

[12] These are estimated separately for each two-digit NIC sector.

[13] I use the "markupest" package for this estimation (Rovigatti (2020)). For about seven firms, there are too few observations in the sector to estimate the production function, so these are missing.

TABLE II

SUMMARY STATISTICS.

				Pre-Program Eligible Firms Only		
	N	Mean	SD	N	Mean	SD
Value Added (Millions of ₹)	2965	9615.04	26,198.84	1600	15,208.00	33,303.38
Capital (Millions of ₹)	2961	22,642.83	63,935.17	1596	34,892.84	82,491.72
Profits (Millions of ₹)	2965	1760.70	6943.45	1600	3285.26	8382.71
Sales (Millions of ₹)	2965	24,392.02	80,298.32	1600	37,772.50	99,903.43
Wage Bill (Millions of ₹)	2965	1807.85	4344.10	1600	2316.02	4846.34
Number of Employees	2962	865.29	2038.95	1597	945.23	2018.58
TFPR	2806	−0.04	0.43	1529	−0.03	0.46
Loans (Millions of ₹)	2961	14,440.63	41,341.99	1596	18,665.99	49,243.33
Interest Payments (Millions of ₹)	2965	1230.98	3369.79	1600	1502.14	3894.82
Dividends (Millions of ₹)	2964	531.03	1854.18	1599	892.93	2325.60
$\mathbb{1}$ (Firm Received Autonomy)	2965	0.47	0.50	1600	0.75	0.43

Note: Pre-program eligible firms are those that earned profits for 3 continuous years and had positive net worth pre-1997. The table includes firms in the balanced panel.

5. EMPIRICAL STRATEGY

5.1. *Impact of Eligibility on Autonomy Status*

Since the main empirical strategy uses eligibility as a proxy for treatment, I begin by testing whether eligibility indeed predicts autonomy. I estimate the cross-sectional regression,

$$\mathbb{1}(\text{Autonomy})_i = \alpha + \mu \mathbb{1}(\text{Eligible})_i + \psi_i, \tag{5}$$

where $\mathbb{1}(\text{Autonomy})_i$ is a dummy variable that takes the value 1 if firm i received autonomy at any point in the sample period, and 0 otherwise. $\mathbb{1}(\text{Eligible})_i$ is a dummy variable that takes the value 1 if firm i is eligible for autonomy, and 0 otherwise. I show results using both pre-program eligibility (the primary measure), and eligibility at any point in the sample period. When using any eligibility as the right-hand side variable, the constant term α measures the probability that ineligible firms received autonomy.

5.2. *Main Specification: Direct Effects of Autonomy on SOEs*

As the theoretical framework in Section 3 demonstrates, autonomy changes managers' choices, which in turn affect firm outcomes. I test this using 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 and/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.

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 any potential endogeneity of the timing of

receiving autonomy; for instance, that a firm might apply for autonomy as demand for their product is increasing.[14]

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

$$y_{ijt} = \alpha + \alpha_i + \gamma_t \phi_j + \delta_t + \beta(\mathbb{1}(\text{Post 1996})_t \times \mathbb{1}(\text{Eligible})_{ij}) + \epsilon_{ijt}, \quad (6)$$

where y_{ijt} = outcome (such as value added) for firm i in sector j in year t, α_i = firm fixed effect, $\gamma_t \phi_j$ = 2-digit sectoral linear trend, and δ_t are year fixed effects. $\mathbb{1}(\text{Eligible})_{ij}$ equals 1 if firm i in sector j was eligible pre-program, and 0 otherwise. $\mathbb{1}(\text{post 1996})_t$ is an indicator variable that is 1 for years 1997 and later, and 0 otherwise. β is the parameter of interest. Standard errors are clustered at the firm level. The sectoral trends account for any concern that pre-program eligible firms may be in sectors that are growing at faster rates; indeed, the event studies show no such differential trends conditional on the fixed effects.

I additionally present event study estimates with year-by-year interactions with pre-program eligibility, showing impacts for 5 years before (when the data begins) and 12 years after 1997 (these 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 value added) as well as the strategic decisions (that explain the underlying mechanisms) allowed under the autonomy program (such as capital investment).

I also estimate versions of equation (6) that are generalized DID using *any* eligibility (instead of pre-program eligibility) as a proxy for treatment. Specifically, I estimate

$$y_{ijt} = \alpha + \alpha_i + \gamma_t \phi_j + \delta_t + \beta(\mathbb{1}(\text{Post Eligibility})_t \times \mathbb{1}(\text{Ever Eligible})_{ij}) + \epsilon_{ijt}, \quad (7)$$

where $\mathbb{1}(\text{Ever Eligible})_{ij}$ takes the value 1 if firm i was ever eligible, and 0 otherwise. $\mathbb{1}(\text{Post Eligibility})_t$ is an indicator variable that is 1 for years when the firm becomes eligible and after, and 0 otherwise. All other terms are the same as in equation (6).

Recent developments in the DID literature have shown that, for staggered designs in the presence of heterogenous treatment effects, the treatment effect estimated using two-way fixed effects can be biased because it includes comparisions with already-treated units (Goodman-Bacon (2021), De Chaisemartin and d'Haultfoeuille (2020), Callaway and Sant'Anna (2021)). While this is not an issue in the specification using pre-program eligibility as a proxy for treatment (since it is not a staggered design), this is a potential concern for the other specifications (including the one using any eligibility as a proxy for treatment). I report results throughout from the estimator that corrects for this approach (De Chaisemartin and d'Haultfoeuille (2020)).[15]

6. MAIN RESULTS

6.1. *Impact of Eligibility Status on Receiving Autonomy*

Since the main empirical strategy uses eligibility as a proxy for treatment, I begin by showing that there is a strong relationship between the eligibility for, and the receipt of,

[14]It is also possible that there are anticipation effects (which induce firms to undertake capital investments in expectation of getting autonomy). This specification also avoids such issues, by comparing firms that were eligible before the program, with those that were not.

[15]The dependent variable in this specification is the first difference of outcomes.

TABLE III

IMPACT OF ELIGIBILITY ON AUTONOMY STATUS.

	1(Firm Received Autonomy)	
	(1)	(2)
𝟙(Firm Was Eligible For Autonomy Pre-1997)	0.603	
	(0.0619)	
𝟙(Firm Was Ever Eligible For Autonomy)		0.563
		(0.0799)
Constant	0.105	−1.67e-16
	(0.0454)	(0.0698)
Dependent Variable Mean	0.430	0.430
Observations	165	165
R-Squared	0.368	0.234

Note: Robust standard errors in parentheses.

autonomy. Table III presents regression results with receiving autonomy as the outcome variable on both measures of eligibility: Column 1 presents results using pre-program eligibility as the independent variable and column 2 any eligibility (pre-program or after). Being eligible pre-program increases the probability of receiving autonomy by 60 percentage points, with a similar effect—56.3 percentage points—for any eligibility. The effects are very statistically significant, and this one predictor has a high r-squared (ranging between 0.23 and 0.37). Note that this incomplete take-up is driven by eligible SOEs not receiving autonomy, rather than ineligible SOEs receiving it: no ineligible SOE receives autonomy during the sample period (shown by the zero constant term in column 2). Overall, 75.3% of pre-program eligible firms, and 61.1% of ever-eligible firms, received autonomy.

To show the changing autonomy status of firms over time, Figure 1 presents the cumulative number of firms over time who have autonomy, split by pre-program autonomy status. This demonstrates that the rate at which firms that were ineligible pre-1997 (and became eligible after) get autonomy after 1997 is quite small: 1 firm in 1997–2005, 3 in 2006–2007, 6 in 2008, and 8 in 2009. This creates a sustained difference in autonomy status across the two groups. Therefore, pre-program eligibility is a suitable proxy for treatment.

6.2. *Direct Impacts on Firm Outcomes*

I begin by documenting impacts on firm exit, restricting the sample to firms that were present in the data in 1992. I define the cumulative exit outcome as a binary variable that takes the value 1 for the first time in the last year a firm stopped reporting financial statements data (and it is 1 subsequently for each year in the data), and zero otherwise (so it is always zero for firms that did not exit).[16] Indeed, there is evidence of differential exit as shown in column 1 of Table A.1, with pre-program eligible firms less likely to exit by 5.8 percentage points, over a mean cumulative exit probability of 0.16, though it is not statistically significant at conventional levels (the p-value is 0.13). Given these differential exit probabilities, I restrict the analysis to the balanced panel of firms that report data for all 18 years.

[16]Since this measure is zero by definition in the first year of data (1992), and only 3 firms exit in the second year, I present results for this outcome from 1994 and later.

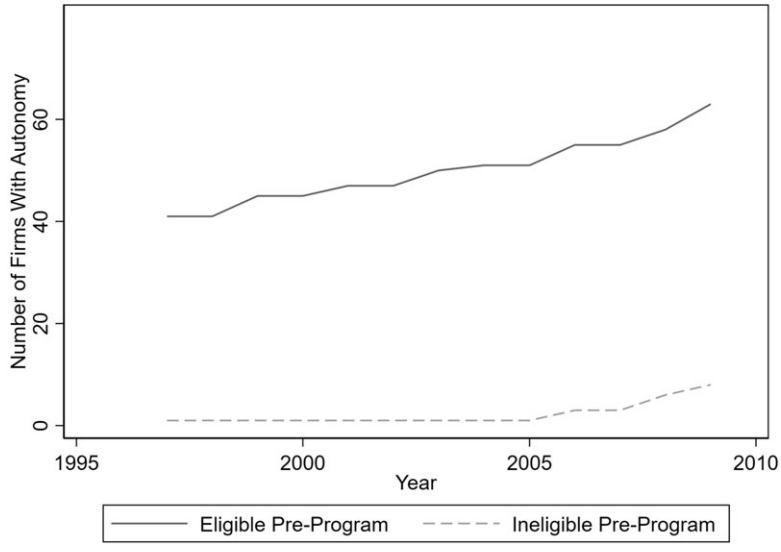


FIGURE 1.—Cumulative Number of Firms Treated Over Time by Pre-Program Eligibility Status.

Next, I show that the data confirm the theoretical prediction that autonomy changed decision-making by managers and thereby firm outcomes. Moreover, as suggested by Claim 1, autonomy leads to managers undertaking a scale expansion of their firms. I use three sets of main outcomes: first, an outcome that is relatively prioritized by the firm, namely value added.[17] Second, I consider outcomes valued relatively more by the government, which impact consumer and employee welfare. I use three such outcomes. The first is markups, which impact consumer welfare. The second is the proportion of temporary employees in the firm's workforce, which is a measure of the firm's reliance on labor with less secure employment contracts (with the hypothesis that the government would prefer the SOE to create full-time secure jobs). The third is whether the SOE is in the process of constructing any employee housing that year. Housing provision is a significant employee benefit valued by the government, but is costly for SOEs. Finally, the third set of outcomes is one that neither the SOE nor the government are incentivized on, but affects allocative efficiency, namely Total Factor Productivity (TFPR).[18]

Table IV presents regression results for value added (the event study estimates are presented in Figure 2(a)). Column 1 shows that firms that were eligible pre-program to apply for autonomy have greater value added by about ₹5.4 billion after the program; a large effect in magnitude relative to mean sales of about ₹9.6 billion. Table V presents results for outcomes related to consumer and employee welfare (with event study estimates presented in Figure 3). There is an increase in markups, a 50% increase relative to the mean, consistent with SOEs moving toward a greater emphasis on profit maximization. In contrast, there is no change in the proportion of temporary employees, which is consistent with the SOE not changing its workforce composition to rely on cheaper labor with less employment security.[19] However, the probability the SOE is in the process of construct-

[17] I present results for other such outcomes (profits) in the Supplemental Appendix.

[18] While data on corruption would be ideal to test whether the change in decision rights impacts additional welfare-relevant outcomes, these data are unavailable.

[19] Results are qualitatively similar if I use total number of temporary employees as the outcome. These are omitted for brevity but available upon request.

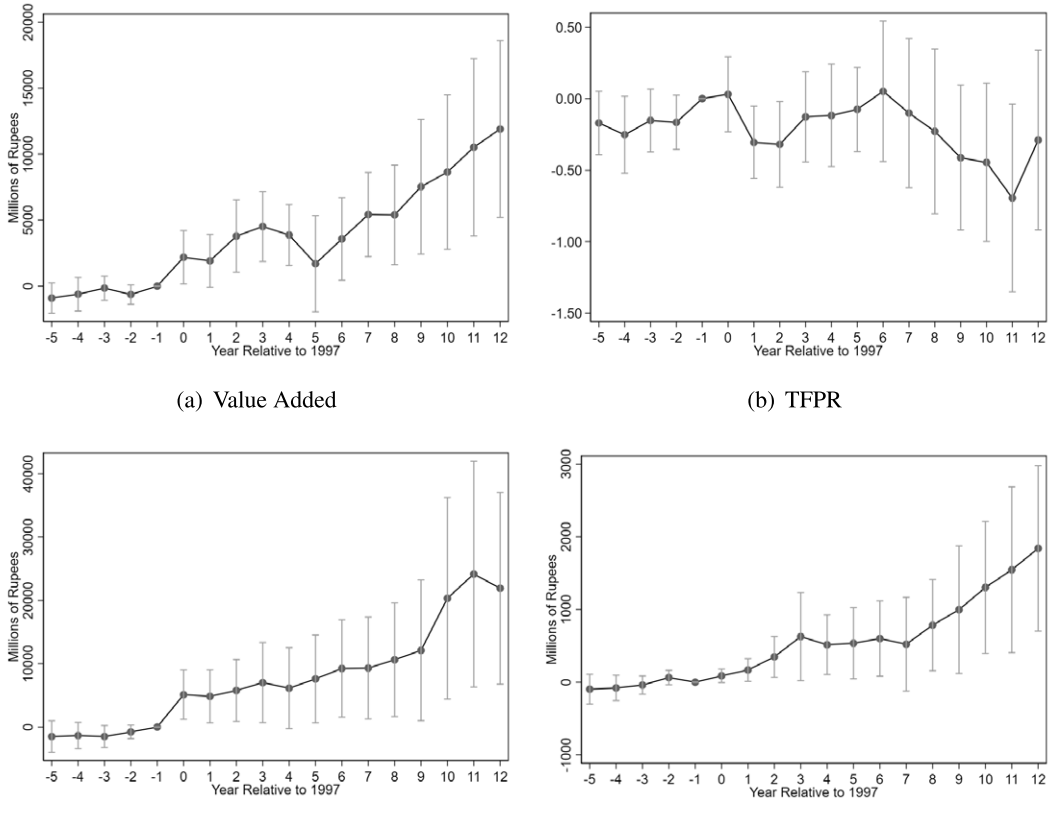
TABLE IV

FIRM OUTCOMES.

	(1) Value Added (Millions of ₹)	(2) TFP	(3) Capital (Millions of ₹)	(4) Salaries and Benefits (Millions of ₹)
𝟙(Pre-Program Eligible) X 𝟙(Post)	5431.702	−0.232	11,035.022	756.027
	(1485.105)	(0.188)	(3936.324)	(285.944)
N	2965	2806	2961	2965
Mean of Dependent Variable	9615.042	−0.442	22,642.828	1807.848

Note: Standard errors clustered at the firm-level in parentheses. Data on all outcomes available from 1992–2009. TFP calculated using the production function estimation method from Ackerberg, Caves, and Frazer (2015). All regressions include firm fixed effects, year fixed effects, and 2-digit sector linear trends.

ing any employee housing falls by 14.5 percentage points, which is a substantial reduction relative to the mean (15.2%). Finally, column 2 in Table IV (the event study estimates are presented in Figure 2(b)) shows that TFPR does not change post-program—the point estimate is negative, and quite large, but also very noisily estimated. Given these results, we



(a) Value Added (b) TFPR

(c) Capital (d) Labor Expenditure

FIGURE 2.—Impacts on Main Outcomes: Event Studies.

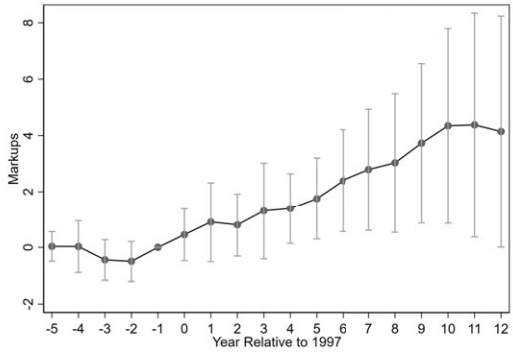
TABLE V

CONSUMER AND EMPLOYEE WELFARE OUTCOMES.

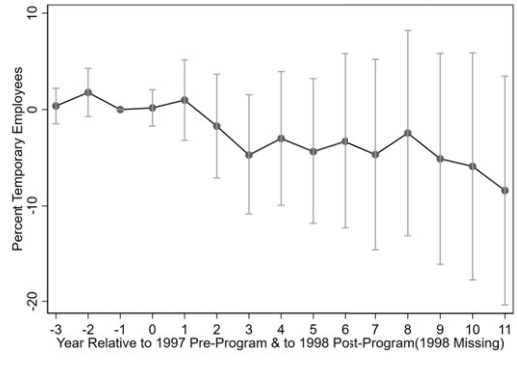
	(1) Markups	(2) Percent Temporary Employees	(3) 𝟙(Any Houses Under Construction for Employees
𝟙(Pre-Program Eligible) X 𝟙(Post)	2.251	−3.543	−0.145
	(0.815)	(3.799)	(0.068)
N	2806	2401	1812
Mean of Dependent Variable	4.344	5.523	0.152

Note: Standard errors clustered at the firm-level in parentheses. Data is available for the outcome in column 1 between 1992–2009. It is available for the outcome in column 2 between 1994–2009, but missing for the year 1998. It is available for the outcome in column 3 between 1994–2006, but missing for the years 2003 and 2005. All regressions include firm fixed effects, year fixed effects, and 2-digit sector linear trends.

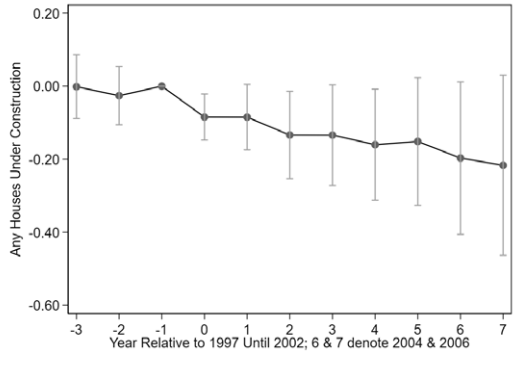
can conclude that productivity was not improved by the autonomy program. These results show that the program's impacts were clearly mixed, in that it improved some firm outcomes but not productivity. In sum, these results are consistent with autonomy improving outcomes valued by managers, a reduction in some outcomes valued by the government,



(a) Markups



(b) Percent of Temporary Employees



(c) 𝟙(Any Houses Under Construction for Employees)

FIGURE 3.—Impacts on Consumer and Employee Welfare: Event Studies.

with no change in productivity, which neither the managers nor the government were directly motivated to change.

These effects are substantial but cumulative over time (as shown in the event studies). How does the magnitude of these effects compare with recent studies on firm interventions, such as the provision of consulting? In terms of magnitudes, these results are in line with results from interventions such as Bruhn, Dean, and Schoar (2018), which find that consulting increases productivity by 0.2 standard deviations. The results on value added in Table IV are similar (about 0.24 standard deviations) but accrue slowly over a much longer time period, for up to 13 years post-program. Another way to interpret the magnitude of these results is to normalize them by the pre-program averages. These results are presented in Table A.9. Value added increases by 103% relative to pre-program levels, over the 13 years post-1996.

6.3. *Mechanisms*

In this section, I uncover the different mechanisms via which the autonomy program impacted firm outcomes.

6.3.1. *Autonomy Levers: Capital and Labor*

The program gave managers decision rights over capital expansion and labor restructuring. Columns 3 and 4 in Table IV present results for capital assets and labor expenditure (the wage bill), respectively (the event study estimates are presented in Figures 2(c) and 2(d), respectively). Capital assets increase by ₹1.1 billion over 13 years (about 90% relative to pre-program levels as shown in Table A.9), and labor expenditures by ₹756 million (about 83% relative to pre-program levels as shown in Table A.9). In line with the theoretical predictions, this is consistent with managers undertaking a scale expansion using both the levers, which the autonomy program granted them.

6.3.2. *Effects by Baseline Levels of Conflict*

In the theoretical framework (and, more broadly, in the theoretical literature on autonomy and delegation), autonomy changes outcomes because the preferences of the manager (the agent) and the government (the principal) differ. Profits from SOEs can either be retained into the firm, or given out as dividends. Dividends accrue largely to the central government (who owns on average 70% of the equity of the firm, 90% including other government entities' holdings such as the state government), while managers would prefer profits to be reinvested into the firm (retained). This incentive conflict between managers and shareholders has been extensively discussed in prior work on private firms (see, for instance, Chetty and Saez (2010), Nam, Wang, and Zhang (2004)).

Motivated by this, I use data on dividends to define a proxy for baseline incentive conflict: if pre-1997, a SOE paid out dividends in *all* profitable years, I define them to have a high level of baseline conflict with the government. On the other hand, if a SOE had any profitable years where they did not pay dividends, I label them to have a low level of baseline conflict.[20] Results are presented in Table VI, in panel A for the subsample of firms with high levels of baseline conflict, and panel B for the subsample with low levels of baseline conflict. I find that the effects of autonomy are concentrated among firms with high levels of baseline conflict across outcomes.

[20]Alternative definitions of this binary variable, such as using the median pre-program proportion of profits paid out as dividends in profitable years as a cutoff, yield similar effects.

TABLE VI

MAIN OUTCOMES BY BASELINE BARGAINING POWER PROXY.

	(1) Value Added (Millions of ₹)	(2) TFP	(3) Capital (Millions of ₹)	(4) Labor (Millions of ₹)
Panel A: Firms with High Baseline Conflict (Dividends Paid in All Profitable Years)				
𝟙(Pre-Program Eligible) X 𝟙(Post)	6848.441	−0.351	15,411.270	1306.009
	(2360.605)	(0.245)	(5434.403)	(420.131)
N	1455	1348	1453	1455
Mean of Dependent Variable	12,420.338	0.211	27,993.844	1820.682
Panel B: Firms with Lower Baseline Conflict (Dividends Not Paid in All Profitable Years)				
𝟙(Pre-Program Eligible) X 𝟙(Post)	3588.765	−0.214	5607.132	156.263
	(1134.063)	(0.336)	(4193.186)	(274.907)
N	1510	1458	1508	1510
Mean of Dependent Variable	6911.925	−1.046	17,486.977	1795.481

Note: Standard errors clustered at the firm-level in parentheses. TFP calculated using the production function estimation method from Ackerberg, Caves, and Frazer (2015). All regressions include firm fixed effects, year fixed effects, and 2-digit sector linear trends.

I also present results in Table A.8 to show that firms that had high levels of baseline incentive conflict were more likely to take up the autonomy progam conditional on eligibility. I show results for both pre-program and any eligibility, and columns 3 and 4 additionally include controls for pre-program mean sales, mean profits, and the interaction of each of these with the relevant eligibility measure. I find that conditional on eligibility, firms with a higher baseline level of conflict are 34–38 percentage points more likely to take up the autonomy program. This is consistent with the theoretical prediction that the value of autonomy to the manager goes up as the incentive conflict increases.

In the Supplemental Appendix, I present and discuss results that use a different measure of baseline returns to autonomy. Specifically, I use the program rules to construct a measure of how much capital expansion treated firms could undertake, which in turn allows me to estimate the heterogeneous returns to the program. These results show that firms that could undertake a higher level of capital expenditure under the program's rules have higher returns to autonomy.

6.3.3. *Changes in Management and Ownership*

Autonomy could also have impacts on firm outcomes via two additional mechanisms. First, it is possible that autonomy was correlated with changes in ownership. For instance, if firms eligible for autonomy were more likely to have even small equity stakes sold to the private sector (even without changes to majority control), the effect of autonomy could reflect lobbying by minority shareholders. To test for this, I use the proportion of government equity as an outcome variable. Results are presented in column 1 of Table VII and show that there were no differential changes in government ownership, which rules out changes in ownership as a potential mechanism. The event study estimates for this outcome are presented in Figure 4 (a).

Second, managerial turnover could be a potential mechanism for the effects.[21] To test for this, I use the probability that the firm posted a vacancy for any position on the

[21]These managers are usually recruited from other public sector enterprises, private firms, promoted from within, and in some cases, from the civil service.

TABLE VII

OWNERSHIP AND MANAGERIAL TURNOVER.

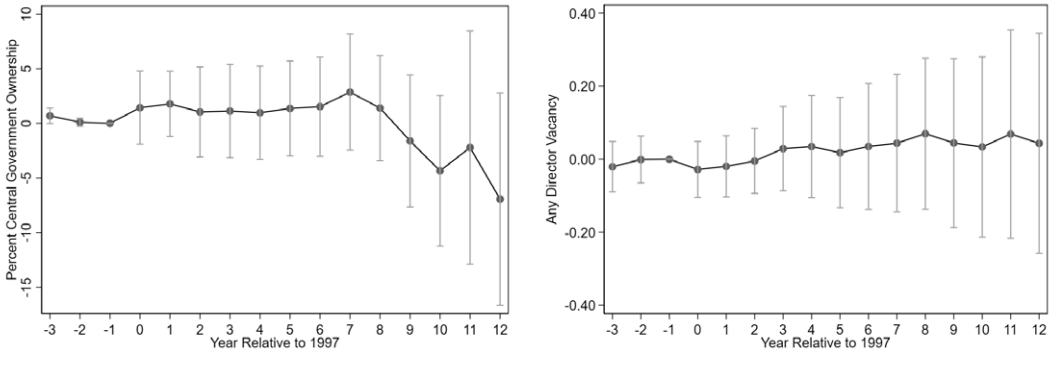
	(1) Percent of Central Government Ownership	(2) 𝟙(Any Vacancy in Board of Directors)
𝟙(Pre-Program Eligible) X 𝟙(Post)	−0.101	0.028
	(2.270)	(0.086)
N	2562	2563
Mean of Dependent Variable	70.313	0.015

Note: Standard errors clustered at the firm-level in parentheses. Data is available for all outcomes between 1994–2009. All regressions include firm fixed effects, year fixed effects, and 2-digit sector linear trends.

board of directors (Director, Chairman, or Managing Director) to proxy for managerial turnover.[22] Results are presented in column 2 of Table VII: while the point estimate is positive, it is not statistically significant (the event study estimates are presented in Figure 4 (b)). Overall, neither changes in ownership nor changes in management are potential mechanisms for the impacts of autonomy. This indicates that the impacts of autonomy are the result of the same set of managers behaving differently under the same ownership as before.

6.3.4. *Incentive Effects of Earning More Autonomy*

It is possible that managers were motivated by the status or career benefits conferred from managing SOEs with greater autonomy. In this section, I consider whether the ability to earn more autonomy was driving the effects of the program, that is, are firms responding to autonomy (e.g., undertaking investments) to earn a higher level of autonomy? Specifically, for the first 10 years of the program, firms could earn their way either from no autonomy to one of the two lower levels (Miniratna Categories II and I), or go



(a) Percent of Central Government Ownership (b) 𝟙(Any Vacancy in Board of Directors)

FIGURE 4.—Impacts on Government Ownership and Managerial Turnover: Event Studies.

[22]Results are similar if I use only vacancies for Chairman or Managing Director, which is the equivalent of CEO, or the cumulative probability of ever posting a vacancy.

TABLE VIII

MAIN OUTCOMES NET OF THE INCENTIVE EFFECTS OF EARNING AUTONOMY.

	(1) Value Added (Millions of ₹)	(2) TFP	(3) Capital (Millions of ₹)	(4) Salaries and Benefits (Millions of ₹)
𝟙(Treatment) X 𝟙(Post)	3357.860	−0.220	5503.472	398.221
	(1078.117)	(0.171)	(4212.336)	(105.192)
N	2038	1937	2034	2038
Mean of Dependent Variable	4676.058	−0.759	11,060.651	1208.410

Note: Standard errors clustered at the firm-level in parentheses. Data on all outcomes available from 1992–2009. TFP calculated using the production function estimation method from Ackerberg, Caves, and Frazer (2015). All regressions include firm fixed effects, year fixed effects, and 2-digit sector linear trends. Treated firms are those that entered the program at the Mini-Ratna Category-I level of autonomy. Other treated firms are omitted.

from being Miniratna Category-II to Miniratna Category-I. The highest level of autonomy (Navratna) was granted by the government rather than earned by a firm until 2006. Therefore, for the first 10 years of the program, impacts on Miniratna Category-I firms were not driven by the desire for greater autonomy since this could not earned. I compare treatment effects of these firms relative to control firms in Table VIII, and find that the effects are positive and statistically significant. Thus, while it is possible that incentive effects of earning more autonomy are part of the total program effects, there are substantial positive effects of the program net of these effects.[23]

7. ADDITIONAL RESULTS AND ROBUSTNESS CHECKS

In this section, I present results on additional outcomes, and using alternative specifications for the main outcomes.

7.1. *Other Outcomes*

I begin by documenting how borrowing patterns changed as a result of the autonomy program. Results are presented in Table A.4. Specifically, pre-program eligible firms increase borrowing from sources other than from the central government by ₹7.3 billion, about 50% relative to the mean, though the effect is not statistically significant. Government loans (defined as loans extended by the central government) fall, by Rs. 4.5 billion (mean government borrowing is Rs. 2.5 billion).[24]

To understand the borrowing margin better, I merge in data from the Prowess database, which contains information on banker names. These data have gaps (45% of firm-year observations report at least one banker), but 85.45% of firms report data at least once during the panel period (1992–2009). Conditional on reporting a banker, all SOEs report at least one lender that is either the central government or a publicly owned bank, and

[23] Finally, it is possible that autonomy gave managers more time on their hands. While the results from Table A.2 and Table VI indicate that the ability to undertake capital expansion and baseline preference differences seems like they are primary mechanisms, data on managerial time-use for this context is not available, making this is difficult to rule out definitively as one of the mechanisms for the impacts of the program.

[24] By using the ratio of interest payments to total borrowing as an outcome variable, I show that interest payments per rupee of borrowing do not change. Results are omitted for brevity, but are available upon request.

TABLE IX

FIRM OUTCOMES: GENERALIZED DIFFERENCES IN DIFFERENCES WITH ANY ELIGIBILITY.

	(1) Value Added (Millions of ₹)	(2) TFP	(3) Capital (Millions of ₹)	(4) Salaries and Benefits (Millions of ₹)
𝟙(Ever Eligible) X 𝟙(Post Eligibility)	4193.012	−0.247	10,180.911	692.339
	(1124.774)	(0.204)	(2950.706)	(241.614)
N	2965	2806	2961	2965
Mean of Dependent Variable	9615.042	−0.442	22,642.828	1807.848

Note: Standard errors clustered at the firm-level in parentheses. Data on all outcomes available from 1992–2009. TFP calculated using the production function estimation method from Ackerberg, Caves, and Frazer (2015). All regressions include firm fixed effects, year fixed effects, and 2-digit sector linear trends.

98% report a publicly owned bank as lender.[25] Using these data, I create a binary variable that is 1 if the firm reported a private bank as a lender (and zero otherwise), and use that as an outcome variable. Results are presented in column 4 of Table A.4. I find that this is indeed changed by the program: pre-program eligible firms are 31.4 percentage points more likely to report a private bank as a lender after 1997 (the mean probability across the sample of reporting a private bank is 35.8%, so this is a substantal effect relative to the mean). I also tested whether pre-program eligible firms are more likely to report any banker post-1997, and do not find any evidence that is the case. Column 3 of Table A.4 indicates that the results are not driven by the differential probability of reporting a banker in the CMIE data.

Furthermore, it is possible that firms become eligible post-program in order to avail of the benefits of autonomy. Results using any eligibility as a proxy for treatment are presented in Table IX, and are similar to the main results. As additional context, 126 firms in the balanced panel are eligible at some point between 1997–2009, of which 89 (70%) were eligible pre-program (before 1997). Of the remaining, only 22 firms become eligible 3 years or more after the program (i.e., only 22 firms could have potentially changed their behavior and become profitable for 3 consecutive years to get autonomy). Of these, only 5 actually receive autonomy. Overall, it seems that the autonomy program alone was not enough to induce a large number of firms that were ineligible for it in 1997 to become eligible, or that they were not able to do so in large numbers.

7.2. *Robustness Checks*

Next, I discuss three primary robustness checks. First, to ensure my results are not driven by the firms that could potentially lobby to make the rules and get autonomy, I employ a "donut" estimator, and drop the thirteen firms that are exactly eligible under the profitability condition. That is, these firms were only profitable in the 3 years before the program was launched. If there was lobbying to set the rules, these firms were the likely constituency conducting the lobbying. I also drop the nine firms that were just ineligible; that is, they earned profits for only 2 of the 3 years pre-program. Results are presented in Table X. I find that results are similar to the main results, indicating that lobbying by firms with 3 years of profits are not driving these effects.

[25] In India, some of the largest banks are publicly owned, and extend loans to both firms in the public and private sector.

TABLE X

MAIN OUTCOMES: DROPPING JUST-ELIGIBLE AND JUST-INELIGIBLE FIRMS PRE-PROGRAM.

	(1) Value Added (Millions of ₹)	(2) TFP	(3) Capital (Millions of ₹)	(4) Salaries and Benefits (Millions of ₹)
𝟙(Pre-Program Eligible) X 𝟙(Post)	4853.699	−0.245	11,694.644	904.438
	(1428.953)	(0.241)	(3515.340)	(268.748)
N	2570	2422	2566	2570
Mean of Dependent Variable	9738.138	−0.293	23,454.076	1638.599

Note: Standard errors clustered at the firm-level in parentheses. Data on all outcomes available from 1992–2009. TFP calculated using the production function estimation method from Ackerberg, Caves, and Frazer (2015). All regressions include firm fixed effects, year fixed effects, and 2-digit sector linear trends.

Second, I test whether pre-program eligibility, in absence of receiving autonomy, impacts firm outcomes. Results comparing pre-program eligible firms who did *not* receive autonomy with pre-program ineligible firms are presented in Table A.10. There is no effect on any of the outcomes, indicating that eligibility in absence of receiving autonomy has no effects.

Third, to additionally rule out demand shocks as a confounding explanation, I utilize data on capacity utilization for manufacturing SOEs (which is 61% of the sample) between 1993–2006. If pre-program eligible SOEs were facing growing demand (conditional on sector-time controls and firm fixed effects), then these firms should differentially increase existing capacity utilization before or while investing in new assets. Results are presented in Table A.1, and show no effects on capacity utilization.

Supplemental Appendix A presents results with alternative specifications, as well as additional results. These include a test of heterogeneous effects for firms that have differential abilities to use the autonomy program to undertake capital expenditure (I construct this measure using the rules of the program). It also includes results for additional outcomes including profits (in Table A.1), as well as results showing that the effects are not driven by the strategic reporting of profits. Furthermore, I show that the results are robust to considering only the SOEs that reported positive profits at least once pre-1997.

8. CONCLUSION

The existence of earned autonomy programs across a range of organizational settings indicates that this is a common model governments employ in devolving autonomy. Such programs are used to improve productivity across a variety of different settings in the public sector, from natural resource management and manufacturing to health and education. However, it is not clear that giving autonomy to well-running organizations has positive effects, since these firms may be less constrained overall, and so the gains to autonomy may be low for such firms. This paper shows that such programs can improve certain firm outcomes (such as value added), but come at the cost of others (such as employee benefits and higher markups). Furthermore, the results indicate that the effects are due to existing managers changing their behavior, rather than autonomy causing managerial turnover and attracting newer managers with potentially higher returns to autonomy.

The lack of positive impacts on TFP stands in contrast with prior work studying the impact of earlier reforms to Chinese SOEs, as well as private sector reforms in India. In the case of Indian private sector reforms, these included reducing constraints on foreign investment (Bau and Matray (2023)), trade liberalization (Topalova and Khandelwal

(2011)), and product market competition (Martin, Nataraj, and Harrison (2017)). Early reforms with Chinese SOEs, starting in the late 1970s, included improvements in product market competition as well as aligning managerial and worker bonuses with firm performance (McMillan (1994), Li (1997), Groves, Hong, McMillan, and Naughton (1994)). Consistent with this explanation, Li (1997) estimates that product market competition and worker bonuses were responsible for 50% of the TFP growth due to this reform, with another 38% being attributable to factor reallocation.

In contrast, the autonomy program in India did not explicitly seek to increase competition for SOEs, and managers were not per se incentivized on TFP. [26] Taken together, these results could imply that better aligning managerial incentives with productivity improvements, or having reforms incorporate product and input market competition, may improve productivity. Furthermore, monitoring mechanisms for SOEs may additionally enhance productivity (Li and Zhang (2022)). Of course, other explanations (such as lack of managerial capacity) are also possible for these differences, and a full comparison of these programs with the autonomy program is beyond the scope of this paper.

The specific context I study is important, since SOEs continue to be a large and influential part of the economies of many countries, and so understanding how such programs impact their performance is policy-relevant. The results show that large gains in certain aspects of SOE performance are possible with organizational reform without changes to ownership. These results contribute to understanding why autonomy affects organizational outcomes and when it can be an effective reform.

The policy does not allow me to separately test the impact of quasirandomly 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, while the impacts of autonomy are present net of the incentive effects of potentially moving to a higher tier, it is possible that there were anticipation effects. These types of dynamic incentives could induce these firms to undertake a different profile of investments relative to a program where such incentives are not present. These and related questions, including whether similar programs generate positive impacts in other settings, are important issues for future research.

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The replication package for this paper is available at https://doi.org/10.5281/zenodo.11431164. The authors were granted an exemption to publish parts of their data because either access to these data is restricted or the authors do not have the right to republish them. However, the authors included in the package, on top of the codes and the parts of the data that are not subject to the exemption, a simulated or synthetic dataset that allows running the codes. The Journal checked the data and the codes for their ability to generate all tables and figures in the paper and approved online appendices. Whenever the available data allowed, the Journal also checked for their ability to reproduce the results. However, the synthetic/simulated data are not designed to produce the same results. Given the highly demanding nature of the algorithms, the reproducibility checks were run on a simplified version of the code, which is also available in the replication package.