

Government Employment Guarantee, Labor Supply, and Firms’ Reaction: Evidence from the Largest Public Workfare Program in the World

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Abstract

Using establishment-level data, we examine the impact of the Indian government’s employment guarantee program on labor and firm behavior. We exploit the staggered implementation of the program for identification and find that the program led to a 10% reduction in permanent workforce in firms. Firms responded to the adverse labor supply shock by resorting to increased mechanization. This significantly increased the firms’ cost of production, leading to a decline in net profits and productivity. These effects manifested primarily in firms paying low wages, having low labor productivity and greater sales volatility, and firms located in states with pro-employer labor regulations.

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I Introduction

*Admitting that the Mahatma Gandhi National Rural Employment Guarantee Act (MNREGA) had pushed Ludhiana's industry on the back foot, the brand manager of Duke, Gagan Jain said, "Acute labor shortage is a reality and we're trying to work things out... Not only unskilled, but even tasks requiring skilled workers have taken a beating, with around 20% personnel taking off"... As it was getting tougher to find people with the required skill set, the future would see adoption of latest technology so that lesser people would have be hired.*¹

A recent but growing body of work in the area of "labor and finance" seeks to understand the impact of regulatory policies governing labor markets on corporate policy (Agrawal & Matsa (2013); Simintzi, Vig & Volpin (2014); Serfling (2016)). The extant literature has, for instance, examined the impact of unemployment insurance, employee rights, employment protection laws, and more recently minimum wages on capital structure, capital expenditure, and firm-level innovation.² Among such policies that affect labor, public workfare programs have become increasingly popular globally.³ There has been a longstanding debate among economists on the effects of such workfare programs on employment, poverty, consumption, and crime. However, surprisingly, the empirical evidence on the effect of such programs on firm-level outcomes such as capital expenditure decisions, firm productivity, and profitability is scant. This study attempts to fill this gap by examining the real effects of a large public workfare program, the Mahatma Gandhi National Rural Employment Guarantee Act (hereafter, referred to as MNREGA) on firms.

Our objectives in this study are two-fold. First, we seek to understand how workfare program impacts the labor supply of both temporary and permanent workforce in the organized private (non-government) sector. Our main data source is the Annual Survey of Industries (ASI), which is the primary source of industrial statistics pertaining to the organized sector in India. Among other things, ASI provides detailed information regarding

¹A statement in The Times of India (TOI) article, dated October 21, 2009, titled, "Traders blame MNREGA for worsening labor crisis."

²See Aaronson, French & MacDonald (2008); Acharya, Baghai & Subramanian (2013); Agrawal & Matsa (2013); Simintzi et al. (2014); Serfling (2016); Alok, Chaurey & others (2018); Gopalan, Hamilton, Kalda & Sovich (2018); Aaronson, French, Sorkin & To (2018); Gustafson & Kotter (2018)

³For instance, Franklin Roosevelt launched the largest workfare program in history at that point to arrest rising unemployment levels during the great depression. In recent times, public workfare has been launched in Korea (1997–1999), Ethiopia (2004), Rwanda (2008), and Sri Lanka (2010), among a host of other countries (Subbarao, del Ninno, Andrews & Rodríguez-Alas (2010)).

employment, capital investments, expenses, and earnings for these firms.⁴ We document that MNREGA crowded out labor supply to the private sector. The second objective of our study is to examine how the labor supply shock impacts a firm’s capital expenditure decisions and profitability. To the best of our knowledge, this aspect of our study is unique to this literature.

Our paper is related in spirit to contemporaneous work that seeks to uncover the impact of minimum wage laws on corporate investment decisions (Cho (2016); Gustafson & Kotter (2018); Geng, Huang, Lin & Liu (2018)) and profitability (Chava, Oettl & Singh (2018); Draca, Machin & Van Reenen (2011)). While related, there are important differences between workfare programs and minimum wage laws. An increase in minimum wage exogenously imposes a price floor (to the extent it is binding) and therefore is unlikely to induce an adverse labor supply shock. If anything, minimum wage laws are likely to adversely affect the demand for labor if firms find it unprofitable to hire labor at the increased wages. In contrast, the ex-ante effects of a workfare program on labor supply (and consequently corporate investments) is a priori ambiguous. First, to the extent that such programs are aimed at attracting the unemployed and low-skilled, it may have no impact on the supply of labor to the private (non-government) sector. If so, the workfare program shouldn’t have any impact on firm-level investments or profitability as well. Second, it is also possible that the wages and labor-leisure trade-off offered by workfare programs may create a more desirable employment opportunity for some gainfully employed workers resulting in higher wages and crowding out labor supply to the private sector. In response, firms can choose to increase the wages to retain its workers if the marginal productivity of labor is positive. Alternatively, firms may substitute labor with capital (Acemoglu & Finkelstein (2008); Acemoglu (2010)) or choose to scale down investments if high operating costs reduce potential cash-flows (Favilukis, Lin & Zhao (2019)).

Third, a workfare program that guarantees employment may also increase the supply of labor to the private sector. This is because a workfare employment-guarantee program creates a fallback option for workers facing job-loss. In this respect, it works similar to unemployment insurance. By reducing the adverse consequence of job loss on workers’ income stream, workfare programs may incentivize the workers to seek employment at riskier firms

⁴To understand the underlying mechanism of MNREGA, it is essential to understand that the shock affects labor supply and not the demand because the effect on labor demand may be driven by macro and industry dynamics. Consequently, we conduct a series of tests to rule out the possibility that our results are confounded by labor demand.

and demand a lower wage premium for bearing risk (Ellul, Pagano & Schivardi (2017); Doornik, Fazio, Schoenherr & Skrastins (2019)). A decrease in wages may make it optimal for firms to substitute capital with labor resulting in a reduction in corporate investments and an increase in employment.

To the extent that labor market policy changes can affect firms' labor hiring and investment decisions, it has implications for both firm value, and economic growth (Merz & Yashiv (2007); Acemoglu & Guerrieri (2008); Belo, Lin & Bazdresch (2014)). However, to the best of our knowledge, the impact of such a labor market shock where the effects on labor and firms can manifest both in terms of price (wages) and quantity (labor supply) on firm policies has not been previously studied. We use such an economic setting. Specifically, we examine the impact of implementation of a large rural employment guarantee program in India, the "Mahatma Gandhi National Rural Employment Guarantee Act" initially aimed to provide at least 100 days of employment to the rural unemployed and the underemployed by engaging them in the building of rural infrastructure. It was stipulated that at least 70% of the expenditure should be on wages alone to make the program labor intensive. MNREGA fits the description of a typical workfare program analyzed in the literature (Besley & Coate (1992)). The program was launched in the year 2006 with an initial outlay of INR 228.46 billion (USD 5.08 billion)⁵ (Imbert & Papp (2015)). In terms of man-days provided and the proportion of government revenue allocated, this is the *largest* employment guarantee program in history. Currently, it provides employment to approximately 50 million households annually, generating about 2300 million person-days of employment.

The biggest obstacle in evaluating the impact of workfare programs on outcomes, such as corporate employment and investments, is that these programs are typically launched as a remedial measure during economic downturns. Thus, the identification of the effects of such programs is confounded by macroeconomic conditions as well as other contemporaneous fiscal and monetary policy measures that governments may undertake in dealing with recessions. Further, since such programs are launched across the country, it becomes difficult to obtain an estimate of the counter-factual level of these outcomes in the absence of the program.

Several salient features of MNREGA help us to circumvent these issues and allows for a relatively clean experimental setting for causal identification. First, the program was implemented in a phase-wise manner starting from 200 districts in the first phase, extending

⁵At the then exchange rate of INR 45 to USD 1.

up to 130 in the second phase, and 285 districts in the third phase. This facilitates the use of the generalized version of the difference-in-differences methodology for multiple time periods and treatment groups (Bertrand & Mullainathan (1998)). We include factory fixed effects and year fixed effects in all our specifications to control for time-invariant factory level characteristics as well as aggregate macroeconomic shocks and trends. Second, our setting overcomes the challenges posed by simultaneous changes in the fiscal as well as monetary policy, which is a major identification hurdle in many settings (Auerbach, Gale & Harris (2010)). The fact that there were no significant changes in the monetary policy during the implementation of the first and the second phases of the program allows us to disentangle the impact of the program. Third, unlike most activist fiscal programs in the west, MNREGA was not announced as a temporary measure. The plan did not mention any expiry date.⁶ Therefore, MNREGA is likely to have caused a permanent change in behavior. Finally, the retrenchment of permanent employees is a very long and arduous process in India, owing to the rigid labor laws (Besley, Burgess & others (2004)). Therefore, any quick change in permanent employment can be attributed to the labor supply rather than demand shocks driven by adverse regional or macroeconomic conditions.

We start our analysis by examining the impact of MNREGA on the composition of factory production workers. Our baseline results show that the implementation of MNREGA leads to an approximately 10% reduction in the number of permanent workers. At first glance, it may seem that any plausible impact on factory employment should have been on temporary or contract workers, particularly, since MNREGA is also for under-employed.⁷ If work under MNREGA would require very little effort due to poor monitoring by the government (Niehaus & Sukhtankar (2013), Muralidharan, Niehaus & Sukhtankar (2016), Banerjee, Duflo, Imbert, Mathew & Pande (2016)) and have added benefits, such as working very close to home,⁸ lower living/incidental expenses, and a lower chance of accidents, then even a permanent worker may consider the option of working for MNREGA for 100 days. It must be noted that these workers can still work in the private sector on a contractual basis for the remaining part of the year. In other words, for workers at the lower end of the wage distribution, the welfare gains

⁶The Indian National Congress Party (INC), which launched the program, lost power in 2014. The Bhartiya Janata Party (BJP), which came to power in 2014, decided not only to continue the program but also increased the allocation of funds for the program.

⁷See also Banerjee & Duflo (2007), Morten (2019), Topalova (2010)

⁸A recent paper Banerjee & Chiplunkar (2018) estimates the compensating differential that relatively low skilled workers need to accept an out-of-state job. They find that male (female) workers on average require 76% (135%) higher wages for an out-of-state job relative to similar within state jobs.

from working under MNREGA (net of leisure and other non-pecuniary benefits, such as being close to family) may exceed the factory wages net of additional living expenses. Given their full-time commitment to the factories, such workers can participate in MNREGA only by resigning from their permanent employment. Our causal evidence is consistent with this thesis.

Next, we perform several time-series and cross-sectional tests to shed light on what could be driving the aforementioned results. These tests are based on the idea that the benefit of leaving a permanent job to work under MNREGA should be higher for workers earning lower wages, those with low labor productivity, and when the risk of job loss is higher. Consistent with our conjecture, we find that the decline in the permanent workforce is limited to establishments associated with low wages, low labor productivity, high sales (output) volatility, and in firms located in states with pro-employer labor laws.

In addition, workers would only leave their jobs to work under MNREGA, if they are relatively sure that the program will continue to exist in the medium and long term. While the workers may not have perfect political foresight, the history of Indian political economy suggests that such schemes tend to outlive the tenure of the government that introduced them. Any residual uncertainty regarding the continuation of the program should be resolved after the party that introduced the program was voted back to power in the year 2009. Consistent with this idea, we find that the attrition of permanent workers increased after the year 2009 in regions more exposed to the program.

As far as the impact of MNREGA on the supply of contract workers is concerned, multiple effects seem to have worked simultaneously, but in opposite directions. It must be noted that contract laborers can continue to work for both MNREGA for 100 days and the private sector for the rest of the year. Thus, while there could be an outflow from the existing pool of contract workers for some part of the year, it is also possible that such outflow is offset by new hires or erstwhile permanent workers joining the ranks of contract workers after completing 100 days of work provided under MNREGA. Our ability to disentangle the two countervailing effects is limited because ASI captures employment in terms of man-days of different kinds of the workforce and is not based on the unique identification of persons employed. On a net basis, we do not find any effect of MNREGA on the number of contract workers. However, to the extent that the effect of MNREGA should be stronger after 2009 elections that resolved uncertainty regarding the continuation of the program, we do find an increase in attrition of contract workers as well after the year 2009 in regions more exposed to

the program.

As we have noted before, due to the existence of rigid labor laws in India (Besley et al. (2004)), the reduction in the number of permanent workers that we document is unlikely to be demand driven. Nevertheless, we perform two additional tests to show that the decrease in permanent labor is indeed supply driven. We report the following main findings.

First, under MNREGA, the government pays minimum wages applicable as per law. We find that the average factory wage is higher than MNREGA wages. Now, if the decline in the number of workers is supply driven, then workers working in factories that pay low wages are more likely to shift to MNREGA when compared to workers employed in factories that pay high wages. We find evidence that is consistent with this idea. Second, we examine the impact on wages of permanent workers. If a decline in the number of permanent workers is due to a lack of demand by firms, then we can expect the wages to decline. Here, we find no change in the wages of permanent workers. The above two tests rule out a demand-based explanation. Third, we examine the impact of MNREGA on the number of managerial (supervisory) staff that serves as a placebo group.⁹ Given the nature of jobs that are provided by MNREGA, we do not expect MNREGA to have any direct impact on the demand or supply of managerial staff. Reassuringly, we do not detect any impact of MNREGA on this group.

Having shown that program indeed impacts the labor supply to firms, we next examine the response of firms to the labor supply shock. Faced with a labor supply shock, it is possible that factories¹⁰ increase wages or cut down the level of production. It is also possible that mechanization, which was not optimal in pre-MNREGA labor market conditions, becomes a preferred option following the implementation of MNREGA (Acemoglu (2010), Acemoglu & Finkelstein (2008)). We find that additional investments in fixed assets (plant and machinery) increases by approximately 12% (24%) as a response to MNREGA. To the extent that such capital entails upfront investments and needs to be financed (Fazzari, Hubbard, Petersen, Blinder & Poterba (1988), Benmelech, Bergman & Seru (2015)), we expect the move towards mechanization to be higher for firms with greater access to finance. We find evidence consistent with this idea. Moreover, the increase in mechanization should be especially greater for the sub-sample of factories that experience a higher outflow of workers. Consistent with this thesis, we find that the increase in mechanization is limited to

⁹Throughout the study, we use the terms “supervisory” and “managerial” workers interchangeably.

¹⁰We use the terms factories and firms interchangeably.

establishments associated with low wages, low labor productivity, high sales (output) volatility, and in firms located in states with pro-employer labor laws.

Subsequently, we examine the operating performance of factories. By revealed preference, it is found that increasing mechanization was not optimal when compared to the use of labor under pre-MNREGA conditions. Consistent with this thesis, we find that the cost of production increases by approximately 8%, while the output (gross revenue) remains unchanged resulting in a significant fall in earnings. However, it is possible that this decline in earnings may be only temporary and driven by the fixed costs associated with implementing and using new technology. Consistent with the fact that mechanization improves operational efficiency, we find a decrease in inventory days and current asset cycle. Importantly, we do not find any significant impact of MNREGA on firm-level output, again strengthening our claim that the labor-capital substitution that we document is unlikely to be driven by a demand shock.

Finally, we also examine the dynamic effects of the impact of MNREGA (See figures (2) and (3)). We find that the number of permanent workers employed in factories drops increasingly over time. Consistent with the long-run nature of labor-capital substitution, we find that capital additions also increase gradually. Most importantly, we do not observe any pre-trends in the data, which is critical for identification in a difference-in-differences setting.

In summary, our study documents that workfare programs may have the unanticipated consequence of crowding out labor to the private sector and incentivize the firms to move towards a more capital intensive production process. Our analysis shows that labor supply shortage engendered by MNREGA encouraged capital investments in plants and machinery and hampered firms' productivity and profitability in the short run. To the extent that mechanization is associated with greater operational efficiency, the labor-capital substitution may result in increased firm productivity over the long-term (Romer (1990)).

II Related literature

Our study contributes to several strands of the literature. First, our study contributes to the growing body of work in the area of "labor and finance" (Simintzi et al. (2014); Serfling (2016)). A significant portion of this literature focuses on the impact of labor market frictions on a firm's capital structure decisions. Agrawal & Matsa (2013) find that higher

unemployment benefits are associated with an increase in firm leverage. [Simintzi et al. \(2014\)](#) find that restrictive labor laws turn labor cost into debt payments in spirit and lead to crowding out of financial leverage by increased operating leverage. In the same spirit, [Serfling \(2016\)](#) find that increased labor firing costs negatively impact firm leverage. [Bena & Simintzi \(2019\)](#) examine the impact of labor costs on firm-level innovation. Using the 1999 U.S.-China agreement as a shock that lowered effective labor cost, they find that the US firms operating in China decreased their share of process innovations. Another study by [Mao & Wang \(2018\)](#), outlines a channel through which access to finance leads to innovation in an environment of labor scarcity. [Mueller, Ouimet & Simintzi \(2017\)](#) examine the impact of wages on firm performance during the financial crisis and find that firms paying higher wages had better performance as measured by sales, profits, and market shares.

There are several other studies which analyze the impact of different labor market frictions on corporate investments and innovation ([Acharya et al. \(2013\)](#); [John, Knyazeva & Knyazeva \(2015\)](#); [Francesco, Weber & Yang \(2017\)](#); [Silva \(2017\)](#); [Alok & Krishnamurthy \(2017\)](#); [Bai, Fairhurst & Serfling \(2019\)](#)) and employment dynamics ([Meer & West \(2016\)](#); [Gopalan et al. \(2018\)](#)). Our work is also related to papers evaluating the impact of financial frictions on firm-level employment ([Falato & Liang \(2016\)](#); [Alok et al. \(2018\)](#); [Ersahin & Irani \(2018\)](#)) Our study furthers the scholarship in this area by providing direct evidence that firms respond to labor scarcity by investing in capital (mechanization).¹¹

Secondly, our study relates to the vast literature that analyses the optimality of workfare when compared to welfare ([Chambers \(1989\)](#)). Historically, globally, countries have used workfare programs as poverty alleviation tools ([Drèze \(1990\)](#); [Katz \(1996\)](#)). However, there are two opposing views in the literature concerning the efficacy of such programs in poverty alleviation. On the one hand, workfare may allow the unemployed to gain skills, eventually helping them to get employment in the private sector ([Himmelfarb \(1984\)](#)). On the other hand, lack of effective monitoring in such programs may even attract productive laborers who were employed in firms, thereby making them dependent on the program and effectively transforming workfare into welfare ([Besley & Coate \(1992\)](#), [Nichols & Zeckhauser \(1982\)](#)). Thus, workfare may also have the unintended consequence of moving firms towards a more capital intensive production process. Our results support the latter view.

¹¹Theoretically, the impact of labor scarcity on technology is ambiguous ([Acemoglu \(2007, 2010\)](#)).

III The Program

A Implementation

MNREGA was launched by the Government of India on February 2, 2006. The stated objective of the program was to provide “livelihood security in rural areas by providing at least 100 days of guaranteed wage employment in a financial year to every household whose adult members volunteer to do unskilled manual work.”¹² In order to achieve the stated purpose of providing maximum employment to the rural poor, the program stipulated that at least 70% of the total expenditure under the program should be incurred on wages.

The MNREGA program is an Act of the Parliament and not a result of an executive order. The Act did not mention an expiry date. Therefore, the program can be completely stopped only if the enabling Act of the Parliament is repealed.¹³ Indeed, the new government headed by the National Democratic Alliance (NDA) that came to power in 2014 not only continued the program but also significantly increased the allocation of funds to MNREGA. The NDA was in opposition when the program was implemented in 2006. Therefore, it is reasonable to conclude that, unlike other fiscal stimulus measures that were temporary in nature, MNREGA has a semblance of permanence.¹⁴ The expectation that MNREGA will continue in the long-run may encourage the movement of workers from factories to MNREGA-related work. The program only defines the type of work to be provided under the program but does not set any measurable output target. The task of monitoring the work is left entirely to the local bureaucracy.

The program was implemented in a staggered manner in three phases. On February 2, 2006, the program was implemented in 200 districts. The program was extended further by including 130 districts on April 1, 2007, and the remaining 285 districts were covered on April 1, 2008. The stated policy of the government was to select most backward districts in the first

¹²Source: National Rural Employment Guarantee Act 2005

¹³Any change in the allocation of funds also requires the approval of the Parliament.

¹⁴India has a history of change in political complexion of the government not impacting the continuation of social sector schemes. Not continuing the previous government’s social sector schemes could be politically expensive. To test the above argument, we looked at the major social sector schemes launched by the Bhartiya Janata Party led National Democratic Alliance government, which was in power between the years 1998-2004. These programs include (i) Annapurna Food Scheme, which was aimed at providing food grains to poor households (ii) Pradhan Mantri Gramodaya Yojana, which was aimed at providing housing for all (iii) Program for Drinking Water Supply for Rural Areas (iv) Pradhan Mantri Gram Sadak Yojana, a program aimed at building rural roads. All of the above flagship programs of the NDA government were continued by the Indian National Congress led United Progressive Alliance (UPA) government. Similarly, all the major social sector schemes launched by the UPA government were continued by the second NDA government which came to power in the year 2014.

phase and the relatively less backward in later phases (Imbert & Papp (2015); Agarwal, Prasad, Sharma & Tantri (2018)).

In total, the federal budget allocated an annual outlay of INR 230.73 billion for MNREGA in the year 2005–2006. This amount increased to INR 340 billion in the year 2014–2015. The initial outlay under MNREGA represented approximately 6.3% of the total tax revenue of the federal government. With regards to the proportion of government revenue allotted, MNREGA has been the biggest employment guarantee program till date. Even in terms of the number of jobs created, MNREGA is unparalleled. In the first eight years of the program, MNREGA generated over 100 million man-days of work.

B Lax monitoring of work provided under MNREGA

The program only defines the type of work to be provided but does not set any measurable output target. The task of monitoring the work is left entirely to the local bureaucracy. India’s official auditor, the Controller and Auditor General of India (CAG), has raised several concerns regarding the quality of assets created under the program and cites poor monitoring as one of the primary reasons for the adverse findings (Siddhartha (2008)). A 2013 program impact assessment report¹⁵ of the auditor states, “the joint physical verification of works in...revealed that the works of installation of hand pumps, construction of tree guards, plantations, construction of roads in full length..etc.,...were not executed though shown completed in records.” The report further states, “...the state government accepted the aforesaid audit observations for three districts.” A 2013 report by PRS India¹⁶ states that only 15.02% of the works get completed under MNREGA. Several newspaper reports have also corroborated these findings. Some have even pointed out a trend of increased absenteeism from MNREGA work.¹⁷

¹⁵The full report is available at <http://www.cag.gov.in/content/report-no-4-2013-performance-audit-mnrega-government-uttar-pradesh>

¹⁶PRS is a non-government organization, which tracks the performance of people’s representatives at all levels and maintains relevant data. The report is available at <http://www.prsindia.org/theprsblog/?p=3013>.

¹⁷Source:<http://www.business-standard.com/article/opinion/surjit-bhalla-does-nrega-really-work-110032700029> <http://www.thehindubusinessline.com/opinion/article2831793.ece?homepage=true> Kumaraswamy_1.html.

IV Data and summary of statistics

A Data

The data used in this study are compiled from three main sources. Our first and primary data source is ASI, an annual survey of registered factories; this survey has been conducted by the Ministry of Statistics and Program Implementation (MoSPI) since 1960. ASI performs a census survey on all the registered factories with 100 or more workers and conducts a sample survey for the units having less than 100 workers. Our data coverage extends to 20 out of 28 states and 5 out of 7 Union territories of India, spanning across 495 districts.¹⁸ The primary unit of enumeration is a factory in the case of manufacturing industries; a workshop in the case of repair services; an undertaking or a licensee in the case of electricity, gas, and water supply undertakings; and an establishment in the case of tobacco-based industries. Hereafter, we will refer to all these establishments as “factories.”

Our data spans the period 2002 to 2010. The factory identifiers are provided for each observation, which lends itself well to a panel structure.¹⁹ For the purpose of our analysis, we restrict our sample to all those census factories that are wholly privately owned and exclude any establishment with a government stake.²⁰ Among other things, ASI provides detailed information regarding the number of workers segregated by the nature of employment (contract, permanent, supervisory/managerial), the number of man-hours worked by each worker type, and total wages and benefits per worker type. The other variables of our interest are the fixed assets (such as land, building, plant and machinery, and computers), capital expenditure on investments, total output (sales), and total input costs. In Table (1), we provide a detailed definition of the main variables used in our study.²¹

In Table (A1) of the online appendix, we provide details regarding our sample construction. As mentioned above, we consider only those factories that are a part of the ASI census. These are the factories that are covered by ASI data every year allowing for a

¹⁸Following [Chaurey \(2015\)](#), [Imbert & Papp \(2015\)](#), and [Bertrand, Hsieh & Tsivanidis \(2015\)](#), we leave out the state of Jammu and Kashmir; seven northeastern states, Mizoram, Meghalaya, Arunachal Pradesh, Sikkim, Nagaland Tripura, and Manipur; and two union territories, Andaman & Nicobar Islands and Lakshadweep. Most studies in India exclude these states as special laws are applicable in these states. These states together constitute less than 13% of India’s geographical area. In total, our dataset covers 495 out of 546 districts of India, as of 2002.

¹⁹The panel structure is available till 31st March, 2008. Post-2008, we follow the approach as cited in [Harrison, Martin & Nataraj \(2013\)](#)

²⁰Census factory data comes under the purview of the Industrial Disputes Act (IDA), 1947.

²¹ASI data does not report firm identifiers. Hence, we cannot match an establishment to a firm. Therefore, we use only factory related performance variables constructed using ASI’s tabulation scheme for each year.

panel-structure. This leaves us with 1,26,586 factory-year observations. MNREGA was first implemented in less developed regions and later extended to relatively more developed regions. Therefore, it is reasonable to expect industrial activity and, consequently, the number of factories to be lower in phase-1 districts and keep increasing phase-wise. Consistent with this idea, 17,554 factory-years are from 162 phase-1 districts, 16,725 are from 103 phase-2 districts and 92,307 are from 230 phase-3 districts.

Second, we collect information regarding labor expenditure, the number of ongoing and completed projects and total employment demanded under the MNREGA from the website of the Mahatma Gandhi National Rural Employment Guarantee Act.²² The unit of observation for this data is a district-year and spans the period between fiscal years 2006 and 2010. This information is available from the fiscal year 2006–07 because the first stage of MNREGA was implemented on February 2, 2006. Third, we gather additional data on the ruling party in each state from the Election Commission of India. These datasets are used for our robustness test, based on the Instrumental Variable (IV) strategy. We explain our IV methodology in greater detail in the online appendix. Finally, we collect data about night lights from the India lights platform.²³

In Table (A1), we provide a brief summary of the data used for easy reference.

B Summary of statistics

Table (2) provides the summary of statistics for the key variables used in this study. The mean (median) number of permanent workers employed in a factory is 173 (72). While on an average, a factory employs 67 contract workers, the median factory does not employ any contractual worker. The mean (median) number of supervisory workers employed turns out to be 27 (7). The mean (median) wage per permanent worker per year works out to be nearly INR 54,247 (40,091), which translates to a daily wage of approximately INR 174 (128).²⁴ For contract workers, the mean (median) annual wage is INR 40,831 (35,682). This translates to a mean (median) daily wage of INR 111 (98). It is noteworthy that the minimum wage stipulated in India is INR 60–90.²⁵ Therefore, a typical permanent worker draws more than the minimum wage (the wage rate applicable to MNREGA). It is not surprising to observe

²²<http://www.nrega.nic.in/> [Accessed in October, 2016]

²³The night lights data is available here: <http://india.nightlights.io>.

²⁴We assume a 6-day working week, which is a norm in Indian factories and assume 52 weeks in a year.

²⁵<http://nrega.nic.in/wages.pdf> [accessed October, 2016]

that the wage of supervisors (managerial worker) is much higher than that of a worker. The mean (median) annual wage of a supervisor is INR 215,380 (138,658).

There is a huge variation in the value of gross fixed assets and plant & machinery additions. The standard deviation is INR 939.7 million and 774.7 million, respectively. The mean (median) value of fixed assets additions and plant and machinery additions is INR 62.27 million (INR 2.06 million) and INR 35.7 million (INR 0.65 million), respectively. We observe that similar variation exists in the ratio of new capital investments per worker; with a mean (median) of INR 1.5 million (INR 0.46 million) and a standard deviation of INR 6.29 million. The mean (median) of the total output produced in the factory, total input of a factory, and profits in INR million is 350.9 (92.34), 229.8 (58.87), and 121.09 (16.67), respectively.

V Empirical strategy

Our empirical tests proceed in three steps. First, we examine if indeed MNREGA crowds out labor supply to factories. Second, using MNREGA as an exogenous labor supply shock, we focus on analyzing the response of factories to this shock. Here, we also examine the cross-sectional heterogeneity in the response of these factories. These tests not only help us in effectively understanding the economic impact of the program but also serve as robustness tests to rule out alternative explanations. Third, we study how the changes in labor supply and demand for capital impacts firm productivity.

As noted in Section (III.A), the program was implemented in a phase-wise manner. It was implemented in 200 districts on February 2, 2006, in 130 districts on April 1, 2007, and, finally, in remaining 285 districts on April 1, 2008. Our empirical design exploits this staggered implementation by estimating a generalized version of the difference-in-differences setup for multiple treatment groups and time periods (See [Bertrand & Mullainathan \(1998\)](#) and [House & Shapiro \(2006\)](#)). Here, a district remains in the “control” group until the program is implemented in that district and, subsequently, becomes a part of the treatment group. For example, all phase-1 districts belong to the control group during fiscal years 2002 to 2006 and serve as a part of the treatment group from the year 2007 onwards. As noted in Section (IV.A), our data spans the period between fiscal years 2002 and 2010. Therefore, we have information for up to four years before the launch of MNREGA and four years after its launch.

Formally, our baseline regression specification is as follows:

$$(1) \quad Y_{it} = \alpha + \nu_i + \delta_t + \gamma \times X_{it} + \beta \times \text{Post-MNREGA} + \epsilon_{it}$$

The unit of observation is a factory-year. *Post-MNREGA* is a dummy variable, which takes the value of one for the factories located in the treated districts in the Post-MNREGA implementation period. For example, if MNREGA was implemented in the district wherein the factory A (B) is located, in the year 2006 (2008), then *Post-MNREGA* would take the value of one for factory A (B) in the years after 2006 (2008). X_{it} refers to the factory level controls. ν_i is a vector of factory fixed effects that absorbs all time-invariant differences across factories. Finally, the δ_t refers to year fixed effects that control for aggregate macroeconomic shocks and trends. Standard errors are clustered at the district level and are adjusted for heteroscedasticity.

The coefficient of interest is β , which captures the change in outcomes in factories located in a treated district before and after the implementation of MNREGA relative to factories located in all other districts that do not implement MNREGA at the same time.

A Identifying assumptions

The identifying assumptions for the consistency of β are two-fold. First, the usual caveat for identification in a difference-in-differences setting requires the presence of parallel trends in our outcome variables of interest (labor and capital investments) before the MNREGA implementation across factories located in the treatment and control districts. We formally verify this assumption in section (VI.D.1).

Second, we require that other than the implementation of MNREGA, there are no other contemporaneous changes in district-level policies or local economic conditions that could differentially impact labor and capital investments across the control and treatment districts. In other words, the phase-wise MNREGA implementation itself is not endogenous. Admittedly, the districts were selected into various phases and not randomly assigned. Therefore, it is important to consider and address the possibility of time-varying unobservable factors, which could have an impact on outcomes. For example, if MNREGA was implemented as a remedial response to an anticipated decline in the private sector employment, then a decline in factory employment that we document would have ensued even

without the implementation of the program. While the absence of differential pre-trends helps us allay these concerns to some extent, we also use an instrumental variable (IV) strategy to further strengthen the causal interpretation of our findings.

We discuss these identification issues in greater detail and our formal tests (including IV) to address such concerns in section (VI.D).

VI Results

A MNREGA and labor supply

As noted in section (V), we start our analysis by first examining the possible impact of MNREGA on formal employment in the private sector. Specifically, we analyze the impact of MNREGA on the number of both permanent and contract labor employed in factories. It might appear that the impact of MNREGA would be more on contract workers. This is because such workers can work under MNREGA for 100 days and continue with their factory work during other days. It also appears that the permanent workers may not be impacted by MNREGA as participation in MNREGA activities would require them to leave their permanent job and all associated benefits. Moreover, MNREGA pays only minimum wages. On the other hand, factories not only pay more than the minimum wage, on an average, but also provide an opportunity for learning and, hence, human capital development.²⁶ In such a case, a priori, it would seem that MNREGA should attract only contract workers.

However, the reality may be more nuanced. First, while the average wages in factories is greater than MNREGA wages, there will be a substantial variation in wages received by different workers within a firm (Mueller et al. (2017)). It is likely that workers leaving permanent employment for MNREGA are those at the lower end of the wage distribution. Second, if poor government monitoring of the work provided under MNREGA would drive workers to invest minimal efforts in accomplishing a task and provide benefits of working very close to home, lower incidental expenses, and lower the chance of accidents, among others, then even a permanent worker may consider the option of working for MNREGA for 100 days. In other words, utility from factory wages net of additional benefits and expenses may be

²⁶Our calculations show that the average wage of a factory worker is 80–100% higher than the minimum wage and that an average worker works for about 295 days in a year. The pre-implementation average wage of a permanent worker is around INR 125 per day. However, as per the MNREGA wage document <http://nrega.nic.in/wages.pdf>, the minimum wage varies around INR 60–75 per day.

lower than the MNREGA wages, and hence a permanent factory worker may find work under MNREGA more attractive. He may either work as a contract worker in factories or do odd local jobs during other days of the year.²⁷ It must be noted that, contrary to permanent workers, contract workers can simultaneously work for factories as well as for MNREGA. Thus, whether MNREGA crowds out employment in the private sector and what type of formally employed workforce (permanent or contractual) it attracts is an empirical question.

In Table (3), we examine the impact of MNREGA on the total number of permanent and contract workers employed in factories. In panel A, we report the estimates from our baseline regression (1) using the number of workers as the dependent variables. In panel B, we report the estimates using the natural logarithm of the number of workers as the dependent variable.²⁸ In columns (1) and (2) of both the panels, we report the results for permanent workers, and in columns (3) and (4) (5 and 6), we report the results for contract (managerial) workers.²⁹

Focusing on columns (1) and (2), the negative and significant coefficient on *Post-MNREGA* indicates that MNREGA leads to a decline in the number of permanent workers employed in factories located in the treatment districts relative to factories located in the control districts. The number declines by 16.68 (17.4) in column (1) ((2)). Given that the mean (median) number of permanent workers in a factory is 173 (72), the above decline is economically significant as well. In terms of percentage, the estimates translate into an approximately 10% (23%) decline in permanent workers for the mean (median) factory.

Results presented in columns (3) and (4) show that the number of contract workers does not change materially. These results do not unequivocally imply that contract workers remain unaffected by MNREGA. It must be noted that ASI data reports the average number of workers employed by a firm in a year, based on the number man-days of work reported, by the firm in a year and not the unique number of individuals employed. For instance, if a contract worker A works for the first six months and another contract worker B works for rest

²⁷Please note that Indian factories employ both “permanent” and “contract” workers (Bertrand et al. (2015)). Almost all labor laws are applicable mostly for permanent workers, and hence factories have less flexibility with respect to such workers. It is not possible for a permanent factory worker to work under MNREGA if the worker retains that employment status. Such workers, if attracted towards MNREGA because of reasons mentioned above, may choose to quit the permanent factory job, perform MNREGA work for 100 days, and choose a different occupation remaining working days.

²⁸We take logs because the distribution of workers is log-normal.

²⁹The ASI data classifies permanent and contract workers as productive workers. The term “productive” is used in the sense that these workers are directly engaged in production. The term does not imply higher efficiency or productivity in the conventional sense.

of the six months during a year, then the ASI data would count it as one contract worker on an annual basis. As discussed above, it is possible that a fraction of permanent workers, who leave their factory jobs and work for MNREGA, may work as contract workers during the non-MNREGA workdays. Therefore, a plausible outflow of contract workers to MNREGA may be offset by a plausible inflow of former permanent workers, thus limiting our ability to empirically identify the impact on temporary workers.

Finally, an analysis of columns (5) and (6) does not reveal any impact of MNREGA on the number of managerial workers employed in factories. This result serves as a placebo test and further strengthens the causal interpretation of our findings. Since MNREGA is targeted towards poorer low-skilled manual workforce, we do not expect it to impact the managerial staff. The results reported in Panel B are qualitatively similar to the results presented in Panel A.

Overall, our findings indicate that MNREGA is associated with a decline in the permanent workforce (and not in the contractual or managerial workforce) employed in factories. The decline is consistent with a fall in labor supply because of MNREGA. However, one can also argue that this decline may potentially be driven by a decline in the labor demand by the factories due to an adverse economic shock. Therefore, in our next set of tests, we seek to rule out one of the possible explanations.

1 What drives the movement of permanent workers?

As mentioned before, it is not immediately evident that MNREGA should cause attrition in the permanent workforce. Thus, the impact of MNREGA on permanent workers needs more discussion and analysis. Theoretically, for workers to leave the security of a permanent job, the non-pecuniary benefits from working under MNREGA such as being close to home has to be significantly large. Consistent with this idea, a recent paper [Banerjee & Chiplunkar \(2018\)](#) estimates the compensating differential that relatively low skilled workers need to accept an out-of-state job. They find that male (female) workers on average require 76% (135%) higher wages for an out-of-state job relative to similar within state jobs. Moreover, these workers can still join as contract workers in factories after they complete 100 days of work under MNREGA. The World Bank enterprise survey data for INDIA provides some evidence consistent with this thesis.³⁰ The survey asks establishments various questions

³⁰<https://www.enterprisesurveys.org/data/exploreeconomies/2014/india>

related to their financing, operations, workforce, etc. One of the questions relevant to our context is “Any production worker left the job for 1-2 months and then worked at the same factory?”. 62% of the respondents answer in affirmative to this question. The survey then further asks, “Why do these workers leave their jobs?”. About 60% of the respondent state that these are agricultural workers that return home. To the extent that MNREGA provides opportunities to work close to home for three months, permanent workers may leave for home for a few months and rejoin as contract workers subsequently. Next, we perform additional cross-sectional and time-series test to understand the drivers underlying the movement of permanent workers.

2 Impact based on level of wages and productivity

Theoretically, we should expect to see a more significant impact of MNREGA on permanent workers when the relative benefit of the permanent job is low. First, to the extent that MNREGA pays only minimum wage and provides low skill work, the benefit from moving to MNREGA should be greater for workers earning lower wages, and those with low labor productivity. While we do not have estimates of productivity or wages at the worker level, we exploit cross-sectional heterogeneity across firms in wages and productivity in these tests. Specifically, we first calculate the average yearly wages of permanent workers at the factory level and average wages per permanent worker at the district-industry level in the pre-MNREGA period. Subsequently, we split our sample into two and classify those factories as low (high) wage factories wherein the average permanent wage is above (below) the district median for the industry to which the factory under consideration belongs. We then repeat our baseline tests on these sub-samples. Column (1 (2)) of Table (4) reports the results for the sub-sample of low (high) wage factories. We find that the decline in the number of permanent workers is limited to factories with low wages. In such factories, the number of workers declines by 2.7%. We do not observe any significant change in the number of workers in factories where workers fall under the high wage category.

Similarly, in columns (3) and (4) of Table (4), we split our sample of factories into two, based on whether the labor productivity measure (total output per worker) is above (high productivity) or below (low productivity) the median output per worker at the industry level in the pre-MNREGA period. In line with our expectation, again, we find a decline in the permanent workforce only for the sub-sample of factories with low worker productivity.

Overall, the results presented above show that MNREGA crowded out labor supply in factories with low wages and low labor productivity.

3 Impact based on labor laws and industry volatility

Next, we exploit the idea that the expected value of a permanent job is likely to be lower for workers if they expect greater job uncertainty. We use two proxies for expected certainty of a permanent job. Our first proxy is based on the idea that the risk of a job loss should be lower in states where labor laws makes it costlier to fire employees. Although labor laws in India are stringent in general, there is significant variation at the state level. [Besley et al. \(2004\)](#) and [Aghion, Burgess, Redding & Zilibotti \(2008\)](#) classify Indian states into those with an employer and employee-friendly labor laws based on their stringency.³¹ Employee friendly states have labor laws that make compulsorily retrenchment of permanent employees almost impossible. It is, therefore, reasonable to hypothesize that the relative benefits of full-time permanent employment are likely to be higher in such states. Thus, the benefits of working under MNREGA have to be particularly greater for workers to leave their permanent jobs in these states. In contrast, the benefits of working under MNREGA is likely to be higher for workers at the margin in states with employer-friendly laws. Our tests reported in columns (5) and (6) of Table (4) provide evidence consistent with this thesis.

Along similar lines, our second proxy for job uncertainty exploits the idea that the risk of a job loss should be higher in more volatile industries. We follow [Dougherty, Frisancho & Krishna \(2011\)](#) and measure industry volatility by the standard deviation of the annual growth rate of factories' output. In columns (7) and (8) of Table (4), we again split our sample into factories in industries with above (column 7) and below (column 8) median industry volatility and repeat our baseline tests. Consistent with our conjecture, we find a decline in the number of permanent workers only for the sub-sample of factories in industries with high output volatility.³²

4 Impact based on expectations regarding continuation of MNREGA

A critical consideration for a permanent worker to leave the job and move to MNREGA work is the relative permanence of the program itself. Very few permanent workers

³¹We note that the labor regulation measures are not available for five union territories and two states. This reduces our sample by 13,755 (11%) factory-year observations, removing 3,765 (12%) factories.

³²In unreported tests, again we do not find any significant impact of MNREGA on contract workers.

are likely to leave their job if they think that the program is short term in nature and is likely to be repealed as soon as there is a change of guard at the federal level. However, as we have discussed in section (III.A), given the typical features of the program such as its enactment through a legislation and political economy-related factors, even a person with no foresight about the outcome of the future election may be reasonably assured that the program will outlive the tenure of the government that implemented it.

Nonetheless, to address any remaining concerns, we conduct tests using the re-election of the incumbent government in the year 2009. Any residual uncertainty among workers regarding the continuation of the program were alleviated after the re-election of the ruling party that initiated MNREGA. Since the program was rolled out in all districts by the year 2008, we cannot use our baseline staggered difference-in-differences strategy. Therefore, in these tests, we focus on the treated sample and exploit cross-sectional heterogeneity in the intensity of treatment to examine the effect of 2009 elections on worker outflow. Specifically, we divide the sample into “treatment” and “control” groups based on the ex-ante (pre-2009) exposure of the district (in which a factory under consideration is located) to MNREGA program. We use the total MNREGA labor expenditures scaled by the number of formal workers in a district as our exposure measure and classify the firms located in districts in the upper (lower) tercile based on our exposure measure as the treatment (control) group. We test the differential change in outcome between the treatment and control group around the 2009 elections. Formally, we estimate the following regression equation:

$$(2) \quad Y_{idt} = \alpha + \nu_i + \delta_t + \beta_1 \times \text{HighExp}_{dt} + \beta_2 \times \text{Post}_{it} + \beta_3 \times \text{Post}_{it} \times \text{HighExp}_{dt} + \varepsilon_{it}$$

where i refers to a firm, d refers to the district and t refers to a year. *HighExp* is a dummy variable that takes the value of one for high exposure districts and zero otherwise. *Post* is a dummy variable that takes the value of one for the year 2009-2010 and zero otherwise. As in our baseline specification (equation (1)), we control for firm characteristics, factory and year fixed effects in all specifications. The errors are clustered at a district level. The coefficient of interest is β_3 , which captures the differential effect of 2009 elections on regions more exposed to MNREGA. Table (5) presents the results from these tests. Focusing on column (1), we find a more significant decline in the permanent workforce in factories

located in more exposed districts relative to less exposed districts.

In column (2) of Table (5), we examine the effect of MNREGA on contract workforce. The effect of MNREGA should be stronger after 2009 elections, thus allowing us to detect the impact on contract workers as well. Exploiting cross-sectional variation in the intensity of MNREGA treatment, in these tests, we do find an economically and statistically significant decline in the number of contract workers in regions with greater MNREGA exposure.

Summarizing, the results presented in this section helps us strengthen the interpretation of our finding regarding the impact on MNREGA on the labor supply of workers.

B MNREGA and wages

In this section, we attempt to rule out that the decline in permanent workers is driven by an adverse economic shock to firms resulting in reduced demand for labor. It is important to note that India's labor laws are rigid, and hence laying off permanent workers can prove to be very costly (Besley et al. (2004)). Hence, even if firms face economic shocks, laying off permanent workers is difficult. Moreover, it would have to be the case that any such adverse shock affects factories in the treatment group more than the control group. Nonetheless, if our results are driven by a decline in the demand for labor, then it would be reasonable to expect a decrease in wages paid to the remaining permanent workers. We test the above proposition using the equation (1) and report the results in Table (6).

The dependent variable in these tests is the amount of wages and natural logarithm of wages in Panels A and B respectively. In columns (1) and (2), the dependent variable is the natural logarithm of the average yearly wage paid to permanent workers. Columns (3) and (4) ((5 and 6)) report the results pertaining to the wages of contractual (managerial) workers. We employ factory- and year- fixed effects in all specifications. In columns (2), (4), and (6), we employ additional factory level control, as described in section (V).

We find that the wages of all the three categories of workers do not change significantly as a result of the implementation of MNREGA. These results provide support for our claim that the decline in the number of permanent workers is unlikely to have been driven by a decline in the labor demand by factories. However, at the same time, it is also important to note that an adverse labor supply shock should potentially have increased wages. While we do find a positive impact on wages, these estimates are statistically indistinguishable from

zero. Alternatively, it is possible that rather than increasing wages, factories may prefer to respond by moving towards mechanized methods of production that are less labor intensive. Therefore, an examination of whether factories respond to MNREGA by increasing capital investments is warranted at this stage.

C MNREGA and capital investments

In this section, we examine the response of factories to the potential labor supply shock pointed out above. When faced with a labor supply shock, factories may respond by either increasing wages or by resorting to mechanization. In other words, factories may prefer to substitute labor with capital. To investigate this thesis, we re-estimate the specification (1) in Table (7). We use four measures as proxies for mechanization by factories and use them as dependent variables in each regression. In column (1 (and 2)), (3 (and 4)), (5 (and 6)), and (7 (and 8)), we use natural logarithm of gross additions to fixed assets, gross additions to plant and machinery, capital per worker and total expenditure on rent, and lease on plant and machinery, respectively, as dependent variables. We find that the addition to fixed assets increases by 11.82%, addition to plant and machinery increases by 23.23%, capital per workers increases by 2.3%, and, finally, expenditure on rent and lease expenses on plant machinery increases by 24.87%. These results show that factories respond to labor shock by resorting to mechanization.

To further strengthen the causal identification, we build on our findings reported in sections (VI.A.2) and (VI.A.3). Specifically, we want to examine if the increase in mechanization post-MNREGA is particularly greater for the sub-sample of factories that experienced a decline in permanent workers. Panel (B) of table (7) reports the estimates for these tests. Consistent with the results reported in sections (VI.A.2) and (VI.A.3), we find that the increase in mechanization is especially greater for factories paying lower wages, associated with low labor productivity, located in states with pro-employer labor laws, and more volatile industries. Further, using a natural experiment that led to change in the definition of a small and medium enterprise and hence exogenously altered the access to formal finance for some firms that depend on the directed lending program, we find that the incremental capital expenditure made by firms in response to MNREGA, critically depends on access to formal finance. These results are reported in Table (A2) and discussed in section (AI) of the online appendix.

These findings also help explain our results concerning wages. Our results indicate that factories prefer to move toward mechanized modes of production rather than increasing wages. Importantly, these results provide compelling evidence against the idea that the decline in permanent employees is driven by a demand shock. If the factories in our treatment group were subject to an adverse economic shock, then we would have expected a decline in both labor and capital.

Summarizing the results so far, we find that a public works program, such as MNREGA, crowds out employment in the private sector, and firms respond to the consequent labor supply shortage through mechanization. It is important to note that, given our difference-in-differences setting for exploiting the phase-wise implementation, any aggregate shock that affects all districts simultaneously should get differenced out. It limits the set of potentially confounding policy changes and other economic shocks. A shock can only confound our estimates if it either affects the districts sequentially in a staggered fashion or its adverse impact on employment is greatest on phase-1 districts followed by phase-2 and phase-3 districts. Nonetheless, to further strengthen our thesis that MNREGA resulted in a labor supply shock and the factories responded by increasing mechanization, we test for the absence of differential pre-trends and verify these findings using an IV approach. We discuss these in detail in the next section.

D Identification concerns and alternative interpretation

Our causal claims face two main identification challenges. First, there could be a pre-existing trend in our outcomes of interest. For instance, factories may be investing already in their plant and machinery while reducing their dependency on the permanent workforce. Therefore, in this case, the DID will capture the differential growth experienced in the pre- and post-periods of MNREGA implementation. Second, the implementation of the program may have followed a countercyclical pattern. These concerns relate to the selection of districts in various phases. Suppose, the selection of a district for MNREGA implementation coincides with an adverse economic shock, then the decline in factory employment may be an artifact of the existing economic situation.

1 Ruling out the pre-existing trend

Any result from a difference-in-differences test is subject to the caveat that the result may be driven by pre-existing trend differentials between the treatment and control groups. We test the existence of a pre-existing trend by estimating the following distributed lag model:

$$(3) \quad \ln[1 + y_{idt}] = \beta_0 + \sum_{-5}^{-2} \theta_k \times Pre[k]_{idt} + \sum_0^3 \theta_k \times Post[k]_{idt} + \delta_i + \gamma_t$$

The dependent variable in these tests is the natural logarithm of permanent workers. $Post[k](Pre[k])$ is a dummy variable that identifies the year $T+k$, where T is the year of MNREGA implementation. For example, if MNREGA is implemented in the year 2008 in a district A, then the dummy variable representing the Pre_{-2} year would take the value of one for factories located in that particular district for the year 2006. The coefficient θ_0 measures the immediate DID effect of MNREGA on the dependent variable. The *marginal* coefficients θ_1 (θ_2) measure the *additional* marginal responses one year (two years) after the implementation year of the MNREGA. Similarly, coefficients $\theta_{-2}, \dots, \theta_{-5}$ capture the difference of trends for each of the dependent variables between the treatment group and the control group in each of the four pre-treatment years.

Figures (2) and (3) graph the entire paths of coefficients, and the dotted lines depict the corresponding 95% confidence intervals. In essence, these graph plots the DID estimates that show the difference between the firms in the treated group and the control group over time. All these coefficients are relative to the year before the implementation of MNREGA in the respective factory's districts. It must be noted that in the years leading up to the year of implementation of the program, we do not observe a statistically significant difference between the treated and the control firms. This clearly rules out the existence of any pre-trend. We find that the trend changes direction after the implementation of MNREGA. Specifically, there is a statistically significant decrease in the number of permanent employees post-MNREGA.

To further rule out the existence of a pre-trend, we perform a placebo test. It must be noted that MNREGA was implemented at the beginning of the fiscal year 2007 (February 2, 2006), 2008 (1st April, 2007), and 2009 (1st April, 2008) in three phases. We use the placebo implementation years and estimate the regression equation (1). The results are reported in Table (A3) of the online appendix. In panel A, we use fiscal years 2003, 2004, and 2005, as

placebo years, representing the three-phased implementations. In panel B, we use 2004, 2005, and 2006 as placebo years. Reassuringly, we do not detect a significant effect when we use the placebo treatment years. We only report the estimates from the analysis with controls in Table (A3) for brevity. However, our results are qualitatively similar without controls as well.³³ Finally, we also test the robustness of our findings using an IV approach. We present the details in section (AII) of the online appendix.

Overall, these tests help us allay any concerns regarding the possibility of our estimates being confounded by the pre-existing trend differences between factories located in the treatment and control districts.

E Impact of MNREGA on factory level performance

We now examine the impact of MNREGA on the operating performance of factories.

1 Impact on factory output

Generally, mechanization is expected to lead to an improvement in operational efficiency. However, in this case, mechanization is caused by a labor supply shock. By revealed preference, factories were better off without the increased mechanization that ensued after MNREGA. Moreover, the marginal productivity of factory labor is positive in India (House & Shapiro (2006)). Therefore, in this context, it is reasonable to expect that mechanization driven by a labor supply shock is likely to cause a deterioration in the operating performance at least in the short-term. On the other hand, it is also possible that MNREGA turns out to be a blessing in disguise by allowing factories to shed workers. Given the rigid labor laws, it would have been extremely difficult to retrench workers. It is possible that less productive workers left on their own due to MNREGA, and hence, benefited the firms indirectly. We use the natural logarithm of total input (total cost of production); output; earnings before interest, taxes, depreciation, and amortization (profit); and output to input ratio as performance metrics. We use the cash ratio as a metric for liquidity. Formally, we re-estimate our baseline regression (1) using these measures as dependent variables

The results are reported in Table (8). The dependent variables are the natural logarithm of the cost of production (column (1) & (2)); gross output (column (3) & (4)); earnings before interest, taxes, depreciation, and amortization (profit) (column (5) & (6)),

³³These results are available upon request.

cash ratio (column (7) & (8)), and output to input ratio (column (9) & (10)). We find that the cost of production increases by 7.5–7.6% (columns 1–2) and the profitability declines by 50 basis points (columns 5–6) post-MNREGA. We do not find any impact on the total output of firms; this finding further rules out the likelihood of our results being driven by an adverse economic shock. At the same time, we observe a significant decline in the output to input ratio, which is a measure of efficiency.

In summary, these results show that MNREGA leads to a decline in the operating performance of factories.

2 Impact on Working Capital

The impact of forced mechanization on working capital is not clear ex-ante. On the one hand, due to minimum batch size requirements and high set up costs, mechanization may lead to a mismatch between the quantity demanded and quantity produced, and hence may lead to an increased inventory. On the other hand, mechanization may facilitate implementation of just-in-time production and inventory and other efficiency improving methods due to quick turnaround time. To examine the above two conflicting hypotheses, we re-estimate equation (1), using the inventory cycle, defined in terms of the number of days, as the dependent variable. The results are reported in columns (1) and (2) of Table (9). We find that the inventory cycle shortens by approximately 25 days. Subsequently, we examine the impact on the number of days it takes to collect the debt. Since there is a change in the production technology, prima-facie, one may not expect an improvement in the debtor cycle. However, it is possible that the standardized production system may reduce the time required for inspection and for returning goods by customers, and hence lead to quick payment. We report the results pertaining to the debtor cycle in columns (3) and (4) of Table (9). Here, we find an improvement of about 2 days in the debt recovery cycle. The improvement, although statistically significant, does not translate into an economically meaningful number.

Finally, we combine the two results and calculate the impact of the total working capital cycle. We report the results in columns (5) and (6) of Table (9). We find that the total working capital cycle improves by about 27 days.

F Understanding the Impact of MNREGA on Firm Performance

As discussed in section (VIE.1), the impact of MNREGA on firm performance is not immediately evident. To the extent that labor laws in India are rigid and make it very difficult for firms to fire workers, MNREGA could potentially be a boon in disguise by allowing firms to shed less-productive workers. On the one hand, this means that the firms may be left with relatively more productive workers ex-post, resulting in a positive impact on firm performance. On the other hand, the costs associated with replacing cheap labor with machinery may still result in lower profits in the short run. However, to the extent that the increased mechanization caused by MNREGA is associated with an increase in operational efficiency, it may also result in increased profitability in the long run. While we acknowledge this possibility, given the nature of our empirical design, we are able to only estimate the short-term impact of MNREGA on the profitability of firms. Nonetheless, we do perform some analysis that helps us better understand this issue.

First, we note that if MNREGA indeed allows firms to circumvent stringent labor laws and shed workers, such effects should be greater in states with *employee-friendly* labor laws (Besley et al. (2004)). However, our results reported in Table (4) and (7) show that the attrition of workers and the increase in capital investments is greater for firms located in states with relatively *employer* friendly labor laws. This result is not consistent with the idea that MNREGA allowed firms to benefit by shedding excess labor, which they would not have been able to shed otherwise due to stringent labor laws.

Second, exploiting heterogeneity based on firm-level labor productivity in columns (3) and (4) of Table (4), we find a significant movement of workers only from firms with low labor productivity. This result supports the idea of firms benefiting by shedding less productive workers. To reconcile these seemingly contrasting results, we create a sub-sample of low productivity firms operating in states with stringent/less-stringent labor laws. Shedding of workers should be theoretically most beneficial to firms facing stringent labor regulations and employing low productivity workers. In the results presented in column (1), Panel (B) of Table (10), we find no statistically significant decrease in the number of permanent workers for this sub-sample. This result contradicts the view that firms benefit from shedding excess workers. This is because there is a potentially counteracting force at work, given the favorable laws, workers in such locations may prefer not to leave their jobs for MNREGA. Indeed, we

find that the outflow of permanent workers is especially greater for low productivity firms located in states with employer-friendly labor regulations. Focusing on columns (1) and (2), of Panel (A) of Table (10), we find a decline in permanent workers only for low-productivity firms operating in states with employer-friendly labor laws.

Next, we examine whether low-productivity firms located in states with stringent labor laws, benefit from the movement of workers. We present the results in columns (3) and (4) of Panel (B) of Table (10). We do not find any significant negative impact on profits or working capital cycle for this set of firms. However, it is still possible that even in states classified as having employer-friendly labor regime, the laws are still constraining as compared to a theoretical first-best benchmark. So at least, in such states, we should expect to see an increase in profitability caused by the voluntary outflow of the permanent workforce. In columns (3) and (4) of Panel (A) of Table (10), we examine the performance of low-productivity firms located in states with less-stringent labor laws. We find that there is a weaker drop in profitability of such firms as compared to the baseline estimates reported in Table (8). However, we also find a significant improvement in the operational efficiency of such firms. These results suggest that MNREGA may result in increased efficiency in the short run and consequently increased profitability in the long run for these set of firms.

Overall, our results indicate a drop in profitability of firms in the short-run. However, the capital for labor substitution caused by MNREGA does result in increased operational efficiency of firms even in the short run, which in turn can lead to increased profitability in the long run. However, given our empirical setup and data limitations, we are unable to comment on long-run effects of MNREGA.

VII Conclusion

In this study, we analyze the impact of the largest workfare program on labor and firm behavior. Using MNREGA, which compelled the state to provide at least 100 days of employment to all citizens, as our experimental setting, we find that MNREGA crowds out labor supply to factories. The number of permanent workers engaged in factories declined by 10% due to MNREGA. Factories resorted to mechanization in response to the labor scarcity. There was a decline in the operating profitability. Overall, our study shows that when faced with an exogenous labor supply shock firms respond by substituting labor with capital.

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Figure 1: Staggered Implementation of MNREGA

MNREGA was implemented in India in three phases: starting with 200 districts in the beginning of fiscal year 2007 (2nd February, 2006), 130 districts in 2008 (1st April, 2007) and the rest of India (285 districts) in 2009 (1st April, 2008) in three phases. This figure highlights the districts that were covered under in each of the three phases 1,2, and 3.

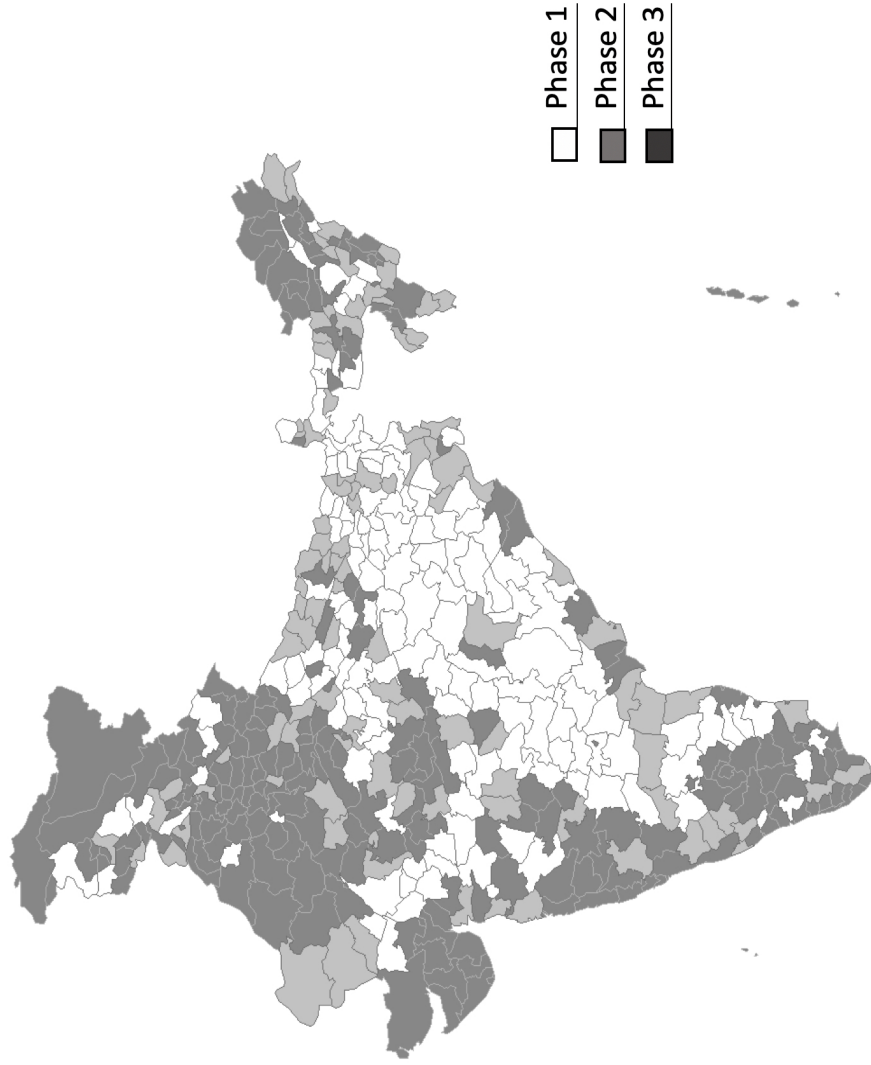


Table 1: VARIABLE DESCRIPTION

The description of variables used in the study is presented below.

Variable	Description
Panel A: Annual Survey of Industries ³⁴	
PERM_WORKER	count; Workers on the factory payroll.
WAGES_PERM	in INR; Yearly wage paid to a permanent worker.
CONTRACT_WORKER	count; Workers hired through contractors
WAGES_CONTRACT	in INR; Yearly wage paid to a contract worker.
FIXED_ASSET_ADD	in INR; Gross additions to the total fixed assets, this includes assets equipment, transport and land.
PNM_ADD	in INR; Gross additions to plant and machinery.
INVESTED_CAP	in INR; Fixed capital and physical capital.
CAPITAL_WORKER	in INR; $INVESTED_CAP / (PERM_WORKER \text{ and } CONTRACT_WORKER)$.
TOTAL_OUTPUT	in INR; Ex-factory value of quantity manufactured + income from services + variation in stocks of semi finished goods + value of electricity generated and sold + value of own construction + net balance of goods sold in the same condition as purchased.
TOTAL_INPUT	in INR; Fuel and material consumption + Operating and non-operating expense + Repairs and maintenance of fixed assets + Insurance charges + Expense on other works.
PROFIT	in INR; (Total output-total input) - depreciation - rent & interest paid - wages, benefits and bonus to employees.
CASH_RATIO	Cash & Cash Equivalents by Total Assets.
OUTPUT_PER_INPUT	Total output per total input.
Post-MNREGA	A dummy variable that takes the value one for factories located in phase 1 districts for fiscal years after 2006 , phase 2 districts for fiscal years after 2007, and phase 3 districts for fiscal years after 2008; and zero, otherwise. ³⁵
Panel B: MNREGA variables for IV regression	
LABOR_EXP	in millions INR; District-wise labor expenditure made each year under MNREGA.
NUMBER_WORKS	count in millions; Total number of ongoing and completed projects every year in each district under MNREGA.
TOTAL_EMP_DEMAND	count in millions; Employment demanded by the MNREGA job card holders. ³⁶

continued ...

³⁴Variables are constructed using the definition from ASI tabulation scheme.

³⁵A fiscal year in India spans April 1st of one year to March 31st of the following year. For instance, the fiscal year 2006 runs from 1st April, 2005 to 31st March, 2006.

³⁶The 100 days of paid MNREGA work per year can only be accessed if they have a MNREGA job card.

... continued

Variable	Description
Panel C: Instrument Variable	
DUMMY_STATE_CENTER	Dummy Variable; 1 if state's ruling party is also the central ruling party, otherwise 0.

Table 2: DESCRIPTIVE STATISTICS

In this table, we report descriptive statistics for the main variables used in our analysis. The sample consists of all open “wholly private owned” factories in the Annual Survey of Industries (ASI) census survey from fiscal year 2002 to fiscal year 2010.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	N	Mean	Q_1	Median	Q_3	SD
PERM_WORKER	126,586	173	19	72	176	453
CONTRACT_WORKER	126,586	67	0	0	32	640
MANAGER_STAFF	126,586	27	2	7	22	111
Total Employees	126,586	299	40	139	304	950
WAGES_PERM (in INR per year)	126,586	54,247	26,284	40,091	63,946	51,360
WAGES_CONTRACT (in INR per year)	126,586	40,831	26,666	35,682	47,972	25,038
WAGES_MANGER_STAFF (in INR per year)	126,586	215,380	75,695	138,658	250,756	356,387
PNM_ADD (in INR Millions per year)	122,847	35.72	0	0.65	6.26	774.70
CAPITAL_WORKER (in INR Millions)	126,574	1.50	0.15	0.46	1.28	6.29
FIXED_ASSET_ADD (in INR Millions)	124,975	62.27	0.13	2.06	13.57	939.69
Total Fixed Asset Gross (in INR Millions)	124,975	419.76	0	21.39	160.11	4,205.98
Debtor Turnover Cycle (1)	1,15,519	112	16.76	46.98	91.21	704.82
Inventory Turnover Cycle (2)	1,26,586	195.64	33.89	74.85	141.21	781.40
Working Capital Cycle = (1+2)	1,26,586	195.16	73.58	134.57	237.34	215.36
TOTAL_INPUT (in INR Millions)	126,586	229.80	9.33	58.87	245.00	380.70
PROFIT (in INR Millions)	126,586	121.09	3.15	16.67	81.92	277.77
TOTAL_OUTPUT (in INR Millions)	126,586	350.90	16.77	92.34	371.60	578.30

Table 3: NUMBER OF WORKERS

This table reports the OLS estimates based on equation (1). The dependent variables in these tests are the number of workers (Panel A) and the natural logarithm of the number of workers (Panel B). A detailed description of variables is provided in Table (1). The parameter of interest is the coefficient on the interaction term Post-MNREGA, which is a dummy variable that takes the value 1 for all treated factories in all fiscal years after the implementation of MNREGA in the district in which the factory is located. In columns (2), (4) and (6), we report the estimates including controls. The controls include firm size (factory size) and age of the factory. The sample consists of all open “wholly private owned” factories in the ASI census survey from the fiscal year 2002 to the fiscal year 2010. The standard errors are clustered at the district level. T-statistics are reported in parentheses. ***, **, * represent statistical significance at 1%, 5% and 10% respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
VARIABLES	PERM_WORKER	CONTRACT_WORKER	MANAGER_STAFF			
Post-MNREGA	-16.68** (-2.5275)	-17.40** (-2.5470)	-1.81 (-0.2474)	-1.92 (-0.2569)	-0.70 (-0.9112)	-0.86 (-1.0849)
Firm size		9.75*** (7.3127)		3.01*** (5.0580)		1.27*** (9.7579)
Age		0.78** (2.3762)		0.03 (0.4119)		0.10*** (3.0306)
Observations	126,586	120,774	126,586	120,774	126,586	120,774
R-squared	0.8928	0.8938	0.9608	0.9613	0.7489	0.7484
Panel B						
VARIABLES	Log(1+PERM_ WORKER)	Log(1+CONTRACT_ WORKER)	Log(1+MANAGER_ STAFF)			
Post-MNREGA	-0.0267* (-1.8876)	-0.0271** (-2.0353)	0.0275 (0.8792)	0.0253 (0.8136)	-0.0022 (-0.1701)	-0.0034 (-0.2750)
Firm size		0.0500*** (14.5783)		0.0419*** (8.4442)		0.0469*** (17.2981)
Age		0.0021*** (3.8004)		-0.0005 (-0.3515)		0.0026*** (3.8986)
Observations	126,586	120,774	126,586	120,774	126,586	120,774
R-squared	0.9269	0.9300	0.8056	0.8081	0.9082	0.9113
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Factory FEs	Yes	Yes	Yes	Yes	Yes	Yes

Table 4: NUMBER OF WORKERS: HETEROGENEITY BASED ON WAGES, LABOR PRODUCTIVITY, LABOR LAWS, AND INDUSTRY VOLATILITY

This table reports the OLS estimates of our baseline equation (1) with $\text{Log}(1+\text{PERM_WORKER})$ as the dependent variable for different subsamples: Column (1) (Column (2)) reports the OLS estimates for the subsample of factories with pre-MNREGA wage per permanent worker below (above) the industry median, within a district respectively. Column (3) (Column (4)) reports the OLS estimates of the factories which have low (high) levels of productivity, i.e., factory's output per worker is below (above) the industry median. In Columns (5) and (6), we split our sample into factories located in states with pro-employer/neutral and pro-employee labor regulation based on Besley et al. (2004) and Aghion et al. (2008). Columns (7) and (8) report the OLS estimates for the factories in industries with above (below) median industry volatility. The parameter of interest is the coefficient on the interaction term Post-MNREGA, which is a dummy variable that takes the value 1 for all treated factories in all fiscal years after the implementation of MNREGA in the district in which the factory is located. The controls include firm size (factory size) and age of the factory. The sample consists of all open "wholly private owned" factories in the ASI census survey from the fiscal year 2002 to the fiscal year 2010. The standard errors are clustered at the district level. T-statistics are reported in parentheses. ***, **, * represent statistical significance at 1%, 5% and 10% respectively.

	Dependent variable: $\text{Log}(1+\text{PERM_WORKER})$							
	Wages Per Permanent Worker		Labor Productivity		Labor Regulations		Output Volatility	
	Below median	Above median	Below median	Above median	Pro-employer	Pro-employee	Above median	Below median
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-MNREGA	-0.0267*	-0.0161	-0.0385*	-0.0048	-0.0304*	-0.0222	-0.0320**	-0.0113
	(-1.9102)	(-0.8537)	(-1.7286)	(-0.3582)	(-1.8226)	(-0.7665)	(-2.1885)	(-0.4757)
Observations	62,653	55,666	56,614	61,701	77,024	29,995	86,953	33,282
R-squared	0.9293	0.9212	0.9321	0.9254	0.9279	0.9272	0.9237	0.9433
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Factory FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 5: IMPACT OF 2009 NATIONAL ELECTIONS

In this table, we examine the impact of MNREGA on the movement of workers before and after the 2009 national elections. The data are organized at a factory year level. The number of PERM_WORKER (CONTRACT_WORKER) is the dependent variable in columns (1) (2)). The sample starts from the year in which MNREGA was implemented in a district. High exposure is a dummy variable that takes the value of one (zero) for districts that are in the upper (lower) tercile in terms of the total MNREGA LABOR_EXP scaled by the total number of workers working in the factories before the year in which national elections were held. Post-2009 is a dummy variable that takes the value of one for the year 2009-2010 and zero otherwise. The controls include firm size(factory size) and age of the factory. The standard errors are clustered at a district level. T-statistics are reported in parentheses. ***, **, * represents statistical significance at the 1%, 5%, and 10% levels.

VARIABLES	Log(1+PERM_ WORKER)	Log(1+CONTRACT_ WORKER)
	(1)	(2)
Post-2009 x High Exposure	-0.0520* (-1.9299)	-0.1392* (-1.9211)
Observations	12,326	12,326
R-squared	0.9828	0.9461
Year FEs	Yes	Yes
Factory FEs	Yes	Yes

Table 6: WAGES PER WORKERS

This table reports the OLS estimates based on equation (1). The dependent variables in these tests are the wage per worker (Panel A) and the natural logarithm of wage per worker (Panel B). A detailed description of variables is provided in Table (1). The parameter of interest is the coefficient on the interaction term Post-MNREGA, which is a dummy variable that takes the value 1 for all treated factories in all fiscal years after the implementation of MNREGA in the district in which the factory is located. In columns (2), (4) and (6), we report the estimates including controls. The controls include firm size (factory size) and age of the factory. The sample consists of all open “wholly private owned” factories in the ASI census survey from the fiscal year 2002 to the fiscal year 2010. The standard errors are clustered at the district level. T- statistics are reported in parentheses. ***, **, * represent statistical significance at 1%, 5% and 10% respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
VARIABLES	WAGES_PERM		WAGES_CONTRACT		WAGES_MANAGER_STAFF	
Post-MNREGA	273.07 (0.2781)	168.38 (0.1849)	220.76 (0.5463)	177.26 (0.4510)	-6,356.35 (-1.1435)	-6,508.77 (-1.2390)
Firm size		1,225.40*** (15.0748)		702.11*** (10.4869)		8,196.99*** (11.5943)
Age		69.80*** (2.7516)		-19.7 (-1.1848)		355.68 (1.5665)
Observations	126,586	120,774	126,586	120,774	126,586	120,774
R-squared	0.8466	0.8493	0.6752	0.6774	0.5473	0.5639
Panel B						
VARIABLES	Log(1+ WAGES_ PERM)		Log(1+ WAGES_ CONTRACT)		Log(1+ WAGES_MANAGER_ STAFF)	
Post-MNREGA	0.0004 (0.0409)	0.0008 (0.0888)	0.0354 (0.5262)	0.0289 (0.4354)	-0.0070 (-0.1922)	-0.0121 (-0.3403)
Firm size		0.0382*** (19.0036)		0.0897*** (8.7878)		0.0729*** (11.5129)
Age		0.0007** (1.9880)		-0.0019 (-0.5848)		0.0000 (0.0086)
Observations	126,586	120,774	126,586	120,774	126,586	120,774
R-squared	0.8260	0.8285	0.7704	0.7728	0.7625	0.7657
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Factory FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 7: MECHANIZATION OF FACTORIES

This table reports the OLS estimates based on equation (1). The dependent variables in these tests are different measures of mechanization. A detailed description of variables is provided in Table (1). The parameter of interest is the coefficient on the interaction term Post-MNREGA, which is a dummy variable that takes the value 1 for all treated factories in all fiscal years after the implementation of MNREGA in the district in which the factory is located. The controls include firm size (factory size) and age of the factory. The sample consists of all open “wholly private owned” factories in the ASI census survey from the fiscal year 2002 to the fiscal year 2010. The standard errors are clustered at the district level. T-statistics are reported in parentheses. ***, **, * represent statistical significance at 1%, 5% and 10% respectively.

Panel A: Full sample

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log(1+ FIXED_ASSET_ADD)		Log(1+PNM_ADD)		Log(1+CAPITAL_WORKER)		Log(1+ Rent for P&M)	
Post-MNREGA	0.1135*** (2.9276)	0.1182*** (3.1249)	0.2363*** (2.7793)	0.2323*** (2.7213)	0.0411** (2.4773)	0.0230* (1.8831)	0.2201* (1.8633)	0.2487** (2.0647)
Firm size		-0.0971*** (-8.4035)		-0.1475*** (-7.6571)		0.0813*** (8.5810)		0.0505*** (3.6610)
Age		0.0032 (1.3210)		-0.0024 (-0.4836)		0.0005 (0.5287)		0.0084** (1.9663)
Observations	124,975	120,774	122,847	118,778	126,569	120,757	126,558	120,752
R-squared	0.7987	0.8063	0.7116	0.7188	0.9072	0.9270	0.5782	0.5790
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Factory FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 7: MECHANIZATION OF FACTORIES: HETEROGENEITY BASED ON WAGES, LABOR PRODUCTIVITY, LABOR REGULATIONS, LABOR LAWS, AND INDUSTRY VOLATILITY

This table reports the OLS estimates of our baseline equation (1) with $\text{Log}(1+\text{FIXED_ASSET_ADD})$ ($\text{Log}(1+\text{PNM_ADD})$) as the dependent variable in Panel A (B) for different sub-samples: Column (1) (Column (2)) reports the OLS estimates for the subsample of factories with pre-MNREGA wage per permanent worker below (above) the Industry median, within a district respectively. Column (3) (Column (4)) reports the OLS estimates of the factories which have low (high) levels of productivity, i.e., factory's output per worker is below (above) the industry median. In Columns (5) and (6), we split our sample into factories located in states with pro-employer/neutral and pro-employee labor regulation based on Besley et al. (2004) and Aghion et al. (2008). Columns (7) and (8) report the OLS estimates for the factories in industries with above (below) median industry volatility. The parameter of interest is the coefficient on the interaction term Post-MNREGA, which is a dummy variable that takes the value 1 for all treated factories in all fiscal years after the implementation of MNREGA in the district in which the factory is located. The controls include firm size (factory size) and age of the factory. The sample consists of all open "wholly private owned" factories in the ASI census survey from the fiscal year 2002 to the fiscal year 2010. The standard errors are clustered at the district level. T-statistics are reported in parentheses. ***, **, * represent statistical significance at 1%, 5% and 10% respectively.

Panel B: Sub-sample tests

	Dependent variable: $\text{Log}(1+\text{FIXED_ASSET_ADD})$							
	Wages Per Permanent Worker		Labor Productivity		Labor Regulations		Output Volatility	
	Below median	Above median	Below median	Above median	Pro-employer	Pro-employee	Above median	Below median
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
Post-MNREGA	0.1045* (1.9606)	0.0853 (1.5296)	0.1686*** (3.0938)	0.0306 (0.5774)	0.1558*** (3.2882)	-0.0538 (-0.7268)	0.1422*** (3.3380)	0.0425 (0.6244)
Observations	62,653	54,900	56,614	61,701	77,024	29,995	86,953	33,282
R-squared	0.7953	0.7971	0.7674	0.7648	0.8082	0.8055	0.8072	0.8016
Dependent variable: $\text{Log}(1+\text{PNM_ADD})$								
Post-MNREGA	0.1946* (1.7656)	0.2196* (1.8865)	0.2600* (1.9410)	0.1586 (1.5486)	0.2565** (2.3036)	-0.0239 (-0.1646)	0.2965*** (3.1506)	0.0579 (0.3524)
Observations	61,452	55,666	55,329	61,020	75,535	29,576	85,193	33,060
R-squared	0.7113	0.7028	0.6886	0.6658	0.7246	0.7059	0.7195	0.7144
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Factory FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 8: PERFORMANCE OF FACTORIES

This table reports the OLS estimates based on equation (1). The dependent variables in these tests are different measures of performance. A detailed description of variables is provided in Table (1). The parameter of interest is the coefficient on the interaction term Post-MNREGA, which is a dummy variable that takes the value 1 for all treated factories in all fiscal years after the implementation of MNREGA in the district in which the factory is located. The controls include firm size (factory size) and age of the factory. The sample consists of all open “wholly private owned” factories in the ASI census survey from the fiscal year 2002 to the fiscal year 2010. The standard errors are clustered at the district level. T-statistics are reported in parentheses. ***, **, * represent statistical significance at 1%, 5% and 10% respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
VARIABLES	Log(1+TOTAL- INPUT)	Log(1+TOTAL- INPUT)	Log(1+TOTAL- OUTPUT)	Log(1+TOTAL- OUTPUT)	Log(1+PROFIT)	CASH_RATIO	CASH_RATIO	CASH_RATIO	OUTPUT_PER- INPUT	OUTPUT_PER- INPUT
Post-MNREGA	0.0763*** (3.3755)	0.0752*** (3.3680)	0.0176 (1.1388)	0.0163 (1.2897)	-0.0048** (-2.5245)	-0.0048** (-2.4552)	-0.0033*** (-2.6156)	-0.0026** (-2.4705)	-1.0166*** (-3.2824)	-1.0593*** (-3.2646)
Firm size		0.1013*** (20.2568)		0.1053*** (22.8744)		0.0050*** (12.0877)		-0.0029*** (-7.5685)		0.0184 (0.7192)
Age		0.0037*** (4.0244)		0.0028*** (3.3502)		0.0001 (0.8787)		0.0002*** (3.3002)		-0.0090** (-2.1375)
Observations	126,586	120,774	126,586	120,774	126,583	120,771	124,701	119,248	126,586	120,774
R-squared	0.9234	0.9276	0.9420	0.9477	0.7468	0.7513	0.9925	0.9948	0.3648	0.3650
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Factory FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 9: INVENTORY, DEBTOR AND WORKING CAPITAL

This table reports the OLS estimates based on equation (1). The dependent variables in these tests are Debtor turnover cycle, Inventory turnover cycle, and Working capital cycle. A detailed description of variables is provided in Table (1). The parameter of interest is the coefficient on the interaction term Post-MNREGA, which is a dummy variable that takes the value 1 for all treated factories in all fiscal years after the implementation of MNREGA in the district in which the factory is located. The controls include firm size (factory size) and age of the factory. The sample consists of all open “wholly private owned” factories in the ASI census survey from the fiscal year 2002 to the fiscal year 2010. We use year and factory fixed effects. The standard errors are clustered at the district level. T-statistics are reported in parentheses. ***, **, * represent statistical significance at 1%, 5% and 10% respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Inventory cycle		Debtor cycle		Working Capital cycle	
Post-NREGA	-25.09*** (-4.1690)	-25.24*** (-4.1278)	-2.09** (-2.4544)	-2.26*** (-2.7158)	-26.61*** (-4.4034)	-26.92*** (-4.3648)
Firm size		1.34** (2.3936)		0.58*** (3.4316)		2.24*** (3.6285)
Age		0.09 (0.8583)		0.02 (0.3626)		0.13 (1.0416)
Observations	126,586	120,774	115,519	110,389	126,586	120,774
R-squared	0.6561	0.6588	0.7715	0.7745	0.6672	0.6692
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Factory FEs	Yes	Yes	Yes	Yes	Yes	Yes

Table 10: LABOR PRODUCTIVITY AND LABOR LAWS: IMPACT ON WORKERS, CAPITAL INVESTMENTS AND FACTORY PERFORMANCE

This table reports the OLS estimates of our regression equation, for our sub-samples based on the combination of labor productivity and labor regulations measures. In panel (A (B)), we report results for factories with below median labor productivity and are located in states with pro-employer/neutral labor (pro-employee) regulations. The parameter of interest is the coefficient on the interaction term Post-MNREGA, which is a dummy variable that takes the value 1 for all treated factories in all fiscal years after the implementation of MNREGA in the district in which the factory is located. The controls include firm size (factory size) and age of the factory. The standard errors are clustered at the district level. T-statistics are reported in parentheses. ***, **, * represent statistical significance at 1%, 5% and 10% respectively.

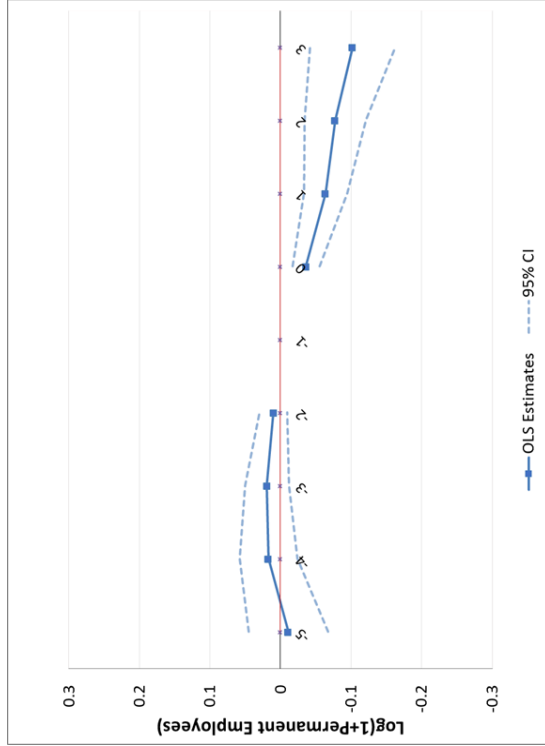
	Panel A : Low labor productivity and Pro-employer regulations				Panel B : Low labor productivity and Pro-employee Regulations			
	Log(1+PERM. WORKER)	Log(1+FIXED- ASSET_ADD)	Log(1+ PROFIT)	Working capital Cycle	Log(1+PERM. WORKER)	Log(1+FIXED- ASSET_ADD)	Log(1+PROFIT)	Working Capital cycle
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Post-MNREGA	-0.0541** (-2.3136)	0.2302*** (3.3341)	-0.0035* (-1.7596)	-24.4034*** (-4.1673)	-0.0279 (-0.4149)	0.0301 (0.3769)	0.0018 (0.7158)	-15.4639 (-1.3253)
Observations	39,362	39,362	39,360	39,362	12,083	12,083	12,083	12,083
R-squared	0.9291	0.7641	0.6582	0.7067	0.9302	0.7784	0.7098	0.6762s
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Factory FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Figure 2: Estimated Employment and Wages Response Dynamics of the Impact of MNREGA

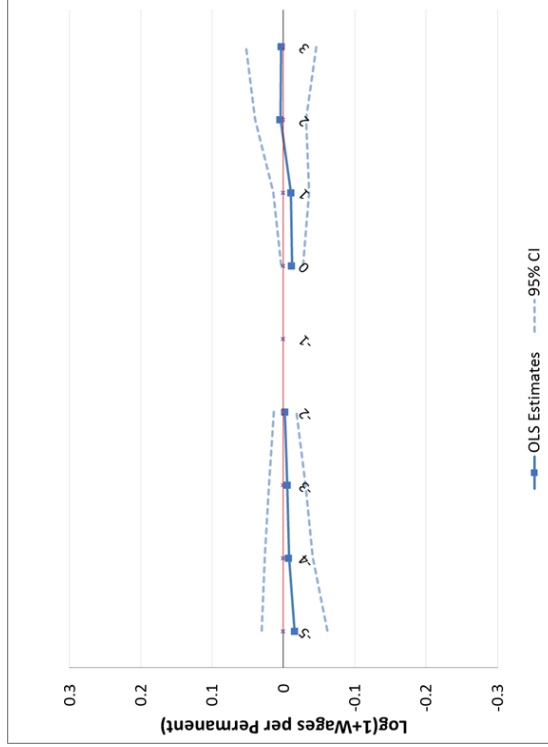
The graphs plots the cumulative coefficient estimates from the OLS estimation of the dynamic version of the difference-in-differences regression:

$$\ln[1 + y_{idt}] = \beta_0 + \sum_{k=-5}^{-2} \theta_k \times Pre[k]_{idt} + \sum_{k=0}^3 \theta_k \times Post[k]_{idt} + \delta_i + \gamma_t$$

where y_{idt} is the dependent variable. The sample consists of all open “wholly private owned” factories in the ASI census survey from the fiscal year 2002 to the fiscal year 2010. The controls include firm size (factory size) and age of the factory. We use year and factory fixed effects. The standard errors are clustered at the district level. $Pre[k]$ ($Post[k]$) is a dummy variable that is set to one k years before (after) the implementation year of MNREGA in the *treated district*. The base category in these tests is the year T-1 and denotes the year before the implementation year T. The figure plots the entire paths of coefficients, along with their corresponding 95 percent confidence intervals (dashed lines), of the natural logarithm of number and wages of permanent workers.



(a)



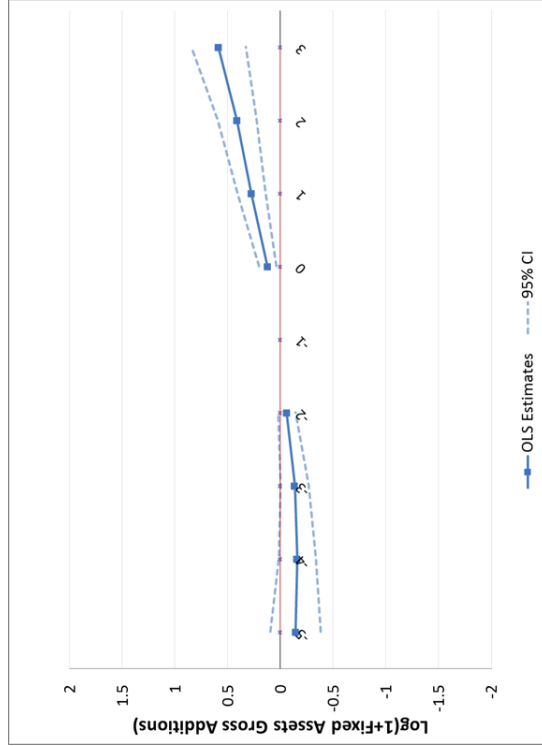
(b)

Figure 3: Estimated Mechanization Response Dynamics of the Impact of MNREGA

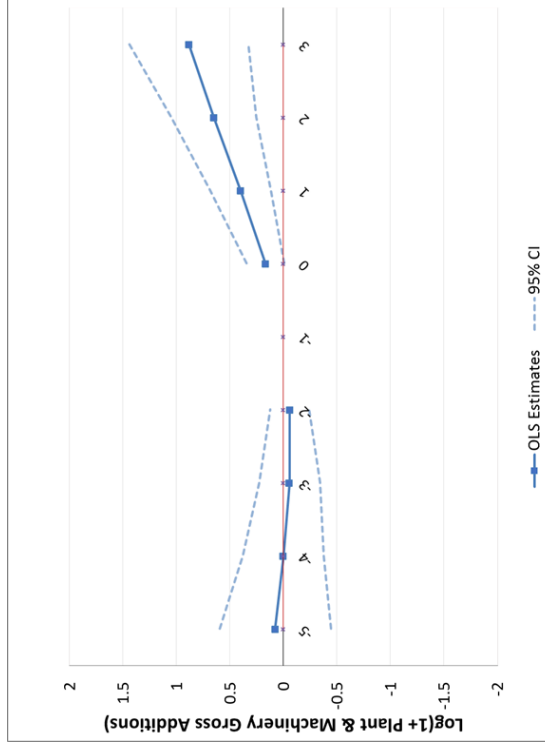
This graphs plots the cumulative coefficient estimates from the OLS estimation of the dynamic version of the difference-in-differences regression:

$$\ln[1 + y_{idt}] = \beta_0 + \sum_{-5}^{-2} \theta_k \times Pre[k]_{idt} + \sum_0^3 \theta_k \times Post[k]_{idt} + \delta_i + \gamma_t$$

where y_{idt} is the dependent variable. The sample consists of all open “wholly private owned” factories in the ASI census survey from the fiscal year 2002 to the fiscal year 2010. The controls include firm size (factory size) and age of the factory. We use year and factory fixed effects. The standard errors are clustered at the district level. $Pre[k]$ ($Post[k]$) is a dummy variable that is set to one k years before (after) the implementation of MNREGA in the *treated district*. The base category in these tests is the year $T-1$ and denotes the year before the implementation year T . This figure plots the entire paths of coefficients, along with their corresponding 95 percent confidence intervals (dashed lines), of fixed assets, and plant & machinery’s gross additions change response as estimated from the above equation.



(a)



(b)

Online Appendix A

This Appendix reports results of additional robustness tests that are briefly described in the text.

Table A1: SAMPLE CONSTRUCTION

Panel A: Annual survey of industries (ASI)	
Period	Fiscal year 2002 to fiscal year 2010
Number of observations (census)	1,26,586
Number of Factories (census)	31,655
Firm-year Observations in Phase 1	17,554
Firm-year Observations in Phase 2	16,725
Firm-year Observations in Phase 3	92,307
States covered	20
Union Territories covered	5
Districts covered	495
Phase 1 Districts	162
Phase 2 Districts	103
Phase 3 Districts	230
Average number of factories in a Phase 1 district	14
Average number of factories in a Phase 2 district	21
Average number of factories in a Phase 3 district	48
Average number of observations per factory in Phase 1	5.46
Average number of observations per factory in Phase 2	5.46
Average number of observations per factory in Phase 3	5.34
States/U.T.s Excluded	Andaman & Nicobar Islands, Manipur, Jammu & Kashmir, Meghalaya, Tripura and Nagaland
States/U.T.s having no factory-year observation	Arunachal Pradesh, Mizoram, Lakshadweep, Sikkim
Panel B: MNREGA related variables for IV regression	
Period	Fiscal year 2007 to fiscal year 2010
States/U.T.s covered	20
U.T. Covered	4
U.T. not available	Delhi
Panel C: Election Commission of India	
Period	Fiscal year 2002 to fiscal year 2010
States/U.T.s covered	25 (20 States + 5 U.T.)
Number of observations	225
State ruling dummy == 1	101

AI Access to finance and mechanization

To the extent that labor expenditure can at least in part be met ex-post from operating cash flows, the capital investments need to be financed upfront (Fazzari, Hubbard, Petersen, Blinder & Poterba (1988)), therefore access to finance is a critical pre-condition for mechanization (Duchin, Ozbas & Sensoy (2010)). We exploit this idea to further sharpen the interpretation of our results on mechanization by firms post MNREGA.

Specifically, we use a natural experiment engendered by a policy experiment in India that randomized the access to finance among small firms and enabled us to analyze whether capital investments were particularly higher for firms that had better access to finance.³⁷ In

³⁷Farre-Mensa & Ljungqvist (2015) show that widely used measures of financial constraints such as K-Z index (Kaplan & Zingales (2000)) or the measure developed by Hadlock & Pierce (2010) do not measure financial

India, banks are mandated to direct 40% of their total credit to priority sectors (Cole (2008)). The definition of priority sector includes key sectors such as agriculture, low-cost housing, and small and medium enterprises (SME). Until the year 2006, a firm was considered as an SME if the total investment in plant and machinery was less than or equal to INR 10 million. The limit was increased from 10 million to 50 million in the year 2006.

The redefinition of an SME led to a large exogenous increase in the number of firms that became eligible for priority sector credit. Prior literature highlights that such redefinition eased credit constraints for firms (Banerjee & Duflo (2014)). The 50 million cut-off lends itself to a sharp regression discontinuity design. This redefinition created a situation wherein firms just below a 50 million cut-off enjoyed better access to finance when compared to firms just above 50 million. It must be noted that the first phase of MNREGA was also implemented in the year 2006. Further, financial constraints are likely to be more severe, and hence priority sector lending program is likely to have a greater effect in regions with lower levels of financial development. We proxy for the level of financial development in a region using bank branch penetration defined as bank branch per 100,000 population.

To formally analyze whether mechanization investments were greater in less financially-constrained firms, we use the regression discontinuity method designed by Calonico, Cattaneo & Titiunik (2014). This method recognizes the fact that the routinely employed polynomial estimators are extremely sensitive to the specific bandwidths employed. Calonico, Cattaneo & Titiunik (2014) show that the conventional and regression discontinuity (RD) tests and the, recently developed, nonparametric local polynomial estimators make bandwidth choices that lead to a “bias in the distributional approximation of the estimator.” Accordingly, based on the suggestion, we report both the biases-corrected as well robust RD estimators. Our bandwidth selection is based on Imbens & Kalyanaraman (2011).

In our RD test, we use gross investments in plant and machinery as the dependent variable. The level of investment in plant and machinery is the running variable, with 50 million being the cut-off. We report the results in Table (A2) in Appendix A. In column (1), our sample consists of factories located in districts with below-median bank penetration and where MNREGA was implemented in 2006. We find that factories that are to the left of the cut-off mechanize significantly more than factories that are placed to the right. It must be noted that the policy experiment exogenously reduced the financial constraints for factories on the left. In column (2), using the same sample as in column (1), we test the impact on wages. Consistent with our baseline results reported in section (VI.B), we do not find any significant discontinuity in wages at the cut-off.

In columns (3) and (4), we examine the impact on mechanization and wages, respectively, in phase-1 districts with high (above median) bank branch penetration. The policy experiment aimed at alleviating financial constraints for small firms is more likely to have a bite for firms with limited access to finance. Consequently, in areas with a high level of bank penetration, we do not expect much (or, at the very least, less) difference in the ability of firms on both sides of the RD cut-off to raise external finance. Consistent with the idea, we do not find any significant discontinuity in either mechanization or wages at the cut-off.

constraints appropriately.

Finally, to rule out the thesis that our findings regarding the increase in mechanization by factories post-MNREGA implementation could potentially be driven by some unobservable time-varying factor that happens to coincide with MNREGA, in columns (5)-(8), we conduct a placebo RD experiment with the sample of factories located in phase-3 districts. MNREGA was not implemented in these districts, as of 2006. Thus, these districts were not experiencing a labor supply shock at the time the priority sector cut-offs for lending were redefined. However, factories located in these areas with less than INR 50 million investment in plant and machinery also exogenously became eligible for priority sector lending. An analysis of columns (5)-(6) provides some evidence of an increase in mechanization by factories to the left of the cut-off. However, the estimated treatment effect is about one-third of the magnitude observed for phase-1 factories in columns (1)-(2) and is statistically indistinguishable from zero using robust RD estimates. This effect is not surprising, given that these factories may be responding to an anticipated fall in labor supply. Again, we do not find any discontinuity for factories located in more financially developed areas.

Table A2: FACTORIES UNDER FINANCIAL CONSTRAINT VERSUS OTHERS

This table reports the regression discontinuity (RD) results for the impact of MNREGA on Log of Gross P&M additions (P&M) and change in Wages (WAGES). The RD specification estimates the significance of $E[Y_i(1) - Y_i(0) | X_i = \bar{x}]$. We use the procedure developed by Calonico et al. (2014) to estimate robust, and bias-corrected standard errors. Z-statistics are reported in parentheses. Total Gross P&M opening balance as of 1st April 2006 is the running variable. As we are using the exogenous change in the priority sector lending limit to manufacturing establishments, in October 2006, from INR 10 million to INR 50 million, we take INR 50 million as the cut-off. T-statistics are reported in parentheses. ***, ** and * represent statistical significance at the 1%, 5% and 10% levels, respectively.

	Phase 1 districts				Phase 3 districts			
	Low financial development		High financial development		Low financial development		High financial development	
	P&M	WAGES	P&M	WAGES	P&M	WAGES	P&M	WAGES
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bias-Corrected	6.586*** (3.5020)	-0.1146 (-0.6832)	-0.0525 (-0.0745)	0.5916 (1.2494)	2.1586* (1.7931)	-0.2411 (-1.3714)	0.0622 (0.0661)	0.0351 (0.3498)
Robust	6.586* (1.7437)	-0.115 (-0.5417)	-0.0525 (-0.0269)	0.5916 (0.9840)	2.1586 (1.2542)	-0.2411 (-0.9934)	0.0622 (0.0558)	0.0351 (0.2452)
Observations	841	842	52	52	2,438	2,438	3,506	3,506
Year			2007	2007			2007	

Table A3: EXISTENCE OF PRE-TREND (PLACEBO TREATMENT)

The table reports OLS estimates based on equation (1) and using a placebo treatment assignment to test for pre-existing trends. A detailed description of variables is provided in Table 1. Note that MNREGA was implemented at the beginning of the fiscal year 2007 (2nd February 2006), 2008 (1st April 2007) and 2009 (1st April 2008) in three phases. In panel A, we use fiscal years 2003, 2004 and 2005 as placebo treatment years representing the three-phased implementation of MNREGA. In panel B, we use 2004, 2005 and 2006 as placebo years. The sample consists of all open “wholly private owned” factories in the ASI census survey from the fiscal year 2002 to the fiscal year 2010. The controls include firm size (factory size) and age of the factory. The standard errors are clustered at the district level. T-statistics are reported in parentheses. ***, **, * represent statistical significance at 1%, 5% and 10% respectively.

	(1)	(2)	(3)	(4)
	Log(1+PERM_ WORKER)	Log(1+WAGE_ PERM)	Log(1+FIXED_ ASSET_ADD)	log(1+PNM_ ADD)
Panel A : Years 2003-2005				
Post-MNREGA	0.0016 (0.1347)	0.0021 (0.2447)	-0.0160 (-0.4012)	-0.1384* (-1.6644)
Firm size	0.0500*** (14.5782)	0.0382*** (19.0076)	-0.0971*** (-8.4190)	-0.1476*** (-7.6724)
Age	0.0021*** (3.8106)	0.0007** (1.9867)	0.0032 (1.3101)	-0.0024 (-0.4923)
Observations	120,774	120,774	120,774	118,778
R-squared	0.9300	0.8285	0.8062	0.7187
Panel B : Years 2004-2006				
Post-MNREGA	-0.0091 (-0.9240)	-0.0044 (-0.7250)	-0.0002 (-0.0056)	-0.0124 (-0.1613)
Firm size	0.0500*** (14.5834)	0.0382*** (19.0001)	-0.0971*** (-8.4197)	-0.1476*** (-7.6801)
Age	0.0022*** (3.8172)	0.0007** (1.9919)	0.0032 (1.3089)	-0.0024 (-0.4951)
Observations	120,774	120,774	120,774	118,778
R-squared	0.9300	0.8285	0.8062	0.7187
Year FEs	Yes	Yes	Yes	Yes
Factory FEs	Yes	Yes	Yes	Yes

AII Alternative identification strategy: instrument variable approach

In section (VI.A.1), we perform several other cross-sectional tests using different economic characteristics to analyze the heterogeneous impact of the program. These tests further strengthen the causal interpretation of our findings. The chances of there being an omitted variable that comoves with MNREGA in both time series and several cross-sectional dimensions are remote. Nevertheless, to address any residual concerns, we use an alternative identification strategy and employ an IV approach. We design our instrument by considering the political economy implications of the program. Prior literature highlights that governments in emerging economies resort to politically targeted fiscal measures to win voter support (Cole (2008); Alok & Ayyagari (2019)). Although MNREGA is funded by the central government, it is implemented by state governments. Therefore, a party ruling at the center is likely to get higher political mileage for allocations made to states that it rules when compared to other states (See Khemani (2007), Arulampalam, Dasgupta, Dhillon & Dutta (2009) and Dinç & Gupta (2011)). Thus, we expect that the expenditure allocated for MNREGA and, consequently, the intensity of MNREGA implementation is likely to be higher in states where the ruling party in the state is same as the party at the center when compared to the states that are under party in opposition at the center.

We exploit this idea to construct our instrument. Specifically, we use a dummy variable (DUMMY_STATE_CENTER) as our instrument that takes the value of one if the ruling party is a state S during a year t that is the same as the ruling party at the center. We restrict our sample to all the states where the ruling party in the state is same as the party at the center or the party in opposition at the center.³⁸ It must be noted that since the timing of state elections is exogenously specified and constitutionally mandated to be held every 5 years, it does not always coincide with the MNREGA implementation; the instrument is unlikely to be related to any plausible time-varying omitted variable that correlates with MNREGA. An example would better clarify our identification strategy. The Indian National Congress (INC) was the ruling party both at the center and in the states of Andhra Pradesh and Maharashtra. While the central government and Andhra Pradesh governments were elected in the year 2004, the Maharashtra government was elected in the year 2005. MNREGA was implemented in the year 2006 in both the states. The identifying assumption (exclusion restriction) is that the victory of the INC in these states is unlikely to directly have an adverse effect of employment in factories other than through its effect on the intensity of MNREGA implementation. Thus, while our DID tests rely on a staggered roll-out of MNREGA for identification, our estimates using the IV approach are identified through a randomized variation in the intensity of treatment.

In Table A4, we report the results of the first stage and formally verify whether our instrument satisfies the inclusion restriction. Specifically, we examine if our instrument

³⁸We exclude states ruled by *regional* parties, which have formed a coalition with either the ruling party or the party in opposition at the center. This is because it is difficult to disentangle the effect in the states that are under allied parties as the alliances are often withdrawn. In essence, all regional parties are potential allies for the two main national parties in India, the Indian National Congress and the Bharatiya Janata Party.

correlates with the subsequent intensity of the MNREGA implementation. Each observation represents a factory-year. It must be noted that in these tests we focus only on the post-MNREGA period and restrict our sample to the states under either the ruling party or the party in opposition. We use the following three proxies to capture the intensity of the treatment: LABOR_EXP (columns (1), (4), (7), and (10)) is the total wage expense related to MNREGA workforce, NUMBER_WORKS (columns (2), (5), (8), and (11)) is the total number of public infrastructure projects undertaken through MNREGA, and the TOTAL_EMP_DEMAND refers to the *Number of Workers* (columns (3), (6), (9), and (12)) registered with MNREGA that demanded work. Focusing on Table A4, we find that our instrument is positively correlated with all the three measures and the correlation is statistically significant at the 1% level. Further, we follow Sanderson & Windmeijer (2016) to mitigate the concerns of under-identification and a weak instrument bias. Overall, these results show that MNREGA implementation is more intense in districts that belong to states ruled by the same political party that rules at the federal level.

In the second stage, we analyze whether the decrease in permanent workforce employed and an increase in mechanization is greater in factories located areas with greater MNREGA intensity. In Table A5, we report the estimates from these tests. Focusing on columns (1)-(3), we find that, consistent with our baseline results reported in Table 3, there is a statistically significant decline in the number of permanent workers for factories located in states with greater MNREGA intensity. Moreover, in line with our DID results, we find no significant impact on wages (columns (4)-(6)) and a statistically significant increase in mechanization (columns (7)-(12)).

Summarily, the results from IV estimates corroborate the baseline findings of our difference-in-differences empirical strategy. In subsequent analysis, we only report the results based on our baseline DID empirical strategy.

Table A4: FIRST STAGE IV REGRESSION

This table reports the estimates from the first-stage of our IV regressions. We use three proxies to capture the intensity of treatment: MNREGA’s labor expenditures (INR in millions), Number of works (in millions), both completed and ongoing, and total employment demanded (in millions). *State-Center* dummy is our instrumental variable that provides exogenous variation in the intensity of treatment. *State-Center* takes the value one if the ruling party in the state is the same as the party in power at the center and 0 otherwise. The sample consists of all open “wholly private owned” factories in the ASI census survey from the fiscal year 2002 to the fiscal year 2010. The controls include firm size (factory size) and age of the factory. We report Sanderson- Windmeijer (SW) first-stage chi-squared p-value and F-statistic tests for under-identification and weak instruments, respectively, of individual endogenous regressors in each column. The standard errors are clustered at the district level. T-statistics are reported in parentheses. ***, **, * represent statistical significance at 1%, 5% and 10% respectively.

	(1)	(2)	(3)
VARIABLES	LABOR_EXP	NUMBER_WORKS	TOTAL_EMP _DEMAND
DUMMY_STATE_CENTER	0.0042*** (3.33)	0.0202*** (4.06)	0.0628*** (3.61)
Observations	28,897	28,897	28,897
SW Chi-sq p-value	0.001	0.000	0.000
SW F-statistic	11.10	16.49	13.05
Prob > F	0.005	0.001	0.0034
Year FEs	Yes	Yes	Yes
Factory FEs	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Table A5: SECOND STAGE IV REGRESSION

This table reports the estimates from the second-stage of our IV regressions. We use three proxies to capture the intensity of treatment: MN-REGA's labor expenditures (INR in millions), Number of works (in millions), both completed and ongoing, and total employment demanded (in millions). DUMMY_STATE_CENTER is our instrumental variable that provides exogenous variation in the intensity of treatment. DUMMY_STATE_CENTER takes the value one if the ruling party in the state is the same as the party in power at the center and 0 otherwise. The sample consists of all open "wholly private owned" factories in the ASI census survey from the fiscal year 2002 to the fiscal year 2010. The controls include firm size (factory size) and age of the factory. The standard errors are clustered at the district level. T-statistics are reported in parentheses. ***, **, *, represent statistical significance at 1%, 5% and 10% respectively.

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
		Log(1+PERM_ WORKER)		Log(1+WAGES_ PERM)		Log(1+FIXED_ ASSET_ADD)		Log(1+PNM_ ADD)				
LABOR_EXP	-7.3069*			2.3536		32.0549		99.5593*				
	(-1.7719)			(0.4090)		(1.5351)		(1.9402)				
NUMBER_WORKS		-1.5238**		0.4908		6.6848*		20.5310**				
		(-2.0719)		(0.4206)		(1.9468)		(2.2169)				
TOTAL_EMP_ DEMAND			-0.4919*		0.1584		2.1579		6.7188**			
			(-1.8196)		(0.4076)		(1.6047)		(2.0137)			
Observations	28,897	28,897	28,897	28,897	28,897	28,897	28,897	28,570	28,570	28,570	28,570	28,570
R-squared	0.9686	0.9675	0.9685	0.8457	0.8455	0.8457	0.8853	0.8826	0.8851	0.8192	0.8102	0.8185
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

AIII Impact on District Level Growth

We start with the impact of MNREGA on growth. If activity levels of firms, in general, are adversely affected due to MNREGA, and full-time workers leave those areas where the manufacturing firms are located, it is reasonable to expect a decline in economic growth. Manufacturing is likely to be directly affected by firm growth slowing down caused by a short-run drop in profitability. Unfortunately, reliable district-level GDP data are not available. As an alternative, we use the night lights data and examine the effect of MNREGA on the Night-time Light Intensity.³⁹ The measure of night-time Light Intensity, also called Luminosity Index is used in the economics literature as a measure of economic growth and output (Henderson, Storeygard & Weil (2011); Chen & Nordhaus (2011)). Thus, the availability of time-series of night lights data at the district level allows us to examine the effect of MNREGA on the local economic activity.

We employ our baseline staggered difference-in-differences regression in these tests, albeit with district-month as the unit of observation. The dependent variable is the monthly median value of the district level Night-time Light Intensity. Table A6 summarizes these results. We find a significant decline in visibility during the night. The results are economically significant; a one standard deviation movement leads to a decline of about 13% in the median visibility at night.

Table A6: DISTRICT LEVEL OUTCOME: NIGHT-TIME LIGHT VISIBILITY

In this table, we examine the effect of the MNREGA on the Night-time light visibility at the district level. We use the Night-time light data at the district level from 2002 and 2010 from the api.nightlights.io. The data is organized at district month level. The dependent variable is the monthly weighted median of night light visibility of a district. The parameter of interest is the coefficient on the interaction term Post-MNREGA, which is a dummy variable that takes the value 1 for all treated districts in all months after the implementation of MNREGA in the district. We include district and month fixed effects in all specifications. The errors are clustered at a district level. ***, **, * represents statistical significance at the 1%, 5%, and 10% levels.

VARIABLES	Night light visibility (1)
Post-MNREGA	-0.1729*** (-5.7921)
Observations	52,911
R-squared	0.5641
Month FEs	Yes
District FEs	Yes

³⁹Source: The India Lights project is a collaboration between Development Seed, The World Bank, and Dr. Brian Min at the University of Michigan. <http://api.nightlights.io>