

Cambridge Working Paper Economics

Cambridge Working Paper Economics: 1626

EMPLOYMENT GUARANTEE SCHEMES AND WAGES IN INDIA

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4 April 2016

Using a novel data-set of district-wise program expenditure, we estimate the impact of large employment schemes on agricultural wages in India. Depending on the underlying theoretical mechanism, private wages can either respond to contemporaneous fluctuations in program expenditure or be sensitive to the stock of expenditure incurred under such programs. We first find that although program expenditure varied substantially both across and within districts, every district was covered under employment guarantee during the 2001-2010 period. Next, we empirically contrast the “spot” versus the “stock” effect of employment schemes on wages. Identification of program impact is achieved by partialling out a host of district and year specific controls. Exploiting the fund allocation process of these schemes, we further check for potentially endogenous district-year fluctuations. We find a significant positive impact on wages through the stock effect. In contrast, we do not find an immediate jump in wages suggesting weak spot effects.

Employment Guarantee Schemes and Wages in India

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Abstract

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Keywords: Public works, Workfare, NREGA, Public capital, Wages.

JEL Classifications: H53, I38, J31, J38

*I would like to thank Toke Aidt, Douglas Gollin, Sriya Iyer, Kaivan Munshi, Anand Shrivastava, Melvyn Weeks, and seminar participants of Cambridge macroeconomic workshop for comments and suggestions. I am also very grateful to Datanet, and Ministry of Rural Development (India) for help in data collection. Finally, I am very grateful to my supervisor Giancarlo Corsetti for his continued guidance and support. Financial assistance from Suzy Paine Fund is gratefully acknowledged.

1 Introduction

The recent implementation of National Rural Employment Guarantee Act (hereafter NREGA), has revitalized the debate on the impact of workfare programs on the labor market. The program was implemented in three phases and enveloped 200, 330, and all the districts of the country by phase I (2006), II (2007), and III (2008) respectively. During 2009-2010, NREGA generated around 2.6 billion work-days, and employed around 55 million households making it the largest public workfare program in the world. The total expenditure under NREGA amounted to around 0.6% of the GDP (or around 5% of the agricultural output) during the same year. It is hence important to understand the welfare implications of such large scale policy interventions on nonparticipants through its impact on short-term manual labor wage rate. The question is highly policy relevant since such “casual” labor provides an important source of income for the poor [Banerjee and Duflo (2007)].

This paper starts by documenting the existence of NREGA’s predecessor: Sam-poorna Grameen Rozgar Yojana (hereafter SGRY) which was implemented in all the districts of the country in 2001. To compare the two schemes and for the empirical analysis, we use a novel data-set of district-wise annual expenditure incurred under SGRY and NREGA over the ten year period: 2001-2010. To the best of our knowl-edge, this is the most exhaustive and disaggregated expenditure data on employment schemes used in any comparable analysis for a developing country. We find the two programs to be strikingly similar in terms of their functionality and general program objectives.

An important empirical finding of this exercise is that SGRY continued to be in operation exclusively in the “non-NREGA” districts during the early phases of NREGA implementation in 2006 and 2007. This is an important revelation as it blurs the distinction between “treatment” and “control” districts as defined on the basis of the phase-wise implementation of NREGA.¹ In this regard, we motivate the provision of employment guarantee as a continuous treatment where NREGA can be best understood as an intensification of an already existing rural workfare policy by the government.

¹This however does not threaten the validity of the results from difference-in-difference framework used in recent studies as the results can be interpreted as the effect on wages due to the *intensification* of employment provision (or program impact) rather than the implementation of a previously non-existent employment guarantee policy.

Theoretically, Ravallion (1990) discusses public works to increase private wages by increasing the demand for labor or improving the bargaining power of the worker.² Basu, Chau, and Kanbur (2009) discuss other “spot market effects” of Employment Guarantee Schemes (hereafter EGS) like gains in efficiency that may increase private wages by alleviating the distortions in the labor market that arise due to oligopsonistic market power of employers.

Apart from such spot market effects, employment schemes have long been acknowledged to have productivity enhancing effects. That is, the productive assets created under the employment programs as public capital can increase agricultural productivity. Binswanger et al. (1993) and Fan et al. (2000) highlight the strong positive relationship between public infrastructure investment and agricultural output in India. Drèze (1990) discusses the role of Maharashtra’s EGS in increasing agricultural productivity in the long run. Similar workfare programs in Bangladesh have also been commended in increasing agricultural production by increasing and maintaining rural infrastructure [see for e.g., Alamgir (1983) and Thomas (1990)]. More recently, Aggarwal et al. (2012) discuss the construction of wells under NREGA as having productivity enhancing effects in the agriculture sector among other positive spill-over effects.³

Hence apart from spot market effects like efficiency gains or labor demand effects, the build up of productive public capital may further increase private wages. Although our data does not allow us to separately identify the effect due to each channel, one cannot ignore the possibility of wage increases due to public asset accumulation, especially since part of the expenditure under both SGRY and NREGA represents an investment to develop and maintain public infrastructure. In the paper, we provide evidence that productive works like flood control and irrigation projects were indeed carried out under both these programs. Since part of the EGS expenditure represents an investment component which can raise wages by increasing worker productivity, we use the stock of EGS expenditure as our main explanatory variable to measure the impact of EGS on wages. We use our novel data-set of district-wise EGS expenditure over the ten year period from 2001-10 to construct the stock of EGS expenditure.

A concurrent study by Berg et al. (2015) reports significant increase in wages

²Since our wage data is on agricultural wages, we use the terms *private*, *field*, or *agricultural wages* interchangeably in the paper.

³Basu (2013) presents a theoretical model discussing labor market responses to a productive EGS.

using “exposure” to NREGA (defined as the number of months a district was under NREGA) as opposed to zero immediate impact using regression difference-in-difference (DD).⁴ Although similar in concept to the definition of exposure in [Berg et al. \(2015\)](#), using the stock of EGS expenditure allows us to account for the substantial district level heterogeneity in the provision of public employment.⁵ The medical literature also provides some relevant analogues where under the assumption that a treatment has long term effects, the treatment impact is based on aggregate (rather than individual) dose exposures [see for e.g., [Chen et al. \(2001\)](#), [Martins-Filho et al. \(2010\)](#), [Nysom et al. \(1998\)](#), and [Schaubel and Wei \(2011\)](#)].

The challenge of using the stock of EGS expenditure is that this measure is highly endogenous and can be strongly correlated with district level characteristics such as the share of scheduled caste population, baseline agricultural productivity, and other demographic features like proneness to floods or droughts. As is discussed in detail in the paper, baseline identification of the program impact is achieved after partialling out district and year fixed effects, differential trends, and other important controls. For robustness, we further check for potentially endogenous district-year specific fluctuations in EGS expenditure by exploiting the process of fund allocation in the two schemes. Our results are robust to this and other robustness checks.

To motivate our empirical specification, we present a simple model of asset accumulation that suggests wages to be a non-linear (concave) function of the stock of EGS expenditure. The model draws heavily from [Basu, Chau, and Kanbur \(2009\)](#) wherein we allow labor productivity to be increasing in the capital generated under an EGS in a multi-period dynamic framework. Finally, by constructing an artificial data-set, we conduct a falsification test to show that if increase in wages occur only due to the spot market effects, then contrary to our empirical results, the stock of EGS expenditure is irrelevant in explaining private wages.

The concurrent literature on NREGA exploits the phase-wise implementation of the program to estimate the labor market effects of the program [see [Azam \(2012\)](#), [Berg et al. \(2015\)](#), and [Imbert and Papp \(2015\)](#)]. The first contribution of this paper is to employ a novel data-set of district-wise employment expenditure over 10 years

⁴Similar to [Berg et al. \(2015\)](#), we do not find any positive or significant jump in wages using regression Difference-in-Difference. See Appendix A2.

⁵See for e.g., [Imbert and Papp \(2015\)](#), [Drèze and Khera \(2009\)](#), and [Drèze and Oldiges \(2009\)](#) for a discussion on the large cross-state differences in the provision of public employment under NREGA.

that spans NREGA and the workfare program before it. This allows us to account for the substantial heterogeneity that exists in the provision of public employment under such schemes, both within and between districts. This enables us to treat the EGS as a continuous treatment using the stock of expenditure as our explanatory variable. Second, by comparing results from different empirical specifications, we contrast the relative strengths of spot market effects and the “stock effect” of workfare programs. Similar to Berg et al (2015), we find no immediate jump in wages which may indicate weak spot market effects of such schemes. On the contrary, we find positive and significant stock effect of EGS on wages.

Thirdly, based on the empirical evidence that such programs also undertake productive public works, we formalize the effect of EGS on wages through increase in labour productivity. We extend the Basu, Chau, and Kanbur (2009) model by allowing for worker productivity to be increasing in the stock of public capital generated under the program. This empirically motivates the stock of EGS expenditure as the variable relevant in capturing the impact of employment schemes on wages. Finally, using the financial records gathered as part of the data collected for this study, we exploit the fund allocation process to supplement our baseline identification of the EGS impact on wages.

Our results suggest that the annual growth of wages due to employment guarantee schemes is between 2.3-2.9% per annum. This impact is large especially when compared with the average growth of 3% per annum for the districts in our sample. Interestingly, we find similar response of wages under both workfare programs. Our results specific to NREGA’s impact on wages are in general agreement with recent studies by Azam (2012), Berg et al. (2015), and Imbert and Papp (2015). Zimmermann (2012) reports insignificant impact on wages, but the confidence intervals reported in the study are sufficiently large to accommodate our results.

The following section discusses and compares SGRY and NREGA in detail. In section 3, we discuss Basu, Chau, and Kanbur (2009) model of EGS with asset accumulation. Data is discussed in section 4 while Results are reported in Section 5. Section 6 concludes.

2 NREGA and its predecessor

In this section we discuss NREGA and its predecessor SGRY, which was in operation from 2001 to 2008 until it was completely subsumed under NREGA. Importantly, we find that SGRY was operational in most of the districts that did not receive NREGA during 2006-2008. This blurs the distinction between treatment and control districts. Below we discuss and compare SGRY and NREGA.

2.1 SGRY

Provision of unskilled manual labor work on demand (employment guarantee) has been a tool to combat extreme poverty by the Central government at-least since the introduction Food for Work program in 1977-78. Maharashtra's Employment Guarantee Scheme which was introduced in 1972 is an example of a state run employment guarantee program. Such employment schemes before 2000 were however substantially different from SGRY and NREGA in the sense that they worked as sub-schemes for larger rural development programs. Also, the objectives of employment creation and rural infrastructure development were never comprehensively addressed by a single program.⁶

Sampoorna Grameen Rozgar Yojana (SGRY) was launched as a nationwide program on 25th September 2001 to address the issues of employment generation and rural infrastructure creation. With the introduction of SGRY, previous employment programs like Jawahar Gram Samridhi Yojana (JGSY) and Employment Assurance Schemes (EAS) were discontinued. The motivation to implement SGRY was to integrate different programs for wage employment into one universal scheme.⁷ Like NREGA, SGRY envisaged generation of wage employment and creation of rural infrastructure by provision of labor intensive public projects.⁸ The program cost was divided between the Central and State government in the ratio 75:25.

⁶See Bahal (2015) for a detailed discussed on all major Rural Development Programs in India since 1980.

⁷SGRY operated as two streams initially, with program funds being equally distributed between the two. From 2004-05, SGRY operated as a single program.

⁸Details regarding the types of rural infrastructure projects undertaken under SGRY and NREGA are discussed in Section 3.

2.2 NREGA

NREGA was enacted in September 2005 and the program came into existence from 2006. At present, the program operates in all the districts of the country. NREGA entitles 100 days of guaranteed unskilled manual work (annually) to every rural household at the state defined minimum wage. Starting from 2006, the program was implemented in three phases. During 2006, NREGA was implemented in 200 *most backward* districts of the country. The criteria of judging the *backwardness* of a district was based on measures like agricultural productivity, past agricultural wage level, and the density of scheduled castes and scheduled tribes in a district.⁹

Additional 130 districts were enveloped into the program's second phase by 2007. By the third phase in 2008, NREGA was implemented in all the districts of the country. Like its predecessor, NREGA aims to generate wage employment and develop rural infrastructure through the provision of public works.

2.3 NREGA as a more intense EGS

This section highlights the similarities between the two schemes and motivates NREGA as a more intense employment scheme in comparison to SGRY. As Figure 1 shows, adjusted for 2000 prices, nearly 40 billion Indian rupees were spent on SGRY at the national level in its first year in 2001. The expenditure at the national level under SGRY progressively increased to nearly 60 billion rupees by 2005. In comparison, the national expenditure under NREGA in 2006 was nearly 70 billion rupees which substantially increased to 115 billion and 180 billion in 2007 and 2008 respectively. This increase was primarily due to the scale up of NREGA during its second and third phases.

Figure 2 shows the annual employment generated under SGRY and NREGA at the national level in millions of man-days generated. As expected, the trend of employment generated under the two programs closely matches the aggregate expenditure trend in Figure 1. Figures 1 and 2 highlight that although at a relatively smaller scale, significant amount of expenditure and employment generation did occur under an employment scheme before NREGA. It is important to mention here that the

⁹Given that past values of such statistics were used, the index was essentially based on district specific and time invariant fixed effects. This ranking however was not perfectly adhered to during the implementation of the program because of the substantial political bargaining involved between the central and state governments.

overlap of expenditure as seen in Figure 1 during the years 2006 and 2007, does not imply that both the programs were simultaneously in operation in all the districts. Rather, SGRY continued to be in operation in only those districts that did not receive NREGA until 2008.¹⁰

This point is clearly highlighted when we compare district-wise expenditure in 2006 in Figures 3a and 3b. Figure 3a shows the rollout of NREGA in its first phase in 2006. The “non-NREGA” or “late-phase” districts are in white while the shaded districts are the 200 phase I districts. In contrast, Figure 3b, shows the “actual” employment expenditure that occurred in the year 2006 which includes the expenditure under SGRY. Similarly, Figures 3c and 3d show that even during the phase II implementation of NREGA in 2007, SGRY was operational in the non-NREGA districts.

Hence comparison of Figures 3a and 3c with Figures 3b and 3d respectively highlights that SGRY continued to be in operation during the early phases of NREGA (in 2006 and 2007) in the non-NREGA districts. This is an important revelation as it blurs the distinction between “treatment” and “control” districts as defined on the basis of the phase-wise implementation of NREGA. The absence of any districts with “zero coverage” of employment guarantee policy in 2006 and 2007 was a result of the implementation process of NREGA where the late phase districts were in-fact supposed to have SGRY operational in them (until NREGA finally enveloped them in later phases). Chapter III of NREGA (2005) deems SGRY to be the action plan of the Act for the districts which weren’t covered under the first two phases of NREGA rollout:

“...the Annual Action Plan or Perspective Plan for the Sampoorna Grameen Rozgar Yojana (SGRY) ... in the concerned area ... shall be deemed to be the action plan for the Scheme for the purposes of this Act.” p.3, NREGA (2005).

Hence, the provision of employment guarantee seems to be a continuous process for the period of our study, both before and during the phase-wise implementation of NREGA.

Finally, we compare key features of both the programs to highlight the striking similarities between the two programs. As Table 1 compares, both programs were implemented in all of the districts of the country with their primary objectives being

¹⁰Only 3 out of around 600 districts reported positive expenditure by both the programs during 2006 and 2007. Results do not change if we drop the concerned districts.

provision of wage employment and infrastructure creation. Both schemes involved similar types of labor intensive public works. The program cost was largely borne by the central government for both the schemes. Finally, both programs encouraged female workers to join the public works by keeping a minimum female participation target of 33%.

Hence, apart from the higher expenditure under NREGA, the two programs were indeed very similar. In this regard, NREGA is best understood as an intensification of an already existing policy of employment guarantee. An important feature that may however distinguish NREGA to be structurally different from SGRY is that unlike an employment guarantee *program* (like SGRY) where the demand for work can be substantially rationed (due to fixed budget constraints), NREGA is an Act where employment guarantee is envisioned as a “right”. However, such a theoretical distinction has not precipitated into ground reality.

As Dutta et al. (2012) report, rationing of the demand for work is substantial even under NREGA. They report a country average rationing rate of nearly 44% with some states rationing the demand for work to as high as 84%.¹¹ Himanshu and Sharan (2014) highlight the case of unmet demand in NREGA due to a supply driven approach and bureaucratic/administrative capacity (or lack thereof). This further reinstates that NREGA can be viewed as a *more intense* (but not structurally different) scheme relative to SGRY.

3 Model of capital accumulation under an EGS

In this section we motivate worker productivity (and hence private wages) to increase due to the build-up of productive public capital under an employment guarantee scheme. We show that under some basic assumptions this motivates private wages as a non-linear (concave) function of the *stock* of EGS expenditure.¹² However, asset accumulation may not be the only channel that justifies the relevance of using the stock of EGS expenditure while measuring the impact on field wages. As a falsification

¹¹Dutta et al. (2012) argue that rationing may as well be unavoidable if the maximum level of spending under the Act is exogenously fixed while the offered wage rate cannot fall below a socially acceptable minimum wage.

¹²More generally, the idea that the existing stock of public capital is relevant to increases in growth and productivity have been discussed in Aschauer (1989), Corsetti (1992), and Futagami et al. (1993).

test, we show in Appendix A1 that if EGS impacts private wages only by affecting the spot market for labor, then (contrary to our empirical findings) the stock of EGS expenditure is not relevant in explaining private wages. Therefore, our model simply aims to highlight a plausible mechanism that may help explain our empirical results of positive and significant increase in wages when measured by the stock of EGS expenditure versus no immediate impact using standard regression difference-in-difference (see Appendix A2).¹³

Apart from the creation of wage employment, the schemes studied in our paper also generate productive durable assets for sustainable rural development. Under both SGRY and NREGA, the demand for work is met by hiring labor for *public works*. These public works involve projects on rural connectivity, flood control and protection, water conservation, drought proofing, micro irrigation, and land development among other such projects. Under NREGA, high priority projects in water conservation and water harvesting form the majority of the works undertaken under the scheme. Given that our wage data pertains to agricultural field wages, accumulation of such public capital can indeed increase labor productivity in the agriculture sector and hence be an important mechanism through which EGS impacts field wages.

Table 2 provides state-wise summary of the average number of works (in thousands) initiated and completed (annually) over the SGRY and NREGA regimes. Although the available data regarding creation of physical assets under both SGRY and NREGA is limited to the information on the *number* of works undertaken and completed, Table 2 provides evidence that productive works were indeed carried out under both the schemes without quantifying the number of durable assets generated under the schemes.¹⁴

To motivate a simple reduced form relationship between wages and employment expenditure in our empirical analysis, we discuss a simplified version of the EGS model as discussed in Basu, Chau, and Kanbur (2009) (hereafter BCK) by allowing for worker productivity to be increasing in the stock of EGS capital. Since the public capital generated under the scheme is not directly observed, we approximate it as a constant returns to scale function of the EGS expenditure. Below we discuss the asset accumulation channel by introducing a dynamic framework in the BCK model.

¹³Although beyond the scope of this study, empirically validating the different channels through which an EGS may impact private wages is an important area for future research.

¹⁴The difference in the size, scale, and nature of projects under SGRY and NREGA may explain why the completion rate under NREGA is lower than that of SGRY.

Workers

There is a unit mass of workers where the utility function of a worker is defined by: $U_t(\chi, w_t) = w_t - b\chi$ where w_t is the private wage at time t , b is the private sector specific cost while χ is the individual specific cost of working which is supposed to be uniformly distributed between $[0, 1]$. Both b and χ are assumed to be time invariant. Workers are assumed to supply 1 unit of labor inelastically unless the cost of employment is higher than the wage earned. Without loss of generality, we can normalize the worker's reservation utility to zero to obtain the following inverse labor supply relation: $w_t(l_t) = bl_t$ for $l_t \leq 1$.

Employers

Since our objective is to highlight the increase in private wages due to the build up of public capital under an EGS, we switch off *spot market effects* like gains in efficiency that are obtained by assuming employers to have oligopsonistic market power as in BCK.¹⁵ The insights obtained from the productivity channel are invariant to the assumed market power of the employers. We therefore assume that there are a large number of employers $N \rightarrow \infty$ representing a competitive labor market structure. Like in BCK, labor is assumed to be the only input of production with a marginal (and average) value product of labor a_t while wage w_t is the only cost that the employers incur during production. A representative employer hence maximizes his objective function: $\underset{l_t}{Max} [a_t - w_t] l_t$. Hence, wage w_t is simply equal to the productivity a_t while aggregate employment equals a_t/b .

This invites the same interpretation as in the BCK: aggregate unemployment can exist if productivity is low enough or the cost of employment is high enough. To introduce the role of EGS capital in increasing productivity, we define a_t as a concave function of the existing stock of capital generated under the EGS. We are agnostic about the functional form of capital accumulation under EGS. It is assumed that the capital stock accumulates every period under an active EGS while the existing capital depreciates geometrically at the rate $1 - \delta$. Hence $a_t = a_0 h(G_t)$ where $h(0)$ is normalized to 1, $h'(\cdot) > 0$, and $h''(\cdot) < 0$ while G_t is the existing stock of EGS capital at the beginning of period t . Therefore, $G_t = g_{t-1} + \delta g_{t-2} + \dots + \delta^{t-1} g_0$ where g_t is the productive capital generated under the EGS at time t .

¹⁵Appendix A1 discusses the role of the stock of EGS expenditure if the increase in wage is solely due to such gains in efficiency.

We substitute the unobserved productive capital generated in every period by approximating it as a constant returns to scale function of the expenditure in year t : e_t which is observed for all districts.¹⁶ Therefore we can represent productivity $a_t = a_0 f(E_t)$ where $f(\cdot)$ is a concave function with similar properties as the function $h(\cdot)$ and $E_t = \sum_{j=1}^t e_j \delta^{(t-j)}$ as the stock of EGS expenditure. The parameter δ can therefore also be interpreted as the present useful component of the past EGS expenditures. The value of $\delta = 1$ minimizes the sum of squared residuals of our preferred empirical specification (see Appendix A3 for details). Equilibrium in the labor market implies that the pre-EGS private labor at the start of period t is: $l_t^0 = a_t/b$, while the pre-EGS private wage is $w_t^0 = a_t$ where the productivity at the start of the period t is a function of the stock of EGS capital which exists at the beginning of period t .

EGS

Let l_t^p be the private employment in the presence of EGS. Let EGS wage be w_t^g and τ_t^g as the wage and access cost of EGS. Like in the BCK model, we introduce the access cost τ_t^g defined as the relative ease with which workers can access EGS work compared to the private work. That is, the cost of accessing EGS work $b_t^g = b/(1 + \tau_t^g)$. The concept of introducing the relative cost of EGS work as a multiplicative component to the worker specific cost enables EGS to selectively target workers with relatively high individual cost.¹⁷ Then with $\tau_t^g \geq 0$ and $w_t^g \leq w_t^0 = a_t$, supply of labor to the private sector is met with the following condition:¹⁸

$$\chi \leq \frac{(a_t - w_t^g)(1 + \tau_t^g)}{b\tau_t^g} = l_t^p$$

While the condition of being unemployed even after an EGS is given by:

$$\chi \geq \frac{w_t^g(1 + \tau_t^g)}{b} = l_t^{total}$$

¹⁶For public works under both SGRY and NREGA, the labor to capital expenditure ratio is usually maintained at a fixed proportion of 60:40 (see Table 1). Given that expenditure on capital and labor increases in roughly fixed proportions of total expenditure, constant returns to scale is a reasonable assumption.

¹⁷Otherwise EGS will either not hire any workers or will completely displace the private workforce. See BCK for details.

¹⁸As explained in BCK, we do not consider the case of $\tau_t^g < 0$, since this corresponds to EGS wage $w_t^g > a_t$ which goes against the stated objective of EGS providing wages at the *minimum wage* rate to avoid competition with the private sector.

$l_t^g = l_t^{total} - l_t^p$ if and only if $l_t^{total} \geq l_t^p$ [i.e. if $a_t \leq w_t^g(1 + \tau_t^g)$]. Depending upon the value of EGS wage and access (w_t^g, τ_t^g), we have 3 cases at hand:

Firstly, if EGS wage and access are jointly so low that: $w_t^g(1 + \tau_t^g) < a_t$, then the introduction of EGS has no impact on private employment. The labor supply facing the private employers is the same as without EGS. Private employment and wages in equilibrium are at the pre-EGS level while no labor is hired in the EGS. At the other extreme, if $a_t < w_t^g$, then EGS completely crowds out private labor (assuming $\tau_t^g \geq 0$). The only other non-trivial option where EGS hires a positive number of workers without completely displacing private labor is:

Proposition *If $w_t^g \leq a_t \leq w_t^g(1 + \tau_t^g)$, the supply of labor to the private sector contracts compared with the pre-EGS case. Hence the equilibrium labor in private sector is less than the pre-EGS level. That is $0 \leq l_t^p = \frac{(a_t - w_t^g)(1 + \tau_t^g)}{b\tau_t^g} \leq l_t^0$. EGS employment $l_t^g = \frac{w_t^g(1 + \tau_t^g)}{b} - l_t^p$. Private wage stays at $w_t^0 = a_t$. Hiring positive workers in EGS results in creation of productive assets which increases worker productivity and consequently private wages in the next period to $a_{t+1} = a_0 f(G_{t+1}) > a_0 f(G_t) = a_t$.¹⁹*

Hence as the proposition above shows, private wages can increase even in the absence of spot market effects as long as the EGS employs a positive amount of labor which results in the build up of public capital generated under such employment schemes.

Furthermore, as is shown in the BCK model, wages can increase contemporaneously due to efficiency gains by correcting the distortions in the labor market that arise due to oligopsonistic market power of employers. In fact, even the asset accumulation channel can be motivated to raise EGS wages contemporaneously by assuming a two season model as discussed in Basu (2013).²⁰ Given the annual frequency of our data in the empirical analysis, we include contemporaneous expenditure in the calculation of the stock of EGS expenditure to account for such possible upward pressures on wages emanating from other channels.

¹⁹In comparison to our proposition, BCK discuss cases in which post-EGS private employment may be higher than the pre-EGS level. This result emanates from the assumed market power of the employers. Since private wages increase in all their cases and since the focus of our analysis is the impact on wages, we circumvent a lengthy discussion on private employment by assuming competitive labor market.

²⁰Basu (2013) discusses hiring of labor in EGS and generation of public capital in the *lean* (or dry) season with the productive gains being realized in the *peak* (rainy) season.

4 Data

Data on program expenditure over the two employment schemes is collected from Ministry of Rural Development (MoRD), Government of India. Although information on NREGA is publicly available (www.nrega.nic.in), data on the now defunct SGRY was collected from MoRD and Datanet (India) on request.²¹ Data on agricultural wage rates is from Agricultural Wages of India (AWI) series. We deflate both the wage and employment expenditure data to 2001 prices using Consumer Price Index for Rural Laborers (CPI-RL) collected by the Labor Bureau, Government of India. Unless otherwise mentioned, all the variables in the empirical analysis are in real, per-capita terms.

Employment expenditure data The Ministry of Rural Development reports district-wise annual physical and financial statements for both the programs. Physical statements include details like the number of public works planned, initiated, and completed while the financial statements give statistics on the opening balance, total availability of funds, and total expenditure. We use “total expenditure” figures to construct the stock of EGS expenditure.

Data on SGRY is from the year of its implementation in 2001 to its last operational financial year 2007-08. Data on NREGA is from the financial year 2006-07 to 2010-11. Hence we have ten years (2001-2010) of data on employment expenditure (and other related variables) for all the districts of the country. At any point in time, only one of the two schemes were active in a district.²² To the best of our knowledge, we are the first to consider such disaggregated employment expenditure data which spans two major employment programs over the period of ten years to understand the impact of EGS on wages in India.

Agricultural wage data We use wage rate data of rural labor as reported by the Agricultural Wages of India (AWI). The AWI data series was initiated by Ministry of Agriculture in 1951. The uniqueness of the AWI data series is the availability of monthly wage rate data at district level. This provides us with a much more continuous data in comparison with the National Sample Survey data sets which are

²¹To check the reliability of the SGRY data, the district-wise estimates were aggregated to state-level and were compared with the corresponding state-level estimates which are published in the MoRD annual reports. Apart from minor differences, the match was near perfect.

²²As noted earlier, the two programs did not coexist together in a district even during the phase-wise implementation of NREGA.

conducted quinquennially.²³ The AWI data is the most reliable source of agricultural wages in India and has been extensively used for time series analysis pertaining to agricultural wages in India.²⁴ We take the average of male and female wages to construct our measure of agricultural wages. Since the employment expenditure data is of annual frequency, we convert the monthly wage data to annual frequency by taking 12 month averages. Appendix A4 discusses construction of wage and other variables in detail. Since the wage data is not available for all the districts in the country, our final district level panel data is for 10 years from 2001-2010 and covers 151 districts over 13 major states of India.

5 Empirical model and results

Regression framework

We empirically estimate the non-linear relationship between wages and the stock of EGS expenditure (as motivated in section 3) as a log-log model. We therefore estimate various specifications of:

$$\log(w_{i,t}) = \alpha_i + \theta_t + \xi_i t + \beta \log(E_{i,t}) + \gamma I_{NREGA} + \phi \mathbf{X}_{i,t} + v_{i,t} \quad (1)$$

where the subscripts i and t denote district and year respectively. $w_{i,t}$ is the field wage, and $E_{i,t}$ is the stock of EGS expenditure as defined in section 3 (with $\delta = 1$).²⁵ I_{NREGA} is an indicator variable which takes the value one for a district from the year NREGA was introduced and zero before that. The coefficient of I_{NREGA} : γ estimates the effect of NREGA or the *intensification effect*. District and year fixed effects are controlled by α_i and θ_t respectively while $\xi_i t$ control for district specific (differential) trends. Finally, $\mathbf{X}_{i,t}$ is a vector of other controls discussed below.

A serious threat to our empirical strategy is the problem of endogeneity. Below, we illustrate how our empirical specification corrects for the endogeneity bias. We further check for potentially endogenous district-year fluctuations in program expenditure in section 5.1.

²³Recently, *thin rounds* (with lesser number of observations) have been conducted at a higher frequency. However there are issues like the reliability and representativeness of thin round estimates.

²⁴See for e.g., Ravallion et al. (1993), Özler et al. (1996), Himanshu (2005), and Berg et al. (2015).

²⁵In Appendix A3 we show that the results are largely robust to non-zero rates of depreciation.

If district specific indices (fixed effects) like demographics, proneness to drought, and relative poverty ratios are influential in determining the level of expenditure, then omitting such unobserved and time-invariant district effects may result in an omitted variable bias.²⁶ To highlight this point, we first estimate Equation (1) with $\log(E_{i,t})$ and I_{NREGA} as the only regressors. To account for the problem of serial correlation, we report standard errors clustered at the district level in all the regressions. Furthermore, given the heterogeneity in the size of the districts in our panel, all the regressions are weighted by district population.²⁷ As the first column of Table 3 shows, the coefficient of $\log(E_{i,t})$ is estimated to be -0.7 which is highly significantly different from zero ($t > 3.5$) while the additional impact of NREGA is estimated to significantly increase wages by 20%. Intuitively, if the level of EGS expenditure is positively related to the backwardness of a district, then the estimated coefficient $\hat{\beta}$ should be expected to be suffering from a large negative bias towards zero.

To account for fixed effects in our model, we add district fixed effects α_i and the indicator variable I_{NREGA} (to measure any additional effect due to the NREGA) in the second column of Table 3. As the estimates show, the elasticity of wage increases to 0.06 which is highly significant at 0.1% level. The coefficient on I_{NREGA} reports a significant 3.8% additional increase in wages due to NREGA. This finding lends support to our hypothesis that the estimate $\hat{\beta}$ in column 1 was suffering from a large negative bias due to omitted fixed effects. Another important source of endogeneity can be a change in the supply of EGS at the national level in a given year due to (say) an aggregate negative shock to the agriculture sector. To account for such possibly endogenous increases or decreases in the outlay of funds at the national level, we further include year fixed effects (θ_t) to our specification in column 3 of Table 3. Year fixed effects also control for aggregate events like bad weather shocks (poor monsoon) that may affect the agriculture sector or field wages at a national level. Including year fixed effects further increase our estimate $\hat{\beta}$ to 0.085 which is significantly different from zero at 1% level. The coefficient of I_{NREGA} ceases to be significantly different from zero after the inclusion of year fixed effects.²⁸

²⁶See Bahal (2015) for a discussion of how relative indices like state to national poverty ratio can largely be categorized as a time invariant, state-specific feature.

²⁷Apart from minor changes in the point estimates, the basic result of our analysis remains the same if we do not weight our regressions.

²⁸Dropping I_{NREGA} does not impact the estimate $\hat{\beta}$ in Table 3, we nevertheless continue to include it in all our specifications.

Next we check for a possible bias due to the presence of district specific trends. Controlling for differential trends allow us to check for any potential bias that may result if the growth in the stock of EGS expenditure reflects the evolution of district specific indicators that may in turn be correlated with the growth of field wages. Growth of indicators like the general development or the capability to implement the scheme can be district specific and are hence important to control for. Including heterogeneous trends however may also confound the analysis as the empirical specification may not be able to distinguish the treatment effect from differential trends.

Column 4 of Table 3 estimates $\hat{\beta}$ after adding district specific linear trends ($\xi_i t$) as additional controls. Encouragingly, the estimate of $\log(E_{i,t})$ is highly stable at 0.08 and continues to be significantly different from zero at 1% level. This suggests that most of the endogeneity may be attributed to the *between* district variation in $\log(E_{i,t})$. Figure 4 motivates this point by plotting the district-wise growth of the stock of EGS expenditure over time for four major states: Rajasthan, Orissa, Madhya Pradesh, and Andhra Pradesh. As Figure 4 shows, most of the *within* district variation in $\log(E_{i,t})$ correspond to the first, second, and third phases of NREGA implementation in each of the states during the years 2006, 2007, and 2008 respectively (highlighted in grey). Such within variation is likely to be exogenous as these were mostly due to a change in policy at the national level which was independent to current or prospective fluctuations in local wages. This may explain why inclusion of district specific trends does not change $\hat{\beta}$ substantially.²⁹

Finally, in column 5 of Table 3, we add additional regressors like an indicator for state election-year and the proportion of Scheduled Castes and Scheduled Tribes population in a district. These variables are important to control for since political motivations at the time of state elections and other district characteristics may influence the overall level of expenditure in a district. These variables are together represented by $\mathbf{X}_{i,t}$ in Equation (1). The estimate of $\log(E_{i,t})$ is robust to the inclusion of these additional controls and remains significant at 1% level.

We treat this final specification with all controls as our preferred specification. Hence Table 3 provides evidence that after controlling for district and year fixed

²⁹Controlling for district-specific trends however is important in our case since without such controls, the general growth in wages over time may be misrepresented as the effect on wages due to $E_{i,t}$ (which includes a trend by construction).

effects, elasticity of wage is extremely stable and robust to the inclusion of district-specific trends and other important controls. Based on the average annual growth of the stock of EGS expenditure along with the estimated elasticity of wage implies an average wage growth of 2.86%. Since annual growth of $E_{i,t}$ follows a distribution with some outliers, we also compute annual wage growth based on median growth of $E_{i,t}$ which estimates wage growth at 2.34%.³⁰ The estimated growth in wages due to the two workfare programs is large especially when compared with the average growth of 3% per annum for the districts in our sample.

5.1 Available funds as an instrument for actual expenditure

Although our preferred specification of Equation (1) deals with the problem of endogeneity emanating from both time invariant heterogeneous effects and district-specific trends, it is still susceptible to district-year specific shocks that may influence field wages and EGS expenditure simultaneously. For example, a *local* negative weather shock may adversely affect the agriculture sector (and hence wages) while resulting in higher than expected EGS expenditure due to a higher demand for public work. See Drèze (1990) which discusses a high take up of public employment by laborers under Maharashtra’s EGS during the famine of 1970-73. Such weather shocks, along with any other events like local conflicts may negatively impact wages and result in higher than expected expenditure. If true, our estimated elasticity of wage is still likely to suffer from a negative omitted variable bias. We exploit the fund allocation and expenditure process of the schemes to first identify the expenditure variations that can possibly be classified as endogenous. We then check for any potential bias due to such fluctuations using availability of funds at the district level as an instrument for actual expenditure.

Fund allocation process

Under both the programs we study, districts receive funds from the central government at the start of the fiscal year. Given the nature of the accounting process which rolls over to the next financial year, there is a possibility that the total actual expenditure (at district level) may exceed the funds made available at the start of

³⁰The average and median growth rates of $E_{i,t}$ are stable over the two program regimes. Hence our implied wage growth estimates are not sensitive to any particular program regime. Below we test whether constant elasticity over the two program regimes can be assumed.

the fiscal year. Any previous obligations resulting from excess expenditure in the last fiscal year are shown as negative *opening balance* in the following year which are met by appropriate *releases* from the central government. Hence *availability of funds = releases + opening balance*.

Utilization of available funds

We define utilization ratio = $100 * e_{i,t} / e_{i,j}^a$, where $e_{i,j}^a$ is the availability of funds for district i in year t . Figure 5 highlights year-wise utilization of funds as a percentage of funds made available for the 151 districts considered in our study. The observations marked in red show over-utilization while the observations in blue represent under-utilization. A valid concern towards the estimates reported in our preferred specification is that if over utilization of funds correspond to (say) weather or any other local negative shock, then our OLS estimates may still be downward biased since we do not control for such district-year events in Equation (1).³¹ Similarly, if under-utilization of available funds reflects actual demand for the program (which is low under good market conditions), then again our estimates can be expected to be downward biased.

Determinants of fund availability

Government records suggest that apart from district and year specific effects (which are already controlled for in our preferred specification), the utilization of funds (at district level) during the *previous* year is an important indicator in deciding the availability of funds in the current year.³² This change in the availability of funds is *after* settling any previous obligations that are reflected in the opening balance (which are met by the appropriate releases). To further clarify this point, we show in column 1 of Table 4 that a 1% increase in last year’s utilization ratio increases the funds released by 0.92% in the current year. In column 2 of Table 4 we regress fund availability on last year’s utilization ratio. As column 2 shows, a 1% increase in the utilization ratio in the previous year results in a 0.42% increase in the availability of funds in the current year. Given that part of the releases goes towards meeting any previous obligations, the higher response of releases (in comparison with fund availability) to past utilization ratio is expected. Comparing results from column 1 and 2 imply that the positive association between availability of funds and past uti-

³¹Nearly 9% of the total observations represent events where funds were over-utilized.

³²See for example nrega.nic.in/presentations/implement_NREGA.ppt.

lization is not an accounting relation and indeed reflects pre-determined adjustments intended to reduce the gap between fund availability and expenditure needs.

Given the flexibility accorded in the system where actual expenditure can accommodate fluctuations in the demand for EGS due to unexpected events like a weather shock, we may use fund availability as an instrument for actual expenditure to bypass such *potentially* endogenous and unexpected district-year expenditure fluctuations as shown in Figure 5. However, if fund availability moves with anticipated future shocks, then this may threaten its validity as an instrument. To address this concern, we choose the average rainfall a district receives (in millimeters) during the forthcoming rainy season as our measure of shock and check its influence on fund availability in column 3 of Table 4.³³ We discuss rainfall data in Appendix A4.

We choose rainfall as our measure of shock since: 1) fluctuations in rainfall are truly independent to wage and the level of EGS expenditure in a district; 2) local weather shocks are the most relevant in causing disruptions to the agriculture sector in India which may also prompt a higher injection of funds in EGS as part of aid expenditure; and 3) weather events are systematically forecasted and this measure *may* inform the availability of funds next year. The regression in column 3 however shows that availability of funds is invariant to the amount of rainfall a district receives in a particular year during the rainy season.³⁴ The results in Table 4 hence suggest that changes in fund availability are largely pre-determined and do not correspond to anticipated future district-year shocks.³⁵ The lack of any motivation to efficiently predict availability of funds, given the flexibility to under or overspend (as shown in Figure 5) supports our hypothesis that availability of funds is independent of future district-year shocks. We therefore use availability of funds as an instrument for actual expenditure to check for potentially endogenous district-year fluctuations in EGS expenditure.

OLS versus 2SLS

Keeping the same set of controls as in our preferred specification (Equation 1),

³³We define rainy season (also known as monsoon in India) as the months of June, July, and August. Results are invariant to choosing other criteria of measuring rainfall.

³⁴Although actual rainfall figures are not available at the start of the fiscal year, if the forecasted value of such shocks are indeed influential in determining fund availability, then so should be the actual estimates.

³⁵We rule out serial correlation between shocks by checking for correlation between events that represent rainfall above or below one standard deviation from the mean.

we use two alternative instruments for our suspected endogenous variable: $\log(E_{i,t})$. Firstly, we instrument $\log(E_{i,t})$ with the stock of funds made available $E_{i,t}^A = \sum_{j=1}^t e_{i,j}^a \delta^{t-j}$ where $e_{i,j}^a$ is as defined above. Equation (2) represents the first stage regression equation corresponding to this instrument:

$$\log(E_{i,t}) = \alpha_i + \theta_t + \xi_i t + \beta \log(E_{i,t}^A) + \gamma I_{NREGA} + \boldsymbol{\xi} \mathbf{X}_{i,t} + v_{i,t} \quad (2)$$

Secondly, we use only the contemporaneous availability of funds $e_{i,j}^a$ to instrument for $\log(E_{i,t})$ with Equation (3) as the corresponding first stage regression equation:

$$\log(E_{i,t}) = \alpha_i + \theta_t + \xi_i t + \beta \log(e_{i,j}^a) + \gamma I_{NREGA} + \boldsymbol{\xi} \mathbf{X}_{i,t} + v_{i,t} \quad (3)$$

For comparison with estimates obtained under the two-stage least squares (2SLS), column 1 of Table 5 reports the results obtained under our preferred specification (column 5 of Table 3). Columns 2 and 3 of Table 3 respectively report the first and second stage results while using the stock of funds made available: $\log(E_{i,t}^A)$ as an instrument for $\log(E_{i,t})$. While columns 4 and 5 respectively show the first and second stage results when we use only the contemporaneous availability of funds: $e_{i,j}^a$ as an instrument for $\log(E_{i,t})$.

The dependent variable in OLS and both second stage estimations of 2SLS is $\log(w_{i,t})$. As expected, there is a near one to one correspondence between the stocks of EGS expenditure and funds made available (column 2). The first stage regression of $\log(E_{i,t})$ on $\log(E_{i,t}^A)$ (with all controls) yields an estimate of 0.97 which is highly significant. The coefficient of $\log(E_{i,t})$ in second stage is estimated to be at 0.07 which is very close to the OLS estimate of 0.08 as reported in column 1.

Given that the 2SLS and OLS estimates of $\log(E_{i,t})$ are so close to each other, we test whether the suspected endogenous regressor $\log(E_{i,t})$ can indeed be treated as exogenous. The *Endog. Test* (last row) which is based on the difference of Sargan-Hansen test statistics reports the *p-value* of 0.33. The estimated test statistic and the corresponding *p-value* suggest that we cannot reject the null of exogeneity of $\log(E_{i,t})$.

Finally we do a robustness check on our 2SLS analysis by instrumenting $\log(E_{i,t})$ by $\log(e_{i,j}^a)$. The first stage regression results of Equation (3) are reported in column 4 of Table 5. The estimated first stage results imply that a 1% increase in contemporaneous availability of funds increases the stock of EGS expenditure by roughly a

quarter of a percent.³⁶ The F statistics for both our instruments is well above 20 and hence do not pose a threat of weak instruments problem.

The corresponding coefficient of $\log(E_{i,t})$ in the second stage (column 5) is estimated to be 0.8 which is the same as the OLS estimate. Since the 2SLS estimate is essentially same as the OLS estimate, the endogeneity test results in a highly insignificant test statistics (p – value of 0.98) again suggesting that $\log(E_{i,t})$ can be treated as an exogenous regressor in our preferred specification.

Similar OLS and IV results gives us confidence on the reliability of the estimates obtained in our preferred specification of Equation (1). The results also suggest that seemingly endogenous district-year shocks as shown in Figure 5 may most likely be caused due idiosyncratic fluctuations in the supply side (like political or administrative will) that are largely uncorrelated to fluctuations in local wage rates. Supporting a similar point, Imbert and Papp (2015) conclude that the substantial inter-state heterogeneity in the provision of public employment under NREGA may be due to supply factors like administrative capacity or political will rather than demand factors like labor market conditions or poverty.

Hence, the stock effect seems to capture an annual wage increase of 2.3-2.9% due to employment schemes. On the contrary and in agreement with Berg et al. (2015), we find no immediate jump in wages due to the implementation of NREGA which signifies weak spot effects. We replicate Berg et al. (2015) results in Appendix A2. Below we discuss further extensions on the stock effect under our preferred baseline specification of Equation (1).

5.2 Robustness

Monte Carlo Placebo Simulations

While using a panel of district-year observations, it is important to check for the power of standard statistical tests. We conduct a series of Monte Carlo placebo simulations where we randomly assign the time-series of the stock of EGS expenditure: $\{\log(E_{i,2001}), \log(E_{i,2002}), \dots, \log(E_{i,2010})\}$ among districts³⁷. The rationale behind

³⁶However unlike Equation (2), the first stage relation between $\log(E_{i,t})$ and $\log(e_{i,t}^a)$ in Equation (3) does not represent any structural relationship between the two variables and is data driven.

³⁷Randomizing observations across districts and years will destroy the interpretation of the variable as a stock measure and result in a zero estimate by construction. In that respect, our simulations are more conservative as we shuffle the entire time series.

such an experiment is that if the stock of EGS expenditure has no influence on field wages, then the placebo coefficients obtained from shuffling the regressor and matching up with different values of the dependent variable should not be too different from the true (non-randomized) coefficient.³⁸

Figure 6 plots the cumulative distribution function of the placebo estimates along with the true (non-randomized) coefficient as estimated in column 5, Table 3. In each simulation, a coefficient is estimated for the regression of field wages on the shuffled stock of EGS expenditure variable under our preferred specification of Equation (1). As can be seen, the placebo estimates never exceed the true coefficient. Hence the effect of EGS on wages is estimated to be the largest only when we align a district to its true stock of EGS expenditure series. The simulations also validate the specificity of the tests as only 1.2% of the placebo coefficients were found to be statistically significant and greater than zero.³⁹

Effect over different program regimes

Until now we have assumed that the response of wages to EGS expenditure is similar over both the program regimes. Although Equation (1) allows for growth of wages to be different on average under NREGA by controlling for I_{NREGA} , it assumes constant elasticity of wage under the two programs. Hence to check whether wages have become more or less responsive under NREGA, we estimate separate elasticities for SGRY ($I_{NREGA} = 0$) and NREGA ($I_{NREGA} = 1$) regimes. If projects under NREGA were comparatively more productive than those undertaken in SGRY, then this may imply a higher productivity (and hence wage) increase than usual. Table 6 compares the elasticity of wage as estimated under our preferred specification in column 1 to the case where separate elasticities are estimated for different programs by interacting I_{NREGA} with $\log(E_{i,t})$.

The coefficient of I_{NREGA} continues to be insignificantly different from zero. The elasticity of wage under SGRY regime is estimated to be slightly higher at 0.11 and is highly significant ($t > 3$) while the corresponding elasticity of wage under NREGA continues to be very similar to our baseline elasticity of 0.08 which also is highly significant ($t > 3$). However, we cannot reject the equality of the two coefficients even at 10% significance level. Hence, our results suggest that the elasticities of wage

³⁸See for example Kennedy (2003) and Shoag (2010) for a discussion on the sampling distribution of the test statistics employing randomized simulations.

³⁹This is consistent with the desired specificity of the tests.

are very comparable under the two program regimes.

Other robustness checks

We test whether there is any sub sample heterogeneity based on the characteristics that define how well or how intensely the employment guarantee schemes were implemented. A useful criterion to measure the degree of implementation of such schemes is the rationing rate of public employment. Dutta et al. (2012) define rate of rationing as the proportion of laborers who wanted but did not get work in EGS. To check whether the relationship between $\log(w_{i,t})$ and $\log(E_{i,t})$ is structurally different for states based on the extent of rationing they did, we split the overall sample of 13 states into 6 *less rationing states* (rationing < 40%) and 7 *high rationing states* (rationing > 40%).

The criterion of selecting the benchmark value of 40% is simply to divide the aggregate sample into two (near) equal halves. Column 1 of Table 7 reports the results of estimating separate elasticities by interacting an indicator variable ‘Ration’ which takes the value one if state-wise rationing is less than 40% and zero otherwise.⁴⁰ The elasticities for the high and low rationing groups are estimated to be around 0.09 and 0.08 respectively. Both the estimates are very close to the aggregate sample elasticity as reported in column 5, Table 3. Given the loss of observations, there is a substantial increase in the standard errors for both the estimates. Both estimates however are still significant at the standard (5%) confidence level. As expected, we cannot reject the null of equality of the two coefficients ($p - value = 0.76$). This is evidence to support that the underlying relationship between wages and expenditure is stable across states and is largely independent of the extent of rationing.

Another important variable that may help ascertain the extent or intensity of program implementation is the annual average per-capita expenditure incurred over a district. Column 2 of Table 7 reports separate elasticities for sub-samples divided on the basis of district-wise *low* and *high* annual average expenditure respectively. Like in the case of rationing, this is achieved by interacting an indicator variable ‘Avg Exp’ which takes the value one (zero) if the average expenditure of a district is above (below) the median value of the series $(\sum_{t=2001}^{2010} e_{i,t})/10$. The results are very similar to the rationing case of column 1. The elasticities for the *low* and *high* average

⁴⁰The indicator variable ‘Ration’ takes value one for less rationing states: Rajasthan, Tamil Nadu, Himachal Pradesh, Andhra Pradesh, West Bengal, and Madhya Pradesh and zero for the rest of the seven states in our sample.

expenditure groups are estimated to be around 0.09 and 0.07 respectively which are significant at 5% confidence levels. Again, the equality of the two coefficients cannot be rejected ($p\text{-value} = 0.70$).

The results hence support our preferred specification which assumes a constant elasticity of wage over the entire sample. It is worth mentioning that constant elasticity of wage doesn't necessarily imply absence of any *scale effects* because of rationing, average expenditure, or how well the program is implemented.⁴¹ Any such differences in the intercept or growth rate of wages at the district level are controlled (eliminated) by the fixed effects (both fixed and time varying) in our model. In that spirit, our results can best be understood as providing a lower bound estimate of the impact that such schemes have had on wages since 2001. Appendix A5 discusses other results like the program impact on male and female field wages separately.

6 Conclusion

Large public workfare programs that are involved in the creation of public infrastructure along with the provision of public employment can increase private wages apart from the spot market effects like efficiency gains and labor demand effects. In this paper, we motivate growth in wages due to the increase in productivity as a result of the build up of productive public capital. This channel motivates the stock of EGS expenditure as the relevant measure to capture the impact of EGS on private wages. Our empirical specification of using stock of EGS expenditure is however not hinged solely on the accrual of public capital. Improved capability to implement such schemes more efficiently due to the accumulation of administrative capital can also contribute to explain the stock effect of such programs. As a falsification exercise, we show that if the underlying mechanism of wage increase is solely due to a spot market effect like gains in efficiency, then the stock of EGS expenditure is not a relevant measure in capturing wage increases.

Another contribution of this study is to employ a novel data-set of district-wise annual employment expenditure over the period 2001-10 to construct the stock of EGS expenditure to estimate the impact of EGS on wages. Our baseline identification is achieved after controlling for district and year fixed effects, differential trends, and

⁴¹Imbert and Papp (2015) for example report higher impact on wages for star states that implemented NREGA well.

other controls. In line with our expectations, this exercise shows the correction of a large negative bias in the estimate of the elasticity of wage. Finally, we exploit the fund allocation process and use the availability of funds as an instrument for actual expenditure to check for potentially endogenous district-year fluctuations that may further bias the results. The results from this exercise support our baseline identification. We find agricultural wages to have grown by 2.3-2.9% per annum due to such large workfare programs.

The importance of the stock effect highlighted in the paper has broad implications for impact evaluation of similar policies. The key insight of the study is that the impact of a policy change may not solely depend on individual doses of policy treatment. Rather, the variable of interest may be sensitive to a measure that captures the aggregate exposure to the new policy. The stock or aggregate exposure effect has been extensively discussed in the medical literature and is a promising area of future research in impact evaluation of economic policies.

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Appendix

A1. Simulating the spot market effect of EGS on wages

We construct an artificial panel data-set of 100 districts and 10 years using a model of EGS as discussed in BCK. We show that if the underlying data generating process is a model where private sector wages change only because of the movements in the spot market for labor, then the stock of EGS expenditure has no power in explaining variations in wages. In this case, the contemporaneous EGS expenditure is the relevant variable to capture the impact of EGS on wages. Using the BCK model, we consider the non-trivial case where the EGS hires a positive amount of labor. The increase in private wages is due to efficiency gains from the introduction of an EGS which alleviates the distortions created by oligopsonistic market power of employers. We regard the increase in wages due to efficiency gains as a spot market effect.

In constructing the data, we use the following assumptions. i) Public wage $w_{i,t}^g$ is assumed to be equal to a constant μ plus a random error $\varepsilon_{i,t} \sim N(0, \sigma^2)$. ii) The marginal (and average) value product of labor a_i is assumed to vary uniformly across the hundred districts between $[a_1 \ a_2]$. iii) Employers are assumed to have monopsonistic or oligopsonistic market power.⁴² iv) Each district is assumed to have a unit mass of working population. Since only $w_{i,t}^g$ (and not l^g) enters the expression of private wage $w_{i,t}$ in BCK, we keep EGS employment to be fixed at l^g for all observations.⁴³

Given assumptions i)-iv), we ensure EGS to hire a fixed number of workers l^g by varying the *access* to EGS $\tau_{i,t}^g$ (which is controlled by the government).⁴⁴ In other words, this assumes that the government aims to create a fixed number of EGS employment in every district and the variation in EGS expenditure emanates solely from exogenous fluctuations in government wage in a district $\varepsilon_{i,t}$. Figure A1.1 plots the log of private wage (y-axis) before and after the EGS for the hundred districts (x-axis) in ascending order of labor productivity. The figure highlights the increase in private wages (in red) due to the *gains in efficiency* in the case of $N = 10$ employers.

⁴²We conduct 10 different simulations by varying the number of employers in each simulation from 1 (monopoly) to 10 (oligopoly).

⁴³A regression of $w_{i,t}$ on EGS expenditure $e_{i,t} = w_{i,t}^g l^g$ is meaningful only as long as variations in EGS expenditure emanate from fluctuations in public wage.

⁴⁴Like in BCK, we ensure that the access to EGS is always less costly than access to private employment. That is $\tau_{i,t}^g \geq 0$.

Using the data as generated under the assumptions discussed above, we estimate Equations (A1.1) and (A1.2) to obtain the elasticities of wage with respect to the flow (β_f) and stock (β_s) of EGS expenditure respectively.⁴⁵

$$\log(w_{i,t}) = \alpha_i + \beta_f \log(e_{i,t}) + v_{i,t} \quad (\text{A1.1})$$

$$\log(w_{i,t}) = \alpha_i + \beta_s \log(E_{i,t}) + v_{i,t} \quad (\text{A1.2})$$

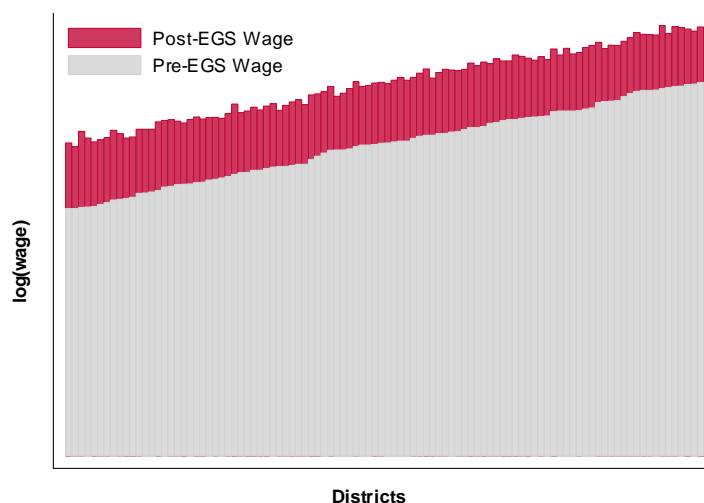
Figure A1.2 compares elasticities of wage under the two specifications while varying the number of employers (x-axis).⁴⁶ In the case the employer has monopsonistic market power ($N = 1$), Figure A1.2 shows that the elasticity of wage using contemporaneous expenditure β_f is estimated to be 0.43 which is highly significantly different from zero ($p < 0.001$). On the contrary, using the stock of EGS expenditure as in Equation (A1.2), β_s is estimated to be equal to zero (up to 3 decimal places). The efficacy of EGS to increase private wages diminishes as the economy tends to a more competitive labor market (as N increases). This is expected as the gains in efficiency work by reducing the distortion created by the market power of the employers. In the limiting case of perfect competition, there is no distortion to correct as wages tend to be equal to the marginal product of labor a_i . This is reflected in Figure A1.2 where β_f decreases as the number of employers increase. On the other hand, the elasticity of wage β_s as obtained under Equation (A1.2) is always estimated to be equal to zero, irrespective of the assumed number of employers.

This exercise hence motivates that if EGS impacts private wages only by affecting the spot market for labor, then the stock of EGS expenditure is not relevant in explaining private wages. On the contrary, in our empirical analysis above, we find a significant and positive impact of the stock of EGS expenditure on wages in contrast to a zero contemporaneous impact (as shown in Appendix A2). This shows that the increase in wages cannot solely be attributed to changes in the spot market for labor.

⁴⁵The interpretation of all the variables in Equations (A1.1) and (A1.2) is the same as in the main text. We only need district fixed effects in the regression since we assume labor productivity (a_i) to be time invariant and district specific. Since the data generated has no trend or year effect by construction, we do not control for such effects.

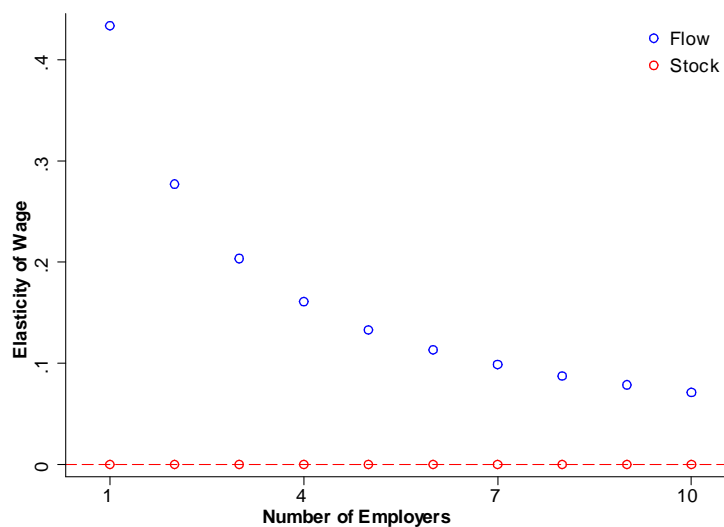
⁴⁶For comparison, we keep the series of public wage ($w_{i,t}^g$), labor productivity (a_i), and public employment generated under the EGS (l^g) to be the same under the N simulations. Only the number of employers and correspondingly, the access to EGS work ($\tau_{i,t}^g$) is allowed to vary.

Figure A1.1: **Wage increase due to efficiency gains**



Note: The figure shows increase in private wages due to efficiency gains under employers having oligopsonistic market power. Districts are ordered in ascending order of worker productivity.

Figure A1.2: **Comparing Stock and Flow Specifications**



Note: The figure compares $\hat{\beta}_f$ with $\hat{\beta}_s$ as estimated under Equations (A1.1) and (A1.2). The figure plots coefficients for 10 different sets of simulations by varying the number of employers (from monopoly to oligopoly).

A2. Berg et al (2015)

Here we replicate the findings of Berg et al. (2015) where the standard regression difference-in-difference (DD) framework is unable to capture any immediate increase in wages due to an EGS. We estimate Equation (A2) where the variables have the same interpretation as in Equation (1). Berg et al. (2015) estimate a very similar regression difference-in-difference model albeit with a monthly frequency AWI data. I_{NREGA} is the NREGA implementation indicator as defined in section 5. The coefficient of $I_{NREGA} : \gamma$ estimates the impact of NREGA on wages. This specification however ignores the continued existence of SGRY in the control districts.

$$\log(w_{i,t}) = \alpha_i + \theta_t + \gamma I_{NREGA} + v_{i,t} \quad (\text{A2})$$

Column 1 of Table A2 shows the estimated coefficient $\hat{\gamma}$ with just an intercept as an additional control. The estimated coefficient indicates that the early-phase NREGA districts had approximately 8% higher wages than late-phase NREGA districts. This is remarkably similar to the ($\approx 7.3\%$) estimate of Berg et al. (2015) for a similar specification. However, the estimated impact of the regression DD does not continue to hold once we account for district and year fixed effects in column 2. Controlling for district and year fixed effects, $\hat{\gamma}$ drops to -0.02 being insignificantly different from zero. This again matches well with Berg et al. (2015) results of zero impact after controlling for district and year fixed effects. As is discussed in their study, this highlights the absence of a strong contemporaneous impact of EGS on wages.

Table A2: **Impact on Wages using Simple Difference-in-Difference**

	$\log(w_{i,t})$	$\log(w_{i,t})$
I_{NREGA}	0.080*** [0.021]	-0.021 [0.019]
District Effect	No	Yes
Year Effects	No	Yes
Observations	1493	1493
Adjusted R^2	0.012	0.847

All regressions are weighted by district-level population. The standard errors reported in square brackets are clustered at district level and are robust to heteroskedasticity.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

A3. Estimating δ

We are agnostic about the rate of accumulation and estimate δ from the data. We first fix the value of δ , estimate all the parameters in Equation (1) and compute the corresponding sum of squared errors. We then repeat the same experiment for a large number of values of δ between $[0, 1]$. Finally, we choose the value of δ that corresponds to the minimum of sum of squared errors so obtained. Persson and Tabellini (2009) use a similar procedure to identify the structural parameters in their model in order to construct the stock of *democratic capital*.⁴⁷ Similar to them, we find a corner solution of $\delta = 1$ that best fits the data.

However, considering zero depreciation implies that all past treatments are weighted equally and independently to each other, which may be a strong assumption. To check for the robustness of our results, we relax this assumption. Below we compare our results (under our preferred specification) obtained by assuming zero depreciation ($\delta = 1$) to cases where we impose ad-hoc depreciation rates of 2.5% ($\delta = 0.975$) and 5% ($\delta = 0.95$). Our results are largely robust to these assumed rates of depreciation. However, it is worth noting that as depreciation rate increases from zero to complete depreciation, $E_{i,t}$ tends to $e_{i,j}$ which only allows for spot market effects (see Appendix A1).

Table A3: **Results with different discounting rates**

	$\delta = 1$	$\delta = 0.975$	$\delta = 0.95$
$\log(E_{i,t})$	0.081**	0.077**	0.073**
	[0.025]	[0.024]	[0.023]
All Controls	Yes	Yes	Yes
Observations	1493	1493	1493
Adjusted R^2	0.912	0.912	0.912

The dependent variable is $\log(w_{i,t})$. All regressions are weighted by district-level population. The standard errors reported in square brackets are clustered at district level and are robust to heteroskedasticity.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

⁴⁷Identification of the structural parameters in their case is based on maximizing the envelope of likelihood function (corresponding to logit estimation). See Persson and Tabellini (2009) for details.

A4. Data

Agricultural wage data The data reports daily wage rates for four main categories of rural labor: *skilled labor*, *field labor*, *other agricultural labor*, and *herdsman*. Skilled labor is further disaggregated into blacksmith, carpenter, and cobbler. *Field labor* - the category central to our analysis - reports wage rates for ploughing, sowing, reaping, and weeding.⁴⁸ With the exception of skilled labor (which reports wages only for men), wages are reported for men, women, and children for the rest of the above mentioned operations. We exclude wages reported under children as most of the observations are missing under this category. We take the average of male and female wages to construct our measure of field wages. Hence we have monthly wage data for ten years from 2001-2010. The AWI series are reported in the agricultural year format which starts from July to June of the next calendar year.

Matching wage and expenditure data: The monthly frequency AWI data is not of a very good quality with missing data for some of months. Furthermore, the annual publication of AWI data sporadically includes or excludes data at the district and even state level.⁴⁹ Data for nearly 40% of the districts is reported for less than 6 out of 10 years. We first improve the signal to noise ratio of wage data by converting the monthly AWI data to annual frequency by taking 12 month averages from April to March of the next calendar year to match the frequency and period of the employment expenditure data which is reported in the Indian financial year format.⁵⁰

To allow for comparison over the two program regimes and to ensure the reliability of our results, we consider as balanced wage data as possible. We circumvent the problem of missing data by restricting our analysis to only those districts for which the wage data has been reported for at-least nine out of the maximum 10 years.⁵¹ There are 13 major states and 151 districts that satisfy this criterion.

One possible objection to the elimination of districts with incomplete data can be the correlation of the backwardness of a district with unavailability of the wage data.

⁴⁸ Andhra Pradesh, Karnataka, and Maharashtra do not give operation-wise details for field labor and instead furnish data for the group (field labor) as a whole.

⁴⁹ Data for new states like Chhattisgarh, Jharkhand, and Uttarakhand is not available before 2005.

⁵⁰ The Indian financial year starts from April to March of the following year.

⁵¹ The results don't change at all if we restrict our analysis to a complete balanced panel but we lose nearly 160 observations. Similarly, results largely remain the same if we tend towards a more unbalanced panel.

If this is true, then restricting the analysis to 151 districts will result in the proportion of early phase NREGA districts to be substantially lower in this sub-sample compared to the aggregate sample. This however is not the case. The proportion of phase I and phase II districts at nearly 37% and 58% respectively in the aggregate sample is closely matched by the sub-sample proportion of 42% and 64% for phase I and phase II districts respectively.

Rainfall data We use remote sensed rainfall data from the Tropical Rainfall Measuring Mission (TRMM) satellite.⁵² See Fetzer (2014) for a detailed discussion on the consistency and the quality of TRMM data over any other remote sensed or ground-based data.

A5. Other results

Effect on male and female wages

We check whether there are any differential effects of such programs on male and female wages. The results below are discussed by replacing $\log(w_{i,t})$ in Equation (1) by $\log(w_{i,t}^m)$ (male wages) and $\log(w_{i,t}^f)$ (female wages) respectively. Table A5.1 reports wage elasticities based on gender as well as over different program regimes. Column 1 of Table A5.1 reports elasticity of male wages over the aggregate sample while column 2 shows male wage elasticities separately over SGRY and NREGA regimes. Similarly, columns 3 and 4 of Table A5.1 report elasticities of female wages over the aggregate sample and over separate regimes respectively. The elasticity of male wages is estimated close to the average wage elasticity at 0.07 which is significantly different from zero at 1% level.

In comparison, the elasticity of female wages is nearly 57% higher at 0.11 which is also highly significantly different from zero ($t > 3$). The higher response of female wage rates to EGS is expected since on average, female wage rates are already 18-20% lower than its male counterpart due to general gender biases and other social restrictions. Higher growth in female wage rates is expected given that the wages offered under EGS are equal for men and women. Based on average growth rates of the stock of EGS expenditure, annual male wage growth rate is estimated at 2.37% while female wages are estimated to grow at nearly 3.71% per annum. A test on the equality of coefficients over the two program regimes cannot be rejected even at 10%

⁵²Thanks to Thiemo Fetzer for sharing the rainfall data.

level for both male and female wages. Our result of higher impact of EGS on female wage rates is in accordance with similar findings by [Azam \(2012\)](#).

Effect on skilled wages

The skilled wage category include wages for carpenter, cobbler, and blacksmith. Given that EGS aims to provide only unskilled manual labor work, one should not theoretically expect any effect on such semi-skilled wages that are higher in the wage distribution when compared to field wages. Column 1 of [Table A5.2](#) reports a regression of log of skilled wages $\log(w_{i,t}^s)$ on $\log(E_{i,t})$ keeping the same controls as in Equation (1). Surprisingly, the estimated elasticity of skilled wage is positive and statistically significant over the aggregate sample and over the SGRY regime. What explains these counter-intuitive results?

One explanation can be that the initial rise in unskilled wages due to SGRY exerted an upward pressure on semi-skilled wages as well. Such positive spill-over effects are plausible since all such activities together represent the rural labor market as described by the Agricultural Wages of India. However, since the productivity gains due to such EGS (as discussed in section 3) are mostly limited to unskilled work, one should not expect a sustained increase in semi-skilled wages. This indeed is reflected by the insignificant elasticity of skilled wages estimated under the NREGA regime in column 2 of [Table A5.2](#). We reject the test of equality of coefficients over the two program regimes at 5% level. Our result of insignificant impact on skilled wages under the NREGA regime is supported by similar findings of [Berg et al. \(2015\)](#).

Table A5.1: **Effect on Field Wages for Men and Women**

Dependent variable:	$\log(w_{i,t}^m)$		$\log(w_{i,t}^f)$	
	(1)	(2)	(3)	(4)
$\log(E_{i,t})$	0.067** [0.026]		0.105*** [0.030]	
SGRY		0.101** [0.033]		0.131*** [0.037]
NREGA		0.063* [0.026]		0.102** [0.031]
All Controls	Yes	Yes	Yes	Yes
Observations	1499	1499	1495	1495
Adjusted R^2	0.915	0.915	0.884	0.884

$w_{i,t}^m$: male wages. $w_{i,t}^f$: female wages. All regressions are weighted by district-level population. The standard errors reported in square brackets are clustered at district level and are robust to heteroskedasticity. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A5.2: **Effect on Semi-skilled Wages (Men)**

Dependent variable:	$\log(\text{Skilled wage})$	
	(1)	(2)
$\log(E_{i,t})$	0.062* [0.027]	
SGRY		0.103** [0.034]
NREGA		0.051 [0.027]
All Controls	Yes	Yes
Observations	1689	1689
Adjusted R^2	0.917	0.917

All regressions are weighted by district-level population. The standard errors reported in square brackets are clustered at district level and are robust to heteroskedasticity.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Tables and Figures

Figure 1: National Employment Expenditure

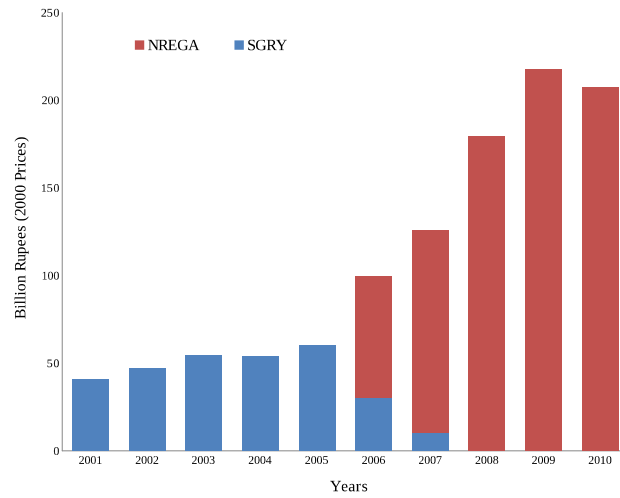


Figure 2: National Employment Generated

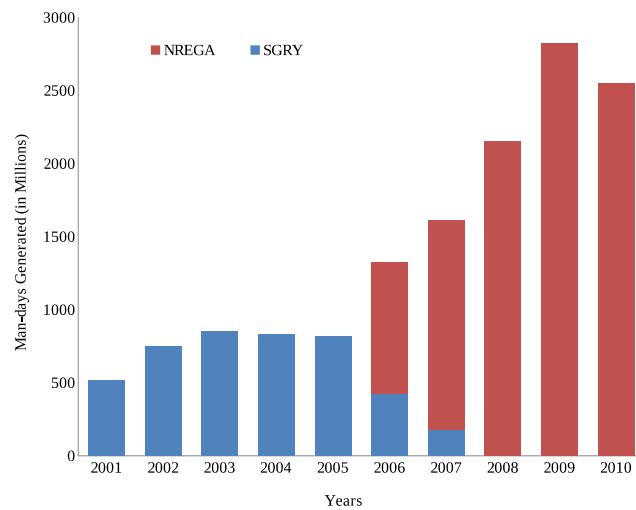
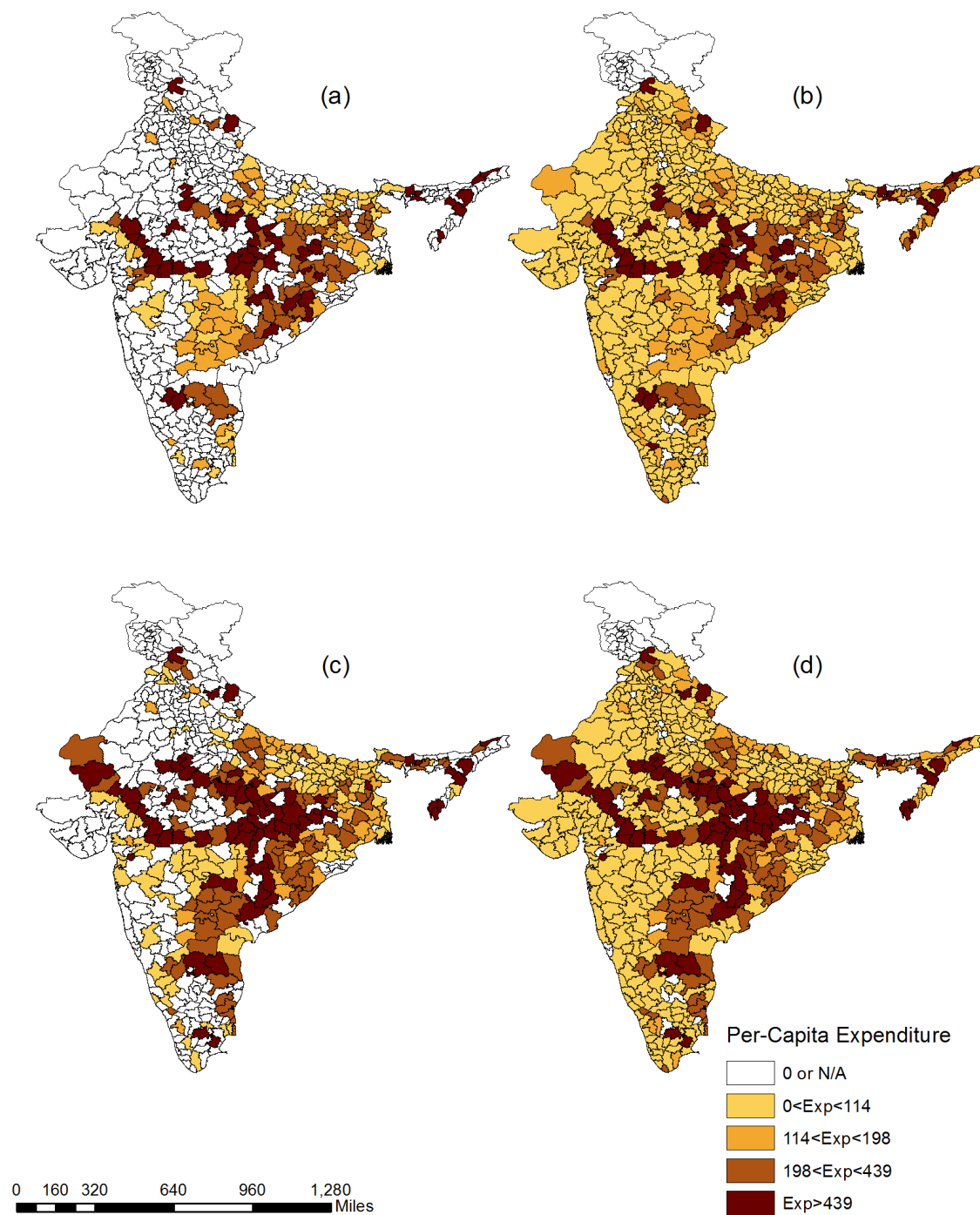


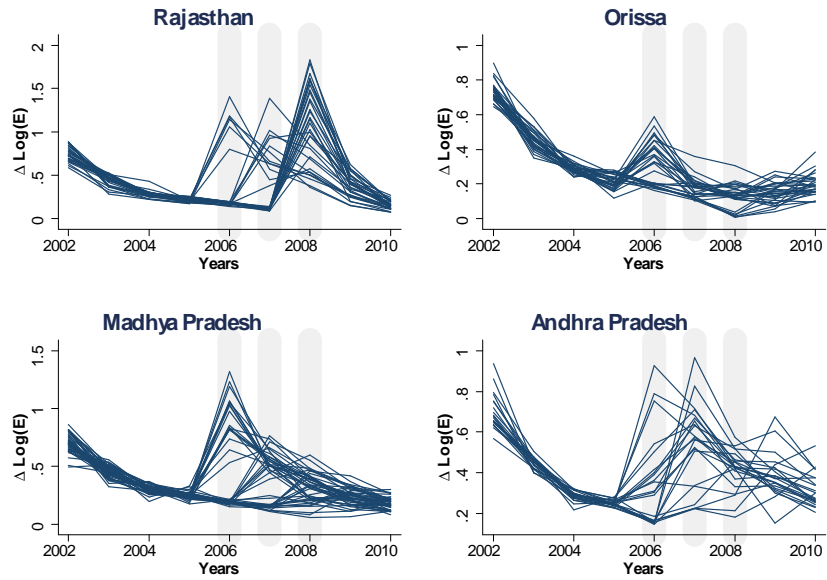
Figure 3: Expenditure under NREGA and SGRY during 2006 and 2007



(a): Expenditure under NREGA in 2006. (b): Expenditure under NREGA and SGRY in 2006.

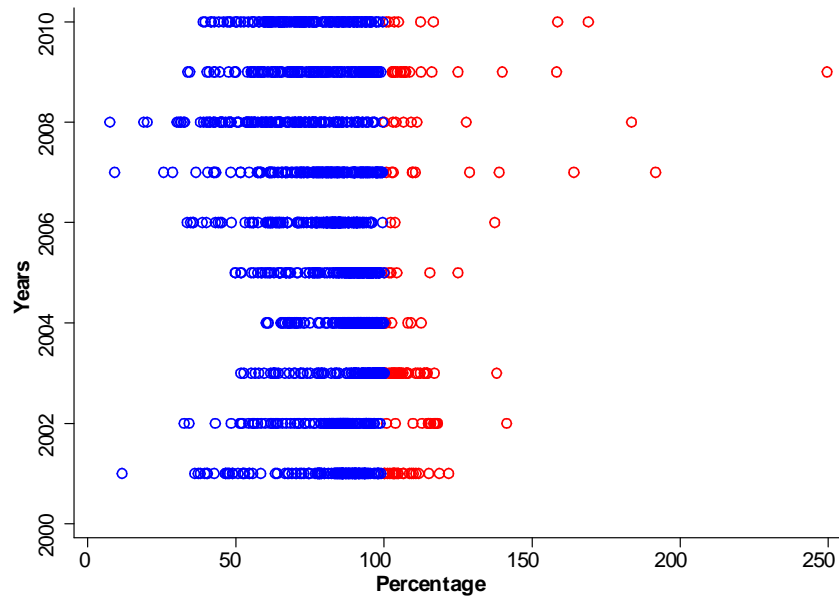
(c): Expenditure under NREGA in 2007. (d): Expenditure under NREGA and SGRY in 2007.

Figure 4: Annual Growth Rate of the Stock of EGS Expenditure



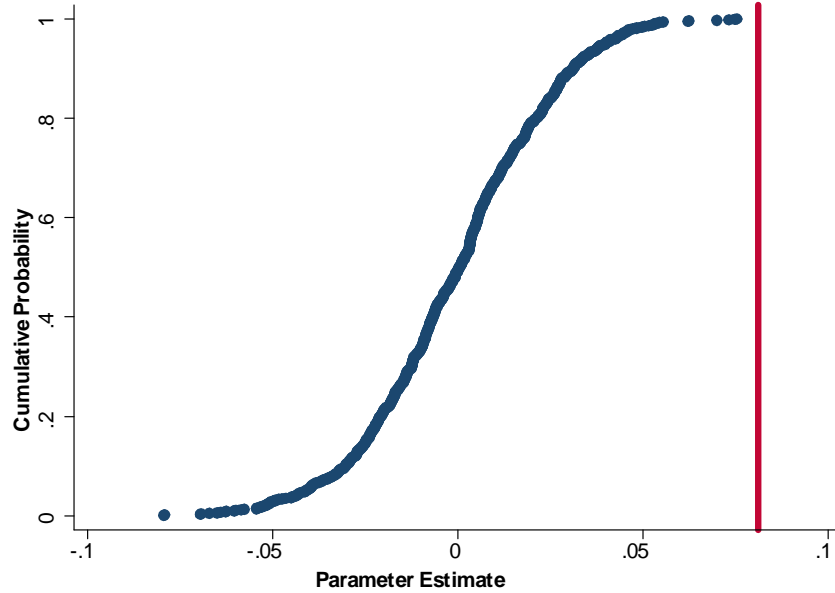
Note: The figure plots district-wise growth of the stock of EGS expenditure for four major states. The large spikes in growth correspond to phase I, II, and III of NREGA implementation during 2006-2008 (highlighted in grey).

Figure 5: Actual Expenditure as a Percent of Fund Availability



Note: The figure shows year-wise utilization of funds as a percent of funds made available. The observations marked in red show over-utilization while the observations in blue represent under-utilization.

Figure 6: **Placebo Tests using Randomization**



Note: The figure displays the CDF from one-thousand Monte Carlo simulations. The entire time-series of the stock of EGS expenditure is randomly reassigned across districts with the time-series structure preserved. In each simulation, a coefficient is estimated for the regression of field wages on the shuffled stock of EGS expenditure variable, keeping all other controls of Equation (1). All regressions are weighted by district population. The red vertical line shows the coefficient estimated without randomization. None of the randomized regressions produce a point estimate equal to or larger than the non-randomized estimate.

Table 1: **Comparison of SGRY and NREGA**

Description	SGRY	NREGA
Centrally Sponsored Scheme	Yes	Yes
Districts covered	All	All
Objectives:		
a) Wage Employment	Yes	Yes
b) Infrastructure Creation	Yes	Yes
c) Food Security	Yes	No
Centre:State Cost Ratio	75:25	90:10
Female participation target	33%	33%
Restrictions on public works:		
a) Ban on contractors	Yes	Yes
b) Ban on heavy machinery	Yes	Yes
c) Wage:Capital Cost Ratio	60:40	60:40
Implementing authority	Panchayati Institutions	Panchayati Institutions

Table 2: Productive Public Works under SGRY and NREGA

States	Number of Works in SGRY (in thousands)			Number of Works in NREGA (in thousands)		
	Initiated	Completed	% Completed	Initiated	Completed	% Completed
AP	87.22	70.68	81.04	738.09	375.77	50.91
AS	86.13	64.01	74.32	20.08	8.99	44.77
BR	91.24	56	61.37	124.48	57.56	46.24
GJ	52.57	44.96	85.52	91.73	71.82	78.29
HP	17.98	13.37	74.32	37.48	20.16	53.8
HR	28.09	27.11	96.49	6.28	3.57	56.78
KA	130.57	93.21	71.39	220.72	56.84	25.75
KL	26.18	12.11	46.24	73.2	48.42	66.15
MH	112.31	73.99	65.88	25.28	10.24	40.51
MP	138.5	118.16	85.31	457.59	194.63	42.53
OR	70.84	60.47	85.36	137.07	26.69	19.48
PB	21.81	18.39	84.35	7.12	3.16	44.31
RJ	52.32	44.02	84.14	144.77	54.37	37.56
TN	97.51	88.4	90.66	37.52	15.1	40.25
UP	266.26	212.85	79.94	368.5	231.12	62.72
WB	139.43	97.23	69.74	140.20	81.04	57.8
India	1600.65	1235.67	77.2	3028.43	1458.13	48.15

The table reports the average number of works taken up and completed under SGRY and NREGA for each of the 16 major states of India. All India data includes data on all 27 states (excluding Goa). Works include public projects undertaken under the employment guarantee schemes. These include projects on 1) Rural Connectivity, 2) Flood Control and Protection, 3) Water Conservation and Water Harvesting, 4) Drought Proofing, 5) Micro Irrigation Works, and 6) Land Development among others. Under NREGA, district-wise data is provided for works on each of these sub-categories. For SGRY, only state-wise data on aggregate works is available.

Table 3: **Impact on Wages using Stock of EGS Expenditure**

Dependent variable:	$\log(w_{i,t})$				
	(1)	(2)	(3)	(4)	(5)
$\log(E_{i,t})$	-0.070*** [0.019]	0.061*** [0.010]	0.085** [0.030]	0.079** [0.025]	0.081** [0.025]
I_{NREGA}	0.200*** [0.028]	0.038* [0.016]	-0.035 [0.021]	-0.037 [0.020]	-0.038 [0.020]
District Effect	No	Yes	Yes	Yes	Yes
Year Effects	No	No	Yes	Yes	Yes
Trend Effects	No	No	No	Yes	Yes
Other Controls	No	No	No	No	Yes
Observations	1493	1493	1493	1493	1493
Adjusted R^2	0.037	0.829	0.851	0.912	0.912

Data is annual from 2001 to 2010 at district level. The dependent variable in all the regressions is the log of real field wages (in 2001 prices). $E_{i,t}$ is the stock of EGS expenditure. For a district, I_{NREGA} takes the value zero (one) before (after) the implementation of NREGA. Column (1) reports regression with $\log(E_{i,t})$ and intercept only. Column (2) adds I_{NREGA} and districts fixed effects. Columns (3), (4), and (5) progressively add year effects, time trends, and other controls. Other controls include state elections-year indicator and density of scheduled caste and scheduled tribe population in a district. All regressions are weighted by district-level population. The standard errors reported in square brackets are clustered at district level and are robust to heteroskedasticity.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4: **Determinants of Fund Availability**

	(1)	(2)	(3)
Dependent variable:	$\log(\text{Releases}_{i,t})$	$\log(\text{Availability}_{i,t})$	$\log(\text{Availability}_{i,t})$
Utilization Ratio $_{i,t-1}$	0.924*** [0.175]	0.417*** [0.086]	0.415*** [0.086]
$\log(\text{Rain}_{i,t})$			0.068 [0.064]
District Effect	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes
Trend Effects	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes
Observations	1052	1359	1359
Adjusted R^2	0.768	0.850	0.850

The dependent variable in column 1 is the log of funds released at the start of the fiscal year. The dependent variable in columns 2 and 3 is the log of the funds made available at the start of the fiscal year. $\text{Rain}_{i,t}$ refers to the average rainfall (in millimeters) during the rainy season. All regressions are weighted by district-level population. The standard errors reported in square brackets are clustered at district level and are robust to heteroskedasticity.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5: **Comparing OLS and 2SLS**

	OLS	2SLS		2SLS	
	(1)	First Stage	Second Stage	First Stage	Second Stage
		(2)	(3)	(4)	(5)
$\log(E_{i,t})$	0.081** [0.025]		0.067** [0.025]		0.082* [0.035]
$\log(E_{i,t}^a)$		0.971*** [0.026]			
$\log(e_{i,t}^a)$				0.269*** [0.019]	
District Effects	Yes	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes	Yes
Trend Effects	Yes	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes	Yes
Observations	1493	1510	1493	1510	1493
F-stat Instrument > 20		Yes		Yes	
Endog. Test (p-value)			0.329		0.975

The dependent variable in columns (1), (3), and (5) is $\log(w_{i,t})$. The dependent variable in columns (2) and (4) is $\log(E_{i,t})$. The first set of 2SLS results use the stock of funds available: $\log(E_{i,t}^a)$ as instrument, while the second set of 2SLS results use contemporaneous availability of funds: $e_{i,t}^a$ as the instrument. The endogeneity test is based on the difference of two Sargan-Hansen statistics with the null hypothesis that the suspected endogenous regressor can actually be treated as exogenous. All regressions are weighted by district-level population. The standard errors reported in square brackets are clustered at district level and are robust to heteroskedasticity.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 6: **Effect over different program regimes**

Dependent variable:	$\log(w_{i,t})$	
	(1)	(2)
$\log(E_{i,t})$	0.081** [0.025]	
I_{NREGA}	-0.038 [0.020]	-0.035 [0.021]
SGRY		0.111*** [0.032]
NREGA		0.077** [0.026]
All Controls	Yes	Yes
Observations	1493	1493
Adjusted R^2	0.912	0.912

The table compares the elasticity of wage obtained from the aggregate sample (column 1) to the case where separate elasticities are estimated over the two program regimes. We interact I_{NREGA} with $\log(E_{i,t})$ to report separate elasticities under SGRY and NREGA regimes. All regressions are weighted by district-level population. The standard errors reported in square brackets are clustered at district level and are robust to heteroskedasticity.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 7: **Stability of Estimates**

Dependent variable:	$\log(w_{i,t})$	
	(1)	(2)
More Ration $\times \log(E_{i,t})$	0.087** [0.033]	
Less Ration $\times \log(E_{i,t})$	0.076* [0.029]	
Low Exp $\times \log(E_{i,t})$		0.087** [0.031]
High Exp $\times \log(E_{i,t})$		0.072* [0.034]
All Controls	Yes	Yes
Observations	1493	1493
Adjusted R^2	0.912	0.912

The first column compares wage elasticities when we interact $\log(E_{i,t})$ with indicator variable Ration. Column 2 reports separate elasticities for the sample of districts below and above the median value of average annual expenditure in a district. All regressions are weighted by district-level population. The standard errors reported in square brackets are clustered at district level and are robust to heteroskedasticity.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$