Differing from Difference-in-Differences: Evidence from Employment Programs in India

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Using evidence from the largest public works programs in India, this paper shows how difference-in-differences may fail to identify the impact of a policy intervention if (i) the existence of an older, similar program blurs the distinction between treatment and control groups, and (ii) if the policy has weak "spot effects", i.e. the outcome variable – the private sector wage – does not respond to contemporaneous fluctuations in public employment. On the other hand, the build-up of program capital – "stock effects" – are shown to better explain the impact of public works on private wages. A falsification test supports the hypothesis that stock effects are the dominant underlying mechanism through which employment guarantees affect the private labor market.

In 2006 India implemented the largest public works program in the world: The National Rural Employment Guarantee Act (NREGA). NREGA guarantees one hundred days of employment per year to every rural household whose adult members volunteer to do unskilled manual work. The program beneficiaries receive state-defined minimum wages and are expected to work in public works that are aimed at generating and improving village infrastructure. This allows NREGA to be a self-targeting anti-poverty policy where the government, as an employer of last resort, provides income insurance to the rural poor and in the process also creates productive public infrastructure. During 2009-10, NREGA generated around 2.6 billion work-days and employed around 55 million households. The total expenditure under NREGA amounted to around 0.6 percent of the GDP (nearly 5 percent of the agricultural output) during the same year.

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A program of this size and scope naturally raises many questions for research and policy. A key question, however, is to understand the general equilibrium labor market effects of such large-scale policy interventions. More specifically, it is important to understand the impact of such employment guarantee schemes (hereafter EGS) on short-term wages for comparable manual labor work in the private sector. The question is obviously highly relevant since wage income from casual labor provides an important source of income for the poor (Banerjee and Duflo, 2007). Furthermore, recent studies have found that increases in private wages due to NREGA represent a substantial proportion of the welfare gain for the program beneficiaries as well as non-beneficiaries.[1]

A recent and growing literature on NREGA has attempted to identify the general equilibrium labor market effects of the program. However, no clear consensus seems to have emerged since studies have often reported contrasting results. While Azam (2012) and Imbert and Papp (2015) report real private wages to be higher by 4-5 percent in districts that received NREGA as a pilot in 2006 or 2007 relative to the districts that received the program later in 2008, Berg et al. (2013) and Zimmermann (2012) do not find any statistically significant impact of NREGA's implementation on real private wages.[2] While the difference in results can partly be attributed to different econometric methodologies and data used, it is important to reconcile what explains such contradictory findings especially when much of the literature identifies NREGA's impact using difference-in-difference estimation; i.e. by comparing the evolution of wages over time for districts that received NREGA early in 2006 or 2007 to those that received the program later in 2008.

This paper discusses how difference-in-difference estimation may fail to capture the labor market effects of large public workfare programs when two key assumptions of (i) the existence of distinct treatment-control groups, and (ii) contemporaneous response of private wages to employment guarantees do not hold. Related to the first assumption, the paper starts by documenting the existence of NREGA's predecessor: Sampoorna Grameen Rozgar

[1] Muralidharan, Niehaus and Sukhtankar (2016) note that for "NREGS beneficiaries, increases in program income accounted for only 10% of the increases in total income, with the remaining 90% attributable to increases in private sector earnings". Similarly, Imbert and Papp (2015) find that increases in private sector wages represent "a third of the total welfare gain for the poor".

[2] While Berg et al. (2013) do not find any impact of NREGA on wages using standard difference-in-difference model, they find wages to have increased when they redefine treatment as the number of months a district was exposed to the program.

Yojana (hereafter SGRY) which was implemented in all the districts of the country in 2001. We find that while SGRY ceased to exist in the "treated" districts which received NREGA in 2006 and 2007, the older program continued to be in operation exclusively in the "non-NREGA" or "control" districts which received NREGA only from 2008. This is an important revelation as it blurs the distinction between treatment and control districts based on the phase-wise implementation of NREGA. The relevance of this finding is further underscored by the facts that i) the two programs are strikingly similar in terms of their functionality, scope, and objectives and ii) nearly 14 percent of the districts covered under NREGA during 2006 and 2007 spent less than the average per capita program expenditure incurred under SGRY in the control districts during both these years.

Categorizing the phase-wise implementation of NREGA as a binary treatment implicitly ignores the continued existence of a similar older program which can confound the true impact of the EGS. A difference-in-difference estimation that identifies treatment and control districts using an indicator variable may underestimate the impact of the EGS on wages if the actual gap in program provision between early- and late-phase districts is lower than what is assumed. On the other hand, if during NREGA's phase-wise implementation the provision of public employment declined (relative to the previous years) in the "control" districts that were still under the older program, then difference-in-differences will underestimate the gap of program provision between treatment and control districts and hence overestimate the program impact.

The second assumption relates to the ability of the empirical model to identify the policy impact depending on the underlying mechanism through which public works affect private labor markets. Based on how responsive private wages are to employment guarantees, the different underlying mechanisms proposed in the literature can be classified as either "spot effects" or "stock effects" of the program. Increase in private wages due to i) a contraction of private labor supply (Ravallion, 1990 and Imbert and Papp, 2015); ii) an improvement in the bargaining power of workers (see Drèze and Sen, 1991 and Basu, Chau and Kanbur, 2009); or iii) the provision of a de facto wage floor are all examples of spot market effects of employment guarantees. While the structural mechanism through which public works increase private wages is different for each of the above channels, such spot market effects can be empirically measured through difference-in-difference estimation where the very existence of an employment guarantee (or lack thereof) can affect wages contemporaneously.[3]

On the other hand, it is possible that the level of wages is better explained by the stock of program provision over time or the "program capital". One example of program capital can be the build-up of physical infrastructure created under the public works which has long been acknowledged to increase worker productivity and hence wages.[4] Alternatively, program capital may also refer to the build-up of state capacity to better implement a program. Irrespective of the actual underlying mechanism, if differences in program capital better explain the variability in wage rates across districts, then difference-in-difference estimation may again fail to accurately reflect wage increases due to the program.

This is so since a comparison between treated and control districts, especially over short time intervals, may not adequately capture the stock effect as discussed above. One of the key differences between studies that employ difference-in-difference estimation and report large (Azam, 2012, Imbert and Papp, 2015) versus zero wage impact (Berg et al., 2013) of NREGA is indeed the time period between which the pre- and post-intervention wage rates are compared between treatment and control districts.

We first begin by estimating a standard difference-in-difference model using annual agricultural wage rate data from Agricultural Wages of India (AWI) for 2001-2010. Here, the program impact is captured using the indicator variable that distinguishes treatment and control groups based on NREGA's phase-wise implementation, ignoring the continued existence of the older program in the control districts. Under this specification, we do not find any impact of NREGA on wages. Consistent with our discussion above, this reduced form empirical finding can be indicative of a substantial downward bias in the difference-in-difference estimates when the level of treatment between the treatment and control districts is overestimated. In addition, this may also indicate weak spot effects of the employment guarantees on the private labor market.

[3] Basu, Chau and Kanbur (2009) for example discuss how under imperfect competition, better bargaining power of workers and efficiency gains can increase private wages even without hiring any workers in the employment guarantee program.

[4] Binswanger, Khandker and Rosenzweig (1993) and Fan, Hazell and Thorat (2000) highlight the strong positive relationship between public infrastructure investment and agricultural output in India. Drèze (1990) discusses the role of Maharashtra's employment guarantee program in increasing agricultural productivity in the long run. More recently, Aggarwal, Gupta and Kumar (2012) discuss the construction of wells under NREGA as having productivity enhancing effects in the agriculture sector among other positive spillover effects. Basu (2013) presents a theoretical model discussing labor market responses to a productive EGS.

To address the above concerns, we redefine treatment as the amount of spending in a district under the two employment programs. We use a novel dataset of annual program expenditure for every district in the country over a ten-year period 2001-2010.[5] To the best of our knowledge, this is the most exhaustive and disaggregated expenditure data used in any comparable analysis on public works in a developing country. Using program expenditure data allows an alternative identification to the indicator variable approach where we instead exploit the substantial within and across district variation in program expenditure to identify the impact of employment guarantees on private wages. Importantly, the program expenditure data allows us to separately measure the spot and stock effects of public works.

Secondly, therefore, we estimate the spot market effects of employment guarantees by estimating the impact of contemporaneous program expenditure on agricultural wages. A key challenge in using expenditure data is that program spending can be endogenous to private wages. If the level of program expenditure is sensitive to local labor market conditions and district characteristics like the level of poverty, agricultural productivity, and other demographic features like proneness to droughts or floods, then this will confound the true impact of employment guarantees on wages. Our baseline identification is therefore achieved after partialling out district and year fixed effects together with other important controls and district-specific trends. We follow-up our fixed effects with trends estimation by instrumenting actual expenditure with funds that are made available at the district-level at the start of each fiscal year.

Our choice of instrument stems from the fund allocation process from the central government to the district level. We find that financial accounts of the programs we study roll over from one financial year to the next. This allows actual program expenditure in a district to be different from the funds made available in a given year. Instrumenting actual expenditure with funds made available hence allows us to check for any potentially endogenous district-year fluctuations that represent under- or over-utilization of funds. The results indicate positive but small spot effects of employment guarantees on wages. We find contemporaneous program expenditure to explain wage increases of around 0.3 percent per annum. Hence, the zero impact in the difference-in-differences model can be explained by weak spot effects together with a possible downward bias in the results due to misspecified treatment-control groups.

[5]The expenditure data for SGRY is between 2001 and 2007 and for NREGA is between 2006 and 2010. The two programs never co-existed together in a district.

Thirdly, we check if private wages are more responsive to the stock of program capital rather than the contemporaneous availability of employment guarantee. Since part of the expenditure under both the programs represents an investment to develop and maintain public infrastructure, we proxy program capital with the stock of program expenditure. To motivate our empirical specification, we present a model of asset accumulation that suggests wages to be a non-linear (concave) function of the stock of program expenditure. The model draws heavily from Basu, Chau and Kanbur (2009) wherein we allow labor productivity to be increasing in the capital generated under the public works in a multi-period dynamic framework. Our results in the stock specification, both with and without instrumenting actual expenditure with fund availability, are roughly an order of magnitude higher than the corresponding specifications that use contemporaneous program expenditure. We find the stock effect to explain wage increases between 2.2-2.8 percent per annum.

Finally, we conduct a falsification test to show that if wages increase only due to the spot effects of public works, then contrary to our empirical findings, the stock of program expenditure should be irrelevant in explaining private wages. We begin with Basu, Chau and Kanbur (2009) model of employment guarantees where the introduction of an EGS increases private wages by affecting the spot market of labor by i) eroding the market power of employers (contestability effects); and ii) improving the bargaining power of workers (reservation wage effect) in a monopsonistic or oligopsonistic labor market structure. We construct an artificially simulated dataset based on the model and find large and positive spot effects of an EGS on private wages as expected. Consistent with the model, we also find the magnitude of the spot effect to decrease as we move from monopsony to a more competitive market structure.[6] However, using the same artificially generated data, we do not find the stock of program expenditure to be relevant in explaining any increase in wages. Therefore, the falsification test supports the hypothesis that stock effects are likely to be the dominant underlying mechanism through which public works affect the private labor market.

The rest of the paper is structured as follows. The following section discusses and compares SGRY and NREGA in detail. Data is discussed in section II while results are reported in section III. The falsification test together with other robustness results are discussed in section IV. We conclude in section V.

[6] In a limiting case of a perfectly competitive labor market, there is no increase in wages due to an EGS since there is no distortion to begin with.

I. 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.

A. SGRY

Public works have been a tool to combat extreme poverty by the central government in India at least since the introduction of 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. However, employment schemes before 2000 were 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.[7]

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.[8] Like NREGA, SGRY envisaged generation of wage employment and creation of rural infrastructure by the provision of labor-intensive public projects. The program cost was divided between the central and state government in the ratio 75:25.

B. 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.

[7]See Bahal (2017) for a discussion on key rural welfare programs in India since 1980.

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

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 for judging the *backwardness* of a district were 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.

C. SGRY During NREGA Implementation

This section highlights the similarities between the two programs and also discusses the continued existence of SGRY during the phase-wise implementation of NREGA. 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 trend in national expenditure. Figure 1 and Figure 2 highlight that although at a relatively smaller scale, a significant amount of expenditure and employment generation did occur under the program before NREGA. It is important to note that the 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 some of the districts. Rather, SGRY was exclusively present only in those districts that were not covered by NREGA until 2008.

This point is clearly highlighted when we compare district-wise expenditure in 2006 in Figures 3(a) and 3(b). Figure 3(a) shows the rollout of

[9]Given 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 due to the substantial political bargaining involved between the central and state governments.

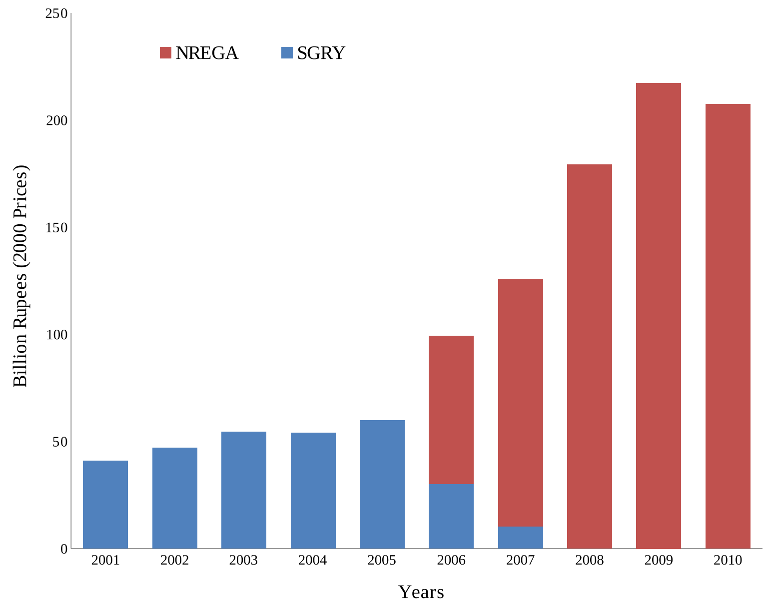

FIGURE 1. NATIONAL EMPLOYMENT EXPENDITURE

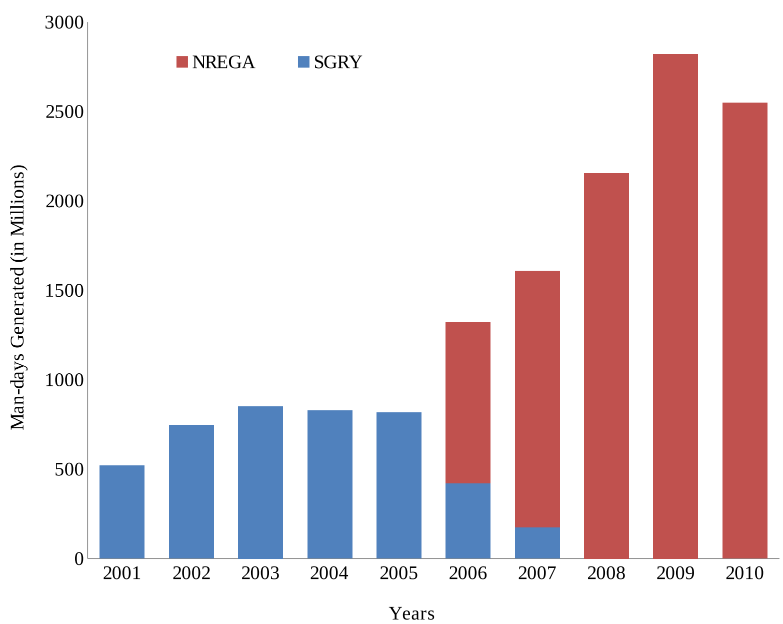

FIGURE 2. NATIONAL EMPLOYMENT GENERATED

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 3(b), shows the *actual* employment expenditure that occurred in the year 2006 which includes the expenditure under SGRY. Similarly, Figures 3(c) and 3(d) show that even during the phase-II implementation of NREGA in 2007, SGRY was operational in the non-NREGA districts. Hence, a comparison of Figure 3(a) and 3(c) with Figure 3(b) and 3(d) respectively highlights that SGRY continued to be in operation in the non-NREGA districts during the phase-wise implementation of NREGA in 2006 and 2007.

The relevance of this finding is underscored by the fact that nearly 14 percent of the districts covered under NREGA during 2006 and 2007 spent less than the average per capita expenditure incurred under SGRY in the "non-NREGA" districts during both these years. 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 district with no coverage of employment guarantee policy in 2006 and 2007 was a result of the implementation design of NREGA where the late-phase districts were in fact supposed to have SGRY operational in them (until NREGA finally enveloped them in phase II or III). The Act itself notes: "... until any such Scheme [NREGA] is notified by the state government ... Sampoorna Grameen Rozgar Yojana (SGRY) ... shall be deemed to be the action plan for the Scheme [NREGA] for the purposes of this Act." p.3, NREGA (2005). The provision of employment guarantee, therefore, seems to be a continuous process for the period of our study.

Further, the two programs were strikingly similar to each other in their scope and objectives. As Table 1 shows, both programs were implemented in all of the districts of the country with their primary objectives being the provision of wage employment and infrastructure creation. Both schemes involved similar types of labor-intensive public works and were largely funded by the central government. Finally, both programs encouraged female participation in the program by keeping a minimum female participation target of 33 percent. NREGA, therefore, can be best understood as an intensification of an already existing employment guarantee policy of the government. A valid concern, however, can be that NREGA "guaranteed" employment and was therefore structurally different and not comparable to SGRY where the level of employment provision was dictated by the program budget. However, such a theoretical distinction has not precipitated into ground reality.

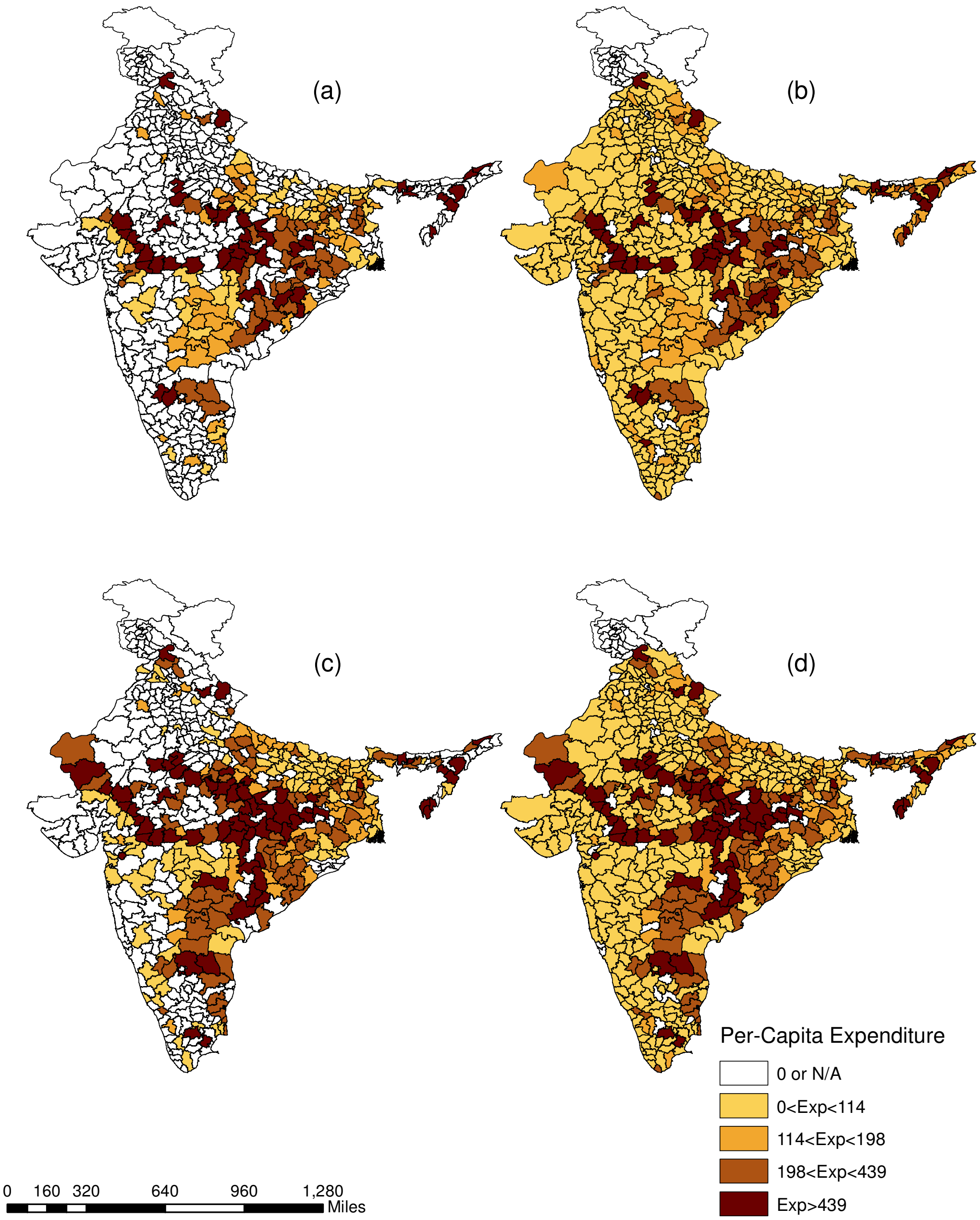

Figure 3. Expenditure under NREGA and SGRY during 2006 and 2007

Note: (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.

Table 1— Comparison of SGRY and NREGA

Description	SGRY	NREGA
Centrally Sponsored Scheme	Yes	Yes
Centre:State Cost Ratio	75:25	90:10
Female participation target	33%	33%
Districts covered	All	All
Primary Objectives:		
a) Wage Employment	Yes	Yes
b) Infrastructure Creation	Yes	Yes
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	Village Panchayat	Village Panchayat

As Dutta et al. (2012) show, rationing of the demand for work under NREGA is substantial with a country average rationing rate of around 44 percent with some states rationing more than 80 percent of the demand for work under the program. They 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. Himanshu and Sharan (2014) highlight the case of unmet demand in NREGA due to a supply-driven approach and the lack of bureaucratic or administrative capacity. This further reinstates that NREGA can be viewed as a more intense (but not structurally different) scheme relative to SGRY.

II. Data

Data on program expenditure over the two employment schemes is collected from the Ministry of Rural Development (MoRD), Government of India. Although information on NREGA is publicly available (nrega.nic.in), data on the older program (SGRY) was collected from MoRD and Datanet (India) on request.[10] Data on agricultural wages is from the Agricultural Wages of India (AWI) series published by the Ministry of Agriculture, Government of India. 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.

A. 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, availability of funds, and actual expenditure. We use data on "actual expenditure" when referring to program expenditure in the empirical models below. 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. As noted earlier, the two programs never coexisted together in a district even during the phase-wise implementation of NREGA. Hence, we have ten years (2001-2010) of data on employment expenditure (and other related variables) for all the districts of the country. To the best of our knowledge, this is the most disaggregated and exhaustive data on program expenditure used in any comparable analysis.

B. 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 the district level. This provides us with a much more

[10] To 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.

continuous data in comparison with the National Sample Survey data sets which are conducted quinquennially.[11] 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.[12] 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 A discusses the 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 134 districts over 12 major states of India.

III. Empirical Model and Results

A. Difference-in-Differences (DD)

We begin by estimating the general equilibrium effects of employment guarantees on private wages by estimating the standard DD model that exploits the phase-wise implementation design of NREGA but ignores the continued existence of the older program in the control districts. The treatment impact is captured by comparing agricultural wage rates in the treated districts that received NREGA early in 2006 or 2007 relative to the control districts that received NREGA from 2008. We estimate the difference-in-difference model illustrated in Equation 1 where $w_{i,t}$ is the field wage, 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 $I_{NREGA} : \gamma$ is the DD estimate that captures the impact of NREGA on wages. District and year fixed effects are controlled by α_i and θ_t respectively while $\xi_i t$ control for district-specific trends. The subscripts i and t denote district and year respectively. To account for serial correlation, all regressions report standard errors clustered at the district level that are robust to heteroskedasticity as well. Given the heterogeneity in the size of the districts in our sample, all regressions are weighted by district

[11] Recently, *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.

[12] See for e.g., Ravallion, Datt and Chaudhuri (1993), Özler, Dutt and Ravallion (1996), Himanshu (2005), and Berg et al. (2013).

population.[13]

$$\log(w_{i,t}) = \alpha_i + \theta_t + \xi_i t + \gamma I_{NREGA} + \upsilon_{i,t} \tag{1}$$

Table 2 reports the DD estimate γ obtained from different variations of Equation 1. Column 1 shows the estimated coefficient $\widehat{\gamma}$ with just an intercept as an additional control. The estimated coefficient indicates NREGA to have increased wage rates by around 7.2 percent. However, as is shown in column 2, the estimated impact of the program becomes statistically insignificant from zero once we add district and year fixed effects. To control for the possibility that NREGA's phase-wise implementation design may be correlated with differential growth paths of districts that may bias the results even after controlling for time-invariant district fixed effects, we further add district specific trends to our specification in column 3. Adding heterogeneous trends, however, has little impact on $\widehat{\gamma}$ which continues to be insignificant from zero.

Table 2—Impact on Wages using Simple Difference-in-Difference

	(1)	(2)	(3)
I_{NREGA}	0.072**	-0.023	-0.030
	[0.023]	[0.019]	[0.020]
District Effects	No	Yes	Yes
Year Effects	No	Yes	Yes
Trend Effects	No	No	Yes
Observations	1340	1340	1340

For a district, I_{NREGA} takes the value zero (one) before (after) the implementation of NREGA. All regressions are weighted by district-level population. The standard errors reported in square brackets are clustered at the district level and are robust to heteroskedasticity.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Hence, the difference-in-difference specification does not capture any impact of NREGA on private wages after partialling out year and district

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

average effects.[14] The difference-in-difference results may, therefore, reflect an underestimation of the true impact of the program on wages by overestimating the gap in program provision between treated and control districts. Alternatively, since the specification in Equation 1 mostly captures contemporaneous effects of the policy intervention – especially with annual frequency data – the difference-in-difference estimate may also be small if employment guarantees have weak spot effects on the labor market.

B. Spot Effects

To address the above concerns, we redefine treatment as the amount of real per capita spending under the two programs in any given district. Measuring the provision of employment guarantee as a continuous treatment helps avoid underestimation of program impact that may occur under the difference-in-difference specification. The program expenditure data further allows us to distinguish between the spot and stock effects of employment guarantees on private wages.

We first study the spot effects of employment guarantees by estimating the elasticity of agricultural wages w.r.t. contemporaneous program expenditure. We estimate various specifications of:

$$\log(w_{i,t}) = \alpha_i + \theta_t + \xi_i t + \beta \log(e_{i,t}) + \phi \mathbf{X}_{i,t} + \upsilon_{i,t} \tag{2}$$

where $e_{i,t}$ is the contemporaneous program expenditure in district i and year t and $\phi\mathbf{X}_{i,t}$ is a vector of additional controls discussed below. All other variables have the same interpretation as in Equation 1. The elasticity of wage w.r.t. contemporaneous expenditure: β estimates the spot effect of employment guarantees on private wages.

An obvious challenge to identify program impact on wages is that program expenditure can be endogenous to local economic conditions, district characteristics, and hence wages. If, for example, program expenditure is responsive to time-invariant district effects like demographics, proneness to drought, and general agricultural productivity, then the elasticity estimate may suffer from omitted variable bias. To illustrate this point, column (1) of Table 3 reports the estimate of elasticity obtained from estimating Equation 2 with $\log(e_{i,t})$ and an intercept as the only regressors. As can be seen, $\widehat{\beta}$ is estimated to be around -0.05, significant at 95 percent confidence level.

[14]Our results from DD specification are very comparable to those of Berg et al (2013) who also report NREGA to increase wages by 7.3 percent in the model without district and year controls but find no program impact after adding the additional controls.

Intuitively, if the level of program expenditure is positively correlated to the backwardness of a district, then $\widehat{\beta}$ should be expected to be suffering from a large negative bias.

Results reported in column (2) indeed lend support to our hypothesis where the elasticity estimate increases to 0.1 (highly significantly different from zero) after controlling for district fixed effects. In column (3) we further add year fixed effects to our model. The inclusion of year fixed effects controls for any endogenous changes in the outlay of funds at the national level in response to aggregate shocks (like bad weather) that may affect agricultural wages on average. Including year fixed effects lower $\widehat{\beta}$ to 0.04 but the estimate remains statistically different from zero ($p < 0.001$). The overestimate of β in column (2), without year effects, may hence be indicative of a pro-cyclical employment policy.

In column (4), we add a vector of other important controls $\mathbf{X}_{i,t}$ which include an indicator for state election-year, the density of Scheduled Castes and Scheduled Tribes in a district, and the indicator I_{NREGA} which controls for a change in program regime. These variables are important to control for since political motivations at the time of state elections or during the implementation of NREGA, together with other district characteristics may influence the overall level of expenditure in a district.[15] The inclusion of these additional controls raises the estimate of $\log(e_{i,t})$ slightly to 0.05 which remains significant at 99.9 percent confidence level.

Finally, in column (5), we control for 134 district-specific trends. Controlling for differential trends is important to check for any potential bias that may result if growth in spending is correlated with economic progress at the district level. If the growth in program expenditure is higher for low-growth districts, then the elasticity estimate in column (4) will be an underestimate of the true β. Alternatively, the omission of heterogeneous trends may also overestimate β if, ceteris paribus, high-growth districts spend more due to better capacity to implement such programs as a result of better governance and infrastructure. However, adding heterogeneous trends may also confound the analysis if apart from alleviating endogeneity concerns, the treatment effect is also filtered out.

Column (5) reports $\widehat{\beta}$ to be just 0.02 after controlling for heterogeneous trends. The elasticity estimate is now different from zero only at 95 percent confidence level and is roughly 60 percent smaller than the estimate reported in column (4). Based on the average growth in per capita program expen-

[15]For robustness, we later check whether the elasticity estimate $\widehat{\beta}$ is different over the two program regimes.

Table 3— Impact on Wages using Flow of Program Expenditure

	(1)	(2)	(3)	(4)	(5)
$log(e_{i,t})$	-0.045*	0.097***	0.042***	0.048***	0.021*
	[0.021]	[0.013]	[0.012]	[0.013]	[0.010]
District Effects	No	Yes	Yes	Yes	Yes
Year Effects	No	No	Yes	Yes	Yes
Other Controls	No	No	No	Yes	Yes
Trend Effects	No	No	No	No	Yes
Observations	1340	1340	1340	1340	1340

The unit of observation is a district-year. The dependent variable in all the regressions is the log of real field wages. $log(e_{i,t})$ is the log of program expenditure. While column (1) has $log(e_{i,t})$ and an intercept as the only regressors, columns (2), (3), (4), and (5) progressively control for district fixed effects, year fixed effects, other controls, and heterogeneous time trends. Other controls include I_{NREGA}, 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 the district level and are robust to heteroskedasticity.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

diture of 16.1 percent per annum, the specification in column (4) suggests employment guarantees to have increased private wages by $16.1 \times 0.48 = 0.77$ percent per annum. Alternatively, adding district-specific trends in column (5) implies a wage growth of only 0.34 percent per annum.

‘Availability’ of an Instrument

Although our empirical specification in Equation 2 deals with the problem of endogeneity emanating from both time-invariant heterogeneous effects and district-specific trends, it may still be susceptible to district-year shocks that may influence field wages and program expenditure simultaneously. For example, local negative weather shocks may adversely affect the agriculture sector (and hence wages) while resulting in higher than expected program spending due to increased demand for public works. Drèze (1990), for example, discusses high take-up of public employment by laborers under Maharashtra’s employment guarantee 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 accounting process of the programs to find unexpected increases or decreases in spending that may possibly represent endogenous fluctuations. Below we discuss the accounting process of the programs in detail and motivate the 'funds made available' – from the central government to the districts – as an instrument for actual program expenditure which allows us to check for potentially endogenous under- or over-utilization of funds.

Accounting Process

For both the programs we study, funds from the central government are released to the states which are then released to the districts. Since both the programs are in principle demand driven, actual expenditure (at district level) can be higher or lower relative to the total funds made available in any given year. If actual expenditure exceeds the total funds made available, then this is shown as a negative opening balance in the following year – hence the financial accounts roll over from one financial year to the next. For a district i in year t, the opening balance together with the funds released from the state determine the availability of funds. Hence: *availability of funds*$_{i,t}$ = *funds released*$_{i,t}$ + *opening balance*$_{i,t}$.

Potentially Endogenous Fluctuations

We first define utilization ratio $= 100 * e_{i,t}/e^{a}_{i,j}$, where $e^{a}_{i,j}$ is the availability of funds for district i in year t. Figure 4 highlights year-wise utilization of funds as a percentage of funds made available for the 134 districts considered in our study. The observations marked in red show over-utilization while the observations in blue represent under-utilization.

If deviations in actual expenditure relative to fund availability represent demand shocks – as is theorized by the employment guarantee act – then this may confound the impact of employment guarantees on wages as calculated in Table 3. Specifically, if over-utilization of funds – which represent 9 percent of the total observations – correspond to weather or any other local negative shocks, then our elasticity estimates in Table 3 may be downward biased since we do not control for such district-year events in Equation 2.

Similarly, if under-utilization of available funds reflects poor demand for employment under the program when private labor market conditions are good, then again the estimates reported in Table 3 will be downward biased. Given the flexibility accorded in the system where actual expenditure can

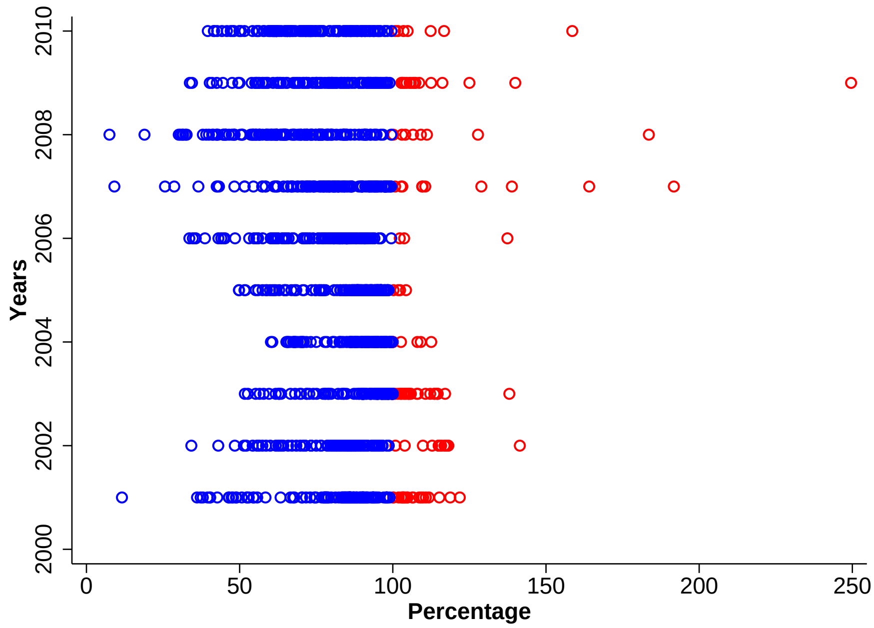

Figure 4. Actual Expenditure as a Percent of Fund Availability

Note: The figure shows the 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.

exceed or be less than the funds made available – possibly reflecting changes in the demand for the program – we use fund availability $e^a_{i,j}$ as an instrument for actual expenditure $e_{i,t}$ to check the effect of such *potentially endogenous* observations on the elasticity estimates reported in Table 3.[16]

It is straightforward to establish the relevance of fund availability as an instrument, given that the level of expenditure in a district is largely dictated by the total funds made available in that fiscal year. We further establish this point later using the first stage regression results during the two-stage least square (2SLS) analysis. For instrument validity, we also need to establish that fund availability affect private wages only through actual expenditure and not through any other channel. While it is intuitive to understand that available funds themselves cannot affect the private labor

[16]From 2012, funds are directly transferred from the state to the worker's account through the electronic fund management system (eFMS). This implies that the more recent data has little scope for an accounting design where fund availability and actual expenditure are different. However, this has no bearing on our results since our data is up to the financial year 2010-11.

market – via spot or stock effects – unless the funds are actually spent on public works or worker's salaries, there can be other factors that influence both fund availability and wages in a district. Below we discuss the determinants of fund availability and find that the fluctuations in our instrument are largely pre-determined and independent of future district-year shocks that are likely to be anticipated.

What Determines Fund Availability?

At the start of each fiscal year, the central government makes funds available at the district-level that are channeled through the state government. Fund availability is based on the labor budget that is submitted by the district authorities to the state government in the previous year. The labor budget encompasses the material and wage costs of the public works that are expected to be undertaken in the following year. Naturally, the size and population of a district, along with other factors like agricultural productivity and the level of rural poverty dictate the level of fund availability. We already control for such factors in Equation 2 through the district fixed effects that include both time-invariant and district-specific trends. Apart from the labor budgets submitted by the districts, fund availability is obviously sensitive to the overall funds that are earmarked for such welfare schemes in the union budget set by the central government. The overall national budget for the program, in turn, can be influenced by a variety of political and economic factors. Specific to our case, all such aggregate factors are controlled by the year fixed effect in our model.

We further note that apart from the factors discussed above, the level of expenditure in the past is also an important determinant of fund availability in the current year. That is, ceteris paribus, fund availability in year t is more for districts with a higher rate of utilization in the year $t-1$. To check for this, we first show how the *funds released* in a year respond to the previous year's rate of fund utilization. As can be seen from column (1) of Table 4, a 1 percent increase in last year's utilization ratio increases the *funds released* by 0.92 percent in the current year. Given that part of the releases goes towards meeting any previous obligations that occur due to over-expenditure in the previous year, the positive response of releases to past utilization ratio is expected. Next, we regress *fund availability* on last year's utilization ratio in column (2) of Table 4. According to the estimate in column (2), a 1 percent increase in the utilization ratio in the previous year results in a 0.42 percent increase in the availability of funds in the current year. This change in the availability of funds is *after* settling any previous obligations (reflected in the opening balance) through appropriate releases. Comparing results from column (1) and (2) imply that the positive association between fund availability and past rate of utilization is not an accounting relation and indeed reflect pre-determined adjustments intended to reduce the gap between fund availability and expenditure needs.

Finally, we check if fund availability is sensitive to shocks to the agricultural labor market that are also likely to be anticipated. If true, this may threaten the validity of fund availability 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. We define rainy season (also known as monsoon in India) as the months of June, July, and August.[17] We use remote sensed rainfall data from the Tropical Rainfall Measuring Mission (TRMM) satellite (see Appendix A for details).

While fluctuations in rainfall are independent to private wages and program expenditure, such negative local weather shocks can cause significant disruptions to agricultural output and wages in India which may also result in higher program expenditure due to increased demand for work. Importantly, it is possible to anticipate such weather shocks since they are systematically monitored and forecasted. However, as column (3) of Table 4 shows, availability of funds is invariant to the amount of rainfall a district receives in a particular year during the rainy season. While both fund availability and rainfall belong to the same fiscal year, the rainfall variable represents future shocks since fund availability is determined before the fiscal year starts.[18]

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.[19] The lack of any motivation to efficiently predict the availability of funds, given the flexibility to under- or over-spend, supports our hypothesis that fluctuations in the availability of funds are independent of shocks to the agricultural labor market at the district level. Therefore, we use fund availability as an instrument for actual expenditure to check for potentially endogenous district-year observations as shown in Figure 4.

[17]Results are invariant to choosing other criteria of measuring rainfall.

[18]We use actual rainfall as a proxy for anticipated rainfall assuming that the forecast error is independent to actual rainfall.

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

Table 4— Determinants of Fund Availability

	(1)	(2)	(3)
Dependent variable:	$log(Rel_{i,t})$	$log(Avail_{i,t})$	$log(Avail_{i,t})$
Util. Ratio$_{i,t-1}$	0.981^{***}	0.403^{***}	0.395^{***}
	[0.171]	[0.086]	[0.087]
$Rain_{i,t}$			0.117
			[0.072]
District Effects	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes
Trend Effects	Yes	Yes	Yes
Observations	933	1206	1206

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. Util. Ratio$_{i,t-1}$ is expenditure taken as a percent of fund availability in the previous year. $Rain_{i,t}$ 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 the district-level and are robust to heteroskedasticity.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

OLS versus 2SLS

Table 5 compares the OLS estimates of Equation 2 with 2SLS regression results where we instrument actual expenditure $e_{i,j}$ with fund availability $e^a_{i,j}$. Equation 3 represents the first stage regression equation where all the variables have the same interpretation as before. We make the OLS-2SLS comparison of the last two specifications of Table 3 which only differ in the use of district-specific trends. For comparison, columns (1) and (2) of Table 5 show the OLS results reported columns (4) and (5) of Table 3 respectively. The first set of 2SLS results in Table 5 correspond to the case where the first and second stage regressions control for all the regressors in Equation 2, except for differential trends. The first stage regression of

$\log(e_{i,t})$ on $\log(e^a_{i,t})$ yields an estimate of 0.94 which is highly significant.

$$(3) \qquad \log(e_{i,t}) = \alpha_i + \theta_t + \xi_i t + \beta \log(e^A_{i,t}) + \gamma I_{NREGA} + \xi \mathbf{X}_{i,t} + \upsilon_{i,t}$$

Table 5— Comparing OLS and 2SLS

	OLS		2SLS		2SLS	
			First Stage	Second Stage	First Stage	Second Stage
$log(e_{i,t})$	0.048***	0.021*		0.072***		0.021*
	[0.013]	[0.010]		[0.015]		[0.010]
Availability			0.940***		0.981***	
			[0.039]		[0.063]	
District Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes
Trend Effects	No	Yes	No	No	Yes	Yes
F-stat IV> 20			Yes		Yes	
Sargan-Hansen				0.000		0.943
Observations	1340	1340	1340	1340	1340	1340

The dependent variable in OLS and second-stage 2SLS regression is $log(w_{i,t})$. The dependent variable in first-stage 2SLS regression is $log(e_{i,t})$. Sargan-Hansen gives the p-value of the endogeneity test 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 the district-level and are robust to heteroskedasticity.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The F-statistic for the instrument well exceeds the threshold of 20, and hence do not pose a threat of weak instruments problem. The coefficient of $\log(e_{i,t})$ in the second stage is estimated to be at 0.072 which is 50% greater than the corresponding OLS estimate of 0.048 in column 1. Based on the average growth in per capita expenditure, wage elasticity of 0.072 implies a wage growth of around 1.6 percent per annum. Given the difference in the 2SLS and OLS estimates of $\log(e_{i,t})$, the endogeneity test based on the difference of Sargan-Hansen test statistics rejects the hypothesis that the suspected endogenous regressor $\log(e_{i,t})$ can, in fact, be treated as exogenous.

On the other hand, the second set of 2SLS results which in addition to other controls include district-specific trends yield a 2SLS estimate which is the same as the corresponding OLS estimate in column (2). Consequently, the implied growth in wages is 0.34 percent per annum – same as the OLS case. Given that the OLS and 2SLS estimates are identical with differential trends, the endogeneity test in this case cannot reject the null that $\log(e_{i,t})$ is exogenous.

Hence, while the inclusion of district-specific trends alleviates endogeneity concerns by allowing inference independent of the parallel trends assumption, such specifications may fail to distinguish treatment effects from differential trends especially if treatment effects emerge only gradually (Angrist and Pischke 2014). This does not necessarily imply that there is no impact of the treatment but rather that public works do not result in sharp contemporaneous fluctuations in private wages, indicating weak spot effects on the labor market.

C. Stock Effects

We now study how program capital affects private wages in a district. Program capital can be understood as the stock of productive public infrastructure generated by public works programs. While there is concrete evidence that productive infrastructure projects – mostly irrigation works – were in fact undertaken under the two programs (see Appendix B for details), there is no explicit measure of the public infrastructure created. We, therefore, measure program capital as the stock of program expenditure $E_{i,t}$.

In Appendix B, we show that an increase in worker productivity and hence private wages due to asset accumulation suggests private wages to be empirically modeled as a concave function of the stock of program expenditure $E_{i,t}$. However, it is important to note that program capital may not be limited to the accumulation of physical assets and may encompass other administrative and institutional capital as well that may build-up over time and lead to sizeable stock effects on wages.

To measure the stock effect of employment guarantees on wages, we estimate Equation 4 where all variables have the same interpretation as before except the regressor $log(E_{i,t})$ – the stock of program expenditure – which is defined as the sum of the present useful component of past expenditures $\{e_{i,1}, \ldots, e_{i,t}\}$. Hence $E_{i,t} = \sum_{k=1}^{t} e_{i,k}\delta^{(t-j)}$ where the parameter δ is the geometric rate at which the program capital depreciates. To determine δ, we conduct a grid search between zero and one and choose the value of δ that minimizes the sum of squared residuals of Equation 4. This exercise yields a corner solution of $\delta = 1$ which implies no depreciation of the program capital. Appendix B discusses the grid search method in greater detail and tests the robustness of assuming non-zero depreciation rates. Table 6 reports the elasticity of wage w.r.t. the stock of program expenditure under various specifications of Equation 4.

$$\log(w_{i,t}) = \alpha_i + \theta_t + \xi_i t + \beta \log(E_{i,t}) + \gamma I_{NREGA} + \phi \mathbf{X}_{i,t} + \upsilon_{i,t} \tag{4}$$

Table 6— Impact on Wages using Stock of EGS Expenditure

	(1)	(2)	(3)	(4)	(5)
$log(E_{i,t})$	-0.013	0.075***	0.086*	0.088*	0.077**
	[0.015]	[0.010]	[0.034]	[0.034]	[0.027]
District Effects	No	Yes	Yes	Yes	Yes
Year Effects	No	No	Yes	Yes	Yes
Other Controls	No	No	No	Yes	Yes
Trend Effects	No	No	No	No	Yes
Observations	1340	1340	1340	1340	1340

The dependent variable in all the regressions is the log of real field wages (in 2001 prices). $E_{i,t}$ is the stock of program expenditure in district i, year t. 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 an intercept as the only regressors. Column (2) adds districts fixed effects. Columns (3), (4), and (5) progressively add year effects, other controls, and district-specific time trends. Other controls include I_{NREGA}, 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 the district level and are robust to heteroskedasticity.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Analogous to the spot effect case, the elasticity estimate suffers from a large negative bias in column (1) – with $\log(E_{i,t})$ and an intercept as the only regressors – which is corrected once we control for district-specific time-invariant fixed effects in column (2). Adding year fixed effects and other controls in columns (3) and (4) respectively further increase the elasticity estimate to around 0.9 which is significant at standard 95 percent confidence levels. Finally, since the regressor $log(E_{i,t})$ includes a trend by construction, we also control for differential trends in column (5). Fortunately, the inclusion of heterogeneous trends in column (5) does not change the results much with $\widehat{\beta}$ around 0.08 ($t > 2.8$). Hence, based on the elasticity estimate reported in column (5) – our preferred specification – and average annual growth of 35 percent for $E_{i,t}$ implies a wage growth of approximately 2.7 percent per annum. The stock effect of employment programs on private wages is therefore estimated to be approximately an order of magnitude greater than the spot effect reported in the corresponding OLS specification of column (5), Table 3.

OLS vs 2SLS

Analogous to the spot effects case, we check the validity of the OLS results by instrumenting actual expenditure with fund availability to check for potentially endogenous district-year fluctuations. Keeping the same set of controls as in our preferred specification (with differential trends), we use two alternative instruments for our suspected endogenous variable $\log(E_{i,t})$. First, we instrument $\log(E_{i,t})$ with the stock of funds made available $E^a_{i,t} = \sum_{j=1}^{t} e^a_{i,j}\delta^{t-j}$ where $e^a_{i,j}$ is as defined above. Equation 5 represents the first stage regression equation corresponding to this instrument:

$$\log(E_{i,t}) = \alpha_i + \theta_t + \xi_i t + \beta \log(E^A_{i,t}) + \gamma I_{NREGA} + \xi \mathbf{X}_{i,t} + \upsilon_{i,t} \tag{5}$$

Second, we use only the contemporaneous availability of funds $e^a_{i,j}$ to instrument for $\log(E_{i,t})$ with Equation 6 as the corresponding first stage regression equation:

$$\log(E_{i,t}) = \alpha_i + \theta_t + \xi_i t + \beta \log(e^a_{i,j}) + \gamma I_{NREGA} + \xi \mathbf{X}_{i,t} + \upsilon_{i,t} \tag{6}$$

For comparison, column 1 of Table 7 shows the OLS results of our preferred specification (column 5, Table 6). Columns 2 and 3 of Table 7 respectively report the first and second stage results while using the stock of funds made available: $\log(E^a_{i,t})$ as an instrument for $\log(E_{i,t})$.

As expected, there is a near one to one correspondence between the stock of program expenditure and the stock of funds made available (column 2).

Table 7— Comparing OLS and 2SLS

	OLS	2SLS		2SLS	
		First Stage	Second Stage	First Stage	Second Stage
$log(E_{i,t})$	0.077**		0.064*		0.079*
	[0.027]		[0.027]		[0.038]
$log(E^a_{i,t})$		0.978***			
		[0.028]			
$log(e^a_{i,t})$				0.262***	
				[0.019]	
All Controls	Yes	Yes	Yes	Yes	Yes
F-stat IV> 20			Yes		Yes
Sargan-Hansen			0.415		0.929
Observations	1340	1340	1340	1340	1340

The dependent variable in OLS and second-stage 2SLS regression is $log(w_{i,t})$ while $log(e_{i,t})$ is the dependent variable in the first-stage regressions. The first set of 2SLS results uses the stock of funds available: $log(E^a_{i,t})$ as instrument, while the second set of 2SLS results use the contemporaneous availability of funds: $e^a_{i,t}$ as the instrument. The Sargan-Hansen endogeneity test reports the p-value of the null 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 the district-level and are robust to heteroskedasticity.

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

The first stage regression of $\log(E_{i,t})$ on $\log(E^a_{i,t})$ yields an estimate of 0.98 which is highly significant ($F > 20$). The coefficient of $\log(E_{i,t})$ in the second stage is estimated to be at 0.064. Given that the 2SLS and OLS estimates of $\log(E_{i,t})$ are not very different, the Sargan-Hansen endogeneity test is not able to reject the null that the suspected endogenous regressor $\log(E_{i,t})$ is indeed exogenous.

Finally, we do a robustness check on our 2SLS analysis by instrumenting $\log(E_{i,t})$ by $\log(e^a_{i,j})$. The first stage regression results of Equation 6 are reported in column 4 of Table 7. The estimated first stage results imply that a 1 percent increase in contemporaneous availability of funds increases the stock of EGS expenditure by roughly a quarter of a percent. The F statistics for this instrument is also well above 20 alleviating any weak instrument problems. The corresponding coefficient of $\log(E_{i,t})$ in the second stage regression (column 5) is estimated to be 0.08 ($p < 0.05$) which is roughly identical to the corresponding OLS estimate in column 1.

Since the 2SLS estimate is essentially the same as the OLS estimate, the endogeneity test is again unable to reject the null that $\log(E_{i,t})$ is, in fact, exogenous ($p \approx 0.93$). Similar OLS and IV results give us confidence on the unbiasedness of the elasticity estimate $\widehat{\beta}$ obtained in the preferred specification of Equation 4. The results also suggest that after partialling out various district and year effects, the potentially endogenous district-year fluctuations of actual expenditure from fund availability may be due to idiosyncratic supply-side factors 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.

Depending on the choice of instrument, the 2SLS estimates suggest wages to increase between 2.2-2.8 percent per annum which is again roughly an order of magnitude greater than the spot effects calculated in the analogous 2SLS specification (with differential trends) in Table 5. Our empirical analysis hence illustrates that difference-in-differences may fail to capture the treatment effect if (i) the binary treatment model does not successfully identify the treatment and control groups and (ii) if the outcome variable is more responsive to the stock of program capital rather than contemporaneous fluctuations in program provision.

IV. Falsification Test and Robustness Results

A. Falsification Test

We now conduct a falsification test to show that if the underlying data generating process is a model where private sector wages change only because of spot market effects, then the stock of program expenditure has no power in explaining private wages. In this case, contemporaneous program expenditure is the relevant variable to capture the impact of public works on wages.

We begin with Basu, Chau, and Kanbur (2009) (hereafter BCK) model of employment guarantees where private employment and wages change solely due to the impact of public works on the spot market for labor. In the model, the labor market is composed of a small number of employers (N) with monopsonistic or oligopsonistic market power together with a unit mass of workers with same marginal value product of labor (a) but different cost to access private jobs (bx) where x is the worker-specific cost which is uniformly distributed between zero and one while b is the private sector-specific cost which parameterizes any locational, informational, skill-related costs and disutilities associated with private employment. Private wage (w_0) – without the employment guarantee – is therefore always lower than the marginal value product of labor (a). In particular, $w_0 = a/(1+n) < a$ where $n = 1/N$.

The introduction of an employment guarantee affects the spot market for labor by (i) eroding the market power of employers inducing them to pay higher wages and hire more workers (contestability effects) and (ii) by increasing the reservation wage of workers to work in the private labor market. As BCK show, private employment can be higher or lower relative to its level without employment guarantees depending on whether contestability or reservation wage effect dominates which in turn depends on the combination of public wage w_g and *ease of access* to public works parameter τ_g.[20] Relevant to our case, we limit our attention to private wages which are always higher with the employment guarantee than without it. Assuming that a positive amount of workers are hired in the employment program, revised wages in the private sector (w) are given by

$$w = (a + nw_g)/(1+n) > w_0(n) \tag{7}$$

As the new expression for private wages show, the impact of employment guarantee on wages increases with the distortion in the labor market.[21]

To conduct the falsification test, we first construct an artificial panel dataset of 100 districts and 10 years. We construct the data based on the following assumptions: (i) public wage $w^g_{i,t}$ 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 100 districts between $[a_1\ a_2]$; (iii) employers are assumed to have monopsonistic ($N = 1$) or oligopsonistic ($N > 1$) market power, i.e. N is small; and (iv)

[20]BCK define $1 + \tau_g = b/b_g$ as the relative ease with which workers can access EGS employment over private employment.

[21]This is expected since if we begin with the perfectly competitive case where $n \to 0$, there is no distortion to begin with and private wages are always equal to the marginal value product of labor a (both with and without the employment guarantee).

each district is assumed to have a unit mass of working population.

Since the expression of private wage $w_{i,t}$ in Equation 7 contains the wage offered in the public works $w^g_{i,t}$ and not the number of workers employed in the employment program, we keep public employment to be fixed at l^g for all observations. This is so since regressing $w_{i,t}$ on program expenditure $e_{i,t} = w^g_{i,t} l^g$ is meaningful only as long as variations in EGS expenditure are solely due to fluctuations in public wage.

Given assumptions (i)-(iv), we ensure a fixed number of workers get hired in the employment guarantee l^g by varying the access to public works $\tau^g_{i,t}$ (which is controlled by the government). Similar to BCK, we assume that it is always less costly to access public employment $b_g x = bx/(1+\tau_g)$ than private employment (bx). That is $\tau^g_{i,t} \geqslant 0$. In other words, this assumes that the government always hires a fixed number of workers in the EGS and that any variations in program expenditure emanate solely from exogenous fluctuations ($\varepsilon_{i,t}$) in government wage $w^g_{i,t}$. Figure 5 plots the log of private wage (y-axis) before and after the employment guarantee 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 spot market effects discussed above for $N = 10$ employers.

Using the data as generated under the assumptions discussed above, we estimate Equation 8 and 9 to obtain the elasticities of wage with respect to the flow (β_f) and stock (β_s) of EGS expenditure respectively.[22]

$$\log(w_{i,t}) = \alpha_i + \beta_f \log(e_{i,t}) + \upsilon_{i,t} \tag{8}$$

$$\log(w_{i,t}) = \alpha_i + \beta_s \log(E_{i,t}) + \upsilon_{i,t} \tag{9}$$

Figure 6 compares elasticities of wage under the two specifications while varying the number of employers (x-axis). Each tuple (β_f, β_s, N) in Figure 6 is based on a separate simulation where the data is generated by varying the number of employers from $N = 1$ (monopoly) to $N = 10$ (oligopoly). The objective of conducting multiple simulations is to see how the impact of employment programs vary based on the degree of market distortion (employer market power), keeping everything else constant. Therefore, we keep the series of public wage ($w^g_{i,t}$), labor productivity (a_i), and public employment generated under the employment program (l^g) to be the same for all

[22]We only need to control for district fixed effects α_i 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.

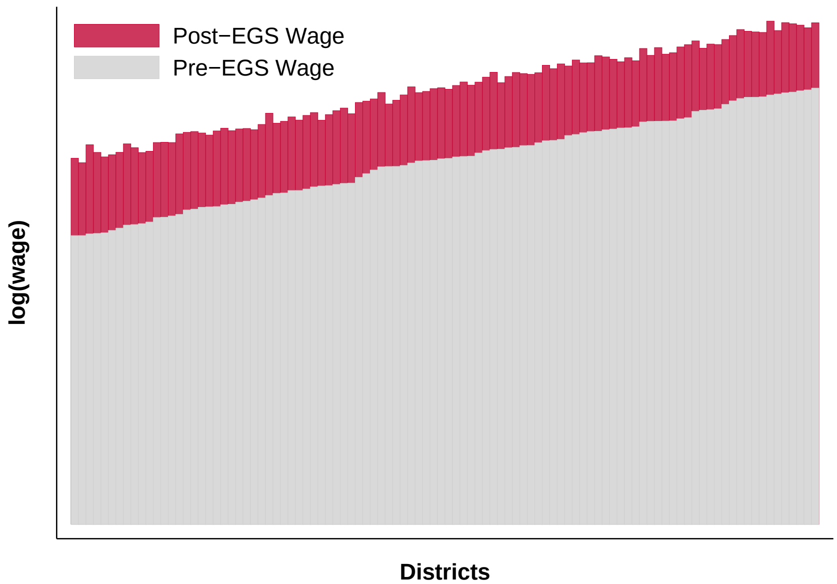

Figure 5. Wage increase due to efficiency gains

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

10 simulations. Only the number of employers N and correspondingly, the access to EGS work ($\tau^g_{i,t}$) is allowed to vary. For the case with N=1 (monopsony), Figure 6 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 program expenditure as in Equation 9, β_s is estimated to be equal to zero (up to 3 decimal places). Further, the impact of the employment guarantee (β_f) diminishes as N increases. This is expected as the gains in efficiency (or contestability effects) become smaller as the economy tends to a more competitive labor market. On the other hand, the elasticity of wage β_s as obtained from Equation 9 is always estimated to be equal to zero, irrespective of the assumed number of employers.

The falsification test hence confirms that if employment guarantees impact private wages only by affecting the spot market for labor, then the stock effect is not empirically relevant in explaining private wages. Contrary to this, our empirical analysis finds the spot effect of employment guarantees to be small and close to zero while the stock effect is estimated to be

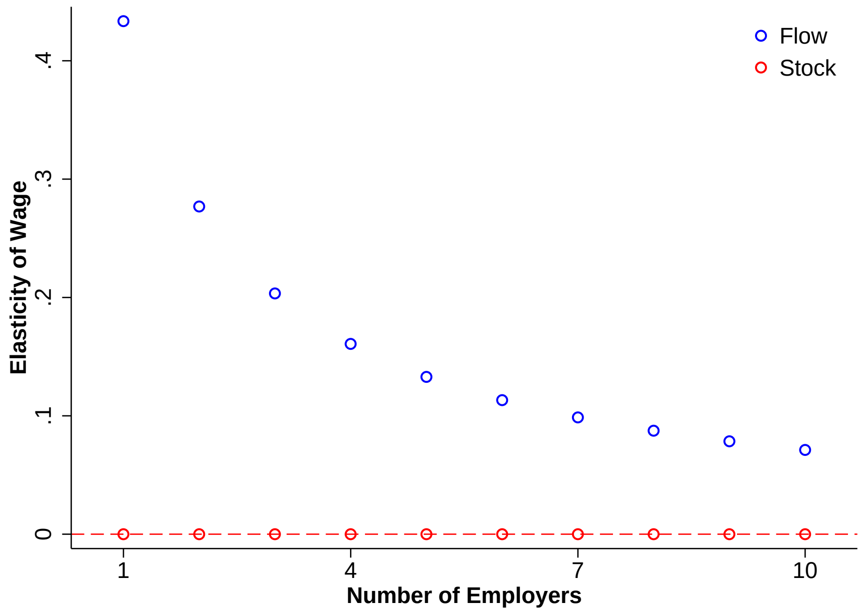

FIGURE 6. COMPARING STOCK AND FLOW SPECIFICATIONS

Note: The figure compares $\hat{\beta}_f$ with $\hat{\beta}_s$ as estimated by Equation 8 and 9 respectively. The figure plots coefficients for 10 different sets of simulations by varying the number of employers (from monopoly to oligopoly).

roughly an order of magnitude larger in comparison. This hence reaffirms the importance of program capital – whether infrastructural, institutional, or administrative capital – in measuring the treatment effect of employment guarantees.

B. Robustness

In this section, we first check for the stability of the elasticity estimate as obtained in our preferred empirical model (Equation 4). Until now we have assumed that the response of wages to program expenditure is similar for both the programs. Although we allow the level of wages to be different (on average) under the NREGA regime by controlling for I_{NREGA}, the elasticity of wage is assumed to be constant for both the programs. We now check whether wages have become more or less responsive under NREGA by estimating 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. Column (1) of Table 8 compares the elasticity of wage estimated under our preferred specification to the case where separate elasticities are estimated for the two programs by interacting I_{NREGA} with $\log(E_{i,t})$ in column (2).

Table 8— Effect over different program regimes

	(1)	(2)
$log(E_{i,t})$	0.077**	
	[0.027]	
I_{NREGA}	-0.036	0.186
	[0.021]	[0.176]
$log(E_{i,t})\|_{I_{NREGA}=0}$		0.110**
		[0.034]
$log(E_{i,t})\|_{I_{NREGA}=1}$		0.072**
		[0.027]
All Controls	Yes	Yes
Observations	1340	1340

Regressions are weighted by district-level population. Standard errors reported in square brackets are clustered at the district-level and robust to heteroskedasticity.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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 highly significant ($t > 3$) while the elasticity of wage under NREGA at 0.7 ($t > 3$) is closer to the full sample elasticity of 0.08. We cannot reject the equality of the two coefficients even at 90 percent confidence level. Hence, our results suggest that the stock effect of employment guarantees on private wages is quite similar for SGRY and NREGA.

Next, we test whether there is any sub-sample heterogeneity based on how well the employment programs 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 the rate of rationing as the proportion of laborers who wanted but did not get work in the public works. 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, we split the overall sample of 12 states into six *less rationing states* (rationing < 40 percent) and six *high rationing states* (rationing > 40 percent).

The criterion of selecting the benchmark value of 40 percent is simply to divide the aggregate sample into two (near) equal halves. Column 1 of Table 9 reports the results of estimating separate elasticities by interacting an indicator variable I_{ration} which takes the value one for states with rate of rationing greater than 40 percent and zero otherwise.[23] The elasticities for high and low rationing groups are estimated to be around 0.09 and 0.07 respectively. Both the estimates are very close to the aggregate sample elasticity as reported in column 5, Table 6. Given the loss of observations, the standard errors are relatively larger for the sub-sample elasticity estimates. Both estimates, however, are still significant at the standard 95 percent confidence level. As expected, we cannot reject the null of equality of the two coefficients ($p - value = 0.66$). 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 criterion that may help ascertain the extent or intensity of program implementation is the annual average per-capita expenditure incurred in a district. Column 2 of Table 9 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 I_{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 similar to the rationing case of column 1. The elasticities for *low* and *high* average expenditure groups are estimated to be around 0.08 and 0.07 respectively which are significant at 95 percent confidence levels. Again, the equality of the two coefficients cannot be rejected (p-$value = 0.87$).

The robustness results hence suggest that the effect of program capital on wages is fairly stable over the entire sample. It is worth mentioning that constant elasticity of wage doesn't necessarily imply the absence of any *scale effects* because of rationing, average expenditure, or how well the program is implemented.[24] Any such differences in the intercept or growth rate of wages

[23] I_{ration} is zero for Rajasthan, Tamil Nadu, Himachal Pradesh, Andhra Pradesh, West Bengal, and Madhya Pradesh and one for the rest of the six states in our sample.

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

Table 9— Stability of Estimates

Based on Rationing		Based on Expenditure	
	(1)		(2)
I_{ration}	1.051	I_{exp}	0.922***
	[2.034]		[0.264]
$log(E_{i,t})\big\|_{I_{ration}=0}$	0.069*	$log(E_{i,t})\big\|_{I_{exp}=0}$	0.079*
	[0.031]		[0.032]
$log(E_{i,t})\big\|_{I_{ration}=1}$	0.086*	$log(E_{i,t})\big\|_{I_{exp}=1}$	0.073*
	[0.035]		[0.035]
All Controls	Yes	All Controls	Yes
Observations	1340	Observations	1340

Column (1) 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 the district-level and are robust to heteroskedasticity.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

at district level are partialled out by fixed effects and differential trends in our model. In that respect, 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 C discusses other results like the program impact on male, female, and semi-skilled wages using our preferred stock specification.

V. Conclusion

New anti-poverty programs implemented by governments across the world often have similar predecessor programs. Prospera, a recent conditional cash transfer program in Mexico was preceded by Oportunidades which in turn was built on Progresa. Similarly, India's National Rural Employment Guarantee Act (NREGA) had SGRY as its predecessor. Taking evidence from India's public works programs, this paper makes three important contributions to the existing literature on impact evaluation of such government interventions.

First, the paper highlights the need to be cautious while using the initial roll-out of 'new' programs as a treatment-control research design. The paper shows how a difference-in-difference model that derives policy impact based on a comparison between early- and late-phase NREGA districts, may fail to identify the policy impact on private wages. This can be indicative of underestimation of program impact in a binary treatment model where the actual gap in employment provision between the treatment and control districts is lower than what is assumed due to the continued existence of the older program exclusively in the control districts.

Secondly, identification based on difference-in-differences may be further confounded if public works have weak spot effects on the labor market to begin with. Using a novel dataset of program expenditure at district-level for SGRY and NREGA, we find spot effects to explain wage increases of only 0.3 percent per annum. Our results are robust to endogeneity concerns. Apart from controlling for various district characteristics and fixed effects, we instrument actual expenditure with funds made available at the district level to check for potentially endogenous under- or over-utilization of funds.

Finally, we find program capital – as measured by the stock of program expenditure – to explain wage increases between 2.2-2.8 percent per annum, roughly an order of magnitude larger than the spot effects. A falsification test shows that our results are not a statistical artefact. If the data generating process is based on a model where public works increase private wages solely by affecting the spot market for labor, then contrary to our empirical findings, it is the contemporaneous program expenditure and not the stock of program spending that successfully captures the treatment impact.

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A. Data Appendix

Agricultural Wage Data: The AWI 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. States like 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. 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. To construct our measure of agricultural wages, we take the average of the field wages for men and women. Hence we have monthly wage data for ten years between 2001 and 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 the months. Furthermore, the AWI data sporadically excludes data for some districts and states for some years. Data for nearly 40 percent of the districts is reported for less than 6 out of 10 years. Data for new states like Chhattisgarh, Jharkhand, and Uttarakhand is not available before 2005. 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 program expenditure data which is reported in the Indian financial year format (from 1 April to 31 March of the next calendar year). Second, we restrict our attention to a balanced panel data of 134 districts (from 12 major Indian states) for 10 years which gives us a total of 1340 observations.

Since we loose districts with incomplete wage data, a possible objection can be the correlation of the backwardness of a district with unavailability of the wage data. If this is true, then restricting the analysis to 134 districts should 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 percent and 58 percent respectively in the aggregate sample is closely matched by the sub-sample proportion of 42 percent and 63 percent for phase I and phase II districts respectively.

Rainfall Data: We use remote sensed rainfall data from the Tropical Rainfall Measuring Mission (TRMM) satellite.[25] 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.

B. Model of EGS with Capital Accumulation

Here we show that increases in private wages (and worker productivity) as a result of the build-up of productive program capital motivates the stock of program expenditure as the relevant empirical measure in evaluating the impact of public works on private wages. More generally, the idea that the stock of public capital is relevant to increases in growth and productivity has been discussed in Aschauer (1989), Corsetti (1992), and Futagami, Morita and Shibata (1993). Accumulation of physical capital, however, is not the only channel that can explain the relevance of the stock effects of employment guarantees. As the falsification test in the main paper shows: if public works affect only the spot market for labor, then contrary to our empirical findings, the stock of program expenditure is not relevant in explaining private wages. Therefore, the model below simply aims to highlight a plausible mechanism that may help explain our empirical results of a positive and significant increase in wages when measured by the stock of program expenditure.

Apart from creating public employment, SGRY and NREGA also created productive public infrastructure through various land development and irrigation projects. For example, projects on water conservation and water harvesting form the majority of the works undertaken under NREGA. Accumulation of productive infrastructure generated under these projects can indeed increase labor productivity and hence wages in the agriculture sector. While information on the type and number of assets generated is limited, Table B1 provides evidence that productive works were indeed carried out under the two programs. The table provides state-wise information on the average number of public works that were initiated and completed in a year under SGRY and NREGA. It is important, however, not to proxy program capital with the total number of completed projects given the differences in size, scale, and nature of the public works together with the heterogeneity in the kind of assets created.

To motivate a simple reduced form relationship between wages and the stock of employment expenditure in our empirical analysis, we extend the model of EGS as discussed in Basu, Chau and Kanbur (2009) (hereafter

[25]Thanks to Thiemo Fetzer for sharing the rainfall data.

Table B1— Productive Public Works under SGRY and NREGA

	Number of Works in SGRY		Number of Works in NREGA	
States	Initiated	% Completed	Initiated	% Completed
AP	89,369	81.53	7,38,094	50.91
AS	90,650	77.66	20,080	44.77
BR	1,06,449	61.37	1,24,475	46.24
GJ	56,914	88.56	91,726	78.30
HP	18,383	75.65	37,476	53.8
HR	30,362	98.20	6,281	56.78
KA	1,30,978	71.17	2,20,720	25.75
KL	27,222	47.80	73,195	66.15
MH	1,14,496	66.70	25,276	40.51
MP	1,39,892	85.92	4,57,587	42.54
OR	68,235	89.21	1,37,067	19.48
PB	21,778	86.88	7,123	44.31
RJ	53,724	86.55	1,44,769	37.56
TN	98,255	93.23	37,523	40.25
UP	2,74,814	81.6	3,68,503	62.72
WB	1,39,553	69.78	1,40,200	57.8
India	16,21,413	78.76	30,28,426	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.

referred as BCK) by allowing the 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.

B1. Workers

There is a unit mass of workers where the utility function of a worker is defined by: $U_t(w_t, x) = w_t - bx$ where w_t is the private wage at time t, b is the private sector specific cost while x is the individual specific cost of working which is supposed to be uniformly distributed between [0 1]. Both b and x 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 $l_t \leq 1$.

B2. 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 contestability and reservation wage effects that are obtained by assuming employers to have oligopsonistic market power as in BCK (see falsification test above for these). The insights obtained from the productivity channel are invariant to the assumed market power of the employers. Hence for the ease of algebra, we 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 maximises his objective function: $\underset{l_t}{Max} \left[a_t - w_t\right] l_t$. 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, $G_t = g_{t-1} + \delta g_{t-2} + \cdots + \delta^{t-1} g_0$ where g_t is the productive capital generated under the EGS at time t.

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.[26] 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 past EGS expenditures. The value of $\delta = 1$ minimizes the sum of squared residuals of our preferred empirical specification (Equation 4). 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.

B3. 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.[27] 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:[28]

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

[26]For 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.

[27]Otherwise, EGS will either not hire any workers or will completely displace the private workforce. See BCK for details.

[28]As 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.

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

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

$l_t^g = l_t^{total} - l_t^p$ 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:

First, 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 the creation of productive assets which increases worker productivity and consequently private wages in the next period to $a_{t+1} = a_0 f(G_{t+1}) > a_0 f(G_t) = a_t$.*[29]

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 the 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).[30] Given the annual frequency of our data in the empirical

[29] In 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.

[30] Basu (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.

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.

B4. 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 4 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 the 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*.[31] Similar to them, we find a corner solution of $\delta = 1$ that best fits the data.

Table B2—Results with different discounting rates

	$\delta = 1$	$\delta = 0.975$	$\delta = 0.95$
$log(E_{i,t})\|_{\delta=1}$	0.077^{**}		
	[0.027]		
$log(E_{i,t})\|_{\delta=0.975}$		0.073^{**}	
		[0.026]	
$log(E_{i,t})\|_{\delta=0.95}$			0.069^{**}
			[0.025]
All Controls	Yes	Yes	Yes
Observations	1340	1340	1340

The dependent variable is $log(w_{i,t})$. Here $E_{i,t} = \sum_{j=1}^{t} e_{i,j}\delta^{(t-j)}$. Here $e_{i,j}$ is the expenditure under the employment guarantee for district i and year j. All regressions are weighted by district-level population. The standard errors reported in square brackets are clustered at the district-level and are robust to heteroskedasticity.

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

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

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. Table B2 compares the results from our preferred specification which are obtained by assuming zero depreciation ($\delta = 1$) to cases where we impose ad-hoc depreciation rates of 2.5 percent ($\delta = 0.975$) and 5 percent ($\delta = 0.95$). 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 captures the spot effects of employment guarantees.

C. Other Robustness and Results

C1. 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 stock of program expenditure series of district i – $\{E_{i,2001}, E_{i,2002}, \cdots, E_{i,2010}\}$ – to some other district. The rationale behind such an experiment is that if the stock of program expenditure has no influence on agricultural wages, then the estimates obtained from shuffling the observations of the variable $E_{i,t}$ should not be too different from the true (non-randomized) estimate.[32] It is worth noting that we do not randomize observations of $E_{i,t}$ across districts and years as this 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 across districts.

Figure C1 plots the cumulative distribution function (CDF) of the placebo estimates along with the true (non-randomized) coefficient as estimated in column 5, Table 6. In each simulation, a coefficient is estimated for the regression of field wages on the shuffled stock of program expenditure variable under our preferred specification of Equation 4. Consistent with the desired specificity of the tests, the simulations find only 1.8 percent of the placebo coefficients to be statistically significant and greater than zero. Furthermore, only five out of a thousand randomized estimates (0.5 percent of the total observations) are greater than the non-randomized estimate. Hence, the stock effect of employment guarantees on wages is estimated to be the

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

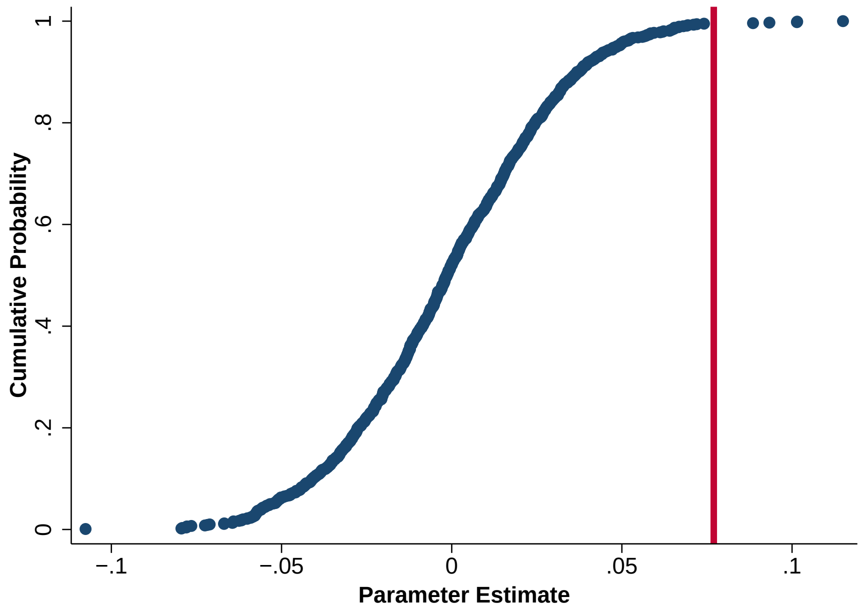

Figure C1. Placebo Tests using Randomization

Note: The figure shows the CDF of the elasticity of wage w.r.t. the randomized stock of program expenditure obtained from 1,000 Monte Carlo simulations. The red vertical line shows the estimate obtained from the non-randomized $E_{i,t}$.

largest only when we align a district to its true stock of EGS expenditure series.

C2. Effect on Male and Female Wages

Here we check whether the employment guarantees affected male and female wages differently. Using the same set of controls as in Equation 4, Table C1 reports wage elasticities based on gender as well as over different program regimes. Column 1 of Table C1 reports elasticity of male wages over the aggregate sample while column 2 shows male wage elasticities separately for SGRY and NREGA regime.

Columns 3 and 4 show the same set of results but for female wages. Based on the average growth in $E_{i,t}$ and the elasticities of male and female wages reported in columns (1) and (3) respectively, employment guarantees are shown to increase male wages by 2.1 percent and female wages by 3.54 per annum on average (where both estimates are significant).

Table C1—Effect on Field Wages for Men and Women

Dependent variable:	$log(Male\ Wages)$		$log(Female\ Wages)$	
	(1)	(2)	(3)	(4)
$log(E_{i,t})$	0.060^{*}		0.101^{**}	
	[0.028]		[0.032]	
$log(E_{i,t})\big\vert_{I_{NREGA}=0}$		0.098^{**}		0.129^{***}
		[0.035]		[0.038]
$log(E_{i,t})\big\vert_{I_{NREGA}=1}$		0.055		0.097^{**}
		[0.029]		[0.032]
All Controls	Yes	Yes	Yes	Yes
Observations	1340	1340	1340	1340

All regressions are weighted by district-level population. The standard errors reported in square brackets are clustered at the district level and are robust to heteroskedasticity. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The greater impact of employment programs on female wage rates is expected since, relative to men, women earn 18-20 percent lower on average due to gender biases and other social reasons. On the other hand, employment programs offer equal wages for men and women, inducing a higher growth in female wage rates. Columns (2) and (4) continue to show a greater impact on female wages for both the program regimes. While the wage elasticities for male and female wages under the SGRY regime are noticeably larger than the respective wage elasticities during NREGA, a test on the equality of coefficients over the two program regimes cannot be rejected even at 90 percent confidence level for both male and female wages. Our result of a higher impact of employment programs on female wage rates are consistent with similar findings by Azam (2012).

C3. Effect on Skilled Wages

The skilled wage category includes wages for carpenter, cobbler, and blacksmith. While the employment programs we study are mostly expected to affect wages of unskilled manual labor work – represented by agricultural field wages in our study – it is possible that positive spillovers from the public works increase wages for skilled work as well. Column 1 of Table C2 reports a regression of skilled wages on the same controls as in Equation 4. The estimated elasticity of skilled wage at 0.06 is lower than the elasticity for unskilled wages and is barely significant at 95 percent confidence level. Splitting the sample based on program regimes in column 2 shows the elasticity of skilled wages over the SGRY regime to be significantly greater (and different) than the corresponding estimate over NREGA regime (which is not significant).

There is hence evidence to support that the initial rise in unskilled wages exerted an upward pressure on wages that are slightly higher in the wage distribution. Such positive spill-over effects are plausible since activities under 'field wages' and 'skilled wages' are fairly comparable and together represent the rural labor market (as is described in the Agricultural Wages of India data). The insignificant elasticity of skilled wages under NREGA is also expected if the increase in program capital (like wells, irrigation projects, etc) mostly improves agricultural wages through gains in worker productivity but does not result in sustained increases in skilled wages.

Table C2—Effect on Semi-skilled Wages (Men)

Dependent variable:	log(Skilled wages)	
	(1)	(2)
$log(E_{i,t})$	0.060*	
	[0.030]	
$log(E_{i,t})\big\|_{I_{NREGA}=0}$		0.118**
		[0.038]
$log(E_{i,t})\big\|_{I_{NREGA}=1}$		0.047
		[0.030]
All Controls	Yes	Yes
Observations	1500	1500

All regressions are weighted by district-level population. The standard errors reported in square brackets are clustered at the district level and robust to heteroskedasticity.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$