# The Spillover Effects of Public Works on Migration, Labor Allocation and Wages: Evidence from National Rural Employment Guarantee Scheme, India

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# The Spillover Effects of Public Works on Migration, Labor Allocation and Wages: Evidence from National Rural Employment Guarantee Scheme, India

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**Abstract**

Rural workfare programs guaranteeing work at above market wages are intended to provide security to the unemployed during the agricultural off-season and are an increasingly used feature of labor market policy in developing countries. In recent work, India's National Rural Employment Guarantee Scheme (NREGS), the largest rural workfare program in the world, has been attributed with crowding out work, raising private sector wages and lowering rural-urban migration. However, the empirical literature is agnostic about the sign and magnitude of spillovers generated by the large scale program. This paper studies the spatial spillover effects of NREGS on migration, time allocation and casual wages in areas which *did not receive* the program over the study period.

Standard economic theory predicts that wage differentials across labor markets linked by migration should lead to equalization of wages in a competitive equilibrium. This analysis exploits the plausibly exogenous variation in wage differentials introduced by the staggered rollout of NREGS across contiguous program and non-program districts. It then tests the hypothesis that the program generated labor market spillovers to non-program districts using a nationally representative employment survey. Our results show that on average, real wage for casual labor increased by 2.7% with every additional program neighbor in non-program districts. Additionally, the impact of having only program neighbors was estimated to be a 17.5% rise in real wage for casual labor in non-program districts, relative to districts without any NREGS neighbors. The effects on individual level labor supply, non-labor force participation and unemployment are not statistically significant. Together, these results provide empirical support for predicted effects from theory.

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## 1. Introduction

Rural workfare programs guaranteeing work at a pre-determined wage are intended to provide security to the unemployed and underemployed during the agricultural off-season and are an increasingly used feature of labor market policy for governments worldwide. Recent examples include public work programs in India, Bangladesh, Pakistan, Philippines, Egypt, Botswana, Kenya and Chile (Subbarao 1999). India's National Rural Employment Guarantee Scheme (NREGS), the largest rural workfare program in the world, guarantees every rural household 100 days of employment at the state specific minimum wage and its impact on labor market outcomes has been subjected to much scrutiny. It has been attributed with crowding out work, raising private sector wages in general and casual labor in particular (Azam 2012, Berg et al. 2012, Imbert & Papp 2014, Zimmerman 2013). Additionally, the slowing down of short term migration from rural to urban areas (Imbert & Papp 2014, Bhatia & Ranjan 2009, Jacob 2008) has also been estimated. The wider literature on NREGS has demonstrated the program's ripple effects – the increased use of labor-saving agricultural technology (Bhargava 2014), reduction in primary school enrollments (Li & Sekhari 2013), increase in child labor by older children (Islam & Sivasankaran 2014), positive impact on grade progression and test scores (Mani et al. 2014), and a rise in consumption, nutritional intake and asset accumulation (Liu & Deininger 2010).

The growing empirical literature has exploited NREGS's staggered rollout[1] and identified causal estimates of its impact on labor market outcomes by employing difference-in-difference (Azam 2012, Berg et al. 2012, Imbert & Papp 2014) or regression discontinuity methods (Bhargava 2014, Zimmerman 2013). This literature though, is surprisingly agnostic about the sign and magnitude of labor market

[1] The timeline of NREGA's three-phase rollout was 199 districts in Phase I (Feb 2006), 128 districts in Phase II (April 2007) and the remaining 261 districts in Phase III (April 2008).

spillover effects.[2]. While non-market spillovers for the untreated in treated units have been estimated in randomized controlled trials including deworming externalities (Miguel & Kremer 2004), cash transfers (Angelucci & DeGiorgi 2009),information spillovers (Kremer & Miguel 2004, Oster & Thornton 2012, Miller & Mobarak 2013) and general equilibrium labor market effects of rainfall insurance (Mobarak & Rosenzeig 2014), it is relatively rare in observational studies. In an exception, Angelucci & DeMario (2015) estimated that Mexico's PROGRESA increased the consumption of ineligible households in treated villages by 10% relative to ineligible households in control villages and were also able to rule out general equilibrium effects as the mechanism.

Conceptually, the spillover effect estimated in this analysis is fundamentally different from the studies mentioned above. While the aforementioned papers investigated the indirect effect of treatment on ineligible sub-units (individuals and households) within *treated* units, this analysis focuses on the spatial spillovers from treatment to individuals within *control* units. Spillovers of both kinds lead to the violation of the Stable Unit Treatment Value Assumption (SUTVA), i.e. the treatment status of any unit does not affect the potential outcomes of the other units (Imbens & Rubin 2009), but the latter has not been investigated in the empirical literature. In particular, non-experimental approaches to estimating NREGS's causal impact assume that control districts' labor market outcomes are the appropriate counterfactual for the treated districts in absence of the workfare program, as a group ("parallel paths assumption" for DID estimators) or locally (for RDD estimators). This assumption is invalidated if changes in control districts' outcomes occurred precisely because of proximity to treatment districts, leading to biased causal impacts (Duflo et al. 2007), incorrect cost-benefit calculations and potentially wrong policy decisions about continuing or discontinuing the program. A major contribution of this paper is that it is the first known estimation of NREGS's general equilibrium labor-market spillovers in control districts, i.e non-program districts.

[2] In the theoretical literature, Fields & Raghunathan (2014) have modeled the effect of NREGS on inter-temporal productivity spillovers as part of a two-period seasonal agriculture market.

Standard economic theory predicts that wage differentials across labor markets linked by migration should lead to equalization of wages in a competitive equilibrium. In fact, capital flows, migration, and goods trade are each sufficient for equalization of wages (Robertson 2000). Given that the study period for this analysis is relatively short and that demand for NREGS is seasonal in the agricultural labor market, it is unlikely that capital flows across neighboring districts, in the form of lumpy investments in land and equipment would be the driving mechanism for equalization. The case for goods trade is even weaker because of the similarity in relative endowments and the mix of goods produced across neighboring districts. It must be highlighted here that in a long-run general equilibrium setting, *nominal* wage differentials could persist even in absence of capital flows and goods trade, if, shocks to demand or supply of labor in a local market are fully capitalized in the price of land (Rosen 1979, Roback 1982), thus equalizing *real* wages spatially. This result from the Rosen-Roback framework, the "workhorse of spatial equilibrium analysis" (Glaeser 2001) though is dependent on the highly restrictive assumptions that the local labor supply is infinitely elastic and that the elasticity of housing supply is zero (Moretti 2011). Given that high migration costs have been documented in India (Imbert & Papp 2014 estimate it to be as high as 60% of migration earnings), the applicability of this theoretical framework is limited. Yet, it does necessitate the use of real wages for the purposes of empirical analysis.

In principle, the enforcement of a mandated NREGS minimum wage in excess of market wage in some districts and its absence in neighboring districts would have created wage differentials across interlinked labor markets. *Ceteris paribus*, two kinds of migration-linked spillovers could affect residents of non-program districts. First, if the NREGS raised private sector wages in program districts, residents of non-program districts could seasonally migrate or commute to program districts if their expected wages are higher, *even if they don't participate in the program*[3], thus lowering aggregate labor supply and raising wages in their home districts[4]. Second, if NREGS lowered out-migration or commutes from program districts, non-program destinations would again experience a reduction in labor supply. Together, these amplifying effects could raise wages in the non-program districts, thus resulting in spillovers for "control" districts.

Previous estimations of NREGS's labor market impacts have justified abstracting away from migration spillovers by citing low levels of permanent inter-district and rural-urban in-migrants, but this reasoning is flawed on two counts: i) seasonal inter-district migration is fairly common in some parts of India (Impert & Papp 2014) and inter-district commuters, unaccounted in migration data, are part of the private-sector casual labor market affected by NREGS, and ii) despite the focus on rural-urban migration, rural-rural migration still accounts for two-thirds of migration – 67.12% (Census of India, 2001) – and 28.6% cite employment related reasons for their migration (National Sample Survey 2007-08). In the subset of seasonal migration, this proportion is reversed with more than two-thirds migrating to urban areas[5]. While the NSS definition of seasonal migrant could understate their actual numbers[6], by some estimates, about 35–40 million laborers – almost half the number of casual laborers outside agriculture and 10% of

[3] Commuting to a program district in order to work in NREGA is ruled out in principle, by the rules of the program, which specify that only village residents are eligible for job cards and work allocation. This does not rule out long term in-migrants benefiting from the program but such migration is low and deterred by high costs.

[4] This is similar to the effect on emigration on labor market outcomes in source countries, a question not given great attention in the empirical literature, with a notable exception being Mishra (2007), which estimated that the outflow of Mexican workers to the US between 1970 and 2000 has increased the wage of an average Mexican worker by about 8%. In related work, Robertson (2000) finds that U.S. wage shocks are transmitted from border to interior cities in Mexico by way of labor migration from interior to the border.

[5] In absolute numbers, there were an estimated 15.2 million short-duration out-migrants, of whom 12.9 million (85.1 per cent) were male, and 13.9 million (71 per cent) were rural out-migrants. The overall out-migration rate was 1.33 - 1.72 for rural areas and 0.4 for urban areas.

[6] NSS 64 asked individuals whether they had migrated for 1-6 months in the last 365 days. This is likely an underestimate because: i) in many cases, the seasonal/circular migration cycle is longer than six months, and ii) quite often, entire households and not individuals participate in seasonal migration (Srivastava 2011)

agricultural laborers (about 9 million) could be seasonal migrants (Sinha 2011). The salience of seasonal migration to labor-markets impacted by NREGS is indicated even in NSS data – 56.6% of all seasonal migrants report working in construction (36.2%) and agriculture (20.4%), the industries most likely to compete with the government program for workers.

This paper studies the spatial spillover effects of NREGS on Phase III districts which did not receive the program during the study period (July 2005 to March 2008) by exploiting their plausibly exogenous variation in market linkages to neighboring program districts. In spirit, this approach is similar to McKinnish (2005) which scrutinized welfare migration in the U.S. and found that at a state border with a large cross-border benefit differential for Aid to Families with Dependent Children (AFDC), border counties in the high benefit state experiences higher program participation and expenditures relative to interior counties. It is also related to estimations of spatial spillovers from natural resource booms to counties not experiencing booms (Allcott & Keniston 2014) and the effects of civil wars on economic outcomes in neighboring countries (Murdoch & Sandler 2002). This analysis is distinct from the aforementioned in that it focuses on *indirect* private sector impacts - wage and time allocation impacts – of a government program in *areas where it was not introduced*. It also tests the hypothesis that the transmission channel for these impacts is seasonal migration, mediated in turn by variation in geographic proximity to program districts. Our results show that on average, the real wage for casual labor increased by 2.7% with every additional NREGS neighbor, conditioned on not receiving the program. Additionally, the impact of having only NREGS neighbors was estimated to be a 17.5% rise in casual wage for non-program districts, as compared to districts without any NREGS neighbors. The effects on individual-level labor supply, leisure and unemployment are not statistically significant. Together, these results provide empirical support for predicted effects from theory.

The rest of this paper is organized as follows. Section 2 provides a basic theoretical framework for the analysis and develops predictions for empirical estimation. Section 3 describes the measure of exposure intensity used in this analysis and presents preliminary evidence of its relationship with seasonal migration. Section 4 describes our data, with estimation strategy outlined in section 5. Section 6 presents the main results along with a placebo analysis while section 7 offers concluding remarks.

## 2. Theory

This section presents a simple model motivating the optimization problem faced by an individual in a non-program district with an outside option of seasonal migration or commuting to a neighboring NREGS district. This individual has a utility function u($c_i$, $l_i$) over consumption and leisure, with the function increasing and concave over both arguments. Her time endowment, ***T*,** is split between leisure $l_i$ and either work within the home district $(L_i)$ or work outside the home district $(L_i^O)$**.** The home district wage is $w^d$ while both migrants and commuters earn $w^o$ outside the district. We assume that $w^{o\prime}(E)>0$, i.e $w^o$ is an increasing function of exposure intensity *E*, a measure of proximity to NREGS neighbors and thus, the degree of linkage with the labor markets where the program is implemented. This assumption implies that relevant $w^o$ increases with the number of program neighbors, a stronger statement than the fact that as the choice set of $w^o$ expands, the maximum of the set cannot decrease. It is noteworthy that this assumption might not hold if neighboring districts are subject to different policies designed to stop the movement of labor, i.e. keep labor markets segmented, thus delinking the relationship between that number of program neighbors and the maximum market wage offered outside a non-program district. Since the introduction of NREGS was not conditioned on any mobility restrictions for non-participants and this model aims to capture intra-country mobility within India, which constitutionally guarantees the right to move and reside in any part of the country (Part III, Constitution of India), there is little evidence of market segmentation and thus minimum cost to making the stronger assumption of increasing relationship between exposure intensity and out of district wage for our purposes.

Work outside the home district is associated with an additional variable cost $v_i$ (heterogeneous across individuals). While transportation cost is an example of a variable cost for commuters, it is fixed for seasonal migrants. Meanwhile, rent is an example of variable cost for seasonal migrants but is not a cost at all for commuters. This model abstracts from the distinction between the variable costs faced by seasonal migrants and commuters because it does not change the individual's problem. The marginal wage rate faced by her depends on which wage rate is higher – home district wage ($w^d$) or the net out of district wage ($w^o - v_i$). Individuals also have non-labor income $y_i$, which can be thought of as profits from agricultural production in this setting. It is worth noting that this model assumes that there is no capital involved in the production function, thus ruling out capital flows across labor markets by assumption. Individuals choose consumption and leisure to solve:

$$\max u(c_i, l_i) \quad \text{s.t.} \quad c_i + W_i l_i = y_i + W_i T \tag{1}$$

$$L_i + L_i^{O} + l_i = T \tag{2}$$

$$W_i = \max\{w^d, w^o - v_i\} \tag{3}$$

$$y_i = \Pi_i = f(D_i) - w^d D_i \tag{4}$$

Solving the first order conditions, the individual's standard demand functions for leisure and consumption are given by:

$$l^* = l^*(W_i, y_i + W_i T) \tag{5}$$

$$c^* = c^*(W_i, y_i + W_i T) \tag{6}$$

In this model, it is optimal for an individual to either work in the district or outside depending on the marginal wage rate, but not both. Without loss of generality, we can assume variable costs associated with work outside the district to be of three types - high ($v_H$), medium ($v_M$) or low ($v_L$). Further, we can assume that given a {$w^d$, $w^o$} pair, workers with low variable costs ($w^o – v_L > w^d$) work outside while those with medium ($w^o – v_M < w^d$) and high variable costs ($w^o – v_H < w^d$) optimally allocate their labor to the home district. The aggregate labor supply within home district is $\sum_i^N L_i^* (W_i, y + W_i T)$ and aggregate labor supply outside home district is $\sum_i^K L^o{}_i^* (W_i, y + W_i T)$ with N representing the number of "stayers", K representing the number of out of district workers and (N+K) being the total population of the home district.

### *2.1 Implications of a Positive Shock to Exposure Intensity*

By assumption, a positive shock to *E* corresponds to an increase from $w^o$ to $w^{o\,\prime}$, thus raising the returns to working outside home district for all types. The effects of this shock on the population of non-program district are outlined for the three possible types of individuals below:

Type 1: $w^{o\,\prime} – v_L > w^o – v_L > w^d$

This is the sub-population of K individuals which worked outside the district before the shock to exposure intensity. As their marginal wage rate increases because of the shock, they could cut back on their work outside the district and buy more leisure or increase labor supply outside the district, the ambiguity being driven by the offsetting substitution and income effects in a single-market labor model. It is worth highlighting that substituting towards more work in the home district is not an optimal response for this type.

Type 2: $w^{o\,\prime} – v_M > w^d > w^o – v_M$

For this type, a subset of the N "stayers", the shock switches their marginal wage rate from the district wage to the net out of district wage. Their optimal response is to switch from work in home district to work outside the district. They could then cut back on work and buy more leisure or increase labor supply due to income and substitution effects, as long as their labor is supplied outside the district.

Type 3: $w^d > w^{o\,\prime} - v_H > w^o - v_H$

In case of these remaining "stayers", the shock to exposure intensity and net out of district wage does not overcome the high variable costs associated with their work outside the district. As a result, their labor supply response does not change due to the shock.

Aggregating individual labor supply within the home district, it is evident that the number of pre-shock "stayers"(N) would decrease as a result of the shock and a switch in marginal wage rate for the second type. *Ceteris paribus*, this would shift district level labor supply inwards and raise home district wage $w^d$. Across non-program districts, the magnitude of rise in $w^d$ increases with the size of the shock to exposure intensity.

Prediction 1: For a given out of district wage $w^o$, a positive shock to exposure intensity $E$ will lead to higher equilibrium wages $w^d$ in the non-program districts if the wage differential between NREGS and non-program districts exceeds variable cost $v_i$ for some subset of the non-program district population.

Prediction 2: For a given out of district wage $w^o$, a positive shock to exposure intensity $E$ will result in lower aggregate employment in non-program districts but the effect on individual leisure and labor supply is ambiguous.

It is evident from the formulations above that a positive shock to both out of district wage ($w^o$) and exposure intensity ($E$), will only increase the magnitude of the predicted increase in equilibrium wage $w^d$ for non-program districts. Since the introduction of NREGS in neighboring districts simultaneously raised market wages in those districts *and* induced variation in exposure intensity for non-program districts, upward wage pressure is amplified with higher exposure intensity.

## 3. Exposure Intensity and Migration

Enacted as law in September 2005, NREGS guarantees 100 days of work annually at a state-level minimum wage to every rural household. In 2010-11, NREGS provided 2.27 billion person-days of employment to 53 million households and its budget represented 0.6% of India's GDP. Rolled out in 199 districts in February, 2006, the act was gradually extended to 128 districts in April 2007 and to the rest of rural India in April 2008 (Table 1).

**Table 1:** NREGS Rollout

| **Phase** | Phase I | Phase II | Phase III |
|---|---|---|---|
| **Rollout date** | Feb 2006 | April 2007 | April 2008 |
| **Number of Districts** | 199 | 128 | 261 |

**Table 2:** NSS Survey Rounds & NREGS Rollout

| **Timing of Survey** | Before Phase I | After Phase II | After Phase III |
|---|---|---|---|
| **Round** | NSS 61 | NSS 64 (Subround 1–3) | NSS 64 (Subround 4) |
| **Survey Year** | July 2004 – June 2005 | July 2007 – March 2008 | April 2008 – June 2008 |
| **Number of Districts** | NREGS = 0 | NREGS = 327 | NREGS = 588 |

This analysis is restricted to the 261 Phase III districts which received NREGS last. The study period is from July 2004 – March 2008, when these districts did not receive the program but were exposed to it by virtue of labor market linkages to program districts. Using data from the nationally representative National Sample Survey's (NSS) 61$^{st}$ and 64$^{th}$ rounds conducted before and after introduction of NREGS (Table 2), the spillover impacts of NREGA on wages and labor allocations in non-program districts can be estimated. In order to capture exposure intensity, the following measure is constructed and computed for each Phase III district:

$$E = (\textit{Number of contiguous NREGS neighbors})/(\textit{Total number of contiguous neighbors}) \qquad (1)$$

*E* takes on fractional values and increases with every additional contiguous neighbor that receives the program, assuming that the total number of contiguous neighbors does not change. It is noteworthy that in principle, any two districts are neighbors, for that matter any two points in space can be considered neighbors, depending on how neighborhood is defined. Given the absence of theory in this area, empirical choices in the literature have largely been driven by data constraints and the objective of the analysis. Since the objective of this exercise is to capture spillover impacts driven by degree of linkage between labor markets, first-order contiguity, sharing of a boundary, is used as the criterion for neighborhood (see Murdoch & Sandler 2002, McKinnish 2004, Robertson 2000 for similar criteria). It is possible that some distant labor markets are better linked by idiosyncratic transport or social networks than adjacent districts but this study abstracts from those linkages because on average, contiguity is more robust to wider range of district characteristics[7]. Our preferred ratio based measure differs from the studies cited above in that it gives higher weight to districts with more program neighbors, but also penalizes large districts which happen to have more adjacent neighbors by adjusting the measure downward. To address concerns about the definition of neighborhoods, this analysis investigates the robustness of this definition using the following alternatives: i) a simple count of number of contiguous program neighbors, which does not account for the possibility that districts with more neighbors are *ex-ante* likely to have more NREGS neighbors, and ii) a relatively coarse dichotomous indicator for whether the district has at least one NREGS neighbor, which constrains exposure to be a binary variable and does not account for intensity.

Combining spatial data on district boundaries with information on NREGS rollout, our measure of exposure intensity was computed for each Phase III district in our sample. It is then linked with migration information collected as part of NSS 64 in order to validate the motivating assumption that seasonal migration varies with the exposure intensity of Phase III districts. Since migration information was not

[7] Robertsen (2000) defines two criteria - correlation in wage shocks between U.S. and Mexico cities and the rate of convergence to equilibrium after the shocks - for measuring market linkages. The study finds that Mexico's border cities are more integrated with the U.S. on both counts, relative to its interior cities.

collected in NSS 61 (the round immediately preceding NREGS), it is not possible to directly estimate the effect of exposure intensity on out-migration. Yet, some cross-sectional inferences can be drawn about the empirical relationship between seasonal migration, exposure intensity and the impact of NREGS, by exploiting NSS survey design, which comprises of four quarterly sub-rounds (July – Sep 2007, Oct – Dec 2007, Jan – Mar 2008 and April – June 2008).

As a first step of empirical analysis, it is useful to identify seasonal migration patterns across district exposure intensities. Fig. 1 & 2 graph the number and fraction of seasonal migrants at the household level across three district exposure intensity categories – Bottom ($E$<=0.33), Middle (0.33<$E$<=0.66) and Top ($E$>0.66) – in early and late phase districts, before and after the last phase rollout in April 2008. It is evident that the seasonal out-migration increased with exposure intensity for all districts. After Phase III districts received NREGS, seasonal migration declined across all categories, in contrast with districts which already had the program, thus reflecting the well-documented migration reducing impact of the program. Interestingly, this decline was not equal across district exposure intensity categories. In particular, the decline in migrant fraction of the household was proportional to exposure intensity, suggesting seasonal out-migration served as a substitute for the program and this substitutability increased with labor market linkages with NREGS districts, proxied by exposure intensity. Disaggregating short term out-migration by destination demonstrates that this pattern for Phase III districts is exaggerated when the destination is rural, where NREGS was primarily targeted (Appendix Fig. 2). Although the construction of these categories is somewhat arbitrary, this graphical trend increases our confidence in the ability of our preferred measure to capture the relationship between exposure intensity and seasonal migration.

In the absence of migration information from the previous survey round (NSS 61), it is instructive to compare "any time in the past" migration statistics computed from NSS 64[8] with the seasonal migration

[8] NSS 64 also asked household heads about how many members had migrated at any point in the past. The number of long-term migrants and their fraction of the household is computed from this question.

trends outlined above. Fig. 4 shows that when the time horizon is changed from the last one year to any time in the past and longer migration spells are accounted for, out-migration decreases with exposure intensity, an inverse of the seasonal migration trends graphically demonstrated previously. This distinction is further validation that our measure of exposure intensity captures variation in seasonal migration and commutes to NREGS districts and does not reflect pre-existing out-migration trends driven by other factors. Interestingly, the post-NREGS decline out-migrant fraction observed earlier is offset in Bottom and Middle intensity categories.

## 4. Data

As outlined in section 2, this analysis utilizes two sources of data: i) two rounds of employment household surveys carried out by India's National Sample Survey Office (NSSO) and ii) spatial data on district boundaries. The individual is the primary unit of analysis and the sample is restricted to adults aged 15 to 65[9] with secondary education or less in 261 Phase III districts. NSS survey is comprised of four sub-rounds, designed to coincide with *rabi* and *kharif*, the two growing seasons in Indian agriculture as well as post-harvest quarters. The study period for this analysis is restricted to be from July 2004 to March 2008 by dropping the 4th sub-round from NSS 64, to ensure that labor market changes in Phase III districts after they received the program in April 2008 do not contaminate the working sample. Since the survey is uniformly distributed across sub-rounds by design, this does not systematically change the working sample. Together, seven sub-rounds of data drawn from two rounds of the NSS are used.

In the NSS surveys, employment was measured in three different approaches: i) *usual status* with a reference period of one year, ii) *current weekly status* with a one-week reference period and iii) *current daily status* based on the daily activity pursued during each day of the week preceding the survey. This

[9] NSSO defines individuals aged 16 to 65 as the "economically active population" and uses this sample to calculate employment and unemployment rates. This analysis adopts the same convention.

analysis utilizes the *current daily status* measure to construct weekly time allocation for each individual in three mutually exclusive and exhaustive categories - work days (casual labor, salaried work, domestic work, public sector work and self-employment), unemployment and non-labor force participation (NLFP)[10]. We also compute time allocated to, and daily wages for individuals who worked as casual labor since this segment of the labor force is most likely to be directly impacted by spillover from public sector casual labor offered by NREGS. This wage has been inflation adjusted, using quarterly CPI for agricultural workers. The table below summarizes household level statistics for all the dependent variables used in the analysis before and after the shock for the sample of Phase III districts.

Weekly Time Allocation & Casual Labor Wages in Phase III Districts

| | Pre-Shock | | Post-Shock | |
|---|---|---|---|---|
| | Mean | SD | Mean | SD |
| Unemployment Days | 0.33 | 1.28 | 0.30 | 1.24 |
| Work Days | 3.76 | 3.26 | 3.56 | 3.32 |
| NLFP Days | 2.91 | 3.30 | 3.14 | 3.38 |
| Casual Days | 0.82 | 2.05 | 0.76 | 2.00 |
| Casual Labor Wage (Nominal) | 58.79 | 36.72 | 77.39 | 45.75 |
| Casual Labor Wage (Real) | 58.23 | 36.37 | 63.80 | 37.78 |

Note: Casual Labor Wage is reported in Rupees/Day

## 5. Estimation

In our strategy, the spillover effects of NREGS on casual wages and weekly time allocations are estimated by exploiting the exogenous shock to variation in exposure intensity across non-program districts due to the introduction of NREGS neighbors. These regressions are at the individual-district-quarter level of analysis. There are two main advantages to this approach, as compared to a household-

[10] NSS 61 and 64 recorded the time disposition of respondents during the week preceding the interview, coding the intensity of their activities as 0.5 or 1 for each day. In this analysis, these activities are classified in one of the four categories and the intensity of that category is summed across the week to get weekly time allocations.

district-quarter level approach: i) individual level factors like age and gender which affect migration[11], time allocation and wages, as well as household level factors like caste and monthly per capita expenditure (MPCE) can be controlled for[12], and ii) greater efficiency of estimates due to higher number of observations.

*51 Exposure Intensity as a Ratio*

Although a difference-in-difference estimation could be carried out by constructing "high" and "low" exposure intensity categories, any such categorization would be necessarily arbitrary and the estimated coefficients would sensitive to such a choice. Further, the interpretation of such an estimate is not obvious given that any constructed category would mask substantial heterogeneity. In light of these comments and the properties discussed in section 3, the first class of specifications estimated in this analysis (2), employs our preferred ratio-based measure of exposure intensity, $Exposure_i$, which takes on values between 0 and 1 and is time-invariant for a district. $Post_{jt}$ is an indicator variable which is 0 for all non-program districts in the first four quarters predating the program (NSS 61) and is 1 in the post-shock periods (NSS 64). The variable of interest is the interaction term, $Exposure_j*Post_{jt}$, and the parameter $\beta_3$ estimates the impact of exposure intensity on individual level dependent variables. Inflation adjusted daily wage for casual labor and time allocations and serve as the dependent variables ($Y_{ijt}$) in separate regressions, thus enabling the estimation of extensive changes in labor supply as well as the observation of intensive substitution across time allocation categories. The full specification is:

$$Y_{ijt} = \alpha + \beta_1 Post_{jt} + \beta_2 Intensity_j + \beta_3 Post_{jt}*Intensity_j + \beta_4 Trend_t + \beta_5 DistrictFE_j + \boldsymbol{\Theta X_{ijt}} + \varepsilon_{ijt} \qquad (2)$$

[11] "The socioeconomic profile of the short-duration/seasonal out-migrants is very different from the other migrants. These migrants are much more likely to be from socially deprived and poorer groups, have low levels of education, less land and more likely to be engaged in casual work. They are also more likely to be of prime working age (two-thirds are15-29 years old) and are predominantly male" (Srivastava 2011).

where $i$ indexes individual, $j$ indexes district and $t$ indexes subround-year while $\mathbf{X}_{ijt}$ represents individual (age, gender) and household-level controls (caste, monthly per capita expenditure). The sampling weights provided in the surveys are used to weight all variables after adjusting for sample restrictions. A linear time trend is included to control for secular changes in labor market outcomes across the sample. District fixed effects, which account for time-invariant characteristics are also included. The error term $\varepsilon_{ijt}$ captures the individual level heterogeneity in variable costs. Since shock to exposure intensity is at the district level, the errors within a district may be correlated. To account for this correlation, standard errors are clustered at the district level for all reported results.

### *5.2 Exposure Intensity as Simple Count*

The second set of estimated specifications is nearly identical to the first, except now the measure of exposure intensity, $ProgramNeighbors_j$, is a simple count of number of contiguous NREGS neighbors. Although the count-based measure does not penalize larger districts like the ratio-based measure, it does provide us with the conceptual benefit of easily interpretable estimates which can be understood as the marginal effect of additional neighbors. In the post-shock quarters, this measure takes on values between 0 and 8, i.e. the maximum number of NREGS neighbors in this sample. While the results for this class of specifications in (3) are potentially more sensitive to large districts than the ratio-based measure, they serve as a robustness check for the ratio-based measure of exposure intensity, particularly if the two sets of estimates differ substantially and are inconsistent with each other.

$$Y_{ijt} = \alpha + \beta_1 Post_{jt} + \beta_2 ProgramNeighbors_j + \beta_3 Post_{jt} * ProgramNeighbors_j + \beta_4 Trend_t + \beta_5 DistrictFE_j + \mathbf{\Theta X}_{ijt} + \varepsilon_{ijt} \quad (3)$$

where $i$ indexes household, $j$ indexes district and $t$ indexes subround-year. $\mathbf{X}_{ijt}$ represents household-level controls.

*5.3 Exposure as Binary Indicator*

In the third set of estimated specifications, the measure of exposure intensity is an indicator for whether the non-program district had at least one contiguous NREGS neighbor, a difference-in-difference approach where "treatment" is having a program neighbor. In other words, this "treatment effect" measures the average difference in potential outcomes for exposed versus unexposed non-program districts, although there exists significant heterogeneity within the category of exposed districts. Results for the class of specifications in (4) are nevertheless presented as a benchmark to illustrate differences in estimates when the heterogeneity of intensity is not accounted for.

$$Y_{ijt} = \alpha + \beta_1 Post_{jt} + \beta_2 ProgramNeighbor_j + \beta_3 Post_{jt}*ProgramNeighbor_j + \beta_4 Trend_t + \beta_5 DistrictFE_j + \mathbf{\Theta X}_{ijt} + \varepsilon_{ijt} \quad (4)$$

Irrespective of measure, the main source of potential bias in employing this econometric strategy is a violation of the identifying assumption that district level exposure intensity is quasi-random across pre and post-NREGS quarters, conditional on quarter and district fixed effects, individual and household-level controls. If exposure intensity is correlated with differential district-level trends in outcomes, then the estimates of spillover impacts on non-program districts are likely be biased. Here, it must be pointed out that while district selection of districts in each phase was non-random, it was neither predictable nor transparent, making it unlikely for districts to influence program assignment. A vague selection criterion of "backward districts first" was announced *ex-post*, after the program had already been partially implemented. The definition of "backward" was based on a two-step algorithm (the explicit criteria of which are still unknown) utilizing pre-determined characteristics like the proportion of India's poor resident in a state and the development index of districts within a state, the latter first published in a 2003 Planning Commission Report. Since three new states were created after the data was collected for this report, these indicators were updated in 2009 (Zimmerman 2013). Given that districts were unlikely to influence their own assignment into the program, it is even more improbable that Phase III districts would be willing or able to determine their exposure to the program, i.e. how many of their contiguous neighbors received it. Still, it could be the case that Phase III districts with higher exposure intensity systematically trend differentially from lower exposure intensity districts before receiving NREGS neighbors. Since only two rounds of NSS data are available for this analysis, a placebo test comparing pre-existing differential trends across non-program districts of varying exposure intensities is not possible. While a temporal placebo sample is not available, we do have a spatial placebo sample in the form of program districts for which exposure intensities should have no impact (since they receive NREGS themselves) unless they are correlated with differential trends in outcomes. The results of this test are summarized in section 7.

A second, less serious source of bias is that if Phase III residents correctly anticipated the program rollout and the identified district neighbors which would NREGS in the first two phases, their behavioral response could be to migrate to NREGS districts *before* the shock, thus resulting in diminished or no spillover effects on labor markets being estimated. In fact, a large scale migration of this sort would be a threat to any evaluations of the program's impact, not just for the estimation of spillovers associated with it. The same factors that make it improbable for district administrations to influence their exposure intensity make it harder for individuals to have anticipated which districts would receive the program and migrate to them. In the unlikely case of this concern being valid, our estimates would serve as a lower bound for spillover effects.

## 6. Results

### *6.1 Exposure Intensity as Ratio*

In Tables 3 and 4, the results from estimation of (2) using our preferred measure of district exposure intensity are presented. The dependent variables in the first three columns are weekly time shares for unemployment, work and non-labor force participation days which are mutually exclusive and exhaustive of time endowment. Time allocated to casual labor is also reported in the fourth column because the spillovers from NREGS, given its targeting at low skilled manual labor, are expected to impact the market for casual labor. In Table 3, the results show that the introduction of NREGS did not lead to statistically significant time allocation differences between those non-program districts with only program neighbors and those with zero program neighbors. This absence of statistical significance is consistent with the theoretically ambiguous predicted effects of a positive shock to out of district wages on home district leisure and labor supply at the individual level. In Table 4, we estimate that on average, inflation adjusted daily casual wage increased by 18.8% (significant at 5%) after the introduction of NREGS for non-program districts with only program neighbors, as compared to non-program districts with zero program neighbors. When additional individual and household level controls are added, this increase falls to 17.5%, remaining significant at 5%. This result provides empirical support to the prediction of a rise in non-program district wages due to the spillover effects from NREGS. Although the analysis is not strictly comparable, the basic result is consistent with Robertson (2000) which found that U.S. wage shocks are transmitted faster to Mexico's border cities, relative to its interior cities. It is also in line with the finding that population, employment and earnings spillovers were concentrated in counties that did not experience a resource boom but were in the same state as counties experiencing a boom (Alcott & Keniston 2014).

### *6.2 Exposure Intensity as Simple Count*

In Table 5, we estimate specification (3) using the second measure of exposure intensity and find that the spillover impacts on time allocation are again statistically insignificant. For inflation adjusted daily casual labor wage in Table 6, every additional contiguous NREGS neighbor results in an increase of 2.9% (significant at 10%) in the post-shock period for non-program districts. Adding individual and household controls, this impact decreases to 2.7% but remains significant, thus corroborating the results in Tables 4. Together, the results from these two tables are in line with theoretical predictions as well. Further, they are broadly consistent with estimates using the ratio-based measure, since an average non-program district has about four program neighbors.

### *6.3 Exposure as Binary Indicator*

The pattern of results presented in Tables 3 and 5 is replicated in Table 7 as spillover impacts on time allocation are again statistically insignificant. In Table 8 though, the increase in inflation adjusted daily casual labor wage is positive (8.5%) but insignificant for non-program districts with at least one program neighbor, relative to districts with no program neighbors. This inconsistency with Tables 4 and 6 should be read as a reflection of the coarse measure of exposure being used in this specification.

### *6.4 Placebo Analysis*

In order to ensure that the effects estimated for non-program districts are genuinely spillovers and not being driven by a correlation between unobserved variables and exposure intensity, a placebo analysis is carried out. The class of specifications in (3) is implemented using out-of-sample placebo districts, i.e. Phase I and Phase II districts which received NREGS early. Since these districts received the program during the study period and their home wage presumably rose, *a priori* they should not experience spillover effects from exposure to other contiguous NREGS neighbors. In other words, if one is concerned that an important unobservable is driving the effects attributed to exposure intensity in the non-program sample, we should expect to observe differences in outcomes in the placebo sample as well. In particular, the impacts on wages, for which positive and significant effects have been estimated, would be replicated in the placebo sample.

Looking at the results in Table 10, we observe that changes in inflation adjusted casual labor wages are statistically insignificant in program districts with only program neighbors, relative to program districts with no program neighbors. This result validates the interpretation that the main sample results are not being driven by a spurious correlation across all districts with comparable exposure intensity but spillover effects mediated through exposure intensity in only non-program districts. Table 9 shows that time allocation changes in the placebo sample are not statistically significant like the main sample results, but since predictions from theory are ambiguous in this case, no conclusion can be drawn from this result.

## 7 Conclusion

The results from this analysis demonstrate that exposure to NREGS produced significant spillover effects in the form of higher real casual labor wage in districts where it was not rolled out. These results suggest that the gains from rural workfare programs are not completely accrued by the jurisdictions in which they are implemented. Since workfare programs are often started at the level of sub-national governments - the precursor to NREGS was the state of Maharashtra's Employment Guarantee Scheme, started in early 1970s (Murgai & Ravallion 2005) - there is a strong incentive to free-ride on a neighboring state or district's program given its spillover benefits. Conversely, if a state chooses to discontinue a workfare program, district-level labor markets in neighboring states will also experience the end of spillover benefits. In either case, policymakers need to know the relevant parameters estimated in this paper to assess how their jurisdiction is likely to be impacted by workfare programs in adjoining jurisdictions. Another upshot of these spillovers is that in periods of economic distress when the stabilization benefits workfare programs are most needed, government revenues also decline, thus making them less likely to be enacted given the benefit of waiting for a neighboring jurisdiction to start the program.

In future work, the robustness of these results could be validated by employing more refined measures of exposure intensity like the number of NREGS neighbors within a predefined radius or measures which account for the extent of shared boundaries between neighbors; a shortcoming of the intensity measure is that it weights all contiguous neighbors equally regardless of the size of contiguity. This work can also be extended by developing and testing household-level models which incorporate seasonal migration choices into production and consumption decisions.

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**Fig. 1**

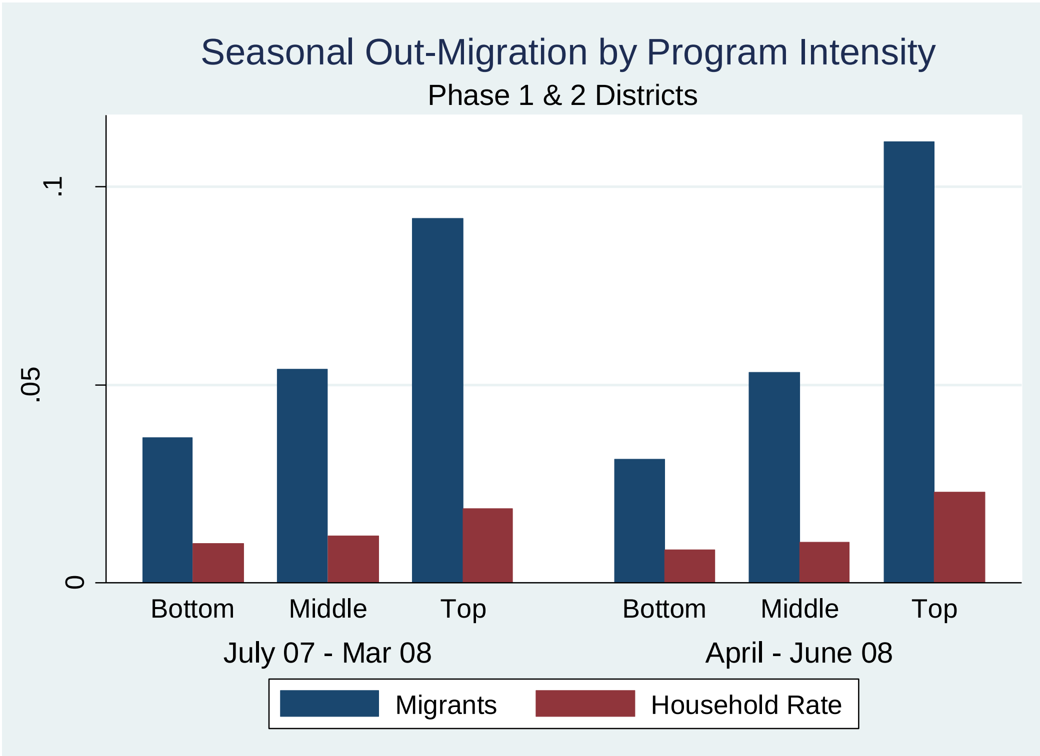

Seasonal Out-Migration by Program Intensity
Phase 1 & 2 Districts
0
.05
.1
Bottom
Middle
Top
Bottom
Middle
Top
July 07 - Mar 08
April - June 08
Migrants
Household Rate

**Fig. 2**

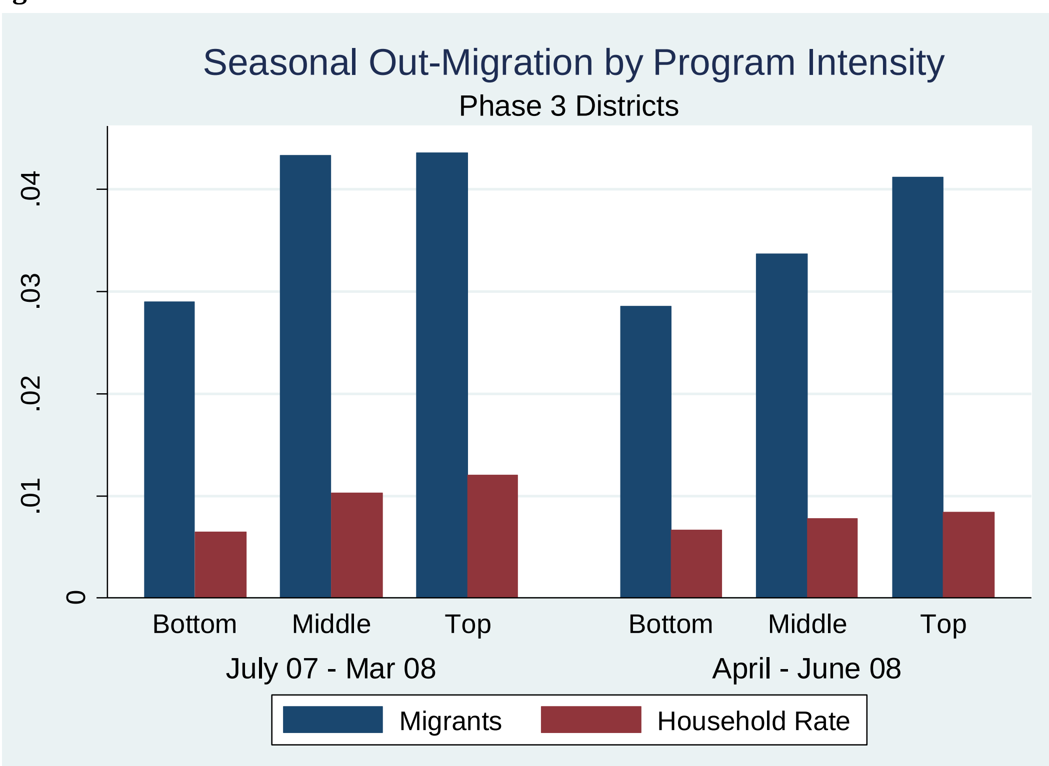
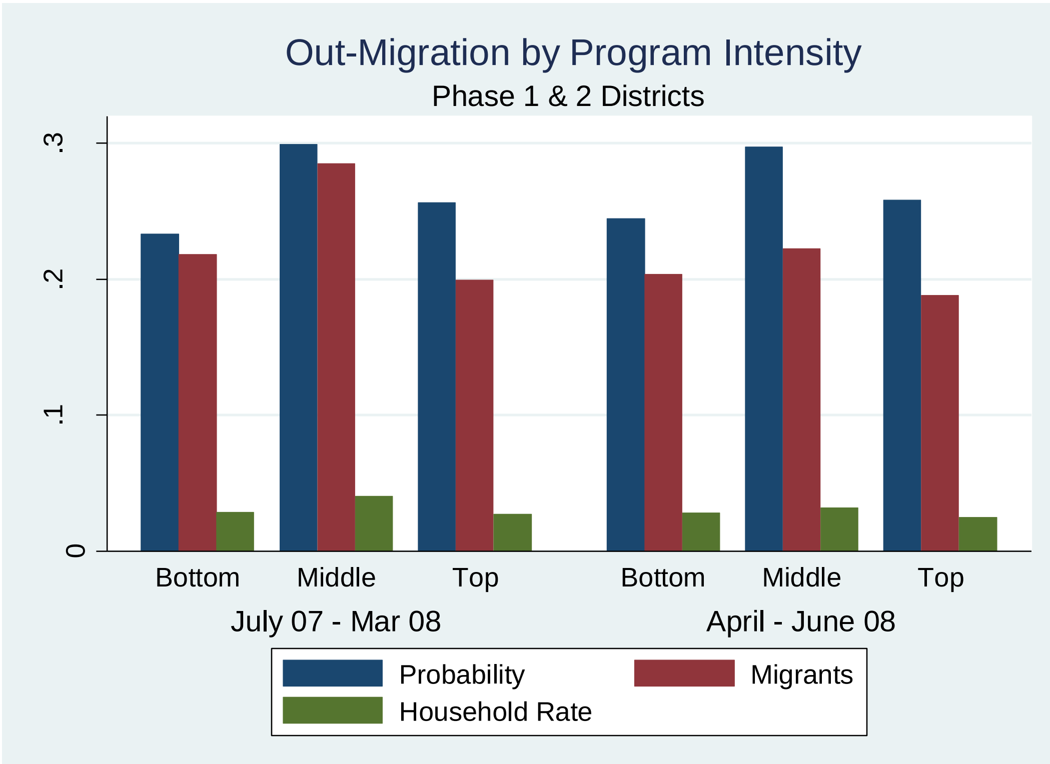
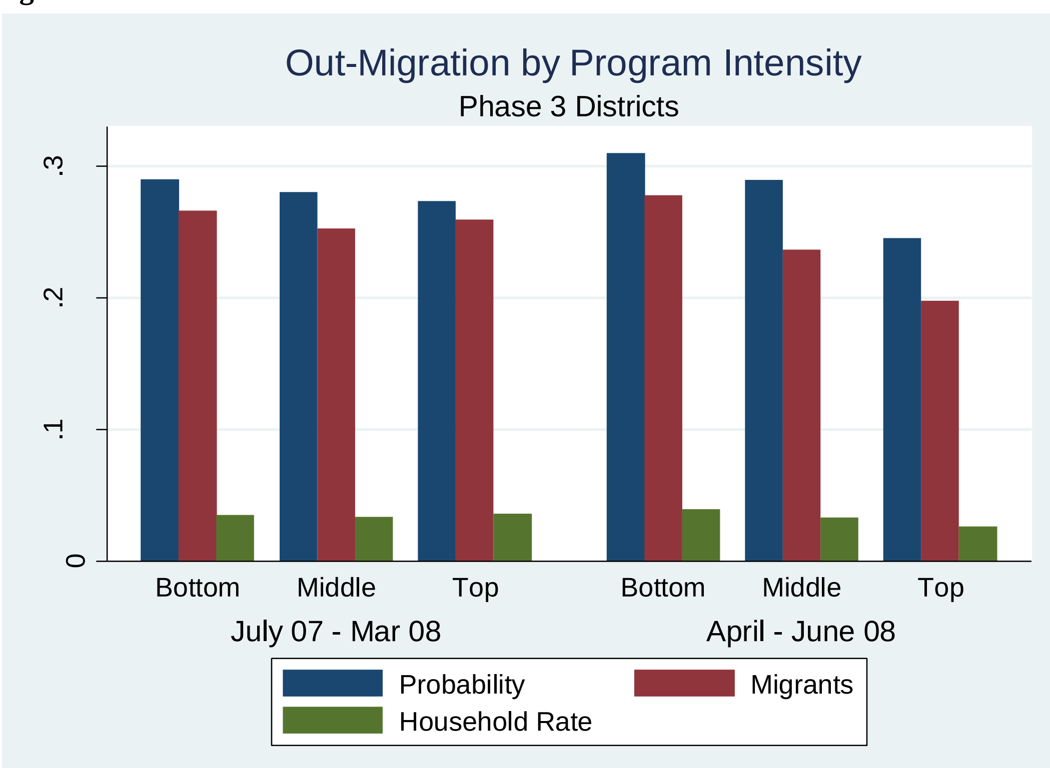

Seasonal Out-Migration by Program Intensity
Phase 3 Districts
0
.01
.02
.03
.04
Bottom
Middle
Top
Bottom
Middle
Top
July 07 - Mar 08
April - June 08
Migrants
Household Rate

Fig. 3



Out-Migration by Program Intensity
Phase 1 & 2 Districts
0
.1
.2
.3
Bottom
Middle
Top
Bottom
Middle
Top
July 07 - Mar 08
April - June 08
Probability
Migrants
Household Rate


Fig. 4



Out-Migration by Program Intensity
Phase 3 Districts
0
.1
.2
.3
Bottom
Middle
Top
Bottom
Middle
Top
July 07 - Mar 08
April - June 08
Probability
Migrants
Household Rate

**Table 3: Spillover Effects on Individual Time Allocation**

| | Unemployment Days | NLFP Days | Work Days | Casual Days |
|---|---|---|---|---|
| Post | -0.080** | 0.299*** | -0.218** | -0.015 |
| | (0.041) | (0.080) | (0.088) | (0.076) |
| Exposure | 0.130*** | 0.324*** | -0.454*** | 1.514*** |
| | (0.034) | (0.079) | (0.084) | (0.079) |
| Post*Exposure | 0.046 | -0.130 | 0.084 | -0.031 |
| | (0.064) | (0.147) | (0.160) | (0.150) |
| Age | -0.008*** | -0.021*** | 0.028*** | -0.003*** |
| | (0.001) | (0.002) | (0.002) | (0.001) |
| Male | 0.301*** | -3.873*** | 3.573*** | 0.750*** |
| | (0.016) | (0.075) | (0.075) | (0.037) |
| MPCE | -0.000 | 0.000 | -0.000 | -0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) |
| Constant | 0.202*** | 5.866*** | 0.932*** | -0.538*** |
| | (0.035) | (0.083) | (0.084) | (0.055) |
| N | 133,077 | 133,077 | 133,077 | 133,077 |

Note: Each column represents results from a separate regression. Exposure is defined as the ratio of contiguous NREGS neighbors to total number of contiguous neighbors. Caste indicators, trend and district fixed effects are included in all regressions. Standard errors are clustered at district level. Estimates are only reported to three decimal points, hence the signs and significance on coefficients of value 0.000.

**Table 4: Spillover Effects on Individual Casual Wage**

| | Log(Real Wage) | Log(Real Wage) |
|---|---|---|
| Post | 0.091 | 0.055 |
| | (0.056) | (0.052) |
| Exposure | -1.520*** | -1.617*** |
| | (0.039) | (0.043) |
| Post*Exposure | 0.188** | 0.176** |
| | (0.088) | (0.084) |
| Age | No | 0.000 |
| | | (0.000) |
| Male | No | 0.428*** |
| | | (0.021) |
| MPCE | No | 0.000*** |
| | | (0.000) |
| Caste | No | Yes |
| Time FE | Yes | Yes |
| District FE | Yes | Yes |
| Constant | 4.765*** | 4.485*** |
| | (0.015) | (0.069) |
| N | 11,323 | 11,323 |

Note: Each column represents results from a separate regression. Exposure is defined as the ratio of contiguous NREGS neighbors to total number of contiguous neighbors. Standard errors are clustered at district level. Estimates are only reported to three decimal points, hence the signs and significance on MPCE coefficients of value 0.000.

**Table 5: Spillover Effects on Individual Time Allocation**

| | Unemployment Days | NLFP Days | Work Days | Casual Days |
|---|---|---|---|---|
| Post | -0.086** | 0.238*** | -0.152* | 0.002 |
| | (0.042) | (0.083) | (0.091) | (0.075) |
| Neighbors | 0.016*** | 0.010 | -0.026* | 0.246*** |
| | (0.006) | (0.013) | (0.015) | (0.013) |
| Post* Neighbors | 0.009 | 0.001 | -0.010 | -0.014 |
| | (0.011) | (0.025) | (0.028) | (0.025) |
| Age | -0.086** | 0.238*** | -0.152* | 0.002 |
| | (0.042) | (0.083) | (0.091) | (0.075) |
| Male | 0.016*** | 0.010 | -0.026* | 0.246*** |
| | (0.006) | (0.013) | (0.015) | (0.013) |
| MPCE | 0.009 | 0.001 | -0.010 | -0.014 |
| | (0.011) | (0.025) | (0.028) | (0.025) |
| Constant | 0.119*** | 3.433*** | 3.449*** | -0.223*** |
| | (0.028) | (0.057) | (0.062) | (0.049) |
| N | 135,062 | 135,062 | 135,062 | 135,062 |

Note: Each column represents results from a separate regression. Exposure is defined as the ratio of contiguous NREGS neighbors to total number of contiguous neighbors. Caste indicators, trend and district fixed effects are included in all regressions. Standard errors are clustered at district level. Estimates are only reported to three decimal points, hence the signs and significance on coefficients of value 0.000.

**Table 6: Spillover Effects on Individual Casual Wage**

| | Log(Real Wage) | Log (Real Wage) |
|---|---|---|
| Post | 0.098* | 0.062 |
| | (0.055) | (0.051) |
| Neighbors | -0.240*** | -0.256*** |
| | (0.007) | (0.007) |
| Post*Neighbors | 0.029* | 0.027* |
| | (0.015) | (0.015) |
| Age | No | 0.000 |
| | | (0.000) |
| Male | No | 0.428*** |
| | | (0.021) |
| MPCE | No | 0.000*** |
| | | (0.000) |
| Caste | No | Yes |
| TimeFE | Yes | Yes |
| DistrictFE | Yes | Yes |
| Constant | 4.952*** | 4.488*** |
| | (0.014) | (0.036) |
| N | 11,584 | 11,584 |

Note: Each column represents results from a separate regression. Exposure is defined as the ratio of contiguous NREGS neighbors to total number of contiguous neighbors. Standard errors are clustered at district level. Estimates are only reported to three decimal points, hence the signs and significance on MPCE coefficients of value 0.000.

**Table 7: Spillover Effects on Individual Time Allocation**

| | Unemployment Days | NLFP Days | Work Days | Casual Days |
|---|---|---|---|---|
| Post | -0.063 | 0.324*** | -0.261*** | -0.083 |
| | (0.046) | (0.092) | (0.093) | (0.107) |
| Neighbor | -0.091*** | -0.011 | 0.102** | -0.809*** |
| | (0.023) | (0.052) | (0.051) | (0.052) |
| Post* Neighbor | 0.000 | -0.093 | 0.093 | 0.069 |
| | (0.042) | (0.099) | (0.096) | (0.101) |
| Age | -0.008*** | -0.021*** | 0.028*** | -0.003*** |
| | (0.001) | (0.002) | (0.002) | (0.001) |
| Male | 0.301*** | -3.874*** | 3.573*** | 0.750*** |
| | (0.016) | (0.075) | (0.075) | (0.037) |
| MPCE | -0.000 | 0.000 | -0.000 | -0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) |
| Constant | 0.315*** | 5.968*** | 0.717*** | 0.570*** |
| | (0.036) | (0.083) | (0.081) | (0.064) |
| N | 135,062 | 135,062 | 135,062 | 135,062 |

Note: Each column represents results from a separate regression. Exposure is defined as the ratio of contiguous NREGS neighbors to total number of contiguous neighbors. Caste indicators, trend and district fixed effects are included in all regressions. Standard errors are clustered at district level. Estimates are only reported to three decimal points, hence the signs and significance on coefficients of value 0.000.

**Table 8: Spillover Effects on Individual Casual Wage**

| | Log(Real Wage) | Log (Real Wage) |
|---|---|---|
| Post | 0.098 | 0.053 |
| | (0.077) | (0.070) |
| Neighbor | 0.437*** | 0.468*** |
| | (0.053) | (0.049) |
| Post*Neighbor | 0.081 | 0.085 |
| | (0.067) | (0.062) |
| Age | No | 0.000 |
| | | (0.000) |
| Male | No | 0.428*** |
| | | (0.021) |
| MPCE | No | 0.000*** |
| | | (0.000) |
| TimeFE | Yes | Yes |
| DistrictFE | Yes | Yes |
| Constant | 4.283*** | 3.781*** |
| | (0.044) | (0.060) |
| N | 11,584 | 11,584 |

Note: Each column represents results from a separate regression. Exposure is defined as the ratio of contiguous NREGS neighbors to total number of contiguous neighbors. Standard errors are clustered at district level. Estimates are only reported to three decimal points, hence the signs and significance on MPCE coefficients of value 0.000.

**Table 9: Spillover Effects on Individual Time Allocation (Placebo Districts)**

| | Unemployment Days | NLFP Days | Work Days | Casual Days |
|---|---|---|---|---|
| Post | -0.003 | 0.348*** | -0.345*** | 0.100 |
| | (0.057) | (0.104) | (0.121) | (0.114) |
| Exposure | 0.215*** | 1.511*** | -1.727*** | -0.225** |
| | (0.041) | (0.089) | (0.100) | (0.095) |
| Post*Exposure | 0.044 | -0.087 | 0.043 | 0.026 |
| | (0.055) | (0.123) | (0.140) | (0.130) |
| Age | -0.008*** | -0.022*** | 0.031*** | -0.005*** |
| | (0.000) | (0.002) | (0.001) | (0.001) |
| Male | 0.321*** | -3.915*** | 3.593*** | 0.813*** |
| | (0.018) | (0.082) | (0.075) | (0.045) |
| MPCE | -0.000*** | 0.000*** | -0.000*** | -0.000*** |
| | (0.000) | (0.000) | (0.000) | (0.000) |
| Constant | 0.433*** | 4.550*** | 2.017*** | 1.268*** |
| | (0.044) | (0.107) | (0.104) | (0.116) |
| N | 171,695 | 171,695 | 171,695 | 171,695 |

Note: Each column represents results from a separate regression. Exposure is defined as the ratio of contiguous NREGS neighbors to total number of contiguous neighbors. Caste indicators, trend and district fixed effects are included in all regressions. Standard errors are clustered at district level. Estimates are only reported to three decimal points, hence the signs and significance on coefficients of value 0.000.

**Table 10: Spillover Effects on Individual Casual Wage (Placebo Districts)**

| | Log(Real Wage) | Log (Real Wage) |
|---|---|---|
| Post | 0.207*** | 0.155*** |
| | (0.049) | (0.046) |
| Exposure | -0.628*** | -0.629*** |
| | (0.057) | (0.058) |
| Post*Exposure | 0.067 | 0.080 |
| | (0.057) | (0.053) |
| Age | No | 0.001*** |
| | | (0.000) |
| Male | No | 0.356*** |
| | | (0.017) |
| MPCE | No | 0.000*** |
| | | (0.000) |
| Caste | No | Yes |
| TimeFE | Yes | Yes |
| DistrictFE | Yes | Yes |
| Constant | 4.196*** | 3.775*** |
| | (0.045) | (0.059) |
| N | 17,642 | 17,642 |

Note: Each column represents results from a separate regression. Exposure is defined as the ratio of contiguous NREGS neighbors to total number of contiguous neighbors. Standard errors are clustered at district level. Estimates are only reported to three decimal points, hence the signs and significance on MPCE coefficients of value 0.000.