# SPILLOVER EFFECTS OF PUBLIC WORKS ON LABOR MARKETS: EVIDENCE FROM NATIONAL RURAL EMPLOYMENT GUARANTEE SCHEME, INDIA

By

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# A DISSERTATION

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#### ABSTRACT

# SPILLOVER EFFECTS OF PUBLIC WORKS ON LABOR MARKETS: EVIDENCE FROM NATIONAL RURAL EMPLOYMENT GUARANTEE SCHEME, INDIA

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Public works guaranteeing work at pre-determined wages are intended to provide security to the seasonally unemployed. These programs, also referred to as "cash-for-work" or workfare, are an increasingly used feature of labor market policy for developing countries. Long durations of guaranteed employment at close to, or above the prevailing market wage are likely to put upward pressure on the local market wage rate through two channels - higher competition for casual labor and increased enforcement of minimum wages (Berg et al. 2012, Subbarao 2003). When implemented non-uniformly across proximate and interlinked labor markets, these programs are also likely to generate spillovers from assigned to unassigned areas. Accounting for these spillovers is necessary and indeed, critical for a comprehensive cost-benefit analysis of large-scale public works programs, increasingly being viewed as anti-poverty schemes.

Impact evaluations of the world's largest public works program, India's National Rural Employment Guarantee Scheme (NREGS), have estimated a variety of labor market effects (Azam 2012, Berg et al. 2012, Imbert and Papp 2015), but this literature is agnostic about general equilibrium spillovers generated by the program, potentially biasing these estimates downwards. This dissertation first tests for the presence of general equilibrium spillovers generated by NREGS in neighboring but unassigned labor markets, exploiting the plausibly exogenous spatial and temporal variation in exposure induced by the program's staggered rollout. Next, it presents evidence for the theory that changes in short-distance, seasonal migration to contiguous program areas is the mechanism generating cross-district spillovers in exposed areas. Lastly, this study presents treatment effect estimates of the program's rural, urban and overall impact, after accounting for cross-district spillovers.

This analysis demonstrates that accounting for the downward bias introduced by general equilibrium spillovers results in substantially larger treatment effect estimates. In fact this study's spillover-robust point estimate of the program's effect on rural unskilled wages is nearly three times previous estimates (Imbert and Papp 2015, Berg et al. 2012). Further, the hypothesis of wage spillovers across districts being as large as direct treatment effects cannot be rejected, thereby implying that nearby exposed but unassigned districts experience the same magnitude of effects as assigned districts. Since public works programs are often started by subnational governments which do not completely accrue the gains from them, these wage spillovers show the existence of a strong incentive to free-ride on a neighboring jurisdictions. Another important takeaway from these findings is that given finite resources, a strategic selection of highly connected labor markets could widen the geographic scope of spillovers from public works programs, thus implying cost-saving for the implementing government. While *post-facto* accounting for general equilibrium spillovers is necessary for choosing among a menu of policy alternatives to public works, the parameters discussed in this study are critical inputs that also enable: i) policymakers to know how their jurisdiction is likely to be affected by introduction and discontinuation of public works programs in neighboring jurisdictions, and ii) a more costeffective design of public works by strategic selection of eligible areas.

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## **CHAPTER ONE**

#### **INTRODUCTION**

# 1.1 Introduction

Public works programs guaranteeing work at a pre-determined wage are intended to provide security to the unemployed and underemployed in the short-term. These public works programs (also referred to as "cash-for-work" or workfare) are an increasingly used feature of labor market policy for developing countries. Between 2007 and 2009, many countries started public works programs in response to the food, fuel and finance crises, with 24 being supported by the World Bank (Zimmerman 2013). Recent examples include flagship programs in Argentina, Ethiopia, and India, among others (Subbarao et al. 2012). Long durations of guaranteed employment at close to, or above the prevailing market wage are likely to put upward pressure on the local market wage rate through two channels. First, public works increase the demand for casual labor, in turn increasing the competition for casual labor. Second, by providing minimum statutory wages, public works increase the pressure on the agricultural sector to pay the minimum wages (Berg et al. 2012, Subbarao 2003).

Since large-scale public works programs raise market wages, it follows that they could widen local wage differentials if implemented non-uniformly across proximate labor markets. In labor markets linked by migration, these local wage differentials should equalize at equilibrium with corresponding changes in aggregate employment, thus leading to spillovers from assigned to unassigned areas. This type of cross-group spillover effect (also referred to as "between" spillovers) presents a challenge for the estimation of causal impacts through natural experiment approaches because it leads to a violation of the Stable Unit Treatment Value Assumption

(SUTVA), and consequently result in biased estimates (Imbens and Rubin 2009). Accounting for cross-group spillovers is necessary for policy evaluation and is particularly important given the need for cost-benefit analyses of large-scale public works programs, increasingly being used as anti-poverty schemes to provide higher and more stable incomes to the poor during economic shocks. Policy decisions about continuing, expanding, or restricting public works programs depend on a precise estimation of their impacts, which could be understated in the presence of spillovers to non-participants (Murgai and Ravallion 2005).

This dissertation first tests for the presence of general equilibrium cross-group spillovers generated by the National Rural Employment Guarantee Scheme (NREGS) in the labor markets of unassigned areas. Next, it presents evidence for the theoretical construct that short-distance, seasonal migration is the mechanism generating cross-district spillovers. Lastly, we present intent to treat (ITT), indirect treatment effects (ITE) and total intent to treat (TITT) estimates of the program after accounting for cross-district spillovers.

### **1.2** Research Gap and Motivation

In an experimental or quasi-experimental study designed for causal inference, identification requires that the SUTVA assumption, i.e. that the treatment status of any unit does not affect the potential outcomes of other units, holds. In a cluster randomized study or grouplevel treatment assignment, two types of spillovers lead to violation of SUTVA. The first type consists of spillovers from eligible to ineligible subjects within units that are assigned treatment. The second type is interference, or cross-group spillovers from treatment assigned to unassigned or control units. In the NREGS context, the initial assignment of the program was non-random. Given this context, previous quasi-experimental approaches to estimating the program's causal

impact on labor market outcomes assume that unassigned districts are the appropriate counterfactual for assigned districts as a group - "parallel paths assumption" for difference in difference (DID) estimators (Imbert and Papp 2015, Azam 2012) - or locally for regression discontinuity design (RDD) estimators (Zimmerman 2013, Bhargava 2014).

The aforementioned empirical approaches ignore SUTVA violations of both types. Firstly, by restricting analysis to only rural areas, this strategy ignores urban areas within assigned and unassigned districts on the grounds that urban households were ineligible for the program. While attempting to estimate ITT effects, this partial equilibrium approach discounts possible ITE in urban labor markets despite belonging to the unit of assignment and thus, likely to be affected by changes in rural labor demand and unskilled wages. Although the empirical choice of abstracting from within-district spillovers is justifiable on the grounds that the objective of those exercises was to estimate ITT, the TITT, an overall effect of the program on assigned districts, is also parameter of policy interest given the high likelihood of general equilibrium spillovers. Secondly, the assumption of zero interference is invalidated if changes in the outcomes of a subset of unassigned districts occurred precisely because of exposure to assigned districts, leading to biased causal impacts (Duflo et al. 2007). Both violations lead to incorrect cost-benefit calculations and potentially flawed policy decisions about continuing or discontinuing the program. Given that the unbiased estimation of ITT, ITE and TITT requires the assumption of no interference to hold, this study first focuses on the detection of cross-district spillovers as a test for SUTVA violation of the second type. In fact, one of the major contributions of this study is that it is the first known estimation of general equilibrium spillovers from the world's largest public works program. Second, I demonstrate how various estimates of the program's treatment effects differ after accounting for cross-district spillovers.

In principle, the enforcement of a mandated minimum wage in excess of market wage in nearby areas could have changed rural-urban wage differentials within districts, as well as wage differentials across assigned and unassigned labor markets. This, in turn, would have the first-order effect of changing the migration and commuting behavior of rural residents in assigned areas. Using NSS data from 2004-05, Murgai and Ravallion (2005) showed that 75% of all casual laborers in India, the group targeted by NREGS, worked for less than the state-specific minimum wage, making the program attractive to them<sup>1</sup>. Even in 2009-10, after the program rollout was complete in all Indian districts, two-thirds of agricultural labor days were paid less than the minimum wage for agricultural unskilled labor (Dutta et al. 2014).

Given this wage differential and increased labor demand, two kinds of migration-linked spillovers could affect residents of unassigned districts. First, if the NREGS raised private sector wages in assigned districts, residents of unassigned districts could seasonally migrate or commute to assigned districts if destination wages are higher, even without participating in the program<sup>2</sup>, thus lowering aggregate labor supply and raising wages in their home districts<sup>3</sup>. Second, if NREGS lowered out-migration or commutes from program districts, unassigned

<sup>&</sup>lt;sup>1</sup> In India, the legal enforcement of The Minimum Wages Act 1948 is shared between the central and state governments. Yet, enforcement is weak due to the profusion of minimum wages across states and sectors, poor human resource capacity, and low availability of funds in the state labor departments. There are 45 central government labor regulations (on which states can make further amendments) and in addition, hundreds of state laws (see Debroy (2005) and Anant et al. (2006)). Consequently, there is a gap between the number of available officers and their demand in enforcing these regulations, thus reducing the effectiveness of and pressure on the existing staff (Soundarajan 2013).

 $<sup>^{2}</sup>$  Commuting to a program district in order to work in NREGS 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.

<sup>&</sup>lt;sup>3</sup> 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.

destinations would experience a reduction in labor supply. Together, these amplifying effects could raise wages in the unassigned districts, resulting in wage spillovers for districts considered to be the control group in previous evaluations. The same discussion applies to rural-urban migration patterns, thus generating within-district spillovers. While increased out-migration from urban to rural areas is less likely given that urban wages tend to be higher, reduced in-migration would also lead to urban wage spillovers.

Previous estimations of NREGS's labor market impacts have justified abstracting away from migration spillovers by citing the low fraction of rural, inter-district migrants in the population, but this reasoning is flawed on two counts. First, the relevant statistic for gauging the importance of migration spillovers is the fraction of mobile workers – short-term migrants and inter-district commuters – relative to total number of unskilled casual workers, not the entire population. Given that NREGS is designed to target unskilled labor with its timing, wage and type of work offerings, the size of inter-district labor flows relative to the market for unskilled labor is important. Although the NSS definition of short-term migrant leads to an estimated 6.75%<sup>4</sup> of all casual workers falling under the category, this is likely an underestimate<sup>5</sup>. By other estimates, about 10% of agricultural laborers could be seasonal migrants (Srivastava 2011)<sup>6</sup>. Further, inter-district commuters are unaccounted for in NSS, and there is no other nationally representative survey that records commuting data. Second, while the decline in rural-urban

<sup>&</sup>lt;sup>4</sup> Author's calculation using NSS 64 data.

<sup>&</sup>lt;sup>5</sup> 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). In absolute terms, there were an estimated 15.2 million short-duration out-migrants, of whom 12.9 million (85.1 per cent) were

<sup>&</sup>lt;sup>6</sup> Their salience to labor markets affected by NREGS is also reflected by the fact that in 2007-08, 56.6% of all seasonal migrants reported working in construction (36.2%) and agriculture (20.4%), sectors most likely to compete with the government program for workers.

migration dominates the popular discourse and empirical literature with respect to NREGS's effect on migration in India, inter-state rural-urban migrants accounted for 36.4% of all shortduration migration in 2007-08 (Srivastava 2011). An almost equal amount of seasonal migration occurred across districts in the same state, which suggests that changes in cross-district wage differentials could have had a sizable effect on the labor markets in close proximity to NREGS districts.

It is well-established 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, by themselves, sufficient for equalization of wages (Robertson 2000). Given that the timeframe for this study is relatively short and that the demand for NREGS is seasonal in the agricultural labor market, it is unlikely that capital flows in the form of lumpy, long-term investments in land and equipment would be the driving mechanism for equalization within or across districts. The case for goods trade across neighboring districts is even weaker because of the similarity in relative endowments and the mix of goods produced. It must be highlighted here that in a long-run general equilibrium setting, nominal wage differentials could persist even in the 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. Given the seasonality of NREGS and the relatively short study period of this analysis, this long-run result from the Rosen-Roback framework, the "workhorse of spatial equilibrium analysis" (Glaeser 2001), has limited applicability to this study. Nevertheless, it does necessitate the use of inflation-adjusted wages for empirical analysis.

# **1.3** Objectives of the Study

The four objectives of this dissertation are the following:

- Construct spatial measures for capturing local variation in exposure of unassigned districts to nearby assigned districts.
- Test for the presence of cross-district spillovers i.e. the effect of exposure on wages and time allocation in unassigned districts.
- Present evidence for short-distance, seasonal migration being the mechanism generating cross-district spillovers
- Estimate intent-to-treat and total average treatment effects, after accounting for crossdistrict spillovers

# **1.4** Organization of the Dissertation

The rest of this dissertation is organized as follows. Chapter 2 provides an overview of the employment guarantee scheme and connects the empirical literatures on public works, migration and general equilibrium spillovers. Chapter 3 presents the basic theoretical framework for the analysis and develops predictions for empirical estimation. Chapter 4 describes data sources, construction of outcome variables and the measures of exposure used in this analysis. Chapter 5 discusses the estimation strategies used to compute cross-district spillovers and treatment effects. Chapter 6 presents the results, placebo tests and robustness checks associated with estimation of cross-district spillovers. My most conservative results show that, conditional on not receiving the program, the real wage for casual labor increased by 6.3% in exposed districts relative to unexposed districts. When heterogeneity of exposure intensity within exposed districts is accounted for, I estimate that a 10% increase in exposure intensity leads to a 1 % rise

in casual wage and an increase in weekly labor force participation. It also presents preliminary evidence of short-distance, seasonal migration being the mechanism generating cross-district spillovers. Chapter 7 presents ITT, ITE and TITT estimates after accounting for bias due to cross-district spillovers and finds that spillover-robust point estimates are larger for these parameters. Chapter 8 offers concluding remarks.

#### **CHAPTER TWO**

#### INSTITUTIONAL CONTEXT AND REVIEW OF LITERATURE

# 2.1 Introduction

Rural public works schemes have a long history in India. The Indian government has introduced programs like the Drought-Prone Area Programme in the 1970s, the National Rural Employment Programme (NREP) in 1980, the Rural Landless Employment Guarantee in 1983, the Jawahar Rozgar Yojana (JRY) in 1989, and the Sampoorna Grameen Rozgar Yojana in 2001 (Berg et al. 2012). The state government of Maharashtra also introduced its own long-running Employment Guarantee Scheme in the mid-1970s, which has been extensively studied (Basu 1981, Dreze 1990, Gaiha 1997, Ravallion et al. 1993). The key objectives of these programs have been: i) provision of wage employment to unemployed and underemployed landless agricultural laborers during the slack agricultural season, ii) create productive assets in rural areas, and iii) promote decentralized governance by assigning the responsibility of implementation to the Gram Panchayats (GPs), the lowest tier of government (Berg et al. 2012). In terms of design and objectives, NREGS builds on its predecessor programs<sup>7</sup>. In terms of scale, the scheme is the best funded anti-poverty program in India. The program provided 2.27 billion person-days of employment to 53 million households, and its budget accounted for 0.6% of India's GDP in 2010 – 11 (Ministry of Rural Development 2011). Given its association with a legally justiciable right to work, geographic scope and sheer financial scale, NREGS represented

<sup>&</sup>lt;sup>7</sup> Although a nominal goal of NREGS is to generate productive infrastructure, the World Bank (2011) writes, "the objective of asset creation runs a very distant second to the primary objective of employment generation...Field reports of poor asset quality indicate that [the benefit from assets created] is unlikely to have made itself felt just yet." Since this study focuses on the initial implementation years that are referred to by the World Bank, a general increase in productivity of agricultural assets is not a concern.

a structural break in the history of Indian public works programs because of the increased onus of implementation it placed on all levels of bureaucracy and administration.

# 2.2 Institutional Features of National Rural Employment Guarantee Scheme

Enacted as law in September 2005, India's National Rural Employment Guarantee Scheme is the largest public works program in the world and legally guarantees every rural household 100 days of employment at a state-specific minimum wage for casual manual labor<sup>8</sup>. Apart from being the member of a rural household residing at the same location, there are no other restrictions or eligibility requirements associated with this entitlement. Policymakers have assumed that the program is self-targeting, given the nature of work and wage rate offered, which would only attract only the poor. Rolled out non-randomly in 200 of the most "backward"<sup>9</sup> Indian districts in February 2006, the act was gradually extended to 130 districts in April 2007 and to the rest of rural India in April 2008 (Table 2.1). Although the actual assignment mechanism to each phase is unknown beyond the generic description of backward, the government guaranteed each state would receive at least one district in the first phase.

<sup>&</sup>lt;sup>8</sup> In 2009, the central government uncoupled NREGS wages from state-level statutory minimum wages and set an all-India uniform wage of 100 rupees per day, but adjusted for state-specific inflation (Dutta et al 2014). Since this change occurred after the study period in this paper and state-level variation in program wage still exists, I continue to work with the assumption that program wage is mandated at the state level for this analysis.

<sup>&</sup>lt;sup>9</sup> The Planning Commission used district-level data on caste composition, agricultural productivity and agricultural wage rates from the mid-1990s to calculate a "poverty index" and ranking for 447 districts in 17 states (Planning Commission 2003). This index was then combined with state poverty headcounts, which are not publicly available, to allocate early phase districts to states, with each state receiving at least one district in Phase I. Comparing the list of 200 Phase I districts with only the poverty index ranking, it is clear that higher ranked districts of richer states received the program because of the imperative to introduce the program in at least one district of each state, thus leading to a wider geographic spread than warranted by only the index.

Table 2.1.1: Phased Rollout of NREGS and Study Classification of Districts				
Phase	Phase I	Phase II	Phase III	
Rollout date	Feb 2006	April 2007	April 2008	
Number of Districts	200	130	261	
Study Classification	Early	Early	Late	





**Figure 2.2: Early vs Late Districts Classification** 



### 2.2.1 Wage Rates

NREGS wages are set to be either a piece rate or a daily wage rate in each project. In either case, the rates are mandated to be at least equal to the state government prescribed minimum wage, even though it funds only 25% of the cost of material and wages of skilled and semi-skilled workers. The federal government bears the entire cost of wages of unskilled manual workers and the remaining 75% of the cost of material and wages of skilled and semi-skilled workers (Azam 2012, Ministry of Rural Development 2008). In practice, Imbert and Papp (2015) find that piece-

rate payment, the more common mode of payment, translated to daily earnings which were almost always below the state-set wage levels in 2007-08. Despite this seeming discrepancy, the reported earnings for NREGS workers exceeded average daily earnings for casual workers by 12 percent (Imbert and Papp 2015). This study takes the view that program wages were in general higher than market wages and also, close to the state statutory minimum wage, an assumption supported by Azam (2012) and Dutta et al. (2012). As mentioned in Chapter 1, this mandate generates upward pressure on market wages by increasing the aggregate demand for casual labor and by increasing minimum wages enforcement, consequently also raising the reservation wage for unskilled workers.

## 2.2.2 Work Offerings

All adults applying for work under NREGS are entitled to public works employment within 15 days, failing which it is state responsibility to provide unemployment compensation. The work need not be continuously provided and can be distributed in multiple projects through the year. In order to obtain work, adults of a rural household residing within the GP jurisdiction must apply for a job card at the local GP. After verification, the GP is supposed to issue a job card free of cost, within 15 days of application. Applicants have no influence over the choice of project but its location must be provided within 5 kilometers of the applicant's village, failing which an additional 10% of wages should be paid to meet additional transportation and living expenses. It is to be noted that this study's analysis of cross-district spillovers does not assume that workers cross district lines and access the program. In fact, the results presented in chapter 6 flow from general equilibrium effects presumed to follow from migrants being able to work in the private sector labor market of NREGS districts.

The types of projects allowed under NREGS include land development, irrigation related earthworks, soil and water conservation and reforestation. In particular, the work undertaken is short-term, unskilled, manual work like building of roads, digging wells and tanks, construction and repair of embankments, and planting of trees, among others. The jobs provided are similar to private sector casual labor jobs and work provision tends to be concentrated during the dry season, when competing private sector demand for unskilled labor in agriculture is low. In 2009-10, of those who report working in public works in the past week, 45 percent report they usually or sometimes engage in casual labor, while only 0.2 percent report that they usually or sometimes work in a salaried job (Imbert and Papp 2015). The similarity of these public sector jobs and casual labor jobs motivates this study's focus on casual wages in the empirical analysis. No contractors or machinery is allowed in implementation of NREGS works and a 60:40 wage to material ratio is required to be maintained<sup>10</sup>.

## 2.2.3 Attractiveness of NREGS work for Women

There is also a strong gender component to the program's provisions. Firstly, it is mandated that men and women are paid equally and in cash<sup>11</sup>. In their field survey, Khera and Nayak (2009) found that while the average program wage earned by women was up to 80% higher than market wage for agricultural and casual labor, a substantial jump in their earning potential. Consequently, NREGS has been attributed with raising women's wages more than men's, on account the larger differential between program and market wages for women (Azam 2012). Further, at least one third of the NREGS workforce in a village is required to be female and childcare facilities are to be

<sup>&</sup>lt;sup>10</sup> Complete NREGS operational guidelines can be accessed at <u>http://nrega.nic.in/Nrega\_guidelinesEng.pdf</u>

<sup>&</sup>lt;sup>11</sup> In a 2008 survey of NREGS workers in six Hindi speaking states in North India, only 30% of female respondents reported earning cash income from sources other than the program in the past three months. The comparable figure for men was 55% (Khera and Nayak 2009).

provided at worksites when more than five children under the age of six are present<sup>12</sup>. The stipulation to provide work within 5 kilometers of the residence also differentially benefits women who simultaneously provide household labor and less likely to leave villages in search of paid work. Lastly, social norms made NREGS work more attractive for some women because of the embarrassment associated with asking anyone in the village for work (Khera and Nayak 2009). In chapters 6 and 7, this dissertation examines the gender-differentiation of cross-district spillovers as well as treatment effects.

# 2.2.4 Seasonality of Implementation

Many field reports and studies have documented that very little NREGS activity takes place once the monsoon season starts (Niehaus and Sukhtankar 2012, Imbert and Papp 2015). In some cases, while this is driven by demand for the program, in others supply side constraints are at work. While monsoon rains make construction projects difficult, big landowners have also repeatedly complained about labor shortage and demanded NREGS work be banned during the peak agricultural season so they do not compete with the program (Centre for Science and Environment 2008, Institute of Applied Manpower 2007, Khera 2009, Khera and Nayak 2009, NCAER-PIF 2009, Samarthan Centre for Development Support 2007). Imbert and Papp (2015) find that rural adults spent thrice their time on public works during the first six months of the year, as compared to during the last six months, when the monsoon rains had arrived. These findings motivate the season-differentiated analysis of spillovers in chapters 6 and 7 which check for seasonal concentration of effects.

<sup>&</sup>lt;sup>12</sup> Section 5 and Schedule II of NREGA, 2005.

## 2.3 Review of Literature

## 2.3.1 Theoretical Literature on Employment Guarantee Schemes

Theoretical models of the effect of employment guarantee schemes (EGS) on labor market outcomes can be divided into two categories based on their underlying assumptions: i) dual-sector models with perfectly competitive rural labor markets and urban unemployment based on Harris-Todaro (Ravallion 1987, Raghunathan and Fields 2014) and ii) models which incorporate labor market imperfections and market power, thus focusing on the degree to which EGS changes bargaining power and introduces contestability (Basu 2008, Basu et al. 2009, Mukherjee and Sinha 2011). Both sets of models acknowledge that government hiring at above prevailing market wages could lead to rise in equilibrium market wages. Given that EGS objectives also consist of smoothing consumption over the agricultural off-season, models of both varieties have sought to incorporate seasonality as well. In a competitive model, Raghunathan and Fields (2014) outline the intertemporal wage spillovers that reduce peak season wages with a large employment guarantee in the slack season. On the other hand, Basu (2008) combines seasonality with an assumption of labor market imperfections in the form of tied-labor contracts and *productive* EGS to show that increase in peak season casual wage depends on the productivity of tied agricultural workers. Mukherjee and Sinha (2011) also emphasize the relative productivity of assets created by NREGS to be able to answer the question of the scheme's impact on private sector employment and agricultural production.

#### 2.3.2 Empirical Literature on NREGS

The impact of NREGS on labor market outcomes has been subjected to scrutiny by a recent and growing empirical literature. On rural employment outcomes, the program has been attributed with increasing labor force participation (Azam 2012) and crowding out private sector work (Imbert and Papp 2015, Zimmerman 2013). In terms of wage outcomes, higher private sector (Imbert and Papp 2015), and unskilled labor wages (Azam 2012, Berg et al. 2012) have been estimated; Zimmerman (2013) has also found evidence supporting the program's success in serving as a safety net, with take-up increasing after bad rainfall shocks and men moving out of private casual sector into more risky family employment. Further, the slowing down of short-term ruralurban migration has been documented, accompanied with a reduction in urban unemployment (Jacob 2008, Ravi et al. 2012, Imbert and Papp 2014).

The wider literature on NREGS has demonstrated the program's ripple effects – increased use of labor-saving agricultural technology (Bhargava 2014), reduction in school enrollments among older children (Li and Sekhri 2013, Shah and Steinberg 2015), increase in child labor (Shah and Steinberg 2015, Islam and 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 and Deininger 2010). In work focused on implementation, Dutta et al. (2013) have shown that there exists considerable unmet demand for the scheme, complementing the finding that awareness of the program remained low in Bihar three years after its introduction (Ravallion et al. 2013). Focusing on leakage in the program, Niehaus and Sukhtankar (2012) find empirical support for the theory that "golden goose" effects reduce theft from piece-rate projects.

# 2.3.3 Literature on Spillovers

The growing empirical literature on NREGS has frequently exploited its staggered rollout<sup>13</sup> by employing quasi-experimental methods like difference-in-difference (Azam 2012, Berg et al. 2012, Imbert and Papp 2015, Islam and Sivasankaran 2014, Li and Sekhri 2013, Liu and Deininger

<sup>&</sup>lt;sup>13</sup> 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).

2010, Ravi et al. 2012) and regression discontinuity (Bhargava 2014, Zimmerman 2013) to identify its impacts on labor market outcomes. However, these studies are agnostic about the sign and magnitude of spillovers from assigned to unassigned districts, reflecting the general paucity of quasi-experimental studies that test for "between" spillovers (Bayliss and Ham 2015), a problem which exists in the experimental literature as well<sup>14</sup>. Multiple randomized experiment based studies have estimated spillovers from treated peers to ineligible individuals or households within treated units; examples include deworming externalities (Miguel and Kremer 2004), cash transfers effects of PROGRESA on ineligible households (Angelucci and De Maro 2015, Angelucci and De Giorgi 2009), information spillovers (Oster and Thornton 2012, Miller and Mobarak 2013), and general equilibrium effects of rainfall insurance (Mobarak and Rosenzweig 2013). The corresponding estimations of between spillovers in experimental designs are limited to cross-school deworming externalities (Miguel and Kremer 2004) and cross-village spillovers of PROGRESA on school participation (Bobba and Gignoux 2014).

This dissertation examines the labor market spillovers from assigned to unassigned districts by exploiting the plausibly exogenous variation in local exposure to program neighbors. It then demonstrates how accounting for cross-district spillovers changes the intent-to-treat and total average treatment effect estimates. To the best of my knowledge, the only other similar attempt in a quasi-experimental setup is Clarke (2015)'s cross-municipality spillover-robust estimation of the teenage-pregnancy reducing effects of contraceptive reforms in Latin America. Given that crossdistrict migration is presumed to be the underlying mechanism driving between spillovers, this approach is also similar in spirit to McKinnish (2005), which studied welfare migration in border counties of U.S. states with large cross-border benefit differentials for Aid to Families with

<sup>&</sup>lt;sup>14</sup> In the theoretical literature, Fields and Raghunathan (2014) have modeled the effect of NREGS on inter-temporal productivity spillovers as part of a two-period seasonal agriculture market.

Dependent Children (AFDC). The study found that border counties in the high-benefit state experienced higher program participation and expenditures relative to interior counties. It is also related to the estimation of spatial spillovers from natural resource booms to counties not experiencing booms (Allcott and Keniston 2014) and the effects of civil wars on economic outcomes in neighboring countries (Murdoch and Sandler 2002).

#### **CHAPTER THREE**

#### THEORETICAL FRAMEWORK

# 3.1 Introduction

In the theoretical models of employment guarantee schemes surveyed in chapter 2, while the direct impacts on affected labor markets was focused on, the question of spillovers to nearby areas has not been satisfactorily addressed. This dissertation is concerned with cross-district and rural-urban spillovers within districts, and both types of spillovers are, qualitatively, general equilibrium processes due to labor flows induced by wage differentials. Although in principle dual sector models are a good theoretical framework, capturing general equilibrium spillovers, in practice that is not the case. Extensions of the Harris-Todaro model with public works (Ravallion 1987) impose fixity of urban wage and combined size of labor force, thus forcing labor market impacts in geographically distinct urban areas to be restricted to reduced urban unemployment. Given empirical work showing that NREGS raised urban wages (Ravi et al. 2012, Imbert and Papp 2014), these models' assumptions are overly restrictive. "Surplus labor" models tracing their lineage to Lewis (1954) assume that rural wages are unaffected unless a large enough employment guarantee absorbs the surplus labor of low productivity households, potentially resulting in reverse migration and higher urban wage as well (Mukherjee and Sinha 2011). While the absence of a rural wage impact is inconsistent with previous results (Imbert and Papp 2015, Berg et al. 2012), these models do allow for the possibility of higher rural and urban wages as a consequence of a large enough employment guarantee. This study utilizes a theoretical framework which does not impose assumptions of fixed urban wage, labor force, or size of employment guarantee. Further, it is motivated as a model for unassigned areas and areas eligible for assignment. This classification is general enough that it holds for cross-district spillovers as well as rural-urban spillovers within districts in the NREGS context.

In the theoretical framework employed for this study, I set up a utility maximization model where individuals choose where to work based on information about market wages at origin and nearby destination locations. All labor markets are competitive, thus ruling out the contestability effect of EGS in imperfect competition models which posit market power of employers. Wages and employment in each market are determined by labor flows across them. The introduction of a large-scale EGS raises market wages and changes the labor flows in both assigned and unassigned areas, consequently leading to general equilibrium spillovers. The same conclusions apply when considering unassigned locations to be urban areas within assigned districts or unassigned districts. This framework explicitly models the decision to work at home or destination,

#### 3.2 Model

This section presents a simple model motivating the optimization problem faced by an individual in an origin district with an outside option of seasonal migration or commuting to a neighboring destination. Since the results can be extended to the case with many destinations without loss of generality, this model is presented to frame the labor movement mechanism driving the results. 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  $l_i$ , work at origin  $(L_i^{O})$  and work at destination  $(L_i^{D})$ . The origin district wage is  $w^{O}$ , while both migrants and commuters earn  $w^{D}$  at the destination district. Work outside the home district is associated with an additional variable cost  $v_i$  which is heterogeneous across individuals. While transportation cost is an example of a variable cost for commuters, it is fixed for seasonal migrants. Meanwhile, additional rent at the destination is an example of a variable cost for seasonal migrants but not 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 she faces depends on which wage rate is higher – the origin wage ( $w^{O}$ ) or the net destination wage ( $w_{I}^{D}$ ). The problem is symmetric for residents of destination districts, except the cost associated with working outside the district is  $u_{i}$  and net origin wage  $w_{I}^{O}$ . Individuals also have non-labor income  $y_{i}$ , which can be thought of in this setting as profits from agricultural production. It is worth noting that the production function  $f(D_{i})$  only allows the use of labor input  $D_{i}$  thus ruling out capital flows across labor markets by assumption. Hence, individuals choose consumption and leisure to solve:

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

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

$$W_i = \max\left\{w^0, w_i^D\right\} \tag{3}$$

$$W_j = \max\left\{w^{\mathcal{D}}, w_j^{\mathcal{O}}\right\} \tag{3'}$$

$$y_i = \Pi_i = f(D_i) - w^0 D_i \tag{4}$$

Solving the first order conditions for an individual resident at the origin, the standard demand functions for leisure and consumption are given by:

$$l^* = l^* (W_i, y_i + W_i T)$$
(5)

$$c^* = c^* (W_i, y_i + W_i T)$$
(6)

In this model, conditional on labor force participation, it is optimal for an individual to either work in the home district or outside-depending on the marginal wage rate-but not both. For a given { $w^0$ ,  $w^p$ } pair and wage differential ( $w^0 - w_i^p$ ), origin residents with low variable costs work at the destination and are henceforth termed "leavers." On the other hand, individuals with high variable costs optimally allocate their labor to the origin, and are hereafter referred to as "stayers." It is noteworthy that stayers also comprise individuals who optimally choose to not participate in the labor force, given their preferences and reservation wage. Symmetrically, the wage differential is  $(w^D - w_j^O)$  for all destination residents. Let N be the population at the origin and R be the number of stayers. Similarly, let K be the population at the origin and S be the number of stayers. The aggregate labor supply at the origin is the sum of the individual-level labor supply of R stayers and labor inflow from destination  $(K - S)L_D$ :

$$L_{S}^{O} = \sum_{i}^{R} L_{i}^{O^{*}}(w^{O}, y_{i} + w^{O}T) + (K - S)L_{D}$$
(7)

while the aggregate labor supply at the destination is the sum of individual-level labor supply of S stayers and labor inflow from origin  $(N - R)L_O$ :

$$L_{S}^{D} = \sum_{i}^{S} L_{j}^{D*} (w^{D}, y_{j} + w^{D}T) + (N - R)L_{O}$$
(8)

Since aggregate labor demand equals aggregate labor supply at both the origin  $(L_D^0 = \sum_j N D_i^* (w^0))$ ) and destination  $(L_D^D = \sum_j K D_j^* (w^D))$ , the equilibrium conditions before the introduction of NREGS can be written as:

$$\sum_{i} L_{i}^{O} (w^{O}, y_{i} + w^{O} T) + (K - S) L_{O} = \sum_{i} D_{i}^{*} (w^{O})$$
(9)

$$\sum_{i} L_{j}^{D*}(w^{D}, y_{i} + w^{D}T) + (N - R)L_{D} = \sum_{j} L_{D}^{*}(w^{D})$$
(10)

where origin wage  $w^0$  and destination wage  $w^0$  clear their respective labor markets. At equilibrium,  $(w^0 \ge w_i^0)$  holds for the *R* stayers at origin. Similarly,  $(w^0 \ge w_i^0)$  holds for the *S* stayers in destination districts. Without additional assumptions, the relationship between market level wages at the origin and those at the destination is ambiguous.

#### 3.2.1 Introduction of NREGS

In this setup, it can be assumed, without loss of generality, that only destination districts are eligible for NREGS assignment. For the subset of destinations that receive the program, the guaranteed employment  $G(w^{0})$  at above-market wage<sup>15</sup> shifts local aggregate labor demand outwards.

$$L_D{}^D = \sum_{j} {}^N D_j^* (w^D) + G(w^G)$$
  
s.t.  $w^G > w^D$ 

For equation (10) to continue to hold, labor supply must increase. This can happen through three channels: i) reduced outflow of leavers (S increases), ii) increased work by existing stayers, and iii) increased inflow of workers from origin, i.e., (N - R) increases. Regardless of channel(s), destination wage  $w^{\rho}$  and aggregate employment rise at equilibrium. This is not surprising because an employment guarantee at an above-market wage rate is likely to raise market wage as well as draw marginal workers, whose reservation wage was previously higher than the prevailing wage, to the labor force. Notably, these workers could also come from outside the assigned destinations, discussed next.

The rising destination wage  $w^D$  simultaneously changes the labor allocation decision of residents at origins proximate to assigned destinations. As the wage differential ( $w_i^D - w^O$ ) widens,

<sup>&</sup>lt;sup>15</sup> This is supported by empirical evidence on the minimum wage guaranteed by the program exceeding the prevailing market wage for casual labor in fifteen of the eighteen states in India in 2007 - 08 (Azam 2012).

previously marginal stayers would choose to allocate their labor to a nearby assigned destination, unless  $v_i$  is too high for *all* stayers. The amplifying effects of reduced inflow (K – S decreases) and increased outflow of labor at the origin (R decreases) lead to an inward shift in aggregate labor supply ( $L_S^O$ ), which continues until  $w^O$  rises sufficiently and equalizes the wage differential for remaining stayers (eq. 7). The rising origin wage could induce the remaining stayers to either supply more labor or buy more leisure at the individual-level, with this ambiguity being the result of offsetting income and substitution effects. It is noteworthy here that if the destination remains unassigned, the labor market equilibrium characterized by equations (9) and (10) remains unchanged. In other words,  $w^O$  remains unchanged, and the origin experiences no spillovers to wage and aggregate employment if it is not "exposed" to an assigned destination.

# 3.2.2 Exposure Intensity and Unassigned Labor Markets

I can now extend the model outlined above to multiple destinations to allow greater variation in exposure, as measured by the number of assigned neighbors for origin districts. I also assume that for a given  $w^{D}$ , the *effective* out of district wage  $w_{E}^{D}$  for origin residents is a monotonically increasing function of exposure intensity *E*, a measure of linkages between origin and destination labor markets where the program is implemented.

$$W_E^D = g(W^D, E), g_E(W^D, E) \ge 0$$

It is axiomatic that as the choice set of assigned destinations expands for origin districts, the highest  $w^{D}$  offered outside the district is non-decreasing. It is noteworthy that this assumption might not hold in practice if a subset of neighboring assigned destinations are subject to policies designed to stop the movement of labor, i.e., ones that keep labor markets segmented, thus delinking the

relationship between the number of assigned neighbors and the maximum market wage offered outside a non-program district. 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). Since the introduction of NREGS was not conditioned on any mobility restrictions for non-participants from neighboring areas, there is little *a priori* evidence of market segmentation and thus minimal cost to making this assumption. Given the assumption that the new wage differential ( $w_E^D - (w^O - v_i)$ ) is an increasing function of exposure intensity, the following predictions are generated from this model:

Prediction 1: Given a program wage higher than the destination wage ( $w^G > w^D$ ), wage in unassigned districts ( $w^O$ ) is increasing in exposure intensity *E*.

*Prediction 2:* Given a program wage higher than the destination wage ( $w^G > w^D$ ), aggregate employment in unassigned districts is decreasing in exposure intensity *E*.

*Prediction 3:* Given a program wage higher than the destination wage ( $w^G > w^D$ ), individual-level labor supply for residents of unassigned districts is ambiguously related to exposure intensity *E*.

Although the discussion above assumes that the guarantee offered by NREGS is fully enforced, there is evidence that the employment guarantee offered by NREGS is fuzzy, with extensive rationing of work and significant unmet demand for it in some states (Dutta et al. 2014). In such a scenario, the labor market spillovers to unassigned districts, like private sector labor market impacts in assigned districts, will be decreasing in the level of rationing. Hence, the extent of cross-district spillovers generated by the program is fundamentally an empirical question. Lastly, although the model described here is written to capture general equilibrium spillovers from assigned districts to nearby unassigned districts, the same model can be rewritten (with new notation) to outline the nature of spillovers from assigned *and* eligible rural areas to ineligible urban areas. In the latter case, the variation isdue not to exposure but to treatment assignment. For the sake of brevity, I summarize the predictions for that case below:

*Prediction 1:* Given a program wage higher than the rural wage ( $w^G > w^R$ ), the urban wage  $w^U$  increases with assignment.

*Prediction 2:* Given a program wage higher than the rural wage ( $w^G > w^R$ ), urban employment in decreases with assignment.

*Prediction 3:* Given a program wage higher than the rural wage ( $w^G > w^R$ ), labor supply of urban individuals is ambiguously related to assignment.

### **CHAPTER FOUR**

## **EMPIRICAL MODEL**

# 4.1 Introduction

This chapter presents the empirical models used for the estimation of cross-district spillovers and spillover-robust treatment effects in chapters 6 and 7. Section 4.2 discusses the difference-in-difference approach taken for estimating cross-district spillovers and the plausibility of attendant identifying assumptions. Section 4.3 outlines the parameters of interest for direct, indirect and district-level treatment effects, sample restrictions, as well as specifications implemented to estimate them. It also describes the placebo test carried out in chapter 7 to support the identifying assumptions for spillover-robust treatment effect estimations.

# 4.2. Cross-District Spillovers

## 4.2.1 Empirical Framework

The first type of spillovers estimated in this dissertation is cross-district or "between" spillovers which have also been described as spatial externalities in some group-level randomization studies (Baylis and Ham 2015, Bobba and Ginoux 2014). Given the paucity of experimental and quasi-experimental studies calculating cross-group spillovers, there isn't a widely accepted name for the parameter of interest yet. Clarke (2015) terms this parameter the Average Treatment Effect on Close to Treated (ACT) and I will continue with that usage in this chapter. In Clarke's (2015) formulation, which closely parallels mine, this is a case when treatment is not precisely geographically bounded, i.e. those living in control areas 'close to' treatment areas are able to access treatment. Specifically, individuals 'defy' their treatment status, by travelling or moving to treated areas, or where spillovers from treatment areas are diffused through general

equilibrium processes<sup>16</sup>. In the few group-level randomization studies which compute cross-group spillovers, the exogenous variation in local density of treatment is exploited to generate estimates (Miguel and Kremer 2004, Bobba and Gignoux 2014). This quasi-experimental study takes a similar approach to estimating ACT. Again, following Clarke (2015), this estimator can be defined as:

$$ACT = E[Y_1(i, 1) - Y_0(i, 1) | C(i, 1) = 0]$$
(1)

where C(i, t) indicates if individuals are close to treatment, but not treated. Only one of  $Y_1(i, t)$  or  $Y_0(i, t)$  is observed for a given individual *i* at time *t*. The realized outcome can thus be expressed as:

$$Y(i, t) = Y_0(i, t) \cdot (1 - D(i, t))(1 - C(i, t)) + Y_1(i, t) \cdot D(i, t) + Y_1(i, t) \cdot C(i, t)$$

where, depending on an individual's time varying treatment and close status, either  $Y_0(i, t)$ (untreated) or  $Y_1(i, t)$  (treated or close) is observed. D(i, t) is an indicator for treatment assignment.

Given that early districts were non-randomly assigned NREGS, it is possible that the variation in local density of treatment for unassigned districts, termed exposure in this study, is also not random. However, consistent and unbiased estimates of cross-district spillovers can still be derived if any systematic differences between exposed and unexposed districts are constant over the study period. Specifically, if a difference-in-difference (DID) estimator is used to compute

<sup>&</sup>lt;sup>16</sup> Similar to the measure of exposure used to define proximity in this analysis, Clarke (2015) defines a function for "R(i, t) where: R(i, t) = f(X(i, t)) > 0 if an individual resides close to, but not in, a treatment area. R(i, t) = 0 other wise. X(i, t) is an individual covariate measuring distance to treatment and  $f(\cdot)$  is a positive monotone function. As treatment occurs only in period 1, R(i, 0) = 0 for all i. Similarly, as living in a treatment area itself excludes individuals from living 'close to' the same treament area, R(i, t) = 0 for all i such that D(i, t) = 1."

cross-district spillovers to unassigned districts, the identifying assumption is that in absence of exposure, labor market outcomes of exposed and unexposed districts follow parallel paths. Although this assumption is not directly testable, I present graphical evidence supporting the absence of differential trends in the pre-study period in the next section. Further, pre-exposure balancing tests are conducted on a range of economic and social indicators for the two groups of unassigned districts in chapter 5. Lastly, an extensive set of placebo tests for the pre-study period are discussed in chapter 7.



Figure 4.1: Scheme for Analysis of Cross-District Spillovers

The subset of Indian districts used in this analysis is late assignment districts, as depicted in Figure 4.1. In this empirical approach, I employ a DID estimator to compute the effect of exposure on changes in labor market outcomes of exposed districts, relative to unexposed districts. In other words, cross-district spillovers on wages and time allocations in late districts are estimated by

exploiting the plausibly exogenous shock to variation in exposure to assigned early neighbors. I estimate variations of the following specification:

$$Y_{idt} = \alpha + \beta_1 Post_t + \beta_2 Exposure_d + \beta_3 Post_t * Exposure_d + \beta_4 \delta_t + \beta_5 \mu_d + \Theta X_{idt} + \varepsilon_{idt}$$
(1)

where *i* indexes individual, *d* indexes district, and *t* indexes quarter-year. In specification (1), no assumption is made about whether *Exposured* is binary or continuous; the DID estimator is fairly flexible and only interpretation of the coefficients generated changes with the measure of exposure. Although Figure 4.1 disaggregates late districts into exposed and unexposed, finer disaggregation is also possible. In fact, chapter 5 discusses three alternative measures of exposure (one binary and two continuous) and primary results are reported for all of them in chapter 6. *Postt* is an indicator variable which is 0 for all late districts in the first four quarters predating the program (July 2004 – June 2005) and is 1 in the post-NREGS quarters (July 2007 – March 2008). The parameter of interest is  $\beta$ 3, which estimates the impact of exposure on individual-level time allocation and wage variables. When the measure of exposure is binary, then the estimate can be represented as below:

$$\widetilde{\beta3} = (\overline{Y}_{Post=1} - \overline{Y}_{Post=0}) - (\overline{Y}_{Exposure=1} - \overline{Y}_{Exposure=0})$$

This impact of exposure captures labor market spillovers in exposed districts relative to unexposed districts, conditional on non-assignment. Inflation-adjusted log daily wages for casual labor, salaried labor and time allocations and serve as dependent variables ( $Y_{idt}$ ) in separate regressions and  $X_{idt}$  represents individual, household, and district- level controls. Specifically, individual-level factors like age, education, and gender which are correlated with seasonal

migration<sup>17</sup>, and household-level factors like caste group and land possessed are controlled for. Additionally, the rural population fraction of the district is interacted with *Post<sub>t</sub>* to capture timevarying changes in relative size of a district's rural population. Quarter-year fixed effects ( $\delta_t$ ) are included to control for seasonal and secular changes in labor market outcomes through the study period. Since the exposure variation being exploited in this estimation is at the district-level, district fixed effects ( $\mu_d$ ), which account for pre-existing time-invariant district characteristics, are also included. The rural fraction of the district population is also interacted with the indicator for *Post<sub>t</sub>* to control for temporal change in the number of rural residents. The error term  $\varepsilon_{idt}$  captures individual-level heterogeneity in the variable costs associated with working outside the home district. To account for intra-district correlation of individual-level errors, standard errors are clustered at the district-level for all reported results. All regressions are implemented at the individual-district-quarter level of analysis and sampling weights provided in the surveys are used to weight these estimates.

A limitation of the empirical strategy described in this section is that it is implemented on only the set of late assignment districts, thus abstracting from the program's treatment effects on early assignment districts. In order to disentangle cross-district spillovers from treatment effects in a single specification, I undertake a separate analysis on all districts that is discussed in section 4.3 and the results of which are reported in chapter 7.

<sup>&</sup>lt;sup>17</sup> "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).

# 4.2.2 Plausible Exogeneity of Exposure

The main source of potential bias in employing this econometric strategy is a violation of the parallel paths assumption; i.e. if exposed and unexposed district outcomes trend differentially even in the absence of NREGS, conditional on quarter-year and district fixed effects and individual and household-level controls. If exposure is correlated with differential district-level outcome trends and is actually a proxy for some other unobservable, then estimated cross-district spillovers are likely to be biased. One way to indirectly test for this possibility is to see whether estimating (1) on pre-program data generates similar effects attributable to "exposure". In order to carry out this check formally for all outcome variables, specification (1) is implemented using a placebo sample consisting of two survey rounds (1993 – 94 and 1999 – 2000) conducted before the introduction of NREGS. An expanded set of placebo tests are also conducted by changing the cutoff separating pre and post-exposure quarters. All placebo test results are reported in chapter 6.

Although the binary measure of exposure will be defined in the next chapter, it is useful to graph outcome trends for the groups of exposed and unexposed districts. Figures 4.2 and 4.3 below show the *annual* evolution of real casual wage and work through the placebo (1993 – 2000) and study period (2004 – 2008). The line markers identify the Phase I and Phase II assignments, which cannot be distinguished in estimation given the timing of the surveys used. The first graph provides visual support for parallel wage trends during the pre-study period and a relative increase for exposed districts during the study period. The graphs for work and time spent searching for work provide limited support for parallel trends for parts of, but not the entirety of the pre-study period. Regardless, given that the formal placebo test utilize quarterly observations in chapter 6, this annual trend should not be taken as definitive evidence for non-parallel trends in the pre-study period.

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Figure 4.2: Casual Wage in Placebo and Study Period

Figure 4.3: Work in Placebo and Study Period



Source: NSS 50, 55, 61, 64

Source: NSS 50, 55, 61, 64



Figure 4.4: Search for Employment in Placebo and Study Period

There are two additional threats to exposure being plausibly exogenous. Firstly, if late districts were able to manipulate their exposure to the program, then it could be the case that exposure is correlated with other unobserved variables that differentially affect outcome trends for exposed districts. Here, it must be emphasized that my analysis is restricted to late districts and is thus conditioned on receiving NREGS last. While late district administrators may have tried to manipulate early reception of the program, it is not clear how they would have influenced being exposed to early district neighbors, given that they were unsuccessful. Since the central government's selection of early districts was non-random with economically underdeveloped districts being selected first, the sample of late districts is richer than early districts<sup>18</sup>, but that is

Source: NSS 50, 55, 61, 64

<sup>&</sup>lt;sup>18</sup> Zimmerman (2013) has reconstructed this two-step algorithm using state poverty headcounts from the 2001Census to imperfectly predict assignment for a RD design based impact evaluation of NREGS. Since poverty index and corresponding rank is missing disproportionately for late phase districts, this analysis cannot utilize these ranks as additional controls.

not the comparison being made in my analysis. Unless exposure is systematically correlated with differential outcome trends *within* the sample of late districts, the non-random rollout of the program does not threaten the internal validity of this analysis. As discussed earlier, the balance between exposed and unexposed districts for five of the seven outcomes used in this paper and the absence of pre-existing non-parallel casual wage trends increases my confidence that the two groups of late districts do not experience differential outcome trends, even if they are systematically different from early districts as a group.

A second, less serious source of bias is that if late district residents correctly anticipated the program rollout and the identified early districts which would receive NREGS before their home district, 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 evaluation of the program's impact, not just the estimation of spillovers associated with it. Given that the assignment of early phase districts is imperfectly predicted even using the index made publicly available after the introduction of the program (Zimmerman 2013), it is quite improbable that individuals would have correctly anticipated which districts would receive the program early and migrate to them. Furthermore, recent work has shown that public knowledge about the program remained low in Bihar, one of the poorest states in India, even three years after its initial implementation (Ravallion et al. 2013). In this setting, it is highly unlikely that anticipatory migration by unskilled workers, which requires information about the program and its wage offerings in other districts before implementation, is a relevant concern. In the unlikely case of this concern being valid, my estimates simply serve as a lower bound for spillover effects.

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# Figure 4.5: Conceptual Scheme for Analysis of Direct and Indirect Treatment Effects



Note: Direct Intent-to-Treat Effect (ITT) compares rural areas of assigned and unassigned districts. Indirect Treatment Effect (ITE) compares urban areas of assigned and unassigned districts. Total Intent-to-Treat Effect (TITT) compares assigned and unassigned districts.

# 4.3 Treatment Effects and Cross-district Spillovers

## 4.3.1 Direct Treatment Effects

Previous evaluations of NREGS have focused on causally estimating its intent-to-treat (ITT) effects, i.e. its direct impact on program-eligible rural residents of assigned districts (Imbert and Papp 2015, Berg et al. 2012). The identifying assumptions of ITT estimation are unconfoundedness and SUTVA, i.e. i) the absence of spillovers to ineligibles within treatment units, as well as ii) no interference between treatment and control. Since the proposed estimation of cross-district spillovers in the previous section is a test for detecting interference, this section discusses an estimation strategy for estimating ITT and adjusting it to account for cross-district spillovers. The ITT parameter and DID strategy used in previous empirical work on NREGS is summarized below:

$$ITT = E(Y_1 - Y_0 | T = 1, E = 1) = E(Y | T = 1, E = 1) - E(Y | T = 0, E = 1)$$
(2)

$$Y_{idt} = \alpha + \beta_1 Post_t + \beta_2 Treat_d + \beta_3 Post_t * Treat_d + \beta_4 \delta_t + \beta_5 \mu_d + \Theta X_{idt} + \varepsilon_{idt}$$
(3)

In (3), *Treat<sub>d</sub>* is an indicator for program assignment and  $\beta$ 3 is the parameter of interest. Similar to Clarke (2015), this specification can be augmented to include a fixed effect of an indicator for *Exposure<sub>d</sub>* and an interaction term *Post<sub>t</sub>\*Exposure<sub>d</sub>*. This specification (4) allows for the estimation of cross-district spillovers, with  $\beta$ 3 represents the spillover-robust ITT coefficient  $\beta$ 5 and captures ACT. Since the sample is restricted to only rural areas, the ACT should be interpreted as an estimate for cross-rural spillovers. Notably, the identifying assumptions for this estimator are: i) parallel trends between assigned and unexposed districts, and ii) parallel trends between exposed and unexposed districts.

$$Y_{idt} = \alpha + \beta_1 Post_t + \beta_2 Treat_d + \beta_3 Exposure_d + \beta_4 Post_t * Treat_d + \beta_5 Post_t * Exposure_d + \beta_6 \delta_t + \beta_5 \mu_d + \Theta \mathbf{X}_{idt} + \varepsilon_{idt}$$
(4)

#### 4.3.2 Indirect Treatment Effects

Next, the estimation of within-district spillovers from rural eligibles to urban ineligibles is carried out in chapter 7. In the program evaluation literature, this parameter has been termed the indirect treatment effect (ITE), defined as the average effect of the treatment on ineligibles (Angelucci and De Giogi 2009, Angelucci and Di Mario 2014). Since treatment is at the group-level in these studies, ACT is not equivalent to the familiar Average Treatment Effect on the Untreated (ATU) estimated in individual-level randomization studies. In a context of group-level randomization with targeting within groups, computing ITE involves a comparison of ineligible individuals within treatment units with ineligible individuals within control units (Angelucci and Di Mario 2014):

$$ITE = E(Y_1 - Y_0 | T = 1, E = 0) = E(Y | T = 1, E = 0) - E(Y | T = 0, E = 0)$$
(5)

In this study's quasi-experimental approach, the assigned but ineligible group (T=1,E=0) consists of urban residents of early districts. The chosen comparison group consists of urban residents of late districts, unassigned during the study period (T=0, E=0). While both rural and urban residents of late districts were unassigned and ineligible during this period, rural areas become eligible later and *a priori*, had a positive probability of being selected for early assignment. In contrast, the urban areas had zero probability of being eligible for the program, given its targeting to rural households. Further, given that the outcomes of interest, unskilled wages and employment opportunities, differ substantially across rural and urban locations, the urban outcomes in late districts provide more valid counterfactuals for urban outcomes of early districts. In principle, imposing this sample restriction on the comparison group is similar to defining the common support for a propensity score matching (PSM) estimator's identification. A standard DID estimator is used to compute the unadjusted indirect treatment effects or within-district spillovers of NREGS and the specification employed is:

$$Y_{idt} = \alpha + \beta_1 Post_t + \beta_2 Treat_d + \beta_3 Post_t * Treat_d + \beta_4 \delta_t + \beta_5 \mu_d + \Theta X_{idt} + \varepsilon_{idt}$$
(6)

While the same DID specification is used to calculate ITT and ITE in (3) and (6), the former is restricted to rural areas and the latter to urban. In this case, the  $\beta$ 3 parameter represents crossurban spillovers. Similar to (4), the cross-district spillover-robust estimates of ITE are computed in (7) by augmenting (6), and implementing on an urban sample:

$$\mathbf{Y}_{idt} = \alpha + \beta_1 Post_t + \beta_2 Treat_d + \beta_3 Exposure_d + \beta_4 Post_t^* Treat_d + \beta_5 Post_t^* Exposure_d + \beta_6 \delta_t + \beta_7 \mu_d + \mathbf{\Theta} \mathbf{X}_{idt} + \varepsilon_{idt}$$
(7)

Here, it must be emphasized that detection of non-zero, spillover-robust ITE estimates provides evidence for violation of SUTVA *within* early assignment districts. Further, the spilloverrobust ITT and ITE estimates rely on modified "parallel paths" assumptions of augmented DID, which could be violated in the rural or urban sample. In the next chapter, pre-assignment balancing tests are conducted on a range of economic and social indicators for urban and rural areas in early and late assignment districts.

## 4.3.3 Total Intent-to-Treat Effect

Another parameter of interest in this exercise is the Total Intent-to-Treat Effect (TITT), which is the overall effect of the program at the district level. In a randomized design, this parameter's equivalent is the Total Average Treatment Effect (TATE), a weighted average of direct (ITT) and indirect treatment effects (ITE) (Angelucci and Di Maro 2015). In a quasi-experimental design like this analysis which uses survey data, the analogous approach is to pool rural and urban areas within districts and apply survey weights to a composite DID estimation. If one considers the whole district as the relevant local economy and accounts for cross-district spillovers, this parameter is arguably of greatest interest because it informs policymakers about the aggregate treatment effect of the program. The robustness of these results is checked by implementing the augmented DID specifications which jointly estimates TITT and cross-district spillovers. All specifications are the same as (3) and (4), except the sample now pools rural and urban areas.

# 4.3.4 Placebo Tests for Spillover-Robust Treatment Effects

In order to ensure that the spillover-robust treatment effects are not reflective of preexisting differential trends, a placebo analysis is carried out in chapter 7 to estimate the effects of fake assignment and exposure. It must be noted that this test is setup to detect non-parallel outcome trends due to assignment *and* exposure in the pre-study period, thus helping support the two identifying assumptions required for the augmented DID specification. The augmented DID specification is implemented using two pre-study rounds of data – NSS 50 (July 1993 – June 1994) and NSS 55 (July 1999 – June 2000). In order to make the construction of the pre-study period comparable with the study period, the last sub-round of NSS 55 is dropped. The test is conducted for rural areas, urban areas and pooled areas at the district level. It is possible that the test rejects the null of parallel trends for some but not all of the restrictions. Consequently, the support for identifying assumptions may not necessarily be equal across the spillover-robust ITT, ITE and TITT specifications.

## **CHAPTER FIVE**

# DATA SOURCES AND DESCRIPTION

# 5.1 Introduction

This chapter describes data, construction of key variables and compares pre-program characteristics of the district groupings utilized in the estimation of cross-district spillovers and treatment effect parameters. Section 5.2 discusses the sample restrictions and construction of outcomes variables used for estimations in chapter 6 and 7. Section 5.3 outlines the binary and continuous measures of exposure employed to capture proximity-based linkages between assigned and unassigned districts. Section 5.4 discusses pre-program summary statistics for the groups compared in the cross-district spillovers and treatment effect estimations.

# 5.2 Data

## 5.2.1 Sample Restrictions

In chapter 6 and 7, the primary estimations utilize two sources of data: i) two rounds of employment surveys – NSS 61 (July 2004 – June 2005) and NSS 64 (July 2007 – June 2008) and ii) spatial data on district boundaries based on the Indian Administrative Census (2001). The individual is the primary unit of analysis, and the sample is restricted to adults aged 15 to 59 without tertiary education, in order to closely resemble the demographics of unskilled labor market participants likely to be affected by NREGS<sup>19</sup>. The NSS survey is comprised of four quarterly subrounds designed to coincide with *rabi* and *kharif*, the two growing seasons in Indian agriculture, as well as post-harvest quarters. The study period is restricted to July 2004 – March 2008 by dropping the last sub-round from NSS 64, to ensure that Phase III program assignment (April

<sup>&</sup>lt;sup>19</sup> National Sample Survey Organization (NSSO), the agency which carried out the NSS, defines individuals aged 15 to 59 as the "economically active population" and uses this sample to calculate employment and unemployment rates. This analysis adopts the same convention. Total Intent-to-Treat effects are also differentiated by age in section 7.3 and the sample used then includes 13 and 14 year olds as well.

2008) does not contaminate the working sample. Since households surveyed within districts are uniformly distributed across sub-rounds by design, this restriction does not systematically change the working sample. Together, seven sub-rounds of data drawn from NSS 61 and NSS 64. In order to conduct placebo tests during the pre-study period, NSS 50 (July 1993 – June 1994) and NSS 55 (July 1999 – June 2000) are used.

# 5.2.2 Construction of Outcomes

This analysis utilizes the *current daily status* measure of NSS employment surveys to construct weekly time allocation for each individual in three categories: i) work days (casual labor, salaried work, domestic work, public sector work and self-employment), ii) search days, and iii) labor force participation<sup>20</sup>, an aggregate of the first two categories. I also compute daily wages for individuals who worked as casual laborers since this segment of the labor force is most likely to be directly impacted by spillover from public sector casual labor offered by NREGS. Daily wages for individuals who worked as salaried laborers is also used as an outcome variable to validate this assumption and ensure that this analysis captures spillovers induced by NREGS and not unrelated trends correlated with exposure. Both wages have been inflation-adjusted using state-level, quarterly CPI for agricultural workers.

Here, it must be noted that all time allocation and wage outcomes are observed for residents as the surveys do not track leavers. It is thus possible that the wage and labor outcomes of unassigned individuals who migrate to nearby program neighbors are not observed in the post-

<sup>&</sup>lt;sup>20</sup> 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 three categories and the intensity of that category is summed across the week to get weekly time allocations. There are two other measures of employment also available in the NSS: usual status (based on a recall of a year) and current weekly status (based on recall of previous week). However, since these variables capture only the principal activity of each individual during a given reference period, they cannot shed light on intensive margin changes in time allocation in response to NREGS so they are not used in this analysis.

NREGS period and this attrition is more likely to occur in districts with more program neighbors.

# 5.3. Measures of Exposure to Assigned Districts

The estimation of cross-district spillovers in chapter 6 is restricted to Phase III districts which received NREGS last. Only early districts were assigned the program during the study period. Consequently, nearby late districts were unassigned but exposed to the program by virtue of proximity and labor market linkages to neighboring early districts. The maps shown in Figures 5.1 and 5.2 visually depict the spatial distribution of districts across NREGS phases and the classification of early and late phase districts used in this analysis.

Table 5.1.1. 1155 Survey Rounds & TAREOS Ronout				
Timing of Survey	Before Phase I	After Phase II	After Phase III	
Round	NSS 61	NSS 64 (Sub-round 1-	NSS 64 (Sub-round 4)	
Survey Year	July 2004 – June	July 2007 – March 2008	April 2008 – June	
Number of	NREGS = 0	NREGS = 330	NREGS $= 588$	

Table 5.1.1: NSS Survey Rounds & NREGS Rollout

5.3.1 Exposure as Binary Indicator

In order to capture exposure, the following binary measure is computed for each late district:

$$E_{1} = \begin{cases} 1 \text{ if at least one contiguous neighbor is "Early"} \\ 0 \text{ otherwise} \end{cases}$$

In other words, a late district is considered exposed if it shares a border with one or more early district neighbors and is classified as unexposed if surrounded by late district neighbors. It is noteworthy that in principle any two districts or, for that matter, any two points in space can be considered neighbors, depending on how neighborhood is defined. Given the absence of theory about the geographic scale of labor markets, empirical choices in the literature have largely been driven by data constraints and the objective of the analysis. While spatial data on a finer scale identifying smaller administrative units is available, the NSS data used to construct outcomes in this analysis is only identified up to the district- level, making finer measures of neighborhood

redundant. Additionally, since the objective of this exercise is to capture spillover impacts driven by variation in proximity to NREGS, in otherwise similar labor markets, first-order contiguity is used as the criterion for neighborhood (see Murdoch & Sandler 2002, 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, first-order contiguity enables greater comparability of exposed and unexposed districts similar to and in close proximity to each other. The map in Figure 5.3 of the appendix highlights all districts that are classified as exposed using this measure. In the working sample of 215 late districts used for the main analysis drawn from India's largest states, 80% (173) of the districts are exposed and the remaining 20% (42) districts are categorized as unexposed using this measure.



**Figure 5.1: Exposure in Early and Late Phase Districts** 

Note: The category of unexposed districts showed in this map includes Early districts, for the purpose of explication. The working sample used for analysis is restricted to Late districts.

Since  $E_1$  constrains exposure to being a binary variable and does not capture heterogeneity across exposed districts, the following measure of exposure intensity is also computed for late phase districts.

$$E_2 = \frac{Number \ of \ contiguous \ "Early" \ neighbors}{Total \ number \ of \ contiguous \ neighbors}$$

*E2* takes on fractional values ( $E_2 \in [0, 1]$ ) and increases with every additional contiguous early district, assuming that the total number of contiguous neighbors does not change. This measure gives higher weight to districts with more early neighbors that the binary measure of exposure, but also penalizes large districts which might have more adjacent neighbors by virtue of size, by adjusting the ratio downward. It also enables this analysis to investigate the robustness of spillover effects to an alternative measure of exposure which captures intensity. In the working sample, while late districts have 5.5 districts on average, the mean number of early neighbors is 1.9, with a standard deviation of 1.6. Conditional on exposure to at least one early neighbor, the average exposure intensity is 0.38 and standard deviation is 0.28 (see Table 5.2.1). Given the similarity in moments for the unconditional and conditional distribution, it is safe to infer that there is little bunching of districts at the high exposure intensity end of the distribution.

Table 5.2.1. Exposure and Exposure Intensity – Late Districts				
	Mean	S.D.		
% Exposed	80.5			
% Unexposed	19.5			
Neighbors	5.5	1.6		
Early Neighbors	1.9	1.6		
Exposure Intensity	0.35	0.26		
Exposure Intensity   Exposure==1	0.38	0.28		

 Table 5.2.1: Exposure and Exposure Intensity – Late Districts

## 5.3.3 Exposure Intensity as Population-Weighted Share of Neighbors

As an additional robustness check, I also compute a population-weighted measure of exposure intensity which adjusts  $E_2$  for the rural population residing in each neighbor.

# $E_{3} = \frac{Rural \ population \ in \ contiguous \ "Early" \ neighbors}{Total \ rural \ population \ in \ contiguous \ neighbors}$

The marginal benefit of employing  $E_3$  is that it accounts for the relative population density of assigned neighboring rural labor markets. In other words, it adds value when eligible neighboring rural residents are clustered in a few districts, i.e.  $E_2$  is low but not reflective of the density of exposure. In chapter 6 estimations,  $E_2 \in [0, 100]$ , i.e. it is scaled in percentage points. If rural population in early neighbors is positively correlated with the number of early neighbors, then there is little gain to be had in using this measure.

# 5.4 Pre-program Comparisons

## 5.4.1 Exposed versus Unexposed Late Districts

As discussed in chapter 4, the analysis of cross-district spillovers is restricted to late districts. In particular, this consists of 215 districts from the eighteen largest Indian states<sup>21</sup>. Using data from NSS 61 and 64 surveys, conducted before and after introduction of NREGS (see Table 5.1.1), the spillover effects of NREGS exposure on wages and labor allocations in late districts can be estimated. In light of the above discussion, it is useful to compare wage and labor outcomes across unexposed ( $E_1 = 0$ ) and exposed ( $E_1 = 1$ ) late districts before the introduction

<sup>&</sup>lt;sup>21</sup> The sample drops union territories, the conflict affected state of Jammu & Kashmir, and small, sparsely populated north-eastern states. Completely urban districts are also dropped from the sample.

of the program. Table 5.3.1 summarizes individual-level statistics for all outcome variables used in the analysis, including salaried days during 2004 – 05, the survey year preceding Phase I assignment. Of the time allocation categories, only the difference in search days is statistically significant across exposed and unexposed districts for the average economically active adult. In terms of wages, salaried wage is similar across both groups but casual wage is lower in exposed districts. Higher search days in conjunction with lower casual wage suggest fewer unskilled jobs relative to labor supply in exposed districts. If this unobserved characteristic of exposed districts is time-invariant, the use of district fixed effects in the estimation strategy would control for this difference. Further, although the pre-exposure differences in search days and real casual wage are significant across exposed and unexposed districts, they do not invalidate the difference-indifference estimation strategy. It may be recalled that the DID strategy allows for pre-existing differences but relies on the identifying assumption of parallel trends in outcomes. Since this assumption cannot be directly tested with the two rounds of survey data used in my study sample, extensive placebo tests are reported in chapter 7.

	Unexposed	Exposed	
	Mean	Mean	p-value
Labor Force Participation	4.08	4.11	0.687
Work	3.80	3.74	0.512
Search	0.28	0.37	0.049
Casual Work	0.87	0.93	0.501
Real Casual Wage (86-87)	18.92	16.11	0.010
Real Salaried Wage (86-87)	32.01	31.61	0.783

Note: These estimates were computed using NSS 64 (July 2004 – June 2005). Casual Labor Wage and Salaried Labor Wage are reported in Rupees/Day (1986 – 87 prices).

Since the analysis of cross-district spillovers is conditional on being a late district, the non-random rollout of the program is not inherently a threat to its internal validity, unless proximity to early phase districts is systematically correlated with unobservable individual and

district-level characteristics. Given that we can't compare unobservables, it is instructive to compare observable characteristics across late districts as an indirect way to test if the nonrandom rollout necessarily implied systematic differences across exposed and unexposed districts. In order to do so, Table 5.3.2 reports averages for individual and household-level characteristics during the pre-exposure period using NSS survey data. It also reports averages for demographic variables using district-level data from the 2001 Census. It is clear from the census statistics that unexposed districts are more rural, comprise of larger households, and have a higher ratio of women to men (overall and caste-differentiated) than exposed districts. However, they do not differ from exposed districts in terms of population size, caste distribution of population, and literacy (overall and gender-differentiated). Since the census is decadal, I do not have information on these characteristics for the post-exposure period in my sample (July 2007 – March 2008) to explicitly control for them. If time-invariant though, these district-level characteristics are accounted for by including district fixed effects in my regression specification. On the other hand, the comparison of means for individual and household-level variables using NSS data has much higher power given the large size of my study sample. It shows that in the pre-exposure period, the economically active population of unexposed districts was younger, less likely to belong to Scheduled Tribes or Other Backward Castes, and more likely to be male and belonging to the other caste category. In terms of land possession, literacy and likelihood of belonging to Scheduled Castes, there is no statistical difference between unexposed and exposed districts. All these variables are also part of the controls in the specifications implemented in chapter 7. It is also possible that other time-varying individual, household or district-level unobservables are correlated with exposure. I carry out robustness

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checks by way of pre-study period placebo analyses and impose sample restrictions in order to investigate the effect, if any, of these unobservables.

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Controls	Unexposed	Exposed	p-value	Source
Age	31.8	32.4	0.019	NSS 61
% Male	51.3	49.8	0.015	NSS 61
% Scheduled Caste	19.3	19.2	0.993	NSS 61
% Scheduled Tribe	1.3	4.9	0.000	NSS 61
% Other Backward	40.7	45.9	0.071	NSS 61
% Others	38.7	29.9	0.004	NSS 61
% Literate	30.4	32.8	0.339	NSS 61
% Land Possessed <	72.4	77.1	0.130	NSS 61
Population	1,925,621	1,837,777	0.673	2001 Census
% Rural Population	64.4	71.3	0.026	2001 Census
Sex Ratio	906.7	939.9	0.007	2001 Census
% Literacy	59.8	59.3	0.783	2001 Census
% Female Literacy	59.4	58.7	0.746	2001 Census
% Male Literacy	80	79.7	0.860	2001 Census
% Scheduled Caste	2.5	4.2	0.236	2001 Census
% Scheduled Tribe	15.6	16.6	0.397	2001 Census
Sex Ratio (SC)	595.7	827.7	0.000	2001 Census
Sex Ratio (ST)	897.8	941.6	0.000	2001 Census
Household Size	5.7	5.4	0.050	2001 Census
Number of Districts	42	173		

 Table 5.3.2: Pre-Exposure Summary Statistics – Late Districts

Note: The NSS 61 sample consists of late assignment individuals between the ages of 15 and 59, interviewed from July 2004 to June 2005, before the introduction of NREGS. The statistics for late assignment district-level characteristics are taken from 2001 Census, which is representative of the entire district population. Sex ratio is reported as the number of women per 1000 men.

## 5.4.2 Early versus Late Districts - Rural

The estimations of unadjusted ITT, ITE and TITT compare early and late assignment districts, without accounting for cross-district spillovers. While the direct effect (ITT) restricts the comparison to rural areas, the indirect effect (ITE) comparison is restricted to urban areas. Table 5.4.1 reports means and p-values generated by comparisons of individual-level rural outcomes in early and late districts during 2004 - 05, the year immediately preceding NREGS. Rural casual wage is higher in unassigned late districts, which is to be expected given that initial assignment was partly based on lower agricultural wages. Differences in time allocations cannot

be detected across the two groups. Table 5.4.2 reports means and p-values for comparisons of pre-program individual and household-level characteristics in rural areas of the assigned and unassigned districts. It is evident that the average rural resident in assigned districts was demographically very different from the average rural resident on all dimensions except age and likelihood of belonging to Scheduled Castes. In particular, assigned rural residents were poorer, more likely to be male, literate, small landholders and belonging to Scheduled Tribes or Other Backward Castes. Given that early assignment was targeted to districts with lower agricultural wages and higher SC and ST populations (historically disadvantaged social groups), these demographic differences combined with lower returns to unskilled labor despite equivalent time allocations, are not surprising. In all the DID and augmented DID specifications estimated in chapter 7, all characteristics reported in Table 5.4.2 are included as controls.

	Late Districts	Early Districts	
	Mean	Mean	p-value
Labor Force Participation	4.32	4.21	0.142
Work	3.93	3.87	0.427
Search	0.39	0.33	0.102
Casual Work	1.10	1.17	0.228
Real Casual Wage (86-87)	15.77	12.63	0.000
Real Salaried Wage (86-87)	28.27	29.72	0.312

 Table 5.4.1: Pre-Exposure Rural Outcomes across Assignment

Note: These estimates were computed using NSS 61 (July 2004 – June 2005). Casual Labor Wage and Salaried Labor Wage are reported in Rupees/Day (1986 – 87 prices). N (Late)=74,750 and N(Early)=110,729 for time allocation outcomes. N (Late)=7,811 and N(Early)=12,401 for real casual wage. N (Late)=4,558 and N(Early)=5,086 for time allocation outcomes.

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Controls	Late	Early	p-value	Source
Age	32.39	32.50	0.408	NSS 61
% Male	50	49	0.038	NSS 61
% Scheduled Caste (SC)	21	22	0.546	NSS 61
% Scheduled Tribe (ST)	5	13	0.000	NSS 61
% Other Backward Caste	46	42	0.071	NSS 61
% Others	28	24	0.055	NSS 61
% Literate	37	47	0.000	NSS 61
% Land Possessed < 1ha	78	82	0.002	NSS 61
Household MPCE	994.16	831.04	0.000	NSS 61
Number of Districts	215	288		

 Table 5.4.2: Pre-Exposure Summary Statistics – Rural Areas

Note: The NSS 61 sample consists of late phase individuals between the ages of 18 and 59, interviewed from July 2004 to June 2005, before the introduction of NREGS. Household monthly per capita expenditure is reported in 2004-05 prices.

#### 5.4.3 Early versus Late Districts - Urban

Table 5.5.1 reports means and p-values generated by comparisons of individual-level urban outcomes in early and late districts before NREGS. In terms of initial outcomes, it is clear that urban areas of assigned and unassigned districts were not as comparable as rural areas, with the differences in time allocations being statistically indistinguishable from zero only for search and casual work. On the other hand, the pre-program individual and household-level demographic characteristics in urban areas are more similar than rural areas of assigned and unassigned districts (see Table 5.5.2). Although assigned urban residents were richer and more likely to be male and ST, they are similar to unassigned urban residents on other dimensions. Given that we control for these characteristics, these observable differences do not invalidate chapter 7 ITE estimates *per se*. Yet, they do not increase our confidence in the comparability of assigned and unassigned urban areas.

	Late Districts	Early Districts	
	Mean	Mean	p-value
Labor Force Participation	3.62	3.43	0.007
Work	3.34	3.14	0.007
Search	0.28	0.29	0.676
Casual Work	0.51	0.54	0.508
Real Casual Wage (86-87)	20.78	15.89	0.000
Real Salaried Wage (86-87)	34.17	39.12	0.005

Table 5.5.1: Pre-Exposure Urban Outcomes across Assignment

Note: These estimates were computed using NSS 61 (July 2004 – June 2005). Casual Labor Wage and Salaried Labor Wage are reported in Rupees/Day (1986 – 87 prices). N (Late)=44,534 and N(Early)=41,052 for time allocation outcomes. N (Late)=3,153 and N(Early)=2,992 for real casual wage. N (Late)=6,881 and N(Early)=5,519 for time allocation outcomes.

Table 5.5.2: Pre-Exposure	Summary Statist	tics – Urban Areas		
Controls	Late	Early	p-value	Source
Age	31.94	32.05	0.581	NSS 61
% Male	51	50	0.057	NSS 61
% Scheduled Caste (SC)	15	18	0.101	NSS 61
% Scheduled Tribe (ST)	2	4	0.011	NSS 61
% Other Backward Caste	43	37	0.107	NSS 61
% Others	39	41	0.641	NSS 61
% Literate	21	22	0.441	NSS 61
% Land Possessed < 1ha	72	75	0.173	NSS 61
Household MPCE	1,327.49	1,226.50	0.021	NSS 61
Number of Districts	215	288		

Note: The NSS 61 sample consists of late phase individuals between the ages of 18 and 59, interviewed from July 2004 to June 2005, before the introduction of NREGS. Household monthly per capita expenditure is reported in 2004-05 prices.

In order to investigate whether wage and time allocation effects are concentrated among

i) men or women, ii) rural or urban areas, and iii) dry or rainy season, the heterogeneity in labor

market spillovers is estimated along the gender, sector, and season dimensions in the next

chapter. Preceding that analysis, it is useful to examine the comparability of pre-program

outcomes and characteristics across exposed and unexposed late districts, for each category. This

exploration enables an understanding of pre-existing differences between categories in restricted

sub-samples as well as the overall sample. As an example, it is clear that the pre-program casual

wage was higher across the board – i.e. for men, women, rainy season, dry season, rural and

urban areas - during 2004-05. Similarly, monthly per capita household expenditure, a direct

indicator of living standards, is higher in unexposed districts for all restricted samples but the difference is nearly insignificant during the rainy season, when agricultural incomes are highest in the more rural exposed districts. The seasonal comparison for other categories is instructive as well, given that seasonal migration is the mechanism presumed to be driving spillovers estimated in the next chapter.

## 5.4.4. Exposed versus Unexposed Districts – Men

Table 5.6.1 reports statistics generated by comparisons of men's outcomes in unexposed and exposed late districts. It is evident that men in unexposed districts spent more time working in the labor force and less time searching, than their counterparts in exposed districts. Salaried work comprised nearly 35% more of the time spent working in unexposed districts, while casual work was statistically identical in both groups. Conversely, the returns to casual work were higher in unexposed districts, while the return to salaried work was the same in both groups. Demographically, men in unexposed districts tend to be nine months younger and less likely to belong to ST and OBC groups. While literacy and land possession indicators are statistically similar, men in unexposed districts live in significantly richer households (Table 5.6.2).

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	Unexposed	Exposed	
	Mean	Mean	p-value
Labor Force Participation	5.99	5.83	0.004
Work	5.60	5.31	0.003
Search	0.39	0.52	0.046
Casual Work	1.25	1.32	0.582
Salaried Work	1.34	0.98	0.049
Real Casual Wage (86-87)	20.62	18.42	0.077
Real Salaried Wage (86-87)	33.35	34.73	0.400

 Table 5.6.1: Pre-Program Male Outcomes across Exposure

Note: These estimates were computed using NSS 61 (July 2004 – June 2005). Casual Labor Wage and Salaried Labor Wage are reported in Rupees/Day (1986 – 87 prices). N (Unexposed)=10,661 and N(Exposed)=48,228 for time allocation outcomes, reported in days/week. N (Unexposed)=1,481 and N(Exposed)=6,574 for real casual wage. N (Unexposed)=1,926 and N(Exposed)=7,344 for salaried wage.

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Unexposed	Exposed	p-value	Source
31.29	32.06	0.004	NSS 61
0.20	0.19	0.940	NSS 61
0.01	0.05	0.000	NSS 61
0.41	0.46	0.080	NSS 61
0.38	0.30	0.005	NSS 61
0.19	0.22	0.138	NSS 61
0.72	0.77	0.124	NSS 61
1,214.25	1,052.75	0.004	NSS 61
10,661	48,228		
215	288		
	31.29 0.20 0.01 0.41 0.38 0.19 0.72 1,214.25 10,661 215	Onexposed         Exposed           31.29         32.06           0.20         0.19           0.01         0.05           0.41         0.46           0.38         0.30           0.19         0.22           0.72         0.77           1,214.25         1,052.75           10,661         48,228           215         288	OnexposedExposedp-value31.2932.060.0040.200.190.9400.010.050.0000.410.460.0800.380.300.0050.190.220.1380.720.770.1241,214.251,052.750.00410,66148,228215288

Table 5.6.2: Pre-Program Summary Statistics – Men

Note: The NSS 61 sample consists of Late district men between the ages of 18 and 59, interviewed from July 2004 to June 2005, before the introduction of NREGS. Household monthly per capita expenditure is reported in 2004-05 prices.

#### 5.4.5. Exposed versus Unexposed Districts – Women

In Table 5.7.1, women's outcomes in unexposed and exposed late districts are compared, like the corresponding comparisons in Table 5.6.1. In contrast to men, women in unexposed districts spent less weekly time in the labor force and worked less than women in exposed districts. Given the broader trend in India of lower female labor force participation at higher levels of income, the higher wealth in unexposed districts explains the aforementioned contrast. Further, weekly time allocated to *both* salaried and casual work was statistically identical in both groups. Similar to men though, the returns to casual work were higher in unexposed districts, while the return to salaried work was the same in both groups. In terms of demographic indicators, women, like men, tend to be younger and less likely to belong to ST and OBC groups in unexposed districts. Again, literacy and land possession indicators are statistically identical but women in unexposed districts live in significantly richer households, as measured by monthly per capita expenditure.

	Unexposed	Exposed	
	Mean	Mean	p-value
Labor Force Participation	2.06	2.41	0.053
Work	1.90	2.18	0.083
Search	0.16	0.23	0.096
Casual Work	0.47	0.55	0.301
Salaried Work	0.18	0.24	0.318
Real Casual Wage (86-87)	13.94	10.69	0.000
Real Salaried Wage (86-87)	21.41	18.74	0.104

Table 5.7.1: Pre-Program Female Outcomes across Expo	osure
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Note: These estimates were computed using NSS 61 (July 2004 – June 2005). Casual Labor Wage and Salaried Labor Wage are reported in Rupees/Day (1986 – 87 prices). N (Unexposed)=10,343 and N(Exposed)=50,052 for time allocation outcomes, reported in days/week. N (Unexposed)=480 and N(Exposed)=2429 for real casual wage. N (Unexposed)=303 and N(Exposed)=1,866 for salaried wage.

	<b>Table 5.7.2</b>	: Pre-	-Program	<b>Summary</b>	Statistics –	Women
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Controls	Unexposed	Exposed	p-value	Source
Age	32.29	32.66	0.168	NSS 61
% Scheduled Caste (SC)	0.19	0.19	0.944	NSS 61
% Scheduled Tribe (ST)	0.01	0.05	0.000	NSS 61
% Other Backward Caste	0.41	0.46	0.067	NSS 61
% Others	0.39	0.30	0.004	NSS 61
% Literate	0.42	0.43	0.715	NSS 61
% Land Possessed < 1ha	0.73	0.78	0.146	NSS 61
Household MPCE	1,259.39	1,075.51	0.002	NSS 61
Observations	10,343	50,052		
Number of Districts	215	288		

Note: The NSS 61 sample consists of Late district women between the ages of 18 and 59, interviewed from July 2004 to June 2005, before the introduction of NREGS. Household monthly per capita expenditure is reported in 2004-05 prices.

# 5.4.6. Exposed versus Unexposed Districts – Rainy Season

In Table 5.8.1, pre-program outcomes in unexposed and exposed late districts are

compared during the rainy season, defined to be the last two quarters of the year (July -

December 2004). Except the returns to casual labor, which are higher to unexposed districts, all

outcomes are statistically identical in both groups. Both groups are also statistically identical in

terms of age and gender breakdown (Table 5.8.2). Since women and men are pooled in this

sample restriction, residents of unexposed districts are less likely to belong to ST and OBC

groups during the rainy season, reflecting the trends observed for men and women separately.
Again, the difference in literacy and land possession indicators is statistically insignificant, while households in unexposed districts are richer during the rainy season (borderline significant at the 10% level). Since the demand for casual labor is highest in these months, the high degree of comparability between the two groups of districts is further corroboration of low out-migration during this period.

	Unexposed	Exposed	
	Mean	Mean	p-value
Labor Force Participation	4.06	4.16	0.325
Work	3.75	3.78	0.818
Search	0.30	0.38	0.173
Casual Work	0.88	0.94	0.618
Salaried Work	0.73	0.61	0.199
Real Casual Wage (86-87)	18.52	15.87	0.030
Real Salaried Wage (86-87)	31.85	31.19	0.681

Table 5.8.1: Pre-Program	Rainy	Outcomes	across	Exposure
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Note: These estimates were computed using the last two sub-rounds of NSS 61 (January – June 2005). Casual Labor Wage and Salaried Labor Wage are reported in Rupees/Day (1986 – 87 prices). N (Unexposed)=10,544 and N(Exposed)=49,101 for time allocation outcomes, reported in days/week. N (Unexposed)=958 and N(Exposed)=4,493 for real casual wage. N (Unexposed)=1,068 and N(Exposed)=4,501 for salaried wage.

## Table 5.8.2: Pre-Program Summary Statistics – Rainy

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Controls	Unexposed	Exposed	p-value	Source
Age	32.01	32.30	0.316	NSS 61
% Male	0.51	0.50	0.159	NSS 61
% Scheduled Caste (SC)	0.19	0.19	0.968	NSS 61
% Scheduled Tribe (ST)	0.01	0.05	0.000	NSS 61
% Other Backward Caste	0.39	0.46	0.047	NSS 61
% Others	0.40	0.31	0.007	NSS 61
% Literate	0.31	0.33	0.413	NSS 61
% Land Possessed < 1ha	0.74	0.77	0.305	NSS 61
Household MPCE	1,239.32	1,065.42	0.010	NSS 61
Observations	10,544	49,101		
Number of Districts	215	288		

Note: The NSS 61 sample consists of Late district individuals between the ages of 18 and 59, interviewed from July 2004 to December 2004, before the introduction of NREGS. Household monthly per capita expenditure is reported in 2004-05 prices.

### 5.4.7. Exposed versus Unexposed Districts – Dry Season

In Table 5.9.1, pre-program outcomes are compared during the dry season (January – June 2005). In a key difference from the pattern discussed for the rainy season, weekly time allocated to search is higher in the exposed districts during the first two quarters of the year. Casual wage remains higher to unexposed districts in this period, while other outcomes are statistically identical in both groups. Interestingly, there is a statistically significant difference in age and gender during the dry season, with exposed districts consisting of older residents and fewer males, perhaps reflecting the out-migration of young males in these months when seasonal migration peaks (Table 5.9.2). Interestingly, the residents of unexposed districts are no longer less likely to belong to OBC groups during the dry season, the 3 percentage point increase perhaps reflecting a concentration of these social groups in the influx of seasonal migrants. Again, the difference in literacy and land possession indicators is statistically insignificant, and households in unexposed districts remain richer during the dry season.

	Unexposed	Exposed Districts	
	Mean	Mean	p-value
Labor Force Participation	4.10	4.07	0.770
Work	3.85	3.70	0.145
Search	0.25	0.36	0.014
Casual Work	0.86	0.93	0.426
Salaried Work	0.82	0.60	0.124
Real Casual Wage (86-87)	19.33	16.37	0.007
Real Salaried Wage (86-87)	32.16	32.05	0.951

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Note: These estimates were computed using the first two sub-rounds of NSS 61 (July – December 2004). Casual Labor Wage and Salaried Labor Wage are reported in Rupees/Day (1986 – 87 prices). N (Unexposed)=10,460 and N(Exposed)=49,179 for time allocation outcomes, reported in days/week. N (Unexposed)=1,003 and N(Exposed)=4,510 for real casual wage. N (Unexposed)=1,161 and N(Exposed)=4,709 for salaried wage.

Controls	Unexposed	Exposed	p-value	Source
Age	31.53	32.42	0.003	NSS 61
% Male	0.51	0.49	0.006	NSS 61
% Scheduled Caste (SC)	0.20	0.20	0.986	NSS 61
% Scheduled Tribe (ST)	0.01	0.05	0.000	NSS 61
% Other Backward Caste	0.42	0.46	0.236	NSS 61
% Others	0.37	0.29	0.021	NSS 61
% Literate	0.30	0.33	0.312	NSS 61
% Land Possessed < 1ha	0.70	0.77	0.107	NSS 61
Household MPCE	1,233.06	1,062.91	0.002	NSS 61
Observations	10,460	49,179		
Number of Districts	215	288		

Table 5.9.2: Pre-Program Summary Statistics – Dry

Note: The NSS 61 sample consists of Late district individuals between the ages of 18 and 59, interviewed from January to June 2005, before the introduction of NREGS. Household monthly per capita expenditure is reported in 2004-05 prices.

## 5.4.8. Exposed versus Unexposed Districts – Rural Areas

In Table 5.10.1, pre-program rural outcomes in unexposed and exposed late districts are compared. While all time allocation outcomes are statistically identical, both salaried and casual wages are higher in unexposed rural areas, indicating higher labor demand across sectors in these districts. Further, the average unexposed rural resident is younger, more likely to belong to OBC groups and less likely to belong to non-disadvantaged caste groups (Table 5.10.2), reflecting the trends observed for men and women separately. While the literacy indicator is statistically identical across groups, households in unexposed districts are richer and also, less likely to possess land smaller than 1 hectare.

	Unexposed	Exposed	
	Mean	Mean	p-value
Labor Force Participation	4.30	4.32	0.868
Work	3.98	3.93	0.644
Search	0.33	0.40	0.213
Casual Work	1.09	1.10	0.912
Salaried Work	0.45	0.37	0.208
Real Casual Wage (86-87)	17.72	15.38	0.027
Real Salaried Wage (86-87)	31.13	27.56	0.016

### Table 5.10.1: Pre-Program Rural Outcomes across Exposure

Note: These estimates were computed using NSS 61 (July 2004 – June 2005). Casual Labor Wage and Salaried Labor Wage are reported in Rupees/Day (1986 – 87 prices). N (Unexposed)=11,864 and N(Exposed)=62,886 for time allocation outcomes, reported in days/week. N (Unexposed)=1,277 and N(Exposed)=6,534 for real casual wage. N (Unexposed)=735 and N(Exposed)=3,823 for salaried wage.

	Table 5.10.2:	<b>Pre-Program</b>	<b>Summary</b>	Statistics –	Rural
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Controls	Unexposed	Exposed	p-value	Source
Age	31.94	32.48	0.027	NSS 61
% Male	0.51	0.50	0.164	NSS 61
% Scheduled Caste (SC)	0.22	0.21	0.360	NSS 61
% Scheduled Tribe (ST)	0.01	0.06	0.000	NSS 61
% Other Backward Caste	0.42	0.47	0.179	NSS 61
% Others	0.35	0.27	0.028	NSS 61
% Literate	0.36	0.38	0.583	NSS 61
% Land Possessed < 1ha	0.73	0.79	0.088	NSS 61
Household MPCE	1,126.35	967.33	0.000	NSS 61
Observations	11,864	62,886		
Number of Districts	215	288		

Note: The NSS 61 sample consists of Late district rural residents between the ages of 18 and 59, interviewed from July 2004 to June 2005, before the introduction of NREGS. Household monthly per capita expenditure is reported in 2004-05 prices.

### 5.4.9. Exposed versus Unexposed Districts – Urban Areas

In Table 5.11.1, pre-program urban outcomes in unexposed and exposed late districts are compared. Excepting search days, all time allocation outcomes are statistically identical across the two groups but casual wages are higher in unexposed urban areas. In contrast with the rural sector, there is no statistically significant difference in salaried wages across exposed and unexposed areas, suggesting that urban labor demand in the formal sector is equal in both groups of districts. Further, the average unexposed rural resident is younger, more likely to belong to

OBC groups and less likely to belong to non-disadvantaged caste groups (Table 5.11.2),

reflecting the trends observed for men and women separately. While the literacy indicator is

statistically identical across groups, households in unexposed districts are richer and are less

likely to possess land smaller than 1 hectare.

	Unexposed	Exposed	
	Mean	Mean	p-value
Labor Force Participation	3.69	3.60	0.463
Work	3.49	3.29	0.107
Search	0.19	0.31	0.002
Casual Work	0.50	0.52	0.780
Salaried Work	1.33	1.18	0.285
Real Casual Wage (86-87)	23.69	19.99	0.013
Real Salaried Wage (86-87)	32.52	34.72	0.274

Table 5.11.1: Pre-Program Urban Outcomes across Exp	osure
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Note: These estimates were computed using NSS 61 (July 2004 – June 2005). Casual Labor Wage and Salaried Labor Wage are reported in Rupees/Day (1986 – 87 prices). N (Unexposed)=9,140 and N(Exposed)=35,394 for time allocation outcomes, reported in days/week. N (Unexposed)=684 and N(Exposed)=2,469 for real casual wage. N (Unexposed)=1,494 and N(Exposed)=5,387 for salaried wage.

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Controls	Unexposed	Exposed	p-value	Source
Age	31.49	32.07	0.070	NSS 61
% Male	0.53	0.50	0.016	NSS 61
% Scheduled Caste (SC)	0.14	0.16	0.312	NSS 61
% Scheduled Tribe (ST)	0.02	0.02	0.717	NSS 61
% Other Backward Caste	0.38	0.44	0.123	NSS 61
% Others	0.46	0.37	0.010	NSS 61
% Literate	0.21	0.21	0.786	NSS 61
% Land Possessed < 1ha	0.71	0.73	0.724	NSS 61
Household MPCE	1,423.77	1,299.65	0.156	NSS 61
Observations	9,140	35,394		
Number of Districts	215	288		

# Table 5.11.2: Pre-Program Summary Statistics – Urban

Note: The NSS 61 sample consists of Late district urban residents between the ages of 18 and 59, interviewed from July 2004 to June 2005, before the introduction of NREGS. Household monthly per capita expenditure is reported in 2004-05 prices.

## **CHAPTER SIX**

### **EMPIRICAL RESULTS: CROSS – DISTRICT SPILLOVERS**

# 6.1 Introduction

This chapter presents results from the estimation of between spillovers from assigned to unassigned districts. Section 6.2 tests for the presence and magnitude of wage and time allocation spillovers using measures of exposure, exposure intensity and population-weighted exposure intensity. Heterogeneity in labor market effects along the gender, rural-urban, and season dimensions is discussed in section 6.3. Placebo tests for the presence of differential outcome trends are reported in section 6.4. A series of robustness checks are employed and presented in section 6.5. Section 6.6 discusses the empirical relationship between exposure intensity and short-distance seasonal migration, the mechanism presumed to be driving between spillovers to unassigned districts. The last section summarizes key findings from this chapter.

## 6.2 Labor Market Spillovers from Assigned to Unassigned Districts

### 6.2.1 Exposure and Spillovers

In this section, specification (1) is implemented to compute estimates for the Average Treatment Effect on Close to Treated (ACT) parameter discussed in chapter 4. The binary measure of exposure, which classifies unassigned late districts with at least one assigned neighbor as exposed, is used as the measure of proximity. The results from this estimation are presented in Table 6.2.1. The dependent variables in the first two columns are logs of real casual and salaried labor wages, conditional on positive weekly earnings. The next three columns report weekly time allocations for individual-level labor force participation, work and search days. Since time allocated to work and search aggregate to labor force participation for every

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individual in a given week, changes in these two categories necessarily aggregate to the change in labor force participation.

Comparing exposed and unexposed late districts before and after the introduction of NREGS, I estimate that on average, real casual wage increased by 8.7% in exposed districts, relative to unexposed districts and this effect is significant at the 5% level. This result provides empirical support to the theoretical prediction of origin wages in unassigned districts rising faster with exposure to assigned neighbors. The absence of a significant effect on salaried wage provides support that the estimated effect on casual wage is not a reflection of a secular rise in wages across the informal and formal sectors in the economy. Further, it supports the conjecture that the casual labor market is relevant market for measuring between spillovers. Figure 6.1, graphs the casual wage trends for exposed and unexposed districts. It is clear that these trends move in parallel before exposure and narrow afterwards. This is the relative increase due to exposure that is captured by the results in Table 6.2.1.

On the other hand, I estimate no statistically significant effect of exposure on any time allocation category. Given the limitations of repeated cross-section data, which does not track leavers in both rounds, I cannot test for lower aggregate employment due to exposure, one of the predictions from the theoretical model. As mentioned in chapter 5, this estimation strategy detects changes in weekly labor force participation, work and search, averaged over residents of late districts. Further, given the absence of leavers in the post-exposure period, time allocation averages are likely to mechanically change more in the exposed districts if leavers differ from stayers on dimensions other than the variable cost of working outside the district. In particular, the estimated effects of exposure on average labor force participation and work are likely to be

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attenuated if leavers are more likely to participate in the labor force and work more. The absence of effects on any of the time allocation categories could be explained by this attenuation. Alternatively, small variation in the binary measure of exposure combined with the small fraction of casual workers relative to the overall sample could contribute to small effects not being detected. Figure 6.2 graphs the trends in labor force participation for exposed and unexposed districts. As is evident, these trends are non-parallel before exposure, thus not allowing an identification of relative change due to exposure.



Figure 6.1: Casual Wage and Exposure in Study Period



Figure 6.2: Labor Force Participation and Exposure in Study Period

Source: NSS 61, 64

Table 6	.2.1: (	Cross-1	District S	Spillovers -	<ul> <li>Exposure (</li> </ul>	to A	Assigned N	<b>Veighbors</b>

	1	1	0	0	
	(1)	(2)	(3)	(4)	(5)
	Log deflated	Log deflated	Labor force	Work	Search
	casual wage	salaried wage	participation		
Post*Exposure	0.087**	0.004	0.051	-0.001	0.052
	(0.032)	(0.062)	(0.082)	(0.082)	(0.036)
Controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes
Observations	19,677	17,513	192,124	192,124	192,124

Note: Each column represents results from a separate regression. The sample consists of late phase individuals between the ages of 15 and 59, interviewed from July 2004 to June 2005 and from July 2007 – March 2008. Labor force participation days, work days and search days are calculated using time allocation responses for the week preceding interview. Log casual wage and log salaried wage is the log of earnings per day worked for individuals who report working in casual labor and salaried work respectively. Daily casual wage and salaried work wage are deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Intensity measures the share of contiguous assigned neighbors as a fraction of all neighbors. Individual-level controls (age, age squared, gender, indicators for literacy, land possessed, and caste group), rural fraction of district population, year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 10% level.

## 6.2.2 Exposure Intensity and Spillovers

In order to capture the sensitivity of labor market outcomes to exposure intensity, specification (1) is now implemented using the share of early assignment neighbors as the exogenous shock for late districts. I estimate that, on average, spillovers from NREGS resulted in real casual wage increasing by 1.03% with every additional 10% increase in exposure intensity (significant at the 5% level). For example, a late district with ten neighbors would experience a 1.03% increase in casual wage with every additional neighbor receiving program assignment. Comparing the magnitude of these spillovers to previous evaluations of the program, it is instructive that a 50% increase in exposure intensity is roughly equivalent to the 4.7% and 5.1% increases in casual wages in program districts estimated by Imbert and Papp (2015) and Berg et al. (2012), respectively. In other words, program assignment to half their neighbors has approximately the same impact on casual wages in late districts as being assigned the program. In Table 6.2.2, the change in real salaried wage remains statistically insignificant, corroborating the result from Table 6.2.1 and increasing our confidence that the estimated impacts on casual wages are not a reflection of secular wage increases across all labor markets in exposed districts, relative to unexposed districts.

Interestingly, the time allocation results differ across exposure and exposure intensity, with labor force participation now increasing with exposure intensity. The average labor force participation in completely exposed districts increased by 0.21 days/week, relative to completely unexposed districts (significant at the 5% level). This 5.1% increase in average labor force participation is accompanied by positive but statistically insignificant increases in work and search days. Despite the absence of significance on time allocated to work and search separately, the interpretation of the labor force participation estimate is a cumulative increase in the time

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spent working and searching for employment for the average resident of exposed districts, relative to unexposed districts. Given that the theoretical model's predicted effect of exposure on individual-level labor supply of stayers is ambiguous (as discussed in chapter 3), rising casual wages accompanied with increased time spent in the labor force is indicative of an upward sloping labor supply curve in this wage range. It may be recalled that this outcome is not binary but an aggregate of time spent working and searching for work, thus supporting this interpretation. Further, these results suggest that while the dichotomous exposure variable performs fairly well in terms of approximating impacts on casual wages, accounting for heterogeneity of exposure is enables a better estimation of time allocation impacts.

	(1)	(2)	(3)	(4)	(5)
	Log deflated	Log deflated	Labor force	Work	Search
	casual wage	salaried wage	participation		
Post*Intensity	0.103**	-0.072	0.212*	0.128	0.083
	(0.051)	(0.091)	(0.114)	(0.138)	(0.066)
Controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes
Observations	19,677	17,513	192,124	192,124	192,124

Table 6.2.2: Cross-District Spillovers – Intensity of Exposure to Assigned Neighbors

Note: Each column represents results from a separate regression. The sample consists of late phase individuals between the ages of 15 and 59, interviewed from July 2004 to June 2005 and from July 2007 – March 2008. Labor force participation days, work days and search days are calculated using time allocation responses for the week preceding interview. Log casual wage and log salaried wage is the log of earnings per day worked for individuals who report working in casual labor and salaried work respectively. Daily casual wage and salaried work wage are deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Intensity measures the share of contiguous assigned neighbors as a fraction of all neighbors. Individual-level controls (age, age squared, gender, indicators for literacy, land possessed, and caste group), rural fraction of district population, year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 10% level.

# 6.2.3 Population-weighted Exposure Intensity and Spillovers

As an additional robustness check, I also implement specification (1) using a populationweighted measure of exposure intensity instead of exposure intensity, thus accounting for the size of neighboring rural labor markets in which NREGS was introduced. Since this measure is scaled from 0 - 100%, the estimates generated by this specification can be interpreted as the spillover effects due to every 1 percentage point increase in the neighboring populace eligible for the employment guarantee. In Table 6.2.3, I estimate that, on average, real casual wage increased by 0.1% with every additional percentage point of neighboring populace eligible for NREGS (significant at the 5% level). In other words, if half the rural population residing in neighboring districts happened to be eligible for the program, it would lead to a 5% increase in casual wage, relative to districts with no eligible population in neighboring districts. This estimate is remarkably similar to the magnitude of spillovers due if half the neighbors were assigned the program, thus indicating that the number of early assignment neighbors is highly correlated with the eligible rural population residing in neighbors; i.e. accounting for possible spatial clustering of eligible rural residents in low exposure intensity districts does not change the wage spillover estimates. The corresponding effects on weekly time allocation outcomes are statistically insignificant though, possibly reflecting greater noise in the population-weighted measure than in the exposure intensity variable.

	(1)	(2)	(3)	(4)	(5)
	Log deflated casual wage	Log deflated salary wage	Labor force participation	Work	Search
Post*WeightedIntensity	0.001**	-0.000	0.001	0.001	0.001
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
Controls	Yes	Yes	Yes	Yes	Yes

 Table 6.2.3: Cross-District Spillovers – Intensity of Population-Weighted Exposure to

 Assigned Neighbors

#### Table 6.2.3 (cont'd).

District FE	Yes	Yes	Yes	Yes	Yes
Quarter-year FE	Yes	Yes	Yes	Yes	Yes
Observations	19,677	17,513	192,124	192,124	192,124

Note: Each column represents results from a separate regression. The sample consists of late phase individuals between the ages of 15 and 59, interviewed from July 2004 to June 2005 and from July 2007 – March 2008. Labor force participation days, work days and search days are calculated using time allocation responses for the week preceding interview. Log casual wage and log salaried wage is the log of earnings per day worked for individuals who report working in casual labor and salaried work respectively. Daily casual wage and salaried work wage are deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Intensity measures the share of contiguous assigned neighbors as a fraction of all neighbors. Individual-level controls (age, age squared, gender, indicators for literacy, land possessed, and caste group), rural fraction of district population, year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 10% level.

### 6.3 Heterogeneity of Spillovers

### 6.3.1 Cross-District Spillovers by Gender

Given that seasonal migrants who leave for employment purposes are overwhelmingly male in India (Ravi et al. 2012), it is of interest to evaluate whether spillovers due to exposure are gender-differentiated. In order to estimate these gender-differentiated impacts, indicators for men and women are interacted with the binary exposure term in specification (1) and the coefficients are reported in Table 6.3.1. This formulation splits the spillover effect on late assignment districts by gender by replacing *Posti\*Exposured* with *Posti\*Exposured* \**Meni* and *Posti\*Exposured* \**Womeni*. The baseline category is thus unexposed individuals during the pre-program period and both the aforementioned parameters are estimated relative to this counterfactual. The dummy variable for men is retained in the specification to separate the gender-specific spillover effect from the long-run difference in labor market outcomes between men and women. Additionally, I report effects on time allocated to casual labor, a subset of days worked, in order to focus on the gender-specific labor responses of stayers in this particular market. I estimate that, on average, casual wage for women increased by 9.3% more in exposed districts relative to the increase in unexposed districts. On the other hand, the

increase in casual wage for men was estimated to be 8.5%. The null hypothesis of equal casual wage spillovers by gender cannot be rejected though. This failure to reject the hypothesis of equal spillovers can be explained by two offsetting effects on casual labor markets in late districts. While NREGS raised women's market wages more than men's on account of a larger differential between program and market wages in early assignment neighbors (Azam 2012), it is also the case that seasonal migrants tend to be predominantly male. *Ceteris paribus*, the casual wage differential between assigned destinations and home districts would likely be higher for women. Judging from pre-existing seasonal migration trends though, the return to migration is likely higher for men because of lower variable costs associated with working outside the district. It is likely that this lower cost of working outside the district is driving the equality of wage spillovers by gender in exposed late districts.

Comparing time allocation spillovers by gender, I estimate that male labor force participation increased by 0.19 days/week due to exposure and almost all of it translated into increased casual work, i.e. 0.17 days/week. Time allocated to employment search rose by 0.08 days/week for men, even as the change in search days is not statistically significant for women. Further, the null hypothesis of equal spillovers by gender is rejected in this case. Despite no evidence for gender-differentiated wage spillovers, the presence of time allocation spillovers for men could be a result of much higher power in the latter test. It is also possible that the elasticity of labor supply is positive among male stayers while income and substitution effects induced by higher wages could be offsetting each other for women. Lastly, the simultaneous increases in male search for work, labor supply in the informal market and casual wage, signal

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the presence of search costs or wage rigidities which prevent the labor market from completely clearing in late districts.

			<b>V</b> -		
	(1)	(2)	(3)	(4)	(5)
	Log deflated casual wage	Labor force participation	Work	Search	Casual Work
Post*Exposure *Women	0.093***	-0.077	-0.108	0.031	0.044
	(0.034)	(0.096)	(0.098)	(0.035)	(0.063)
Post*Exposure *Men	0.085**	0.188**	0.113	0.075*	0.169**
	(0.034)	(0.089)	(0.087)	(0.041)	(0.076)
Controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes
Observations	19,677	192,124	192,124	192,124	192,124

Table 6.3.1: Cross-District Spillovers, by Gender

Note: Each column represents results from a separate regression. The sample consists of late phase individuals between the ages of 15 and 59, interviewed from July 2004 to June 2005 and from July 2007 – March 2008. Labor force participation days, work days and search days are calculated using time allocation responses for the week preceding interview. Log casual wage and log salaried wage is the log of earnings per day worked for individuals who report working in casual labor and salaried work respectively. Daily casual wage and salaried work wage are deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Exposure is an indicator for whether district has at least one contiguous assigned neighbor. Individual-level controls (age, age squared, gender, indicators for literacy, land possessed, and caste group), rural fraction of district population, year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 10% level.

#### 6.3.2 Cross-District Spillovers by Sector

A second source of heterogeneity in spillovers is the sector of employment. Given a preexisting casual wage differential (see Figure 9.1 in Appendix) and difference in type of unskilled work offerings across rural and urban areas *within* late districts, it is conceivable that exposure induced out-migration is non-uniformly distributed across sectors. In the theoretical framework laid out in chapter 3, the effective post-exposure wage differential between destination and origin is a function of exposure intensity. It is likely that origin wage for unskilled workers in urban areas was so high before NREGS that the change in returns to migration was not large enough to warrant incurring the variable cost associated with working in neighboring districts. In this case, the casual wage spillovers documented in Tables 6.2.1, 6.2.2 and 6.2.3 are likely to be concentrated in rural areas, where the change in returns to migration is large enough. On the other hand, it is possible that change in exposure intensity is larger in urban areas, relative to rural areas within late districts. If it is the case that urban labor markets are more closely integrated with assigned neighbors than rural labor markets, then there would exist a rural-urban differential in exposure intensity within late districts. This in turn would lead to wage spillovers being concentrated in urban areas.

In order to estimate sector-differentiated impacts, indicators for rural and urban are interacted with the binary exposure term in specification (1) and the coefficients are reported in Table 6.3.2. Similar to the approach taken in the previous sub-section, this formulation splits the spillover effect on late assignment districts by sector, by replacing  $Post_t * Exposure_d$  with *Post*<sub>t</sub>\**Exposure*<sub>d</sub> \**Rural*<sub>d</sub> and *Post*<sub>t</sub>\**Exposure*<sub>d</sub> \**Urban*<sub>d</sub>. The dummy variable for rural sector is retained to separate the sector-specific spillover effect from the long-run difference in labor market outcomes between rural and urban areas. I estimate that, on average, casual wage increased by 5.8% in exposed rural areas relative to the increase to unexposed rural areas. On the other hand, the increase in casual wage for urban areas was estimated to be nearly four times higher at 20.4%. The null hypothesis of equal casual wage spillovers by sector can be rejected at the 0.1% level of significance. This failure to reject the hypothesis of equal spillovers is supportive of the theory that the urban labor markets in late districts are more closely linked to neighboring early districts than rural areas. This could be interpreted as reflecting better physical connectivity (eg. roads, modes of transport, phone networks) and/or social networks between the urban areas of late districts and assigned neighbors.

Comparing time allocation spillovers by sector, I next estimate that exposure resulted in labor force participation increasing by 0.21 days/week in rural areas, and decreasing by 0.29 days/week in urban areas (both significant at the 1% level). This led to casual work increasing by 0.26 days/week in rural areas but decreasing by 0.24 days/week in urban areas (both significant at 1% level). The opposite substitutions of labor force participation for casual work in urban and rural areas can be interpreted as evidence supporting the existence of a backward bending labor supply curve in the former and a positive sloping curve in the latter. The combination of a higher pre-exposure casual wage and larger spillover effects in urban areas likely induced a movement along the labor supply curve to its backward bending range and stayers choosing leisure over consumption. This is corroborated by a nearly one-to-one correspondence between reduced time allocated to weekly work and weekly labor force participation. Conversely in rural areas, the low pre-exposure casual wage combined with a relatively small spillover effect increased labor force participation and the search for work (0.08 days/week) in the lower wage range where the substitution of consumption for leisure dominates the income effect. The null hypothesis of equal sectoral effects on time allocated to labor force participation, work, casual work and search can be rejected at the 0.1% level of significance, further confirming that the magnitudes of labor market spillovers experienced by rural and urban areas are different within late districts.

	(1)	(2)	(3)	(4)	(5)
	Log deflated	Labor force	Work	Search	Casual
	casual wage	participation			Work
Post*Exposure*Rural	0.058*	0.207**	0.132	0.076**	0.264***
	(0.032)	(0.090)	(0.089)	(0.037)	(0.064)
Post*Exposure*Urban	0.204***	-0.286***	-0.287***	0.002	-0.239***
	(0.038)	(0.087)	(0.088)	(0.037)	(0.071)
p-value(Rural=Urban)	0.000	0.000	0.000	0.003	0.000
Controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Quarter-year FE	Yes	Yes	Yes	Yes	Yes
Post*State FE	Yes	Yes	Yes	Yes	Yes
Observations	19,677	192,124	192,124	192,124	192,124

Table 6.3.2: Cross-District Spillovers, by Sector

Note: Each column represents results from a separate regression. The sample consists of Late district residents, interviewed from July 2004 to June 2005 and from July 2007 – March 2008. Search days, non-labor force participation days and work days are calculated using time allocation responses for the week preceding interview. Log casual wage and log salaried wage is the log of earnings per day worked for individuals who report working in casual labor and salaried work respectively. Daily casual wage and salaried work wage are deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Treatment indicates if district is Early. Exposure is a binary indicator for all districts with at least one Early neighbor. Individual-level controls (age, age squared, gender, indicators for literacy), household-level controls (land possessed, religion and caste group), district-level control (rural fraction of population), year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 10% level.

The inference of backward bending labor supply is tested directly, when cross-district spillovers are split by the household-level monthly per capita expenditure (MPCE), a measure of welfare (see Table 9.6.1 in Appendix). Classifying households above the average MPCE as high-income households and those below as low-income in the sample, the pattern exhibited by estimates is extremely similar to that discussed above in the rural-urban decomposition. Similar to high-income urban areas, exposure increased the wages of high-income households (16.7%) by more than twice the 7.9% increase experienced by low-income households. Further, assignment decreased labor force participation by 0.22 days/week for the high-income households. The effects on search, work and casual work days are similarly opposite in magnitudes and the test for equality of coefficients across categories is again rejected for each outcome. The 0.22 days/week decrease

in casual work for high-income households and a nearly equal increase for low-income households is a strong indicator more than two-thirds of the change in labor supply is driven by changes in time allocated to unskilled labor, which is presumably affected by the exposure shock.

### 6.3.3 Cross-District Spillovers by Season

Since the provision and demand for NREGS work is highly seasonal and concentrated in the agricultural off-season, it is worth examining empirically whether spillovers are differentiated by season. After defining the first two quarters of the year as dry and the last two as rainy, I interact these indicators with the binary exposure variable and the coefficients are reported in Table 6.3.3. By replacing  $Post_t * Exposure_d$  with  $Post_t * Exposure_d * Dry_t$  and  $Post_t * Exposure_d * Rainy_t$ , this formulation splits the spillover effect on late districts by season. The season-year fixed effects are retained. I estimate that casual wage in exposed districts increased by 7.6% (significant at 10%) during the rainy season and 11% during the dry season (significant at 1%). Although the magnitude and precision of the effect is larger in dry season, the null hypothesis of equal increase in both seasons cannot be rejected at any significance level. Further, search in exposed districts increased by 0.07 days/week in the rainy season (significant at 10%), but this coefficient is again not statistically different from the positive effect during the dry season. The effect of exposure on other time allocation outcomes remains insignificant in both seasons. This absence of differential impacts by season could be indicative of two phenomena: i) casual wages in late districts exhibit downward rigidity during the rainy season, or ii) seasonal migrants do not necessarily return to late districts in the rainy season, when NREGS provisioning is low in assigned neighbors, thus maintaining the relative shortage and high wages of unskilled workers.

	(1)	(2)	(3)	(4)	(5)
	Log deflated	Labor force	Work	Search	Casual
	casual wage	participation			Work
Post*Exposure*Rainy	0.076*	0.040	-0.031	0.072*	0.092
	(0.041)	(0.097)	(0.094)	(0.041)	(0.062)
Post*Exposure*Dry	0.110***	0.073	0.059	0.013	0.128
	(0.042)	(0.090)	(0.102)	(0.042)	(0.108)
p-value(Rainy=Dry)	0.530	0.749	0.411	0.198	0.718
Controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Quarter-year FE	Yes	Yes	Yes	Yes	Yes
Post*State FE	Yes	Yes	Yes	Yes	Yes
Observations	19,677	192,124	192,124	192,124	192,124

Table 6.3.3: Cross-District Spillovers, by Season

Note: Each column represents results from a separate regression. The sample consists of Late district residents, interviewed from July 2004 to June 2005 and from July 2007 – March 2008. Search days, non-labor force participation days and work days are calculated using time allocation responses for the week preceding interview. Log casual wage and log salaried wage is the log of earnings per day worked for individuals who report working in casual labor and salaried work respectively. Daily casual wage and salaried work wage are deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Treatment indicates if district is Early. Exposure is a binary indicator for all districts with at least one Early neighbor. The first two quarters of the year are classified as dry and the last two quarters are categorized as rainy. Individual-level controls (age, age squared, gender, indicators for literacy), household-level controls (land possessed, religion and caste group), district-level control (rural fraction of population), year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 10% level.

### 6.4 Placebo Tests

In order to ensure that the effects estimated for late districts are genuinely spillovers and not being driven by a correlation between unobserved variables and exposure, a placebo analysis is carried out, and the effects of fake exposure to early program districts are estimated. In other words, if the parallel paths assumption underlying the difference-in-difference estimation is violated, pre-existing differential outcome trends unrelated to exposure but geographically correlated with exposed districts would be reflected in the placebo sample as well. In particular, the impacts on casual wages, for which positive and significant effects have

been estimated in the main sample, would be replicated even with fake exposure if this concern

was valid. To carry out the placebo analysis, specifications (1) and (2) are implemented using

two rounds of data - NSS 50 (July 1993 - June 1994) and NSS 55 (July 1999 - June 2000) -

which preceded the introduction of NREGS. In order to make the construction of the pre-study period comparable with the study period, the last sub-round of NSS 55 is dropped from the placebo analysis as well. Since no districts received the program between 1993 and 2000, *a priori*, exposure to contiguous neighbors which received the program in 2006 later should have no effect on casual wages in late districts. In the results reported in Table 6.4.1, I estimate that changes in casual wage and time allocation outcomes are statistically insignificant in pseudo-exposed late districts, relative to pseudo-unexposed late districts. Additionally, there is no statistically significant effect of exposure intensity on casual labor wage and time allocation outcomes in Table 6.4.2. These results help validate the interpretation that the study period results are not driven by pre-existing differential trends in labor market outcomes but are instead genuine spillover effects generated by exposure-induced seasonal migration.





Source: NSS 50, 55



Figure 6.4: Labor Force Participation in Restricted Pre-Study Sample – Placebo Test I



Figure 6.5: Search in Restricted Pre-Study Sample – Placebo Test I



Source: NSS 50, 55



Figure 6.6: Work in Restricted Pre-Study Sample – Placebo Test I

Source: NSS 50, 55

Table 6.4.1: Placebo	Test I – Ex	posure to .	Assigned	Neighbors

	(1)	(2)	(3)	(4)
	Log deflated	Labor force	Work	Search
	casual wage	participation		
Post*Exposure	-0.049	0.054	0.005	0.049
	(0.156)	(0.149)	(0.139)	(0.044)
Controls	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Quarter-year FE	Yes	Yes	Yes	Yes
Observations	16,916	165,853	165,853	165,853

Note: Each column represents results from a separate regression. The sample consists of Late district residents, interviewed from July 1993 to June 1994 and from July 1999 – March 2000. Search days, non-labor force participation days and work days are calculated using time allocation responses for the week preceding interview. Log casual wage and log salaried wage is the log of earnings per day worked for individuals who report working in casual labor and salaried work respectively. Daily casual wage and salaried work wage are deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Treatment indicates if district is Early. Exposure is a binary indicator for all districts with at least one Early neighbor. Individual-level controls (age, age squared, gender, indicators for literacy), household-level controls (land possessed, religion and caste group), district-level control (rural fraction of population), year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 10% level.

	(1)			(4)
	(1)	(2)	(3)	(4)
	Log deflated casual wage	Labor force participation	Work	Search
Post*Exposure Intensity	0.008	-0.140	-0.170	0.030
	(0.145)	(0.188)	(0.186)	(0.080)
Controls	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Quarter-year FE	Yes	Yes	Yes	Yes
Observations	16,916	165,853	165,853	165,853

## Table 6.4.2: Placebo Test I - Intensity of Exposure to Assigned Neighbors

Note: Each column represents results from a separate regression. The sample consists of Late district residents, interviewed from July 1993 to June 1994 and from July 1999 – March 2000. Search days, non-labor force participation days and work days are calculated using time allocation responses for the week preceding interview. Log casual wage is the log of earnings per day worked for individuals who report working in casual labor. Daily casual wage is deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Treatment indicates if district is Early. Intensity measures the share of contiguous assigned neighbors as a fraction of all neighbors. Individual-level controls (age, age squared, gender, indicators for literacy), household-level controls (land possessed, religion and caste group), district-level control (rural fraction of population), year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 10% level.

Although the temporal placebo sample described above is constructed to most resemble the study period, this analysis can be extended in two ways. First, we can include the last subround in the post pseudo-exposure period (NSS 55) since there is no corresponding introduction of NREGS in the placebo sample, the criterion that justified the exclusion of the last sub-round from the study period. Second, the robustness of this placebo analysis can be further validated by changing the cutoff quarter used to define when fake exposure kicks in for the pseudoexposed districts. Incorporating both these extensions, I construct a placebo sample comprising eight survey quarterly sub-rounds, and incrementally reduce the post-exposure period by changing the cutoff from the first sub-round to the penultimate sub-round. One of the major drawbacks to this approach is that two of the seven cutoffs result in comparing a one-quarter period with a seven-quarter period, thus making it likely that outlier quarters lead to rejection of the null of parallel trends. Ideally, the weights attached to these results should be symmetrically decreasing from the comparison of four pre-exposure quarters with four post-exposure quarters. In order to remain conservative, I treat the estimates generated by these seven cutoffs equally. Table 6.4.3 summarizes the results from twenty-eight regressions, with four wage and timeallocation outcomes for each of seven specifications generated using different cutoffs. Of these twenty-eight estimates, only one (search days) is statistically significant when the post-exposure period is restricted to the last survey quarter. Given that the probability of generating one significant estimate from twenty-eight is smaller than 5%, we fail to reject the null that this result can be distinguished from random chance. Further, given that this estimate is generated by the comparison of seven pre-exposure quarters with one post-exposure quarter, it can be concluded that the result reflects one unusual quarter rather than systematically different outcome trends across late districts during the pre-study period. The absence of accompanying significant effects on other time-allocation outcomes and casual wages further corroborates this conclusion. In the Appendix (see Table 9.4.1), I report the estimates for an identical set of placebo analyses, using the exposure intensity measure.



Figure 6.7: Casual Wage in Pre-Study Period – Placebo Test II

Source: NSS 50, 55

Figure 6.8: Labor Force Participation in Pre-Study Period – Placebo Test II



Source: NSS 50, 55



Figure 6.9: Work in Pre-Study Period – Placebo Test II

Figure 6.10: Search in Pre-Study Period – Placebo Test II



Source: NSS 50, 55

	(1)	(2)	(2)	(4)
	(1)	(2)	(3)	(4)
	Log deflated	Labor force	Work	Search
	casual wage	participation		
Post = Oct 93 - June 00	0.081	-0.202	-0.165	-0.037
	(0.083)	(0.146)	(0.154)	(0.036)
Post = Jan 94 – June 00	0.122	-0.137	-0.114	-0.023
	(0.077)	(0.139)	(0.141)	(0.028)
Post = Apr 94 – June 00	0.005	-0.087	-0.089	0.002
	(0.078)	(0.143)	(0.140)	(0.037)
Post = July 99 – June 00	-0.054	0.015	-0.006	0.021
	(0.152)	(0.154)	(0.146)	(0.040)
Post = Oct 99 – June 00	-0.028	0.035	0.075	-0.040
	(0.125)	(0.123)	(0.122)	(0.032)
Post = Jan 00 – June 00	-0.057	0.045	0.093	-0.047
	(0.112)	(0.100)	(0.101)	(0.030)
Post = Apr 00 - June 00	-0.029	-0.057	0.027	-0.083**
	(0.083)	(0.105)	(0.114)	(0.042)
Controls	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Quarter-year FE	Yes	Yes	Yes	Yes
Observations	19,628	194,117	194,117	194,117

Table 6.4.3: Cross-District Spillovers in Pre-Study Period – Changing cutoffs

Note: Each row reports results from a separate specification and every cell represents results from a separate regression and. Each specification classifies a progressively shorter post-exposure period, ranging from the second sub-round of NSS 50 to the penultimate sub-round of NSS 55. The sample consists of Late district residents, interviewed from July 1993 to June 1994 and from July 1999 – March 2000. Search days, non-labor force participation days and work days are calculated using time allocation responses for the week preceding interview. Log casual wage is the log of earnings per day worked for individuals who report working as casual labor. Daily casual wage is deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Treatment indicates if district is Early. Exposure is a binary indicator for all districts with at least one Early neighbor. Individual-level controls (age, age squared, gender, indicators for literacy), household-level controls (land possessed, religion and caste group), district-level control (rural fraction of population), year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 10% level.

## 6.5 Robustness Checks

# 6.5.1 State Policies

In India's federal structure, state governments often initiate social programs and policies that directly or indirectly affect markets for unskilled labor. While it is unlikely that multiple states enacted programs correlated with the timing and geography of NREGS exposure in late districts, the existence of these programs could serve to confound the labor market spillover effects estimated in the primary analysis of this study. In order to control for state-specific outcome trends (of which state government run programs are a subset), I include an interaction between *Post* and state fixed effects in specification (1) and the results are presented in Table 6.5.1. Interestingly, the magnitude of spillover effects on casual wage becomes smaller when time-varying state fixed effects are controlled for, with exposure leading to a 6.3% increase in late districts (significant at 10% level). This is the lowest estimate of wage spillovers estimated in this chapter and serves as a useful benchmark of the lower bound of our estimates for comparison purposes. In contrast with the estimation without time-varying state fixed effects, an increase in weekly search of 0.08 days (at 1% level) is also estimated while positive effects on labor force participation and work remain statistically insignificant.

Tuble 0.5.11. Cross District Spinovers, with Time varying State Effects						
	(1)	(2)	(3)	(4)	(5)	
	Log deflated	Log deflated	Labor force	Search	Work	
	casual wage	salary wage	participation			
Post*Exposure	0.063*	-0.010	0.088	0.077**	0.011	
	(0.035)	(0.061)	(0.086)	(0.036)	(0.084)	
Controls	Yes	Yes	Yes	Yes	Yes	
District FE	Yes	Yes	Yes	Yes	Yes	
Quarter-year FE	Yes	Yes	Yes	Yes	Yes	
Post*State FE	Yes	Yes	Yes	Yes	Yes	
Observations	19,677	17,513	192,124	192,124	192,124	

 Table 6.5.1: Cross-District Spillovers, with Time-varying State Effects

Note: Each column represents results from a separate regression. The sample consists of Late district residents, interviewed from July 2004 to June 2005 and from July 2007 – March 2008. Search days, non-labor force

#### Table 6.5.1 (cont'd).

participation days and work days are calculated using time allocation responses for the week preceding interview. Log casual wage and log salaried wage is the log of earnings per day worked for individuals who report working in casual labor and salaried work respectively. Daily casual wage and salaried work wage are deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Treatment indicates if district is Early. Exposure is a binary indicator for all districts with at least one Early neighbor. Time-varying state-level fixed effects are also included in the form of an interaction between post and state identifiers. Individual-level controls (age, age squared, gender, indicators for literacy), household-level controls (land possessed, religion and caste group), district-level control (rural fraction of population), year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 10% level.

#### 6.5.2 Low Coverage States

Since early phase districts were concentrated in eastern and central Indian states, the late districts in these states are more likely to be exposed and have higher exposure intensity than late districts in western and southern India. It could be the case that the spillover effects estimated in the study sample are driven by "high coverage" states where late district individuals have greater choice in terms of migration to high wage destinations. To test this hypothesis, specifications (1) is estimated on a restricted sample consisting only of late districts in large, "low coverage" states, defined as having less than half of their districts receiving the program early. Employing the measures of exposure and exposure intensity, the results in Tables 6.5.2 and 6.5.3 follow the same qualitative pattern observed in study sample results, but with larger magnitudes of increases in casual wage due to exposure (13.5%) and exposure intensity (18.3%). The effects are significant at 1% and 10% levels of significance, respectively, despite the reduced sample size. Estimating larger impacts in "low coverage" states relative to the original sample suggests that there may be diminishing returns to exposure and exposure intensities in terms of casual wage spillovers. Changes in salaried wage and time allocations continue to remain insignificant.

	(1)	(2)	(3)	(4)	(5)
	Log deflated	Log deflated	Search	Labor force	Work
	casual wage	salaried wage		participation	
Post*Exposure	0.135***	-0.001	0.015	0.066	-0.081
	(0.042)	(0.080)	(0.051)	(0.114)	(0.124)
Controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes
Observations	8 029	8 013	69 352	69 352	69 352

 Table 6.5.2: Cross-District Spillovers in Low Coverage States – Exposure to Assigned

 Neighbors

Note: Each column represents results from a separate regression. The sample consists of late phase individuals between the ages of 15 and 59, interviewed from July 2004 to June 2005 and from July 2007 – March 2008. Search days, non-labor force participation days and work days are calculated using time allocation responses for the week preceding interview. Log casual wage and log salaried wage is the log of earnings per day worked for people who report working in casual labor and salaried work respectively. Daily casual wage and salaried work wage are deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Exposure is an indicator for whether district has at least one contiguous assigned neighbor. Individual-level controls (age, age squared, gender, indicators for literacy, land possessed, and caste group), rural fraction of district population, year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 10% level.

 Table 6.5.3: Cross-District Spillovers in Low Coverage States – Intensity of Exposure to

 Assigned Neighbors

	(1)	(2)	(3)	(4)	(5)
	Log deflated	Log deflated	Search	Labor force	Work
	casual wage	salaried wage		participation	
Post*Exposure	0.183*	-0.020	0.031	-0.079	0.048
	(0.104)	(0.091)	(0.083)	(0.218)	(0.238)
Controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes
Observations	8,029	8,013	69,352	69,352	69,352

Note: Each column represents results from a separate regression. The sample consists of late phase individuals between the ages of 15 and 59, interviewed from July 2004 to June 2005 and from July 2007 – March 2008. Search days, non-labor force participation days and work days are calculated using time allocation responses for the week preceding interview. Log casual wage and log salaried wage is the log of earnings per day worked for people who report working in casual labor and salaried work respectively. Daily casual wage and salaried work wage are deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Exposure is an indicator for whether district has at least one contiguous assigned neighbor. Individual-level controls (age, age squared, gender, indicators for literacy, land possessed, and caste group), rural fraction of district population, year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 10% level.

## 6.6 Seasonal Migration and Exposure Intensity

Although the spillover impacts on casual wages estimated in this analysis are motivated as resulting from changes in flows of seasonal migrants to and from late phase districts, the direct impact of exposure on seasonal migration could not be estimated because migration information was not collected in NSS 61 (the round immediately preceding NREGS). While this information was collected in NSS 55 (July 1999 – June 2000) and NSS 64 (July 2007 – June 2008), differences in the definition of short-term migrants across rounds prevent comparability<sup>22</sup> and thus, direct estimation of exposure's direct impact on short- term migration. However, some inferences can be drawn about the empirical relationship between exposure intensity and seasonal migration by exploiting the temporal dimension of NSS 64's survey design, which comprises four quarterly sub-rounds (July – Sep 2007, Oct – Dec 2007, Jan – Mar 2008, and April – June 2008), broadly coinciding with the agricultural cycle.

As a first step, it is useful to visually observe the relationship between seasonal (shortterm) migration and exposure intensity, and further explore whether this pattern differs for short-distance seasonal migration. Figure 6.11 in the appendix graph two measures: i) the percentage of seasonal migrants in the population, and ii) the percentage of seasonal migrants in the population who moved to a destination outside the district, within the same state. Both measures are graphed across three exposure intensity terciles, before and after program assignment to late districts (April 2008). Firstly, Figure 6.9 shows that the relationship between short-distance seasonal migration and exposure intensity is inverted U-shaped in both periods, supporting the higher wage spillovers estimated in low-coverage states relative to the entire

 $<sup>^{22}</sup>$  NSS 55 defines short-term migrants as individuals who stayed away from their usual place of residence for 2 – 6 months during the last year. NSS 64 changed this period to 1 – 6 months.

sample. Secondly, it is evident that the responsiveness of seasonal migration to program assignment varies by distance and exposure intensity in the April – June 2008 quarter, which coincides with the pre- monsoon dry season. Seasonal migration to all destinations increases, suggesting that overall seasonal migration continues to serve as a coping mechanism in the dry season despite the introduction of NREGS, supporting the findings of significant unmet demand for the program (Dutta et al. 2014). On further disaggregation, we find that seasonal migration to districts within the same state declines, and this decline is proportional to exposure intensity. This result is an indicator that in the absence of program assignment, seasonal, short-distance migration was the relevant form of labor movement induced by exposure to assigned neighbors. In other words, seasonal short-distance migration to assigned neighbors served as a substitute for program assignment in late districts in the pre-assignment period.



**Figure 6.11: Intensity of Exposure vs Seasonal Migration in Late Districts** 

Source: NSS 64

## 6.6.1 Cross-district Spillovers, by Landholding Size

One of the key concerns in the results presented above is the composition of seasonal migrants from exposed districts. If it is the case that seasonal migrants consist of predominantly low productivity workers, then the average wage of stayers will be mechanically higher after exposure. Since productivity is unobserved, this hypothesis cannot be tested directly. If we assume that individual-level productivity is positively correlated with household land holdings, we can test whether the exposure shock led to smaller wage increases for large landholders, relative to small landholders. In order to implement this test, I utilize the NSS cutoffs of landholdings under 1 hectare and over 1 hectare to construct indicators for small and large landholdings respectively. Similar to previous heterogeneity results, the cross-district spillovers are now split by this indicator variable. Interestingly, while small landholders do experience a positive and significant increase in casual wage -8.8% - the point estimate is not statistically distinguishable from a noisy 3% increase for large landholders. Further, the changes in time allocated to labor force participation and work are positive (and bigger) for large landholders, in contrast with small landholders, which is inconsistent with a larger wage increase for small landholders. While not conclusive, these findings do not support the hypothesis that cross-district wage spillovers are driven by seasonal migration of low productivity casual workers.

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$- \cdots - \cdots$							
	(1)	(2)	(2)	(3)	(4)		
	Log deflated	Log deflated	Labor force	Work	Search		
	casual wage	salaried wage	participation				
Post*Exposure*Large	0.030	-0.069	0.314**	0.298**	0.016		
	(0.083)	(0.101)	(0.139)	(0.140)	(0.044)		
Post*Exposure*Small	$0.088^{***}$	0.006	0.031	-0.024	0.055		
	(0.032)	(0.063)	(0.080)	(0.080)	(0.036)		
p-value (Large=Small)	0.462	0.411	0.008	0.004	0.205		
Controls	Yes	Yes	Yes	Yes	Yes		
District FE	Yes	Yes	Yes	Yes	Yes		
Quarter-year FE	Yes	Yes	Yes	Yes	Yes		
Observations	19,677	17,513	192,124	192,124	192,124		

Table 6.6.1: Cross-District Spillovers, by Land Possession

Note: Each column represents results from a separate regression. The sample consists of individuals between the ages of 15 and 59, interviewed from July 2004 to June 2005 and from July 2007 – March 2008. The indicator for land is classified as zero if the household possesses 1 or more hectare of land. Search days, labor force participation days and work days are calculated using time allocation responses for the week preceding interview. Log casual wage is the log of earnings per day worked for individuals who report working in casual labor. Daily casual wage is deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Treatment indicates if district is Early. Exposure is a binary indicator for all districts with at least one Early neighbor. Individual-level controls (age, age squared, gender, indicators for literacy), household-level controls (land possessed, religion and caste group), district-level control (rural fraction of population), year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 1% level.

# 6.7 Chapter Summary

This chapter analyzes the general-equilibrium labor market spillovers from assigned to unassigned districts during the study period (July 2005 - March 2008), exploiting the plausibly exogenous variation in exposure to program neighbors. The results demonstrate that contiguity with NREGS neighbors definitively raised casual wage in unassigned late districts. Comparing the magnitude of wage spillovers to program impacts, it is striking that the most conservative impact of exposure (6.3%) is higher than previously estimated intent-to-treat effects on dry season casual wage - 4.7% (Imbert and Papp 2015)<sup>23</sup> and agricultural wage - 5.1% (Berg et

<sup>&</sup>lt;sup>23</sup> In the Appendix, I report results from a partial replication of Imbert & Papp (2015), which generates very similar ITT effects of 4.8% increase in casual wage during the dry season (Table 9.1a). In a separate estimation, I also impose the same study period restrictions as the sample used for computing between spillovers and exclude the April – June 2008 quarter (NSS64's last sub-round). The estimated increase in dry season rural casual wage is 6.6%

al. 2012). Alternatively, this comparison can be made using the upper bound of wage spillovers estimates, which is generated when heterogeneity in exposure intensity is accounted for. Here, program assignment to half their contiguous neighbors is roughly equivalent - 5% increase in casual wage - to program assignment for late districts. Interestingly, the effect on casual wages is higher in urban areas than in rural areas. Regardless of preferred estimate, it is clear that previous intent-to-treat estimates of NREGS's effect on rural unskilled wage were biased because of a violation of the assumption of no interference across assigned and unassigned districts. Although the contexts are not strictly comparable, wage spillovers to contiguous unassigned areas are similar to the Alcott & Keniston (2014) finding that earnings spillovers from natural resource booms were concentrated in nearby counties, relative to faraway counties. Further, given the evidence supporting short-distance seasonal migration being a function of exposure to assigned districts, the findings are along the lines of welfare migration to high-AFDC benefit border counties of neighboring states documented by McKinnish (2005).

The findings for time allocation spillovers, while more tentative for the entire sample, are more definitive when disaggregated by gender and sector. Labor force participation, an aggregate of work and search days, increased by 7.2% in late districts surrounded by assigned neighbors, relative to late districts without any assigned neighbors. Although this effect is not replicated when employing the binary measure of exposure, it is accentuated among men (6.4% increase). Further, a rural-urban decomposition suggests that rising casual wages produces opposite effects on labor supply in rural and urban areas. Labor force participation, the search for work and casual labor increases in rural areas, where pre-exposure wages were lower and wage spillovers

for the latter (Table 9.1b). Neither estimation includes the extensive set of district controls used by Imbert & Papp (2015).
are estimated to be smaller, indicating that the elasticity of labor supply is positive in a lower range of wages. The opposite trends hold in urban areas, pre-exposure wages were higher and estimated wage spillovers are larger, suggesting a backward bending labor supply curve. Evidence supporting the presence of a backward bending labor in the informal urban sector for unskilled workers is interesting because such behavior is usually exhibited at the higher end of the wage distribution and typically among formal sector workers.

### **CHAPTER SEVEN**

## **EMPIRICAL RESULTS: SPILLOVER – ROBUST TREATMENT EFFECTS**

## 7.1 Introduction

The chapter seeks to demonstrate the gains to accounting for cross-district spillovers when estimating three parameters of interest for researchers and policymakers. Section 7.2 presents unadjusted and spillover-robust estimates for Intent-to-Treat, Indirect and Total Intentto-Treat effects of NREGS. The credibility of these estimates is also evaluated by utilizing the pre-study period to conduct placebo tests. In section 7.3, the heterogeneity in total intent-to-treat treatment effects along the gender, season, age and sector dimensions is explored. The last section summarizes key findings from this chapter.

# 7.2 Direct, Indirect and Total Treatment Effects

As demonstrated in the previous chapter, the identifying assumption of zero cross-district spillovers is violated in our study period. This implies that all comparisons of early and late assignment districts which exploit the program's staggered rollout, are likely to produce downward biased estimates of labor market effects. Given this caveat, proceeding with estimations of direct ITT, ITE and TITT have the useful property of providing a lower bound for the program's true treatment effects. Further, these biased estimates serve as a benchmark against which the adjusted ITT, ITE and TITT can be evaluated. As demonstrated in this section, the adjusted treatment effects on wages can be up to 2.5 times the biased estimates of treatment effect.

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#### 7.2.1 Direct Treatment Effects in Rural Areas

The natural starting point for an analysis of treatment effects is the intent-to-treat (ITT) effect of NREGS on rural areas, where every household was eligible for the program. In order to do so, I compare rural labor market outcomes in assignment districts (treatment) to unassigned districts (control), before and after the introduction of the program. The dependent variable in the first column is log of real casual wage, conditional on positive weekly earnings. The next three columns report weekly time allocations for individual-level labor force participation, work and search days. As the results reported in Table 7.2.1 demonstrate, assignment increased rural casual wage by 4.8% (significant at the 5% level) in early districts, relative to late districts. This result is consistent with the theoretical prediction of a fully enforced rural employment guarantee at minimum wage raising the market wage earned by unskilled workers. Further, average force participation increased by 0.1 days/week (significant at the 10% level). Since labor force participation is an aggregate of work and time spent searching for work, this increase partly reflects a substitution of leisure for work, even though the increase in weekly work days is not statistically significant by itself. These time allocation results suggest that in this wage range, the substitution effect dominates the income effect from a higher marginal wage rate.

Interestingly, the magnitude of the ITT effect on casual wage is nearly identical to Imbert and Papp's (2015) ITT estimate during the dry season, as well as the estimates generated from my partial replication of their seasonal analysis, despite different empirical choices (see Table 9.1.1 in Appendix). It is worth reiterating that the working sample constructed for this ITT estimation is different from Imbert and Papp's (2015) sample in the following ways: i) the last quarter of the post-assignment period (NSS 64's April – June 2008 sub-round) has been excluded from the study period to rule out contamination due to program assignment to late districts, ii) the sample is restricted to individuals between the ages of 15 and 59 (as opposed to 18 - 60), with less than tertiary education, and iii) the regression does not include the same set of extensive district-level controls.

				)	
	(1)	(2)	(3)	(4)	(5)
	Log deflated casual wage	Labor force participation	Work	Search	Casual work
Post*Treat==1	0.048**	0.102*	0.064	0.039	0.060
	(0.023)	(0.058)	(0.061)	(0.031)	(0.052)
Controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Quarter-year FE	Yes	Yes	Yes	Yes	Yes
Observations	40,883	313,386	313,386	313,386	313,386

 Table 7.2.1: Direct Intent-to-Treat Effects in Rural Areas (ITT)

Note: Each column represents results from a separate regression. The sample consists of rural residents of Phase I and Phase II (early assignment) districts between the ages of 15 and 59, interviewed from July 2004 to June 2005 and from July 2007 – March 2008. Search days, labor force participation days and work days are calculated using time allocation responses for the week preceding interview. Log casual wage is the log of earnings per day worked for people who report working in casual labor. Daily casual wage is deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Treatment indicates if district received NREGS in Phase I or II. Individual-level controls (age, age squared, gender, indicators for literacy), household-level controls (land possessed, religion and caste group), district-level control (rural fraction of population), year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 1% level.

In this sub-section, I implement specification (4) to estimate ITT effects and ACT simultaneously, thus correcting for the bias introduced by a cross-rural violation of SUTVA. The adjusted ITT is now estimated to be a 13.7% increase in rural casual wage, while ACT is computed to be a 10.3% increase in rural casual wage (significant at the 1% and 5% levels, respectively). We fail to reject the null hypothesis of equal spillover-robust ITT and ACT impacts, an indicator that cross-rural spillovers may be as large as direct treatment effects. The larger adjusted ITT point estimate on casual wage is not surprising given that failing to control for the positive cross-rural wage spillovers biased the previous ITT wage estimates downwards.

Given the cross-rural wage spillover results documented in chapter 6, it is evident that casual wages rose in the subset of unassigned but exposed districts as well, thus attenuating ITT estimates when this utilizing the *entire* set of unassigned districts as a control group. The spillover adjusted ITT effect for time allocated to search increased by 27.2% (0.09 days/week) while the labor force participation coefficient is no longer distinguishable from zero, in contrast to unadjusted ITT (Table 7.2.1). Notably, the point estimates for cross-rural wage and time allocation spillovers in this specification are very similar to the estimates from the restricted sample of unassigned rural areas (see Table 9.2.1 in Appendix).

-	(1)	(2)	(3)	(4)	(5)
	Log deflated	Labor force	Search	Work	Casual work
	casual wage	participation			
Post*Treat==1	0.137***	0.143	0.092*	0.052	0.154*
	(0.044)	(0.089)	(0.049)	(0.090)	(0.086)
Post*Exposure==1	0.103**	0.048	0.063	-0.014	0.112
	(0.044)	(0.090)	(0.048)	(0.093)	(0.080)
P-value (Treat = Exposure)	0.162	0.311	0.425	0.374	0.425
Controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Quarter-year FE	Yes	Yes	Yes	Yes	Yes
Observations	40,331	313,386	313,386	313,386	313,386

1 able 7.2.2 Spillover-Robust Intent-to-1 reat Effects (111
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Note: Each column represents results from a separate regression. The sample consists of rural residents between the ages of 15 and 59, interviewed from July 2004 to June 2005 and from July 2007 – March 2008. Search days, labor force participation days and work days are calculated using time allocation responses for the week preceding interview. Log casual wage is the log of earnings per day worked for individuals who report working in casual labor. Daily casual wage is deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Treatment indicates if district is Early. Exposure is a binary indicator for all districts with at least one Early neighbor. Individual-level controls (age, age squared, gender, indicators for literacy), household-level controls (land possessed, religion and caste group), district-level control (rural fraction of population), year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 1% level.

#### 7.2.2 Indirect Treatment Effects in Urban Areas

Following Angelucci and De Giorgi's (2009) approach to estimating the indirect treatment effects

(ITE) of PROGRESA on ineligibles within treatment villages, I compare the urban residents of Early and

Late districts before and after the introduction of the program. It may be recalled that these indirect treatment effects are within-district spillovers from rural to urban areas. As reported in Table 7.2.2, I estimate that NREGS resulted in urban casual wage increasing by 7.8% (significant at the 5% level). Although the magnitude of this indirect effect is seemingly larger than the direct effect of NREGS on rural areas, the null hypothesis of equal direct and indirect effects cannot be rejected using this restricted urban sample. In terms of time allocation, average labor force participation also increased by 2.6% (0.11 days/week), which is quite similar in magnitude to the estimate for the rural sample. While increased labor force participation translated to positive point estimates on time allocated to work and search for work, these coefficients are not statistically significant.

This pattern of within-district spillovers mirrors the ITT effects on wages and labor force participation. Although unadjusted for cross-urban spillovers, these within-district spillovers provide initial evidence that the geographical and sectoral reach of the program's indirect effects was not limited by its intended targeting to rural households. The finding of higher urban unskilled wage and labor force participation align with Ravi et al. (2012) calculation of a 27.9% reduction in rural-urban migration and a 38.7% reduction in urban unemployment due to NREGS. Qualitatively, the result of urban labor market outcomes being affected by a rural employment guarantee provides support for the theoretical predictions from chapter 4. Although these within-district spillovers are mediated by general equilibrium effects, the indirect effect on ineligibles is similar to consumption of ineligible households being affected by PROGRESA in Angelucci and Di Giorgi (2009).

	(1)	(2)	(3)	(4)	(5)
	Log deflated	Labor force	Work	Search	Casual work
	casual wage	participation			
Post*Treat==1	0.078**	0.108*	0.079	0.028	-0.022
	(0.039)	(0.060)	(0.064)	(0.031)	(0.052)
Controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Quarter-year FE	Yes	Yes	Yes	Yes	Yes
Observations	10,207	138,498	138,498	138,498	138,498

 Table 7.2.3: Indirect Treatment Effects in Urban Areas (ITE)

#### Table 7.2.3 (cont'd).

Note: Each column represents results from a separate regression. The sample consists of urban residents of Phase I and Phase II (early assignment) districts between the ages of 15 and 59, interviewed from July 2004 to June 2005 and from July 2007 – March 2008. Search days, labor force participation days and work days are calculated using time allocation responses for the week preceding interview. Log casual wage is the log of earnings per day worked for individuals who report working in casual labor. Daily casual wage is deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Treatment indicates if district received NREGS in Phase I or II. Individual-level controls (age, age squared, gender, indicators for literacy), household-level controls (land possessed, religion and caste group), district-level control (rural fraction of population), year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 1% level.

Next, the augmented DID specification (6) is implemented on a restricted sample of urban areas to adjust indirect treatment effects for cross-urban spillovers. Table 7.2.4 demonstrates that NREGS raised casual wages in exposed early assignment urban areas by 10.7%, and this effect is significant at the 5% level. The cross-urban exposure effect of NREGS on wages is positive but statistically insignificant. Further, the indirect and exposure effects on all time allocation categories are statistically insignificant as well. The absence of cross-urban spillovers implies that exposure to assignment does not have an effect on unassigned urban areas. This is an interesting result because despite the absence of cross-urban spillovers, the point estimate for spillover-robust wage ITE is higher than unadjusted ITE (Table 7.2.3). Simultaneously, the spillover-robust ITE for labor force participation is now indistinguishable from zero. Lastly, the absence of cross-urban spillovers is again very similar to the large wage and time allocation spillovers detected using the restricted sample of urban unassigned areas (see Table 9.3.1 in Appendix).

	(1)	(2)	(3)	(4)	(5)
	Log deflated	Labor force	Search	Work	Casual work
	casual wage	participation			
Post*Treat==1	0.107**	0.112	0.037	0.075	-0.040
	(0.052)	(0.097)	(0.032)	(0.099)	(0.093)
Post*Exposure==1	0.038	0.006	0.012	-0.005	-0.023
	(0.049)	(0.102)	(0.030)	(0.103)	(0.096)
P-value (Treat = Exposure)	0.102	0.103	0.444	0.250	0.756
Controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Quarter-year FE	Yes	Yes	Yes	Yes	Yes
Observations	10,207	138,498	138,498	138,498	138,498

### Table 7.2.4: Spillover-Robust Indirect Treatment Effects (ITE)

Note: Each column represents results from a separate regression. The sample consists of urban residents between the ages of 15 and 59, interviewed from July 2004 to June 2005 and from July 2007 – March 2008. Search days, labor force participation days and work days are calculated using time allocation responses for the week preceding interview. Log casual wage and log salaried wage is the log of earnings per day worked for individuals who report working in casual labor and salaried work respectively. Daily casual wage and salaried work wage are deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Treatment indicates if district is Early. Exposure is a binary indicator for all districts with at least one Early neighbor. Individual-level controls (age, age squared, gender, indicators for literacy), household-level controls (land possessed, religion and caste group), district-level control (rural fraction of population), year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 1% level.

#### 7.2.3 Total Intent-to-Treat Effects

The last two sections demonstrate that the labor market effects of NREGS were not confined to rural areas, which were solely eligible for the program. Since rural-urban linkages has been shown to generate urban labor markets in the previous empirical work (Ravi et al. 2012, Imbert and Papp 2014), there is a strong case to treat assignment districts as a single unit. Moreover, the placebo tests conducted on the pre-study sample (reported in next sub-section) show that pre-existing differential wage and time allocation trends can be completely ruled out only for the spillover-robust TITT specification, further strengthening the case for focusing on deriving correct estimates for this parameter. This aggregation of rural and urban areas allows us to generate a population-weighted estimate of the overall effect of the program on district-level labor market outcomes. Comparing early and late districts through the study period, I estimate that NREGS assignment raised casual wage by 4.9% (significant at the 5% level). It is instructive that this unadjusted TITT point estimate is much closer to the corresponding estimate from ITT than ITE, reflecting the substantially higher proportion of rural households in the population. Further, labor force participation is also found to have increased by 2.4% (0.1 days/week), which is also extremely similar to the unadjusted ITT point estimate.

				( )	
	(1)	(2)	(3)	(4)	(5)
	Log deflated	Labor force	Work	Search	Casual work
	casual wage	participation			
Post*Treat==1	0.049**	0.098**	0.059	0.040	0.057
	(0.022)	(0.050)	(0.053)	(0.027)	(0.044)
Controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Quarter-year FE	Yes	Yes	Yes	Yes	Yes
Observations	50,538	451,884	451,884	451,884	451,884

Table 7.2.5: Total Intent-to-Treat Effects in Rural and Urban Areas (TITT)

Note: Each column represents results from a separate regression. The sample consists of all residents between the ages of 15 and 59, interviewed from July 2004 to June 2005 and from July 2007 – March 2008. Search days, labor force participation days and work days are calculated using time allocation responses for the week preceding interview. Log casual wage is the log of earnings per day worked for individuals who report working in casual labor. Daily casual wage is deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Treatment indicates if district is Early. Exposure is a binary indicator for all districts with at least one Early neighbor. Individual-level controls (age, age squared, gender, indicators for literacy), household-level controls (land possessed, religion and caste group), district-level control (rural fraction of population), year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 1% level.

In this sub-section, I again implemented the augmented DID specification in order to

adjust TITT estimates after controlling for cross-district spillovers. Table 7.2.6 shows that the spillover-robust effect of assignment on casual wage is an increase of 12.6% and this effect is significant at the 1% level. The cross-district exposure effect of NREGS on wages is an increase of 9.1% (significant at the 5% level), which is very similar to the point estimate - 8.7% – derived using the restricted sample of late districts (see Table 6.2.1). On the other hand, the TITT effects on labor force participation and search are found to be 0.15 and 0.08 days/week respectively

(significant at the 10% and 5% levels, respectively). Cross-district spillovers for labor force participation cannot be detected though, again mirroring the chapter 6 estimate (Table 6.2.1).

	(1)	(2)	(3)	(4)	(5)
	Log deflated	Labor force	Search	Work	Casual work
	casual wage	participation			
Post*Treat==1	0.126***	0.145*	0.079**	0.066	0.139*
	(0.036)	(0.081)	(0.037)	(0.082)	(0.073)
Post*Exposure==1	0.091**	0.057	0.047	0.009	0.098
	(0.035)	(0.080)	(0.036)	(0.081)	(0.068)
P-value (Treat = Exposure)	0.117	0.310	0.268	0.369	0.369
Controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Quarter-year FE	Yes	Yes	Yes	Yes	Yes
Observations	50,538	451,884	451,884	451,884	451,884

 Table 7.2.6: Spillover-Robust Total Intent-to-Treat Effects (TITT)

Note: Each column represents results from a separate regression. The sample consists of residents between the ages of 15 and 59, interviewed from July 2004 to June 2005 and from July 2007 – March 2008. Search days, labor force participation days and work days are calculated using time allocation responses for the week preceding interview. Log casual wage is the log of earnings per day worked for individuals who report working in casual labor. Daily casual wage is deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Treatment indicates if district is Early. Exposure is a binary indicator for all districts with at least one Early neighbor. Individual-level controls (age, age squared, gender, indicators for literacy), household-level controls (land possessed, religion and caste group), district-level control (rural fraction of population), year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 1% level.

### 7.2.4 Placebo Tests for ITT, ITE and TITT

In order to ensure that the spillover-robust treatment effects are not reflective of pre-

existing differential trends, a placebo analysis is carried out, and the effects of fake assignment

are estimated. To conduct the placebo analysis, the augmented DID specification is

implemented using two pre-assignment rounds of data - NSS 50 (July 1993 - June 1994) and

NSS 55 (July 1999 – June 2000). In order to make the construction of the pre-study period

comparable with the study period, the last sub-round of NSS 55 is dropped. In the results

reported in Table 7.2.7, it is evident that rural casual wage trended downwards due to

assignment and exposure in the pre-study period, even as the null of parallel time allocation trends cannot be rejected. The corresponding results for urban areas, reported in Table 7.2.8, are more definitive in that both wages and time allocation trends are not parallel in assigned and unassigned urban areas. Lastly, when rural and urban areas are pooled within districts, the null of parallel trends cannot be rejected for all wage and time allocation outcomes in Table 7.2.9. Cumulatively, these results significantly increase our confidence in spillover-robust TITT effects and the validity of spillover-robust ITE effects is brought most into question. While the spillover-robust ITT time allocation effects are now more credible, the converse is true for wage impacts. Given this inference, the next section focuses on heterogeneity of spillover-robust TITT effects, which are the most credible.

	(1)	(2)	(3)	(4)
	Log deflated	Labor force	Search	Work
	casual wage	participation		
Post*Treat==1	-0.332***	0.163	0.043	0.120
	(0.058)	(0.144)	(0.048)	(0.139)
Post*Exposure==1	-0.141**	-0.046	0.051	-0.096
	(0.062)	(0.149)	(0.047)	(0.145)
Controls	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Quarter-year FE	Yes	Yes	Yes	Yes
Observations	46,384	307,070	307,070	307,070

Table 7.2.7: Placebo Test for Spillover-Robust Intent-to-Treat Effects (ITT)

Note: Each column represents results from a separate regression. The sample consists of residents between the ages of 15 and 59, interviewed from July 2004 to June 2005 and from July 2007 – March 2008. Search days, labor force participation days and work days are calculated using time allocation responses for the week preceding interview. Log casual wage is the log of earnings per day worked for individuals who report working in casual labor. Daily casual wage is deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Treatment indicates if district is Early. Exposure is a binary indicator for all districts with at least one Early neighbor. Individual-level controls (age, age squared, gender, indicators for literacy), household-level controls (land possessed, religion and caste group), district-level control (rural fraction of population), year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 1% level.

	(1)	(2)	(3)	(4)		
	Log deflated	Labor force	Search	Work		
	casual wage	participation				
Post*Treat==1	0.866***	-0.049	-0.178***	0.129		
	(0.115)	(0.175)	(0.063)	(0.167)		
Post*Exposure==1	0.837***	0.416***	-0.029***	0.444***		
	(0.021)	(0.024)	(0.007)	(0.021)		
Controls	Yes	Yes	Yes	Yes		
District FE	Yes	Yes	Yes	Yes		
Quarter-year FE	Yes	Yes	Yes	Yes		
Observations	7,926	103,922	103,922	103,922		

Table 7.2.8: Placebo Test for Spillover-Robust Indirect Treatment Effects (ITE)

Note: Each column represents results from a separate regression. The sample consists of residents between the ages of 15 and 59, interviewed from July 2004 to June 2005 and from July 2007 – March 2008. Search days, labor force participation days and work days are calculated using time allocation responses for the week preceding interview. Log casual wage is the log of earnings per day worked for individuals who report working in casual labor. Daily casual wage is deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Treatment indicates if district is Early. Exposure is a binary indicator for all districts with at least one Early neighbor. Individual-level controls (age, age squared, gender, indicators for literacy), household-level controls (land possessed, religion and caste group), district-level control (rural fraction of population), year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 1% level.

	(1)	(2)	(3)	(4)
	Log deflated	Labor force	Search	Work
	casual wage	participation		
Post*Treat==1	-0.083	0.209	0.011	0.198
	(0.150)	(0.147)	(0.043)	(0.136)
Post*Exposure==1	-0.048	0.049	0.048	0.001
	(0.151)	(0.150)	(0.043)	(0.140)
Controls	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Quarter-year FE	Yes	Yes	Yes	Yes
Observations	54,310	410,992	410,992	410,992

#### Table 7.2.9: Placebo Test for Spillover-Robust Total Intent-to-Treat Effects (TITT)

Note: Each column represents results from a separate regression. The sample consists of residents between the ages of 15 and 59, interviewed from July 2004 to June 2005 and from July 2007 – March 2008. Search days, labor force participation days and work days are calculated using time allocation responses for the week preceding interview. Log casual wage is the log of earnings per day worked for individuals who report working in casual labor. Daily casual wage is deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Treatment indicates if district is Early. Exposure is a binary indicator for all districts with at least one Early neighbor. Individual-level controls (age, age squared, gender, indicators for literacy), household-level controls (land possessed, religion and caste group), district-level control (rural fraction of population), year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 1% level.

# 7.3 Heterogeneity of Total Intent-to-Treat Effects

### 7.3.1 Spillover-Robust Total Intent-to-Treat Effects, by Gender

In this section, my analysis disaggregates NREGS' labor market impacts by gender, after accounting for cross-district spillovers. Previous work has demonstrated that the program's guarantee of cash wages equal with men having a higher direct effect on rural women's wages and labor force participation (Azam 2012, Khera and Nayak 2009). It is plausible that access to the program reduced rural-urban migration differentially for women given its provision of women friendly work close to home. This differential reduction in female in-migrants could generate higher unskilled wages for urban women relative to men, particularly if male and female unskilled labor are not perfectly substitutable. On the other hand, the *level* of employment related rural-urban migration by women is quite low and tends to be concentrated among men<sup>24</sup>. If within-district spillovers to urban markets are driven by the level of reduced inmigration by rural males, the resulting rise in unskilled wage could be concentrated among men. Given these offsetting effects, it is not clear *a priori* if any gender-differentiation is to be expected for the indirect effects of NREGS. This ambiguity is extended when we seek to compute a spillover-robust TITT which is an aggregation of direct and indirect labor market effects.

In order to detect gender-differentiated labor market impacts empirically, indicators for men and women are interacted with the  $Post_{jt}*Treat_j$  term in the augmented DID specification and the coefficients are reported in Table 7.3.1. I find that, casual wage for men increased by 10.5% in early districts, relative to late districts. On the other hand, the increase in casual wage

<sup>&</sup>lt;sup>24</sup> 43% of male rural-urban migrants cite employment as their reason for migration; the corresponding figure for women is only 2% (Ravi et al. 2012). While the determinants of higher employment-related migration by men are beyond the scope of this analysis, higher barriers to out-migration can be considered as a larger variable cost associated with rural women working outside their origin.

for women was estimated to be larger -16.9% (both significant at the 1% level). Since the undifferentiated point estimate for wage impact in Table 7.2.5 -12.6% – is a weighted average of these gender impacts, it is not surprising that the former falls in between the two gender estimates. The null hypothesis of equal wage increases by gender can be safely rejected. It is not immediate whether the larger wage impacts for women are being driven by the program's direct impact on rural areas, or by its indirect impact on urban areas. Yet, it is clear that the net effect on women's unskilled wage is larger than the corresponding net effect on men's unskilled wage. The point estimate for cross-district wage spillovers -9% – remains very similar to the estimated generated in the previous section.

Disaggregating indirect time allocation effects by gender, I estimate that assignment increased male labor force participation 0.28 days/week (significant at the 1% level). More than half this time translated to increased work – 0.16 days/week (significant at the 10% level) – and the rest comprised increased search for employment (0.12 days/week, significant at the 1% level). In particular, casual work increased by 0.25 days/week, which is further corroboration that these effects are driven by program assignment to NREGS. The corresponding effects on time allocation for women are insignificant. The null hypothesis of equal time allocation effects by gender is rejected for all three outcomes. While caveats relating to whether direct or indirect effects drive these larger impacts for men apply here as well, these tests confirm that the TITT effects estimated in Table 7.2.5 were driven by time allocation changes reinforce each other, it is likely that reduction in male rural-urban migration affects net time allocation while reduction in female rural-urban migration affects unskilled wages. While the first is a level effect, the latter is a change effect.

L L					
	(1)	(2)	(3)	(4)	(5)
	Log deflated	Labor force	Work	Search	Casual
	casual wage	participation			work
Post*Treat*Men	0.105***	0.276***	0.158*	0.118***	0.254***
	(0.036)	(0.088)	(0.087)	(0.041)	(0.082)
Post*Treat*Women	0.169***	0.025	-0.018	0.043	0.033
	(0.037)	(0.091)	(0.091)	(0.037)	(0.073)
Post*Treat*Exposure	0.090***	0.057	0.010	0.047	0.098
	(0.035)	(0.080)	(0.081)	(0.036)	0.254***
p-value (Men=Women)	0.001	0.001	0.016	0.001	0.001
Controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Quarter-year FE	Yes	Yes	Yes	Yes	Yes
Observations	50,538	451,884	451,884	451,884	451,884

Table 7.3.1: Spillover-Robust Total Intent-to-Treat Effects (TITT), by Gender

Note: Each column represents results from a separate regression. The sample consists of urban residents between the ages of 15 and 59, interviewed from July 2004 to June 2005 and from July 2007 – March 2008. Search days, labor force participation days and work days are calculated using time allocation responses for the week preceding interview. Log casual wage is the log of earnings per day worked for individuals who report working in casual labor. Daily casual wage is deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Treatment indicates if district is Early. Exposure is a binary indicator for all districts with at least one Early neighbor. Individual-level controls (age, age squared, gender, indicators for literacy), household-level controls (land possessed, religion and caste group), district-level control (rural fraction of population), year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 1% level.

### 7.3.2 Spillover-Robust Total Intent-to-Treat Treatment Effects, by Season

Given our knowledge about NREGS design and previous work showing higher effects during the dry season (Imbert and Papp 2015, Zimmerman 2015), it is pertinent to enquire if the program's TITT effects vary on the season dimension. In principle, if reduced in-migration to urban areas is concentrated in the dry season when NREGS provisioning is higher in rural areas, then we should expect to find higher unskilled wages in both rural and urban areas, during the dry season. This would translate to higher unskilled TITT effects in the dry season, even when the analysis is at the district level. Alternatively, if the demand and supply for public works is not differentiated by season, then reductions in rural-urban migration and consequently, changes in unskilled wage should be equal across seasons as well. The corresponding differentiation of time allocation by season follows the same reasoning.

To test the null hypothesis of equal labor market effects across seasons, indicators for dry and rainy quarters are interacted with the *Postjt\*Treatj* term in the augmented DID specification and the coefficients are reported in Table 7.3.2. I find that, assignment raised casual wage by 14.5% during the rainy season (significant at the 10% level). A similar magnitude of increase during the dry season remains – 11.6% – is estimated. The null hypothesis of equal casual wage increases by season cannot be rejected. Further, the cross-district wage spillover is estimated to 9.1%, again very similar to the point estimate from undifferentiated TITT specification.

Disaggregating indirect time allocation effects, I find evidence supporting the prior that NREGS provision was concentrated during the dry season. First, time spent working is found to be positive and higher in the dry season even though the point estimates are not statistically significant in either season. Second, casual work, most likely to be affected by the employment guarantee, is 0.19 days/week higher during the dry season and this change is larger than the rainy season. Labor force participation increased by 0.17 days/week (significant at the 10% level) during the dry season but cannot be distinguished from the statistically insignificant in both seasons and the null of equal increases cannot be rejected. It is plausible that the failure to reject the null of equal increases for wage, labor force participation and search days, is due to the binary indicator for dry and rainy season unable to capture the temporal variation in monsoon rainfall across districts. However, the large change in casual work overcomes this

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coarseness of the season indicator. The cross-district spillover estimates are statistically

significant for all time allocation categories.

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	(1)	(2)	(3)	(4)	(5)	
	Log deflated	Labor force	Work	Search	Casual work	
	casual wage	participation				
Post*Treat*Rainy	0.145***	0.103	-0.001	0.104**	0.041	
	(0.036)	(0.085)	(0.088)	(0.042)	(0.082)	
Post*Treat*Dry	0.116***	0.166*	0.100	0.066*	0.187**	
	(0.038)	(0.085)	(0.085)	(0.038)	(0.073)	
Post*Exposure	0.091***	0.056	0.009	0.047	0.098	
	(0.035)	(0.080)	(0.081)	(0.036)	(0.068)	
p-value (Rainy=Dry)	0.178	0.223	0.080	0.186	0.002	
Controls	Yes	Yes	Yes	Yes	Yes	
District FE	Yes	Yes	Yes	Yes	Yes	
Quarter-year FE	Yes	Yes	Yes	Yes	Yes	
Observations	50,538	451,884	451,884	451,884	451,884	

Table 7.3.2: Spillover-Robust Total Intent-to-Treat Treatment Effects (TITT), by Season

Note: Each column represents results from a separate regression. The sample consists of urban residents between the ages of 15 and 59, interviewed from July 2004 to June 2005 and from July 2007 – March 2008. The first two quarters of the year are classified as dry and the last two as rainy. Search days, labor force participation days and work days are calculated using time allocation responses for the week preceding interview. Log casual wage is the log of earnings per day worked for individuals who report working in casual labor. Daily casual wage is deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Treatment indicates if district is Early. Exposure is a binary indicator for all districts with at least one Early neighbor. Individual-level controls (age, age squared, gender, indicators for literacy), household-level controls (land possessed, religion and caste group), district-level control (rural fraction of population), year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 1% level.

## 7.3.3 Spillover-Robust Total Intent-to-Treat Effects on Child Labor

Recent empirical work has shown that NREGS assignment is associated with a higher likelihood of older rural children (aged 13 - 17) working, and lower likelihood of being enrolled in school (Shah and Steinberg 2015, Islam and Sivasankaran 2014, Li and Sekhri 2013). Since these results are driven by the unintended increase in the opportunity costs of schooling due to the program's employment guarantee, it is logical to explore whether assigned districts, when treated as whole units, experience similar changes, given general equilibrium mediated higher returns to market work as unskilled labor. Including children aged 13 and 14 in the working sample, I now define 13 - 17 year olds as "young" and 18 - 59 year old individuals as "old". The following estimation interacts these age categories with the *Postjt\*Treatj* term to examine if the magnitude of change in casual wages and time allocation is different across these groups. The results, summarized in 7.3.4 show that, casual wage for child labor increased by 7.9% in assigned districts, relative to unassigned districts (significant at the 10% level). The increase in wage for older unskilled workers is even larger and the point estimate is 12.4%. Larger wage gains for older workers could be driven by imperfect substitutability between young and old, but this specification does not allow a further exploration of this conjecture. Despite the inclusion of younger individuals in the working sample, the point estimate for cross-district spillovers is still similar to the undifferentiated TITT estimate.

In terms of time allocation outcomes, the spillover-robust TITT effects exhibit an interesting pattern, with opposite effects for older and younger workers. Labor force participation increased for older workers by 0.26 days/week, but decreased by 0.41 days/week for younger workers in early districts, relative to late districts (both significant at the 1% level). This translates to older workers working 0.16 days/week more but younger workers reduce work by 0.39 days/week (significant at the 10% and 1% levels respectively). The opposite labor force participation changes also extend to 0.10 days/week more search for employment (significant at the 1% level) for older workers and decreased search for younger workers. Finally, older workers work 0.19 days/week more in unskilled jobs while younger workers work 0.14 days/week less, confirming that the time allocation results are driven by changes in unskilled work. The null hypothesis of equal time allocation effects by age categories can be rejected for all time allocation outcomes. These tests support the explanation that in the presence of a larger change

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in marginal wage rate for older workers, parents re-optimize and work more, while reducing hours worked by their children. This results in increased work and time spent searching for work, reflected in higher time allocations for work and search for the older workers. It is not immediate if this behavioral change is driven by a large income effect or lack of substitutability of labor between older and younger workers for the type of unskilled employment which now commands a higher wage. Lastly, the cross-district spillover estimates are insignificant for all time allocation categories. Given the seeming contradiction between these results and previous empirical work showing increased child labor, it must be emphasized that this sample pools older and younger workers from both rural and urban areas. When the population is defined to be only workers aged 15 - 17 and the last survey quarter is retained, previous results can be partially replicated. The point estimates for child labor force participation are positive and significant but all other time allocation outcomes are insignificant (see Appendix Table 9.5.1).

	(1)	(2)	(3)	(4)	(5)
	Log deflated	Labor force	Work	Search	Casual work
	casual wage	participation			
Post*Treat*Young	0.079**	-0.412***	-0.386***	-0.026	-0.134**
	(0.039)	(0.091)	(0.086)	(0.037)	(0.068)
Post*Treat*Old	0.124***	0.257***	0.156*	0.101***	0.185***
	(0.036)	(0.079)	(0.079)	(0.036)	(0.070)
Post*Exposure	0.086**	0.054	0.005	0.049	0.092
	(0.035)	(0.078)	(0.078)	(0.035)	(0.064)
p-value (Young=Old)	0.021	0.000	0.000	0.000	0.000
Controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Quarter-year FE	Yes	Yes	Yes	Yes	Yes
Observations	51,182	486,718	486,718	486,718	486,718

	Table '	7.3.3: S	pillover-Robi	ıst Total Intent	-to-Treat Eff	ects (TITT)	by.	Age
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Note: Each column represents results from a separate regression. The sample consists of urban residents between the ages of 13 and 59, interviewed from July 2004 to June 2005 and then from July 2007 – March 2008. Individuals between the ages of 13 and 17 are classified as young and those between the ages of 18 and 59 are classified as old. Search days, labor force participation days and work days are calculated using time allocation responses for the week preceding interview. Log casual wage is the log of earnings per day worked for individuals who report working in casual labor. Daily casual wage is deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Treatment indicates if district is Early. Exposure is a binary indicator for all

#### Table 7.3.3 (cont'd).

districts with at least one Early neighbor. Individual-level controls (age, age squared, gender, indicators for literacy), household-level controls (land possessed, religion and caste group), district-level control (rural fraction of population), year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 1% level.

## 7.3.4 Spillover-Robust Total Intent-to-Treat Effects, by Sector

The following estimation interacts indicators for rural and urban areas with the *Postjt\*Treatj* term to test if the magnitude of change in casual wages and time allocation varies by sector within districts. Assignment resulted in casual wage increasing in urban areas increased by 27.4% while the corresponding increase in rural areas is smaller, at 10.6% (point estimates and p-values for test of equality are significant at the 1% level). The larger wage gains for urban areas within assignment districts are very similar to the finding of larger cross-district wage spillovers in chapter 6 (see Table 6.3.2), supporting the interpretation that general equilibrium spillovers generate both patterns. The point estimate for cross-district spillovers (8.6%, significant at 5% level) in this specification remains similar to the undifferentiated TITT estimate.

In terms of time allocation outcomes, the spillover-robust TITT effects exhibit opposite signs for rural and urban workers. Assignment increased rural labor force participation by 0.27 days/week, but decreased urban labor force participation by 0.30 days/week (both significant at the 1% level). This translated to rural workers working 0.09 days/week more (significant at the 5% level) but the change for urban workers cannot be detected statistically. Employment search increased by 0.17 days/week for rural workers and decreased for urban workers by 0.33 days/week. The null hypothesis of equal time

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allocation effects by sector can be rejected for all three time allocation outcomes, thus confirming that time allocation changes are larger in the rural sector. The rural and urban point estimates and tests again mirror the sectoral cross-district spillover estimated in chapter 6 (see Table 6.3.2). Similar to cross-district spillovers, these opposite substitutions of labor force participation for work in urban and rural areas within districts can be interpreted as support for the existence of a backward bending labor supply curve in the former and an upward sloping curve in the latter.

	(1)	(2)	(3)	(4)
	Log deflated	Labor force	Search	Work
	casual wage	participation		
Post*Treat*Urban	0.274***	-0.295***	-0.326***	0.031
	(0.041)	(0.096)	(0.098)	(0.039)
Post*Treat*Rural	0.106***	0.266***	0.174**	0.092**
	(0.035)	(0.085)	(0.086)	(0.038)
Post*Exposure	0.089***	0.065	0.017	0.048
	(0.034)	(0.080)	(0.082)	(0.036)
P-value (Urban=Rural)	0.000	0.000	0.000	0.014
Controls	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Quarter-year FE	Yes	Yes	Yes	Yes
Observations	50,338	451,884	451,884	451,884

 Table 7.3.4: Spillover-Robust Total Intent-to-Treat Effects (TITT), by Sector

Note: Each column represents results from a separate regression. The sample consists of all residents between the ages of 15 and 59, interviewed from July 2004 to June 2005 and from July 2007 – March 2008Search days, labor force participation days and work days are calculated using time allocation responses for the week preceding interview. Log casual wage is the log of earnings per day worked for individuals who report working in casual labor. Daily casual wage is deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Treatment indicates if district is Early. Exposure is a binary indicator for all districts with at least one Early neighbor. Individual-level controls (age, age squared, gender, indicators for literacy), household-level controls (land possessed, religion and caste group), district-level control (rural fraction of population), year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 1% level.

Repeating the exercise from the previous chapter, the inference of backward bending

labor supply is again tested more directly, when spillover-robust TITT effects are split by the

household-level MPCE (see Table 9.6.2 in Appendix). Classifying households above the average

MPCE as high-income households and those below as low-income, the estimates exhibit a

pattern very similar to the rural-urban decomposition. Assignment is found to have increased the wages of high-income households by nearly twice the 11.8% increase experienced by lowincome households. Further, assignment decreased labor force participation by 0.18 days/week for the high-income households and increased it by 0.24 days/week for the low-income households. The effects on search, work and casual work days are similarly opposite in magnitudes and the test for equality of coefficients across categories is again rejected for each outcome. Cross-district wage spillovers are computed to be 9%, further validating the previous estimates derived in this chapter. Repeating this analysis for the restricted sample of rural areas produces a similar pattern in the high and low income household estimates as well (See Table 9.6.3 in Appendix).

Lastly, splitting assignment by landholding size, a measure of wealth rather than income, produces more mixed estimates (see Table 9.7.1 in Appendix). Assignment increased the wages of both large and small landholders but the difference cannot be statistically distinguished from zero. While assignment decreased time allocated to casual work by 0.28 days/week for the large landholders, the same outcome increased by 0.17 days/week for small landholders, providing some support for the backward bending labor supply explanation. In contrast to the household expenditure results though, the effects on labor force participation and work have the same sign and the test for equality across landholding categories cannot be rejected. Cross-district wage spillovers are computed to be 9.1%. In the interest of completeness, I also repeat this exercise using the restricted sample of rural areas (see Table 9.7.2 in Appendix). Since the pattern of spillover-robust ITT estimates is extremely similar to the spillover-robust TITT estimates, the above discussion is not repeated.

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# 7.4 Chapter Summary

This chapter demonstrates the differences between adjusted and unadjusted treatment effect estimates of NREGS. In the case of TITT effects on unskilled wages, adjusting for crossdistrict spillovers produces point estimates that are 2.5 times the unadjusted TITT point estimates. For time allocation outcomes, the corresponding difference is a factor of 1.5. Further, the placebo tests discussed in the chapter show that even spillover-robust ITT and ITE estimates may not be credible, given the differential outcome trends in assigned and unassigned urban areas, even before the introduction of the employment guarantee. In combination with the policy relevance of TITT, this finding strengthens the case for focusing empirical research on computing precise estimates of the district-level impacts of the program. Lastly, the heterogeneity in TITT impacts along the gender, season and sector dimensions is examined and these estimates largely follow the patterns already documented in chapter 6. In addition, TITT effects on child labor, relative to older workers are also explored. In contrast with previous work, these results support the explanation that the rise in unskilled wages was larger for older workers and their time spent working increased, while children withdrew from the labor market.

### **CHAPTER EIGHT**

### CONCLUSION

This dissertation examines the general equilibrium spillovers generated by India's National Rural Employment Guarantee Scheme. The empirical analysis demonstrates that despite its targeting at rural areas in assigned districts during the study period, the program generated large wage and employment spillovers in nearby unassigned labor markets. This study also shows that accounting for the downward bias introduced by general equilibrium spillovers results in substantially larger treatment effects. These findings have major implications for how public works programs are evaluated by researchers and policymakers.

The first set of findings show that exposure to assigned neighbors raised the returns to unskilled casual labor by at least 6.3% in adjacent unassigned districts, if the most conservative estimates are considered to be the most credible. The corresponding impacts on time allocation are mixed, with spent searching for work increasing by 28.6% (0.08 days/week), but no detectable increase in work itself. When exposure intensity, a finer measure of neighbor linkages is employed, it is found that assignment to half the neighbors raises casual wage by 5% in adjacent unassigned districts. Labor force participation increases by 2.6% (0.11 days/week) as well for the corresponding increase in assignment of neighbors. The exposure intensity findings for cross-district spillovers are robust to the size of rural labor markets in neighboring districts.

The second part of this analysis demonstrates the gains to adjusting treatment effect estimates by accounting for cross-district spillovers. Although spillover-robust estimates are reported for three parameters – ITT, ITE and TITT, the relevant local economy for calculation of treatment effects is not an obvious *a priori* choice. Given NREGS's district-level assignment and targeting of rural households, previous labor market evaluations have chosen to restrict their analysis to rural areas within districts, in order to estimate the ITT parameter (Imbert and Papp 2015, Zimmerman 2015, Berg et al. 2014, Azam 2012). While that approach is a reasonable first cut at estimating treatment effects, it is likely biased downwards because of within-district spillovers to urban labor markets as well as cross-district spillovers to nearby unassigned districts. In order to account for these sources of bias, this analysis aggregates rural and urban areas at the district level and accounts for cross-district spillovers in single estimation strategy. Given the larger geographic and sectoral scope of spillover-robust TITT effects, there is a case for using the entire district as the unit from a policy relevance point of view. Further, TITT estimates have the most credibility for a researcher, as demonstrated by the placebo tests in chapter 6.

Table 8.2.1 compiles point estimates for unskilled wage, using the unadjusted and spillover-robust estimations of chapters 6 and 7 to summarize the differences between former and latter. Column (1) reports the point estimates of unadjusted treatment effects and cross-district while column (2) reports adjusted treatment effects and cross-district spillovers estimates, derived from the spillover-robust specifications in chapter 7. First, it is clear that spillover-robust ITT, ITE and TITT point estimates are uniformly higher than unadjusted estimates, confirming the gains from accounting for spillovers from assigned to unassigned areas. Second, the point estimates for cross-rural, cross-urban and cross-district wage spillovers are very similar across

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columns, suggesting that these estimates are robust to specification choice. Lastly, both crossurban estimates are insignificant. Given the initial outcome differences across assigned and unassigned areas, as well as the detection of non-parallel trends in the placebo period, the adjusted and spillover-robust ITE impacts are not particularly credible. This implies that the ITE estimates below do not necessarily confirm an absence of within-district spillovers. It is just that detecting such spillovers, if present, is not possible given the violation of identifying assumptions for these estimation strategies.

Table 8.2.1 Wage Treatment Effects and Spillovers – A Summary						
	(1)	(2)				
	Unadjusted Estimates	Spillover-robust Estimates				
ITT	0.048**	0.137***				
	(0.023)	(0.044)				
	2.9	3.1				
Cross – Rural Spillover	0.101**	0.103**				
	(0.041)	(0.044)				
	2.4	2.3				
ITE	0.078**	0.107**				
	(0.039)	(0.052)				
	2.0	2.1				
Cross – Urban Spillover	0.039	0.038				
	(0.043)	(0.049)				
	0.9	0.8				
TITT	0.049**	0.126***				
	(0.022)	(0.036)				
	2.2	3.5				
Cross – District Spillover (ACT)	0.087**	0.091**				
	(0.032)	(0.035)				
	2.7	2.6				

Note: The cross-rural and cross-urban spillovers reported in column 1 are compiled from restricted urban and rural sample estimations of between spillovers in order to be comparable to restricted samples used for ITT and ITE estimation. These tables are reported in the Appendix (Tables 9.2, 9.3). The ITT, ITE and TITT coefficients, standard errors (in parentheses) and number of standard deviations away from mean reported in columns 1 and 2 are calculated using Tables 7.2.1 – 7.2.6. All coefficients and standard errors for cross-rural, cross-urban and cross-district are compiled from the estimations employing the binary indicator of exposure.

Table 8.2.2 compiles the corresponding point estimates for time allocated to casual work, following the same format as Table 8.2.1. The first point of comparison shows that the spillover-robust ITT and TITT estimates are larger in magnitude than their unadjusted counterparts and are statistically insignificant. While ITE estimates are also similar in sign and magnitude, they are insignificant. The comments about inability to rule out within-district spillovers given the credibility of these estimates from the previous paragraph apply here as well. The robustness of cross-rural and cross-district wage spillovers to specification choice is again validated here.

Table 8.2.2 Casual Work Treatment Effects and Spillovers – A Summary						
	(1)	(2)				
	Unadjusted Estimates	Spillover-robust Estimates				
ITT	0.060	0.154*				
	(0.052)	(0.086)				
-	1.2	1.8				
Cross – Rural Spillover	0.114	0.112				
	(0.079)	(0.080)				
-	1.4	1.4				
ITE	-0.022	-0.040				
	(0.052)	(0.093)				
-	0.4	0.4				
Cross – Urban Spillover	-0.017	-0.023				
	(0.093)	(0.096)				
-	-0.2	0.2				
TITT	0.057	0.139*				
	(0.044)	(0.073)				
-	1.3	1.9				
Cross – District Spillover (ACT)	0.104	0.098				
	(0.065)	(0.068)				
-	1.6	1.4				

Note: The cross-rural and cross-urban spillovers reported in column 1 are compiled from restricted urban and rural sample estimations of between spillovers in order to be comparable to restricted samples used for ITT and ITE estimation. These tables are reported in the Appendix (Tables 9.2, 9.3). The ITT, ITE and TITT coefficients, standard errors (in parentheses) and number of standard deviations away from mean reported in columns 1 and 2 are

#### Table 8.2.2 (cont'd).

calculated using Tables 7.2.1 - 7.2.6. All coefficients and standard errors for cross-rural, cross-urban and cross-district are compiled from the estimations employing the binary indicator of exposure.

Comparing the magnitude of spillover-robust ITT - 13.7% – to previous evaluations of the program's wage impacts, it is striking that this point estimate is nearly three times the 4.7% and 5.1% increases in rural casual wages estimated by Imbert and Papp (2015) and Berg et al. (2012) respectively. Even the spillover-robust TITT, arguably the parameter of most interest, is 2.5 times previous ITT estimates. Further, the hypothesis of cross-spillover wage spillovers being as large as TITT cannot be rejected, implying that the wage impacts of NREGS assignment are transmitted completely through general equilibrium spillovers. Although similar in magnitudes, the spillover-robust TITT and ITT parameters are estimated at different levels of aggregation and both are of policy relevance. In other words, the cost-benefit analysis of public works must account for general equilibrium spillovers in order to accurately compare this policy tool to its alternatives.

Given the presence and magnitude of cross-district spillovers, it is evident that the gains from public works programs are not completely accrued by the jurisdictions in which they are implemented. Since public works programs are often started at the level of sub-national governments -- the immediate precursor to NREGS was the much studied state-level Employment Guarantee Scheme in Maharashtra, 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 or provincial government chooses to discontinue a public works 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 dissertation to assess how their jurisdiction is likely to be impacted by public works programs in adjoining jurisdictions. Given the benefit to waiting for a neighboring jurisdiction to start the program, it follows that in periods of economic distress when the stabilization benefits of public works programs are most justified, they are least likely to be enacted because of the decline in government revenues.

Another important takeaway from these findings is that given finite government resources, a strategic selection of highly connected labor markets could widen the geographic scope of spillovers from public works programs, thus implying cost-saving. Practically, this would imply a criterion which accounts for the number of neighbors of selected areas, in addition to the economic backwardness criteria that often determine eligibility. Road and rail networks could also be part of a composite index which can be utilized to maximize expected benefits from public works, given other policy objectives. In conclusion, it is clear that while post-facto accounting for general equilibrium spillovers is necessary for choosing among policy alternatives, the parameters discussed in this study are critical inputs that enable a more costeffective selection of areas eligible for a public works program, conditional on that being the selected policy option.

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APPENDICES

	(1)	(2)	(3)	(4)
	Log deflated	Unemployment	NLFP Days	Work days
	casual wage	Days	-	-
Post*Treat*Dry	0.048**	0.069**	-0.097	0.028
	(0.024)	(0.032)	(0.060)	(0.064)
Post*Treat*Rainy	0.038	0.019	-0.125**	0.106
	(0.024)	(0.033)	(0.062)	(0.066)
P-value (Dry=Rainy)	0.599	0.072	0.586	0.168
Controls	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Quarter-year FE	Yes	Yes	Yes	Yes
Observations	47,290	355,849	355,849	355,849

APPENDIX A ADDITIONAL TABLES	
Table 9.1.1: Intent-to-Treat Effects - Partial Replication of Imbert and Papp (201	5)

Note: Each column represents results from a separate regression. The sample consists of rural residents between the ages of 15 and 59, interviewed from July 2004 to June 2005 and from July 2007 – March 2008. Search days, labor force participation days and work days are calculated using time allocation responses for the week preceding interview. Log casual wage and log salaried wage is the log of earnings per day worked for people who report working in casual labor and salaried work respectively. Daily casual wage and salaried work wage are deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Treatment indicates if district received NREGS in Phase I or II. Individual-level controls (age, age squared, gender, indicators for literacy), household-level controls (land possessed, religion and caste group), district-level control (rural fraction of population), year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 1% level.

	Tal	ole 9.1	.2:	Intent-to-	<b>Treat</b>	Effects -	<b>Partial</b>	Rep	lication (	of Iı	mbert	and	Papp	(2015) -	- II
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		<b>⊥</b>	L 1	
	(1)	(2)	(3)	(4)
	Log deflated	Labor force	Work	Search
	casual wage	participation		
Post*Treat*Dry	0.066**	0.063	-0.005	0.068*
	(0.027)	(0.068)	(0.073)	(0.038)
Post*Treat*Rainy	0.038	0.122*	0.098	0.024
	(0.024)	(0.062)	(0.065)	(0.033)
P-value (Dry=Rainy)	0.225	0.317	0.111	0.181
Controls	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Quarter-year FE	Yes	Yes	Yes	Yes
Observations	40,331	313,386	313,386	313,386

Note: Each column represents results from a separate regression. The sample consists of rural residents between the ages of 15 and 59, interviewed from July 2004 to June 2005 and from July 2007 – March 2008. Search days, labor force participation days and work days are calculated using time allocation responses for the week preceding interview. Log casual wage and log salaried wage is the log of earnings per day worked for people who report working in casual labor and salaried work respectively. Daily casual wage and salaried work wage are deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Treatment indicates if district received NREGS in Phase I or II. Individual-level controls (age, age squared, gender, indicators for literacy), household-level controls (land possessed, religion and caste group), district-level control (rural fraction of population), year-quarter and district fixed effects are included in all regressions. All estimates are computed using

### Table 9.1.2 (cont'd).

sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 1% level.

		Prese Prese			
	(1)	(2)	(3)	(4)	(5)
	Log deflated	Labor force	Work	Search	Casual work
	casual wage	participation			
Post*Exposure	0.101**	0.037	-0.028	0.066	0.114
	(0.041)	(0.091)	(0.094)	(0.048)	(0.079)
Controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Quarter-year FE	Yes	Yes	Yes	Yes	Yes
Observations	14,362	120,851	120,851	120,851	120,851

Note: Each column represents results from a separate regression. The sample consists of rural residents between the ages of 15 and 59, interviewed from July 2004 to June 2005 and from July 2007 – March 2008. Search days, labor force participation days, work days and casual work are calculated using time allocation responses for the week preceding interview. Log casual wage is the log of earnings per day worked for people who report working in casual labor. Daily casual wage is deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Exposure is an indicator for whether district has at least one contiguous early-phase neighbor. Individual-level controls (age, age squared, gender, indicators for literacy, land possessed, and caste group), rural fraction of district population, year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 10% level.

	(1)	(2)	(3)	(4)	(5)		
	Log deflated	Labor force	Work	Search	Casual work		
	casual wage	participation					
Post*Exposure	0.039	0.010	-0.003	0.013	-0.017		
	(0.043)	(0.104)	(0.103)	(0.032)	(0.093)		
Controls	Yes	Yes	Yes	Yes	Yes		
District FE	Yes	Yes	Yes	Yes	Yes		
Quarter-year FE	Yes	Yes	Yes	Yes	Yes		
Observations	5,315	71,273	71,273	71,273	71,273		

Note: Each column represents results from a separate regression. The sample consists of rural residents between the ages of 15 and 59, interviewed from July 2004 to June 2005 and from July 2007 – March 2008. Search days, non-labor force participation days, work days and casual work are calculated using time allocation responses for the week preceding interview. Log casual wage is the log of earnings per day worked for people who report working in casual labor. Daily casual wage is deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Exposure is an indicator for whether district has at least one contiguous early-phase neighbor. Individual-level controls (age, age squared, gender, indicators for literacy, land possessed, and caste group), rural fraction of district population, year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 10% level.

- · · ·	(1)	(2)	(3)	(4)
	Log deflated	Labor force	Work	Search
	casual wage	participation		
Post = Oct 93 - June 00	0.165	-0.464**	-0.381*	-0.083
	(0.193)	(0.199)	(0.215)	(0.079)
Post = Jan 94 – June 00	0.183	-0.329*	-0.333*	0.004
	(0.132)	(0.178)	(0.181)	(0.047)
Post = Apr 94 – June 00	0.026	-0.249	-0.265	0.017
	(0.114)	(0.181)	(0.187)	(0.062)
Post = July 99 – June 00	-0.004	-0.188	-0.195	0.007
	(0.144)	(0.198)	(0.199)	(0.077)
Post = Oct 99 – June 00	0.038	-0.171	-0.117	-0.054
	(0.115)	(0.180)	(0.194)	(0.061)
Post = Jan 00 - June 00	-0.019	-0.090	-0.009	-0.081
	(0.107)	(0.141)	(0.149)	(0.063)
Post = Apr 00 - June 00	-0.012	-0.148	-0.112	-0.037
	(0.089)	(0.163)	(0.178)	(0.076)
Controls	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Quarter-year FE	Yes	Yes	Yes	Yes
Observations	19,628	194,117	194,117	194,117

 Table 9.4.1: Cross-District Spillovers in Pre-Study Period with Changing Cutoffs –

 Exposure Intensity

Note: Each row reports results from a separate specification and every cell represents results from a separate regression and. Each specification classifies a progressively shorter post-exposure period, ranging from the second sub-round of NSS 50 to the penultimate sub-round of NSS 55. The sample consists of Late district residents, interviewed from July 1993 to June 1994 and from July 1999 – March 2000. Search days, labor force participation days and work days are calculated using time allocation responses for the week preceding interview. Log casual wage is the log of earnings per day worked for individuals who report working as casual labor. Daily casual wage is deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Treatment indicates if district is Early. Exposure intensity measures share of assigned neighbors to all neighbors. Individual-level controls (age, age squared, gender, indicators for literacy), household-level controls (land possessed, religion and caste group), district-level control (rural fraction of population), year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 10% level.

	(1)	(2)	(3)	(4)
	Labor force	Work	Search	Casual work
	participation			
Post*Treat	0.149*	0.022	0.127	0.011
	(0.088)	(0.040)	(0.085)	(0.052)
Controls	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Quarter-year FE	Yes	Yes	Yes	Yes
Observations	56,083	56,083	56,083	56,083

 Table 9.5.1: Total Intent-to-Treat Effects on Child Labor – Partial Replication of Islam

 and Sivasankaran (2015)

Note: Each column represents results from a separate regression. The sample consists of children between the ages of 15 and 17, interviewed from July 2004 to June 2005 and from July 2007 – June 2008. Labor force participation days, work days and casual work are calculated using time allocation responses for the week preceding interview. Treatment is an indicator for whether district is assigned NREGS during the study period. Individual-level controls (age, age squared, gender, indicators for literacy, land possessed, and caste group), rural fraction of district population, year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 10% level.

	(1)	(2)	(3)	(4)	(5)
	Log deflated	Labor force	Work	Search	Casual work
	casual wage	participation			
Post*Exposure*High	0.167***	-0.217**	-0.168*	-0.049	-0.295***
	(0.044)	(0.086)	(0.086)	(0.036)	(0.068)
Post*Exposure*Low	0.079**	0.149*	0.060	0.089**	0.251***
	(0.032)	(0.085)	(0.085)	(0.037)	(0.066)
p-value (High=Low)	0.006	0.000	0.000	0.000	0.000
Controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Quarter-year FE	Yes	Yes	Yes	Yes	Yes
Observations	19,677	192,194	192,194	192,194	192,194

#### Table 9.6.1: Cross-District Spillovers, by Household Expenditure

Note: Each column represents results from a separate regression. The sample consists of individuals between the ages of 15 and 59, interviewed from July 2004 to June 2005 and from July 2007 – March 2008. The indicator for land is classified as zero if the household possesses 1 or more hectare of land. Search days, labor force participation days and work days are calculated using time allocation responses for the week preceding interview. Log casual wage is the log of earnings per day worked for individuals who report working in casual labor. Daily casual wage is deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Treatment indicates if district is Early. Exposure is a binary indicator for all districts with at least one Early neighbor. Individual-level controls (age, age squared, gender, indicators for literacy), household-level controls (land possessed, religion and caste group), district-level control (rural fraction of population), year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 1% level.

	(1)	(2)	(3)	(4)	(5)
	Log deflated	Labor force	Work	Search	Casual work
	casual wage	participation			
Post*Treat*High	0.216***	-0.179**	-0.179**	0.000	-0.263***
	(0.042)	(0.085)	(0.086)	(0.037)	(0.071)
Post*Treat*Low	0.118***	0.244***	0.141*	0.103***	0.260***
	(0.036)	(0.084)	(0.084)	(0.037)	(0.073)
Post*Exposure	0.090**	0.058	0.010	0.047	0.100
	(0.035)	(0.080)	(0.081)	(0.036)	(0.066)
p-value (High=Low)	0.000	0.000	0.000	0.000	0.000
Controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Quarter-year FE	Yes	Yes	Yes	Yes	Yes
Observations	50,538	451,884	451,884	451,884	451,884

 Table 9.6.2: Spillover-Robust Total Intent-to-Treat Effects (TITT), by Household

 Expenditure

Note: Each column represents results from a separate regression. The sample consists of individuals between the ages of 15 and 59, interviewed from July 2004 to June 2005 and from July 2007 – March 2008. The indicator for land is classified as zero if the household possesses 1 or more hectare of land. Search days, labor force participation days and work days are calculated using time allocation responses for the week preceding interview. Log casual wage is the log of earnings per day worked for individuals who report working in casual labor. Daily casual wage is deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Treatment indicates if district is Early. Exposure is a binary indicator for all districts with at least one Early neighbor. Individual-level controls (age, age squared, gender, indicators for literacy), household-level controls (land possessed, religion and caste group), district-level control (rural fraction of population), year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 1% level.

	(1)	(2)	(3)	(4)	(5)
	Log deflated	Labor force	Work	Search	Casual work
	casual wage	participation			
Post*Treat*High	0.223***	-0.079	-0.098	0.019	-0.222**
	(0.051)	(0.094)	(0.097)	(0.050)	(0.088)
Post*Treat*Low	0.130***	0.187**	0.081	0.106**	0.228***
	(0.044)	(0.089)	(0.091)	(0.049)	(0.087)
Post*Exposure	0.103**	0.048	-0.015	0.062	0.110
	(0.044)	(0.090)	(0.093)	(0.048)	(0.080)
p-value (High=Low)	0.000	0.000	0.000	0.000	0.000
Controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Quarter-year FE	Yes	Yes	Yes	Yes	Yes
Observations	40,883	313,886	313,886	313,886	313,886

 Table 9.6.3: Spillover-Robust Rural Intent-to-Treat Effects (ITT), by Household Expenditure

## Table 9.6.3 (cont'd).

Note: Each column represents results from a separate regression. The sample consists of rural individuals between the ages of 15 and 59, interviewed from July 2004 to June 2005 and from July 2007 – March 2008. The indicator for land is classified as zero if the household possesses 1 or more hectare of land. Search days, labor force participation days and work days are calculated using time allocation responses for the week preceding interview. Log casual wage is the log of earnings per day worked for individuals who report working in casual labor. Daily casual wage is deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Treatment indicates if district is Early. Exposure is a binary indicator for all districts with at least one Early neighbor. Individual-level controls (age, age squared, gender, indicators for literacy), household-level controls (land possessed, religion and caste group), district-level control (rural fraction of population), year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 1% level.

•	(1)	(2)	(3)	(4)	(5)
	Log deflated	Labor force	Work	Search	Casual work
	casual wage	participation			
Post*Treat*Large	0.104*	0.195*	0.159	0.036	-0.282***
	(0.060)	(0.108)	(0.110)	(0.041)	(0.085)
Post*Treat*Small	0.126***	0.142*	0.060	0.082**	0.168**
	(0.036)	(0.081)	(0.082)	(0.037)	(0.073)
Post*Exposure	0.091**	0.057	0.010	0.047	0.097
	(0.035)	(0.080)	(0.081)	(0.036)	(0.067)
p-value (Large=Small)	0.149	0.454	0.194	0.064	0.000
Controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Quarter-year FE	Yes	Yes	Yes	Yes	Yes
Observations	51,231	451,884	451,884	451,884	451,884

# Table 9.7.1: Spillover-Robust Total Intent-to-Treat Effects (TITT), by Land Possession

Note: Each column represents results from a separate regression. The sample consists of individuals between the ages of 15 and 59, interviewed from July 2004 to June 2005 and from July 2007 – March 2008. The indicator for land is classified as zero if the household possesses 1 or more hectare of land. Search days, labor force participation days and work days are calculated using time allocation responses for the week preceding interview. Log casual wage is the log of earnings per day worked for individuals who report working in casual labor. Daily casual wage is deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Treatment indicates if district is Early. Exposure is a binary indicator for all districts with at least one Early neighbor. Individual-level controls (age, age squared, gender, indicators for literacy), household-level controls (land possessed, religion and caste group), district-level control (rural fraction of population), year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 1% level.
	(1)	(2)	(3)	(4)	(5)
	Log deflated	Labor force	Work	Search	Casual work
	casual wage	participation			
Post*Treat*Large	0.151**	0.110	0.051	0.059	-0.232**
	(0.066)	(0.110)	(0.114)	(0.051)	(0.101)
Post*Treat*Small	0.136***	0.146*	0.052	0.094*	0.188**
	(0.044)	(0.089)	(0.090)	(0.049)	(0.087)
Post*Exposure	0.103**	0.049	-0.014	0.063	0.112
	(0.044)	(0.090)	(0.093)	(0.048)	(0.080)
p-value (High=Low)	0.779	0.592	0.988	0.178	0.000
Controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Quarter-year FE	Yes	Yes	Yes	Yes	Yes
Observations	40,331	313,886	313,886	313,886	313,886

Table 9.7.2: Spillover-Robust Rural Intent-to-Treat Effects (ITT), by La	and F	Possessior
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Note: Each column represents results from a separate regression. The sample consists of rural

individuals between the ages of 15 and 59, interviewed from July 2004 to June 2005 and from July 2007 – March 2008. The indicator for land is classified as zero if the household possesses 1 or more hectare of land. Search days, labor force participation days and work days are calculated using time allocation responses for the week preceding interview. Log casual wage is the log of earnings per day worked for individuals who report working in casual labor. Daily casual wage is deflated using the monthly, state-level price index for agricultural laborers from the Indian Labour Bureau. Treatment indicates if district is Early. Exposure is a binary indicator for all districts with at least one Early neighbor. Individual-level controls (age, age squared, gender, indicators for literacy), household-level controls (land possessed, religion and caste group), district-level control (rural fraction of population), year-quarter and district fixed effects are included in all regressions. All estimates are computed using sampling weights. Standard errors are clustered at district-level to control for intra-district correlation. \*\*\*Significant at 1% level, \*\*Significant at 5% level, \*Significant at 1% level.



APPENDIX B ADDITIONAL FIGURES Figure 9.1: Rural and Urban Casual Wage in Early and Late Districts

Source: NSS 61, 64

Figure 9.2: Rural and Urban Labor Force Participation in Early and Late Districts





Figure 9.3: Rural and Urban Work in Early and Late Districts

Source: NSS 61, 64 Figure 9.4: Work in Early and Late Districts





Figure 9.5: Search in Urban and Rural Areas of Early and Late Districts

Source: NSS 61, 64





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